

THE OXFORD HANDBOOK OF

CLASSICS

IN PUBLIC

POLICY AND

ADMINISTRATION



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Edited by
STEVEN J. BALLA, MARTIN LODGE,
and
EDWARD C. PAGE

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CHAPTER 1

WHAT MAKES A CLASSIC?

Identifying and Revisiting the Classics of Public Policy and Administration

STEVEN J. BALLA, MARTIN LODGE, AND
EDWARD C. PAGE

A few years ago, Dominic Byatt at Oxford University Press approached us about putting together a handbook of classic works in public policy and administration. We were immediately intrigued by the prospect of identifying and revisiting key works that have fundamentally shaped subsequent research in the field.

Our specific task was to select a number of works and then commission contributors to compose chapters summarizing and assessing, in whatever way they deemed fit, the central contribution and legacy of each classic. This task is perhaps unusual in that the resulting volume was not to be an anthology of classic works themselves. Rather, in what represents a first for Oxford University Press, the volume features contemporary scholars reflecting upon key books and articles that have profoundly influenced research in their areas of inquiry.

Given the unprecedented nature of this task, we had a number of decisions to make at the outset. How many classics should be included in the volume? How long should each chapter be? Most fundamentally, what works should we identify as classics of public policy and administration? Our expectation was that individual contributors would make claims about why their particular works are viewed as classics. But how should we approach the task of identifying classic works as a general matter?

STANDARDS FOR IDENTIFYING CLASSICS

Working without any template, we recognized that there are a variety of ways in which classic pieces of scholarship might be identified. At first glance, the notion of a classic appears relatively straightforward. Classics should address important issues,

put forward seminal theoretical ideas, assemble novel empirical evidence, spawn fruitful research programs, and carry practical applications for real-world concerns. Of course, not all works that might reasonably be considered classics will necessarily meet each one of these standards to an equivalent degree. In addition, each of these standards might reasonably be operationalized in a variety of systematic ways. Although such operationalizations exist, we did not use them in assembling our list of classics. Let us explain why.

One indication of classic status is external recognition at the highest level. For example, the Nobel Prize might serve as a useful marker in identifying research that has fundamentally shaped understanding and subsequent research in an area of inquiry. Although a number of contributors to the field of public policy and administration have been recognized in this way (such as Elinor Osrom, Herbert Simon, George Stigler, and Oliver Williamson, each of whom has authored a classic work included in the volume), many others who have by almost any reckoning been seminal in shaping the field (Aaron Wildavsky, who is represented two times in this volume, comes to mind) have not been so honored. In the end, given that there is no equivalent to the Nobel Prize in Economics in other disciplines that contribute to the study of public policy and administration, referencing this award is of limited utility in assembling the volume's list of classic works.

This "honors" approach might be expanded to include relevant members of organizations such as the American Academy of Arts and Sciences, Académie des Sciences Morales et Politiques, and British Academy for the Humanities and Social Sciences. The standards used by these organizations in deciding upon award recipients, however, do not match our task of identifying classic works. Although such awards confer significant professional recognition upon recipients, they do not carry any necessary insight into the status of particular pieces of scholarship as classics.

A second standard for judging the significance of academic research is the quality of the publication outlet. Publication of books and articles in highly regarded university presses and academic journals, such as Yale University Press and the *American Political Science Review*, certainly represent important achievements in scholarly careers. Exceedingly few works published in these outlets, however, attain the kind of attention and impact one would expect of a classic, making this heuristic of little use in drawing up a list of classic works.

A third standard for determining what constitutes a classic is citation counts as measured via online resources such as Google Scholar, Web of Science, and Scopus (Bakkalbasi et al. 2006). As a publicly available service, Google Scholar is a particularly convenient way of assessing the extent to which books and articles have received peer recognition through citation. Each citation count index has its own strengths and weaknesses, based on attributes such as coverage of sources, search algorithms, and language biases (Neuhaus et al. 2006). More generally, disciplines display considerable range in terms of citation counts of works in the field. In addition, citation "cartels," gender effects, and university incentive systems affect citation practices across areas of inquiry. Finally, a strict reliance on citation counts runs the risk of passing over those

works whose contributions have become so classic and mainstream that references are no longer made to these original sources. This phenomenon has been called “obliteration by incorporation” (Merton 1979). In the end, although citation counts offer insight into how widely works have been recognized in contributing to subsequent research, it is advisable to heed an old warning about the “promiscuous and careless use of quantitative citation data . . . It is preposterous to conclude blindly that the most cited author deserves a Nobel prize” (Garfield 1963, as cited in Merton 1979).

A fourth standard for identifying classics is to rely on the judgment of experts in the field. One such approach would be to conduct a survey of scholars. Another approach would be to assemble relevant syllabi and reading lists. Finally, one might draw from existing lists and discussions of canonical, great works in the field (e.g. Sherwood 1990; Kasdan 2012; Fan 2013). Critical evaluations of existing efforts (e.g. Meier and O’Toole 2012; Raadschelders 2012), however, point to the need for an alternative approach. This need is especially salient given our interest in identifying works that fundamentally opened up areas of public policy and administration to ongoing social science research. To the extent that our objective is distinctive, previous efforts, although informative, are ultimately of limited utility in precisely specifying our list of classics.

OUR REPUTATIONAL METHOD

In the end, our approach in assembling a list of classics in public policy and administration is a variant of the reputational method that has been used in other fields (e.g. Hunter 1953). Reliance on reputation as a means of identifying classic works is not without its shortcomings. Works associated with scholars with well-established reputations might naturally be considered classics over comparable works by lesser known figures in the field. This phenomenon has been called the “Matthew Effect” (Merton 1968), in reference to the following passage from the Gospel of Matthew: “For unto every one that hath shall be given, and he shall have abundance: but from him that hath not shall be taken away even that which he hath.” This “rich get richer and poor get poorer” phenomenon in attributing credit for classic contributions is likely a byproduct of a range of peculiarities and distortions in the marketplace for academic intellectual property.

To implement our reputation-based approach, we devised a process for identifying works that the three of us could agree have fundamentally shaped understanding and influenced subsequent research in the field of public policy and administration. The seminal nature of these particular works will not be universally recognized among contemporary scholars, and works that are not included in the volume are undoubtedly considered classics by some of our peers. Such disagreement underscores that there is no objective standard for determining what constitutes a classic in a field of scholarly inquiry.

Our method consisted of each of the three of us independently drawing up lists of works that we would be inclined to include in a volume of 40 or so classics in public

policy and administration. Our initial selections reflected to a significant degree not only our own personal tastes, but also works that we believed had some chance of being ratified by our fellow editors. We were also cognizant of making selections that we deemed would be plausible, from the perspective of readers of the volume, for inclusion in a collection of classic works. Our initial lists collectively contained 104 pieces of scholarship. There was complete, three-way agreement on 14 selections, which were immediately noted for inclusion in the volume.

To fill in the remaining chapters, we considered one another's detailed statements regarding the merits of inclusion of works that we ourselves had not initially identified. At this stage, we also weighed issues other than substantive merit in isolation. For example, we sought to avoid the inclusion of multiple works that essentially cover the same area of research in public policy and administration (e.g. given that Jack Walker's "The Diffusion of Innovations among the American States" was to be included, we left Virginia Gray's seminal "Innovation in the States: A Diffusion Study" off of the list). We were cognizant of geographic origins, specifically targeting for inclusion works produced by scholars from outside of the United States. We paid heed to publication dates, seeking to avoid both the idea that age itself confers classic status and the contrary notion that serious social science research did not commence until a particular starting point of our own liking.

Our final list included 46 classics, including 32 works that acquired two or three endorsements after the second stage of deliberation. There were two forms of attrition that explain why there are fewer than 46 classics covered in the volume. In some instances, we were unable to secure a commitment from the contributors whom we targeted. In other instances, scholars who initially indicated their willingness to contribute to the volume ultimately did not produce their intended chapters.

Given these considerations, the works included in the volume consist of contributions for which the three of us could together make a plausible case for inclusion. They had to pass muster with three scholars who come from varied academic backgrounds, have different areas of substantive expertise, and are inclined toward a number of disparate methodological practices. Our collective breadth is also reflected in the fact that we represent two continents, Europe and North America, that have historically produced seminal contributions in public policy and public administration. A different set of editors would certainly have come up with a different list, but our selected works, both individually and as a collection, warrant the kind of attention entailed in a volume assessing classic works in the field of public policy and administration.

THE LIST OF CLASSICS

Table 1.1 presents a list of the volume's classics organized by citation counts. Rather than present precise citation counts for each individual work, the table organizes the works into five categories: 25,000 or more citations, 10,000 or more citations, 5,000 or

Table 1.1 Classics in public policy and public administration, by Google Scholar citation count

25,000 or More Citations

Oliver E. Williamson, *Markets and Hierarchies: Analysis and Antitrust Implications*

Robert D. Putnam, *Bowling Alone: The Collapse and Revival of American Community*

Mancur Olson, *The Logic of Collective Action: Public Goods and the Theory of Groups*

10,000 or More Citations

Herbert A. Simon, *Administrative Behavior: A Study of Decision-Making Processes in Administrative Organization*

Elinor Ostrom, *Governing the Commons: The Evolution of Institutions for Collective Action*

Albert O. Hirschman, *Exit, Voice, and Loyalty: Responses to Decline in Firms, Organizations, and States*

John W. Kingdon, *Agendas, Alternatives, and Public Policies*

5,000 or More Citations

George J. Stigler, "The Theory of Economic Regulation"

Charles E. Lindblom, "The Science of 'Muddling Through'"

Graham T. Allison, *The Essence of Decision: Explaining the Cuban Missile Crisis*

Michael Lipsky, *Street-Level Bureaucracy: Dilemmas of the Individual in Public Service*

Michael D. Cohen, James G. March, and Johan P. Olsen, "A Garbage Can Model of Organizational Choice"

Peter A. Hall and David Soskice, *Varieties of Capitalism: The Institutional Foundations of Comparative Advantage*

E.E. Schattschneider, *The Semi-Sovereign People: A Realist's View of Democracy in America*

Christopher Hood, "A Public Management For All Seasons?"

C. Wright Mills, *The Power Elite*

1,000 or More Citations

Jeffrey L. Pressman and Aaron Wildavsky, *Implementation*

David B. Truman, *The Governmental Process: Political Interests and Public Opinion*

Frank R. Baumgartner and Bryan D. Jones, *Agendas and Instability in American Politics*

Michael Crozier, *The Bureaucratic Phenomenon*

James Q. Wilson, *Bureaucracy: What Government Agencies Do and Why They Do It*

Theodore J. Lowi, *The End of Liberalism: The Second Republic of the United States*

Aaron Wildavsky, *The Politics of the Budgetary Process*

Ian Ayres and John Braithwaite, *Responsive Regulation: Transcending the Deregulation Debate*

(Continued)

Table 1.1 (Continued)

Theodore J. Lowi, "American Business, Public Policy, Case-Studies, and Political Theory"

Mathew D. McCubbins and Thomas Schwartz, "Congressional Oversight Overlooked: Police Patrols versus Fire Alarms"

Anthony Downs, "Up and Down with Ecology: The 'Issue-Attention' Cycle"

Hugh Hecllo, "Issues Networks and the Executive Establishment"

Mathew D. McCubbins, Roger G. Noll, and Barry R. Weingast, "Administrative Procedures as Instruments of Political Control"

Terry M. Moe, "The New Economics of Organization"

Jack L. Walker, "The Diffusion of Innovations among the American States"

Paul A. Sabatier, "An Advocacy Coalition Framework of Policy Change and the Role of Policy-Oriented Learning Therein"

Fritz W. Scharpf, "The Joint-Decision Trap: Lessons from German Federalism and European Integration"

V.O. Key, Jr., *Public Opinion and American Democracy*

Less Than 1,000 Citations

Herbert Kaufman, *The Forest Ranger: A Study in Administrative Behavior*

Thomas R. Dye, *Politics, Economics, and the Public: Policy Outcomes in the American States*

Richard Rose, *Do Parties Make a Difference?*

Robert K. Merton, et al., *Reader in Bureaucracy*

Harold D. Lasswell, *The Decision Process: Seven Categories of Functional Analysis*

Carol H. Weiss, *Evaluation: Methods for Studying Programs and Policies*

more citations, 1,000 or more citations, and fewer than 1,000 citations. To assemble these citation counts, which are accurate as of December 2013, we searched by title for each work on Google Scholar. The counts include only citations of the original books or articles themselves, not of closely related prior or subsequent works by the authors of the classics. For example, Robert Putnam's book *Bowling Alone* was preceded by an article bearing the same primary title. In the table, the *Bowling Alone* citation count is drawn only from the book, which we have deemed to be the classic contribution in this particular research program. Similarly, Charles Lindblom followed up his classic article "The Science of 'Muddling Through'" with a subsequent article, "Still Muddling, Not Yet Through." Although well-cited in its own regard, the follow-up article is not included in the citation count for this classic contribution.

The single most referenced work in the volume is Oliver Williamson's *Markets and Hierarchies*, which has garnered in excess of 28,000 citations. Robert Putnam's *Bowling Alone* and Mancur Olson's *The Logic of Collective Action* also have been cited a comparable number of times. All of the classics that have been cited more than 10,000 times are books. The most referenced article in the collection is George Stigler's "The Theory of Economic Regulation," which has been cited approximately 8,000 times. Robert Putnam's "bowling alone" project offers an especially instructive comparison. Published as an article, the project has garnered nearly 11,000 citations. As substantial as this count is on its own merit, it pales in comparison to the more than 27,000 times that the book version of the project has been cited.

Six of the volume's works, all of which pass muster as classic contributions by a number of relevant standards, have received fewer than 1,000 citations. The bottom line is that citation counts, although readily available and useful in certain respects, constitute by our reckoning an incomplete gauge when it comes to assessing the place of pieces of scholarship in particular areas of inquiry.

What other patterns emerge from our list of classics? Consistent with our orientation, many of the works have influenced subsequent research across the field of public policy and administration. Albert Hirschman's *Exit, Voice, and Loyalty* is emblematic in this regard. Hirschman's seminal contribution has been acknowledged in research on such disparate topics as war, urban poverty, and the management of organizations. Another example of a work that has had wide-ranging influence is Jack Walker's "The Diffusion of Innovations among the American States." This article introduced the study of the diffusion of innovations into a variety of areas of the field of public policy and administration, ranging from domestic programs to international norms and organizations.

Some works on the list are classics by virtue of their absolutely seminal contributions to specific areas of research. Although the influence of such seminal works might not reach beyond these areas, their contributions are nevertheless considered classic by virtue of the fact that they have redefined research agendas on fundamentally important topics in public policy and administration. One example in this regard is "Administrative Procedures as Instruments of Political Control" by Mathew McCubbins, Roger Noll, and Barry Weingast. This article established an agenda for a rational choice, new institutionalism approach to understanding and examining administrative structure and process. This approach, which offers an alternative to legalistic, normative perspectives, has fundamentally affected the trajectory of research on political control of bureaucracy, especially in the context of the United States separation of powers system.

Some works on the list are classics, at least in part, because they have become "must reads" that researchers in the field of public policy and administration would be embarrassed to admit publicly they have not read. Works meeting this criterion typically appear on the syllabi for core courses in graduate programs in public policy

and administration. In addition to being one of the more highly cited classics on our list, John Kingdon's *Agendas, Alternatives, and Public Policies* is also an example of a work that it is widely considered required reading for scholars of public policy and administration.

The works on our list of classics vary dramatically in their publication dates. The vast majority of the works were published decades ago. A number of works, in fact, are more than a half-century old. Examples of such historic works include the *The Governmental Process* by David Truman (1951) and *The Forest Ranger* by Herbert Kaufman (1960). On the other extreme, two works included in the volume—Robert Putnam's *Bowling Alone* and Peter Hall and David Soskice's *Varieties of Capitalism*—have been published since 2000, which is quite recent by academic standards of the passing of time.

The presence of such recently published works suggests that what constitutes a classic in public policy and administration evolves over time, with new works joining the established canon. Is, however, the list simply growing in length, or are works falling off the list as time passes? One indication of attrition and replacement is that foundational works from very early in the study of public policy and administration, such as Max Weber's *Economy and Society*, do not appear on the list.

Our list of works suggests that the crafting of modern-day classics is especially likely in areas in which new research agendas draw off of seminal developments in domestic and global economics, politics, and society. Seminal events and trends such as climate change, the end of the Cold War, the spread of the internet, the September 11, 2001 terrorist attacks, and the global financial crisis present new realities and challenges that naturally attract the attention of academic researchers. Pieces of scholarship that arise as byproducts of these events and trends attain classic status not because of the underlying importance of the novel issues they address. Rather, it is the way in which these works, in the process of responding to world developments, develop seminal theoretical ideas and assemble novel empirical evidence that defines their contributions as classic. An example along these lines is *Agendas and Instability in American Politics* by Frank Baumgartner and Bryan Jones. This book documents the rise of consumer, environmental, and other public interest movements during the 1960s and 1970s. In tracking these developments, the book makes a seminal contribution to research on agenda setting and the processes through which policy monopolies are created and destroyed, synthesizing research programs that had to that point been viewed as discrete, incompatible perspectives on political mobilization and institutions.

IMPLICATIONS FOR THE FIELD OF PUBLIC POLICY AND ADMINISTRATION

How, in the end, might this collection of classics contribute to understanding of the field of public policy and administration? In addition to its most direct contribution of offering dozens of original assessments of classic works, the volume is poised to

make four broad contributions. These contributions concern the way in which classics are perceived, the way in which key questions in public policy and administration are identified and conceptualized, the way in which the field is related to economics, political science, and other social science disciplines, and the ways in which future classics volumes might be similar and different.

Turning first to the way in which classics are understood, one unanticipated outcome of our reputational selection process was the substantial agreement between the three of us regarding what specific works constitute classics of public policy and administration. This agreement was underscored when we circulated our list of classics to scholars whom we were recruiting as chapter contributors. Given that we experienced little overall disagreement, it appears that there is something of a core of classic works that is understood by scholars in the field, regardless of their geographic location, substantive expertise, and methodological orientation. What unites this core is the assembling of theoretical ideas and empirical evidence that have relevance beyond a single sector, national experience, or area of public policy and administration.

Second, although consisting of substantively disparate works, our collection of classics reveals a number of commonalities in the way in which scholars have approached the study of public policy and administration. One such commonality is the aforementioned correspondence of research priorities with developments in economics, politics, and society. Other commonalities are substantive, in that researchers in public policy and administration have, across the generations, demonstrated ongoing interest in topics such as the determinants of public policy and the effects of institutions on decision-making.

As a case in point, earlier classics, such as Robert Merton et al.'s *Reader in Bureaucracy* and Herbert Simon's *Administrative Behavior*, were concerned with the effects of bureaucratic organization. By contrast, later classics, such as Elinor Ostrom's *Governing the Commons* and George Stigler's "The Theory of Economic Regulation," reflected the emergence of more critical views regarding the role of government and hierarchy. These later classics not only reflected the economic, political, and social realities of their generation, they also influenced government policy in areas as disparate as deregulation and international aid.

Third, the volume contributes to discussions regarding the relationship of the field of public policy and administration to social science disciplines. This contribution especially holds in the context of political science, given that all three of us are political scientists by training. We therefore have not brought the perspective of economists, lawyers, sociologists, or other disciplinary specialists to the task of identifying classic works in public policy and administration.

In the United Kingdom, public administration has, over time, remained central to the activities of political science departments. By contrast, in North America and continental Europe, public administration has for decades been institutionally separate from political science. This separation is not without consequence, as it has been pointed out that within political science there is often a lack of awareness of insights generated by research in public administration (Meier 2007). In continental Europe, the separation stems from a much earlier demonstrated interest on the part of

academics in public administration, and its relationship to the study of the law, than in political science. On the other side of the Atlantic, a key driver of the separation was the notion of establishing public administration as an endeavor distinct from political decision-making (Roberts 1994). In addition, North America witnessed the emergence of public policy schools, offering analytical skills that were not necessarily relevant in the context of questions that are at the heart of political science research (Lynn 1996). Our collection of classics, by bringing together works that are of relevance to both the field of public policy and administration and the related social science discipline of political science, demonstrates areas of common concern among academic communities that have historically been fragmented across university departments and professional associations.

In displaying the shared research concerns of political science and public policy and administration, the collection of classics by itself does not take a stance in ongoing substantive and methodological debates. To take one example, it is often the case that political science treats bureaucracy as a problem for democracy, while public administration scholarship generally regards professional bureaucracy as a beneficial force that is threatened by politics. The volume's classic works collectively suggest that key questions in public policy and administration concern both the politics of organization and the organization of politics. Such concerns ultimately render the study of public policy and administration inseparable from political science inquiry, regardless of theoretical perspective or methodological disposition.

Finally, our collection of classics naturally invites speculation regarding what a similar volume might look like in, say, 50 years. Although such prediction is fraught with inherent difficulty, we would expect modest changes in a number of directions, both in terms of the characteristics of the authors of classic works and the substantive content of these pieces of scholarship. One expectation is for greater diversity in the gender, race, and ethnicity of authors of classic works, as the composition of scholars of public policy and administration continues to diversify. Another possibility is that future assemblies of authors will reflect the ongoing globalization that is occurring both in higher education and in economics, politics, and society more generally.

In terms of substance, and building off of recent developments in various social sciences, we might expect that future collections of classics will reflect "behavioral" approaches to the study of public policy and administration. In addition, we suspect that works incorporating particular topics will appear more regularly in collections of classics, given the increasing importance of these topics in world affairs and thus their increasing level of attention on the part of researchers in public policy and administration. Such themes might include organizational issues in development, the causes and consequences of the application of technology in public policy and administration, and environmental issues such as sustainability. Across areas of inquiry, it is not unreasonable to expect the emergence of "big data" to influence the kinds of analysis that are conducted in works that go on to be considered classics. Relatedly, the proliferation of information about heretofore difficult to access polities and societies, such as China,

Brazil, and the African continent, can be expected to lead to the incorporation of such contexts into a global research agenda on public policy and administration.

While acknowledging these possible directions for future research in public policy and administration, our overarching expectation is for stability in what is considered a classic work in the field. At its core, the field is characterized by fundamental concerns that are unlikely to go away any time soon. Issues such as the framing of problems and solutions, who has access to decision-making processes, and the effects of institutional structures on political and administrative outcomes will continue to be at the heart of the field into the foreseeable future.

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CHAPTER 2

HERBERT A. SIMON, *ADMINISTRATIVE BEHAVIOR: A STUDY OF DECISION-MAKING PROCESSES IN ADMINISTRATIVE ORGANIZATION*

MICHAEL MINTROM

HERBERT Simon (1916–2001) was an intellectual giant, best known for contributing the concepts of “bounded rationality” and “satisficing” to the vocabulary of the social and behavioral sciences. Simon trained as a political scientist at the University of Chicago in the 1930s and early 1940s, where Harold Lasswell and Charles Merriam—both major contributors to the development of political science—served as mentors. Early in his academic career, Simon engaged in extensive, empirically grounded studies of city governance and administration. His close observation of local political hierarchies and their service delivery systems generated several books and contributed to his doctoral studies. *Administrative Behavior*, first published in 1947, had its origins in his doctoral dissertation. The book was highly influential. Over the subsequent fifty years, it appeared in three further editions (1957, 1976, and 1997). Each edition retained the original 1947 text but augmented it in various ways. The 1997 edition—the edition referenced here—contained commentaries by Simon at the end of each of the original chapters. Those commentaries discussed developments in Simon’s thinking and that of others during the half-century from when the book first appeared. In Simon’s own judgment, *Administrative Behavior* laid the foundation for all the subsequent developments in his own research. It stimulated much research by others. That research contributed

to advances in at least five separate disciplines: political science, economics, cognitive psychology, computer science, and artificial intelligence. The influence of *Administrative Behavior* continues in various streams of research either relevant to, or falling within, the fields of public policy and public administration. Toward the end of this chapter, four legacies are noted. They concern the study of political decision-making, agenda setting, choice architecture, and the development of expertise.

In *Administrative Behavior*, Herbert Simon showed how organizations can be understood in terms of their decision processes. Simon self-consciously cast himself as a scientist. “[T]his volume deals with the anatomy and physiology of organization and does not attempt to prescribe for the ills of the organization. Its field is organizational biology, rather than medicine; and its only claim of contribution to the practical problems of administration is that sound medical practice can only be founded on thorough knowledge of the biology of the organism” (p. 305). The starting point for Simon’s science was the psychology of decision-making. In his own words: “[D]ecision-making is the heart of administration, and . . . administrative theory must be derived from the logic and psychology of human choice” (p. xi). Simon saw himself as contributing to an intellectual tradition where others before him had engaged in close analysis of administrative behavior. He acknowledged as influences the writings of Chester Barnard (1938), Luther Gulick (1937), and Frederick Taylor (1915). Yet Simon saw establishing a science of administration as an essential corrective to the ad hoc amassing of “principles”—or what he preferred to term “proverbs”—of administration. The “principles” commonly found in the extant literature referred to aspects of task specialization, unity of control, and span of control in organizations. Simon viewed those “principles” as frequently contradictory. *Administrative Behavior* set forth “a vocabulary . . . for the description of administrative organization”. It also took acknowledgment of “the limits of rationality” as the starting point for developing “a complete and comprehensive enumeration of the criteria that must be weighed in evaluating an administrative organization” (p. 47).

BOUNDED RATIONALITY AND SATISFICING

Herbert Simon received the Nobel Prize in Economics in 1978. The award honored his contributions to knowledge of decision-making in organizations. Simon’s most notable contribution to the economics literature was “A Behavioral Model of Rational Choice” published in the *Quarterly Journal of Economics* in 1955. That article elaborated on ideas concerning the bounded nature of rationality and its implications as first articulated in *Administrative Behavior*. Simon’s emphasis on bounded rationality challenged orthodoxy in economics. He asserted that, in the presence of cognitive limitations, decision-makers “satisfice” rather than “maximize.” That is, they tend to choose actions that are “good enough” rather than engage in exhaustive comparison of all possible alternatives. Satisficing typically involves rapid elimination of alternatives

in a hierarchical fashion, meaning that certain decision-paths are completely ignored, and opening the risk that any chosen action may be significantly less desirable than others that were not considered. Nonetheless, given search and decision costs, the practice of satisficing appears more efficient in many instances than does the pursuit of maximal outcomes. This view is at odds with the full rationality assumption central to mid-twentieth-century mainstream microeconomics, and which underlies traditional models of individual choice and market processes.

Simon rehearsed his argument concerning rationality in chapters 4 and 5 of *Administrative Behavior*. Chapter 4, “Rationality in Administrative Behavior” established that “[t]he problem of choice is one of describing consequences, evaluating them, and connecting them with behavioral alternatives” (p. 85). Chapter 5, “The Psychology of Administrative Decisions” presented the book’s fundamental intellectual contribution. Here we find a discussion of the limits of individual rationality and the means by which organizations can structure the “environment of choice” (p. 92). Simon claimed that individuals are both boundedly rational and intendedly rational, and that appropriate organizational forms can promote more rational decision-making.

Simon opened chapter 5 by stating: “It is impossible for the behavior of a single, isolated individual to reach any high degree of rationality. The number of alternatives he must explore is so great, the information he would need to evaluate them so vast that even an approximation to objective rationality is hard to conceive” (p. 92). This was the human predicament as Simon saw it. To elaborate:

- (1) Rationality requires a complete knowledge and anticipation of the consequences that will follow on each choice. In fact, knowledge of consequences is always fragmentary.
- (2) Since these consequences lie in the future, imagination must supply the lack of experienced feeling in attaching value to them. But values can be only imperfectly anticipated.
- (3) Rationality requires a choice among all possible alternative behaviors. In actual behavior, only a very few of all these possible alternatives ever come to mind. (pp. 93–4)

The human limits are both cognitive and informational. All relevant information about future consequences of actions cannot be readily accessed. Even if it could be, cognitive limits would inhibit effective analysis in service of good decision-making. The number of alternatives to be assessed stretch brain power. However, while bounded in their rationality, Simon recognized that humans do intend to make good decisions. That intended rationality drives the search for improved methods of decision-making. Individuals take various actions with the purpose of improving the rationality of their choices. These include remembering (including making use of informal and formal libraries, files, and records), habit formation (which allows conservation of mental effort), and conscious direction of attention. Yet deliberate social organization is the

mechanism that does most to raise the rationality both of individuals and of groups. The reason lies in the ability of organizations to channel and focus the attention of individuals, allowing for the development of expertise and the coordination of individual actions. Commenting on his original discussion of rationality, Simon observed: “The central concern of administrative theory is with the boundary between the rational and the nonrational aspects of human social behavior. Administrative theory is peculiarly the theory of intended and bounded rationality—of the behavior of human beings who satisfice because they have not the wits to maximize” (p. 118). Simon suggested that “organizations permit the individual to approach reasonably near to objective rationality” (p. 93) and the bulk of chapter 5 of *Administrative Behavior* is devoted to an explanation of how they do so.

Individual purposive behavior can compensate somewhat for bounded rationality. The capacity to learn—what Simon terms “docility”—is central to the human ability to do so. “By the use of experimental method, by communication of knowledge, by theoretical prediction of consequences, a relatively little bit of experience can be made to serve as the basis for a wide range of decisions. As a result a remarkable economy of thought and observation is achieved” (p. 99). Organizations compensate further for individual limitations through deliberate construction and control of the environment of individual choice. This involves systematic planning, task allocation, communication, and control. Simon observed that the organizational influences on the individual are of two kinds:

- (1) Organizations and institutions permit stable expectations to be formed by each member of the group as to the behavior of the other members under specified conditions. Such stable expectations are an essential precondition to a rational consideration of the consequences of action in a social group.
- (2) Organizations and institutions provide the general stimuli and attention-directors that channel the behaviors of the members of the group, and that provide those members with the intermediate objectives that stimulate action. (p. 110)

From these observations, Simon concluded that organizations are fundamental to the achievement of human rationality. In his words:

The rational individual is, and must be, an organized and institutionalized individual. If the severe limits imposed by human psychology upon deliberation are to be relaxed, the individual must in his decisions be subject to the influence of the organized group in which he participates. His decisions must not only be the product of his own mental processes, but also reflect the broader considerations to which it is the function of the organized group to give effect. (p. 111)

Subsequent chapters in *Administrative Behavior* explored in greater detail how specific organizational structures and practices serve the greater social purpose of achieving rational decision-making.

ADMINISTRATIVE BEHAVIOR: THE STRUCTURE OF THE ARGUMENT

Simon divided *Administrative Behavior* into eleven chapters. Chapter 1, “Decision-Making and Administrative Organization,” provided an introduction to the argument and introduced major topics to come. Chapter 2, “Some Problems of Administrative Theory,” served to situate Simon’s thesis within the broader tradition and contemporary concerns of public administration scholarship. It is here where the claim is first made for placing decision-making at the heart of the analysis of administrative behavior. Chapter 3, “Value and Fact in Decision-Making,” observed that the administrative practice of getting things done would be meaningless in the absence of goals or objectives. Simon referred to the selection of goals and objectives as “value judgments.” In contrast the selection of strategies intended to achieve specific goals and objectives he termed “factual judgments.” As already noted, chapter 4, “Rationality in Administrative Behavior,” set forth Simon’s reasons for focusing on decision-making in organizations, and for using the decision as the central unit of analysis for understanding administrative practices. Again as already noted, chapter 5, “The Psychology of Administrative Decisions,” laid out the limits of individual rationality, suggested ways that compensation could be made for those limits, and presented appropriately designed institutions and organizations as the means by which societies can approximate rational decision-making on the part of both individuals and collectivities.

In the second half of *Administrative Behavior*, Simon developed a series of elaborations upon selected themes introduced in chapters from the first half. Chapter 6, “The Equilibrium of the Organization,” explored why individuals would be prepared to conform their own actions to broader organizational goals. Simon noted that wages served as a primary motivation for employees, but suggested that other inducements were also necessary, such as good will, loyalty, and prestige. Chapter 7, “The Role of Authority,” explored the typical division of labor in organizations, noting the traditional hierarchy of decision-making and why it was efficient. Chapter 8, “Communication,” elaborated on the theme developed earlier that effective interaction among individuals is crucial to the achievement of desired collective outcomes. Simon argued that good communication is fundamental to effective teamwork. Chapter 9, “The Criterion of Efficiency,” explored the importance of conserving resources in organizational contexts. Competition for scarce resources—in any setting—forces a need for efficiency and Simon observed that review processes in organizations are almost always focused on promoting the pursuit of efficiency. Chapter 10, “Loyalities and Organizational Identification,” explored how individuals in organizations develop attachments and loyalties to the broader enterprise. Simon suggested that people identify with organizations through the process of substituting the organization’s objectives for their own personal motives. Identification brings with it the economic advantage of decreasing the degree of control and oversight higher-level decision-makers must give to

individual actions. A risk, however, is that slavish identification with specific organizational goals and habituated practices toward them may inhibit the making of effective choices when new factors enter the choice environment. The final chapter, chapter 11, “The Anatomy of Organization,” reiterated Simon’s claim that the distribution and allocation of decision-making functions should serve as the central object of research on administrative behavior. Here Simon set forth his views on where research on administrative behavior could be most fruitfully developed. These centered around the functions of planning, devolution of decision-making, influence, and review.

FOUR LEGACIES IN PUBLIC POLICY AND PUBLIC ADMINISTRATION

Administrative Behavior was conceived as a conceptually coherent, logical explanation of the structuring of organizations and institutions. As such, it laid the groundwork for development of a theoretically driven, empirical science of administration. In the years following its publication, Simon engaged in a wide range of both conceptual and empirical explorations of decision-making practices, all of which had their antecedents in this initial conceptual work. Many other scholars—some in collaboration with Simon—contributed to that broader enterprise. The pay-offs have been significant. Here, I discuss four legacies of *Administrative Behavior* relevant to, or falling within, the fields of public policy and public administration. They concern the study of political decision-making, agenda setting, choice architecture, and the development of expertise.

Incrementalism

Over the past fifty years, public policy scholars have developed a range of conceptions of policy-making processes (Birkland 2010; Sabatier 2007). Simon’s work on bounded rationality has greatly informed that scholarship. In 1959, Charles Lindblom introduced the argument that, when undertaking policy formulation, policy-makers never engage in “root and branch” assessment of policy alternatives, but rather work through a process of “successive limited comparisons.” In arguing for development of a science of policy-making based on observation of actual policy-making practice, as opposed to theoretical ideals, Lindblom directly cited Simon’s work on bounded rationality and decision-making. Aaron Wildavsky’s (1964) subsequent study of government budgeting processes gave central place to bounded rationality as an explanation of decision-making behavior in government. Wildavsky termed it “incrementalism.” The long-accepted wisdom among policy scholars is that politicians rarely pursue policy changes that depart from the status quo. Rather, they make small

or incremental policy changes, while keeping broader policy and institutional settings in place. The predominance of incrementalism in decision-making practice can be explained by information problems, limited attention, and risk aversion. Political decision-makers rarely have the time needed to pay close attention to policy problems. When they make changes, therefore, they tend to be risk-averse. The result is the making of change through small steps. This process was elaborated most clearly by Charles Lindblom (1968) in his classic explanation of the policy-making process. Today, incrementalism remains a highly useful way of characterizing the process of policy change under normal circumstances.

The Politics of Attention

While policy-making processes across a broad range of settings exhibit incrementalism most of the time, instances are also observed of significant, dynamic policy change. Various efforts have been made to explain the forces that lead to instances of dynamic change. Among these, the work of John Kingdon (1984) on agenda setting in policy-making has been highly influential. The heart of Kingdon's study is a model of decision-making that owes much to work conducted by Herbert Simon in collaboration with James March (see March and Simon 1958; Cohen et al. 1972; Cyert and March 1963). According to Kingdon, windows of opportunity arise for non-incremental policy change when there is alignment between a problem, the surrounding politics, and the portrayal of the possible policy response. At such times, major policy changes can be observed. Subsequent, systematic investigations of policy change over long periods of time have confirmed two things. First, areas of public policy are often characterized by long periods of stability punctuated by brief periods of dynamic change. Incrementalism explains policy development during the periods of stability but not the period leading up to the dynamic change. Second, significant change requires concerted effort on the part of advocates to gain and channel the attention of politicians toward the making of significant policy change. This second observation is consistent with Simon's discussion of the structuring of decision-maker attention in *Administrative Behavior*.

Since the early 1990s, Frank Baumgartner and Bryan D. Jones (1993, 2002, see also Jones and Baumgartner 2005) have led a highly fruitful public policy research agenda exploring stability and change in public policy. In so doing, they have indicated the significant role that attention plays in change processes. These authors have been explicit about the influence of Simon's earlier examination of attention and decision-making, as found in *Administrative Behavior* (see also Jones 1999, 2003). Meanwhile, related research has explored the practices of the change agents—sometimes termed policy entrepreneurs—who deliberately marshal advocacy coalitions, present evidence, and engage in problem framing to influence the political agenda and promote policy change (Mintrom and Vergari 1996; Mintrom and Norman 2009).

Choice Architecture

In *Administrative Behavior*, Simon observed that humans can achieve “[a] higher degree of . . . rationality . . . because the environment of choice itself can be chosen and deliberately modified” (p. 92). Indeed, an essential insight of the work is that smart environments can make people smarter. Much of the literature produced over recent decades concerning the theory of the firm has served to elaborate on this point (Cyert and March 1963; Williamson and Winter 1991). The links to the literature on public policy and public administration have been much more tenuous, although it could be said that March and Olsen’s (1989) classic contribution to the new institutionalism literature demonstrates a direct line of influence from Simon. Bryan D. Jones’s *Politics and the Architecture of Choice* (2001) highlighted how Simon’s conceptual work in *Administrative Behavior* could support analysis of political decision-making and the structures that enable it. Jones explores how groups of people adapt to make more rational decisions in government and, in turn, how the very institutions of government are occasionally subject to change in the service of better decision-making.

With the publication of *Nudge* (2008), Richard H. Thaler and Cass R. Sunstein greatly raised the profile of choice architecture as a matter of significant importance to policy designers and public managers. Drawing upon developments in cognitive psychology and behavioral economics—fields influenced significantly by Simon’s early work—Thaler and Sunstein review problems of bounded rationality and how the structuring of the environment of choice can improve individual decision-making and, hence, collective outcomes. In their view, considerable scope exists for governments to support better individual or family decision-making regarding financial planning, healthy living, and the selection of public services, such as schools and hospitals. Simon’s argument that structures can compensate for limitations in human cognition is currently enjoying a renaissance. Given the profundity of the argument, and the enormous expansion in data collection and analysis concerning choices and their consequences, this renaissance will continue for decades. Investigations of how choice architecture can promote smart decisions remain in their infancy.

Expertise and Learning Organizations

In discussing the psychology of administrative decisions (*Administrative Behavior*, chapter 5), Simon noted how individuals and organizations can build expertise in decision-making. Actions that are often attributed to genius (e.g. the choices of chess masters, musical virtuosity, sporting prowess, the diagnostic skills of medical specialists) are, in fact, the result of memorization and pattern recognition (Ericsson et al. 2007). Towards the end of his career, Simon became increasingly interested in artificial intelligence, the modeling of human expertise, and organizational learning (Simon 1991). Others, influenced by Simon, contributed to our understanding of

the development of expertise and the evolution of learning organizations. For scholars of public policy and public administration, these developments are of significant interest. In particular, advances in the use of systems for codifying and accessing knowledge hold the promise of improving individual and collective decision-making. Large corporations are becoming increasingly adept at collecting relevant data from their operating contexts and “competing on analytics” (Davenport and Harris 2007). Governments, likewise, have opportunities to assemble knowledge dispersed across multiple sites and use it to guide both the making of better decisions at the local level and the improvement of higher-level system design and executive decision-making. As they do so, they will further exhibit the purposive rationality that Simon observed at work in large organizations, decades before the invention of computer systems that have greatly eased the pursuit of rational decision-making.

CONCLUSION

In *Administrative Behavior*, Herbert Simon cogently argued for the development of a science of administration where organizational decisions would represent the primary units of analysis. In constructing a conceptual framework to guide that science, Simon drew heavily on insights from cognitive psychology. In the years following its first publication in 1947, *Administrative Behavior* has proven a rich source of inspiration for researchers from several disciplines who have investigated institutional and organizational practices across many settings. Here, consideration has been given to how *Administrative Behavior* has influenced the fields of public policy and public administration. That influence has been huge. Indeed, several other classics of public policy and public administration featured in this volume have an intellectual lineage that traces directly back to *Administrative Behavior*. By eschewing a tradition of scholarship where “principles” were no more grounded than “proverbs,” Simon set an intellectual path for himself and others that was grounded in logic and that favored rigorous empirical testing of claims. Simon knew prescriptions designed to address administrative problems would prove useful, and withstand the tests of time, only when they were based on correct diagnostics. *Administrative Behavior* was intended to guide the science of administration that would ultimately produce such diagnostics. The richness of the extant legacy confirms the fundamental contribution of the scientific method to the advancement of knowledge concerning human behavior in organizations.

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CHAPTER 3

DAVID B. TRUMAN, *THE GOVERNMENTAL PROCESS: POLITICAL INTERESTS AND PUBLIC OPINION*

WYN GRANT

IN *The Governmental Process* David B. Truman (1951, 1971) offers what has come to be regarded as a classic pluralist analysis of interest groups and their relationships with political decision-makers. He explained, “The principal task of this book has been to examine interest groups and their role in the formal institutions of government in order to provide an adequate basis for evaluating their significance in the American political process” (Truman 1951: 505). Writing in 1980, Moe commented that “Today, Truman’s *The Governmental Process* is widely recognised as the representative text” (Moe 1980: 151). Vogel (1988: 5) ranks *The Governmental Process* alongside V. O. Key’s (1942) *Politics, Parties and Pressure Groups* and Robert Dahl’s *Who Governs?* as the most influential studies of American politics from a pluralist perspective. Along with Latham and the earlier work of Bentley, “More than any other scholarly efforts, the works of these writers put interest groups on the political map—increasing knowledge about their numbers, types and activities, stimulating widespread recognition of their political importance, and encouraging empirical research” (Moe 1980: 150).

The immediate reception of Truman’s work was highly enthusiastic. It was described in the *American Political Science Review* as “a milestone towards a systematic formulation of the dynamics of government” (Leiserson 1951: 1193). When Rothman turned his fire on the pluralists in 1960 he chose Truman’s book as “the most comprehensive and sophisticated statement of group theory” (Rothman 1960: 16). Rothman’s critique argued that Truman had neglected social movements, crises, change, and Marxist analysis. He argued that there was a mismatch between what Truman said he was going

to do and what he actually did, which was no accident “for Truman’s explicit theory really had no explanatory power whatsoever” (Rothman 1960: 25).

Truman greeted this onslaught with disdain, stating “I see no useful purpose to be served by accepting the Managing Editor’s invitation to reply in kind” (Truman 1960: 494). Truman then nevertheless spent two pages replying to what he viewed as serious misrepresentations of his position, arguing that what he was doing was examining interest groups and their role, not setting out a general theory of politics (although an implicit theory at least could be said to underpin the book). Truman concluded by stating that an attempt to reply to Rothman’s critique was “virtually pointless, since the object of his concern is largely of his own making” (Truman 1960: 495). Truman seems to have got the better of these exchanges and some 15 years later his status as the author of a “modern classic” (Garson 1974: 1505) seemed assured.

One charge that has been made against Truman is that he borrowed from the work of earlier authors without adding anything new. Truman is explicit in acknowledging his debt to Bentley as “the principal bench mark for my thinking. In fact, my plans for this study grew out of my experience in teaching from Bentley’s work” (Truman 1951: p. ix). Indeed, Olson, as part of his attempt to overturn the conventional wisdom represented by Truman, presented him as a disciple of “his master” who “tended to follow every twist and turn in Bentley’s account” (Olson 1965: 124). Pendleton Herring is extensively cited, but the references are generally slight and often involve borrowing a nice turn of phrase from Herring, but not a theoretical approach. Schnattschneider’s study of the Smoot-Hawley Tariff Act of 1930 is praised and used as a source of relevant evidence. However, what Truman did was to provide a more comprehensive account of the functioning of interest groups within the political system than any of these authors.

Subsequent interpretations of his work were qualified by the insights of Olson, who provided a major critique of Truman, and Lindblom, who argued that business was an interest unlike any other, or by approaches that took the state as the unit of analysis rather than the group, emphasizing the way in which it shaped interest group activity. Truman had a rather elliptical although very elegant and erudite writing style in which the reader accompanies the author on an intellectual journey. One possible consequence was that he and his fellow pluralist writers “left considerable confusion in their wake. And this confusion, it seems, actually operated to enhance the influence of pluralist notions about group membership” (Moe 1980: 150). Rational choice approaches as represented by Olson were subsequently seen to offer greater rigor. However, more recent work on group populations has turned to Truman as a useful source of relevant insights. His analysis of the importance of internal dynamics of groups has been relatively neglected, which is arguably a deficiency of contemporary analysis.

Truman wanted to provide a systematic account of the role of interest groups in the political process to counteract more speculative, moralizing, and polemical accounts which emphasized their pathological elements. One of his objectives was to “detoxify” interest groups and to show that they were not a threat to the stability and survival of representative democracy. An interest group was defined as “any group, that on the basis of one or more shared attitudes, makes certain claims upon other groups in the

society for the establishment, maintenance or enhancement of forms of behavior that are implied by the shared attitudes” (Truman 1951: 33). He preferred “interest group” to “pressure group” as “the more inclusive and more nearly neutral term” (Truman 1951: 39).

This is a rather broad definition which, apart from the use of the rather loose term “group,” could apply to an interest, e.g. the financial services sector rather than an organization representing bankers. One of the reasons why Truman favored a broad definition was that it enabled him to identify potential as well as actual interest groups. In practice, the principal focus in the book was on what are conventionally regarded as organized interest groups. “[In] the subsequent analysis, it is the so-called organized groups that occupy center stage, and his study of how interest organizations participate in, affect, and are affected by the larger political system” (Moe 1980: 153).

Truman emphasized that “the book is an analysis of the *American* system. Its propositions were intended to apply to that system” (Truman 1971: p. xxvi). There was some discussion of “Political Groups Abroad,” mostly with reference to Britain. However, “the bulk of the book is concentrated upon the distinctive features of the American experience, which inevitably restrict the application of the book’s inferences to other settings” (Truman 1971: p. xxvi). In fact the book was extensively used to guide research elsewhere, even if the relative importance of the “channels of access” to decision-makers identified by Truman varied in other settings, not least in terms of the lesser importance of the legislature.

Truman saw his book retrospectively as “an early part” (Truman 1971: p. xix) of the behavioral movement in American political science. Moe notes (1980: 150), “the concept of interest group was advanced as a tool for understanding political behaviour generally.” Yet, methodologically, many of the hallmarks of behavioralism are absent from Truman’s account. His main way of proceeding is through the consideration of an empirically rich and varied set of examples of interest groups in action, in effect a series of case studies but without any rigor in their selection in the way that contemporary proponents of multi-method analysis would demand. The book lacks any formal quantification and the relatively few tables are essentially descriptive compilations of data.

There is, however, an implicit link with structural functionalism, although leading theorists of the time such as Talcott Parsons are never cited. Groups formed an important part of the inputs into an equilibrating system. Truman is certainly not setting out a radical critique of the existing power structure, indeed he evidently believes that it is broadly balanced and capable of adjusting to achieve a new equilibrium when that is required by changing social configurations.

Where it does anticipate behavioralism and much contemporary analysis is in its advocacy of an interdisciplinary approach, although Truman (1951: 393) has little time for “the economist’s abstracted formulas.” In the first section of the book he draws “heavily upon findings and formulations in social psychology and applied social anthropology” (Truman 1951: p. ix). Moe suggests that Truman’s heavy reliance on the findings of small group research leads him into a trap, in effect a scaling up problem.

“The inferential leap from small groups (for which data is plentiful) to large interest organizations (for which there is very little) is frequent and usually inconspicuous” (Moe 1980: 160), although Moe emphasizes that Truman is well aware of what he is doing.

OVERLAPPING MEMBERSHIP AND POTENTIAL INTEREST GROUPS

Truman saw two elements of his account as “of crucial significance and [requiring] special emphasis. These are, first, the notion of multiple or overlapping membership and, second, the function of unorganized interests, or potential interest groups” (Truman 1951: 508). It was these concepts that he saw as central to the compatibility of interest group activity with representative democracy and hence as a principal means of rebutting the more pathological accounts of “the lobby.” For Truman it was “multiple memberships in potential groups based on widely held and accepted interests that serve as a balance wheel in a going political system like that of the United States” (Truman 1951: 514). They were innovative concepts in the form developed by Truman and require careful consideration.

Truman acknowledged that the “brilliant” pluralist thinkers of the early twentieth century “recognized multiple patterns of group affiliations or loyalties, nevertheless they did not consistently see in these the functional basis of the institutions of government” (Truman 1951: 46). Truman asked himself the question, “How does a stable polity exist in a multiplicity of interest groups?” His essential answer was “the fact that memberships in organized and potential groups overlap *in the long run* imposes restraints and conformities upon interest groups on pain of dissolution or failure” (Truman 1951: 168). For Truman “It is the competing claims of other groups *within* a given interest group that threaten its cohesion and force it to reconcile its claims with those of other groups active on the political scene” (Truman 1951: 510).

Truman defined membership not in terms of formal dues paying but sharing basic attitudes or even just a broad sympathy with the stance taken by others. For him “The phenomenon of the overlapping membership of groups is thus a fundamental fact whose importance for the process of group politics, through its impact on the internal politics of interest groups, can scarcely be exaggerated” (Truman 1951: 158). Overlapping memberships produced a propensity to compromise, a willingness to have regard for and to take account of the views of others in reaching a political settlement. The absence of such a spirit of compromise is a problem for the contemporary American political system. What has changed is that political parties have become more ideologically polarized than the locally oriented entities that Truman discussed at length, although like many American political scientists of his generation he had a yearning for party government.

The lack of a systematic comparative dimension in the book leads Truman to set up a hypothetical example which would be challenged by subsequent empirical research. He imagines “a situation in which virtually all interaction takes place within social strata and in which there are few or no organized groups whose membership is drawn from more than one class. Overlapping memberships would then tend to reconcile differences within rather than between social levels” (Truman 1951: 520). This hypothetical example would fit the “pillars” into which Dutch society was at one time organized. Yet as Lijphart (1977) was able to show, political leaders in such societies were able to develop a form of elite accommodation or consociational democracy which functioned at least as well as the very different model of American democracy.

Truman was forced to admit that even in the United States there were limits to the pattern of overlapping memberships: “There is little overlapping in the memberships of the National Association of Manufacturers and the United Steelworkers of America, or of the American Farm Bureau Federation and the United Automobile Workers. Overlapping membership among relatively cohesive interest groups provides an insufficient basis upon which to account for the relative stability of an operating political system” (Truman 1951: 510). Hence, Truman implied that the concept of potential interest groups was even more important and it was here that critics such as Olson concentrated their fire.

For Truman (1951: 511), “Any mutual interest ... any shared attitude, is a potential group. A disturbance in established relationships and expectations anywhere in the society may produce new patterns of interaction aimed at restricting or eliminating the disturbance.” How would one actually recognize and identify these potential groups given that they do not have any organizational form? Truman placed considerable faith in social psychological research on attitudes, admittedly an area in which considerable progress had been made at the time he was writing, with a widely used definition being formulated by Allport in 1935. “Indications of these potential groups are to be found in attitudes widely held, but not at the moment the basis of claims and interactions upon others” (Truman 1951: 159). In other words, these were latent groups ready to spring into action when sufficiently provoked. But could they and would they? Who would be prepared to undertake the task of organization and bear its costs? This was the basis of much of the later criticism of Truman’s notion of potential group and it has its origins in Bentley’s work.

Truman himself could see that there were some problems with the concept. “Obstacles to the development of organized groups from potential ones may be presented by inertia or by the activities of opposed groups” (Truman 1951: 511). Action might not be taken quickly enough: “Because the unorganized interests may not be a central concern of most individuals and because these interests may have to be activated in the face of insistent violations, there is no guarantee that they will become operative in time to avoid profound disturbance or collapse” (Truman 1951: 516). He saw the effectiveness of interpersonal communication as being of crucial importance to the mobilization of unorganized interests. Truman admitted that (1951: 523) “Potential groups may remain latent as a result of deficiencies in the means of communication.”

This opens up the interesting possibility that the rise of social media may make it easier for interests to organize, an issue that will be returned to later in the chapter.

“The rules of the [political] game” is a central theme for Truman. “[E]xpectations concerning ‘the rules of the game’ themselves are interests, largely unorganized but overlapping more or less insistently with those that appear in more obvious form.” In his introduction to the second edition, he listed examples of the prevalent rules to “include acceptance of the rule of law over a resort to violence or arbitrary official action, the guarantees of the Bill of Rights, effective modes of mass participation both in the institutions of government itself and in the organized groups in the society generally, and a measure of equality of access to the fruits of the social enterprise” (Truman 1971: p. xxxvii). Truman felt that an institutional solution to any required adjustments in policy was at hand in the form of the constitutional arrangements of the United States. “The widely held, unorganized interests are reflected in the major institutions of the society, including the political” (Truman 1951: 513). These institutions, and in particular the separation of powers and the accompanying system of checks and balances, also provided protection against undue concentrations of power (Truman 1951: 324).

Truman was so not so naïve as to think that constitutional arrangements by themselves would ensure fair play. He saw the leaders of government in its branches as also serving as leaders of unorganized groups. “Part of the official’s task is the regular representation of these potential groups in the actions of government” (Truman 1951: 449). This is quite a heavy responsibility, given the need to discern what these interests are and what they might require to satisfy them, particularly given that Truman thought that a failure to discharge this task might result in political violence. A special responsibility was reserved for the judiciary. For Truman (1951: 486) judges “To a greater degree than legislators or executives they are, in a sense, leaders of widely shared but unorganized interests i.e., potential groups, which must be effectively represented in court decisions” (Truman 1951: 486). This assumes an activist judiciary that was prepared to look beyond black letter law. This was not, perhaps, an unreasonable assumption in the US case given that the landmark *Brown v. Board of Education* Supreme Court decision on segregation was to come three years later in 1954. It would have not applied to the British judiciary of the period which was anything but activist and highly respectful of “Crown privilege.”

Truman saw legal redress as particularly important for “Negroes” (he was using the prevalent terminology of the time to describe African-Americans). He recognized that in many parts of the southern states they were “virtually unorganized and denied the means of active political participation” (Truman 1951: 511). He also thought that they were getting better treated and more effectively organized, although Rothman was quite scathing about Truman’s treatment of “the Negro” (Rothman 1960: 21, 25).

Truman was aware of the realities of power in American politics and provided a subtle account of its exercise, but he does sometimes veer in the direction of a Panglossian account of society and politics in the United States. He is far from ignorant of other political systems, but is not beyond a few side swipes, for example, at what he sees as the tendency in continental European research “to treat the interest group as inherently

pathological” (Truman 1971: p. xxv). He picks up on the fact that in Sweden groups “have become not only a vehicle for political demands but also, to a remarkable degree, administrative agencies of the state” (Truman 1951: 10). He thus briefly anticipated the literature later to be developed by Schmitter (1979) and others on the emergence of a democratic variant of corporatism.

Truman does tie himself in knots trying to explain the will of the wisp, “now you see them, now you don’t” character of the potential group. This aspect of his writing provided an attractive target for Olson, who offered a lacerating attack which was quite effective in undermining Truman’s contribution. The consequence was “the marginalization of Truman” (Jordan and Halpin 2012: 253). Olson argues that Truman took far too benign a view of the impact of interests on the political process and that “the needs of all groups in society will . . . tend to be reflected in effective political pressure and appropriate government policies” (Olson 1965: 125n.). In fact, Olson argues that Truman undermined the case for needed reforms: “So confident was Professor Truman of the generally salutary efforts of group pressures that he belittled almost all attempts to improve the system of legislation and lobbying” (Olson 1965: 125).

Methodologically, Olson contends that it is difficult to prove or disprove the existence or otherwise of potential groups. Truman particularly among the pluralists emphasized the unorganized and inactive group that would be aroused “if its interests were seriously threatened.” This threat waiting in the wings would lead politicians to be “almost as solicitous of the unorganized and inactive group as they are of the organized and active interest group.” This assertion is difficult test empirically because “the analytical pluralist can say that the damage to its interests was not serious or that there was in fact no group interest” (Olson 1965: 128).

Olson is scathing about the assumption that interests just organize spontaneously when the need arises. Truman and the other pluralists overlook the free rider problem. “They must show *why* the individual member of the large, latent group will voluntarily support the group goal when his support will not in any case be decisive in seeing that the group goal is achieved, and when he would be as likely to get the benefits from the attainment of that goal whether he had worked for its attainment or not” (Olson 1965: 129). He derides the notion that groups will arise spontaneously when they are needed as a pluralist version of the “anarchistic fallacy” (Olson 1965: 130–1). One could erect a partial and qualified defence of Truman in the sense that there are interests that are under-organized but relatively influential. For example, organizations representing the elderly in Britain have tended to be relatively weak, in part because they are a heterogeneous group with divergent interests, e.g. those with substantial private pensions versus those entirely dependent on the state. However, the elderly do vote more than other groups in the population and are very defensive of their entitlements, especially universal benefits. Politicians have to pay some regard to their preferences, even in a time of fiscal crisis.

However, Truman is in greater difficulty in terms of his account of the role of business interests in a democratic polity. His perspective admitted that business was a privileged interest but that position had been earned through its contribution to the

prosperity of American society. Olson and Lindblom have subsequently argued that there are structural and systemic reasons why business is an interest like no other, although Truman's sympathizers have recently erected a defence of his position.

TRUMAN AND BUSINESS INTERESTS

Truman maintains that "inequalities in the opportunities open to groups, of course, depend in large part on the structures and values of a given society" (Truman 1951: 248). Critics of this position would argue that it begs the question of how that structure and those values arose in the first place and were then perpetuated. Truman's view is that the highest status in society is given to those groups whose achievements are particularly valued. The achievements of business in the United States have been such as to give it "a status of the highest order" (Truman 1951: 249).

He sets out to critically examine "a widespread theory that in the United States and similar societies 'business' groups always enjoy a controlling influence as interest groups." This is a rather crude version of a more sophisticated argument that business has certain inbuilt advantages and, although it certainly does not win all of the time, it does so more often than not. However, Truman sets up as a straw man Brady's (1943) *Business as a System of Power*, although he admits in a footnote that "the broad thesis advanced in this book is of dubious validity" (Truman 1951: 10n.).

Truman argues (1951: 256) that "business groups are faced with problems of internal cohesion no less than other groups in society," a claim that has been supported by subsequent research. There are, for example, divergences of interest and outlook between large and small firms, those reliant on government contracts and those who are not, importers and exporters, financiers and manufacturers, and different parts of the supply chain. Business is not a homogeneous interest that can arrive at an agreed position without difficulty. One only has to consider the issues of climate change where the perspectives of the renewables industry and insurers are different from those of the oil and coal industries, while even within a particular industry one firm's view may contrast with that of another, in part because of market position (Grant 2011).

For Truman (1951: 257), "The matter seems to boil down to the fact that at least on the American scene there are significant differences between economic and political power." Truman maintains that what is different about political power is that it involves eliciting consent from a heterogeneous mass of people, "an overlapping congeries of groups, organized and potential" (Truman 1951: 258). Truman places considerable emphasis on the power of public opinion. "It is likely, therefore, that economic power can be converted into political power only at a discount, variable in size" (Truman 1951: 259). Vogel (1988) was later to argue that business power can fluctuate over time.

It is not so much that what Truman says about business influence is wrong, as that it does not say all there is to be said. Lindblom's (1977) analysis of the privileged position of business in *Politics and Markets* has been highly influential, coming as it

does from someone located within the pluralist tradition. Lindblom's analysis was based "on the absolute dependence of governments for their popularity and legitimacy on economic success, and their perception that they depended for that success on the business community" (Crouch 2010: 155). Lindblom argued that business executives did not have to pressure, bribe, or coerce governments to respect their policy preferences. Truman did to some extent anticipate this point when he admitted that "The favoured position of business groups is furthered by the existence of an economic system under which businessmen's confidence and expectations are of crucial importance to the health of the economy" (Truman 1951: 255). Yet Truman did not follow through the implications of this insight, preferring to focus his efforts on the search for "a more moderate conclusion" (Truman 1951: 256) than the view that business was dominant or controlling.

Lindblom believed that fundamental issues such as private property and enterprise autonomy are kept off the political agenda with disagreements between government and business being confined to secondary issues such as the particulars of regulation. Even the global financial crisis has not stimulated the emergence of a credible alternative paradigm to neoliberalism (Grant and Wilson 2012). "Businessmen are influential in enormous disproportion" (Lindblom 1977: 200). It should also be noted that Lindblom was writing before the onset of globalization and the enhanced influence of international business.

Truman's view of the limits of business influence was further undermined by the work of Olson, through the notion of "privileged" and "intermediate" groups which could be formed in the oligopolies that characterized much of the economy. "A 'privileged' group is a group such that each of its members, or at least some of them, has an incentive to see that the collective good is provided, even if he has to bear the full burden of providing it himself." As for an intermediate group, that was to be found where "no single member gets a share of the benefit sufficient to give him an incentive to provide the good himself, but which does not have so many members that no one member will notice whether any other member is or is not helping to provide the collective good" (Olson 1965: 50).

The existence of such groups in Olson's view undermined the basic pluralist claim that demands by one group would be countervailed by others, "so that the outcome will be reasonably just and satisfactory" (Olson 1965: 127). In fact outcomes would be asymmetrical and oligopolistic industries could secure such objectives as tariff protection or a tax loophole. "The smaller groups—the privileged and intermediate groups can often defeat the large groups—the latent groups—which are normally supposed to prevail in a democracy" (Olson 1965: 128).

Jordan and Halpin (2012: 255) suggest that perhaps Olson's "pro-business-bias argument was so convenient for the political preferences of most political scientists that they accepted it uncritically." Certainly the elegance of Olson's theory is not necessarily matched by supporting empirical evidence. Yet it would appear that Truman was inclined to take too benign a stance towards business power.

TRUMAN'S ENDURING RELEVANCE

There are three areas in which what Truman had to say is of continuing relevance: his explanation of why group numbers tended to grow; his analysis of the factors underlying group effectiveness; and his emphasis on the internal dynamics of groups which has become somewhat neglected. Finally, it is suggested that it may be possible to examine the impact of social media on group politics within his framework.

With renewed interest in population levels of groups in different societies and the rates at which they are growing, Truman's reflections on this topic have acquired a new relevance. Indeed, it has been argued that Truman was "mostly right" (Jordan et al. 2012: 146) in his analysis. Truman argued that an increasingly fragmented economic system and increased technological and economic change prompted a growth in the number and range of interests that could be represented. This could not simply be explained in terms of the division of labor and functional differentiation between the employers and employed. One had to examine the pattern of interaction between groups. Forming one association might stabilize one set of relations but could disturb "the equilibriums of other groups or accentuate cleavages among them" (Truman 1951: 59). Hence, the formation of associations tended to occur in waves, a proposition to which subsequent empirical research has lent support. In particular, Truman emphasized the importance of the disturbances brought about by war and by economic planning (Truman 1971: 76). Jordan and Greening (2012: 85) found "associational proliferation in a planned economy" and also that in Britain "the birth rate of trade and business bodies peaked during the wars."

Truman refers to group effectiveness at a number of points in his book. He does not attempt to aggregate these variables into a typology as later writers have attempted to do (Grant 2000). He suggests that the techniques utilized "to enhance and exploit the degree of access that a group has been able to achieve are enormously complicated and varied" (Truman 1951: 391). However, he does isolate many of the factors that other writers have used. In particular, he suggests that three interdependent factors affect the extent to which a group secures effective access to the institutions of government: the group's strategic position in society; its internal characteristics; and factors relating to the governmental institutions themselves (Truman 1951: 506). He also isolates a number of specific factors considered by later writers. "Money is not the only variable determining the influence of the group but, like formal organization, it is highly significant" (Truman 1951: 251). In particular, it facilitated the hiring of professional staff who are identified as a key resource, with particular emphasis on "The key importance of the paid executive of a trade association" (Truman 1951: 134). The "skills and strengths of the group leadership" (Truman 1951: 164) are also identified as crucial. Of course, all forms of leadership are situation contingent and successful groups adjust their leadership as the political environment changes.

Truman was very interested in how interest groups operated and were structured internally, devoting two chapters to internal politics. He considered that their ability

to overcome instability and secure cohesion was a crucial variable that influenced a group's effectiveness. He believed "that the character of a group's relationship to the governing process is in part a function of the group's internal structure and of political behavior within its ranks" (Truman 1951: 13). The internal politics of groups have been relatively neglected in recent work. In part this is because there has been a move away from case studies of individual groups or campaigns that characterized much of the early literature. The emphasis on policy networks in recent literature leads to a focus on the external relationships of the group rather than its internal dynamics. This is not to say that Truman was not alert to the importance of coalition building between different groups: "In a society that frequently uses majority rule as a technique in making decisions, groups often find it essential to make alliances in order to assert their claims effectively" (Truman 1951: 251).

Truman recognized that "Associations in our culture are expected to be 'democratic'" (Truman 1951: 138). However, he acknowledged that this expectation could set up tensions which undermined the internal coherence of groups and hence their effectiveness. Pressure groups have to develop decision-making structures that combine an ability to take account of the often divergent interests and viewpoints of their members whilst being able to respond to changing events and develop coherent policies that will have an impact on decision-makers. Truman spent some time discussing the role of the "active minority" in interest groups with reference to the work of Michels. At a time when there is a greater emphasis on transparency and empowerment, a failure to create more representative structures, or to treat members as more than supporters who provide funds, may ultimately endanger the representative legitimacy of groups. A return to Truman's treatment of these issues might pay dividends.

Truman argues (1951: 25) that it is interactions that define groups, not shared characteristics. In particular, "The quality and character of the mass media . . . and of the various means of interpersonal communication—rumors, letters and conversations—are of fundamental importance in assuring the influence of unorganized interests" (Truman 1951: 517). The social media have increased the frequency of such interactions in a way that Truman could not have anticipated. Given Truman's framework, this would imply that the rate of group formation should increase as it becomes easier for individuals to identify those with shared beliefs, to initiate campaigns and to mobilize others to support them. Whether these campaigns are too transitory to influence political outcomes remains an open question pending further research, but Truman is surely correct to emphasize the importance of the means of interpersonal communication.

CONCLUSIONS

Truman's analysis is a product of its time. This is, for example, evident in his account of gender roles in which the father makes the financial decisions and the mother takes the lead in parenting (Truman 1951: 27). He was very much concerned with the stability of

the system of representative government, but he saw no incompatibility between interest group activity and democracy, seeking to establish such groups as a legitimate part of “the weft of the fabric” (Truman 1951: 46).

Truman admits (1971: p. xxix) that he “relied heavily” on the category of potential groups and it is here that Olson’s critique is at its most effective, although Rothman had already detected logical flaws in the notion of potential groups (Rothman 1960: 23). Although Truman thinks that some of Olson’s ideas are helpful and indeed underpin and develop some of his own thinking, he also emphasizes the limited applicability of Olson’s theory. Olson admitted that it did not apply to “altruistic” groups and it is these that have grown most rapidly since he wrote his book (Christiansen 2012: 174). Some of Truman’s ideas about the importance of interpersonal communication acquire a new significance in an age of social media and would not be generated by a rational choice model. Truman still repays reading (it is probably easier to understand his argument if one begins with the conclusion), but we need to read his analysis critically in the light of what we have learnt from the rational choice work of Olson and others. He made a major contribution to opening up and stimulating the study of interest groups, providing his critics with an expansive target that helped them to develop their own arguments. If his book had not been written, the study of interest groups would not have developed as quickly or in the relatively systematic way that it did.

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CHAPTER 4

ROBERT K. MERTON ET AL., A READER IN BUREAUCRACY

EDWARD C. PAGE

A Reader in Bureaucracy's claim to “classic” status does not rest on any single set of ideas or arguments in the book, since no such thesis is presented. While the senior editor, Robert Merton, is “among the most influential sociologists of the twentieth century” (Calhoun 2010: 1), the book is not generally regarded as an important part of his *œuvre*. It gets little mention in accounts of Merton’s life and thought, and even in one exception where it is mentioned, in Crother’s (1990: 207) essay entirely devoted to Merton’s work on bureaucracy, the mention is brief and points out that no “Mertonian stance [is] actively pursued” in it, even though a “closer examination” might reveal a “subtle underlying direction.” Neither is it a classic by virtue of being directly cited. It gets 318 citations according to Google Scholar, as against nearly 19,000 for Merton’s (1949) *Social Theory and Social Structure* and 4,000 for his *Sociology of Science* (Merton 1973). Merton’s own essay in the collection gets more citations (1,400) in its guise as an article in *Social Forces* (Merton 1940) than the *Reader*. Of course, citations pose a particular problem in understanding Merton’s influence, not least because of the principle he identified of “obliteration by incorporation”: as ideas initially set out by one person become accepted as part of the common stock of knowledge in the discipline, the originator is rarely acknowledged (Calhoun 2010: 12ff.).

The *Reader* has 54 separate chapters. Three (by Carl Friedrich, Alvin Gouldner, and Frederic Burin) were specially written for the collection. Eight come from work published before the 1920s, a group that includes include three extracts each from Max Weber and Roberto Michels, one from Ernst Troeltsch and one from Thomas Tout, the British medieval historian. The remainder are extracts from books, journals, and reports published between 1931 and 1951 whose authors include Walter Rice Sharp, Chester Barnard, Martin Lipset, Herbert Simon, Rheinhard Bendix, Phillip Selznick, Hans Gerth and C. Wright Mills, and Alvin Gouldner (who has three essays). Some of the extracts contained in the book are derived from works which have the status of classics in several fields—Max Weber (1947) across a range of sociological specialisms,

that of Ernt Troeltsch (1949) in the sociology of religion, Selznick's (1949) *TVA and the Grass Roots* in management sociology, Lipset's (1950) *Agrarian Socialism* in the analysis of class and class conflict, Barnard's (1938) *Functions of the Executive* in management, and Simon's (1947) *Administrative Behavior* in many fields including political science, public policy, and public administration.

The chapters in the *Reader* covered a significant part, but certainly not all, of what could be termed public policy before the big bangs of the behavioral revolution of the 1960s and the expansion of and differentiation in social science subdisciplines, above all in this respect the development of policy studies in the early 1970s. Since that time the study of bureaucracy has developed in a range of subdisciplines, above all: public administration, public management, public policy and political economy, regulation, historical sociology, and the sociology of organizations. Some of the contributions, or rather the original books and articles from which they have been drawn, remain influential and/or enduring in their own right in one or more of these fields. Their status as "classics" cannot be adequately followed through in a single essay, still less in a book designed to look at the field of public policy. In what sense then, might this collection, with extracts from so many larger works that went on to be classics in different fields, be considered as a classic in public policy with relevance to contemporary public policy?

It would be wrong to say that the *Reader* was a beacon of sociological theoretical insight in a world of dull formal legalistic public administration. Dull formalism has long been, and remains, a significant part of the study of public policy and administration and mid-century public administration had a range of interesting authors writing about administration and bureaucracy that could not be classed as dull in this way. If we take Sherwood's (1990) selection of books identified by a panel of scholars "as most influential" and include only those published before 1952 (and exclude works extracted for the *Reader*) we might have a somewhat jaundiced view of the field at large: there is only Paul Appleby's *Big Democracy* (1945) and *Policy and Administration* (1949); Fesler's (1949) *Area and Administration*; Pendleton Herring's (1936) *Public Administration and the Public Interest*; Gulick and Urwick's (1937) *Papers on the Science of Administration*, and a couple of manuals produced by the Institute for Training in Municipal Administration. These are certainly fine works, but one can find more of contemporary theoretical interest outside this list. The *Reader* contains but a sample of some of the exciting work going on, and older work arousing interest, around that time—the work of Selznick, Simon, Gouldner, and Merton himself. And there are many others: Simon, Smithburg, and Thompson's (1950) *Public Administration* textbook was reissued 40 years later as a work that could stand at least in the company of Simon's (1945) *Administrative Behavior* and Morstein Marx's (1946) *Elements of Public Administration* (see Simon and Thompson 1991).

In fact, Merton's *Reader* does not appear in Sherwood's 1990 list of most influential books. It was initially not at all well received either. William Robson (1952: 275) complained "one puts down this book with the feeling that there is something radically wrong with the methodology of the social sciences for it to be possible for such a

compilation to appear under the auspices of a distinguished advisory board containing some of the leading scholars in American academic life.” A more sympathetic review complained that there was no logic behind including well-known books in a reader (why bother putting them in a reader when the material was likely to be familiar or easily obtainable?) and another that the passages are too short. The *Reader* did have influence by virtue of its staying power as a widely read text: Clegg (2011: 4) notes that it was “the first widely used course text for students of organizations . . . and still the standard reader when I entered university in the mid 1960s” at a time when “the world of organizations and the world of bureaucracy were seen as largely coterminous.”

What is most distinctive about this collection is that it reflects a sociological approach to understanding public administration and policy that was in vogue in the 1950s through to the 1970s and that has since largely gone out of fashion. Its status as a classic rests on the quality of the ideas and thinking extracted from the contemporary intellectual environment and placed in the book and not on any novelty or distinctiveness at the time. A sociological approach to public policy has to some degree persisted in contemporary public policy studies in the form of, among other things, “sociological institutionalism,” “bringing the state back in,” and implementation studies. One would be hard pressed to argue that such sociological perspectives have entered/re-entered/persisted in the analysis of public policy in these or other areas because of the *Reader* or the works from which it extracts. Rather the book is classed as a classic because it stood for a long time as an important statement of some of the leading sociological issues in the study of bureaucracy current in the 1950s and 1960s. The interest in it as a classic is whether the issues raised in it have any relevance or interest for the contemporary study of public policy.

Deriving themes from a long and admittedly bitty book like this is bound to be difficult, and the editors’ own division of the material into eight sections is of little use in this respect. In deriving the four themes discussed here—the debate with Weber, formality and informality, social stratification and bureaucracy, and the problematization of authority—I have tried to reflect what I take to be the concentration of the 54 chapters, highlighting especially recurring themes. There are themes I have left out (giving little attention to the “methods of study” section of the book), but the bulk of the chapters touch on at least one of these themes at some point.

THE DEBATE WITH WEBER

Max Weber’s three pieces included in the *Reader* are those that remain the most commonly used parts of his work in the study of bureaucracy: one setting out the characteristics of the “ideal type” of bureaucracy (the 10 characteristics including those covering hierarchy and specialization), a second containing his preconditions of bureaucracy (above all the development of a money economy and the increase in state tasks), and the third covering the bureaucratizing tendencies in modern society as evidenced

by the tendency for charismatic forms of rule to become “routinized” and start to resemble the bureaucratic ideal type. Weber’s influence is pretty pervasive throughout the *Reader*: the other three themes outlined in this chapter take their cues from Max Weber. For instance, Gouldner’s contribution on class and rules, discussed here under social stratification and bureaucracy, explicitly takes Weber as a point of departure.

However, it is worth noting that Weber is not treated with particular reverence. Gouldner (p. 48) writes “It sometimes appears that Weber’s theory of bureaucracy is used as a finished tool rather than as a set of hypotheses which, while suggestive as guides to research, must be subordinate to actual findings,” and for Friedrich (p. 28) he generated “working hypotheses ... which he did not pause to test against empirical data.” Such sentiments are also found in other direct treatments of Weber’s work in the *Reader*: influence is not hero worship. Weber, of course, had no chance to respond to the criticisms levelled against him and the collection does not offer a balanced assessment of his work. Nevertheless it is interesting to note one broad similarity in the critical accounts: they point to the difficulty of extracting the concept of bureaucracy from its social and historical surroundings.

The direct debate with Weber refers above all to the historical-comparative ambitions of the ideal type. T. F. Tout’s contribution to the *Reader*, an extract from his *English Civil Service in the Fourteenth Century* (Tout 1913), which looks at the growth of bureaucratic features as the administrative structures of England developed from the expansion of the royal household, is cited as an illustration “in part” of Weber’s (p. 59) approach. More critically Friedrich’s “Observations on Weber’s Analysis of Bureaucracy” takes Weber to task for a main reason still common and current in debates about Weber’s sociology (see Aronovitch 2012): the status of the ideal type as an “intuitional typology” or a set of “mental constructs which are neither derived by a process of deductive ratiocination ... nor built up from empirical data by relevant inference.” However, rather than leave it at this, Friedrich sets out his own “recurring elements” in the development of modern bureaucracy derived from his 1937 *Constitutional Government and Politics*: “centralization of control and supervision, ... differentiation of functions, qualifications for office, objectivity, precision and continuity and secrecy,” and adds “the first three ... are organizational and the latter three behavioural” (p. 29). The interesting contrast with Weber is that this framework suggests that these recurring elements “engender counter-trends which are the result of the inherent disadvantages of excessive centralization, differentiation of functions and so forth.” He further challenges Weber’s endowment of the “ideal type” with positive values through associating it with “effective” and “valid” state activity. While it may be “an exaggeration” to suggest that Weber is thereby supporting authoritarian government, it is “nevertheless striking that Weber’s fully developed bureaucracy is most nearly represented” by authoritarian institutions: the army, businesses without any employee or labor representation in management, and “a totalitarian party and its bureaucratic administration.”

That the tendencies within bureaucratic organization are not necessarily benign is discussed in three contributions from Roberto Michels containing between them

the outline of the main argument of his *Political Parties* (Michels 1915). Parties, even radical socialist parties, develop hierarchical organizations as a matter of necessity, and along with it a strong leadership stratum. The organization and objectives of the party become ends in themselves and the pursuit of party survival and advantage lead it to compromises with existing power structures so that even a revolutionary socialist party “continues to employ radical terminology” but “in actual practice fulfils no other function than that of a constitutional opposition.” Among political leaders the tendency to bureaucratization produces a disregard for the membership and its original aims; among party officials it produces, as in every bureaucracy, Michels argues, “place-hunting, a mania for promotion and obsequiousness towards those upon whom promotion depends . . . arrogance towards inferiors and servility towards superiors” (p. 143). While Michels does not state his famous iron law of oligarchy in these passages (“Who says organization says oligarchy,” Michels 1915: 418), he makes a claim nearly as striking: “Bureaucracy is the sworn enemy of individual liberty, and of all bold initiatives in matters of internal policy” (p. 142).

Burin’s analysis of National Socialism constitutes an extreme-case test of one the central assumptions of Weberian theory—the resilience of bureaucracies in the face of constitutional and political change: political leadership may change but the bureaucracy changes little. While the thrust of Weber’s approach to bureaucracy is to state that it constitutes a form of governmental machinery able to serve radically different types of political masters, this is manifestly not the case, Burin argues, because to accommodate the Nazi state the Weimar bureaucracy was fundamentally restructured and purged. Two other broader points are raised here. First, Weber’s insistence on bureaucracy being based on technical expertise, central to his ideal type, overestimates its political importance as the traditional forms of bureaucracy were “submerged in a chaos of irrationality and violence” (p. 43). Second, and more important, the incorporation of a range of radically different forms of state structure and organization within one model of bureaucratic organization casts doubt on its ability to describe any particular state bureaucracy; a point which Gouldner (p. 48) takes up: “The Weberian ideal-type of bureaucracy is . . . a theory relatively innocent of spatio-temporal cautions. Weber finds bureaucracy as far back as Egypt, the later Roman Principate and China from the time of Shi Hwangti. Weber’s thesis maintains that bureaucracy has existed in an essentially similar form, regardless of the great differences in social structures in which it was enmeshed.”

SOCIAL STRATIFICATION AND BUREAUCRACY

The relationship between bureaucracies and the social structures in which they are enmeshed is certainly the kind of issue one would expect from a sociological approach to bureaucracy, and the linkage between social class and status and bureaucracy is a second major theme of the collection. This set of concerns is represented in a variety of

ways. One obvious concern, a feature of thinking about bureaucracies since the 1960s at least, is a concern with how bureaucrats' social backgrounds shape their behavior as bureaucrats, is remarkably understated in this collection. While Kingsley (1944) is often cited as an early exponent of "representative bureaucracy," apparently because of the title of his book, the extract from it in the *Reader* highlights how such an interpretation misunderstands his central point: that the relationship between the dominant social classes outside the bureaucracy and top bureaucrats shapes the nature of government. In particular he argued, from his observation of the British civil service in the 1930s, that the pattern of delegation of decision-making to the British civil service depends to a significant extent on the congruence between the political traditional ruling class and the background of the administrative elite. He explains it using terms we tend to associate more with the late than the earlier twentieth century. Giving responsibility to the civil service is based on psychology. "It is to be sought in an identity of aim and point of view, a common background of social prejudice, which leads the agent to act as if he were the principal. In the first instance, it is a matter of sentiment and understanding rather than of institutional forms" (p. 221). Walter Rice Sharp points to a different kind of importance of the social status of bureaucrats: the diminution of the social status of bureaucracy (in 1931) with increasing numbers "of recruits to the French civil service coming from lower social strata than a generation ago ... As France moves forward in the direction of the 'industrialization' of values, the prestige of serving the state tends to diminish. It is common to hear the term 'fonctionnaire' treated with mild contempt" (p. 303).

The problematic link between bureaucracy and the analysis of class conflict, particularly Marxian analysis, forms the basis of one of the lengthiest sections in the *Reader*, Gerth and Mills's "Marx for the Managers." It criticizes Burnham's (1941) *Managerial Revolution*, from which the editors did not offer an extract in the *Reader*. Starting from a Marxist perspective, Burnham argued that, as that managers formed a distinct class, the revolution that would defeat capitalism would not be a proletarian revolution, but a revolution leading to control by managers. Gerth and Mills point out the contradictions in Burnham's theory and argue, among other things, that it is based on an inflation of the autonomy of managers from other centers of power and misunderstands the dynamics of revolutions. This general problematic of the relationship of bureaucratic leaders to centers of power is taken up in the context of economic power by essays by Berle and Means and Gordon on the relationship between ownership, management, and control of business corporations.

Two essays by Gouldner highlight what might be termed the ideological character of the terms we use to understand bureaucracy which serve to hide power imbalances based on class. In perhaps the least impressive of the two, on "red tape," he argues that what is "red tape" and what are acceptable formal rules required to facilitate good administration depend upon the frame of reference of the observer, and the frames of reference are shaped by wider social values such as a belief in equality and feelings of powerlessness. Gouldner points to the now familiar link between conservative thinking and condemnation of red tape. Yet he also suggests that Marx's railing against

“alienation, impersonalization, and dull, routine work finds little echo among the present Marxist epigone” (p. 418) and shows that red tape has not always been a conservative issue. Nevertheless, red tape as social critique is “especially acceptable to conservatives” because it “enables the individual to express aggression against powerful and prestige-laden organizations, while still permitting him to be ‘counted in’” (p. 418).

That the apparently neutral language used to describe and analyse bureaucracy hides inequalities within it is more convincingly set out in Gouldner’s essay on the relationship between social structure and rules. While rules, according to Weber, secure predictability in administration, if one asks: “Just what do the general rules make predictable and for whom is this being made predictable?”, the answer is based substantially upon social class. Not only, he argues, are certain rules (covering sickness, lateness, and holidays) more likely to be relaxed “as one goes up in the hierarchy,” the degree of “predictability” they bring depends upon social class because on matters “of most concern to the workers the rules are such as to minimize their ability to predict” and thus “[b]ureaucratic rules fulfil typically different functions for different ranks in the industrial bureaucracy” (p. 49). He extends this logic to the norm of “impersonality” which is applied variably to different groups: “impersonal behaviour evidently tends to be strongest between status levels, while studies of informal group structure among operatives indicate that, at least on the lowest levels, impersonalized behaviour is minimal among formal equals” (p. 50).

FORMALITY AND INFORMALITY

A third major theme of the collection is the notion that formal rules and hierarchy have at best a limited role in explaining bureaucratic behavior. Lipset (p. 229) points to the “lack of a sociological approach among political scientists. For the most part they have not raised questions about the social origins and values of government administrators and the relationship of such factors to government policy.” Roethlisberger and Dickson make an analogous point concerning the status and power structures within an organization: “many of the actually existing patterns of human interaction have no representation in the formal organization at all and others are inadequately represented by the formal organization.” The editors of the *Reader* are keen not to exaggerate the importance of the kinds of social interactions stressed by the “human relations” school. They point to the “rediscovered” importance of “small groups and interpersonal relations which form, typically not according to plan, at many points in these large organizations,” though they are at pains to point out that it would “be a disservice to have renewed interest in these groups crowd out systematic concern with the formal structure” (p. 241). One can extend this interest in informality to the norms and values that develop within the relationships and structures which “reflect the spontaneous efforts of individuals and subgroups to control the conditions of their existence” (p. 195).

Dimmock's essay suggests that proximity and familiarity among workers in an organization tend to produce common understandings among workers that are not shared by their superiors, with the consequence that "workers frequently resist particular policies and programs . . . and the executive finds himself powerless to cope . . . This subtle resistance is particularly likely to be encountered in a governmental program where the bulk of the employees are civil servants who have come up through the ranks and only the top level of the organization is brought in from the outside for policy decision and executive direction. The situation which results is one of the as yet unresolved problems of government" (p. 403). The tendency of humans to resist "being treated as means" is a dominant theme in Selznick's *TVA and the Grass Roots*, also reproduced here, and how individuals within the organization offer this resistance has direct implications for whether and how it achieves programmatic objectives.

The systematized commitments of an organization define its character. Day-to-day decisions, relevant to the actual problems met in the translation of policy into action, create precedents, alliances, effective symbols and personal loyalties which transform the organization from a profane, manipulable instrument into something having a sacred status and thus resistant to treatment simply as a means to some external goal. That is why organizations are often cast aside when new goals are sought. (p. 201)

What kinds of "informal" organizations and relations are aired in the *Reader*? While the basic point of informal norms and power relations within organizations is made throughout the book, several articles are essentially case studies of the sets of beliefs or forms of behavior that are distinctive to particular organizations or positions within them. There are descriptions of the working norms or beliefs of British and French higher civil servants, two on US navy officers, one on the US army, and one on the building industry.

THE PROBLEMATIZATION OF AUTHORITY

The "informal" power structures relate to a wider notion developed in some of the readings in the *Reader* that authority is not a straightforward "tool" of government or a "resource" bestowed on some (above all those who are superior in a hierarchy) but is highly problematic both to understand and exercise, and this is a fourth main theme in the *Reader*. Barnard's "definition of authority" makes it clear that authority rests on the acceptance of any command or instruction, and a communication will only be accepted as "authoritative" if its recipient understands it, believes it is "not inconsistent with the purpose of the organization," believes it to be "compatible with his personal interest as a whole," and is able to comply with it. Simon takes up this discussion and summarizes: "a subordinate is said to accept authority whenever he permits his behavior to be guided by a decision reached by another, without independently examining

the merits of that decision” (p. 189). Simon refers to Barnard’s “zone of indifference,” which he relabels the “zone of acquiescence”; superiors exercise authority not by convincing the subordinate but by “obtaining his acquiescence” (pp. 190–1).

Turner’s discussion of the position of disbursing officers in the US Navy raises the conflict between two different aspects of authority: command and rules. Their “distinctive problem . . . is that of reconciling orders from superiors with regulations when they seem to conflict” (p. 373). This points to a much broader set of conflicts which, like many debates in this volume, leads to reference back to issues arising from Max Weber’s approach. Turner goes on to argue that “the conflict between regulations (as interpreted) and orders from superiors is not limited . . . to military organizations. The conflict is incipient in every bureaucratic structure because the rational type of authority, as Weber has indicated, involves recognition both of rules and the right of officials to issue orders . . . the ideal type . . . is itself a compromise between two ideal extremes, utilizing and compromising two channels of authority which may be in conflict” (p. 374).

A third way in which authority is problematized is through its exercise undermining the achievement of organizational goals—the exercise of authority creating dysfunctions for bureaucratic organization. Walter Rice Sharp’s essay on “La Paperasserie” or “red tape” of French bureaucracy is based on the argument that hierarchy runs the danger that “routine operations will become sterilizing ends in themselves.” While the dysfunctions of hierarchical authority are developed in several essays, Merton’s own “Bureaucratic Structure and Personality” sets out the issue in a more generalized way: that the psychological characteristics that hierarchical bureaucratic systems engender in those who work in them serve to undermine the functioning of the bureaucracy as it seeks to meet its goals.

WHAT DID 1950S SOCIOLOGY DO FOR US?

How much of the *Reader* lives on in the form of contemporary public policy studies *directly* descended from the intellectual agenda of the sociological study of bureaucracy in the 1950s? Surprisingly little. In general, approaches based on sociological theory do not enjoy a particularly focal role in the contemporary study of public policy, with the study of public policy in France, where the sociological character of the treatment of administration and bureaucracy is continually affirmed, being a notable exception. While sociological theories associated with postmodernism and governmentality made a big impact in the field of sociology after the 1980s, they never really took off in the study of public policy (see Miller and Fox 2007). The “anthropological” approach to analyzing how policy-makers think and act is still a small part of public policy and is not self-consciously sociological or anthropological in its methodology: the word “anthropology” is only mentioned in the title but not in the text of an article which describes itself as viewing “public administration as political anthropology” (Rhodes

2005). Perhaps the strongest representation of sociological thought in the contemporary study of public policy and administration comes in the notion of “sociological institutionalism.” One of the other institutionalisms against which sociological institutionalism is often contrasted, “rational choice institutionalism,” serves as a reminder that in mainstream public policy analysis economics as a discipline has increased in its influence and sociology has diminished (see Hall and Taylor 1996).

As discussed already, several of the works appearing in the *Reader*, such as that of Chester Barnard and Philip Selznick, may well have had an important influence on organizational and management sociology since. Yet Herbert Simon aside, one would be hard pressed to find anything more than the odd reference to one or two of the authors in the *Reader* in contemporary mainstream public policy studies. So the main question for an assessment of this particular classic is not what influence it has had directly on subsequent scholarship in the field of public policy studies, but what its lack of influence means: do we miss any particular insights about bureaucracy and public policy by neglecting the kind of sociological perspectives contained in it?

The Adjectivization of Weber

Do we miss the use of Weber as a focal point of the study of public administration and policy? We need to be careful here because Weber makes a very strong appearance in contemporary public policy, but mainly as an adjective: as an exponent of “Weberian bureaucracy.” This has become a shorthand for a description of organizations believed to rely on things deemed to be traditional, although different formulations stress different features: they may include formal authority, hierarchy, rules, large-scale state employment, and centralization. The adjective was popularized in Osborne and Gaebler’s (1992) *Reinventing Government* and has become a common means of contrasting a form of organization commonly characterized as hierarchical and rule-based with ostensibly less traditional (often termed “new public management”) forms of organization (see e.g. Gualmini 2008). Weberian scholarship, of course, continues within sociology, but the *debates* within public administration and public policy have all but disappeared. The critiques of Weber in the *Reader* are generally far less superficial and correspondingly more damaging than the common fare of the contrast between “new public management” and Weber. But the central question for this chapter is not whether the term “Weberian” might be playing fast and loose with what Weber said or meant but whether we miss his central influence.

For the most part, the answer has to be “no.” Weber’s understanding of the historical development of bureaucracy might be of interest in proposing broad “world-historical trends” such as “demystification,” secularization, and the relationship between capitalism and bureaucracy in political development. Yet the chapters in the *Reader* dealing with the historical analysis have tended to show the flimsiness of the Weberian approach in understanding broad world-historical trends as a basis for explaining similarities and differences between modern bureaucracies, largely because of the ideal

type's general silence on the diverse forms bureaucracy takes in modern systems. Thus seminal studies of bureaucratic development have tended to develop their own theoretical frameworks explaining development largely independent of Weberian analysis. Rokkan's models of state-building, which initially concentrated on political mobilization (Lipset and Rokkan 1967), do not make any substantial use of Weber but develop their own explanations and terminology (based on center-periphery relations) when they move on to try and describe the dynamic forces underpinning state institutions. Much the same can be said for other approaches to state development that concentrate more closely on the development of bureaucracies, including the work of Tilly (1975), Skowronek (1982), and Carpenter (2001). The historical discussion might draw inspiration or even some concepts from Weber, but discarding the focus on Weber has, if anything, liberated the historical sociology of the bureaucratic state from an awkward framework rather than hindered it.

Formality and Informality

One of the central points made in several contributions to the *Reader* is that it is impossible to understand how an organization works without an understanding of the norms and conventions through which it operates. Here the insights of the *Reader* are hardly missed since this particular sociological approach persists in contemporary policy studies. A range of classic studies of the operating norms and routines of public bureaucracies have shaped the contemporary study of public policy (in ways that are documented elsewhere in this volume) by Herbert Kaufman (1960, 1981), Hugh Hecló alone (1977), and with Aaron Wildavsky (1974). Moreover, in the 1970s and 1980s literature on street-level bureaucracy (Lipsky 1980) as well as, in urban studies, "urban managerialism" saw some increase in interest in detailed studies of the norms and routines of street-level bureaucrats (Brown 1988; Lambert et al. 1978). Contemporary studies of the importance of norms and assumptions of officials for how they exercise discretion can be found in the socio-legal and regulation literatures (see e.g. Hawkins 1983; Hutter and Manning 1990; Halliday 2000; Gilad 2009) and work on "public service bargains" (Hood and Lodge 2006; Salmonsén and Knudsen 2011).

The wonder is less that there remains a modest literature based on understanding the norms and behavior that make an organization work than that there are any at all. Developing insights based on the kind of empirical analysis required to understand such norms is labor-intensive and time-consuming, not least in the effort to gain access and permission to do an in-depth study of how an organization works. It is also distinctly unprofitable in terms of contemporary academic careers. As articles gain in status in political science and books decline, the investment of academic capital in large projects likely to require a book to bring them to fruition becomes riskier. Smaller scale studies involving lower effort provoke now the same sort of reaction as Robson (1952: 275) offered in his hostile review of the *Reader*: they mean that "local and particular phenomena are elevated to the status of universal truths." Interview methodology is

regarded by many as unreliable in comparison with the systematic analysis of indicators or survey data. It is scarcely surprising that the basic insight that organizational norms and practices matter for understanding how an organization works should be approached for the most part in the new institutionalist literature in the form of identifying variables that might be explored as causes of organizational change, often broad systemic variables (see Gorges 2001), rather than in the form of a painstaking study of how the institution actually works. Moreover “sociological institutionalism,” originally conceived to refer to “culturally-specific practices” within bureaucracies (Hall and Taylor 1996), now appears by common consensus to mean little more than processes of imitation, following its near complete identification with the “mimetism” arguments of DiMaggio and Powell (1983; see Beckert 2010).

Social Stratification and Bureaucracy

As Lipset (p. 230) reminds us in the *Reader*, “The behavior of an individual or group in a given situation cannot be considered as if the individual or group members had no other life outside the given situation one is analyzing.” Aside from continued interest in “representative bureaucracy,” where one looks at the degree to which social characteristics of bureaucrats, above all their ethnicity or gender, affect their behavior (and much of this work concentrates not on the policy-making work of bureaucrats but on “street level” bureaucrats and policy delivery¹), the impact of broader questions of class and status on bureaucracy has received little interest in the study of public policy. Perhaps the exception to this can be found in French public policy studies where the Birnbaum (1977) tradition has been developed by scholars such as Genieys (2008) and Georgakakis (2012). Genieys in his essay on C. Wright Mills in this volume points to the increasing interest in relations between the economically powerful and state elites in US political science, but the subject still remains largely outside the mainstream of European and US policy studies.

Gouldner’s arguments in the *Reader* draw attention to the tendency to use terminology when analyzing bureaucracy which appears to be universal but which in fact hides a partial perspective. Thus, it will be recalled, he challenges the notion that Weber’s ideal type of bureaucracy maximizes predictability: for some it does and for others, especially those in lower social groups, it does not. We might make the same argument about the partiality of many core aspects of language used to understand bureaucracy in recent literature. Thus, for example, while “flexibility” is an oft-used characteristic to describe “new public management” reforms. When one asks for whom such arrangements are flexible we find the possibility of managerial flexibility and new forms of rigidity lower down. In a study of the Australian Tax Office, Anderson et al. (2002) for example find:

In practice, we also see evidence of movement towards a managerial controlled employment relationship. Union representation and the degree to which staff are

able to participate in decision-making have declined considerably and decision-making has been centralised to senior managers. The structure of work has been reorganised to meet client needs and temporary employees are engaged to fit with organisational requirements ... [The resulting] frustrations also have an effect on the morale of staff in the performance of their work. When combined with repetitive work, lack of employee participation, increased workloads and insecure employment staff morale has declined rapidly.

As Evans (2011: 371) suggests when examining the discretion of English social workers, “professional status has an influence on the extent of freedom that an occupational group exercises.”

Gouldner’s specific point—that rules affect lower status groups within a bureaucracy differently to higher status groups—is acknowledged by Braithwaite (2002), when discussing the apparent “certainty” that can be introduced in regulatory processes. He highlights the possible tendency of the strategy of regulating through legal rules to be discriminatory in favor of the rich:

As citizens go about activities like paying taxes, creative compliance thus creates a law that is perceptually unclear to ordinary people, and therefore uncertain for them, and uncertain in practice for the rich who more clearly perceive and exploit this uncertainty. (Braithwaite 2002: 57)

He does not, however, go on to examine the possibility that the alternative to tight rules, broader principles that allow significant discretion in the precise way in which organizational objectives can be achieved, can introduce other forms of inequality based upon status and class. For example, Halliday et al. (2009) show how the exercise of discretionary behavior placed social workers dealing with the criminal justice system in Scotland (through writing reports on offenders which will be read by judges) in a subordinate position to lawyers. It produced significant “status anxiety” among social workers who “feared that the cultural and symbolic capital they retained within the social work field was undervalued in the symbolic economy of criminal justice, putting them in a position of relative inferiority,” because of the power and status imbalances between the social work and legal professions.

The Problematization of Authority

The problematization of authority poses few difficulties for early twentieth-century public administration and policy. A concern with authority and its uses has become especially prominent in the study of public policy with the growth in interest in delegation—the passing over from one body to another the authority to make decisions, whether it is the ability to make discretionary rules or how to apply and enforce rules and norms (see Thatcher and Stone Sweet 2002; Huber and Shipan 2002). In addition there has been a revival of interest in a “tools” approach to government, according to which authority is a major tool government uses to shape its environment (Hood

1983; Lascoumes and Le Galès 2007). Much of the argumentation about authority within the *Reader* echoes the considerations characteristic of these literatures. Taking principal-agent arguments concerning the delegation of authority, the basis of the hypothesized relationship between superior principal and subordinate agent is that the agent's reaction (i.e. the likely obedience) to any command by the principal depends upon the values of the agent (whether s/he is being asked to do something s/he'd rather not), the incentives for obeying, the disincentives for disobeying, and the chances of being caught either disobeying or obeying. Such argumentation is similar to the discussion of the nature of authority by Barnard and Simon in the *Reader*: the ability to use authority to get obedience depends on the "zone of indifference" in which commands are acted on without question or recalcitrance. And the size of the zone of indifference is related to the availability of sanctions and rewards. So far there is little difference between principal-agent approach and Barnard's perspective—indeed Barnard, an economist by training and familiar with sociological thought is often included in accounts of the development of principal-agent theory (see Laffont and Martimort 2001; Gabor and Mahoney 2013). Moreover, Barnard's description of the "zone of indifference" being "wider or narrower depending upon the degree to which the inducements exceed the burdens and sacrifices which determine the individual's adhesion to the organization" comes close to the language of modern conventional principal-agent analysis.

The significant differences between the kind of sociological approach of the *Reader* and contemporary principal-agent analysis are of emphasis rather than principle. Three main emphases are worth noting. The first emphasis is that while the principal-agent approach might certainly apply to intra-organizational processes, indeed early formulations concentrated on this purpose (Laffont and Martimort 2001), it is rarely used in this way in contemporary public policy. Contemporary delegation is predominantly studied from an interinstitutional perspective: for example, the legislature as principal and the bureaucracy as agent or the minister as principal and the independent agency as agent. In the US, as Krause (2010) has noted, there is even little academic interest in applying the framework outside legislative–executive relations. Yet patterns of intra-organizational delegation are at least as important in understanding how decisions are made. Many of the contributions in the *Reader* show a concern with how different hierarchical levels in the same organization interact with each other; to some degree this concern is found in subsequent studies of street-level bureaucracy, but we know little enough about the daily interactions between those at the top of ministerial bureaucracies and (service delivery aside) virtually nothing about any relationships with those lower down.

A second emphasis in the sociological analysis is the impact of longer term variables in shaping the exercise of authority. While principal-agent approaches tend to emphasize the specific bargain or contract in the delegation of tasks—what was included and not included in the act of delegation, what safeguards and supervision were contained in the arrangement for delegation and such like—the sociological perspective includes longer term perspectives as crucial for understanding what commands or rules are obeyed and which ignored. These include: patterns of recruitment, socialization, the

norms of the work group, and the ideology of the agency or organization. It is the elaboration of the impact of such broader features on the exercise of authority and bureaucratic behavior that makes Kaufman's (1960) *Forest Ranger* such an important and rare piece of scholarship (see Lynn in this volume).

A third emphasis in the sociological treatment of authority as represented in the *Reader* is the emphasis upon constraint: constraint on the part of the superior wielding authority as well as those on the receiving end. Rules bind those who are put in positions of superiority as well as subordinates. The rules that restrict superordinates' discretion in exercising authority, even in delegating it, might be the kinds of cross-cutting rules—say, employment, health and safety, or constitutional law—that not only circumscribe what those in authority may do, but what and how any authority may be delegated. In the terminology of principal-agent analysis, how authority may be delegated and how it may be exercised is not a matter for the specific “contract” surrounding the specific object of delegation/authority, rather it is embedded in a wider array of rules that limit both parties. As Simon points out, when an army officer is given orders to attack, “he is expected to carry out the deployment in accordance with the accepted tactical principles in the army” and for their part the private soldiers' behavior is shaped by a mass of influences including “both direct commands and tactical training and indoctrination” (p. 188).

Moreover, the notion of the zone of indifference highlights the incentives toward accommodation and avoidance of conflict avoidance within bureaucracies. As Barnard puts it (p. 182), “there is no principle of executive conduct better established in good organization than that orders will not be issued that cannot or will not be obeyed”; for the recipient “most persons are disposed to grant authority because they dislike the personal responsibility which they otherwise accept.” Merton's essay takes this even further by suggesting that bureaucracies might be expected to recruit submissive personalities and the working environment of bureaucracy also creates them. Certainly the *Reader* also contains a range of extracts on this theme, especially the jaundiced examination of the conflict between officers and enlisted men in the US Army (Stouffer, p. 268). However, in several extracts in the *Reader* one is given a range of reasons why “shirking and sabotage” (Brehm and Gates 1997), the assumed outcome of bureaucrats being asked to do things they don't want to in recent principal-agent approaches, might not be so common.

CONCLUSIONS

Many of the papers and debates in the *Reader* were central to the study of bureaucracy in the 1950s and 1960s. In style the extracts, or rather the extracts from work published less than 20 years before 1952, are very different from what one might expect to read in contemporary policy studies, possibly even social science. It is often quite speculative. Merton himself bases his empirical theory of bureaucratic behavior (“Bureaucratic Structure and Personality,” a classic in its own right) on little more than a few anecdotes

and insights gleaned from places as diverse as the *Chicago Tribune* and an unreferenced study of professional thieves. Moreover, since the articles in the *Reader* were for the most part selected from a larger pool of possible contributions published in sociological books and journals, one might presume they were more rigorous and interesting than the average output in the area.

The professional freedom to be speculative is what Robson (1952) seemed to be objecting to in his review of the *Reader*. It is this breadth of curiosity and unselfconsciousness about exploring a wide range of variables connected with bureaucratic behavior that underpins those aspects of the *Reader* that I have suggested are most missed in contemporary discussions of public policy. Max Weber comes out of the whole exercise rather badly, it has to be said, and the criticisms levelled by Gouldner, Friedrich, and the rest go beyond the standard fare of US organizational sociological criticism of the later 1950s and 1960s (discussed in Mayntz 1965) which argued that ideal types were not efficient organizations. The contributions in the *Reader* go on to challenge more fundamentally, and interestingly, the claims of the approach. In fact, after reading these criticisms one can be more sanguine about the rather crass treatment of Weber in contemporary public policy literature: it probably matters less than one might think.

In substantive terms a range of features of bureaucratic behavior—the broad impact of norms of working life in bureaux, recruitment, socialization, attitudes to authority, and the wider reciprocal relationship between social structure and bureaucracy—are what we might miss in contemporary public policy studies. However, if we get the urge to include them we probably do not have to look only backwards for inspiration. Many of these concerns still attract significant intellectual attention in fields such as organizational and business sociology as well as socio-legal studies and social policy. The *Reader* is a good reminder that they used to attract the attention of scholars in the field of public administration and policy to a greater extent than they do today.

NOTE

1. The study of the EU bureaucracy offers a partial exception here as the processes of socialization prior to becoming an EU official are assumed to have an especially strong impact on the “style” of policy-making. While opinions differ as to the effect of such socialization, it remains a significant focus of scholarly work on the EU (see Wille (2013), Kassim et al. (2013), and Georgiakakis (2012)).

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CHAPTER 5

HAROLD D. LASSWELL, *THE DECISION PROCESS: SEVEN CATEGORIES OF FUNCTIONAL ANALYSIS*

KARSTEN RONIT AND TONY PORTER

INTRODUCTION

HAROLD Lasswell (1902–78) was a towering figure in political science from the 1930s through the 1970s and a scholar who made important contributions in many fields of the fledgling discipline. According to Gabriel Almond, “few would question that he was the most original and productive political scientist of his time” (Almond 1987: 249), and even “a kind of Leonardo da Vinci of the behavioral sciences” (Smith 1969: 41). Departing from the standpoint of behavioralism he embraced a rich diversity of themes, especially political power, communication, political and social psychology, and political processes, and also invented new methods to study these highly different phenomena. Indeed, these contributions have also attracted attention in different communities without forming any unified appraisal of Lasswell and his work.

It is therefore far beyond the scope of this article to offer a portrait of Lasswell, his visions, or his role in political science (Farr et al. 2006, 2008). Instead our ambition is to present, evaluate, and contextualize a single piece of his academic work, namely *The Decision Process: Seven Categories of Functional Analysis*, written in 1956 and considered one of Lasswell’s signature works. This study and related writings on the decision process have in many ways had a lasting impact on political science and the way we perceive political processes, but they also led to much controversy as to how these processes are organized and should be analyzed, taking studies on political processes in many new directions over time.

This chapter begins with a presentation and discussion of *The Decision Process* and locates this study within Lasswell's general work and more specifically within his studies on decision-making, both prior to and after *The Decision Process*. We follow this with sections on the reception of this work and its impact on the debate on political decision-making, including in the study of international and transnational policy processes. In a number of cases his contribution is degraded as a kind of textbook version of the political process which is considered much more complex in real life. In different ways *The Decision Process* has also had an impact on modern applications in domestic and international politics, either because explicit use is made of his ideas of the structuring of the policy process, or because the basic tenets of his study on decision-making serve as an undercurrent, not instantaneously recognizable to the reader, yet noticeable on closer inspection. We round off the article by suggesting some areas for future application and research.

HAROLD LASSWELL AND THE STUDY OF STAGES IN POLICY-MAKING

Harold Lasswell's career as a political scientist was accomplished in a quite different time from ours both from a societal and academic point of view. He received a broad education at the University of Chicago in the 1920s (economics, sociology, political science), and he developed a strong interest in leadership (management) and psychology which enabled him to become adept and take a pioneering role in many fields within and beyond political science proper. After working for various government bureaux during wartime years, like many other of his contemporaries in academia, he continued his university career in the Yale School of Law where he was offered a chair, later defined as a chair in "law and social science."

His different affiliations over a 50-year period until his retirement in 1970 not only testify to his broad competencies, great curiosity in human behavior and how it plays out in politics, but also to the institutionalization of political science as a discipline. Political science had to distinguish itself from neighboring fields, develop its own language, establish an independent platform, and, in Kuhn's (1962) sense, gradually seek to become a "normal science."

This was also a time when scholars within established social science disciplines began taking an interest in political issues, while scholars outside the traditional disciplines identified problems that were insufficiently appreciated and demanded fresh approaches and paradigms. Depending on the concrete issues at stake different battles were fought in the social sciences. When analyzing the political decision process some rudimentary tools were already available but other disciplines failed to account for institutions and processes not strictly codified. Knowledge about such processes, however, became ever more needed with the growth of the public sector and the stronger

intervention in the economy after the war. But the gathering of an increasing amount of data, made accessible through public and private organizations, require a suitable framework to record and interpret them.

Lasswell's explication of the decision process built on a long American tradition in the study of politics, dating at least back to the work of Bentley's *The Process of Government* (Bentley 1908), which abandoned legalism and traditional institutionalism in favor of a pragmatic study of existing social and political processes. But Lasswell also built on various parts of his earlier works, in particular *Politics: Who Gets, What, When, How?* (Lasswell 1936), which identified time and situational factors—the “when”—as important elements in political processes, and in his work with Abraham Kaplan on the policy process (Lasswell and Kaplan 1950). In the behavioral tradition also, other scholars of Lasswell's generation were in the 1940s and 1950s struggling with the political process in a parallel effort to create some kind of order and offer new tools to unlock decision-making: Herbert Simon's broader interest in human and organizational decision-making (Simon 1945), and David Easton's application of systems theory to analyze political systems and their various processes (Easton 1953).

The Decision Process offers a full categorization of the different stages in decision-making and includes seven stages: intelligence, promotion, prescription, invocation, application, termination, and appraisal. Each of the stages embraces a particular function, often managed by specialized actors in turn. In many cases bodies perform several functions.

Intelligence

Various types of information must be collected, assessed, and filtered into decision-making. In this context data must be critically examined, as not all information is trustworthy or relevant, but also a series of further steps must be taken and planning measures invoked as a consequence of the value and content of the available data. It must also be considered to which bodies this information should be passed on.

Promotion

Data alone will not deliver decisive arguments and it is important that solid and convincing arguments are found for particular actions. However, several options may be available and different policy alternatives may be offered in the recommendation of policy. In some cases agencies tasked with the promotion of policy may be asked to provide such alternatives and specify the various costs and benefits with each of them.

Prescription

As a consequence of the former work, rules must be adopted and target identified problems. However, it can be difficult to make decisions because of the uncertainty of recommendations and arguments and because agreements must be found among conflicting parties according to a set of formal procedures. Yet, a further concern is to prescribe rules in ways so that they reach different relevant audiences and become legitimate.

Invocation

There are some expectations with regard to the compliance with the rules adopted. Thus it must be spelled out clearly what the rule-making bodies demand or otherwise see as desired behavior among the regulated, but this invocation has a preliminary character and it is meant to prepare for the later and stricter application of rules.

Application

At this stage the conduct of actors, both those that oversee rules and those supposed to comply with them, must ultimately adjust their behavior so as to be in line with the spirit and word formulated in the original formal or informal acts of prescription. In these contexts it is important to examine those actors that participate in the application processes.

Appraisal

After rules have been prepared, adopted, and implemented, it is time to assess the various successes and failures. It is important to specify who are tasked with carrying out the appraisal function and examine the work of the agencies endowed with application. Usually these achievements will be measured against the prescriptions, and reasons for successes and failures must be studied carefully.

Termination

One of the results of the appraisal function may simply be to terminate programs, although this is of course not always the case. Even if rules are difficult to apply, and unexpected barriers appear at some stage in the process, there may be reasons to

continue or adjust rules. But if rules and strategies are terminated, information may be collected to prepare new rules, and so to speak restart the policy cycle.

This heuristic model has since been termed the “stage model” because it identifies some general patterns of the policy process and locates several key functions in a time perspective. Lasswell did not see the model as the ultimate answer to the policy process, because “in matters of this kind it is not a question of arriving at a permanent classification” (Lasswell 1956: 1–2). Indeed, the model has been discussed from many different standpoints; the model has been accepted, refined, abandoned, but also replaced with other models.

TRACING LASSWELL’S IMPACT ON THE DISCIPLINE: CONTESTING THE STAGES PERSPECTIVE

On a number of occasions Lasswell revisited and elaborated on parts of the stages-model presented in *The Decision Process*. Attention was also given to some of the individual stages, such as intelligence and appraisal (Lasswell 1975), but no revision of the model as such was undertaken. Other writers have taken a keen interest in showing how the model was flawed, although criticism has primarily been directed against the general assumptions underlying the model rather than the text itself. Some of the basic tenets of Lasswell’s study, however, have been used in many contexts with regard to understanding the policy cycle and its individual stages.

Criticism has been directed at different aspects of this far from finished and final model. It is difficult to categorize the different kinds of criticism, because scholarly contributions tend to address a related group of shortcomings.

First, it is argued that the actors are seen too much in the light of rational behavior, where information will be collected to guide actors, and where solutions are gradually identified and adopted through the different stages of the policy process to address problems in society in an increasingly rational way. Such rationality cannot be attributed to the policy process, however, because there are numerous barriers to rational decision-making both among the actors and in the institutional set-up.

Second, it is stressed that decisions are not taken in a value-free environment because many economic, social, and political factors constrain the kind of issues that are raised in the policy process, and the way they are dealt with. Indeed, some issues cannot force their way onto the political agenda and are excluded from decision-making (Bachrach and Baratz 1964). During the policy process some actors and some interests have much better chances to influence decisions than others.

Third, criticism has also been raised against the proposed order of the policy process. It is argued that there is not such a strict logic and the different stages do not follow a highly predictable pattern of sequences. This kind of criticism is related to specific

stages and their functions or associated with the development of alternative models. And again, these models do not always address the time factor crucial to the stages model but give more priority to understanding the role of actors, the use of knowledge or complex situations in policy processes, and thus may be valid in their own right but tend to sidestep the relevance of stages.

In the decades following the publication of *The Decision Process* many different attempts were made to make sense of decision-making, but it is very important to add that many criticisms were not directed at Lasswell and *The Decision Process* per se but rather against “rationalistic” thinking. Charles Lindblom saw the policy process as characterized by a number of small and incremental changes (“muddling through”) where not the best solutions but the most realistic options are found and adjusted through the policy-making where these steps are the basic feature rather than the stages (Lindblom 1959). Amitai Etzioni developed his model of “mixed scanning” which offers another version of decision-making combining insights from the two former models, assuming that experience is built up and that the steps referred to are taken within some kind of framework and that actors behave on the basis of bounded rationality (Etzioni 1967). However, these criticisms did not so much challenge that public policy included a number of key functions but rather engaged with the problem of actor rationality. Pointing to the chaotic character of decision-making and the lack of regularities, Cohen et al. (1972) hold that decision-making is not always characterized by problems looking for solutions. In their “garbage can model” it is argued that also choices, situations, and solutions are looking for problems, and although this model pertains to all sorts of organizations and not the policy process alone, it also is relevant for understanding decision-making.

A range of other models have since discussed public policy and decision-making and offered fine-grained analysis and critique of the stages perspective. Some models that have gained prominence are, for instance, the “advocacy coalition framework” (Sabatier and Jenkins-Smith 1993), the “punctuated equilibrium model” (Baumgartner and Jones 1993), the “institutional analysis and development framework” (Ostrom 2005), “the actor centered-institutionalism perspective” (Sharpf 1997), “policy shifts” (Jones 1994), and “gradual institutional change” (Mahoney and Thelen 2010).

Each of them carve out key elements of the public policy-making, but they do not necessarily take issue with Lasswell’s work or other contributions focusing on the role of stages and related functions in the policy process. The general perspective is not one of stages, however irregular and fuzzy these may be, and emphasis is shifted toward other dimensions to frame decision processes, such as policy changes, learning, values, and rules. Hence it is not precisely clear in what sense the stages thinking is irrelevant or even invalid or whether some insights can perhaps be saved for future analysis.

The time factor features prominently in some of the approaches but in a somewhat different sense to that suggested by studies on stages. Time factors are important in both a micro and a macro perspective: Over time actors learn to know each other, adjust their behavior, and build relevant alliances founded on both common interests

and ideas; time is further important because significant changes in policy typically occur over a longer period of time, and, hence, must be observed in such a historical context.

Alternative approaches to decision-making challenge the old schema proposed by Lasswell but the current debate is so rich and diverse and serves so many purposes that explicit confrontation and frank critique of the stages perspective rarely occurs. Nevertheless, ample critiques point to some of its obvious shortcomings, such as the amount of rationality attributed to actors, the exaggerated order of things, the rigidity of functions included in the stages, the lack of societal and historical contextualization, etc. However, it would be too simple to see the different approaches as merely competing frameworks that necessarily exclude each other. They engage with different sets of problems, and employ different concepts, and, hence, are in many ways complementary approaches to policy processes. While these approaches offer actual models, and are less potent in the analysis of particular stages identified in *The Decision Process* and other works on policy processes, other strands of literature have taken up this challenge and scrutinized particular stages.

IMPACT ON RESEARCH: APPLYING THE STAGES PERSPECTIVE IN DOMESTIC POLITICS

In many ways models associated with the policy cycle outlined by Lasswell have been criticized or simply ignored, yet they have not only survived, but been further elaborated and applied in multiple contexts. It is somewhat paradoxical that, while the interest in developing new full-scale models of stages has somewhat waned, the interest in examining particular functions and time sequences has grown considerably (Jann and Wegrich 2007; Howlett et al. 2009). This illustrates that stages are still key points of departure for the study of policy-making in significant parts of the literature. Interestingly criticisms of stages thinking coexist with its advancement. Over time several attempts have been made to reproduce or reformulate the model, sticking to the original idea that time sequences matter, and that there is some kind of flow in the policy process.

Building on the same template some adjustments of the model have been made, including the reinterpretation and renaming of some of the stages. Brewer simplified the model somewhat and included fewer stages (initiation, estimation, selection, implementation, evaluation, termination) (Brewer 1974), and today many scholars refer to at least some of the categories rather than to the original categories formulated by Lasswell. However, more fine-grained models have also been offered. In terms of the further specification of stages the most elaborated model, however, is no doubt the one created by Dror which identifies three general stages—meta-policy-making, policy-making, and post-policy-making—plus a number of sub-stages (Dror 1989). It

is problematic to assert that these stages, and related sub-stages, are always part of the policy process and follow each other in a logical and inevitable order, but if we see such elements of the policy process as forming rather a pool of stages and functions to draw from, the perspective of these time factors is useful.

Most studies using insights from Lasswell do not attempt to offer a full-fledged model embracing the entire policy process. Most applications have taken other directions and addressed some of the main stages included in his original model, but under different names. Indeed strong traditions have evolved with regard to some of the stages, in particular agenda-setting, implementation, and evaluation, and these traditions have become so successful that they tend to become decoupled from broader debates of the policy process. It is difficult to issue precise birth certificates for these three clusters of studies but with the risk of oversimplification implementation studies gained momentum from the 1970s, agenda-setting analyses from the 1980s, and evaluation studies from the 1990s. While the implementation literature seems to form part of a more classic tradition for studying the relations between political and bureaucratic institutions, studies on agenda-setting are more related to the modern proliferation of media and communication strategies, and studies on evaluation are related to the development of new auditing tools and practices to control government agencies.

Implementation is an important element in the policy process and studies examine how decisions are executed and political ambitions and goals are translated and brought into action on the ground by various administrators (Hill and Hupe 2009). Because decisions are often of a relatively general nature and allow some flexibility for administrators there can be a discrepancy between the original intentions of decision-makers and their practical implementation, making the character and extent of goal displacement a key problem in research. Implementation studies therefore scrutinize various top-down and bottom-up relations between principals and agents, often under the explicit assumption that goals are disrupted in the critical implementation stage, and hence pose a serious problem for democracy.

Agenda-setting forms the early part of the policy process and has given rise to a range of studies focused on this particular task (Kingdon 1984; Jones 1994). Not all problems in society are addressed by government and issues must struggle for attention and resources. It is therefore of major concern which issues manage to reach the political agenda, and which actors succeed in placing their interests and ideas before others, because this selection can be crucial to decision-making. It is not necessarily the formal decision-makers who have the power in the agenda-setting stage of the policy process and the role of other actors and their various alliances therefore need scrutiny.

Evaluation forms the last and final stage of the policy process and a variety of criteria are used for assessing the outcome of policy. Although evaluation is made in other stages, key tasks are performed after experiences with implementation, and in some cases after programs are terminated. Evaluation has become very important for assessing the effectiveness of policy, with knowledge filtered back not only

to decision-makers but also to society. Ultimately the evaluation function is needed to improve decision-making and adapt or revise strategies. In this way evaluation is related to different stages of the policy process (Howlett et al. 2009).

Today there are strong research traditions within agenda-setting, implementation, and evaluation studies respectively. These strands of research have their own paradigms and concepts and form rather strong clusters in the research on public policy, public administration, comparative politics, and other fields. Insights gained from these studies only partly inform the general debate on the policy process, however. Although models or theories on the entire policy cycle are not formulated on the basis of the study of particular stages, studies of each of these stages are typically linked with one of the other stages.

There is certainly a huge potential for using the experiences won from the study of each of the stages, but it is a paradox that these advancements are not sufficiently exploited in the current development of theories on the policy cycle and brought together to form a synthesis. The time factor is important for identifying these separate stages, but time factors are also included herein when examining how agenda-setting, implementation, and evaluation processes are organized.

IMPACT ON RESEARCH: APPLICATIONS IN INTERNATIONAL AND TRANSNATIONAL PROCESSES

One of the most prominent developments in the study of public policy processes in recent decades has been the increasingly transnational character of these processes (Doern et al. 1996). Theories that were originally developed to analyze domestic processes also have a potential to be further explored when studying various processes beyond the nation state. Many perspectives from Lasswell's understanding of the decision process have entered these studies but rarely explicitly, rather as an undercurrent as at least some kind of stage thinking intuitively makes sense.

As with the study of globalization more generally, it is clear that some of these transformations, which seemed unprecedented when they were first widely noticed in scholarly work, involve a reversion to earlier levels of international interdependence that had been temporarily eclipsed in the unusual degree of state autonomy that characterized the global system immediately after World War II. Lasswell's work is interesting because it is influenced by both the types of interdependence that preceded World War II, and by his own remarkable enthusiasm for theorizing the type of multi-dimensional complexity that has come to characterize transnational policy processes today.

Throughout his career Lasswell moved freely across domestic/international and disciplinary boundaries in his thinking. His 1923 "Political Policies and the

International Investment Market” addresses themes that are familiar in the international political economy literature today. His 1935 *World Politics and Personal Insecurity* creatively brought together political economy, communications, and psychological themes. His *World Politics Faces World Economics* (1945) analyzed the relationship between politics, security, and trade after the war. His very influential founding, with McDougall and other colleagues, of what came to be known in international legal scholarship as the New Haven School, similarly went beyond approaches that focused narrowly on formal law or state power to analyze the complex transnational social contexts that produced and explained world politics, law, and policy (Reisman et al. 2007; Hathaway 2007; Koh 2007). Many contemporary scholars, without necessarily knowing about or building on Lasswell’s contributions, work on themes that Lasswell addressed.

It is consequently interesting and useful to reflect on the significance of Lasswell’s work on policy stages for transnational policy processes today. Lasswell’s work, like contemporary work on global governance, recognizes the importance for transnational policy of understanding the degree to which governance involves much more than formal state institutions, including as well informal ordering and social contexts. The following overlapping themes are particularly relevant, and we will comment briefly on each: focusing on functions rather than particular formal institutions, recognizing the significance of informal rules and authority, recognizing the importance of time, and negotiating the tension between scientific expertise and policy engagement.

An innovative feature of Lasswell’s work on policy stages is that it moved beyond formal institutions, such as legislatures and courts, to analyze the functions that needed to be carried out across different stages if an authoritative decision was to be made. Today scholars have recognized that this is particularly important in transnational policy processes where formal institutions may be weak or absent. Elsewhere we have identified the stages involved in self-regulatory processes that can cross state boundaries in the regulation of business activities (Porter and Ronit 2006), and conceptualized the processes in terms of agenda-setting, problem identification, decision, implementation, and evaluation. This model applies models from the policy sciences at the transnational level. Other works have referred to agenda-setting, negotiation, implementation, monitoring, and enforcement (Abbott and Snidal 2009), or designing, joining, monitoring, and complying (Ronit 2012). Lasswell’s approach to functions avoids the fatal problems what were identified in functionalist approaches of his contemporaries such as Talcott Parsons. Those approaches were rightly criticized for their determinism, while Lasswell’s approach, albeit also challenged in many ways, emphasizes agency and creativity.

The transnational significance of informal rules and authority and non-state actors was also central to Lasswell’s work (McDougall 1984: 93). His analysis of the decision process took a view of law and power that involved much more than conventional analysis of states recognized. This was a precursor to and consistent with the shift in contemporary scholarship on global governance away from an overemphasis on the

power politics played by autonomous sovereign states or on formal treaties that they negotiate. The growing role of soft law in global governance is widely recognized today (Weiss 2000). In other areas Lasswell's extensive work on symbols, communication, and propaganda anticipated the centrality of language and meaning for contemporary constructivist or post-structural approaches to transnational governance, policy, and power. These concerns are evident throughout his work on policy stages. For instance intelligence, promotion, prescription, and invocation are all about the production of knowledge, meaning, and power.

As noted, a key aspect of Lasswell's analysis of stages is the way that it brings time into the analysis of policy, and shows how time matters. Pierson (2004) has usefully criticized public policy approaches that focus on equilibria or other generalizable timeless patterns that disregard the importance of when and how fast something happens. Despite widespread interest in time and social theory, the significance of time is only beginning to be recognized in the analysis of transnational policy processes. Yet many of the new policy instruments that have become central to transnational governance, such as best practices, rankings, and peer review (Porter 2012), can be seen as responses to the acceleration of policy problems: formal law is simply too slow and inflexible. An interesting way to theorize these changes is Sabel and Zeitlin's (2010) concept of experimentalist governance. This model involves the setting of general revisable framework goals, measures to gauge their achievement, the encouragement of multiple types of rule-making to achieve those goals, a rigorous process of reporting and peer evaluation to determine what works best, and mechanisms for revising the goals, measures, and rules. The model was first explored in the context of the European Union, and more recently at the global level. There are strong similarities with the dynamism of Lasswell's policy stages, especially the appraisal and termination stages where policies are evaluated and a new cycle is started.

A distinctive feature of Lasswell's work was his attempt to reconcile his enthusiasm for scientific methods and his normative commitment to influencing policy to promote human dignity. He saw academics as having a particularly important normative role to play in the policy process: "political scientists, especially those with training and experience in international relations, have an advantage in various transnational operations" (Lasswell 1963: 19). Lasswell and his New Haven School colleagues have been criticized for failing to adequately conceptualize the theoretical basis for their promotion of human dignity, and for their application of this concept to world politics, which at times in the 1960s seemed to critics to use pretentious scientific jargon to rationalize support for US foreign policy. Today decades of relentless criticism of the entanglement of power and scientific truth claims has helped make these shortcomings in Lasswell's work glaringly obvious (Saberi 2012). At the same time, large swathes of official and scholarly literatures on transnational policy processes continue to be presented as value-free, objective, technical, or scientific studies, with as little explicit connection as possible to the big normative questions about power and inequality that Lasswell saw as integral to all

his work, including his analysis of policy stages. Lasswell provides lessons relevant to the ongoing history of the efforts to integrate science and normative commitments—efforts which continue to be highly relevant today.

CONCLUSIONS AND THE POTENTIALS FOR FUTURE RESEARCH

Lasswell's *The Decision Process* and related work has had a pioneering impact and ongoing relevance to the study of domestic and transnational policy processes, and it has provoked, inspired, and helped prepare the ground for a large literature on stages. This literature has refined, altered, or gone beyond Lasswell's work on stages and their social contexts, even if at times scholars are unaware of Lasswell's influence. A distinctive characteristic of Lasswell's work is the ease with which it travels across the boundary between domestic and transnational policy processes. Its sensitivity to the need to go beyond an analysis of formally organized governmental structures to consider the multi-dimensional complexity of power, authority, and the social context of policy processes, resonates strongly with contemporary themes in the study of public policy.

Why then should we return to Lasswell and his work on decision processes? One reason is simply to pay tribute to one of the founders of the contemporary study of policy sciences. We can also enjoy being tourists in a different time, looking out at the world of the mid-twentieth century from the distinctive vantage point of one of its most creative social theorists. Reading Lasswell can remind us that many of the concepts that seem so new, such as time, experimentalism in governance, reliance on informal governance mechanisms, the contributions to governance of non-governmental actors, and the complex entanglements of power, values, and science, can be discerned in his work, a half-century ago.

Often research topics such as transnational governance seem to be analyzing new trends, when they actually reflect recurrent features of world politics that were temporarily obscured by the centralization of power in states following World War II. Lasswell's own formative years preceded the state centrism that followed the war, and his sensitivity to the way that policy processes extend beyond formal state mechanisms reflects this. It is important to be reminded of earlier historical periods that challenge tendencies that wrongly treat our own period as unique. We also can learn from his failures to adequately reconcile his normative and scientific commitments. But most importantly, his enormously creative and free-ranging intellect, and his enthusiasm for tackling big, complex, and important conceptual questions, can inspire us to ask similarly how to organize more effective and ethical policy processes and authoritative decisions in a world where older bureaucratic mechanisms and narrow fields of expertise are no longer adequate to the scale and speed of the domestic and global problems we face.

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CHAPTER 6

C. WRIGHT MILLS, *THE POWER ELITE*

WILLIAM GENIEYS

ANY account of C. Wright Mills's *The Power Elite* (2000a [1956]), needs to avoid two traps: discussing it as a conspiracy theory or as author (Horowitz 1983). This work is one of the most radical critiques of the foundations of US liberal democracy. To this end, Mills takes up the dilemma that Raymond Aron suggests can be found in elite theory: "the theory of divided and united elites brings us back to the old idea that freedom depends on a system of checks and balances ... A unified elite means the end of freedom. But when the groups of the elite are not only distinct but become a disunity, it means the end of the State" (Aron 1950: 143). Mills argues that the emergence of a power elite in the United States after 1945 requires a reconsideration of the foundations of democratic pluralism because competition for power and alternation in office at different levels of government are so profoundly altered (Mills 2000a: ch. 1). Access to positions of national power became limited to members of three elite groups: the "corporate rich," the "warlords," and the members of the "political directorate." Eisenhower's presidency (1953–61) served as a conspicuous sign that political roles were interchangeable in these highest echelons of power since he was a former army general and chief of staff (1945–8) before becoming president of Columbia University (1948–50). It is a paradox of history that Eisenhower's farewell address in 1961 warns of the threat to American democracy arising from the development of a "military-industrial complex" (Rose 1967: 36).

Thus, the theory of Mills has been mistakenly linked with the theory of the *Three C's* of elite theory ("group consciousness, cohesion and conspiracy") set out by James Meisel (1958: 361). In response to the possible accusation that he simply elaborates this conspiracy theory, Mills argues: "the conception of the power elite, accordingly, does not rest upon the assumption that American history since the origins of World War II must be understood as a secret plot, or as a great and co-ordinated conspiracy of the members of this elite" (2000a: 292). The hidden basis of elite political power does not lie in its ability to develop and execute any particular collective project, but in its ability

to monopolize positions of power while at the same time leading US citizens to believe that this form of politics is the “normal” way democratic institutions work.

To understand the rise and the fall of the elite model, it is necessary to return to the following four strands of Mills’s thought. First, Mills’s plea for intellectual eclecticism helps us understand how he developed the basic features of a modern sociology of elites. The second strand relates to his discussion of the relationship between the emergence of a unified elite and the end of democratic pluralism. A third strand addresses the issue of America’s disputed but constantly updated political heritage. The fourth concerns the formation of state elites as the “true” power elite.

THE SOCIOLOGICAL IMAGINATION AND THE ORIGINS OF *THE POWER ELITE*

It is impossible to understand the intellectual foundations of *The Power Elite* without taking into account the implicit and explicit debt to earlier elite theorists (Mills 2000b [1959]: 195–204). Indeed, Mills’s is a very eclectic approach combining insights from Pareto, Mosca, Michels, and Veblen, as well as from Ortega y Gasset, Burnham, Lasswell, Mannheim, Schumpeter, and Aron. As a sociologist of the radical left Mills used these sources to develop an analysis of power which incorporates both Marxian and Weberian analysis (Genieys 2011).

Mills seeks to avoid the over-economistic approach of Marx which saw power as tied to notions of “class struggle” and the “dominant class.” Such terms are not only overladen with ideological content they also offer little operational help in developing a sociological account of power. Mills does not accept the simplistic idea that the economically powerful unilaterally take the major decisions of national importance (Mills 2000a: ch. 12). The idea that economic class is always politically dominant seems wrong insofar as in it there is “not given sufficient autonomy to the political and the agents and it says nothing of the military as such” (Mills 2000a: 284). Mills invites us to consider how processes of modernization have granted to those who wield economic, political, and military power an unprecedented degree of political autonomy. In addition, Marxist approaches underestimate the conflicts *within* social classes, while Mills, with echoes of Weber, shows that we cannot understand the character of the heights of power without recognizing that the logic of movement between the different parts of the top elite has fundamentally altered. Indeed, Mill’s approach to elites is based on a stratified and hierarchical approach to power. The elite is not a collective entity engaged in a struggle to appropriate the means of production. Rather, within each stratum of the elite different groups, “upper” and “lower” strata, compete with each other to gain social prestige and political power (Horowitz 1983: 261). Weber’s influence can also be seen in Mills’s analysis of the rationalization of modern societies, the bureaucratization of institutional forms and the centralization of power and political

authority.¹ Given, he argues, that the United States had largely escaped the European feudal heritage, the resulting weakness of state institutions and bureaucratic power favors the emergence of a power elite (Bottomore 1964: 88).

Mills's own concept of elitism, merges two contrasting approaches from the Italian school of the theory of elites: that of Pareto and Mosca (Genieys 2011: ch. 5). From Pareto he gains the insight that the era of the power elite is not something left over from an earlier period of history, but a result of process of degeneration within democratic political institutions. Mills appropriates the distinction between "governing" and "non-governing" elites and the notion of the circulation of elites to develop the sociological characteristics of *The Power Elite* (Bottomore 1964: 32). He argues that the formation of a unified elite is made possible by the development of contacts between certain "social branches" (*idée d'une circulation entre les différentes branches d'activités sociales dans une société donnée*). This development has given political autonomy to an elite group because it has prevented the circulation of elites: lower strata within government can no longer reach higher levels of power (Mills 2000a: ch. 12, 292–7). From Mosca, Mills clearly takes his own version of a "ruling class" (Mills 2000a: 277). He uses the notions of "elite" and "clique" *status groups*, whose sociological contours are designed to be more precise than those of Mosca's ruling class.² However, Mills retains the idea he so admired in Mosca, that the ruling class is "a top clique and there is this second and larger stratum, with which (a) the top is in continuous and immediate contact, and with which (b) it shares ideas and sentiments and hence, he believes, policies" (Mills 2000b: 203–4). This allows him "to pick up in a neat and meaningful way the Paretian distinction of governing and non-governing elites, in a way less formal than Pareto" (2000b: 204). Using these ideas Mills seeks to show empirically how the differentiation between these two layers of governing elites, initially just a distinction between those who held political office and those who did not, became a structural distinction because it opposes *status groups* whose social origins are radically different. However, if the notion of a closed *top stratum* occupying the heights of power comes from his reading of Pareto and Mosca, he addresses the question of the decision-making power of the elite on the basis of ideas adapted from other elite theorists (Genieys 2011: 197–214).

From Veblen he derives a ferociously critical account of the role of capitalist businessmen. From Michels, he took the idea that hierarchical and centralized political institutions foster the emergence of oligarchic decision-makers. While he contests James Burnham's vision of an era of "managers" deriving their power from that of capital (Gerth and Mills 1942; Mills 2000a: 147), he borrows the idea that decision-making power is passed into the hands of an elite whose organizational expertise makes its members interchangeable. Mills revisits Mannheim's idea of an unparalleled talent for ideology shaping (2000a: 326). Schumpeter helped shape his understanding of the character of social stratification and the role played by the "creative destruction" of the great entrepreneurs (Mills 2000a: 96). Lasswell's (1941) "garrison state" model drew attention to the increasing importance of the decision-making power of "warlords" in US democracy (2000a: chs 8 and 9). On the basis of all these contributions Mills lays

the foundations for the modern sociology of elites by pointing to the end of a vertical circulation of elites and its replacement by a horizontal circulation among interchangeable members of various upper echelons (Genieys 2011: 206). Thus, while seeking to describe the power of “the powerful and the arrogant” (this was the original title of his book), Mills’s reading of the wider literature gradually led him to set out the sociological spectre of a unified elite at the top of the American democracy.

THE *POWER ELITE* AS HERALD OF THE DEMISE OF AMERICAN PLURALISM?

The intellectual agenda of *The Power Elite* is an analysis of the conditions under which democratic pluralism can be reversed. On this point the Mills thesis is very clear: only through offering an account of the history of the development of democratic institutions can we understand how a society at first so open and pluralist as the United States of America gradually fell after World War II under the yoke of a unified elite made up of leading figures from the military, finance, and politics (Mills 2000a: chs 1 and 12). Indeed, political pluralism, invented by the writings of the founding fathers like Jefferson and Madison, idealized by Tocqueville and theorized by Bentley and Truman became a sociological artifact as civil society and citizen groups were dispossessed of their shares of democratic power. The constitutional doctrine of *checks and balances* has become distorted by the way Congress works and key institutions of democratic representation no longer occupy anything more than an intermediary role in American political life (Mills 2000a: ch. 11). Thus in contemporary America faith in pluralism is the ideological veneer that hides the real power of the “higher circles.”

To understand this new situation, as Mills saw it, we need to look at longer term changes in the social structure of the elite. In an earlier piece of research analysing the historical transformation of “American business elites,” he examines parental class background, social mobility, educational attainment, and political activity of its members (Mills 1945). The analysis shows a high degree of upward mobility running from the very beginning of the nineteenth century. Nevertheless with the first industrial revolution, the business elite consolidates its economic power and gradually closes off entry into its ranks through providing its own children with higher levels of education and by becoming increasingly directly involved in political affairs. In *The Power Elite*, Mills extends his observations to the twentieth century by showing that the “very rich,” an economically, socially, and politically homogeneous group, came to prominence within the business elite (2000a: chs 5 and 7). At the same time, the rise of “warlords” to the heights of power was demonstrated by the increased presence of figures from the military among governing elites. In addition, the process of bureaucratization, common to both sectors, promoted circulation in positions of power between members of the business elite and the military elite. With these connections to each other

the new elite could better defend its interests through shaping policy decisions.³ This is reflected in particular by the placing of the technical expertise of the military elite at the disposal of industrial armaments elites, thus bypassing scrutiny by Congress and the other parts of executive government.

Moreover, these elites, plus members of the political directorate, were able to colonize the higher reaches of power because they had very homogenous social characteristics. The overwhelming majority came from the urban upper middle class and shared Protestant values. This social background was given substantive importance as the members of these groups were socialized in venues such as social clubs, business groups, and East Coast Ivy League universities. Here they developed common tastes and social practices which made subsequent collaboration between them appear “natural” and helped maintain their group identity. These attributes form the sociological foundations for the *Power Elite* which is then defined in these terms: “The people of the higher circles may also be conceived as members of a top social stratum, as a set of groups whose members know one another, see one another socially and at business, and so in making decisions, take one another into account. [The elite] according to this conception, feel themselves to be, and are felt by others to be, the inner circle of ‘the upper social classes.’” (2000a: 11). Nevertheless, as Raymond Aron (1960: 157) wisely commented, if there is no doubt that the leaders of the military and industry leaders have close relations with each other by virtue of the weapons orders placed by the military and the post-retirement positions offered to the generals in the armaments industry, where is the proof that these personal or social links are strong enough to shape American diplomacy and military expenditure decisions?

BEYOND THE POWER ELITE: RULING ELITE OR NEW OLIGARCHY?

The power elite thesis has been the subject of heated controversy between its heirs and critics (Horowitz 1964; Domhoff and Ballard 1968; Dahl 1958, 1961), and far from subsiding the debate has recently gained strength (Domhoff 2005, 2006, 2007a and b; Winters and Page 2009; Jacobs and Soss 2010). The Marxist critique of *The Power Elite* was somewhat ambiguous. If at first, Mills was accused of “vulgar Marxism” and rejected completely (Balbus 1971), the work was later more favorably reassessed in the context of a more sophisticated form of structural Marxist analysis (Domhoff 2005 [1967]). Paradoxically, one of the harshest criticisms came from another figure from the radical new left, Daniel Bell (Horowitz 1983: 262). He denounced the imprecision and universalist pretensions of the concept of the power elite (Bell 1958). Mills’s model was, according to Bell, static and ahistorical. It neglected the role of ideology by conceptualizing political institutions as “granite blocks” with a summit, but without identity or ideas, and underplayed the conflicts that went on within them. In short, while

Mills argues that political power is autonomous, the whole analysis is geared toward the study of power, not politics (Bell 1958: 250).

The pluralist sociologist Talcott Parsons (1957) also emphasized the fuzzy conceptual apparatus of Mills's *Power Elite* (e.g. "class" and the "higher immorality," the "very rich" and the "corporate rich") which prevented him from seeing that there are several competing groups (business, finance) that can influence political decisions. He also criticized him for addressing just one aspect of power, its distributive aspect, which functions as a zero-sum game and confuses sectional and common interests. Parsons emphasized how Mills obscures the infighting between government agencies whose staff are recruited differently and underestimates the growing power of politico-administrative elites in relation to economic interests (Parsons 1957: 217 et seq.). The growing specialization and differentiation of each sector of social activity then makes it unlikely that a unified elite will develop. The theoretical and empirical criticism developed by the pluralist political scientist Robert Dahl is radical (1958, 1961). Dahl criticizes Mills for basing his definition of a governing elite on an approach which confuses the potential use of real power with the actual exercise of power, thus ignoring the fact that inequality among elites in resources associated with political influence does not mean that any one elite has a monopoly (Dahl 1958). Those elite groups with apparently fewer resources can mobilize them when a decision which affects them is being taken. In addition, the fact that a less privileged elite can prevail in some important political decisions does not mean they prevail in all of them. Thus Dahl argues that Mills's thesis should be properly tested in a rigorous study of the actual exercise of power through a detailed analysis of how decisions are made in practice (1961). Horowitz concludes that Robert Dahl "stands Mills on his head by reversing the conclusions reached in his work" (1983: 277).

G. William Domhoff, continuing in the Millsian tradition, asked *Who Rules America?* (2005 [1967], 2006, 2007a and b). First, Domhoff moved the discussion of elites closer to Marxist theories of class by combining Baltzell's (1964) insights on the Protestant establishment with those of Mills (2005: 17) to show how the authority of the US *upper class* challenges pluralist theory. Domhoff then emphasized the role of clubs, taking the example of the *Bohemian Club*, to show how the Ruling Elite is based upon homogenization, social reproduction, and a consensus on social values (Domhoff 2005: 269). Domhoff stressed that members of this class, around 1 percent of the US population, occupy the majority of positions of influence in all sectors of social activity. Nevertheless, he said that this "ruling elite is a well-defined group" because of the long-term social interactions and interconnections between them (Domhoff 1978: 123). Overlapping networks in "policy-formation organizations" allows the elite to shape public policy decisions (Domhoff 2005: 269). In *Who's Running America* Thomas R. Dye confirmed the oligarchic tendencies in national *policy-making* in the United States via "policy-planning organisations"—think tanks like the Brookings Institution or the American Enterprise Institute—by showing that it is elites within government institutions who pull the strings (1983, 2001). The Mills-Domhoff tradition has also given rise to the "power structure" approach (Domhoff 2005, 2007a and b). By identifying highly

structured networks of elites in the major institutions of American politics and social life and by the pointing out the interaction and connections between the individuals at their core, one can substantiate the argument that a *ruling elite* governs America. Gwen Moore's (1979) empirical research on the structure of the US national elite shows the rise to prominence of "central circles" composed of the most powerful and influential national elites. Other studies have examined the role of elite networks ("*policy-planning elites*") which have increased their power in "expert bodies" (such as *policy planning organizations*) separate from major departments and agencies and can thus shape the development of public policies (Burriss 1992).⁴

More recently, the notion of the power elite has been revived even more explicitly. The imperious behavior of the "Bush Elite" temporarily undermined a range of basic features of American liberal democracy (Higley and Burton 2006). We can also include Shamus R. Khan's (2011) analysis of how some US colleges, including the very exclusive St Paul's School, have adapted their curricula to foster "new qualities" such as "cultural omnivorism" so that future members of the elite can adapt to a globalized world. Recent work challenging a pluralist perspective has shown that decision-making elites give weight to the views of business and expert groups and ignore public opinion (Jacobs and Soss 2010: 351). Observing the effects of financial globalization on increasing social inequality, Jeffrey Winters and Benjamin Page have set out a new theory of oligarchy (Winters and Page 2009; Winters 2011). These "new Elitists," like Mills and Domhoff focusing on the US case, see oligarchy resulting from the accumulation of political and material inequalities in one small group: "the one per cent" (Winters and Page 2009: 733–7). These "oligarchs" mobilize the extreme wealth concentrated in their hands, not only to become wealthier, but more importantly to influence economic, monetary, fiscal, and redistributive policies. Through means such as lobbying, financing election campaigns, shaping public opinion through the media, and influencing the appointment of constitutional court judges, these oligarchs form a new power elite, where the role of the military is replaced by leading figures from the financial sector. Nevertheless, the point raised by Dahl that such analyses might be confusing potential with actual power remains unresolved in this work on the new financial oligarchy (Winters 2011; Murray and Scott 2012). Nevertheless, other research linking the transformation of state power to changes in public policy mean that the Mills tradition is being updated in fruitful ways.

THE EMERGENCE OF A POLICY POWER ELITE WITHIN THE STATE

For Mills, the state plays a fundamental role in the creation of *The Power Elite*. Indeed, the state is involved in the transformation of the power structure at various levels. Mills implicitly adopts a Weberian approach to the state-building process and recognizes

that the centralization of political power and monopoly of coercion by the military could also be found in the United States. Yet on the other hand the lack of development of a professional and autonomous bureaucracy, capable of acting as a counterweight in the process of decision-making, paved the way for the emergence of a power elite. Everything pointed to Mills's elite, embedded as it was in the power structure, appropriating the resources of a weak state so that it could later be emancipated from it.⁵ Therefore the nature of elite power can be explained as a result of the weakness of the US state (Birnbbaum 1984). Conversely, in European societies with strong states, the sociological characteristics of the power elite are described and analyzed through the prism of the feudal legacy using terms such as "the state nobility" (Bourdieu 1996).

In fact this is how the British sociologist Ralf Miliband, Mills's friend and collaborator, sought to reconcile the existence of a power elite with Marxist analysis, by suggesting that a *state elite* with autonomous political power puts itself at the disposal of the dominant economic class (1969). Prewitt and Stone (1973: 73–7) pointed out two limitations of this approach: the possibility that the business elite itself is made up of competing interests and the possibility that some public policies might be developed independently of private interests. William Domhoff (1990, 1996) also addresses the issue of the autonomy of the state and its elite to argue that the decision-making process is controlled by a *ruling elite* whose power extends far beyond the porous borders of the American state through its influence on "policy formation organizations." The ruling elite then shapes the content of public policies since such bodies incorporate networks of business and state elites brought into contact with each other through devices such as the "revolving door."

Without questioning the strong similarities between the social backgrounds of technocratic-bureaucratic and business elites a number of authors have emphasized that bureaucratic officials and political appointees, many from the private sector, can develop public policies that differ from those advocated by interest groups (Laumann and Knoke 1987; Genieys 2010). It thus appears as if, at a level of decision-making below that which forms the focus of Mills's analysis, socially homogeneous and cohesive elite groups, committed to defending program of public welfare, are asserting their power within the state (Genieys 2010). Stephen Krasner (1978) opened this line of research by showing that, despite the weakness of the American state, an elite group well established within the executive branch, above all the White House and the State Department, used the notion of the "defense of the national interest" to impose its foreign policy on big corporations. William Domhoff criticized this argument by showing that it underestimates the close social relationships between business elites and the State Department in networks such as that found in the Advisory Committee on Problems of Foreign Policy (of the pre-war years), a policy unit within the State Department (1990).⁶ A study by neo-Marxist writer Theda Skocpol casts light on other aspects of the relationship between the character of the state and the nature of power elites and their varying levels of cohesion (1979, 1985). In her work on states and social revolutions she adopted a "true elitist" perspective to show the autonomy of the state from civil society (Mann 1993). Her analysis goes on to highlight the role of particular

sectorally specialized bureaucratic elite groups in the formulation and implementation of New Deal policies (Skocpol 1985: 48–54). Skocpol and her collaborators have shown how small elite groups managed in some fields to shape state action, sustaining their views in the face of opposition from interest groups (Skocpol and Finegold 1982; Skocpol and Ikenberry 1983). Building on this research, and testing the continued relevance of Mills's thesis, my own work on health insurance and defense policies in France showed that small elite groups acted in some areas of public policy as the New Custodians of the State (Genieys 2010). With this empirical constant, we may give up the vain search for a single power elite in its entirety, but rather to search for sectoral elite structures within democratic states. In this perspective the study of public policy is a fertile terrain in which to explore this new reality.

NOTES

1. It should be noted here that Mills claims his development of the distinction between class and status groups is derived from Max Weber.
2. Mosca's intellectual influence on Mills is faintly echoed in *The Power Elite*. Yet Mills clearly explains his debt to Mosca during a lecture at Columbia University on the theme "On Intellectual Craftsmanship" (Mills 2000a: 386 n. 7). Although the lecture was delivered at Columbia College in Apr. 1952 it was to be published three years after the publication of *The Power Elite* (1956) as a companion to his other famous book *The Sociological Imagination* (Mills 2000b: 203–4 [1959]).
3. Mills (2000a: 288–9) points out that "The inner core of the power elite consists, first, of those who interchange commanding roles at the top of one dominant institutional order with those in another ... In their very career, however, they interchange rôles within the big three and thus readily transcend the particularity of interest in any one of these institutional milieu."
4. To ascertain that this "inner circle" consists of those elites with most power and influence, Moore (1979: 675) compares their reputation with the elite who occupy the same type of position in the area but does not belong to the network in question. He then performs a correlation between social origin and belonging to the central circle of power to show how belonging to the upper class promotes the kind of networking specific to this group of elites.
5. For Mills (2000a: 267) "the idea that the power system is a balancing society leads us to assume that the state is a visible mask for autonomous powers, but in fact, the powers of decisions are now firmly vested within the state. The old lobby, visible or invisible, is now the visible government. This 'governmentalization of the lobby' has proceed in both the legislative and the executive domains, as well as between them. The executive bureaucracy becomes not only the center of power but also the arena within which and in terms of which all conflicts of power are resolved or denied resolution. Administration replaces electoral politics; the maneuvering of cliques replaces the clash of parties."
6. Domhoff (1990: 144) comments that its members are in regular contact with the "policy-planning network" including the Council on Foreign Relations, composed of a majority of members from JP Morgan, Ivy League professors, and international business lawyers, or with the Committee for Economic Development.

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CHAPTER 7

CHARLES E. LINDBLOM, “THE SCIENCE OF MUDDLING THROUGH”

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INTRODUCTION

IN “The Science of Muddling Through” (1959), a remarkably short contribution, by Yale political scientist Charles E. Lindblom, the essence of several years of thinking about decision-making in complex situations like government and bureaucracy was distilled into a few principles. In such situations, Lindblom argued, efforts to critically assess each and every possible course of action were doomed to failure and analysts and decision-makers grappling with policy and other difficult subjects would be much better advised to focus their attention on the merits of marginal or “incremental” changes from the status quo.

While the reasons for this advice are many and the empirical verification of the superiority of this method still remains elusive, the article influenced several generations of thinking about public policy decision-making and its impact continues to be felt in the prevailing orthodoxies of our contemporary era.

Origins

Incrementalism, or the study of “muddling through” behavior on the part of actual administrators and executives, was originally developed by Lindblom as a decision-making model in the early 1950s while he was at Yale’s Economics Department and found some space in *Politics, Economics and Welfare* (Dahl and Lindblom 1953). The concept was rooted in his studies of decision-making processes in corporations

and government, which he argued were characterized less by rational calculation than the logic of political bargaining (Lindblom 1955).

Between 1953 and 1963 Lindblom and his colleagues developed the notions of bounded rationality and satisficing activity among decision-makers, which Herbert Simon (1949, 1955) had first set out in the 1940s when writing about administrative behavior.¹ Working from observations of actual governmental decision-making, this new crop of scholars rejected the possibility of rational, maximizing policy processes and outcomes that had been suggested by economists and planners like Jan Tinbergen in previous years as both optimal goals and empirical reality, instead suggesting "the intelligence of democracy" was its ability to aggregate multiple possible positions into only a few limited options for action which decision-makers could evaluate thoroughly (Braybrooke and Lindblom 1963; Dahl and Lindblom 1953; Lindblom 1955, 1958b, 1959).

Like Simon, Lindblom treated decisions as bounded and limited human activities rather than as precise, scientific undertakings capable of optimization or maximization (Lindblom 1977; Jones 2002; Knott et al. 2003). He argued that at the core of his alternative view of "incremental" or "disjointed incrementalism" was the recognition decisions required political agreement and trial and error learning processes lay at the core of policy changes (Lindblom and Cohen, 1979).

The Science of Muddling Through (1959)

"The Science of Muddling Through" was an early exposition of incrementalism since the term had not yet been coined; Lindblom here calls it the method of "Successive Limited Comparison" or "marginal" analysis. Lindblom has as his goal in the article to describe the "scientific" qualities of the "muddling through" method, that is, a form of limited consideration of alternatives often castigated by adherents of more comprehensive maximization or "rational" methods. As he put it in the article: "Successive limited comparison is, then, indeed a method or a system; it is not a failure of method for which administrators ought to apologize" (Lindblom 1959: 87). This is important because successive limited comparison is a very common approach to policy- and decision-making in practice and was often argued to be ill-suited to address complex issues in many real-life situations (Lindblom 1959: 88). Lindblom distinguished this method from simply random or accidental choice processes but, more importantly, argued it was in fact better suited to complex decision-making situations than its more "rational" alternatives. He attributed this superiority to multiple factors. First of all, he argued, like Simon, decision-makers face limits in their ability to "calculate" optimal decisions. Goals, objectives, and constraints are generally not agreed upon, nor is their ranking likely to be. Hence, decision-makers are usually unable to formulate the appropriate criteria they need to make an "optimal" decision (Lindblom 1959: 82). The successive limited comparison model is useful according to Lindblom because it sidesteps these issues by focusing on marginal alternatives to already existing policies; it intertwines evaluation

and empirical analysis by focusing the attention of the decision-makers “on marginal or incremental values” (Lindblom 1959: 82).

As a result, in this approach a “good” policy is defined as one that is agreed upon, not a policy for which there is agreement on values, or which optimizes some abstract standard or criteria, which Lindblom argued was the (unattainable) goal of rational models (Lindblom 1959: 83). Furthermore, he argued, incrementalism simplifies the analysis involved in the policy process. First it does so simply by limiting “policy comparisons to those policies that differ in relatively small degree from policies presently in effect” (Lindblom 1959: 84). This in practice excludes immediately unrealistic options and maximizes the “returns” of available knowledge. Second, and relatedly, both the potential relevant consequences of many possible policies and the values attached to them are routinely and safely ignored.

This “incompleteness” in policy-making and in the pattern of decisions leads to the question of how to ensure that it does not simply taint the process and result in poor decisions. Lindblom argues an imperfect but sufficient remedy is the pluralist nature of democratic administrations and the partisan attitude of decision-makers which ensures important variations and alternatives to existing arrangements—important in the sense they affect some major actor—will be raised and considered (Lindblom 1959: 85). While not necessarily strong enough to impose a certain policy outcome, in most cases such partisan activity, Lindblom argued, may be sufficient to veto large changes.

Finally, the successive limited comparison model employs “a process of successive approximation to some desired objectives in which what is desired itself continues to change under reconsideration” (Lindblom 1959: 86). This approach, he argued, fitted the reality of policy-making and limited the potential for large mistakes. By using small, successive steps, policy-makers can exploit their knowledge of similar steps they undertook in the past and can quickly assess whether their predictions about the policy’s effects are correct and amend the policy accordingly (Lindblom 1959: 86).

THE IMPACT OF THE SCIENCE OF MUDDLING THROUGH ON THE DEVELOPMENT OF INCREMENTALISM AND DECISION-MAKING STUDIES IN THE POLICY SCIENCES

Incrementalism grew out of Lindblom’s work to posit actors face both organizational and cognitive limitations and deal with an uncertain and complex world (Knott et al. 2003), about which they disagree. A way to get past these roadblocks is to try to simplify (rather than overcome) them. Incrementalism focuses on minimizing the impact

of the inevitable mistakes of policy-makers, and recognizes not all possible solutions are considered. Among the tools used to do so are a variety of shortcuts that include the use of rules of thumb, reliance on market forces, focusing on a few, select, policy options, and limiting the range of decision to the immediate future (Lindblom 1959, 1968, 1979).

Incrementalism evolved over a long period of time. Lindblom's own work developed over the two decades following publication of "The Science of Muddling Through" through to his own rejoinder ("Still Muddling, Not Yet Through," 1979 article in *Public Administration Review*) and to later developments in his text *The Policy-Making Process* (1984; and Lindblom and Woodhouse 1993). Following publication of "The Science of Muddling Through" incrementalism became a household concept in the social sciences spurring an extremely broad set of analyses. The topics studied using this model ranged from city politics to US foreign policy (Schraeder and Borstelman 1994), from budgets (Wildavsky 1964) to decarceration approaches (Stolz 1984), and from television violence (Timmer 2004) to technological innovation (Rehn and Lindahl 2012).

In "Still Muddling, Not Yet Through," Lindblom's 1979 rejoinder to critics of his own classic work, he distinguished three variations on the methods of successive limited comparisons, of which only one, he noted, would generally produce the results his 1959 work had anticipated. *Simple incremental analysis* only deals with policies incrementally distant from the status quo and lacked the sophistication of his own model's links to representative and interest articulation systems in society. At the other end of the scale, *strategic analysis* includes the strategically defined pathways to simplifying complex problems which he argued often failed to deliver on promises of optimal or maximal decisions and outcomes. In between was his own method of *disjointed incrementalism* in which he bundled the methods he had previously written about.²

However, providing a clear-cut definition of incrementalism was difficult and ultimately Lindblom (1979: 517) settled on defining it as a "mutually supporting set of simplifying and focusing stratagems" for public policy decision-making. However some of the confusion most likely stems from Lindblom's (1961: 306) statement that the "incremental method is characterized by its practitioner's preoccupation with: (1) only that limited set of policy alternatives that are politically relevant, these typically being policies only incrementally different from existing policies." This creates a situation in which "incremental" seems to mean both small changes away from an existing policy and a framing process that limits the choice of policy alternatives.

Lindblom always stressed that a very strong correlation existed between the complex and unpredictable nature of the policy field, which actors must deal with, and the adoption of incremental strategies as a way to tackle this complexity. According to Lindblom, there are two reasons why decisions typically do not stray far from the status quo. First, since bargaining requires distributing limited resources among various participants, it is easier to continue the existing pattern of distribution rather than try to negotiate the redistribution that would almost certainly arise under radically new proposals. Since the benefits and costs of present arrangements are known to the

policy actors (unlike the uncertainties surrounding new arrangements) agreement on major changes is also more difficult. The result is typically either continuation of the status quo or agreement to make only small changes to it. Second, the standard operating procedures of bureaucracies also tend to promote the continuation of existing practices. The methods by which bureaucrats identify options and the procedures and criteria for choice are often laid out in advance, inhibiting innovation and perpetuating existing arrangements (Gortner et al. 1987: 257).

From a cognitive point of view, however, incrementalism favors an interpretation of decision processes leaning toward incomplete knowledge and limited capacities, coupled with possibly shifting preferences. It also fits only situations where large constraints are placed on the actors' degree of choice. In a society practicing slavery, for example, the issue from the point of view of the slaves is both grave and ideally requires a quick, non-incremental move toward abolitionism.

THE CONTINUING IMPACT OF MUDDLING THROUGH: BUDGETING AND PUNCTUATED EQUILIBRIUM ANALYSIS

While it is relatively simple to trace the origins of incrementalism to a few relatively inconspicuous pages in *Politics, Economics and Welfare* (Dahl and Lindblom 1953), we should note that, following the publication of "The Science of Muddling Through," Lindblom "grew" the concept over various stages spanning 40 years. Over this period what had started as a simple analysis of decision processes evolved not only into a more refined exploration of the topic but also expanded to broach the strategic framing of decisions and the relationship between the costs of decision-making and the cognitive and ideological limits of human beings.

Following Lindblom's lead, functionally, incrementalism expanded from the original decision-making concept, to being applied to at least two other very important areas: budgeting and management. For example, in the management literature (Camillus 1982) we find Quinn's (1980) concept of logical incrementalism. This model argues that a continuous process of integration exists between the formulation of a strategy and its implementation. Here crises may be utilized as stepping stones leading to change. Logical incrementalism supports a highly flexible strategy for implementation through the employment of decentralized decision-making and broad framing of a strategy. Quinn (1980) describes this not as "muddling through" but as a logical strategy under complex conditions. But the greatest impact of incremental thinking was in the area of budgetary policy and politics. Budgetary incrementalism is premised on the notion that "this year's budget is based on last year's budget, with special attention given to a narrow range of increases or decreases"

(Davis et al. 1966: 529). The model examines the integration of incremental steps into the complex and contentious process of financial resources allocation and was, and is, the dominant mode of thinking about this significant area of government activity (Schick 1983; Wildavsky 1964, 1988).

Another major impact of incrementalism was on "punctuated equilibrium" thinking about decision-making outcomes (Howlett and Migone 2011). In punctuated equilibrium models incrementalism represents one of two main decision-making modes: in which periods of incrementalism are routinely punctuated by short-lived periods of more radical change or "policy punctuations" (Baumgartner and Jones 1993).

Both of these impacts and areas will be discussed here.

Impacts on Budgetary Analysis

That incrementalism could apply to budgeting was something that Lindblom himself had noted early on in his work (Lindblom 1961), where he argued the received wisdom of a rational comprehensive approach to budget making was incorrect and incremental models explained much more closely the process in question.

Generally speaking, the seminal works in the field are considered to be Wildavsky's (1964) and Davis et al.'s (1966) works on budgetary incrementalism. This model separated incrementalism as method of budgetary calculation and as a description of the relation among actors involved in the budget process (Good 2011: 43).

As a method of budgetary calculation, Wildavsky (1964: 15) argued "budgeting is incremental, not comprehensive. . . it is based on last year's budget with special attention given to a narrow range of increases or decreases. Thus, the men who make the budget are concerned with relatively small increments to an existing base." In practice, the recursive process of budgeting was perfectly suited to the appearance of incremental strategies. Starting from the most recent baseline, actors would only consider small changes to future budgets, never really addressing full-scale changes. This limited the costs inherent in budgeting because it limited the scope of the analysis (since now only a few potential changes were analyzed) and because it did not involve going over past political struggles that had already been settled when the previous budgets had been agreed upon. When describing the relation between actors, the idea was that when faced with long-term regularities in these interactions we are faced with incremental situations, while a change in the pattern signifies a non-incremental one (Dempster and Wildavsky 1979: 375).

Wildavsky presented a case for incremental approaches in the face of highly routinized behavior and potentially highly disruptive political confrontation on levels of funding. The model was developed rapidly in a broad subfield by authors like Fenno (1966), Sharkansky (1968), and Rickards (1984). Budgetary incrementalism, much like administrative/policy incrementalism, was interpreted in a variety of ways and soon a large number of definitions for incremental behavior in budgeting

emerged (Berry 1990), from a type of “regular” relationship among actors (Dempster and Wildavsky 1979: 375), to the lack of effects on budgeting from external variables (Coward et al. 1975).

This subfield generated a broad debate on how to define what was incremental and what was not. Of course, how we define an increment has a direct bearing in whether we find a pattern of incremental decisions (Anderson and Harbridge 2010) so the question is anything but trivial. There also was a debate on whether a decremental change would count as an incremental move. Here authors differed, with some arguing that only increases would count as incremental (Wanat 1978; Gist 1977), and others accepting changes in both directions (Wildavsky 1964; Sharkansky 1968).

Notwithstanding its limitations, the incremental model became the stock analytical approach for budgeting research until the late 1990s when—in parallel with other developments in the policy-making area—the notion of punctuated equilibrium gained ground in explaining patterns of budgeting and other kinds of decisions, quickly establishing itself as the new orthodoxy (Breunig and Koski 2009, 2012; Jones et al. 1998, 2009). In particular, the incremental model was unable to show sustained proof that incremental patterns are the rule (see e.g. Anderson and Harbridge 2010; Jones et al. 1997) and even Aaron Wildavsky’s co-authors cautiously re-examined the applicability of his theory (Wildavsky and Caiden 2003).

Impact on Punctuated Equilibrium Thinking

Beginning in the mid-1990s, punctuated equilibrium models emerged as a solid alternative to incrementalism (Baumgartner and Jones 1993) but incorporated many of its elements (Howlett and Migone 2011). These new models quickly became dominant in policy-making analysis and in budgetary analysis because of their superior empirical basis and explanatory power (Jones and Baumgartner 2012).

In particular, Baumgartner and Jones’s production in the field of public budgeting and agenda-setting provided the empirical backing needed to support the notion that incremental policy-making, while common and dominant, was in practice only one of two modes of decision-making in which periods of incrementalism are routinely punctuated by short-lived periods of more drastic change or “policy punctuations” (Baumgartner and Jones 1993). Hall (1993) presented the clearest single statement of this position on policy dynamics, although not working explicitly within a punctuated equilibrium framework *per se*.

Hall’s policy change model and classification system is the most often cited in the literature and empirically applied. His work questioned the dominant approach of extant scholarship which tended to argue that all change was incremental (Hecl 1976; Rose 1976). Studying diverse cases of economic policy in Great Britain and France, Hall argued it was necessary to distinguish between the means and ends of policy-making

and between abstract and concrete policy decisions to better understand processes of policy development and stability. According to Hall, this approach revealed three main policy elements that could change at different rates (small scale, typical, incremental and larger-scale, rarer, paradigmatic form) affecting overall policy dynamics in different ways. "First order" changes took place when the calibrations of policy instruments, like mandated increases in passenger safety, changed in the context of existing institutional and instrument boundaries. "Second order" changes involved the alteration of dominant types of policy instruments, which are used within an existing policy regime, for example, when switching from administered emission standards to an emissions tax. "Third order" changes included changes in overall abstract policy goals: to continue with the pollution case, the 1990s shift that occurred in many polities from a focus on ex post end-of-pipe regulation to ex ante preventative production process design.

Furthermore, Hall connected every change process to a different specific cause agent and to a specific overall pattern of a Kuhnian "paradigm shift", which Baumgartner and Jones linked to "punctuated equilibrium" models developed in areas such as paleobiology. Here first and second-order shifts were thought to be generally incremental and usually resulted from activities endogenous to the policy subsystem while third-order changes were "paradigmatic," occurring as anomalies that arose between the policy implementation expected and actual results. The events that triggered the anomalies and the policy-makers' responses (for example, contestation within a policy community on which action to pursue, or the emergence of new visions about policy problems and/or solutions) were typically connected to exogenous events, especially societal change (Sabatier 1988).

Four important methodological, epistemological, and causal elements emerged from this research approach in the last two decades, all stemming ultimately from Lindblom's ruminations in "The Science of Muddling Through." Together they form the basis for the emergence of a new post-incrementalist "punctuated-equilibrium" orthodoxy in the policy sciences. First, it is broadly accepted that any policy development analysis must be historical in nature, covering periods of years or even decades (Sabatier 1993). Second, it is generally agreed political institutions and their embedded policy subsystems act as the primary mechanisms of policy reproduction, during both incremental periods and punctuations (Botcheva and Martin 2001; Clemens and Cook 1999; Howlett and Ramesh 2003). Third, "paradigmatic" change, where a fundamental realignment of most aspects of policy development occurs, is usually believed to take place only when policy institutions are transformed. Without these processes policy change is hypothesized to follow "incremental" patterns (Deeg 2001; Genschel 1997). Fourth, many students of policy dynamics agree paradigmatic transformations (punctuations) usually result from the effects of "external perturbations" causing widespread disruptions in the ideas, beliefs, actors, institutions, and practices of an existing policy subsystem (Sabatier and Jenkins-Smith 1993; Smith 2000; Thelen 2003, 2004).

CRITIQUES OF INCREMENTALISM

While incrementalism was the dominant model of public policy decision-making for over 40 years and continues to influence and inform present-day models and thinking, the terms and concepts first developed in “The Science of Muddling Through” faced a continual series of challengers and detractors (cf. Lustick 1980; Etzioni 1967; Dror 1964). Between 1950 and 1970 the chief criticism had normative or theoretical bases. Its opponents critiqued what they considered to be incrementalism’s overly political nature and, like the adherents to more maximizing or “synoptic” models critiqued by Lindblom, continued to believe and assert following incremental prescriptions would lead to sub-optimal decisions which would simply reinforce the status quo. Those who supported more “rationalistic” models argued synoptic reasoning was possible, that it was actually practiced by rational actors (Arrow 1951; Downs 1957), and that, theoretically, it was possible to master complex, multiple interrelated variables if one engaged in a process of complete analysis and planning. Thus, in this period, the two “schools” differed mainly in the faith they had in the ability of policy-makers to design (and implement) successful solutions to complex problems.

If we do not count the much noted issue of the problems with the definition of an increment (Berry 1990; Weiss and Woodhouse 1992), criticism of incrementalism in general fell within five major categories (Redner 1993). First, it was contended that it lacks goal-orientation. However, this weak critique can be dispelled by even a brief review of the literature (Lindblom and Woodhouse 1993). A more serious charge is that it is inherently conservative, couching a pro-elite bias in its structure. It is true that incremental tools could be used to maintain the status quo. However, this does not seem attributable to incrementalism *per se*, but rather to the distribution of power in the system where decisions are made. As Hayes (2001: 35) noted, we can speak of incrementalists like Lindblom as “meliorative liberals” for whom society and its institutions require change but where the fallibility of individuals and of their laws and traditions leads us to question the capacity of rational approaches and prefer systemic rationality to articulated rationality.

Two other critiques focus on the practical applicability of the model. The first is that it only fits stable, non-crisis situations. However, it is unclear why other approaches would have more success in this situation considering crisis decision-making is unstable by definition. Second, incrementalism is said to be vulnerable to threshold effects. In this case, small marginal changes, which theoretically are easily reversible, cannot be if a threshold is crossed. Once again, this is a problem extending to all decision-making models, and not just incrementalism.

Finally, it is charged the meaning of incrementalism is confused. This is a concrete issue dependent on the fact that Lindblom was rather late in clearly outlining the meaning of the term, and on the latitude other authors granted themselves in applying it.

We should also note, as already mentioned, a correlated problem emerged alongside incremental research: how to measure incremental steps in practical terms. What separated an incremental from a non-incremental change? Part of the debate—whether only small steps counted as incremental—was misguided in that Dahl and Lindblom (1953: 82) had noted the process could deliver either small steps or larger adjustments.

In the first half of the period of incremental orthodoxy, the main issue was a problem of interpretation. In general, this led to a theoretical compromise where incrementalism was seen as descriptive of the practices decision-makers actually followed in many of their day-to-day activities (Smith and May 1980), while the synoptic model was presented as prescriptive of how these practices should be reformed in order to produce better results (Schulman 1975).³ While in most cases authors argued that decision-makers should blend these two approaches (Etzioni 1967), it is in no way certain that such methodological syntheses were either possible or feasible. Smith and May (1980), for example, argued against these synthetic attempts. They noted that the frequency with which scholars criticized current practices and demanded changes led them to conclude that there was very little connection between synoptic and prescriptive assumptions, as Lindblom had predicted would be the case in general practices.

By the end of the 1970s it was evident to most observers that neither model could explain both what *is* and what *ought to be* in the realm of decision making. Incrementalism did not give us very convincing prescriptive guidelines for policy-making behavior and practices, and synoptic models were not very persuasive when attempting to explain how people behaved in practice. By the early 1980s, it had become clear to many scholars the continuously running debate between those who advocated rationalism and those who stood for incrementalism was interfering with empirical work and further theoretical development of the field. As Smith and May (1980: 156) argued: "A debate about the relative merits of rationalistic as opposed to incrementalist models of decision-making has featured for some years now and although the terms of this debate are relatively well known it has had comparatively little impact upon empirical research in the areas of either policy or administrative studies." Instead of continuing with this mostly sterile debate, the authors suggested:

We require more than one account to describe the several facets of organizational life. The problem is not to reconcile the differences between contrasting rational and incremental models, nor to construct some third alternative which combines the strongest features of each. The problem is to relate the two in the sense of spelling out the relationship between the social realities with which each is concerned. (Smith and May 1980: 156)

Throughout the 1980s, policy scholars aware of these limitations in both rational and incremental models of decision-making sought various alternatives. Notwithstanding Smith and May's (1980) admonition, some scholars insisted upon synthesizing the two models (Etzioni 1986). On a different level, others embraced the elements of

capriciousness and unpredictability inherent in decision making and developed models premised on more or less random interactions of floating groups of policy-makers, such as the “garbage can” model pioneered by Cohen et al. (1972, 1979). If neither of these theoretical directions proved particularly fruitful, a third effort to empirically clarify the nature of decision-making styles, and the likely conditions under which they would be employed, generated more lasting value and continues to inform present-day work on the subject.

In the field of budgetary incrementalism the question of what constituted an increment was also quite relevant. Bailey and O'Connor (1975) argued this problem with definition stemmed from the confusion that derived from incrementalism being used to describe both processes and outputs. They argued once authors properly separated the two, thinking about incrementalism as bargaining (the process) as potentially but not necessarily correlated to an incremental output, interesting research results could follow. Dempster and Wildavsky (1979: 375) argued the step to be taken was to think of the difference between incremental and non-incremental in terms of thinking not so much about the size of the increment but rather of whether the process was “following or departing from an established pattern of behaviour.”

Ultimately, the “neoincremental” school argued incrementalism should be redefined as a series of evaluative processes aiding decision-making by limiting analysis to familiar policies and to a subset of possible consequences, employing a trial-and-error approach, and tending toward problem remediation rather than positive goal attainment (Weiss and Woodhouse 1992; Lindblom and Woodhouse 1993; Hayes 1992, 2001).

At the core of the neoincrementalist agenda was the project of recapturing the original goals of incrementalism and partisan mutual adjustment. Neoincremental writers noted that incremental analysis has a tendency both to conflate policy goals, values, and empirical problems (rather than creating an ordinal sequence for them), and toward fragmenting analysis along the partisan spectrum, assigning to each actor a step in the problem (Weiss and Woodhouse 1992: 256).

This ultimately meant developing methods to deal effectively with disagreement and uncertainty in policy-making (Weiss and Woodhouse 1992; Woodhouse and Collingridge 1993). Neoincrementalists laid out the steps that could help in achieving this goal. Decision-makers could introduce precautionary elements in their policies if some of the potential outcomes of the policies might lead to unacceptable risks. Second, if resistance to change is an issue, flexibility could be introduced into policies. Lastly, barriers to learning can be reduced by employing extra resources in learning from experience (Woodhouse and Collingridge 1993: 146). Woodhouse and Collingridge (1993, p. 146) noted how

First, since we do not want to step over a cliff while learning from experience, it makes sense to protect against unacceptable risks where feasible. Second, since learning usually takes awhile under the best of circumstances, it makes sense to arrange policy so that it can be changed fairly readily when negative feedback is perceived to warrant. Third, because people and organizations do not automatically

learn to do better—indeed, we often have great difficulty at learning—it makes sense to prepare deliberately for learning.

CONCLUSION

There is no doubt “The Science of Muddling Through” (Lindblom 1959) represented a central moment in the study of decision- and policy-making. And half a century after its publication its insights remain at the center of the discipline. To Lindblom (1959: 87–8; 1979: 518–19). Incrementalism always represented the best available option rather than the final word in the field. However, the problematic habit of describing incrementalism as small-step change was common and we cannot say that neoincrementalism rescued the notion from what can be considered to be its “muddied” state (Woodhouse and Collingridge 1993: 131). Ultimately it was the emergence of punctuated equilibrium models in the 1990s that absorbed the debate about incrementalism and moved its study forward.

This, however, leaves a series of complex questions at least partially unanswered. When embracing punctuated equilibrium models we are still faced with the task of understanding and explaining why and how equilibrium comes about and persists. This generates at least three important areas of enquiry:

1. What is it exactly about institutions and subsystems, which allow them to hold policy-making in equilibrium?
2. What factors lead to the overloading of an equilibrium and promote paradigmatic change or “policy punctuations” and how context dependent are these?
3. What exactly is it that is being held in equilibrium or changing, e.g. policy goals, policy ideas, or various types of policy specifications, or all of these together?
4. Furthermore, as we are now looking at punctuations as important watershed moments, do they always occur rapidly, as is suggested by the “avalanche” metaphor often used to describe them or can they also occur over long periods of time through consecutive incremental changes?

Interestingly, of course, these are all questions which motivated and informed Lindblom’s seminal work “The Science of Muddling Through” and underscoring its continued relevance in the contemporary era.

NOTES

1. When co-authoring Lindblom took on the policy-making side of each project, while the co-author supplemented this analysis. For example, Robert Dahl provided examples from American politics and Braybrooke, philosophical or conceptual analysis.

2. There was debate about how effective “muddling through” actually was; partially this depended on the vagueness of the process and on the complexity of actually measuring it. Bendor (1995) proposed a formal model solving this issue. For an interesting modeling of the effects of complexity and limited information on the decision-maker, see Knott et al. (2003).
3. It should be noted that Schulman (1975) did not argue that a wholesale return to complete synoptic analysis was possible. Rather he advocated that some specific types of policies, e.g. space exploration, do not seem to fit well with incremental strategies and trial-and-error approaches.

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CHAPTER 8

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THOMAS R. DYE, *POLITICS, ECONOMICS AND THE PUBLIC: POLICY OUTCOMES IN THE AMERICAN STATES*

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MICHAEL GOLDSMITH

THOMAS Dye's book was first published in 1966. To understand its importance in the political science literature on public policy it is necessary to set it in the context of the state of political science at that time. I first came across the book in 1970 while on sabbatical in Canada and was impressed then. Rereading the book for the purpose of writing this chapter, I remained impressed and was struck by two things. First, how well written the book was, and how carefully the author set out his hypotheses, set about testing them, and wrote up his results. In this sense the book remains a model for graduate students setting out on the task of writing up a thesis. Second, by today's standards the book is pretty basic in its use of statistical methods. It is for this latter reason that the reader should have some understanding of quantitative political science in the mid-1960s, along with the state of computing and computers. For example, when I joined the profession in 1963, my university had only just obtained its first computer, housed in its own building, and with a memory capacity of around 32KB! We social scientists had to wait for the memory to reach 64KB before we could attempt basic analysis of survey data and had to input the data ourselves using paper tape. Data runs could take up to a week. Any errors in the punching resulted in a program failure, the need to find the error on the tape, and to repunch the data accordingly. Lucky were those who could use punch cards or had access to the early statistical packages then becoming available. In the United States Michigan was very much the center of quantitative studies, especially of electoral behavior, while in the United Kingdom David Butler was coming to terms with the new survey methodology with his collaborative study of the 1964 general election (Butler and Stokes 1966).¹

At the time when Dye's book saw the light of day, most political scientists were statistically illiterate (and many remain so), and those who dared to undertake quantitative work were dependent on statistics texts² in order to understand terms such as correlation, statistical significance, and regression analysis. Some sense of the importance of Dye's book can be gained from the way in which similar output studies (as this form of work came to be known) spread rapidly, first in the United States and then in Britain.³

The reason for the rapid spread of similar studies lay not in the innovative methodological approach adopted by Dye (indeed similar smaller scale studies had already appeared in article form),⁴ but more in its *substantive* focus. Much of US political science in the 1950s and early 1960s had been concerned with seeking to understand voter behavior, the role of interest groups, or decision-making processes at national and local levels. In the latter context, the work of Dahl and others⁵ was important in terms of seeking to stress the importance of politics and political science as against the claims of sociologists in the form of the community power debate. Much of this work was concerned with the input side of politics, reflecting a concern with understanding the demands placed on the political system⁶ for action. What Dye's book did was to switch attention away from a concern with inputs to one with the outputs of political systems, with a focus on policy. As with the community power debate, the concern here was to see how important politics was in determining public policy. Most importantly, Dye was concerned with seeking to understand how far political variables impacted on policy in comparison with economic ones, in particular economic development. Such a focus not only opened up new areas for research, but also raised questions about the status of political science as a discipline in relation to other social sciences, particularly economics. If "economics or sociology could explain everything" then what room was there for a discipline of political science, let alone for the activities of politicians and politics! So Dye's book is important not only for its innovative substantial focus and for its methodological developments, but also for its part in the intellectual debates in the social sciences of the late 1950s and 1960s.

There is a last sense in which Dye's book is an important contribution to the literature in that it is a comparative study of all the American states. Again at the time comparative studies were fairly limited, generally cross-national and covering only a small number of countries. What Dye's book did was to reveal clearly how useful and important state and local government is as an area for comparative study, something scholars have built on subsequently.

THE BOOK ITSELF

Put simply, Dye's book seeks to assess the influence of political and economic variables on a number of policy areas in the American states. He examines five policy areas—education, welfare, highways, tax/revenue policy, and public regulatory policy. His concern is with the impact of economic development and political variables such as

party and electoral systems on each of these policy areas. He adopts an Eastonian systems approach⁷ to organize his thinking and is meticulous in explaining how his dependent and independent variables are constructed and measured. He is also careful in explaining his use of partial and simple correlations and the problems associated with making inferences from such correlations, and with the limits involved in operationalizing the measures he uses, helpfully reminding us that “transforming the language of explanation into the language of research is a difficult task” (Dye 1966: 28).

His main assumption is that economic development is “the principal input variable shaping the character of state political systems and the kinds of policy outcomes encountered in state politics” (p. 28). This assumption is clearly important, shaping as it did not only Dye’s approach, but that of much subsequent work in this area. Clearly the level of economic development of a locality, city, region, state, or nation is an important determinant of its capacity to act on the problems or issues that it faces. Well-developed areas economically can draw on greater resources to deal with policy problems than can those which are less well developed. The political issue is whether or not such entities have the political will, desire, and means to take action. Amongst the factors which will determine whether or not action is taken are such things as the party and electoral systems—but these alone are not necessarily the primary factors. Dye’s quantitative approach does not allow him to consider softer qualitative factors which might contribute to policy outcomes, a criticism which should be made of many similar studies in this area rather than seeking to undermine the importance of Dye’s contribution.

Dye uses four functionally interrelated phenomena to measure economic development—urbanization, industrialization, wealth, and education. States which are more urbanized, industrialized, are wealthier, and whose populations are well educated will have better policy service levels than those that are poorer in all these respects. Using US census data, he measures each of the factors as follows:

Industrialization—1 minus the % of the workforce engaged in agriculture, fisheries, and forestry;

Urbanization—% of population living in urban areas;

Wealth—median family income in dollars (1959);

Education—median school year completed by population, age 25 or over.

Dye shows that these measures are significantly related, but argues that these relationships are not close enough to lend “empirical support to the contention that they are measures of the same phenomenon” (p. 33) and goes on to suggest that these different measures may have different impacts upon states’ politics and public policies. The 50 US states are the principal unit of analysis in Dye’s study, with hypotheses having to be tested with reference to all of them. Dye’s makes one exception—being concerned to see how far the inclusion of the then largely Democrat controlled group of southern states affects explanations of public policy—so that all the correlation analysis is performed on both the 50 states and on the 39 non-southern states.⁸

Dye first seeks to establish the impact of economic development on state political systems. Though the variations in politics between the 50 states are almost limitless, Dye selects four for analysis—the division of two-party control between the states, the level of interparty competition, voter turnout, and the degree of legislative malapportionment. Here we immediately recognize the limitations of the approach generally and of Dye's study in particular. However well specified the theoretical model may be, its empirical operationalization depends on the measures adopted and the quality of the data available. This latter is a particular problem when one attempts comparative analysis: finding measures for which good standardized comparative data are available remains a problem, though now much improved over time. If one adds to this the limited computing capacity available, together with the limited statistical knowledge and skills among social scientists at the time of Dye's study, one can only admire the way in which he undertook his research. While one might readily agree with Dye's measures of economic development, the political measures he was obliged to adopt can be considered rather limited. Successive researchers in this field continually sought to improve and increase such measures, as they also sought to improve measures of particular public policies.

Dye is first concerned to examine the relationship between economic development and his political variables. He shows that at that time Democrat and Republican controlled states differed significantly in terms of income and educational levels, with the former having lower levels than the latter (p. 52), though this relationship depends up on the inclusion of the Democrat controlled southern states.⁹ But he suggests that the fact that Republican states tend to be wealthier and better educated than their Democrat counterparts may affect policy differences—that is, wealth and education levels may be a better indicator of policy differences than party success (p. 53). A similar argument applies to the level of interparty competition: “high level of income and education tend to foster party competition” (p. 58) and thus “policy differences between competitive and non-competitive states may be a product of their different levels of wealth and education *rather than* [this author's emphasis] a direct product of party competition.” Dye (p. 63) further suggests that the “relationships between participation¹⁰ and income and education are particularly important in the study of policy outcomes”—but that again differences in such outcomes may well reflect differences in levels of wealth and education rather than in voter turnout.¹¹

Dye then turns to what is the substantive part of the book—an investigation of five policy areas, namely education, welfare, highways, tax/revenue, and public regulatory policy.¹² We will look at Dye's results in relation to each of these in turn.

Educational Policy

Dye shows clearly that “economic development is an important determinant of a state's willingness and ability to provide educational services” (p. 80), with income/wealth being the most important factor. But he also suggests “Wealthy, urban, industrial states

do not consistently spend more for education than for other public funds” (p. 85): such states simply spend more on all public functions than do poorer ones. Economic development also appears as “the single most important determinant of ... educational quality” (p. 96).

How far do political variables affect education policy? Initially Dye suggests that “there are many significant associations between partisanship and public policy outcomes” (p. 106), but most disappear once economic development is controlled, with the rest virtually dropping out when the Democratic controlled southern states are removed. Dye concludes that in many important respects strong Democrat and strong Republican states resemble each other over education policy more than they resemble states under divided control (p. 109). He also shows that economic development explains more of the differences in education policy than does party competition, while malapportionment has no or little effect. So overall, for Dye, economic development, especially wealth, is a better predictor of education policy than are the political variables he uses.

Welfare Policy

Here Dye considers states’ health and welfare policies, but his analysis is complicated by the involvement of the federal government in shaping state programs. Furthermore, it is the states who determine recipients’ benefits, eligibility requirements, and program administration ... all of which lead to variation in state welfare programs (p. 123). Dye shows that economic development is significantly related to welfare benefits, with it being the most important variable explaining welfare payments. As Dye says (p. 125): “It is far better to be indigent in a wealthy state than a poor one.” But he goes on to show that it is federal government policy in both welfare and health areas that helps to offset variations in the states’ levels of wealth, while also discouraging high benefit levels (pp. 132–40). In terms of his political variables, he shows that none of them has any real impact on welfare policy outcomes at state level (pp. 138–48). Again it is economic development which Dye sees as explaining variations in states welfare and health policies.

Highway Policy

Highway policy is another area in which the federal government was heavily involved at the time of Dye’s study. Public highways were then the second most costly function of state governments, with per capita expenditures significantly related to urbanization and industrialization (p. 157). However, this relationship was an inverse one, in that rural states spent more per capita than did urban ones. Dye (p. 159) states that “the tendency for rural agricultural states to emphasize this particular function of state government is unmistakable.” Furthermore, he shows that it is Republican control of

state legislatures which helps explain this pattern (p. 172). However, other political variables (party competition/voter turnout) have no effect on highway policy, and there is “little empirical evidence that malapportionment affects highway policy” (pp. 173–9).

Tax and Revenue Policy

Dye is careful in unpacking the various elements of tax and revenue policy and in underlining the different elements according to the level of government concerned. Thus local governments at the time were more dependent on local taxes than state governments, who relied more on federal grants and sales taxes for revenue (pp. 178–96). Trends over time suggest to Dye that economic development played a significant part in affecting state tax/revenue policies. He shows that income, urbanization, and education levels are all significantly related to revenue and tax levels (p. 188). In terms of the tax burden (i.e. taxes paid in relation to personal income) he shows that it is the level of industrialization that is most closely related to the tax burden: higher levels of industrialization produce lower levels of state-local tax burdens (p. 190). In terms of the political variables which Dye uses, he shows yet again that none of them have an effect on tax and revenue policies. Only the relative reliance placed upon state and federal sources of revenue as distinct from local ones is affected by partisanship: Dye shows that “there is still a significant relationship between Democratic politics and increased state and federal percentages of state-local revenue and increased reliance upon property taxation” (p. 206).

Public Regulatory Policy

In many ways this topic is the least satisfactory in terms of Dye’s analysis, partly because, as he notes, the topic refers to a “wide variety of outcomes reflecting public control” (p. 210). As a topic it covers everything from the number and type of governmental action, through such things as gambling and divorce laws to policing, crime and prison policy. The problem is that Dye appears to have decided that regulatory policy covers those topics on which he can obtain good comparative data across all 50 states, rather than approaching the subject in a more systematic fashion. However, taken overall, Dye shows a strong relationship between economic development and the number of local governments (p. 217), but not between economic development and public employment (p. 219). It is strongly related to crime rates (p. 222), but not to divorce laws and gambling regulation, size of police forces, and capital punishment, though there is a relationship between education levels and prison populations (pp. 222–8). Partisanship appears to have little impact on regulatory policy, though Democratic control is related to higher crime rates; and larger prison populations, but Dye (p. 234) suggests that in practice it is unlikely that system variables such as party control actually bring about these results. Party competition, voter turnout, and malapportionment also have little

impact on regulatory outcomes. Effectively, once again, Dye sees the level of economic development as the main determinant of states' public regulatory policy.

Dye then spends two chapters exploring the impact of party and electoral systems on policy. In the case of the former, while noting that party systems vary considerably across the 50 states, Dye argues that there is little to suggest that Democrats and Republican success in state politics is "predictive of differences in policy outcomes" (p. 239). He makes the point that each state party is more a product of its state-wide constituency than of any national organization, and that state parties "tailor policies to local conditions" (p. 240). He undertakes an analysis of the independent effect of partisanship, and shows that "a great deal of the association between partisanship and public policy is really a product of the influence of economic development variables" (p. 246) and that generally party control of a state government is not a good predictor of state policy outcomes. He goes on to make the very broad claim—hence the importance of the book generally—that "the linkage between socioeconomic inputs and policy outcomes is an unbroken one, and that characteristics of political systems do not independently influence policy outcomes" (p. 259). But he rightly qualifies this claim in the subsequent paragraph when he writes:

All we have shown is that party competition in the American states does not seem to have a measurable impact on policy outcomes, once socioeconomic variables are controlled. This is not to say that party competition does not vitally affect state political systems or processes. Quantification, regardless of its degree of sophistication, necessitates a simplification of very complex processes. Perhaps the influence of party competition is so subtle and diverse that it defies quantification. (p. 259)

Perhaps not surprisingly, Dye comes to a similar conclusion in assessing the impact of electoral systems on policy outcomes, but applies the same caveats as those already noted. Neither voter turnout nor malapportionment appears to affect the policy choices of state governments. These findings do not stop Dye from stressing both the importance of voter participation for the health of democratic politics and the case for reapportionment of electoral districts where this would produce greater equality of representation.

Dye's book encouraged an outburst of similar studies in the United States over the next 10 years or so, each with an increasingly more sophisticated use of statistical analysis, covering more and more variables of both a socioeconomic and political nature. Three authors bear mention here: Hofferbert, Lineberry, and Sharkansky. Writing separately and together, these three produced studies (see References) which continued to set the framework for the study of public policy in the United States during the 1970s and which explored further the relationship between economics, politics, and public policy, perhaps overwhelmingly suggesting that economics determined policy, not politics. However, notwithstanding both the increased computational power and more sophisticated statistical packages available (and however many doctoral theses were given over to the subject), the overall increase in statistical explanation hardly merited

the effort involved—as one unknown commentator put it, “the guy with the largest deck of (punched) cards won”—and by the mid-1980s the systems analysis approach of David Easton which had provided the basic framework for this work seemed to be forgotten.

Nevertheless, the 1970s saw a number of similar studies in the United Kingdom, drawing on the approach and techniques adopted by Dye. Interestingly, it was a mix of social scientists who went down this road, not just political scientists. Key work was undertaken by Noel Boaden (1971), social administrator Bleddyn Davies (1968), economists Nicholson and Neville Topham (1971, 1972), Americans James Alt (1971), James Danziger (1978), and Paul Peterson (1979a, b), with a later fuller study by Jim Sharpe and Ken Newton (1984).¹³ What made the topic of interest to British social scientists was the importance of a more distinct left–right party difference than perhaps existed in the United States. The Labour party at the time was clearly much more in favor of redistributive policies with a broader conception of welfare than either its Conservative opponents, or indeed the Democratic and Republic parties in the United States. Furthermore, local politics were both more politicized and partisan than was the case in the United States and included many cities and other local authorities which were effectively one-party states for either the Labour or Conservative party. If politics was to make a difference to policy and policy outputs, then surely they would appear in the British case. Largely replicating Dye’s approach, Boaden clearly suggested the importance of the Labour/Conservative split in his study of county borough policies in a number of areas, a finding supported by Davies’s study and the work of the US scholars cited. By contrast, Nicholson and Topham’s work did not find that party differences had any impact on the areas they examined—but it has to be said that their work was more concerned with capital expenditures and housing investment than with current expenditures and welfare services such as education and social services.

The most important study in the United Kingdom was undoubtedly that undertaken by Sharpe and Newton. Though they reference Dye’s book (Sharpe and Newton 1984: 27, 96), they really do not draw very much on his early work and indeed prefer to draw on some of his later work (Dye 1969, 1976), where they claim he was really one of the economic determinists in this field. Sharpe and Newton’s position is essentially that all public policies are the result of political processes and not the result of some transmission process whereby socioeconomic characteristics of city (and by extension state) populations are almost automatically translated into policies. Their study was probably the most sophisticated at the time in the United Kingdom. It dealt with both county boroughs and counties in England and Wales, and sought to test a range of possible explanations for policy outputs, particularly focusing as they do on variations in the economic hypothesis and on the political dimension. Additionally, Sharpe and Newton’s analysis reviews policy over time, as distinct to the simpler cross-cutting approach adopted by earlier work. Through a systematic analysis of different approaches, Sharpe and Newton suggest that central place theory and the type of place a city is (e.g. seaside town, major manufacturing center) affect the kinds of services such cities provide.

But it is their detailed examination of the effect of party on policy and expenditure which is most important. Using a number of independent political variables, they show both “turnout and uncontested seats emerge as predictors of service expenditure” (Sharpe and Newton 1984: 181), but that the percentage of Labour-held seats and the number of years of Labour control prove to be the best predictors, especially for redistributive and welfare oriented services. They conclude: “Suffice it to say at this point that party and party systems have been revealed as being critical determinants of local expenditure” (Sharpe and Newton 1984: 203). In their final chapter, they begin by making the important point that quite how the economic transmission model transformed inputs into outputs or policies was never adequately discussed and suggest, rightly in my view, that the whole process of policy-making involves political factors. Politicians have to understand the nature of the socioeconomic environment in which they operate and then prioritize those policy arenas they deem important for policy and expenditure.

Since that time a number of scholars have returned to examine some of the issues raised by Dye’s initial study and subsequent literature, if not quite on the macro scale undertaken by Dye. Given that computers, statistical packages, and graduate training have improved remarkably since Dye’s study, as has the quality of governmental expenditure and other data, it is not surprising to find that many such studies are more sophisticated than Dye’s. In recent years there has been an increase in their number, though space permits only a limited discussion of themes.¹⁴ One special continuing focus post Newton and Sharpe has been on the impact of parties and party competition on the performance of public services (Boyne et al. 2011; Barilleau et al. 2002; Ferreira and Gyourko 2009; Ansolabehere and Snyder 2006). The general conclusion of these studies is that greater party competition does see parties trying to improve service performance (Barilleau et al. 2002; Boyne et al. 2011), that there is an incumbency effect (Ferreira and Gyourko 2009), and that winning parties distribute funds to those areas which gave them most support (Ansolabehere and Snyder 2006). As a last example, a study of Spain’s largest municipalities (Garcia-Sanchez et al. 2012) shows that municipalities controlled by left parties are in a worse financial condition than those controlled by right-wing ones. But there is also counter-evidence: in a study of West German Lander Galli and Rossi (2002) show that the party variable does not play a systematic role in spending decisions, though they do note the effect of impending elections on the spending inclinations of different parties. Similarly, despite suggesting the incumbency effect, Ferreira and Gyourko (2009) also show that the party identification of mayors has no effect on the allocation of local public spending. So clearly the importance of parties in determining public policies and expenditures at subnational levels continues to be an issue for debate.

Another related area which has opened up since Dye’s time and in which his approach has been adopted is that of local agenda-setting. A couple of recent examples include Heidbreder’s 2012 study of agenda-setting in the United States, which shows that Democrat and Republican governors vary in terms of the attention they give to different policy demand indicators, including the gubernatorial party identification,

who controls the state legislature, and policy needs as reflected in state environmental data. In another study, Lax and Phillips (2012) suggest that while there is a degree of congruence between public opinion and public policy, in practice policy is more likely to be influenced by partisanship and interest groups, as well as the varying perceived importance of different issues.

CONCLUDING REMARKS

Almost 20 years cover the two main studies discussed here, and 40 since the more recent ones. Dye's book was produced in the mid-1960s, that of Sharpe and Newton in the mid-1980s. Over that period of time there has been an explosion of output studies in the United States, a slightly smaller outburst in the United Kingdom, and a somewhat smaller shower in Western Europe, though there has been a general upsurge in studies seeking to explain policies and service expenditures in recent years. Furthermore, technical developments in information technology (especially computing power) and in statistical packages over that time made it possible for political scientists to process even larger amounts of output data and to improve their measurement of variables, dependent and independent. But the statistical methods remained largely the same—correlation and regression analysis—and even allowing the refinements in both the number of variables to be included and in their measurement, the level of explanation achieved remained less than perfect. Sharpe and Newton's study may well be more refined than that of Dye, but the level of explanation achieved by this type of quantitative approach to policy analysis remained (and remains) less than satisfactory.

Yet Dye's book remains important as a landmark study in policy studies. It paved the way for a succession of similar studies, including those in Britain and Western Europe. It furthered the development of quantitative methods and the use of quantitative data, and sowed the seeds for further work in policy analysis, such as that which looked at the outcomes of public policies (their intended and unintended consequences) and to the study of policy implementation associated with the work of Wildavsky and others.¹⁵ Looking back with hindsight, one can see the limitations of the study—the quality of the data used; the limited statistical methods as compared with some of the successor studies—but one has still to admire the care with which Dye sets up his model and his hypotheses, and the equal care with which they are tested. Similarly, his presentation of results is clear—something not always found in some political science writing drawing on quantitative methods and data—and he is conservative in the way he presents his results. The book may seem antiquated to some readers today—but for this author it remains a model to which graduate students could well turn when seeking inspiration as to how to present their own work.

NOTES

1. The most widely used statistical package in the social sciences, SPSS, was not invented until 1968 and a user manual produced in 1970. I first discovered it in 1970, by which time it was widely available in North America, with Britain and Europe following a little later in the 1970s.
2. Probably the most important, and certainly that referred to by Dye, was Hubert M. Blalock, Jr, *Social Statistics*, New York: McGraw-Hill, 1960.
3. There was a whole host of US studies in the late 1960s and early 1970s, with Britain following closely behind in the mid-1970s. But such studies never really caught on in the rest of Europe, one exception being Poul-Eric Mouritzen's 1991 study.
4. See e.g. Dawson and Robinson (1963) and Cutright (1965).
5. Dahl (1961); Hunter (1953); Polsby (1963).
6. The phrase is used in the Eastonian sense (Easton 1965). As an approach designed to help organize understanding of politics, systems approaches were very influential during the late 1960s and early 1970s.
7. See Easton (1965) for a detailed exposition of a systems approach to politics. While this is an influential heuristic device for organizing one's approach to studying public policy, it is useful to remember Dearlove's comment that "the real world is not made up of little black boxes" (1973: 208).
8. As Dye notes (p. 44), nearly all the significant relationships between economic development and public policy which are found in the 50 states also occur when the southern states are removed from analysis.
9. Once the southern states are removed, Dye shows there is only a relationship between party success and industrialization—an increase in industrialization leads to an increase in Democrat success.
10. As measured by voter turnout.
11. As far as malapportionment is concerned, while Dye demonstrates that malapportionment was severe in most legislative chambers, it appears to be unrelated to economic development and more a feature of constitutional compromises at the time of establishing the various legislatures (pp. 63–9).
12. Under this heading, Dye examines such things as number of local state and local governments as well as school districts; legislative output; public employment; crime rates; number of police; gambling regulation; police protection, and prison policy.
13. Sharpe and Newton estimated that there some 40 such studies, the majority adopting what they refer to as a demographic approach to outputs analysis. The pair were also responsible for stimulating some related European work, mainly through the European Consortium for Political Research. See e.g. Sharpe (1979); Newton (1980, 1981). The best of the European work included that by Hansen (1981) and Mouritzen (1989).
14. E.g. space prohibits a discussion of studies seeking to explain variations in expenditures on specific services such as education, health, crime prevention.
15. See e.g. Pressman and Wildavsky (1973).

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CHAPTER 9

HERBERT KAUFMAN, *THE FOREST RANGER: A STUDY IN ADMINISTRATIVE BEHAVIOR*

LAURENCE E. LYNN, JR.

THE BIRTH OF A CLASSIC

WHY is it, Herbert Kaufman, a young political scientist at Columbia University, wanted to know, that an organization as geographically dispersed and decentralized as the United States Forest Service could exhibit such a uniform execution of its policies and programs? Why is it the case that the ranger districts which care for the National Forests of a vast continent do not degenerate into “many local, unrelated policies”? How do the agency’s leaders control so well what their forest rangers think and do in the field?

Kaufman’s pursuit of answers to these questions culminated in *The Forest Ranger: A Study in Administrative Behavior*, a scrupulous work of public administration scholarship now in its sixth decade in print. When first published in 1960, the book was viewed as traditional in its questions but path-breaking in its research methods. The study and its arresting findings were praised by professionals in forestry, political science, sociology, and public administration. Admiring journal reviews of the book were appearing as late as 2012.

The Forest Ranger is about what is now called “direct government,” that is, public organizations whose own employees, not “third parties,” produce goods and services authorized by public policies. From a theoretical perspective, the book is about Weberian bureaucracy, a type of organization which is the primary instrument of direct government.

In his preface, Kaufman claimed that his book was about neither politics nor policy. Rather, it was about the planning and work of the “lower echelons” of the Forest Service in translating policy, the product of politics, into operating results, the product of administration. As he put it later, the book exemplifies “neutral competence,” the impartial and competent execution of public policy mandates by agency officials, a core value of public administration professionalism. “The evidence indicates the Forest Service has enjoyed a substantial degree of success in producing field behavior consistent with headquarters directives and suggestions. . . . What this study is concerned with is the way the field men are induced to carry out tangibly the terms of headquarters agreements” (Kaufman 1960: pp. x, xi).

The book’s subtitle identifies what was then distinctive about Kaufman’s intellectual project: it was “A Study in Administrative Behavior” that yielded new and detailed insights, obtained using a carefully constructed research design, into the nature of administrative practice in a hierarchical public agency. “In depicting the tension at the heart of his narrative,” said Larry Luton in a review of the book’s 2006 republication (2007: 167),

Kaufman had to detail the complex dynamic whereby the rangers felt that they had a lot of discretion while at the same time took actions that Kaufman described as “voluntary conformity” (1960, 198). Integrative techniques included “preformed decisions” in the form of authorizations, directions, prohibitions, and the development of appropriate attitudes. In addition to the statutes and presidential proclamations that set very general directions, he described how the Forest Service Manual, technical handbooks, guides, plans, budgets, reports, records, official diaries, memoranda, letters, circulars, directives from inspectors, reviews and approvals from higher administration, and complaints from the public facilitated awareness of the policies that top management wanted to pursue.

Because his research design carefully distinguished between “Tendencies toward Fragmentation” and “Techniques of Integration,” readers were able to understand the challenges of managing public bureaucracies with decentralized and dispersed operations producing services ranging from revenue collection to the administration of criminal justice. Kaufman disavows any intent to be prescriptive. *The Forest Ranger*, however, is a subtly normative work in that it portrays how bureaucratic organizations marshal public service values on behalf of authoritative expressions of the public will. For example, he presents a detailed account of “hierarchical specialization” that describes and justifies unity of command in a chain of delegation and control, long an important issue in the field of public administration.

Kaufman’s Afterword to the 2006 republication of *The Forest Ranger* explains why he wrote the book and celebrates, in hindsight, how prophetic he thought it was. A concomitant of effective policy control, he had said in 1960 (pp. 234–8), is organizational inflexibility in the face of change. Such inflexibility did indeed lead to problems as the agency confronted rapid, complex change beginning in the 1960s, a possibility he had

acknowledged without actually predicting it (Halvorsen 2003). Whether or not he intended the book to be prophetic, he surely intended it to influence the attention paid to administrative organizations and behavior by the profession of public administration. That his book is still in print and regarded by many to be a classic of public administration research is a testament to his success in that regard.

As I will suggest in this chapter, *The Forest Ranger's* standing among public administration's iconic works may be due less to the book's now dated insights and research methods than to Kaufman's entire *œuvre*, to his meticulous scholarship on issues of continuing interest to the profession and to his commitment to the value of neutral competence despite the uniquely American interpenetration of politics and administration. Said Kaufman in 1969 in contemplation of how the environments of administrative agencies were being altered by changing public values (Kaufman 1969: 12):

Precisely what shape the subsequent resurgence of neutral competence will take in the years beyond, it is impossible to prophesy now. But if the hypothesized cycle of values is at all valid, then strange as it may seem to this generation of reformers, innovators of tomorrow will defend many of the very institutions (as transformed in the course of current controversies) under attack today. And many a forgotten tome and obscure article on public administration, long gathering dust on unpatronized shelves and in unopened files, will be resurrected and praised for its prescience, only to subside again into temporary limbo when another turn of the wheel ends its brief moment of revived relevance.

No matter the ongoing and significant changes in public administration theory, research, and practice, *The Forest Ranger* and its author have remained well patronized.

WHO WAS HERBERT KAUFMAN?

Born in 1922, Herbert Kaufman studied political science at the City College of New York and Columbia University's Department of Public Law and Government.¹ His education interrupted by a three-year tour of duty with the United States Army during World War II, he began his study of the US Forest Service when he returned to Columbia University, where he was awarded an internship at the National Institute of Public Administration (NIPA), then led by Luther Gulick, a seminal contributor to administrative thought and practice.

The management and operations of the United States Forest Service was the subject of Kaufman's first major research project (and of his doctoral dissertation), which was supported by NIPA, then engaged in a study of American forestry. He was tutored in his early work both by mentors such as Gulick, Arthur Macmahon, and Wallace Sayre, the latter two on the Columbia faculty, and by the increasingly influential work of Herbert Simon, whose first book, *Administrative Behavior: A Study of Decision-Making Processes in Administrative Organization*, was published in 1947. Kaufman "set out

to study the ways in which an organization as large and geographically expansive as the U.S. Forest Service was able to control or influence the behavior of its lowest-level line managers, the district rangers” (Burton 2012: 625). His field research completed, Kaufman, probably not coincidentally, decided to complete a book manuscript after joining the faculty of Yale University, home of the school of forestry that trained most of the forest rangers whose behavior he had studied. The manuscript’s 1960 publication by the Johns Hopkins University Press was sponsored by Resources for the Future, a research organization concerned with natural and renewable resources. Kaufman was a professor of political science at Yale from 1953 to 1969.

Having grown, by his own admission, “stale” as a teacher, Kaufman moved to Washington, DC, to become a senior fellow in the Governmental Studies Program of the Brookings Institution, which was to publish his later books. The subjects of those books and of his many articles in scholarly journals suggest the depth of his interest in bureaucracy and bureaucrats, administrative systems and behavior, and the evolution of public administration as a profession. The topics of his books included the administrative behavior of federal bureau chiefs (my personal favorite), red tape, organizational adaptation and change, and monitoring subordinate behavior.

Although he published articles in political science journals, Kaufman’s favorite outlet became the *Public Administration Review* (PAR), the journal of the American Society for Public Administration. The appearance of his articles there spanned nearly four decades, from his first, “Administrative Decentralization and Political Power,” in 1969 to his last, “Administrative Management: Does its Strong Executive Thesis Still Merit our Attention?” in 2007. In 1996, PAR published an autobiographical essay, “Music of the Spheres: A Lifetime of Study of Public Administration”, in which Kaufman said: “as I look back on [my career], I am astounded to see that it had a logical consistency I was not aware of as I lived through it” (1996: 237).

Before publication of *The Forest Ranger*, Kaufman had circulated drafts of chapters within the Forest Service out of concern, which turned out to be unfounded, that the imputation of unity of thought among the agency’s rangers would offend them. Two decades later, another study of the Forest Service revealed that the agency management studied by Kaufman had been a deliberate application of Prussian methods of public management. A measure of Herbert Kaufman the man may be taken in the following excerpt from his 1996 autobiographical essay:

The discovery [that the management of the Forest Service was inspired by Prussian methods of administration] troubled me. It put me in the mind of *Brave New World* and *1984*, and while I never believed or claimed that the members of the service were victims of the kind of total brainwashing described in those novels, the mere *tendency* toward mind control disturbed me [as did] the realization that the success of the Forest Service sprang in large measure from that very program of instilling agency-inspired thought patterns in those who joined it. (1996: 133)

Kaufman was reinforced in his belief about the risks of bureaucratic inflexibility by the unwelcome implication that thought control was a source of that inflexibility.

In his last published essay, a 2008 article in the *American Review of Public Administration*, Kaufman returned to the subject that energized his first research project, bureaucratic control, but with the wisdom acquired since his student days. “Public bureaucracies,” he wrote, “warrant the attention of political scientists because bureaucrats help determine the contents and effectiveness of public policies” (Kaufman 2008: 256). But, he said, the study of bureaucracy is “beset by ambiguities about when and to what extent their behavior is controlled or controlling.” The student of bureaucracy must contend, he said, with the “incessant changes in their roles [that] may mean that we cannot hope for universal, long-enduring generalizations about their place in the polity.” Thus “we must continuously observe, analyze, and reassess the influence they exert and the influences on them.” The scientific study of bureaucracy flourishes today.

THE FOREST RANGER’S EARLY RECEPTION

The Forest Ranger does not appear to have been as widely reviewed following its publication as one might expect. The reason might well have been some confusion over what the book was about. Was it about the US Forest Service, or was it a case study intended to produce findings generalizable to other bureaucracies? It was, of course, both, but its most interested readership at first may well have been among foresters and natural resource specialists, as Resources for the Future sponsorship suggests. However, its influence within his own field of public administration was to grow steadily as favorable reviews appeared and its reputation spread.

A 1960 review in *PAR*, for example, was written by Lyle Craine of the University of Michigan’s School of Natural Resources. Craine recognized that, on the subject of decentralization of service delivery, “Professor Kaufman has given us something new. . . . [I]t is a study that will have interest and significance for the general administrator and student of administrative practice and theory as well as the professional forester” (Craine 1960: 227, 230). Craine continued: “Professor Kaufman, with disciplined precision, concentrates his analysis on how the Forest Service assures that ‘. . . the daily decisions and actions of the lower echelons make concrete realities of policy statements and declared objectives of the leadership. . . .’” (Craine 1960: 227).

In the same year, *Journal of Sociology* reviewer Robert W. Avery saw Kaufman as “explaining how the United States Forest Service has maintained unity and exacted conformity among its dispersed rangers. . . . The point and counterpoint of fragmentation and integration give the book its pervasive theme” (Avery 1960: 103). But, because Kaufman himself saw his findings as impressionistic and tentative, Avery characterized the book as “an argument rather than a report of empirical findings.” As Avery saw it, Kaufman leaves the reader with a question rather than an answer, albeit one with considerable significance: “Why it is that men find their independence when

they take as their own the goals and values of those upon whom they are manifestly dependent.”

A similar reaction was that of Roscoe Martin, author of a study of the Tennessee Valley Authority, another direct government organization. In the *Administrative Science Quarterly*, Martin said that “another book needs to be written, a book suggested by Marion Clawson’s dilemma in his Foreword to the present volume: ‘how to devise and operate an agency which will operate consistently, in the sense of reducing to the minimum variations from established, organization-wide norms while at the same time preserving individuality and stimulating creative thinking and action on the part of its men’” (Martin 1961: 116). Kaufman, these reviewers were saying, had raised questions of compelling interest to the field of public administration, opening an intellectual door, not closing one.

Also drawing praise was the lucidity of Kaufman’s exposition, a quality undoubtedly contributing to the book’s early and enduring popularity. Despite its almost obsessively meticulous scholarship, the book is a pleasure to read. Martin noted that Kaufman “writes with zest and conviction; clearly he believes what he is saying” (Martin 1961: 115). No cognitive aerobics, he said, are needed from the reader. Kaufman’s zest had also been noted by Craine: Professor Kaufman, he said, seeks here “to portray an organization accurately, to capture the drama, the excitement, the spirit of administration” (Craine 1960: 230). Kaufman, it was apparent, cared about those he had studied, and he inspired others to care, too.

Then, however, in public policy terms, and rather unexpectedly, the earth began to move under the district rangers’ feet.

WHAT IS A NATIONAL FOREST FOR?

In the decade following publication of *The Forest Ranger*, the political and policy environment that had enabled the Forest Service to unify and control its decentralized operations began to change, dramatically and irrevocably. The demands of numerous organized interests strengthened the centrifugal forces confronting the agency. The timber-oriented district ranger operations were exposed to pressures to protect the environment and, at the same time, enhance public access to public forest lands for recreational use. Under the mandates of legislation such as the Multiple Use—Sustained Yield Act, the Wilderness Act; the National Forest Management Act, the National Environmental Policy Act, the Cooperative Forestry Assistance Act, and the Forest and Rangelands Renewable Resources Planning Act, not to mention the advent of computerized information technologies, the Forest Service became a very different place than that studied by Kaufman. A National Forest, it turned out, was more than just trees.

By 1980, in response to shifting public values, the Forest Service had redirected its focus to managing forest land as an integrated ecosystem instead of as a particular

natural resource: timber. In 1989 the Chief's New Perspectives initiative stressed ecosystem management and sustainability in order to align timber management with other forest values including biodiversity, water quality, and recreation. Connie E. Bullis (1984: 204) concluded, "The U.S. Forest Service has seen a transition in its role from that of expert to that of servant vis-a-vis its environment" (quoted by Tipple and Wellman 1991: 424).

In the light of these developments, Kaufman wrote about administrative decentralization with barely a mention of *The Forest Ranger* and its findings. His work had begun to reflect the extent to which his earlier findings concerning Forest Service management had been overtaken by events. As well, a 1969 essay reflected changes concurrently taking place in the intellectual environment of public administration, organization studies, and the study of professions. "As the regional officers get more and more involved in regional complexes," he wrote, "they will become more and more ambassadors from the regions to the chief executives instead of the executives' men in the regions" (Kaufman 1969: 12).

The Forest Ranger had by no means been relegated to dusty shelves, however. Rather, it became for many a baseline for appraising the extent to which the environment of the Forest Service and other natural resource agencies had been transformed. Noted a 1991 review on the occasion of the book's thirtieth anniversary in print, "[t]he relatively closed system of the 1950s has been opened through environmental legislation and increased public involvement. Earlier emphasis on efficiency and economy is now offset by a stress on representativeness and responsiveness" (Tipple and Wellman 1991: 421). Concluded Terence Tipple and Douglas Wellman (1991: 423):

For the service to realize returns on its investments in responsiveness and representativeness, it feels it cannot manage simply by controlling implementation of preformed decisions as it did in the late 1950s. Rather, it must further loosen the reins and allow the managers of the 1990s to exercise their individual judgment in today's expanded areas of discretion. This management philosophy stands in stark contrast to that portrayed by Kaufman. Because of these changes in mission and process, as well as changes in organizational theory, the 1990s version of *The Forest Ranger* would probably differ significantly from the original work. ... [W]hile much of what Kaufman found is still present, it now exists in a more complex and diverse setting, and it would have to be reported as such.

The reviewers dramatized the extent of change in the Forest Service with an anecdote. "A front-page New York Times story recently described a dissident group of Forest Service employees called 'Forest Service Employees for Environmental Ethics' who are publicly denouncing agency directions." Such rebellion in the ranks was unthinkable in 1960. Continued Tipple and Wellman, "*The Forest Ranger* described public administration largely as 'the planning, execution and evaluation of preformed decisions'" (1991: 425). The methods of public management identified and appreciated in *The Forest Ranger* were obsolete.

Kaufman himself said it well in his 1996 autobiographical essay (1996: 134):

Today, 35 years after the appearance of *The Forest Ranger*, the Forest Service is beleaguered within and without by environmentalists, recreationists, defenders of endangered species and ecologists, and others who believe its traditional philosophy puts too much emphasis on timber production and is therefore out of keeping with changing social and political values and with advances in the understanding of ecological processes. How it emerges from this turmoil will tell us much about the long-range consequences of methods that served it well in the past.

Fifteen years after the thirtieth anniversary of its publication, in 2006, *The Forest Ranger* was republished by its original patron, Resources for the Future, with two new forewords which reasserted the book's significance. Said Luton: "Kaufman recognized that the very factors that had contributed to the Forest Service's success in the 1950s might well have reduced the agency's ability to adapt to future challenges. We now know that the homogeneity of the Forest Service did make it difficult to adapt to some of the demands it faced during the second half of the 20th century—demands from the environmental and civil rights movements, as well as from recreationists and developers" (Luton 2007: 166).

In another review of the 2006 edition of *the Forest Ranger*, Thomas M. Koontz (2007: 152) reported the

results from a survey of line officers in 48 state forest agencies, as well as state forest statute analysis [which indicated] systematic state–federal differences in legal constraints, citizen interactions, and the forest administrators themselves. Though these differences foster state administrator decision making based on professional expertise, they do not encourage the incorporation of stakeholder views into agency policy making, nor do they yield a bureaucracy that represents a diverse constituency. State forest administrators perceive substantial external challenges to their professional discretion[.]

Koontz notes, however, that "[I]t remains to be seen whether state forest agencies will change to more closely resemble their federal counterpart." Apparently, *The Forest Ranger's* lingering relevance had itself become decentralized.

Not all centralized bureaucracies have been transformed into polycentric, multi-mission agencies to the same extent as the US Forest Service. Many law enforcement, intelligence, air traffic control, and revenue collection agencies—agencies performing inherently governmental functions—continue to be primarily top–down agencies relying on the kinds of integrative management strategies identified in *The Forest Ranger*. Such agencies have continued to attract the interests of researchers and have their own research literatures. More generally, methods of analyzing such organizations, that is, the social science of public bureaucracies, organizations in general, and public administration have changed as much as the work of the forester.

THE FOREST RANGER AS SOCIAL SCIENCE

The Forest Ranger's enduring popularity largely rests, as already noted, on Kaufman's careful and deliberate choice of a research design and on the systemic organization of his fieldwork. He carefully chose five ranger districts to study in depth, arguing that this number and his particular selection strategy (no outliers) would enable him to emulate the kind of representative coverage sought by cultural anthropologists, the discipline whose ethnographic methods he thought most appropriate to his research objectives. He also said that, as the Forest Service was embedded in and inseparable from the US Department of Agriculture, the whole hierarchy of the Department should be studied, not just the field echelons.

Kaufman offered painstaking justifications for each step of his research design. He analyzed his field data by what he called "ticking off variables." He went on to examine the interplay among these many variables, what he called the "behavioral dynamics." He integrated the information from his field interviews and observations into what he called a "composite picture," that is, a comprehensive explanation of how the Forest Service as an organization accomplishes its mission.

Kaufman provided a detailed account of what he termed the "centrifugal forces" impinging on the field agents of this dispersed, decentralized agency and the methods employed by agency management to overcome their disunifying effects on operations. Each of the centrifugal forces and each of the unifying strategies merited attention from scholars and practitioners specializing in those particular aspects of administrative behavior. For example, Kaufman gives a detailed account of how agency instructions deconstruct and promulgate statutory authorizations to ensure an unvarying interpretation of policies by the distinct rangers. Another section elaborates on how reporting was designed to detect and discourage deviations from policy. Thus *The Forest Ranger* was, and arguably still is, a much respected account of what is often called the "bureaucratic paradigm" of public administration.

Admiration for the book's research design and execution within public administration was, as already suggested, immediate. "The author's skilled and intelligent balancing of insights from anthropology, sociology, and psychology with traditional administrative and organizational observations is one of his important contributions," said Craine (1960: 227). "The interdependencies of personnel, budgeting, communications, public relations and all the tools of administration are given sharper meaning and significance as they are brought together in an organic unity with what have hitherto been discrete bodies of literature on human relations, informal organization, bureaucracy, as well as the sociology, anthropology, and social psychology of administration." In other commentaries, Kaufman's research design was variously characterized. Herbert Simon described it as "systematized common sense," a characterization associated with methods ranging from those of Sherlock Holmes to the scientific method itself. "Mr. Kaufmann's methodology," said Roscoe Martin, "if standard,

is sound” (Martin 1961: 115). In a sense, said Craine (1960: 227), this is an “ecological study ... [thus] giving reality to the study of administration. ... [I]t is a systematic examination of the whole species, ‘forest ranger.’”

Kaufman’s research design was hardly state-of-the-art social science, however. As he began his study of the Forest Service, he was guided by traditionalist mentors such as Gulick, Macmahon, and Sayre. At the same time, the work of Max Weber was becoming widely accessible to Anglophone scholars following the publication of *From Max Weber: Essays in Sociology* (1946). Weber’s ideas gave impetus to the sociological study of bureaucracy and of “the bureaucratic personality.”

Concurrently, stimulated by the work of Chester Barnard, David Easton, and Simon, among others, behavioralism, the scientific study of individual, group, organizational, and political behavior using rigorous approaches to hypothesis formulation and testing was gaining influence in political science, public administration, and the behavioral disciplines. In addition, the more formalized study of political delegation and control, incentives, the role of information, and “agency” (the dynamics of relationships between “principals” and “agents”) was beginning to influence social science scholarship, to culminate later in the emergence of a positive theory of political economy, a “new economics of organization,” rational choice institutionalism, and the systematic study of public management.

Within public administration, theory-based, as opposed to ethnographic, approaches to the analysis of bureaucratic administration had begun to influence thinking and research. Such authors as Mary Parker Follett, Schuyler Wallace, and John Millett were offering more nuanced and intellectually creative ideas about the organization and administration of public agencies than those informing *The Forest Ranger*. Indeed, in the 1940 debate over how to control bureaucracy between Carl Friedrich and Herman Finer, Friedrich used the language of principals and agents to depict the need for discretion and judgment in public administration. In reaching conclusions similar to those implied by Kaufman’s findings, Donald Kingsley argued that “the essence of responsibility is psychological rather than mechanical” (1944: 282). True responsibility, he argued (precisely anticipating a principal-agent model of governance), “is to be sought in an identity of aim and point of view, in a common background of social prejudice, which leads the agent to act as though he were the principal” (p. 282).

In the changing intellectual context of the times, then, Kaufman’s research might be characterized by the field’s more analytic scholars as conservative and underconceptualized. Many explanations of what “the lower echelons” do and why they do it might well have been explored in a study of the Forest Service. Weber’s influence was reflected, for example, in the work of Kaufman’s contemporary, the French sociologist Mihel Crozier. *The Bureaucratic Phenomenon*, published in 1964, explored administrative behavior with more scientific rigor than that employed by Kaufman.

Substantively, scholarly developments that gained momentum after World War II later made it conventional wisdom that “The implementation process is not the aftermath of policy-making, but a continuation of both politics and policy-making by administrative means.” A large literature on “street-level bureaucracy” depicts the

complex role of “the lower echelons” in the formulation of public policy as clients and customers co-produce it. Thus Kaufman’s study of how the “men [sic] in the field” do their work and about how they are induced (Kaufman’s word) to do what headquarters wants them to do could not, in retrospect, be considered a work on the cutting edge of research on the democratic control of bureaucracy.²

IS *THE FOREST RANGER* STILL A CLASSIC?

A “classic” might be defined as a seminal work that serious students of a field or subject of inquiry and practice can read with profit and, what’s more, *should* read. Such works might have been ahead of their time, adumbrating ideas or concepts that subsequently were more fully developed by others. A classic work might represent an important stage in a field’s intellectual development or a breakthrough into new ways of thinking or new methods of analysis. A classic might be exemplary in its science or its craft.

The Forest Ranger was unquestionably an accelerant for scholarly interest in what Kaufman believed “cried out” for systematic study: how organizations translate the concrete expressions of public policy into action at operating levels. That intellectual path had been blazed by Barnard’s and Simon’s earlier, and classic, books. But Kaufman’s question, reflecting a distinctly hierarchical perspective, is far less salient today than it was then. Kaufman was not ahead of his time, nor did he advance new and lasting ideas.

Kaufman’s case study, however, was an exemplary realization of Simon’s ideas designed and conducted in an exemplary manner. It came, moreover, at a time when the profession of public administration was in the throes of an intellectual “identify crisis” brought on, ironically, by Simon’s (unmerited) critique of the profession’s adherence to supposedly vague and inconsistent “principles” of administration (Hammond 1990). It was, therefore, a very good book at the right time to uphold values, in both research and practice, that the field of public administration rightly held, and still holds, dear.

The already noted 2007 reprise of *The Forest Ranger* by Larry Luton clearly expresses why the profession of public administration has so long admired this particular book. “[T]he case study,” says Luton, “exemplifies some of the best aspects of that research approach: using descriptive explanation to present a snapshot that brings much detail into insights and general conclusions” (2007: 168). Moreover, Luton goes on,

[b]ecause Kaufman was cognizant of the challenges involved in generalizing from a case study, he was modest about the bases of his explanations. He did not attempt to assert absolute causal relationships. His narrative gives an account of [as Kaufman put it] “the numerous . . . factors” involved in understanding “the way field men are induced to carry out tangibly the terms of headquarter agreements.” (1960: p. xi, quoted by Luton 2007: 166).

Richard Nathan wrote in a foreword to the 2006 republication that “Kaufman’s study teaches us about the need to dig deeply” (2006: p. xiv) in order to answer fundamental questions about public organizations. In the same vein, “It may be,” said Jason Burton in a belated 2012 *PAR* review of the 2006 republication (Burton 2012: 625) “that *The Forest Ranger* is [of value first and foremost] as an example of how to conduct a case study.”

As a student of public administration over a long career, Herbert Kaufman was *sui generis*. So, too, were other scholars of his generation such as Hugh Hecllo, Richard Neustadt, Aaron Wildavsky, and James Q. Wilson. Yet all of them might fairly be called historical institutionalists; each sought to advance our practical understanding of political and administrative systems and processes in an expressive, recognizable style. The generation of scholars that followed, among them Eugene Bardach, Barry Bozeman, Paul Light, and Beryl Radin, may be similarly characterized as producing unique works of empirical scholarship in a historical institutionalist style. Furthermore, scholars including among many others Daniel Carpenter, William Gormley, and Christopher Pollitt, have invested the institutional case study genre with analytic structure and more opportunities to generalize. Though their fields and disciplines might have been gravitating toward a more formal, scientifically based empiricism, their research and their voices stand out, as did Kaufman’s in *The Forest Ranger*: all are clear, sensible, careful, and insightful contributors to our understanding of democratic governance.

The Forest Ranger was not among the top 20 “most important books in public administration” of the preceding 50 years in Frank Sherwood’s well-known 1990 survey (Sherwood 1990). Yet, despite its sometimes Orwellian depictions of managerial manipulation and street-level conformity, the book is, as Luton (2007) notes, regularly cited as a classic work, “intensely engaged with complex and important issues in a way that has engaged others in this field to think and talk about it for decades” (Luton 2007: 168). Moreover, although not classics in a similar way, Kaufman’s later books have been similarly influential in their own right, many with insights that remain relevant.

Perhaps the real classic, however, is Herbert Kaufman the man. As Luton puts it (2007: 168):

The Forest Ranger . . . exemplifies some of the best aspects of its author, giving us insight into why he has had such a successful and respected career. It is honest, from its admission of how the Forest Service became the subject of the study to its recognition of the limits of its research approach. It is attentive to details in its treatment of the dynamics involved in the push and pull of disintegrative and integrative forces. It is ethical in its treatment of the forest rangers, respectful of their privacy, appreciative of their approaches to dealing with their myriad challenges, and analytically independent in reaching its own conclusions.

All of Kaufman’s work can be similarly characterized.

Once an aspiring law student who thought the subject of public administration “tedious,” Herbert Kaufman—the man, the scholar, and the author of a revered book—has become, himself, a classic of public administration. His books are readily available. Pick up any one or two, or more, and read them.

NOTES

1. Sources of biographical background are “Herbert Kaufman,” at: <<http://papedia.wikispaces.com/Herbert+Kaufman>> and Kaufman’s essay, “Music of the Spheres: A Lifetime of Study of Public Administration” (Kaufman 1996).
2. The seminal work for evolving scholarship on administrative behavior was a book that was to become another classic, Michael Lipsky’s *Street-Level Bureaucracy: Dilemmas of the Individual in Public Service* (1980). Lipsky’s classic work draws more than four times as many hits on Google as *The Forest Ranger* and six times as many on Google Scholar.

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CHAPTER 10

E. E. SCHATTSCHNEIDER, *THE SEMI-SOVEREIGN PEOPLE: A REALIST'S VIEW OF DEMOCRACY IN AMERICA*

DONLEY T. STUDLAR

AGENDA-SETTING AS THE “CONFLICT OF CONFLICTS”

ALTHOUGH E. E. Schattschneider (1892–1971) wrote extensively on US political parties as well as an early book on the politics of the tariff (Schattschneider 1935, 1942, 1948, 1969; Adamany 1972; Mileur 1992; Murphy 1992), his most renowned contribution to the study of politics was his slim (147 pages) 1960 book, *The Semi-Sovereign People: A Realist's View of Democracy in America*, in which he set forth a loosely connected explanation of how the US political system operated. Although ostensibly designed to be a “defense of parties” manifesto against the then-popular group theories of politics, its legacy depends more on the fact that it was the inspiration for more empirical work on various dimensions of the public policy process, especially agenda-setting, both in the US and abroad. *The Semi-Sovereign People* is a seminal work in the study of agenda-setting—the contentious, if often hidden, processes through which policy issues arise for policy debate; this is the “conflict of conflicts.”

The Semi-Sovereign People is one of the foundational pieces in the study of public policy, enhanced by the author's proclivity for the quotable observation. Here are a few examples:

Democratic government is the greatest single instrument for the socialization of conflict in the American community. (Schattschneider 1960: 13).

A democratic society is able to survive because it manages conflict by establishing priorities among a multitude of potential conflicts. (p. 66)

the definition of alternatives is the supreme instrument of power (p. 68)

All forms of political organization have a bias in favor of the exploitation of some kinds of conflict and the suppression of others because organization is the mobilization of bias. Some issues are organized into politics while others are organized out. (p. 71)

The flaw in the pluralist heaven is that the heavenly chorus sings with a strong upper-class accent. Probably about 90% of the people cannot get into the pressure system. (p. 35)

The socialization of conflict is essential to the democratic process. (p. 180)

SUMMARY OF THE ARGUMENT

While Lasswell (1956) had specified a series of policy stages, Schattschneider was the first to develop the idea of how some issues are organized into political conflict at a particular time while others, which may be equally worthy, are neglected. Thus the political agenda is limited, and there is not equal opportunity for all voices to be heard. This is not only the result of underlying social conditions, but also the product of a “mobilization of bias” in institutional procedures.

Schattschneider viewed politics in the US as a democratic struggle in which business interests, the dominant group represented in Washington, worked in conjunction (although not complete harmony) with the Republican party to maintain its privileges. The pressure group system is not completely pluralistic. The better-represented business class seeks to keep issues that might challenge their interests from even becoming matters of public controversy. While these interests actively work to keep the public quiet, the government uninvolved, and the political agenda limited, at times in a democracy the public and the government can counteract them (Cairney 2012).

According to Schattschneider, the struggle over the agenda is one of privatization versus socialization of conflict. In any political controversy the public is a potential resource for increasing the scope of conflict, which is expected to benefit one of the contending sides. This affects their strategies: the side fearing loss has an incentive to expand the scope of the conflict by encouraging public engagement; the more advantaged side would prefer to keep the conflict limited, preferably private. This competition to privatize or socialize conflict often occurs through publicly invoking widely held symbolic values such as “freedom” or “equality” (Cairney 2012). Although Schattschneider does not discuss this, mobilization can also lead to counter-mobilization by the other side, including changes to other venues (Hayes 1992; Baumgartner and Jones 2009).

There are more potential conflicts than can be heeded. The people are “semi-sovereign,” only able to exercise their power selectively to help determine issues worthy of government attention. The general public normally tends to play a passive role, but if excited and mobilized, it can “change the game of politics” through the pressure of numbers in a democratic system.

However, structures of government, such as established institutional procedures, also limit the agenda. Thus there is both a social and an organizational mobilization of bias in the political process. There is not equal opportunity for participation in key decisions by all potential participants, in contrast to Dahl’s (1961) conception of the “slack” of potential participation (Cairney 2012). Established interests want to keep the public apathetic and the government uninvolved, or at least involved only on their terms.

Schattschneider saw popular rule as sporadic and organized around particular issues rather than consistent and broad. Thus he favored more programmatic, unified political parties as enhancing the involvement of the public on a consistent basis across a range of issues rather than allowing individual politicians to be “bought off” by powerful interest groups. Schattschneider’s work on agenda-setting led to more skeptical views of pluralism, such as those of Kariel (1961), Bachrach and Baratz (1962), Lowi (1964), Crenson (1971), and Lukes (2005).

The “second face of power,” or the capacity to keep issues “private” in Schattschneider’s terms, rather than part of the public political agenda, became an important dimension of the ongoing pluralist/elitist academic debate of that time (Hunter 1953; Mills 1956; Dahl 1961; Polsby 1963; Domhoff 1967). The pluralists contended that it was more important to examine decision-making on particular issues rather than to ascertain the socio-economic background and networks of politically involved actors. But how did these issues reach the political agenda? Schattschneider was a key figure in this development, not through a concept of “false consciousness” or the capacity of elites to configure the public agenda exclusively through mutual agreement, but by the competition of many potential policy issues for attention, the necessity for prioritizing them, and the degree to which the public, as a potential instrument in this struggle, became involved. It was Schattschneider who emphasized that the content of the political agenda was a conflictual subject, as important as the struggle over preferred solutions and likely outcomes of public policy disputes. This perspective has continued to inform studies of public policy over the years, not only in the US but also comparatively (Howlett 1998a, b; Studlar 1993).

IMPLICATIONS OF THE ARGUMENT

In this wide-ranging essay on US politics, Schattschneider manages to touch, at least obliquely, on a number of issues that have become standard, more elaborated topics in the field. These include policy equilibrium and change (Sabatier and Jenkins-Smith

1988; Baumgartner and Jones 1993), especially incrementalism (Lindblom 1959; Hayes 1992); politics and markets (Lindblom 1977; Hall and Soskice 2001), the dynamics of group formation and maintenance (Olson 1965, 1982), mobilization and socialization of conflict (McAdam et al. 1996; Baumgartner and Mahoney 2005), the intensity problem (Dahl 1956; Scholzman et al. 2012), the organization of political cleavages into party systems (Lipset and Rokkan 1967; Mair 1997), the configuration of interests and institutions for particular policies (policy typologies) (Lowi 1964; Smith 1969; Tatalovich and Daynes 1988), social capital (Putnam 2000), focusing events (Birkland 1997), policy inheritance (Waltman 1987; Rose and Davies 1993), historical institutionalism (Hall and Taylor 1996; Hay and Wincott 1998; Pierson 2000), cartel parties (Katz and Mair 1995), institutional biases in agenda-setting (veto players) (Lijphart 1999; Tsebelis 1995), and the problem of nonvoting (Franklin 2004). Much of this literature has been written with little, if any, acknowledgment of its debt to Schattschneider. But his main contribution was to help define and develop the subfield of agenda-setting, including policy framing as a way of broadening or narrowing the conflict, which subsequently has become a major focus not only in public policy studies but in the broader lexicon of political commentary (Cobb and Elder 1983; Cobb et al. 1976; Kingdon 1984; Nelson 1984; Baumgartner and Jones 2009).

It was left to subsequent contributors to disaggregate Schattschneider's rather broad conceptualizations. For instance, agenda-setting has been disaggregated into governmental, nongovernmental, media, and specialist group agendas (Cobb and Elder 1983; Nelson 1984; Kingdon 1984); the rise, fall, and sometimes resurrection of particular issues (Downs 1972), different types of processing of issues (Cobb et al. 1976; Baumgartner and Jones 1993), the sometimes sudden transformation of the policy agenda through a dramatic focusing event (Kingdon 1984; Birkland 1997), the use of alternative institutional venues when an attempted mobilization (or preferred policy solution) is frustrated in one institution or level (Baumgartner and Jones 2009; Studlar 2002, 2010) and especially how issues may be framed by different groups and parties, leading, in electoral terms, to "party ownership" (favored by the public) on particular issues (Klingemann et al. 1994).

The punctuated equilibrium theory (PET) of Baumgartner and Jones (2009), however, may be the most clearly Schattschneiderian in inspiration since it adopts and attempts empirically to validate his ideas of changing the US political agenda through challenges to policy equilibria and dominant framing of issues in the policy subsystems (composed of interest groups, executive agencies, and Congressional committees) in the US central government. These challenges usually involve alternative images and definitions of issues, including preferred policy solutions, mobilization of the mass public through policy entrepreneurs and the media, and institutional turbulence through the utilization of alternative venues as challenges to existing ones that dominate a policy subsystem. Schattschneider had nothing to say about the role of policy entrepreneurs outside of parties and also nothing about the role of the media, but, according to Baumgartner and Jones, they are important parts of a "Schattschneiderian mobilization" stemming from negative feedback on current policies, as opposed to a "Downsian

mobilization” of enthusiasm based on positive feedback. Furthermore, based on their studies of a few issues, Baumgartner and Jones, in contrast to many other scholars, consider rapid change to be possible in the US. The PET theory in the US has inspired similar studies comparatively, especially in European parliamentary systems, where it has been tied to studies of party cleavages and issue framing (Baumgartner et al. 2009; Green-Pedersen 2007a, b; Engeli et al. 2012).

Expansion of conflict through different framing of issues on the agenda has increasingly been recognized as an important element in the policy process. As Peters and Hogwood (1985) point out, there is more “policy succession” than “policy innovation,” thus a constant struggle for “re-framing” of old issues ensues. These changing frames serve the interests of particular groups and range from minor changes leading to instrument calibrations to full-scale paradigm changes (Hall 1993; Capano and Howlett 2009). Especially in multiple-venue political jurisdictions such as the US and the EU, policy framing and venue shopping often operate together (Baumgartner and Jones 1991; Daviter 2007).

As Schattschneider indicated, in order for agenda challenges to occur, there must be a socialization of conflict. In tobacco policy in the US, for instance, this occurred in the 1980s as scientific evidence, interest group promotion, and media attention on the issue of the hazards of second-hand smoke moved it from being predominantly a low-level political issue involving individual choice to one that had social implications and therefore was worthy of broad public attention and greater government regulation of the behavior of smokers, tobacco manufacturers, and retail merchants (Troyer and Markle 1983; Nathanson 1999; Studlar 2002).

Even in a system of representative institutions, policy may not follow majority public opinion (Smith and Tatalovich 2003; Fleming 2012). There also is evidence that political participation of various kinds is unequal in democracies and that it is related to different political views, with the poor in economic and political resources generally being more “leftist” on socioeconomic policies, and the rich being more conservative (Headey and Muller 1996; Schlozman et al. 2012; Page et al. 2013).

In recent empirical studies, Schlozman et al. (2012) and Page et al. (2013) have confirmed Schattschneider’s findings about political participation in the US. Participation in the population varies widely. Even though there are now more groups representing formerly diffuse interests such as the environment and consumers, the pluralist chorus still sings with an upper-class accent. Higher socioeconomic groups are better represented because of their greater resources in finances, cognitive skills, slack time, organizational abilities, and political contacts. Furthermore, increasingly representation is based on paid professionals rather than voluntary effort, increasing the “resource deficit.” Professional and government-based groups in the education and health fields have particularly proliferated. This biases what politicians hear because it is lower level demographic groups that are more likely to be in need of government services in their daily lives and to articulate that need (see also Verba et al. 1995). There is a similar pattern for groups in the European Union, with business groups predominating in official representation in Brussels (Mahoney 2008; Greenwood 2011).

Schattschneider gave little attention to how conflict expansion could be resisted, but this subsequently has been the subject of considerable research, for instance, studies of how cultural meanings and arguments are used to avoid, redefine, subvert, and deny reframing of issues that would disadvantage currently dominant groups (Edelman 1964; Cobb and Ross 1997).

Baumgartner and Jones (2009) point out that interest group conflict over policy tends to precede institutional conflict. Following Madison in the *Federalist Papers*, Dahl (1956) identified the “intensity problem” as critical. Why do some groups manage to get the attention of policy-makers while others do not? Intense minorities have the resources and willingness to contact government officials on behalf of their views while the majority is more diffuse, passive, and lacks the resources to compete (Olson 1965; Schlozman et al. 2012). Recently there has been a developing literature, especially in the US, on conditions for group organization and mobilization (Lowery and Gray 2004; Baumgartner and Leech 1998; Schlozman et al. 2012).

One way in which democratic governments have tried to balance contending interests is through what some analysts call “neopluralism” (Lukes 2005), that is, financial subsidies and sometimes capacity-building training for “public interest” advocacy groups. This has occurred on issues such as tobacco control in several countries and multiculturalism and women’s rights in Canada (Pal 1995; Cairney et al. 2012). The European Union has funded diffuse groups in such areas as consumer affairs, environmental policy, and social policy (Greenwood 2011). The problem with such sponsorship, especially for direct financing, is that they may become dependent on such subsidies, which can be withdrawn if the sponsoring government is displeased with the organization.

Organizational bias can occur in various forms. One common variety, however, is through the necessity of having any adopted policy pass through a multiplicity of veto players, especially ones requiring an extraordinary majority, such as amendments to the US Constitution, the US Senate filibuster, and Qualified Majority Voting (QMV) in the EU Council of Ministers. Having such formidable barriers may even discourage discussion of some issues, such as reform of the Electoral College in the US.

POLICY AGENDAS IN TIME AND SPACE

While the phrase “conflict of conflicts” has entered political science lexicon, in fact there are few systematic analyses of the priorities of issues on the governmental or public agenda, including potential issues that are excluded deliberately or by negligence. This relates, of course, to the “second face of power” or “non-decisions” debate. Soon after Schattschneider wrote his masterpiece, the “rights revolution” occurred in several democracies, notably the US, where issues that had been largely “privatized” (equal rights for women, homosexuals, and ethnic minorities, environmentalism, smoking)

or subject to organizational bias (civil rights for blacks) became subjects of public controversy.

As noted previously, Schattschneider completely ignores the role of the media. Ironically, the first US televised presidential debates between John F. Kennedy and Richard Nixon took place in the same year, 1960. Subsequently, the media have become digitalized and fragmented, with fewer sources having a huge, diverse audience. While the more popular media have become more personality-focused and opinionated, the simple proliferation of media venues could allow more volatility in policy issues on the agenda, for example, there is the possibility of some issues becoming “viral” over the internet. The translation from internet popularity to the political agenda, however, is not an uncomplicated one. Furthermore, one of the problems of a fragmented, “narrowcast” electronic media is “selection bias,” meaning that people may have their information and opinions reinforced rather than broadened. There have been multiple studies, especially in the US, examining the question of whether the “digital divide” in internet access reinforces, ameliorates, or exacerbates participatory inequalities and, at least by implication, the political agenda. The overall evidence thus far suggests that acts to reinforce those inequalities, even considering that younger generations are more likely to become politically active through internet technology (Norris 2002; Mossberger et al. 2008; Schlozman et al. 2012; Cantijoch et al. 2012).

Schattschneider’s study generalizes based on the US experience and has only one reference outside that country, to the role of general elections in the UK party system in bringing policy issues to the fore. This is part of his long advocacy of greater “party government” for the US (p. 110). Like most US analysts of his time, however, he does not recognize that even in a “responsible party system,” there are issues on which parties are reluctant to take stands, for instance those defined as “issues of conscience” or “morality policies” (Christoph 1962; Engeli et al. 2012; Larsen et al. 2012; Pennings 2010). Furthermore, even the ostensibly representative British parliamentary system, with minimal veto players compared to others, has its share of organizational biases in agenda-setting, for instance through legislative rules for debate. When Northern Ireland had a devolved government, 1922–72, procedural rules made it difficult for the Westminster parliament even to discuss issues arising from that part of the United Kingdom. The main legal recourse of the British parliament in response to the growing civil discord there was to reimpose “direct rule,” which it eventually did in 1972 (Rose 1971; Crynes 1993).

Comparative politics offers a potential way of studying the “conflict of conflicts,” namely by comparing issue agendas over different countries at the same time as well as dynamically. While some studies exist on this topic for political parties, especially through research on the relationship of party manifestos and governmental agendas (Klingemann et al. 1994; Pennings 2010), what little there is on public agendas across countries is usually limited to a few issues (Green-Pedersen 2007a, b; Engeli et al. 2012). Furthermore, there also are few studies across time of agendas, priorities, and potential issues, despite the many references to Downs’s (1972) study of one issue, in one country, over a limited time period (Peters and Hogwood 1985). Most studies of

policy cycles are concerned with the agenda status of particular issues or comparing types of issues rather than examining the rise and fall of policy issues more broadly across countries (Capano and Howlett 2009; Hogwood and Peters 1982; Lundqvist 1980; Vogel 2003). Studies of globalization of public policy, with international dimensions having a similar impact on policies across countries, should consider evidence on policy agendas as well as outcomes. But thus far there is more research on the latter (Orenstein 2008; Swank 2006; True and Mintrom 2001; Kollman 2009; McGann and Sandholtz 2012).

Multi-level governance, especially through federalism/devolution and the European Union, increasingly has been recognized (Hooghe and Marks 2003; Hooghe et al. 2010). Thus a policy rebuffed at one level of government may be considered at another, although, once again, this depends on institutional rules (mobilization of bias). Furthermore, the opportunities for agenda-setting afforded by multiple venues may not necessarily lead to stability of the issue on the agenda, especially when compared to more partisan cleavage-based stability in countries with fewer venues (Green-Pedersen and Wolfe 2009).

More generally, there is the increasing problem of how to engage the broader public in the manifold and complex policies of a modern system of governance at any level. For instance, the European Union, has a demos (public) of 500 million people, most of whom are poorly informed about the responsibilities of the EU and how the different institutions interact. There is longstanding concern about the “democratic deficit” in this organization, most readily identified by low and declining turnout for European Parliament elections, and some attempts have been made to address this issue. The EU attempt to broaden participation in political debate through funding of “public interest” groups to balance those based in the private sphere has been discussed. Also, there are now more public consultations concerning possible legislation proposed by the European Commission as well as other reform measures in the Lisbon Treaty (Hix and Høyland 2011). Nevertheless, as previously noted, interest group representation in Brussels remains heavily weighted toward business groups.

In general, Western democracies have suffered from the problem of declining voter turnout and increasing mistrust of government. The role of business-affiliated groups and wealthy individuals in financing of political campaigns and lobbying has, if anything, increased. These phenomena depend somewhat on the laws in different countries, but nowhere is the role of money in a democracy more important today than in the US, whether it be in campaigns for the presidency and Congress or state referendums. Many of these political donations are anonymous, further confounding accountability. Studies have documented increasing social inequality in the US and elsewhere (Bartels 2008); others have confirmed Schattschneider’s hypothesis that, as inequality increases, voting participation declines (Solt 2010). Furthermore, in the US attempts at voter suppression through costly voter identification laws, deceptive information, and intimidation have occurred. Thus, electoral opportunities for a political agenda more broadly reflective of the interests of the entire US population are lost, as

demonstrated by the absence of discussion of the problems of the poor (an increasing share of the population) in recent US political campaigns.

In some ways the US has now achieved Schattschneider's (1942, 1947) goal of relatively unified "party government" (Adamany 1972; Mileur 1992) by two competitive parties, even if in separate institutions (presidency and Congress). Nevertheless, the unintended consequences of this more nationalized, party-divided debate have left the opportunities for agenda-setting perhaps even more limited than previously. Partisan redistricting for Congressional seats by state governments has reduced the number of competitive seats at general elections. Furthermore, the two-party cartel (Katz and Mair 1995), using state-level legislation and agreed rules for presidential debate formats, has effectively forbidden broad discussion of several issues that other, minor parties espouse. In the 2012 US presidential campaign, discussion of gun control was avoided by both parties and there was a notable "climate silence" on global warming. In contrast, minor parties raised a range of other issues, including aiding the poor, improving environmental protection, liberalizing drug laws, reducing the cost of higher education, establishing term limits, banning large-scale financial contributions to campaigns, reducing legal immigration, and revoking some national security restrictions.

CONCLUSION

Schattschneider remains perhaps the world's most quotable academic policy analyst. While his work lacks explanatory detail, does not cover all elements of agenda-setting, and needs a comparative perspective, it is richly suggestive. He was able to generalize, even from a limited US focus, about agenda-setting, with implications for several other topics. Only a few of these are discussed in this chapter. That he did this at a time when policy analysis was in its infancy has made *The Semi-Sovereign People* an international classic.

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CHAPTER 11

V. O. KEY, JR., *PUBLIC OPINION AND AMERICAN DEMOCRACY*

CHRISTOPHER WLEZIEN

V. O. Key's *Public Opinion and American Democracy* (POAD) was published in 1961, near the beginning of the behavioral revolution in political science. The book was motivated by a concern for opinion's impact on policy. In the early pages of the book (1965: 7), Key wrote: "Unless mass views have some place in the shaping of policy, all the talk about democracy is nonsense." That said, it mostly is a book about public opinion. There is some treatment of the linkage between opinion and policy; this may be what the book is best known for. Of particular note is his emphasis on "opinion dikes"—that the public's impact on policy is not directive, but constraining. Still, the primary focus for Key is the public. This seems to reflect the assumption that an understanding of the connection between opinion and policy must begin with public opinion.¹ It was especially true back in 1961, when our understanding of public opinion was nascent.

In the book, Key addressed this imbalance. He provided an empirical statement, relying in part on public opinion data. The surveys were conducted in the United States (US) and, while the context is a feature of his examination, the perspective is broader. Key was not only interested in describing opinion in the US but analyzing it and offering general statements about the public and the "open interplay of opinion and policy" (Lasswell 1941: 15). In the course of his investigation, he confirmed much of what other scholars had found and contributed original observations and insights. Many of these remain true today.

More importantly, Key challenged the conventional wisdom in sociology as well as the emerging wisdom in political science about the origins and dynamics of public opinion. He was sensitive to the fact that opinion is not only a cause of politics; it is a consequence as well. That is, public opinion did not emerge independent of politics; it is a product of what politicians do. There was admittedly little evidence for this at the time, even in the book itself. There is more now and the impact of politics and policy on

opinion is better understood. The impact of public opinion on policy is too. Key almost certainly would be pleased to see the progress political scientists have made in disentangling Lasswell's "open interplay."

This chapter considers Key's book and its impact. I begin with a brief characterization of the academic literature at the time *POAD* was written before turning to the book itself—what Key argued and showed. I then consider how research has evolved and is evolving, and its relationship to what Key offered some 50 years ago. Special attention is paid to the connections between opinion and policy. As we will see, scholarship has come a long way, but Key's imprints remain visible.

THE CONTEXT

When Key wrote *POAD*, scientific public opinion polling had arrived and actually was flourishing. Analysis of data had supplanted the primarily speculative mode of much of the earlier writing. Key recognizes the optimism of the Progressives and then the "disenchantment" after World War I, most notably, Lippmann's *The Phantom Public*. In that book, Lippmann (1925) attacked—some would say "destroyed"—the assumption of a homogeneous, fully informed, and monitorial public. Key noted that what Lippmann destroyed really was a straw man, something that bore little resemblance to the expectations of most of modern democratic theory. One exception was Schumpeter (1950), who echoed Lippmann's pessimism. Mills's *Power Elite* (1956) went one step further, and posited that public opinion was simply a creation of the ruling elite. None of these strong, normative statements about the public relied on actual public opinion data. This also is true of more optimistic "pluralistic" models of politics in representative democracies, for example, Truman's (1951) conjecture about "potential groups" as well as Dahl's (1971) later work. Optimism is implicit in Deutsch's (1963) cybernetic models and Easton's (1965) model of a political system as well, both of which rely heavily on negative feedback.

Some research relied on data, much of which focused on voting behavior. There is the classic sociological literature that emphasized interests (Berelson et al. 1954). From this perspective, public opinion largely reflects one's socioeconomic circumstances. There also is the social psychological literature reflected in *The American Voter* (AV, Campbell et al. 1960), which came out in print just before Key's book. (The AV was authored by the very same people who hosted Key while he wrote *POAD*; indeed, they provided the data he used.) For Campbell et al. (1960) public opinion was not determined solely by interests, but ultimately reflected political identification, especially with political parties. While party identification tended to reflect sociological circumstances, the association was not perfect and, even to the extent it was, those circumstances were not the determining mechanism(s).

The growth of scientific public opinion polling spawned a flow of research on public opinion (see Shapiro 2011, for a comprehensive review). There was work on the

existence of opinion and its direction, whether supportive of or opposed to government action. There also was research on divisions of opinion across subgroups, especially those associated with socioeconomic class. This provided important guideposts but remained largely disconnected from politics and political science.

THE BOOK

POAD is a decidedly empirical book. Key posited empirical relationships and tested some of them, relying on survey data collected by the Survey Research Center at the University of Michigan. He provided both description and analysis organized into five main sections: patterns of opinion distribution, structural differences, properties, formation, and linkage.

In the first section of the book, on “patterns,” Key considers the general distribution of opinion. He focuses on the degree of consensus, conflict, and concentration. The section is empirical, but is driven mostly by theoretical and conceptual concerns. For instance, when assessing consensus, Key contemplates different empirical manifestations, including a permissive consensus. He does much the same with conflict, especially as regards the modality of opinion across issues. The treatment of concentration recognizes that opinion consensus and conflict pertain to issues on which large portions of the public have opinions, which frequently is not true. This fits neatly with later work on attentive “issue publics” (Converse 1964; Hutchings 2003; also see Neuman 1986; Lupia and McCubbins 1998).

Key then examines the “structural” distribution of opinion. Here he is interested in whether and how (and why) people’s opinions differ, and empirical analysis is central. Key considers various cleavages—geographic, occupation, social class, and political stratification. Interestingly, he reveals a lot of what he refers to as “congruence,” or similarity of opinion. (This differs from the more common usages of the word in studies of representation, as I will discuss.) Even where there is non-congruence of opinion across groups, say, regions or socioeconomic class, Key finds that it often is not huge.² He highlights the limits of “objective” self-interest as an explanation for public opinion, noting Kornhauser’s (1949–50) earlier work. Perhaps the most interesting chapters in the section address the interrelationships of opinion on different issues, which foreshadows Converse’s (1964) classic work on belief systems. Key finds modest levels of what Converse calls “constraint” in the American public—that is, cleavages on different issues tend to be cross-cutting, not reinforcing. Another interesting chapter identifies different political strata, mostly in terms of their activism, and then demonstrates corresponding differences in opinion.

After probing distribution, Key turns to what he refers to as the “properties” of opinion. While researchers had already probed in some depth the direction and general structure of opinion in the US, less attention had been paid to other qualities. Of special interest to Key were intensity, stability, and latency. Unsurprisingly, he posited and

found that intensity and stability varied across characteristics of issues and individuals, including party identification, which comports nicely with Campbell et al. (1960). More compelling is the treatment of latency. Here Key grapples with the activation of underlying opinion. Political elites are critical here, though their roles are limited. Key notes (1961: 270) that “It is quite a feat to induce the public to listen, much less to manipulate it.”³ He then assesses the effects of political events on opinion activation as well as reorientation. Particularly fascinating is his examination (pp. 278–9) of public support for Roosevelt’s Supreme Court reorganization plan during the winter and spring of 1937. As the Court reversed its position on state minimum wage legislation and decided to uphold critical pieces of legislation, for example, the Social Security Act, the public became less supportive of reorganization. Attention to the events (or the ensuing debate) is critical to this response, and it is a recurring theme in the chapter. As Key (p. 282) states, “the problem of opinion latency . . . is one of public attention.” Low levels of attention, he argued, allow governments a certain latitude—a permissiveness—and encourages politicians to avoid taking actions that might antagonize the public. The latter may be less broadly true today than it was then.

Key next considers the formation of opinion. Here he stresses the usual suspects—family, education, and the media—and corroborates some established truths and extends others, including the role of group loyalties and identification. Perhaps most interesting is the role of the mass media, and much of what he writes may be true today even despite dramatic changes in its structure and role. A lot had been said about the role of the media at that point in time but little was really known. Key provided little in the way of evidence but did offer quite a lot in the way of theory. He (p. 369) emphasized choice: “People may choose to attend or not to attend to the media. They may choose among the media. And they may choose among the messages of any particular medium.” This is not surprising, as choice is critical to media influence; it is easy to overlook, however. He also viewed the media less as cue-giver and more as a “common carrier,” providing largely homogeneous coverage and supporting the status quo. Both have changed dramatically since Key’s book was published—choice and the heterogeneity of content have exploded. These changes appear to be related (Prior 2007).

In the final empirical section, Key visits the driving motivation for the book, the linkage between opinion and politics. Elections are important, of course, but Key stresses responsiveness: “The vexing analytical problem comes in the comprehension of the extent to which, and the processes whereby, public opinion is linked to the actions of government in the periods between elections” (p. 413). As discussed, he conceives of the interaction between the public and government as a two-way flow, and explicitly identifies “feedback” in a very general way. There are chapters on the usual mediating institutions—parties and interest groups—and elections as well.⁴ These emphasize the translation of opinion into policy. Key highlights the role of parties and the issue content of elections in driving the representation of mass preferences. There also is a chapter on representation itself that provides a little evidence of correspondence, specifically between constituency demographic characteristics (not opinion)

and representatives' roll call voting records. As Key himself admits, this analysis just begins to scratch the surface.

The concluding chapter of the book is focused on the implications of his analysis (and theorizing) for the functioning of representative democracy (pp. 535–42). Key contemplates the division of labor in a democracy, again placing a real premium on the attentive public, the critical monitorial segment (also see Neuman 1986). He also ruminates on exactly how opinion matters for politicians and policy. To Key, the public does not really direct politicians; it constrains them: he sees politicians as guided by the public in a loose way, via a system of “opinion dikes” (p. 552). There are a number of reasons, including the fact that preferences typically are general (not specific) and that opinions often are not intense. This allows elected officials a good amount of discretion, according to Key. It is much like what Stimson (1991) referred to as a “zone of acquiescence,” within which the public allows politicians to shift policy pretty much at will. It leaves one thinking that elections ultimately are very blunt instruments of accountability (see Goodin 2000).

THE SUBSEQUENT RESEARCH ON PUBLIC OPINION AND POLICY

This foregoing only incompletely summarizes what Key did in what is a long and thoughtful book—it was necessary to sample. The same is true when documenting its impact over five decades. I focus on research addressing the relationships between opinion and policy. There are three reasons for doing so. First, it is the driving motivation for Key's book. Second, it is one of the defining relations of representative democracies. Third, it fits the remit of this *Handbook*—the classics of public policy. Here, I consider both the representation of opinion in policy and the feedback of policy on opinion.

Opinion Representation

There has been an explosion of work on the representation of public opinion since the publication of *POAD*. The research has been the subject of a number of recent reviews (Kuklinski and Segura 1995; Manza and Cook 2002; Burstein 2003; Weakliem 2003; Brooks 2006; Wlezien and Soroka 2007; Wlezien 2011; Shapiro 2011). As Key surmised, the work shows that public opinion matters for what politicians do. This has been demonstrated in many ways, with roll call votes, party and government positions, and policy itself. That there is a variety of evidence for representation is not to say that it holds on all issues, in all places, and all times, as we shall see.

Roll call votes were the focus of the earliest research on representation, some of which Key noted in the book. This work mostly was focused on “dyadic representation” between the opinion of geographic constituencies and the behavior of their representatives. Miller and Stokes’s (1963) ground-breaking study showed that representation varied across issues, which led scholars to explore the conditions under which representatives reflected public opinion. McCrone and Kuklinski (1979) effectively integrated a general principle of scholarship by positing that representatives follow public opinion only on certain salient issues where the public sends clear signals and the representatives see themselves as “delegates.” Achen (1978) challenged all of the work in the area for its reliance on correlational analysis, which does not really demonstrate a match between what the public wants and gets.

Some work focuses on the positions of elected representatives and their districts—a dyadic representation relating the opinions of the public and politicians. Certain early work focused on French legislators, or “deputies” (Converse and Pierce 1986). More recently there has been work on members of US House members (Bafumi and Herron 2010). While this kind of research may avoid the pitfalls of correlational analysis, all of the dyadic work is vulnerable to questions about causality. That is, the evident relationship may reflect the influence of representatives on opinion and not the other way around (Hill and Hurley 1999).

Dyadic representation is important but does not reveal whether national opinion is represented in collective decisions. Weissberg (1978) first raised the issue in his reanalysis of the Miller-Stokes data, in which he demonstrated a closer relationship between broad public opinion and Congressional action. Other scholars have examined the general ideological or policy dispositions of governments and the public, mostly in outside the US (Lijphart 1999; Miller et al. 1999; Powell 2000). Yet others focus on party positions (Budge et al. 2001; Ezrow 2010).

There has been much less research on actual policy. Partly the difficulty is matching up public opinion and policy. We do not have good measures of the public’s preferred levels of policy in most areas. There are measures of absolute preferences in selected areas, such as abortion and gun control. In other areas, survey organizations commonly ask about relative preferences. For instance, should the government “do more”? Or, is the government doing “too little”? This clearly complicates assessments of what Weissberg (1976) refers to as “majoritarian congruence”—the assessment of the actual match between opinion and policy. That is, we don’t know from the data we commonly have what the public actually wants.⁵ As already noted, we do have measures of support for specific policies, and we can assess how they comport with what policy-makers do, as some scholars recently have done (especially see Lax and Phillips 2012). Such studies of majoritarian congruence remain rare.

Much of the extant work assesses the *correspondence* between opinion and policy at particular points in time, say across states (e.g. Erikson et al. 1993) or school districts (e.g. Berkman and Plutzer 2005) or countries (Brooks and Manza 2007). Much as for dyadic representation, this research assesses whether there is an association between public support and policy commitment, and most of it demonstrates that there is—is

there more (less) policy where support is higher (lower)? Other work focuses on the *consistency* between public preferences and policy. Does policy change where the public supports policy change? Monroe (1979) was the first to undertake this kind of research, focusing on the US, and many others have followed in other countries (Brooks 1985; Brettschneider 1996; Petry 1999). There is more recent work in the US, including another installment from Monroe (1998) and important new work from Gilens (2012).

Yet other research examines the *covariation* between policy change and opinion change—what Weissberg called “covariational congruence.” Page and Shapiro (1983) pioneered research on this congruence in their analysis of the US. As sufficient time series data became available, scholars conducted analyses of “dynamic representation.” This allows a more explicit assessment of actual responsiveness to opinion, and a large and rapidly growing number of scholars have adopted the approach (e.g. Hartley and Russett 1992; Stimson et al. 1995; Wlezien 1995, 2004; Erikson et al. 2002; Eichenberg and Stoll 2003; Hobolt and Klemmensen 2008; Jennings 2009; Soroka and Wlezien 2010; Hakhverdian 2010, 2012). There now is a lot of evidence of policy responsiveness in a number of policy areas and countries. This is important, but it should be kept in mind that responsiveness does not necessarily imply majoritarian congruence. It may be, after all, that politicians change policy in response to changing opinion but that the level of policy under- or overshoots what the public actually wants. Making this determination is not easy in most policy domains given the difficulty of measuring the public’s preferred levels of policy.⁶

Issues and Representation

Politicians do not represent public opinion equally in all policy areas. This was apparent from the original exploration by Miller and Stokes (1963) of dyadic representation. Key surmised that “salience” would matter, and much research bears this out. That is, policy behavior more closely reflects opinion in areas that the public considers to be important (e.g. Kuklinski and Elling 1977; McCrone and Kuklinski 1979; Hill and Hurley 1999; Wlezien 2004; Druckman and Jacobs 2006; Soroka and Wlezien 2010). The attention the public pays also may matter (Jones 1994). The relationship between issues and dominant cleavages may as well (Hill and Hurley 1999).

There is good reason to think that the information politicians have is important. Geer (1996) argues that scientific public opinion polling has made it easier for politicians. In a related way, Burstein (2006) argues that the tendency for survey organizations to ask mostly about salient issues tends to inflate the amount of representation scholars detect.⁷ It also may make it easier to represent domain-specific preferences (instead of general public “mood”) in salient areas (Wlezien 2004; Druckman and Jacobs 2006). In one of the more intriguing studies of the representation connection, Butler and Nickerson (2011) show that providing information to politicians can substantially increase majoritarian congruence on non-salient issues.

Parties and Representation

Key stressed the importance of political parties in mediating the relationship between the public and policy. They clearly do aggregate preferences. Whether and to what extent they enhance majoritarian or covariational congruence is less clear. Research increasingly emphasizes party polarization. Bafumi and Herron (2010) demonstrate that members of Congress are more extreme than their constituencies, and that this leads to “leapfrog representation” when Congressional control changes partisan hands. Hussey and Zaller (2011) show that party affiliation matters at least as much as constituency preferences in Congressional roll call voting. Lax and Phillips (2012) reveal that unified party control of state governments leads to policies that are off center—either to the left or right of the median citizen in the states. The influence of political parties on policy at the US national level is well-documented (Erikson et al. 2002; Wlezien 2004; Bartels 2008; also see Monroe 1983; Wood 2009). The same is true in other countries, though trends there, for example, as regards polarization, may be different (Kayser and Wlezien 2011; Dalton et al. 2012). The bottom line is fairly clear: parties do not neatly represent the median citizen or voter; it may be that they best represent the median partisan (also see Canes-Wrone 2006).

Who Gets Represented?

The attentive public is critical in Key’s view of representative democracy. Devine (1970) built on the general idea in his polyarchical model of politics (also see Dahl 1971). Other scholars consider that the attentive public can differ across specific issues, leading to “issue publics” (see e.g. Hutchings 2003). More recent work picks up Key’s emphasis on how political strata matter for representation. Griffin and Newman (2005) show that US members of Congress pay more attention to the opinions of the voting public than the non-voting public. Scholars have examined whether certain organized groups have more influence. Jacobs and Page (2005) show that US foreign policy-makers rely mostly on the opinions of business and labor leaders as well as experts.⁸ There is special interest in the representation of income groups. Leading the way here are Larry Bartels (2008) and Marty Gilens (2012), who show that the rich are much better represented than the poor. Of course, unequal representation ultimately depends on differences in preferences across groups, which may not be as common or as great (or as consequential) as expected, as Key suggested (also see Enns and Wlezien 2011).

Institutions and Representation

Key considered the importance of political parties (and interest groups) but not of other political institutions. Other scholars have done so, particularly across countries. Here,

electoral institutions have been central (see Lijphart 1999, and especially Powell 2000). Powell shows that proportional systems enhance the representation of the median voter in the wake of elections, at least with respect to broad ideological dispositions. Blais and Bodet (2006) and Golder and Stramski (2010) do not, however. Powell (2011) himself suggests that the difference in the results may reflect differences in party polarization, a possibility that is developed and assessed more fully in Dalton et al. (2012). Leaving aside representation in the wake of elections, Wlezien and Soroka (2012) show that governments in proportional systems may be less responsive between elections.

Government institutions are receiving increasing attention in comparative analysis. Strøm (2003) posited that parliamentary systems might effectively represent voter preferences on election day but not very well in between elections because of the difficulty of controlling the executive. Hobolt and Klemmensen (2008) show that the responsiveness of government to public priorities is greater in the US, where there is balance between the executive and legislature, than in parliamentary systems. Analyses of public preferences and policy reveal much the same (Soroka and Wlezien 2010; Wlezien and Soroka 2012).

In recent years, subnational institutions have received increasing attention, particularly in the US. In their analysis of state-level representation, Lax and Phillips (2012) demonstrate that the level of majoritarian congruence is greater in states with term limits and where legislatures are highly professionalized. Berkman and Plutzer (2005) focus on school districts, and they find that representation is more pronounced for districts that are dependent on other levels of government for funding. Mullin (2008) considers government delivery and pricing of water in various parts of the country, and she shows that governance arrangements are crucial but not in a one-size-fits-all manner—that is, circumstances matter. Specifically, when water delivery faces a crisis, say, owing to a drought, general governments are more responsive; during normal periods, special districts are more responsive. This is just a sample of the evolving research on the impact of institutional arrangements.

Policy Feedback on Opinion

Research on the influence of politics and policy on public opinion also has exploded. Central to much of the work are political elites. The now-classic treatise is Zaller's (1992) book, *The Nature and Origins of Mass Opinion*. He sees elites as critical to opinion activation and formation, though he does not actually demonstrate reorientation (or conversion) of sentiment. Elites surely help to activate preferences (also see Geer 1996). They provide cues to supporters (also see Lenz 2012). They also attempt to change people's minds (see Jacobs and Shapiro 2000). Whether and the extent to which elites effectively can change public opinion is less clear, however. There is evidence that they do not—especially see Edwards's (2006) interesting analysis of presidential leadership. This does not mean that politicians never lead. The extent to which they do depends

substantially on the support of other political elites (also see Brody 1991) and is conditioned by other factors (Chong and Druckman 2007).⁹

There is pervasive evidence of negative feedback of policy on opinion—that is, the public behaves like a thermostat (Wlezien 1995, 2004; Soroka and Wlezien 2010). When policy goes up, public support for *more* policy tends to go down, other things being equal (also see Erikson et al. 2002). Of course, other things can and do change, including the (effective) preferred levels of policy; research shows that the economy matters (mostly in a pro-cyclical way) and that there also may be an underlying trend (Stephenson 2001; Soroka and Wlezien 2010). What is most important for this discussion is that negative feedback holds across policy domains in the US, though salience plays an important moderating role. It also holds across countries, though government institutions, especially federalism, appear to matter (see Wlezien and Soroka 2012).¹⁰

There is positive policy feedback on opinion as well. This is most likely in areas of emerging government involvement, as during the Great Depression in the US, or when expanding into new areas (see Campbell 2005).¹¹ It may be true at other times, at least for some individuals. Given the research, positive feedback often is overwhelmed by negative feedback, at least in the short run. This fits with Key's suppositions about the public and elites, as it implies that the public effectively notices and responds to what policy-makers do. It provides an important incentive for politicians to respond to public preferences, as the public is effectively monitoring what they are doing, at least collectively. Perhaps more importantly, thermostatic feedback makes responsiveness possible, as the public preference signals being sent to policy-makers are, at least to some extent, informed by what policy-makers have done. Politicians thus have the motivation *and* means for representing public preferences, at least in highly salient policy areas, particularly under certain institutional arrangements. This is not to say that representative democracy is perfect, but that it works to some degree, perhaps about as well as we can expect.

THE LEGACY

Key's book clearly is an important influence and guidepost in modern political science research. It also was substantially correct in its assessments of public opinion and representation, and Key most likely would have been sanguine about scholarly developments over the past 50 years. He may have been a little too optimistic about the functioning of representative democracies, particularly the effectiveness of elections as an accountability mechanism. This optimism is reflected in *Public Opinion and American Democracy* but is most evident in his next and last book, *The Responsible Electorate*, published in 1966, just after his untimely death at only 55 years of age. Key saw voters as independent and mostly rational actors, choosing between candidates on the basis of their positions on issues of policy. While this may be partly true, there is a lot more to the story than that. Party identification powerfully structures

perceptions of candidates and political reality itself (see Campbell et al. 1960, for the classic statement).¹² And the important “floating voters” who tip elections one way or another rely heavily on factors, such as late economic growth, over which politicians seemingly have little direct control (Zaller 2004; Erikson and Wlezien 2012). There still may be enough—or the threat of enough—issue content in election results to motivate politicians to pay attention to the public. After all, it otherwise would be hard to explain the policy responsiveness to opinion that we observe in the US and other representative democracies.

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NOTES

1. This was the motivation for the creation of *Public Opinion Quarterly* journal back in the 1930s (Shapiro 2011).
2. This remains true today, at least for certain divisions of the population (Soroka and Wlezien 2007; Enns and Wlezien 2011). It is a pattern that seems to warrant more serious investigation given its potential political importance (especially see Gilens 2012).
3. He also (1961: 270) added that “The probabilities are that the kinds of stimuli that arouse latent opinions widely within the public are not the simple verbal stimuli of politicians. They are, if one may guess, more usually objective conditions that affect or capture the attention of the masses of people.” The literature on the economy and election outcomes supports Key’s conjecture, as trends in the real economy tend to matter despite the attempts of disadvantaged candidates to alter the interpretation or subject. For a somewhat dated summary of the vast literature, see Lewis-Beck and Stegmaier (2000). Some work explicitly addresses public reactions to both economic conditions and political rhetoric, and supports the direct and indirect importance of the real economy (De Boef and Kellstedt 2004).
4. Winning and losing feature in the latter, and this loosely comports with Christopher Anderson’s work (see e.g. Anderson et al. 2005).
5. To be clear, this is not the fault of the survey organizations; just a reflection of the nature of public preferences in most policy areas. Consider, for instance, asking how much welfare spending people want. Consider how you would answer the question.
6. Questions about actual causality also remain. Are politicians actually responding to the public, or are they and the public both simultaneously responding to something else?
7. There are two possible mechanisms: (1) the increased information that polling on salient issues provides to politicians and (2) the upward bias in analyses of representation owing to the (salience) bias in the sample of issues for which public opinion data exist.
8. Note that Smith (2000) finds little direct influence of business groups on domestic policy in the US.

9. Of course, there are instances of outright manipulation. Consider public opinion in support of Vietnam escalation in the wake of the Gulf of Tonkin incident, which partly reflected President Johnson's distortion of what happened (Page and Shapiro 1992). Such cases seemingly are rare.
10. Of course, the degree of federalism also varies across policy domains within countries (Soroka and Wlezien 2010).
11. As regards the latter, consider public support for the Medicare Drug Benefit plan between 2004 and 2006, where there is a hint of increasing support after passage that decayed some and then crystallized after the plan went into effect (in Jan. 2006). This analysis was provided by Robert Shapiro in email correspondence, and is based on Kaiser Family Foundation data.
12. There is a lot of recent work on the subject (see e.g. Wlezien et al. 1997; Bartels 2002; Evans and Pickup 2010; Lenz 2012). For a consideration of the implications of evolving partisanship for the effects of other variables, see Kayser and Wlezien (2011).

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CHAPTER 12

MICHEL CROZIER, *THE BUREAUCRATIC PHENOMENON*

PHILIPPE BEZES

THE CLASSIC

MICHEL Crozier's *The Bureaucratic Phenomenon* ([1964] 2010) is one of the most internationally prominent French studies in public administration. The book's in-depth study of two (French) bureaucracies represents a seminal contribution in the tradition of early organizational sociologists like Herbert A. Simon, Peter M. Blau, Alvin W. Gouldner, or Philip Selznick. The core focus was on the ordinary functioning of increasingly powerful (public and private) bureaucratic organizations in the 1950s and 1960s. Like his colleagues, Crozier challenged a view, (unfairly) associated with Max Weber, that was said to overemphasize the formal and rational organizational structure of bureaucracy (Weber's "ideal type") and that, at the same time, paid insufficient attention to informal relations, unofficial norms, and resistance among social actors.¹ Crozier developed his approach by empirically exploring the relationships between formal rules, informal practices, interacting behaviors between strata, and cultural forms. Crozier's specific contributions were to develop a relational theory of power and a systematic program that explored bureaucracy as an "organizational system."

Michel Crozier's *The Bureaucratic Phenomenon* was published in the United States in English in 1964 by University of Chicago Press, only a year after its French publication in 1963 by Editions Le Seuil. As noted, *The Bureaucratic Phenomenon* has been widely referred to in scholarly and peer-reviewed journals, with 2,108 references on Web of Science from 1964 to 2013 in the fields of sociology, organization studies, political science, and public policy and administration.² Figure 12.1 shows the 30 journals that cite the book most frequently (827 citations) according to the decade of citation. *The Bureaucratic Phenomenon* is cited across journals (*Administrative Science Quarterly*

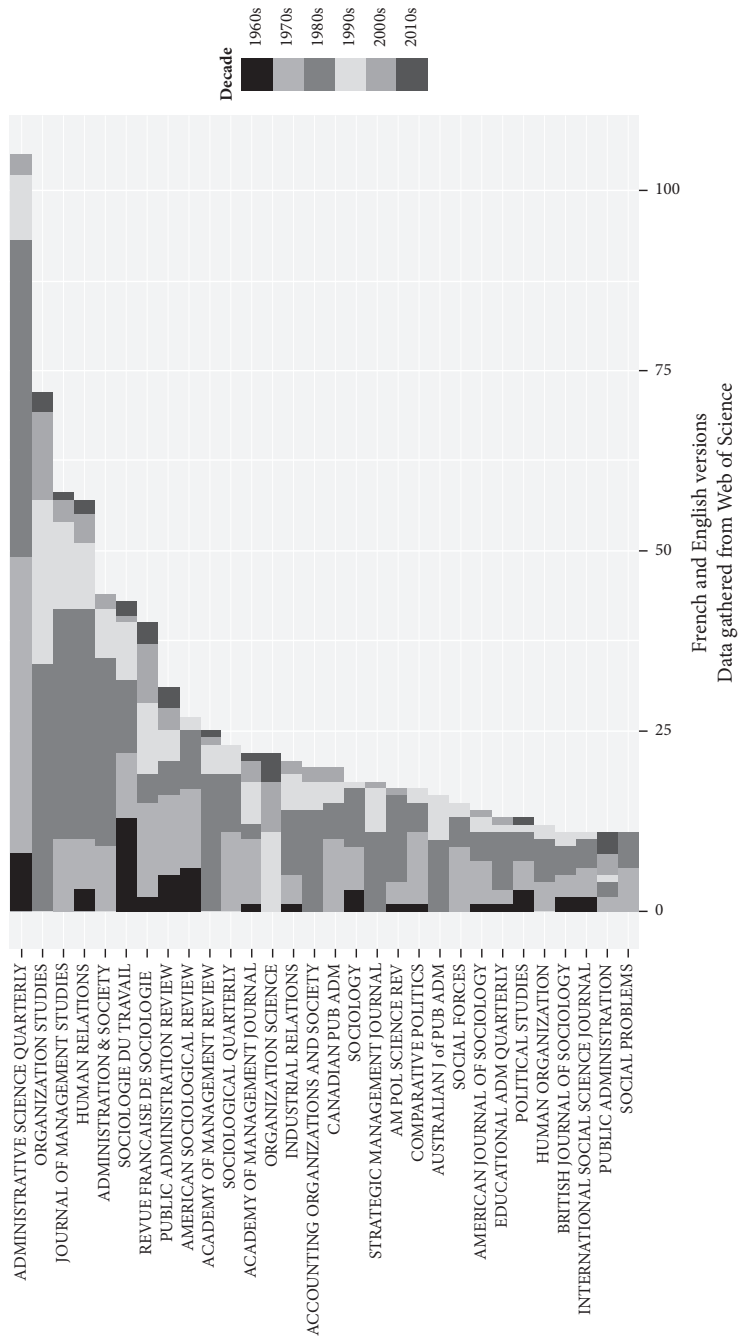


FIG. 12.1 Citations of Crozier's *Bureaucratic Phenomenon* in social science journals 1964–2013

Source: Compiled and kindly supplied by Etienne Ollion and Andrew Abbott from Web of Science data (see Ollion and Abbott 2013).

is clearly at the top) and disciplines (including sociology, public administration, and political science).

Equally significant is the evolution of citations across the six decades. Citations peak in the first half of the 1980s. This is followed by a degree of decline, but with continued prominence in organization theory. In terms of the social science field³ (Figure 12.2), sociology, and, to a lesser extent, public administration made up most of the citations during the first years. However, since the 1980s, *The Bureaucratic Phenomenon* increasingly features in studies in the (growing) fields of organization and management studies.

Furthermore, *The Bureaucratic Phenomenon* also featured prominently among later classics. For example, James D. Thompson's *Organizations in Action* (1967) emphasizes the issue of uncertainty control in organizations in relation to the structure of power. Similarly, James G. March and Johan P. Olsen's *Rediscovering Institutions* (1989) stresses the importance of the power of information and expertise. Michael Lipsky's *Street-Level Bureaucracy* (1980) is interested in role tensions and how rules order workers' role conceptions. It also considers how bureaucratic relations and street-level bureaucracies reflect and perpetuate the values of wider society (1980: 180–1). James Q. Wilson's *Bureaucracy* (1989) refers to the specificities of administrative cultures, as does Bernard S. Silberman in his *Cages of Reason* (1993). Silberman explains why "all bureaucratic roles and organizational structures did not end up alike" (1993: 6), an anomaly not sufficiently explained by scholars like Crozier. Silberman's framework

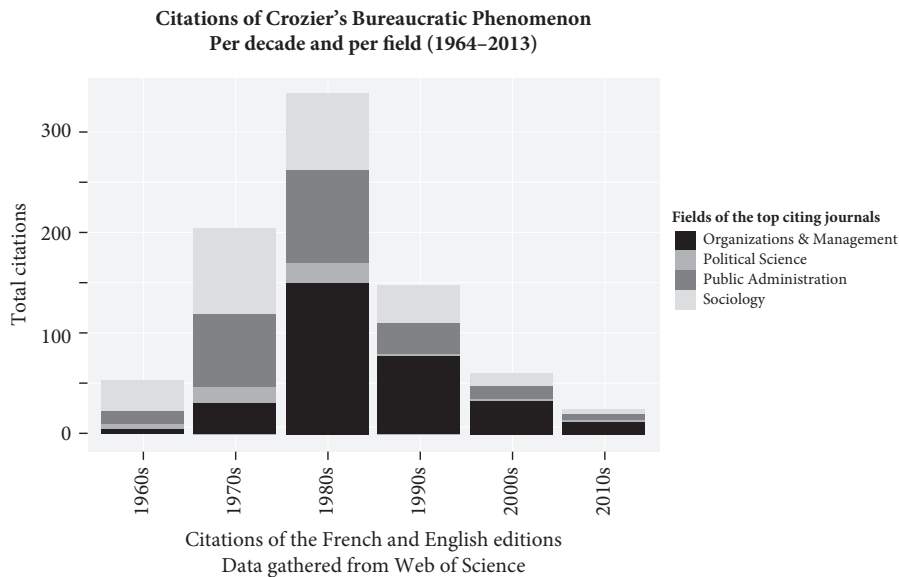


FIG. 12.2 Citations of Crozier's *Bureaucratic Phenomenon* in social science journals by decade and field 1964–2013

Source: Compiled and kindly supplied by Etienne Ollion and Andrew Abbott from Web of Science data (see Ollion and Abbott 2013).

builds on Gouldner's (and Crozier's) argument about uncertainty about succession in relation to rules, expertise, and power. More recently, Crozier is mentioned as an influence among scholars who explore how performance management affects administrative behaviors (Moynihan 2008), how policy changes brought about by welfare reforms reshape the identities, power, and practices of the "New Welfare bureaucrats" and caseworkers (Watkins-Hayes 2009), and how bureaucrats shape policies and policy-making (Page and Jenkins 2005; Page 2012).

Crozier's *The Bureaucratic Phenomenon* represents a classic in at least three ways. First, his in-depth study of public bureaucracies in the French context contributes to the tradition of early organizational sociologists including Herbert Simon's *Administrative Behavior* (1947), Philip Selznick's *TVA and the Grass Roots* (1949), Alvin Gouldner's *Patterns of Industrial Bureaucracy* (1954), and Peter Blau's *The Dynamics of Bureaucracy* (1955). These scholars studied informal practices, rules, and cultural forms that shape bureaucrats' behaviors and the way they carry out their work. These practices affect the overall functioning of public bureaucracies and, moreover, the production of public policies.

Second, *The Bureaucratic Phenomenon* is a classic because it linked its empirical, situated, and practices-oriented approach to a more general argument about the French administrative organizational culture and its linkages to the labor movement, the educational system, and the government's political-administrative nexus. In a clear echo of Tocqueville's comparison of France and the United States, Crozier's analysis offered an insightful analysis of the French state and society. It has formed the basis for ongoing debates "in search of France" (Hoffmann 1963). The French strong resistance to face-to-face authority relationships and its manifestation in centralization and the development of rules which protect strata from each other has been a classic feature in describing the French statist culture.

Third, *The Bureaucratic Phenomenon* is a classic in its understanding of "power" in the study of organizations. Power, as a phenomenon, is conceived as the central problem in the study of organizations. As a concept, power describes a form of social relationship where relations between actors both within and between organizations are structured by the control of "sources of uncertainty." These "sources of uncertainty" are socially constructed in organizations and usually relate to expertise, critical resources (finances, knowledge, know-how, etc.), technologies, and all forms of unpredictability created by the processes of rule-based rationalization of bureaucracies. According to Crozier, an understanding of the real functioning of bureaucracies cannot be gained from studying the ongoing flow of newly created rules. Instead, one has to identify "parallel power relations" that are based on dependence between actors. These relations are often revealed by conflicts. His idea of the "strategic bureaucratic organization" is at the heart of a well-established sociological research tradition in France and was further developed in *Actors and Systems* (Crozier and Friedberg 1980). Crozier's relational and strategic approach to power is widely cited (for instance, Clegg et al. 2009; Scott and Davis 2007). Erhard Friedberg, in his introduction to the new edition in 2010, notes that the book is "a perfect example of the cross-fertilization of American and European

sociological traditions that took place in the 1950s and early 1960s during the post-war reconstruction of sociology in continental Europe” (Friedberg 2010).

The Bureaucratic Phenomenon is therefore a classic in more than one sense. The reciprocal cross-fertilization (importing academic American organizational theories into France and telling Americans about France at the time of De Gaulle) was a good strategy for success. Above all, *The Bureaucratic Phenomenon*’s recipe for making it to classic status was to combine three distinct aspects: two in-depth case studies, a relational theory of power applied to bureaucracies as organizational systems, and a distinctive and path-breaking cultural analysis of France more generally.

INTELLECTUAL BACKGROUND AND CENTRAL ARGUMENTS

According to Pierre Grémion (1992), Crozier’s intellectual background is strongly related to his personal career and the influence of American sociologists (see also Friedberg 2010; Pavé 1994; and Michel Crozier’s memoirs, Crozier 2002, 2004).

After business studies at HEC, the most prestigious business *Grande Ecole* in France, Crozier (1922–2013) received a scholarship in 1947 to study the American labor-union movement in different American states. His first book, *Syndicats et ouvriers d’Amérique*, published in 1951, bears witness to Crozier’s early sympathies with the Parisian left, as well as to the importance of the debates about the roles played by unions in the aftermath of the Second World War (Grémion 1992).⁴ Crozier joined the CNRS (National Center for Scientific Research) in 1952 to produce a Ph.D. on the middle classes (under the supervision of Georges Gurvitch). Crozier carried out “an impressive empirical sociological research project including investigation of a *centre des chèques postaux* in Paris, several factories in the public tobacco monopoly (named SEITA), a bank, some insurance companies and the Ex-Servicemen’s Ministry” (Grémion 1992: 6). These studies became the empirical basis for *The Bureaucratic Phenomenon* as well as two earlier books (*Petits fonctionnaires au travail*, 1955, and *Le monde des employés de bureau*, 1965, published in 1971 in English by Chicago University Press under the title *The World of the Office Workers*). During this period, Crozier participated in several “productivity missions” to the United States.⁵ In 1959, he accepted an invitation to the Center for Advanced Studies in the Behavioral Sciences in Palo Alto where he was exposed to his major intellectual influences (Robert Dahl, Herbert Simon, Alvin Gouldner). Crozier also established contacts with other political scientists (for instance, Albert O. Hirschman), and with observers of French society, such as Stanley Hoffmann. Hoffmann had established a program called “In Search of France,” one that was widely discussed in *The Bureaucratic Phenomenon* (Hoffmann 1963; see Hoffmann 1994).

According to Grémion (1992), two particular influences of US-based academics can be identified in Crozier's work. The first main influence is Gouldner's *Patterns of Industrial Bureaucracy*. Gouldner explored repeated processes of rationalization in a small gypsum factory and identified 'the social processes leading to different degrees of bureaucratization' (Gouldner 1954: 27). Five core elements of Gouldner's approach can be found in Crozier's work, although they have been transformed in *The Bureaucratic Phenomenon* (see Crozier [1964] 2010: 181–3). First, Gouldner linked a case-study perspective to a theoretical framework, behavioral analysis, and generalization about bureaucratization. This was the approach Crozier adopted in *The Bureaucratic Phenomenon*. Second, *Patterns of Industrial Bureaucracy* argued that all bureaucratic roles and organizational structures would not end up alike and that bureaucratic forms are context-related and much more contingent than the rarified legal-rational bureaucracy would let us believe. Third, variations in bureaucratic rationality emerge as bureaucratic rule-making and the process of bureaucratization are context-dependent since all strata and interested groups in a bureaucracy (leaders, managers, supervisors, workers, etc.) do not pursue the same goals and make distinct uses of bureaucratic rules according to their positions and degrees and power: "certainly bureaucracy is a man-made instrument and it will be made by men in proportion to their power in a given situation". This requires "an effort to see how this system of differentially distributed power relates to the growth of bureaucratic organization" (Gouldner 1954: 27). Bureaucratic rules have many "latent functions" (Merton's distinction between manifest and latent) in an organization offering more gains than losses in their collective functioning (for instance allowing bargaining compliance, reducing role tensions). Fourth, Gouldner also theorized a bureaucratic vicious circle (1954: 177–9), namely a self-reinforcing process where low motivation from workers generates more bureaucratic rules by the new manager, leading to closer supervision and more control with positive effects (mitigating some interpersonal tensions) but also negative effects (apathy and further low motivation), thereby generating new rules. Finally, Gouldner provides an original account of environmental uncertainty in organizations, related to times of leadership succession when there are no highly specified rules governing this succession. Gouldner discusses how new leaders will resort to strengthening existing rules and creating new ones to maximize the security of their positions.

The second major influence on the *Bureaucratic Phenomenon* is *Organizations* (1958) by James C. March and Herbert A. Simon (Grémion 1992: 8–10). Crozier called this book "extremely penetrating analyses" (Crozier [1964] 2010: 178). March and Simon's influence can be found in a number of places: the concept of bounded rationality, the emphasis on the processing of information within an organization, and the idea of multiple decentralized "satisfaction-structured" decisions by individuals at each level of the organization. Individuals are provided with various degree of freedom and institutional constraints according to their respective position in the organization (distinct information access, various forms of socialization to norms, etc.).

Further scholarly influences in *The Bureaucratic Phenomenon* can be found. Grémion (1992) notes how Crozier proposed a synthesis between structural

functionalism, decision neo-rationalism, and cultural anthropology. One aspect is the relational conception of power. This relational understanding of power draws on Robert Dahl's approach (1957) to power as a relationship between actors (individual or collective) where "the power of a person A over a person B is the ability of A to obtain that B do something he/[she] would not have done otherwise." Peter M. Blau, although mentioned several times, specifically for *The Dynamics of Bureaucracy* (1955), seems less central to *The Bureaucratic Phenomenon*. However, Blau's *Exchange and Power in Social Life* (1964) also emphasized the interplay between micro-structures composed of individuals and macro-structures that impose constraints on social relations. Another latent influence, according to Grémion (1992: 7), is cultural anthropology, especially the works by Abraham Kardiner, although this is not cited in the book. This line of cultural anthropology suggests that every society is characterized by a particular style of authority which structures societal patterns. Authority-shaping institutions are viewed as functional "security systems," i.e. as generic means of defense and adaptation in the face of the many problems encountered by individuals in societies. This argument echoes Crozier's perspective stating that "the bureaucratic phenomenon has appeared to be an indispensable means of protection which individuals need all the more as they depend most exclusively on brutal and coercive means for coordinating those activities necessary for achieving their ends" (Crozier 2010 [1964]: 294).

The design of *The Bureaucratic Phenomenon* is central to the overall theme of the book. It contains two case studies (parts 1 and 2) of the Clerical Agency (an agency of the postal saving bank that processes applications for short-term loans); and the Industrial Monopoly (the French national manufacturer of cigarettes and matches, SEITA, processing a raw material into an industrial product). The book also contains two more general parts. Part 3 develops an organizational theory perspective on bureaucracy based on power and uncertainty ("bureaucracy as an organizational system"), while part 4 proposes a cultural perspective on France ("bureaucracy as a cultural phenomenon: the French case"). This "clinical approach," as labeled by Crozier, echoes the approaches developed by other classics in organization theory (Blau's *Dynamics of Bureaucracy*, Gouldner's *Patterns of Industrial Bureaucracy*, or Selznick's *TVA and the Grass Roots*). It "bears upon particular cases and generalizes only from an intimate understanding of these cases" (Crozier 2010: 4), that is, what unites these accounts is that they place considerable value on the exploratory phase before developing hypotheses and theory. They develop a bottom-up perspective based on empirical observation and careful factual analysis, and they offer description and ambitious theorizing on bureaucracies.

Turning to the three central themes, the first strong argument of *The Bureaucratic Phenomenon*, drawn from the two case studies, is the idea that bureaucratic rules grant considerable discretion to shop-floor strata and workers. This discretion could not be identified by those who would follow a narrow Weberian perspective where hierarchy would be defined as a compulsory and performative set of rules. The first reason is that, in both organizations, subordinate strata (workers, occupationally based groups, etc.) benefit from legal protection (highly protected job tenure). They are in a good position

to comply with the rules and discharge their obligations by following the instructions issued to them and thus develop their own internal regulations such as, for example, peer-group solidarity. This means that front-line supervisors have very few immediate social control functions and will only exercise those control functions through impersonal rules. In part, they do so in order to avoid intruding into aspects of personal authority.

The second reason is that power in the organizations is not top-down, hierarchy-based, and standardized but expertise-dependent. Power is generated by the control over “the areas of uncertainty.” In the Industrial Monopoly case, the maintenance workers control the uncertainty caused by the recurrent breakdowns of machines and are powerful because they make other groups dependent on their expertise and their problem-solving capacities. These maintenance workers and their superior, the plant’s technical engineer, have high prestige and morale. The adoption of impersonal rules then does not routinize and standardize organizational life. It does not prevent unanticipated uncertainties, unforeseen events, and contingencies: these uncertainties give personal power to the “problem solvers” or the “strategic resources-owners.” They build parallel power relations that make other groups dependent on their discretionary actions. For Crozier, understanding the bureaucratic phenomenon does not require an examination of rules but the identification of the main issues and mechanisms at stake within the complex network of intra-organizational relations where “power” is constructed. Conflicts between groups within a bureaucracy help to identify the “areas of uncertainty” where groups struggle and try to defend and impose, with more or less success, their indispensability and their related discretionary power. Crozier explores this relational theory of power further in the book co-authored with Erhard Friedberg, *Actors and Systems: The Politics of Collective Action* (1977 for the French version; 1981 for the English). That book systematizes the idea of organizations as “fields of interdependent actors, yet partially autonomous actors, who continually (re)construct the rules of their coordination and cooperation” (Friedberg 2010: p. xvi). In itself, *The Bureaucratic Phenomenon* offers a fascinating analysis of the detailed political tactics and strategies through which expert groups strive to establish effective closure of, and control over, “jurisdictional domains” of expert technique and practice relevant to changing pattern of socio-technical and economic uncertainties that face organizations (Reed 1996: 578).

The second theme emerges from the analysis of the relationship between the strata. It reveals that bureaucracies institutionalize distance between the lowest and the highest ranks. Both case studies point to power relations between various groups of workers and employees, supervisors, managers, and management and executive leaders. Different grades avoid each other and regard rule adherence as rational. The isolation of strata is illustrated by the observation that immediate supervisors with information lack the inclination to exert power, while managers and leaders lack the strategic information from the ground for effective decision-making and then only legislate on formal bases, away from where activities take place. Centralization of decision-making appears to be a basic feature of bureaucracies that are best described by a great distance between deciders and occupational groups that hold relevant information. Both

managing and leading groups are reduced to impotence when they want to introduce changes or strengthen their controls. The “making of the bureaucracy” relies on the creation of hierarchical strata, insulated from each other, leading to impersonal decision-making and abstract rules. This reinforces workers’ and employees’ frustration and feelings that no one is interested in them. Formal hierarchies are powerless. Bureaucracies are not self-correcting as the isolation among strata and their structural patterns result in self-reinforcing behaviors that favor conformity to abstract rules. Change, therefore, cannot be incremental and gradual but can only emerge in response to crises. Based on Gouldner’s argument about succession, Crozier suggests that change in bureaucracy will only be introduced through crisis and repeated moments of reorganization where leaders will manipulate abstract rules and adjust the organization to changes in its environment.

Crozier also develops an original perspective that relates the bureaucratic patterns of organization he observes to the social and cultural setting in which it functions (e.g. French society). The third key contribution of this book therefore is to consider bureaucracy not only as a universal phenomenon, but as one that is embedded in the distinct cultural and institutional patterns of specific states and societies. Crozier’s work can be seen as an early contribution to the study of administrative behaviors that reflect deeply embedded cultural structures. The main issue is “the way authority relationships and conflict situations are handled in a given society” (1964: 237). Two features are identified by Crozier as specifically French cultural traits. The first is the isolation of the strata and their struggle for (maintaining) privileges (Crozier 1964: 214). The French system organizes strict equality among members of the same strata and provides protection at this horizontal level, but this leads to difficulties in admitting new members, in negative solidarity between peer-groups (against authority), and in the weakness of intergroup and intra-organizational relations. This results in a characterization of French group life as marked by the avoidance of face-to-face relationships (*l’horreur du face à face*) and the avoidance of open conflicts. There is a high value, for individuals and groups, in preserving their personal and collective independence. This results in low cooperation and low participation in collective action but also in fear of overlapping responsibilities.

This leads to a second cultural feature that defines a specifically French pattern of authority. Relationship-avoidance mechanisms favor impersonal and distant forms of leadership. This reproduces an inherited absolutist and aristocratic style. Since authority cannot be exerted by cooperation, work-based voluntary team association, and informal communication, the French “art of the state” relies on written rules, formal hierarchy, and deference to status and rank. French sociologist Philippe d’Iribarne later called this the “logic of honour” (d’Iribarne 1989).

Crozier applies his model to different institutional systems in French society: the educational system, the industrial relations system, the “politico-administrative” system, and the colonial system. In each of these domains, Crozier characterizes the same key themes he had identified in the bureaucratic system. Analyzing the French educational system, for instance, he concentrates on a few traits that resemble those observed

for the bureaucratic apparatus: the high centralization and impersonality of the organizational structure; the wide gap between the teacher and the student; the abstract content of the teaching divorced from real-life requirements; the overpriority given to selection purposes over functional training purposes. In the area of industrial relations, Crozier points out the difficulties of direct communication between workers and their unions, and between workers and management. He diagnoses the existence of three parallel, closely interdependent but very far apart operational subsystems within the politico-administrative system: the administrative subsystem, the deliberative but “overly esoteric” political and policy-making subsystem that fails to make decisions and to involve all groups in problem-solving, and what he terms the primitive and rough “revolutionary grievance-settling” subsystem.

These three central themes reflect the richness of *The Bureaucratic Phenomenon*. Its richness allowed it to appeal to different audiences and distinct intellectual concerns (empirical case studies on bureaucracy, theoretical piece on bureaucracy as an organization, cultural views on France), thereby further advancing its chances becoming a classic.

THE RELATIONAL CONCEPTION OF POWER AND ITS CHALLENGES

The Bureaucratic Phenomenon is usually classified as a structural-functionalist work of the late 1950s (Reed 2009). Alternatively, it has been associated with late 1970s studies that approach power with a “strategic contingency perspective” (Scott and Davis 2007: 190–3; Clegg et al. 2009: 124). Actors and groups within any organization cope with important sources of uncertainty (Hickson et al. 1971). The “contingency” perspective enjoyed widespread currency (Child 1972), as did the so-called “resource dependency” perspective, developed by Pfeffer and Salancik (1974, 1978). To quote Clegg et al. (2009: 127), they all explore the question “who has what resources?” This question has remained at the heart of the study of bureaucracy (Clegg et al. 2009; Scott and Davis 2007: 183–219; see also Lukes 1974). However, the original and seminal orientation taken by Crozier’s perspective is also acknowledged: “Power was conceived not as reproducing normal authority and hierarchy but as undermining it, never more famously than in Crozier’s (1964) maintenance workers, lowly individuals in the pecking order, who were supposed to have organizational power because of their control over uncertainty!” (Clegg et al. 2009: 134).

Crozier’s perspective was established in the French context where Crozier created the group (later Centre) for Sociology of Organizations.⁶ This group attracted a high-powered group of researchers that explored change in the French public bureaucracy (Jean-Pierre Worms, Pierre Grémion, Catherine Grémion, Jean-Claude Thoëmig, Erhard Friedberg). Crozier and Friedberg further developed the relational theory of

power and the idea of strategic bureaucratic organization around the concepts of “system” and “environment.” The book *L'acteur et le système* (Crozier and Friedberg 1977; in English, *Actors and Systems*, 1980) constituted the theoretical manifesto of this French school of sociology of organizations. Organizations are understood as “fields of strategic interaction between individual and collective actors who have to manage their interdependence around the control of uncertainties affecting their respective capacity of pursuing their interests through their necessary cooperation around collective goals” (Friedberg 2010: p. xvi). They are “both means and constraints for collective action, they are an instrument for taming power and bargaining relations, they are a social construct generated by a social and political process” (Friedberg 2010: p. xvi).

At the same time, a number of criticisms have become increasingly prominent. They come in three varieties. The first set of criticisms addressed the structural-functionalist dimension of early organizational sociologists of bureaucracies. *The Bureaucratic Phenomenon* constitutes a landmark in the institutionalization of the structural-functionalist movement. In this structural-functionalist perspective, subparts, strata, and groups are assumed a priori to form part of a supposedly homogeneous “system” or an equilibrium whose functions and dysfunctions favor maintenance and integration. However, Crozier offered a “strategic” and power-centered version of the structural-functionalist approach: “Instead of describing bureaucratic dysfunctions merely as the automatic consequence of the ordering of human and technical factors necessary for achieving a superior form of rationality, we have tried to understand them as the elements of more complex equilibria affecting the patterns of action, the power relationships and the basic personality traits characteristic of the cultural and of the institutional systems of a given society” (Crozier 2010 [1964]: 294). Although Crozier’s approach was a “strategic” perspective that proceeded from individuals to the system or the culture, this system-centered view was criticized for reifying behaviors, interactions, practices, and roles (Silverman 1970; Ackroyd 1992; Perry 1979; Reed 2009). Their meanings are always accounted for, “irrespective of what these unitary actors do,” since “what they do does not enter into the theory” (Clegg et al. 2009: 124). They are then incorporated into a functionally cohesive system where even dysfunctions help maintain the system. In *The Bureaucratic Phenomenon*, the continuous reinforcement of organizational rules as a defensive strategy for leaders, the embedded vicious bureaucratic circles in the organizations, and the systematic search for central dysfunctions are used to confirm the structural-functionalist logic (see Grémion’s argument about the equivalence between neurosis in cultural anthropology and malfunctioning in Crozier’s theory: 1992: 10). This structural-functionalism became more important in contingency and resources-oriented approaches that developed in the 1970s. By contrast, it will decrease in the later theoretical perspective developed in *Actors and Systems* (Crozier and Friedberg 1977, 1980). They propose a “dialectical reconciliation” (Astley and Van de Ven 1983) between system-structural view and strategic approach. This later work somehow transformed how *The Bureaucratic Phenomenon* was received and interpreted.

The second type of criticisms addressed the “reductionist” aspects in Crozier’s work. The empirical micro-perspectives developed by Blau, Gouldner, Selznick, or Crozier in the 1950s were behavior-, decision-, and practices-centered and exemplified by the organizational framework put forward by March and Simon (1958). Their approach sought to explore the universal bureaucratic phenomenon and its specificities as an institution. Later theories of the 1970s that were informed by these earlier works lost this focus: they systematized more generic system-centered approaches and aimed to examine the organizational phenomenon in its multiple forms. These decision-making and behavioralist perspectives disaggregated the classic objects of political science (the state, the bureaucracy, the government, politics) in favor of “reductionist” views that concentrated on processes, flows, and a dominant view of a fragmented power (for this argument in comparative perspective for the United States and France, see Bezes and Pierru 2012). This critique of the disappearance of state and bureaucracy studies in favor of generic decision-making processes and organization-centered analyses was developed by Theodore J. Lowi. Lowi pointed to Herbert A. Simon as the main culprit for this disappearance, but it is undoubtedly the case that his critique applied to all those endorsing a relational perspective on power:

The decline and transfiguration of public administration gives us the key to public policy. Traditional public administration was almost driven out of the APSA by the work of a single, diabolical mind, that of Herbert Simon. Simon transformed the field by lowering the discourse. He reduced the bureaucratic phenomenon to the smallest possible unit, the decision, and introduced rationality to tie decisions to a system not to any system but to an economic system. His doctorate was in political science; his Nobel award was in Economics. . . . It is in this context that modern public policy became a hegemonic subdiscipline in political science, overshadowing behavioralism itself. (Lowi 1992: 4; see Simon’s reply, 1993)

This criticism was also articulated in France. *Actors and Systems* was attacked by political scientists as a “non-political theory of the political” which lacked any reference to questions of general interest, legitimization, or relations between power and domination (Leca and Jobert 1980). Instead, Crozier and colleagues were criticized for their generic and ad hoc sets of interrelated games within “concrete action systems” where actors are implicated in different types and different layers of games with various margins of liberty and zones of uncertainty. For Leca and Jobert, “Crozier’s sociology corresponds ‘to the crisis of the State’ and is ‘the sociology of an (imaginary) withering away of the State’ where there is no political center and where the political system has no (or lost his) coordination and regulatory role” (1980: 1166–70).

Third, *The Bureaucratic Phenomenon* could not explain changes in bureaucracy that occurred under the label of “New Public Management” (NPM) during the 1980s and 1990s in particular (Schwartz 1994; Christensen and Lægreid 2001; Suleiman 2003; Bezes 2009; Pollitt and Bouckaert 2001/2011). Regardless of whether NPM was seen as a set of contradictory doctrines (Hood 1991), an ideology (Pollitt 1990), or as a combination of new ideas and instruments (Aucoin 1990), organizational changes were widely

regarded as a systemic shift in values, rules, and practices away from the bureaucratic model. Instead of exploring the “bureaucratic phenomenon,” attention shifted to “neo-managerial” administration, “new” kinds of organizational forms and administrative techniques (such as agencies, performance management systems, contract, flexible employment). Interest in informal practices and parallel power relationships declined. If NPM techniques were said to bring new forms of domination and clear redistribution of power between strata, sociologists and political scientists were eager to explore the new formal model and its empirical variations. Crozier’s approach that focused on disseminated power within sets of relationships appeared of limited value when NPM reforms appeared to bring with them new rules and a large redistribution of power.

These criticisms and the changing administrative reality have shaped later developments in the study of bureaucracy. Four reorientations can be identified that directly challenge the “Simon/Blau/Gouldner/Crozier project” and its many variations. Crozier defended an approach that viewed “power as strategy” with a context-related and somewhat “depoliticized” conception of power; he also put emphasis on “bureaucracy as an organization,” made of interdependencies, interpersonal relationships, and interest-based strategies around rules. It is therefore not surprising that scholarly critique has developed on a challenge to these two lines. Further developments of research on bureaucracies can be categorized in two dimensions whether critiques focus on the relational conception of power or on the bureaucracy as an organization; whether they insist more on the material and formal forces at stake in a bureaucracy or on its cultural and social dimensions. Mapping further theories of bureaucracy creates the typology shown in Table 12.1.

Table 12.1. Typology of research in bureaucracy: culture and power

	Valuing Material and Formal Forces in a Bureaucracy (Interests, Rules, Capacity)	Valuing Cultural and Social Forces in a Bureaucracy
Micro-perspective— Revisiting the relational conception of power	From interests in organizations and context to preferences and utility functions Rational choice theory	From a relational theory of power based on ad hoc interactions and interdependence to a relational theory of power involving rulers and ruled and aimed at observing the technologies of power and government Foucauldian perspectives
Macro-perspective— Revisiting bureaucracy as an organization	From bureaucracy as an <i>organization</i> to bureaucracy as an <i>institution</i> with various patterns of formal rules, capacities, and autonomy Historical neo-institutionalism	From bureaucracy as an organization to bureaucracy as cultural and normative institution with formal structures, rationalized myths, categories and norms. Sociological or organizational neo-institutionalism

The relational perspective on power has been challenged in two ways. One is the influence of rational choice influenced accounts. Since the 1970s, rational choice has been said to have launched the “modern advent of the scientific study of bureaucracy” (Krause and Meier 2003: 7). In a section in their *Politics, Policy and Organizations*, Krause and Meier state that

in his classic treatise *The Bureaucratic Phenomenon*, Crozier (1964) arrived at conclusions similar to Tullock’s [*The Politics of Bureaucracy*, 1965] ... Both Tullock and Crozier separately conclude that bureaucratic agencies are sufficiently ossified that they require abolishment. This lack of responsiveness to constituency demands, whether it be from elected officials, citizens, or pressure groups, is attributable to bureaucratic inertia that occurs within the organizational setting. (2003: 6)

It is not surprising that John Brehm and Scott Gates’s *Working, Shirking and Sabotage* (1997) also acknowledges the value of sociological and organizational studies on “bureaucratic discretion” (Simon, Kaufman, Lipsky, Wilson, etc.) before presenting rational choice models (public choice, transaction costs, principal-agent, delegation theory) (1997: 1–23). The two authors insist that the two literatures need to be integrated (p. 21) in order to develop a better understanding of the problem of bureaucratic control. They put forward their “enhanced principal agency” model in order to specify the relationship between a single supervisor and her bureaucratic agent. This world where actors’ interests are reduced to generic “preferences” under constraints sometimes claimed to be related to the early sociologists of bureaucracy. However, it is crucial to note that Simon responded to Lowi’s critique by stressing that there was hardly any common ground between behavioralists and those advocating models built on economic rationality (Simon 1993).

On the other hand, Crozier’s relational approach of power has also been challenged by Foucault’s capillary theory of power (Miller and O’Leary 1987; Miller and Rose 2008). Of course, some connections with Crozier can be made. For example, Michel Foucault analyzes power and politics without going back to the state as locus, origin, or outcome (Foucault 2004: 78–9) and links knowledge-expertise with power (the inextricable couple *savoir/pouvoir* in Foucault’s works). However, the focus moves away from exploring the sources of a contingent and relational power in bureaucracies (expertise, knowledge, uncertainty) toward an approach which concentrates on the technologies of power and government in social and cultural contexts. This produces individuals as subjects by simultaneously regulating and disciplining their conduct. While Foucault still considers power relationally, he explores its structuring, framing, and domination effects on individuals in his “Governmentality” project (Foucault 1978, 2004, 2009). The governmentalization of the state is developed through various instrumentalizing forms of authority with major effects on individuals: through bureaucratization, professionalization, or scientificization, power becomes inextricably linked to the authority of expertise (Miller and Rose 2008). Governmentality, as presented in Foucault’s

lectures at the Collège de France (Foucault 2004) and by his followers, emphasized modern states' governing technologies from the eighteenth to the twentieth century. More recent studies examine new governing technologies in neo-managerial and neo-liberal bureaucracies, such as the "audit explosion" (Power 1999) and the diffusion of steering techniques in bureaucracies (new accounting systems, benchmarking, evaluation, targets, indicators, rankings, contracts, etc.). These technologies are said to have important moral dimensions "constructing the autonomy of self" and the "governable person" (Miller and O'Leary 1987).

The organizational dimension of bureaucracy (the idea of "strategic bureaucratic organization") has also been challenged and this has followed two distinct pathways where one observes a re-evaluation of formal and cultural structures as objects of inquiry.

The first venue has been both a continuation of and a departure from the initial Simon/March project. The "new institutionalism" has continued an interest in intra-organizational and decision-making-centered perspectives. However, the literature has emphasized "organizations as institutions," which means that organizations are regarded as normative and cultural-cognitive systems providing stability and meaning to social life (Meyer and Rowan 1977; March and Olsen 1989; DiMaggio and Powell 1983). These studies propose a "bringing rules back in."

Three arguments are usually made to justify this shift in attention. The first is that rules and structures are viewed as "rationalized myths" that establish procedures, understandings, and meanings and constitute collective beliefs, thereby creating legitimacy. These norms of rationality are the causal vehicles of bureaucratization (Meyer and Rowan 1977) and are proliferating globally. Furthermore, formal structures prescribe actions for actors inside and outside the organization, shape, categorize, and classify how actors see the world, perceive the problems they face, and define their identities. Understanding organizations then requires paying attention to the "logic of appropriateness" (March and Olsen 1989) inside the organization but also in institutional environments. The third is that research has to refocus on analyzing the creation and diffusion of institutional myths in organizational fields (DiMaggio and Powell, 1983) because organizations are assumed to become more alike in their structural features due to the strength of isomorphic effects. Tolbert and Zucker (1983), for instance, examined how many cities adopted civil service reforms from 1880 to 1930 in response to increasing normative pressures, while more recent scholars explore the circulation and institutionalization of management knowledge, standards, or management reform myths, diffused by consultancy firms (Saint-Martin 2001; Sahlin-Andersson and Engwall 2002).

The second research agenda also insists on paying a renewed attention to the organizational dimension of bureaucracies but does so under the umbrella of historical institutionalism. By "bringing the state back in," Theda Skocpol (1985) criticized pluralist studies of the political system, the "bureaucratic politics" and the policy-making process (notably Graham Allison's *Essence of Decision* and Halperin's *Bureaucratic Politics and Foreign Policy*) that includes Crozier's strategic and relational conception of bureaucracy. These approaches are said to over-fragment the state, viewing

government as an arena where interest groups, social movements, and public organizations are treated individually as competing groups around resources. Pluralism (a movement Crozier could be said to be part of) is then viewed as ignoring the specific role, rules, capacities, and autonomy of bureaucracies, a critique it shares with neo-Marxists who are accused of economic reductionism in their emphasis on superstructures and class struggles. The claim is to build an historical neo-Weberian comparative sociology of the state (see Katznelson 1992; Steinmetz 2005; Bezes and Pierru 2012). State capacities are systematically investigated through the identification of specific organizational structures within public policies. The analysis of these formal structures emerges from a neo-Weberian understanding of the comparative sociology of the state. The state's role, namely its rules, resources, information, and expertise, and its autonomy from societal actors matters (for instance, Skocpol and Finegold 1982; Skocpol 1985, 1992; Hacker 2002). Intra-relational approaches of bureaucracy like Crozier's where the administration is decentered to access informal practices and individual games and behaviors is replaced by neo-institutionalist perspectives that favor interrelational analyses where bureaucracies are conceptualized as collective actors with (various degrees of) autonomy due to their formal capacities and to their cognitive and intellectual influences on policy-making.

BACK TO CLASSICS? MANY ROUTES BACK TO *THE BUREAUCRATIC PHENOMENON*

Given these debates and challenges, how can a perspective informed by Crozier's work contribute to contemporary research on bureaucracy? Four pathways can be noted in particular.

The first pathway is about the idea of an ongoing struggle between expert power and control within public administrations. *The Bureaucratic Phenomenon* suggests that one should pay attention to the ways in which the bases of expert power are constructed, defended, and also reshaped. Following Crozier's argument, expert power and dynamics of control constitute an unstable outcome of many interactions and constraints. To quote Reed, "Crozier's analysis [also] pinpoints the explanatory significance of the inter-organizational or institutional level of analysis in accounting for the restructuring of the expert division of labor in (post)modern societies and its impact on organizational control systems" (Reed 1996: 578). Accordingly, a number of accounts have explored the changing relationship between expertise and control in contemporary administration. One dominant view of these changes has been developed by the literature on public professions (for a synthesis, Bezes and Demazière 2012). NPM reforms are then interpreted as an attack by "new strata of managers" against professional groups and their expertise within professional bureaucracies (Mintzberg 1979). These groups organize and operate the public services and benefit

from highly specialized types of expertise in sectors like healthcare (particularly the hospital sector), justice, the police, education, higher education, or the welfare state (for instance, Dent 2003; Exworthy and Halford 1999). Reforms are said to attempt to undermine professionals' power and reinforce the control over their activities and expertise by promoting rationality, commodification, standardization, and accountability. These dynamics often blend with the advance of neoliberalism that is associated with market logics, individualization, performance review, and choice by "consumer-clients." These developments undermine the main components of professionalism: their claim to specialized knowledge, their autonomy, their discretionary judgment on the job, and their occupational effectiveness and costs. Contemporary reforms then appear like strategies aimed at reinforcing managerial control over powerful professional groups and at reducing the "sources of uncertainty" they have to build and reproduce their power (Ackroyd et al. 1989; Kirkpatrick et al. 2009).

At the same time, however, the growth in "regulation inside government" (Hood et al. 1999) and the renewal of managerial forms of control have favored the development of new experts within the state. Inspectors, auditors, specialized experts in re-engineering, quality management, or IT but also consultants from outside the state (Saint-Martin 2001) have benefitted from the "audit explosion" (Power 1994), from the development of new technologies for "governing at a distance" by performance management, and by an increasing number of control bureaucracies (Hood et al. 1999). They certainly form a new stratum of personnel and experts in charge of management and managerial control (Diefenbach 2009). These elements confirm the centrality of the ongoing power struggles over the control of expertise within the state. It therefore illustrates how Crozier's analysis of the dynamics of expert power and control remains crucial (Reed 1996). A "Crozierian perspective" would certainly not take for granted the opposition between professionalism and managerialism: it would investigate the variety of interactions that professionals engage in with other groups within the organizations (peers, subordinates, managers, etc.) (for a pledge for more complex narratives to understand the redistribution of power between managers and professionals, Farrell and Morris 2003; see Bezes and Demazière, 2012). More generally, Crozier's inheritance is to pay attention to the changing relationships between authority and expertise. While the struggle between managers versus professions is an obvious place to start, the growing importance of "policy bureaucracies" (Page and Jenkins 2005; Page 2012) in contemporary contexts where the state is ever-growingly a "policy state" (Orren and Skowronek 2011) is another field of exploration. For example, Edward C. Page suggests that the ordinary work of "policy bureaucracy" should be taken more seriously by analysing the specific activities of middle-ranking officials "who helped originate policies" (Page and Jenkins 2005). Referring to Gouldner (1954) and Crozier (1964), Page explores "policy as a bureaucratic activity" because it helps revisit the struggle between expertise (held by the specialized policy professionals) and hierarchy (the—often large—span of control exerted by managers and top executive bureaucrats and leaders) and the question of division of labor. If, as suggested by Orren

and Skowronek, “policymaking has become the dominant driving principle—la raison d’être—of contemporary [American] government in every aspect—institutional, cultural, behavioral” (2011: 3) because the state has become more than ever a provider of a huge number of inherited and new public policies to citizens and interest groups, this classic trade-off between hierarchy (formal rules and discipline) and expertise (giving power to policy bureaucrats “who know more about the particular subject in hand than the people supposed to be in charge,” Page and Jenkins 2005: p. vii) is even more central in the contemporary period. The research question is a classic question in the tradition of Gouldner/Crozier: “how can expertise be brought to bear within a hierarchical system in which commands can legitimately only come from superiors?” (Page and Jenkins 2005: 13). Page shows that policy tasks do matter and give discretionary powers (“routinizing, regularizing and adjusting,” Page 2012: 168–72).

The second pathway that continues Crozier’s (but also Gouldner’s or Blau’s) project is rooted in the analysis of the effects of performance management tools. While NPM was (and still can be) said to provide an alternative to bureaucracies, many empirical studies on the development of performance indicators systems (PIS) have identified new forms of bureaucratization (Bevan and Hood 2006; Moynihan 2008; Brodtkin 2011). The ideas of exploring parallel power relations beneath performance rules and devices or of “vicious bureaucratic circles” regain relevance. Evelyn Z. Brodtkin (2011), for instance, uses Merton’s concept of ‘goal displacement’ and Lipsky’s (1980) “coping mechanisms” to analyze the behaviors of US welfare street-level bureaucrats who develop diverse strategies to adapt to NPM techniques (achieving participation rates, caseload reduction target, etc.). Hood (2011) or Moynihan (2008) insist on the need to explore the contrasting uses of PIS according to the different organizational levels and positions (politicians and top officeholders, street-level bureaucrats, middle managers, actors outside state structures). A clear echo of Crozier’s thoughts is the idea that PIS are not actually governed according to the formal rules of performance management theories or the expected effects of instruments but much more by other parallel and “latent” structuring relationships. New classics in the field resort to a parallel explanatory system of relationships (distinct from Crozier’s uncertainty and conflict avoidance) to understand the “real games” beneath targets and indicators. In Moynihan’s *The Dynamic of Performance Management* (2008), the concept of ambiguity in organizational life is at the center of the many social constructions and interrelations generated by PIS. In Hood’s *The Blame Game* (2011), the central structuring issue for strategies is “the politics of blame avoidance” understood as “a process of interaction among the players in these different worlds, as they combine or conflict and seek to pass the blame onto those in other worlds” (Hood 2011: 22). These perspectives result in the important idea of decoupling (as noted in Gouldner’s or Crozier’s research): organizations often develop double organizational processes with formal organizations and set of rules (developed to increase organizations’ external legitimacy and to abide by international dominant rules) that do not coincide with the parallel relations that are actually in place for effective action, production, and coordinating its activities, and which are often referred to as the “informal organisation” (Brunsson and Olsen 1993: 9; Meyer

and Rowan 1977). PIS are often said to be loosely coupled to the effective steering of public organizations and often phenomena of “dissociation” (which consists for professional groups in adopting PIS formally but without it affecting their actual work: Dent et al. 2004).

Concerning the third pathway, in contrast to Crozier’s emphasis of bureaucracy as an organizational system, the *Bureaucratic Phenomenon* rarely features among those studies that have taken a cultural approach towards the study of public administration (Hood 1998; Du Gay 2005; Hood and Lodge 2006; Schedler and Proeller 2007). Crozier’s “highly speculative” argument (Crozier 2010: 212) was that organizational behaviors and actions (specific relationships between groups and strata, reliance on the authority system) are not only organization-based but also “nationally cultural.” This means that different organizations within the same country are supposed to share the same kind of interpersonal and intergroup relationships since they are all embedded in the same cultural and social system. Crozier emphasized a bottom-up approach that started from identifying relationships within national organizations (case studies), then working out general and abstract schemes from these case studies (the “overtones” of a cultural bureaucratic model, 2010: 212) in order to search for correspondences with other patterns that could be identified in other wider institutional and societal systems in the country.

This approach has not been really further explored or successfully implemented. Geert Hofstede’s study (1980/2001) seeks to grasp the specificities of national cultures. Among other cultural features (like individualism/collectivism, masculinity/femininity, long-term/short-term orientations), he includes “power distance” and “uncertainty avoidance” as decisive traits (quoting Crozier 1964: 84 and 147, among many others) to be measured. However, Hofstede’s perspective is not bottom-up or practices-centered but quantitative (based on his IBM data from the 1970s) and macro. In the field of public administration, recent perspectives emerge from different epistemological influences. Hood (1998) and Hood and Lodge (2006), for instance, apply grid-group cultural theory (Douglas 1986; Thompson et al. 1990) but do not cite Crozier. Diverse cultural patterns—fatalist, hierarchist, individualist, and egalitarian, as reflected in different public service bargains (Hood and Lodge 2006)—are said to combine a long-term perspective with more contemporary reform themes, thereby leading to hybridization. National stereotypes are found to be of limited value in the study of different public service bargains and their change over time.

There have not been many empirical studies that seek to explore how organizational practices in bureaucracies are embedded in and framed through cultural normative and cognitive structures. In France, paradoxically, the cultural perspective would not be referred to the Crozier school but much more to the strong influence, in sociology and political science, of Pierre Bourdieu and Michel Foucault. Jean-Michel Eymeri’s work (2001) about the making of the French administrative elites (the “guardians of the state”) within the National School of Administration (ENA), and later on in ministries, follows this orientation and emphasizes French “cultural” features: the

importance of socialization processes, the strong logic of appropriateness, or the making of a state culture with great attention paid to clarity of thought and capacity for abstraction and synthesis. Other important studies, focused on street-level bureaucrats in the French context, show interest in “bureaucratic culture” (Dubois 1999; Weller 1999; Spire 2005) but not as a national pattern. In the United States, Steven Maynard-Moody and Michael Musheno (2003) as well as Celeste Watkins-Hayes (2009) illustrate street-level bureaucrats’ professional identities and behaviors (police officers and teachers for the former, caseworkers in welfare state institutions for the latter) in relation to users/citizens: these identities in the making are seen as active elements in forming and reforming organizational culture. This reflects existing administrative cultures, but leads to a reconstitution at the same time. These studies do not address the issue of an “American” administrative culture; however, Watkins-Hayes (2009) considers some specific “American” issues, such as racialized professional identities for African-American or Latino caseworkers. Frank Dobbin’s *Forging Industrial Policy* (1994) could be seen as a paradigmatic example of “national cultural perspective.” Comparing the evolution of railway policy in the United States, France, and Britain in the nineteenth century, Dobbin establishes “national policy logics” as “institutionalized meaning systems.”

Culture-oriented perspectives therefore have clearly a role to play in the study of public administration and public policy. However, Crozier’s work plays only a limited role in contemporary discussions. This remarkable absence is arguably due to the fact that Crozier was developing a strategic and system-oriented perspective and a cultural approach that focuses on France specifically. In addition, Crozier’s image of France as a stalled society with a blocked state has been challenged as being too schematic and mechanistic as well as outdated (see Lamont’s discussion, 1992: 49–54). Whether the French preference to avoid face-to-face relationships across hierarchical levels, to prefer personal relationships to protect them from higher strata, and to avoid collective participation is still pertinent remains an open question.

The fourth pathway is related to Crozier’s argument about bureaucratic change. Crozier, as noted, emphasized systematic blockages within the bureaucratic system that resulted in only periodic and radical periods of change, that originated in leaders’ initiatives. The blockages were due to weak initiatives for lower echelons, limited adjustment to changing environments, a reliance on crisis as a dominant mode for change, and thus gave justification for top–down interventions:

From the above analysis, it emerges that change in a bureaucratic organization must come from the top down and must be universalistic, i.e. encompass the whole organization *en bloc*. Change will not come gradually on a piecemeal basis. It will wait until a serious question pertaining to an important dysfunction can be raised. Then it will be argued about and decided upon at the higher level and applied to the whole organization, even to areas where dysfunctions are not seriously felt. Only in this way can the impersonality system be safeguarded. (Crozier [1964] 2010: 196)

On the one hand, a long list of studies have documented the importance of “external” environmental factors for change, and of crisis triggering administrative reforms (see the theoretical framework by Pollitt and Bouckaert 2011 or the “transformative” approach by Christensen and Lægreid 2001). The impact of economic and fiscal crises, computerization and IT developments, European restructuring and pressures from local governments, political dissatisfaction with civil services or social demands for greater social equity, democratization, or empowerment (on this, Wise 2002) have been identified as crucial components in transformations processes of bureaucracies. Attention has been paid to the role of purposive and proactive political leaders and policy entrepreneurs in administrative reforms (Savoie 1994; Arnold 1998; Aberbach and Christensen 2001; Bezes 2001; Barzelay and Gallego, 2010).

On the other hand, more recent studies have also suggested how much an exclusive focus on political and administrative leaders as well as on radical episodes can be misleading and too simplistic. This is where Crozier’s argument appears to be outdated. Change, accordingly, should not be seen as a short-term event, but one that emerges from long-term dynamics of change (Pierson 2004) that involve “lateral,” “low-profile” (Bezes 2010), or gradual (Thelen 2003) forms of institutional change. Bureaucratic systems do not change *en bloc*, but develop through the interaction of diverse, separate, but interacting streams (Bezes and Lodge 2007). With diverse research strategies, large comparative approaches for detecting the transformation of bureaucracies have been developed, articulating politico-administrative regimes, reforms, trajectories, and possible convergence/divergence schemes (Pollitt and Bouckaert 2011; Christensen and Lægreid 2001, 2007a; Capano 2003; Bezes 2009; Ongaro 2009; Bezes and Parrado 2013). With different theoretical frames, these studies address the issue of how far administrative reforms have challenged the inherited institutions in the long term and suggest paying attention to more complex definitions of a “national trajectory of reforms” (Bezes and Parrado 2013). Here, Crozier’s skepticism toward incremental and piecemeal kind of changes has been challenged.

CONCLUSION

The Bureaucratic Phenomenon, as noted, represents a classic for its diverse contributions to sociology, organization theory, and its analysis of France and its institutions.

Main themes in the contemporary study of bureaucracy suggest that bureaucracy is alive and well, regardless of reforms and development of “non-hierarchical” modes of governance. Some authors have noted how hierarchy remains a dominant mode (Courpasson and Clegg 2006; Diefenbach, and Sillince 2011). Others stress the emergence of a neo-Weberian (Pollitt and Bouckaert 2011), or post-NPM (Christensen and Lægreid 2007b), or neoliberal (Hibou 2012) state where bureaucracies still play a role. Besides

other scholars defend the role of bureaucracy as a central part in the development of the “quality of government” agenda (Olsen 2006; Du Gay 2005; Rothstein and Teorell 2008).

As bureaucracy continues to play such a central role, Crozier’s work deserves continuing attention. The relevance of the *Bureaucratic Phenomenon* is somewhat ironic as, for him, bureaucracy was not an institution that deserved to be normatively supported. In his later works, Crozier criticized bureaucracy as a cultural and organizational phenomenon that contributed to the weakening of the governability of Western democracies (see, for instance, Crozier’s report for the Trilateral Commission, 1975: 16–18).

One further durable contribution is, however, methodological. It seems “good social science as usual” to build on Crozier’s epistemological and empirical foundations to explore the central issue of power location and distribution in public organizations. Power is not reflected in organization charts, administrative reform legislations, or the instruction sheets of performance management systems. This basic insight often gets lost among those who still consider formal structures and rules to be the alpha and omega of research. In an age of the niche-improving 8,000-word article, Crozier’s well-informed, in-depth, and rich case studies offer an important counter-example on how to conduct meaningful research.

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NOTES

1. Arguably, these arguments are an unfair criticism of Weber, as they solely concentrate on the ideal type. They thereby do not pay sufficient attention to Weber’s historical discussion that analyzed bureaucracies in their specific context, social practices, and cultural meanings (e.g. Wrong 1970: 51–8). Put differently, “The puzzle is that in the development of American inspired bureaucratic theory and the contingency theory which succeeded it, it was the least original aspects of Weber’s work which received most attention—that is, his inventory of bureaucratic characteristics” (Perry 1992: 87).
2. The data and tables were kindly provided by Etienne Ollion and Andrew Abbott. For details about their analysis of the reception of French sociologists in the USA, see Ollion and Abbott 2013.
3. Data by Etienne Ollion.
4. For the evolution of his intellectual and political position from leftist groups of intellectuals around Sartre and his journal *Les Temps Modernes* to reformist intellectuals around the journal *Esprit*, see Grémion 1992: 13–15.
5. On these missions and the import of some know-how of the American social psychology in the making of Crozier’s distinctive sociology, see Paulange-Mirovic 2013a.
6. On this making of a Sociology of Organizations in France, see Paulange-Mirovic 2013b.

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CHAPTER 13

THEODORE J. LOWI, “AMERICAN BUSINESS, PUBLIC POLICY, CASE STUDIES AND POLITICAL THEORY”

MICHAEL MORAN

A little bit of data can go a long way.

(Lowi 1970: 316)

WHAT IS A CLASSIC?

THE simple question with which I begin quickly leads us to a problem in evaluating works conventionally assigned the status of a “classic.” Classic status can mean two very different things: that a work endures beyond its time, exercising a continuing influence over the way we think, investigate, and write; or that it belongs to a codified, classicized tradition, enjoying the authority of codification but incapable of providing us with a contemporary model of thought, investigation, or exposition. On the former, here is Sainte-Beuve’s famous account:

A classic, according to the usual definition, is an old author canonised by admiration, and an authority in his particular style . . . The idea of a classic implies something that has continuous and continuance, and which produces unity and tradition, fashions and transmits itself, and endures. (Sainte-Beuve 1857/1914)

And here is a very different account from Rosen’s study of “the classical style”:

The principles of “classical” art were codified (or if you like “classicized”) when the impulse which created it was already dead . . . The conventions must remain

conventional, the forms lose their original significance in order to take on their new responsibility of evoking the past. This process of ossification is a guarantee of respectability. (Rosen 1972: 7 and 460)

The question to answer is thus: in which of these senses is Lowi's essay a "classic"? Is it an enduring contribution to the way we understand policy, continuing to shape how we think, investigate, and write; or is it part of a "classicized" tradition, a way of thinking which enjoys the respectability of ossification? Or, put more simply still: does it continue to speak to us as we struggle with the present, or is it a dead end?

I organize the answer to this question as follows. In the next section I summarize the core argument of the selected text, and try to place it in the body of Lowi's work. The succeeding section estimates the impact of the piece, partly measured in terms of conventional bibliometric measures, partly in terms of more diffuse evidence of its intellectual impact. That provides evidence about what one might call the importance of the piece in the intellectual history of the discipline of policy studies, but it does not address the core question about the nature of its "classical" status: a piece of work can be widely cited and be influential even though it is a dead end. The final section of the chapter, therefore, is where the critical opening question about classical status is directly addressed.

THEORY, DATA, AND POLICY

One of the striking features of "American Business, Public Policy, Case Studies and Political Theory" is its form: a long review article, about a single book, *American Business and Public Policy* (Bauer et al. 1963). Both the form and the date of Lowi's piece (1964) provide critical context for his argument. The mid-1960s were a high point of intellectual conflict about the character of American democracy, a conflict which engrossed American social science. 1956 saw the publication of Wright Mills's *The Power Elite*, with its argument that there were embedded hierarchies of power unifying a single elite that subverted American democracy. Mills is a hovering presence in Lowi's argument. 1958 saw the first sustained riposte from what came to be called the pluralists: Dahl's paper, to which Lowi refers, arguing in essence for a case-based approach to the study of power on the grounds that any statements about power had to be contingent on the setting of particular episodes (Dahl 1958).

This context provides the analytical starting point both for Lowi's review of Bauer et al., and for his critique of the pluralist approach to the analysis of decision and policy. "What," he asks, "do all the case studies, including *American Business and Public Policy*, add up to?" (p. 677). The answer is: not much. "The main trouble with all these approaches is that they do not generate related propositions that can be tested by research and experience" (p. 681). While there is a glancing blow here against elitism ("all these approaches") the main target is pluralism. It has to be the main target because, by the time of Lowi's intervention, case-study-based pluralism was dominant in American political science. As he says: "No theory or approach has ever come closer

to defining and unifying the field of political science than pluralism” (p. 678). And the work he reviews—Bauer et al.—is used as an example of the limits of pluralism: “Bauer, Pool and Dexter discovered that the pluralist model was of little use to them” (p. 685). This is a surprising judgment, for there is no explicit attempt in the book to test pluralism: the word is not even mentioned in the text, and the only reference to Dahl is to one of his early works, a 1950 study of Congress and foreign policy (Bauer et al. 1963: 459). It is true that a fair amount of space in Bauer et al., is given to debunking the group approach as popularized by Truman, but that debunking actually provides evidence supporting the case advanced by Dahl. In *American Business and Public Policy* business groups are marked by nothing like the capacity with which they are endowed in the elitist model: on the contrary, their organization is typically incompetent, and their lobbying intelligence primitive. It is the elected actors—in Congress—who are critical to outcomes, a finding very like that outlined in Dahl’s study of New Haven (1961: 200-14). But for Lowi that is by the way: *American Business and Public Policy* is important because, for all its technical adeptness, it shows the dead-end quality of, especially, pluralism. The multiplication of case studies, even of studies as good as Bauer et al., leads nowhere because “the broad gauged theories of politics are not related, perhaps are not relatable, to observable cases. In general, American political science seems to be subject to a continuing fission of theory and research” (p. 687). And the reason is that neither pluralism nor elitism are truly testable theories: “neither approach is a model. Each is, if anything, a self-validating standpoint” (pp. 685-6). In the case of pluralism the critique anticipates one of the main arguments of *The End of Liberalism* (Lowi 1969): that the constitutional theory of the New Deal deformed constitutional understanding in the United States. Pluralism is a legitimizing ideology for the New Deal. It unified political science because “it fitted so nicely the outlook of the revered Federalist \neq 10 and the observables of the New Deal. Group theory provided a rationale for the weaknesses of political parties and electoral process. It provided an appropriate defence for the particular programs pursued by the New Deal and successive Administrations” (p. 678).

I have spent some time on Lowi’s starting point—the book he reviews—and his objections to pluralism because this starting point helps explain why he came to offer the famous typology of policy arenas. The typology is designed to solve two problems: to provide a way of classifying case studies, and thus to end the parade of cases which have no cumulative impact; and to identify the kinds, and the limits, of the cases employed by, especially, the pluralists. In other words, how to “make all the cases add up.” The starting point is that “the reason for the lack of interesting and non-obvious generalizations from cases and other specific empirical material is clearly that broad-gauged theories of politics are not related, perhaps not relatable to, observable cases” (p. 687). And the reason for this is a kind of Nelsonian blindness which afflicts both the pluralists and the elitists: the former simply cannot see a key insight of Mills—that economic structures systematically influence policy processes—and conversely the elitists (and possibly the pluralists) cannot see that the institutional settings of policy struggles vary, and these variations help shape both processes and outcomes. His

remark of 1970 which heads this chapter—that a little bit of data can go a long way—is not a dismissal of empirical work. It is, rather, a very Millsian dismissal of abstracted empiricism and an insistence that data gathering has to be theory informed. It is critical in embarking on a case study to be able to answer about it: what kind of case are we observing here? The key moment of transition in Lowi's argument, when he introduces his typology, is as follows:

The scheme is based upon the following argument: (1) The types of relationships to be found among people are determined by their expectations—by what they hope to achieve or get from relating to others. (2) In politics, expectations are determined by governmental outputs or policies. (3) Therefore, a political relationship is determined by the type of policy at stake, so that for every type of policy there is likely to be a distinctive type of political relationship. (p. 688)

Step 3 is the most important and leads directly to the famous typology. It allows Lowi to recognize the key insight of Mills's work—that what is at stake in decision is a vital influence over what happens in that decision—and allows him also to press home his objection to the dominant pluralist approach: that it cannot differentiate between the significance of different types of decision, and therefore is faced with the endless accumulation of unrelated case studies.

We are now at the moment of revelation of Lowi's famous scheme. In this statement at least there are three types of policy at stake, and each is marked by a different political (including power) relationship. These three types transcend policy issues which are too ephemeral, or domains like agriculture or education which simply reproduce the perceptions of policy-makers. "Each arena tends to develop its own characteristic political structure, political process, elites and group relations" (p. 689). As Lowi put it eight years later in encapsulating his whole approach, "policies determine politics" (1972: 299). The well-known three (in this version, for differences see later) are:

- *Distributive policies*: the key feature of issues in this arena is that they can be "disaggregated and dispensed unit by small unit" (p. 690). They are typically "pork barrel" policies featuring a high degree of clientelism.
- *Regulatory policies* are "distinguishable from distributive in that in the short run the regulatory decision involves a direct choice as to who will be indulged and who deprived." (pp. 690–1). They involve allocative choices which must necessarily favor some and exclude others: "Not all applicants for a single television channel or an overseas air route can be propitiated" (p. 691). This is what seems to give regulatory *politics* their distinctive feature: they are struggles within sectors, because "Decisions cumulate among all individuals affected by the law in roughly the same way. Since the most stable lines of perceived common impact are the basic sectors of the economy, regulatory decisions are cumulative largely along sectoral lines" (p. 691).

- *Redistributive policies* (of which taxation is an example) “are like regulatory policies in the sense that relations among broad categories of private individuals are involved and, hence, individual decisions must be interrelated. But on all other counts there are great differences in the nature of impact. The categories of impact are much broader, approaching social classes. They are, crudely speaking, haves and have-nots, bigness and smallness, bourgeoisie and proletariat” (p. 691).

It now becomes clear how “policies determine politics.” The character of the arena is determined by the kind of outcomes possible. Since distributive policies can be almost infinitely disaggregated, there is no basis for stable coalitions: it is “every man” (or rather every firm or trade association) for himself. The result is weak collective action, the passing of authority to the relevant Congressional committee and a politics of log rolling. By contrast in regulatory policy, “Because individual regulatory decisions involve direct confrontations of indulged and deprived, the typical political coalition is born of conflict and compromise among tangential interests that usually involve a total sector of the economy” (p. 695). Regulatory politics are the typical domain of what Lowi conceives of as pluralism: a struggle between competing sectoral groups battling over the all or nothing character of regulatory decisions.

When we turn to the redistributive arena, Lowi is more elliptical. He argues that there are few studies of this arena, and so generalizations about it have to be drawn from one extended set of cases, those concerning welfare policy of the 1930s. From these is distilled the argument (summarized in his Table 2, p. 713) as follows: “Issues that involve redistribution cut closer than any others along class lines and activate interests in what are roughly class terms. If there is ever any cohesion within the peak associations, it occurs on redistributive issues, and their rhetoric suggests that they occupy themselves most of the time with these” (p. 708).

Before critically analysing this argument I turn to the question of its influence.

CITATION, GENUFLECTION, AND INFLUENCE

Assessing the influence of a scholarly work like this goes to the heart of the question with which we began—what is a classic? This is because classic status may denote two very different things: an ability continually to shape a field; or a kind of fossilized status, where the piece occupies a respected place in the shrine where we keep the discipline’s household gods, occasionally genuflecting to it (the academic equivalent of genuflection being citation).

We can begin with the most primitive and mechanical forms of impact estimation for scholarly work, citation counts. The limits of citation counts have been very well rehearsed, but this dark art is nevertheless an appropriate starting point for at least getting some sense of the public visibility of work. We can start with the most primitive of

all, the crude measures of frequency of citation. Google Scholar (accessed September 12, 2012) gives 2,228 citations. It is not the most popular of Lowi's works: *The End of Liberalism* (Lowi 1969), in which the typology offered in the 1964 article plays only a subterranean role, is his most cited work, at 3,197. "Four Systems of Policy, Politics and Choice" (1972), which, as I will show, does have an important relationship to his 1964 article, comes in third at 1,180. These three pieces are way ahead in popularity: on Google Lowi's next most popular piece (his book *The Personal President*) has 456 citations.

The uncertain character of citation indices is shown by the rather different findings of the standard academic citation index, that provided by the Web of Knowledge. That shows that up to the moment of access for this piece (again September 12, 2012) Lowi's 1964 paper had just 700 citations recorded. But one advantage of the Web of Knowledge is that it charts the citations by date, so that we can get some sense of the "popularity" over time of the piece. It is acknowledged that a work tends to take time to get itself into the collective disciplinary consciousness, so the years immediately after publication will generally be the thinnest for citation. I have therefore divided the periods into decades (with a minor variation at the beginning, and the necessity of shortening the interval right at the end of the period).

The data do conform to some common expectations: the lowest citations are in the early years (despite the fact that the first interval is slightly longer than all others). The two "leading" decades are when we would have expected the piece to be absorbed into the disciplinary bloodstream. But given the shorter last interval there is no sign of "popularity" dropping off: the annual average count for the final period is almost identical with the count for the most "popular" decades.

It is well known that difficulties exist in making sense of these kinds of citation counts. These problems are well rehearsed in a number of systematic efforts to measure, using citation indices, the impact and influence of scholars within the discipline. The most comprehensive recent study by Masuoka et al. (2007) has the advantage of being "benchmarked" against earlier studies such as those by Klingeman et al. (1989). The studies attempt to use citation analysis to measure the salience of individual

Table 13.1 Web of Knowledge citations of "American Business . . ." 1964–2012

Dates	Number of citations
1964–74	95
1975–84	165
1985–94	165
1995–2004	133
2005–12	142

Source: Calculated from <<http://apps.webofknowledge.com>> accessed Sept. 2012.

researchers, not individual publications. That, from our point of view here, is their main drawback. But they are a model of care in attention to the pitfalls of this kind of analysis, especially of analysing the salience of researchers whose careers covered different time periods. Most important of all, they try to escape the problem that size of potential audience (indicated by the size of subfield) inflates the count derived from citations. They do this by ranking scholars within their subfield. (One further acknowledged problem with this is that these are self-assigned: Lowi might as justifiably be assigned to the American politics group since his material, and indeed core argument, are addressed to the American case.) Nevertheless, the key finding from our point of view is that in the subfield “Public Policy, Administration, Public Law,” he is ranked fourth, behind James Wilson, Robert Axelrod, and Theda Skocpol. Citation analysis is of course a measure of *salience* rather than influence. It certainly cannot take us to the heart of the question with which we began: whether a work is a classic in the sense of continuously fashioning research, or whether it has ossified into respectability. To try to answer that question we have to attempt an estimation which goes beyond metrics. It is here that the overall measure of Lowi’s salience in the work of Masuoka et al. is relevant, because it is plain that over the course of his career Lowi has not considered this paper to be an isolated intervention: it is anticipated in his work before 1964, notably his first major book on the role of the New York mayor, which also appeared in 1964 but which plainly reflects many years of earlier research (Lowi 1964b: 125ff). It is barely mentioned in *The End of Liberalism*, but the great theme of that work—the way the rise of a regulatory state in the New Deal dependent on discretionary decision-making undermined the constitution—certainly echoes the historical treatment of the changing character of the regulatory arena in his 1964 paper. In 1972 Lowi repeated much of his 1964 argument, making explicit something which was largely implicit in that paper, and which ran as a thread through *The End of Liberalism*: “the most significant political fact about government is that government coerces” (p. 299). Each policy arena is thus distinguished by a different object of the state’s coercive influence.

Part of the transmitted influence therefore lies in the way the arguments of the 1964 paper are woven into Lowi’s intellectual career. But his conceptual scheme also has another career, in the form of the reactions of other political scientists. The most striking feature of these is the way they reveal the fluidity of his conceptual boundaries. Hayes (1978) in a review article published near the end of the decade—at a moment in other words when the piece might be expected to have entered the “mainstream” of the literature—documented the extent to which discussion and use of Lowi had been dominated by attempts, in the first place, to elaborate the categories and second, to determine how far they corresponded to actual differences in patterns of politics and outcomes. In this latter connection perhaps the most successful was Wilson’s attempt (1973: 327–46) to integrate Lowi’s notion of arenas with his account of regulatory processes in terms of the distribution of benefits and costs. I return to the significance of Wilson’s analysis later.

The careers of concepts—of the kinds of analytical frameworks offered by Lowi in his paper—are notoriously subject to influences that go beyond the sheer intellectual

power of the original argument. The influence of Dahl's analysis of power owes much to the brilliance of his best-selling college textbook, *Modern Political Analysis*, which with an extraordinary combination of brevity and lucidity distilled his arguments for generations of college students of political science. It has now virtually become a franchise: the 6th edition is co-authored, Dahl and Stinebrickner (2002). Lowi has his own franchise: *American Government: Power and Purpose* is in its 11th full edition, and in addition has a digested 12th edition on the market (Lowi et al. 2009, 2012). That book, unsurprisingly, does indeed use Lowi's classification scheme, though the use is strikingly specialized: it is absorbed into a discussion of techniques of regulatory control. Beyond this, however, it is remarkable how rarely Lowi's scheme is used in the teaching of beginning students—the very point where one might expect a set of concepts to implant themselves into the minds of future generations of political scientists, in the way that Dahl's views on power, and for later generations rational choice models, succeeded in doing. My (I accept non-systematic) survey of introductory texts shows the following. Wilson and Dilulio's text (2009) now in its 11th edition makes no mention of Lowi's scheme (Lowi is mentioned in connection with a general survey of the US system). In Dye et al.'s hugely popular text (2011: now in its 15th edition) Lowi is mentioned three times, none of which invoke his scheme. These texts are important because they are where beginners first encounter the classics of the discipline. More striking still is the (non-)treatment of Lowi in more advanced texts which specialize in the area of public policy: Dye's textbook (14th edition, 2012) is probably the leader in the field, and makes not a single mention of Lowi, despite having a whole chapter devoted to models of public policy. Moreover, it persists, in its categorization of policies in using the (for Lowi) despised categories employed by practitioners everyday—health, education, and so on. Likewise, Lowi is not mentioned in Kingdon's (2010) prize-winning study of policy agendas.

The hard test of usage in the classroom, for which textbooks here stand proxy, suggests that Lowi's work is a "classic" in the second sense identified at the beginning of this chapter: an intellectual statue placed with the household gods of the discipline, the subject of respectful genuflection, but not part of the continuing everyday work of the discipline. But usage is not a proper test of intellectual worth: fashions come and go; fine work is neglected, forgotten, and (occasionally) rediscovered. Indeed one function of a volume such as this is to remind us of the continuing relevance of classics that in our time may have only an ossified, respected status. How far does Lowi's classic deserve such an appreciation?

STATUE OR LIVING CREATION?

The intellectual impact of a piece of work can often turn on the creator's ability to encapsulate its argument in a few words. Who can doubt, for example, that Putnam's study of the decline of social capital owed a lot of its success to the riveting title, *Bowling*

Alone? Some years after the publication of the paper which is examined in this chapter Lowi managed something similar with a phrase from his 1972 paper: “policies determine politics” (p. 299) memorably encapsulated what had been said less pithily eight years before: that “for every type of policy there is likely to be a distinctive type of political relationship” (p. 688). In the dazzling three-word summary “policies determine politics” lie the strengths, but also the problems, of Lowi’s scheme.

These three words show what a deadly blow Lowi had administered to pluralism, and how far he had potentially cleared the ground for the re-entry of Millisian elite analysis to political science. Pluralists were hardly unaccustomed to the need to distinguish between cases, but their focus was on the *importance* of the cases as microcosms of the wider policy process: hence the almost endless efforts to build indicators of case importance. But Lowi managed to demonstrate that something more than size mattered. The conclusions one could draw about power were systematically different for different categories of policy types. And while this gave comfort to the elitist theory that could be derived from Mills’s work (Lowi 1964a: 680) Lowi’s analysis should have given elitists food for thought: if “policies determine politics” it follows that not all important policy arenas can be expected to conform to an elitist model.

“Policies determine politics” (or even the more nuanced “for every type of policy there is likely to be a distinctive type of political relationship”) offered a potentially powerful causal model: policy was the independent, politics the dependent, variable. Here the difficulties start, and they are of two kinds: the robustness of the analytical categories; and, more straightforwardly, the empirical accuracy of the hypothesized causal relationship.

It is an elementary principle of causal analysis that, if we hypothesize a relationship between independent and dependent variables, we should be able analytically to define the criteria of variable identification and the boundaries between variables, and operationally to measure how they change. Applying these principles allowed Dahl in 1958 to leave the elitists’ impressive structure in smoking ruins. The target of Dahl’s fire was not, of course, Lowi, but Dahl’s analysis is a masterclass in demonstrating the kinds of tests which a conceptual scheme has to pass. Lowi has difficulty meeting these tests. By his own admission the typology is not exhaustive. How could it be, when eight years after the initial statement (Lowi 1972) he added to the three existing categories (distributive, redistributive, regulatory) a fourth, constitutive. This appears in the context of a shift in the criteria used to derive the initial categories. In 1964 the classification system depended on variation in “types of outputs or policies” (p. 689). But in 1972 the key distinguishing criteria have shifted from output to process: the four categories are laid out in a 2x2 table which classifies (vertical) the likelihood of coercion being used in the policy and (horizontal) the applicability of coercion (1972: 300).

This shift in analytical strategy is mysterious because it is not explained. All the reader is told in 1972 is that the classification system has been worked out in already published sources (Lowi’s endnote refers the reader to Lowi 1964a and 1970) so there is no discussion of the categories in detail. Even more mysteriously, most of the 1972 article is concerned with a detailed historical review of the policy types under presidents,

chiefly from the late nineteenth century to Roosevelt. This produces detailed analysis of the history of the three original policy domains, but, even more mysteriously, nothing more is said about the fourth domain, *constitutive policy*, the examples of which are reapportionment, setting up a new agency, and propaganda. Plainly these examples are important but an important claim of Lowi throughout is that his categories transcend (and are deeper than) the kinds of policy categories that are derived from the thought world of the policy actors themselves—deeper than reapportionment, agency creation, and propaganda, in other words.

Now it is a reasonable response to these observations that a classificatory system in the process of development is precisely that—under development, and therefore subject to change, including expansion. But the trouble with thinking in public in this way is that it means the scheme cannot deliver on the dazzling promise of those three words, “policies determine politics.” It sets the reader off on a hunt for an analytical snark: the final, definitive elaboration of policy types. This is why so much of the secondary literature on Lowi, so well analysed by Hayes in 1978, is concerned with trying to pin down finally the number of, and the boundaries between, the policy domains. The failure to pin these down definitively seriously damages the analytical framework: attempting to use it is the analytical equivalent of the croquet game in chapter 8 of *Alice in Wonderland* where the players find the balls and mallets have a life of their own and constantly change shape. The realization of this fluidity in the scheme may also lie behind one of the most disconcerting of Lowi’s themes in reviewing Bauer et al., as a study in tariff policy. Using Schattschneider’s (1935) study of Smoot Hawley (another neglected “classic”) as a benchmark, he argues that Bauer and his colleagues were observing a moment when tariff policy was “undergoing a transition from distribution to regulation” (p. 699). This is a baffling observation, for the key feature of tariff policy—its infinite disaggregation into ever tinier concessions—is exactly what made it the paradigm of log-rolling, distributive politics. If such a paradigm of distributive politics can transform itself into regulation we truly are like Alice, trying to play a game in which our implements have no fixed shape.

This fluidity also helps explain the second great source of difficulty with Lowi’s scheme: the problem of verifying the proposition that, indeed, “policies determine politics.” In 1972 Lowi set the bar for a test of this:

There is more to the urge for classification than the desire for complexity. Finding different manifestations or types of a given phenomenon is the beginning of orderly control and prediction. Taxonomy before ontogeny *or* phylogeny. Moreover, to find the *basis* for classification reveals the hidden meanings and significance of the phenomenon, suggesting what the important hypotheses ought to be concerned with. (1972: 299, italics in original)

What is more, he continues, in respect of the assumption that “policies determine politics,” the “assumption is without value unless the taxonomy of policies captures features of real government that are politically significant” (1972: 299). But Lowi’s

taxonomy fails the test, for it is indeed failing to capture “features of real government”; that explains why a paradigmatic instance of one type (the tariff issue) can migrate from distribution to regulation. The most important empirical difficulty lies in the category of regulatory politics, perhaps the most significant class not only in the paper under examination here but in Lowi’s work as a whole. The regulatory politics of the New Deal is the subject of the withering analysis in *The End of Liberalism*. Regulatory politics is identified as a quintessentially pluralist arena: “the regulatory arena could hardly be better identified than in the thousands of pages written for the whole polity by the pluralists” (1964a: 695). Moreover, this is not just a contingent connection. As I sought to show in my summary of Lowi’s argument, the pluralist character of the arena is connected in his mind to the kinds of policy choices which are made in regulation: their all or nothing character (only one applicant can be awarded a TV franchise) mobilizes sectoral interests in a typically pluralist pattern. This also helps explain another major theme in Lowi’s argument: that pluralism is also a legitimizing ideology for the regulatory state created by the New Deal.

But the accumulation of cases (the kind of accumulation about which Lowi is dismissive) suggests that there is nothing in regulatory politics which mandates pluralism. Here policies do not determine politics. There certainly are instances of regulation as pluralism, and Lowi provides them. But there are also significant parts of the regulatory world which are anything but pluralist. Take perhaps the most important (and certainly catastrophic) instance of regulatory policy prominent in recent years, the domain of financial regulation. The anatomies of the causes of the great financial crisis trace some of the most important roots of crisis on both sides of the Atlantic to the regulatory world of financial markets (see for instance Engelen et al. 2011). And the world described there is anything but pluralist. It is a Millsian world in which institutionally entrenched elites in markets and regulatory agencies practiced a politics of mystification, creating a hegemonic regulatory ideology which excluded most other actors from civil society (let alone democratic politics) and which allowed markets to pursue sectional gain to a point of speculative mania, destroying societal wealth and imposing on the public purse most of the huge cost of the crisis. That Millsian world in financial regulation is traceable to the historical organization of politics in the financial arena. It has nothing to do with whether the arena is regulatory or otherwise. Here is a striking falsification of the hypothesis that “policies determine politics.”

It may be objected that it is rather severe to test Lowi by the ability of his scheme, elaborated in the 1960s, to make sense of regulatory politics in a particular arena, albeit an important arena, a generation later. But the case of financial regulation shows that regulatory politics do not conform to a single pattern: in the regulatory arena, in other words, policies do *not* determine politics. Regulatory politics can take a diversity of forms. Moreover, that insight was already inscribed in perhaps the most creative adaptation of Lowi, by Wilson in *Political Organizations*. Noting Lowi’s observation that regulatory politics are a domain of pluralism, Wilson argues, to the contrary, that “there is a broad range of other regulatory issues in which group

activity is modest" (Wilson 1973: 16). It is precisely this insight which then leads to Wilson's equally "classic" account of the way regulatory politics vary by the way costs and benefits are distributed. In short, the limits of Lowi's account are not revealed merely by hindsight; they are inscribed in the limitations of his conception of regulatory politics.

Lowi as a Classic

I have argued in this chapter that "classics" can live on in a variety of forms, some more creative than others. Though it continues to collect citations, it is hard to argue that Lowi's paper continues to exert a shaping influence on policy studies. Ironically, the great continuing impact lies in the three words which do not occur in the original paper at all: "policies determine politics." They turn out not to form a supportable hypothesis, but they do amount to a powerful heuristic. After Lowi, any student of policy has to ask three questions. What kind of case am I looking at here? What kind of policy am I examining? And what kind of politics is that policy producing?

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CHAPTER 14

AARON WILDAVSKY, *THE POLITICS OF THE BUDGETARY PROCESS*

JOACHIM WEHNER

Confronted with the vast array of figures in the Budget of the United States, one is likely to think of budgeting as an arid subject, the province of stodgy clerks and dull statisticians. Nothing could be more mistaken.

(Aaron Wildavsky 1964: p. v)

FEW governmental activities are as universal as budgeting, but at the same time have proven so difficult to theorize (Key 1940; Schick 1988). Prior to the publication of Wildavsky's seminal work on the topic, much writing on budgetary processes was heavily descriptive and atheoretical. To outside observers of federal budgeting in the US, the annual process of making decisions about funding for different agencies may have seemed chaotic and unpredictable. Wildavsky showed that budgeting followed a set of clear norms and rules, and that it was characterized by stable patterns of interaction between the various actors. By developing a brilliantly simple yet fundamental theoretical framework for analyzing budgetary decisions, and gifted with an engaging and powerful prose, Wildavsky ensured that the study of budgeting moved beyond the description of formal procedures, and evolved to occupy a central place in public administration scholarship.

Aaron Bernard Wildavsky (1930–93) was the son of Ukrainian immigrants and grew up in Brooklyn, New York. He graduated from Brooklyn College. After serving in the US Army and receiving a Fulbright Fellowship to the University of Sydney, he completed a doctorate at Yale University in 1959. He taught at Oberlin College, before joining the political science faculty at the University of California at Berkeley in 1962, where he stayed for the remainder of his life. Wildavsky was

a prolific writer who authored or co-authored more than three dozen books. Amongst these, *The Politics of the Budgetary Process* (abbreviated to *The Politics* in the remainder of this chapter) is widely regarded as one of the undisputed classics of public administration scholarship today. Yet, its publication had to overcome a number of hurdles. Initial research on the project was, fortunately, funded by a \$600 grant. In retrospect, Wildavsky (1992: 599) wondered, “without being able to resolve the matter in my mind,” whether in the absence of this funding he still would have embarked on the research: “It would have been the best investment I could have made. But I did not know that then.” Funding was not the only obstacle—a staggering nine publishers initially rejected the manuscript. They must have regretted their decision.

This chapter has three main parts. The first summarizes the basic ingredients of Wildavsky’s theory of budgetary incrementalism. The second discusses challenges to incrementalism, which arose mostly in the context of economic and fiscal crisis that altered the way in which budgetary decisions were taken in the US federal government. The third part explores the lasting relevance and importance of Wildavsky’s work on the politics of the budgetary process by noting several elements that have fundamentally shaped scholarship on budgeting, or the way in which we understand and interpret budgetary decisions.

CONTOURS OF BUDGETARY INCREMENTALISM

In an indictment of traditional public administration scholarship that preceded the publication of the book, Wildavsky (1961) pointed out that the language of efficiency at best partially captures and informs budgetary reform, and instead highlighted the essential role of political dynamics. Up to that point, much of the public administration literature had focused on institutions and processes as if they were unrelated to the power relations of political actors. Wildavsky fundamentally challenged this focus as misguided, and exposed it as a possible reason for the failure of reform prescriptions. Wildavsky (1961: 190) concluded his article by musing: “Perhaps the ‘study of budgeting’ is just another expression for the ‘study of politics’ . . .” The article, published in the *Public Administration Review*, later evolved into one of the chapters in *The Politics*, and established a central theme in his work on budgeting: that decisions about the allocation of resources cannot be understood without understanding the politics of the process.

In *The Politics*, Wildavsky sets out a theory of budgetary incrementalism based on the assumption of bounded rationality (Wildavsky 1964). He argues that budgeting is characterized by “extraordinary complexity” (p. 7), yet the capacity of the human mind is “drastically limited” (p. 10). Hence, in the language of Herbert Simon, budgetary

actors “satisfice” rather than “maximize” (p. 12) and resort to an essential aid to calculation—the incremental method (p. 15):

Budgeting is incremental, not comprehensive. The beginning of wisdom about an agency budget is that it is almost never actively reviewed as a whole every year in the sense of reconsidering the value of all existing programs as compared to all possible alternatives. Instead, it is based on last year’s budget with special attention given to a narrow range of increases or decreases. Thus the men who make the budget are concerned with relatively small increments to an existing base.

According to Wildavsky, actors in the budgetary process can be categorized according to their propensity to either spend or conserve public funds. He described line agencies as “advocates of increased appropriations” (p. 18). On the other hand, the Bureau of the Budget—later the Office of Management and Budget—is characterized as a “Presidential servant with a cutting bias,” the Appropriations Committee in the House of Representatives as the “guardian of the Treasury,” and its Senate counterpart as a “responsible appeals court” (p. 160). These roles “fit in with one another and set up a stable pattern of mutual expectations, which do a great deal to reduce the burden of calculations for the participants” (p. 161). Incrementalism contains conflict by leaving the “base” of past decisions untouched and ensuring that agencies get their “fair share” of the total budget (pp. 16–17).

One chapter of the book discusses “budgetary strategies” employed by agencies to maintain funding levels or to augment their allocations. Many of these strategies are astutely observed and immediately recognizable by observers of budgeting, for instance his discussion on how to cultivate a clientele of program beneficiaries, different ways to build confidence in an agency, and how to survive Congressional hearings. My favorite nugget of wisdom in this chapter is the section on expanding the base via “wedging” or “the camel’s nose” (pp. 111–12). This involves getting a small allocation into the budget that can be expanded when conditions are favorable to do so. Once in the budget, a line can remain there as long as is necessary to await its opportunity for expansion. Such sections are entertaining and insightful, even if they are not the most theoretically interesting parts of the book.

Wildavsky’s research for the book drew on approximately 160 interviews that he conducted with various participants of the budgetary process at the federal level of the US government. The interviews create a rich fabric of evidence that is used to document the sketched roles and patterns. Peppered with often funny or quirky quotes, the book is both persuasive and entertaining at the same time. Few scholars of budgeting have managed to make the subject come alive in this way.

Wildavsky complemented the analysis in his book with a classic article in the *American Political Science Review* that reports a statistical analysis of agency budget requests and approved budgets over the period 1947 to 1963 to test the theory (Davis et al. 1966). Fitting a number of alternative regression models to the data, Wildavsky and his co-authors find that two of these best describe the underlying relationship. The amount

that agencies request from Congress is best modeled as a fixed mean percentage of its allocation in the previous year (p. 532). Congress, in turn, tends to approve a fixed mean percentage of an agency's request in a given year (p. 534). The amount of variation in agency requests and Congressional allocations accounted for by these basic models is extremely high. In terms of its quantitative methodology and in the context of political science, the article is extremely sophisticated for its time, requiring a lengthy appendix to explain the basics of regression analysis to the unaccustomed reader.

A perhaps somewhat overlooked aspect of this initial work is that it included a careful analysis of whether and when the patterns of interaction between agencies and Congress were most likely to change. This entailed a series of Chow tests to investigate coefficient instability in the regressions with agency-level spending data, or "shift points" (Davis et al. 1966: 539). In other words, these tests pick up when the fixed mean percentages that are requested by agencies and allocated by Congress become unstable and shift to a new level. The analysis identifies several dozen instances of such shift points, which cluster in the first two budgets of the Eisenhower presidency, highlighting that agency spending is sensitive to political changes. This links to more current research on how political and other variables affect budget changes. The third section returns to this point.

INCREMENTALISM IN CRISIS?

The Politics became a huge success and was published in four editions (1964, 1974, 1979, and 1984). Wildavsky had established a new standard for budgetary scholarship. By the end of that period, however, an increasing number of critics were pointing to a widening divergence between the theory and actual practices. In the context of economic slowdown and a series of crises in the 1970s—oil shocks, recessions, persistent unemployment, and inflation—budgeting had become "decremental" (Schick 1983). The base was no longer exempt from challenge, and with that budgeting had become more contentious and unstable. This changed economic context resulted in greater emphasis on fiscal management through top-down decisions, which upset the previously dominant pattern of bottom-up budgeting that Wildavsky had captured in his book. According to Bozeman and Straussman (1982: 513–14), this meant that incrementalism had "less to offer than before as a general theory of budgeting ... In periods of shrinkage the explanatory power of incrementalism is diminished."

By the 1980s, the formal structure of the budgetary process in the US federal government that Wildavsky had neatly summarized in three simple graphs at the very end of the first edition of his book had changed fundamentally. The 1974 Congressional Budget Act instituted a new procedure in Congress with a concurrent resolution that set out fiscal policy, and was intended to impose an aggregate constraint on detailed budgetary decisions. The Act also created Budget Committees in the Senate and the House to guide the process. In the late 1970s, the federal government experimented—rather unsuccessfully—with zero-based budgeting, another mechanism devised with

the intention to challenge the base and to put a brake on incrementalism. A few years later, the US embarked on a major experiment in rules-driven deficit reduction under the Gramm–Rudman–Hollings Balanced Budget Act of 1985. Incrementalism was under siege by attempts to devise institutionalized challenges to the budgetary base.

It is perhaps surprising that Wildavsky stuck to his theory for so long even as a different economic context undermined a central precondition: growth and the resulting availability of an increment. At the same time, he has to be given credit for acknowledging the limits and scope conditions of incrementalism much earlier than some of his critics may concede. In another book, published in 1975, Wildavsky explores the contextual conditions that foster incrementalism within a systematic comparative framework. He argues that the type of budgetary process depends on a polity's wealth and the degree of predictability in relation to available resources and demands for spending. In his analysis, incrementalism requires both wealth and certainty (Wildavsky 1975: 12). Other combinations of these two dimensions yield different budgetary processes: poverty and certainty leads to "revenue budgeting"; poverty and uncertainty gives rise to "repetitive budgeting"; while wealth and uncertainty engenders either alternation between incrementalism and repetitive budgeting (if uncertainty is due to political instability) or "supplemental budgeting" (if uncertainty is due to administrative incapacity). This work explicitly makes the point that incrementalism is not universal, and highlights the conditions under which this particular pattern is most likely to be observed. The acknowledgment that budgeting may take different forms across time and space gives the book a "transitional" quality (Rubin 1989: 79). It documents the evolution of Wildavsky's theorizing about budgetary processes, and of his thinking about the limits of incrementalism.

Over the years, scholarly criticism of budgetary incrementalism proliferated. In a powerful challenge, Meyers (1994) contradicts Wildavsky by noting that budgetary actors often make complex calculations, and that their roles are numerous and unstable. In his account, reflecting an altered economic and fiscal context, budgeting is competitive and affected by macrobudgetary constraints. Elsewhere, Berry (1990: 182) identifies no less than 12 distinct and often logically unrelated meanings of the concept of incrementalism in the literature and concludes: "The term has become too many things to too many people to be useful in research." An earlier piece by Dempster and Wildavsky (1979) had attempted to deal with some of the meanings ascribed to the term in the literature, but perhaps not successfully enough.

It took Wildavsky another decade, until 1988, to make the intellectual leap from acknowledging the limits of incrementalism in his comparative work to conceding its demise in the context of the US federal budgeting. In *The New Politics of the Budgetary Process*, his classic theory is relegated to a single chapter. The remainder of the book describes a new and messy reality of budgeting that lacks the predictability and order of classic incrementalism. While *The New Politics* makes a number of insightful analytic contributions—for instance on the rise of entitlements—it does not deliver an overarching new theory of post-incremental budgeting. Maybe Wildavsky would have delivered such a new theory of the budgetary process had he not died in 1993, a year after publishing the second edition of *The New Politics*.

By the time of his death, Wildavsky's thinking about budget reform and institutional engineering, too, had evolved considerably (Jones 1996). Initially, his critique of reform proposals had been scathing (Wildavsky 1961). Yet, the top-down procedure instituted in Congress around the Budget Committees and the concurrent resolution in the mid-1970s resembled some of the proposals that Wildavsky had derided as "a typical reform" that fails to recognize the importance of political power relations in his 1961 paper and the related chapter in his 1964 book (pp. 133–4). Despite this strong early skepticism of institutional engineering the budgetary process, he later became a proponent of far-reaching reform and proposed a constitutional spending limit in order "to restore the reality of self-control to our government" (Wildavsky 1980: 126). His views on the acceptability of deficits moderated again during the 1980s, but he continued to think about appropriate rules to guide budgetary decisions. Hence, his attitude to institutional engineering had changed from skeptic and opponent to proponent of very specific—and fundamentally political—reforms to the design of the budgetary process.

WILDAVSKY AND BUDGETING TODAY

Despite criticisms leveled against it, *The Politics* remains the most widely cited study on the subject, and constitutes a towering benchmark against which scholarship on budgeting is judged. This section reflects on some of the reasons for this remarkable durability, and briefly considers how Wildavsky's work continues to shape our understanding of budgetary processes today. It makes no claim of completeness.

One major reason for the durability of the theory is that, although incrementalism may not be universal, its lure is universally recognized. Budget reformers around the world expend much energy on the fight against budgets on autopilot. For instance, one of the World Bank's major publications on budgeting of the past two decades, the influential *Public Expenditure Management Handbook*, sees incrementalism as a central problem that reforms are meant to overcome (World Bank 1998: 102): "In practice, most budgets are incremental, taking last year's allocation as base, adding a small percentage for inflation and, perhaps, a little real growth. Incremental budgeting usually reflects a mismatch between policies and resources and excessive focus on funding, at the expense of policy, in budget deliberations." The resulting reform prescriptions may be conceptually appealing—such as performance-based budgeting, the adoption of some sort of medium-term expenditure framework embedded in policy, and strengthening top-down elements in resource decision, amongst others—but they often fail to break the budget's built-in trajectory (e.g. Schick 2003). In fact, no econometric time series analysis of fiscal policy or program-level outlays is complete without a lagged dependent variable to capture the path dependency of budgets highlighted by Wildavsky. The attraction of incrementalism remains powerful and instinctively recognizable to budget practitioners and observers.

Research has advanced, however, in investigating when incremental patterns are likely to prevail. As noted earlier, Wildavsky did acknowledge that allocation patterns may shift periodically, in particular in response to political changes. A more recent literature on punctuations in budgets has investigated departures from incrementalism in more depth (Jones et al. 2009). For instance, Breunig and Koski (2012) examine data from the US states, and find that budgetary changes are more incremental in some expenditure categories (e.g. education and welfare) while others see more frequent extreme changes, or punctuations (e.g. parks and hospitals). Other research finds that changes to the composition of budgets are affected by the alternation of political parties in government, as well as their ideological distance (Tsebelis and Chang 2004). A separate literature highlights the role of divided government and budget institutions in accounting for differences in fiscal adjustment in response to economic shocks (Alt and Lowry 1994; Poterba 1994). Overall, there remains scope to draw on these different strands of research in order to deepen the analysis of what accounts for departures from incremental patterns especially at a sector or program level.

Another lasting appeal of Wildavsky's book is its distinction of the roles of different actors, which introduced an analytically powerful distinction between "spenders" and "savers" that he later described as one of the "constants in budgeting, no matter where practiced" (Wildavsky 1975: 9):

Everywhere there are spenders and savers. Those in charge of the great purposes of government naturally tend to identify their fortunes with the interests entrusted to their care. So they defend against cuts and seek increases whenever they can . . . Guardians of the treasury will not do well if they are forced into inflationary measures or constantly have to raise taxes because they cannot control spending. So they cut and trim and otherwise try to keep spending within hailing distance of revenues.

This fundamental and universal distinction permeates the budgeting literature to this day. For instance, Niskanen (1971)—for a few years a colleague of Wildavsky at Berkeley—models the interaction between a budget-maximizing bureaucrat and a legislative sponsor. Unlike Wildavsky, Niskanen assumes rational actors; he develops a microeconomic theory of bureaucracy. Nonetheless, the basic roles attributed to the two main actors are very familiar to students of Wildavsky. The more recent fiscal institutionalist literature is based on models highlighting the spendthrift tendencies of line ministers and deals with how they can be contained. According to this literature, one solution is to delegate decision-making authority over the budget to the finance minister, who has greater incentives to internalize the full cost of decisions (von Hagen and Harden 1995). This work, too, relies on assumptions about stable roles and conflicting incentives that were so eloquently described by Wildavsky.

At a most basic level, Wildavsky's work remains relevant because of its fundamental understanding that budgeting is driven by politics, and that the budgetary process embodies power relations between different actors. This may—hopefully—seem like an obvious point to some, especially political scientists. Yet, it has to be noted that

much writing on budgeting, especially of the more applied public financial management variant, still too often fails to incorporate this fundamental insight. Misguided ideas of “best practices” that should be spread globally remain common among large segments of the practitioner community (Andrews 2013). The track record of budget reforms in the developing world is awash with well-intended technical “solutions” that failed to account for local political realities. If only more budget reform advisers would read Wildavsky’s book!

At the same time, the more recent literature on fiscal institutions goes considerably further in analyzing the political conditions that foster the emergence of certain institutional arrangements. The basic argument in this work is that different kinds of institutions can help to contain pro-spending pressures in budgeting, and that which of these is most appropriate depends on the party-political context that prevails (Hallerberg et al. 2009). For instance, single-party governments can rely on delegation to a strong finance minister, while ideologically dispersed multi-party governments instead may agree to a fiscal contract that forms part of a coalition agreement. This work has moved considerably beyond the old-style reform prescriptions that propagate technical reforms to essentially political procedures, which Wildavsky had criticized in his early writings.

CONCLUSIONS

Wildavsky made the compelling case that, given the complexities of financial decisions and the cognitive limits of decision-makers, budgeting relies on a set of simple and stable decision rules that define the relationship between actors. Typically, agencies would base budget requests on their previous allocation plus a little extra, and Congress would remove some of the padding. Past decisions became part of a “base” that would be left largely unchallenged, allowing the annual budgetary process to focus on significant changes and additional funding for new programs. In the world of classic incrementalism, everybody knew how to behave and what to expect.

As macroeconomic conditions deteriorated in the 1970s, budgeting became more conflictual and Wildavsky’s theory of incrementalism fit increasingly poorly with actual decision-making processes in the US federal government. Wildavsky first acknowledged that incrementalism was not universal in his early comparative work in the mid-1970s, which recognized wealth and certainty as fundamental preconditions. He ultimately abandoned his theory of incrementalism in the context of the US federal government, but never quite managed to wrestle a new theory from a more messy budgetary reality in an era of fiscal constraints.

Nonetheless, the enduring centrality of Wildavsky’s work in the study of budgeting is testimony to the fact that he captured several essential elements. Although not universal, incrementalism remains instantly recognizable as a default pattern of budgeting in good economic times. In addition, Wildavsky’s understanding of the different roles

of various decision-makers, while stylized, remains central to much scholarly work on budgeting. Perhaps most fundamentally, since Wildavsky, it is hard to imagine serious scholarly work in this area that fails to acknowledge the importance of politics and political power relations. These are major and lasting contributions to the literature on budgeting, and Wildavsky managed to make them in an immensely readable language. In the process, he succeeded in rescuing this potentially “arid subject” from the “stodgy clerks and dull statisticians.”

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CHAPTER 15

MANCUR OLSON, *THE LOGIC OF COLLECTIVE ACTION: PUBLIC GOODS AND THE THEORY OF GROUPS*

DAVID LOWERY

MANCUR OLSON, COLLECTIVE ACTION, AND PUBLIC POLICY

MANCUR Olson's two books—*The Logic of Collective Action* (1965) and *The Rise and Decline of Nations* (1982)—have assumed classic status among students of the politics of interest representation and have been employed frequently by public policy scholars to understand the mix of policies governments adopt in the face of lobbying. Both have been cited thousands of times. Still, policy scholars have been considerably more receptive over time to Olson's ideas than have students of interest politics. As Baumgartner and Leech (1998: 75) concluded in their analysis of Olson's ideas by interest organization scholars, "the problems discussed by Olson may have been given more prominence in the interest-group literature than they deserve." Such skepticism may surprise students of public policy who cite Olson in explaining policy outcomes via reference to free riding.

To address the puzzlement likely arising from Baumgartner and Leech's rather strong claim about the status of Olson's work among interest organization scholars, this chapter will first briefly review the core argument in Olson's most important work, *The Logic of Collective Action*. I then review the many ways research by interest politics scholars has modified, hedged, and sometimes contradicted Olson's claims on individual mobilization. In the third section, I consider institutional mobilization and discuss how it should bear on our assessment of the collective action hypothesis. And finally,

I discuss the policy implications of Olson's analysis of collective action problems both in terms of how the diversity of interest communities should bias public policy outcomes and, more directly related to Olson's analysis in *The Rise and Decline of Nations*, how these outcomes influence economic growth. Finally, I discuss how we all—both students of organized interests and students of public policy—still over- and underappreciate Olson's contributions.

OLSON'S ARGUMENT ON INDIVIDUAL MOBILIZATION

Why individuals join organizations posed no great puzzle to pluralists. Joining to secure common goals seemed entirely natural to Truman (1951: 505). Olson challenged this pluralist view in *The Logic of Collective Action* by rejecting its core assumption that participation is natural. While acknowledging that individuals might wish to join together to resolve disturbances in society, Olson pointed out that the sought policies are typically collective goods. New policy will benefit everyone disturbed under the old status quo whether or not they advocated change since potential beneficiaries cannot be excluded from access to collective goods, and collective goods are jointly consumed. Olson asked what rational individuals do when asked to join an organization to secure collective goods. His answer was that they would do nothing. Since they would benefit from the adoption of a new policy whether or not they worked to secure it, they would free ride on the efforts of others. But since all of the potential members of a group are presumed to be rational, all would act similarly. To Olson (1965: 5–52), then, no one will undertake the costs of forming groups pursuing even widely shared objectives involving collective benefits.

Still, many membership organizations do in fact form. To account for this, Olson identified three ways in which organizations may overcome free riding. The first is its prohibition by compelling or coercing all beneficiaries to join. Closed-shop labor unions, according to Olson (1965: 66–97), are classic examples of forced riding.

A second solution entailed noting that, while free riding is always a potential problem, it is more severe in some cases than others. It is least troubling for small groups, groups pursuing large stakes, and, especially, small groups with large stakes. In such cases, collective goods begin to take on the attributes of selective goods. Since it makes little sense to free ride on oneself, incentives to participate will, therefore, increase as each person's share of the collective good increases. Social norms against free riding and opportunities to observe it also become more powerful with small numbers. This means that organizations with smaller numbers of potential members and/or pursuing larger stakes are more likely to mobilize than those with fewer potential memberships and/or smaller stakes.

The third and most general solution to the problem of collective action identified by Olson is the provision of selective benefits. Selective benefits, unlike collective benefits, are consumed individually. Therefore, Olson argued, groups routinely exchange selective material benefits for membership. For example, Olson suggested that doctors join the American Medical Association (AMA) for its provision of low-cost malpractice insurance and medical journals rather than a concern about the collective benefits derived from the policies on which the AMA lobbies. Indeed, Olson argued that selective incentives are so important to securing membership that collective goods are produced only as a “by-product” of the provision of selective benefits. Accordingly, there may be little connection between the reasons motivating individuals to join groups and the collective goods for which they lobby (Olson 1965: 132–68).

CHALLENGES TO OLSON’S MOBILIZATION THESIS

Much of the research done on individual-level mobilization since Olson’s publication of *The Logic of Collective Action* has, however, sharply delimited its implications. Three types of challenge were raised against his analysis. The first is that many individuals do seem to join together for collective action. Indeed, the timing of Olson’s book was ironic given the explosion of membership organizations—associated with the anti-war, women’s, civil rights, consumer, and environmental movements—shortly after its publication. Given Olson’s argument, the shared interests associated with these movements should not have been sufficient to promote the emergence of interest organizations, and it seems unlikely that all were joining in order to secure the selective incentive of a t-shirt.

More systematically, studies of joining and contributing in the lab and the field have found far higher levels of participation than might be expected given the logic of collective action. Laboratory studies by Marwell and Ames (1979, 1980, 1981) and Marwell et al. (1988) found only mixed evidence of free riding. Experimental subjects often contributed to producing collective goods, even if participation was incomplete across all members and contributions were lower than pluralists might have expected. And in the field, while finding some class bias in rates of participation, Verba et al., in their 1995 book, *Voice and Equality*, found that joining is common. Fully 79 percent of their respondents reported being members of voluntary associations, and 41 percent belonged to four or more. Moreover, involvement is quite active; 42 percent reported that they were active members, 65 percent had attended a meeting in the prior 12 months, and 55 percent contributed funds. When voluntary organizations are defined broadly, participation rates seem far more extensive than Olson might allow.

The second type of challenge raised against the stark implications of Olson’s argument goes further to identify *why* problems of collective action might not be so severe.

Three key arguments merit attention. The first concerns Olson's narrow definition of selective incentives. In the case of the AMA, for example, Olson mentions only collective goods the medical organization lobbies for and a narrow list of selective material benefits—low-cost insurance, journals, and professional training. But Salisbury (1969), building on the work of Clark and Wilson (1961), argued that selective incentives are not restricted to material benefits. Those who join may also receive an expressive benefit by acting on an issue they care about. Those who join may also receive solidary benefits from participation—camaraderie from affiliating with like-minded people, social status, or a sense of accomplishment. Critically, expressive and solidary rewards are, like material incentives, selective benefits. Therefore, unlike collective goods, they cannot be accrued by free riding. Solidary and expressive benefits, therefore, discourage free riding in the same manner as material selective incentives. Indeed, surveys conducted by Walker (1991) suggest that leaders of membership groups often rank expressive and solidary benefits as equally or even more potent than material benefits in securing participation. But unlike material incentives, solidary and expressive benefits are plausibly related to the collective benefits pursued by organizations. Solidary benefits are likely to be higher from interacting with those who share our interests, and the satisfaction we derive from expressing ourselves is even more clearly related to the collective goods sought by organizations. This means that lobbying is unlikely to be a mere by-product of providing material incentives.

A second way in which free-riding problems might be mitigated lies in the context in which groups form. Olson implied that atomistic individuals make cool calculations of the costs and benefits of participation. Many voluntary organizations, however, arise from within existing organizations—churches, schools, and places of work—where members already have strong social ties. Verba et al.'s (1995; also see Chong 1991) analysis of civic volunteerism, for example, highlights the importance of such contexts in promoting participation. It is via organizations, they argue, that individuals become engaged with issues and link them to their personal lives via work with others. Personal ties within organizations then provide modes by which individuals are contacted and recruited into voluntary groups. Working within organizations also provides individuals with the critical skills and resources they need to participate effectively in political activity, such as the ability to organize ideas and speak in public. Thus contexts facilitate mobilization by allowing large groups to act as if they were smaller.

The third argument about why we observe more joining than might be expected addresses another aspect of Olson's model of individuals as rigorous calculators of costs and benefits. Deciding to free ride involves a calculation that one's contribution to a group will not be missed. But Moe's (1980: 208) survey of members of Minnesota voluntary groups indicates that joiners often overestimate the importance of their contributions in securing collective benefits. While each member's contribution was small, a majority of the members of all the organizations studied claimed that its absence would increase the chance of the organization failing. As a result, Moe (1981: 540) concluded that, "Politically based membership ... is much more common than Olson's model would predict." John Mark Hansen's (1985: 79–96) analysis of the histories of three

national interest groups further suggests that this kind of misperception is especially likely when the collective good involves defending members from a threat. If individuals overestimate the importance of their contributions, especially when the collective good involves a threat, then members of even large latent groups may act as if they were part of a small group.

The third challenge to Olson's stark analysis greatly expands the menu of options organizations might employ to secure membership beyond forced riding and selective material incentives. That is, a number of scholars have identified many ways in which organization formation is subsidized. If some outside actor reduces the costs of joining for potential members, then successful mobilization will be more likely. Subsidization often comes from within organizations. Among the most important sources of support is that provided by individual patrons, wealthy individuals who care intensely about an issue (Rothenberg 1992). Examinations of the early histories of membership groups indicate that many were either founded or initially sustained by the contributions of one or a handful of wealthy individuals (Walker 1991: 79; Gais 1996). Equally important are individual entrepreneurs. Many organizations are founded by dedicated individuals who sacrifice mightily to sustain lobbying on issues that they care deeply about (Salisbury 1969: 10). Such entrepreneurs, of course, have powerful selective incentives to subsidize the formation of organizations in that they, if successful, eventually lead them.

Subsidization is also provided from outside of the organization. Indeed, while often expected in Europe, public subsidization is also common in America. Walker (1991: 79) noted that 11.4 percent of profit and 33.0 percent of not-for-profit associations initially relied on government grants and 20.3 percent of citizens' groups also relied on public funds. The histories of many groups highlight the key role of initial government sponsorship even for many organizations that would seem unlikely dependents, including the American Farm Bureau Federation, the National Rifle Association, the American Legion, and the National Organization for Women (Walker 1991: 31). Indeed, Olson (1965: 149) noted the Farm Bureau example when discussing his by-product theory of lobbying. Other organizations—including foundations and business firms—also often sponsor membership groups, primarily via financial support (Walker 1991: 31; Nownes and Cigler 1988: 71). A final source of subsidies lies in the efforts of political candidates. Simply put, candidates need votes. And not surprisingly, they seek out and mobilize voters, especially membership groups that are a rich, lumpy source of votes (Walker 1991: 29–30).

Given the various ways in which subsequent research has sharply delimited Olson's 1965 analysis, does this mean that the logic of collective action is not important? In one sense, the answer is certainly yes. We observe far many more voluntary organizations and higher levels of individual contributions to them than we might expect given Olson's conclusion that both are severely depressed by free riding. Further, this suppression cannot be fully accounted for by reference to forced riding and selective material incentives alone, the limited menu of options provided to organizations by Olson

in the face of incentives to free ride. But this certainly does not mean that Olson was wrong.

Indeed, we, along with Olson, may in fact have long missed the most important implication of the logic of collective action. Yes, organizations have found—and certainly needed to find—many ways to overcome free riding. This means, however, that free riding almost certainly indirectly shapes these organizations. The need to provide solidary and expressive benefits along with material incentives determines how voluntary groups interact with their members. The cognitive biases that lead members to join at higher rates and to contribute more than might be expected surely influence the messages that organizations use to secure participation. Dependence on entrepreneurs and patrons surely enhances the importance of their roles in voluntary groups. And dependence on outside sponsors in government and the private sector surely raises the importance of inter-organizational relationships in membership group agendas and influences how and on what they lobby. In short, while the primary or direct effects of the logic of collective action were surely overstated, the secondary or indirect effects of the problem of free riding are likely very large and very important in terms of how they influence the behaviors of voluntary groups toward their members, toward other organizations, and toward public policy. Given the plausibility of these secondary or indirect effects in light of more than four decades of research delimiting Olson's analysis of individual mobilization, it seems fair to conclude that voluntary organizations almost certainly live in an Olson-like world, even if not precisely the one he imagined in *The Logic of Collective Action*.

COLLECTIVE ACTION AND MOBILIZING INSTITUTIONS

Although Olson (1965) discussed firms, his theories of collective behavior have most often been applied to individuals. Indeed, Olson (1965: 143) suggested that businesses are typically organized into oligopolies of small numbers of firms so that incentives to free ride present few barriers to organization. But free riding plausibly also bears on the decisions of institutions to lobby. And this is very important given that institutions, not membership groups, now comprise the majority of lobby registrants (Salisbury 1984; Gray and Lowery 2001). But it is also important in two ways that have somewhat contradictory implications for our understanding of Olson's contributions. First, finding evidence of free riding among institutions would enhance the generalizability of his logic of mobilization, making it even more important for our understanding of interest politics. But second, if we find that institutions are also subject to free riding, then this must, as we will see later, seriously undercut the application of Olson's ideas to public policy since this largely rests on how differences in incentives to free ride lead to business dominance of interest systems. But despite its obvious import, institutional

mobilization is rarely studied, even though there are many more institutions in society than there are institutions actively lobbying.

Institutions are not individuals. This makes applying Olson's logic to institutions more complicated in some ways and easier in others. The task is easier given that institutions are plausibly less subject to some of the cognitive biases and social norms that influence individuals. Salisbury (1984: 74) noted, for example, that "institutional leaders estimate that investment in political representation would be beneficial to the interest of the organization. One might treat this as a rational calculus problem." This is, of course, the same kind of calculus problem that Olson thought promotes free riding by individuals. But institutions are likely to make somewhat colder calculations of costs and benefits.

The main complication is that institutions participate in politics in many ways, including lobbying or sponsoring a PAC (political action committee) on their own or in an association with similar organizations. Indeed, many do both. Mapping the full range of institutions' political activity is not easy. And, unfortunately, we know less about the mobilization of institutions than we should. With the exception of a few intensive studies of mobilization within narrow economic sectors, most notably by Hart (2001), almost all studies of institutional mobilization have been aggregate analyses where rates of participation across economic sectors or jurisdictions are used to test the Olsonian hypothesis that participation rates should decline as the number of potential institutions that might lobby rises.

Most such studies of institutional mobilization have examined the sponsorship of PACs by large business firms. Thus, they consider a relatively narrow form of political participation by only one type of institution. While somewhat limited in focus, the majority of studies of PAC sponsorship by business firms find that two factors important to both Olson and Truman seem to influence political activity (Andres 1985; Grier et al. 1991, 1994; Humphries 1991; McKeown 1994; Masters and Keim 1985; Mitchell et al. 1997; Hansen and Mitchell 2000). First, stakes seem to matter a great deal. Firms that are heavily regulated, have significant contracts with government, and/or have been charged with criminal activity sponsor PACs more frequently than those with fewer stakes in public policy. Thus, firms seem to employ PACs as an investment strategy, engaging in politics to secure economic returns. Second, resources matter too. Firms with more assets, greater sales, and/or more employees are more likely to sponsor PACs. As with individuals, then, political participation by institutions increases as policy stakes rise and resources for political activity are more readily available.

There is, however, considerably more controversy over the problem of free riding that was uniquely important to Olson's (1965) analysis. Some have argued that firms are fully exposed to incentives to free ride since government policies, especially government regulations, bear on all of the firms in an industry (Grier et al. 1991). Policies, in this view, are collective goods, and the collective action problem should apply here as well as to individuals. Others, however, suggest that many of the items business firms lobby for, including many already noted like contracts from government, are selective benefits available only to the firms that lobby for them (Hansen et al. 2005). Further,

not all firms within an industry may share a common interest. Large firms, for example, because they can readily bear the costs may seek more stringent regulation as a competitive strategy (Bartel and Thomas 1987; Crandall 1983).

Unfortunately, PAC studies provide us few firm answers on the proclivities of firms to free ride. Some studies find that firms in more concentrated industries—those with fewer firms—are more likely to sponsor PACs than those with greater opportunities to free ride on the political activities of others. But several other studies find no evidence of free riding in the use of PACs. This ambiguity in the empirical tests of the collective action hypothesis using PACs may arise from their limited utility as a measure of the political activity. PACs are, as suggested by a number of studies, a minor and infrequent means by which institutions lobby, and many more institutions lobby than sponsor PACs. Even more problematic, Gray and Lowery (1997) have argued that PACs provide a very flawed locus for testing the free-riding hypothesis since PACs are typically appendages of larger firms used as a specific lobbying strategy. Indeed, in what they term a lobbying arms race, they find that PAC sponsorship rises in states with larger numbers of firms, the exact opposite of what the free-riding hypothesis would suggest (Gray and Lowery 1997).

If PACs provide a poor test of the collective action hypothesis, then perhaps a more valid picture of the extent of free riding can be obtained using lobby registration data. Lowery et al.'s (2004) analysis of lobby registrations by firms in a number of economic sectors in the 50 states seemed to find plausible support for Olson's conjectures on free riding. Among manufacturing firms, for example, only 4 to 6 percent of all firms tend to register in states with few numbers of firms. But in states with many firms, participation rates fall to 1 percent or less. As opportunities to free ride increase with a larger number of manufacturing firms, participation rates decline.

Still, even these positive results for Olson's expectations on institutional mobilization remain ambiguous. Simply put, the same pattern of declining rates of registrations as the number of potential institutions that might lobby becomes larger would also be expected by the energy-stability-area (ESA) model of Lowery and Gray (1995). In this model, it is not free riding that produces the pattern of declining rates of participation but the declining marginal utility of more specific forms of representation as interest systems become larger. Data such as those used in aggregate-level analyses of PACs or lobby registrations simply cannot distinguish between the two models (Lowery et al. 2008). Still, Lowery et al. (2008) make a number of arguments about additional, secondary features of the empirical results that suggest that the ESA model provides a better account of the observed aggregate patterns of participation than does free riding.

At best then, we cannot yet conclude that it is Olson's logic of collective action that accounts for the decline in participation rates as the potential number of institutions that might lobby becomes larger. Nevertheless, whether due to incentives to free ride or due to the declining marginal value of greater representation in crowded interest systems, the strong negative relationship between lobby participation rates and the size of latent populations has important implications for Olson's analysis of public policy.

COLLECTIVE ACTION AND PUBLIC POLICY

Olson's applied his mobilization analysis to public policy in two ways. The first concerns how the cross-sectional diversity of interest systems biases policy outputs. This part of his analysis was fully contained in *The Logic of Collective Action*. Olson (1965: 142) builds on Schattschneider's (1960) observation that business interests tend to dominate lobbying communities. Olson explained this over-representation of businesses by reference to their greater relative ease in overcoming free riding. Given their small numbers, collective benefits are rendered into something more like selective benefits. As Olson (1965: 143) noted, "The higher degree of organization of business interests, and the power of these business interests, must be due in part to the fact that the business community is divided into series of (generally oligopolistic) 'industries,' each of which contains only a fairly small number of firms." In contrast, Olson (1965: 143) noted that, "The multitude of workers, consumers, white-collar workers, farmers, and so on are organized only in special circumstances, but business interests are organized as a general rule." In sum, variations in incentives to free ride shapes interest system diversity, which in turn shapes public policy.

There are, of course, a number of problems with this analysis, including the explosion shortly after *The Logic of Collective Action* appeared in the numbers of groups representing the "latent" interests he cited. More importantly, if, as we have seen, incentives to free ride or something else that has the same effect, like declining marginal returns from further representation (Lowery et al. 2008), diminish business lobbying in crowded interest communities, then the imbalance between business and other types of organizations assumed by Olson is by no means certain.

An even more serious problem is his implicit assumption that the diversity of interest systems mechanically determines public policy. This perspective is not unique to Olson's analysis, although he fully endorsed it by implication. It asserts that government policy will almost inevitably be captured by organized interests (Schattschneider 1960; Stigler 1971; Peltzman 1976; Mitchell and Munger 1991). In extreme versions of this view, organized interests are assumed to act like consumers in a supermarket, interacting almost hardly at all while lining up to sequentially and with certainty purchase goods (Olson 1982; Mueller 1983). If only certain sets of shoppers are allowed into the political supermarket, then public policy will be inevitably biased toward those so privileged.

This conclusion has been theoretically challenged in a number of ways. Denzau and Munger (1986), for example, argue that the electoral incentives of politicians provide powerful incentives for politicians to represent those who are not themselves actively engaged in shopping. And Becker (1985) has suggested that the rents derived by those in a first round of successful purchases increase the incentives of those outside the policy supermarket to enter and lobby in a second round. And perhaps most telling, Lowery et al. (2005) have argued that it is neither differential incentives to free ride nor the

relative wealth of an economic sector that explains differential rates of participation. Rather, they find that different rates of mobilization reflect differences in the economies of scale in industrial production across different industries. It is these variations that directly determine numbers of firms in a sector and are then indirectly replicated in the economies of scale of interest representation that determine lobby registrations. And further, these differences in the economies of scale of interest representation work to reduce the relative representation of concentrated industries as interest systems become crowded.

More to the point, many empirical studies of lobbying influence have not found that public policy outcomes are easily explained by the dominance of business interests (Lowery 2013). Mark Smith's 2000 book, *American Business and Political Power*, found that when business interests are united on a policy proposal, the likelihood of Congress acceding are markedly diminished. Kollman (1998) found that direct lobbying by business in the face of public opposition has little effect. Page et al. (1987) reported that business media advocacy more often than not had the opposite of the intended effect. Gray and Lowery and their students found that the number and diversity of interests lobbying state legislatures have only a marginal influence on policy liberalism (Gray et al. 2004). Further, the density and diversity of interest systems are far more determined by the size and diversity of legislative agendas than the reverse (Gray et al. 2005). And Gerber's 1999 study of referendum voting in the American states found that massive infusions of cash into state referendum campaigns by business interests almost always fail to move voters. Even more telling perhaps, in the latest major study based on a sample of issues lobbyists were working on, Baumgartner et al. (2009) reported that most fail to even get their issue on the agenda. None of these studies is unique. Indeed, three major surveys of the literature—by Smith (1995), Baumgartner and Leech (1998), and Burstein and Linton (2002)—reached remarkably similar conclusions. These findings, therefore, are broadly consistent with the earlier findings of Heinz et al. (1993) in which they described the world of lobbying as lacking nearly any of the certainty of a supermarket.

This is not meant to imply that the literature provides no evidence of influence or that the diversity of interest organizations lobbying on a given issue does not matter. This is certainly not the case. But even within a single policy domain like health care, the mix of different types of interest organizations has been shown to be sometimes influential and sometimes not, largely depending on whether the specific issue under consideration is salient in terms of public opinion (Gray et al. 2007a; 2007b, 2010), a result replicated across other policy domains (Lowery 2013). In short, the simple transactions model of public policy implicit in Olson's (1965) analysis of the cross-sectional policy consequences of differential incentives to free ride has not been unambiguously supported by subsequent research (Lowery and Gray 2004).

The second way in which Olson's analysis of incentives to free ride has been applied to the study of public policy is temporally. His 1982 book, *The Rise and Decline of Nations*, combined his transactions interpretation of lobbying and his analysis of interest system diversity to assess the long-term consequences of interest representation for

economic growth. The argument is straightforward. If interest communities are biased toward those with narrow interests and against those with broad, diffuse interests, and if public policy is readily purchased in a political supermarket, then public policy will be biased toward adopting special advantages for narrow interests that reduce the long-term growth of an economy. This implication follows from the fact that protections from robust markets that provide the foundation for economic growth provide powerful selective incentives for lobbying while long-term growth is a diffuse, collective benefit.

Even worse, Olson (1982) argued that the growth-impeding process accelerates as polities age. That is, Olson asserted that the densities of interest systems increase linearly over time as ever more latent interests discover solutions to free riding. Since such solutions, especially recourse to selective benefits, were assumed to have little relation to collective benefits per se, they should allow interest organizations, once they overcome barriers to free riding, to simply accumulate over time like barnacles on the bottom of a ship. Indeed, Olson (1982: 40) asserted that "Organizations with selective incentives in stable societies normally survive indefinitely." This combination of ever more interest organizations seeking selective benefits in the form of protections against the rigors of the market should lead to ever diminishing opportunities for economic growth. The only escape Olson (1982) allowed from this institutional sclerosis hypothesis is reorganization of the interest system along corporatist lines. That is, if interest systems are dominated by encompassing interest organizations where the collective benefits of shared economic growth might overcome narrow incentives for protections against market competition, then the long-term negative economic consequences of interest representation might be avoided.

Olson's (1982) book and its institutional sclerosis hypothesis attracted an inordinate amount of scholarly attention when it was published, most notably in the collection of articles in Mueller's 1983 book, *The Political Economy of Growth*, and it continues to do so. Indeed, *The Rise and Decline of Nations* has attracted well over 1,800 citations since it was published, and the institutional sclerosis hypothesis about the relationship between interest community structure and growth-impeding policies has been subject to many tests. But while using a variety of methods, they have at best produced very mixed results. This is best illustrated by Heckelman's (2007) meta analysis of 50 tests of the model, an analysis that included my own (Gray and Lowery 1988) replication of Olson's examination of patterns of economic growth in the American states and which failed to support the institutional sclerosis hypothesis. More generally, some aggregate-level assessments have found a relationship between the structure and composition of interest communities and rates of economic growth over time and many others have not.

While this might suggest that Olson's hypothesis remains unproven, but yet viable, there are good reasons to think that such aggregate-level tests rest on shallow foundations. That is, two assumptions underlying Olson's analysis remain problematic. The first is that interest organizations, especially business, are always and readily able to purchase policies, especially policies to secure selective protection against market

competition. But we have seen that there are many reasons to doubt that public policy operates along the model of a simple supermarket that uniformly advantages business interests. Second and perhaps even more fundamental is Olson's assumption that interest organizations, once they overcome problems of free riding via selective material benefits, will survive indefinitely and thus accumulate over time. This assumption was central to Olson's claim that, barring reorganization of interest systems along something like corporatist lines, institutional sclerosis was a terminal disease. Yet, we now know that interest organizations, especially those representing business, frequently die (Gray and Lowery 1995; Berkhout and Lowery 2011a, 2011b), that interest systems can collapse in a catastrophic manner (Brasher et al. 1999), and that the growth of interest systems is typically capped so that births of new organizations decline and the death rates of established ones increase as interest systems become more crowded (Lowery and Gray 1995; Nownes 2005; Nownes and Lipinski 2005). Given lack of support for these two underlying assumptions of Olson's institutional sclerosis model, it is not clear that even unambiguous evidence of any aggregate-level relationship between economic growth and the structure of interest communities would be sufficient to support it.

CONCLUSION

This chapter began with a note of skepticism about Olson's work. That skepticism inhered in how differently students of interest politics and public policy have come to view Olson's contributions, as reflected in Baumgartner and Leech's (1998: 75) conclusion that "the problems discussed by Olson may have been given more prominence in the interest-group literature than they deserve." As we have seen, two findings of the interest politics literature have severely undercut Olson's claims about interest representation and its role in public policy. First and most importantly, organization mobilization is much more complex than Olson assumed. Organized interests have discovered many ways beyond the use of selective material benefits to overcome free riding, and joiners are something more than cool calculators of costs and benefits. And second, Olson's assumption of a simple policy supermarket dominated by business interests not subject to free riding in which an ever greater number of privileged shoppers purchase selective benefits is far too simple a model of the policy process and the role of organized interests in it. Given these findings, Baumgartner and Leech's (1998: 75) conclusion seems warranted.

In another sense, however, both public policy and interest organization scholars may seriously underestimate the true importance of Olson's analysis. This is most readily evident in research on organized interests. We have seen that organized interests have found any number of ways to overcome free riding. Indeed, they turn themselves inside out to solve collective action problems in ways that augment the power of patrons, sponsors, and entrepreneurs. Organized interests are compelled to live in complex

ecologies of organizations where relationships among them may be more important to survival than narrow exchanges with their members (Gray and Lowery 1996). And those relationships with their members in terms of the inducements offered for joining and the messages organizations use to exploit cognitive biases are profoundly influenced by the ever-present incentive to free ride. Thus, the problem of free riding almost certainly influences the structure and behavior of organized interests, if not in the simple way Olson imagined.

It almost certainly influences what they do as well as when they try to influence public policy. There is nothing to suggest that organized interests are unimportant in public policy. Rather, the cumulative body of work that has developed in studies of influence suggests that the impact of lobbying is far more complex, contingent, and uncertain than was assumed by Olson. More to the point, what organizations lobby on, how they lobby, and how successful they are is likely highly conditioned by their internal organizational needs to overcome problems of free riding (Rothenberg 1992; Lowery 2007). Thus, to understand better how organized interests influence policy, we need a richer appreciation of the internal life of interest organizations and how this shapes lobbying goals and tactics. Simple reference to Olson to explain unexpected findings on the role of organized interests should no longer be sufficient. And in terms of the focus of this chapter, this means that the logic of collective action will continue to bear on our efforts to understand public policy outcomes, if, again, perhaps not in the precise way suggested by Olson.

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CHAPTER 16

THEODORE J. LOWI, *THE END OF LIBERALISM: THE SECOND REPUBLIC OF THE UNITED STATES*

THOMAS T. HOLYOKE

CITED well over 15,000 times in scholarly journals, Theodore J. Lowi's *The End of Liberalism* qualifies as one of the most famous books in the history of political science.¹ Its appeal even reaches beyond academic circles with its dire warnings about the growing scope of government power and assumed responsibility in the United States. The delegation of policy-making authority to the executive branch, the abdication of Congressional responsibility, the dominance of interest groups over public policy-making, and a general erosion of representative government responsive to the popular will are all major themes in Lowi's book. First published in the late 1960s, it fit well with the growing skepticism of the role of government and interest groups in American society.

With its fierce criticism of good liberal intentions gone awry, its black cover and provocative title in fiery orange letters, one might be tempted to dismiss it as a conservative polemic against the Democratic party and modern government. But Theodore Lowi is not a talking head leveling vitriol against the political system to make money. He is a thinker making a powerful, well-reasoned argument that resonates even with his critics, though sometimes the prose style is a little dense. While he clearly lays a fair amount of blame on John Kennedy and Lyndon Johnson for building a nanny-state, they were themselves reflections of an American belief that the world's wealthiest society had obligations to its people but were unable to foresee the long-run consequences of alleviating people of personal responsibility and growing government's responsibility. It is why *The End of Liberalism* has been in continuous print since 1969 and is thus worth the effort to assess its impact on political science and public policy scholarship to see whether its arguments and assumptions have held up.²

INTEREST GROUP LIBERALISM AND DELEGATED AUTHORITY

The End of Liberalism is Lowi's indictment of modern policy-making in the United States, his attack on what he considers the liberal view of the state and its relationship with its citizens and ramifications for how government uses policy to serve citizens. No longer is public policy made through the process laid out in the Constitution. No longer are representatives elected by citizen majorities to Congress designing, negotiating, and enacting laws. Indeed, Lowi argues, Congress is not making law at all. Real law-making requires Congress to set clear directions, standards, and penalties (pp. 92–3). This is the rule of law. Only elected members of Congress represent the American people, so only Congress is invested by the Constitution with the authority to make law for the people. Yet since the New Deal era Congress has delegated away its law-making responsibility, and thus the legitimacy of policy-making in a representative democracy, to the executive branch (pp. xv–xvi and 43–4). All Congress requires of administrative agencies is that they solve vaguely defined social problems by engaging with organizations representing the citizens assumed to be at risk or denied justice under existing social structures (p. 92). Together agencies and interest groups craft regulations and spending programs serving these constituencies, usually with the interest groups, the presumed legitimate voices of these constituencies, determining who is regulated and who receives public largess (p. 93). The result is governing through interest groups backed by administrative rule-making rather than legitimate legislative statute—policy without law.

Why did this happen? Lowi argues that liberal theorists and politicians, convinced that the government could and should play a positive role in improving American society, began with the 1930s New Deal to provide redress for the injustices created by social and economic inequalities by using the state's authority (p. 42). Poverty, racial and gender discrimination, economic opportunity, lack of access to education, housing, and medical care all became social problems the government was obligated to solve. Society was complex, so its problems must also be complex, and, beginning with the Great Society of the 1960s, was the culprit for individual problems rather than any particular failures of that individual (p. 201). Such complex social problems were best solved by specialists in the executive branch. Assuming that the constituencies to be helped knew best how to solve their own problems, agency specialists were expected to let organizations representing these populations create, and sometimes even administer, policy solutions (p. 51). Indeed, they often got to define the very problems in need of solving.

So when President Lyndon Johnson's Great Society, the crowning manifestation of liberal thinking for Lowi, threw open Washington's doors to any group of citizens who could claim that society's growing complexity and economic stratifications were denying them justice, organized interests responded in droves. Interest groups need

not even struggle with each other for redress or slices of the public budget. Through its committee and subcommittee system, Congress segmented its policy-making authority into highly autonomous sectors, what scholars call policy domains. Law-makers on committees overseeing these domains responded to demands of organized interests for redress of social and economic grievances by handing legislative responsibility over to new administrative agencies to work with the organizations claiming to speak on behalf of these groups of aggrieved citizens. No group-claimant was presumed to be more or less worthy of government assistance than any other, so every interest was served with policy and benefits. What was provided and how it was provided was devised by their interest group and provided through the administrative agency with little direction or even oversight from elected officials. This is what Lowi called interest group liberalism:

It is liberal because it is optimistic about government, expects to use government in a positive and expansive role, is motivated by the highest sentiments, and possesses a strong faith that what is good for government is good for the society. It is interest-group liberalism because it sees as both necessary and good a policy agenda that is accessible to all organized interests and makes no independent judgment of their claims. It is interest-group liberalism because it defines the public interest as a result of the amalgamation of various claims. (p. 51)

The aggregate result is that, as the role of the state's involvement in society grew, its power and legitimacy was dispersed with policy developed and administered by unelected bureaucrats and organizations claiming to represent aggrieved interests. Conflict between interest groups over the shape of public policy and who benefits from it was mitigated by dividing policy-making responsibilities into highly autonomous domains controlled by an agency and the principal client groups perpetuating the status quo (pp. 38 and 60). It became a new form of interest group-based representative government, what Lowi called (in his second edition) the Second Republic (pp. 271 and 291). So invested were social groups and politicians in this system that not even self-described rule-of-law-favoring conservatives have been able to set limits. Lowi dedicates quite a bit of space to criticizing President Richard Nixon for failing to turn back the liberal vision of government (e.g. p. xiii). It brings to mind Ronald Reagan's similar failure to shrink the size of government or defund the left by stripping public money from interest groups (Peterson 1990).

Much of the rest of *The End of Liberalism* gives examples of interest group liberalism in action, using business regulation, railroad management, urban planning, organized labor, and welfare as case studies. A good example is his analysis of agriculture policy in Chapter 4:

That agricultural affairs should be handled strictly within the agricultural community is a basic principle established before the turn of the century and maintained ever since then without serious reexamination. As a result, agriculture has become neither public nor private enterprise. It has been a system of self-government in

which each leading farm interest controls a segment of agriculture through a delegation of national sovereignty. Agriculture has emerged as a largely self-governing federal estate within the federal structure of the United States. (p. 69)

Lowi describes early twentieth-century Congresses enacting a multitude of price support programs supporting various crops and geographic regions, providing farmers with tax breaks, insurance programs, and cash payments. Control of these programs lay in the US Department of Agriculture (USDA), but they were essentially run by the agricultural groups advising USDA (Browne 1988 provides a good description). Congress vested so much legitimacy in these interest groups as the official voices of American farmers that any effort by law-makers to make these programs more efficient through modification or consolidation was met by impenetrable resistance (pp. 72–5). Even as these agricultural interests made farmers dependent on USDA programs, they were the first to cry “federal encroachment” when any reforms threatening their autonomy were attempted (p. 72). The result is an array of antiquated and wasteful price support programs defended by commodities interest groups and their allies in USDA, all resisting change so fiercely that no Congress, president, or agriculture secretary will ever regain control.

Lowi was equally critical of federal welfare policies, which he argued are so perverse that involved interest groups actually have incentives to keep the people they represent poor lest the flow of federal largess cease (pp. 223 and 235). Thus Johnson’s War on Poverty insured that poverty would continue. His most damning attack is on urban planning policy, which increased geographic racial segregation so much that it ought to be compared to South Africa’s racial segregation policy of Apartheid (p. 237)! The irony, Lowi argues, is that all of this Congressional delegation and interest group empowerment was done with the best of intentions (pp. 233 and 295). By delegating authority, liberal law-makers emasculated themselves, losing the ability to solve the problems they want to solve (pp. 58–9).

CONTROLLING THE TURBULENT PLURALIST OCEAN

Lowi’s argument brought together the study of public policy with interest group politics. If a few powerful interest groups controlled policy in a domain, then to learn which social and economic interests were advantaged on prevailing policy all one had to do was look to see which groups had the greatest access to the administering agency. This conclusion was strikingly at odds with prevailing theory regarding group politics and policy-making in the 1960s. Lowi was not the first to suggest that scholars badly misunderstood the real consequences of interest group pluralism, but so sharp was his argument, and so heavily was it supported by evidence, that it arguably tipped the

debate against proponents of interest group pluralism. *The End of Liberalism* became an obligatory citation in every interest group scholar's literature review.

In its time pluralism, as laid out by David Truman (1951), was a powerful, positive theory of American politics. Powerful because it seemed to explain most political behavior and supported the democratic ideal of broad public participation in governance, albeit through interest groups. Positive because it predicted how public policy could change as new social and economic interests mobilized for political combat. Even later versions of it, like Robert Dahl's (1956) which embraced inequalities in interest group influence as long as it reflects inequalities of citizen interest, still strikes optimistic notes about the role of political organizations in politics and policy-making.

Lowi revealed interest group pluralism's dark side. He accepted the pluralist belief that American society was fundamentally group-oriented, that increased specialization in the American workforce inclined people, as well as their elected leaders, to see their interests as best represented in the advocacy of political organizations (pp. 18–19). Industrialization and new technology forced people to become increasingly specialized in what they did, where they lived, and what they believed, stripping them of their self-sufficiency and alienating them from each other. Such narrow, specialized interests could only be faithfully represented by interest groups (p. 20). Acknowledging that this social complexity and professional specialization is far beyond their ability to regulate by statute, elected officials delegated significant authority to administrative agencies so they would work with large interest groups representing these many interests to resolve conflicts and provide social justice for inequalities created by social stratification and market competition (pp. 27, 30–3).

So liberal politicians harnessed pluralist theory to justify using interest groups as proxies for citizen participation, replacing elected representation, but they also cut out the one element pluralist theory assumed essential for creating policy in the public interest—group competition. Traditional pluralists assumed that competition between organized interests resulted in public policy that balanced the needs all affected constituencies, even if it did not do so equally. But by dividing policy responsibilities into autonomous domains, law-makers calmed the turbulent pluralist ocean. Agencies in each domain could work quietly with just the largest, most politically powerful organizations representing target populations without interference from groups in other domains; these “peak” organizations were the legitimate representatives of those citizens (pp. 30–3). They articulated the constituency's real needs and helped agency officials devise policy solutions and dispense benefits to that constituency, even if that meant shutting out other organized interests with contrary views (p. 58). For instance, to regulate industries and professions, this meant working through the great trade associations:

As much as the administrative employee and internal bureaucratic apparatus help to measure the administrative component in production, so does the *trade association* indicate the degree to which the commercial dimension of the system—that is, the market economy itself—has come also to be an administrative process. The

trade association is basically an administrative structure whose most important mission is regularizing relations among participants in the same industry, trade, or sector. Where the market seeks competition, the trade association seeks to administer. (p. 27, emphasis in the original)

He sums it up, “self-regulation through market competition [becomes] self-regulation through politics” (p. 22).

Lowi’s pessimistic view of pluralism was attractive to scholars inclined to be critical of interest groups during the late 1960s and 1970s because many assumed that powerful interest groups merely served the interests of powerful business interests. It also turned out that Lowi’s argument was more consistent with the structure of America’s policy-making apparatus than traditional pluralism. Congress had certainly expanded and divided its committee system into fairly exclusive policy jurisdictions (Davidson 1990). The executive branch had certainly grown dramatically and decentralized, and seemed quite uncontrollable by elected officials (Hecl 1978). Public budgets were increasing, creating deficits, but seemingly immune to meaningful reform because that might cut off the flow of benefits to constituencies that had become used to them, and used to the self-regulation that decentralization and delegation had provided them.

And interest group influence certainly appeared to have become balkanized into autonomous, self-regulating policy domains, which scholars like Grant McConnell (1966) called subgovernments, though some prefer the term “iron triangle.” Judging by the number of times Lowi cites McConnell in *The End of Liberalism*, it is likely that subgovernments were the very islands of power he was criticizing. Even when traditional pluralism was at its height, scholars were starting to become nervous about concentrations of power and the “capturing” of regulatory agencies by the interests they were supposed to be regulating (Stigler and Friedland 1962; Bernstein 1955). But it is also important to point out that Lowi was a pluralist, rejecting the arguments of C. Wright Mills (1956) and William Domhoff (1967) that all real political power was concentrated in the hands of a small elite (p. 34). He simply considered pluralism’s consequences of dispersing the state’s authority to be dangerous to representative democracy.

Delegated Authority and the Great Recession

Is power dispersed to interest groups that dominate policy domains still an accurate description of policy-making today? One piece of supporting evidence is the growing propensity of highly skilled white-collar professions to be licensed by their trade associations, which essentially is self-regulation backed by government authority. Familiar examples are the licensing and regulation of doctors by the American Medical Association and lawyers by the American Bar Association, but licensing and certification has expanded far beyond these venerable organizations. According to 2011 data from Lobbyists.info regarding trade associations big and small in the United States, of the 7,150 active nationally, 20 percent have some kind of licensing and certification

program for the profession they represent, and some have many. The International Fitness Professionals Association has the most at 47 percent. Indeed, the sheer number of trade associations alone may be evidence that Lowi's argument is as relevant today as in 1969 or 1979.

Certainly some events over the years since Lowi's book appeared seem to conform to his argument that government agencies are often more concerned with preserving the health of the interests they regulate than they are of the public interest. As the national economy collapsed in early 2009, the United States government responded by loaning significant amounts of money to banks and investment firms to keep them afloat. Although a few like Lehman Brothers were allowed to perish, giants like Citibank and Bank of America were saved at taxpayer expense even though many blamed them for having created the dangerous housing bubble that led to the Great Recession. Yet blame also goes to financial regulatory agencies for letting the banking and investing industries create the bubble in the first place. They were more concerned about the financial health of these industries than protecting consumers (Mierzwinski 2010).

In the late 1990s consumers were shifting their money from bank savings accounts to Wall Street investment funds in large numbers, frightening bankers who were forbidden from investing by the Depression-era Glass-Steagall Act of 1933 (Stiroh and Poole 2000). Financial regulators had already evolved a culture of service to the financial institutions they regulated, whether they were banks, savings and loans, or investment companies, often referring to them as "clients" and "customers" (Williams and Jacobsen 1995), a sign that the legal mandate on regulators to keep financial institutions safe and sound meant helping them stay profitable. To help their customer-banks, both the Office of the Comptroller of the Currency in the Treasury Department and the Federal Reserve System stretched their authority to permit banks limited entry into securities investing, and selling insurance too, to remain profitable (Indick and Domenici 1996). The Securities and Exchange Commission (SEC) retaliated on behalf of its client-firms like Bear Stearns and Morgan Stanley (Weidner and Mandaro 1999). Could agencies be more clearly captured by the industries they were supposed to regulate?

Only when the conflict between these economic sectors became unmanageable did regulators and the great trade associations, like the Securities Industry Association and the American Bankers Association, demand that Congress repeal Glass-Steagall and create a general framework for how (not if) these industries might merge and operate. Law-makers responded with the Gramm-Leach-Bliley Act of 1999 (GLB) authorizing the formation of multi-purpose financial institutions to take deposits, invest money, and sell insurance all in one corporate structure. Just how these institutions would merge and operate was left up to the regulators, especially the Federal Reserve which was set up as arch-regulator, the very type of delegation of authority Lowi warned of. To keep these super-firms competitive with European counterparts, Congress the following year passed the Commodities Futures Modernization Act (CFMA) permitting complicated forms of investments called derivatives with only minimal oversight from the SEC and the Commodity Futures Trading Commission. Congress instructed

regulators to simply make sure that these financial institutions conducted their derivatives investing in a manner safe and sound for its health, not the health of investors and consumers (GAO 1999: 10).

GLB and CFMA allowed financial companies to be more ambitious in their profit-making strategies. One strategy was to expand the number of home loans to customers, which they could then bundle together and sell on the secondary mortgage market to other investment firms and government-sponsored institutions like Fannie Mae and Freddie Mac (Andrews 2008). The result was a housing bubble, enormous numbers of loans made to people who could not afford to repay them. Another strategy was to take more creative risks on the future returns of investments (Comiskey and Madhogarhia 2009). Perhaps the most notorious investment vehicle was the credit default swap where sellers of loans made money if loan recipients, including home mortgage recipients, defaulted. Ultimately firms like American Investments Group (AIG) found themselves unable to pay the investors who had sold them the investments when large numbers of mortgage loan recipients began defaulting in 2008 (Mierzwinski 2010).

Where were the regulators? In 2008 testimony before the House Committee on Oversight and Government Reform, former Federal Reserve chairman Alan Greenspan confessed that he had done little to prevent the housing bubble beyond increasing interest rates in 2004 (which may have made the problem worse) because he assumed that risk-averse financial institutions would regulate themselves. “Those of us who have looked to the self-interest of lending institutions to protect shareholders’ equity, myself included, are in a state of shocked disbelief,” he said (Andrews 2008). Administrative agencies allowing “customer” companies and their associations to regulate themselves under the assumption that they alone knew what was best for them and their survival instincts would lead to self-regulation, all enthusiastically supported by a Congress that gave up their own authority to regulate the financial industry, seems to fit comfortably with Lowi’s argument of government delegation, interest group regulation, and policy without law.

Competitive Pluralism

There may be plenty of cases supporting Lowi’s argument, but there is also evidence to the contrary. His argument regarding policy-making separated into autonomous domains quietly dominated by a few organizations is inconsistent with other trends in interest group politics. In 1983 Jack Walker published perhaps the first systematic evidence that the number of interest groups in the United States was increasing, and doing so quickly. This is now called the advocacy explosion of the late 1960s (Knoke 1986). By 2011 there were approximately 8,548 interest groups lobbying in Washington alone, along with anywhere from 19,000 to 26,000 lobbyists (Katel 2005; Holyoke 2014). Walker also found that American interest groups were diversifying, with an especially steep climb in the number of open-membership,

cause-oriented citizen groups. Jeff Berry (1993) argues that most of these citizen groups exist to break the stranglehold that trade associations allegedly have over their policy domains. Not only have they enjoyed many successes in environmental policy and consumer protection policy, but in some cases they forced Congress to be very specific in its standards rather than delegate authority. The Endangered Species Act, for instance, sets fairly clear standards regarding the classification of animal species as threatened and gives regulators little flexibility regarding when they must act to save them (Nie 2008).

It is hard to see how Lowi's argument about policy balkanization and captured agencies empowering a few large interest groups to self-regulate policy domains can be true when there are so many organizations fighting over the limits of policy and regulation. Interest groups in Lowi's view defuse conflict, they do not create it. True, the distribution of groups by policy domain is highly skewed, with over 2,000 lobbying on health care policy while less than 30 work on dairy policy according to the 2010 edition of the interest group directory *Washington Representatives*. Nonetheless, as early as 1978 Hugh Heclo argued that the subgovernment description of autonomous policy-making dominated by a few interest groups was just not accurate, and perhaps never had been. Moreover, the multi-year study of interest group influence in four policy domains conducted by John Heinz, Edward Laumann, Robert Nelson, and Robert Salisbury in the 1980s found almost no evidence of any domination (Heinz et al. 1993). Today interest groups scholars actually tend to start their projects assuming there is significant competition between interest groups over policy, though by no means equal and balanced power (e.g. Holyoke 2011; Brown 2012; Grossmann 2012). There is even a name for it—neopluralism. In sum, the default position today among interest group scholars is that Lowi's argument is no longer true, with some wondering if it ever was.

PUBLIC ADMINISTRATION AND THE RULE OF LAW

Lowi does not explicitly link the enormous delegation of authority to the bureaucracy to the Progressive Era's hope for a non-political government bureaucracy using science and rationality to administer policy, but it is hard to read his indictment of bureaucratic growth and empowerment without thinking of it. Since the publication of *The End of Liberalism*, the executive branch has added the departments of Energy, Education, and Homeland Security. Congressional oversight of the bureaucracy has diminished, and in fact the Congressional work week has shrunk to three days in Washington, with scholars despairing that the legislative branch has any capacity left at all for controlling the executive branch bureaucracy. Elected officials perhaps delegated so much authority in the belief that executive branch specialists were fundamentally better able to solve social and economic problems in

an ad hoc fashion than law-makers could through statute. The rule of law may have been sacrificed in the hope that the rule of science might be an effective and efficient substitute.

Since at least the early twentieth century law-makers and good government reformers have sought to define the fine line between deference to professional and scientific expertise and the responsibilities of legislators to make clear, if hard, choices, which is Lowi's rule of law. Specialists in a domain of public policy presumably know more about the needs of the target constituency and the technical, day-to-day details of implementation, so efficient government demands that Congress give them sufficient scope to act as they deem necessary to serve the public interest. The trick for elected legislators is deciding how much discretion to give before they sacrifice the trust constituents placed in them (Kettl 2009). Above all representative democracy demands that those who make the law must be held accountable to the people who elected them or there is no representation and no democracy (O'Donnell 2004). Elected officials have the crucial responsibility of setting clear legal standards binding agency specialists because this makes the law consistent rather than capricious. Lowi, of course, argues that this representative safeguard has failed, that the high degree of deference by elected representatives to professional administrators has cut citizens off from their government. Because they are specialists, he argues, agency staff make and implement policy, but since they are not elected, the people cannot hold them accountable. The only representation left in Lowi's Second Republic is through the interest group lobbyists (also unelected) with whom Congress requires agencies to consult. Whether agencies are overly influenced by business trade associations is less clear, with some finding little evidence of it (e.g. Golden 1998) and some finding a lot (e.g. Yackee and Yackee 2006). Either way, making public administration more efficient and less political, the Progressives' dream, leaves government unaccountable to its citizens.

While Lowi's argument regarding the controlling grip of interest groups may be wrong, he may be right about the freedom of the bureaucracy from Congressional control. The claim has long been made by scholars that neither the President nor Congress can effectively control the executive branch (e.g. Downs 1967; Niskanen 1971). And while some of the theoretical literature regarding Congressional control of the bureaucracy suggests a variety of circumstances where Congress might assert itself, the empirical evidence finds that Congress is nearly as powerless as Lowi suggests (e.g. Balla 1998; Shipan 2004). Oversight by Congress has almost certainly declined, partly because members of Congress themselves spend less and less time in Washington, DC. Over both domestic and foreign policy, Congressional committees hold fewer hearings (Ornstein and Mann 2006). This means fewer opportunities to require agency leaders to explain why they may have crossed the line between legitimate (bound by the rule of law) and illegitimate (wide discretion) regulatory action, assuming Congress has drawn such lines. Here at least Lowi's impact, even if it is indirect, has been significant.

MANY REPUBLICS

As a way of ending this chapter, it is worth asking whether Theodore Lowi himself still believes the arguments he made over three decades ago in the second edition of *The End of Liberalism*. His later writings on how the welfare state shifted responsibility for joblessness and crime from individual responsibility to society itself being culpable, and arguing public administration would be legitimate if every citizen could be involved, just as every citizen can vote (Lowi 1986, 1993), suggests his ideas have not changed much. What is interesting, though, is his changing view on how representative government in the United States has changed over time. He subtitled his second edition *The Second Republic of the United States*, arguing more poignantly than in the first edition that interest group liberalism has fundamentally changed how the nation is governed, shifting America away from the rule of law to representation by interest groups and rule by technicians and specialists in executive branch agencies. The nation today is thus fundamentally different nation than prior to the New Deal and the emergence of the welfare state.

In his 2008 James Madison Lecture to the American Political Science Association, published in 2009, Lowi presented what appears to be a rather modified view. Citizens of the United States, he argued, have experienced many versions of representative government; there have been many republics. The First Republic was the brief era of very limited government between American independence from Great Britain and the decision at the Constitutional Convention to toss the Articles of Confederation and write a whole new constitution with a stronger central government. Ratification of the Constitution was the dawn of the Second Republic, and it was replaced by the Third Republic in 1833 when the Supreme Court held in *Barron v. Baltimore* that the Bill of Rights did not apply to the states. The New Deal was the foundation for the Fourth Republic for it began a gradual but consistent delegation of legislative authority to the executive branch, though Lowi blames John Kennedy and Lyndon Johnson for promoting a vision of government that seemingly could cure all of society's ills.

George W. Bush and his administration, Lowi argues, created the Fifth Republic, one of nearly absolute presidential power, what some Republicans have called the "unitary executive." After the bailout of automotive companies and the expansion of government it is hard to believe that Lowi thinks any more highly of Barack Obama. This may well be a crucial change in his thinking, or at least a substantial change in his view of how America has evolved since the 1970s, from his book. The difference from his view in *The End of Liberalism* is that he seems to believe that another shift in political power has taken place, that presidents have taken back a fair amount of power from the bureaucracy and interest group community. Administrative agencies are now on a tighter leash from the White House. What roles interest groups still play in developing and implementing policy is unknown. What remains consistent is the irrelevancy of the elected Congress, state governments, and of course the people themselves. In

both cases his views remain attractive to scholars who distrust government and power, whether that power is lodged in the Oval Office or delegated to unaccountable agencies and interest groups. In both cases, the highly decentralized republic, the confederation of states the United States was before the ratification of the Constitution is lost and unlikely to return. In no case are we likely to aspire to what Lowi recommended at the end of *The End of Liberalism*, the creation of “juridical democracy” where the rule of law is restored and majority rule representation returned as the sole legitimate source of the state’s authority.

NOTES

1. A simple search on JSTOR using *The End of Liberalism* as a search term in political science and public policy journals.
2. All of the page citations and commentary here are from Lowi’s heavily revised edition published in 1979.

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CHAPTER 17

JACK L. WALKER, “THE DIFFUSION OF INNOVATIONS AMONG THE AMERICAN STATES”

DAVID LEVI-FAUR

INTRODUCTION

JACK L. Walker’s article “The Diffusion of Innovations among the American States” (*APSR*, 1969) is considered a classic in the study of politics, policy, and administration well beyond the study of American politics (Baumgartner 2006). In late 2013, almost half a century after its publication, the paper is one of the most frequently cited papers in the *American Political Science Review*.¹ Its impact as measured by the number of annual citations is growing, and in many respects it still defines the field. The paper has attracted so much attention partly because it was the first major political science research paper to introduce a diffusion perspective into the field. But it also represents an increase in the interest in the phenomena of diffusion among scholars of international relations, comparative politics, and European and global public policy. Probably the most notable renewed interest is the impact on the more qualitative frameworks of policy learning (Bennett and Howlett 1992) and policy transfer (Dolowitz and Marsh 1996). After Walker’s study was published, diffusion became a major topic in public policy and administration. What Walker tells us—and, as I’ll argue later, we are still digesting—is that interests, behavior, practices, and norms are highly interdependent. The likelihood of change in one actor’s interests, behavioral practices and norms is positively correlated with the likelihood of similar change among other actors. Still, almost half a century after the publication of the study, the full implications of this insight are yet to be absorbed in political science and in the social sciences generally.²

The origins of the diffusion literature in the social sciences lie largely in the discipline of sociology and in the work, inter alia, of Tarde (1903), Ryan and Gross (1943), and Coleman et al. (1957). The cumulative knowledge in the field was meticulously documented by the late sociologist Everett M. Rogers, in five editions of *The Diffusion of Innovations* (1962, 1971, 1983, 1995, 2003). These works on diffusion were long highly influential everywhere but in political science. Invisible walls separated the study of politics from the study of society, and this situation has only partly improved since the publication of Walker's paper. The invisible walls are still there and prevent a more productive exchange within the social sciences and beyond them. Walker's interest in diffusion did not originate in the literature written by sociologists or other social scientists but instead in his own experience as the overseer of an internship program in his first year as an academic in the tenure track. In an interview that he gave in 1984 he told the story of how he became interested in diffusion:

One of my first duties at the University of Michigan was to oversee an internship program in Lansing, the state capital . . . I was in Lansing almost every week and I often spent hours between meetings with nothing to do. As much out of boredom as for any scientific purpose, I often visited legislative hearings while waiting for my next appointment. Discussions in these hearings about the adoption of new legislation usually led immediately to comparisons with the experiences of other states. Bureau chiefs were almost always asked by legislators, many of whom were attorneys, if there were any precedents for the new programs that they were proposing. It appeared that the legislators were trying to control the administrators, in sorting out the many complex issues before them, by emulating the decisions of legislators in other states who had already dealt with similar problems. Legislators were much more inclined to accept a new idea if it had been given a trial in a state that was similar to Michigan. The proposals made by the civil servants were derived from specialized publications and conferences sponsored by their professional societies. The legislators were acting as gatekeepers for proposals arising from these expert networks and only approved of innovations that had proved successful in the states they regarded as legitimate points of comparison. I reasoned that if legislators were employing this same decision rule in all states, a stable pattern of adoption of new ideas must exist, perhaps with the more cosmopolitan states acting as leaders, and the more parochial ones acting as followers.³

Thus, it was a sort of a coincidence that led Walker to the design of an ambitious research program (and to "distract" him from his studies of pluralist forms of power in American politics). Coincidence aside, his ambitions were considerable, and so were the fruits of his work. In what follows I discuss Walker's work and assess its reception in, and its influence on, our field. At the same time I offer some suggestions on the future progress of diffusion scholarship and on the potential of the approach to redefine our understanding of politics and policy. I conclude with the observation that, while the study of diffusion is flourishing as never before, it is still far from fulfilling its potential.

THE ARTICLE AND ITS RECEPTION: AN OVERVIEW

Walker's paper is a study of a rather common but under-researched form of decision-making, a kind of interdependent decision-making where decision-makers interact with one another whether they know it or not.⁴ It deals with "one of the most fundamental policy decisions of all: whether to initiate a program in the first place" (Walker 1969: 880). It is therefore a study of decision-making with regard to the costs and benefits of keeping the status quo relative to an alternative situation where one follows the interest, behavior, practices, and norms of other, close or distant members of a group. In the language of policy diffusion, it is also a study of interdependent decision-making in a collective setting, where the members of a group of actors successively adopt the same policy, in partial or full response to the decisions of other members of the group. The data and the laboratory for the study are the American states, and the explanandum is operationalized as an aggregate measure of the speed of adoption. States, Walker suggested, have traditionally been judged according to the relative speed with which they have accepted new ideas. Speed and group convergence on similar policies is a proxy of leadership. Some states are leaders while others are merely followers and laggards. Innovations are implicitly assumed to be good, and so, more explicitly, is speed. "I assume that the pioneering states gain their reputations because of the speed with which they accept new programs" (Walker 1969: 882). The research questions that Walker therefore poses are, first, why do some states act as pioneers by adopting new programs more readily than others, and once innovations have been adopted by a few pioneers; second, are there more or less stable patterns of diffusion of innovations among the American states; and third, if so, what are they?

To answer these questions Walker collected data on 88 different new programs which were enacted by at least 20 state legislatures prior to 1965, and for which there was reliable information on the dates of adoption. These programs are distributed across 12 areas of government: welfare, health, education, conservation, planning, administrative organization, highways, civil rights, corrections, labor, taxes, and professional regulation. Most of the relevant legislation was adopted during the twentieth century, but 16 of the programs diffused primarily during the latter half of the nineteenth century. Once the 88 lists of dates of adoption were collected, they were used to create an innovation score for each state. The first step was to count the total number of years which elapsed between the first and the last recorded legislative enactment. Each state then received a number for each program, which corresponds to the percentage of time that elapsed between its original adoption and its adoption in that state. The first states to adopt the program received a score of 0, and the last state to do so received a score of 1.⁵ The innovation score for each state is simply 1 minus the average of the sum of the state's score on all issues. The higher the innovation score, therefore, the faster

the state has been, on average, in responding to new ideas or policies. The composite index, which would be challenged shortly after the article was published, resulted in a ranking of the US states according to their innovativeness. At the top of the list, with the highest scores, were some of the largest and richest states such as New York, Massachusetts, and California, while at the bottom of the same list were states such as South Carolina, Wyoming, Nevada, and Mississippi. The index has proved to be most useful for the study of the economic and political correlates of the adoption of diffusion. In some parts of the analysis Walker divided the innovation score over three periods. In the later parts of the paper, and in order to demonstrate the role of diffusion, he went on to deconstruct the index, suggesting that it masked some pertinent information. A more useful representation of the ranking, he wrote, would have to be in the form of a tree.

At the top of the tree would be a set of pioneering states which would be linked together in a national system of emulation and competition. The rest of the states would be sorted out along branches of the tree according to the pioneer, or set of pioneers, from which they take their principal cues. (Walker 1969: 893)

The innovation score was a bold move that condensed time, space, as well as policy attributes such as degrees of contestability and saliency into one big dependent variable represented by a composite score. This score included policies that were diffused between 1870 to 1966. It covered laws that applied diverse policy instruments, from regulation to fiscal expenditures, via the creation of new state organizations. These laws covered everything from the economic to the cultural via the social, with policies having highly diverse impacts, from minor to large and from the highly contested to the highly consensual. And they covered states as different as South Dakota and New York, and from the Rocky Mountain states to Florida. As we will see, the innovation score attracted many admirers, very few followers, and strong criticism.

Having provided a preliminary measurement of the phenomenon of innovation, Walker set out to explain it. Why, he asked, should New York, California, and Michigan adopt innovations more rapidly than Mississippi, Wyoming, and South Dakota? His first step was to look at the usual suspects, that is, far away from diffusion. Based on previous research, it was expected that the larger, wealthier states, those with higher degrees of industrialization and urbanization, would have the highest innovation scores. In these places there were more resources and creativity to allow both a more experimental approach and the wide distribution of resources. On the top of these potential explanations, he assessed the innovation score against certain institutional variables such as the degree of party competition and a state's system of legislative appointments. He hypothesized, first, that parties which often faced closely contested elections would try to outdo each other by embracing the newest, most progressive programs, and that this would naturally encourage the rapid adoption of innovations. Second, those representatives from newly developing urban areas would be more cosmopolitan, better informed, and more tolerant of change, hence more

likely to adopt innovation. His findings went a long way toward corroborations of these expectations. Innovativeness was found to be positively correlated with bigger, richer, more urban, and more industrial states as well as with states which have more fluidity and turnover in their political systems and where legislators more adequately represent their cities.

These results, however, did not satisfy Walker's curiosity, and he set out to deepen his analysis by looking at how innovations spread from the pioneering states to those with lower innovation scores. This is where the diffusion perspective comes in. The analysis is first based on a conceptualization of actors that draws on the founding fathers of behavioral decision-making and the model of bounded rationality. Walker's decision-maker is "struggling to choose among complex alternatives and constantly receiving much more information concerning his environment than he is able to digest and evaluate" (Walker 1969: 889). The limits of rationality imposed by human capacities prevent the decision-maker from maximizing his benefits in every situation. Instead, he chooses a course of action which seems satisfactory enough under the circumstances. The rule of thumb that Walker's decision-makers employ says "look for an analogy between the situation you are dealing with and some other situation, perhaps in some other state, where the problem has been successfully resolved" (p. 889). State decision-makers, he asserts, are constantly looking to each other for guidance on action in many areas of policy, such as the organization and management of higher education or the provision of hospitals and public health facilities. In all cases, however,

the likelihood of a state adopting a new program is higher if other states have already adopted the idea. The likelihood becomes higher still if the innovation has been adopted by a state viewed by key decision makers as a point of legitimate comparison. Decision makers are likely to adopt new programs, therefore, when they become convinced that their state is relatively deprived or that some need exists to which other states in their "league" have already responded. (Walker 1969: 896-7)

What emerges from the study, Walker concluded, is a picture of a national system of emulation and competition and interpretive framework which moves between the poles of rational decision-making on the one hand and sociological institutionalism on the other. To make the point even stronger, he wrote, in fact, "I am arguing that this process of competition and emulation, or cue taking, is an important phenomenon which determines in large part the pace and direction of social and political change in the American states" (Walker 1969: 890). Much of the diffusion research is still based on these two pillars of competition and emulation.

The states in Walker's study are grouped into regions based on both geographical contiguity and their place in the specialized set of communication channels through which flow new ideas, information, and policy cues. Through this nationwide system of communications a set of norms or national standards for proper administration is

established. This system links together the center of research and generation of new ideas, national associations of professional administrators, interests groups, and voluntary associations of all kinds into an increasingly complex network which connects the pioneering states with the more parochial ones (Walker 1969: 896–8). This network-based interpretation of the diffusion process still dominates diffusion studies. Finally, Walker’s interpretation of the diffusion processes hints at the process whereby innovation is coming to be “taken for granted”, an insight which is central to the world-society approach to diffusion (Meyer et al. 1997; Finnemore 1996). Once a program has been adopted by a large number of states,

it may become recognized as a legitimate state responsibility, something which all states ought to have. When this happens it becomes extremely difficult for state decision makers to resist even the weakest kinds of demands to institute the program for fear of arousing public suspicions about their good intentions; once a program has gained the stamp of legitimacy, it has a momentum of its own. (Walker 1969: 891)

All in all, by bringing together pieces of legislation that were not considered as parts of a whole Walker created an innovation score which ranked states according to the speed in which they adopted innovations and offered an original perspective on the determinant of this ranking. A new approach to public policy and public administration was established. As far as I can establish, never before had different acts of government been brought together in such a systematic study of political behavior and decision-making. This was an example to follow in the slowly dawning new era of big data. As we will see, it is this aspect that would attract draw criticism while the diffusion element of the study would be generally embraced as original and useful. The scale of the data collected and analyzed is impressive. So is also the forceful presentation of diffusion.

But what really makes Walker’s article such a frequently cited classic? Walker himself provided part of the answer in the interview already quoted, where he offered four reasons. First, it provided a simple way to derive new meaning from the hundreds of otherwise unrelated case studies of governmental policy-making that had been published over the years. Second, the paper drew both upon studies of individual political actors and upon studies of national patterns of policy-making. Third, it was a quantitative study that appeared just as quantitative research was becoming popular in this field. Fourth, it included a convenient innovation score for each state that many other scholars were able to employ in their own research.⁶ But while Walker’s own testimony offers important insights on the influence of his paper, I think that there is a bigger story to tell, a story which explores which of the various aspects of the paper and the research program are still really influential as well identifying how, and how greatly, these influences affect the current research agenda on diffusion. I turn my attention to these issues in the next parts of the chapter.

THE RECEPTION OF WALKER'S ARTICLE AND THE GRAY—WALKER DEBATE

The wide attention that Walker's paper received was reignited with the publication a few years later of Virginia Gray's seminal study "Innovation in the States: A Diffusion Study" (1973a). While framing her research questions somewhat differently, and while opting for a different research design, Gray dealt essentially with the same issues as Walker did. Her findings were somewhat different, and so was her approach to the study of diffusion. Gray was not shy of pointing out these differences, and this led to the publication of a response from Walker and a rejoinder from Gray in the pages of the *American Political Science Review* (Walker 1973; Gray 1973b). In perspective, the similarities between the two authors seem more striking than the debate that followed. Gray's paper has its own merits, can be considered a classic on its own right, and represents another high point in the history of the field. It included, unlike Walker's, a formal model of diffusion, and also defined diffusion explicitly, drawing on Rogers's nominal definition which became the gold standard in social science studies of diffusion. In addition, Gray emphasized the importance of top-down (or point-source) diffusion (in this case the role of the federal government), which was missing from Walker's analysis.

The 88 different policies that Walker aggregated across time and space in order to build his impressive dataset, however, amounted for Gray to a contentious research practice. Her data, by contrast, were selected purposely from issues areas central to the "have-have not" struggle, where the federal influence was minimal, and reflected the long durability of the issue (see Savage 1978, for a critique of Gray). With this research design she challenged Walker on two additional grounds. First, she didn't find any evidence for regional diffusion. It is not that Gray rejected regional forms of diffusion; she just suggested that she did not find any evidence of it. Yet, since her dataset was confined to have-have not policies, it may well be the case that diffusion is less likely to occur in areas and arenas of strong political conflict.⁷ This is an important issue that is still not discussed enough and is waiting for some serious scholarly treatment.

At the same time, Gray also contested the validity of Walker's index of innovativeness, claiming that states do not remain peculiarly innovative over a long period of time and she was skeptical that a quality called "innovativeness" could ever be isolated and adequately described. "[O]ne may question," she wrote, "the fundamental assumption of a 'composite innovation score'—namely, that 'innovativeness' exists as a single factor among states. Operationally, the question becomes "Do the states which are early to adopt one law also adopt other laws first as well? ... The interesting information that is concealed by a simple average ranking is the range for any one state" (Gray 1973a: 1183). She found that "it was shown that 'innovativeness' is not a pervasive factor; rather it is issue-and time-specific at best" (Gray 1973a: 1185). Gray's assertions on

innovativeness as a trait of states were based on a limited number of cases which were carefully selected in light of criteria that are highly relevant for the study of politics, inter alia the have–have not criteria. Yet there was nothing in the selection criteria which made them more suitable for examining this particular issue. Walker’s research design, with its wider scope, was much more suitable for testing this issue. This disagreement was probably one of the reasons why many scholars did not come back to the issue or continue Walker’s work in this direction.⁸ It is only in the last few years that scholars have returned to this issue with datasets which are wider in scope and generally confirm Walker’s results (Boushey 2010; Nicholson-Crotty 2009; Boehmke and Skinner 2012). Later on Gray, seemingly changing her view on this issue, wrote that “[t]he consistency of state rankings over time is quite remarkable” (Gray 1994: 244).

Let us focus our attention on the rules and purposes that govern the collection of the two datasets, an issue which I find especially useful to discuss. Both Walker and Gray were proponents of Large-*N* analysis, and they both embraced a diffusion perspective for the study of state politics, which hitherto had been mainly grounded in an intra-state tradition. Where they differed, beyond the immediate and salient issue of the innovation index, was on the question of the desired research design and, in particular, on what could be sensibly inferred from each design and the theoretical and scientific values of variations and diversities in the data. Whereas Walker’s design covered 88 programs, Gray covered 12 programs; whereas Walker’s data covered almost a century (1870–1960), Gray’s data covered almost two centuries (1784–1969), and both covered the same number of states (the members of the American federal system). Social scientists usually distinguish research designs according to the number of cases, contrasting Large-*N* and Small-*N* but also, even if less frequently, adding the notation of Medium-*N* (Ragin 2000). In our case, the number of cases will usually refer, in the current terminology, to the number of American states. This practice misses something of importance in the *diversity* of cases and in the creativity with which we can use strategically different research designs. While we have notations for the number of cases, where casing is defined as more of the same thing (hence, Large-, Medium-, and Small-*N*) we do not have appropriate notations for the depth (duration and longitudinality) of cases. The same goes for notations for the diversity and scope of different cases that are part of the phenomenon under study. I therefore denote the depth or duration by Deep-*N* and the diversity of cases covered by Wide-*N*. The notation of Deep-*N*, which may sound strange at first (like many innovations), captures the temporal and historical dimensions of the case selection. Cases in this line of thinking are therefore collections of dimensions, and this goes beyond the distinction of Teune and Przeworski (1970) which places unidimensional cases on the continuum of most-similar versus most-different system design. Recognizing that cases are not “given fact” but instead are scholarly constructions (Ragin and Becker 1992), we use this terminology in order to distinguish between cases according to the diversity of programs or policies they include. At the same time the terminology and notations make our research practices and decisions on which cases to include more

transparent and therefore potentially more contestable, which I take to be good scientific practice.

Table 17.1 demonstrates the architecture of choice on two of these dimensions—diversity and number of systems. (To simplify the example, the table leaves out the depth-longitudinal dimension of shallow-*N* to deep-*N* and focuses only on diversity and number.) To illustrate the differences between research designs, I provide examples from the diffusion literature. The diversity of cases is represented by the notations of Narrow-, Intermediate-, and Wide-*N*. While Walker's design can be described as Wide-*N* (88 programs), Gray's design is narrower (12 programs) and best titled Intermediate-*N*. The two authors covered the same number of American states and therefore do not vary on the number of similar cases.

The distinction between the number of cases and the diversity of cases allows us to emphasize the importance of compound research designs for theory validation.⁹ But there was one more issue at stake here. Walker's research design followed the logic of maximizing validity via inferential strategies that are designed to maximize diversity via random selection of cases. The more the merrier. Gray's research design, in contrast, followed the logic of maximizing validity via inferential strategies which are comparative, case-oriented, and selective. There is less room for debate here on the question of the better design as it is necessary to examine theories against different

Table 17.1 Maximizing what: Large-*N* vs. Wide-*N*

		Diversity of types of cases (Diversity of programs included in the study from Narrow to Wide)		
		Narrow- <i>N</i>	Intermediate- <i>N</i>	Wide- <i>N</i>
Number of cases from the same type	Small- <i>N</i>	One or few similar policy innovations studied in one or a few states e.g. Frenkel (2005)	Intermediate number of diverse policy innovations studied in one or a few states e.g. Jordana et al. (2006)	Highly diverse collection of policy innovations, studied in one or a few states. e.g. Bennett (1997)
	Medium- <i>N</i>	One or few policy innovations studied in medium number of states e.g. Berry & Berry (1990)	Intermediate number of diverse policy innovations studied in medium number of states e.g. Gray (1973a)	Highly diverse collection of policy innovations studied in medium number of states e.g. Walker (1969)
	Large- <i>N</i>	One or a few policy innovations studied in large number of states e.g. Henisz et al. (2005)	Intermediate number of policy innovations studied in large number of states e.g. Simmons and Elkins (2004)	Highly diverse collection of policy innovations studied in a large number of states

datasets rather than in light of one criterion of selection. The more diverse and wider scope of Walker's dataset provides better evidence for the diffusion of policy innovations in the American states. We can say that its external validity is better than Gray's. Yet Gray's design offers evidence that, on issues of distributive and redistributive conflicts, the innovation index is less useful for a more general population of policies. Her observations and findings thus refine the internal validity of those of Walker.

It is also useful to examine the differences between the two datasets and criteria of case selection using the notion of "consilience." The term originated in the work of the British philosopher of science William Whewell (1794–1866), who was also the first to coin the term "scientist." Evidence, argued Whewell, is "of much higher and more forcible character when it enables us to explain and determine cases of a *kind* different from those which were contemplated in the formation of our hypothesis" (Whewell 1840: 230). Consilience is this superior test that makes one theory and one research design far stronger than another. According to Thagard,

To say that a theory is consilient is to say more than that it "fits the facts" or "has broad scope"; it is to say first that the theory explains the facts, and second that the facts that it explains are taken from more than one domain. These two features differentiate consilience from a number of other notions, which have been called "explanatory power", "systematic power", "systematicization", "or unification"... We are not concerned with the explanation of a horde of trivial facts from the same class... In inferring the best explanation, what matters is not the sheer number of facts explained, but their variety and relative importance... (Thagard 1988: 80, 81)

The consilience criteria of validity seem to offer support for Walker's assertion that diffusion matters but also that the innovation index is a valid measure of states' tendency to innovate.

To summarize, one can classify and thus distinguish between different research designs with respect not only to the number of cases but also to their diversity on different dimensions, inter alia depth and width. Cases diverge on different dimensions including longitudinal (Shallow-*N* to Deep-*N*) and type (e.g. Narrow-*N* vs. Wide-*N*). Gray's design thus differed therefore from Walker's not so much in the number of cases (Large to Small dimension) but in scope (Wide to Narrow dimension). Ironically, neither Gray's nor Walker's design became the conventional practice in the discipline. Almost all diffusion studies—and diffusion is a representative case here for wider practices—focus either on a large number of cases of a single issue or on a single case study. Research designs that compound time, type of cases, and an intermediate or medium number of cases in a comparative design like the one performed by Gray or a more diverse (Wide-*N*) number of cases of the sort undertaken by Walker are rather rare. It is, however, encouraging that, after many years in which the innovation index and the ambitious collection of datasets were set aside, there is a new interest in both practices.

WALKER AND DIFFUSION RESEARCH IN PERSPECTIVE

Despite the impressive success of Walker and of the diffusion perspective, there is still much to be desired. My reading of this literature suggests, similarly to Meseguer and Gilardi (2009), that a "true" political economy of diffusion is yet to emerge.¹⁰ Or maybe perhaps "true" is less proper here than a more ambitious theory. We are looking for an approach which will make a more significant impact on the study of political, social, and economic behavior. Impressive progress in the methodology of research is accompanied by critical reflection among the community of diffusion scholars, despite growing interest in the field.¹¹ Thus, for example, Graham et al. write "as political science moves toward its thousandth published article on policy diffusion, the piecemeal and disconnected nature of the research to date has left us intellectually poorer than we should be . . . [w]e are nowhere near having a systematic, general understanding of how diffusion works" (2013: 673). This is not a new conclusion. Berry and Berry, the pioneers of event history methods in diffusion research, wrote more than 20 years ago that "while expanding the scope of policy areas subject to innovation analysis, the research since 1975 has not led to major advances in our conceptualization of state innovation or our empirical approach to its investigation; the same basic approaches have simply been applied in new policy context" (Berry and Berry 1990: 395). Why such dissatisfaction? What is its source? In seeking the causes of this dissatisfaction in the progress and the impact of the diffusion process, I concur with a recent observation by Ethel Solingen, who wrote, "in efforts to understand the nuts and bolts of whatever it is that diffuses, we have often paid less attention to conceptualizing diffusion itself, leaving the notion open-ended, taken for granted, studied more tacitly than explicitly" (Solingen 2012: 631). In other words, we are experiencing a "conceptualization deficit" in the study of diffusion. The new frontiers lie in theory and innovation in research designs and data collection rather than solely in methodological advance. These frontiers require us to invest mainly in what diffusion is rather than in the current focus on its correlates or mechanisms.

Rogers's authoritative definition of diffusion dominates the development of the diffusion literature. This definition, which changed only marginally over the years, was accepted implicitly or explicitly without critical discussion despite its emphasis on the social and communicative aspects of the diffusion process rather than on its political and administrative aspects. It all starts with Walker, who drew on Rogers but avoided directly citing his definition. Instead he defined the subject of his research as simply the relative speed and the spatial patterns of adoption of new programs. It continued with Gray, who offered a definition which was based on Rogers.¹² So did Berry and Berry, who referred to diffusion, following Rogers, as "the process by which an innovation is communicated though certain channels over time among the members of a social

system” (Berry and Berry 1999: 171). This system consists of the governments of the fifty American states and maintains that the pattern of adoption of the policy by the states results from states’ emulating the behavior of other states. This conceptualization of diffusion, which draws so closely on Rogers, is taken for granted either explicitly or implicitly. At the same time, empirical research nowadays seems to draw on an operational definition which was offered by Strang (1991: 325). The term “diffusion,” wrote Strang, refers to all processes in which “prior adoption of a trait or practice in a population alters the probability of adoption for remaining non-adopters.” This is the definition of diffusion that was adopted by Simmons and Elkins (2004: 171–2), and the same approach is reflected in a paper by Graham et al. (2013: 675): “diffusion occurs when one government’s decision about whether to adopt a policy innovation is influenced by the choices made by other governments. Put another way, policy adoptions can be *interdependent*, where a country or state observes what other countries or states have done and conditions its own policy decisions on these observations.” Another way in which scholars operationally captured diffusion is as the varied *rate of adoption* or *relative speed* by which different institutions, events, states, or actors adopt policy innovation (e.g. Berry and Berry 1990).

Little regard for the conceptualization of diffusion itself on the one hand and for a sensible proxy for the diffusion that allows useful operationalization of the rate of adoption on the other created a rather convenient equilibrium. This situation, alongside impressive methodological advances on the one hand and a bias towards the correlates of diffusion (i.e. the mechanism) on the other, create a gap where the explanans is a black box. We know more about the mechanisms of diffusion than about diffusion itself. Yet what we know or conceptualize as diffusion rests on sociological analysis that frames diffusion as a communicative and social interaction rather than a political interaction. The black box of diffusion should be open, and the interaction between adopters and non-adopters over time (Deep-*N*), diversity of programs (Wide-*N*), and group size (Large-*N*) should be understood as political processes, running via political channels of decision rules, with conflicts, powers struggles, and institutions at the center of the definition of diffusion. This exercise requires us to politicize the interaction process and thus to imagine it in a more political context. Thus, interaction as influence rather than the thinner notion of interaction as signal or information; interaction as an agenda-setting process rather than the thin notion of interaction as decision-making; interaction as an exercise in domination and power rather than simply rational discourse; interaction as a legitimization exercise rather than utilitarian one. Such an approach allows us to see the diffusion effect as a particular form of group decision-making which applies not only to policy innovation but to any form of political interaction. In other words, the playing field for diffusion analysis is wider than the current one.

The road to a richer and politically oriented conceptualization of diffusion processes should also be taken in reaction against two self-imposed boundaries that Walker set to the field by distinguishing between invention and innovation on the one hand and between adoption and implementation on the other. “We are studying the relative

speed and spatial patterns of *adoption* of new programs, not their invention or creation," he wrote, and thus set one of these boundaries. And one page on he set the second boundary: "I am not interested in the effectiveness of Oklahoma's civil rights commission, but in where the legislature got the idea . . . and why it acted when it did" (Walker 1969: 882). The invention and the adoption decision were thus separated as fields of study. And similarly, the decision to adopt was separated from the processes of localization, translation, and transplantation. Analytical rigor and focused discussion have many merits, but they also have costs. Invention and innovation are not separate, not even in Walker's study where he counted the first instance of adoption of a policy (e.g. invention) as the starting point of the process of diffusion. They are likewise not separate in the world of entrepreneurs, whether technological or political. Rational models and strategic invention are about the diffusion of inventions that are easy to diffuse; they are not about technological and policy designs that are aimed narrowly. Steve Jobs told us that he knew that his iPad had to wait until the market was ready. It wasn't a technological issue. The market had to be ready first with the iPhone. Jobs sequenced his Apple products and by doing so linked invention with innovation decisions. In other words, diffusion studies in the future can and should look not only at the decision to adopt the innovation but also at the link between the invention and the innovation.

The second boundary set by Walker was that between the decision to adopt and the processes of localization, transplantation, and translation that in his model follow the decision to adopt. Nonetheless, and as we know from the growing literature on translation and localization (Czarniawska-Joerges and Sevón 1996; Jacoby 2001; Acharya 2004; Frenkel 2005), the decision to adopt (or not to adopt) an innovation is strongly connected to the expected constraints embedded in the attributes of the adoption. The greater the ability of the actors to flex, adapt, and localize the innovation, the greater is the propensity to adopt and the faster is the speed of diffusion. The decision to adopt, in other words, is connected to the attributes of the policy and the goals of adopters, and therefore cannot be studied separately without considerable cost.

The boundaries problem of the diffusion approach directly and indirectly affects the tendency to interlink and embed the diffusion perspective within a broader theoretical discussion in the social sciences generally and public policy in particular. Diffusion is the study of decision-making with regard to the adoption of new programs in the context of inter-group effects. Understood in this way, it has wide and immediate implications for, and relevance to, the study of policy change, agenda-setting, the politics of attention, and of course policy-making at the individual level as a behavioral, empirical, and theoretical subject of study. This integration was slow to emerge. Walker saw his paper and research as the amalgamation of three different area of study: studies of decision-making, reference group theory, and the diffusion of innovations (Walker 1969: 883). One example of a move in this direction is a recent effort to link the punctuated equilibrium literature with diffusion, as undertaken by Baumgartner and Jones (1993) and Boushey (2012), linking periods of incremental change with periods of rapid change. While both incremental changes and more radical changes can be diffused, the diffusion of radical changes can be captured more easily by measures of punctuated

equilibrium. Similarly, it makes sense to connect Kingdon's agenda-setting perspective more closely with the diffusion perspective. For example, this can redraw the lines in a diffusion perspective which is guided by the implicit assumption of a problem looking for a solution rather than the possibility of the alternative, that is, solutions looking for problems in the diffusion process (Rapaport et al. 2009).

Yet another way forward in a richer theory of diffusion is to move away from the homogenization of processes and instead emphasize diversity and heterogeneity. Assumptions about spatial and temporal homogeneity should be relaxed and critically examined (Strang and Tuma 1993). Diversity and heterogeneity can be captured and conceptualized along many dimensions of the process, including the diversity of policies, actors, channels, context, and causal processes. By "diversity of policies," I mean the various attributes that make policies less or more likely to spread, directly or indirectly affecting the likelihood and the rate of diffusion. Gray (1973a) suggested distinguishing policies according to their "have–have not" dimension. Fliegel and Kivlin (1966) studied attributes of innovation such as costs, profitability, and risk. More recently Makse and Volden (2011) have examined the likelihood of adoption of successful policies. In doing so they draw on Rogers's identification of five attributes of policy innovation, namely relative advantage, compatibility, complexity, observability, and trialability (Rogers 1995).

By the "diversity of actors," I mean the study of different categories of participants in the diffusion process. This can be done analytically by distinguishing actors according to their place in the chain of diffusion: for example, internal actors (those within the government who may be considering an innovation); external actors (those in the governments from which policies may diffuse); and go-betweens (those who act across multiple governments) (Graham et al. 2013). But this can be also done by examining their function in the diffusion process, for example Mintrom's (1997) study of the role of entrepreneurs, or Balla's (2001) study of the role of professional associations. Another useful categorization is the five roles in the diffusion process: model missionaries, model mercenaries, model mongers, model misers, and model modernizer (Braithwaite 1994). At the same time, the idea is to give voice to the interests and attitudes of real people who have varying preferences, goals, and capabilities: "without a focus on the policy makers themselves, studies of policy diffusion may miss important aspects of politics" (Graham et al. 2013: 684).

By the "diversity of channels," I mean diffusion not only from government to government but also from sector to sector (Jordana et al. 2011) and from one level of government to another (Volden 2006). One can also distinguish between two types of channel along which innovations flow: direct (relational) and indirect (nonrelational). The aspect of diffusion most utilized by social scientists is the direct connection or channel between actors in a social system (Strang and Meyer 1993). Relational models of diffusion highlight information flows between actors through their direct network relations. "The rate at which an item diffuses varies with the level of interaction between actors so that, at high levels of interaction between a prior and potential adopter, there should be higher rates of diffusion of innovations" (Soule and Zylan 1997: 743–4). By the

"diversity of context," I mean the study of diffusion assertion in different contexts, such as the study of the spread of privatization (Levi-Faur 2003) or of revolutions (Weyland 2010) in Europe as compared with Latin America or in the nineteenth century as compared with the twentieth century (Weyland 2012). Finally, by the "diversity of causal effects" during diffusion processes, I mean the varied effects of different causal mechanisms in different stages of the diffusion process (Strang and Soule 1998; Jordana et al. 2011).

CONCLUSIONS: FROM RESEARCH PROGRAM TO RESEARCH PARADIGM?

Jack Walker's study of the diffusion of innovations among the American states is widely considered a seminal study, and for many good reasons. Walker took a theory and a perspective that were highly useful in other social science fields, and applied them in an insightful and forceful way to the study of the public policies of the American states. The innovative part of the study was in neither the explanans nor the explanandum. It was an innovation rather than invention—about taking something from one field and applying it in a creative manner in another. The article still stands out among many other useful studies for the creative manner in which it introduces diffusion to our field, using an ambitious dataset of non-numerical data. All this was accompanied by a clear and systematic analysis of the data and their correlates, which selectively borrowed and brought together different strands of research that were developed elsewhere in the social sciences. It took a while for political scientists outside American politics to begin developing a keen interest in diffusion. It took even longer for the sub-field of international relations to adopt it as its own. Nonetheless, we have nowadays a vibrant field of study and, according to one count, political science journals published nearly 800 articles about policy diffusion up to 2008 (Graham et al. 2013). More than half of these articles were published in last decade of that period. It all essentially started with Walker.

Diffusion studies continue to be influenced and even shaped by the models crafted by Walker, and some of the most interesting new studies published in recent years represent renewed interest in Walker's frame of reference. This renewed interest goes beyond the basic idea of interdependent politics and policy, and touches also on measures of speed and rates of diffusion and the basic mechanisms of emulation and competition as the basic driver of reforms. All the more interesting is the recent revival of scholarly interest in the innovation index and in the creation of big datasets (Boushey 2010; Nicholson-Crotty 2009; Boehmke and Skinner 2012), as well as the interest of scholars such as Weyland (2009) who reframe diffusion, like Walker, in heuristics of bounded rationality. While Walker could draw only on Herbert Simon's work, the efforts of scholars to reconnect diffusion theories with other theories of the field, and thus to

break through the boundaries that were set on diffusion research, are an encouraging sign of future progress in the study of diffusion. What Walker taught us is that interests, behavior, practices, and norms are highly interdependent. The likelihood of change in one actor's interests, behavior, and practices are positively correlated with the likelihood of change in those of other actors. To the extent that this interdependency is common and widespread, we should take diffusion seriously. This observation applies to innovations but at the same time it applies to any human interaction. If the "interaction effect", which is sometimes called also the "diffusion effect," is relevant well beyond the diffusion of innovations, then Walker's research program may develop into a research paradigm, competing with those of rational choice and institutional analysis. In this regard the future of diffusion research seems even more promising than its past and present.

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NOTES

1. By July 2013 the paper was among the five most-cited papers in the *American Political Science Review* according to Thomson Reuters, and among the ten most-cited according to Google Scholar.
2. Interaction, learning, emulation, and transfers in policy and politics have long been noted in passing manner by political analysts. The diffusion of Bismarckian institutions of the welfare state within the West is one example, which is now a commonplace at least for historians of the welfare state (Barker 1944; Lerner 1964). The basic units of modern politics, such as the nation state, were diffused (or imported: see Badie 2000). Nonetheless, the full effect, scope, and complexity of the interdependency between actors' interests, behavior, norms, and practices was recognized only gradually and the understanding of its full significance for social, economic, and political analysis is still constrained.
3. Interview with Walker, Thomson Reuters, Current Currents, Feb. 11, 1985. Courtesy of Frank Baumgartner.
4. On unconscious diffusion, see Lieberman (2000).
5. E.g. if the total time elapsing between the first and the last adoptions of a program was 20 years, and Massachusetts enacted the program ten years after the first adoption, then Massachusetts received a score of 0.5 on that particular issue.
6. See Walker interview.
7. Unfortunately, this line of reasoning was not followed even though regional diffusion continues to be a vibrant field of study.
8. Eyeston (1977) provides support which I found in convincing on a number of cases grounded in Gray's approach. On the other hand, Savage's (1978) findings confirm

Walker's approach. For attempts to reinvigorate the literature on the innovation index see e.g. Canon and Baum 1981; Savage 1985. For a more extensive discussion, see Boehmke and Skinner (2012). It might be useful to note that Soule and Zylan (1997) used Walker's index as the explanans rather than the explanandum.

9. Compound research designs combines cases that vary on selected number of dimensions, e.g. the combination of cross-sectoral and cross-national cases in one research design. In this case one would compare both countries and sectors in one or multi-step research design (Levi-Faur 2004, 2006a, 2006b; Jordana et al. 2011).
10. For useful surveys of the literature, see Meseguer and Gilardi 2009; Gilardi and Füglistler 2008; Shipan and Volden 2008; Graham et al. 2013; Palloni 1998; Wejnert 2002; Karch 2007.
11. For useful surveys of the methodological progress in the study of diffusion, see Berry and Berry (1999, 2007) and Gilardi and Füglistler (2008).
12. "The process by which an innovation spreads is called diffusion; it consists of the communication of a new idea in a social system over time" (Gray 1973a: 1175). In a later formulation diffusion occurs when "an innovation is communicated through certain channels over time among the members of a social system" (Rogers 1983: 14).

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CHAPTER 18

ALBERT O. HIRSCHMAN, *EXIT, VOICE AND LOYALTY: RESPONSES TO DECLINE IN FIRMS, ORGANIZATIONS, AND STATES*

KEITH DOWDING

INTRODUCTION

ALBERT Hirschman led a colourful life not entering academia full-time until his forties (Adelman 2013). Born in Berlin in 1915, he entered university in 1932, completing one semester before Hitler expelled Jewish students. He continued his studies via Paris, London, and Trieste, also serving in the Spanish Civil War for three months. In the early part of World War II he worked in the French underground helping more than 2,000 refugees escape over the Pyrenees, including Hannah Arendt, André Breton, Marc Chagall, Marcel Duchamp, and Max Ernst, before taking that route himself in 1941. He served in the US Army and then acted as interpreter in the first Allied war crimes trial against the German General Anton Dostler. Sitting next to Dostler throughout the five-day trial he was reported to have gone deathly pale as Dostler was sentenced to death.

Hirschman was hired by the Federal Reserve Board and then the Economic Cooperation Administration in Washington, helping to develop thinking behind the Marshall Plan for redeveloping Europe, but was caught up in the McCarthy purges when a security review concluded that he was sympathetic to communism and was sacked. He went to South America working for the World Bank, then took up an offer from Colombia's National Planning Council as a senior adviser. Relocating to Bogotá with his family he became a critic of standard economic development theories,

believing small-scale “unbalanced” development could be advantageous under some conditions. Then in 1956 he accepted an offer as visiting professor at Yale, following this up with a teaching job at Harvard. Hirschman hated teaching and in 1971 he secured first a visiting, then a permanent, position at Institute for Advanced Study in Princeton where he worked until retirement. He died in December 2012 aged 97.

Exit, Voice and Loyalty was published in 1970, becoming an instant classic (Barry 1974). The scope of its influence can be seen from the more than 106,000 citations of the phrase “Exit Voice and Loyalty” in Google Scholar, the book itself garnering over 14,000 citations.¹ Its popularity might well be due to its relatively simple idea that has applications in diverse fields. From his Parisian days in the 1930s where academia was full of grand theorizing he developed an enthusiasm for “petites idées” which he would note down for use later. The interplay of exit, voice, and loyalty can be seen as one of these little ideas and Hirschman’s entire output can be seen in such terms, rather than some overarching grand theory applied to everything. Having said that, the appeal of Hirschman’s book is its relatively simple argument that seems to have many applications in fields as diverse as personal relationships, emigration, workplace relations, political parties, as well as public policy where I shall concentrate my attention.

Hirschman argued that exit and voice are the two ways consumers of a product or service could signal to producers their satisfaction (or dissatisfaction) with that good. Exit can be thought of as the usual market response. If a consumer of a product thinks its quality has declined or another product is better, he or she can take their custom elsewhere. Firms that lose customers that way must respond to such signals or go out of business. By responding to such exit signals firms provide their customers with what they want, ensuring welfare efficiency. Competition between producers also ensures productive efficiency by driving down prices and ensuring cost-effective production processes.

Voice is a rather different signal. Whereas exit is a binary, stay or leave, process, voice can produce a subtler signal. Consumers can tell producers what they want. Firms may conduct market research and tailor products to consumer demand; they can respond to customer complaints. Sometimes voice might be used effectively through a campaign. When Coca Cola introduced “New Formula Coke” consumer resistance caused the company to bring back the old formula, now branded “Classic Coke,” alongside the new one. Yet without the threat of exit, voice is just cheap talk. In that way, voice is always a supplement to exit; it is not an effective way for customers to signal dissatisfaction if they are not also prepared to exit if the company does not respond.

Generally speaking, one would think that increasing the ease of exit—that is, making the threat of exit more credible—would increase the effectiveness of voice. Since competition would increase the ease of exit and competitive practices also provide efficiency directly through exit itself, increasing competition should always provide efficiency gains. This argument would have seemed obvious to Hirschman, a trained economist. However, Hirschman argues that making exit easier might, under certain circumstances, reduce efficiency by silencing voice. He came to this idea through noting a decline in the quality of the Nigerian railway system, partly he believed in

response to greater competition from road traffic (Hirschman 1967). He observed that, when some people—who tended to be richer and more educated—started to use cars, the efficiency of the railways declined. It was not simply because fewer people were using them whilst running costs remained the same, but because the people who left were the most articulate and powerful. Once they stopped complaining—directly to officials of the company and indirectly through political channels—the railway company and its officials could relax, and the quality of the Nigerian rail service declined.

In fact Hirschman identified two ways in which voice would decline. First, the most vocal (whom he termed the “alert”) leave, taking their voice with them. Second, while those left behind, generally the less educated, might still voice, their voice would be less effective. The main issue for Hirschman is that the alert citizens are both more likely to voice and also more likely to exit. But if exit is very easy, then the alert might not bother to voice at all. If state education declines rapidly and private education is cheap (subsidized through tax breaks as in the UK or partly directly funded as in Australia), then richer parents might not vocalize at all, but simply send their children to private schools. Thus Hirschman argues that increasing the opportunity for exit, by encouraging private health care or education, will lead to a decline in the quality of state education and health services. His argument therefore has obvious public policy relevance, even more so in the past 30 years than when he first published his book, as marketizing welfare services has become more popular around the world.

We can note here that, in the public service context, voice is not cheap talk, even if a service is a state monopoly. Even if exit from a nationalized service is impossible, citizens retain threat potential. In a democracy they can voice their disquiet and threaten not to vote for any candidate who will not act to improve service quality. In that context voting can be seen as a form of exit. (In another context, voting can be seen as a form of voice.) Either way, however, it is the threat potential of withholding one’s vote that gives power to the voice option.

Where does loyalty come into this account? Hirschman suggests that some consumers will not exit as readily as they might because they are loyal customers. Someone who has always drunk Coke might not want to switch to Pepsi even if they are not so keen on the new formula. Or some parents who could easily afford private education for their children may choose not to do so since they have an ideological commitment to free state education for everyone. In Hirschman’s account loyalty is supposed to increase the attraction of voice over exit.

We can note two important aspects of Hirschman’s account. The first is the relationship between exit, voice, and loyalty. Exit and voice are partly complementary—voice is only really effective if exit is a possibility—but also rival responses. One could choose to voice, or to exit. One can of course exit noisily or silently, but once one has exited there is little incentive—except perhaps through loyalty—to continue to voice. Hirschman’s main claim in this regard is that making exit easier can reduce voice. The second is the efficiency argument. Exit’s reduction of voice activity would not matter if it also drove efficiency gains. Hirschman argues that it will not do so but rather lead to welfare loss.

It is this second efficiency argument that is most telling, especially given the marketization of public services and the desire to give citizens more choice in public services. Governments have maintained that privatization, the introduction of quasi-markets and increasing choice (exit possibilities) will bring more efficiency to the public sector, but if Hirschman is right then these processes might have the opposite effect. There is empirical work on both aspects of Hirschman's argument, though examining the first—whether exit and voice are rivals—is more developed than the second directly concerning efficiency. Given the stochastic nature of most public goods and services, making judgments over the relative efficiency of changing modes of delivery is difficult. First I consider the influence of the book in various different fields, then look at some criticisms and extensions of Hirschman's framework. Then I consider the evidence for his claim of the exit-voice trade-off and the mediating role of loyalty. Finally I consider evidence on the efficiency of different exit mechanisms.

APPLICATIONS

In the field of comparative politics Hirschman's argument has been applied to protest movements, migration, especially from authoritarian regimes (O'Donnell 1986; Scott 1986; Ma 1993; Hirschman 1995; Evans 1998; Pfaff and Kim 2003; Ådnanes 2004; Pfaff 2006; Okamoto and Wilkes 2008), to the behavior of members of political parties (Eubank et al. 1996; Kato 1998; Pettit 2007; Whiteley and Seyd 2002), and to interest groups (Barakso and Schaffner 2008). Psychologists have applied it to social relationships (Kelley and Thibaut 1978; Rusbult and Van Lange 1996), and to employer-employee relationships where higher dissatisfaction leads to both greater intentions to exit and higher exit of workers (Withey and Cooper 1989; Leck and Saunders 1992), though they also find that greater satisfaction of workers leads to higher voice. Greater loyalty is associated with lower intentions to exit (Lee and Whitford 2008), lower dissatisfaction (Boroff and Lewin 1997), and higher voice (Hoffman 2006), both directly and through representatives (Luchak 2003). Earlier work showed that unionization (which can be seen as providing potential for voice) leads to lower exit (Freeman and Medoff 1984; Miller and Mulvey 1991). In some applications the EVL framework is just slapped on to studies without really being used. Writers label behaviors as "voice" or "exit" but do not utilize Hirschman's argument. That is less true in the party and management literature and some of the later literature on political migration. The best applications of Hirschman's argument, where it really makes a difference to analysis, occur in the field of public policy and the efficient public provision of goods and services. In this area (along with Tiebout) Hirschman created a whole approach to the way in which satisfaction can be signaled to governments in terms of leaving jurisdictions or providers in relationship to private complaining and political activity. Interestingly he critiqued some modern ideas about providing greater choice in public services long before politicians started championing them.

CRITIQUE AND EARLY EXTENSIONS

One of the early criticisms of Hirschman is that he saw exit and voice as alternatives, whereas in fact they could be used in unison (Barry 1974; Laver 1976). There are four logical possibilities: Voice–Non-exit; Voice–Exit; Silence–Non-exit; and Silence–Exit. Another pertinent early criticism is that Hirschman ignored the collective action problem. Whilst voice has a private aspect when one complains about a particular experience and perhaps demands compensation, publicly voicing about the general decline in some service carries costs for benefits that might be widely dispersed. There are incentives to free ride and let others bear the costs. So different sorts of voices might lead to different responses. People might complain to each other “horizontally” rather than upward, which might have long-term effects (O’Donnell 1986). The dynamic nature of exit and voice was also highlighted: people can exit and voice; or they might voice and then exit. The role of loyalty was questioned on the grounds that it was simply a value people place on a good and should be part of a more normal cost–benefit analysis. Others suggested that loyalty might lead not to voice but to silence, as loyalists might not want to rock the boat (Birch 1975).

In an important extension of Hirschman’s argument, the social psychologist Caryl Rusbult produced a fourfold categorization of responses to problems called Exit, Voice, Loyalty, and Neglect (EVLN), constructed in two dimensions—an active–passive dimension and a destructive–constructive dimension. She hypothesized that some people were active–destroyers (exiters), some active–constructors (voicers), some passive–destroyers (neglecters), and some passive–constructors (loyalists). Using survey and experimental evidence she categorized people into one of four personality types (Rusbult and Farrell 1982; Rusbult and Zembrodt 1983; Rusbult and Lowery 1985; Rusbult et al. 1982, 1986, 1988a, 1988b). The EVLN model is not simply an extension of EVL, since its concepts are psychological attributes and not simply behavioral responses to problems. The important implication for Hirschman was how changing incentives might cause the alert (or the active in Rusbult’s terminology) to shift from voice to exit or vice versa rather than predicting that people would respond to the same incentives differently.

David Lowery and William Lyons applied the psychological interpretation of EVL to local public services (Lyons and Lowery 1986, 1989; Lyons et al. 1992; Hoogland DeHoog et al. 1990). They had public policy implications in mind. Their main theoretical thrust was to attack the Tiebout (1956) exit model of local government efficiency. Tiebout argued that in large metropolitan areas more efficient local services might be provided by sets of competing service-providers rather than by one overarching metropolitan government. Citizens could relocate to jurisdictions that provided the mix of local taxes and services they preferred. Lyons and Lowery argue that there is little evidence of Tiebout-consistent exiting; rather we find only a (very) few who move in response to services, whilst others show loyalty, others voice, and others simply suffer

in silence (Lyons and Lowery 1989a). Lyons and Lowery believe that using geographical exit as a means to judge service efficiency will ensure only the active-destroyers are served and not the bulk of the population. Hirschman, of course, shared that concern.

Critics of Hirschman also pointed out that some forms of voice and exit are collective. For example, in the Coca Cola example, what made the company respond was not so much falling sales nor individuals writing independently to the company, but a concerted collective action campaign to restore the original recipe. Similarly, another form of public voice, notably with regard to public services, can be heard through the ballot box. So long as each vote is an independent act and thus largely inconsequential, the incentive to vote against a local government on the basis of failing services is low. This is an example of the well-known collective action problem (Olson 1971; Sandler 1992).

Hirschman was skeptical of Olsonian implications of collective action problems (Hirschman 1982), partly due to his own selfless actions as a young man. However, the collective-private-good continuum raises problems for his EVL account. For example, in Hirschman's account voice is used to complain about falling product quality. This makes sense in a private market where consumers ordinarily only have incentives to voice when complaining. However, where a service is being provided publicly to all, citizens have incentives to voice even when the service is good. Often conflict between rival parties or candidates in elections is over the nature of services, and that gives people incentives to contact the press, tell their representatives, or vote to defend a service with which they are exceedingly satisfied. Thus the private-collective dimension affects the mechanisms that Hirschman identifies. Partly in response to these problems, Keith Dowding and Peter John (Dowding et al. 2000; Dowding and John 2008, 2011, 2012) created a more complex EVL framework that involves various types of exit and voice. They also operationalize the concept of loyalty in a different manner. Unlike the EVLN modifications that took these concepts to be personal psychological attributes, Dowding and John consider exit, voice, and loyalty to be behavioral responses to service quality, but categorized the concepts in terms of structural features concerning the nature of the alternatives.

THREE EXITS, THREE VOICES, AND SOCIAL INVESTMENT

In the context of the provision of state services the fact that voice might be used to support as well as criticize a service is important. Dowding and John (2012, see also Dowding et al. 2000) argue that both exit and voice can take several forms, affecting the mechanism of relative rates of exit and voice in relationship to satisfaction with services. In a public service context exit has four forms. First, consumers might exit altogether from a service—they might never go to a doctor, for example. Hirschman

tends to ignore that form of exit, though it is important if state services are privatized and some can no longer afford them. For some locally provided public services, citizens might exchange one service-provider for another by moving house—a Tiebout Exit (TE). This is relatively common, for example, where school enrolments are based on catchment area, and has a knock-on effect on house prices (Black and Machin 2011). People might also leave the state sector for private sector providers, which can be called “private exit” (PE); or they might exit from one state provider to another, perhaps registering with a different state medical practice (“internal exit,” IE). These different forms of exit will not necessarily produce the same predictions as to the quality of state services. Internal exit might simply lead some state providers to close but produce an overall improvement in the quality of state services; private exit might lead to a continuing decline in the quality of state services.

Voice too can take several forms. Individual voice (IV) is where people complain directly to the producer or provider of a service. Dowding and John (2012) suggest there are two forms of collective voice: “collective voice vote” (CVV) and “collective voice participation” (CVP) which takes the form of collective action through petitions, pressure group activity, or other forms of social mobilization. Again, these distinctions are important since we might not expect the relationship between the different forms of voice and exit that Hirschman assumes in his simpler account. Here, if we make exit easier, then the amount and quality of voice is expected to fall as the better off and more educated alert consumers leave. This might not happen, at least not to the same extent, when we consider the different forms of voice. A citizen who is unhappy with the state health service might go private, and thereby remove individual voice from the state health care system, but still be prepared to vote for a candidate or party who wants to improve state health services. Furthermore, increasing collective voice might be a response not to a decline in quality but to perceived threats to a service. Dowding and John therefore do not see exit and voice as potential responses to a decline in service quality but rather as responses to satisfaction with services. One might voice in order to maintain a service; and one might exit not because of a quality decline, but because one believes a better service can be had elsewhere.

Dowding and John (2012) also operationalize loyalty differently from others. Hirschman at times wrote about loyalty as “brand loyalty”: that is, wanting to continue using a product simply because one always had, suggesting brand loyalty ought to increase the probability of voice to complain about falling quality, but some critics suggested that loyal people might be more likely to suffer in silence (Birch 1975). Dowding and John suggest that loyalty needs an object different from the service itself. Thus someone who dislikes a local school might not want to move to another school catchment area if it would entail leaving friends and family, a particular neighborhood, or house. Similarly someone might be unhappy with local government policies in some service areas, yet be a strong party identifier with that locale. So the measure of loyalty needs to be separate both from the service and from satisfaction with that service. We should expect that, at any level of satisfaction, people who have social links to a community, or who have lived there a long time, are both less likely to exit and more likely

to voice than those who than those without such ties. Secondly Dowding and John suggest that loyalty is in part constituted by past voice activity; that, controlling for other factors, we should expect past voicers to be more likely to be current voicers than past non-voicers. They associate Hirschman's concept of loyalty with that of social capital (Bourdieu 1986; Coleman 1990; Putnam 2000) seeing past and present voice as social investment, which the capital (the links one has in a community) gives a relative return on beyond the specific response that voice gives rise to. To exit would be to give up that past investment as yielding a poor return in terms of the services one is getting.

EVIDENCE FOR EVL RELATIONSHIPS

Before examining evidence for EVL mechanisms, it is important to precisely specify what might be looked for. There are two elements in the Hirschman argument. One is the characterization of EVL and their changing relationship under different external conditions; the second is how they interact with service quality. Whilst both Lyons and Lowery and Dowding and John carefully collected evidence on the first, they only make inferences about the second. In this section I will examine the evidence for the first; and in the next section say more about the second.

Dowding and John's evidence is based on self-reported satisfaction, self-reported voice activity, and self-reported intentions to exit and actual exit. As they have five-year panel surveys, they can report on how individuals who have voiced and remained dissatisfied might choose to exit later. Thus they can look at dynamic aspects of the exit-voice relationship. By interrogating respondents through counterfactuals regarding increases in income and costs for private education and health insurance, they can also examine the behavior of those "locked in" to state schemes.

Dowding and John find that exit or intentions to exit reduce the propensity to voice in most cases. Intending to geographically exit from a jurisdiction reduces the propensity to vote in local elections. If you are not going to gain the benefits of your vote because you are leaving then you are less likely to vote. However, this relationship is weakened if one has community ties through long residency, close relationships with neighbors, and also if one has engaged in past collective and individual voice activity. People who care about a locality will vote even if they intend to move in the near future (see also Dowding et al. 2012). They find the opposite relationship with collective voice participation and individual voice—examples of noisy exit through dissatisfaction. We note here that voting is an anonymous activity and perhaps does not give the same psychological rewards as complaining noisily as one leaves. They find that loyalty as they operationalize that concept can reduce exit and increase voice, but that this applies to collective rather than individual voice. They find that the dissatisfied who individually voice are also more likely to collectively voice than the quietly dissatisfied, demonstrating further evidence of the active-passive (or alert-inert) personality contrast. Once they have exited, however, such individuals will no longer individually

voice. This provides indirect evidence of Hirschman's claim that making exit easier might dampen or reduce (individual) voice.

Further evidence of that claim is provided when they show that those who would like to exit state education or health care, but cannot afford to do so, are more likely to individually voice than those similarly dissatisfied but have no intention of exiting or can exit. This shows that being locked in to a particular service or agency does lead one to complain as there is no other alternative.

They do find the sorts of evidence over time one would expect. Those dissatisfied voice, and if they remain dissatisfied are more likely to intend to exit or exit. Those who were dissatisfied, voiced, and then became satisfied are less likely to intend to exit than the former, though more likely to have that intention than the always satisfied, or the dissatisfied non-voicers. Whilst social investment in most cases reduces intentions to exit, it actually seems to increase the propensity to use private health care. It is not clear why that should be so, though in this case Dowding and John's measures of "loyalty" do not directly correspond to support of local state hospitals. Overall, given their more complex three-exit, three-voice, and social investment account, Dowding and John's evidence supports the general claims of the relationship between exit and voice mediated by loyalty.

EVL MECHANISMS

Recall that Hirschman's concern with the exit-voice mechanism is that increasing the probability of exit will decrease voice, which could reduce the overall quality of a service. His worry, shared by many, is that making it easier for parents to send their children to private schools, or for people to shift to private health care, will reduce the incentives for the richest, most educated, and most vocal people to support state provision. This will affect the poorest and most vulnerable people who will then suffer further. The evidence reported in the previous section does seem to show that making exit easier will affect individual voice, and intending to geographically exit will affect the propensity to vote locally, whereas being locked in to a service seems to increase all types of voice. This supports the relationships that Hirschman assumes. Whilst the inference from this evidence to continuing declining quality in services seems reasonable, it does not directly confirm it. Moreover, before blindly assuming that exit and declining voice will lead to declining public services, one must first consider the incentives that public providers might face.

Hirschman's public policy question concerns the relative efficiency of exit and voice mechanisms. In one sense exit is a crude binary signal: in response to changing product specification, quality, or price, consumers can decide either to buy the product or not. In response to that signal and taking account of signals that producers can see for comparable products, firms can make decisions over product and pricing specifications. The fundamental theorems of welfare economics are that, given various assumptions

about the objectives of consumers and firms, conditions of consumer preferences and technology competitive general equilibriums will be Pareto optimal and every Pareto-optimal state can be realized as the outcome of a competitive equilibrium. But of course as we move away from the conditions that lead to these results we move away from efficient outcomes. With regard to public services, for example, the efficiency of exit varies in a number of dimensions: (1) the stochastic nature of the output; (2) ease of identifying consumers of the service; (3) economies of scale in production; (4) search costs of alternatives; (5) costs of switching suppliers; and (6) heterogeneity of ease of exit across the set of consumers (Young 1976). For each of these conditions exit might not bring about more efficient outcomes. On the other hand, with the subtlety of voice consumers can specify more precisely what they want. The efficiency issue for voice is that talk can be cheap and consumers might not really know what they want until they can compare alternatives. Indeed, without the threat of exit voice is likely to be ineffective.

Adding exit might have an informational effect. Where voice is collective there is an informational problem. Not only does the collective action problem arise in the context of action, it also arises in the context of the acquisition of information. Where collecting information is costly, people are not incentivized to collect it. Thus whilst individuals might not be fully satisfied with some public service, collecting information to see how it might be improved might not be warranted, especially since their collective voice actions might not lead to any worthwhile response. However, exit from a product is decisive, it will bring about the outcome they desire, and thus individuals are incentivized to uncover information about alternatives and the relative value of the public service they currently utilize. Such information can also improve their ability to voice. Hence providing exit alternatives can provide stronger incentives to become informed (Brennan and Lomasky 1993: 222–3; Somin 2004: 1344–7). Providing exit opportunities might thus enhance voice and democracy (Frey and Eichenberger 1999; Warren 2011).

Some evidence does suggest that increasing exit options increases quality all round. Böhlmark and Lindahl (2012) argue that increasing exit in Sweden has increased the quality of both private and public providers. From a situation where virtually everyone went to state school, independent schools unevenly developed across different municipalities. Regressing the change in educational performance on the increase in share of independent-school students between Swedish municipalities they find that an increase in the share of independent-school students improves average performance at the end of compulsory school as well as long-run educational outcomes, though these effects took a decade to come about. Since school expenditures have not risen, this rise in performance seems due to the competitive effects of a growing private sector.

Hirschman's EVL mechanism does not really consider the response of providers to exit and voice signals; nor the precise organizations set up by government for the provision of services. These might make an important difference. In the analyses of different organizational forms of exit and voice, most of the attention has focused on information and incentive problems of the different agents (for examples of such analysis for the provision of health services in Britain, see Glennerster et al. 1999 or LeGrand

2007). The complexity of the configurations that these organizational forms can take is daunting. However, the fact that there has been no progress in the *comparative* analysis of public services within the exit-voice framework can only hamper research on the delivery of particular services.

Dowding and Mergoupis (2003) argue that despite evidence of Tiebout moving, there is little evidence that the Tiebout exit mechanism generates efficiency. Most studies are of urban environments far removed from the institutional conditions (multi-service local providers) that are required for efficiency to emerge. In their empirical analysis of the UK, larger urban districts where exit is more difficult are more efficient. They point to voice mechanisms as the reason why fiscal mobility does not create efficiency.

Evidence from Swiss cantons on exit-voice mechanisms suggests exit might constrain the decisions of governments, forcing them to mimic those of their neighbors. Devereux and Weisbrod (2006) see representative institutions as giving less voice than referendums; thus mimicking behavior occurs in cantons with fewer referendums in contrast to those with direct democratic mechanisms. Schaltegger and Küttel (2002) use annual expenditure and tax data from 1980 to 1998 to see whether revenue expenditure is predicted by a combination of voice and exit (measured by fragmentation) and mimicking. They find that voice decreases expenditure, as predicted, as well as fostering mimicking behavior, with an interaction between voice and the neighborhood factor. There is a strong finding for tax: policy mimicking goes down when there is voice, confirming the exit-voice trade-off.

In traditional monopoly government services the major signal has been voice, in all three forms IV, CVP, and CVV. Governments respond to signals delivered through individuals talking to public servants or politicians, through collective acts and through the ballot box. If, as in Hirschman's example, people move their children from state to private schools, not only will there be no voice left to complain about falling standards in schools, but those voters whose children are not in the state system have no incentive to want more money to be spent on state education. They might not voice directly to reduce the quality of state education, but might well voice in favor of lower taxes or money spent on other public services that they still consume. However, exit from state schools might still bring a response from producers if the government sets up the incentives correctly. For example, teachers and heads would be concerned about falling rolls if their pecuniary benefits were based on enrolment.

Financial support for GP surgeries in the UK is indeed partly dependent upon the numbers of people enrolled in the practice, so GP surgeries do have incentives to care about exit. However, such incentives are not without possible deleterious consequences. The ideal situation for GPs would be to have a full register of healthy patients. If they maximize their clientele but do not have to treat them much, they will maximize their income and time. Similarly, if schools can generate full rolls of self-motivated and clever children then teachers are likely to maximize their utility function, job satisfaction. These are "club" effects where organizations try to gain the types of members they want (Buchanan 1965; Cornes and Sandler 1996; Pollitt et al. 1998).

For local governments, such club effects can lead to zoning laws that maximize the revenue stream from buildings whilst minimizing the number of households living in the jurisdiction (Bogart 1993). Egalitarian concerns about such club effects are again that the well educated and well resourced will be better served than the less well off. GPs would prefer richer (and younger) patients, making it more difficult for poorer ones to register with the GP of their choice; schools would cherry-pick pupils from the best-off households. The choice agenda of the government thus serves the better off. The government response to such problems is to restrict the choices of agencies such as schools and GPs, but doing so will also reduce their incentives to provide better services through the exit mechanism. The general problem, of course, is that which Hirschman recognized. The exit process is a rival to the voice mechanism. Voice has traditionally been the option that is socially preferred for choosing public services because of the egalitarian concern that public services should be open to all equally, not only by negative right, but also by positive outcome. For its other efficiency merits, the market does not operate in that manner, and thus Hirschman's original concerns about privileging exit over voice are as important today as they ever were.

NOTES

1. As accessed Sept. 12, 2013.

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CHAPTER 19

GRAHAM T. ALLISON, *THE ESSENCE OF DECISION: EXPLAINING THE CUBAN MISSILE CRISIS*

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INTRODUCTION

MORE than 40 years after first going to press (1971), Graham Allison's *Essence of Decision: Explaining the Cuban Missile Crisis* still continues to attract a wide readership of scholars, students, and even the broader public. The all-time classic continues to rank high on mandatory reading lists in disciplines ranging from international relations (IR), political science, public administration, to business studies. Defying John F. Kennedy's (1963) statement that "The essence of ultimate decision remains impenetrable to the observer—often, indeed to the decider himself," Allison (born in 1940) engaged in the ambitious attempt to provide a better understanding of governmental decision-making during one of the most challenging crises of the cold war.

The continuing interest in his work shows that the insights of the study go far beyond the 1962 crisis. By successively examining the same international crisis through three conceptual lenses (rational actor, organizational behavior, and governmental politics), Allison not only illustrates the complexity of policy-making processes but also encourages scholars to be more conscious and explicit about the categories and assumptions leading their work (1971: 2; 1999: 2).

This contribution examines the continuing relevance of Allison's work for today's scholars in foreign policy and public administration more broadly. Following a brief introduction on the person and the broader context in which the book took shape, it first gives an overview of Allison's three models and how each of them identifies different explanatory factors for the decisions that were made. While taking the first edition

of the book published in 1971 as a starting point, we also, when relevant, refer to additions made in the second edition of 1999. Published together with Philip Zelikow, it introduced some new insights based on declassified material and other recent scholarly work in the field. Secondly, this chapter analyzes Allison's contribution to the overall academic literature on governmental policy-making. This includes a critical assessment of some of the main weaknesses, as well as Allison's response to them. The conclusion comes back to what in our views are some of the main explanations for the continuing relevance of this seminal work.

BEYOND REALISM: OPENING THE BLACK BOX OF THE GOVERNMENT

Graham Allison's work cannot be understood without referring to Harvard, the university where he first enrolled as an undergraduate and later became a Ph.D. student. When working on his doctorate in political science, which he earned in 1968, Allison was a member and rapporteur of the so-called May Group, named after its chairman, the American historian of international relations and foreign policy scholar Ernest R. May.¹ The group debated and reflected about questions such as the role of bureaucracies in public policy and the "gap between the intentions of the actors and the results of governmental action" (1971: preface, p. ix; 1999: preface, p. xiii). The group was a key source of inspiration and provided feedback as Allison tried to formulate the central arguments of *Essence of Decision*. At that time the interest in organizations and bureaucracies was innovative, if not revolutionary, certainly in the field of foreign policy. Scholars in this area were mostly working in the realist tradition, building on the insights of Edward H. Carr (1939) and Hans Morgenthau (1948). Making a sharp distinction between domestic and international politics, they conceptualized states as unitary, rational, and utility-maximizing actors whose multifarious modes of inner deliberation are of no interest when trying to understand international politics. Treating governments as black boxes, this paradigm hardly paid any attention to the role of the governmental machine in decision-making.

Allison set out to address this as a major conceptual drawback, and proposed two alternative models to the predominant rational actor model. The gist of the model labeled as the "Organizational Behavior Model" was based on insights from organizational theory. Most importantly, Allison made extensive use of the work of the so-called Carnegie school, including Herbert Simon's *Administrative Behaviour* (1947), Simon and James March's milestone *Organizations* (1958), and *A Behavioural Theory of the Firm* by Richard Cyert and March (1963).

His other model explains governmental behavior as being the result of bargaining processes amongst different governmental actors, both at the political and bureaucratic level. For the development of this third model, which is his key contribution to the

academic literature on governmental policy-making, he is much indebted to Richard Neustadt, also a member of the May group. His seminal study on *Presidential Power* (1960) was among the first to point out the importance of bargaining and persuasion in policy-making at the highest levels.

Although the main focus of this chapter will be on the academic significance of his work, it is important to emphasize that Allison did not confine his activities to the secluded circles of academia. Throughout his career, he has been a leading analyst of American foreign and security policy. In the 1960s, he acted as adviser to the Pentagon and, from 1985 onwards, he was on the Defence Policy Board of several US Secretaries of Defence. During the first Clinton Administration, he served as Assistant Secretary of Defence and gave shape to US policy toward the former Soviet Union. As dean of the John F. Kennedy School of Government (1977–89), he maintained close contacts with the US civil service. His continuing engagement in foreign affairs also comes to the fore in the second edition of his book (1999). In the introduction he warns against becoming complacent about the risks of nuclear weapons and appeals for a fundamental rethinking of US foreign policy in the post-1989 world (also e.g. Allison et al. 1996). In his current position as director of the Belfer Center for Science and International Affairs, he remains an important and well-respected voice in the US foreign policy debate, be it on the question whether or not to use airstrikes against Syria or US policy toward an emerging China.²

Limits of the Rational-Actor Paradigm

The case study Allison chose to present his insights on governmental action is that of the Cuban missile crisis, often seen as one of the most dangerous moments of the cold war. These 13 days of extreme tension between the superpowers in October 1962 brought the world to the brink of an atomic apocalypse. The story of the crisis, known as the Caribbean crisis in Russia and the October crisis in Cuba (Allyn et al. 1989), has been told numerous times (see Scott 2012 for an overview). In the late summer of 1962, the Soviet Union had started preparations for the installation of nuclear warheads on the Cuban island. When, on 14 October, the United States (US) found photographic evidence of the presence of nuclear missiles, it urged their dismantlement and return to the Soviet Union. In the following days, both powers struggled to find a way out of this predicament by flexing muscles, proposing deals, and trying to anticipate the reaction of the opponent. At various points, the situation almost escalated into an open war, but on 28 October President Kennedy and Premier Khrushchev finally managed to reach an agreement whereby the Soviets dismantled and withdrew their missiles and the US committed itself never to attack Cuba.

While being a fascinating story for anybody with an interest in international relations, the main merit of Allison's contribution is that he uses this case to make a substantial academic contribution to our understanding of governmental

decision-making. He does so by organizing the content along three distinct theoretical models, or lenses, which are each given a subsequent empirical chapter on the crisis. Thereby, he juxtaposes competing paradigms and encourages systematic comparison of their explanatory power by presenting their core assumptions. Each of those parts addresses the following questions from a different perspective: (1) Why did the Soviet Union place strategic offensive missiles in Cuba? (2) Why did the United States respond with a naval quarantine of Soviet shipments to Cuba? (3) Why were the missiles withdrawn?

Model I builds upon the predominant rational-actor paradigm, and is linked to concepts like rational choice and game theory. Allison reviews the academic literature, as well as lay notions on foreign policy, both of which, he concludes, are dominated by the analogy between the nation and an individual person. Sitting in her armchair, the analyst could think of what she would do, if she were the Soviet Union. The individual is assumed to be rational, i.e. completely informed about all alternatives, and utility maximizing. Typically, a rational-actor explanation sees the US decision to impose a naval blockade as the best choice in terms of US interests. It shows determination but at the same time avoids an immediate aggressive attack and places the ball back in the Soviet camp. In case of the eruption of a military clash, a naval conflict in the Caribbean would place the US in an advantageous position.

Overall, a critical overtone swings throughout the survey of this classical model, which sees the state as a unitary decision-maker with specific goals and objectives. It is portrayed as a too simplistic account of a rather abstract and intricate entity. Thus, the gist of Allison's elaborations on the first model is that its explanatory value is limited.

Organizational Theory as an Alternative

Model II radically opposes the view of governments as centrally controlled and perfectly informed actors. It conceptualizes governmental behavior as the output of large organizations, acting quasi autonomously but coordinated by governmental leaders. In order to reduce the complexity of the tasks they face, organizations operate according to pre-established routines and standard rules. Together with the organizational culture, these rules constrain and shape the behavior of the many individuals in the organization and color its output. Those who want to understand formal governmental decisions have, therefore, to examine the options defined by the different relevant organizations. Although the final decision remains with the government, its choices will be confined to the menu of alternatives presented by the organizations. In the case of the Cuban missile crisis, two major alternatives dominated the debate: an airstrike and a naval blockade. That the air force could not guarantee a sufficient level of success in case of an airstrike resulted in a decision for a naval blockade. According to the explanation provided by Model II, organizations defined "what the President believed

U.S. military equipment and personnel were capable of performing,” and ultimately determined his choice (Allison 1971: 124).

A second key conjecture of this model is that administrative routines may lead to unintended outcomes. A telling example of this is the lack of camouflage for the Soviet missiles on Cuba, which allowed the US to identify them quite easily. Soviet standard operating procedures did not include the use of disguise for nuclear warheads because these were usually installed on Soviet territory (Allyn et al. 1989: 153). While such procedures might have been in line with the preferences of the Soviet Union at the time they were decreed, they certainly undermined its goals in October 1962.

The organizational behavior model clearly distances itself from the view of decision-makers as rational actors. Instead, it builds on Herbert Simon’s notion of bounded rationality. This entails that people are not comprehensively informed about the entire range of alternatives for a decision. Instead of utility maximizing, policy-makers are “satisficing,” i.e. they do not endlessly look for the optimal solution, but choose the one that is good enough to satisfy their needs.

Although the second model heavily builds upon the literature on organizational and administrative theory, especially from the Carnegie school, Allison would be amongst the first to apply it to the field of foreign policy. While one should not underestimate the impact of this model on the study of organizations in foreign policy, scholars tend to draw on the original works rather than on Allison’s adaptation, not least because his interpretation of the Carnegie school is controversial (e.g. Bendor and Hammond 1992; Bendor 2003). His genuine contribution to the study of governmental behavior was the last model.

Governmental Action through the Lenses of Political Bargaining

The third conceptual model is labeled as “Governmental Politics,” but is also often referred to as the “Bureaucratic Politics” model. Whereas the second model sees the heads of the organizations as fixed monoliths, the third one argues that the identity of individual actors matters and sees governmental action as the result of a bargaining process amongst key players at the highest level. This does not only include political leaders but also top-level civil servants. Depending on their organizational affiliation and personal beliefs and interests, these actors will express different preferences. The final governmental decision is the result of this competitive political game. It is the task of researchers to try to display the conflicts and compromises of this “intra-national” bargaining process. In that light, three analytical aspects become crucial to the understanding of foreign policy decision-making: (1) Who plays? (2) Who holds what position? (3) How are the inevitable disputes

solved? Seen through these lenses, the US decision to opt for a naval blockade can best be explained as the result of the bargaining process that took place amongst the President's most respected advisers gathering in the Executive Committee of the National Security Council (ExCom).

The third model was inspired by notions of political bargaining and turf-battles, which are omnipresent in democratic power and party politics. Allison saw a miniature version of this shaping governmental outcomes in foreign policy. He compares the final decision to a "collage" of different pieces that gradually take shape during the negotiation process (1999: 346).

The second and third model are distinguished but closely linked. Allison presents them as "a more refined level of investigation" of the top layer of all the individual departments and institutions that comprise a government (1999: 255). Indeed, in other work (e.g. Allison and Halperin 1972), he seems to analytically combine both models, which prompts the question whether they can really be seen as separate paradigms. In the revised 1999 version of the book, however, Allison returns to a presentation on the basis of his three models, while, unfortunately, omitting to comment on this issue.

A STRONG IDEA IN SEARCH OF FURTHER THEORETICAL REFINEMENT

Allison's most important contribution to the academic literature undoubtedly is his identification of bureaucracies as key agents in the shaping of foreign policy. As already mentioned, these insights were rooted in earlier studies, both at Harvard and beyond. In the early 1960s, there had already been a first wave of scholars, including Hilsman (1959), Huntington (1961), and Schilling (1962), who had advocated the study of the independent role of bureaucracies as a central source of US foreign policy. Also in the field of public administration more broadly, authors like Tullock (1965), Downs (1967), and Rourke (1969) had been putting the development of a theory of bureaucratic decision-making on the research agenda. Of these early studies, Allison's work is amongst the best known and most cited in the field of foreign policy.³

The great attention that it received, however, does not mean that the study was uncontroversial. On the contrary, despite, or maybe because of, its far-reaching impact and prominence, every aspect of the actual content of *Essence of Decision* has been subject to criticism and academic debate: be it with regard to the historic account of the crisis, the theoretical elaborations of each model, or the intricacies of concept operationalization (Smith 1980). The shortcomings are so significant that only few studies applied more than the general notion of bureaucracies as an

independent variable to cases beyond the Cuban missile crisis. Even if they make the explicit reference to *Essence of Decision*, they tend to use highly modified versions of Allison's proposed models (e.g. Monten and Bennett 2010). In what follows we limit ourselves to the theoretical, methodological, and normative debates raised by Allison's work. We include comments on the first as well as the second version of the book, as many saw the revision as insufficient in addressing the earlier weaknesses (see e.g. Houghton 2000).

Rationalist Responses and Criticisms

Not surprisingly, the identification of bureaucratic bargaining as a determining variable in the foreign policy-making process received fierce criticism from the realist corner. A well-known example is Steven Krasner's article (1972) in *Foreign Policy* that radically refutes seeing managerial skills and administrative feasibility rather than rational assumptions as key determinants of governmental policy-making. A key problem in his eyes is that Allison seriously underestimates the power of the President. When appointing his advisers, a political leader tends to choose people close to his own views and values and, generally, appointees will be loyal to their superior. Since individual actors and bureaux depend on presidential support to advance their goals and interests, a chief executive has much more leverage than *Essence of Decision* assumes. For realists the preferences of the administration are not independent and much closer to the values of the political leadership than model III suggests. Rather than being a *primus inter pares*, political leaders matter. If bureaucratic organizations and individual players manage to set the tone, it is not due to their independent power but to a failure of the President to exert control. He is the one who is in charge and to be blamed in case of failure. Thus, Krasner and others have pointed to the serious accountability problem that emerges if decisions are merely the outcome of political bargains, as Allison suggests (Krasner 1972; Rosati 1981). If policy failures were the result of fights in the bureaucratic machinery, it would be difficult to hold leading politicians responsible for their acts. As a result, political elections are at risk of being pointless. With regard to such prospects, some scholars have pointed to the historical coincidence of the widespread cynicism towards politics because of the Vietnam debacle, and Allison's refutation of rationalistic approaches (Freedman 1976; Bernstein 2000).

Apart from the normative concerns that the rejection of the rational-actor model invokes, there are also analytical reasons to question Allison's treatment of this research tradition (Welch 1992). Not only have models of rational decision-making and game theory evolved considerably since the publication of *Essence of Decision*, they allowed for more complex and appropriate conceptions even in the early 1970s. Jonathan Bendor and Thomas Hammond assert that Allison never fully developed model I, but assumed what it would look like if one were to do so (Bendor and

Hammond 1992: 305). Among other things, they criticize him for not including multiple goals, a time-dimension with repeated games, uncertainty, and Pareto inferior Nash equilibria. By including those factors, the explanatory power of the rational-actor paradigm could have been increased considerably. This line of criticism from rationalist scholars is important to understand the limited impact Allison's proposition to study bureaucracies had on the broader field of international relations, where conceptualizations of nations as unitary-actor continue to be dominant.

Bureaucratic Politics Paradigm Remains Elusive

The bulk of comments has concentrated on the third model. Many of the criticisms point to the lack of rigidity of the model's propositions (Holsti 1972; Art 1973; Smith 1980; Bendor and Hammond 1992; Houghton 2000). When presenting his governmental politics paradigm, Allison lists six different so-called "elements" which he describes, illustrates with examples, and tries to grasp by posing questions and making analogies with daily life (1999: 294–313). Much of this description, however, remains rather vague and it is sometimes difficult to discern what the principal building stones of the model are. He, for instance, elaborates a list of factors shaping players' perceptions and preferences, but it is far from clear whether they are all considered to be equally important. Do "goals and interests" weigh more than "parochial interests"? How do they relate to the factor of "stakes and stands"? Another example of the lack of rigidity is his treatment of the issue of power. Although he argues that the impact of each player on the final bargain is supposed to be determined by power, Allison, in neither version of *Essence of Decision*, is decisively clear what this actually implies, especially with regard to the role of the president. He calls it "an elusive blend of at least three elements: bargaining advantages, skill and will in using bargaining advantages, and other players' perceptions of the first two ingredients" (1971: 168). Also here, the reader remains in the dark since there is no attempt to formulate clear propositions on how exactly these factors may impact upon the influence of the players in the game. Furthermore, many scholars (e.g. Houghton 2000; Bendor and Hammond 1992; Krasner 1972) reject the theoretical value of Allison's "Where You Stand is Where You Sit" proposition, according to which "the stance of a particular player can be predicted with high reliability from information about the person's seat" (1999: 307). The critics argue that players in the game do not necessarily always have a stance and, even if they have one, it may not necessarily reflect the position of the organization they represent. With regard to Allison's case of the missile crisis, Krasner for instance points out that Secretary of Defence Robert McNamara took a much more pacific position than one would expect from a representative of the Pentagon (Krasner 1972: 165).

Allison himself is conscious of "the paucity of general propositions" of his new paradigm and partly blames it on "the extraordinary complexity of cases"

(1999: 305). For an author who has theoretical ambitions, this is not very convincing. The lack of clear hypotheses and testable predictions, which has not really been addressed satisfactorily in the second edition, remains one of the most important weaknesses of the book until this day. Even though the almost three decades that lie between the first publication of *Essence of Decision* in 1971 and the second version from 1999 have brought forward a variety of seminal critiques, Allison failed to use this potential appropriately to revise and further develop the argument (Houghton 2000). Instead of reformulating the propositions of the bureaucratic politics model in a more rigorous way, the second version dilutes the analytical clarity further by unsystematically including factors that might influence the preference and power distribution of the game. While this certainly enriches the empirical understanding of a single case, it disregards the theoretical ambitions, which should be guided by the quest for the most parsimonious model.

Case Selection Bias

Methodologically, the focus on only one, rather extreme, case has raised questions about the generalizability of the findings. For Allison, bureaucratic politics is not unique to the US government and he states that “students of crisis behaviour in many governments readily find examples where actions can only be explained by the turmoil or bargaining of internal politics” (1999: 256). Several observers have questioned this, especially when it comes to developing countries where the administration is poorly developed, and authoritarian regimes where power is firmly concentrated at the top (Clapham 1978; Dawisha 1981; Smith 1980). Others have raised questions about the representativeness of the Cuban missile crisis (Art 1973). The case selected for his study is one of the major crises of the cold war period. The US bureaucrats supporting President Kennedy may have been powerful in this particular instance but would they have been equally powerful in other less dramatic cases? After all, ExCom was an entirely new institution, the lack of information about the Soviet Union’s intentions was abundant, and significant uncertainty inevitable. Such crises situations that require policy-makers to think outside the box are not rare in foreign policy, but they are also just a selection of instances that we want to explain analytically.

The title of Allison’s book claims a very high level of generalizability. He asserts not only that his models help to understand foreign policy decision-making, but also the “essence of decision” in other policy fields (cf. 1999: 7). On the one hand, the intention is welcome since it does away with the traditional, but not necessarily convincing, distinction between foreign policy and other fields of governmental decision-making. On the other hand, it remains questionable whether the book can be more than a vague

starting point for a methodologically more sophisticated empirical assessment of its key propositions.

The Influence of an Idea

The idea that bureaucracies have a mind of their own, and are not the ideal-type neutral bureaucrats as described by Weber ([1922] 1978), remains a powerful and appealing idea to this day. It is probably hard, if not impossible, to find a handbook on foreign policy analysis that will not refer to Allison's work as one of the early and principal studies on bureaucratic politics. In his footsteps, several others have undertaken case studies in which they have tried to identify the impact of bureaucratic agents on foreign policy outcomes (see e.g. Halperin 1974; Gallucci 1975). Studying bureaucracies is now accepted as a worthwhile focus, which can improve understanding of foreign policy outcomes. This widespread acceptance can be largely attributed to the appeal *Essence of Decision* has even today.

Still it remains that the idea of studying bureaucracies has not had much of an impact upon the major paradigms in the field of international relations such as realism and liberalism. Still dominating the mainstream of IR, these schools of thought tend to largely ignore Allison's plea to also pay attention to "intra-national politics." This may have to do with several reasons. First, neither Allison nor any of the other scholars working in the bureaucratic politics tradition has taken up the challenge to develop a more refined theoretical framework with testable hypotheses. In the absence of an accepted baseline theory, many of the case studies on the topic stand on their own. As a result, it has been difficult to use their findings for the development of a more robust analytical frame. Without such a common focus, the field struggles with the predicament that it is hard to argue for any generalization based on the individual findings.

Secondly, in many cases it remains quite a challenge and time-consuming to practically implement the process-tracing methods necessary to determine the position of different actors in the bargaining game. It cannot be taken for granted that politicians and senior officials will be willing to share information about political strategies and turf wars. Even in the prominent, well-researched, and by now historical case of the Cuban missile crisis, Allison and other scholars are confronted with problems like selective memory and biased accounts (see e.g. Bernstein 2000). It is obvious that this imposes even more severe restrictions for the research on recent cases, where interviewees might have additional, substantial, interests apart from losing a certain reputation. Unless one has preferential access to internal sources or is entitled to do participant observation it remains very difficult to collect the required data. It is self-evident that such high costs of, and restrictions on, data collection precluded Allison's approach from becoming as prolific and comprehensive as other research programs in

IR. However, the lack of a widely accepted successor for Allison's proposed, but unfinished, bureaucratic politics model only exacerbates this impediment. The vagueness of the theoretical insights, and the related uncertainty of finding "the kernel of the brute" within the governmental machine, often stands in stark contrast to the high costs of such research.

The most important legacy of Allison in the field of IR in general and foreign policy analysis more specifically remains that he has put political and bureaucratic agency on the research agenda. The governmental politics model, however, did not develop into a new and fully fledged IR paradigm. The reasons for the latter, in our view, go beyond the listed theoretical shortcomings and methodological impediments. Governments are more than an arena for internal maneuvering and fighting. And foreign policy is a complex function of multiple variables. The exact constellation of explanatory factors are issue-specific, and can certainly not generally be reduced to the influence of political bargaining among bureaucrats and politicians. It is therefore important to apply the bureaucratic politics model in conjunction with other explanations (see also Hill 2003). In many cases, however, it remains intuitive to first look at economic and geopolitical influences when studying the interaction of nations on the international stage.

As mentioned in the introduction, an important merit of *Essence of Decision* is that it managed to reach out beyond the often rather closed community of foreign policy scholars. Rather than repeating the general mantra of the *sui generis* character of foreign policy, Allison underlines that his findings "can be applied broadly in arenas beyond foreign affairs" (1999: 7). He does not only refer to other public policy fields and regional and local governments but also to nongovernmental organizations such as "the United Nations or Red Cross; schools, universities, and hospitals; business enterprises; and other aggregate actors whom one encounters in normal, everyday life". His message about the overall centrality of bureaucracies in policy-making fell onto fertile ground in the field of public administration. Here, others such as Downs had already pointed to bureaux as being "among the most important institutions in every nation in the world (1967: 1). By echoing this view, Allison managed to fulfill a bridging function between two research communities that traditionally tend to be rather segregated. This does not mean, however, that Allison's model started to be widely applied or used as a basis for further refinement. Also here, the general message that bureaucracies matter was more important than his contribution to the development of an overall theoretical paradigm.

Last but not least, one should not omit to refer to Allison's capacity to reach out to the world of practitioners. His account of the Cuban missile crisis became the unofficial bible for governmental elites attending the J. F. Kennedy School of Government at Harvard and many other schools of public administration (Bernstein 2000). Also his numerous functions in government were a further way to spread his insights. It is another illustration of Allison's capacities to build bridges, this time between academe and practice.

CONCLUSION

Despite the many criticisms, Allison's work continues to be a classic for readers from many different disciplinary backgrounds, and this can be expected to continue for years to come. In our view this continuing interest can be explained by the following.

First and foremost, Allison managed to put bureaucracies on the research agenda of foreign policy analysts. His plea not to treat states as black boxes, but to investigate the role of the governmental machine, its organizations, and political actors, was of particular importance for scholars from the field of international relations. He showed that there is a case for not conceptualizing the state as a monolithic quasi-individual when trying to understand policy-making, but to take the intra-national level into account. The manifold methodological and theoretical shortcomings of the book, however, still leave substantial room for debate on how far-reaching the impact of bureaucracies in foreign policy is, especially in non-crisis situations. As political scientists tend to strive for the most parsimonious theory, this is an important question, but Allison's work alone cannot give a satisfying answer to it. His central contribution is that he fostered this debate, and contested the predominance of classical paradigms of international relations.

A second reason is related to the illustrative and didactic set-up of the study. Most case-study approaches choose to focus on one particular theoretical framework. Additionally, they often still fail to be explicit and clear about their core prepositions. Allison counters with a comparative approach, juxtaposing different paradigms and elaborating on their underlying assumptions. Even though his study is not considered as the most convincing example of such an approach, many of the critiques have been made possible exactly because of Allison's attempt to be as explicit as possible. By doing this, and by using the fascinating case of the Cuban missile crisis, his book remains a useful tool to convey the importance of being conscious and explicit about analytical assumptions, be it in scholarly contributions, or in classrooms.

Thirdly, Allison managed to draw the interest of a wide variety of research communities as well as the world of practitioners, well beyond the realm of foreign policy scholars. His work not only inspired foreign policy analysts, historians, and scholars in the field of public administration, but also researchers in the field of psychology, business administration, and more. To some extent his work was an appeal for an interdisciplinary approach before this had become a buzzword. In the prefaces of both the first and second editions, Allison explicitly expresses the ambition to combine the historian's affinity for detailed narratives, and social scientists' striving for general theorizing (1971: p. vi; 1999: p. x). He takes the view that "artists" and "scientists" can learn from each other and points to the importance of at least getting acquainted with the insights produced by the other group. By attracting such a wide readership, Allison has managed to fulfill a disciplinary bridging function.

A final reason why *Essence of Decision* continues to be a good read is that it provides us with a lesson of modesty, reminding us that “there will always be the dark and tangled stretches in the decision-making process—mysterious even to those who may be most intimately involved” (1999: p. xi). This should however not prevent those with an interest in governmental decision-making from trying to make further efforts to get a better understanding of bureaucratic agency in foreign and public policy and to give further shape to Allison’s ambition to develop a model that can make sense of what is going on within the governmental machinery.

NOTES

1. In 1997, May would, together with Philip Zelikow, transcribe the secret White House tapes made during the Cuban missile crisis (May and Zelikow 1997). These records were an important source for the revised version of *Essence of Decision*.
2. See <http://belfercenter.ksg.harvard.edu/experts/199/graham_allison.html> accessed Oct. 2013.
3. At the time of writing Google Scholar lists c.8,000 citations of the 1st edn. Morgenthau’s *Struggle for Power and Peace* has also 8,000 citations. March and Simon’s *Organizations* has around 18,000.

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CHAPTER 20

GEORGE J. STIGLER, “THE THEORY OF ECONOMIC REGULATION”

CHRISTOPHER CARRIGAN AND CARY COGLIANESE

In the field of regulatory policy, few articles have achieved the impact of George Stigler’s “The Theory of Economic Regulation,” published in 1971. Stigler punctured the idea that regulation arises solely to advance the overall public interest by correcting market failures. He forcefully argued that instead “regulation is acquired by the industry and is designed and operated primarily for its benefit” (p. 3). Although Stigler never used the phrase “regulatory capture” in “The Theory of Economic Regulation,” his article has nevertheless come to be so identified with the idea that regulation serves private interests that it is hard to find any serious discussion of regulatory capture in the last 40 years that does not at least cite Stigler’s work. Indeed, the Social Sciences Citation Index reports that, in the year 2012 alone, over 90 articles cited Stigler’s “Theory”—notwithstanding the passage of more than 40 years since its publication. Clearly, Stigler’s work “changed the way economists analyze government regulation” (Peltzman 1993), and it “has exercised enormous influence over a large body of researchers” from other disciplines as well (Mitchell and Munger 1991).

Stigler’s “Theory” has had this impact—and should continue to—even though it is admittedly far from perfect. Some of its language, for example, exaggerates the power of business, in particular Stigler’s oft-quoted claim that regulation “as a rule” (p. 3) benefits regulated industries. In addition, its empirical evidence failed to rule out competing explanations, including the very possibility of the public interest theory that he sought to challenge. Yet, notwithstanding these limitations, Stigler’s article was at the time of its publication path-breaking in both its theoretical and empirical treatment of business–government relations. Even today, when public policy has expanded well beyond the economic regulation of discrete industries that Stigler studied, his insights remain important for understanding both the motivations of and the influences on government regulators, clearly distinguishing the positive enterprise of *explaining*

regulation from the normative task of *justifying* regulation. More effectively than others before him, Stigler framed vital questions about why and how regulation arises, questions that have preoccupied the most recent generation of regulatory scholars and will rightfully preoccupy generations to come.

STIGLER'S "THEORY"

Stigler made a strong claim in "The Theory of Economic Regulation": regulation is just a product, produced in a marketplace like any other product is. The main difference between regulation and other products is that the political process defines the structure of the market for regulation. As long as the differences between political and economic markets are taken into account, the application of standard concepts like monopoly and oligopoly, and tools of economic analysis like supply and demand, can provide useful answers to important questions about why regulation arises and what forms it takes. Of course, by the time Stigler was writing, other scholars had already recognized the value of a political economy approach to public policy (e.g. Buchanan and Tullock 1962; Olson 1965). Stigler, though, commanded attention because he applied insights from political economy specifically to regulation. He sought to dislodge what he called the "idealistic view" (p. 17) of regulation, namely that regulation advances the public interest, a view that had its hold on many economists and other scholars at the time. According to Stigler, regulation largely advances private interests because of the way political institutions create incentives for political leaders to emphasize an industry's interests over the broader public's interests.

In his "Theory" article, Stigler began by defining the products that government supplies in the regulatory marketplace. He noted that "the state has one basic resource which in pure principle is not shared with even the mightiest of its citizens: the power to coerce" (p. 4). Government uses this power to compel its subjects to pay taxes and follow rules. That power of coercion can be deployed in such a way as to help some individuals and industries at the expense of others. By trying to influence how the state uses its coercive authority, businesses seek to "buy" one or more of government's four main products: subsidies; control over competitive entry; regulation of product substitutes or complements; and the fixing of prices.

Stigler assessed the business value of each of these four products. Subsidies obviously provide firms with direct monetary benefits, but Stigler argued that they are usually not the first choice for business because they typically need to be shared with all firms in a sector, including entrants. For example, leading universities may lobby successfully for increases in research funding only to have other universities compete for these same funds. By contrast, firms much prefer regulations that operate as barriers to entry by potential competitors, or that otherwise disadvantage substitute products or advantage products complimentary to their own. Some of the starkest examples of entry barriers include requirements that regulators approve new trucking routes or the entry of

new airline carriers. But any time a regulation contains a grandfather clause exempting incumbent firms from new requirements, regulation increases the relative costs to new entrants. Just as existing businesses prefer regulatory barriers to entry, Stigler argued that they also favor the creation of institutions that can impose price controls on their sectors, especially if these institutions can be influenced to keep prices at levels higher than competitive rates.

Of course, simply because businesses can use regulation to enhance their profits, this does not mean that every firm will get exactly what it wants in the political marketplace. Stigler explained that the political process does not function like an ordinary market. Instead of products being allocated to the highest bidder, the political market ostensibly gives everyone a say (or at least a representative who has a say). This introduces complexity and uncertainty that firms must factor into their calculation of the expected benefits of regulation. "The channels of political decision-making" are, according to Stigler, "gross or filtered or noisy" (p. 12). For this reason, smaller businesses might actually sometimes reap disproportionate gains through the political process relative to what they would through the economic marketplace. Some firms may not succeed at all.

But still, business holds important advantages in the political process. All voters may have a say, but they also have little incentive to learn about policy proposals and actively express their preferences about them (Downs 1957). Not only does this inhibit their ability to reward political actions taken in their interest, it also limits their capacity to punish those politicians who champion policies that hurt them. The well-known challenges of collective action, enunciated by Mancur Olson (1965) six years before Stigler's "Theory," effectively function to privilege concentrated industry interests over the broader public interest. As a result, businesses with large stakes in regulation often get their way. These firms provide political parties and candidates with financial resources: campaign contributions, fund-raising efforts, jobs for political party members, and contracts with politicians' businesses, such as law firms. They also work to support get-out-the-vote efforts in favor of business-friendly representatives and causes.

In "The Theory of Economic Regulation," Stigler not only developed this theoretical explanation for industry's capture of the regulatory process, but he also sought to bring empirical evidence to bear on it. Reflecting his view that the economics profession was insufficiently attentive to both empirical analysis as well as government regulation (Stigler 1965, 1975), Stigler illustrated his claims in "The Theory" with references to different types of regulatory arrangements, such as oil import quotas. But he also put forward regression analyses of two state-level regulatory schemes—trucking regulation and occupational licensing—to support his political economy account of regulation. In the first of these regression models, Stigler focused on the limits that states placed on truck sizes and weights around the time the trucking industry started to expand in the 1930s. Stigler claimed that the stringency of these limits across different states correlated with variables related to the interests of the agricultural and railroad industries in each state. As his measure for

the importance of trucks to a state's agriculture industry increased, the limits on trucks grew less strict, presumably because the powerful farm lobby would ensure that farmers who needed large trucks would be allowed to use them. However, the shorter the railroad freight lines were in the state, the more restrictive the truck limits were, seemingly because trucks competed more with railroads on shorter routes. In these cases, restrictions on the size of trucks served the railroads' interests in limiting their competitors.

He also examined state licensing of occupations, such as beauticians, architects, lawyers, embalmers, and dentists, because these requirements restrict entry. Stigler hypothesized that state licensing would have occurred earliest in those states where the occupation's political strength was greatest, as measured by the raw numbers of individuals in the occupation as well as their concentration in urban environments (which presumably made it easier for them to act collectively). Stigler's analysis of data from around the turn of the twentieth century generated results that were, in his words, "not robust"—but that showed, "in general, the larger occupations were licensed in earlier years" (pp. 15–16). Stigler also compared licensed and unlicensed occupations in 1960 and found at least "a modicum" of descriptive evidence to suggest that licensing exists not to protect consumers but to limit the ability of potential entrants to practice the profession (p. 17).

For Stigler, then, the idea that regulation benefits business not only grew out of economic theory, but it also found support in what he considered to be an "illustrative" empirical analysis (p. 7). "The Theory of Economic Regulation" aimed to reshape economists' thinking about regulation, making the case for analyzing regulators' behavior using the same kinds of theories and methods economists use to analyze any other producer and consumer behavior. In his concluding comments in "The Theory," Stigler took aim at the simple-mindedness of economists who lambasted the Interstate Commerce Commission (ICC) for supporting railroads to the detriment of overall social welfare:

This criticism seems to me exactly as appropriate as a criticism of the Great Atlantic and Pacific Tea Company for selling groceries, or as a criticism of a politician for currying popular support. The fundamental vice of such criticism is that it misdirects attention: it suggests that the way to get an ICC which is not subservient to the carriers is to preach to the commissioners or to the people who appoint the commissioners. The only way to get a different commission would be to change the political support for the Commission, and reward commissioners on a basis unrelated to their services to the carriers. (p. 17)

Although Stigler never used the precise phrase "regulatory capture" in his article, the closing words he used certainly sounded with the same spirit of resignation about industry dominance of the regulatory process that usually accompanies charges of capture. He left the reader to wonder if there really could be any realistic way to avoid having regulators who were "subservient" to industry.

INFLUENCE AND CRITIQUE

Stigler was by no means the first to note that businesses seek to influence regulatory agencies to their advantage. For some time, political scientists and historians like Samuel Huntington, Marver Bernstein, Gabriel Kolko, and Theodore Lowi had been describing regulatory agencies which regulated industries had manipulated for their own benefit (Huntington 1952; Bernstein 1955; Kolko 1963; Lowi 1969). Nevertheless, for economists and other scholars of regulation, Stigler's article provided the theoretical foundation—largely absent from prior research—upon which a more extensive research effort on the political economy of regulation could be built (Posner 1974). Using his theory, Stigler sought to explain his own discoveries that regulation did little to achieve its goals in controlling utility prices (Stigler and Friedland 1962) or improving the quality of securities offered for sale to the public (Stigler 1964). He also undertook to explain similar empirical findings by others. As Sam Peltzman has described:

While this image [of the regulator captured by the regulated industry] was hardly new . . . , the willingness of many economists to embrace it on the basis of mounting evidence was new. . . . [T]he evidence of capture seemed to ask for an explanation of why regulation had come to work in this seemingly perverse way. The answer [was] provided in Stigler's (1971) article on the theory of regulation. (1993: 822)

Others before Stigler had lamented the success that industry interests had in reorienting regulation, but by using economic principles to explain regulatory activity, Stigler also sought to show how and why regulatory regimes could be acquired—not just altered—by business (Posner 2013). In short, Stigler had articulated what soon became known as the “economic theory of regulation” (Posner 1974; Peltzman 1989).

Peltzman (1976: 211)—in his own widely-cited article—formalized Stigler's theory, acknowledging his “intellectual debt” to Stigler's “pioneering work.” He also extended Stigler's analysis by postulating that regulators face both consumer and industry demands for regulation. He showed formally that a rational regulator will respond not by entirely delivering what a monolithic industry wants to the exclusion of others but by seeking an outcome that optimizes political support from all groups interested in regulation. Subsequent theoretical analyses further broadened Stigler's simple characterization of the interest group environment (Becker 1983; Grossman and Helpman 1994), more carefully distinguished the incentives faced by legislators and their agents (Weingast and Moran 1983; Laffont and Tirole 1991), described how political actors can use regulation to extract rents (McChesney 1987), and illustrated how attention to politicians' motivations can help distinguish between regulatory capture and the pursuit of public interest objectives (Levine and Forrence 1990).

In addition to theoretical extensions, Stigler's economic theory of regulation has prompted a multitude of empirical investigations of business–government relations across a variety of industries, including airlines (Levine 1981), mining (Kalt and Zupan

1984), banking (Kroszner and Strahan 1999), and manufacturing (Maxwell et al. 2000). His work has inspired inquiries spanning a broad set of policy and research domains, including accounting standards (Watts and Zimmerman 1978, 1990), rules for new business entry (Djankov et al. 2002), and trade policies (Hillman 1982; Grossman and Helpman 1994).

But perhaps the greatest indicator of the influence of Stigler's ideas may simply be the reaction that most contemporary readers will likely have upon reading "The Theory" today: it all seems rather obvious. Since the 1970s, thinking about regulation from a political economy perspective has become well-accepted within academic circles and more broadly. The perception that agencies can be captured has become commonplace. One need only point to reactions to various disasters in heavily regulated industries—the mortgage crisis and Great Recession, the Gulf of Mexico oil spill, and the Fukushima Daiichi nuclear accident in Japan—to see indications of Stigler's legacy. Much of the blame for these recent crises has been laid upon regulators who purportedly made themselves too subservient to the industries they regulated (Carrigan and Coglianese 2012).

In spite of its vast impact on regulatory scholarship (or perhaps because of it), Stigler's "Theory" has also invited its share of criticisms. These critiques have ranged from knocking down the strong claims that Stigler appeared to make in certain passages, to challenging the validity of his empirical analyses. To be complete, any consideration of Stigler's "Theory" should acknowledge at least four critiques of his work—even if none of them undercut the core contribution he made in utilizing the tools of theoretical economic analysis to explain how regulation actually gets implemented.

First, Stigler's article can be read to exaggerate the power of business over regulation. Some of his language definitely left this impression. He stated, for example, that his "central thesis" was "that, as a rule, regulation is acquired by the industry and is designed and operated primarily for its benefit" (p. 3). Such a strong claim about business dominance "as a rule" begs to be challenged—and easily so. After all, any suggestion that business holds an iron-clad grip over regulatory policy is contradicted by persistent, and often unsuccessful, business opposition to the imposition of costly regulatory burdens (Kamieniecki 2006). However, notwithstanding Stigler's sometimes forceful language, it would be somewhat unfair to attribute to Stigler the bold claim that regulatory capture occurs "as a rule." He never put forth evidence in "The Theory" that showed regulatory capture occurred with any regularity. Instead, he offered theoretical arguments and very limited empirical evidence from a few regulatory domains (Posner 1974). He also acknowledged elsewhere in his article the "defensive power of various other industries" that could complicate any business's efforts to capture a regulatory agency (p. 8). He specifically stated that his theory "does not mean that every large industry can get what it wants or all that it wants" (p. 11). For these reasons, it should be clear that Stigler did not believe all regulation is acquired by industry. That said, he did seem to think that a lot of regulation came into existence solely to serve industry's interests. As a result, reality seemed to hit Stigler's "Theory" hard within just a few years of its publication. The sweeping deregulation of airlines, telecommunications,

trucking, and natural gas that occurred in the United States in the late 1970s and early 1980s proved hard to square with Stigler's emphasis on regulation as a barrier to entry (Levine 1981; Quirk 1981; Derthick and Quirk 1985), although subsequent extensions of his analysis have been directed toward understanding deregulation (Peltzman 1989). Also complicating Stigler's account were the consumer, civil rights, and environmental movements—and the extensive new forms of regulation that accompanied them but which were opposed by industry. Of course, this is not to say that these new regulatory arenas cannot be helpfully analyzed by referencing political economy theory (e.g. Keohane et al. 1998), only that Stigler himself did not envision these possibilities any more than he considered the prospect of widespread deregulation.

Second, Stigler made little effort to distinguish between legislators and bureaucrats in his work. Legislators and the bureaucrats in regulatory agencies face different institutional environments with different political incentives—with bureaucrats being affected by what legislators do. Without any deep consideration of differing political institutions, Stigler's analysis could not fully explain how business-friendly regulatory regimes emerge (or do not emerge) from the political system (Shepsle 1982; Weingast and Moran 1983). This critique does not invalidate Stigler's political economy approach; it only qualifies the generalizability of some of Stigler's claims. As already noted, many scholars since Stigler have given specific attention to both the differences between legislators and bureaucrats as well as their interactions (Weingast and Moran 1983; Laffont and Tirole 1991). One of the most productive streams of political economy research on regulation in recent decades has centered on the study of regulatory institutions and their design (Carrigan and Coglianese 2011).

Third, Stigler's empirical evidence was, as even he acknowledged, very limited. He analyzed only two types of regulation—trucking and licensing—and found more than mixed support in only one of these. More importantly, and something Stigler did not acknowledge, his evidence could not rule out plausible explanations consistent with a public interest theory of regulation (Carpenter 2013). Although a positive correlation between truck weight limits and the length of railroad freight hauls might be some indication of regulatory capture by railroads, it may also be the case that states with higher weight limits simply have lower population densities. If so, longer railroad freight hauls would be needed to bring goods to a more dispersed population. Having fewer people would also decrease the risks of allowing heavy trucks on the roads, suggesting a reasonable competing explanation for the higher weight limits (Carpenter 2013). Similarly, if occupational licensing counteracts information asymmetries to improve the quality of services offered, it would be consistent with the public interest if these requirements arose where they would have the greatest benefits. In addition to urban areas, the benefits would be greatest where both the demand for and supply of the service were greatest. Rather than demonstrating capture, an alternative explanation for Stigler's negative correlations between licensing dates and both urbanization and occupational size was that regulations simply arose where they were most needed. In a similar vein, Stigler also gave short shrift to the possibility that regulation could at times simultaneously support industry interests and advance the public interest, at

least relative to a status quo of no regulation. He assumed, rather than established, that private interests and the public interest were in conflict.

Finally, although one of the virtues of a political economy approach like Stigler's is its relative simplicity, some scholars may deem this attribute a possible vice. A political economy model treats regulatory officials as subject to only a narrow range of self-interested motivations, an assumption that certainly makes generating predictions more tractable but could be said to undermine verisimilitude, if nothing else. It is sometimes suggested that government officials are motivated by more than their private gain (DiIulio 1994; Golden 2000); they may be called to public service by an underlying belief in the mission of an agency or a desire to pursue policies for the greater good (Kelman 1987; Wilson 1989). Public-interested regulators might even display outward behavior that sometimes looks like capture (Carpenter 2004; Coglianesi et al. 2004). For example, if an agency observes through repeated interactions with certain firms that these businesses faithfully adhere to existing rules, it might sensibly choose to hold those firms to lower levels of regulatory scrutiny relative to newcomers to the industry. Focusing more attention on those with which they have less experience could be a sensible way for public-interested regulators to deploy scarce resources, but an unsophisticated political economic analysis might well associate such behavior with industry influence (Carpenter and Moss 2013). Thus, as with all theories, the circumstances to which Stigler's insights apply ultimately depend on where his assumptions fit, an obvious qualification never reflected in his strongly worded assertions.

This last critique returns to Stigler's main point, which was the rejection of public interest explanations for public policy outcomes. Suggesting that regulators may have hard-to-see public interest motivations indicates that an open debate still lingers, decades after Stigler's "Theory," about the very question he addressed so forcefully. Efforts to resist Stigler's emphasis on regulators' self-interest could perhaps be viewed as challenging the core of his political economy approach. Or it could also be that such resistance simply misses Stigler's main point. As we have noted, a full reading of "The Theory" shows he did not think that industry would always use regulation to get its way, notwithstanding some of his more ambitious theoretical claims. Nor did Stigler even reject the possibility that regulators might face mixed motivations. Rather, he sought to explain the general tendency of regulation to serve industry interests, and he did so by reference to the general tendencies created by incentives embedded within a democratic political system. He sought, in short, to explain what happens "as a rule."

ENDURING VALUE

Stigler's "Theory" article brought to the foreground what remains one of the most vital questions about regulatory institutions: how can they be made to work better to advance public welfare? This question is fundamental for both scholars and policy-makers. Indeed, following nearly every major economic, environmental, and public

health disaster, elected officials put the spotlight on the design of regulatory institutions, often reorganizing existing regulatory agencies or creating new ones in an ostensible effort to prevent bad outcomes from happening in the future (Coglianese 2002; Carrigan and Coglianese 2012). Yet if efforts to fix regulatory institutions are ever to succeed, these efforts must be grounded on solid empirical research about regulators' behavior. As Stigler wrote at the end of "The Theory," "until the basic logic of political life is developed, reformers will be ill-equipped to use the state for their reforms, and victims of the pervasive use of the state's support of special groups will be helpless to protect themselves" (p. 18). Knowledge may not always equate with power in the political process, but it is a necessary condition for effective institutional reform.

Although the empirical evidence presented in Stigler's "Theory" was quite limited—especially when judged by contemporary standards—his effort to test his ideas with statistical analysis offered a template that others have followed in the decades since. His article provided a model in other respects as well, providing at least three additional lessons for the study of regulation and regulatory institutions.

First, researchers (if not also policy-makers) need to remain cognizant of the difference between the empirical and the normative. This is not to say that the two can, or should, ever be neatly compartmentalized. Normative concerns can appropriately motivate the framing of much empirical inquiry, and, as critics of political economy have suggested, normative ideas might well have some influence, as an empirical matter, over public policy behavior (Reich 1990). Still, normative claims cannot substitute for empirical ones, and empirical claims need to be tested, not assumed. One simply cannot expect that policy outcomes will always, or ever, accord with the normative precepts of standard welfare economics. If this seems platitudinous, that is in no small part because of Stigler's "Theory."

Second, although Stigler's choice of language in "The Theory" may at times have sounded absolutist, his empirical analysis actually revealed an appropriately subtle posture toward regulatory capture. Unfortunately, words like "capture"—which Stigler did not use—or "subservient"—which he did—conjure up binary arrangements: a regulator is either subservient or not. But reality is messier. Not only do a variety of non-industry interests get involved in regulatory policy-making, but different industry interests can compete with each other. Stigler recognized as much. After all, one of his empirical tests involved an explicit tension between industries, namely, railroads versus trucking firms. Moreover, another messy aspect of reality is that industry influence is not absolute; no agency is in this sense fully "captured." Influence is instead a matter of degree, and the researcher's challenge is to identify and explain the degree of industry influence. Despite some of his bold claims—perhaps born from the ambitions of his theoretical analysis—Stigler approached his research challenge in precisely this way. His entire empirical analysis treated influence as a matter of degree. After all, he analyzed regulation in continuous terms, using variables like truck length and weight restrictions rather than looking for complete bans on truck transportation. Like any good social scientist, he looked for statistically significant correlations, not perfect

ones. Future research on regulatory capture should continue to adhere to a nuanced conception of capture.

Finally, although he was not explicit about it, Stigler's choice of empirical tests recognized the importance of differentiating between various facets of regulation. Regulation is itself not a monolithic phenomenon, but refers to a complex set of behaviors: policy-making, institution-building, enforcement, and more. Regulatory standards can be defined or structured in different ways. Whatever explains when these standards are established may not explain what they require. Whatever explains the making of these different regulatory standards may not explain how they are enforced. At some regulatory agencies, standard-setting could prove more susceptible to industry influence than enforcement (or vice versa). Stigler tacitly acknowledged the existence of different facets of regulation. Consider his varied empirical tests. One of his analyses centered on the stringency of trucking weight limits, while the other focused on the timing of occupational licensing rules (regardless of how stringent the licensing standards might have been). Giving explicit attention to different facets and types of regulation remains a valuable strategy. Political economy models have illuminated policy-makers' choices about so-called command-and-control regulation and market-based instruments (Keohane et al. 1998). Other forms and facets of regulation—e.g. management-based regulation, information disclosure, voluntary approaches, or cooperative enforcement—could be fruitfully studied through a political economy lens (Carrigan and Coglianesse 2011). New questions can be imagined too, such as whether some alternative forms of regulation are more (or less) resistant to industry influence or whether some are harder (or easier) to use as a barrier to entry.

CONCLUSION

More than 40 years have passed since Stigler published “The Theory of Economic Regulation.” Much has changed during this time, but we still know too little about how to design regulatory institutions to resist capture and enhance the broader social welfare. Yet thanks to Stigler, as well as to the broader literature on the political economy of regulation he inspired, we know much more than we did four decades ago. Back in the 1960s and early 1970s, regulation was viewed by economists primarily as a mechanism deployed to solve market failures—not as a weapon to be exploited by firms seeking to create barriers to efficient competition. Stigler's analysis was game-changing, rousing economists and regulatory scholars to the possibility that regulation could play exactly the opposite role from that intended. This caution, as well as Stigler's overall focus on the role of private interests, remains no less germane to today's highly changed regulatory landscape. Furthermore, his example of theoretical development and empirical verification serves as a model of the kind of systematic thinking about regulation that even now needs greater supply. In the face of serious social and economic problems,

some of which arise despite regulation and some perhaps because of it, we must do more, Stigler admonished, than simply "preach to the commissioners or to the people who appoint the commissioners" (p. 17). We need clear ideas tested by careful empirical analysis. Looking back at Stigler's classic article, today's reader should see "The Theory of Economic Regulation," its limitations and flaws notwithstanding, as an exemplar of the type of research needed to equip decision-makers and reformers to make better regulation and regulatory institutions.

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CHAPTER 21

MICHAEL D. COHEN,
JAMES G. MARCH, AND
JOHAN P. OLSEN,
“A GARBAGE CAN MODEL OF
ORGANIZATIONAL CHOICE”

WERNER JANN

*The main task of scholars is to help good ideas forged by their predecessors
find a new life in the imaginations of their successors.*

(Cohen et al. 2012)

INVENTING THE GARBAGE CAN: THE
ACCIDENTAL ORIGINS

“CONSIDER organized anarchies”—any serious student of organization theory, policy-making or administration will recognize this opening, and if there were a list of “famous first words,” this would probably be among the top ten. The original article (Cohen et al. 1972) appeared more than 40 years ago, and according to the Web of Science its citation frequency has been on a steady rise ever since. Obviously it is, in the words of the original authors, “a solution that still attracts problems” (Cohen et al. 2012).

Like many of the classics in this volume, the article and its successive impact does not fit into the neat disciplinary boundaries which still dominate most of our universities, teaching, and journals. It was published in *Administrative Science Quarterly*, but according to the Web of Knowledge it has been quoted and used in the fields of Management and Business, in Public Administration, Political Science, Education

and Sociology—and these are only the top six of all in all more than 100 science and research categories identified. It has been applied in Economics, Law, Computer Science, Psychology, Accounting, and Geography, and of course in all kinds of policy fields, from health, urban planning, and environmental studies, to engineering, forestry, social work, and the internet. According to JSTOR the article is still amongst the three most quoted and accessed from ASQ.

The garbage can model (GCM) does not only defy ordinary disciplinary borders, it is also the result of an early and unconventional interdisciplinary undertaking, or, in other words, of the chance encounter of participants from different backgrounds looking for problems to try out new ideas about organizational theory and decision-making. The three authors met in the late 1960s at the newly founded School of Social Sciences of the University of California, Irvine. Cohen and Olsen were doctoral students, doing research on institutions of higher education, Cohen together with March on the choice of American college presidents, Olsen, visiting from Norway, on the choice of a dean at a Norwegian university. All were interested in the development of social institutions and theoretical ideas about them, and in combining different approaches, from sociology, organization theory, political science, formal modeling, and simulation. When March moved to Stanford and the different case studies and ideas were ripening, a choice about what do with them had to be made. The group which also included other doctoral students from Scandinavia and the US, met for several weeks in Denmark and Norway in 1970, and the result of all this was not only the Garbage Can article, but also three well-known books, *Leadership and Ambiguity: The American College President* (Cohen and March 1974) and *Ambiguity and Choice in Organizations* (March and Olsen 1976), mainly about organizations of higher education, and *Ambiguity and Command*, about decision-making in the military (March and Weissinger-Baylon 1986), not to forget the two dissertations of Olsen (1971) and Cohen (1972), and finally, last but not least, a lifelong close personal and professional cooperation between March and Olsen, which eventually produced some more social science classics of the last century (March and Olsen 1984, 1989).

OPENING THE GARBAGE CAN: THE CORE INGREDIENTS

The basic ideas of the original article are at the same time simple and, at least for most experienced students and practitioners of organizational decision-making, immediately recognizable and plausible. But they are also contrary to dominant and well-established theories of problem-solving and rational choice. From many years of teaching my experience is that especially veteran practitioners instantly grasp the concept, its explanation, and justification, and they like it, while academic students have problems in understanding what is described, and quite often loathe the concept,

because it goes against their deeply held normative ideas about how decisions should be made, and also how orderly social science should argue.

The basic observation and the main conceptual argument is that decision-making in organizations quite often can be characterized by three main properties (Cohen et al. 1972), by

1. problematic preferences, i.e. goals are either vague, inconsistent, contested, or unstable;
2. unclear technologies, i.e. the connection between means and ends is not well understood; and
3. fluid participation, i.e. the attention and involvement of decision makers is unstable or uncertain.

Organizations in which these properties can be observed are called “organized anarchies,” they discover their preferences through action and interaction more than they act on the basis of clear goals, and these organizations can therefore, “for some purposes,” be described as “collections of choices looking for problems, issues and feelings looking for decision situations in which they might be aired, solutions looking for issues to which they might be an answer, and decision makers looking for work” (p. 1).

The main point is that the classical view of decision-making, where “choice opportunities lead first to the generation of decision alternatives, then to an examination of their consequences, then to an evaluation of those consequences in terms of objectives, and finally to a decision” (p. 2) is quite often a poor description of what actually happens. Instead, in the garbage can model a decision is an outcome of several “relatively independent streams” and their interrelations:

1. problems–concerns inside and outside the organizations which require attention;
2. solutions–answers actively looking for problems to which they may be applied;
3. participants–actors which want to participate in choices and decisions; and
4. choice opportunities–occasions when an organization is expected to produce behavior that can be called a decision, some of these arise regularly (budgets have to be passed, contracts must be signed, etc.), while others are more unpredictable (crises within or outside the organization).

The garbage cans of the model are thus the choice opportunities into which various kinds of problems and solutions are dumped by various participants. The mix of garbage in a single can, i.e. a specific choice opportunity, depends on the mix of cans or opportunities available, on the labels attached to the alternative cans or opportunities, on what garbage, i.e. solutions and problems, are currently being produced, and on the speed with which these are collected and removed from the scene.

These assumptions again lead to the conclusion that decisions, at least in these kind of organizations and circumstances, to a large extent are much more the result

of temporal linkages, the arrival and departure time of independent and exogenous streams of problems, solutions, decision-makers, and choice opportunities, than the consequences of careful analysis and deliberate choices.

In the original article these basic ideas, the "verbal model," are then translated into a computer simulation model of a garbage can decision process, the "formal model," which is written in Fortran, at that time the most common computer language. In this model some new assumptions, e.g. about decision styles (by resolution, oversight, or flight) and properties of organizations (energy load, energy distribution, decision structure, and problem access structure) are added, and the formal model is thus at the same time more complicated and more simple. It is used on decision-making in US universities, where some additional parameters are introduced (organizational slack via large, small, rich, and poor universities), and even some predictions about future developments are made ("among large rich schools decision by resolution triples," etc.), but it is fair to say that it was not the computerized model and its predictions but the verbal formulation of its findings that caught the attention and the inspiration of most researchers.

Before addressing the question how these ideas have inspired—or provoked—different scholars from different fields in the last 40 years, it is useful to sketch what was really new in the GCM and the original article. Where did it differ from previous models and observations, why did it attract so much attention and why—and this may be a slightly different question—did the model become such a well-known concept?

Most of the basic observations of the GCM and their conceptual treatment were not really new at all. Bounded rationality, i.e. imperfect understanding of events and their causes, had been introduced by Simon and was gradually recognized in decision-making theory, as was the general idea of a behavioral theory of decision-making and organizations in general. So in many ways the article was an extension and broadening of ideas formulated by Cyert, March, and Simon at the Carnegie Institute of Technology in the 1950s, the so-called Carnegie school (Cyert and March 1963). Also the observation of conflicting goals and preferences within organizations was not revolutionary, this and the idea of bargaining and partisan mutual adjustment had been the main point of Lindblom's theory of muddling through (Lindblom 1959). Also the combination of unclear goals and means, leading to different forms of decision-making, had been suggested before (Thompson and Tuden 1959).

What was new was the extension and sharpening of these observations and concepts. New were the observations of fluid participation, the multiple and unpredictable decision points, and the integration of these observations into a coherent model. Not only preferences and technologies were uncertain, but also participants and decision opportunities. And new was also the specific use of the terms "ambiguity" and "choice," which were even more highlighted in the title and the contributions of the subsequent book, summarizing the findings of the research group (March and Olsen 1976).

Ambiguity is usually understood as the ability to express more than one interpretation of a given event or fact. Ambiguous circumstances are therefore situations in

which goals, technologies, and participants are not only unclear, unknown, or vague, but where specific and distinct but contradictory interpretations are possible and probably present. Choice on the other hand is different from decision-making, it is not an event, but a stream with many tributaries and inputs, and it is highly contextual. New was certainly also the extension to a temporal understanding of choices, in contrast to the usual intentional or consequential one. Organizations might not necessarily be, as we are used to and were told to believe, primarily tools for substantive, rational problem-solving, but there are quite plausible alternatives to means-end rational interpretations of organizational behavior.

Finally, the provocative wording of the model may have helped. Ambiguity and choice are fine, but garbage cans and organized anarchies are better. The original article fulfilled at least two of the fundamentals of classical rhetoric which have been associated with scientific success (McCloskey 1985; Hood 1999): metaphor and irony. In this it aligns itself with some of the classics in this volume and in organizational theory (muddling through, bowling alone, street-level bureaucrats, organized hypocrisy). As the authors suggest themselves, the unusual label has performed a useful role: "It seems to help each new reader who comes to the ideas to take them in the playfully serious spirit we hope they deserve" (Cohen et al. 2012: 22). But most of all the article became legendary, because it made sense of observations, which many practitioners and informed students of organizational decision-making had made before, but which did not fit into the prevailing concepts and explanations of traditional decision-making theory. With the advent of garbage cans in organization theory it was possible to talk about experiences, observations, and frustrations which were quite common, but until now were only seen as pathologies, and thus very often were not really talked about at all. GCM offered a theoretical interpretation, namely unforeseen and unpredictable decision outcomes as results of quite common and systematic characteristics of organizations.

RECYCLING THE GARBAGE CAN: THE VARIOUS USES

There can be no doubt that "ideas from the original article have flowed in many different directions" (Cohen et al. 2007: 536). Like many other important and well-known works that have had a major impact over a long time, the influence sometimes is often "more noteworthy in terms of breadth rather than depth" (Argote and Greve 2007). I will try to track the main impact by looking briefly at the more conventional fields of organization theory and the original formal model before concentrating on the more specific areas of policy-making, administrative reform, and finally the broad area of institutional theory.

Organization Theory: Making Sense of Everyday Life

The most obvious and imminent impact of the garbage can model was in organization theory. As Charles Perrow put it in one of the earliest reviews of *Ambiguity and Choice*, "one does not have to be a close observer of organizations to find daily events that would have to be labeled as pathological from the point of view of the theories of organizations which we espouse and laboriously test" (Perrow 1977: 295). He admits that "none of us should be surprised" by the fascinating case studies presented in the book, "but the point is that our theories should be surprised, and our favored research techniques do not accommodate it!" (p. 297). Before the advent of garbage can, power theories, or conspiracy theories, or leadership theories, or pure irrational behavior were invented in order to explain what ordinary organizational theories could not explain, but now there was a genuine organizational theory to make sense of everyday observations.

The central assumption of "bounded rationality," the obvious constraints on optimizing or predicting solutions to problems, were extended to the other decision elements, i.e. problems, participation, and choice opportunities were assumed to be severely constrained. Not only the classical demand "find the best solution" is thus problematic, but so are "solve all problems," "let everybody participate," and "use every opportunity to make a decision" (Heimer and Stinchcombe 1999).

The well-known theories of bounded rationality and conflicting goals and preferences in organizations were taken a step further by suggesting a coherent understanding of organizational processes that is not founded on the assumption of forward-looking consequentialism (Cohen et al. 2007: 535). At the same time concepts like "myths" and "loose coupling" were introduced and illustrated. Even more provocatively it was suggested that beliefs and preferences are more results than explanation of behavior. Finally, the symbolic role of choice situations was highlighted, i.e. the primary purpose of a certain decision process was no longer necessarily to produce a specific outcome, but rather, through the airing or "exercising" of problems, participants, and solutions, to maintain, legitimize, or change the organization as a social unit.

All these elements of a more radical "behavioral" decision theory were taken up by some of the most influential scholars of organization theory (Weick 1974; DiMaggio and Powell 1983; Brunsson 1982) and were further elaborated and explored, most eloquently and comprehensively in the writings of James March:

Organizations have many features that move them toward coordinated action, particularly hierarchical control structures and standard operating procedures. Nevertheless they do not reliably display consistent decision coherence. Rather than have decision processes that proceed from consistent intentions, identities, and expectation to coordinated decisions and actions, organizations exhibit numerous symptoms of incoherence. Decisions seem unconnected to actions, yesterday's actions unconnected to today's actions, justifications unconnected to decisions. Beliefs are often unconnected to choices, solutions unconnected to problems, and processes unconnected to outcomes. Organizations frequently

have ambiguous preferences and identities, ambiguous experiences and history, ambiguous technologies, and fluid participation. They are loosely coupled. (March 1994: 192–3).

The Formal Model: Few Followers and Fierce Critics

Even the fiercest critics of the GCM acknowledge that the computer simulation which is part of the original article is one of the most famous simulations in all of the social sciences (Bendor et al. 2001: 169). But it has only attracted very few followers and adherents, and for that sake critics. The reasons why there are so few expansions and developments of the formal model are unclear, but it is obvious that “mainstream organization theorists have overlooked technical challenges and new simulations, attending instead to Cohen, March, and Olsen’s own verbal account of their simulations” (Bendor et al. 2001: 183).

Still, nearly 30 years after its publication the GCM received the unusual honor that the *APSR* published a lengthy article devoted exclusively to a thorough and devastating critique of the original publication, together with a rejoinder from Olsen (Bendor et al. 2001; Olsen 2001). The authors replicated the original simulation, and their central assertion is that the verbal and the formal model are incompatible, the computer model is supposed to represent the disorderly world of garbage can decision processes, but even in the prototypical case it generates an incredible degree of order. Furthermore, the formulations of the verbal model are overly complex, the arguments unclear, it is a conceptual morass and impossible to test. Also the application to universities is faulty, the assumptions and implications are largely unwarranted and questionable, are unrelated to the theory, and offered with scant justification, and so on. All in all, in the view of these critics, the GCM is not an extension, but an alternative to bounded rationality, though an unattractive and confusing one. In order to rescue and revitalize it, standard assumptions of organizational theory, like at least intentional rational actors and the consequences of structural characteristics and choice, should be included.

In his rejoinder Olsen stressed that the original formal model is “one illustrative set of simulations.” The intention was never to establish one decisive theory of organizational choice, but the claim was more modest, to present “a” not “the” model. So from the beginning it should have been obvious that there are other models and explanations which can be specified at several levels of precision. There exist a number of garbage can models, and these modify most of the key assumptions of the “pure model.” For example, the possibilities for intelligent actions and management in garbage can situations have been explored (Cohen and March 1974: 205–15), and indeed also the relevance of structural features of organizations in garbage can situations have been investigated and modeled.

This has been done in one of the most well-known further simulations of the GCM by John Padgett (1980). He starts from the obvious observation that garbage can processes

seem curiously divorced from the familiar structural phenomena of concern to classical organization theorists. He then proceeds to modify the model to embed garbage can flows explicitly within the classical bureaucratic constraints of hierarchical differentiation, standard operating procedures, and centralized control. In the end the elaborate simulation comes up with some remarkable results, most prominently that the heads of organizations maximize control over organizational decision outcomes the most by making no substantive decisions whatsoever: "The thrust of the managerial recommendations to be deduced from the model emphasize unobtrusive structural design, rather than active tactical maneuvering" (p. 584). These are remarkable findings and prescriptions which seem to fit very well with more idiosyncratic observations of successful heads of governments and other leaders, at least much more than standard managerial literature.

Also the formal model has over the years thus seen some interesting enlargements and modifications, but obviously much less than the verbal model. The original formal model was, seen with hindsight, not very sophisticated, and no doubt it could be improved. That this has not happened cannot be blamed on the original authors who went to great lengths to document their simulation, but more on the apparent limitations of formal modeling in decision theory.

Policy-Making: Windows of Opportunities, Networks, and Governance

Even though the garbage can model was very early on recognized in political science (the second citation ever appeared in the *APSR*, Mohr 1973; the first, by the way, was in *The Lancet*, in a piece about the NHS, still today a good example for garbage can decision-making), it took some time until it became part of mainstream policy analysis and public administration. This is quite surprising, since it aims to explain organizational outcomes or at least outputs. In one of the first reviews of *Ambiguity and Choice* Mohr observed:

many of the examples in the book are not truly instances of organizational behavior, but rather of the common sort of multi-organizational behavior that goes into the making of public policy. Remarkably similar examples can be found over and over again in the case-study literature on public administration and policy. It is small wonder that public policy making tends to be full of garbage cans. (Mohr 1978: 1035)

After a while, garbage can became a well-known metaphor, but mainly as a descriptive term. To many "garbage can processes" became just a catch-all expression for some kind of disorder, which was hard to describe and even harder to explain. The metaphor of the "organized anarchies" may have contributed to this misconception, because it suggested not ordinary, but highly decoupled and unorthodox organizations and decision processes.

The slow impact on policy studies and political science in general is surprising, since Johan Olsen, one of the co-authors, was a political scientist by training, and very early on sketched the relevance of GCM for our understanding of political and administrative decision-making. In an article about “Public Policy-Making and Theories of Organizational Choice” he argued already in 1972 against the prevailing assumption of policy-making “in terms of decision-makers’ choosing on behalf of themselves, organizations, nations, or mankind in general” and asserted that the “polity may, under certain conditions, operate as a ceremonial apparatus providing rationalizations of the events taking place” (Olsen 1972: 45, 46).

He already pointed out that, even though the GCM was developed mainly through empirical studies of organizational choice, the major contribution of garbage-can ideas may be related to our understanding of political choices at the macro level. He stressed that policy-making should be seen as a product of processes having dynamics of their own which by their interaction generate outcomes which are not intended by anyone and that “decision” in this model is mostly a post factum construct produced by participants because of their need to find consistent patterns in what they are doing and observing. Finally, he pointed out that accepting a garbage-can view of public policy-making means focusing attention on the ways the “meaning” of a choice changes over time, an observation which in recent years has become known as the “framing” of policy issues.

Still, probably because these ideas did not appear in a mainstream Anglo-Saxon journal, it took more than ten years before they gradually entered conventional policy studies. The ultimate breakthrough was John Kingdon’s study about *Agendas, Alternatives and Public Policies* (Kingdon 1984), which uses the GCM in an original and highly instructive way to understand how new policies get on the political agenda, and how more than gradual political change is possible at all. Kingdon distinguishes, in the tradition of GCM, the independent streams of problems, policies, and politics, and introduces the new and strong metaphors of “political entrepreneurs” and “windows of opportunity.” Since this seminal work merits its own chapter in this volume, its major impact need not be discussed here.

Mostly through Kingdon’s work, garbage can finally found its way into public policy textbooks. And the more policy studies became disillusioned with traditional models of the policy process which were unable to explain policy choices (Jann and Wegrich 2007), and at the same time became even more interested in the possibilities and prerequisites of policy change and policy learning, the more GCM became integrated into new theoretical concepts, like the new concern with ideas, policy learning, and transfer (Sabatier 1988; Hall 1993; Dolowitz and Marsh 1996) or network analysis (Klijn 1996). But it is quite surprising that the ubiquitous governance discourse of recent decades, stressing amongst others the declining importance of top-down hierarchical steering, the co-production of public goods, network management, decentralized or even stateless problem-solving, and so on, has not really been confronted with the basic assumptions and findings of the GCM. Obviously there are still important developments where the concept could be usefully employed.

Public Administration: Symbols and Myths in Administrative Reform

Also in the narrower field of public administration as a discipline, or at least as an academic field, the GCM needed some time before it entered mainstream teaching and research, and in many areas of New Public Management and public sector reform discourses it seems it never arrived. Already in 1976 Olsen had described "Reorganization as a Garbage Can" (Olsen 1976), using a major reorganization in a Norwegian university as example, and some years later March and Olsen used GCM to analyze 12 major reorganization efforts by President and Congress in the United States in the twentieth century (March and Olsen 1983).

Their main conclusion was that short-run outcomes had been meager, to say the least, and that these high-level reforms accounted only for an insignificant share of the total administrative changes that occurred, were seldom followed by any systematic efforts to assess their effects, and were "a source of frustration and an object of ridicule ... and yet are persistently resurrected by the political system" (p. 282). Observations and experiences which again any veteran in any administrative system will immediately recognize.

They observed that reorganization efforts have difficulties in sustaining the attention of important political actors, and as a result, "reorganization efforts often operate in an attention vacuum with respect to those political figures who are likely to be most supportive, and improbable promises of economies are made in an effort to secure attention." In the end "reorganizations tend to become collections of solutions looking for problems, ideologies looking for soapboxes, pet projects looking for supporters, and people looking for jobs, reputations, or entertainment. ... administrative reform becomes associated with issues, symbols, and projects that sometimes seem remote from the initial impetus behind the effort" (p. 286).

In any reorganization effort "change comes to mean many different things to different participants," but not every effort is bound to fail. There are "short-run failures and long-run successes," since "persistent repetition of similar ideas and similar arguments over a relatively long period of time appear to make some difference." This is because reorganizations and reforms have to be viewed as "a form of civic education," they cannot be only understood in instrumental terms, but are expressions of social values. Reorganizations can thus become alternatives to action or even a tactic to creating an illusion of progress where none exists. All in all reorganizations are "a domain of rhetoric, trading, problematic attention, and symbolic action" (p. 291). They are characterized by problematic attention, incremental adaptation to changing problems, and have to be understood as contests about legitimate values and institutions.

These ideas were further elaborated together with Nils Brunsson (Brunsson and Olsen 1993), and if they had been taken into consideration by administrative reformers propagating New Public Management reforms all over Europe and the world, probably some disappointments and surprises could have been avoided. At

least in Europe public administration as an academic endeavor is very much associated with administrative reforms, it is essentially a “reform science.” But even in this core of administrative science one can observe the widespread mutual disregard of mainstream public management prescriptions and organizational theory. Only in Norway did garbage can ideas inform discussions of public sector reform early on (Christensen and Læg Reid 1998), and only after they had been integrated into the more comprehensive new institutionalism did they gradually become common knowledge in administrative sciences.

Institutional Theory: Organizational Factors in Political Life

In its original formulation the pure garbage can model is basically institution free, i.e. structure is treated as exogenous (Olsen 2001, 1993). But from the beginning it was obvious that the way organizations are structured influences the emergence of decision opportunities and garbage cans, i.e. how, when, and which solution, actors, and problems have the opportunity to meet. Starting from this observation GCM finally became also one of the important inspirations to what is known as “the new institutionalism” in political science, and which investigates the origins, dynamics, and possible impacts of the structures that are treated as givens in the GCM.

This direction of theorizing was strictly influenced by the, again, accidental event that in Norway, under the persuasive influence of Olsen, organization theory was developed in departments of political science rather than in business schools. Starting from the assumption of bounded rationality, political models describing and explaining organizations as conflict systems, coalitions, or negotiated orders were put up against the traditional view that organizations are rational structures established to maximize agreed upon purposes.

The unique combination of organizational theory and empirical political science, using organization and decision theory on democracy, bureaucracy, pluralism, and corporatism, was effectively enhanced by a major empirical study of the Norwegian political system, commissioned by the Norwegian parliament and known as the first “power study” (*Magtutredningen*), of which Olsen became one of the two leaders. The study was supposed to investigate problems and appearances of political inequality in the Norwegian welfare state, and from the beginning Olsen argued that political inequality is shaped by and has to be explained through the organizational factors of political life. Furthermore, he argued that a political-administrative system can be seen as the attempt to reduce contextual dependency and to avoid accidental, surprising, and unwanted couplings of solutions, actors, opportunities, and problems. The more ambiguous preferences, goals, and means-ends assumptions are, he argued, the more important it is to study choice opportunities, interpretation, and learning from a perspective of influence, democracy, and organization.

Thus not only traditional hierarchically organized political-administrative systems, but also segmented systems, as observed in the modern Norwegian corporatist state, have to be understood as alternatives of organized anarchies, and there are probably other alternatives. Olsen directed a number of empirical studies, focused on how and to what degree organizational behavior and political outcomes are shaped by political institutions, and he argued, organization theory should look for "a middle way between the formal-legal tradition of political science and an environmental-deterministic view" (in Olsen 1983 a number of these empirical studies are summarized, but unfortunately the main results of the power study have only appeared in Norwegian; Olsen 1978).

The overall argument for this kind of theorizing and empirical exploration was sketched in March and Olsen 1984, which became a perhaps even more famous political science classic than the GCM (the article is still one of the 10 most quoted in the whole history of the *APSR*). Here, and in the further elaboration of this approach (March and Olsen 1989, 2006) the authors took up, systematized, and sharpened some of the observations of the original GCM. They stressed that actors cannot attend to all issues at the same time, that political institutions create choice situations, that they have a highly symbolic importance, create legitimacy and myths about how and why choices are made, and that beliefs and preferences of actors are shaped by roles, duties, and obligations—they are not exogenous to decision-making processes.

Furthermore they argued that political and administrative systems cope with ambiguity, diversity, and inconsistencies in a variety of ways, amongst others by specialization, separation, autonomy, sequential attention, local rationality, and conflict avoidance (March and Olsen 2006: 15). Institutions are thus important to create order and predictability, they fashion, enable, and constrain political actors as they act within a logic of appropriate action, and legitimacy therefore depends not only on showing that actions accomplish appropriate objectives, but also, and perhaps even more so, that actors behave in accordance with legitimate procedures.

The "new institutionalism" in turn inspired some "institutionalist" reinterpretation of the original model, using the assumption that institutions distribute decision power by rules and routines and coin actor identities and their interpretations and situations. Just like Padgett in his adoption of the formal model, Sager and Rielle demonstrate that the organized anarchy paradigm can be usefully applied to "fairly traditional and bureaucratic structures." By doing a comparative case study of the adoption of new alcohol policy programs in Swiss cantons they show that most of the assumptions of the original model hold (especially the contention of independent streams) and that a more institutionalist form of the garbage can model is thus in order (Sager and Rielle 2013: 18). In the same vein Heimer and Stinchcombe argued that the organization of attention, negotiation, and closure (i.e. institutional processes) supplies the history, legitimacy, and network affiliations for different items in decision streams. Elements in streams have histories and varying amounts of legitimacy and charisma, some items are more likely to get put into and pulled out of the garbage can than others, thus order in garbage can processes comes largely from the identities and institutional histories of

the items in decision streams. Garbage comes from somewhere and belongs to someone (Heimer and Stinchcombe 1999: 42).

SUMMARY: DEVELOPING GARBAGE CANS

“Garbage Can” is one of the most well-known metaphors of policy analysis and administration. There can be no doubt that the garbage can model of organizational choice has been an extremely successful inspiration for all kinds and fields of the social sciences since its appearance more than 40 years ago. Garbage cans are found and used in all kinds of disciplines and studies, from management and political science to economics, law, and accounting, and in all kinds of societal and policy sectors. At the same time, as with many other successful concepts, the impact is in many ways broader than deep.

But this is very much in line with the original intentions of the authors who have always insisted that they suggested “a” model of organizational choice, not “the” model, since “the complexity of decision making in an organization is unlikely to be captured by a single model, any more than by reports of a single participant or historian” (Olsen 2001: 191). They have always stressed the playful character of the original paper and that “the spirit has always been to encourage colleagues to play with the basic ideas, rather than defend them endlessly” (p. 192). The amazing success of the concept stems from its plausible assumptions, because it made sense of observations which had been made many times before, but which did not fit into the prevailing explanations of organizational theory. With the advent of the GCM what until now could only be pathologies and blunders of organizational decision-making became the logical consequences of ambiguous goals, technologies, participants, and decision opportunities.

For *behavioral organizational theory* the GCM became thus an important component, encouraging more systematic and comprehensive rejections of the prevailing rational model of decision-making, and also of their more unreflected uses in policy-making and administration (Brunsson 1982). It forms an important part of the criticism and substitution of forward-looking consequentialism with other forms of explanations, like the logic of appropriateness, myths, hypocrisy, loose coupling, and so on. Compared with the success of this “verbal model,” the impact of the *formal model* has been much more limited. It has only inspired few applications and enlargements, but has triggered some of the fiercest criticism, arguing that the GCM is marked by “pervasive confusion,” is impossible to test, and has moved from “model to metaphor,” not the other way round, as should be the case in proper science (Bendor et al. 2001). But even this opinionated and somewhat dogmatic critique did not stop the ever-growing success of the concept, since it ignored the central message of the original article, that the model is an attempt to enlarge rather than replace other interpretations of organizational life.

Despite its obvious relevance for policy-making and administration, it took some years before the theoretical implications and potentials of the GCM became apparent. The breakthrough came with Kingdon, and since then the concept has been an integral part both of criticisms of established approaches (like comprehensive planning or the policy cycle) as well as the development of new causal and explanatory models (like policy transfer and learning, network theory and punctuated equilibrium). Surprisingly, the overwhelming "new public governance" discourse of recent decades mostly ignored the implications of garbage can decision-making for the more decentralized, less hierarchical, more horizontal, and open decision-making structures of "modern governance."

Finally, the GCM has been an important inspiration for the *new institutionalism* in political science. Here, one of the key assumptions of the original model was reversed, i.e. instead of the "institution free" premise, where structures and rules were treated as exogenous, the overriding question now became how the organizational features of political life influence decision processes and outcomes. Also here many of the theoretical and empirical implications are still open. We know that modern democracies have limited capacities for institutional design and reform, but the important issue whether and how institutional arrangements can be influenced and shaped, and how that in turn will influence the structure and outcomes of garbage can processes, deserves our theoretical and empirical attention. Institutional policies are highly complex and uncertain, but if we expect political administrations and institutions to constrain or enable processes and outcomes, we should try to learn more about how they influence the temporal and structural coupling of solutions, actors, opportunities, and problems.

The GCM has certainly enhanced our understanding of public policy-making and administrative behavior. Both have become much more realistic and also more fun to watch. At the same time it has improved our possibilities to act purposely and to influence policy processes and outcomes. Only if we understand organizational choice and institutional arrangements can we try to use and to change them. We will always be a long way from the orderly world of clear preferences, fixed participants, and consequential actions, but we can at least no longer pretend to be the naïve and innocent victims of pathological and irrational processes. And finally, as for example the current discussions about "wicked problems" tell us, there is ample evidence of ever more, more fluid, and more ambiguous actors, technologies, and goals in modern democracies. So look out for more and more sophisticated uses of the GCM in years to come.

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CHAPTER 22

ANTHONY DOWNS, “UP AND DOWN WITH ECOLOGY: THE ‘ISSUE-ATTENTION’ CYCLE”

KUHIKA GUPTA AND HANK C. JENKINS-SMITH

SCHOLARS have studied public opinion for decades, seeking answers to questions such as: does public opinion lead or follow that of elites? How does public opinion affect electoral outcomes? Does public opinion influence the evolution of public policy? In fact, the very notion of democratic governance is incomplete without taking into account the significance of public opinion. In his seminal 1972 article titled “Up and Down with Ecology: The ‘Issue-Attention’ Cycle,” Anthony Downs focused on a similar yet more nuanced concept called “public” or “issue” *attention*. While opinion denotes the structure of beliefs, attitudes, and preferences that are brought to bear on an issue, public attention reflects the relative priority and weight given to a specific issue among the universe of issues that might be considered. Attention is reflected by the allocation of time and energy that an individual spends thinking about an issue (Newig 2004: 153). For instance, an individual might believe that global warming is anthropogenic and harmful to the environment, but not expend much time and effort actively thinking or discussing this opinion.

This distinction was made clear in Downs’s article, where he sketched a succinct and influential model of the cyclical process by which the public gains and loses interest in a particular issue over time. Concentrating on domestic policy (specifically environmental policy in the US), he argued that policy issues go through an “attention-cycle” with distinct stages in which public attention to a problem bolts from “pre-problem” latency to alarmed discovery and enthusiasm, followed by growing recognition of the costs of addressing the issue, through a gradual decline of interest to a “post-problem” stage in which other issues and problems eclipse attention. This cycle is important because it (among other things) helps to explain why some issues attract the finite and often ephemeral attention of policy-makers whereas other issues do not.

The model that Downs proposes in this article has been cited by numerous scholars associated with a variety of disciplines, ranging from international relations, American politics, and public policy to mass communication and public relations. A cursory analysis of these citations—using Google Scholar from 1973 through 2012—shows a sustained growth in reference to his article (Figure 22.1).¹ The article has primarily been cited in peer-reviewed journal articles, though a modest growth in references in books and book chapters is also evident. Over the 1973–2012 period, our analysis indicates that the article was cited 1,049 times in books and articles.² This total does not include a robust reference among “gray literature” sources—unpublished dissertations, theses, papers, and reports—that accounts for roughly 30 percent of all references over the last decade of this analysis. Clearly, then, Downs’s characterization of the issue attention cycle has received sustained (and growing) attention in the scholarly literature.

Our reading of this literature (the articles that cite Downs 1972) indicates that the most enduring contribution of this article has been to the literature on public attention and issue volatility. While, as indicated by Figure 22.1, there has been a great deal of research stimulated by this piece, this chapter aims to provide a coherent summary of the studies that directly test the propositions put forth by Downs. Specifically, we focus on clusters of studies that examine the existence of and linkages among three different attention cycles—public attention cycle, media attention cycle, and government attention cycle. With that in mind, the next section briefly summarizes the main arguments from the article, following which we highlight the various studies that have tested the issue–attention cycle in some form or another. Finally, we discuss promising avenues for future research and important theoretical and methodological questions that remain to be answered.

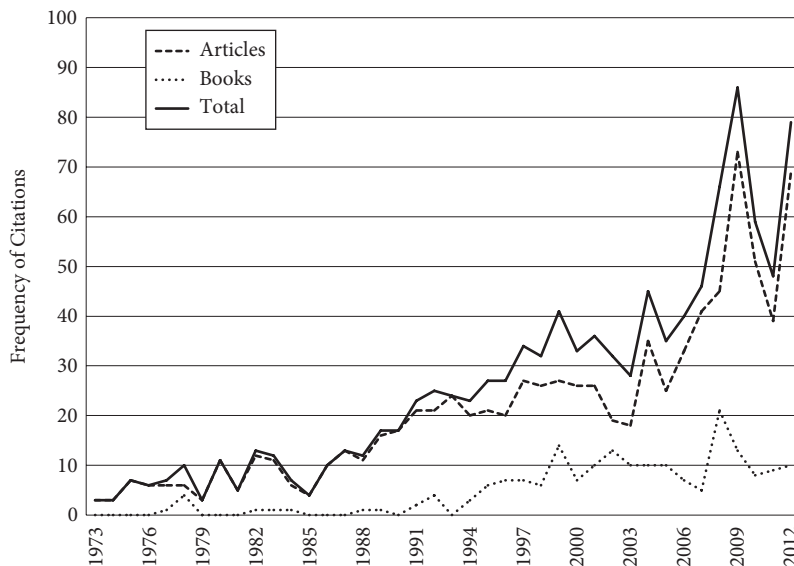


FIG. 22.1 Downs’s citations by year

THE “ISSUE-ATTENTION CYCLE”

Downs (1972) presented a “systematic” model explaining when and for how long the public pays attention to a particular societal problem. The article was published in the midst of rising concern among the American public about environmental issues (it was published soon after the celebration of the first “Earth Day”). Simply put, Downs argued that “public perception of most ‘crises’ in American domestic life does not reflect changes in real conditions as much as it reflects the operation of a systematic cycle of heightening public interest and then increasing boredom with major issues” (p. 39).

Expanding upon this statement, Downs developed an issue-attention model that rests upon three sets of propositions. His first—and most recognized—set of propositions maintain that public attention to a given issue (like ecology) cycles according to a set of five relatively predictable stages: the (1) pre-problem stage, (2) alarmed discovery and euphoric enthusiasm, (3) a period marked by a growing realization of the cost of significant progress, (4) gradual decline of public interest, and (5) the post-problem stage. In the first stage public awareness and concern about the problem is dormant. The issue exists as a societal condition, but it is not widely recognized as an urgent problem. At this stage, the issue exists in the background and, while a few individuals might be paying attention to it, it is largely absent from the public eye. In the second stage, the issue gains mass attention, usually as a result of a big event (or a series of big events). These events bring the issue to the forefront, forcing people to grapple with the problem and look for solutions. The third stage brings with it the weight of realizing how costly solving societal problems can be. This directly leads into the fourth stage of the attention cycle, where much—to the frustration of the affected groups—of the public gets bored and loses interest in the issue. Finally, the issue enters the post-problem stage, wherein it returns to a dormant state, but—because governing institutions and political interests have both been affected (or perhaps “conditioned”) by its prior passage through the cycle—can be awakened (recycled) with relative ease.

In his second set of propositions, Downs introduced three characteristics that predispose an issue towards issue-attention cycles. Those characteristics include: (1) the issue does not affect the majority of the public *as much as* it impinges upon a minority; (2) the situation, arrangement, or behaviors that result from the issue provide significant benefits to the majority or a powerful minority of the population; and (3) the issue is not intrinsically exciting enough to sustain popular interest for prolonged periods of time. In other words, Downs argued that a given issue is likely to move through the issue-attention cycle if the relative benefits of addressing the issue are low (because relatively few are impacted by the problem), the costs of addressing the problem are high (because the arrangements associated with the issue benefit a large and/or powerful portion of the population), and the issue is not dramatic or entertaining enough to capture and sustain public attention, which is a scarce yet sought after commodity.

The third and perhaps most implicit set of propositions Downs offered in this article concern the complex relationship between media attention, public attention, and governmental attention. With respect to the former, his model suggests that the media play an important role in stimulating public attention to a given problem. Some amount of media coverage (in tandem with a dramatic series of events), Downs argued, can spark the interest necessary to push an issue from the dormant stage to the alarmed discovery and euphoric enthusiasm stage. Too much coverage, on the other hand, may bore the public, causing them to lose interest in the issue and stop consuming the media's product. If and when this happens, the media realizes it and they (like the public) will shift their focus to a "new" problem. In other words, media attention is an important driver of public attention, but there is a feedback loop wherein public attention (or lack thereof) guides media attention. With respect to the latter, Downs argued that public attention orients governmental action by putting pressure on elected officials to "do something" about issues that have gone from dormant to highly salient in the minds of the American public. If and when the government attempts to do something, however, the public will realize that the problem is complicated and costly to solve. Thus, public attention drives government attention, but (again) there is a feedback loop wherein government attention may cause disillusion and ultimately a decline in public attention.

TESTING PROPOSITIONS FROM THE ISSUE-ATTENTION CYCLE

Having outlined Downs's contribution, this section discusses a select group of studies that have tested various components of his model. More specifically, we highlight studies that systematically analyze the "cyclical" nature of public attention and/or the relationship between media attention, public attention, and governmental attention. In other words, we focus on studies that have attempted (in some way or another) to test the first and third sets of propositions described above. The studies listed in the section are not meant to provide an exhaustive list; the citation analysis presented earlier makes it obvious that Downs's article has had a widespread impact over long periods of time (1,420 cites by the end of 2012, by our count) spanning a variety of fields. Rather, our goal is to provide a window into a few studies that have directly tested the propositions associated with the issue-attention model. Before we do so, however, it is important to note that Downs's characterizations of these processes were conceptual in nature. He was notably vague in his definitions of key concepts and he did not provide a methodological path for operationalizing and measuring them. Nonetheless, the seminal nature of his article has inspired a host of scholars to look for and study issue-attention cycles and provided them with an opportunity to improvise and innovate.

The first group of studies that we highlight attempt to test Downs's first set of propositions associated with issue-attention cycles: do public attention cycles actually exist? If yes, what factors account for these cyclical patterns? For example, McCombs and Zhu (1995) examined long-term trends of the American public's issue agenda, with a particular interest in issue volatility (the average duration of public attention to a given issue). In so doing, they found a great deal of volatility, which corroborates Downs's basic proposition that public attention is cyclical and short-lived. Moreover, they found that issue volatility and issue diversity (the number of issues competing for public attention at a given point in time) have increased over time, which has created a competitive zero-sum process, wherein limitations in public capacity mean that increased attention to one issue comes at the cost of others. In other words, issue-attention cycles exist because a large number of issues compete for public attention at any given point in time and the public is not able to pay attention to every issue at once. To cope with this, they cycle from "old" to "new" issues in relatively short periods of time.

In subsequent studies, researchers like Newig (2004) have confirmed the existence of issue-attention cycles and have moved on to focus on the question of causation—if we know that the public has a short attention span and that issue diversity is growing, what explains why some issues spark the public's interest whereas others remain dormant? In his preliminary attempt to answer this question, Newig pointed to three "external" factors that make a problem more likely to garner attention: the severity of the problem, the visibility (accessibility of the problem), and the availability of problem-solving resources, and two "internal" factors—the existence of other issues and the issue's previous history. Thus, in addition to verifying the presence of issue-attention cycles, Newig documented tentative support for Downs's proposition that issues that have already traversed the issue-attention cycle (and remain unresolved) are more likely to recapture the public's attention in the future than issues that have not gone through the cycle.

Adding to this work, a second group of studies inspired by Downs (1972) focus on the role that the media plays in setting the public agenda. Following in the footsteps of McCombs and Shaw (1972), early studies of this type show that media coverage is (on average) positively correlated with public attention—the issues that receive coverage in the media are on average similar to the issues that the public is attentive to (Benton and Frazier 1976). This strand of literature also put forth a hypothesis about "mirror-image" media effects. The studies that tested this hypothesis found evidence for the overall correlation between the frequency of media coverage of an issue, and its corresponding salience for the public. In other words, the more coverage any given issue receives in the media, the more public attention it gets (McLeod et al. 1974). At face value, this correlation confirms Downs's implicit proposition that the media are partly responsible for pushing issues from dormancy to euphoria in the minds of the public. As this line of research expanded, scholars have built upon Downs's model to address more nuanced questions, like does varying public sensitivity to social issues make media attention more influential for some issues and not others? Alternatively, is the relationship between media and public attention spurious—do "real world events" represent a

third variable that explains media *and* public attention? And how rapidly does public attention rise and fall in response to different media cues and real-world crises?

With respect to the former two questions, Erbring et al. (1980) analyzed a series of "most important problem" (MIP) polls from the 1974 National Election Study and respondent data on newspaper content as well as "real-world" conditions to study the link between media attention and public attention. In an effort to move beyond media effects and understand the nuanced impact of "audience sensitivity," the authors postulate and find support that any impact of media attention on public attentiveness is a result of the audience's pre-existing connections to those issues. Additionally, they theorize and find preliminary support for the real-world environment as a statistically significant determinant of issue salience among the public *and* the media.

With respect to the third question, Neuman (1990) proposed a "responsiveness" model to help develop and refine the somewhat deterministic model of media effects. He questioned whether the public's response to certain issues is more drastic than others. Is the public more likely to be attentive to some issues than others? If Neuman's hypothesis is correct, it would mean that public responsiveness varies according to the type of issue at hand. He argues that this typology of cases also affects the impact media attention has on public attention. To test this proposition, he systematically analyzed MIP time series and found that crisis issues covered by the media garner the most distinct response from the public, as compared to symbolic crises that receive a much lower response rate. In other words, real-world cues mediate the relationship between media attention and public attention. Again, all of these findings align rather neatly with Downs's conjecture that real-world events drive issues onto the public agenda and start the attention cycle.

A final group of studies evaluate Downs's implicit propositions concerning public attention and governmental action. In one of the first such studies Peters and Hogwood (1985) explored the relationship between fluctuations in public attention to a given problem and corresponding changes in how the government addresses that problem (i.e. the initiation, supersession, or termination of an organization charged with addressing the problem). Upon doing so, they found that peaks in organizational activity tend, as conjectured by Downs, to occur during or after peak periods of public attention.

Howlett (1997) provides a similar test of the relationship between public attention and government attention by examining the extent to which upswings in public interest are followed by changes in government attention, before the issue fades into public oblivion. Using media mentions as a surrogate for public attention and floor debates/committee reports to measure government attention, Howlett was unable to discern a relationship between public and government attention to two different issues—nuclear energy and acid rain. These results led Howlett to question Downs's proposition and suggest that future scholarship focus on the role that institutions might play in mediating the relationship between public and governmental attention. Perhaps, he concludes, the parliamentary structure of Canadian governance accounts for some of the variation between his findings and Downs's model?

As a partial test of Howlett's proposition, Jones and Baumgartner (2004) reanalyzed the relationship between public and government attention in the US, by systematically comparing public attention (as indicated by MIP polls) to governmental activity—as indicated by hearing activity and public laws. Consistent with Downs and contrary to Howlett, their analysis revealed an “impressive congruence” between public attention, the priorities of Congress, and federal law-making. In other words, issue attention appears to elicit government activity—in the US context. This finding was reconfirmed in Jones et al. (2009), which (again) found a high degree of correspondence between public and governmental attention, especially in areas where “institutional friction” is low. This finding may shed partial light on Howlett's suggestion that Downs's model is in some way tied to the institutional configuration of the US government which may, on average, provide less “friction” than governments in other countries around the world, like Canada.

CONCLUSIONS AND DIRECTIONS FOR FUTURE RESEARCH

Downs's conceptual model of the issue-attention cycle has received substantial attention in the scholarly literature. While in many cases the pieces that refer to his model use it to make tangential references to rising issue concerns, a number of efforts have been made to directly test Downs's propositions about the cyclic nature of issue-attention. We have described several of those efforts here, especially those that test the existence of attention cycles in three different yet interconnected realms of public attention, media attention, and government attention. Judging from a majority of the results summarized, it is clear that Downs's model of issue-attention cycle has fared positively. Most studies have found tentative support for the propositions that he put forth, but more importantly, his seminal work has inspired many scholars to build on his model in innovative ways. It is also impressive to look at the numerous directions his model has been taken, and the various subfields it has been applied to. Nonetheless, important questions still remain—what are the causal linkages between public attention, media attention, and government attention? While we have learnt a lot about these relationships from the studies done so far, questions about the nature of feedback loops from these attention cycles remain largely unanswered. Based on our reading of the literature, several directions for future elaboration and testing of models of the issue attention cycle research seem evident to us. Two in particular stand out: one is that scholars will need a more valid and reliable measure of public attention. The second is that policy scholars can gain more theoretical traction by integrating conceptions of public attention with more general models of policy process and change.

Measuring Public Attention

In the studies summarized, Downs's issue-attention cycle has been tested using proxy measures of public attention. This is because the field is still struggling to find good measures for how the public thinks and what it is thinking about. Studies of public opinion have used (and continue to use) a wide variety of methods to measure public attention—nationwide MIP polls, media coverage, and inputs and outputs of governmental organizations. The studies testing Downs's framework have followed the same path, in addition to using measures for tracking governmental activity on a particular issue. However, there are major theoretical and methodological issues with these most commonly used measurement techniques. For instance, MIP polls and other survey data used as a proxy of public attentiveness to an issue do not fully account for the dynamic nature of the attention cycle (Ripberger 2011). Surveys also artificially constrain the kinds of issues from which people are able to choose to identify salience (McCombs and Zhu 1995; Newig 2004). Similarly, using media attention as a proxy for public attention presents theoretical challenges, such as conflating the media's agenda with the public's interests and concerns. Furthermore, it is not always clear which way the causal arrow points: does media attention drive public attention or vice versa? In an effort to avoid some of these pitfalls, new measures of public attentiveness should be considered.

Measures such as internet search trends (Google Trends) and social media outlets such as Twitter and Facebook may provide a closer and perhaps more realistic look at what and how much people are searching for a particular social issue online (Ripberger 2011). These kinds of measures will permit evaluation of issue attention change on a continuous basis, rather than periodic or sporadic measures. Real progress in hypothesis testing, we believe, will require utilization of such measures.

Integration with Broader Models of the Policy Process

Some of the most important theoretical developments in public policy and related fields have focused more broadly on policy change, in which public attention is but one (albeit important) variable explaining policy change. When broadly articulated, these models also provide insights into how variation in issue attention is related to other key variables in the social and policy context. Theoretical elaboration, and innovations in model definition and testing, can be accomplished by assessing whether the issue-attention cycle can be coupled with these more general models.

For example, scholars in the fields of communication and psychology have developed a useful framework for study of the "social amplification of risk" in which certain kinds of issues (those involving a potential threats that evoke high perceived risks) can be subject to very rapid escalation in issue attention because the potential threat has key psychological properties; because media attention is drawn to issues involving

dire threats; and because interest groups concerned with such issues are primed to take advantage of events that activate high perceived risks among the public (Kasperson et al. 1988, 2000). This model places emphasis on the nature of the issue (and its perceived risk content) and the surrounding media and policy milieu in explaining rapid changes in the amplification (or dampening) of public attention. Other models, such as the advocacy coalition framework (ACF), place emphasis on the array of actors within subsystems who regularly follow and seek to change policies in accord with their enduring policy beliefs (Sabatier and Jenkins-Smith 1993). Within these competing coalitions, mobilization of public interest and concern is a key variable in the struggle to shape policy. Perhaps the most ambitious and well-developed of the policy process theories, with respect to public attention, is the policy agendas project led by Baumgartner and Jones (1993, 2009). In that framework, the role of public attention has been conceptualized alongside policy “images,” both of which can be and are manipulated by policy actors seeking policy change. In all of these cases (and others) the question is of the *pattern* within which changing public attention can be explored as part of a larger system of related components in the public policy process. That approach provides issue-attention scholars with a rich fabric of concepts, relations, and operationalizations with which to work.

In sum, while Anthony Downs’s article on the issue-attention cycle has garnered substantial lasting attention among scholars, we believe there is ample room for continued theoretical and empirical development. In our view, the most promising avenues involve better measures and better integration of the concept of issue-attention cycles with larger theories of public policy.

NOTES

1. The analysis was based on citations in Google Scholar, which permits a year-by-year compilation of works citing a particular article. For each year, the listed works were sorted into articles, books/book chapters, and a “grey literature” consisting primarily of unpublished papers, reports, dissertations, and theses. Citations from the gray literature are not shown in Figure 22.1.
2. Note that the number of citations identified varies depending on the completeness of the descriptor in the Google Scholar search frame. In general, the more complete the descriptor, the smaller the returned number of listed citations. Our search term, derived after considerable experimentation, was specified as follows: A. Downs “Up and Down with ecology: the issue attention cycle”.

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CHAPTER 23

CAROL H. WEISS, *EVALUATION RESEARCH: METHODS FOR STUDYING PROGRAMS AND POLICIES*

KATHRYN E. NEWCOMER

INTRODUCTION

IN 1972 Carol Weiss published *Evaluation Research: Methods for Assessing Program Effectiveness*, which was a small book that introduced readers to many of the big issues that continue to plague evaluators and public managers into the twenty-first century. Over her long career, Carol Weiss contributed much to the theory and practice of public program evaluation, and perhaps the most lasting legacy of her 1972 text was to press evaluators, program managers, and overseers to think in a more nuanced fashion about the evaluation enterprise.

In her text, Weiss highlighted the political deliberations surrounding program design and implementation, the importance and ephemeral nature of the theories underlying and supporting public programs, the differential uses of findings from evaluations, and the manner in which evaluation research might contribute to knowledge about social policy interventions over time. Subsequently, she conducted research and published many articles and books that expanded on these key issues that she introduced in her 1972 text.

An examination of the trajectory of her research and writing demonstrates that Carol Weiss was focused on this set of complex and universal issues for over more than four decades. While she continually chipped away at the core issues about evaluation theory and practice, Weiss never tried to oversimplify or downplay the nuanced nature of the foci of her life's work. The only simple truth about her was that Carol Weiss was

dedicated to improving the use of social science methods to further the accumulation of knowledge in order to improve education and other social services. She felt that her revelations about the political context in which social programs work and evaluations are implemented could help both program managers and evaluators do a better job, and thus improve the services delivered.

In this chapter, first, Carol Weiss's professional and intellectual profile is summarized; second, the span of the influence of her seminal text is described; and third, the four major threads of her intellectual legacy are illuminated. The major threads discussed flow from her 1972 text, and are framed here as the following questions: (1) How can evaluation research play a role in improving public policy-making? (2) In what ways are program evaluation studies used? (3) What does theory-based evaluation entail and why does it matter? and (4) What are the implications of the political nature of public programs and the challenges to getting findings used for the design of evaluation studies?

CAROL WEISS: PROFESSIONAL EVALUATOR AND ACADEMIC

Carol Weiss was a Professor of Education at the Harvard Graduate School of Education for virtually all of her professional career. She taught courses on evaluation, organizational decision-making, and research methods. Her approach to evaluation of public programs reflected both her training in political science (BA Cornell and MA Columbia) and sociology (Ph.D. Columbia), and her experience as an evaluator. Reflecting upon her academic training, Weiss acknowledged that three social scientists greatly influenced her approach and research foci—James March, Charles Edward Lindblom, and Lee Cronbach (Alkin 2013: 140). Weiss's focus on the political context in which programs are formed and thrive clearly reflected political scientist Lindblom's focus on the use of information in democratic politics. Her focus on organizational dynamics within programs, and between evaluators and policy decisions-makers, reflected her sociological training, and especially March's renowned work on decision-making. And psychologist Lee Cronbach's interest in understanding the interplay among contextual factors and program outcomes was apparent in Weiss's nuanced approach to analyzing program theory and context to understand program implementation.

Carol Weiss acknowledged that Donald Campbell, a psychologist and one of the first evaluation theorists, also shaped her understanding of research design (Alkin 2013: 141), but Weiss was much more pragmatic, and less positivist in her methodological approach than Campbell. From her 1972 text onward, Weiss consistently acknowledged that experimental designs were superior to other designs, but she

stressed that research questions should drive the choice of methods, not the reverse. And she recommended that both quantitative and qualitative methods should be used when appropriate, and used well, with full disclosure of methodological limitations. She believed that multiple ways of collecting evidence and contributing to accumulation of knowledge were acceptable, and noted that pure objectivity was impossible, "Inevitably, evaluators won't attain complete objectivity, but we can try for it" (Alkin 2004: 155).

Carol Weiss's experience working at the Bureau of Applied Social Research at Columbia University and in the field, including evaluating a Domestic Peace Corps program and advising evaluations across the country for the President's Committee on Juvenile Delinquency in the 1960s, affected how she approached the task of providing systematic advice on evaluation in her 1972 text. She had been out in many local service delivery sites across the US and observed firsthand how programs were implemented in different contexts and how and when policy-makers used (or ignored) evaluation findings.

Carol Weiss might be described as an evaluation theorist and practitioner who throughout her entire career remained sensitive to the politics surrounding evaluation research. She recognized the importance of the reality that programs are brought into being through political processes. She was a realist, acknowledging that evaluators accept the world as it is, so the practice of evaluation tends to assume an establishment orientation.

Weiss consistently highlighted the interesting juxtaposition between "rational" social science research methods and the less rational world of public policies and programs. She said "Evaluation is not only a research activity but also a political activity, whether we like it or not" (Weiss 2004: 157). And she noted that political considerations intrude in the evaluation enterprise in three ways: (1) programs are creatures of political decisions, with both loyal constituencies and opponents, etc.; (2) evaluation findings must compete with values and political interests, thus we need "political-benefit" analyses, too; and (3) evaluation itself takes a political stance, e.g. the evaluators may accept the values and assumptions of the decision-makers (Weiss 2004: 157). Yet, despite her clear grasp of the political and organizational challenges, she remained optimistic about the potential for evaluators to help reform and improve policies and services.

While Carol Weiss has been credited with significant contributions to the way that evaluators think about how to understand the use of evaluation findings, when the major evaluation theorists were sorted by Marvin Alkin in a seminal work on the roots of evaluation thinking in 2004, she was credited as contributing most to the profession's approach to "Methods," or knowledge construction (Alkin 2004). Throughout her career Weiss stressed that evaluators must strengthen the methodological merit of their work to withstand political scrutiny, and she offered much guidance about how to implement social science research methods wisely. With her publication of *Evaluation Research: Methods for Assessing Program Effectiveness*

in 1972, along with an accompanying reader, *Evaluating Action Programs: Reading in Social Action and Education* in the same year, Weiss began her 40-year journey to influence the evaluation enterprise, and it is that span of influence that is described next.

THE SPAN OF INFLUENCE OF EVALUATION RESEARCH: METHODS FOR ASSESSING PROGRAM EFFECTIVENESS

In 1972 the academic field of program evaluation was just emerging with a separate identity, and there was really only one textbook that differentiated evaluation research from social science research design, published by Edward Suchman in 1967. Carol Weiss's approach to designing evaluation studies in an action setting heavily reflected the influence of Suchman, but she went further to highlight the relevance of the political environment surrounding both programs and their evaluation for evaluation design.

An article that Weiss published in 1970, titled the "The Politicization of Evaluation Research," foreshadowed the content of her 1972 text with its focus on the politics surrounding the conduct and use of evaluation research. In her 1970 article she wrote that, in response to the somewhat disheartening political environment,

One hopeful direction is to place less stress on evaluation of over-all impact, studies that come out with all-or-nothing, go/no-go conclusions. More resources should be allocated to evaluations that compare the effectiveness of variant conditions within programs (differing emphases and components of program, attributes of sponsoring agency structure and operation, characteristics of participants) and begin to explain which elements and sub-elements are associated with more or less success. Such an approach produces data of interest across a wide range of programs and has high utility in pointing direction for further program development. (Weiss 1970: 62-3)

This advice that Weiss offered in 1970 was essentially what she expanded upon in her 1972 textbook, and what she consistently provided in her many publications over the next four decades.

When her text came out in 1972 the reviews were mostly positive. Patricia Perri Rieker wrote that, "The book's value derives mainly from the author's ability to translate her cumulative, wide-ranging experiences in conducting evaluations and advising decision-makers into a straightforward, unpretentious and quite readable document" (Rieker 1975: 284). Rieker said she found one of the most important aspects of the book was the brief last chapter on the utilization of evaluation results. In the text, serving as

a precursor to her subsequent research, Weiss pointed out five constraints on the utilization of evaluation results: the evaluators' uncertainty about what role to play in the utilization process, the gap between findings and clear recommendations for alternative courses of action, organizational resistance to change, inadequate dissemination of evaluation findings, and negative findings—all topics that absorbed Weiss's attention for many years.

Similarly, Francis Caro lauded Weiss's 1972 text and said, "In spite of its brevity, the volume is impressive for its inclusiveness and balance" (Caro 1972: 640). Caro also noted that Weiss had emphasized the principles of evaluation methodology that were applicable across a large variety of social programming specialties—and she proved to be correct in that prediction.

James Vanecko offered the only negative review of Weiss's text and said the text "which initially appears to be a simple description and catalog of evaluation research practices and problems . . . takes on the appearance of a rather more analytic discussion of the roles of evaluation researcher and policymaker or program administrator. Since it is more seductive than fulfilling, this analysis is ultimately a disappointment" (Vanecko 1974: 266). With only 128 pages, the text was not designed to be a comprehensive stand-alone textbook that could guide evaluators through the political thickets, and Vanecko's criticism that Weiss raised intriguing analytic questions and then disappointed the reader with oversimplified outlines for consideration of solutions is probably well deserved. Vanecko was especially miffed with what he found to be overly vague advice that "It is important that the evaluator be able to live with the study, its uses, and his conscience at the same time" (Weiss 1972: 18). Weiss was perhaps overly effective in highlighting the many ambiguities involved in evaluating programs in the applied setting, but that reviewer, and perhaps some readers, wanted more simplistic answers—and that was never Weiss's forte.

Many readers in a wide variety of fields have found Weiss's 1972 text to be useful and enlightening. As Figure 23.1 illustrates, the number of subsequent books and articles that have cited the text has been impressive consistently over four decades. While the numbers of citations identified by Google Scholar are impressive (1,419 thus far), what is also significant is the broad span of her influence in such a large number of academic disciplines and professions—from criminal justice and mental health to the physical sciences, as seen in Table 23.1.

The Web of Science provides data on the number of citations that reference the articles that Weiss has published over her career (1,339 thus far), and again, the consistency in the interest in her work over the decades is impressive, as seen in Figure 23.1. The names and the publishing venues of the 10 articles that Weiss published that have been cited the most frequently are provided in Table 23.2, along with the topics that the articles addressed which were initially touched upon in her classic 1972 text. A full 26 years later Weiss published a second longer edition of her text (339 pages), and she elaborated on the guidance that she had initially provided

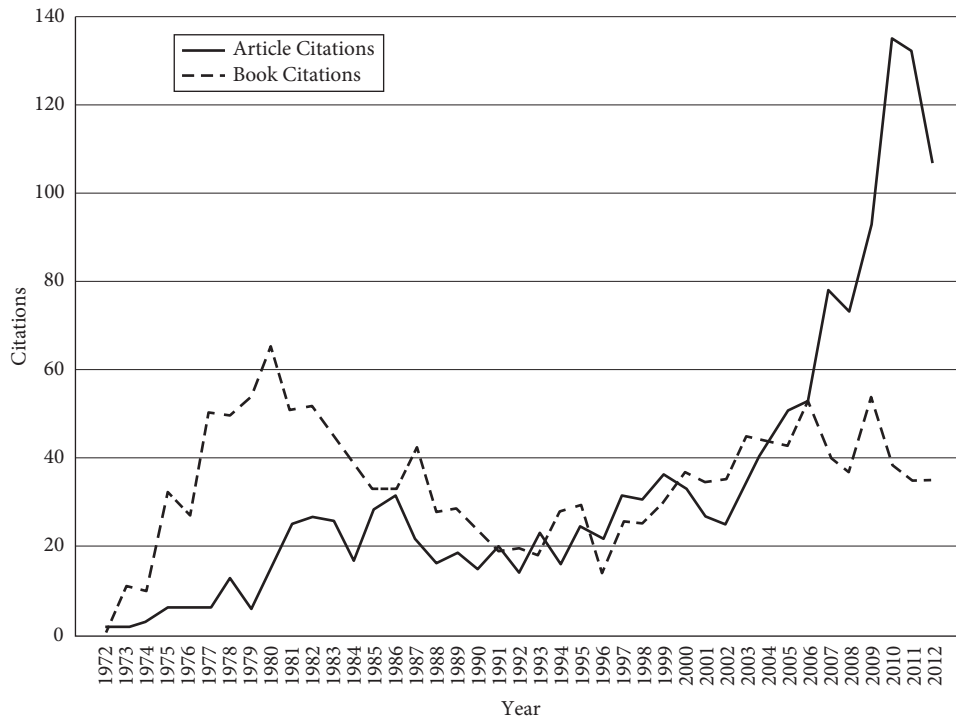


FIG. 23.1. Citations for Carol Weiss's 1972 text and articles 1972–2012

Table 23.1 Topical areas where Weiss's 1972b is cited

Research Areas	Percent of total citations*
Evaluation Methods	27%
International Studies	15%
Social Policy/Social Work	13%
Health/Medicine	10%
Public Policy and Public Administration	8%
Education	8%
Environmental Studies	4%
Criminal Justice	4%
Information Technology/ Business	4%
Psychology	3%
Organizational Behavior	2%
Communication/Language	2%

* Percentages based on the first 1,000 citations in Google Scholar

Table 23.2 Top 10 articles by Carol Weiss that have been cited most frequently and issues they address

Title	Year	Percent of Total Citations	How can evaluation research play a role in improving public policy making?	In what ways are program evaluation studies used?	What does theory-based evaluation entail and why does it matter?
The Many Meanings of Research Utilization	1997	22%	✓	✓	
Knowledge Creep and Decision Accretion	1980	13%	✓	✓	
Research for Policy's Sake: The Enlightenment Function of Social Research	1977	12%	✓		
Truth, Tests and Utility Tests: Decision Makers' Frames of Reference for Social Science Research	1980	7%	✓	✓	
How can Theory Based Evaluation Make Greater Headway	1997	7%			✓
Have we learned anything new about the use of evaluation?	1998	5%		✓	
What America's Leaders Read	2001	4%	✓	✓	
An Alternate Route to Policy Influence: How Evaluations Affect D.A.R.E	2005	3%	✓	✓	
Theory-Based Evaluation in Practice: What do we Learn	2000	3%			✓
Congressional Committees as Users of Analysis	1989	3%	✓	✓	

in the 1972 original, which was still relevant and quite on target (Weiss 1998). The basic themes in her text are now elucidated.

THE MAJOR THREADS OF WEISS'S INTELLECTUAL LEGACY

Carol Weiss organized her 1972 text around five key components of evaluation practice she felt that evaluators need to understand in order to produce relevant and competent evaluation research: (1) the purpose and organizational context of evaluation research; (2) how to formulate evaluation questions and the measures needed to answer them; (3) how to develop appropriate evaluation designs to answer the evaluation questions that have been formulated; (4) implications of the action program context for the conduct of evaluation studies; and (5) the use of evaluation results. To highlight the influence that Weiss's 1972 outline of the key challenges facing evaluators of social programs has had, the topics are now discussed as they addressed the fundamental and still relevant questions she raised.

How Can Evaluation Research Play a Role in Improving Public Policy-Making?

As she foreshadowed in her 1970 article about "The Politicization of Evaluation Research," Carol Weiss had witnessed the lack of use of evaluation findings as well as the rising expectations about what evaluations could deliver, and she wanted to help evaluators navigate the treacherous political and bureaucratic landscapes to provide methodologically sound work that would improve programs and policies. She exhorted evaluators to be sensitive to the nuances involved in determining who the relevant players were, and their stake in the program and thus in the evaluation, as well as the complexity of organizational decision-making.

Quite prophetically, given the push toward evidence-based policy in the early twenty-first century, Weiss cautioned that expectations had risen and there was what she believed a publicized assumption that "The production of objective evidence is seen as a way to reduce the politiking, the self-serving maneuvers, and the log-rolling that commonly attends decision making at every level from the Congress to the local school" (Weiss 1972b: 3). She attributed part of the disillusionment with the contribution of evaluations to improving policies to unrealistic expectations about what hard evidence could be produced, but also to naïve understanding among evaluators about the way that policy-makers and program managers operate. She noted that "An evaluation study does not generally come up with final and unequivocal findings about the worth of a program. Its results often show small, ambiguous changes, minor effects,

outcomes influenced by the specific events of the place and the moment. It may require continued study over time and across projects to speak with confidence about success and failure” (Weiss 1972b: 3). She cautioned that, for decision-makers, evaluation evidence of desired program outcomes is only one input out of many, and she said that “Part of the fault is the remarkable resistance of organizations to unwanted information—and unwanted change. Even evidence of outright failure can leave some institutions figuratively and literally unmoved. Part of the fault lies in the way evaluation itself is structured, staffed, and operated. There are fissures between the intended purposes of evaluation and the kinds of studies conducted.” (Weiss 1972: 3).

Weiss also addressed the identity of evaluation research in a somewhat novel (for its time) fashion, when she distinguished between evaluation research and basic research in her 1972 text. She recognized that evaluation research was sometimes regarded as a lower order of research than basic or pure research. In response, she said:

Evaluation calls for a higher level of skills than research that is under the researcher’s complete control. It is relatively easy to run experiments in an insulated laboratory with captive subjects. But to make research work when it is coping with the complexities of real people in real programs run by real organizations takes skill and some guts . . . He has to know what is in the research methodology texts, and then he has to learn how to apply that knowledge in a setting that is often inhospitable to important features of his knowledge. If he persists in his textbook stance, he runs the risk of doing work irrelevant to the needs of the agency, antagonizing the program personnel with whom he works, and seeing his study results go unused—if indeed the work is ever completed. (Weiss 1972b: 9)

Despite the challenges she identified, Weiss provided guidance to evaluators on how to scope out the purposes, acknowledged and unacknowledged, for the evaluation, which decision-makers were seeking evaluation results, and how the decision-makers expected to use the results. Her intellectual legacy along these lines was fully fleshed out by many other evaluation theorists, but probably most eminently by Michel Quinn Patton in his 1986 book, *Utilization-Focused Evaluation*, as well as in the three subsequent editions of that classic. However, in contrast with Patton’s preoccupation with designing evaluations to support fairly immediate utilization, Weiss was not as worried about immediate application of findings, for she was comforted with the longer view that enlightenment, or accretion of learning through multiple studies of similar programmatic elements, was the more valuable and realistic end to keep in sight (Weiss 1998).

Weiss was persistent and consistent in her efforts to instruct evaluators on how to support enlightenment on which program components worked under what circumstances to accumulate over time too, and she persisted in studying how policy-makers interpret evaluation research results to increase the likelihood that evaluation could be used to improve social programming (Weiss 1977b, 1980, 1989, 1997). A good number of evaluators and academics followed up on Weiss’s concerns about the use of evaluation research to inform policy-making over the next decades (see especially Saxe and

Koretz 1982; McLaughlin et al. 1988; Mohan and Sullivan 2007; Ottoson and Hawe 2009). The different ways in which evaluation studies might be used became a related, yet somewhat distinct focus of her attention throughout her career.

In What Ways are Program Evaluation Studies Used?

Carol Weiss is typically recognized for her contribution to thinking about the use of evaluation research findings in a nuanced fashion. She popularized the distinction between instrumental use, or the immediate use of findings to direct decisions, and conceptual use, or a more extended influence on thinking informed by evaluation findings, and she distinguished various other ways in which policy-makers and bureaucrats use evaluation findings over the years. She conducted research to identify which sorts of uses were made of evaluation findings in which sorts of contexts (see Weiss and Bucuvalas 1977, 1980a, 1980b; Weiss 1989, 1997, 1998; Birckmayer and Weiss 2000).

In her 1972 text, Weiss delineated the key factors that evaluators needed to take into account in order to understand intended use, potential for unintended use, and the actions that evaluators needed to undertake to increase the likelihood that their recommendations would be followed. Again, she highlighted the impact of the politics surrounding the espoused goals, design, and implementation of programs on eventual use of evaluation findings. She highlighted the variety of potential users who may present different concerns and questions to address, i.e. funding organizations, national agencies—both governmental and private, local agencies, project directors, direct-service staff, program clients, and scholars in disciplines and professions. She also highlighted the dilemmas evaluators face when simply trying to understand program goals, and noted that “Fuzziness of program goals is common enough phenomenon to warrant attention” (Weiss 1972b: 27).

The importance that Weiss placed upon the daunting task of framing appropriate and potentially useful evaluation questions was related to how she viewed the use challenge. And, reflecting her training in sociology and organizational behavior, she stressed the need for evaluators to understand how organizations’ cultures affect their receptivity to changes recommended in evaluation studies. She argued that evaluators must strive to present methodologically defensible findings and well crafted feasible recommendations, but that said that will not be sufficient to ensure positive use. She reminded readers that “Organizations tend to find the status quo a contentedly feasible state” and that organizations “have concerns other than achieving their goals,” and have “ideological commitments that may be averse to changing operations” (Weiss 1972b: 114–15). Thus, Weiss started a line of inquiry into the variety of evaluation users and uses that has continued for over 40 years (Caplan 1977; Alkin et al. 1979; Conner 1981, 1998; Leviton and Hughes 1981; Greene 1988; Shulha and Cousins 1997; Johnson 1998; Cousins 2004; and see Caracelli and Preskill 2000 for a nice overview of Weiss’s legacy of distinguishing among different sorts of uses by different stakeholders).

What does Theory-Based Evaluation Entail and Why does it Matter?

In her 1972 text Carol Weiss offered perhaps the first discussion of the notion of basing evaluation studies on the program's theory. She introduced the process model and urged evaluators to collect data on the posited links between program components, and between them and the intended outcomes. She discussed the need to measure both "short-term and longer-range effects," and used terms that subsequently have become commonly used when discussing program logic models and logical frameworks (Newcomer 1997; McLaughlin and Jordan 1999, 2010; Poister 2003; Frechtling 2007). In her 1972 text Weiss said "Programs attempt to set in motion a sequence of events expected to achieve desired goals" (1972: 38). She also noted the importance of empirically examining the causal processes put into place so that a determination could be made on whether the pertinent processes were put into place or not—so that program implementation failure could be distinguished from theory failure. She stressed that "By examining the association between immediate effects and long-range consequences, evaluation can contribute to program practice and program planning—and also to the development of social theory" (Weiss 1972b: 39).

In her guidance on what to measure, Weiss stressed the importance of measuring what the program actually is—program inputs, activities, and important intervening processes or explanatory variables. She said:

It is important to look at program variations for two reasons: (1) They clarify the meaning of "the program." They fill in the details of what the general program description has outlined; they show the range of elements that are encompassed by the program-that-is. (2) They contribute to the analysis of which features of the program work and which do not. It becomes possible to look at the effects of program components and see whether some are associated with better outcomes than others . . . The analysis of program variables begins to explain why the program has the effect it does. (Weiss 1972: 46)

While the notion of theory-driven evaluation was expanded and popularized subsequently by Peter Rossi and Huey Chen (1980; Chen 1990, 2005), among others, Weiss's discussion in 1972 and her further elaboration in 1997 were quite far-sighted. She highlighted the nuanced nature of programs and their expected causal processes, and the need for evaluators to measure, rather than assume, causal links far earlier than other evaluation theorists. Again, interest in the appropriate ways to incorporate program theory in evaluation design that Weiss pioneered has continued over the decades (see Bickman 1987; Weiss 1997; Rogers et al. 2000; Rogers 2007).

What are the Implications of the Political Nature of Public Programs and the Challenges to Getting Findings Used for the Design of Evaluation Studies?

In 1972, and for the rest of her life, Carol Weiss believed that evaluation research “calls for a higher level of skills than research that is under the researcher’s complete control,” and she worked to help evaluators develop the skills that she felt were needed for evaluation work to be useful, and therefore to inform public policy-making and improve social services. Many of the evaluation methods textbooks that were published after Weiss in a variety of fields (see Table 23.2) followed Weiss’s lead in highlighting the importance of the following the design and implementation of evaluation research: the political nature of social programs’ design and delivery, the large number and diversity of program stakeholders who affect the purpose of each evaluation, the need to understand the theory underlying the design of programs, the impact of local context on program implementation, and the need to understand organizational behavior when working with program personnel and policy-makers, both during and after evaluation studies were completed. The evaluation methodology textbooks that dominated in sales for evaluators (and would-be evaluators) of social programs in the years following the publication of Weiss’s 1972 text all elaborated upon the issues that she had outlined (e.g. Worthen and Sanders 1973; Rossi et al. 1979; Rossi and Freeman 1993; Fitzpatrick et al. 2004, 2011; Patton 1990, 2008, 2011; Wholey et al. 1994, 2004, 2010; Mark et al. 2000; Chen 2005).

Carol Weiss exhorted evaluators to develop their political savvy and knowledge of organizational behavior to be able to more effectively: analyze the origins and politics surrounding social programs; frame appropriate and sufficiently nuanced questions; measure program components, outputs, and outcomes validly and reliably; and disseminate evaluation findings to maximize the likelihood of recommendations being implemented. Drawing upon her experience in the field, she recognized that policy-makers and others who did not like an evaluation’s findings would be quick to criticize the methodology employed, and so she recommended transparency and humility when communicating results, “If there are gaps, rival interpretations, or room for doubt, the evaluator who acknowledges limitations can help decision-makers arrive at responsible choices” (Weiss 1972b: 120).

In the end, what clearly motivated Carol Weiss was her perception that social policies were generally not effective and programs were “often poorly managed,” thus better social science knowledge was urgently needed to improve both the policies and programmatic execution. She was pessimistic about the state of social programming in the US in 1972, but optimistic about the role that rigorous evaluation research could play,

Much remains to be done to improve social programming. Many moderate, piecemeal, cheap solutions have been tried, and evaluation research has found them wanting. If we take evaluation results seriously, we will have to embark on more

fundamental social experimentation. Social institutions will have to take greater risks in the search for effective programs. Evaluation can be a partner in this search if it is given the funds and the conditions to test out small-scale experimental projects. As programs are developed on better knowledge foundations, with better structural arrangements and greater integration with allied institutions and overall policies, evaluation has a further role to play. It can gauge the effectiveness of the innovations and determine which features are ineffective and which should be retained for further development. With all of its failings, evaluation research still has the potential for bringing greater rationality to social decision making. (Weiss 1972b: 128)

CONCLUSION

When Carol Weiss published *Evaluation Research: Methods for Assessing Program Effectiveness* in 1972 she provided a blueprint for the issues that she would research for the rest of her career—the influence of evaluation research on public policy, the multiple ways that evaluation processes and products might be used, and the importance for program theory to guide evaluators. She introduced issues that continue to draw the attention of both evaluators and policy-makers over four decades later. Her book, as well as her articles, informed and inspired academics and practitioners in many quite disparate fields—from criminal justice to health policy to conservation. Carol Weiss contributed much to the theory and practice of program evaluation, and as I have noted, she consistently addressed the more thorny issues, and never tried to oversimplify or offer formulaic, easy solutions. She analyzed and explicated challenges faced by evaluators and their audiences in a nuanced fashion, and influenced the rest of us to follow her lead. Carol Weiss made an incredibly valuable and lasting imprint on academics, practitioners, and other stakeholders in the evaluation enterprise.

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CHAPTER 24

JEFFREY L. PRESSMAN AND AARON B. WILDAVSKY, *IMPLEMENTATION*

KAI WEGRICH

INTRODUCTION

IMPLEMENTATION by Jeffrey L. Pressman and Aaron Wildavsky was first published in 1973 and rapidly became one of those case studies that triggered a paradigmatic change in its field, policy research. Comparable to Selznick's *TVA and the Grassroots* in organization research/public administration or Dahl's *Who Governs?* in democracy and power studies, *Implementation* is not only regarded as a key contribution within the field of policy studies, but it is also considered the study that "invented" implementation studies as a major area, and which was in the 1970s and 1980s possibly the most significant subfield of policy research. The book has the reputation of having discovered the study of the implementation stage as the "missing link" (Hargrove 1975) in policy studies and the crucial stage in the policy cycle in which success and failure of policies are decided. Moreover, *Implementation* and subsequent studies in this new field of policy research introduced a more pessimistic view on the capabilities of the state to influence societal developments with ambitious welfare-state policy reform programs. It hence acted as a timely antidote to the ambitions of policy-makers and policy researchers who were involved in the planning of these reforms and were left puzzled by the disappointing outcomes of these policies. Implementation was identified as a complex process that involves long chains of interactions across different levels of government, and for implementation to be successful the cooperation at each of these links needs to be almost 100 percent to avoid a situation where minor conflicts and delays accumulate and lead to overall program failure.

This sceptical view is epitomized in the long and famous subtitle of the book "How Great Expectations in Washington Are Dashed in Oakland; Or, Why It's Amazing

that Federal Programs Work at All, This Being a Saga of the Economic Development Administration as Told by Two Sympathetic Observers Who Seek to Build Morals on a Foundation of Ruined Hopes.” *Implementation* also became famous due to the format of the published book: including the long subtitle, the dictionary definitions of implementation provided in the prelims of the book, and the cartoons added to highlight the complexity of long chains of interaction. Its reputation was also associated with the (pseudo-)mathematical “proof” of the strong claim concerning the likelihood of implementation failure due to the high number of “clearing points.”

When *Implementation* was published in 1973 Aaron Wildavsky had already played a key role in developing a political science/public administration take on policy analysis, distinguishing himself from overly technocratic policy analysis exercises (applying economic techniques to policy choices). He helped to establish policy research as a significant field of research in political science and was the founding dean of the public policy school, established in 1969, at the University of California in Berkeley. His co-author, Jeffrey Pressman, was a member of Wildavsky’s team of graduate students and young faculty members (he passed away prematurely). By the early 1970s, Wildavsky had already published a number of highly influential studies, ranging from the field of community power studies to budgeting. His *Politics of the Budgetary Process* (1964) is regarded another seminal piece of work that has changed the way public budgets are analysed (see Wehner, Chapter 14, in this volume). It is somewhat surprising that a scholar known for a distinctively political view on public policy and administration issues, and an outspoken individualist, would become famous for a work that seems to advocate hierarchy and centralization. This chapter argues that this image is at least partly due to an incomplete reading of *Implementation*. In chapters added to later editions of the book (1979, 1984) Wildavsky engaged more extensively with the limitations of the “top-down” perspective that *Implementation* had established.

IMPLEMENTATION: THE PROJECT AND THE STUDY

While the activity of public authorities was of course a key theme of research in public administration and organization research prior to the publication of *Implementation*, the field of policy research was mainly focused on policy design, particularly in the applied variants of policy analysis, and to some extent on policy outcomes. But, how policy programs and laws were actually put into practice, how resources were allocated across levels of government, how discretion of administrative agencies was used, and how they interacted with the target population in the implementation of programs, and how all this influenced policy outcomes was not studied systematically. “Implementation in recent years has been much discussed but rarely studied,” as Pressman and Wildavsky claim in the preface to the first edition (1984: p. xxi).

The demand for such a focus on implementation resulted from the political context of the 1960s and early 1970s in the US but also in other Western democracies (in particular in Western Europe). This context was special, if not unique, in its combination of a social reform ambition with an extensive involvement of social science in policy-making. Lyndon B. Johnson's "Great Society" and "War on Poverty" introduced a range of social, education, and economic development programs that sought to address the embarrassing scope of poverty, social deprivation, and related crime in the US. The social programs adopted since 1964 came with evaluation clauses that prompted the assessments of the impacts and achievements of these programs, often leading to disappointing results. The project leading to *Implementation* was triggered by such disappointing results but was actually provoked by rather optimistic early assessments of the major program of the Economic Development Agency (EDA) in Oakland, California. According to Wildavsky's autobiographical account (Wildavsky 1993: 107), the interest in the project was prompted by the publication of a book, *Oakland is Not for Burning* (Bradford 1968). Written by one of the key actors of the EDA, the book praised early achievements of the program for the promotion of employment among "minorities" (African-Americans in today's language). Wildavsky and collaborators became skeptical, and when the media joined the praise of the program Wildavsky asked a graduate student to look into that.

Jeff Pressman wanted to know more, so he prepared a more substantial account. As we read it over together, it seemed to us that an awful lot of approvals by diverse agencies were required to make things happen. Therefore I asked Jeff to prepare a list and then a flow chart. That was the beginning of our study of the "Complexity of Joint Action," as the crucial chapter around which our collaboration was called. (Wildavsky 1993: 107)

The EDA's Oakland project was considered a pilot project, an experiment, for the attempt of the EDA to engage in economic development in cities. Originally, the EDA was set up as an agency to promote economic development in deprived rural areas, taking over jurisdiction from the Area Redevelopment Administration (ADA) in 1965 (ADA had been established in 1961). The leaders of the EDA promoted the engagement of the agency in the urban context because this context was identified as critical for fighting poverty and promoting economic development in the future. The city of Oakland was chosen as the place for this first experiment due to its comparatively high unemployment rate (8.4 percent in 1965 compared to a national average of 4.1 percent), an above-average share of minorities, and the city's comparably small size—all of which were formal criteria that qualified Oakland as one of few larger cities for this program. But Oakland was also considered as being at risk of becoming a new site of racial riots that had spread across US cities. There was a sense of urgency to preempt the outbreak of violence, as is reflected in the book title "Oakland is Not for Burning". Against this background the commitment to spend about \$23m was made in a short period of time, and already in 1966 the program was to start in Oakland. Previous

engagement of ADA in Oakland did seem to provide a basis from which to move on with the larger program of the EDA.

The Oakland program consisted of public work grants and business loans amounting to \$23,289,000, which was meant to increase employment opportunities for the hard-core unemployed minorities. Corporations and public works could apply for funding by the EDA or could lease EDA-financed facilities. They had to provide detailed employment plans, which had to be accepted by a multi-actor employment review board. This board also monitored the performance of the employers. The employment-related stream of the program included elements of training for the jobless to help them qualify for the jobs that were supposed to be provided through the loans and grants (Pressman and Wildavsky 1973: 2). This set-up resulted in two parallel chains of implementation: one for the applications for loans and grants and one for the employment plan.

The Oakland program initially comprised four major public works, an airport hangar, a marine terminal, a port, an industrial park, and an access road to the Coliseum. With the exception of the access road to the newly built Coliseum, for which the city government applied, all projects were related to the port of Oakland, also including the maritime port and the airport. The airport hangar project itself was leased by World Airways, a major airline company. At later dates, further public work projects were awarded to the port of Oakland: an air cargo terminal, 20 small aircraft hangars, and an auxiliary airport tower. Choosing the port as the main site for the project was due to the time pressure under which the EDA program in Oakland was developed. The appropriated budget for the Oakland program needed to be spent by autumn 1966 to avoid the money being lost and future appropriations of the EDA reduced. The initial problem during implementation was the major delay in the public works program. In 1969, three years into the program, only \$3m of the \$23m available funds had actually been spent, most of which had been used for the city government's own project, the access road. Only this road, the industrial park, and the small hangars had been completed, whereas the two main public works, the airport hangar, and the marine terminal, were not even under construction (Pressman and Wildavsky 1973: 54).

Yet, from the social policy perspective, the low number of jobs created for the urban poor and jobless was regarded as the main failure of the program. The EDA was criticized for not having used its influence to enforce the employment plans and thus not having been able to fulfill its original goal. By the end of 1970 the marine terminal contributed about 1,000 new jobs, of which about 350 were for "minority" workers. Despite the fact that the EDA only financed one-third of these jobs, officials counted all of them as EDA-induced since they would not have been generated without the existence of the program. The industrial park contributed about 30 new jobs and the air cargo terminal 250, but numbers of minority workers employed were not available for these two projects (Pressman and Wildavsky 1973: 67). What is more, the business loan scheme failed remarkably: from the \$10m available, only \$1m was invested and created only 43 new jobs for poor Oakland residents, instead of the projected 800. The greatest success of the EDA program was the funding of the community-founded West Oakland Health

Council, Inc. with \$1.4m. It was opened in 1969 and created 160 jobs, 150 for minority workers (Pressman and Wildavsky 1973: 80 ff.). In total, the jobs created for ethnic minorities totaled about 500, plus a temporary 180 for the construction of the marine terminal. Taking the severe delay of the EDA program into account, this was still far away from the initially projected 3,000 jobs for the hard-core unemployed in Oakland.

ACCOUNTING FOR IMPLEMENTATION FAILURE

When Pressman and Wildavsky set out to account for this program failure, they felt that there was no analytical apparatus available to them for the study of the implementation process. The results of the literature review they conducted at the beginning of the project, presented in Annex 1 of the first edition (not reprinted in the following editions, which include a longer bibliography on implementation research), was disappointing. While some commentators argue that they could have made a much stronger effort to build their analysis on pre-existing public administration scholarship (Hill and Hupe, 2009: 18), they did make an honest effort to locate and make use of research on implementation. While maybe it is a “founding myth” that the systematic study of the implementation process was the “missing link” (Hargrove 1975) connecting research on policy formulation and evaluation, it is undeniable that implementation studies boomed after publication of *Implementation*.

Some notable studies published prior to *Implementation* did engage with failures of reform—in particular Moynihan’s study of community action programs (*Maximum Feasible Misunderstanding*, 1969) and Martha Derthick’s study of urban development policy (Derthick 1972). But while Pressman and Wildavsky acknowledge Derthick’s work as pioneering for the study of implementation, they considered their own work as entering new territory in the study of public policy.

The first hurdle to take was to define implementation. Initially, Pressman and Wildavsky (1984: p. xxi) simply followed dictionaries and defined implementation as: “to carry out, accomplish, fulfil, produce, complete.” “Programs” or “policies” were the objects to be implemented, but this is where the conceptual difficulties started. The separation of the policy program and the act of policy-making from implementation was all but unproblematic, but such a separation was the starting point and necessary (analytical) precondition for exploring success and failure in policy implementation. As Hill and Hupe (2009: 4) highlight in their book on implementing public policy, the separation of implementation from decision-making is the linchpin of implementation research without which an assessment of implementation would be impossible. At the same time, aspects of implementation are inherent elements of policy, which leads to a dilemma. In Pressman and Wildavsky’s words (1984: p. xxii, also quoted in Hill and Hupe 2009: 4): “We can work neither with a definition of policy that excludes

any implementation nor one that includes all implementation.” This conceptual problem, recognized early, turned out to be the Achilles heel of the type of implementation research Pressman and Wildavsky initiated. The chapters that were added to later editions of the book included reflections on these thorny conceptual issues and implications of the interdependent nature of policy(-making) and implementation. However, the original study firmly rested on the assumption that such a separation is necessary and possible (albeit with full awareness that this was an analytical device coming with obvious disadvantages).

Nevertheless, such understanding of implementation as separate from policy design allowed Pressman and Wildavsky to study two key factors critical for implementation/policy failure or success. The first factor was the “program theory” inherent in a particular policy design. As stated in the preface to the first edition, Pressman and Wildavsky conceptualized a policy as the hypothesis containing initial conditions and predicted consequences; “If X is done at time t_1 , then Y will result at time t_2 ” (Pressman and Wildavsky 1984: p. xxii). They call a program the conversion of such a hypothesis into government action, i.e. the provision of loans and grants for enterprises in Oakland by the federal government (via the Economic Development Agency) to hire minorities. The hypothesis enacted by the program is that the incentives will lead enterprises to hire minorities, in order to carry out construction projects that will lead to the reduction of unemployment among the minority population, and eventually to the reduction of social tensions, hence reducing the risk of an outbreak of violence in Oakland. One of two key insights of *Implementation* is that the policy design, i.e. the program theory, has a major influence on the implementation process—and that this design was one of the main reasons for the failure of the policy in Oakland. As mentioned before, the EDA was established to support economic development in depressed, rural regions rather than to promote employment of citizens from deprived neighborhoods within a prosperous region, such as East Bay California. The design of subsidy programs as the main policy tool of the EDA was geared towards the problem constellation in rural areas. The causal link between providing cheap capital to companies and the hiring of the long-term jobless was flawed, even as loans were based on the condition of hiring the unemployed. The incentives for companies to hire the long-term unemployed, in the context of the labour and capital market of East Bay, were unfavorable. And the indirect nature of the link between cheap capital and the hiring of jobless ethnic minorities required the establishment of an additional control structure, i.e. a tool to account for the “promise” of companies to hire jobless minorities—the employment plan—and some monitoring and sanctioning mechanisms (which lacked credibility and were softened early on).

This policy design added to the complexity of the implementation structure—a structure that turned out to be far more complex than expected when a separate agency (EDA) outside the established machinery of federal, state, and local government was charged with developing and implementing this program. Revealing this “complexity of joint action” (chapter 5) is the second major insight of the study. For the detailed analysis of the complexity of joint action, they considered the EDA public works project

as particularly fitting, because of their seemingly low level of complexity: “The marine terminal and airline hangar projects . . . deliberately included just one federal agency in one city; there would be only one major recipient, the Port of Oakland . . .; and there would be an immediate commitment of \$23 million” (Pressman and Wildavsky 1984: 93). Despite that simple and widely supported project, the implementation structure turned out to be complex, involving various offices of the EDA (the headquarters in Washington DC, the regional office in Seattle, the Oakland presence) and various other federal departments and local agencies.

This—surprising—complexity makes it very unlikely that such a program could ever be carried out without major distortions when it goes through the machinery of governments and administration. Given the large number of points where actors from different levels of government and organizations need to interact and agree on the specification of a program, and given the divergent interests of those actors involved, it is very likely that the program will be delayed at those “clearing points.” Claiming to apply a conservative measure, Pressman and Wildavsky identify 70 such “clearing points” in the Oakland public works program. They argue that even a small probability of disagreement at one clearing point (they run calculations with 85, 90, 85, and 99 percent probability) would lead to a cumulatively extremely high probability of delay. They also argue that iterative interaction (rather than a one-off yes or no decision at a clearing point) would not change this picture substantially. Empirically, minor delays of the program at each of the clearing points lead to a major overall delay before the program finally arrives at the street level.

Such delays and also divergences from the original policy at “clearing points” are not triggered by notable political rivalry or contestation; in methods-speak, the program presents an unlikely case of implementation failure (for political reasons) because it received widespread support. Pressman and Wildavsky (1984: 99–102) identify seven reasons why actors that might agree with the substance of a program would still oppose the means of implementation, ranging from incompatibility with other commitments to different assessments of priorities to different organizational roles and responsibilities. Conflicts at clearing points also lead to the emergence of unexpected decisions that extend the planned chain of implementation. The essence of this analysis of the complexity of joint action and the role of clearing points is best summarized in the original work:

What had looked like a relatively simple, urgent, and direct program—involving one federal agency, one city, and a substantial and immediate funding commitment—eventually involved numerous diverse participants and a much longer series of decisions than was planned. None of the participants actually disagreed with the goal of providing jobs for minority unemployed, but their differing perspectives and sense of urgency made it difficult to translate broad substantive agreement into effective policy implementation. (Pressman and Wildavsky 1984: 113)

It is in particular this second insight that leads to the condemning diagnosis that stuck: implementation failure should be considered as normal and even seemingly

simple programs can turn out to be complex to implement. Hence one of the major lessons resulting from the study is: keep it simple! Reduce the number of vertical links and clearing points! By implication, centralization is considered as an enhancer of congruent implementation (cf. Hupe 2010). Related to the first conceptual innovation, the focus on program theory, a similar message of simplicity is derived: not only should goals be as clear as possible, but they should also be addressed in the most direct way. Instead of indirect subsidies of investment or public works with a parallel procedure controlling for the hiring of minorities, this goal should be targeted directly, i.e. subsidizing outcomes. “If policy analysts carry bumper stickers, they should read, ‘Be Simple! Be Direct!’ or ‘PAYMENT ON PERFORMANCE.’” (Pressman and Wildavsky 1984: 159).

It is remarkable how these two insights of *Implementation* and the underlying research design became a model for the emerging field of implementation studies and at the same time a major target of criticism leading to a division in this field (that planted the seeds for its own decline). It is similarly remarkable how a scholar who was already renowned for introducing politics as a key dimension in policy analysis, and one who was a prolific individualist (though with a dose of egalitarianism), would become the key figure for a mechanistic, top-down view on implementation which advocates for centralization. I would argue that such a view is due to a limited reception of *Implementation* (or maybe reading that does not go far beyond the famous subtitle). The original wording concerning the opportunity to centralize—or to “simplify”—is far more cautious and sceptical than the typical “textbook” summary of the work suggests. Yes, they argue for keeping things simple, also concerning the implementation structure, but they do so on the basis of an analysis that brings to the fore the inevitable organizational politics of policy implementation—the “top-down” view is an analytical tool to shed light on these mechanisms.

IMPLEMENTATION: THE EMERGING AND DIVIDED FIELD

Implementation triggered the rapid establishment of a field of study, or was at least the first of a range of studies that explicitly used the implementation perspective to explore the fate of reform policies when put into practice. Other representatives of this first generation include Hargrove (1975), who stressed that implementation was the “missing link” of policy research, and Bardach (1977), who referred to the EDA project in the opening paragraph, emphasized the political dimension of implementation. Bardach considered implementation a continuation of politics and explored the various games being played between agencies and actors in implementations. The implementation paradigm rapidly diffused from the US to other countries, in particular to Europe. In the UK, the identification of the “limits of administration” (Hood 1976) was developed

in a more deductive way but it equally stressed that one-to-one implementation is virtually impossible since this would assume the existence and enactment of machine-like features that are simply not available in public administration of democratic societies. In Germany, a major research program on the implementation of policy programs was launched in the 1970s, and it similarly built on disappointing results of ambitious reform programs (Mayntz 1980, 1983; Wollmann 1980). Results of implementation were supposed to be shaped by the program design, the implementation structure, and the characteristics of the target population. Among the key insights of this research program was that implementation problems differ across the main policy instruments used. Implementation of regulatory policies comes with different challenges and has different Achilles heels than implementing financial incentives or information campaigns. An incentive program such as the Oakland grant and loan program for business comes with the difficulty of setting the level of incentives and the criteria for eligibility in a way that maximizes the impact of the tax money invested, i.e. avoiding both underutilization of the available funds and pure windfall gains for companies.

The common ground of these studies was that they focused on the implementation of individual programs formulated at some higher level and implemented further down. Following Pressman and Wildavsky's perspective, the main interest was on those factors leading to deviations from the original objectives. This perspective, charting the pathways of particular policies in a chronological way from the top down to the street level of implementation, was hence (critically) named the "top-down approach" to implementation study. Following the lead of Pressman and Wildavsky the two most prominent factors explaining implementation failures were interorganizational coordination problems and poor policy design based on flawed assumptions about cause-effect relations (see e.g. Hogwood and Gunn 1984). Implementation research developed into a growth industry with an ambition to develop a theory of implementation. One of these attempts at theory building was made by Mazmanian and Sabatier (1981). They developed a model intended to capture all relevant variables for explaining (successful) implementation, including 17 variables related to the character of the problem, the social and political context, and the capacity of the legislature to structure the implementation process. While coming to an overall less pessimistic assessment of the possibility of successful implementation than Pressman and Wildavsky, the conditions for successful policy implementation identified—sound program theory, clear directives, leadership skills, and support—remained broadly in line with the previous work.

At the same time, this common ground of implementation studies did not seem to capture the full reality of the implementation process. Other studies suggest that both negotiation processes between government agencies from different levels and between street-level bureaucrats and the policy-takers are constantly reshaping the original policy. For example, the specification of eligibility criteria for subsidy programs (according to the specific context of the labour market and the economic context in a particular region) actually *made* the policy. Moreover, this policy was made within networked relations between government agencies, unions and employers' associations, and companies. According to a study by Scharpf (1983) successful policy implementation of

subsidy programs depended on the quality of the network structures of labour agencies, with those societal actors providing the information base for the specification of the policy design at the implementation stage. The problematic assumption of top-down implementation perspectives, and today's conventional views about implementation problems, is that 1-1 implementation is actually desirable and a precondition for effective public policy.

At the same time, an opposing view on implementation developed. The “bottom-up” perspective suggested that, in order to capture the reality of policy, we need to start from the street level to explore how street-level bureaucrats and agencies of policy implementation deal with a network of programs, rules, and regulations that very often contradict each other and are underspecified and overly bureaucratic at the same time. Lipsky's classic analysis stresses the various coping strategies, applied by “street-level bureaucrats” to deal with the tension between demands of policy and the available resources (Lipsky 1980; see also Chapter 27 in this volume). The bottom-up perspective suggests that, in order to understand the success and failure of policy implementation, research has to consider policy as the outcome of implementation not as the input to the process. Hull and Hjern (1987) have developed a network analysis perspective on implementation research, starting from the substantial policy problem, as defined by the researcher, to map out the actors involved and their relationship. From the bottom-up perspective, the key to improving policy design lies in a process that Elmore (1979–80) called “backward mapping”—starting from the situation of the street-level bureaucrat, the social worker, teacher, health professional, their needs and constraints—policy research should follow different policies backwards to the point of origin in order to find out the point where decisions have been made that complicate matters for the street level. The controversy between “top-down” and “bottom-up” approaches has shaped the field of policy implementation since the late 1970s, and various attempts to develop synthesis models have been made. At the same time, implementation research lost its role as the leading subfield of policy analysis.

BEYOND IMPLEMENTATION: COORDINATION, COLLABORATION AND NETWORKS

In many ways, implementation research was losing its edge since it became increasingly obvious that it is impossible to develop a general theory of implementation. The attempt by Mazmanian and Sabatier already showed the futility of the ambition to develop a generic theory of policy implementation. One key problem for the development of such a theory lies in the difficulty of defining exactly what implementation is and where it starts—with the policy design where important parameters for implementation are set? Or only at the stage where administrative activities start? And how can it be taken into account that administrative activities do not merely put programs into practice,

but in doing so shape policy, by interpreting and specifying more general prescriptions in programs? How to deal with the problem that, during implementation, criteria for evaluation and the goals of programs change, making attempts to evaluate program success problematic? In a chapter added to the 1979 edition of *Implementation*, Majone and Wildavsky argue that implementation should be considered as a source of learning since the clear-cut evaluation of policy achievement comparing initial aims to actual achievements is problematic, given the evolving nature of policy-making during the implementation process.

The findings of implementation studies exploring these questions—in particular that politics and policy-making continue into implementation—contributed to something of a paradigm shift in policy research, namely the departure from the policy-cycle or stages model as the key heuristic device to approach the policy process (Sabatier 2007; cf. Jann and Wegrich 2007). Paul A. Sabatier developed his advocacy coalition framework by departing from the implementation, and thereby from the policy-cycle perspective, suggesting that policy implementation is an inherent element of policy-making. In this framework, activities of implementation are considered as parts of the wider struggle between advocacy coalitions over dominance in policy subsystems. With the integration of implementation into the overall dynamics of the policy process, the implementation perspective and respective questions, concerning successful implementation of policies got lost—at least in the debates about theories of the policy process (Sabatier and Mazmanian 1980). Despite attempts to revive implementation research, for example by applying more rigorous quantitative methods (Goggin et al. 1990), the intellectual trend in policy research shifted away from implementation research toward models that capture dynamics in the policy process across individual stages of the policy cycle, although agenda-setting became an increasingly important focus of policy research.

Despite this decline of implementation studies as the dominating subfield of public policy research, some of the key concerns raised by Pressman and Wildavsky continued to be influential. In particular, the notion of “coordination” is a focus for debates about how to align the activities of a range of organizations and actors in policy-making. In the US context, such debates explore the role of interorganizational coordination in policy implementation (see in particular Goggin et al. 1990). This work has been described as the third generation of implementation research and seeks to assess the effect of different interorganizational structures on policy implementation. In the European context, debates of coordination go beyond questions of implementation and involve issues of decision-making. Starting from the logic of policy-making in federal systems, a key concern was the logic of policy-making in systems of “joint decision-making” or “negotiation systems,” in which governments are limited in their capacity to act by the veto position of other actors, such as states in federal systems. This debate focused on the ability of bargaining systems to make welfare-maximizing decisions in the absence of hierarchical powers (Scharpf 1985, 1993). Overall, this debate is characterized by the acknowledgment of the limits of hierarchy. Not only is dispersion of powers, horizontally towards societal actors and vertically across levels of government, a fact that

cannot be ignored. More complex policy problems also require the active involvement of a high number of organizations from different sectors and different levels of government. And, while the “received wisdom” of *Implementation*—that the higher the number of involved organizations the higher the risk of failure—still resonates strongly in these debates, an alternative (maybe complementary view) emerged. This view stressed the potential of bargaining systems and networks to not only overcome potential roadblocks in decision-making but also to allow a flexible adaptation to complex problems (see Bowen (1982) for the development of such arguments contesting Pressman and Wildavsky). Informal networks spanning organizational boundaries are seen as key for such adaptive behaviour, which some consider to be superior to hierarchical organization when it comes to solving coordination problems (Chisholm 1989).

Such debates about the merits of non-hierarchical coordination, informal interaction, and emergent networks have partly been provoked by implementation studies showing that the behaviour of implementing agencies is better captured as a process of bargaining and negotiation, such as Bardach’s (1977) conception of implementation of a series of games between the different political and administrative organizations. Also, Pressman and Wildavsky’s brief discussion of hierarchy and bargaining as two different ways to achieve coordination foreshadowed this debate. Further research has expanded on the non-hierarchical character of policy implementation, administrative activity, and policy-making more widely. The notion of “co-operative administration” (Benz 1994), for example, highlights the tendency of agencies to engage in a bargaining type of exchanges with policy-takers even if a hierarchical enforcement of rules and regulation would be legally possible. In areas of regulatory policy implementation in particular, such as environmental policy or workplace health and safety, such tendencies have been identified. But also in the implementation of subsidy programs, engaging with the target population in a cooperative exchange has been identified as an important success factor. Factors leading agencies to adopt such conflict avoidance strategies include limited resources for hierarchical control and enforcement and the shadow of political power if formal powers are “excessively” used, for example, vis-à-vis business with economic significance or local political support.

What started out as implementation research has developed into an interest in the logic of collaborative administration and collaborative governance more widely. The development in the literature on (multi-level) governance has been used to directly engage with Pressman and Wildavsky’s work. Hupe (2010), for example, challenged the core claims of *Implementation* concerning “incongruent policy implementation.” However, in general, these debates connected to other debates and literatures beyond the domain of policy implementation and policy research more generally. The whole “collaborative governance” approach (Ansell and Gash 2008), for example, has developed links to debates in the field of democratic theory. This debate is interested in how the participation of affected citizens, i.e. the incorporation of their views, values, and preferences in the design and implementation of public policy, facilitates support for and hence success of policies. The “deliberative turn” in policy analysis takes this agenda forward to the design of deliberative forums of participatory or collaborative

governance. Consensus-oriented decision-making is seen as an alternative to both hierarchical and managerial styles of governance.

In sum, the departure of policy research from ambitious efforts to develop theories of implementation and the loss of implementation as a perspective that drives the debate have not resulted in a loss of interest in all key issues that have been raised by *Implementation*. While the idea of implementation as a linear process has been rejected, the problem of coordination has remained on the agenda of public policy and public administration research. And, with the departure from the (top-down) implementation paradigm it seems that the pessimistic view following almost naturally from the top-down implementation perspective has given way to a more nuanced understanding of the merits of non-hierarchical forms of coordination, although these studies are less interested in linking these assessments to policy outcomes and hence success/failure. The significance of the program theory for the (success/failure) of implementation as the second key insight from Pressman and Wildavsky's study has, however, not featured as high on the agenda of policy research. This might be collateral damage from the turn towards governance that stresses patterns of interaction between state and societal actors as more important than the policy design itself.

IMPLEMENTATION—OF A KIND

Of course, implementation remained an important concern of policy-makers and a key subject of applied policy research, i.e. contract research. In academic research outside the core academic field of public policy and public administration the number of implementation studies has actually grown since the mid-1980s (Saetren 2005; Lester et al. 1987). The top-down perspective, including the key variables identified by Pressman and Wildavsky—the complexity of the implementation field and the program theory, plus the clarity of goals—are the core variables that thousands of applied implementation studies refer to when assessing success or failure of policy implementation. In core public administration and public policy research, the bottom-up, or street-level perspective, remained one of the most fruitful areas of policy research, often combining a policy/public administration perspective with sociological or anthropological perspectives and methods. Studies in the field of welfare administration (Brodkin and Marston 2013) and prison management (Lin 2000) have transposed the bottom-up perspective into current times.

Some of the core questions raised by implementation (both the book and the field) have been pursued through the lens of other frameworks. For example, research on “enforcement” has flourished in the emergent interdisciplinary field of regulation since the late 1980s (Gunningham 2010). The key question pursued is what makes target populations comply with rules and regulations, such as environmental or workplace health and safety standards. In many ways, research on enforcement has taken over from policy research the interest in the implementation of public policy, but using a different

analytical apparatus and producing its own classic (see Lodge, Chapter 38 in this volume). Also in the field of public management, an interest in “implementation-like” questions emerged: responding to the increasingly widespread use of performance management tools in many countries, public management research has explored how such approaches to steering agencies and street-level bureaucrats—in fields such as health, welfare, or education—work (or not) in practice. Again, the very significant findings from this research had limited repercussions in policy research, which has departed towards exploring how policies develop in the context of the diversity of actors involved in policy-making. Another perspective in studying interorganizational implementation is based on the cultural theory approach that Wildavsky helped to transpose from the field of anthropology to social science more generally, including political research. Via the adaptation as a theory to explore different styles of public management (Hood 1998), the fourfold typology of grid/group cultural theory has been used to explore modes of control in interorganizational relations and policy implementation (Douglas and Wildavsky 1982; Hood et al. 2004; Lodge and Wegrich 2005).

In short, the concerns raised by *Implementation* are hardly forgotten, despite the decline of implementation as a (sub)field of policy implementation. Other issues, however, receive less attention, in particular how policy design (and program theory) are linked to policy implementation. While there has been a sparking of interest in policy design and instruments recently, not least fueled by the belief in the power of rational planning and design tools, such as randomized control trials, the nitty-gritty issues of administrative implementation do not feature in these debates.

CONCLUSION

Reflecting on *Implementation*, four characteristics stand out that make this case study a classic. First, it is the establishment of a field that is associated with the book. While some consider the path-breaking nature of the study (at least in part) a result of self-proclamation, there is no doubt that *Implementation* is a landmark study. Second, a pessimistic, or critical, view on the ability of government policies to achieve its objectives and actually influence societal or economic developments was introduced, and a view that did not rely on the grand forces of power but on bread and butter administrative issues. Third, *Implementation* shaped a perspective, but also coined terms that stayed with us, at least for a while. The “complexity of joint action” and in particular the “clearing points” are terms for which the study became famous. Fourth, the particular case and its detailed analysis in a case study that combines a narrative with deductive reasoning is a landmark character of the study. Rereading this classic, one discovers the huge potential of such a method that possibly would not survive the scrutiny of Ph.D. boards in many of today’s political science departments.

The key claim of this chapter is that indeed the questions raised by *Implementation* remain relevant today, but that policy research—in particular since the “governance

turn”—has become somewhat disengaged from the bread and butter type of issues raised by *Implementation*. Some of the issues and questions are addressed through different conceptual lenses, but others are somewhat out of sight. It makes little sense to lament the trajectory of academic debates, not least because of the limited nature of the top-down perspective and also the original study by Pressman and Wildavsky. But what the present author is somewhat envious of is the unique context that triggered this particular classic, not only in Berkeley/Oakland, California, but also across the Western world. It was the combination of a reformist political spirit with an (if naïve) openness to academic contributions and an intellectual development that had a strong manifestation in the creation of new schools for public policy that made *Implementation* possible. The more downbeat observer might add that such a public administration perspective only became relevant, and always was limited to, finding explanations for how and why things go wrong. However, the current author feels that this intellectual attitude is much needed in an age that is characterized by a disinterest in the bread and butter issues of public administration—either because governance is all about the non-hierarchical world of collaboration across (all sorts of) boundaries or because of the similarly delusional view that these issues are taken care of in the design of policy instruments, based on randomized control trials or other approaches to rational policy design.

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CHAPTER 25

OLIVER E. WILLIAMSON, MARKETS AND HIERARCHIES: ANALYSIS AND ANTITRUST IMPLICATIONS

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In 1975, Oliver Williamson published *Markets and Hierarchies: Analysis and Antitrust Implications*, a book that was to become a foundational statement of a new and influential way of understanding firms operating in markets. Over time, Oliver Williamson used this foundation to develop what is now known as “transaction cost economics.” The book itself is notable because it marked the first use of the term “New Institutional Economics” to describe this new way of understanding markets and hierarchies. For those of us interested in government operations, Williamson’s views are important if only because they have become a dominant perspective on when you should—and should not—contract out government operations. The impact of this viewpoint is impressive. Yet, one of this chapter’s contributions is to argue that all views on policy formation and implementation are eventually replaced.

The main purpose of this chapter is to make four general points about Williamson’s approach and the impact of his book. The first point is that the arguments made in *Markets and Hierarchies* are an interesting example of what policy analysts sometimes call “the politics of ideas.” The second point is that the roots of this approach lay in Williamson’s particular interest in antitrust policy, along with the peculiarities of his training. The third point is that the impacts of Williamson’s “markets and hierarchies” (M-H) approach are far-reaching. The fourth point is that Williamson’s transaction cost economics viewpoint may have a unique future in policy studies, if only because its heterodox nature means that other and alternative approaches are now viable due to the path-breaking nature of Williamson’s *Markets and Hierarchies*.

The starting point is what I will refer to as “the politics of ideas.” While much has been written on this viewpoint, we can summarize its implications in the following way. As Paul Quirk noted in 1988, “to a very great extent, the direction of policy change

depends on the state of opinion about the public interest. That opinion includes the values and attitudes of the mass public; the general ideologies of the attentive public and political elites; the more specific policy and program doctrines of practitioners in each area; and the pertinent theories and research findings of policy analysts and social scientists—including, not least, economists” (Quirk 1988: 35). The upshot of this position is that the viewpoints of elites, often academics, on what is quality policy are important explanations for the evolution of policy.

This viewpoint is notable if only because the original point of the “politics of ideas” theory was to help explain the deregulation movement in the United States. As reflected in the work of Stigler in 1971, economists had come to believe that regulation is the result of supply and demand: the supply of regulation as a function of the goals of politicians who have the authority to set policy, and the demand for regulation by some set of constituents (based on their policy preferences and lobbying/influence efforts). Of course, the way this came to be interpreted was that regulated industries demand regulation for themselves in order to construct barriers to entry that allow them to enjoy monopoly-like rents. Others who echoed and elaborated on this viewpoint included Peltzman (1976), Noll and Owen (1983), and others, although over time the views came to reflect the fact that reality was much more complex than portrayed in Stigler’s original formulation.

What then caused deregulation in the US? Through various studies, the answer emerged that it was probably some combination of market shifts, the activities of regulated industries who might have desired deregulation to become more competitive, and consumers mobilized to demand deregulation. Along with this, the “politics of ideas” viewpoint argued that politicians (elites) and economists had come to see deregulation as preferred if only because in their view regulation was intended to protect the rents of regulated interests.

Williamson’s *Markets and Hierarchies* is part of the politics of ideas, as are all theories and empirical studies done by researchers interested in human behavior. However, Williamson’s work has come to play several important roles, a position certified when he won the Nobel Prize in economics in 2007. This chapter documents those roles.

In the next section I describe the genesis of this research agenda, dating to Williamson’s time at Carnegie Mellon University and with the US Department of Justice antitrust division. In the third section I describe the project itself as represented in the book. Then I turn to the impact of this research agenda across multiple fields. Finally, I return to Williamson’s research as part of the long-term development of a politics of ideas about firms in markets.

THE ROOTS OF THE PROJECT

There are three key roots of Williamson’s “markets and hierarchies” approach and its analogue, transaction cost economics. These three routes are represented by his time at Carnegie Mellon University, his experiences with the US Department of

Justice Antitrust Division, and the pioneering research of Ronald Coase and John R. Commons.

Oliver Williamson earned his Ph.D. from Carnegie Mellon University in 1963, later noting that this was “by far the most important event in my intellectual development” (quoted in Kelly 2010). To be clear, during this time he was trained by future Nobel laureates such as Herbert Simon, Franco Modigliani, Merton Miller, and Robert Lucas. Fellow graduate students at CMU Finn Kydland and Edward Prescott also won the Nobel Prize. After his time at Carnegie Mellon, he first served on the faculty at the University California at Berkeley, then at the University of Pennsylvania, and then Yale University, before returning to Berkeley. At Berkeley, his appointments include professorships in the business school, the department of economics, and the law school.

What is unique about this set of experiences is that Williamson fits between major lines of research in multiple fields that study the role of firms and other hierarchies in markets. On one hand, his research agenda has its roots in the work of people like Herbert Simon, a political scientist by training but a polyglot in terms of his ability to command multiple research literatures in numerous fields all related to the long-term development of organizations. This experience gave Williamson a toolkit affiliated more with the sociological literature on organization theory.

On the other hand, Williamson also received strong training in the use of mathematics for modeling social and organizational behavior. Until Williamson, economists had largely seen firms as “black boxes”—as production functions that served mostly as a building block for more important models of market operations. Williamson understood the value of mathematical modeling, if only for illuminating unexpected outcomes, while he also understood that firms were more than just black boxes. As he later noted, these experiences made him “cognizant of the complexities associated with large organizations and attentive to imperfect information and conflicting incentives” (Shapiro 2010: 141).

Williamson spent 1966 and 1967 at the Antitrust Division of the US Department of Justice as Special Economic Assistant to Donald Turner, the Assistant Attorney General for Antitrust from 1965 to 1968. Turner has come to be seen as a leader in giving antitrust enforcement sounder economic foundations. Williamson has called this the “transition years” of antitrust enforcement (Shapiro 2010: 138).

While the purpose of this chapter is not to recount the history of antitrust enforcement, this history plays an important role in the evolution of *Markets and Hierarchies*. The short version is that the conventional wisdom at that time was that the purpose and effect of many vertical practices was actually to enhance market power over firms and to allow them to construct entry barriers. This had been seen for some time as a myopic viewpoint. Ronald Coase has noted “one important result of this preoccupation with the monopoly problem is that if an economist finds something—a business practice of one sort or other—that he does not understand, he looks for monopoly explanation. And as in this field we are very ignorant, the number of un-understandable practices tends to be very large, and the reliance on monopoly explanation, frequent” (Coase 1972: 67).

It was in this context that Williamson came to study economics. He focused originally on barriers to entry and more broadly, antitrust economics. The main questions in antitrust economics at that point in time centered on the effects of different market structures on innovation and/or economies of scale. In 1968, Williamson invented what is now seen as the “naïve trade-off model” to help people better understand why horizontal mergers should be allowed. He also studied vertical mergers and vertical contracting arrangements. For instance, two articles written in the early 1970s were particularly important (Williamson 1971, 1973). Specifically, he was very influenced by a case that he had engaged with during his time at the Department of Justice. This case centered on the operations of the Schwinn Company and its franchising arrangements for the sale of bicycles in diverse locations. The case had made its way up to the United States Supreme Court, and Williamson had come to disagree with the Supreme Court’s approach to solving this case. He later noted: “as of 1966, however, my efforts to place a more favorable construction on Schwinn’s restrictions and to reshape Schwinn got precisely nowhere ... I viewed contracting organization from a combined economics and organizational perspective, which was a byproduct of my (unorthodox) training at Carnegie” (Williamson 2003: 64). Yet, nine years later, he named new institutional economics and laid the foundation for transaction cost economics. Indeed, his ideas about horizontal mergers were adopted as policy in 1997—thirty years later (Shapiro 2010).

As noted, Williamson’s work was not the first to identify firms as a missing piece in our analytic understanding of the behavior of markets. In 1937, Ronald Coase had sought to provide a rationale for the existence of firms. In essence, the question was, if the market is perfect for exchange, why do firms exist? Coase’s answer was that market participants incur costs during exchange. He introduced the concept of transaction costs as a way of counting up the negative side of required behaviors that market participants incurred when doing deals in the market: the costs of “preparing, entering into, and monitoring the execution of all kinds of contracts,” as well as costs of implementing mechanisms inside firms that make them work.

While Coase’s work was truly path-breaking, Coase was not the only economist worried about transaction costs. Commons also sought to understand the behavior of organizations in economic environments. Commons was one of the founders of the field of industrial relations—almost an early labor economist in spirit. His goal was to establish (along with other economists like Thorstein Veblen) a branch of economics he called institutional economics. He sought to construct an approach to economic analysis that would build in roles for social, political, and economic organizations in a model of economic events. In this approach, called by some “old institutional economics,” the analysis emphasized institutions in order to criticize conventional economics, mainly because neoclassical work ignores the non-economic environment that helps define how individuals make decisions (Kaufman 2007).

Neoclassical economic theory by and large assumes zero frictions and complete contracts. Yet, as Commons and others noted, market economies depend explicitly on the state’s decision to permit private property. In essence, the existence and size of firms depend in part on the power of ownership in the costs of transferring ownership. To

better understand these dynamics, Commons invented the concept of “transaction.” He described a range of different transactions, some of which are more interesting than others for the purposes of this chapter. For instance, a particular type of transaction he found useful was the “managerial transaction.” In these transactions firms incur costs naturally due to the process by which they hire and fire individuals. As Kaufman notes, for Commons “when the firm purchases labor, in a world of incomplete contracts it gains a property right only to an ill-defined and highly variable amount of labor power embodied in a human being” (2007: 6). Effectively, firms rely on the fact that contracts are incomplete in order to outperform markets.

In sum, Commons gave us transactions. Coase gave us transaction costs. The two views combined to help Williamson develop a theory of transaction cost economics that helped us better understand the concept of contract. His understanding of contract was informed by a rich tradition of interdisciplinary research found at Carnegie Mellon University, accentuated by a practical policy interest dating to his time at the US Department of Justice.

THE *MARKETS AND HIERARCHIES* APPROACH

This book, published in 1975, was a foundational statement for both new institutional economics and transaction cost economics. Essentially, the book is about exchange and trust relationships (Ouchi 1977). In a nutshell, both organizations and markets are useful mechanisms for exchanging goods and services. Yet, there are differences between the two institutional arrangements. For instance, consider markets as an institutional arrangement. Markets really do not rely on trust per se; markets work when parties to a transaction can determine a fair price. In such instances, if the parties find it difficult to get information, or if the information they obtain is not believable, markets may be deficient mechanisms for exchange. More importantly, when markets are marked by uncertainty, or small numbers of competitors, or if (in Williamson’s view) the deciders are “boundedly rational,” then prices do not play the important role of serving as “sufficient statistics” for those interested in exchanging goods and services. In Williamson’s view, if people are “opportunistic with the guile,” and markets lack key characteristics, then alternative mechanisms of exchange may perform better than markets.

To put the problem in transaction cost terms, perhaps the parties to an exchange cannot agree on a price, but unfortunately the expenses of figuring out what will happen in the future, or whether they can trust each other, may be so high that these other affiliated costs associated with the exchange prevent the exchange from occurring at all.

It is in these situations that hierarchies—what others sometimes call “internal production”—may perform better than the markets themselves. In a hierarchy, two parties have relatively few incentives to lie to one another, or at least perhaps

surveillance is easier, or maybe people are simply more cooperative. Any of these events will reduce the cost of exchange.

This is exactly how Williamson comes to justify vertical mergers. Recall that his original interest was antitrust policy, and a significant portion of *Markets and Hierarchies* is meant to justify certain market practices in evidence and under fire by traditional economics. These market practices are justifiable (defensible) once the conditions are clear that certain other market practices are not efficient because of the presence of transaction costs.

The book has 13 chapters and is of unusual format for a book considered highly influential in general public policy circles. The first two chapters are general in their focus and impact. Chapter 1 describes the “new institutional economics” approach. Chapter 2 fleshes out Williamson’s “organizational failures” framework. The first two chapters are a synthetic statement of the “M-H” approach.

This argument block is of general importance for those focused on policy implementation by varieties of organizations. The organization failures framework posits:

- (1) Markets and firms are alternative instruments for completing a related set of transactions; (2) whether a set of transactions ought to be executed across markets or within a firm depends on the relative efficiency of each mode; (3) the costs of writing and executing complex contracts across a market *vary with the characteristics of the human decision makers who are involved with the transaction on the one hand, and the objective properties of the market on the other*; and (4) although the human and environmental factors that impede exchanges between firms (across a market) manifest themselves somewhat differently within the firm, the same set of factors apply to both. (Williamson 1975: 8)

This leads to a comparison of two kinds of limits: the limits within organizations to their ability to facilitate transactions, and the failures of markets. There are two related structures to compare: market structure and internal organizational structure.

In this framework, though, Williamson starts with the assumption that “in the beginning there were markets” (1975: 20). But he moves forward from this assumption by engaging layers of attributes for those people populating the models and environment in which they interact: bounded rationality, uncertainty, opportunism, and small numbers. He then adds two extra conditions: information impactedness (almost a state of asymmetric information) and atmosphere (the possibility that the attitudes of market participants are not separable—that they want similar things). These attributes and conditions combine to define the organizational failures framework—a statement of how environmental and human factors combine—together with the degree of information impactedness, within an atmosphere—to define the various conditions for exchange.

Following these two chapters, Williams spends two additional chapters describing organizations and their relative abilities to control those who work for them. Chapter 3 focuses on why the organizational building blocks of peer groups and simple hierarchies evolve. Chapter 4 turns to the superior–subordinate relationship in order to

delineate different ways (“modes”) of labor contracting and collective organization, and their efficiency characteristics.

In some ways, this section of the book is a refresher on organization theory for economists who happen to read the book—or a nod to organization theorists who want to see the relations between markets and hierarchies. Yet, Williamson covers several aspects of superior–subordinate relations in ways that presage significant changes to the structural analysis of organizations. For instance, Williamson spends substantial effort detailing what he calls “individualistic bargaining models,” which effectively lays out the case for the analysis of contract: of the differences and similarities among contingent claims contracts and sequential spot contracts, and the differences between either of those contracts and “the authority relation.” As Williamson puts it “The upshot is that none of the above contracting schemes has acceptable properties for tasks of the idiosyncratic variety” (1975: 72). So, for him it is natural to turn to the organization—“internal labor market structures”—as potentially optimally efficient. The broader point is that economists should consider the possibility that organizations can outperform (be preferred to on efficiency grounds) markets.

Up to this point those generally interested in policy, policy implementation, bureaucracy, or political organizations may find the points and emphases of *Markets and Hierarchies* useful and illuminating. But in Chapter 5 Williamson turns to the main point of his concern: the structure of markets and the structure of organizations as a synthesis. Chapter 5 turns to vertical integration and intermediate product markets, a theme that continues into Chapter 6. Chapter 7 also expands on vertical integration, but now through the lens of the limits there are to firm size. Chapter 8 turns to classical questions in the study of firms: multidivisional structures, optimal divisionalization, and peculiarities of real firms in real markets like the M-form; Chapter 9 continues this theme with an extended discussion of conglomerates.

Some might consider these layers an homage to classical questions in the organization theory of firms, but the better perspective is to consider Williamson’s points in Chapter 1 about the differences between the M-H framework and an important “touchstone” in the field: the “structure-performance-conduct” paradigm (e.g. see Chandler 1962). In the paradigm, the assignment of transaction to a particular structure (broadly, to organizations or to markets, and narrowly, to specific types of organizations) is “mainly taken as a datum” (Williamson 1975: 8). In contrast, in *Markets and Hierarchies* Williamson seeks to derive the conditions under which transactions are allocated to organizations instead of markets, and then across different types of organizations.

The upshot of this is that Williamson spends significant time in the central core of the book discussing structures and allocative choices that are of little interest to most working on topics in policy, implementation, bureaucracies, or other political organizations. Chapters 10, 11, and 12 continue this fixation by turning to questions largely relegated to antitrust policy. In Chapter 10 market structures are discussed with reference to technical and organizational innovation. Chapter 11 elaborates on monopolization as a consequence of having “dominant firms.” Chapter 12 states the problem

directly: “[the position that oligopoly is just as damaging as monopoly] fails to make allowance for the advantages of internal organization as compared with interfirm contracting in adaptational respects, and it gives insufficient standing to the differential incentives and the propensity to cheat that distinguish internal from interfirm organization” (Williamson 1975: 234). In Chapter 12, oligopoly is defined as a problem of contracting. By the time Williamson decides to wrap up the argument, he is dividing the contributions of his own work into two types: the elaboration of a transactional paradigm that focuses on organizational failure, and specific antitrust implications for those worried about such things.

Three themes warrant emphasis at this point. First, Williamson builds a theory from first principles—although some of those first principles are less well-known in economics than in other fields (like organization theory). Second, this theory is marked by its emphasis on organizations and markets as choices in a design space defined by contract variations. Third, this emphasis helps Williamson better understand the real choices of real people to select different transactional forms, some of which neoclassical economics defines as illegitimate. Together these themes portray a book of stark contributions, all intended for different audiences, some of which overlap.

IMPACTS OF *MARKETS AND HIERARCHIES*

Here are a few simple impacts of the “markets and hierarchies” and “transaction costs” approaches as represented in this book. This book is the first statement of New Institutional Economics as a viewpoint on markets and their long-term operation. This book served as the birth of the modern transaction cost economics viewpoint, where the argument was made using the same methods followed by modern neoclassical economics. And of course, the book birthed the M-H approach: “markets and hierarchies.”

The purpose of this section is to describe the impacts of the book in broader context. First, I look at the impacts of the book’s publication in the near term (the “immediate” effects). Second, I describe the longer term effects, as represented by those who have populated the transaction costs “space” in academic economics. Finally, I focus on a specific impact of this literature for those interested in public policy and administration: the “contracting out” movement in government, both in the US and around the world. In this last section, I describe ways in which Williamson’s work has enlightened and burdened the application of “markets and hierarchies” to the real decisions of real public decision-makers.

But before turning to these broader impacts, I first want to make clear the most proximate impact of this book in terms of the application of economic theory to practical policy problems: the effects of *Markets and Hierarchies* on antitrust policy. As noted, a full discussion of antitrust policy is beyond the scope of this chapter, but suffice it to say that Williamson’s main concern was talking to those interested in antitrust, and his impact in that realm has been great. This is a point largely ignored in most academic

treatments of transaction cost economics in affiliated disciplines. Shapiro notes that Williamson's impact is "enormous" (2010: 138), and Joskow argues that Williamson's framework is significantly powerful for our understanding of antitrust remedies, antitrust law's scope, and important practical decisions such as value of antitrust remedies over other regulatory enforcement mechanisms (2002). If anything, as Shapiro notes, those who work in this arena "owe him a great institutional debt for helping to build the economics capability of the Antitrust Division and thus put antitrust law and antitrust enforcement on a firmer economics foundation" (2010: 145). This contribution of this book is somewhat neglected because of antitrust policy's technical nature, but as we now recognize, ideas like those of Williamson often help change the world.

But the more valuable focus in this section is on the immediate, the long-term, and the tertiary impacts of *Markets and Hierarchies*. For instance, in his review of this book, William Ouchi, an eminent business school professor with a lifelong interest in various management strategies, characterized the contribution in the following way: "No student of organizations should fail to read this book. Never mind the title; it is about organizations, and it is readable, clear, and powerful. . . . In a sense, this book obsoletes the works by March and Simon, by Thompson, and by anyone who has attempted to understand the implications of bounded rationality, technology, and uncertainty for the structure and functioning of organizations" (Ouchi 1977: 540). Moreover, "With *Markets and Hierarchies*, the promise first made by Chester Barnard and given impetus by the genius of Herbert Simon can truly be said to be reaching fruition" (Ouchi 1977: 544). Nutzinger, evaluating the book for *Kyklos*, noted "Among new and old paradigms in economic theory, Williamson's book . . . is one of the most promising approaches toward a broader understanding of economic activities" (Nutzinger 1977: 376).

Of course, not all early comments were laudatory. For instance, one commentator noted that the book consisted of 13 chapters, only two of which had not appeared in some form before publication in that format. Also noted was the "ghastly mangled jargon-laden English in which it is written" (O'Brien 1976: 620). Samuels in an insightful analysis in the *Journal of Economic Literature* called into question some of the key building blocks in the analysis: the lack of foundations for the organizational failures framework, the lack of a discussion of organizational objective functions (or goals), and notably a lack of consideration of "Albert Hirschman's work on organizational slack, exit, and voice" (Samuels 1977: 138). This last point is particularly interesting, if only because Williamson himself recognized the power of Hirschman's "exit, voice, and loyalty" (EVL) framework for understanding the actions of individuals inside organizations (the main topic of his chapter on peer groups): "The authors of EVL and MH plainly believe they are onto a new way of organizing a wide set of social science phenomena" (Williamson 1976: 372).

The longer term impact of *Markets and Hierarchies* is clearer. At a minimum, the book named "New Institutional Economics" and laid the foundation for "transaction cost economics", in its modern form. In 1986, R. C. O. Matthews in his presidential address to the Royal Economic Society claimed that "the economics of institutions has

become one of the liveliest areas in our discipline” (Matthews 1986: 903). Yet it is also clear that *Markets and Hierarchies* played a role in building this research agenda only because other, affiliated frameworks emerged that helped turn academic attention to the mechanisms of governance.

For instance, Douglass North’s accomplishments, also lauded with the award of a Nobel Prize, formed the basis for multiple literatures all concerned with the use of economic theory and methods to understand economic and institutional change, as well as the effects of institutional choices on long-run economic growth. “Institutions matter” is the fundamental lesson drawn from North’s 1990 book *Institutions, Institutional Change and Economic Performance*. Williamson’s co-winner of the Nobel Prize, Elinor Ostrom, made singularly important contributions in her *Governing the Commons* (1990). Williamson, in his own statement on the state of New Institutional Economics, included “Kenneth Arrow, Friedrich Hayek, Gunnar Myrdal, Herbert Simon, Ronald Coase, and Douglass North,” but also other figures such as Armen Alchian, Chester Barnard, Richard Cyert, James March, Alfred Chandler, and a number of names from legal theory literatures on contract (Williamson 2000: 600).

I will focus on one particular long-term impact of the approach Williamson formed in this book, and that continued throughout his career. One key attribute of *Markets and Hierarchies* is that Williamson sought to develop an approach to understanding the Coasian claim about the differences between spot contracts and internal production using the emerging language of a special sort of economics. Specifically, he sought to build an approach that would be accessible to those more interested in building mathematical models than the legal language of contract. As Kreps notes, the key development was the emergence of an economics of information that led to the analysis of incomplete contracts using a formal apparatus (Kreps 1996).

Specifically,

Initial developments in information economics were around five years old, the revelation principle was nearly contemporaneous, and the widespread embrace of the language of game theory for dealing with dynamics, small numbers, and incomplete information was about five years in the future. Without these tools to work with, it was perhaps inevitable that Williamson’s theory would be rendered without the mathematical apparatus of mainstream theory. But as those tools worked their way into the mainstream, it was equally inevitable that the ideas set forth in *Markets and Hierarchies* would be reworked and further developed in symbols. (Kreps 1996: 561)

But Kreps goes on to say that the elaboration of Williamson’s views using the modern language of economics has largely been incomplete—mostly because that language cannot easily address the theories embedded in the book. In one important way, then, Williamson’s arguments became a natural point of application for the tools of modern information economics, so economists like Oliver Hart and Jean Tirole were offered a place to make strong and immediate contributions. Just as Williamson’s work

benefitted from the development of tools, so did the tools benefit from Williamson's pointing attention to the problem of contract.

The final point is special attention to the broader impact of Williamson's views on assets and contract, and their importance to our modern understanding of organizational arrangements within and among firms. Put simply, Williamson told us that certain arrangements among firms make sense because the nature of the decision and environment provided incentives to not rely on traditional market transactions. He elaborated on this defense of "non-markets" over time, producing transaction cost economics along the way. Firms "contracted out" only under special conditions, and if they did, they were careful about the nature of the contracts they wrote.

In modern public administration, we are naturally concerned about the choice of governments to also "contract out" services. Of course, governments have long chosen to "buy" instead of "make" all kinds of products—from pencils to jet fighters. Yet, over time, more services are contracted out and more government "employees" are actually contractors. And along the way, the field of public administration has struggled to understand the "why government contracts out" and "should government contract out" questions.

In practice, there are many reasons why governments contract out everything from health services to job training. Williamson's work has come to play an important role because *Markets and Hierarchies* is where we started in trying to understand key attributes of decisions like asset specificity. The field lives in Williamson's shadow now.

Unfortunately, this was not the point of Williamson's original work. In his Nobel Lecture he spends little time discussing this evolution, just as he also spends little effort in most of his work, for one simple reason: he started down this road because of an inordinate interest in the conditions under which antitrust law should be applied to firms who choose alternative governance arrangements over the use of spot contracts in traditional markets.

Over time, the work of Williamson has reached well beyond its original intent—for good and bad reasons. On one hand, the M-H paradigm and its successor, transaction cost economics, have incredible explanatory power for broad classes of social phenomena. On the other hand, sometimes that power loses resolution because the institutional frameworks are different and have alternative purposes. It is useful when examining such cases to go back to the beginning to understand the theory's genesis.

CONCLUSION

The lasting legacy of *Markets and Hierarchies* is the genesis of the transactions cost paradigm, which has fundamentally changed the language of public policy studies. At a variety of levels—from the modeling of policy processes to the calculation of optimal

policies for areas like antitrust—the frameworks introduced in this book have altered the course of policy.

This legacy was assisted by the development of other modeling tools and theories from an array of actors throughout the social sciences and legal studies. Most notably, formal modelers chose the contracting paradigm as a unique opportunity to explore the implications of conditions like asymmetric information for rational choice. This was a marriage of topics and techniques that has borne great fruit.

Unfortunately, applications of these frameworks in areas outside antitrust often ignore the basic roots of the enterprise, although these extensions also offer opportunities for expanding the framework's range of application. But these moves also suggest a fundamental dynamic. Just as Coase and Common's ideas required Williamson's work for achieving greater range of application, so did Williamson's views require the work of formal modelers. At some point another view will emerge that eclipses the transaction cost framework—probably one derived from new microfoundations of choice found in the neural or behavioral economics paradigms now under development.

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CHAPTER 26

HUGH HECLO, “ISSUE NETWORKS AND THE EXECUTIVE ESTABLISHMENT”

VOLKER SCHNEIDER

INTRODUCTION: HUGH HECLO

THE article on “issue networks” is Hugh Heclo’s most well-known publication, already a classic in policy analysis and public administration (Heclo 1978). It has been translated into Spanish (Heclo 1993) and it has been republished in some text collections on government and public administration (Young et al. 1993; Stillman 2009). In a way, it summarizes some of the books that Heclo wrote in the early 1970s. His remarkably innovative insights into modern politics cannot be understood without a short look into his biography.

Hugh Heclo grew up first in Ohio and later Washington, DC. In the American capital, his mother worked at various governmental institutions, so even as a teenager he became acquainted with the inner face of American national politics. He first enrolled at George Washington University (where he received his bachelor degree) “living in a dorm three blocks from the White House” (Pffiffer 2007). He received his master’s degree from Manchester University in England and Britain was also where he held his first teaching job at the University of Essex. He also worked at that time as a research associate for Aaron Wildavsky in a project on politics and policy-making in Whitehall. The results of this study became widely known through his co-authored book on *The Private Government of Public Money: Community and Policy Inside British Politics* (Heclo and Wildavsky 1974). This book, presented in a marvelous writing style, is one of the first studies in which the relational perspective in government and public administration is emphasized (Parry 2003). The book stresses the importance of personal

relations within governmental administration as social capital without using this specific term. Heclo and Wildavsky pointed to the complexity of "ties that bind" and the "criss-crossing networks" in the British executive community, where relationships of mutual trust and confidence were conceived as the most pervasive feature in explaining British policy-making (1974: 14).

Around the same time, Heclo was doing his Ph.D. in political science at Yale University using a study of the evolution of the British-Swedish welfare states. His dissertation was published as *Modern Social Politics in Britain and Sweden: From Relief to Income Maintenance* (Heclo 1974b). Nowadays, this book is considered a classic in policy analysis, as it was one of the first studies to use cognitive concepts in the explanation of policy processes emphasizing interpretation, deliberation, and learning. A famous quotation of this book is "Governments not only 'power' (or whatever the verb form of that approach might be), they also puzzle. Policy-making is a form of collective puzzlement on society's behalf; it entails both deciding and knowing" (1974b: 305). The book is also an excellent example for a combination of actor constellations and historical influences in policy-making and administrative structures. It received the Woodrow Wilson award from the American Political Science Association for the best book published in the United States during 1974 on government, politics, or international affairs, and was republished with a new introduction in 2010 by the European Consortium for Political Research in its Classics Series (Heclo 2011).

Following his Ph.D. Heclo became a research fellow at the Brookings Institution, one of the oldest and most respected think tanks in Washington where he started a study on politics and policy-making at the federal level in the US. "Because I had just spent almost two years talking and writing about how Whitehall worked, it was a no-brainer to think of trying to do the same in Washington. So my idea was to study how, in real life, political agendas get translated into public policies in the executive branch." (Pfiffner 2007). The project produced a series of new findings that were published as *A Government of Strangers: Executive Politics in Washington* (Heclo 1974a). This book won a prestigious award presented by the National Academy of Public Administration for outstanding contributions to the literature of public administration. Core findings of this project were also published in Heclo (1977). In *A Government of Strangers*, Heclo described American government as a highly differentiated and fragmented system. In contrast to the centralized and highly integrated British system in which everyone knows everyone else, the US system essentially appeared as "a government of strangers," which was deeply divided into largely autonomous technical and organizational subdomains, as the book title aptly summarizes (Heclo 1974a).

A few years later, Hugh Heclo took up professorships at Harvard University and George Washington University. He is a distinguished Professor of Public Affairs at George Mason University in Washington. In 2002, Heclo was awarded the American Political Science Association's John Gaus lifetime achievement award. He is also an elected member of the American Academy of Arts and Sciences and the National Academy of Public Administration. His most recent books are *Christianity and American Democracy* (Heclo et al. 2007) and *On Thinking Institutionally* (Heclo 2008).

THE ISSUE NETWORKS CONCEPT

The article “Issue Networks and the Executive Establishment” integrates four analytical perspectives

- An actor-centered or agent-based dynamic perspective, in which explanations are not primarily drawn from social and political macro-structures, but rather on the interaction of individuals and organizations.
- A relational perspective in which the positive and negative ties, by which policy-makers are bound to in the policy process, are studied in further detail. What is new about this perspective is the emphasis on middle-men and brokers.
- A cultural or cognitive view in which the analysis is focused less on the material interest of policy actors, but rather on their intellectual involvement. This new perspective concentrates on the various ways in which actors understand and interpret their situations, and how they learn from personal experience and the experiences of others.
- A long-term perspective in which recent changes and policy shifts are contextualized in terms of enduring policy developments. This view also emphasizes lasting trends and the embeddedness of large-scale social and political structures.

Essentially, the issue network concept summarizes the most important empirical findings that Hecló documented in his book *A Government of Strangers* at that time. In this book, he describes the American executive establishment as a vertically and horizontally differentiated and highly fragmented system in which it would be useless to look “for the few who are powerful” as was done in some elitist or instrumentalist views of the modern state in that period. Hecló contrasted his description of the American situation with the then-popular concepts of “iron triangle” and “subgovernment”. In these theoretical approaches, policy-making is largely shaped by a small and stable circle of Congressmen, bureaucrats, and interest groups. In contrast to this “disastrously incomplete” view, as he called it, Hecló employs the concept of “issue networks” to point to the highly intricate and diversified webs of influence by which modern American policy-making is shaped. “Issue networks . . . comprise a large number of participants with quite variable degrees of mutual commitment or of dependence on others in their environment; in fact it is almost impossible to say where a network leaves off and its environment begins.” (Hecló 1978: 102). Participants in these fluid networks are officials and organizations from the governmental and nongovernmental sectors. The network affiliations among participants are rather loose and fluctuating, since participants move in and out constantly. Hecló describes the typical participant to be “neither a craft professional nor a gifted amateur, the modern recruit for political leadership in the bureaucracy is a journeyman of issues” (p. 106) It is a wandering craftsman who moves from one committee or policy issue to another to sell its advice and competences, and to gain new experiences. The primary interest of network

members is intellectual or emotional commitment, whereas material interests are rather secondary, and the major criterion for access and inclusion is a reputation of expertise on particular policy issues or areas.

With respect to the long-term and embeddedness perspective, the article also provides for an evolutionary or developmental explanation in the emergence of a new phenomenon in which several long-term trends interact.

A major driver for this socio-political change is policy growth and policy accumulation, the intervention of governments in an ever-broadening spectrum of policy areas. The most obvious indicators for these transformations are not only the growth in governmental spending, but also an expansion and increase of regulations in increasingly more issues and policy areas.

A closely related concept is administrative growth and differentiation, a long-term expansion process of public and semi-public bureaucracy at all political levels. Public administration increased in local governments, state governments, and in particular, at the federal level. As is common in bureaucracies, growth implies increasing vertical and horizontal differentiation, i.e. layering and specialization. This in turn breeds more heterogeneity. New organizational species emerge. In this context Hecló refers to administrative middlemen, intermediary organizations, third-party payers, consultants, contractors, and many others. Consistent with a long tradition of fragmented and decentralized administration, this process would lead to a "fraying at the center," which is the subtitle of the article.

A parallel trend is the growth and spread of interest groups which is amplified by increasing state intervention. Hecló observed a "politicization of organizational life throughout the nation" (p. 89) A largely non-intended effect of policy-making and implementation is the proliferation of hybrid interests that emerge around the "differential effects of these policies" (p. 96) A growing number of private and semi-private organizations and issue groups will mobilize. Hecló uses a very figurative language and speaks of the "blossoming of policy participants and kibitzers" (p. 96). The growth of law firms in Washington is seen as a highly visible indicator. The increasing number of private organizations influencing and participating in public policy processes implies that the boundaries between private and public authority become increasingly blurred. "Policy making is becoming an intramural activity among expert issue-watchers, their networks, and their networks of networks. In this situation, any neat distinction between the governmental structure and its environment tends to break down." (pp. 105–6)

A corollary of these trends is the increasing relational complexity within the networked policy-making arrangements. More and more heterogeneous actors imply increasingly diverse relations. Hecló uses a gorgeous analogy to describe this development:

With more public policies, more groups are being mobilized and there are more complex relationships among them. Since very few policies ever seem to drop off the public agenda as more are added, congestion among those interested in various

issues grows, the chances for accidental collisions increase, and the interaction tends to take on a distinctive group-life of its own in the Washington community. One scene in a recent Jacques Tati film pictures a Paris traffic circle so dense with traffic that no one can get in or out; instead, drivers spend their time socializing with each other as they drive in endless circles. Group politics in Washington may be becoming such a merry-go-round. (p. 97)

The complex picture of multiple relations and dynamic processes is summarized as a “loose-jointed play of influence” (p. 102). Relational complexity not only exists between policy actors, but also between policy areas. Differentiation and proliferation of new policy areas also increase interdependence and its related unintended effects:

The growth and spread of policies also increases the likelihood of multiple, indirect impacts of one policy on another, of one perspective set in tension with another, of one group and then another being mobilized. This sort of complexity and unpredictability creates a hostile setting for any return to traditional interest group politics. (p. 123)

A logical consequence of all of these developments is the growing importance of specialized policy knowledge. Increasing organizational complexity and specialization yield a specific impact on the cognitive dimension of the policy process. Policy specialists who understand the complexity of issues and the relational complexity of the Washington environment are becoming more and more appreciated. A generalized rating for this appreciation would be “recognized reputation”:

More than mere technical experts, network people are policy activists who know each other through the issues. Those who emerge to positions of wider leadership are policy politicians-experts in using experts, victuallers of knowledge in a world hungry for right decisions. (p. 103)

In this context, Hecló stresses that reputation and prominence in the policy process depend little on bureaucratic evaluations of objective performance, on an individual’s decision-making power, or on ratings in the general public, but rather more on mutual esteem.

What matters are the assessments of people like themselves concerning how well, in the short term, the budding technopol is managing each of his assignments in and at the fringes of government. (p. 107)

In a relational perspective, these networks are systems of distributed information processing in which policy intermediaries are mobilized to deliver specialized policy knowledge.

Knowing what is right becomes crucial, and since no one knows that for sure, going through the process of dealing with those who are judged knowledgeable (or at least continuously concerned) becomes even more crucial. Instead of power

commensurate with responsibility, issue networks seek influence commensurate with their understanding of the various, complex social choices being made. (p. 103)

Issue networks are conceived as knowledge-sharing groups who deal with particular public policy areas and are skilled in handling certain complex policy issues. Network members have a common base of information and a common understanding of policy problems and their functional logic. However, common policy knowledge does not imply agreement on issues or convergence of interests. Issue networks may or may not transform into shared-action groups or shared-belief groups. Networks consist of people who regard each other as knowledgeable and relevant in a policy discourse in which issues are defined and redefined, Evidence is contested and weighted, and alternative policy options are envisioned and evaluated. Issue networks thus have an important deliberative function: "Participants are not just ambassadors from clearly defined interest groups and professions" (p. 117). Interests and policy positions are not given and stable, but are bargained and debated. Issue networks provide a common framework for discussion, a common language, and a mutually familiar frame of argumentation and perception of the major societal issues. In Hecló's view, network members are a new species of policy-makers who differ a lot from "old party" politicians. He writes:

today's political executive is likely to be a person knowledgeable about the substance of particular issues and adept at moving among the networks of people who are intensely concerned about them. (p. 116)

Issue networks have important functional advantages compared to traditional American policy-making arrangements in that they are compatible with the growing importance of issue-based politics in contrast to traditional party identification. They provide important coordinating functions between policy-makers within the Congress and the executive branch. They increase the "maneuvering room" of political executives by a "loose-jointed play of influence" (p. 117). In a long-term perspective these new arrangements strengthen the position of the executive branch.

These networks imply, however, some ambiguity. From a normative standpoint, Hecló not only sees advantages but also some serious problems. The most important relates to democratic legitimacy and accountability, when unelected network members get better informational access and have more decision-making influence than elected persons from the legislative or executive branch. By this mechanism issue networks increase the power of the latter. In this respect, Hecló points to a very interesting cognitive effect of these structural and institutional developments. Democratic politics and party competition would imply the idea of reducing complexity to a few choices that are understandable to the general public. However, in Hecló's perspective issue networks tend to "search out complexity in what might seem simple" (p. 119). Experienced policy-makers recognize that policy objectives are usually vague and difficult to evaluate. Action related to one policy goal is often inconsistent with other actions. In Hecló's view reputation as a knowledgeable participant is gained by "juggling all of these complexities" (p. 119) in a context where other members of the

issue network do the same. This produces the somewhat paradoxical effect that “more informed argumentation about policy choices produces more incomprehensibility” (p. 121). An increasing number of policy specialists in these network arrangements “may widen participation among activists but deepen suspicions among unorganized non-specialists” (p. 121). In a normative political perspective there may be increased participation and less democratic legitimacy, and at the same time more knowledge and more ignorance (p. 121).

LIMITS AND IMPACTS

Hecló's concept of issue networks is clearly a powerful descriptive and explanatory concept of development trends related to the American situation in government and public administration. The three key features of the American political system are its specific form of vertical decentralization due to its federalist structure, its fragmented and decentralized public administration due to the large number of regulatory institutions and semi-autonomous agencies, and a highly pluralist and competitive interest group system. However, the structural implications of this specific context cannot be generalized. In continental Europe, Japan, and even in the British system, which was categorized for a long time by a highly professional and centralized public administration, the characterization of a participant in the policy process as a wandering journeyman would be rather misleading. Even in the United States, such a statement is hardly generalizable across all issue areas. For instance, in their seminal study on *The Organizational State*, Laumann and Knoke (1987) completed a rigorous analysis of event–issue linkages in two US policy domains (health and energy). Their findings have shown a large variation in the patterns of issue–event participation, in some there was stability, in others more fluidity. Hecló's observation of fluidness certainly is true and plausible in new policy areas and highly specialized policy issues, but also in the US there are policy domains with rather stable patterns of participation in policy-making. A similar study on the labor policy domain compared the US with Germany and Japan where it discovered in all countries relatively stable “action sets” across multiple issues (Knoke et al. 1996).

A general problem in Hecló's study is his sampling strategy. It remains uncertain if his cases are representative of “typical” American policy-making arrangements. A large portion of his observation is essentially anecdotal evidence which would have to be tested in a systematic study of the Laumann and Knoke (1987) type. It is thus clear that Hecló's observation cannot be generalized across all actors, issues, and political systems. For instance, in a meta-analysis of quantitative network studies related to the participation of organized business and multinational corporations, the author of this chapter has discovered a large variation of participation patterns and a strong impact of various structures of political systems on policy-making (Schneider 2006). This argument is essentially related to methodology. Hecló's study is solely based on qualitative

methods. Although he conducted about 200 interviews, he did not use a standardized questionnaire in order to ask the same battery of questions to all policy-making participants. To have convincing evidence for the phenomenon of "issue networks," all policy actors would have to be asked about participation with respect to all policy issues and policy events. This criticism is similar to that of Parry who diagnosed a lack of the "systematic and quantitative analysis that its subject-matter invited" (Parry 2003) in Hecló and Wildavsky's "private government." In the meantime, some studies have been conducted where the phenomenon of issue networks has been studied more intensively. But only a few rely on rigorous quantitative methods.

However, in retrospect, this critique cannot diminish the influence and the inspiration that this article has had on a whole spectrum of studies on modern policy-making arrangements. It is indeed very impressive how many new ideas this relatively short article has initiated not only for entire subdiscipline of policy analysis and public administration, but for political science in general. This article—or the larger study behind it—is quoted in many prominent textbooks (John 1998; Peters 2001), and most notably, in the standard literature on policy networks (Kenis and Schneider 1991; Rhodes and Marsh 1992).

The aforementioned type of study, using both qualitative and quantitative methods, has blossomed since the early 1990s. Overviews are given by several reviews (Börzel 1998; Thatcher 1998; Rhodes 2006) and a structured bibliography (Schneider et al. 2007). Interestingly, even in studies in which policy networks are more determined by a stable pattern of participation in political processes, and therefore which very much differ from Hecló's fluid and specialized policy-making arrangements, Hecló is cited as "classic" (Lehmbruch 1989; Falkner 1998). In particular in many of the articles that include typologies of policy networks, Hecló's "issue network" is listed as a special type (Schneider and Werle 1991; Marsh and Rhodes 1992; Waarden 1992). Additionally, a number of prominent quantitative studies on policy networks refer to Hecló's studies (Laumann and Knoke 1987; Heinz 1993; Knoke et al. 1996; Mintrom 1997).

Yet beyond this general and rather diffusive influence of Hecló's work on network studies, we can identify very specific areas of social scientific discourse in which some of the ideas outlined have been conceived as particularly inspiring.

- The observation that specialized knowledge in policy processes plays an increasingly important role is now generally accepted. Meanwhile, there are several streams of theorizing in this analytical perspective (e.g. policy learning, epistemic communities, and advocacy coalitions) and most refer to Hecló. Remarkably, the most important heads of these different currents within this "cognitivist" or "culturalist" movement quote Hecló's classic article (Sabatier 1988; Bennett and Howlett 1992; Haas 1992; Hall 1993; Rose 1993).
- The observation that policy growth is not limited to the growth of government spending, but that it also includes a significant increase of regulatory intervention has been intensively discussed in the debate on regulation where many of

the main contributions relate to Heclo's classic article (Weingast and Moran 1983; Hood et al. 2001; Lodge 2003).

- The observation that policy growth and increasing regulatory intervention are also related to the growth and proliferation of interest groups and social movements has been taken up by a number of studies on interest groups in the US and also in Europe. In many studies that emphasize issue-based characteristics of political mobilization Heclo is quoted (Hojnacki 1997)
- Last but not least, the observation of complex and fluid patterns, of problem definition, and issue creation has been taken up in the agenda-setting literature. This literature was revived during the late 1980s and 1990s, particularly by the "multiple streams" and "punctuated equilibrium" frameworks (Kingdon 1984; Baumgartner and Jones 2009). This literature has been influenced strongly by Heclo's paper on issue networks.

Even though many of Heclo's observations apply only to the United States, he has made us aware of a number of general trends that apply more or less to many other advanced societies and political systems. One undeniably "large process" that Heclo calls attention to, which summarizes many of these facets, is the increase in political complexity at all levels of political life (Schneider 2012). Both the problems and the political arrangements that are related to problem-solving or problem "processing" have become more differentiated and diversified. In this context, the sheer mass and variety of political issues has increased. Also related to this are the number and diversity of private and public actors involved in such policy formation processes. To cope with these complexities, national political systems seem to react by increasing institutional differentiation and informalization. Specialized committees, working groups, and task forces are just a few examples where such informal institutional adaptation processes are applied (Leifeld and Schneider 2012).

Finally, Heclo's idea of issue networks has also had a direct impact by inspiring computer scientists to program a search machine to find "issue networks" in the internet. This internet robot has been called the "Issue Crawler," a software on a webserver (www.issuecrawler.net) that detects and visualizes thematic networks on the internet (Rogers 2002a, 2002b). This type of research, using the concept of issue networks in a quite different meaning to Heclo's specific concept, is on the way to become a new academic growth industry.

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CHAPTER 27

MICHAEL LIPSKY, *STREET-LEVEL BUREAUCRACY: DILEMMAS OF THE INDIVIDUAL IN PUBLIC SERVICE*

LUCY GILSON

INTRODUCTION

THE publication in 2010 of the 30th anniversary edition of Lipsky's 1980 book, *Street-Level Bureaucracy: Dilemmas of the Individual in Public Services*, indicates the enduring contribution of this classic work. The breadth of scholarship drawing inspiration from Lipsky is indicated by the nearly 7,000 citations of this book in Google Scholar. The book's continuing relevance globally and across sectors lies both in the continued importance of public services and its focus on what remains their central challenge: "how to treat citizens alike in their claims on government and how at the same time to be responsive to the individual case when appropriate" (Lipsky 2010: p.xii). The practical relevance of the analysis is, moreover, made clear by the response to it of those involved in policy implementation: it not only makes sense, but it also encourages them to reflect on how to work differently (e.g. Brodtkin 2012; Rowe 2012).

In this chapter, I will briefly outline the core features of the theory of street-level bureaucracy (SLBy indicating bureaucracy) and then discuss what I see as its key contributions to the field of public policy analysis. This discussion illuminates the analytic and practice relevance of this book (Brodtkin 2012).

In analytic terms, Lipsky's book brought public administration work into conversation with public policy and political science. Lipsky was neither the first to consider

administrative discretion (Davis 1969; Wilson 1978) or organizational influences over agents' behaviour (Argyris 1964; McGregor 1960). Nonetheless, his comprehensive and eloquent analysis, derived across diverse work settings, presented a timely challenge to those considering implementation primarily from a political science perspective. His consideration of what the street-level bureaucrats (SLBs) responsible for public service delivery actually do in policy implementation, and how their actions differ from the policy pronouncements of central level planners, contradicted the assumptions of top-down analysts, as exemplified by Pressman and Wildavsky (1973). Key among these assumptions were that policy goals were clear, knowable, and operationalizable, and that policy is decided by politicians and simply implemented by public administrators. By indicating the limits of central control over SLBs' behavior and proposing alternative strategies for holding them to account for their actions, Lipsky was, moreover, "in many respects the founding father of the bottom up perspective" (Hill and Hupe 2009: 52).

The practical relevance of the book is, meanwhile, political—to use research on street-level bureaucracies to improve the performance of public social welfare agencies, bolstering political support for, and generating greater investment in, them. Although Lipsky's insights on how to improve the performance of these agencies remain pertinent today, the still limited research around these ideas indicates that this practical project remains a particular priority for future work.

STREET-LEVEL BUREAUCRACY: CORE ELEMENTS

Lipsky's (2010) preface to the anniversary edition provides a good summary of the core elements of the theory, from the author's own perspective. Street-level bureaucracies are the public services whose workers "interact with and have wide discretion over the dispensation of benefits or allocation of public sanctions" (p. xi) and through whom citizens "experience directly the government they have implicitly constructed" (p. xi). Although there are many different types of public service workers, their work is not only "often highly scripted to achieve policy objectives" (p. xii) originating in the political process, but at the same time it commonly requires them to improvise in order to respond to the particular needs of individual clients.

Exercising discretion as they interact with citizens, public service workers (street-level bureaucrats) lack the time, information, or other resources to respond "according to the highest standards of decision-making" (p. xi) in their field to each individual case. They are put under pressure by the key features of their work settings, including: chronically inadequate resources; an ever growing demand for services; vague or conflicting organizational expectations and policy goals; difficulties in measuring their performance; clients who do not voluntarily choose the services.

These workers manage their difficult jobs by developing common routines of practice covering ways of organizing their work, modifications of how they understand their jobs, and modifications of how they conceive of their clients (essentially stereotyping more and less deserving clients). The routines influence, in turn, the way they do their work—such as managing and conserving resources (e.g. by building slack time into their days to give them the capacity to respond to unpredictable situations)—and have consequences that include controlling clients so they cooperate with procedures and rationing services to them, e.g. by imposing financial or time costs on clients. Often there is, therefore, also a need to establish strategies to manage the consequences of routine practices, e.g. by referring “difficult” cases to more specialized workers, or complaints systems. Ultimately, moreover, “the decisions of street-level bureaucrats, the routines they establish, and the devices they invent to cope with uncertainties and work pressures, effectively *become* the public policies they carry out” (p. xiii, emphasis in original).

Lipsky acknowledges that SLBs are often committed to public service and have high expectations for themselves in their careers, but argues that the demands of their work settings challenge these expectations. In these settings they cannot deal with clients on an individual basis and have to develop work practices and orientations that allow the mass processing of their clients (e.g. teaching a class of children not an individual child). They justify their coping mechanisms to themselves as pragmatic and reasonable, given their work settings, but these mechanisms distort service ideals or put SLBs in the position of “manipulating citizens on behalf of the agencies from which citizens seek help or expect fair treatment” (p. xv). Their clients, meanwhile, are “bureaucratic subjects” who, in accessing services, “must strike a balance between asserting their rights as citizens and conforming to the behaviors public agencies seek to place on them as clients” (p. xvi). The client’s dilemma is particularly acute if s/he is from a different socioeconomic, or racial background to public employees. At best, SLBs “invent modes of mass processing that more or less permit them to deal with the public fairly, and appropriately and thoughtfully. At worse, they give in to favouritism, stereotyping, convenience, and routinizing—all of which serve their own or agency purposes” (p. xiv)

An important note in the revised edition is that Lipsky did not intend to use the term street-level bureaucrat to apply to all public service workers. Instead, he saw SLB as public service employment of a particular sort, performed under certain conditions and pressures in which coping behaviors “may widen the gap between policy as written and policy as performed” (p. xvii). In other conditions, however, coping behaviors “reflect acceptable compromises between the goals of enacted policy and the needs of street level workers.” So “perhaps it is best to imagine a continuum of work experiences ranging from those that are deeply stressful and the processing of clients is severely under-resourced, to those that provide a reasonable balance between job requirements and successful practice. Workers’ places on that continuum may change over time as they gain experience, as caseloads and assignments vary, or as the workplace itself adopts new approaches or engages new clienteles” (p. xviii)

Finally, he notes that in his view, despite popular discontent with the work of SLBs, there is simply no alternative to people making decisions in public services. His contribution is therefore to “locate the problems of street-level bureaucrats in the structure of their work, and attempt to identify conditions that would better support a reconstituted public sector dedicated to appropriate services and respect for clients” (p. xix).

STREET-LEVEL BUREAUCRACY: CONTRIBUTIONS

Recent scholarship drawing on Lipsky’s ideas,¹ combined with reflection on the current relevance of his work to a new field of application, health policy and systems analysis in low- and middle-income countries (LMICs),² suggests that it has made three key contributions to the field of public policy analysis. These are its recognition that: (a) street level bureaucrats (SLBs) have discretion and power in implementation, and so their practices are what citizens experience as policy and have political consequences; (b) SLBs’ behavior is systematically influenced by the organizational and institutional environment in which they work, rather than being primarily a response to personal preferences and interests; and (c) efforts to control SLBs’ behavior only undermine their responsiveness to clients, so new approaches are needed to support them as the face of a responsive public bureaucracy.

WHAT IS DISCRETION AND WHY DOES IT MATTER?

The very notion that SLBs have discretionary power was central to Lipsky’s challenge to the top-down policy theorists, and the nature of discretion and its consequences have remained two important areas of scholarship.

The Nature of Discretion

Lipsky argues that human agency is central to policy implementation, and that SLBs will always make their own decisions on at least some issues. Discretion occurs “when- ever the effective limits on [the public official’s] power leave him free to make a choice among possible courses of action and inaction” (Davis 1969: 4). In other words, discretion is the space between the legal rules in which actors exercise choice—the sphere of an actor’s autonomy for decision-making (Hawkins 1992 and Galligan 1990, respectively, cited in Loyens and Maesschalck 2010). Lipsky also argues that this space exists

because, on the one hand, policy goals are often not clearly stated and policy details are not finalized before implementation; and, on the other hand, in pursuing policy goals, SLBs are asked to be responsive to the complex challenges clients present, not all of which can be predicted.

Subsequent work has illuminated different forms of SLB discretion. One distinction is that between strong and weak discretion (Evans and Harris 2004, drawing on Dworkin 1977). Strong discretion entails both deciding the criteria for decision-making and making the decisions, and is commonly exercised by professional groups, such as doctors. Sharing occupational characteristics, these groups are given valued status by society, perhaps in part because they are seen as altruistic, and so are also trusted to use their competence and expertise to make decisions in unpredictable and complex situations hidden from public view (Hupe and Hill 2007). Weak discretion, meanwhile, entails applying a standard or rule, or making a decision within the rules. Ellis (2011) contrasts the “value discretion” of professionals, for example, with the weaker and more informal discretion at the disposal of most SLBs, who decide what rule to apply in particular situations, or how to interpret a rule in a particular situation.

Empirical work brings alive these conceptual distinctions. An ethnographic study of UK child welfare services initiated in 2007, for example, examines social worker practices in an era of enhanced performance management and procedural standardization, involving the use of information technology to drive and record practice (Wastell et al. 2010). The study shows that performance indicators became an accepted part of organizational life, clearly influencing routine practice. However, social workers exercised discretion in the way they applied the established procedures (through their coping strategies)—such as “playing the system” to buy the time needed to conduct more thorough assessments of children and their families than these procedures allow. They also continued “to exercise professional discretion through categorizations, ‘diagnostics’ and the social sorting of children and families” (Wastell et al. 2010: 317). The authors also argue that discretion can be seen in the language that social workers used about their work: “[s]treet level language gives the power to define, reaffirms the invisible trade and reinforces the occupational identity whilst at the same time rattling the cage of the rule-bound bureaucrat” (p. 317). Weak discretion thus encompasses doing their work in ways SLBs feel is appropriate and, as Lipsky also noted (1980), in ways that maintain their own sense of identity and self-esteem.

More recent empirical work has, meanwhile, brought new perspectives on strong discretion. Drawing from American SLBs’ personal narratives, Maynard-Mooney and Musheno (2000) have introduced the notion of “citizen agent” to contrast with the “state agents” portrayed by Lipsky. The term seeks to highlight that SLBs see themselves as working in their client’s interests rather than as an extension of the state, guided by their own judgment of each person’s worth. Similarly, Durose (2011) describes UK local government workers involved in a range of community and health development activities as “civic entrepreneurs.” They draw on their local knowledge and resources, including policy resources, to experiment and innovate in engaging with hard to reach community groups and confronting “wicked” problems like social exclusion. These

authors argue that citizen agents are “rule saturated” not “rule bound” and, acting like any professional, use their discretion pragmatically in response to client need (Durose 2009; Maynard-Moody and Musheno 2000).

Variation between SLBs in their discretion may reflect differences in jobs or changing expectations of SLBs in general. In a hospital, for example, there are both professionals, such as doctors and nurses, with stronger discretion, and non-professional groups, such as clerical and ancillary workers, with weaker discretion. Some jobs, meanwhile, demand multiple levels of discretion to match the varied nature of expected tasks (Piore 2011). Durose (2011), finally, discusses how expectations of front-line workers have changed over time in the UK, in line with changing bureaucratic forms. In the 1970s, Weberian bureaucrats were expected to follow rules, the SLBs of the 1990s were expected to use discretion to ration services, and the civic entrepreneurs of the 2000s were expected to engage actively with community clients. As Lipsky noted (2010: p. xviii) there is, therefore, a “continuum of work experiences” within street-level bureaucracies.

Why Discretion Matters (1): The Political Consequences

In practical terms, SLB discretion matters because its use is a political act, “potentially building or undermining support for government as a vehicle for advancing social welfare, equity and justice” (Brodkin 2012: 947). Through their decisions SLBs influence citizens’ levels of access to public services or welfare benefits, as well as their experience of that access (Hupe and Hill 2007). Sometimes their exercise of discretion has critical consequences for the life chances of their clients (Marinetto 2011). SLB discretion, therefore, also mediates the broader relationship of state and citizen—as SLBs “teach clients to behave properly” and the public trusts SLBs to make significant decisions about citizen welfare (2010: 235–6). SLBs are, quite simply, the daily reality of the state in most people’s experience and so their behaviors signal the value the state/society places on different people. In their own view, street-level workers are “empowered citizen agents, who in their decisions to ration resources, provide access to programmes, and sanction individuals, both communicate and convey social status” (Maynard-Moody and Musheno 2000: 355).

A major strand of SLB empirical work continues to provide evidence confirming that “the types of coping strategies that Lipsky identified are both prevalent and plentiful ... robbing services of their substantive value and skewing the distribution of benefits” (Brodkin 2012: 943). Ellis (2007), for example, examines British experience of replacing direct care schemes for older and disabled people with cash payments that, in principle, allow clients greater choice and autonomy in deciding what personal assistance they receive and from whom. In addition to their formal role of rationing available resources, Ellis finds evidence that SLBs ration information to limit demand, and stereotype clients—for example, by assuming older clients do not

want decision-making autonomy and that middle-class clients were better able to take advantage of direct payments. These attitudes toward clients are essentially personal judgments about who does and does not deserve support.

Although few studies of SLB behavior have been conducted in the health sector, by definition health professionals might be assumed to work primarily in the patient's interests—like citizen agents. However, some LMIC studies, for example, have shown how professional values may go hand in hand with attitudes and behaviors toward patients that are replete with SLB coping strategies and that have distributional consequences. Patients are quite strongly “controlled” in health facilities through queuing and patient flow systems, and informal practices such as the timing of tea breaks, are subject to stereotyping (such as judgments over who “deserves” access to family planning or abortion services), and can experience health providers as demeaning and even abusive towards them (Harrison et al. 2000; Walker and Gilson 2004). Patients' poor experiences in health facilities may, therefore, lead them to distrust providers, to delay seeking care, or even deter them from accessing care altogether (Gilson 2007). The least powerful patients commonly bear the brunt of SLB behavior, and its consequences, including increased costs, can threaten family livelihoods (Goudge et al. 2009; Russell and Gilson 2006) and exacerbate exclusion and impoverishment (Tibandebage and Mackintosh 2005).

However SLB behavior is always complex. Horton (2006) presents an ethnographic account of experience in a US mental health clinic located in a hospital serving a low-income and largely Latino immigrant patient population, in 2003–5. The majority of clinicians were also from Latin America, but from more privileged backgrounds, and worked in the clinic because they felt it offered better care for its target population than either the mainstream public or private sector services. They played dual roles in the clinic, informally acting as patient advocates in the wider bureaucracy and formally working as therapists—with both roles important to their care for their clients. Patient advocacy roles included, for example, supporting victims of political violence facing deportation in their engagement with the legal and immigration system. Although important to patient treatment, and acknowledged by the hospital administration, this work was not seen as part of their job, and had to be done out of working hours on top of usual workloads. At the same time, within the clinic their professional practices were being squeezed by cost containment measures intended to promote clinician productivity, all of which most affected the uninsured and immigrants with serious psychosocial issues. The measures included time limits on appointments, denying free care to uninsured patients, and providing group rather than individual therapy. In response to these pressures the clinicians themselves also decided to deny future care to any patient after three successive failures to arrive for a prebooked appointment. As Lipsky (1980) noted, SLBs have two mindsets—the professional, involving discretion and autonomy and the bureaucrat, compliant with supervisor's directives—with different consequences for particularly vulnerable patient groups.

There is also wider evidence of the complex realities but positive potential of SLB discretion. They work as principled agents fulfilling their professional goals in the

Danish welfare system (May and Winter 2007), or combine coping strategies with professional behavior, rising above the demands of their jobs as US school psychologists to provide needed services (Summers and Semrud-Clikeman 2000), or moderate practice to accommodate the non-functional features of policy in Finnish and Swedish psychiatric services (Markström et al. 2009; Saario 2012). Studies that regard SLBs as civic entrepreneurs, meanwhile, note how they use discretion creatively, bending policy rules to be responsive to community concerns but also trying to pursue government agendas (Durose 2009; Markstrom et al. 2009; Maynard-Moody and Musheno 2000).

A much smaller body of work has so far considered the influence of SLB actions on the relationship between state and citizen, and further research is important. There is, for example, a little evidence of how SLBs: mediate social status and identities, such as race and gender (Brodkin 2012); act corruptly (Staranova and Malikova 2007) or offer responsive services (Berenson 2010), in wider contexts of bureaucratic failure. At a conceptual level, meanwhile, political scientists (Rothstein and Stolle 2008, 2001) pick up on Lipsky's discussion of trust and SLBs. They argue that people's experience of the local political institutions responsible for public policy implementation and specifically, the impartiality and fairness of their treatment, affects their trust in these institutions. This institutional trust influences, in turn, levels of "generalized trust" or social capital, in society at large. In similar vein, Mark Moore (1995) has coined the term "public value" to capture the two potential benefits of public services: producing things (services) of value to citizens and operating in fair, efficient, and accountable ways that meet their desire for a well ordered society.

Why Discretion Matters (2): Understandings of Policy, Accountability, and Legitimacy

In analytic terms, discretion matters because it challenges the dominant account of public policy implementation—and how to manage it to achieve policy goals/public value. Within the top-down perspective on policy-making, central-level decision-makers are assumed to hold the dominant power, and implementers are expected to comply with the instructions and guidelines laid down for them (Barrett 2004). Underpinning this perspective is the normative judgment that, within a bureaucracy, implementers are accountable through their superiors to the legitimate (democratically) elected government. They must, therefore, align with the democratic will and the rule of law (Ellis 2011). SLB deviation from policy prescriptions and guidance is, in this view, illegitimate.

The empirical evidence shows, however, that policy as experienced by clients is embodied in the formal and informal practices of SLBs, whatever paper and rhetorical goals are formally established (Brodkin 2012). Human agency and interactions are key influences not only over how policy is implemented but also over what policy *is*; indeed, for all bottom uppers, policy-making is still in progress at the moment of

delivery (Hudson and Lowe 2004; Parsons 1995). Ultimately, the reality of SLBs' agency means that expecting compliance with centrally imposed rules is unrealistic. It suggests instead that they must be empowered to perform, to exercise their discretion to be responsive to clients—and to be held accountable for that (Barrett 2004).

This represents the crux of the top-down/bottom-up debates: the distinction between what ought to be and what is, and between the traditional top-down notions of accountability within Weberian bureaucracies and the emerging understandings of multiple accountabilities within networked systems (Hupe and Hill 2007).

At one level, this point reflects very different understandings about the nature of policy. Moving beyond an instrumental view of policy and a linear view of policy-making, the very notion of discretion and the idea of policy as practice reflects understanding of the socially constructed and constituted nature of public policy. Policy is not fixed by central-level planners but negotiated through power and discourse in the course of its implementation (Laws and Hajer 2006). The idea that SLBs work within rules that define their power and yet, influenced by their values and interests, use their discretion to redefine those rules, reflects broader discussion about the interplay between structure and agency. Barrett, a British contemporary of Lipsky, for example, notes how she was influenced by the notion of structuration (Giddens 1984)—the “understanding [that] structures or rules of the game determine the *status quo* of power relations, but since these are socially constructed they are also susceptible to change through human agency” (Barrett 2004: 257). She argues: “This has offered a new way of looking at concepts of power and negotiation in implementation as the dialectic between structure and agency, which reinforces a view of performance, or what happens in practice, as a function of the scope or limitations of scope for action (rules and roles), and the use made of that scope (values and interests).”

At another level, judgments about legitimacy themselves reflect broader political imperatives and ideological paradigms (as Lipsky himself noted, 1980: ch. 12). The benefit of hindsight provided by reflecting on the 30 years since the initial publication of Lipsky's book allows sight of the evolution of thinking about bureaucracy. Whilst the global context may be broadly similar (economic uncertainty, growing poverty, increasing demands on public services), there have been huge changes in approaches to public administration and management (Ellis 2011).

The era of the Weberian bureau-professional regime, in which access to public resources was controlled by professionals applying bureaucratic standards and eligibility rules that sought to ensure equal and fair treatment for all, was overtaken, globally, by the new public management era. Linked to neoliberal economic thinking, this era brought performance monitoring and market mechanisms into the public sector and entailed a tightening of top-down control. At the same time, however, bottom-up theorists continued to track practice and develop thinking around the dispersed power within, and networked nature of, bureaucracies (Barrett 2004). New understandings of governance have subsequently emerged that recognize the interplay of policy and action and the range of forces shaping action within bureaucracies, as well as more relational understandings of accountability (Durose 2011; Hupe and

Hill 2007). In the UK, for example, the Thatcherite emphasis on market mechanisms was moderated by New Labour's emphasis on consensual governance, involving partnerships and networks between public sector and community groups (Ellis 2011), and inclusive policy processes that also engaged with public sector staff (Durose 2011). In Europe, more generally, decentralization of authority, combined with "activation policies" intended to reduce client dependency on the welfare state, has brought new roles for SLBs (Rice 2013). In the US, meanwhile, Mark Moore's work on public management and public value (1995) has opened up discussion both about the nature of public value and the related strategies of public leadership (as distinct from private value and leadership).

Each of these different eras represent different ways of understanding the role of the public sector in society, and different ideas about its primary imperatives. The balance between efficiency, equity, and responsiveness, as well as different approaches to management within it, are primary concerns. Ellis (2011) posits, therefore, four different forms of SLB action and discretion, framed by the balance between professionalism and managerialism, and the degree of formality/informality (and related legitimacy) with which discretion is exercised. The notions of state agent and citizen agent capture something similar and highlight the critical difference in understandings of legitimacy: are the rules of policy, developed through vertical lines of political and bureaucratic accountability, the touchstone of bureaucratic legitimacy or is that touchstone rather the bureaucrat's responsiveness to the client and community?

WHAT SHAPES SLB BEHAVIOR?

Lipsky's second critical contribution to understanding policy implementation is his recognition that SLB discretionary behavior is patterned by the structural conditions of the working environment, rather than being the random acts of self-interested individuals behaving badly as envisaged in, for example, public choice theory (Downs 1967; Niskanen 1971). Individual SLBs are, therefore, not solely or even primarily to blame for the challenges the public experiences in accessing public services, as their behaviors are shaped by their broader work environment. Lipsky (2010: p. xv) talked about the "corrupted world of service" in which SLBs work. He argues that they are caught in fundamentally tragic situations where they simply cannot put their ideals into practice, and instead lower their expectations of themselves and clients. This is the "dilemma of the individual in public services," as the subtitle of the book has it.

This insight is confirmed by empirical evidence. Studies show that SLBs generally do not oppose policy aims or deliberately work to subvert them, but instead find that being responsive to clients is simply "incompatible with their work lives" (Brodin 2012; in the health sector e.g. see Walker and Gilson 2004). Lipsky gave particular attention to resource constraints, workload pressures, policy ambiguities,

bureaucratic efforts to exert greater control, and relations with clients as structural influences over SLB behavior. However, he also acknowledged a continuum of workplace experiences, as noticed earlier Lipsky, 2010.

Subsequent empirical work provides evidence of four main categories of influence: (a) individual decision-maker characteristics (such as professional norms, personal interests, moral values, gender, ethnicity, role definition, personal meanings); (b) organizational characteristics (internal structure, rules and constraints, organizational routines and culture, workload pressures); (c) client attributes (levels of need, or perceptions of clients); and (d) extraorganizational factors (e.g. broader community, laws, regulations, media, other service agencies) (Loyens and Maesschalck 2010). Jewell and Glaser (2006), for example, derive and test an empirical framework of six influencing factors: SLB authority to influence clients; role expectations reflected in SLB attitudes to work and clients; workload; client contacts (frequency, regularity, quality, time); personal knowledge and expertise; and incentives (formal and informal, including intrinsic rewards).

However, the mix of influencing factors play out in different ways in different situations—depending on the scope and nature of SLB discretion in a particular task or job. Empirical work has, on the one hand, demonstrated that resource constraints and managerial interventions—such as target setting, incentives, and the use of information technology—influence SLB behavior, narrowing or containing even weak discretion (Brodkin 2012). On the other hand, street-level factors are also clearly influential in some settings and perhaps especially where SLBs have wider or stronger discretion (May and Winter 2007).

At an individual level, recent work has again demonstrated how personal beliefs and norms about fairness influence the personal judgments “citizen agents” make about the relative worth of individual citizens (Marinetti 2011; Maynard-Moody and Musheno 2000). Relatedly, Evans (2010) argues that professional status is important as it entails commitment to values that focus on service user wellbeing (over economic priorities), and brings a greater degree of decision-making autonomy than held by other SLB groups. He criticizes Lipsky for overlooking this influence. Other individual-level influences include, for example, SLBs’ understandings of their jobs (Bergen and While 2005: UK community nurses) or policies (Pennay 2012: Australia, policy officers and drinking laws), feelings of competence and awareness of the responsibility that comes with power (Ydreborg et al. 2008: Sweden, social insurance), and knowledge of local situations (Durose 2011: UK, local government).

Beyond the formal features of organizational settings, the broad SLB literature also highlights the web of horizontal and vertical relations in which SLBs are nested as important influences over their behavior (Hupe and Hill 2007). SLBs themselves identify three key relationships (Maynard-Moody and Musheno 2000; see also Marinetti 2011). Those with: citizen clients, fellow street workers, and the system within which they work (including elected officials, even the media). SLBs see themselves as independent moral actors in opposition to the system, which offers some loose support but also can intrude into their work. Interactions with clients can bring both a sense

of accomplishment and threat, but interaction with peers, their primary reference group, is generally one of mutual support and learning. Collaboration across sectors/organizations can, nonetheless, be undermined by perceptions of relative interprofessional status (Halliday et al. 2009).

Another potentially important relationship, although overlooked by Lipsky originally, is that between SLBs and their line managers. Evans (2010) distinguishes between central and local managers. He presents evidence to show that shared professional commitment (in this case, among UK social workers) allows front-line workers and their managers to collaborate in addressing the needs of service users as they judge best, despite higher level managerial pressure to focus on expenditure control and performance management. Although Brewer (2005) also finds evidence of front-line supervisors' influence, the limited available evidence is equivocal. For example, a Danish study of employment policy implementation at local government level uses statistical analysis, unusually, to test various related hypotheses (May and Winter 2007). It determines that the amount of supervision and degree of delegation have some, but relatively little, influence over how caseworkers interpret policy guidelines in their interactions with clients—in comparison to the influence of local politicians and, most importantly, SLBs' own understanding of policy goals, personal acceptance of those goals, and perceptions of their own knowledge of relevant policy rules.

Managerial and other relational influences are, however, recognized in Piore's (2011) more recent and, for SLB literature, unusual exploration of the influence of organizational culture. Starting from the understanding that SLB is a particular organizational form distinct from classic Weberian bureaucracy or markets, Piore draws on sociological theory and case studies (of labor inspection organizations in Latin America and southern Europe and the US Department of Defense's research arm) to understand how organizations shape behavior beyond incentives or bureaucratic rules. He actively looks for explanations of innovative and entrepreneurial behavior (like that of civic entrepreneurs). He argues that in an SLB decisions are made within a framework of tacit rules and procedures, embedded in the organizational culture, passed on through the socialization of new organizational recruits, and reinforced, and evolving, through discussion among peers and managers. Theory suggests that organizational culture is likely to have greater influence over individuals where they depend on their organizational colleagues for approval and support, and where organizations operate in a hostile environment. In these settings, managers might encourage SLBs' innovative, entrepreneurial behavior by shaping the organizational conversations that, in turn, shape practice.

Overall, therefore, SLB theory has contributed understanding about relationships within organizations (Friedman 2006) and fed into wider work on the sociology of organizations (Hill and Hupe 2009). The very notion of discretion, as discussed earlier, is tied into consideration of the structure–agency dialectic. Based on this, Rice (2013) develops what she calls a “micro-institutionalist theory of policy implementation” that presents an overarching framework of influences over the caseworker–client interaction (Figure 27.1). On the one hand, the standardized interactions between caseworker

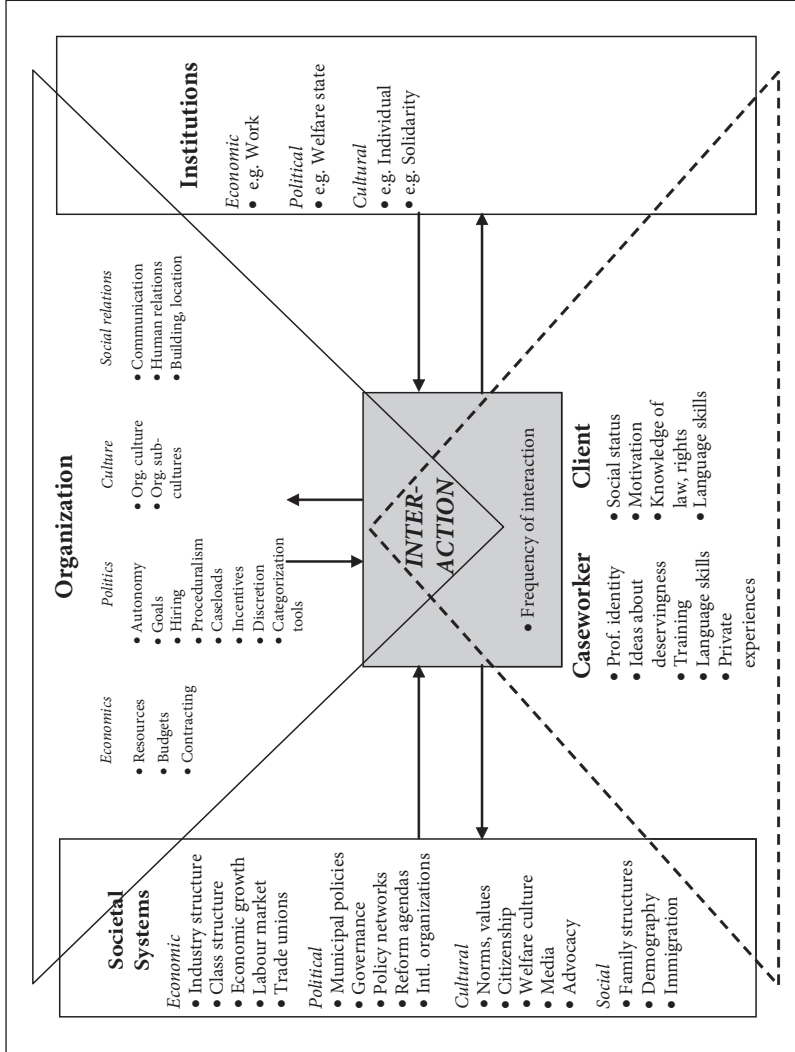


FIG. 271 Overview of systems and institutions influencing the caseworker–client interaction (Source: Rice 2013)

and client become part of the broader institutional framework guiding those interactions and restricting what actions are permitted or are regarded as relevant or appropriate. On the other hand, that institutional framework may change not only as a result of systemic forces (such as ideological change) but also as a result of individuals doing things differently in that interaction. She argues that, whilst legal and regulatory documents, staff, and budgets represent the building blocks of the welfare state, it only becomes real as a political institution when caseworkers (SLBs) meet citizens and take decisions. But the moment “at which the welfare state becomes a manifest reality in the interaction between caseworker and clients is also the moment at which it starts to evolve and possibly to change” (Rice 2013: 1043). Her framework bridges the micro-level of the caseworker–client interaction, the meso-level of the implementing organization, and the macro-level of the wider societal context. She argues it develops Lipsky’s thinking in making explicit the interrelatedness between the caseworker–client interaction and its wider institutional and systemic context.

HOW CAN SLBs BE BETTER SUPPORTED TO OFFER PUBLIC VALUE?

The third critical contribution of Lipsky is his recognition that attempting to control SLBs to contain or prevent their discretion, as proposed by top–down theorists, only leads them to stereotype and disregard client needs (Hill and Hupe 2009).

Subsequent empirical work has only proved the point. Although the managerialist interventions of target setting, incentives, and the use of information technology, together with cost containment interventions, influence the scope and exercise of discretion, they do not control it (Brodkin 2012). They may encourage “compliance without conviction” (Wastell et al. 2010) or produce fresh conditions and requirements for covert rationing (Evans 2010; Keiser 2010). Most critically, as Lipsky predicted, the efficiency gains that are achieved through managerial intervention “squeeze out” SLB responsiveness to client need, and so have negative consequences for quality and efficacy (e.g. Marinetto 2011), and as yet little known consequences for equity and discrimination. Whether the (unanticipated) consequences are judged as positive or negative is ultimately an ideological or political judgment about the role of the state in society and the importance of responsiveness as a public sector goal. At the very least, however, Brodkin (2012) urges caution in using incentives to influence SLB behavior and public sector performance.

These experiences offer important insights for health system development in LMICs. Strongly influenced by global organizations, international power relations, and national interest groups, particularly medical professionals, health policy implementation in LMICs is commonly seen to be a function of exercising central authority within machine-like organizations. The solutions to the gap between policy goals and

implementation experience are, therefore, often seen to lie in action to guide individual implementers' behavior—such as clinical guidelines and performance-based (or results-based) payments (e.g. Lewin et al. 2008).

So, what other managerial approaches can support SLBs, and hold them accountable as they allocate public resources and mediate state–citizen relationships? In the revised edition, Lipsky (2010: 235–6) notes that the need for human judgment in policy implementation means that the “central challenge for management is to improve workers' capacity to render that judgement dispassionately.” Whilst treating everyone alike is necessary to build public trust, he argues that exceptions can be acceptable when neither discriminatory nor the result of favoritism, and where SLBs have the skills, training, and experience to exercise discretion properly (see also Rothstein 1998).

Recognition of the moral reasoning underpinning SLB discretion (Maynard-Moody and Musheno 2000) points to the importance of strengthening SLBs' reflective practice. Lipsky (1980) suggested, for example, that it is important to create regular moments of reflection among peers and with managers, to review practice and learn from experience (as also noted subsequently by others thinking about SLB: Brodtkin, 2012; Rothstein and Stolle 2001; Rowe 2012). Deliberative and reflective practice is itself a recognized and growing area of conceptual and empirical work (Schon 1983; Ghaye 2008), including consideration of the role of communities of practice in supporting learning (Wenger 1998). A Dutch example of efforts to improve individual ethical practice and, ultimately, public trust in the tax administration is instructive (van Blijswijk et al. 2004). The approach combined external oversight of standardized rules and codes of conduct with processes to encourage deliberation among people within the organization about their personal practice (through training, use of counselors, and reflection groups), in an effort to develop their moral compasses and ethical judgment.

Although intending to influence individual behavior, processes of deliberation and reflective practice are, therefore, likely to recognize and build on the complex sets of relationships within which SLBs are nested. Hupe and Hill (2007) provide a comprehensive conceptual account of these relationships and their relevance to street-level accountability. Rather than, as in top–down approaches, seeing accountability as purely an issue of compliance to rules (enforcement) or targets (performance), they allow for co-production, entailing professional or participatory forms of accountability (reflecting bottom–up perspectives). Indeed, understanding accountability as “a social relationship in which an actor feels obligation to explain and justify his conduct to some significant other” (Bovens 1998: 172, cited in Hupe and Hill 2007), the multiple relationships within which SLBs work, therefore, provide various ways through which citizens can hold SLBs to account. All are likely relevant in supporting improved SLB performance, within balanced approaches that take account of different types of SLBs, organizations, and role expectations.

Although there is clearly great scope and need for more empirical research to understand accountabilities as practiced at street level, some ideas about these

different dimensions can be derived from existing work. Based on analysis of SLB failure in UK social work, Marinetto (2011), for example, points to the potential role of peer accountability, exercised through informal professional relationships, in encouraging SLB responsiveness to client needs. Lipsky (2010) himself highlighted models of SLB practice that encourage open discussion of potential errors in decision-making and teamwork to enable learning and support decision-making. Hill (2003), meanwhile, actively explores the role of professional organizations, academics, and other interest groups as “implementation resources” supporting learning. Their support can take the form of basic and in-service skills’ training, but they can also provide a range of other intellectual resources—theoretical perspectives, insight into what policy means, ideas on “best” practices” for implementors or on organizational technologies for implementation. Although outside government, these groups may be seen to be more prestigious and carry more legitimacy than government-based resources.

Another professional resource available to SLBs, and one not originally well recognized by Lipsky, are managers—where they work to offer professional support and guidance rather than just acting as agents of hierarchical control (Evans 2010). Recognition of co-production as a mode of implementation also directs attention to the role of trust as a managerial mechanism, instead of rules or contracts (Hill and Hupe 2009; see also Gilson 2003, and Gilson et al. 2005 for consideration of the role of trust in LMIC health systems). Related managerial strategies focus on leadership of people by building their individual capabilities, building teams, and shaping organizational culture (Mintzberg 2009). In thinking about how to shape organizational culture, Piore (2011), meanwhile, highlights the managerial role of encouraging interpretive conversations within an organization. These create spaces for the tacit knowledge of SLBs to be heard and shared, supporting the organizational sense-making (Weick 2009) that underpins current SLB practices as well as enabling change in those practices. Reflecting institutionalist thinking, this managerial approach emphasizes the role of ideas, narratives, and meanings in shaping SLB behavior, and, again, the role of deliberative and reflective practice in influencing behavioral change. Piore also suggests a link to the wider world of systems thinking and continuous quality improvement strategies (such as benchmarking, the Toyota method, the Six Sigma approach, etc). All of these, as also identified by Lipsky in the revised edition (2010), use indicators to stimulate discussion rather than primarily to judge performance.

Relationships with clients are another stimulus for better practice and line of SLB accountability (Hill and Hupe, 2009). Lipsky (2010) suggested clients could become a stronger reference group for SLBs by, for example, involving them in the definition of good practice at street level, empowering them to be more involved in decision-making, or moving towards models of care that are based on team not individual relationships. In LMICs local health facility committees have been quite widely established as mechanisms of community accountability, with variable success (McCoy et al. 2012; Molyneux et al. 2012). Finlay and Sandall (2009), meanwhile, discuss a new UK model

of midwifery care that offers possibilities for relationship continuity and a focus on the experience of service users. Where SLBs work as citizen agents, their accountability to the community is, moreover, both acknowledged and encouraged. They “engage with the community and develop strategies aimed at achieving community-centered or ‘civic’ ends” (Durose 2009: 991), building relationships and sharing information with, as well as signposting for, their community clients (Durose 2009).

Finally, Brodtkin (2012) points to the potential value of backward mapping (Elmore 1979) as a process for thinking through how policy itself can enable SLBs to engage appropriately in their human interactions with clients. Paying more attention to policy delivery as policy is developed was encouraged under the New Labour government in the UK (1997–2010). The principle of policy inclusiveness encouraged policy-makers to consider those involved in policy implementation at an early stage of policy design and to think through the possible policy impacts on intended beneficiaries (Bochel and Evans 2007). The importance of framing policy in ways that enable front-line workers, rather than seeking to control them, has also been picked up in systems thinking work on UK health policy. Chapman (2004: 91) specifically suggests that policy outputs “should be as unprescriptive about means as possible.” They should establish the direction of change clearly, set limits on implementation strategies, allocate resources for reasonable lengths of time without specifying how they must be spent, clearly specify areas of discretion for local managers and workers, and specify core evaluation requirements (including feedback by end users).

CONCLUSIONS

Lipsky’s seminal work illuminates the essential human and political features of policy and implementation, providing insights that remain ground-breaking 30 years later. For practitioners and researchers alike, this is not just a classic book—it has persistent relevance. Perhaps most importantly, its still relatively untested proposals about how to strengthen the performance of public sector bureaucracies offer value worldwide today. In LMICs, for example, efforts to improve the responsiveness of health systems could focus on encouraging reflective practice, trust-based workplace management, and applying sense-making as a way of shaping organizational culture.

Future research globally and across sectors should, finally, follow Lipsky’s example in seeking to understand street-level bureaucracies from the inside out (Brodtkin 2012), for example, through ethnographic work (e.g. Maynard-Moody and Musheno 2000) or interpretive policy analysis (e.g. Durose 2009). The gaps in current research show the particular value of better understanding the forces shaping SLB behavior and of testing proposals for reframing them to support public service improvements. In the face of fluctuating political and ideological support for public services there is also a continued need to demonstrate the impact of different approaches to delivering public services in people’s lives.

NOTES

1. A scan of the main and continuing lines of SLB scholarship across continent and sectors was conducted as background for this chapter. Using the simple search term “street level bureaucracy” (with particular but not exclusive interest when the term, or the word Lipsky, was used in the paper title) I initially searched for recent, published journal papers within Thomson Reuters’s Web of Knowledge (a general database) and the Cumulative Index to Nursing and Allied Health (a health database). This was followed by a hand search for unusual references from review papers and a limited number of more specific geographic and author searches in Google Scholar (e.g. SLB Africa; SLB Asia; SLB India; SLB Rothstein).
2. The emerging field of health policy and systems research takes seriously the idea that implementers are people with agency—recognizing their roles in bringing policy alive through their practices and so becoming what the health system is experienced as by patients and citizens (Lehmann and Gilson 2013; Ssenkooba et al. 2007). In line with broader SLB thinking, health systems are, therefore, understood as relational systems in which people are influenced by each other and their broader institutional environment—requiring new approaches both to managing (Gilson 2012) and researching within them (Gilson et al. 2011; Sheikh et al. 2011). The broader SLB literature, thus, offers insights and ideas to stimulate this area of research.

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CHAPTER 28

RICHARD ROSE, *DO PARTIES MAKE A DIFFERENCE?*

MANFRED G. SCHMIDT

RESEARCH QUESTION, RESEARCH DESIGN, METHODS, AND FINDINGS

THE institutional core of a democracy may be conceived of as a market in which policies are delivered in exchange for demand and support from the mass public. Political parties, parliaments, and governments play a major role in this market. But what parties actually do in parliament and in office is controversially debated. Do parties matter? Are differences in the party composition of government associated with, and causally related to, different policy outputs as conventional theories of party government assume? Or is there something stronger than parties outside politics, such as social and economic trends?

Questions like these inspired Richard Rose's *Do Parties Make a Difference?* The first edition of this book was published in 1980 and an expanded second edition appeared in 1984. Its author's key question is: does it make a difference whether Britain is ruled by a Conservative or a Labour government, and, if so, what are the differences that result in terms of policy outputs and outcomes (Rose 1984a: p. xi)?

Most party members and most politicians would presumably argue that election manifestos, policy positions, and policy choices of the party in office differ substantially, if not fundamentally, from the record of their opponent. They would therefore conclude that the correct answer to Rose's question is "Yes." But political rhetoric is to be distinguished from political reality, Rose reminds the reader in the preface to *Do Parties Make a Difference?*: "What parties say is not what parties do" (Rose 1984a: p. xiii).

This innocuous-sounding statement is the overture to a fundamental challenge of two widely held views on British politics. The first of these concerns the

parties-do-matter hypothesis. This view predicts that parties make a difference in public policy and argues that big differences between parties generate big differences in policy outputs. The second view, related to the first one, is the proposition that “adversarial politics” is the trademark of Britain’s post-war politics. According to the adversarialists, British politics, fueled by the all-or-nothing differences in the electoral system, “emphasizes the rhetoric of Adversary politics” (Rose 1984a: p. xiii). The “adversary system is,” in the words of S. E. Finer (1975a: 3), “a stand-up fight between two adversaries for the favour of the lookers-on.” Adversarial politics is a continuous relentless contention between the two major political parties in Britain not only before and during election campaigns, but also after elections. Adversarial politics is a non-cooperative game, in which “a powerless Opposition confronts an all-powerful Government, in the hope of winning itself a more favourable verdict at the next general election” (Finer 1975a: 3). Adversarial politics might potentially be attractive for the “lookers-on.” However, it is for various reasons “inimical to the good conduct of the nation’s affairs” (Finer 1975a: 12). The party which wins the general election and takes over government is unduly influenced in its policy-making by the extreme wing of the party. Moreover, the incumbent party represents only a minority of the electorate. Furthermore, adversarial politics is conducive to an exaggerated degree of “discontinuity in national policies” (Finer 1975a: 14). Last but not least, adversarial politics generates “uncertainty” (Finer 1975a: 14) on a massive scale.

Rose’s proposition that “What parties say is not what parties do” questions the adversary model. Whether adversary politics or consensus politics characterizes Britain’s politics is in his view an open question. Questions like these need to be examined in “a rigorous analysis” of what governments “actually do” (Rose 1984a: p. xii). R. Rose lays claims for precisely this type of analysis in his study.

To study the role of different parties in government requires a longer time period. Rose chose to examine the period between 1957 and 1979 (in the first edition of his book) and 1957 to 1983 (in the second edition of 1984, which covers the first Thatcher government and the general election in 1983). The Conservative Party won the 1983 election “by a landslide giving the party the greatest number of MPs since 1935” (Rose 1984a: 163), while Britain’s Labour Party suffered a crushing defeat—at least partly due to its radical 1983 election manifesto, which critics had good reason to brand as “the longest suicide note in history” (Rose 1984a: 169).¹

Rose answers the question of whether parties make a difference in a novel way. His answer is, to quote a review of the book, “not much” (Gwyn 1981: 1295). In Rose’s own words the record of party government in Britain from 1957 to 1983 “rejects a ‘big bang’ theory of parties making all the difference to Britain’s government, let alone to society at large” (Rose 1984a: 152). Of course, there have been “*particular* differences in the way in which parties approach political problems and in the specific content and timing of a substantial amount of legislation” (Rose 1984a: 152). But the overall pattern is marked by “both continuity and change” (Rose 1984a: 152). Moreover, viewed in a long-term perspective, Britain’s “party government is best characterized by the dynamics of Moving Consensus” (Rose 1984a: 152). Moving consensus means a process of (usually

time-lagged) adaptation of one party towards a position that has been held by the opponent (Rose 1984a: p. xxxii). Muted party differences in policy-making and moving consensus ultimately result in “Consensus politics” rather than adversary politics (Rose 1984a: 185). And while politicians believe that their do’s and don’ts make a difference, there are forces “stronger than parties” around.

Rose derives his conclusions from an extensive examination of commonalities and differences of Britain’s Labour Party and the British Conservative Party in five major dimensions.

The first dimension concerns what voters and parties think. According to Rose, the data reveal a substantial amount of agreement about issues and policies among voters for both parties. The data show, he argues, “that the parties recognize this by campaigning with programmes that tend to converge towards the median voter” (Rose 1984a: p. xxiii). The data also indicate a considerable proportion of cross-class support: the Conservative Party mobilizes a substantial proportion of the working-class vote and the Labour Party wins a non-trivial share of middle-class votes. Moreover, the extent of interparty agreement on issues is mostly relatively strong and interparty disagreement is, all in all, comparatively weak (Rose 1984a: p. xxv).²

According to Rose, these data support the consensus model and reject the adversary model. Rose sees broadly similar patterns in election manifestos. Manifestos matter and some of them display clear-cut policy differences, such as in the 1983 election, but the difference is altogether limited (Rose 1984a: 72–3).

Legislation also supports the consensus model. While the Members of Parliament generally disagree with their opponents far more than their voters do, the major part of legislation in the House of Commons is consensual. This is largely attributable to the government’s exercise of political restraint. Rather than fully exploiting its all-powerful position, the government normally “does not use its undoubted legislative majority to carry through many controversial bills” (Rose 1984a: 90).

In contrast to this, reorganization of government and, above all, reorganization of the rules of the political game, such as changes in election law, devolution issues, and local government, provoke massive conflict. But there are again noteworthy exceptions—above all the reorganization of central government and the civil service. Both issues “have been treated as Consensus matters by successive Labour and Conservative governments” because “the leaders of both parties agree about the desirability of getting the machinery of government ‘right’ and training the ‘best’ people as civil servants” (Rose 1984a: 104).

Finally, managing the economy has been an area in which the conventional view of British politics suggests that Conservative and Labour governments differ to a large degree. Rose’s interpretation of data on government inputs in the economy and economic outcomes favors a different conclusion. Inputs such as minimum lending rate, public sector borrowing requirements and public expenditure, and macroeconomic outcomes, above all levels of economic wealth, economic growth, unemployment, take home pay, and change in the retail price index, were largely determined by secular economic trends. Even more important is that it made little difference in managing the

economy which political party was in office. Moreover, none of the parties in power between 1957 and the early 1980s was capable of stopping Britain's economic decline. In this period "Britain has become one of the poorer and weaker economies of Europe" (Rose 1984a: 138), regardless of which political parties was in power.

Party government may be a good idea, but it is by no means certain that it guarantees good governance. This is one of the general conclusions suggested by Rose's data on economic policy, reorganization of government, legislation, election manifestos, and preferences of voters and Members of Parliament.

Rose's conclusions also contradict the parties-do-matter view. This is his second major conclusion. His findings are also at odds with the adversary politics view. According to Rose, there are differences in the way Britain is governed in the period from 1957 to the early 1980s. But the differences "are not as expected" (Rose 1984a: 142). The differences in office between Labour and Conservatives "are less likely to arise from contrasting intentions," as the adversary model suggests. The differences rather arise "from the exigencies of government. Much of a party's record in office will be stamped upon it by forces outside of its control" (Rose 1984a: 142). Britain's parties are "not the primary forces shaping the destiny of British society; it is shaped by something stronger than parties" (Rose 1984a: 142). Stronger than political parties have been Britain's political institutions and processes, forces outside the parties' control, such as societal changes and public opinion, national and international economic "secular trends" (Rose 1984a: 141), and global politics.

APPRAISAL

Richard Rose's *Do Parties Make a Difference?* offers an interesting and thought-provoking analysis of British politics from the late 1950s to the early 1980s. It also represents a conceptually guided exposition and interpretation of macro-, meso-, and micro-level data from a wide variety of sources. Its author indeed used "the skills of a reporter, a historian, a data analyst or a mixture of all three," which Rose (1997: 139) recommended as a golden rule for social scientists. His book gives also a novel and interesting answer to a major question of political science and politics in general. Furthermore, the author of *Do Parties Make a Difference?*, a political scientist of American origin with an intimate knowledge of British politics, proves that he loves writing (Rose 1997) and that he writes very well. A reliable indicator of this capacity is that Rose manages to attract the reader's attention from the first paragraph of the book until the concluding section. Furthermore, Rose demonstrates in his study that he has taken the three golden rules for creating a good book again seriously: "Make it new," "Go for bear," and "catch your dependent variable" (Rose 1997: 134–5).

Reviewers have therefore rightly praised Rose's book as "a fascinating and highly readable study of the recent operation of party government in Britain" (Kunsmann 1981: 816) and an "excellent" contribution to party government (Gwyn 1981: 1296). *Do*

Parties Make a Difference? was also classified as “one of the more important (books) on British politics in recent years” (Sharpe 1981: 273). Even those critics who were not convinced by Rose’s rejection of the adversary model, conceded that his book was “well-written and stimulating” (Bogdanor 1981: 644) and “useful”, albeit somewhat “unusual” (Brand 1981: 97). And *The Economist* praised the author for having “the knack of asking important questions and providing intriguing ... answers to them,” even if these answers were “not always convincing” (*The Economist* 1980: 83).

Do Parties Make a Difference? was praised, but it also provoked harsh criticism. Not all reviewers were convinced that Rose was right in rejecting the adversary model. According to *The Economist* (1980: 84), Rose “caricatures” the adversary model of British politics. And in V. Bogdanor’s view, Rose “misinterprets the arguments of his opponents” (1981: 643). The adversary model, Bogdanor added, only claims “that the parties seek to implement contrasting policies” (Bogdanor 1981: 644). Of course, intentions have often little impact upon outcomes, as Rose’s book convincingly shows. That might be regarded as “a valuable conclusion, but not one which need worry Finer et al.” (Bogdanor 1981: 644).

Another reviewer pointed out that the adversarial politics model rests on a cross-national comparative view, above all on the difference between a plurality electoral system and proportional representation. A proper test of the adversary model would therefore require a comparative framework, not a one-country study as in Rose’s book (Sharpe 1981: 273).

These were not the only methodological peculiarities of *Do Parties Make a Difference?* which provoked criticism. Did Rose really operationalize precisely the adversary model and the consensus model (Brand 1981: 97; Spaeth 1981: 60)? Why did he not examine in more detail the nature and the causes of the “secular trends” which, in his view, governed most the management of the economy (Gwyn 1981)? And why did Rose not consider the hypothesis that at least some of these secular trends, such as increasing levels of public expenditure, were influenced by political decisions, for example on the size and the composition of social expenditure? Did Rose’s selection of policy indicators, a further critical choice, not suffer from a certain bias? Rose’s focus on economic indicators for example meant that he “gives short shrift to libertarian issues like divorce, censorship and the closed shop” (*The Economist* 1980: 84).

Moreover, quite a few reviewers of Rose’s book questioned the view that the parties in office did not produce clear-cut differences in policy outputs. Were party differences not stronger and more stable than Rose assumed in his deconstruction of the adversary model? Did the author of *Do Parties Make a Difference?* possibly understate “the frequency and importance of policy changes in fields like housing, taxation and trade union law” (*The Economist* 1980: 84)? Should an author interested in party differences not study more fully than Rose did “under what circumstances political parties are likely to make a difference in policy making” (Gwyn 1981: 1296)? Did the choice of 1957 to 1979/1980 as period of investigation not exclude governments with firm majorities and clear-cut policy differences before 1957? And did this decision not ignore evidence in favor of the adversary model (Sharpe 1981)?

THE THATCHER GOVERNMENT AND COUNTERFACTUALS

These questions were not unrewarding. For example, a significant proportion of the literature on the Thatcher governments suggests that long-term party effects have been stronger than the party effects which Rose identified in his 1984 book. The hypothesis of a “Thatcher revolution” (Fry 2008) may exaggerate the policy consequences of the Thatcher governments (1979–90), but it can hardly be disputed that the policy changes of these governments were significant and had a large impact on Britain’s polity and society—above all in industrial relations, economic policy, monetary policy, housing policy, education, and local government. Comparative studies on policy outputs of the Thatcher government and the more moderate policy consequences of Germany’s coalition between the Christian Democratic Party and the Liberal Party support this view (Lehmbruch et al. 1988). Furthermore, comparative data on economic outcomes and public expenditure indicate a dramatic decline in Britain’s inflation rates since the early 1980s, a significant decrease in public expenditure as a percentage of GDP from the post-1984 period until the beginning of the twenty-first century,³ and a more moderate pace in spending on social policy than in those OECD nations which were governed by center or center-left parties (Schmidt 1997). It thus was indeed somewhat “premature to say that Mrs Thatcher’s government will have little or no effect” on the fluctuation around secular economic trends (*The Economist* 1980: 84).

A further difficulty facing the exercise of the kind Rose has so ambitiously mounted concerns the type of comparison. Rose is an expert in Britain’s politics *and* in making comparisons across both time and space (Rose 1984b; 1997). In this study, he focused on a longitudinal comparison across a quarter-century of British public policy. This had a cost; it omitted cross-national comparisons. The methodological result is that Rose gives a very rich picture of the processes of policy change over time within one country, while not being able to take into account counter-factual cases found in cross-national comparisons that operate at a high level of abstraction (see e.g. Schmidt, 1997). Significantly, on finishing this book he began a major comparative study of the growth of government, concluding that differences between programmes, not parties, have driven the growth of government (Rose, 1984b).

COUNTERFACTUALS AND FINDINGS FROM CROSS-NATIONAL STUDIES

Sharpe raised the counterfactual issue in his insightful review of *Do Parties Make a Difference?* Sharpe argued that Rose had refrained from constructing “a plausible

counterfactual” (Sharpe 1981: 274). But to discover the “true extent of the party effect” requires that one knows “what would have happened if one of the parties had not existed?” (Sharpe 1981: 274). In the light of the policy experience of countries in which a major leftist party, such as Britain’s Labour Party, does not exist, for example the United States of America, Japan, and Ireland, it is safe to conclude that a leftist party has a major impact on policy. Sharpe’s counterfactual needs to be complemented by cases, in which a major market-oriented secular conservative party, such as Britain’s Conservative Party, does not exist. The Federal Republic of Germany is a major example of this constellation of forces. The main opponent to Germany’s major party of the left, the Social Democratic Party, is the Union of the Christian Democratic Party and the Christian Social Union, its Bavarian sister organization. But in contrast to Britain’s Conservative Party, the German Christian Democratic parties are pro-market *and* pro-welfare state (Schmidt 2010b). For this reason, Germany’s Christian Democrats are far more moderate in economic policy and far more interventionist than Britain’s Conservative Party.

In the light of these counterfactuals, the correct answer to the question which Rose posed in the title of his book is “Yes” (Sharpe 1981: 274)—rather than “not much” or “yes and no,” which, according to Rose (1997: 136), Schumpeter would have chosen if he had read Rose’s study.

Sharpe’s counterfactual is particularly convincing because it receives support from findings of cross-national studies on the impact of parties on public policy. Quite a few of these studies identified significant party effects on public policy.⁴ Some of the party effects were short-term in nature, others were of long-term quality, and quite a few of them were conditional upon the impact of context variables. Most of these studies also pointed to significant policy differences between countries in which market-friendly parties were in power, such as in the US, and OECD member countries which were governed by center or center-left parties. Moreover, quite a few of these studies predicted Britain’s position both before 1979 and after the general election in 1979 reasonably well (see, for the pre-1979 period, Cameron 1978; Castles 1982b; Schmidt 1982, 1983). The alternation of Labour and Conservative governments and the presence of a strong left and a strong secular conservative party placed Britain in the middle of a left–right scale of the party make-up of government. And the presence of a left party in government in almost half of the period from the early 1950s to the late 1970s accounted for a significant, albeit not dramatic increase in the size of big government (Cameron 1978: 1255; Castles 1982b: 73; Schmidt 1983: 262–3). Furthermore, predictions derived from these studies suggested that a big change in government, such as in Britain in 1979, and a longer period of uninterrupted rule of the new party in office would result in massive policy changes.

PARTISAN THEORY OF PUBLIC POLICY

Rose's book raised important questions and answered these questions in a novel, interesting, and thought-provoking way. This study remains of great value for research on public policy even if Rose's main findings need to be updated in the light of more recent studies and data on post-1984-Britain and in view of findings of comparative studies on parties and public policy both before and after the general election in Britain in 1979. The findings from cross-national research and from within-country comparisons, such as Turner (2012), lend support to view that the question *Do Parties Make a Difference?* must be answered as follows: Parties do normally matter in terms of policy outputs and outcomes. Larger differences in the party composition of governments tend to be associated with, and are often causally related to, differences in the level and change of public policy indicators.⁵

Moreover, the comparative literature on partisan impacts on public policies has also identified conditions under which stronger and weaker partisan effects on public policy occurred. Particularly large party effects, for example, tend to be generated when the incumbent parties command a large majority in parliament. Large party effects can also be expected from a relatively homogeneous single governing party or a coalition government with moderate policy differences between the coalition partners. Moreover, parties tend to have a stronger impact on public policy when a relatively small number of veto players exists, when the opposition parties are divided, and when there is a substantial room to maneuver in national policies.

Furthermore, patterns of democracy, such as the difference between majoritarian and non-majoritarian democracies (Lijphart 2012) and the difference between federalism and unitary states make a difference on partisan impacts on public policy. The parties-do-matter view tends to fit unitary nations better than federalist countries. In unitary states, such as in the Scandinavian countries, Greece, New Zealand until the 1980s, and Britain, policy-makers can in principle capitalize on a significantly larger degree of political maneuverability than in decentralized federalist democracies, such as Germany and Switzerland. In the latter group of countries consensus requirements reduce the potential for large party differences in public policy. Policy-makers in decentralized federalist countries therefore tend to have less freedom of maneuver. Consider two non-majoritarian cases: first, an all-inclusive coalition, such as Switzerland during World War II and in most of the post 1959-period, and, second, a democracy, in which the major opposition party is de facto, via the second chamber of parliament, co-governing. A major example is Germany in most of the period of the red-green government (1998–2005) and in the case of the Christian Democratic-Liberal coalition which came to power in 2009. In neither case was there leeway for policy-making of the kind that standard partisan theory presupposes: There was no leeway, for example, for solo runs of the incumbent party A, followed, after a change in power, by solo runs of the former opposition party B. The freedom of choice available in

the non-majoritarian democracies tends to be restricted to choosing between bargaining, exit, and blockade of the decision-making process. When bargaining prevails, policy is normally premised on the lowest common denominator of the coalition partners. That denominator tends to generate a large degree of policy continuity and is associated with limited short-term elasticity in policy-making. Because policies result under these circumstances from extended compromise-seeking, it is impossible for the voters to attribute the output to the political actors in government or in the opposition party. Under these circumstances the causal patterns that partisan theory postulates for the relationship between voters' preferences, policy choices of government, and policy outputs do not exist.

“SOMETHING STRONGER THAN PARTIES”

All in all, the view that parties do matter in public policy⁶ can be regarded as a useful analytical instrument for a better understanding of the determinants of public policies in modern democracies. Compared with other hypotheses in the literature, partisan theory is relatively successful in explaining policy outputs and outcomes.

Of course, caveats have to be taken into consideration as *Do Parties Make a Difference?* also convincingly shows. The party composition of government is only one variable among a wide variety of determinants of public policy. Partisan theory needs to be complemented by alternative schools of thought in order to capture those forces which might turn out, in Rose's words, to be “something stronger than parties” (1984a: 142). In his comparative research on the growth of government, Rose emphasizes differences in the dynamics of programmes as a major determinant. Among other forces, the impact of social and economic factors on policy-making deserves first mention as Marxist and non-Marxist contributions on the impact of modernization processes on public policy suggest (Wilensky 1976). Power resources theories, such as Esping-Andersen (1990), are also a potentially important tool for explaining policy outputs and outcomes. Moreover, the political-institutional architecture of democracies tends to make a difference in policy-making efforts, policy outputs, and outcomes (Scharpf 1991). Furthermore, international factors are important determinants of policy-making at the level of the nation state. Major examples include the impact of globalization on the one hand and membership in the European Union on the other (Garrett 1998; Scharpf 1999). Last but not least, path dependence matters a great deal for policy making. The core of this view is the assumption that the choices for policy makers are massively constrained by the impact of political decisions in the past. “Policy makers are heirs before they are choosers” is a succinct summary of this view. The quote is not from *Do Parties Make a Difference?* It is the overture to Rose's second major study of British public policy in a comparative theoretical framework (Rose and Davies, 1994: 1) *Inheritance in Public Policy*.

NOTES

1. The author of the tag was Labour MP Gerald Kaufmann. The 1983 election manifesto of Britain's Labour Party, led at that time by Michel Foot, a representative of Labour's left wing, demanded a wide variety of radical policy changes. These included the renationalization of previously privatized industries, the abolition of the House of Lords, Britain's withdrawal from the European Community, and unilateral nuclear disarmament.
2. Although it must be added that the exceptions are not trivial. They include issues such as membership in the European Community, combating unemployment, income redistribution to ordinary people, promotion of comprehensive schools, and taking troops out of Northern Ireland (Rose 1984a: p. xxv).
3. General government total outlays (as a percentage of GDP) fell from 1984 (44.7%) to 1997 (40.4%), the year of the change in power from the Conservative government to the Labour government of Prime Minister Tony Blair. The decline in public expenditure continued until 2000 and was followed by a moderate increase in the following period and a massive expansion since the recession in 2008 (OECD, *Economic Outlook*, 2012/2: 231, and earlier issues of *Economic Outlook*).
4. See for publications prior to Rose (1984a): D. A. Hibbs's (1977) path-breaking analysis on political parties and macroeconomic policy, D. R. Cameron's (1978) masterpiece on the expansion of the public economy, E. R. Tufte's (1978) instructive study on the impact of parties and the electoral calendar on the management of the economy, J. D. Stephens's (1979) cross-national analysis of welfare states, and F. C. Castles's (1982a) edited volume on the impact of parties in a wide variety of policy areas, including unemployment, inflation, and the growth of the tax state (Schmidt 1982). For reviews of post-1983/4 studies on the relationship between parties in office and public policy see e.g. Schmidt 1996, 2010a, 2012. With the exception of a footnote on Hibbs (1977) (Rose 1984a: 195), Rose's book on party differences did not consider the cross-national comparative literature on parties and public policy.
5. The underlying causes of party differences are mainly to be found in differences in the parties' social constituencies, programs, leadership strategies, and exigencies of party competition and coalition building.
6. See for a detailed reconstruction of partisan theory and its relevance for explaining social policy outputs Schmidt 2010a.

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CHAPTER 29

JOHN W. KINGDON, *AGENDAS, ALTERNATIVES, AND PUBLIC POLICIES*

SCOTT L. GREER

SOME of the great works in public policy need sympathetic rereading to recover their contributions. Some of the great works in public policy need to have their legacies clarified after they were reduced to catchphrases used without comprehension. Some have had half their contents forgotten, perhaps deservedly. Some need to be brought back from remote library stacks and the case for them made from scratch.

John Kingdon's *Agendas, Alternatives, and American Public Policy* has none of these problems (Kingdon 2003). At the start of 2013, Google Scholar reported around 10,000 citations to the book, and it is conventional to teach it as the “multiple streams” perspective in political science.¹ It has extraordinary staying power, simplicity, and, remarkably for theoretically powerful political science, verisimilitude. Practitioners recognize the framework and its applicability. Scholars in political science draw on it. It is also a good and illuminating textbook of American politics in 2013, a noteworthy feat for a book researched and published between 30 and 40 years ago. Even if there is a large subsequent literature on, for example, American Congressional staffers, Kingdon teaches the reader enough to deal intelligently with them today (pp. 41–3).

This chapter first reviews what the book says, trying to identify the interlocking innovations that made it so important. It then argues that the book is in a curious position: endlessly replicated and demonstrated, but not as fully exploited as it could be. The rest of the chapter discusses potential theoretical directions that would go beyond further replication of an already well validated study.

WHAT THE BOOK DOES

Most major works are more cited than read, reduced to a handful of catchphrases. *Agendas* is probably a victor in the obscure competition to have its contents remembered—a tribute to the coherence, simplicity, and completeness of its architecture as a theory and a book. Even the less remembered aspects of the theory turn out to be cited in dozens if not hundreds of pieces.

Methods

One of the most striking things about *Agendas, Alternatives, and Public Policies* is its method. The book is rooted in the kind of qualitative research more commonly associated with sociologists than with political science. Interview quotes dot its pages, and quantitative evidence is mostly the presentation of coded information (about e.g. the frequency with which a given kind of person was deemed influential). Kingdon explained that the work was frankly inductive, starting with a very large program of interviewing. Rather than test a theory, the qualitative data was a rich seam to mine in the development of a theory. The simple method of choosing some policy areas and interviewing informed participants allowed him to gain a great deal of information without distorting the evidence.

Kingdon's approach is contrary to the kinds of approaches that view all social science through the prism of variable-based statistical interpretation (King et al. 1994). It is also contrary to a bureaucratizing tendency in qualitative research that turns it into the work of a junior interviewer with a computerized protocol. The book's approach is also contrary to a tendency in comparative politics, where a focus on country case selection is not always matched by interest in the selection of interviewees, formulation of questions, nature of the sample, and handling of the data. The data handling and case selection, discussed in an appendix, is a model for the commonsensical, but rigorous, collection and use of data.

Agenda-Setting and Alternative-Specification

The first conceptual innovation was to identify and distinguish two political processes that are clearly differentiated from the better known aspects of politics: agenda-setting and alternative specification. Agenda-setting is what the name implies; it is putting an issue on the agenda. An informed observer can almost always say what the agenda is, and it can be identified by counting things like legislative hearings, press articles, or surveys. It is primarily the property of the visible players in politics, politicians, and a

few other prominent elites; they talk about some things and not others. Kingdon did not discover agendas or their making. Rather, his distinctiveness was to take it as a dependent variable with very indeterminate content.²

It is not clear where agendas begin and end; he writes about the federal agenda, but there are also state and local agendas. Furthermore, it seems that agendas off of the big federal agenda (e.g. uninteresting to the President, Congressional leadership, and major media outlets) might have their own politics, just scaled down. What might look like off-agenda incrementalism in policy to an observer of the entire political system might look like a major agenda shift to a specialist. Sometimes, when there is a new government with lots of new policies and appointees, big and little agendas might shift at the same time; but at other times, agendas and alternatives shift in ever-smaller policy worlds whose interconnections are little exploited.

Alternative specification is a second, distinctive, process. It is the definition of specific policy solutions: emissions trading or carbon taxes for climate change policy, for example. It is much more the work of the “invisible” players, the civil servants, lobbyists, academics, specialist journalists, and legislative staff. They are interested in the substance and workings of policy, and remain interested in their policy area even when the political agenda is elsewhere.

The identification of these two processes already sets up Kingdon’s work as an alternative to the more technocratic existing theories of policy-making. If agenda-setting and alternative specification are both somewhat autonomous processes, with their own logics and players, then it is far from clear that a political system will identify a problem and search for a solution. Students of public policy who hope that identifying a problem and noting its “policy implications” will lead the political system to seek for answers will start to understand their disappointment. It also starts to identify what develops into Kingdon’s attack on the much more coherent and powerful theory of incrementalism, the theory that inertia and cognitive limits make changes in existing policies, budgets, and organizations small: why should the alternatives be small-bore, and why should leading politicians, when they collectively focus on an issue, reliably opt to tweak a few clauses of a law or nudge a budgetary allocation up or down by a few percent?

Notably, neither agenda-setting nor alternative specification is legislation. If anything, the book’s framework is a more complete guide to the whole policy process in some other countries, such as the UK or New Zealand. There, once the political stream (government) has agreed that something is on the agenda, and has chosen an alternative, passage is virtually guaranteed. The numerous veto points and ambient chaos of the United States political system mean that the trip from agenda to passage is unusually hazardous, while in a parliamentary, unicameral system, or any system with fewer veto points, it is possible (though sometimes getting less possible) to just collapse the decision phase into the agenda-setting phase (Zahariadis 1995).

Three Streams

The second conceptual innovation is the three streams: policy, politics, and problem. This came from Kingdon's interactions with some of the founders of complex systems theory at the University of Michigan. One of the foundational articles of the field, about a dean search, modeled the decision not as the rational, linear process of HR texts but rather as a "garbage can" (Cohen et al. 1972). A variety of diverse interests, objectives, and ideas all got dumped into a single decision, and what emerged came from the interaction of those ill-assorted objects (the resemblance to the garbage can stems from the diverse nature of the contents, which have no other obvious connection). A decision point led to different streams of thinking, interests, and participants coinciding and produced an outcome that came not from the formal process, nor the interests of any one participant, but rather from the element of timing and chance—what was in the can when the decision was made.

Kingdon adapted and reduced the number of parts in the theory. He kept the insight of the garbage can, which is that unlike things mix and what is drawn out at any given time is hard to predict. He then identified three different streams, each with their own logics and, often, separate players.

One stream is *policy*. This is the ongoing discussion of policy options among experts—the largely invisible academics, staffers, civil servants, and others who take an area of policy and make it their own, sometimes becoming legends in that world for their expertise while remaining unknown in the political world. This world is engaged in the spadework of alternative specification, discussing policy alternatives. It is also engaged in entrepreneurship and advocacy; the "policy entrepreneurs" Kingdon discusses are constantly looking for ways to promote their preferred policy solutions for whatever reason, and arguing with each other in a process that presumptively winnows out impractical ideas or ideas that diverge from shared commitments of the policy community. The discovery and naming of this stream, and the policy entrepreneurs within it, seems especially novel.

Much of the time the agenda is not where advocates would have it; poverty advocates carp when the agenda is on the environment, environmentalists complain when the focus is on tax policy. When the spotlight is not on their area, policy communities will be doing two things. One is rehearsing their act for when they get their moment on the agenda—polishing arguments, conducting studies, building or losing personal credibility and networks. The other is trying to sneak into the spotlight, refurbishing their proposal to make it a solution to whatever is being discussed. For the more cynical (practitioners or scholars) the problem is of sales: policy advocates have a product and they sell it as the solution to anything you want. Just as the comedy salesman pitches the product as equally good for removing oil from the garage floor and topping a desert, a policy advocate will propose a preferred policy as the solution to practically anything (as we see when defense contractors saw global health expenditures rising and promptly became interested in "human security," or with the many, not all of whom

have a financial interest, who think increased IT expenditure will solve essentially any problem). A sunnier way to put it would be that these experts and advocates are constantly updating a range of thought-through solutions to reflect today's problems.

The policy stream, then, is a world (Hecklo 1978). It has some more intellectual, politically peripheral people who are the academics—their creativity and ability to test ideas to destruction can make them relevant, but they are usually filtered through less academic entrepreneurs who like to play the game, are good at it, and locate themselves in the cities where the political game is played. There is almost always a policy stream, with entrepreneurs and some credible-sounding policy ideas. This stream and its place in politics attract metaphors. Kingdon spoke of streams. Or we can say that advocates are like horseshoe players, throwing their policies until they catch; or that policies are like Christmas trees, covered in justifications that advocates add as they try to connect them to a current issue.³

The *political* stream is a very different creature. It is where we find the more visible generalist politicians and their staff, including in some countries the highest level civil servants. “The agenda” is not what policy people in a given community talk about; it is what the political stream talks about. So the roving interest of the political stream is crucial, for without a politician policy debate is just talk.

Politicians have interests that much research discusses. Much empirical evidence and theoretical assumption points out that they seek office, with the unassailable argument that it is hard to get any policy made in opposition. The individual politician has his or her own incentives: to a good public and elite reputation. The party has its collective incentives as well—to please voters and supporters (hence the importance of the “national mood,” something the political stream has to find and interpret). In some systems at some times we can reduce the political stream to “what the leaders of the government in office want”; in others, such as the European Union or the United States, it requires a statement about a larger and less hierarchical group of players. The tension between the collective interests of parties, coalitions, and factions and the interests of individual politicians provides much opportunity for empirical work and journalistic commentary on political careers and strategies.

Once the political stream is interested, it is easy to see. Manifesto commitments, speeches, hearings, legislative proposals, media campaigns, and discussions by the elite press are all ways that political elites signal to the public and each other that they view something as a problem and want to solve it with a policy.

The core fact about the political stream is that its members are very busy and dealing with far more than they can handle. Politicians famously enter politics with broad agendas and find that the day-to-day life of a politician involves handling situations they would not have chosen. They and their staff become expert at balancing demands on their time as well as budgets (Fox 2013). So how do they choose, collectively, their agenda? They need a third stream—not just the policy that might be chosen and the politician who might choose it, but a reason for the politician to choose it. That is the problem.

Right there, at the introduction of the *problem* stream, we note a difference. Kingdon lists sources of problems: unforeseen problems such as disasters (he notes a number of transportation policies prompted by accidents: the financial collapse of a railroad, a plane crash); government statistics (which, if released, can look like a crisis for many reasons); legislative deadlines (a favorite of American legislators). They are “focusing events” that bring public focus to bear on a problem. The problem stream, unlike the political and policy streams, does not have people who inhabit it full-time (except, perhaps, journalists and government statisticians). Given the way problems emerge, it seems like it might be as much a geyser as a stream.

One of the simplest innovations, almost an aside, is one of the most elegant parts of the theory: the distinction between a problem and a condition (p. 109). A problem is something that is regarded, across the three streams, as probably solvable. A condition might be bad, but we cannot solve it, and so we live with it. Sometimes the nature of the problem stream, or a change in the policy stream, redefines a condition as a problem or vice versa, as with climate change. This is the high-political version of the basic office politics rule that one does not raise a problem without proposing a solution. It also connects Kingdon’s work with many analyses of how conditions become problems.

Window of Opportunity and Coupling

The “window of opportunity” then comes about when the three streams can come together: the politicians agree there is a problem that needs solving, and the policy stream contains responses and advocates for the responses. The confluence of the three streams, should they come together, produces an agenda item and specified alternative(s). As with any meeting, items on the agenda might not go to a vote, but when politicians agree that there is a problem and a suitable policy, then it is on the agenda and the window of opportunity is open. A policy might happen.

Coupling and the window of opportunity might be the most theoretically difficult parts of *Agendas*. It is much easier to tell when they have happened after the fact. Part of the difficulty is that, while the original garbage can model had an exogenous decision point (a dean had to be hired), the actual moment of set agenda and specified alternatives is endogenous to Kingdon’s model, which in turn encompasses much of politics. The main response is empirical: the political agenda can be identified by looking at a variety of instruments such as press coverage, legislative activities, and manifestos. The specified alternatives can then be found by reading the relevant texts and interviewing. The theoretical difficulties of figuring out whether there has been coupling, whether things are on the agenda, and what the alternatives are, might actually be worse than the empirical ones; it is a stock interviewer’s tactic to open an interview by asking “what is on the agenda in your area?”

Coupling does not need to happen; nothing means that three three streams ever flow together. Likewise, coupling—with the consequent presence of alternatives on

the agenda—does not need to produce action. It is common to read *Agendas* as a textbook of politics, but it clearly demarcates the agenda-setting process from the decision process, about which Kingdon wrote a different book (Kingdon 1973). The size of the gap between agenda-setting and legislation is a variable, over time, country, and policy field.

Ideas: Not Just Important, But Researchable

The theory is interesting for its handling of ideas and probabilities—in other words, its handling of the stubborn problem of social sciences that its objects of study have free will and use it to act on ideas. Handling ideas in social sciences is not easy. In the “three Is” undergraduate teaching framework of many politics classes, ideas tend to turn into either extensions of interests and institutions, or start to take over both. The instability is natural—rules and interests are indeed located in people’s heads and communications, so ideas are everywhere, and people tend to have ideas that reflect their social roles and paychecks, so interests and institutions permeate ideas.

Ideas are vitally important in *Agendas*, and have a much more stable and definable position than in many theories. First of all, they matter; the decision process is about decisions between ideas. Even if an idea is a transparent reflection of somebody’s private financial interest, it is still an idea when it is proposed and gets onto the agenda. Second, alternatives are not necessarily pure creatures of ideology or interest; they exist and live or die in a definable policy world where clear social processes (publication, academic credentials, civil service hiring and role, rich donors for think tanks, consultancy firms’ search for the next big thing) are at work. They can be studied in a perfectly pragmatic way: what is the network of people and interests propounding something? How near or close are they to the political stream, and how skilled are they in political entrepreneurship? Third, the ideas have their own autonomous logic, particularly when they are in the hands of the policy stream, that drives anybody discussing them to certain consequences. Policy-making is full of such trade-offs: some things are costly, or bring consequences that must be gamed out. The separation of agenda-setting and the decision process in *Agendas*—in the United States especially—explains how a neat idea can turn into mysterious hodge-podge of a law. It should be no surprise that countries with a closer connection between the agenda-setting and alternative specification processes also have neater laws that are easier to explain.

An important extension—and empirical finding—in *Agendas* is that “nothing is new” (p. 141, also p. 71). On one hand, this has been known since, at least, Ecclesiastes 1: 9. On the other hand, the enormous amount of journalism, self-promotion, and empirical research about the origins and transmission of ideas suggests that the idea bears repeating. Even ideas with a comparatively clear and specific lineage, such as the Health Maintenance Organizations of Kingdon’s case study, turn out to be rebrandings or tweaks of much older ideas. This is what the sociology of the policy stream would

lead us to expect, as would the highly strategic behavior of policy entrepreneurs, some of whom want credit for having ideas and others who assign the credit to people in the political stream (see also Page 2006).

Process and Probabilities

Finally, it is important that this is a probabilistic theory of political process. Intellectual roots in the garbage can theory and empirical roots in many interviews mean that it is highly sensitive to the role of chance in a way many theories are not. The whole policy stream, and policy community, might think one way, but an outlandish idea can sneak in from the fringes, escape most of the vetting, and become an alternative on the agenda. *Agendas* is comfortable with probability because of a deep structural difference between it and most theories, which try to explain outcomes such as votes in light of variables such as campaign finance or party factions. Because *Agendas* is about a process, it can comfortably state that the strangest things can come out of a garbage can. A more directional theory would have an uncomfortable residual to explain. It also has direct implications. For example, students of comparative policy sometimes ask why policies diverge. If policies come at the end of agenda-setting and alternative specification processes, then we should expect every jurisdiction to diverge because the odds of their having identical policy, political, and problem streams are vanishingly small (Costa-i-Font and Greer 2013; Greer 2006). The logical next move is not to investigate the fact of divergence, but rather the constraints on policy and mechanisms linking different agendas (Baumgartner and Jones 1993).

WHAT MORE COULD BE DONE WITH IT

It is frankly not very interesting to look at a journal and read, once again, that a study has found that Kingdon's framework explains policy development in some area, whether it is the European Union's new interest in "quality of life" (Bache 2013) or a social welfare program in Arkansas (Craig et al. 2010). The reader who is not interested in the statistics policy of the EU, or in practical learning from Arkansas for early childhood policy advocates, might be permitted to move on.

The endless output of such studies, in different fields, shows the usefulness of the framework when making sense of reality, but do we really need more confirmations of the basic usefulness of the theory at this point? Ten thousand citations should really make it clear that three streams can be found and interact.

Nor do they appear to advance the tournament of public policy theories whose opening whistle can be conveniently be dated to Sabatier's *Theories of the Policy*

Process (Sabatier 1999). It can seem like public policy research is stuck in an endless penalty phase in which every player scores (reading public policy journals, one imagines Team Sabatier and Team Kingdon booting balls late into the night, every ball hitting the net). The problem is that a variety of frameworks can adequately describe most events in public policy. These are all good theoretical frameworks; not much happens in policy that cannot be understood with the advocacy coalitions framework, or Kingdon's framework, or redescribed in the language of rational choice theory or historical institutionalism. So stacking up more demonstrations of their basic usefulness is not very theoretically productive. What such articles frequently are doing is simply smuggling an interesting case study and explanation of an interesting or consequential issue into a political science journal by making a low-stakes theoretical claim. The problem is that endlessly replicating Kingdon's work might discourage those who would theoretically develop it, or public policy in general. What ambitious Ph.D. student should be encouraged to publish one more of several thousand studies showing that Kingdon's book describes the movement of policy ideas, and perhaps politics more broadly?

Clearly, the most impressive program of elaboration is that conducted by Frank Baumgartner (once Kingdon's student at Michigan) and Bryan Jones. Their program is covered elsewhere in this book by Peter John. Their Policy Agendas Project mapped the three streams for American federal policy, creating a very large database of media and governmental activity that allows the user to watch the attention of the American political system shift around during the years. This approach is now being expanded to cover much of Western Europe (Baumgartner et al. 2008). In *The Politics of Attention*, they pursued Kingdon's attack on incrementalism to its logical conclusion, finding (to oversimplify) that when something does get on the agenda, it might very well change a great deal (Jones and Baumgartner 2005). Incrementalism, with very small increments, is what is happening most of the time in most policy areas because they are not on the big political agenda. Once the powerful generalists of the political stream are engaged, though, they might well opt for much more radical ideas nurtured somewhere in the policy stream rather than the small changes of incrementalism.⁴

Kingdon, in the second edition of *Agendas*, shows interest in the sort of complex systems theory associated with the Santa Fe Institute (pp. 222–5). This is wholly in keeping with his comfortable acceptance of probabilities and complexity. What is less clear is whether complex systems theory will produce findings that bridge the highly abstract and the very specific. Like many previous efforts to develop mathematical models of politics, there is a big gap between the abstraction and the local circumstances, and when an abstract model is tested locally, it is never clear whether we have a confirmation of the model or a case study of one place. Complex systems models might have no problem admitting the historical nature and contingency of politics, but that is not the same thing as doing the mid-level spadework to get the parameters right.

Geology: See How Time Erodes the Streams' Banks

One direction is to spread the analysis out over time, incorporating the insights of both practitioners and the many scholars who have validated the proposal that “policies create politics.” Kingdon finds that people in the policy stream—experts, academics, sometimes civil servants—are important in the specification of alternatives, but play little to no role in setting the agenda. This stands to reason; political energy and problems can both demand attention, but a professor with an idea is one of many people who need not get attention. Over time, though, many advocates try to change this. For example, policy advocates do not just focus on specifying alternatives; they will also try to create problems. This can be as simple as a press release or a conversation with a journalist that draws attention to a buried report or a hard to read but potentially damning statistic.

This is the biggest difference between Kingdon's work on agenda-*setting*, and the work of Cobb and Elder on agenda-*building*. Cobb and Elder focus on the efforts by different groups to get onto the agenda—whether it is outsiders trying to get in, or insiders trying to mobilize some enthusiasm outside the capital city's ring road (Cobb et al. 1976)—and the differentials of power that it reveals. This is a shift of emphasis from Kingdon, rather than a completely different approach; *Agendas* just files that under the many activities of entrepreneurs, and a good bit of resource mobilization might be the prerequisite to becoming a policy entrepreneur at all.

Policy entrepreneurs can also work to create or disguise future problems to their own benefit. At one time, more data and further research are policy alternatives, ones that academics are famous for proposing. But if adopted, such alternatives change the politics just as surely as deregulation changes the structure of transport industries. Kingdon notes that data are a major source of problems; a bad statistic can (unpredictably) turn into a cause of action. This means that creating data series and public statistics is a policy alternative that makes an investment in future problems. Manipulating data problems can work in reverse as well. In the United States, Congressional allies of the National Rifle Association passed legislation that made it impossible for the federal government to finance or conduct research on gun violence, or even publish data (Mayors Against Illegal Guns 2013; Cagle and Martinez 2004). This amounted to choking off the policy stream and perhaps the problem stream as well—an investment by gun supporters in having fewer future problems and policy alternatives to contend with. Albert Hirschman pointed out the many worthwhile projects that could happen only because advocates avoided learning or disclosing their initial costs—the “hiding hand” of development (Hirschman 1967).

In the same way, actors in the political stream can try to manipulate the other streams (Cobb and Elder's focus). Much political discourse is elite-driven; if legislators or governments start to talk about a problem, then the press is likely to start talking about it as a problem. This is an unreliable mechanism, but it shows up when a politician begins to discuss a topic that he or she thinks will rebound

to electoral advantage. Sometimes politicians have their own preferences with regard to policy, leading them to try to manufacture or blow up problems, and limiting the ability of the broader policy stream to influence alternatives (Jacobs and Shapiro 2000).

In other words, over time there is some blurring in the streams. What is a policy alternative or advocate at one time becomes a creator of problems at another, and the political stream includes people who sometimes try to manipulate problems and alternatives. The problem stream, which unlike the other two streams has no permanent staff or residents, is the one that people in both other streams will frequently try to shape.

Know More

A second direction is to root Kingdon's analysis more deeply: know more. The book already has remarkable staying power across time (the United States has, after all, changed since the Carter Administration) and across space (e.g. Mätzke and Ostner 2010; Zahariadis 2003; Baumgartner 1989). This broad applicability, so frequently confirmed, means that we could turn the question around. Rather than ask whether *Agendas* applies to a given policy area (it probably does), we could use the streams in *Agendas* to motivate inquiry into political systems, using the three streams to identify what matters and what does not (Greer 2004). If an agenda with alternatives has only three ingredients, then it should be possible to investigate those three streams in order to identify and rethink biases and power in politics.

This second direction would expand the research agenda by examining the structures and biases of the political, policy, and problem streams—which is what Kingdon himself attempted in a subsequent book (Kingdon 1999). The asymmetry of the problem stream, for example, means that the origins of problems is a deeply intriguing process. It has also been thought about in depth (Stone 2011), and students of media and communications have done an enormous amount of work on it. To turn a condition into a problem, or a problem into a condition (Bachrach and Baratz 1962), is a great political achievement with concrete actors at work as well as luck. Research on media dynamics, public opinion, and framing (Snow et al. 1986; Goffman 1974) all mean that we can fill out the characteristics of a given policy stream in a given country.

Policy communities, likewise, vary and shape those streams (Page 2006). Consider the fates of the Clinton and Obama health reform efforts in the United States, as Kingdon did in the 2010 update to the second edition. The Clinton health plan picked one coverage expansion idea out of a diverse health policy stream. In the aftermath, the US health policy community converged on a very narrow range of ideas, marginalizing most alternatives to the basic mixture of policies that eventually passed in the Affordable Care Act (McDonough 2011).

Comparative civil service studies—and chapter 2 of *Agendas* is very much an American case study that should get broad reading—shed much light on the differences. In some countries, the civil servants will occupy the roles they occupy in Kingdon's study, as bit players who "staff out" domestic policy ideas, working out their details without ultimately deciding. In other institutional settings, they will have more influence. In some systems, whole blocks of policy experts go into opposition when the government changes, retreating to other employment and awaiting influence; in others, such as the UK, a given policy community will anticipate change in government and shift its preoccupations accordingly. In some systems, economists dominate discussion while other disciplines grumble; in others, lawyers are ubiquitous, though often fighting a rearguard action against economists. Academia exists in varying degrees of proximity to policy, ranging from the politico-academic grandee figures of Southern Europe, who frequently are employed in both politics and policy at the same time and might have their own newspaper column as well (e.g. Mario Monti) to the much more autonomous, critical, and politically irrelevant professors of the French university system (Fourcade 2010). Think tanks can fill the gap between academia and politics, providing more congenial homes for policy entrepreneurs than universities will necessarily offer—and also providing opportunities for people with money to buy the status and policy influence that a more autonomous and perhaps meritocratic university system refuses them.

The political stream varies. Studies focused on the decision phase, such as analysis of party strategies, can be read backwards into the political stream and its actions (Zahariadis 2003; Herweg 2013). For example, a simple spatial model of partisan competition suggests that some governments will systematically seek solutions that please voters on their frontiers with other parties (Kitschelt 1994; Greer 2004). Another model of parties, as coalitions of groups with policy preferences, makes explicit something that Kingdon usually leaves implicit—namely, some parties and political figures like some kinds of ideas and might even have their own entrepreneurs along with them (Cohen et al. 2008). The agenda-setting and alternative specification cycle can be nested, as with American candidates who endorse various policy ideas to solve campaign problems and then find themselves stuck with a policy that is hard to sell to some other constituency such as general election voters or legislators.

This agenda would lead to studies that make it explicit how and why different steams vary, using Kingdon's framework to shed light on the biases of ideas and ideological debate: why given countries or policy communities talk a certain way, and what exactly is starting to change when they shift direction. It would combine *Agendas'* advantages—attentiveness to ideas, attentiveness to chance, clear understanding of roles—with the power of other theories more focused on what party leaders want, how the press operates, or how individual politicians make their careers. It would be a window into a study of comparative public policy as comparative political development.

CONCLUSION

The prospect offered by *Agendas* is that scholars and practitioners can have a more finely grained understanding of political process. Understanding a given part of the world as the political or policy stream, and understanding the construction and use of problems, makes sense of everyday activities.

It has direct and practical lessons for those who would influence policy: *Know your place*. A scholarly article will have no impact unless an entrepreneur brings it forward, and even then its impact is uncertain. *Persist and be opportunistic*. One of Kingdon's interviewees compares policy entrepreneurs to surfers waiting for a wave. Max Weber compared politics to the boring of hard boards, and Kingdon's interviewees to surfboards. Both have their truth (Page and Jenkins 2005: 1): persistence is almost a necessary condition to policy entrepreneurship and membership in a policy community, but recognizing or forcing a coupling is an opportunistic act. While a very few ideas vault from marginality to favor, they are usually nothing new. *Understand other streams*. When a window of opportunity opens, it is probably too late to connect with people in other streams—policy entrepreneurs, politicians, and the journalists who trumpet problems are frequently connected, and those connections will shape what happens next. And finally, *accept chance*—good political advice at least since Machiavelli wrestled with *Fortuna*, and one whose evidence base includes *Agendas, Alternatives, and American Public Policy*.

But while it is a rare fate for a book to be an important textbook, almost unrevised, for more than 30 years, and to be validated as research in thousands of studies, it risks being trapped in the endless tournament of public policy theories rather than developed further. This chapter has sketched out two directions. One is to follow many practitioners' intuitions, by thinking about how over the longer term the beds of the streams may be altered and their dividing banks eroded. The other is to use it as the basis for a larger study of the biases in society and politics that lead to policies—solving backwards from biases in policies to discover the biases in policy, politics, and problems that produce different policies and reproduce different societies and polities. *Agendas* is a powerful tool for understanding policy-making. We should also exploit it as a powerful tool to understand societies and polities.

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NOTES

1. A label apparently given by Paul Sabatier (1999).
2. In contrast to e.g. Downs, who seems to view it as a subsidiary functional part of the issue-attention cycle (1972), or Cobb and Elder, who have a more complex definition of the agenda and focus on people trying to change it (1971).
3. All of these suggestions came at various events where I presented the framework to practitioners in the UK, EU, and US. It bears repeating that it would be counterproductive to present most frameworks in political science to practitioners.
4. In this they are discussing a topic of equal interest to historical institutionalists, who have not produced many really nuanced discussions of how abrupt major change happens (though see Tuohy 1999; Pierson 2004; Béland 2005) and have in part turned to exploring more incremental kinds of change that fit better with their incrementalism-focused theoretical programs (Streeck and Thelen 2005). It seems a pity that historical institutionalism has not solved its problem of explaining radical change with greater inquiry along the lines of *Agendas*.

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CHAPTER 30

MATHEW D. MCCUBBINS AND THOMAS SCHWARTZ, “CONGRESSIONAL OVERSIGHT OVERLOOKED: POLICE PATROLS VERSUS FIRE ALARMS”

WILLIAM F. WEST

“CONGRESSIONAL Oversight Overlooked: Police Patrols versus Fire Alarms” challenged the conventional view that the legislature played an ineffectual role in monitoring and controlling the bureaucracy. Writing in 1984, Mathew McCubbins and Thomas Schwartz argued that scholars misunderstood the purpose of oversight. Congress’s assessment of program implementation did not have to be comprehensive or even systematic to serve its interests as they related to the administrative process. Responding to fire alarms allowed legislators to allocate limited resources to issues that were salient to their constituents and to ignore the vast majority of bureaucratic actions that had little bearing on their electoral fortunes.

The following chapter describes and evaluates the theory of fire alarm oversight. McCubbins and Schwartz’s argument has had an important influence on how scholars think about Congress’s relationship to the bureaucracy. It has been especially prominent as a component of public choice perspectives that stress the legislature’s ability to control policy implementation through means other than the provision of a priori substantive guidance in statutory law. As such, it has been used to counter analyses that equate delegation and the rise of an administrative state with the decline of legislative power. Yet the original theory of fire-alarm oversight is also conceptually vague and internally inconsistent in key respects. In addition, empirical analyses have called into

question its central thesis that Congress relies predominantly on reactive strategies for gathering information about agency performance.

THE THEORY: POLICE PATROLS VERSUS FIRE ALARMS

There was something approaching a consensus among political scientists of the 1960s and 1970s that congressional oversight was sporadic and that it fell considerably short of the “continuous watchfulness” prescribed by the Legislative Reorganization Act of 1949. As Seymour Scher observed in an early empirical study, “In practice committee review is a spasmodic affair marked by years in which agencies are virtually ignored followed by spurts of committee interest in agency activities” (1963: 530). Lawrence Dodd and Richard Schott similarly argued 16 years later that, “Overall, congressional attention to bureaucratic agencies is haphazard. Even the committees most responsible for oversight . . . fail to devote the bulk of their hearings to investigations of agencies” (1979: 170). Scholars and even some legislators viewed the Congress’s lack of attention to program implementation as an unfortunate abdication of its responsibility, the systemic consequences of which were reinforced by the delegation of authority to the bureaucracy and the growth of presidential power in the legislative process (Huntington 1965). Oversight was a “vital yet neglected congressional function” (Pearson 1975: 88).

Several factors allegedly explained Congress’s failure to perform more systematic oversight. One was that it lacked the necessary resources. As Morris Ogul noted:

The plain but seldom acknowledged fact is that this task, at least as defined . . . [as continuous watchfulness], is impossible to perform. No amount of congressional dedication and energy, no conceivable increase in the size of committee staffs, and no extraordinary boost in committee budgets will enable Congress to carry out its oversight obligations in a comprehensive and systematic manner. The job is too large for any combination of members and staff to master completely. (1976: 5)

Even so, scholars offered a number of reasons why oversight was not a high priority for legislators. One was that it was not interesting. Although the level of enthusiasm varied across committees and subcommittees, some members did not care enough about program implementation within their assigned areas of responsibility to justify the effort oversight required (Scher 1963; Bibby 1966). As a member of the House told Ogul, “Nothing is going on of any particular importance on the Post Office and Civil Service Committee” (1976: 60). Another explanation was that legislators who belonged to the President’s party were reluctant to embarrass the administration by asking hard questions about program implementation. The ineffectiveness of committee-based

oversight was also allegedly grounded in its decentralized structure that paralleled the division of responsibility for authorizing and funding programs. Legislators were reluctant to criticize what they had created or to disrupt the cozy subgovernment relationships through which they and their bureaucratic allies served constituent interests (Scher 1963; Ogul 1976; Dodd and Schott 1979).

From a theoretical standpoint, perhaps the most influential explanation for the ineffectiveness of oversight was its inefficiency in relation to other means of garnering "support, influence, and prestige" (Scher 1963). David Mayhew (1974) argued in an influential book that legislators were self-interested actors who had limited time and other resources and who were motivated by the need to be reelected. The primacy that Mayhew and others accorded to the electoral incentive was consistent with historically low rates of turnover during the second half of the twentieth century and with the accompanying observation that congressional service had increasingly come to be viewed as a career.¹ With the exception of occasional opportunities to address sensational issues (such as the thalidomide controversy of the 1960s), oversight was a mundane activity that seldom uncovered problems that were of interest to important constituents. It suffered in comparison with the enactment of new legislation, casework, and other activities that were more conducive to the electoral strategies of "advertising," "credit claiming," and "position taking."

Enter McCubbins and Schwartz. Like Mayhew and most other public choice theorists, they assumed that congressional behavior could be deduced from legislators' desire to be reelected. This was a proximate goal even for politicians who were motivated by genuine policy concerns. Nor did McCubbins and Schwartz take exception with the observation that legislative attention to program implementation was sporadic. This was especially so if one focused on hearings and other formal manifestations of programmatic review. Where they differed from other scholars was in how they defined oversight and how they conceived of its effectiveness.

McCubbins and Schwartz used the analogy of police patrols and fire alarms to develop their argument. As with a squad car patrolling a beat in search of criminal behavior, the former consisted of proactive efforts to identify instances in which bureaucratic agents had failed to comply with the goals of their legislative principals. In contrast, the latter was a reactive strategy that allowed legislators to confront salient issues that arose in program implementation without attempting to monitor all areas of bureaucratic activity. Salient issues were defined in political terms as agency actions that were objectionable to important constituents.

Although McCubbins and Schwartz felt that Congress employed both proactive and reactive strategies to some extent, they argued that it preferred fire-alarm oversight as a more efficient use of its resources for reasons that were generally agreed upon in the oversight literature. One was that the bureaucracy was too large and too complex in its operations to allow for effective police patrols. Another was that considerations of sound administration, per se, were not particularly important to Congress. As with any endeavor, the effectiveness of oversight in this regard was contingent on one's assumptions about its goals. What legislators cared about according to McCubbins and

Schwartz was representing constituents who were affected by agency initiatives and who could support or hinder their efforts at reelection. These included voters within their districts as well as other groups with resources to influence elections through campaign contributions, endorsements, and the like.

A core premise of fire-alarm theory was that politically important constituents would complain about bureaucratic actions that they disliked. This shifted the burden of gathering information from Congress to others, creating greater efficiency in the use of scarce legislative resources. A complementary assumption was that many institutional features of the administrative process were designed to facilitate reactive intervention. As McCubbins and Schwartz noted:

Congress establishes a system of rules, procedures, and informal practices that enable individual citizens and organized interest groups to examine administrative decisions (sometimes in prospect), to charge executive agencies with violating congressional goals, and to seek remedies from agencies, Courts, and Congress itself. Some of these rules, procedures, and practices afford citizens and interest groups access to information and to administrative decision-making processes. Others give them standing to challenge administrative decisions before agencies and courts, or help them bring alleged violations to congressmen's attention. Still others facilitate collective action by comparatively disorganized interest groups. Congress's role consists in creating and perfecting this decentralized system and, occasionally, intervening in response to complaints. Instead of sniffing for fires, Congress places fire-alarm boxes on street corners, builds neighborhood fire houses, and sometimes dispatches its own hook-and-ladder in response to an alarm. (1983: 166)

Like other public choice theorists, McCubbins and Schwartz assumed that institutional arrangements could be explained in terms of political actors' self-interest. The view of administrative procedures as fire-alarm boxes thus challenged perspectives that explained constraints on the bureaucracy as reflections of normative premises about "how policy should be made" (Diver 1981). Although the Administrative Procedure Act's notice-and-comment requirements were often viewed in terms of "process values" such as rationality and responsiveness to affected interests, for example, McCubbins and Schwartz argued that their underlying goal was to ensure that constituents had ample warning of agency rule-making initiatives that they might wish to appeal to Congress. Other statutory constraints on administration allegedly served a similar purpose. Some of these were generally applicable requirements (e.g. the National Environmental Policy Act) and others were program-specific (e.g. mandatory consultation with advisory committees).

McCubbins and Schwartz employed an expansive definition of oversight in several respects. Unlike some scholars who equated oversight with programmatic, committee-based review, they included casework on behalf of constituents by individual legislators and their personal staffs. McCubbins and Schwartz also noted that oversight did not have to be labeled as such: it could occur as Congress was considering appropriations or authorizing legislation, and it could involve informal communications between elected officials (and their staffs) and bureaucrats. Although

these definitional assumptions were not unique in the literature, some early studies had equated oversight with formal hearings (e.g. Scher 1963). As discussed shortly, the original concept of fire-alarm oversight also extended beyond direct monitoring and influence by Congress to include indirect or passive control through statutory provisions that created participatory advantages for favored constituents in the administrative process.

Although it was hardly distinctive as an empirical observation, another important definitional feature of fire-alarm oversight was its assumption about congressional motives. Some scholars had assumed that the only legitimate role of oversight was to ensure faithful and effective compliance with statutory goals. They further assumed that the legislative process was the only proper mechanism for Congress to correct problems that it identified in the administrative process (e.g. Harris 1964). These assumptions were consistent with a "strict" or "formalistic" separation-of-powers analysis that drew a functional distinction between legislation, on the one hand, and execution/administration on the other (Strauss 1984).

In contrast, fire-alarm theory assumed that legislators did not particularly care whether administration was consistent with statutory objectives and that they were willing to exert direct influence over the bureaucracy without resorting to new legislation. The "congressional intent" that informed fire-alarm oversight instead consisted of legislators' current preferences. Indeed, McCubbins and Schwartz assumed that original statutory intent often did not exist. Like some other public choice theorists (e.g. Fiorina 1977), they argued that the delegation of policy-making authority to the bureaucracy often served legislators' electoral interests by providing opportunities to accept credit for administrative actions that constituents liked and to criticize and perhaps correct actions that constituents did not like. The purpose of oversight was often not to ensure that agencies promoted a priori policy objectives but to make policy incrementally in the context of individual issues that arose in response to agency initiatives. Elaborating on this, McCubbins and Schwartz observed that:

We see no reason to believe . . . that acts of legislation reflect well-defined or unalterable legislative goals. . . . Rather, legislative goals are refined, elaborated, and even changed over time in response to new problems—including complaints against executive agencies—and to changes in preferences and political alignments. In answering fire-alarm, congressmen not only enforce compliance with legislative goals, they help decide what those goals are. (1984: 171)

EVALUATING THE THEORY

The theory of fire-alarm oversight has had an important influence on scholarship in political science, public administration, and administrative law. As a crude illustration, the number of citations to the original article on Google Scholar is 1,786 and counting. Indeed, it is almost obligatory to acknowledge McCubbins and Schwartz

in any examination of legislative oversight in the United States. As discussed shortly, the contribution of fire-alarm theory lies as much in its heuristic value as in its accuracy as a description of reality. Still, recent scholarship has drawn on the concept to help explain legislative performance in areas ranging from the financial crisis of 2008 (Khademian 2011) to the confirmation of federal judges (Scherer et al. 2008) to the strategic use of citizen suits as a way of protecting congressional prerogatives in the administrative process (Smith 2006). The insights offered by McCubbins and Schwartz have also been applied beyond American borders to understand parliamentary–bureaucratic relationships in Western Europe (Saalfeld 2000), Israel (Friedberg 2011), and elsewhere. Recent studies have even relied on the concept of fire-alarm oversight to explain efforts by China’s authoritarian regime to control the bureaucracy through the courts (Ginsburg 2008) and inter-branch bargaining over the disposition of spent nuclear fuel in Russia (Stulberg 2004).

Perhaps the key contribution by McCubbins and Schwartz has been to point out that effectiveness in the conduct of oversight is dependent on one’s frame of reference. This in turn has potentially important implications for our understanding of Congress’s systemic role. Along with early work by Barry Weingast (1984) and others (e.g. Weingast and Moran 1983), the idea that legislators can effectively protect their interests and shape policy through oversight is an important underpinning of the so-called “congressional dominance” school (e.g. McCubbins et al. 1987, 1989; Bawn 1995, 1997). This perspective challenges the popular view that agencies are substantially insulated from political control by so-called “asymmetries of knowledge” and its corollary that the delegation of policy-making authority to the bureaucracy has led to a steady decline of legislative power since the New Deal. Although delegation is prevalent enough under the congressional dominance view, it is not synonymous with the abdication of congressional control.

This is not to say that the theory of fire-alarm oversight is above criticism. One charge is that McCubbins and Schwartz constructed a straw man. Although Scher (1963) employed a more restrictive definition of oversight, for example, his study of three legislative committees clearly described the essential dynamics that provided the basis for McCubbins and Schwartz’s thesis 20 years later. Like McCubbins and Schwartz, he argued that legislators allocated scarce time and effort strategically and that conducting proactive oversight was not rationally tied to their self-interest. And like McCubbins and Schwartz, Scher observed that legislative intervention in the administrative process was often informal and was precipitated by constituent complaints. As he noted, committees were the place for “the regulated to bring their grievances” and committee hearings were only necessary “when nothing else would do any good” (1963: 535).

Again, one might counter that the central contribution of fire-alarm theory is not its description of legislative behavior but its connection of that behavior to Congress’s institutional goals. Yet although this may be a valid point, the theory is subject to a number of other criticisms. Some are conceptual. Notwithstanding the pretensions of public choice theory to logical rigor, the arguments posed by

McCubbins and Schwartz illustrate fundamental contradictions in that literature that have yet to be resolved. Other issues are empirical. Although proactive oversight may have once been a low priority for Congress, evidence suggests that this may no longer be the case.

Ambiguous Distinctions

The distinction between police-patrol and fire-alarm oversight relies on a number of poorly defined assertions. The argument that proactive oversight is motivated by a desire to serve the public interest while reactive oversight occurs in response to "particularized" interests glosses over the vagueness of these terms and their ultimate subjectivity in many contexts. Do environmental groups represent broad national concerns about the well-being of future generations, for example, or do they represent the preferences of zealots who are willing to sacrifice low housing costs or food prices on the altar of their own ideology? Were congressional pressures on the Office of Education to extend eligibility criteria for student loans to the middle class a response to "special interests," as Harold Bruff and Ernest Gellhorn's (1977) contend in their influential study of legislative vetoes? In some cases, the distinction between general and special interests seems to be little more than a label for policies that scholars either like or dislike.

In so far as the distinction between public and parochial interests might be reasonable, moreover, McCubbins and Schwartz fail to explain why reactive oversight cannot occur in response to the former or why proactive oversight might not serve the latter. Were congressional inquiries into the intelligence failure that led to the invasion of Iraq not a reactive response to broad national concerns? Cannot proactive legislative monitoring of distributive programs occur in response to parochial interests? The distinction becomes further muddled by the assertion that the effectiveness of fire-alarm oversight has been enhanced by the rise of what many scholars refer to as "public interest groups." As McCubbins and Schwartz observe, "nowadays even 'disadvantaged' groups often have public spokesmen" and "sometimes Congress passes legislation, as part of its fire-alarm policy, that helps comparatively disorganized groups to act collectively" (1984: 172).

Another poorly developed distinction is the equation of the police-patrol oversight with committee hearings and fire-alarm oversight with less formal means of reviewing and correcting administrative action. This may be attributable to the fact that Scher (1963) and others used the infrequency of hearings as a measure of sporadic and ineffective oversight. In any case, there is no apparent reason to assume that oversight hearings cannot occur in response to complaints about agency behavior. Because of their visibility, in fact, hearings may be especially useful as a way for politicians to reassure constituents that bureaucrats have violated legislative intent. Nor is there an apparent reason to think that police-patrol oversight must take place through hearings. As discussed later, Joel Aberbach's (1990) work demonstrates that a good deal of

proactive information gathering by Congress occurs through networked relationships with agency officials and other informal means.

Perhaps the most puzzling assertion by McCubbins and Schwartz is that proactive and reactive oversight are respectively centralized and decentralized strategies. Here as well, the authors are open to the charge of attacking a straw man: if this distinction was intended to identify legislative behavior that had been overlooked, it ignored the fact that the decentralized character of oversight was already well-recognized in the literature. It was emphasized by Ogul (1976), for example, and was perhaps the central theme in Dodd and Schott's (1979) *Congress and the Administrative State*. Although it is not stated, the distinction offered by McCubbins and Schwartz may be an effort to reconcile police patrols and fire alarms with alternative models of decision-making that differentiate between top-down, comprehensive analysis and pluralistic "muddling through" (Lindblom 1959). Again, the authors characterize delegation and fire-alarm oversight as a way of establishing policy incrementally in the context of individual issues that arise in the course of implementation.

In any case, the association of proactive oversight with centralization and reactive oversight with decentralization conflates strategies that can vary independently of one another (Aberbach 1987, 1990). Just as police patrolling actually occurs within (decentralized) precincts, for example, there is no reason to assume that congressional committees and subcommittees cannot proactively monitor bureaucracy within their areas of responsibility. As discussed shortly, Aberbach (1990) demonstrates that this often occurs. Even individual legislators and their staffs sometimes take the initiative to seek out information about agency performance.

Conversely, there is no reason to equate reactive oversight exclusively with decentralization. Although Ogul and Bert Rockman observe that congressional oversight is usually "decentralized and localized in committees" (1990: 12), they add that the creation of ad hoc investigatory bodies is occasionally precipitated by "larger forces in Congress as a whole" in response to events that command national attention (such as Iran Contra and Watergate or the intelligence failures associated with 9/11 to use a more recent example). In contrast, Ogul and Rockman characterize centralized, proactive oversight as a "null set" (1990: 12, 13) from an empirical (if not a conceptual) standpoint. Although McCubbins and Schwartz note that Congress engages in "some" centralized police-patrolling, they offer no examples of the mechanisms through which this might occur.

Delegation, Ex Ante Controls, and the Purpose of Oversight

The most important conceptual issue raised by the original version of fire-alarm oversight is its inclusion of statutory provisions that impose passive, ex ante controls on bureaucracy. Again, these consist of "administrative procedures" that afford

opportunities for constituents to exert direct influence over program implementation (without involving Congress) or that are otherwise designed to influence what agencies do. Familiar illustrations are environmental and consumer-protection statutes that provide heightened standing for intended beneficiaries to sue agencies to issue regulations (e.g. Ackerman and Hassler 1980; Schwartz 1982). Ex ante controls may also impede administrative action as a concession to program opponents. An example here would be statutory constraints, such as hybrid rule-making procedures or cost-benefit analysis, that place a higher burden of justification on agencies that would undertake regulatory initiatives (e.g. West 1985).

Some administrative procedures may perform double duty by facilitating reactive oversight and by strengthening participatory opportunities for stakeholders. The requirement that agencies publish a notice of proposed rule-making in the Federal Register might serve both purposes, for example. The incorporation of ex ante controls as an element of fire-alarm theory is nevertheless problematic in several respects. Perhaps the most obvious is its inconsistency with a commonsense understanding of oversight as the review and correction of bureaucratic decisions. Legislators do not need to perform oversight in so far as they are able to design effective autopilot systems.

If this is largely a semantic objection, a more fundamental criticism is that ex ante structuring of the administrative process and ex post oversight are logically at odds with one another (West 1997). The former is an expression of original legislative intent in a political if not a substantive sense. Because institutional choices have predictable implications for who wins and who loses, that is, a program's design logically embodies the interests of whatever coalition was responsible for its passage (Moe 1989). It is in this sense that McCubbins and Schwartz characterize fire alarms as "rules, procedures, and informal practices that enable individual citizens and organized groups to ... charge executive agencies with violating congressional goals" (1984: 166). Yet ex post control via oversight will only promote original goals in so far as original goals exist and Congress's preferences remain stable. As discussed earlier, McCubbins and Schwartz reject both of these assumptions at other points in the exposition of their theory. In the first instance, they characterize oversight as a process through which legislators flesh out inchoate policy objectives incrementally; in the second instance, they observe that "the goals congressmen pursue in answering alarms related to particular laws need not be the goals they had in mind when they enacted those laws" (1984: 171).

This internal contradiction in the theory of fire-alarm oversight foreshadowed a critical challenge that would continue to vex congressional dominance theorists. Although McCubbins and others have subsequently distinguished between ex post fire-alarm oversight and ex ante "deck stacking" as alternative-but-complementary strategies for ensuring that agencies serve legislative interests (e.g. McCubbins et al. 1987), they have struggled to reconcile the tension between the two. If legislative "principals" want to ensure that bureaucratic "agents" do not "shirk" from or "sabotage" original congressional intent, then why put fire alarms in place that will facilitate control by future coalitions that may have different preferences? Conversely, if legislators want to retain

flexibility to respond to a changing political environment, then why stack the deck in favor of current preferences?

The tension posed by these questions reflects competing views of delegation in the public choice literature. As it has evolved, much principal-agent theory assumes that Congress delegates authority to the bureaucracy because it *has* to (e.g. Bawn 1995). Although this poses the risk that agencies will pursue their own agendas or constituency interests that deviate from legislative intent, it is often allegedly necessary given the legislature's lack of technical expertise and its inability to predict the specific issues that will arise in the course of implementation (so-called "asymmetries of information"). Yet a competing assumption is that Congress cedes policy-making discretion to the bureaucracy because it *wants* to. Under this view, delegation is motivated by politics rather than by concessions to the need for administrative effectiveness. It allows legislators to claim credit while avoiding the fallout that results from the articulation of specific policy goals, and it provides opportunities to intervene in the administrative process on behalf of constituents later on (e.g. Fiorina 1977). Some scholars even argue that a primary purpose of delegation is to allow "special interests" and their congressional and bureaucratic patrons to warp policy as it is implemented (e.g. Lowi 1969).

Although McCubbins and Schwartz explicitly endorse the latter view of delegation, their incorporation of *ex ante* controls is consistent with the former. Within a short time, moreover, McCubbins had moved into the camp of principal-agent theorists who view the problem of control as a matter of ensuring that bureaucrats comply with Congress's original "political intent" while reframing *ex post* fire-alarm oversight as a means to that end (along with *ex ante* deck stacking) (e.g. McCubbins et al. 1987, 1989). Barry Weingast underwent a similar transformation. Although his earlier empirical work on oversight stressed the relationship between the changing preferences of congressional committees (as imputed from measures of their members' ideology) and variation in the output of regulatory agencies (Weingast 1984; Weingast and Moran 1983), he soon joined McCubbins and others in asserting that the purpose of congressional control was to prevent deviations from original intent.

In important respects, then, inconsistencies regarding the purpose of fire-alarm oversight presaged conceptual issues that continue to challenge principal-agent theory. Does Congress stack the deck in favor of original coalitions or does it conduct oversight to serve the interests of later coalitions? That delegation occurs for different reasons and that one can answer in the affirmative to both of these questions militate against an elegant theory of congressional-bureaucratic relations. Similarly, the potential for *ex ante* and *ex post* strategies to work at cross purposes raises interesting questions about the efficacy of legislative control. It would be misleading to imply that public choice theorists have remained unaware of this tension. In large measure, however, the principal-agent literature has focused on the stringency of *ex ante* controls as a function of the perceived likelihood that the preferences subsequently brought to bear through congressional (as well as presidential) oversight will deviate from original intent.

Proactive versus Reactive Strategies

Scholars have questioned McCubbins and Schwartz's argument on empirical as well as conceptual grounds. The most prominent among these critics is Joel Aberbach (1990, 2002), whose *Keeping a Watchful Eye* still ranks as the definitive study of legislative oversight. Although fire-alarm theory does not address the issue in a direct way, for example, its assertion that congressional attention to administration is triggered by complaints from affected interests implies that most oversight is critical of agency performance and corrective in nature. In contrast, Aberbach's interviews with 89 committee staffers indicate that most considered themselves to be agency advocates within their jurisdictions and that none viewed themselves as opponents (1990: 166). This finding is consistent with an extensive literature describing harmonious "subgovernment" relationships in which legislators and bureaucrats cooperate to serve clientele who benefit from program implementation in particular areas (e.g. Maass 1951).

Aberbach's data also indicate that, although legislative staff often employ reactive strategies as well, many are highly knowledgeable and experienced in their committees' areas of responsibility and have well-developed networks that they use to seek out information about agency performance. As he notes (1990: 96):

Top congressional staffers present a picture of an environment rich in sources of information about agencies and programs. Aside from agency personnel (who, by all measures, are relied upon heavily by the committees), individuals, groups, and the mass and specialized media, and congressional sources (for example, members, staffers, and congressional support agencies) all provide information. Some information is gathered through formal congressional proceedings like committee hearings of official agency reports. Informal sources and the media are also widely used.

And later (1990: 96):

The responses of top staffers suggest a committee approach to keeping track of agency initiatives that is surprisingly active. While reactive monitoring is the modal category characterizing the monitoring styles of the committee units studied, a majority of the top staffers give answers on tracking indicating either active or intermediate monitoring. These respondents present a picture of committee units characterized by a monitoring style involving a significant level of initiative to seek out information. These units do not merely react to information that appears in front of them at the initiative of others.

Aberbach's explanation for the legislature's emphasis on proactive monitoring is grounded in institutional changes that occurred well before McCubbins and Schwartz published their article. Indeed, he makes the case that oversight was hardly a sporadic and neglected activity by the early 1980s. If hearing days are indicative of the growth of legislative activity more generally (that is, if informal oversight increased in a like

fashion), for example, the amount oversight roughly tripled during the 1970s. This was facilitated by a similar expansion in the number of committee staff as well as by the augmentation of other resources of in-house expertise such as the congressional Research Service and the General Accounting Office (Aberbach 1990, 2002).

Other evidence of Congress's growing emphasis on oversight during the 1970s might be cited as well. For example, the 1974 Report by the House Select Committee on Committees suggests that the desire for more and better oversight was at the forefront of efforts at legislative reform. Consistent with the Select Committee's recommendation, the number of House subject matter committees with oversight subcommittees increased from 4/16 in 1973 to 10/16 in 1980 (West 1998). Congress's emphasis on oversight was also manifested in its increased reliance on legislative veto provisions that strengthened its ability to influence policy implementation without resorting to the passage of new laws. Although this device dated to the Hoover administration, more than two-thirds of the vetoes that were in existence at the time of the 1983 *Chadha* decision had been enacted since 1970 (West and Cooper 1983; Cooper 1985).

Underlying the growth in oversight during the 1970s was the further decentralization of authority from committees to subcommittees—especially in the House. Because it afforded a means by which rank-and-file legislators could cultivate power and constituency support within particular areas (Bibby 1966), this development led to more oversight as a career-building activity. The expansion of oversight was also a function of broad systemic changes (Aberbach 1990). These included the continued growth of bureaucracy as a locus for policy-making discretion coupled with efforts by President Nixon to advance objectives through the administrative process that he could not achieve through a Democratic legislature (Nathan 1983). They also included fiscal concerns and the public's increased misgivings about the size of government and the effectiveness of many programs. There was a growing sense of disillusionment with Great Society initiatives, for example, as well as a backlash against regulation that began in the mid-1970s and that intensified into the 1980s.

To some extent, then, McCubbins and Schwartz's article may have been dated by the time it was published. The legislature may have preferred more exclusive reliance on fire alarms in an era when money was relatively plentiful, citizens were not especially concerned with the size of government, and tensions with the President were relatively muted and generally confined to areas other than administration. In turn, this may have been symptomatic of the low priority Congress placed on oversight according to many scholars. As Aberbach notes, "the explanations they [McCubbins and Schwartz] develop for the asserted predominance of a firealarm approach to securing information on agencies and programs dovetail nicely with standard explanations given in the 'Congress' neglected function' literature for the lack of oversight—greater opportunities in other areas" (1990: 99).

Again, McCubbins and Schwartz take exception to those who would equate reactive and sporadic oversight with congressional neglect. Be this as it may, Aberbach argues that the incentives and capabilities for proactive oversight have increased for the same reasons that oversight became more important generally:

If one assumes some changes in the environment and in congressional resources, ... the fire-alarm information strategy looks a little less attractive and the police-patrol approach more attractive. ... In an environment in which it is harder to pass new programs or expand old ones because of perceived budget scarcity and a skeptical public, where there are signs of public discontent with government, where the size and complexity of government concern citizens and elected officials alike, where Congress and its committees are struggling to protect their position after a period of intense conflict with the president and his appointees, where both congressional committee and support agency staff have been rapidly augmented (in part to help Congress address these changes), then it makes more sense for Congress and its committees actively to seek out information. (1990: 100)

One might add that this list of factors describes Congress's environment as accurately today as it did in 1990. Concerns over the size of government, alleged waste and inefficiency, and the economic costs attributed to bureaucratic regulation have hardly abated and are shared by many Democratic as well as Republican legislators.

Given the theoretical impact of McCubbins and Schwartz's argument, it is surprising that the choice between proactive and reactive oversight has received little empirical attention since the publication of Aberbach's study. This may reflect the difficulty of discriminating between these two strategies through means other than survey data that describe the impressions of legislators and their staffs. A recent exception to this neglect is an innovative study by Steven Balla and Christopher Deering (2012) that codes the determinants of legislative oversight hearings by several committees in both the House and Senate as being either event-driven or the product of routine, ongoing legislative activities. At least with regard to hearing-based oversight, the authors' findings are consistent with Aberbach's analysis. As they conclude, "police patrols have in recent decades become a prominent feature of Congress's bureaucratic surveillance system" (2012: 11).

The Effects of Oversight: What Fire-Alarm Theory Does Not Address

One should hasten to add that McCubbins and Schwartz describe important elements of reality. Few would take exception with their observation that oversight is highly decentralized (e.g. Dodd and Schott 1979). If Congress does not rely predominantly on fire alarms, moreover, a significant amount of oversight does occur in response to complaints from affected interests. Aberbach himself notes that "the modal (most frequently occurring) approach to tracking [agency performance] ... is reactive" (1990: 94). Yet if McCubbins and Schwartz offer a plausible if exaggerated argument that reactive oversight serves Congress's institutional interests, their analysis fails to discriminate among widely varying interpretations of its policy effects.

The view of congressional involvement in the administrative process as decentralized and driven by the confluence of “particularized interests” and members’ electoral concerns seems less-than-flattering on its face. Indeed, if oversight was once characterized as a sadly neglected function, some would no doubt contend that its expansion in the 1970s and beyond bears out the the adage to be careful what you wish for. Scholars today are just as apt to argue that there is too much oversight of the bureaucracy rather than too little. To its critics, legislative “meddling” exacerbates the irrationality and the inequity of a political system in which institutional power is highly fragmented and interest groups represent some elements of society much better than others (e.g. Schattschneider 1960; Lowi 1969).

Yet indictments of oversight are hardly uniform in their assessments of its effects. Some view it as an appeals process through which well-organized interests can subvert the implementation of programs that were intended to serve the public (e.g. Fritschler 1969; Bruff and Gellhorn 1977).² Others contend that oversight is driven by the mutually reinforcing interests of legislators, bureaucrats, and the clientele groups that agency programs serve (e.g. Maass 1951). Still other critics view oversight as chaotic free for all in which multiple committees and subcommittees give mixed signals to the bureaucracy and otherwise distract agency executives from their managerial responsibilities (e.g. Rabkin 1990; Lazarus 1991; Williams 2007).³

If it is often criticized in various ways, however, other analyses cast oversight in more positive terms as an activity that can serve as a counter to agency capture or the parochialism that often accompanies bureaucratic specialization. If oversight sometimes exposes agencies to competing perspectives, moreover, this may not be undesirable where administration requires the accommodation of conflicting interest and social values. Frank Baumgartner and Bryan Jones (1993) argue in their work on agenda-setting that entrepreneurial oversight is a key instrument in the disruption of established issue monopolies. The fact that the oversight process is a free for all is what makes it an arena for the “escalation of conflict” and the creation of more inclusive subgovernments.

The theory of fire-alarm oversight is consistent with analyses that stress both the negative and positive effects of congressional intervention in the administrative process. Reactive and decentralized oversight that is motivated by legislative careerism can reinforce the pathological effects of “interest group liberalism” (Lowi 1969), for example, or it can promote responsiveness and a healthy accommodation of interests. One might defend McCubbins and Schwartz and many other scholars with a positive orientation by noting that their analysis is not intended to assess the desirability of oversight. Still, the assertion that oversight serves Congress’s institutional needs begs the question of how it affects systemic values such as administrative effectiveness and political representation. These issues have become more salient with the increased importance of the administrative process as an arena for competition between the two political branches of American government and the constitutional issues that has posed. For example, the alleged parochialism of legislative oversight is often cited to

endorse stronger presidential control over the bureaucracy as a source of coordination pursuant to broad national interests (Moe and Wilson 1994; Kagan 2001).

CONCLUSION

The theory of fire-alarm oversight is vague in some respects and oversimplified and contradictory in others. If it proves to be an exaggeration, however, its influence on scholarship over the past three decades attests to the fact that it describes important dimensions of Congress's relationship to the bureaucracy. As is true of public choice perspectives in general, moreover, it identifies important concepts and propositions as a basis for institutional analysis. For example, the assertion that legislative involvement in the administrative process is sporadic and reactive furnishes a point of departure for Aberbach's (1990) examination of how staff gather information and of the systemic changes that have led to an expansion of proactive oversight. If it is difficult to sustain on logical grounds, the effort to integrate ex post and ex ante strategies under a single theory of legislative control draws attention to competing hypotheses regarding the purposes of delegation and oversight. In these and other respects, the best overall assessment of the theory of fire-alarm oversight is that it is highly stimulative.

NOTES

1. The rise of careerism was plausibly intertwined with a number of institutional developments that included the increased advantage of incumbency and the decentralization of power through the committee and seniority systems.
2. e.g. efforts at consumer protection by the Federal Trade Commission, alone, have fallen prey to legislative vetoes, restrictive statutory amendments, appropriations cuts, and informal pressures in response to complaints from used-car dealers, cigarette manufacturers, the insurance industry, and other well-organized and influential interests.
3. Such charges are especially common in highly visible areas, such as environmental protection (Lazarus 1991) and Homeland Security (Williams 2007), but even an agency as (relatively) small and non-controversial as the National Oceanic and Atmospheric Administration was subject to oversight hearings by eight House committees and four Senate committees between 2005 and 2009 (West 2011).

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CHAPTER 31

TERRY M. MOE, “THE NEW ECONOMICS OF ORGANIZATION”

JASON A. MACDONALD

In “The New Economics of Organization” (hereafter NEO) Terry Moe (1984) argues that scholars of public bureaucracy would benefit from the example of economists studying why firms exist in the marketplace. Moe notes that this question was in part a reaction to neoclassical economic theory which viewed the firm as a black box. Unsatisfied with this view both from the standpoint of its description of reality and because it left scholars with a poverty of understanding concerning the nature of organizations, a number of economists and organizational theorists conducted path-breaking research that expanded and improved knowledge about the benefits firms offer market participants and how firms operate.

Most important from the standpoint of political science research on the bureaucracy and on policy-making in a separation of powers system in which representative political instructions delegate authority to the bureaucracy, Moe expounds principal-agent theory, which he describes as integral to the NEO’s analysis of organizations. Briefly, principal-agent theory sets out a general relationship between a principal, who possesses legitimate authority over decision-making within an organization, and an agent, who is contracted to perform tasks for the principal. Several features of this relationship are essential to understand and are generally related to the difficulty that principals have in ensuring that agents comply with principals’ directives and work on behalf of principals’ interests. One feature is that agents possess greater expertise with respect to tasks than principals. For example, bureaucrats at the Environmental Protection Agency (EPA) (agents) know more about how to clean up pollution than members of Congress and the President (principals). Another feature is that agents possess “hidden information,” known also as “moral hazard,” about themselves that principals lack. Importantly, individuals often have priorities that lead them to value being an agent. However, these characteristics are likely to make them poor agents. For example,

pyromaniacs may be the last people one wants to serve as forest rangers. But they may very much like these positions and will not note their proclivity for starting fires on their job applications to the Forest Service. Another feature of the relationship is “hidden action” whereby agents are in position to engage in behavior that principals cannot monitor. These features empower agents to make decisions on behalf of principals that lead to “agency losses,” i.e. outcomes that do not maximize the interests of principals and that may be very different from what principals envisioned when they contracted with agents authority to act on their behalf. In short, principal-agent relationships are characterized by difficulties for the principals related to superior information that agents possess and the difficulty of monitoring what agents do on a day-to-day basis.

To reduce agency losses, Moe explains, principals resort to a combination of “contract design” and “monitoring.” With contract design, agents are constrained to pursue courses of action that limit their ability to use hidden action to impose agency losses on principals in service of any different goals the agents have due to moral hazard. Such contracts can also specify sanctions for behavior by agents that lead to agency losses, with the intention of reducing the incentives agents have for making decisions inconsistent with principals’ interests. With monitoring, principals utilize resources in an attempt to render agents’ actions observable. In doing so, principals can place themselves in a position to reduce agency losses.

In encouraging political scientists to adopt this framework to study how political principals, such as Congress and the President, influence bureaucratic agencies, Moe is careful to note that aspects of the political process necessitate careful application of the paradigm. For example, he notes that politicians are constrained by the Civil Service System (p. 768) when it comes to their ability to fire bureaucrats. Therefore, the ability of political principals to effectively employ contract design to specify sanctions for bureaucratic agents is limited. As a result of this, and other features of the political process, scholars need to consider what mechanisms political principals can and cannot employ effectively in working toward the creation of an institutional theory of bureaucratic policy-making. To be clear, what is institutional about such a theory is that, in the NEO, Moe envisions considering the institutional features of Congress and the presidency—and how officeholders within these institutions respond to constituents and interest groups—in order to understand how they go about trying to take advantage of bureaucratic manpower and expertise in order to meet their goals.

Moe did not leave it up to other scholars to follow up on his encouragement to create an institutional theory of the bureaucracy. Rather, he authored major papers elaborating on how rules governing political institutions combined with the incentives of officeholders, interest groups, and bureaucrats to affect bureaucratic policy-making. In this section, I discuss several contributions in which Moe forwarded ideas and challenges influential to trajectory of research on this topic. In some cases, these ideas were developed concurrently with other scholars. In other cases, these ideas originated largely from Moe. I also highlight how subsequent work, more or less directly, responded to these ideas. In discussing the progression of research, I elaborate on the arguments and evidence in the work I highlight. By necessity, I omit consideration of

some fine work; however, doing so allows me to focus in greater detail on many of the contributions that I examine.

INCENTIVES AND OPPORTUNITIES IN CONGRESSIONAL INFLUENCE OVER THE BUREAUCRACY

In one article, Moe (1987) critiqued earlier studies (Weingast and Moran 1983; McCubbins and Schwartz 1984) emphasizing that Congress dominates the bureaucracy, ensuring its decisions reflect Congressional priorities. Noting these studies assumed that control exists rather than established it, Moe argued that the authors confront neither the hidden action problem, which makes it difficult for any principal to ensure compliance from any agent, nor recognizes the presence of multiple agents in the US separation of powers law-making system (pp. 482–6). In the context of the challenges of hidden action and multiple principals, what might control look like? Moe considers various mechanisms through which Congress might influence agencies, including influence over agencies' budgets (pp. 486–8), new legislation (pp. 488–9), and influence over presidential appointments (pp. 489–90) before moving on to critique Weingast and Moran's (1983) analysis of Congressional influence over the Federal Trade Commission.

Although Moe's critique of this study spans most of the article, the most relevant contribution from the standpoint of subsequent research was his emphasis on the need to specify mechanisms through which control occurs. In discussing mechanisms of influence and control, Moe offers a skeptical assessment of Congress's ability to corral agencies. For example, leveraging agencies by threatening their budgets may be of limited utility. Among other arguments, he notes that cutting resources from agencies that pursue different priorities from Congressional majorities—or committees—does not improve agency responsiveness but merely reduces resources for programs. In addition, Moe argues hidden action makes it difficult for Congress to use any tool to influence agencies.

This pessimistic assessment of Congress's capacity, as a principal, to induce bureaucratic compliance is rooted in the principal-agent perspective that Moe expounded in the NEO. Specifically, Moe considered tools available to Congress and found them wanting in their ability to mitigate problems that lead to agency losses. Importantly, though, different scholars working on the same problem of whether Congress can control bureaucratic policy-making examined Congressional control tools as well and came to a different, positive, conclusion. In a series of articles, Mathew McCubbins, Roger Noll, and Barry Weingast highlight contract design, a feature of the principal-agent framework within the NEO, as a mechanism available to Congress to limit agency losses when delegating authority to the bureaucracy. In

addition to considering why Congress delegates more or less authority, McCubbins (1985) argues that complex policies—those policies where there is a lot of uncertainty about the connection between the policies government pursues and the effects of those policies—lead Congress to require that agencies adhere to a large volume of procedural requirements in making policy decisions. These requirements are meant to constrain the range of decisions agencies can make in order to prevent agencies from pursuing courses of action that undermine the political and policy interests of the law-making coalition. McCubbins et al. (1987, 1989) expand on this characterization of procedures as a contract design tool, arguing that such procedures allow Congress to engage in “deck stacking.” In deck stacking, Congress can “cause the political environment in which an agency operates to mirror the political forces that gave rise to the agency’s legislative mandate long after the coalition behind the legislation has disbanded” (McCubbins et al. 1987: 262). Along these lines “deck stacking” is a contract design mechanism in a principal-agent relationship—one intended by law-making coalitions to place bureaucratic agencies on “auto-pilot” to ensure that the agencies create policies that engender the political and policy goals of the law-making coalition.

Subsequent research on deck stacking, though, cast doubt on its effectiveness. For example, Balla (1998) examines the rule-making process conducted by the Health Care Financing Administration (HCFA) to alter Medicare reimbursements across physician specialties after Congress enacted legislation with the understanding that the agency would reduce specialists’, and increase general practitioners’, reimbursements to reduce costs. Balla finds, contrary to this expectation, that HCFA did not favor the interests of generalists who participated at higher levels during the notice and comment process of the rule-making. This finding—a direct test of the deck-stacking thesis—casts doubt on the effectiveness of this political mechanism in the hands of Congressional majorities to foster bureaucratic compliance.

Another line of research on Congress’s capacity to foster compliance by bureaucratic agents involves Congressional oversight. Oversight entails Congress—largely through the committee system—monitoring how agencies make policy decisions. Such monitoring is meant to negate the hidden action problem, though Moe is skeptical of its capacity to do so, and to allow Congress to instruct agencies to alter their decisions so that they correspond to Congressional priorities. Such oversight can take two forms: “police patrol” oversight in which Congress monitors agencies routinely to ensure agencies respond to Congressional priorities and “fire-alarm” oversight when Congress responds to problems brought to its attention by citizens and interest groups (McCubbins and Schwartz 1984). In a comprehensive study of oversight, Aberbach (1990, 2002) documents that oversight by Congressional committees increased substantially during the early 1970s, attributing this increase largely to increases in Congressional staff and to the increasing presence of divided government that made it harder for members to enact new legislation. In addition, by marshaling data from scores of interviews, Aberbach emphasizes that oversight is a carefully orchestrated process conducted at the committee level (1990: ch. 4), that members and their staffs participate in well-developed communication networks with bureaucrats, and that

participants in Congressional oversight believe that it is an effective—even if it is not a perfect—tool to foster agency compliance. Finally, Aberbach is clear that much of this oversight is police patrol in nature—that Congress conducts oversight routinely to monitor agencies proactively—although his coding of oversight hearings cannot speak to whether this is the case.

Balla and Deering (2013) conduct research that speaks directly to the volume of police-patrol versus fire-alarm oversight conducted by Congress, developing reliable coding criteria to distinguish committee hearings that were sparked by events (fire-alarm) from hearings that were the product of routine monitoring of programs by committees (police-patrol). Coding all the committee hearings in the *Congressional Information Service* reference volumes from eight (four House and four Senate) committees during four Congressional sessions in the four decades from the 1970s to the 2000s, Balla and Deering show that the overwhelming majority of hearings are characterized by committees conducting routine, or police-patrol, oversight. Of the over 3,000 hearings coded, roughly 84 percent of the House, and 85 percent of the Senate, hearings were characterized as police-patrol oversight. Despite Aberbach's (1990, 2002) authoritative work, this research is the first to code hearings for whether their origin was police patrol or fire alarm.

Together with Aberbach's work, Balla and Deering's research indicates that oversight is a much more effective tool at reducing hidden action than Moe recognized. That such a large percentage of such a large number of hearings is driven by routine committee work emphasizes that committees are committed to using oversight to reduce the problem of hidden action in agencies. This focus, of course, says nothing about Congress's ability to use oversight to reverse agencies' decisions—a matter I return to at the end of this chapter.

Moe also touched on the reauthorization of laws as a mechanism for Congress to induce compliance and judged it wanting. As part of a broader argument about reauthorization to the effect that Congressional committees control their agendas through short-term authorizations of programs, and protect policy compromises struck during the authorizations in doing so, Hall (2004) considers how committees make use of oversight to prepare to reauthorize bills. In particular, such oversight constitutes "police patrol" oversight because committees proactively conduct it as part of the reauthorization process. In examining the reauthorization of several programs, he shows that stakeholder groups whose interests were prioritized by law-makers on the relevant committees were able to secure statutory language in the reauthorization of Head Start, mass transit, and commodities trading that promoted the groups' interests (Hall 2004: ch. 6). For example, environmental groups who prioritized mass transit as a means of reducing auto emissions succeeded in placing language in the 1991 Surface Transportation Efficiency Act to the effect that metropolitan areas that did not meet Clean Air Act standards could no longer use highway funds to build roads. However, such areas could use highway funds to construct mass transit. By writing specific policies into the reauthorization of what was originally the Urban Mass Transportation Act of 1964, committees were able to promote environmental goals prioritized by

avored groups (Hall 2004: 79–80). From a principal-agent standpoint, then, the use of short-term reauthorizations enabled Congress to reduce the hidden action problem by employing information from stakeholders to provide agencies with specific instructions. In other words, the hidden action problem was negated because Congress legislated policy.

This perspective is consistent with Gailmard's (2002) formal model of oversight and bureaucratic policy-making. Gailmard models a legislature's costs of investigating bureaucratic agents and the cost to these agents of "subverting" legislative priorities. Among other formal results, Gailmard shows that law-makers investigate agencies through oversight when they disagree with agencies on policy matters. Critically, the information that the legislature generates from oversight enhances its ability to write policy itself, leading to less delegation. This result is very much in keeping, then, with Hall's finding that Congress employed information from hearings to reauthorize laws in a way that reduced bureaucratic discretion through the authorship of policy details that responded to stakeholders' priorities. Hidden action is much less of a problem for the legislature when it possesses the capacity to engineer effective policies itself.

More generally along these lines, research on discretion shows that Congressional majorities use this tactic to limit agency losses. This research theorizes about, and assesses, why legislatures provide more or less discretion to agencies to make policy (Epstein and O'Halloran 1999; Huber and Shipan 2002). To some degree, this research moves away from matters of political influence over agencies. As Huber and Shipan (2000) note, sometimes law-makers are able to influence agencies and sometimes they are not. Regardless, delegation occurs routinely. As such, it makes sense to model why more or less authority is delegated to agencies as opposed to worrying about whether control exists. On the other hand, it is clear that when law-makers write specific statutes, they reduce the ability of bureaucrats to alter the substance of public policy as it is embodied in law. To be sure, hidden action remains a formidable problem. However, the detailed specification of policies gives bureaucrats less "wiggle room" to use hidden action to alter legislative priorities. Therefore, in finding that legislative coalitions write more detailed statutes—and provide less discretion—to agencies when there is policy disagreement between agencies and the legislature, this research indicates that Congress—as well as state legislatures and legislatures in a comparative context—can reduce agency losses by building policy expertise.

Research on bureaucratic structure also finds that Congress manipulates the structures of agencies when it anticipates that Presidents will influence agencies to make policy decisions contrary to Congressional priorities. In particular, Lewis (2003) finds that Congress is more likely to seal off agencies from presidential influence by mandating that agencies are independent regulatory commissions—as opposed to placing agencies within cabinet departments or creating independent agencies whose appointees are removable by presidents—in his study of agency creation. Wood and Bohte (2004) observe similar findings. Law-making coalitions also manage how the courts will conduct judicial review to foster bureaucratic responsiveness to legislative goals (Shipan 1997).

A final mechanism through which Congress can influence agencies, also related to legislation, is via the appropriations process. Although Moe (1987) was skeptical of Congress's ability to use its power of the purse to limit agency losses, research on limitation riders in appropriations bills suggests Congress can block policy decisions on which Congressional majorities exist more easily than previously appreciated. MacDonald (2010) notes that limitation riders are provisions in appropriations bills that forbid agencies from spending funds for specific purposes. By virtue of being contained in appropriations bills that must pass lest the government cease to operate, they are easier for Congress to enact than bills that reverse agency decisions through the "normal" legislative process. Hundreds of limitation riders forbid agency decisions on an annual basis. In addition, MacDonald (2010) finds that there are a higher volume of limitation riders when there is reason to expect that Congress will experience policy disagreement with agencies under divided government, indicating that limitations are employed to prevent agency losses.

INCENTIVES, OPPORTUNITIES, AND PRESIDENTIAL INFLUENCE OVER THE BUREAUCRACY

In "Presidents and the Politics of Structure," Moe and Scott Wilson (1984) consider the incentives and institutional tools that Presidents possess to influence agency policy-making. Moe and Wilson argue that Presidents are unique in that they are held responsible for "national performance," especially that of the economy. This accountability prompts the President to desire a "unified, coordinated, centrally directed bureaucratic system" because it assists him in improving how well the government responds to challenges that, if not well addressed, prevent the President from being viewed as a strong leader by the public and the press, undermining how well he is viewed historically. Therefore, Presidents seek maximum leverage over agencies to solve problems on behalf of a "large, heterogeneous, national constituency" (Moe and Wilson 1984: 11–12).

To create a centrally directed bureaucracy, presidents avail themselves of formal control over the executive branch and "realities of political life" (Moe and Wilson 1984: 14). Within the executive branch, Presidents take advantage of their ability to organize the White House and Executive Office of the President (EOP). In turn, this administrative apparatus corrals the bureaucracy. Although influencing agency policy-making is challenging, Presidents are positioned to use the EOP and the White House to do so by "politicizing" agencies and by "centralizing" agency decision-making to as practical an extent as is possible under the President's purview. In "politicizing," the President places appointees within agencies to direct agency decisions. In "centralizing," the President creates rules that agencies must follow when making policy decisions. These

rules facilitate the President's ability to guarantee that agency decisions respond to his political and policy priorities (Moe and Wilson 1984: 17–19). Most notably, President Reagan issued Executive Order 12,291 to instruct the Office of Information and Regulatory Affairs within the Office of Management and Budget to engage in cost-benefit analysis of agency regulations, empowering the President to alter regulatory decisions to which he was opposed (Moe and Wilson 1984: 39–40).

Importantly, Moe and Wilson stress that much of the President's ability to influence the shape of bureaucratic structures and the substance of bureaucratic decisions is that the "realities of political life" grant the President "residual decision rights" that "allow him to take unilateral action at his own discretion when the formal agreement is ambiguous or silent about precisely what behaviors are required" (1984: 14). For example, after Congress did not reauthorize OIRA's existence, President Reagan instructed to the agency to continue with its reviews of agency regulations (Moe and Wilson 1984: 40). Although there was no law authorizing review, there was also no law forbidding it. By unilaterally instructing OIRA to continue to review, President Reagan acted in a gray area in which there was no clear legal or constitutional outcome that was dictated. In doing so, he ensured that the status quo in future negotiations with Congress would be one with regulatory review, improving his ability to make such review a permanent feature of the policy-making process, which occurred in 1986 (Moe and Wilson 1984: 40). In summary, Moe and Wilson identify both the institutional advantages that the President enjoys in influencing agencies as well as note the basis for these advantages—an executive positioned to organize his own administrative office that he can then use to oversee agencies, who, critically, benefits from ambiguous situations because of his ability to act unilaterally and his authority under the Constitution's vesting clause.

One major extension of Moe's perspective on how Presidents employ their residual rights through centralization to shape bureaucratic policies is through subsequent work on "unilateral" presidential policy-making. Moe and Howell (1999: 138) argue that the Constitution, in vesting the President with "executive power" and mandating that he "take care that the laws be faithfully executed," invites "presidential imperialism." This is the case because, given the combination of the President's advantage in moving first (making policy choices in the executive branch before Congress can act), given that executives such as the President must naturally enjoy leeway to operate with some discretion, given the magnitude of decisions made within the executive, and given the expertise and manpower at their disposal (Moe and Howell 1999: 137–8), Presidents will use these advantages to realize their political goals in a way that promotes the power of the Presidency at the expense of Congressional power. In addition, Congress faces collective action problems in responding to presidential actions that aggrandize power in the executive branch. Congress has been complicit historically in facilitating the growth of presidential power through delegation; statutory constraints on presidential discretion are not particularly effective (contrary to the argument made by McCubbins et al. 1987, 1989); and though the courts can constrain unilateral policy-making, courts rely on the President to implement many decisions, and, as such, have reason to yield to

Presidents so that their decisions have the effect that justices intend (Moe and Howell 1999: 141–53). Moe and Howell then document the growth in unilateral policy-making through executive orders and treaties, stressing that this growth was mostly welcomed by Congress and the courts.

Howell (2003) extends this work on unilateral presidential power further by extending it into a full-blown theory of policy-making in the US separation of powers system. Howell extends Krehbiel's (1998) pivotal politics model by altering the order of play through which changes to the status quo are made. The pivotal politics model assumes that Congress acts first in proposing to change policies. Importantly, Congress cannot alter all status quo policies so that outcomes reflect the priorities of Congress's median member. This is because multiple veto points in the law-making system empower other, pivotal, actors to block proposals, i.e. bills, to change the status quo. For example, Congress may like to move a policy away from what the President prefers. However, when the legislator who controls whether a presidential veto is overturned prefers the President's position to the policies that would be embodied in the law that Congress would enact, Congress cannot change the status quo. This explains why many policies experience gridlock. That is, even when a majority of law-makers in Congress would like to change the status quo, they cannot do so given that other, pivotal actors, prefer the status quo to what a majority of law-makers would enact. As a result, either small policy change, or no change at all occurs—even under unified government when one might otherwise expect it to be easy for Congress to enact laws.

This perspective constitutes a major contribution to understanding policy-making in the US. Howell, however, stresses that, due to delegation of authority to the executive branch, and to the President's constitutional position as chief executive, the President is often in a position to make a policy decision changing the status quo before Congress can pass a law. Therefore, in fact, the President moves first, as Moe and Howell (1999) stress. This power allows the President to create a new status quo. In creating a new policy, the President ensures that the legislator who decides whether a veto is overturned prefers the President's policy to the policy that would be embodied in the law that Congress would enact. In this way, the President can preempt Congressional law-making by taking unilateral action—often through executive orders directing the bureaucracy to make policy decisions. Importantly, bureaucratic decisions created through unilateral action are closer to the President's political and policy priorities than would be the case if Congress was allowed to move first and enact legislation.

In modifying the pivotal politics model, then, Howell brings important insights to understanding presidential influence over agencies. The origin of these insights stretches back to Moe's emphasis on the President's incentive to centralize policy-making (Moe and Wilson 1994) that is itself a product of Moe's exhortation in the NEO to pinpoint the incentives to, and opportunities of, actors within the separation of powers system to influence bureaucratic policy-making. In addition, Howell refocuses the understanding of presidential power. Whereas such power had traditionally been understood to be based in the President's capacity to persuade (Neustadt

1960), Howell forced scholars—not merely those scholars working in the areas of bureaucratic politics and policy-making in the separation of powers system but also presidential scholars and scholars of American politics—to recognize that critical presidential policy-making power is rooted in the institution of the Presidency. In this way, Moe's emphasis in the NEO to develop a thorough understanding of bureaucratic policy-making engendered a major contribution to understanding American political institutions related to—but beyond—bureaucratic policy-making.

From the standpoint of politicization, Lewis (2008) considers the goals of Presidents and bureaucrats, as well as the likely effect of populating agencies with political appointees, on the performance of agencies. In doing so, he develops predictions about when Presidents will politicize agencies by placing a high volume of appointees within them. Lewis argues that both Presidents and bureaucrats have preferences for the shape of policies over which agencies have leverage, noting that a high volume of appointees could reduce the effectiveness of agencies in realizing policy goals and that, all else equal, Presidents want agencies to be competent. These assumptions, grounded in interviews with elites knowledgeable about the appointment process, lead Lewis to predict that Presidents increase political appointees within agencies as policy disagreement that he experiences with agency personnel increases. In addition, Presidents decrease appointees within agencies as the degree to which Presidents prioritize effective decision-making within agencies increases (Lewis 2008: 58–9). In addition, Lewis predicts that Congress resists politicization when there is policy disagreement between the branches, as occurs during divided government (2008: 60–1). Finally, Lewis predicts that agencies will be populated with a higher volume of patronage appointees as the agency and President's policy viewpoints become more congruent. The basis for this prediction is that appointees share policy views with the President and wish to build careers in politics. The best way to do so is to gain experience in areas of policy friendly to both their, and the President's, ideological perspective so as to gain connections with stakeholders to whose interests they can make positive contributions. As Lewis notes, not many Republican appointees seek jobs in the Department of Labor (2008: 63–4). Among other empirical support, Lewis examines data over nearly a 20-year span from 1988 to 2005 on appointments to 256 agencies, finding support for these predictions (2008: ch. 5).

The finding that Presidents politicize agencies with appointees when the President disagrees with agencies' policy priorities is consistent with prior research on the effect of presidential appointments on agency policy-making. In particular, Wood and Waterman (e.g. 1994) show that agencies alter policy decisions to correspond to the President's priorities subsequent to appointments that are clearly intended to bring agencies into line. Given the effectiveness of this tool, then, it makes sense that Presidents use it when their priorities conflict with those of agencies.

In this research on appointments, Lewis provides support for, and extends, Moe and Wilson's (1994) emphasis on politicization as a means of influencing bureaucratic decisions. As noted, Moe and Wilson argue that Presidents strive to obtain control of agencies as means of pursuing their political goals. That Presidents politicize with

appointments when they disagree with agencies supports this perspective. However, Lewis's finding that Presidents back off when agencies face complex policy challenges highlights that Presidents also recognize that their interests require technical competence—something that harkens back to Moe's (1985) view that Presidents desire "responsive competence." Nevertheless, Lewis's (2008) analysis suggests that the President must trade these goals off, indicating that, in trying to foster competence, Presidents may be limited in achieving responsiveness. Likewise, in promoting responsiveness, presidents sacrifice competence, as is indicated by findings presented later by Lewis showing that the quality of agency performance declines with politicization through appointments. In addition, Lewis's findings highlight that Presidents are also constrained in politicizing agencies—both by Congress and by the priorities of many of their appointees. In summary, research on centralization and politicization has directly responded to Moe's exhortation to understand presidential influence over bureaucratic policy-making but has also contributed to research on American political institutions more broadly.

CONCLUSIONS AND FURTHER RESEARCH

Research on Congressional and Presidential influence on the bureaucracy makes it clear that these institutions, and their officeholders, have the incentive to shape bureaucratic policy, possess voluminous tools to do so, and spend time and effort in this vein. Research on the Presidency indicates that centralization and politicization enhance the capacity of President's to take advantage of bureaucratic capacity to achieve their political goals. In addition, Congressional tools allow law-makers to mitigate problems of hidden action as well as exercise a greater measure of influence over agencies than Moe (1987) anticipated, even if it remains unclear how or whether some of these tools work to bring agencies into line.

Along these lines, one avenue of future research must be to assess why Congress conducts so much police-patrol oversight, as Balla and Deering demonstrate (2013). On the one hand, there is substantial oversight. On the other hand, research on the separation of powers policy-making system stresses the difficulty of using oversight to influence agencies. For example, Shipan (2004) notes that agencies do not have the incentive to respond to oversight when they know that committees overseeing policy-making cannot pass new legislation, for example, because of the difficulty of overturning a presidential veto. But it is hard to imagine that Congress would conduct so much routine oversight if this policy-making tool was ineffective. This is the case especially because members are not conducting these hearings to win support from groups; that is, most oversight is not fire-alarm driven. In summary, the frequency of police-patrol oversight combined with the recognition that it should be difficult in the separation of powers system for Congress to use this tool to foster agency compliance suggests that scholars need to answer why such oversight

occurs. Either Congress conducts it for reasons having nothing to do with achieving policy ends or such oversight is effective at mitigating the hidden action problem and fosters Congressional influence. Shipan (2004) answers part of this question—oversight should be effective when agencies recognize that committees can overturn agency decisions with new legislation. But, due to Balla and Deering's (2013) work, it is clear that so much police-patrol oversight is done that much of it occurs when committees are not in a position to enact new legislation. How can we explain this oversight?

Along the lines of the discussion of Gailmard's (2002) work, perhaps one purpose behind the large volume of police patrol oversight is to produce the expertise necessary to avoid worrying about hidden action by bureaucrats. Of course, members of Congress may not think about it in as strategic a manner as is suggested here. Rather, in responding to stakeholders' interests through the process of writing new laws (e.g. Moe 1989), committees may build expertise to facilitate their capacity to author detailed statutes. That is, research through oversight may have as much, or more, to do with building policy expertise in order to write laws effectively as it has to do with uncovering a lack of compliance by agencies. Critically, though, in writing the details of laws, Congress limits agency losses.

Along these lines, it is important that scholars improve the understanding of the degree to which the mechanisms discussed are part of a "Two Way Street" (Krause 1999). By this, I mean that bureaucrats should not merely be envisioned as agents who may take the opportunity to engage in hidden action to pursue their own ends rather than ends valued by their political principals. Rather, bureaucrats are active participants in the political process. The importance of accounting for the feedback that the bureaucracy provides to representational political institutions is something Moe himself insisted on in the NEO (1984: 758) in order to move toward "a comprehensive theory of organizational behavior." As such, although I do not address bureaucratic feedback in this chapter, bureaucrats communicate with Congress—at a minimum during the oversight process—and the President and in doing so can influence the shape and scope of authority that they receive. Research on bureaucratic policy-making increasingly recognizes this feature of the policy-making process (e.g. Carpenter 2001; Krause 1999). For example, I have noted that one means of reducing agency losses has been for Congress to write detailed statutes. However, if the substance of these statutes was recommended, or even written, by bureaucrats themselves, then it would not be appropriate to view such statutes as representing control of agencies. Therefore, scholars interested in understanding the politics of law-making, policy-making, and bureaucratic structure would do well to conduct research designed at uncovering the circumstances under which agencies contribute more or less to the actions that their overseers take. In addition, scholars would do well to try to understand the circumstances under which political principals view such bureaucratic participation more or less favorably and incorporate bureaucratic views into decisions about structure and policy to a greater or lesser degree.

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CHAPTER 32

MATHEW D. MCCUBBINS, ROGER G. NOLL, AND BARRY R. WEINGAST, “ADMINISTRATIVE PROCEDURES AS INSTRUMENTS OF POLITICAL CONTROL”

SEAN GAILMARD

BEGINNING in the 1980s, Mathew McCubbins, Roger Noll, and Barry Weingast—collectively, “McNollgast”—introduced a new perspective on the origins and effects of the procedures that public bureaucracies employ when making and implementing public policy. This perspective was built on concepts of positive political theory and held that major institutions of the legal and procedural environment of bureaucratic agencies were designed purposively by Congress to influence the choices of those agencies in their application of administrative discretion over public policy. Their work became a touchstone of the theory of “Congressional dominance,” the notion, in contrast to influential work on the topic in the 1970s, that Congress is quite effective at directing bureaucratic agencies to pursue policies in its own interests. It created a seismic shift in how political scientists understand the political environment of public bureaucracies and has long since percolated into the thinking of scholars of administrative law and public policy-making from a variety of disciplines. Scholars from myriad fields continue to extend, critique, and grapple with the arguments and insights that McNollgast presented over 25+ years in a series of articles.

Still the most cited of these is the original paper by McCubbins, Noll, and Weingast, “Administrative Procedures as Instruments of Political Control” (1987),

published in the then-youthful *Journal of Law, Economics, and Organization*. At the time of writing, this paper has garnered nearly 2,000 citations on Google Scholar, at an accelerating rate over time. In this chapter I review and contextualize the seminal arguments of McNollgast, with special emphasis on this 1987 paper. I explain the content of the original 1987 article, explore the nature of its originality and reasons for its importance, and discuss its extraordinary fecundity in generating extensions as well as critiques.

MCNOLLGAST (1987): THE CORE ARGUMENT

This section lays out the logic of the argument in “Administrative Procedures as Instruments of Political Control.” The context of this argument in the literature prior to the article, and its influence on subsequent literatures, are laid out in subsequent sections.

The essential point of the original 1987 McNollgast paper is that administrative procedures can be reimagined as devices by which Congress enhances its control or influence over the policy decisions of the bureaucracy. This paper viewed bureaucracy-wide procedures, such as those created in the watershed Administrative Procedure Act (APA) of 1946, the National Environmental Policy Act of 1969, and the Freedom of Information and Government in the Sunshine Acts passed in the 1970s, through this lens. McNollgast’s related paper, “Structure and Process, Politics and Policy: Administrative Arrangements and the Political Control of Agencies” (1989, published in the *Virginia Law Review*) turned their core logic to understanding specific additions to or modifications of the general APA contained in the specific statutes which various agencies administer.

These papers do not present a specific model, either formal or verbal. They do not present formal hypotheses with tests or any structured empirical analysis. They do not present case-study analysis as scholars understand it today. What they do present is an interpretive lens through which to view the bureaucratic policy-making apparatus in the executive branch, and the compatibility of that apparatus with the democratic aspirations of the United States. The 1987 paper lays out several assumptions, unexceptionable in the positive political theory tradition (PPT) in which McNollgast worked (and continue to work), and works through the logic of how effectively legislators might be able to influence bureaucratic policy-making in light of those assumptions. Though the starting assumptions were in line with much of the PPT literature, the conclusions about bureaucratic control and interpretations of administrative structures were quite original, and at odds with much of the substantive work on these topics at the time. While there is no empirical analysis of a statistical or case-study variety, there are many examples of specific administrative procedures that are interpreted through the lens of the logical framework developed in the paper.

Logical Development of the Argument

There are several key premises in the original 1987 paper that lead to the conclusion that administrative procedures can be interpreted as devices to enhance political control. An important background assumption in this article and the entire McNollgast *œuvre* is that of rationality on the part of all actors involved in the institutional design and policy-making process. McNollgast stands as a signal exemplar of the PPT approach to the analysis of bureaucratic institutions, and it inherits this commitment to the rational choice postulate as an explanatory and interpretive device from that framework. In this context rationality implies that each political actor responds intelligibly and reliably to a given constellation of incentives created by its political-institutional environment, and that any political actor understands how any other political actor will respond to its incentives. More specifically, actors contemplate the choices available to other actors, understand how other actors evaluate the benefits and costs of various choices, and anticipate that other actors will choose the action most advantageous to themselves from their own point of view in any given situation.

Substantively, the key premise of McNollgast (1987) is that Congress is the architect of many pillars in the institutional environment of bureaucracies, and these affect bureaucratic incentives to choose among various policies, regulations, and implementation decisions. Though the paper does not articulate a specific model, embedded in its depiction of American policy-making is a sequence of choices by political actors that can be thought of as a proto-game. The sequence entails a choice by Congress of the legal-procedural institutions in which bureaucratic agencies act, and a subsequent choice by such an agency about the specifics of public policy or implementation. As Congress is a rational actor itself, we can therefore assume that it chooses the institutional environment for bureaucratic agencies that best suits its own purposes.

Those purposes, as stipulated by McNollgast, involve exerting influence or control over public policy. The Congressional incentive to exert this influence stems from the notion that policy outcomes affect the ability of members of Congress to hold their seats, and the common assumption in the PPT tradition that members of Congress wish above all to secure reelection. Given the complexity of modern policy, the "policy outcomes" of interest to members of Congress are not determined solely through legislation or any simple declaration of Congressional will. They are mediated by bureaucratic agencies, through the regulations they issue to give life to legislation and their implementation of statutes granting them regulatory or enforcement power.

Yet the ideal choices of bureaucratic agencies on these implementation, regulation, and enforcement decisions, left to their own devices, are unlikely to coincide with the ideal choices of members of Congress. The reason is that bureaucrats bring their own values to bear in making these decisions. These values may be informed by personal beliefs and preferences of bureaucrats, professional norms imbued through the specialized training bureaucrats receive, received wisdom within the bureaucratic agency, the perspective of interest groups with which bureaucrats frequently interact

and on which they may depend for future employment, or even (but not necessarily) aversion to effort or maximization of agency “slack.” This implies that the uncontrolled decisions of bureaucratic agencies will not coincide with those that maximize the electoral fortunes of members of Congress. In short, as recognized in much literature prior to McNollgast, Congress faces a principal-agent problem with respect to the bureaucracy.

Inasmuch as electoral fortunes depend on policy outcomes, policy outcomes depend on decisions of bureaucratic agencies, and the uncontrolled decisions of bureaucratic agencies do not coincide with those preferred by Congress, Congress therefore has an incentive to influence or control the decisions of those agencies. From a positive standpoint, members of Congress, as rational actors, recognize this problem and attempt to solve it as best they can. One approach to controlling those decisions is to monitor them and apply sanctions when and if they fail to coincide with those preferred by Congress. If Congress’s preferred decisions are known and understood by bureaucratic agencies, then the monitor–sanction approach follows a more or less standard logic of rational deterrence theory. The legal-institutional environment of bureaucracies in the US is such that the monitoring and sanctioning can be executed either by Congress itself, through oversight, or through the courts, through judicial review of agency actions.

McNollgast contend that neither legislative oversight of bureaucratic agencies nor judicial review of agency actions are sufficient to realize the desired level of influence by Congress over bureaucratic policy decisions. While these channels of influence may be important, McNollgast contend that they are not alone up to the task. With respect to legislative oversight, McNollgast contend that both monitoring and sanctions are limited in their efficacy. Monitoring through legislative oversight is costly, in the sense that time spent on it is time taken away from other valuable activities such as fundraising, campaigning, legislating, and constituent service (cf. McCubbins and Schwartz 1984). Moreover, inasmuch as bureaucratic agencies possess more information about their activities than Congress (and its support agencies such as the Congressional Budget Office (CBO) and Government Accountability Office (GAO)), monitoring alone may be insufficient to reveal the difference between agency policy choices and those preferred by Congress. The problem is one of eliciting private information held by the agency.

Compounding the costs and inherent limitations of monitoring by Congress are the limits on and costs to sanctions Congress can impose. Criminal liability for bureaucratic noncompliance applies only in limited cases, and cutting budgets or programs, while perhaps costly for bureaucrats, is a double-edged sword that may be costly for Congress too, inasmuch as the programs in question are electorally useful. Moreover, McNollgast point out that tying up the legislative process with attempts to sanction bureaucrats involves undesirable delay and distraction.

Another channel of monitoring of bureaucratic policy decisions is judicial review. As long as bureaucratic agencies have made discretionary decisions with respect to policy or enforcement, federal courts have stood to review those actions when agencies are sued in federal court. Administrative law is the body of law stipulating the principles of this review. Constitutional guarantees of due process for individuals

affected by bureaucratic decisions are one rationale for this review. In addition, the US Constitution does not permit Congress to delegate its power to make law (the “nondelégation doctrine”); thus, individuals can appeal to federal courts to ensure that agency decisions are made pursuant to specific statutory provisions enacted by Congress, and not reflective of extra-legal innovations made by agencies themselves. Federal courts can invalidate agency decisions that do not meet these standards of review.

Nevertheless, McNollgast argue that judicial review is also not a perfectly reliable mechanism of bureaucratic control from Congress’s point of view. First, legislative dictates are not always specific enough for a third party such as a court to determine whether an agency has in fact acted consistent with the wishes of Congress. Second, judges bring their own values to bear in judging, be they garden-variety ideological values of the sort other political actors are assumed to follow or legal norms inculcated by training and service in the legal profession. Either way, judges are not automatons that pursue and implement the will of Congress. Third, even if judges are aware of and wish to implement the will of Congress, they often cannot compel bureaucratic agencies to make a specific decision, and may instead simply remand a case for further consideration by the agency (e.g. when the evidence offered by an agency is insufficient to justify its decision in the eyes of a court).

Both legislative oversight and judicial review of agency actions are “ex post” mechanisms of control. They operate after the fact, after the relevant agency decisions are made. McNollgast recognized that Congress, in its search for influence, is not limited to these ex post mechanisms. Instead, they recognize, as did numerous prior scholars, that the procedures by which agencies render policy decisions can affect those decisions. These procedures affect both the set of actors that can present information to bureaucratic agencies and the extent to which agencies must consider that information in making decisions. In this way, these procedures affect the information presented to agencies about the consequences of their policy choices, both for the policy outcomes themselves and in terms of the political implications of their choices. Such procedures shape the incentives of agencies to render any particular decision in the first place. They are “ex ante” mechanisms of control.

McNollgast argue that administrative procedures ensure that bureaucracies obtain important information about the technical and political aspects of policy outcomes, and respond to this information. In the case of regulation or “rule-making”—the articulation by bureaucracies of general rules having the force of law—the Administrative Procedure Act of 1946 defines a presumptive set of procedures agencies must follow to issue rules that will pass muster in federal court. By far the most common approach to rule-making is “informal” or “notice and comment” rule-making. This requires agencies to provide public notice of their intent to issue a rule on a given topic in the *Federal Register*, solicit comments from interested parties about the rule in question, and (pursuant to judicial rulings on the topic, e.g. *Citizens to Preserve Overton Park v. Volpe* (1971)) produce a final rule (also published in the *Federal Register*) that is, at the least, not manifestly incompatible with the information presented by interested parties. If the agency fails to comply with these procedural requirements, its policy will reliably

be contested in federal court by some (negatively) affected party, and that policy will be invalidated by the reviewing court.

This notice and comment process, while defined in procedural terms, is in fact deeply political. It requires agencies to assemble information on the *politically* relevant constellation of interests on a policy, to wit, those that are organized enough to submit comments. McNollgast contend that these interests are those which are also likely to be organized enough to matter in electoral politics. After all, the goal for a reelection-oriented Congress is not to maximize the net social benefits of a policy, but rather to identify and respond to those interests which will take note of a policy and act on it in the electoral arena. In essence, notice and comment rule-making is skewed toward those interests that have the wherewithal to make their preferences relevant to Congress. It is not an accident, McNollgast contend, that this is exactly the sort of information Congress would tap if it had to make fine-grained policy decisions itself. Since administrative procedures furnish this information to agencies and force their responsiveness to it, they induce agencies to act in a way more congruent with Congressional preferences than they would absent these administrative procedures. In addition, since agency adherence to these procedures is enforced in federal courts, the procedures outsource the costs of compliance with the will of Congress to the judiciary.

One of the most enduring and influential ideas laid out in McNollgast (1987) is that of “deck stacking.” This idea stems from the fact that administrative procedures structure the influence of various interests in agency policy-making. If a given set of interests is privileged in this process, then the outcomes of the process will be disproportionately responsive to those interests. If Congress wishes to deliver a flow of benefits to a specific set of interests, then it can simply arrange an administrative process in which those interests are given disproportionate weight in agency proceedings. Even if a statute, on its face, contains a broad delegation that an agency should “go forth and do good,” the procedural requirements empowering specific interests will tilt the decisions made by the agency toward those interests. By devising such procedures, Congress can deliver a flow of benefits to those interests without even knowing the specific outcomes they desire to achieve. In addition, this flow of benefits to favored interests will persist even after the legislative coalition seeking to confer them has faded from power. Regardless of the fate of coalitions in the legislature, an agency operating under procedures that provide an advantage to specific interests will continue to tilt its decisions in their favor. In this way, deck stacking allows coalitions in Congress to tailor policy outcomes in its interests, without even knowing the specific contours of the outcomes it desires, and without having to secure its own longevity in the legislature.

Another lasting insight from McNollgast (1987) stems from the realization that a policy or program’s enacting coalition in Congress may not simply wish to present a gift to a specific favored interest. What Congress does in hammering out the details of a policy, thus determining its incidence of benefits and costs, is balance the competing claims of various interests. As the political relevance and balance of those interests changes, Congress would wish for the tilt of benefits and costs among those interests to change as well. McNollgast argued that one function of administrative procedures is

to replicate the conflict among interests to which Congress attends in the administrative process. As the organizational power of groups changes—a key dimension of their political relevance to Congress—so too will the force of their participation in administrative proceedings. If bureaucracies must respond to and change policy in light of these changes in the underlying political power of the interests with which they interact, then administrative procedures can induce bureaucracies to change policies over time in roughly the same way that the enacting coalition in Congress would want to change those policies, if it had the authority to do so. McNollgast refer to this aspect of administrative procedures as “autopilot,” because they induce relatively “automatic” (from Congress’s point of view) changes in policy in light of relevant political conditions. Autopilot means that Congress can attain its desired policy outcomes without dedicating additional time to legislative horse-trading. Moreover, combined with deck stacking, it means that an enacting coalition in Congress can deliver not just a flow of benefits over time consistent with its interests, but changes in those flows of benefits consistent with its interests, even after its ascendancy in Congress has lapsed.

While the theoretical logic of McNollgast (1987) is arguably best known for introducing the metaphors of deck stacking and autopilot to illustrate the power of ex ante controls in and of themselves, McNollgast also argue that ex ante controls are complements for ex post controls, in that the former amplify the effectiveness of the latter. Notice and comment rule-making requires the agency to announce its intent to regulate not only to interested parties, but to Congress as well. Requirements of a public record of comments, prohibitions on ex parte contacts between the agency and specific interests, and public disclosure requirements of the Freedom of Information Act and Government in the Sunshine Act mitigate the information asymmetry between Congress and agencies about the policy-making activities of the latter. These procedural requirements on the agency reduce the cost for Congress of acquiring information about agency activities.

Applications of the Argument

The logic just sketched provides the core theoretical framework offered by McNollgast (1987). The balance of their original 1987 paper applies this logic to interpret specific administrative procedures enacted by Congress. For example, the National Environmental Policy Act (NEPA) of 1969 required a large swath of agencies to consider the impacts of their policy decisions on the environment. This consideration was not even a twinkle in the eye of Congress in designing regulatory frameworks administered by agencies prior to the 1960s, yet in that decade the environmental movement gained considerable political ascendancy. Faced with this political change, Congress could have responded by changing each agency’s regulatory mandate in a piecemeal fashion to require attention to environmental impacts—a tall and prohibitively costly order. The approach in NEPA, by contrast, was to stipulate new requirements on agency policy-making in general. In this way, Congress designed a general administrative

procedure that, at much lower cost, shifted agency decisions in the preferred direction by empowering a new interest to be heard in agency policy-making.

Likewise, Congress has tailored requirements for agencies to subsidize participation by specific interests, such as consumer groups or small businesses, in regulatory proceedings of agencies that often make rules effecting those interests. These “intervenor funding programs” increase the attention that agencies must pay to these interests. Intervenor programs can be turned on and off by Congress relatively easily as political circumstances dictate. Both NEPA and intervenor programs affect the enfranchisement of specific interests to which Congress wishes to pay special attention by structuring the interest group environment and information to which agencies must respond. These effects are implemented through legislative channels that do not require Congress itself to assess the merits of any specific argument of these interests; instead, that requirement is outsourced to the agencies.

THE ORIGINALITY AND IMPORTANCE OF McNOLLGAST

Overall, McNollgast’s 1987 paper and closely related 1989 paper present a penetrating and wide-ranging lens for interpreting the effects of administrative procedures. The metaphors of deck stacking and autopilot vividly suggest that Congress can design institutions to ensure adherence of policy choices to the preferences of Congress, without incurring many of the usual costs of ascertaining political and technical information necessary to make an informed decision. These institutions can effect this adherence, in fact, even after a legislative coalition responsible for their design loses its clout. In short, McNollgast’s analysis suggests that a legislative coalition need have neither the information nor the political power to enact its preferred policy at a given time, and yet that policy may be enacted anyway. What it needs instead are administrative procedures to direct an agent along its preferred path.

This argument has both beguiled and provoked scholars for more than 25 years. The reasons for the significance of this argument are both normative and positive. Normatively, the argument is important because it offers new hope for the compatibility of the administrative state with constitutional precepts of legislative direction of public policy, and ultimately, with at least some notions of democratic control of the state. For about as long as there has been an “administrative state” in the US, scholars have worried about its legitimacy. If unelected bureaucrats make policy decisions with the force of law, is Congress *de facto* delegating its legislative power? Even as we overlook concerns over the nondelegation doctrine, does Congress, by delegating so extensively, lose control over the contours of public policy?

The argument ebbs and flows, but scholars have never had difficulty finding justifications for the administrative state in constitutional terms, despite reasons for continued

worry (and thus, continued scholarship). James M. Landis (1938) is but an early and forceful example. McNollgast's take on the effects of administrative procedures offers an original vision of a specific channel by which Congress might ensure ultimate control over the content of policy, even as it grants broad, sometimes plenary authority over various policy areas to individual agencies. By reconciling extensive delegation and involvement in policy-making by bureaucrats with Congressional determination of policy, McNollgast provide a rationale for optimism in evaluating the legitimacy of bureaucratic policy-making (but see Stephenson (2008) for a general argument that tight political control may not justify this conclusion). In turn, the problem of ensuring not just legislative, but popular, control over policy-making returns to the classic problem of popular control of Congress. McNollgast have nothing to say about this problem, of course, but it is one with which every constitution of a representative government must grapple. McNollgast simply offer reason to hope that a shift to administrative government will not make the problem of popular control of policy worse, or at least not as much worse as has sometimes been feared (see also Calvert et al. 1989).

This normative thread of the legacy of McNollgast dovetails with the positive aspects of its performance. The first of the positive themes affected by McNollgast deals with the actual extent of legislative direction of bureaucracies. Through the 1970s, influential literatures in economics and political science argued that, as a matter of fact, executive branch bureaucracies were quite poorly controlled by Congress. In economics, Niskanen (1971) initiated an influential line of argument arguing that bureaucrats, endowed with preferences to maximize their budgets, private information about the costs of government programs, knowledge of the legislature's demand for these programs, and some (poorly motivated) power to make take it or leave it offers of program budgets to Congress, are able to make programs far larger than Congress prefers, and correspondingly (in the respect his model is able to inform) poorly controlled by Congress. In political science, Lowi (1979) argued that broad delegations of policy-making authority to bureaucracies, languid legislative oversight of these bureaucracies, and capitulations of Congress as a whole to intense preferences in interest groups and Congressional (sub)committees, amounted to *abdication* by Congress of the responsibility and authority to govern. Needless to say, Lowi, like Niskanen, took a dim view of the prospects for Congress to exercise any meaningful control over agencies. Hecló (1977), though focused on interactions between career bureaucrats and political appointees within bureaucracies, rather than the bureaucracy and Congress, also cast doubt on the prospects for overhead control of the policy decisions of agencies. Hecló argued that information asymmetries, long tenure, and strong preferences all contribute to the ability of agency personnel to resist political direction from above.

McNollgast irrevocably changed the discussion about these issues. The paper centered the discussion of administrative institutional structure as a matter of legislative choice. Given that premise, an observer of these institutions, however byzantine or bizarre, must ask: what problem might this institution solve for Congress? Put differently: why would Congress design or passively tolerate bureaucratic institutions that significantly undermine its own goals? For instance, given how poorly Congress fares

relative to the bureaucracy in Niskanen's model—the clearest example of the genre to argue that not only does the bureaucracy run amok, but Congress knows it is the worse for it—the McNollgastian perspective insists that there must be more to this structure than meets the eye.

While this premise is foundational for the McNollgast analysis, it did not originate there. For instance, Fiorina (1981) explicitly proposes a germ of the same idea, arguing “Congress gives us the kind of bureaucracy it wants” (p. 333). The principal-agent metaphor had been asserted in this domain since at least 1975 (Mitnick 1975), and that metaphor trains attention on what the principal can do to mitigate (even if not completely eliminate) this problem. But the literature prior to McNollgast considered primarily *ex post* mechanisms of control to deal with this agency problem—mechanisms which the literature had, as had McNollgast, found too feeble to be very effective. What is original to McNollgast 1987 is their focus on specific aspects of the administrative procedures—*ex ante* mechanisms—as the object of Congressional choice, and their sustained application of PPT to explain numerous details pertinent to that focus. McNollgast did not simply raise the notion that Congress-as-principal might have strategies to deal with this agency problem; they identified a set of well-known institutional forms and procedures as examples of them.

The second positive theme affected by McNollgast deals with the legal analysis of administrative law. A major strand in this literature focused on the ability of administrative law to ensure due process for those affected by agency decisions, to prevent public decisions that were “arbitrary” or “capricious,” to ensure consistency of treatment of agencies before courts and individuals before courts as well as agencies, and the like (Stewart 1975). It is not a normative claim to assert that this is what administrative law exists to do, and that was the focus of much legal literature on the topic. McNollgast and sympathetic colleagues writing in law reviews made the politics of statutory administrative law—its conscious design with an eye to picking winners and losers in battles over policy—an integral part of the overall discussion in the legal literature on administrative law (Farber and Frickey 1991; Breyer et al. 1992; Kagan 2000; Bressman 2007; Stephenson 2008, 2010).

THE MCNOLLGAST TRADITION AND DEVELOPMENT OF SUBSEQUENT LITERATURE

Scarcely a paragraph of the core theoretical argument in McNollgast (1987) has failed to generate subsequent literature elaborating, extending, testing, or critiquing its theoretical framework. Some of the papers and books elaborating specific elements of their argument have become recent classics in their own right. Much of this work has continued in the PPT tradition of McNollgast, and added explicit formal models that encapsulate new insights.

Extensions and Applications of the McNollgast Perspective

McNollgast postulated that administrative procedures map into the degree of constraint agencies face in their policy discretion, that agencies often have greater information about policy consequences than legislators (as did many other scholars), and that greater discretion would offer agencies greater scope to apply their information in policy choice. Epstein and O'Halloran (1994) formalized these arguments (elaborated in Epstein and O'Halloran 1999) by leveraging the canonical Holmström (1984) model of delegation. Epstein and O'Halloran themselves ushered in a new phase of analysis of administrative structures and delegation, because their delegation model more pointedly reveals the agency loss which principals face from delegation (to wit, it inherently allows agencies to pursue their own policy agendas in their own model) than did McNollgast.

Gailmard (2002) extended this model to consider the possibility, raised in McNollgast, that agencies might seek to evade the formal constraints of administrative procedures in pursuit of their own preferred policies; Volden (2002) extended it to include the role of the President as another, competing principal in the design of statutory procedures, with results that echo the point of Moe (1987) (and McCubbins et al. (1989), who present a spatial depiction of policy drift in a system with multiple principals) that this may attenuate the ability of Congress to induce agencies to hew strictly to its own goals. Ting (2001) developed a model to formalize McNollgast's insight that applying sanctions to bureaucrats entails costs for Congress—in Ting's case, the (crude) sanction is budget-cutting, and the (endogenous) cost is that legislators are constrained to consider either "good but wasteful" policies or "bad but efficient ones." This result again crystallizes the agency loss of blunt and costly mechanisms of control. In addition, Huber and Shipan (2002) introduced a similar but distinct (i.e. not necessarily identical to Holmström 1984) model of delegation which formalizes a key idea in McNollgast, that fashioning constraints on agency discretion carries an opportunity cost for legislators, and the benefits of greater policy responsiveness must be balanced against this cost.

An important part of McNollgast's argument about the effects of procedures such as notice and comment rule-making is that it helps agencies acquire information about the technical and political consequences of policy. But McNollgast took this acquisition of information as given (and subsequent literature, especially Epstein and O'Halloran (1994) and related work, assumed that the expertise about all relevant policy outcomes already existed in the bureaucracy), neglecting the micro-level incentives of agencies to acquire information and its possible trade-offs. Bawn (1995) began to consider these trade-offs. Articulating a model that assumed that administrative procedures which facilitate political control by Congress come at the expense of agency expertise, and vice versa, Bawn showed that Congress may have to consider expertise of agencies and political control of agencies as competing goals, not necessarily both perfectly attainable from one set of administrative structures.

Gailmard and Patty (2007) considered the microfoundations of agency incentives to acquire policy information. The very rationale for the development of expertise, they argue, may be for agencies to tilt public policy in a direction consistent with the agency's own values. Expertise that must be applied in pursuit of some principal's vision of good policy may not be valuable to the agency, and thus not as attractive to acquire. Gailmard and Patty formalize this argument, showing that bureaucratic discretion over policy—and therefore Congressional deference to (rather than control of) bureaucrats may be a necessary (and implicit) “incentive payment” for information acquisition. In addition to establishing why there might be a trade-off between political control and expertise, this paper implicitly suggests that Congress has an interest in influencing important bureaucratic decisions besides the policy choice highlighted in McNollgast. To wit, bureaucrats' investment in information is also a decision, and it is in the interest of Congress to influence that too.

Gailmard and Patty (2012) elaborate this argument and point out that in many cases the key purpose of bureaucracies to Congress is to elicit information from or share information with political actors besides Congress itself, such as interest groups and the President. In such cases (e.g. eliciting information from regulated interests, or sharing information with the president about national security or economic policy), Congress's ideal bureaucratic agency may be unresponsive to Congress itself, and responsive to these other actors instead. The administrative structures that best serve the interests of Congress, therefore, may be ones that undermine its own political control and intensify its disagreements with bureaucratic agents (see also Boehmke et al. 2006). While Gailmard and Patty (2012) argue that Congress may not “control the bureaucracy” in any sense obvious to observers, their point is still squarely consistent with McNollgast in that it argues that administrative structures effecting this lack of control are the best ones Congress may be able to design for its own purposes. Congress may simply benefit from imperfect control over agencies, and if so, we should expect to see bureaucratic institutions consistent with that.

Another theoretical extension relates to the complementarity between *ex ante* and *ex post* controls claimed by McNollgast. This introduces a tension not explored in their article, but touched on in subsequent literature. If *ex post* controls are effective at influencing bureaucratic policy-making, and the original, enacting coalition does not exercise these *ex post* controls (because it has faded from power), then some other coalition is necessarily empowered to influence bureaucratic policy-making. In this sense, the complementarity between *ex ante* and *ex post* controls is a double-edged sword from the perspective of the Congressional coalition that chooses *ex ante* procedures. This original coalition might wish to enact procedures that ensure a flow of bureaucratic policy decisions consistent with its own interests, regardless of the interests of future Congresses. This provides a rationale for “red tape” in bureaucracies, raising the difficulty of exerting influence by future Congresses (Horn 1995). Yet this perspective is naturally consistent with the overall thrust of McNollgast, inasmuch as it interprets administrative structures as designed to ensure policy choices consistent with the will of an enacting coalition. The issue is simply that the enacting coalition

may wish to commit future, possibly different coalitions in Congress to follow its will, and this might rationalize structures that make oversight and ex post control more difficult, not less.

Empirical Tests

A large stream of papers has attempted to operationalize McNollgast in empirically testable terms. Though some of these papers find important cases in which operationalizations of McNollgast's core theory do not explain their findings, on the whole this literature has found that McNollgast's arguments significantly help in explaining the contours of administrative procedure. Some of this literature focuses on bureaucracy-wide procedures, as analyzed in McNollgast (1987); other papers focus on agency-specific administrative procedures, as explored in more detail in McNollgast (1989). This literature is large, and space precludes a full survey; only some of the more important contributions are considered here.

Bawn (1997) argued that legislators on committees with oversight jurisdiction over agencies would be less inclined to support ex ante controls over agencies (because they themselves have better access to ex post controls) than legislators not on those committees, and found this expectation borne out in legislation on delegation to the Environmental Protection Agency. Shipan (1997) tested the proposition, albeit closer to McNollgast (1989) (because it deals with procedural design of a specific agency, not bureaucracy-wide structures), that Congress chose judicial review provisions for the Federal Communications Commission in the 1930s with an eye to influencing ultimate policy decisions. He found that members allied with interest groups who believed the FCC would pursue their goals itself fought to curb judicial review, while those allied with interests threatened by the FCC fought for expansion of judicial review. Balla and Wright (2001) applied McNollgastian ideas to the analysis of advisory committees to federal agencies (as structured by the Federal Advisory Committees Act), arguing that these committees affect the flow of political and technical information from interest groups. They find, consistent with McNollgast (1987), that interests which were active in the legislative debate over a bill were more likely to be represented in an advisory committee to the EPA on the same issue. Potoski and Woods (2001) field an original survey of pollution control agencies at the state level and find that, in the view of these agencies and consistent with McNollgast (1987), administrative procedures have a significant effect on interest group and legislative influence. Wood and Bohte (2004) develop a theory of procedural design in the spirit of McNollgast, but focusing in particular on conflicts between the enacting coalition in Congress and potential future coalitions. They argue, in particular, that as enacting coalitions expect these conflicts to grow, they should be more likely to enact procedures that raise the difficulty for future legislative coalitions to influence agency policy choices, and find statistical results consistent with this expectation over a large span of time and range of agencies in the federal government. MacDonald

(2010) originated the analysis of “limitation riders” in spending bills, which preclude agencies from spending money for a specific purpose in the fiscal year to which the rider applies, as another avenue of legislative control over the policy decisions of specific agencies. MacDonald showed that these riders are more likely to be deployed when agency policy choices stray too far from the preferences of legislative majorities, demonstrating that they are important tool of procedural control over specific agencies.

On the other hand, Balla (1998) and Spence (1999) found more limited evidence of political control arising from procedural design. Balla (1998) found that notice and comment procedures in the Health Care Financing Administration did not induce it to be more attentive to intended beneficiaries of a policy change than to the traditional interest of physicians. Spence (1999) found mixed evidence for the effects of administrative procedures and structures on decisions of the Federal Energy Regulatory Commission (FERC): although these *ex ante* control devices did have an effect, FERC was at times successful in resisting political control by Congress. Spence’s findings amplify the point to be raised shortly, that the preferences of the bureaucracy itself cannot be neglected.

Critiques

The logic and applications of the McNollgast perspective has generated its share of criticism. Their original papers appeared alongside commentaries and critiques published in the same journal issues. Terry Moe (1987) published a critique of the “Congressional dominance” school, including but not limited to McNollgast (1987). With respect to McNollgast (1987), Moe contended that the concept of control is ambiguous, and McNollgast may overstate the prospects for attaining it. While McNollgast (1987) is replete with references to a central concept in the principal-agent literature, that principals in asymmetric information problems with agents rarely achieve their most desired results, this is a notion honored more in the breach than the observance in the original McNollgast paper. By the time the paper reaches the fine-grained analysis of administrative structures, the thread of agency loss is difficult to locate; it seems at least in their verbal expression that administrative procedures may well ensure that Congress’s most preferred policy is not only achieved, but that this occurs at lower cost than Congress would incur by doing the job itself. It is not clear what losses Congress faces in this depiction, and there is reason for suspicion of any principal-agent analysis implying this conclusion.

Another issue raised by Moe (and numerous other scholars, e.g. Hammond and Knott (1996), and indeed by McNollgast themselves in 1987) is that bureaucratic agency problems involve multiple principals. Multiple principals (or common agency) problems typically involve more agency loss for principals than bilateral agency problems, so McNollgast’s effective reduction of the problem to one between (a unitary) Congress and an agency may overstate Congress’s prospects for control. In particular,

the original McNollgast paper does not devote substantial attention to interactions between agencies and the President. Since Presidents have developed formidable tools of influence over the bureaucracy (Moe 1985) and bureaucratic structure (Lewis 2003), such as regulatory review by the Office of Management and Budget (Wiseman 2009) and veto power over changes in legislative delegations or other statutes to control agencies (Volden 2002), neglecting the President overstates the potential for unilateral control of bureaucratic policy-making by Congress. Similarly, federal courts exercise their own influence over agency policy decisions (Canes-Wrone 2003), further complicating the prospects for unilateral control by other principals (see also Whitford (2005) and Gailmard (2009)). One implication of this multi-principal interaction may be the existence of space for bureaucrats to articulate their own values and put their stamp on the programs they administer (Krause 1999). In addition, exertions of control by one principal may significantly alter the mix of control tactics pursued by others (Ferejohn and Shipan 1990).

Beyond the space for "uncontrolled" bureaucratic action carved out by multiple principal problems, several scholars have argued that McNollgast understates the role of the bureaucracy itself in public policy decisions. Brehm and Gates (1999) identify a significant effect of bureaucratic culture on policy and enforcement decisions. Krause (1999), Carpenter (2001), and Moe (2006) contend that bureaucratic preferences may affect legislative preferences, not only vice versa. Waterman and Meier (1998) contend more generally that the premise of goal conflict between principals and agents may be overstated. This does not critique the McNollgast logic in situations where there is goal conflict, but it does suggest that the scope of the argument may be more limited than McNollgast claimed.

McNollgast mention several times that one benefit of administrative procedures from Congress's point of view is that they make it difficult for bureaucrats to assemble coalitions of interest groups in pursuit of their own interests as against those of Congress. But Carpenter's (2001) epic presentation of bureaucratic network and expertise development in the progressive era suggests that this is more than a possibility. Carpenter meticulously documents how bureaucrats used both expertise and networks with interest groups to show how they achieved not just discretion over policy (as might be granted and controlled by Congress), but autonomy—the ability to pursue their own vision of good public policy even when explicitly at odds with that of key coalitions in Congress. To be sure, the episodes Carpenter depicts occurred decades before the creation of the administrative structures which McNollgast feature as, in part, designed to prevent these occurrences. Yet it is not clear how these structures would inoculate Congress from the effects of the bureaucratic networking Carpenter depicts, for none of it is precluded by modern administrative procedures. Moe (2006) presents an analysis, analytically quite different but substantively similar to Carpenter's in this respect, demonstrating contemporary bureaucratic interest group mobilization to influence the very preferences Congress brings to bear in legislative-bureaucratic interaction. In short, important work subsequent to McNollgast has indicated that the issue of bureaucratic interaction with

interest groups to insulate the pursuit of their objectives from Congressional control is not straightforward to overcome with administrative procedures. Nevertheless, this work is consistent with one version of McNollgast's point, which is that these problems would be more severe for Congress without current administrative procedures.

In light of Gailmard and Patty (2012), it is not clear that evidence of bureaucratic influence on policy or even autonomy is inconsistent with McNollgast, or at least a slight expansion of it. Principals can benefit from committing to leave policy-making authority to agents (because it provides an incentive to invest in expertise) even when their actions conflict with Congressional preferences, and can benefit from committing to make bureaucratic agents responsive to other political actors besides Congress itself. So findings of bureaucratic autonomy or influence of non-legislative factors on bureaucratic decisions are not necessarily inconsistent with the view that bureaucratic procedures promote the ultimate interest of Congress. On the one hand, this flexibility of the McNollgast perspective in its broadest terms raises questions of whether it is falsifiable even in principle. On the other hand, McNollgast, like much of rational choice theory in general, is important because it suggests the possibility that decisions which seem to undermine some actors' interests may actually be compatible with them. This is important if for no other reason than that it provides a conceptual lens through which to interpret observed arrangements.

CONCLUSION

McNollgast (1987) is important because it identifies the foundations of bureaucratic policy choice in the preferences of Congress, and it articulates an original and provocative argument about how administrative procedures establish that foundation. Several scholars before McNollgast had begun to make the first point (e.g. Fiorina 1981; Weingast and Moran 1983; McCubbins and Schwartz 1984; McCubbins 1985). But before McNollgast, scholars were either agnostic on the institutional sources of congressional influence (especially Weingast and Moran) or focused exclusively on ex post mechanisms (especially McCubbins and Schwartz) that, though perhaps more effective than previously thought, are still limited in their efficacy. By focusing on administrative procedures as ex ante controls and identifying new functions for them such as deck stacking and autopilot, McNollgast (1987) offered a richer account of strategies available to Congress than had come before, and demonstrated conceptually how these tools could work. In addition, McNollgast gave a wholly new theoretical overlay on statutory aspects of administrative law, inducing a new understanding of these institutions. McNollgast has continued to be important not only because scholars seek to clarify, extend, and critique their ideas, but because those ideas provided a new lens through which to interpret administrative structures and the determinants of agency policy.

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CHAPTER 33

PAUL A. SABATIER, “AN ADVOCACY COALITION FRAMEWORK OF POLICY CHANGE AND THE ROLE OF POLICY-ORIENTED LEARNING THEREIN”

PAUL CAIRNEY

THE ADVOCACY COALITION FRAMEWORK

THE Advocacy Coalition Framework (ACF) is one of the most influential approaches to public policy to emerge from the 1990s. It is highly cited and its approach has been used in over 80 studies of public policy, primarily in the US but also in a wide range of developed countries. It is one of a small number of prominent approaches developed in the US after Hecló's (1978: 94–7) famous identification of a departure from the simple “clubby days of Washington politics” toward “complex relationships” among a huge, politically active population. Issues which were once “quietly managed by a small group of insiders” have now become “controversial and politicized” (Hecló 1978: 105). Its key aim is to make sense of such complex policy-making systems which:

- contain multiple actors and levels of government;
- process policy in very different ways, from intensely politicized disputes containing many actors in some areas, to issues that are treated as technical or specialist and processed routinely, largely by policy specialists, out of the public spotlight;
- produce decisions based on limited information and often high levels of uncertainty and ambiguity; and

- take considerable time (perhaps “a decade or more”) to turn decisions into outcomes.

The ACF is distinctive in a number of ways. First, it is relatively ambitious, seeking to produce what we might consider to be the closest thing to a general theory of policy-making. Second, it has been subject to an unusually high number of revisions in the light of experience and a desire to extend its insights beyond the US. Third, the framework is largely predicated on the idea that people engage in politics to translate their beliefs, rather than their simple material interests, into action. Consequently, at its heart is a system in which coalitions of actors with different belief systems interact and compete to dominate policy subsystems. Fourth, the description of subsystems appears to be different from most conceptualizations of policy communities or networks, which often describe the great potential for insulated relationships between groups and government (Cairney 2012: 203). Rather, a wide range of actors are involved within each coalition. Fifth, it represents not only an approach to the study of contemporary public policy but also a set of ideas about how we should conduct scientific inquiry. For example, its rejection of the use of cycles and stages to explain policy-making is based as much on scientific criteria as conceptual or empirical concerns. Consequently, its proponents—and Paul Sabatier in particular—have influenced the discussion of public policy as a discipline and a branch of science.

A SUMMARY OF THE ACF

An advocacy coalition contains “people from a variety of positions (elected and agency officials, interest group leaders, researchers) who share a particular belief system—i.e. a set of basic values, causal assumptions, and problem perceptions—and who show a non-trivial degree of coordinated activity over time” (Sabatier 1988: 139). The ACF focuses on the interaction between competing advocacy coalitions within a policy subsystem which, in turn, operates within a wider political system and external environment. Its description of subsystems paints a picture of a relatively open, multi-level policy-making system:

Our conception of policy subsystems should be broadened from traditional notions of iron triangles limited to administrative agencies, legislative committees, and interest groups at a single level of government to include actors at various levels of government, as well as journalists, researchers and policy analysts who play important roles in the generation, dissemination, and evaluation of policy ideas. (Jenkins-Smith and Sabatier 1993a: 179)

This picture results partly from a focus on the role of ideas in policy-making—actors may be influential because they articulate important ideas, not simply because they can exercise power. It focuses in particular on the importance of belief systems: many

actors may be influential because they share a set of beliefs with a large number of others; translating those beliefs into policy decisions and outcomes is a common project. Beliefs are the “glue” that keeps a large number of actors together. There are three main types:

- *Deep Core Beliefs.* These regard an actor’s “underlying personal philosophy,” often expressed as a point on the left/right-wing continuum (Sabatier 1993: 30). Examples include: beliefs on whether people are evil or socially redeemable; how we should rank values such as freedom and security; and whose welfare should count the most (Sabatier, 1998: 103).
- *Policy Core Beliefs.* These regard “fundamental policy positions.” Examples include: the proper balance between government and market and the proper distribution of power across levels of government (Sabatier 1993: 31; 1998: 110).
- *Secondary Aspects.* These relate to the funding, delivery, and implementation of policy goals (1993: 31).

Core beliefs span most policy areas and are the least susceptible to change in light of empirical evidence (“akin to a religious conversion”—Sabatier 1993: 31, 36). They are too broad to guide policy-specific behavior. Instead, policy core beliefs are employed within particular subsystems. Although policy core beliefs are more susceptible to change, they are generally stable within the period of study (over the period of one policy cycle of a “decade or so”—Sabatier and Weible 2007: 193). Any “enlightenment function” may take place over decades because beliefs are “primarily normative—and thus largely beyond direct empirical challenge” (Sabatier 1993: 44). In most cases, change refers to “secondary aspects,” when beliefs on the routine delivery of specific policies are refined according to new information (1993: 31, 221).

Coalitions interact within policy subsystems, defined simply as a broader “set of actors who are involved in dealing with a policy problem” (Sabatier 1988: 138). It includes a varying number of coalitions (usually from one to four), policy “brokers,” whose role is to minimize conflict and produce workable compromises between coalitions, and a “sovereign” or “government authority” to make policy decisions and oversee the policy-making infrastructure. Although there is generally more than one coalition, it is not unusual for one coalition to dominate the subsystem for long periods or for a negotiated settlement to favor the beliefs of one coalition. Further, although brokers and sovereigns are separated analytically, it is often difficult to know where coalitions end and policy-makers begin, since governmental organizations may often appear to hold, and act on, beliefs consistent with those of a particular advocacy coalition.

Policy subsystems exist within a wider system (Figure 33.1) that sets the parameters for action and provides each coalition with different constraints and opportunities. It includes:

- factors that are “relatively stable” over the time period generally studied (a “decade or more”), such as “social values” and the broad “constitutional structure”;

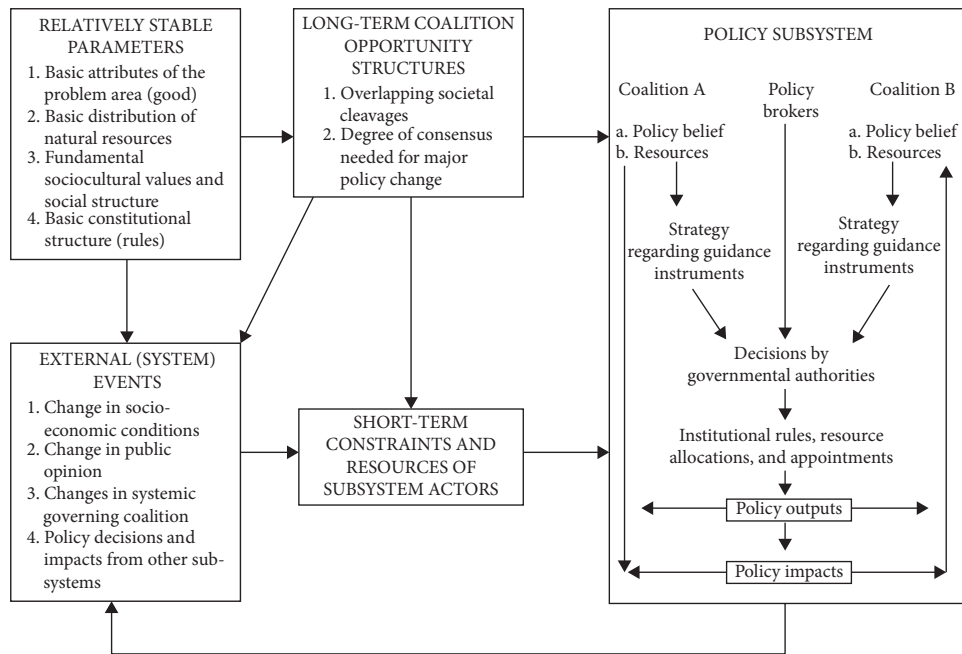


FIG. 33.1 The ACF Flow Diagram (2007 version)

Source: Weible et al. 2009: 123

- the “long term coalition opportunity structures” related to the nature of political systems—e.g. are there multiple, open venues or a small number of centralized processes? Do governments control majorities or need to cooperate with other parties?; and,
- “external (system) events,” such as socioeconomic change, a change in government, or the impact from decisions made in other subsystems.

Although the ACF is built on a critique of the “stages heuristic” (to be discussed shortly), it still focuses on change over a “decade or more” to allow (albeit notionally) for a full policy cycle. Coalitions interact, a decision is made, institutions are set up or modified, the impacts of policy outputs are evaluated, and the information is interpreted differently by each coalition learning from previous decisions and adapting their strategies (in line with that new information and external events) during the next cycle.

There are two main sources and types of change in this framework (if we exclude major changes to coalitions following an “enlightenment function” that may take decades to occur). The first is relatively minor policy change that takes place within subsystems: coalitions engage in policy learning, adapting the secondary aspects of their belief systems in light of new information. Learning has a particular meaning

in the ACF (compare with other forms of learning described by Bennett and Howlett 1992). It takes place through the lens of deeply held policy beliefs; coalitions learn on their own terms—selecting the information they hold to be most relevant and acceding only to change which does not undermine the coalition's main source of cooperation.

Coalitions also engage in a form of learning to adapt to the beliefs of another coalition, particularly when its views become “too important to ignore” (Jenkins-Smith and Sabatier 1993b: 43). However, they do not simply accept the arguments of other coalitions and adopt their preferred policies. Learning is a political process and “not a disinterested search for ‘truth’” (Jenkins-Smith and Sabatier 1993b: 45; Sabatier 1988: 151). Information on the success of policy is limited and subject to framing by each coalition. In some cases, there are commonly accepted ways to measure policy performance. In others, it is a battle of ideas which takes place in the context of a tendency of coalitions to “exaggerate the influence and maliciousness of opponents” (Weible 2007: 99). Technical information is often used “primarily in an ‘advocacy’ fashion” (Jenkins-Smith and Sabatier 1993b: 45; Sabatier 1988: 152) and a dominant coalition can successfully challenge the data supporting policy change for years (Sabatier 1998: 104). This process generally does not produce major changes in policy or the balance of power between coalitions. Rather, coalitions interpret and frame new information in line with their core and policy beliefs. In most cases, learning follows the routine monitoring of policy implementation, as members consider how policy contributes to positive or unintended outcomes and whether their beliefs regarding the best way to solve the policy problem are challenged or supported by the evidence.

The second is relatively major change prompted largely by external events. Events may set in motion “internal” or “external shocks” to subsystems, with the potential to alter coalitions and the balance of power between them. An internal shock relates to the effect of major external change on a coalition's belief system, akin to a crisis of confidence. The event prompts a coalition to revisit its policy core beliefs, perhaps following a realization by many actors that existing policies have failed monumentally, and their consequent departure to a different coalition. An external shock has the added element of coalition competition—another coalition uses the experience of a major event to reinforce its position within the subsystem, largely by demonstrating that its belief system is best equipped to interpret and solve the policy problem. In other words, the major event is not enough to cause an external shock; it also has to be exploited successfully by a competing coalition which is better equipped to learn and adapt. Such processes may vary, from the election of a new government with beliefs that favor one coalition over another, to a “focusing event” such as an environmental crisis that undermines the ability of a coalition to defend current policy or allows another coalition to successfully redefine the policy problem and seek new solutions. In each case, some coalitions may be a source of stability and there is a process of mediation within subsystems. While many of these external factors—such as global recession, environmental crises,

and demographic changes—appear to have an independent causal value, coalitions influence how sovereigns understand, interpret, and respond to them. External events provide new resources to some coalitions. It is then up to those coalitions to use them.

In other words, coalitions have to exercise power effectively to maintain or improve their positions within subsystems. This regards a mix of resources available to them—resources to gather and interpret information, the weight of public opinion and their ability to rally active public support, their funding—and the skillfulness of their leadership (Sabatier 1993: 29; Weible 2006: 99–100; Sabatier and Weible 2007: 201–3).

ACF EXPANSION AND REVISION

The ACF is based on a “Lakatosian” approach to science (Jenkins-Smith, in correspondence, 2010; Cairney 2012: 219). In effect, the ACF as a scientific project resembles an advocacy coalition: secondary aspects are amended in the light of empirical testing, while the core remains relatively insulated and the policy core is unlikely to change in the absence of a monumental failure to explain its object of study (Cairney 2013). As in an advocacy coalition, its main authors are unlikely to reject the ACF as a broad understanding of the policy-making system, but they are more likely than most policy scholars to revise certain aspects of their theory in the light of experience. There is a difficult balance to maintain: a “Popperian” attachment to falsifying hypotheses derived from the theory operates alongside a theory that is revised to maintain its value.

This revision process has taken place regularly since 1993 and the ACF flow diagram in Figure 33.1 differs markedly from the original model derived from studies of the US (in areas such as environmental policy). Sabatier and Jenkins-Smith’s review sought to clarify:

- the role of administrative organizations (as potential members of coalitions but also relatively subject to belief-change following a change of government) and “hierarchically superior jurisdictions” which may, in rare cases, override subsystem policy in the face of opposition from the dominant coalition (1993: 217; Sabatier 1998: 119);
- the impact of technical information on policy (Sabatier and Jenkins-Smith 1993: 219);
- the difference, within policy core beliefs, between “normative precepts” that are unlikely to change, and “precepts with a substantial empirical content” that are more likely to change in the light of new evidence (Sabatier and Jenkins-Smith 1993: 220–1); and,

- that an external event will not cause subsystem change unless at least one coalition has the skill to exploit its new opportunity (Sabatier and Jenkins-Smith 1993: 222).

Further revisions were prompted by the rise in ACF applications to countries other than the US. For example, the assumption of relative subsystem stability may only apply to “mature” subsystems and they may not yet have developed in “nascent” subsystems (Sabatier 1998: 110–14; Sabatier and Weible 2007: 192). Sabatier and Weible (2007: 199) also sought to address wider criticisms:

that it is too much of product of its empirical origins in American pluralism. It makes largely tacit assumptions about well-organized interest groups, mission-oriented agencies, weak political parties, multiple decision making venues, and the need for supermajorities to enact and implement major policy change.

The ACF diagram was revised to reflect: (a) the generally higher “degree of consensus needed to institute a major policy change” in political systems with proportional representation elections and a norm of minority or coalition government (Sabatier 1998: 121; Sabatier and Weible 2007: 200); and (b) the often-lower availability of multiple, open venues in “corporatist” systems characterized by centralized decision-making, restricted to a small number of leaders of business groups and unions (2007: 200). Both factors combine to produce the relatively new “long-term coalition opportunity structures” box in Figure 33.1. The main authors also generally advise caution when applying the ACF to countries in which we might make different assumptions about, for example, the role of the civil service (only some countries have politically appointed bureaucrats), the relationships between levels and types of government (not all countries have a US-style constitution setting out their respective responsibilities), and the balance of power between political parties (Sabatier 1998). A combination of such factors suggests that subsystems, as the main focus of analysis, can be defined in quite different ways in different systems (Cairney 2012: 214).

A further major revision by Sabatier and Weible (2007: 204–7) identified two sources of policy change: the distinction between internal and external shocks, already described, and “alternative dispute resolution”, which refers to the conditions (including a widespread recognition of stalemate, a process that includes stakeholders who respect a neutral chair, and an ability to resolve issues empirically) in which major policy change can result from negotiated agreements between “previously warring coalitions” (2007: 205–7). More recent revisions include attempts to provide better descriptions of the role of public opinion, from simply being subsumed under “socioeconomic conditions” as a source of external constraint, towards treating it as a resource to be used and manipulated by coalitions (Jones and Jenkins-Smith 2009: 39).

HOW THE ACF HAS SHAPED THINKING AND RESEARCH

According to John (2003: 481), the ACF was one of two approaches (alongside punctuated equilibrium theory) to mark a “punctuation” in thinking about public policy.” Its most famous exposition (Sabatier and Jenkin-Smith 1993) has been cited over 2,200 times and several related works have been cited from 1,200 to 1,870 times in the last 15–20 years. It has produced over 80 applications in publications by the authors, their colleagues, and other inspired scholars (Weible et al. 2009) and is now an almost automatic inclusion in textbooks and overviews of the policy literature (including Birkland 2005; Cairney 2012; John 2012; Smith and Larimer 2009). It has been used regularly to explain policy-making outside the US: in the European Union and some member states; in countries such as Canada, Australia, Sweden, and Japan; and, in fewer cases, Africa, South America, and Asia (Sabatier and Weible 2007: 217–20; Weible et al. 2009: 125). Some of the most cited examples include: forestry policy (Elliott and Schlaepfer 2001a, 2001b); drug policy in Switzerland (Kübler 2001); California marine protected area policy (Weible 2006), and the US President’s National Economic Council (Dolan 2003).

However, such applications have been limited in two main ways. First, most ACF case studies “remain within environmental and energy policies” (Weible et al. 2009: 125) which could exaggerate, for example, the importance of the European Union (environmental policy is the most Europeanized, compared to areas such as health and education with often-minimal EU involvement) and international cooperation or international organizations (Litfin 2000).

Second, the ACF is often applied in a rather loose way, often without adopting the methods, recommended by its main authors (Jenkins-Smith and Sabatier, 1993c), that tend to be used by authors who became closely associated with the ACF (e.g. see Weible 2007: 104; Zafonte and Sabatier 2004). The ACF may be used, as part of a desktop exercise, to provide one of many conceptual perspectives on an empirical case study that has been produced relatively independently of the ACF method (Cairney 2013: 9). Case studies may be “ACF-inspired” without testing any of its hypotheses (Weible et al. 2009: 128)—a limitation that may seem particularly problematic for a framework designed to generate hypotheses to allow confirmation or revision.

That said, many of the 80 studies covered by Weible et al. (2009) confirm that a major external event is a necessary but insufficient condition for major subsystem change (18 studies) and that it often, but not always, prompts a significant number of members to defect to other coalitions (13 studies). Others (20 studies) broadly confirm that policy learning across coalitions tends to be more likely in less contentious areas with relatively high potential to resolve issues empirically. In other words, despite these limitations, the ACF has still been subject to an unusually high amount

of testing in a field (political science and public policy) that has not produced many studies in which people examine the world using a common framework organized by a small number of scientists (other examples include the Policy Agendas Project (PAP),¹ and the Institutional Analysis and Development Framework (IAD), Poteete et al. 2010).

The ACF and Science

A further aspect of the ACF's influence is that represents not only an approach to the study of contemporary public policy but also a set of ideas about how we should practice science. The ACF itself was generally modeled on a "Lakatosian" approach to science and Paul Sabatier (1999, 2007) in particular sought to use some of these ideas about the scientific study of policy-making to set the agenda for the evaluation—and possible combination or rejection—of the most popular theories of public policy.

Sabatier (2007b: 330; 1999) highlights three main advantages to the use of multiple theories: it provides "some guarantee against assuming that a particular theory is the valid one"; it shows us that "different theories may have comparative advantages in different settings"; and, the knowledge of other theories "should make one much more sensitive to some of the implicit assumptions in one's favoured theory." He then outlines a set of principles that can be used to decide how useful a theory might be in general or in particular situations. A similar approach was used by Eller and Krutz (2009) in a special issue of the *Policy Studies Journal*.

It is important not to enforce this set of criteria too strongly. For example, Cairney (2013: 10) combines their (Sabatier 2007b; Eller and Krutz 2009) arguments to produce a simple set of principles for scientific comparison:

1. A theory's methods should be explained so that they can be replicated by others.
2. Its concepts should be clearly defined, logically consistent and give rise to empirically falsifiable hypotheses.
3. Its propositions should be as general as possible.
4. It should set out clearly what the causal processes are.
5. It should be subject to empirical testing and revision.

As stated, these principles are difficult to live up to fully (indeed, it is debatable if even the ACF lives up to them—Cairney 2013: 10). They have the potential to present a misleading picture of scientific research, ignoring the extent to which studies are generally *not* replicated (we accept a large number of findings on trust); people follow different rules and engage on different terms when discussing research (without a way for us to decide which terms are the most appropriate); and a complex world does not allow us to falsify hypotheses in a straightforward way (if at all). In other words, there is an inescapably subjective element to scientific research in which we pursue our beliefs, and confirm or deny the value of theories, by using criteria that do not allow us to validate

one claim and reject another unproblematically (Cairney 2013: 11–12; see Parsons 2000: 129 for stronger criticisms). On this basis, people like Radaelli (2000: 134) present important objections to the unquestioning adherence to these principles, calling for a balance between our needs to generalize (to explain a small part of all cases) and to be more specific (to explain a large part of particular cases).

Yet we can infer Sabatier’s attitude to science in more than one way—although he makes a public commitment to certain principles, he does not necessarily use them in an overly narrow or too punitive way. Take, for example, the use of these principles in a rather hard way to question the value of the “stages heuristic” (a focus on the policy cycle as it follows certain stages, including agenda setting, formulation, legitimation, implementation, evaluation, and maintenance/termination). Sabatier (1991: 147) argues strongly that it “has outlived its usefulness and must be replaced, in large part because it is not a causal theory.” This argument is outlined in greater depth in texts such as Jenkins-Smith and Sabatier (1993a, 1994), but with a combination of criticisms based on scientific principles (it “does not provide a clear basis for empirical hypothesis-testing”) and an argument, that can be found in the wider literature, about its descriptive limitations (which have already prompted a whole series of alternatives to stage-based research— Jenkins-Smith and Sabatier 1994: 177–8; Cairney 2012: 41). In other words, this is a combination of a specific attachment to scientific principles with a broader (and perhaps more openly subjective) focus on the extent to which an approach is valuable. Further, Sabatier (1999) proved, at least temporarily, able to humor the approaches of which he did not approve (also note Sabatier’s (1986) earlier engagement with the implementation literature) and we can see the same mixed attitude towards multiple streams analysis in Sabatier (1991, 1999, 2007b).

In other words, if we read the language used by Sabatier (2007b: 8–10), it often appears softer on scientific criteria; establishing a commitment to them but being reasonably open to challenge. For example, “Each framework must do a *reasonably good job* of meeting the criteria of a scientific theory” and “be the subject of a *fair amount* of recent conceptual development and/or empirical testing” (2007: 8, emphasis added). Further, Sabatier (2007: 8) uses rather subjective criteria for the inclusion of theories in his edited books, including a statement that could just as easily have been made by Fischer (2003): “A number of currently active policy scholars must view it as a viable way of understanding the policy process” (Sabatier 2007: 8).

Indeed, Sabatier’s own reflection on his scientific bent (2000: 137) describes perhaps an impish description of his position—“presuppositionist neo-positivist”—and a more serious, broad commitment to good communication between scientists, summed up by the Popper-like phrase “be clear enough to be proven wrong.” As Jenkins-Smith (2013) describes, “it was fine to be wrong as long as you were *clearly* wrong (and hence could learn from it) ... clarity begets clarity, mush begets mush.” This broad attitude is appropriate in scientific fields where we rely on the knowledge of others, we seek ways to communicate and learn from each other, we need to decide whom to listen to (and whom to ignore), and we want to avoid fruitless debates at cross purposes (Cairney 2013: 15).

We may also want to discourage people from stretching their theories too much to match the evidence rather than seek a more convincing theory. For a “Lakatosian” approach to science, this is an incredibly tricky balancing act: between adapting your theory continuously when you admit you are wrong (risking the criticism that you are changing your ideas too much), and making sure that you are adapting to be more right rather than simply to protect the core of your argument (which includes, for the ACF, the identification of “non-trivially coordinated coalitions based on belief-systems,” the “relative autonomy of subsystems,” and the importance of focusing on policy change over a full cycle—Jenkins-Smith, in correspondence, 2010). The irony for the ACF is that it may be built on a relatively open, honest, and dynamic approach, but have the potential to look otherwise.

CONCLUSION: THE ACF’S LEGACY ON, AND LINKS TO, THE WIDER WORLD OF PUBLIC POLICY

There are not many policy frameworks or theories that try to provide an overview of the entire policy process; to try to explain the interaction between “the five core causal processes . . . institutions, networks, socioeconomic process, choices, and ideas” (John 2003: 488). For example, although it perhaps remains most relevant to the study of US politics (in some policy areas), it also became useful as a way to restate the importance of policy subsystems in countries like the UK facing an alleged stagnation of policy networks research (see e.g. Dowding, 1995: 147–50). Further, although it was by no means the first policy framework to discuss the important role of ideas (it owes much to the work of Majone 1980, and came after Kingdon’s 1984 influential book), it treats their importance as part of a wider explanation, perhaps reducing the potential for critics to suggest that ideas are given explanatory value independent of their acceptance and use by actors within coalitions. It is difficult to get this balance right, but the ACF often succeeds:

Raw political power may carry the day against superior evidence, but the costs to one’s credibility in a democratic society can be considerable. Moreover, resources expended—particularly in the form of favors called in—are not available for future use. Thus those who can most effectively marshal persuasive evidence, thereby conserving their political resources, are more likely to win in the long run than those who ignore technical arguments. (Jenkins-Smith and Sabatier 1993b: 44–5; compare with the similar tone in Majone 1989: 2; Kingdon 1984: 131–3; Hall 1993: 291–2).

Similarly, it goes some way to address the charge, made against previous systems and socioeconomic theorists (from Easton (1953) to Dawson and Robinson (1963)

and Hofferbert (1974)) that external factors determine, or largely explain, outcomes (Cairney 2012: 113). External factors are important, and may prompt major change, but "shocks" refer largely to the ways in which coalitions compete to adapt to, and interpret, such events. The ACF has a less developed view about the role of institutions—although its focus on a myriad of actors and organizations, at multiple levels of government, helps us understand institutions as forms of behavior linked to rules and norms accepted or challenged by different coalitions (Sabatier 1993: 25; Sabatier and Weible 2007: 194; Cairney 2012: 217). Finally, there is much less of a focus (than in most other accounts of policy-making) on "bounded rationality" (Simon 1976) as the starting point for considering how policy-makers consider and make choices, partly because issues about how someone might articulate and pursue their interests is subsumed within a consideration of the construction and operation of belief systems (Cairney 2012: 215–16; 282–3).

This degree of conceptual coverage and academic ambition may be the key to its potential longevity in the policy literature. Like the PAP (Policy Agendas Project) and IAD (Institutional Analysis and Development Framework), the ACF is one of a small number of approaches in policy studies linked, initially and very strongly, to a small core group of authors, before it became established as an approach that could be used without the involvement of its founders.

NOTE

1. <<http://www.policyagendas.org>>.

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CHAPTER 34

FRITZ W. SCHARPF, “THE JOINT-DECISION TRAP: LESSONS FROM GERMAN FEDERALISM AND EUROPEAN INTEGRATION”

ADRIENNE HÉRITIER

INTRODUCTION

IN 1988 Fritz Scharpf developed the concept of joint decision trap (JDT). He defined it as a decision-making mechanism extending across two levels, in which the higher level government's decisions are dependent on the unanimous or consensus agreement of the lower level governments. The interlinking of decision-making processes lead to suboptimal policy outcomes because each lower level actor can block higher level decisions (Scharpf 1988: 259; 2006). The concept and theory of JDT which have been widely applied in European policy research offer important insights into the dynamic of a significant part of European decision-making, but by no means all aspects. As Scharpf himself emphasized one and a half decades later, the JDT was not meant to and does not take into account all areas of European decision-making, but applies to the inter-governmental mode. In 2001 he insisted on distinguishing at least three decision-making modes: the “intergovernmental” mode likely to end in a JDT, the “joint-decision” mode, and the “supranational-hierarchical” mode (Scharpf 2001). In a later publication he speaks about mutual adjustment, Commission-led problem-solving, and hierarchical steering by the Commission and the European Court of Justice (ECJ) (Scharpf 2006).

By analytically distinguishing these other modes beyond JDT Scharpf also developed an important theoretical alternative to the theoretical debate between (liberal)

intergovernmentalism and neofunctionalism, offering a more differentiated analytical view of different processes of integration. Additionally, he prompted a strand of research addressing the question of how to escape from the deadlock situation of JDT through covert integration or subterfuge (Héritier 1999; Falkner 2012). Covert integration is defined as a process which takes place *outside* the formal European political decision-making arenas, such as deepening integration through vague political policy mandates which are subsequently specified by executive and judicial actors, delegation to regulatory authorities, the crowding out of national regulation through European regulatory. They all may lead to deepening integration, defined as a transfer of policy-making competences to the higher level/supranational level; and/or the demandingness and detailedness of policy prescriptions issued by the supranational level.

The chapter proceeds as follows: it first presents the definition of the concept of JDT and its important contribution to theoretical and empirical and research on European decision-making; the article next looks at the critique of the model of JDT and the identification of the limits of its explanatory power; in a third step covert routes of European integration integration resulting from JDT are discussed and empirically illustrated. The chapter concludes by summarizing the continuing heuristic value of JDT.

DEFINITION AND THEORETICAL EXPLANATION OF JDT

Scharpf (1988) defined JDT as a decision-making mechanism extending across two levels, in which the higher level government's decisions are dependent on the unanimous or consensus agreement of the lower level governments, leading to suboptimal policy outcomes (Scharpf 1988: 259; 2006). In more detail, the theoretical argument underlying Scharpf's JDT is based on the assumptions of the existence of two levels of interlinked decision-making arenas, the existence of a unanimity or consensus rule, the distinction between substantive policy choices and institutional choices, i.e. the assumption that on issues of institutional reform, member governments represent not only the interests of their constituents but also their own institutional self-interests to preserve autonomy and influence; the existence of transaction costs of intergovernmental negotiations and a compulsory negotiation system that an actor cannot leave if dissatisfied with the course of negotiations (Scharpf 2006: 848; 1997). It also assumes that actors/member states have diverging interests, but faced with a joint collective action problem in which problems within their territories can no longer be resolved through autonomous policy choices, these governments may reluctantly delegate competencies to higher-level institutions. Nevertheless, they will try to maintain as much influence as possible over the exercise of these competencies. In order

to prevent decisions violating their own preferences, they will insist on unanimity or qualified majority voting, even though the outcomes are likely to be inefficient from a problem-solving perspective (Scharpf 2006: 849).

Hence, self-interested bargaining between member states is likely to create sub-optimal outcomes, resulting either in blockages or lowest common denominator compromises. The suboptimality follows from the circumstance that member states participate directly in decisions taken with formal or de facto unanimity in the central arena without a mechanism of representation that filters out the interests of the lower units/member states at the higher decision-making level (Scharpf 1988: 239, 254). The JDT results from the fact that the European level is not (yet) able to act, while at the same time member governments are no longer able to do so (Scharpf 1988: 258). “Once a binding rule is agreed upon, individual action is no longer permitted, and the veto of one or a few governments will prevent all others from correcting or abolishing the rule in response to changed circumstances or preferences” (Scharpf 2006: 848). Thus, if a measure of market integration and trade liberalization prevents a member state from taking *national* policy measures to deal with negative effects of this measure of market integration, but at the same time due to the unanimity or consensus rule and the diverse preferences of member governments, *European* action to address these negative consequences does not materialize, the JDT snaps shut. Neither the national nor the European level is able to produce a decision, or if a decision is produced it is—due to the divergence of interests of member states—at the level of the lowest common denominator.

JOINT DECISION TRAP ONLY ONE ROUTE OF DECISION-MAKING

JDT, as Scharpf (2006) himself argues, should not be mistaken for the only decision-making mode in European policy-making. Given the described assumptions of linked two-level arenas, diverse preferences of member states, consensus, or unanimity rule at the higher level, it offers an accurate description and explanation of a deadlock or lowest denominator outcomes in *intergovernmental* European decision-making. But intergovernmental decision-making is not the only mode of European integration. Scharpf also emphasized the importance of other modes of decision-making, in particular, joint problem-solving under the leadership of the Commission (or the Community method) and a non-political hierarchical (Commission and ECJ led) mode of European decision-making (Scharpf 2006)

As to the important mode of problem-solving through Community method, Scharpf admitted:

I am embarrassed to have ignored the Commission's potential role in reducing the transaction costs of consensual policy solutions through its monopoly of legislative initiative. This mechanism is central to what I now call the "joint-decision mode" and what the Commission likes to call the Community method. However, if solutions could be proposed by a single, central agent, that agent would need to explore only ($N \cdot a$) policy impacts to develop a win-win solution which (if the solution space is not empty!) should be acceptable to all veto players. In the real world, this possibility of "intelligent design" may allow the Commission to present creative proposals that go beyond the trivial exploitation of fixed policy preferences . . . Relying on extensive consultations with interest groups, national and sub-national officials and independent experts, the Commission may be able to assess the hardness or pliability of the interests and constraints defended by all member governments, and to develop innovative win-win solutions which—though departing from the initial policy preferences of some or all veto players—may still be preferred to the status quo by all (or at least a qualified majority of) member governments and a majority in the European Parliament. That is, of course, not the end of the story, given the unpredictable vagaries of national politics and intergovernmental and inter-institutional group dynamics. Nevertheless, as long as the Commission is accepted as an "honest broker" by the Member States, and as a politically neutral guardian of the European public interest, its agenda-setting role should ensure more successful negotiations than one should expect from strictly intergovernmental bargaining. At the theoretical level, I would thus have to soften the pessimistic implications of the joint-decision trap, not by much. Transaction costs still rise with the number of Member States and the diversity of their preferences and, in any case, the good services of the Commission will not help if the solution space is empty—i.e. if problem solving solutions would require uncompensated sacrifices by at least some participants. In theory, it is true, compensation might be achieved through side payments or through package deals combining asymmetric solutions in different policy areas. But side-payments, which often facilitated European compromises in the past, are increasingly constrained by the EU budget, whereas package deals have always been difficult to achieve in the narrowly specialized councils of ministers. In any case, not all sacrifices can be compensated, and the difficulty of reaching negotiated agreement increases with the heterogeneity of Member State conditions, interests and preferences. (Scharpf 2006: 851)

Besides intergovernmental, JDT-prone decision-making, and the joint decision mode, there is the important hierarchical mode of intervention

in which the Commission, the European Court of Justice or the European Central Bank are able to exercise policy-making functions without any involvement of politically accountable actors in the Council or the European Parliament. This mode of governing was completely ignored in the original article—even though, having previously worked on the policy-making functions of the judiciary myself (Scharpf, 1966), I should have been alerted to it by Joe Weiler's path-breaking analysis of the "Dual Character of Supranationalism" (1982). Its origins go back to the 1960s, when the Court had succeeded, over the feeble opposition of some member governments, in establishing the twin doctrines of the "supremacy" and the "direct

effect” of European law (Alter, 1996). As a consequence, not only the “primary law” of the treaties, but also the “secondary law” of European regulations and directives came to take legal precedence over all national law, including parliamentary legislation, plebiscites, and the national constitution—and since they also had direct effect, European rules could be invoked as the supreme law of the land by any party in legal proceedings before national courts. To become effective, these doctrines depended on the willingness of national courts to accept the decisions and preliminary rulings of the European Court as the authoritative interpretation of European law. Once this condition was secured, however, the power to interpret became a power to legislate that was sanctioned by the respect for the rule of law engrained in the political cultures of Member States (Alter, 1996; de Búrca, 2003; Stone Sweet, 2003). (Scharpf 2006: 852)

Hence, Scharpf himself has pointed out the limits of the application of the concept and theory of JDT. Apart from Scharpf’s self-critical putting into proportion of JDT and emphasizing of the importance of other decision-making modes, the JDT argument was criticized from additional viewpoints. Guy Peters dwells on its exclusive focus on central formal political arenas and points out that JDT overlooks that many policy issues are by no means decided in these arenas, but rather at the bureaucratic level, and hence do not end up in a JDT. Moreover, he underlines that JDT focuses on individual decisions as one-time interactions while *de facto* decisions are linked over time and constitute ongoing negotiations leading to an incremental change of policy outcomes (Peters 1997). From a different multi-level perspective Blom-Hansen emphasizes that, in a deadlock situation, the central government, in our case the Commission, can try to exit the federal-regional arena in order to try to get the deadlocked process moving in another arena (Blom-Hansen 1999). In a similar way McRoberts argues that—given a JDT situation—regional arenas may be source of policy innovation and put the central arena under competitive pressure and/or serve as a policy model (McRoberts 1993).

By mentioning the shift to a different political arena, Blom-Hansen refers to the strand of literature stimulated by the JDT argument which poses the question: faced with a JDT and a deadlock, how can it be overcome or circumvented? (Héritier 1999; Majone 2005; Pollack 2003; Falkner 2011).

ESCAPE ROUTES FROM JDT

In my 1999 book *Policy Making and Diversity in Europe: Escape from Deadlock* I started out from the striking contrast between obstacle-ridden decision-making processes in Europe, often ending in deadlock, on the one hand, and institutional change and rapid policy movements, on the other. Referring to Scharpf’s JDT I pointed to the widespread and ubiquitous use of informal strategies and process patterns that circumvent political impasses, metaphorically referred to as escape routes or subterfuges of European

policy-making. I argued that these escape routes, such as switching negotiation arenas, the treaty-base game (i.e. selecting a decision-making rule with the least political costs), multi-level games; and the early self-commitment of actors, and vague framework commitments have become second nature to European policy-making (Héritier 1999: 2, 1997, 2014; Falkner 2011; Tallberg 2000; Majone 2005). Falkner (2012) conceptualizes and theorizes different exit routes from the "joint decision making trap." She distinguishes between supranational hierarchical steering (ECJ and Commission decisions), such as a reinterpretation of the status of EU law and/or a threat of litigation, pressurizing by cooptation of powerful private actors or triggering public discourse, playing the treaty-base game and arena shifting, sequencing, sizing, or watering down, exceptions or opt-outs, side payments and package deals, on the one hand, and socialization processes, on the other, leading to a convergence of preferences of actors induced by following the same values of integration, such as strategic constructivism: framing and second round effects, learning, the impact of the shadow of future cooperation.

Recently (Héritier 2012, 2014) I systematically theorized and empirically illustrated the most important modes of covert integration that may operate as escape routes from deadlock. *Covert integration* is defined as a process which takes place *outside* the formal regional political decision-making arena. *Deepening integration* refers to an outcome and is defined along two dimensions: (i) a transfer of policy-making competences to the higher level/supranational level; and/or (ii) the degree to which the policy prescriptions issued by the supranational level are detailed and demanding.

Why and how do different patterns of covert integration emerge, patterns that may lead to a deepening of integration, i.e. more demanding and detailed common policy requirements and a power shift to the supranational level and from the legislator to the executive and judiciary? Different strands of theory offer different answers. I mainly use *rational choice institutionalism* to explain covert processes of institutional and policy change. Moreover, I will offer an alternative view of the causes and processes of covert integration through the lenses of sociological institutionalism.

The *rational institutionalist explanations* build on the *theory of continuous institutional change* (Héritier 1997, 2007, 2012; Farrell and Héritier 2003, 2004; Stacey and Rittberger 2003), which may also be applied to the change of policy measures. It emphasizes the renegotiation or reinterpretation of incomplete institutional rules and policies. Channels of covert integration may be considered as an institutional and a policy change that emerges once a formal political decision of integration has been taken that offers the possibility of being renegotiated and specified in the course of its application. The explanation is based on the assumptions of goal-oriented, boundedly rational actors, seeking to reduce transaction costs of information, negotiation, and monitoring of contracts and/or to maximize their institutional power and thereby their power over policy outcomes. Answers to why patterns of deepening integration appear may be derived from the existence of external problem pressure, the wish to reduce transaction costs, specific institutional conditions, and the relative bargaining power of the involved actors when redefining incomplete institutional or policy rules.

The most important patterns of covert integration are the following:

- Members' formal political commitment to mere framework policy goals may inadvertently lead to ambitious integration in individual policy issues during implementation and specification by executive or court action.
- Similarly, international agreements framed in general terms—through executive action or court rulings—may lead to a competence shift and more stringent supranational regulation.
- Delegating policy-making to independent regulatory bodies at the regional level may result in a strengthening of supranational policy-making.
- The use of soft, voluntary modes of policy-making under certain conditions may give rise to a deepening formal integration.
- The introduction of parallel options of regulation, regional and national, may lead to a crowding out of national regulation.

From a sociological institutionalist perspective, deepening integration may be accelerated by the frequent interaction of decision-makers in small circles resulting in a socialization into convergent norms.

Under the first pattern, arising from political commitment to framework policy goals, I argue that—given problem pressure and a demand for a coordination of national policies—it is crucial whether decisions to coordinate at the higher level represent complete or incomplete contracts. If member states have similar preferences and agree in a detailed decision to upload competences to the higher level, which also clearly circumscribes the power given to supranational actors, a limited transfer of national powers has occurred in a complete contract and in an overt way in the main political arena. If by contrast member states have diverse preferences regarding the desired policy solutions and appropriate limits of supranational power, the outcome of the decision process in the main arena is likely to be vaguely formulated (an incomplete contract) and/or at the lowest common denominator. An incomplete contract—for strategic reasons and reasons of substantive uncertainty (Cooley and Spruyt 2007)—leaves many details yet to be specified, and thereby opens the door for subsequent institutional and policy changes (Héritier 2007). These changes often happen outside the formal political arena. The renegotiation may give rise to informal rules regarding the handling of national powers emerging alongside the formal political arena. The outcome of the rebargaining of the incomplete contract will be determined by the most powerful actors (as defined by their fall-back position), the existing decision-making rule, and exogenous events. When specifying the incomplete contract, supranational executive actors may form an alliance with judicial actors in interpreting the details of the contract and—through court rulings—make an inroad into national competences previously not formally mandated.

In short, a deepening integration may result from the fact that—given external problem pressure—the formal political decision-makers due to diverging preferences and consensus or unanimity rules commit themselves to only vaguely formulated institutional rules or policy goals. Given the ambiguity of the rule or policy mandate,

implementing actors, i.e. executive actors, and judicial actors, are able to redefine the generally stated goal. Depending on the preferences and the relative power of the actors involved in the renegotiation of the incomplete contract and given institutional restrictions, deepening integration may ensue (Héritier 2007). Therefore one may expect that "An incomplete institutional rule or policy may lead to a deepening of integration in the course of its application if pro-integration executive and judicial actors with pro-integration preferences specify the incomplete rule" (Héritier 2014: 235). An empirical example is offered by Moritz Weiss' analysis of the incipient European arms procurement policy. The pressure of technological innovation and economies of scale induced the Commission to propose a common European arms procurement policy implying the *non-use* of Article 346 of the Lisbon Treaty entitling state authorities to circumvent common market rules in this area. When the Commission initiatives regularly met with strong opposition from member governments, the Commission was reinforced by ECJ rulings stating that "now-Article 346 does not imply an automatic exclusion of armaments from the public procurement regulations of the Union" (Weiss 2013: 17) and—even more strongly—changing the dividing line of acquisitions of military goods and services in the sense of a public procurement within the Single Market (Weiss 2013: 17). Accordingly, first steps of a common arms procurement policy were introduced through a court-supported initiative of the Commission to differently interpret the exemption article in the Lisbon Treaty.

Under the second pattern of deepening integration through external contracts, the internal decision-making processes and the resulting policy solutions and institutional rules are affected by existing international agreements, bilateral treaties, and transnational governance in all areas of policy-making (Katsikas 2011). Under which conditions do they give rise to covert patterns of deepening integration within a regional polity? Assuming that there are ambiguous provisions in the treaty with external actors which in the course of its application may be renegotiated or reinterpreted by executive and judicial actors, this may lead to more deepening of integration. Hence, we expect that "If agreements with external actors include vague provisions which are specified by implementing executive actors or courts with pro-integration preferences, deepening integration will follow." A case in point is the Open Skies Agreement of 2007/2010 concluded between the European Union and the United States of America. This international agreement with the US obligates member states—if they wish to impose night flight bans at their national airports of a certain size—to empower the Commission to approve of the decision, and in case of non-approval, take legal remedies *before* the decision is implemented. In other words, the international agreement clearly empowers the Commission to take such decisions to the detriment of national and subnational actors (Héritier and Karagiannis 2011).

Another escape route from a deadlocked main decision-making arena is "Deepening integration through delegation to independent regulators." The explanation based on the principal-agent approach suggests the emergence of deepening integration if the specification of the general policy goal is deliberately taken out of the political arena and regulatory capacity and the power of detailing a policy are transferred to an independent regulatory authority or a technical body at the supranational level (Coen

and Héritier 2005; Gilardi 2001; Radaelli 1998; Mattli 2003). The causes and political motives behind the act of delegation (McCubbins 1985: 721; Majone 1994) have been extensively discussed in the literature, the most frequently mentioned motivations being substantive uncertainty and the need for expertise; political uncertainty; policy credibility; and blame shifting. Principal-agent theory starts out from the assumption that the agent through his policy expertise has an informational advantage over the principal (Miller 2005; Fiorina 1982, 1986; McCubbins 1985; McCubbins et al. 1987; Horn and Shepsle 1989; Pollack 2003; Epstein and O'Halloran 1999; Franchino 2004). Political uncertainty refers to the wish to ensure policy stability over time, i.e. the wish of governments to protect their policy choices from being dismantled by their successor governments ("credible commitment," Moe 1990; Majone 1994). A different form of political uncertainty argument was presented by Fiorina (1982) who argued that interest groups with diverse preferences might mutually block each other in the legislative process and might therefore be unable to come to an agreement supporting detailed legislation. In order to avoid stalled decision-making, legislators linked with different interest groups will agree that authority be delegated to bureaucracy or agencies with the task of taking detailed decisions, while legislation takes only the form of framework legislation. The shifting of responsibility or "blame shifting" (Fiorina 1982) is another important reason for delegation that is discussed in the literature. If the electoral costs of regulation are expected to be greater than the benefits, a rational legislator would delegate rather than legislate.

When delegating, principals still seek to control—to some extent—the agent's activities. In *ex ante* controls they insert incentives into the contract that seek to align the agent's preferences with those of the principals. In *ex post* controls they use rules to control the agent and to avoid agency drift, such as the assigning of the burden of proof to one specific party, the prescription of consultation, transparency, and public disclosure rules; they also may limit the timing of delegation (McCubbins et al. 1987; Epstein and O'Halloran 1999; Franchino 2004). McCubbins and Schwartz (1984) highlight mechanisms of *ex post* contractual control that allow a principal to discover problems of agency drift. Legislators can either seek to control bureaucrats by gathering their own information or force the agent to disclose information at oversight hearings ("police-patrol" oversight), or they can turn to those interest groups affected by the agent's decisions ("fire-alarm" oversight). Hence, assuming that the agent is more integration friendly than the principals, it is argued that delegation will result in agency drift unless the agent is kept under tight control. It is therefore expected that "In policy areas under delegation there will be more indirect deepening of integration than in policy areas without delegation"; and "If principals do not engage in control over the agent, delegation will lead to deepening integration." An illustration of this pattern of covert integration may be drawn from Waltraud Schelkle's discussion of the delegation of power to an agent, the European Central Bank (ECB). Under the fiscal compact, by delegating authority more powers to the ECB and the Commission, member states "engage in institution building that they find hard to sell to their sceptical electorates ... Hence they try to shift the most scandalizing parts, the extension of quasi-fiscal

resources and the surveillance of drastic adjustment programmes in the crisis countries, to the ECB and the Commission respectively." Thereby, she argues, the EU has "made decisive steps beyond the regulatory polity towards a fiscal union through the monetary backdoor" (Schelkle 2014: 106).

Soft modes of governance may serve as another possible covert escape route from a deadlock situation in the Council of Ministers. Under this pattern deepening integration may result from a general commitment to a policy goal that is non-binding. In recent years new modes of governance, not based on legislation, have increased in salience in European policy-making (Héritier 1999, 2003). Since European integration has reached a stage where the core activities of the member states are addressed, member states hesitate to lend political support to harmonization through legislation because governments see their sovereignty endangered. Hence a method of cooperation has been developed to avoid the classical form of legislation through directives and regulations. Instead it relies on an open method of coordination (Scott and Trubek 2002; Sabel and Zeitlin 2007). From a rational institutionalist perspective it is argued that—given external problem pressure—members agree that action is needed, but hesitate to transfer formal competences to the supranational level. In order to initiate measures of collective action they therefore engage in "soft" measures, such as the exchange of information regarding their policy goals, information about which instruments they apply to reach these policy goals, and the publication of the results of their endeavours in terms of policy outputs (legislation), but also outcomes (resources invested) and their impact (contribution to the solution of the policy problem). These results may be published in an overall benchmarking exercise, showing how countries perform in order to incite the laggards to catch up with the leaders. But all this happens on a strictly voluntary basis. No formal controls are applied by the supranational level, nor are there sanctions in case a member disregards the defined policy goals. Instead they only commit themselves to voluntary coordination linked with informal sanctions such as a loss of reputation in case of non-compliance (De La Porte and Pochet 2002; de Ruiter 2008; Sabel and Zeitlin 2007; Héritier and Lehmkuhl 2008). Elsewhere I have argued that it makes a difference whether soft modes of governance are employed in policy issues in which all gain from cooperation or they are employed in policy areas in which there are clear losers and winners. In the latter case, it is unlikely that the losers would voluntarily observe the defined policy goals (Héritier 2002). Hence we may expect that if all members gain from the coordination of policies, or agree with the deepening of integration on a voluntary basis, it will happen. Therefore we can expect that "In the absence of similar pro-integration preferences of members, soft modes of governance will not lead to deepening integration."

An empirical instance of a failed attempt at deepening integration on a voluntary basis is again offered by Moritz Weiss's analysis of the Commission establishing the European Defence Agency to help member states "in their provision of armaments for crisis management within the ESDP ... and the creation of a European Defence and Equipment market" (Weiss 2012: 6). The Code of Conduct which was created provides for coordinative capacity with information to member states, but no "management

or enforcement rule” (Weiss 2012: 7). Anand Menon, discussing the same policy, concludes that member states dominate EDA and seek to preserve control over defence issues (Menon 2014: 7).

Regulatory venue shopping may serve as another channel of escaping a JDT. Assuming the existence of several national and supranational options of regulation on the “potentially rich range of institutional alternatives available to states ... on international institutional choice as an unfolding process regarding *which* institution to choose from among a range of alternatives” (Jupille et al. 2013: 22), selection is most likely when several existing institutions “occupy the same issue space” and could handle the problem, but none is “focal” (2013: 11). Different jurisdictions might partially overlap with respect to specific issues and actors. As a result an existing institution for an existing issue “can be undermined by competition of other institutions seeking to take on this issue” (Jupille et al. 2013: 11). Alternative institutional venues may be actively created by actors for exactly this purpose (Alter and Meunier 2009). At the same time the possibility of choosing between regulatory options may also facilitate exit from one regulatory arena and thereby non-compliance with strict regulations (Alter and Meunier 2009).

In an issue space with multiple regulatory options or institutions, the supranational regulatory option which also covers transborder aspects may be more attractive. It is therefore expected that national actors would prefer the supranational regulation to their national regulation *if* the supranational regulation is not stricter than its own national regulation. This may result in supranational regulation crowding out national regulation. Hence it may be expected that, “given a choice between regulatory arenas, actors will—*ceteris paribus*—choose the supranational offer rendering national regulatory arenas obsolete; therefore deepening integration will ensue” (Héritier 2014: 241). An example which illustrates this pattern is the Common European Sales Law (CESL) Regulation proposed by the Commission in 2011. It would be an optional law designed to sit alongside a member state’s already existing domestic contract law. It would be used for cross-border sale of goods. A number of countries, such as Germany, the UK, Belgium, Austria, and the UK, consider the proposal problematic because it includes numerous unclear terms which would be interpreted differently by national courts and would add legal complexity and uncertainty (e.g. House of Commons 2011: 11; Deutscher Bundestag 2011). Other objections refer to the risk that having competing legal orders would impede the necessary harmonization in areas such as consumer protection. In other words being able to choose regulatory venues would lead to less stringent consumer protection.

From a *sociological institutionalist* theoretical perspective, escaping deadlock may occur without taking explicit decisions but through a process of socialization into mutually shared norms of behavior in favor of a deepening of integration. It emphasizes factors and processes changing the cognitive and normative orientations of the actors who are involved in a joint decision-making process (Wiener and Diez 2003; Olsen 2010; Schimmelfennig 2010). V. Schmidt (2008) describes mechanism of habituation, socialization, policy learning, and persuasion through public discourse. Such

soft influences on the preferences of national actors may play an important role in achieving agreements regarding deepening integration. Along similar lines Falkner (2011) stresses that socialization may be an important channel of overcoming the "joint decision making trap" analyzed by Scharpf (1988). It has been argued that if decision-makers frequently meet in relatively small circles that are firmly anchored in a particular social and organizational context (Johnston 2001; Checkel 2005: 810; Lewis 2007) the socialization into specific rules of appropriate behavior happens more easily. Over time, these policy choices turn into routines or habits, discharging actors in both new and established choice situations. National representatives in joint higher-level decision-making bodies "may go native" (Lewis 2007; Joerges and Neyer 1997). Deepening integration happens through intensive high-level policy dialogue (Puetter 2012) in which nation states become member states. However, a socialization process in favor of integration is more likely to occur in some policy areas than in others. In redistributive policies linked to polarized cleavages in the decision-making arena, preference convergence is less likely to occur, whereas in a policy area in which all actors gain from joint action it is more likely to emerge. One may therefore expect that "A convergence of preferences in the sense of deepening integration emerges under the conditions of small groups of actors which interact frequently" and "In redistributive issues *ceteris paribus* a convergence of preferences in the sense of deepening integration is less likely to emerge." Empirical cases which may illustrate this mechanism may be found in Rittberger's analysis of the increasing competences of the European Parliament in European decision-making processes, which emphasizes the importance of democratic values inducing all concerned institutional actors to give more formal power to the European Parliament (Rittberger 2005). The loss of institutional competences and an ensuing deadlock situation may be overcome by the strong normative convictions of strengthening democracy by strengthening the powers of the directly elected body.

CONCLUSION

Is Scharpf's JDT still a key to the understanding of European governance? I have argued that JDT has opened important new analytical perspectives on the process of European decision-making and European integration. As subsequently further developed by Scharpf himself, however, JDT refers mostly to the intergovernmental avenue of European decision-making and did not take into account other modes of decision-making, such as the Community method and the hierarchical intervention by the Commission and the European Court of Justice. By pointing out the Catch-22 situation of intergovernmental decision-making ending up in a JDT, and then pointing out other possible decision-making modes, Scharpf has made a most important contribution to the analysis of European integration. In doing so he helped overcome

the somewhat stale debate between (liberal) intergovernmentalism and neofunctionalism prevalent in the 1990s and offers a much more differentiated picture of European decision-making.

Moreover, by pointing out the JDT, Scharpf has also given rise to research focusing on ways around the JDT. This research draws the analytic attention to covert integration mechanisms occurring outside the formal political arena, such as vague formal policy commitments that are subsequently specified by executive and judicial actors; delegation to regulatory authorities; and competing policy options crowding out national policy options, all potentially resulting in deepening integration in spite of a deadlock in the main political arena.

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CHAPTER 35

JAMES Q. WILSON, *BUREAUCRACY: WHAT GOVERNMENT AGENCIES DO AND WHY THEY DO IT*

WILLIAM T. GORMLEY, JR.

At my university's Master's in Public Policy program, we have a rule known as the "James Q. Wilson" rule. It states that only faculty who teach our public management course may assign Wilson's *Bureaucracy* book. This rule was adopted when we discovered that faculty members teaching diverse courses were assigning Wilson's irresistible book. Students were delighted to read *Bureaucracy*, but they wondered whether they should be reading it again and again! Hence the Wilson rule.

When Wilson passed away on March 2, 2012, he was hailed as one of the leading political scientists of his generation. George Will (2012) went so far as to refer to him as "the most accomplished social scientist of the last half century." In a blurb on the jacket cover of *Bureaucracy*, the late Daniel Patrick Moynihan described Wilson as "our Weber." This accolade conveys the high esteem with which Wilson was and still is regarded by scholars and practitioners. It would be equally apt, however, to call Wilson "our Homer." Unlike most social scientists, Wilson wrote with poetic grace and with a keen appreciation for the epic stakes of apparently mundane bureaucratic behavior. As bureaucrats steer between the Scylla of rigidity and the Charybdis of arbitrariness, Wilson, from the grave, is there to chronicle their behavior and to describe their journey.

What, then, are we to make of Wilson's legacy? If Wilson rivals Weber in stature and in wisdom, he is nevertheless no Weberian. From the beginning of his book, Wilson makes it quite clear that he rejects a "top-down" or hierarchical approach to the study of bureaucracy. Instead of starting with laws or politicians, he begins his book with a deft and thoughtful portrait of "operators" or, in Michael Lipsky's (1980) words, "street-level bureaucrats." Their behavior, he asserts, deviates from law-makers' stated

goals because those goals are often vague and sometimes inconsistent. Therefore, we should pay at least as much attention to how operators define their “tasks” as to how elected officials struggle to articulate broader objectives.

As befits a past president of the American Political Science Association, Wilson’s magnum opus is better described as a study of “bureaucratic politics” than public management or public administration. He is less interested in bureaucratic structure, budgeting, and personnel management than most students of public administration, less convinced that political appointees can control civil servants than most students of public management. Like other students of bureaucratic politics (Aberbach and Rockman 2000; Moe 1985; Wood and Waterman 1994), Wilson sees bureaucracies as embedded in a complex web of institutions.

Wilson’s approach differs even more sharply from that of neoclassical economics. Unlike economists, who view bureaucrats as utility maximizers, Wilson believes that bureaucrats have diverse motives, including material, solidary, and purposive incentives. Wilson is especially scathing in his denunciation of William Niskanen’s (1971) “bureaucratic imperialism” hypothesis, which he regards as naïve and simplistic. If bureaucrats are so committed to increasing their agency’s budget and size, he notes, then why did the US Department of Agriculture try to jettison the Food Stamp program? And why did the FBI resist recommendations that it include narcotics in its jurisdiction? (Wilson 1989: 182).

Wilson also takes aim at “principal-agent” models which predict widespread “shirking” by bureaucrats. As he points out, most bureaucrats seem to be quite committed to their jobs, including, in some instances, difficult and dangerous tasks, like teaching and policing (p. 156). Economic theories of bureaucratic behavior don’t do a very good of explaining this phenomenon.

What does explain a good deal of bureaucratic behavior, according to Wilson, is “organizational culture,” which he defines as “a persistent, patterned way of thinking about the central tasks of and human relationships within an organization” (p. 91).

WILSON’S CONTRIBUTIONS

Organizational Culture

From Wilson’s perspective, every bureaucracy has a distinctive culture, which plays a critical role in shaping the behavior of individual bureaucrats. The State Department’s culture places a high value on “caution.” The Army Corps of Engineers places a high value on building dams and dredging harbors. While admitting that the concept of culture can be a bit “mushy” (p. 92), Wilson nevertheless argues that organizations have cultures just as surely as people have personalities (p. 93).

According to Wilson, some organizational cultures are more functional than others. More successful bureaucracies have managed to create and sustain a particular culture that is happily internalized by all or most of its members. In Wilson's words, "When an organization has a culture that is widely shared and warmly embraced by operators and managers alike, we say the organization has a sense of *mission*" (p. 95). Agencies with a strong sense of mission include the Army Corps of Engineers, the FBI, NASA, the Marine Corps, and the Social Security Administration (p. 158).

Wilson's examples of successful organizational cultures are contestable. He cites the Texas prisons under George Beto as a positive example, because Beto managed to keep inmates under control without spending huge amounts of money. Yet, as Wilson himself acknowledges, the Texas prisons were a powder keg. Shortly after Beto's resignation as director of the Texas Department of Corrections, excessive discipline and human rights violations resulted in prison riots and a judicial takeover of the state prison system. Wilson also celebrates a particular public school because its principal was successful in maintaining order. At times, Wilson seems to equate successful cultures with ones that place a premium on law, order, and discipline—values he has celebrated in some of his other famous publications (Wilson 1975; Wilson and Kelling 1982).

Wilson does, however, shed considerable light on how organizational cultures are formed, why they matter, and why they stubbornly resist change over time. Of central importance are professional norms, which become deeply embedded in many organizations. For example, engineers have dominated the National Highway Traffic Safety Administration (NHTSA), resulting in an emphasis on "changing the automobile rather than the driver or the highway" (p. 62). Similarly, the Forest Service has been dominated by professional foresters, committed to "progressive conservation" (p. 97). For many bureaucrats, their allegiance is not simply to an organization but rather to a set of norms they learned in college or graduate school.

One wonders what Wilson would make of recent attempts to transform bureaucratic cultures. The Federal Emergency Management Agency (FEMA) was reinvented to become more nimble and more responsive in the wake of natural disasters, only to be submerged within the Department of Homeland Security, where it proved incapable of responding to Hurricane Katrina. Recently, though, FEMA responded admirably to Hurricane Sandy. Perhaps some policy learning has occurred. School reformers have tried to remake public schools in Denver, CO, Washington, DC, and elsewhere, with performance pay and other reforms. In both cities, signs of educational progress are evident. Wilson argues that organizational cultures are hard to change. But he also recognizes that change is possible.

Interests

Some years ago, both political scientists (Bernstein 1955) and economists (Stigler 1971) advanced a theory of bureaucratic behavior which came to be known as "capture theory." The premise of that theory was that over time regulatory agencies tend to be "captured" by the very interests they are supposed to be regulating. For example, the

Federal Communications Commission eventually tilted towards the broadcasting industry, while the Food and Drug Administration came to favor the pharmaceutical industry. This startling assertion, if true, raises questions about the legitimacy and the utility of government regulation.

Wilson finds little evidence to support capture theory, at least not today. As he puts it, “It is now rare to find an agency that can deal in a purely clientele relationship with a single affected group” (p. 84). He cites developments such as mass mailings and access to the courts, which make it difficult for a single regulated interest to dictate terms to a given regulatory agency.

Wilson is not the only scholar to criticize capture theory. Hugh Hecló (1978) did this, in an article on the emergence of “issue networks” that are much more porous and dynamic than the “iron triangles” of old. However, Wilson goes well beyond a critique of capture theory to develop his own analytic framework for thinking about the relationship between interests and administrative agencies. Building on a previous framework (Wilson 1973), he sorts issues—and agencies—into four categories, based on whether the benefits and costs of the policy proposals they consider are narrowly concentrated or widely distributed.

If a policy proposal offers concentrated benefits but distributed costs, something akin to capture emerges. Wilson calls this situation “clientele politics.” If, however, a policy proposal offers distributed benefits, concentrated costs, then “entrepreneurial politics” is likely, with a policy activist championing a new law or a new regulation that special interest groups are likely to find unappealing. If both costs and benefits are concentrated, then “interest group politics” arises, with two mighty interest groups clashing. Finally, if both costs and benefits are distributed, then we see “majoritarian politics,” with considerable involvement by members of the general public.

The Federal Communications Commission, created to secure property rights for broadcasters, illustrates clientele politics. The National Highway Traffic Safety Administration, created in response to a moral crusade led by consumer advocate Ralph Nader, is a good example of entrepreneurial politics. The Occupational Safety and Health Administration, which originated out of a clash between business and labor organizations, illustrates interest group politics. The Antitrust Division of the Justice Department—a response to a public inflamed by abuses by large corporations—is a good example of majoritarian politics.

Wilson’s framework improved upon an earlier typology developed by Lowi (1964), whose framework was one-dimensional. Wilson’s key insight—that you need to look simultaneously at how many people are expected to benefit *and* how many people are expected to be harmed by a given policy proposal—has helped scholars to differentiate across issue areas and across administrative agencies, thus avoiding the dangers of oversimplification. It has also helped us to understand why some administrative agencies pursue some version of the public interest, while others seem dangerously beholden to special interest groups.

On the other hand, Wilson’s framework presents some difficulties to scholars and students who seek to apply it. First, it is often difficult to pinpoint the threshold where

a narrowly concentrated benefit becomes a widely distributed one. If a particular public policy helps all poor children, are the benefits widely distributed? If another public policy benefits senior citizens and most of us become seniors eventually, are the benefits widely distributed or narrowly concentrated? Second, Wilson urges us to focus on policy proposals, as opposed to issues. But, in practice, a proposal with distributed benefits and concentrated costs often triggers a counterproposal with concentrated benefits and distributed costs. To the naked eye, the politics of the one closely resembles the politics of the other. If so, maybe Lowi was right to focus on issues rather than policy proposals. Third, Wilson urges us to focus on perceptions, rather than underlying realities. But perceptions are often fleeting and difficult to discern. And whose perceptions really matter? Those of the attentive public or the citizenry as a whole? In short, Wilson's framework, like others that have been advanced (Gormley 1986), has its own limitations.

Accountability

In the US today, we expect civil servants to be accountable to managers and to political executives. This is not new. What is new is that we now expect bureaucrats not simply to follow instructions and comply with the law but also to produce results. This emphasis on results was engrained in the Government Performance and Results Act of 1993 (Radin 2006), in the Program Assessment Rating Tool of the Bush administration (Gilmour and Lewis 2006), in a movement to produce "organizational report cards" for many different types of organizations (Gormley and Weimer 1999), and in efforts to make public school teachers more accountable through the use of "value-added models" that try to pinpoint the effectiveness of individual teachers in shaping student test results (McCaffrey et al. 2003).

Well before these developments became commonplace, Wilson developed a framework for thinking about accountability—how easy or difficult it is to hold different types of agencies accountable and what form accountability might take. Wilson began by distinguishing between outputs and outcomes. Then he asked whether outputs and/or outcomes can be directly observed.

If outputs can be observed but outcomes cannot, you have what Wilson calls a "procedural" agency. Procedural organizations, like the Occupational Safety and Health Administration (OSHA) or the US Air Force during peacetime, focus obsessively on means, which can be measured, rather than ends, which cannot be. A common pathology is "beancounting" at the expense of real progress (Gormley 1989). If outcomes can be observed but outputs cannot, you have what Wilson calls a "craft" agency. Craft organizations, like the Forest Service or the armed forces during wartime, feature bureaucrats whose actions are very difficult to observe but whose success or failure can be measured (e.g. by focusing on the amount of logging that takes place in national forests or on ground gained in war). "Production" agencies, like the US Postal Service or the Social Security Administration, have the enviable ability to measure both outputs and outcomes. In

principle, they should be easier to manage and their performance should be superior. “Coping” agencies, like public schools and police departments, are doubly cursed. Because it is hard for them to hold bureaucrats accountable, they sometimes focus on output measures whose connection to desired outcomes is debatable (e.g. the number of tickets issued by police officers or the number of students who advance to the next grade).

Wilson’s assessment of “coping” organizations is especially gloomy: “In coping organizations effective management is almost impossible” (p. 175). Nevertheless, he notes that schools can change significantly when outside groups insist on it (p. 224) or when an outsider literally takes over the organization (p. 229). Michelle Rhee’s appointment to head the Washington DC Public Schools would be a good example of the latter. However, Rhee’s subsequent resignation is a sobering reminder of how fragile school reform efforts can be. Schools also illustrate the hazards of classifying a type of bureaucracy for all time. As value-added models of teacher performance have become more commonplace and as classroom visits by principals and others have become more meaningful and more consequential, accountability has become notably stronger, though undoubtedly much more remains to be done (Gormley and Balla 2013).

Bosses

In contrast to principal-agent theorists, who stress the difficulties that principals have in controlling agents, Wilson argues that the bureaucracy’s bosses have many means at their disposal for influencing bureaucrats. Congress, he argues, frequently “micro-manages” bureaucracy, though the preferred form of intervention seems to have changed over the years, from favor-seeking to procedural constraints. He also reminds readers of the special difficulties of controlling coping agencies: “Legislators ordinarily do not dominate the work of public schoolteachers and police patrol officers for the same reason that school superintendents or police chiefs cannot readily manage the work of their subordinates” (pp. 244–5).

Presidents, Wilson notes, have contrived numerous strategies for controlling bureaucracies, such as administrative reorganization and interagency coordination. The creation of the Office of Management and Budget during the Nixon administration was a signal event in facilitating presidential control. On the other hand, the creation of the civil service made presidential control more problematic. As with Congress, Wilson notes, production and craft agencies are more easily controlled (by Presidents and by Congress) than procedural and coping agencies.

In discussing the courts, Wilson conveys deep suspicions about the propriety of increasingly prominent judicial interventions into the administrative process. He marvels at the frequency with which administrative agency rules are challenged in federal court and he documents some of the costs of strong judicial review. He also notes that the courts have greater ease in controlling practices rather than outcomes: “it is easier to tell a school to stop excluding blacks than to tell a school how to improve the educational achievements of blacks” (p. 291).

According to Wilson, micromanagement by Congress, Presidents, and federal courts has grave consequences. “With some conspicuous exceptions the result of this process has been to deflect the attention of agency executives away from how the tasks of their agencies get defined and toward the constraints that must be observed no matter what the tasks may be” (p. 367). For Wilson, it is the mundane but critical definition of tasks that really matters. Everything else is a distraction.

American Exceptionalism

Having discussed organizational culture at some length, Wilson also posits the presence of a national culture that distinguishes US bureaucracies from their counterparts in other countries. Drawing on work by one of his former students, Steven Kelman (1981), Wilson notes that OSHA inspectors tend to “go by the book” when they are enforcing workplace safety regulations. In contrast, Swedish Worker Protection Board inspectors are more likely to offer advice and information (see pp. 295–6).

Wilson extracts from this and other examples the proposition that regulatory enforcement in the US tends to be “adversarial,” while regulatory enforcement in other Western countries tends to be “consensual” (p. 297). He also links these traits to broader regime characteristics. Thus, he argues, parliamentary systems are more prone to consensual norms, while presidential systems gravitate more towards adversarial norms.

Another contrast is the US penchant for “procedural” solutions to substantive problems. In Wilson’s words (p. 331), “In this country, we have a profound bias toward solving problems by adopting rules.” Anticipating one of the key themes of the new public management movement, Wilson advocates fewer rules, more bureaucratic discretion. For example, he notes that police officers need to make judgment calls when asked to handle “disorderly conduct” cases. And it is “virtually impossible” to define disorderly conduct (p. 338). Similarly, schoolteachers and school principals need discretion in meeting the needs of individual students. For this reason, Wilson objects to the Education for All Handicapped Children Act (1975), which requires schools to adopt an individualized education plan (IEP), enforceable in court, for every disabled child (p. 338).

One reason to take Wilson’s concerns seriously is that “when there is a mismatch between legal rules and bureaucratic realities, the rules get subverted” (p. 338). Thus, he notes, many teachers, frustrated by IEPs, choose not to label certain children as disabled, in order to avoid the dreaded rules. Wilson also notes that middle-class parents with disabled children have been more likely to use the legal tools available to them on behalf of their children than disadvantaged parents (p. 339). Thus, arguably, the students who need help more have been less likely to receive it.

Although Wilson makes it clear that he prefers the European paradigm to US practices, he does acknowledge, in his final chapter, that the US system is less sclerotic than it might otherwise be. That is because we have combined an obsession

with rules and an obsession with access (p. 377). It is this paradox, Wilson believes, that has helped to rescue us from some of the worst excesses of Weberian bureaucracy.

Values

A common view of public policy is that markets are better at promoting efficiency, while governments are better at promoting equity. Wilson subscribes to only half of this adage. “Government bureaus are less likely than private agencies to operate efficiently, at least with respect to the main goals of the organization,” Wilson says (p. 349). On the other hand, Wilson disagrees that governments have the edge in promoting equity: “The economic market distributes goods and services on the basis of willingness to pay; the political market distributes them on the basis of law and influence. Which system produces the fairest outcome is not always obvious.” (p. 354). Wilson’s refusal to acknowledge a clear governmental advantage in promoting equity and social justice sets him apart from many other observers (Okun 1975; Osborne and Gaebler 1992).

Because he views markets as more efficient than governments, Wilson is very sympathetic to proposals for privatization, whether through contracts or vouchers. He cites with approval empirical studies that suggest an advantage to markets in delivering physical services. He finds proposals to privatize prisons intriguing, even though he recognizes that issues could arise with respect to the use of deadly force. He warmly endorses housing vouchers, which he pronounces a success. And he recommends that we experiment with school vouchers, to see where they might lead.

Wilson’s “bottom line” is deeply conservative, in the sense that he prefers to rely on markets whenever possible. Still, he does recognize that efficiency shouldn’t be—and indeed cannot be—the sole determinant of how we structure institutions to deliver public services. He recognizes, for example, that choices must be made between promoting efficiency and avoiding arbitrariness, between pursuing fairness and facilitating responsiveness. And he stresses that, while one can evaluate production agencies based on efficiency considerations, it is “impossible” to evaluate coping and procedural organizations by that criterion alone (p. 332). He also acknowledges, belatedly, that “both markets and governments have their imperfections” (p. 368).

Although privatization is one of Wilson’s recommendations, his main preference is to “deregulate the government” (p. 369) or to let bureaucrats be bureaucrats. Wilson believes that street-level bureaucrats should enjoy more discretion and that they will perform better if they do. He believes that it is better to give agencies a strong sense of mission (p. 371) than to impose a detailed list of dos and don’ts. He also believes that we need to “judge organizations by results” (p. 373) rather than by their compliance with procedural requirements.

WILSON'S IMPACT ON OTHER SCHOLARS

Wilson's *Bureaucracy* has been cited by many scholars since its publication in 1989 (over 3,000 times, according to Google Scholar). But citations only hint at the depth of Wilson's impact on the field. In fact, Wilson's book has helped to shape scholarly thinking on bureaucratic politics in many different ways. This is especially true for scholarly studies (both books and articles) that lie at the nexus between public administration and public policy.

Although many of Wilson's ideas and insights have stimulated expressions of approval and applications to specific cases, four of his contributions have been especially influential: his bottom-up approach to studying bureaucracy; his emphasis on organizational culture; his typology of policy proposals that differ in the perceived breadth of benefits and costs; and his typology of administrative agencies that differ in the ease with which outputs and outcomes may be observed.

Bottom-Up Approach

Wilson's bottom-up approach to studying bureaucracy has been embraced by a number of distinguished scholars. In *Working, Shirking, and Sabotage*, John Brehm and Scott Gates (1997), like Wilson, reject a hierarchical view of bureaucracy. In their words: "Supervisors at all levels of public bureaucracies face constraints not dreamed of by managers in private firms" (p. 11). Building on this idea, they carefully examine the relationship between supervisors and front-line staff in a variety of public agencies. They conclude that "bureaucrats do respond to their supervisors" (p. 197) but that "functional preferences" and "solidary preferences" of civil servants are better predictors of bureaucratic behavior (p. 195). Functional preferences are those where subordinates acquire utility from performing their designated tasks, while solidary preferences are those where subordinates acquire utility from friendships with fellow workers (p. 75). These ideas, incidentally, echo themes from some of Wilson's earliest work (Clark and Wilson 1961).

If Wilson's bottom-up approach draws our attention to street-level bureaucrats, it also draws our attention to the tasks they perform—another of Wilson's preoccupations. In *Teaching, Tasks, and Trust*, Brehm and Gates (2008) build an entire book around this central premise—namely, that tasks are of critical importance. "Our premise is that bureaucratic subordinates must choose a variety of discrete activities, tasks, and allocate effort accordingly" (p. 63). Their key insight is that the ability of supervisors to assign tasks to subordinates depends on the level of trust between supervisors and subordinates, which in turn depends on how successful supervisors have been in shielding subordinates from threats to their autonomy.

In *Collision Course*, a study of how education policy-making changed in response to the No Child Left Behind Act (NCLB), Paul Manna (2011) devotes an entire chapter to

teacher tasks. He notes that tasks are shaped by external constraints, by teachers' own beliefs, and by circumstances (2011: 106–8). He then goes on to show how NCLB has affected tasks relating to testing and test preparation: thanks to NCLB, testing and test preparation became more important; teaching to the test became more widespread; also, resources flowed to those subjects that were being tested.

Organizational Culture

In the 1980s, hundreds of savings and loan associations went bankrupt, despite a regulatory system that was supposed to prevent this from happening. In *Public Spirit in the Thrift Tragedy*, Mark Rom (1996) uses Wilson's concept of organizational culture to understand the Federal Home Loan Bank Board, the Federal Savings and Loan Insurance Corporation (FSLIC), and the Federal Reserve Board. He finds that inadequate expertise weakened the FSLIC, making it difficult for the agency to acquire and sustain a culture of competence.

In discussing organizations and organizational cultures, Wilson stresses the critical role played by professions which infiltrate bureaucracies and often come to dominate them by virtue of the strong intellectual norms that guide and unify practicing professionals. In *Checking on Banks*, Anne Khademian (1996) applies these insights to better understand the behavior of the Office of the Comptroller of the Currency (OCC), the Fed, and the Federal Deposit Insurance Corporation (FDIC). The FDIC, she argues, was dominated by risk-averse examiners, who wanted to preserve the integrity of the Bank Insurance Fund at all cost (Khademian 1996: 116–24).

Dan Carpenter (2001) discusses the organizational culture of the Postal Service, the Forest Service, and the US Department of Agriculture in an important study of bureaucratic growth and transformation. A key theme, building on Wilson's work, is that each of these departments had a distinctive organizational culture. Agencies with more functional cultures (e.g. the Department of Agriculture) were better able to prosper over time than agencies with more dysfunctional cultures (e.g. the Department of Interior).

Wilson also notes that many organizations have subcultures. Building on this idea, Cornell Hooton (1997) applies it to his study of the Urban Mass Transit Administration (now known as the Federal Transit Administration). In particular, he notes that the grants office and the planning office have different cultures. The grants office focuses on getting grant applications from local governments approved (as quickly as possible), while the planning office focuses on evaluating and critiquing grant applications. (Hooton 1997: 66–8). The agency as a whole seems to lack a single, consistent purpose (Hooton 1997: 67).

R. Kent Weaver (2010) has used some of Wilson's insights into organizational culture to develop an "implementation analysis" to guide policy-makers. Weaver notes, for example, that there is often a mismatch between a bureaucracy's culture and the new tasks that legislators expect the bureaucracy to perform. This may lead to shirking, subversion, or other bureaucratic pathologies. The solution, Weaver argues, is for legislators to think strategically about the kind of organization that is best suited to

carrying out (or implementing) a particular task. In some instances, that organization may be not an existing bureaucracy but a new one. In other instances, new leadership or structural changes may signal to the existing bureaucracy that new ways of thinking and behaving are required.

In a related paper, on citizen compliance (or non-compliance) with government laws (and rules and regulations), Weaver (2009) borrows Wilson's concept of "situational imperatives" to illustrate how non-compliance may occur. In less developed countries, Weaver notes, many children work, to prop up family incomes. This situational imperative, he observes, facilitates truancy and limits compliance with compulsory education laws. Solutions might include government nutrition programs for families and/or conditional cash transfers.

Brehm and Gates also utilize Wilson's concept of organizational culture to explain police officer behavior. They find that police departments differ in their attitudes toward perpetrators and citizens and that this has effects on the behavior of police officers (Brehm and Gates 1997: 196).

Costs and Benefits Typology

In *Reforms at Risk*, Eric Patashnik (2008) systematically analyzes implementation successes and failures for six significant policy changes involving the tax code, health care, agriculture, government procurement, airline deregulation, and acid rain. He constructed his sample of cases with Wilson's typology in mind (Patashnik 2008: 14), to ensure that diverse patterns of interest group activity would be represented.

In *Policy Entrepreneurs and School Choice*, Michael Mintrom (2000) concludes that when policy entrepreneurs promote school choice through the use of vouchers, in the US and elsewhere, this illustrates diffuse benefits, concentrated costs, or, in Wilson's words, "entrepreneurial politics." He does, however, argue that "the pace of policy change" (Mintrom 2000: 122) can be an important source of policy innovation. Thus, he is less convinced than Wilson of the paramount importance of costs and benefits. As Mintrom sees it, Wilson's framework, though helpful, is incomplete.

One of the more systematic applications of Wilson's cost-benefit typology can be found in Elaine Sharp's (1994) study of the dynamics of issue expansion. Focusing on disability rights and fetal research policy-making at the federal level, she uses Wilson's typology to illuminate changes over time in the politics of each issue. Thus, for example, she shows that disability rights policy-making illustrated "majoritarian politics" at the legislative adoption stage but "entrepreneurial politics" and then "interest group politics" at the agency rule-making stage. This application nicely illustrates two important points—that politics can vary with the specific policy proposal under consideration and with the specific institutional venue as well.

In another creative use of Wilson's cost-benefit typology, in the book, *Disarmed*, Goss (2006) adapts Wilson's cost-benefit typology to produce her own sets of principles

to guide advocates who seek to convert a cause into a social movement. The embryonic gun control movement is a case in point. In Goss's (2006: 50) words: "The central challenge to issue entrepreneurs who want to mobilize the public is to alter individuals' cost-benefit calculation in a way that will increase the odds of participation. To do so, issue entrepreneurs must turn the cost-benefit calculation on its head, by concentrating the benefits of participation at the individual or small-group level and distributing the costs to the broader society." More concretely, Goss (2006: 51) recommends that advocates must "1) socialize the costs of participation; 2) personalize the benefits; and 3) boost the subjective likelihood that personal costs will yield social benefits." This ingenious reformulation of Wilson's framework recognizes that perceptions of costs and benefits may be altered through clever "issue framing" (Chong and Druckman 2007) and identifies a strategy for rethinking benefits and costs that could enhance the influence of public interest advocates.

Outputs and Outcomes Typology

Many scholars have used Wilson's typology on the observability of outputs and outcomes as a starting point for thinking about distinctive characteristics of particular agencies or organizations. For example, Manna (2011: 32) notes that until the 1980s public schools functioned as classic "coping" agencies—school principals and political leaders had difficulty observing what teachers did (outputs) and they also had difficulty determining what students learned (outcomes). That began to change in the 1980s, and it changed dramatically in 2001, with the passage of NCLB. Although Manna does not say so explicitly, the implication is that public schools are no longer simply coping agencies. In short, it may be hard—but not impossible—to observe outputs and outcomes at certain types of agencies, such as schools. The premise behind teacher performance pay reforms is that teaching outcomes can be measured and linked back to teachers.

In *Teaching, Tasks, and Trust*, Brehm and Gates (2008) use Wilson's typology to tease out implications for relationships between superiors and subordinates in different kinds of bureaucracies. For production agencies, they argue, "the paramount problem is selection of subordinates with the right mix of functional preferences, or, distribute perks that adequately reward the bureaucrat for completing those tasks for which the bureaucrat does not have a preference" (Brehm and Gates 2008: 106). In contrast, craft agencies "may prefer that their bureaucrats complete a diverse set of tasks" (Brehm and Gates 2008: 106). Under such circumstances, agency heads may prefer to hire bureaucrats with "heterogeneous preferences" to make it easier to match tasks to qualifications.

Not everyone has uncritically embraced Wilson's typology. In *The Forging of Bureaucratic Autonomy*, Daniel Carpenter (2001: 359) notes that the Post Office was more than simply a "production" agency between 1870 and 1920. In fact, he argues, a given agency may be a production agency in some of its functions, a coping agency in

some of its other functions. Thus the most appropriate unit of analysis may not be the agency but rather a particular administrative function.

CONCLUSION

In much of his published work, Wilson articulated views on public policy that can only be described as conservative. He was tough on crime, a strong proponent of traditional marriage roles, wary of child care, skeptical of the welfare state, and supportive of free enterprise. In *Bureaucracy*, one also sees evidence of Wilson's conservatism—for example, in his support for school vouchers, privatization, and marketplace solutions.

Yet, much of *Bureaucracy* is an eloquent plea to treat bureaucrats and bureaucracy with more empathy, deference, and respect. Wilson emphatically rejects the economic premise that bureaucrats are “utility-maximizers.” He dismisses “capture theory” as a caricature of regulatory agencies that does not fit the facts. And he bitterly denounces politicians and judges who “micro-manage” the bureaucracy. From his vantage point, bureaucrats are trying to execute some extremely difficult and challenging tasks. We should let them get on with it: let bureaucrats be bureaucrats.

These days, Wilson's public policy preferences and views of bureaucracy are seldom found together in the same individual. When was the last time you heard a Tea Party member defend the government bureaucracy? Of course, Wilson was no Tea Party flame-thrower. And his defense of bureaucracy has its limits. In education policy, for example, he would rather bypass public school bureaucracies than reform them.

Still, one wonders what Wilson the conservative found so appealing in the bureaucracy that so many others have criticized. One answer is that bureaucracies, in practice, are seldom as hierarchical as Weber expected them to be. Wilson openly admires that fact, which he views as a cause for celebration, not concern. Another answer is that, while Wilson believes in markets, he also believes in government, provided that the right agency is chosen for the right job. From his vantage point, the best government bureaucracy is one whose organizational culture fits snugly with its legislatively ordained tasks. Once that has been achieved, Wilson believes that bureaucrats should enjoy a substantial dose of discretion and trust.

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CHAPTER 36

ELINOR OSTROM, GOVERNING THE COMMONS: THE EVOLUTION OF INSTITUTIONS FOR COLLECTIVE ACTION

AMY R. POTEETE

ELINOR Ostrom's *Governing the Commons*, published in 1990, speaks to an interdisciplinary audience, reaching beyond the social sciences to ecologists and other natural scientists. Her arguments and analytical approach found an enthusiastic audience within economics and the environmental sciences, as well as among scholars of rural development and policy studies. The research design is innovative and brilliant. Rereading the text today, one finds positions that foreshadow more recent concerns with path dependency, causal complexity, and pragmatist methodology, among other topics.

SHORT EXPOSITION OF THE WORK

Governing the Commons presents itself as a direct challenge to conventional economic models of collective action. These conventional models assume that the management of shared natural resources presents a social dilemma, in which the pursuit of individual interests leads to depletion of the shared resource, a socially inferior outcome. Previous challenges to “the tragedy of the commons” model had established that common property exists and can be sustainable by identifying cases of successful communal management. Ostrom demonstrated that successful common property arrangements exist in more than an isolated handful of cases, but also highlighted

variation in the durability and sustainability of institutions for resource management developed by resource users. Ostrom identified a set of design principles associated with long-enduring instances of common resource management. These design principles continue to set the agenda for research on the conditions for sustainable management of shared natural resources.

In *Governing the Commons*, three influential models represent the conventional wisdom: the tragedy of the commons, the prisoners' dilemma, and the logic of collective action. Each of these models predicts that a set of individuals will be unable to engage in collective action without outside intervention. When applied to natural resources, these models suggest that sustainable management can be achieved only if the resources are state owned or subject to individual private property rights—in other words, if collective action problems are avoided by designating a single manager. Yet successful self-governance occurs and cannot be explained by these conventional models. Ostrom insists that the problem lies not with the internal consistency of the underlying theory, but with the metaphorical application of these models to situations in which their underlying assumptions are not met. People who share a natural resource system do not engage in anonymous once-off interactions. Rather, they interact repeatedly and can discuss strategies for overcoming challenges to resource management. The empirical world is characterized by diversity—not by uniformity. Because conditions are diverse, there is no panacea or unique solution to the challenge of collective action. Rather, there are diverse solutions. Self-governance is possible and there are multiple ways to achieve it, but it is not inevitable.

Governing the Commons sets out to explain

how a group of principals who are in an interdependent situation can organize and govern themselves to obtain continuing joint benefits when all face temptations to free-ride, shirk, or otherwise act opportunistically. Parallel questions have to do with the combinations of variables that will (1) increase the initial likelihood of self-organization, (2) enhance the capabilities of individuals to continue self-organized efforts over time, or (3) exceed the capacity of self-organization to solve CPR [Common Pool Resource] problems without external assistance of some form. (p. 29)

To account for variable and changing capacity to overcome collective action problems, Ostrom takes a two-step approach. First, rather than reject the conventional wisdom out of hand, Ostrom revisits and tweaks the underlying assumptions. Second, she engages in an inductive process that yields a framework for analysis. Although three of her four research questions are framed in general terms, the volume focuses on common pool natural resources. Common Pool Resources (CPRs) refer to resources that can be depleted and for which exclusion is costly but not impossible (unlike public goods).

Ostrom adopts an assumption of weak rationality that is consistent with the conventional wisdom she critiques. Weak rationality means that people behave in an

intentional manner and do the best that they can, but that they have imperfect information and engage in trial-and-error learning. Behavior is influenced by time horizons and discount rates, the structure of situations, and norms of behavior. Interdependent action characterizes CPR situations, even if those situations differ in many respects. Given these assumptions, self-governance presents three puzzles: (1) While rules may solve collective action problems, there is a second order problem of providing rules. (2) Credible commitment influences expectations about the rule adherence of others that in turn influence willingness to adhere to rules oneself. Repeated interactions can give rise to trust and community, which help overcome the challenge of providing rules by increasing the credibility of commitments to rule adherence. Credible commitment, however, depends on monitoring. (3) Mutual monitoring may occur, but enforcement is a public good and its provision is problematic.

These seeming dilemmas can be escaped by revisiting the underlying assumptions. First, where the conventional wisdom equates collective action problems with the prisoners' dilemma, Ostrom contends that collective action situations vary in structure. CPRs, for example, present both problems of provision (investment in the resource system and related infrastructure) and problems of appropriation (the avoidance or limitation of rent dissipation and the assignment of spatio-temporal access rights). While all of these problems involve interdependent action and thus collective action, some of these problems are easier to overcome than others.

Second, where the conventional wisdom treats each collective action problem as an isolated situation, Ostrom draws attention to repeated interactions in multiple interacting arenas and levels of analysis. As many scholars had noted, repeated interactions create opportunities to develop reputations that provide an incentive for cooperation. Further, the assumption that people either cannot communicate or that communication is disregarded because people cannot make credible commitments, does not apply to people who share common pool resources and interact in a variety of other situations. The Institutional Analysis and Development (IAD) framework distinguishes three levels of rules and analysis. Constitutional rules define membership in the decision-making group and the procedures for making decisions, including decisions about changing rules at any level. Collective choice rules define policies to guide actions, management, and procedures for adjudication of conflicts. They are developed and changed within the parameters of constitutional rules. Operational rules guide day-to-day decisions; they are essentially rules of implementation or operationalization within the context of collective choice rules. Arenas of action refer to the various situations in which decisions are made, whether in a formal setting like a council meeting or through informal interactions like a discussion in a cafe. Actors interact in multiple arenas and shift across levels of rules. The interactions across and shifts between arenas and levels can be leveraged to build trust and change the perceived structures of situations, thus facilitating collective action.

Having laid out her assumptions about human behavior and the social world, Ostrom identifies factors associated with variation in the success of collective action through a four-stage inductive process. First, she presents a set of long-enduring,

self-organized, and self-governed CPRs in which resource users confront very different situations and have developed diverse solutions: communal meadows and forests in Törbel, Switzerland (Alps); the common lands of three Japanese villages; three Spanish *huerta* irrigation institutions; and the Philippine *zanjera* irrigation systems. Despite the diversity of these situations and the solutions developed by the resource users, they share several common features. Ostrom refers to these eight common factors as “design principles”:

- (1) Clearly defined boundaries—of the set of users and of the resource system;
- (2) Congruence—between appropriation and provision rules and local conditions;
- (3) Collective choice arrangements with broad participation of the resource users;
- (4) Monitoring—occurs actively and monitors are either appropriators or accountable to appropriators;
- (5) Graduated sanctions;
- (6) Conflict resolution mechanisms—readily available, low-cost, local arenas for conflict resolution;
- (7) At least minimal recognition of rights to organize by external governmental authorities;
- (8) Nested enterprises—for larger systems.

The second stage of the analysis considers cases of institutional creation and change related to the management of several groundwater basins in California. Although there is no list of variables or conditions comparable to the design principles, several themes emerge, suggesting the importance of the spatial size of the resource; the size of the group (number of resource users); asymmetries in use, dependence on the resource, and the structure of the resource; information; the threat of imposed decisions; and the importance of a supportive political context.

Third, Ostrom analyzes cases of CPR failures, possible recovery following failure, and fragility. There are four new cases of failure: the fisheries of Alanya and the Bay of Izmir in Turkey; the Mojave (San Bernadino) water basin in California, and the Sri Lankan fishery of Mawelle. In each of these cases, the national government took actions that undermined the effectiveness of local rules and local limits on who could use the resource. In all four cases, the heterogeneity among the set of resource users made it difficult to impossible to reach a consensus on resource management. The Gal Oya irrigation system in Sri Lanka counts as the only case of possible recovery. An earlier system of self-governance had failed when confronted with centralization, distrust, rigid rules, and enforcement problems. Decentralization and overt efforts to build trust seemed to be leading to recovery, although the system is deemed fragile. Ostrom also considers self-governance in a case of Nova Scotia fisheries to be fragile because the local system of management is not recognized by Canada’s federal government.

Finally, Ostrom categorizes each of the cases as robust, fragile, or a failure (table 5.2, p. 180). For this exercise, institutional performance is deemed fragile in the Alanya, Gal Oya, and Nova Scotia fishery cases. Three of the four cases of groundwater basin

management are considered failures during the early period but robust during the later period. Then, for each case, Ostrom evaluates whether each of the eight design principles is present, weakly present, or absent. All eight principles (or seven, for the smaller and more isolated systems in Törbel and Japan, where the principle of nested enterprises is considered irrelevant) were present in the six cases of robust institutional performance. Some (no more than three of eight) of the design principles are weak or absent in cases with fragile institutional performance. Most of the design principles (five or more of eight) are absent in cases deemed to be failures.

Governing the Commons is perhaps best known for its demonstration that collective action occurs much more frequently than predicted by conventional models and for the eight design principles associated with enduring institutions for local management of common pool resources. It is most often criticized as insufficiently attentive to conflict and to the role of the state and supra-local politics more generally.

AN ENDURING FRAMEWORK: THE DESIGN PRINCIPLES

Ostrom presented the design principles with humility, acknowledging that they had been derived inductively from a limited set of cases and that further research would be needed to evaluate their generality and to refine them. Ostrom shies away from calling the design principles necessary or sufficient conditions for successful collective action. If anything, Ostrom was overly modest. Even if her sample was limited and unrepresentative, it included a mix of historical and contemporary cases related to diverse natural resource systems (i.e. fisheries, forests, and irrigation systems). Faced with this evidence, both the possibility of self-governance and the variability in its occurrence became difficult to deny.

The design principles attracted considerable attention from scholars and practitioners. Efforts to work with the design principles prompted a variety of clarifications and modifications that might be considered friendly amendments. Several design principles have multiple components. Clear boundaries (principle 1), for example, refer to both the boundaries of the group of resource users and the boundaries of the resource system. Likewise, principle 4 specifies both that monitoring should occur and that monitors should be either resource users or accountable to resource users. While Ostrom's original statement about congruence (principle 2) does not include multiple components, Cox et al. (2010) observe that it is open to multiple interpretations; it could refer to congruence in the sense of the technical fit of the rules to the local conditions or to congruence in the sense of proportionality in the costs and benefits arising from adherence to rules. They suggest splitting each of these principles into two parts, bringing the number of design principles up to 11. Application of the framework to natural resource systems subject to multiple uses requires further additions (Edwards and

Steins 1998). The notion of clear boundaries requires clarification, for example, to deal with multiple non-exclusive groups of resource users that use resources with varied if overlapping spatial distribution (e.g. mushrooms, firewood, walking paths, or shrimp, migratory fish, scuba diving).

With these modifications, and despite the analytical challenges of dealing with multiple uses, the design principles have held up well. A meta-analysis of 91 scholarly evaluations of the design principles found that they were strongly supported by detailed case studies, statistical analyses, and synthetic studies of natural resource management, regardless of the natural resource sector under consideration (forestry, grazing lands, irrigation, fisheries, multiple use, or other) (Cox et al. 2010). Only abstract analyses that emphasized logical considerations were more critical than supportive on average. When Cox and colleagues considered the design principles one-by-one, they found the strongest support for principle 4B (that monitors should be either resource users or accountable to resource users) and principle 2B (congruence as proportionality in costs and benefits arising from rule adherence), followed by principle 1A (clear boundaries of the set of resource users) and principle 2A (congruence as technical fit between rules and local conditions) (2010).¹

Ostrom's design principles have survived evaluation in diverse settings, but they are not the only or uncontested explanations on offer. Other scholars have identified a wide array of other factors that also appear to influence collective action for the management of shared resources. An effort to synthesize work in this area just over a decade after publication *Governing the Commons* identified 35 distinct factors associated with successful management of common pool resources (Agrawal 2001). In her own later work, Ostrom developed an even more expansive framework for the study of socio-ecological systems (Ostrom 2007, 2009). A plethora of factors allows for multiple causation and other forms of causal complexity. At the same time, however, it poses important analytical challenges. Ostrom defended her expanded framework by comparing it to the ontological approach used in medicine, in which doctors narrow in on a diagnosis by working through a lengthy checklist of possible symptoms (Ostrom 2007, 2009). While some scholars welcomed this move toward a more expansive framework (e.g. Young 2011), the original design principles had the advantage of being more concise and thus more accessible. Further, any list of possibly important conditions must be combined with a solid understanding of the dynamics of socio-ecological systems to guide the assessment of which conditions are important in particular contexts. The very complexity of socio-ecological systems that makes an ontological approach attractive, however, also presents challenges for the application of such an approach.

NOT ENOUGH POLITICS?

Critics of *Governing the Commons*, and of Ostrom's work on collective action more generally, complain that it gives inadequate attention to politics and to supra-local dynamics. Politics, conflict, the state, and other supra-local dynamics do feature in

both the case narratives and the design principles. The case narratives describe conflicts of interest among groups of resource users and the role of the state in either fostering local initiatives (e.g. in California) or, more frequently, undermining previously functional local systems for resource management. The narratives also note threats to local self-governance arising from market pressures. In highlighting the importance of accessible and low-cost mechanisms for conflict resolution and supportive relations with larger-scale organizations, the design principles acknowledge these factors. And yet, Ostrom's framework focuses on characteristics of local groups of resource users who are assumed to have a common—if not perfectly aligned—interest in managing shared natural systems and treats the state and other factors as exogenous background variables.

Ostrom's conceptualization of distributional conflict as something to be managed and of the state as primarily a threat to local initiative is typical of what Knight (1992) calls sophisticated theories of institutional provision:

Issues of distribution are explicitly acknowledged in these intentional theories, usually to explain the suboptimality of political institutions. But the interpretations given in these accounts maintain a primary focus on collective benefits and coordination. Implicit in these arguments is the idea that inefficiency and suboptimality are somehow the product of state intervention in the natural order of things. (Knight 1992: 13)

According to Knight, these approaches reflect a naïve understanding of the role played by conflict and competition in motivating institutional change. Institutions do not manage conflict so much as arise out of conflict and bargaining. Knight's critique resonates with a wide variety of other approaches that see competition and conflict as pervasive. Competition and conflict is a fundamental source of dynamism because actors are constantly looking for opportunities to redefine situations, whether to consolidate existing advantages or improve their situation. Repertoires of contention vary widely (Poteete and Ribot 2011; Tilly 1978), with common elements ranging from Scott (1985)'s "everyday forms of resistance" to coalition building (Sabatier and Jenkins-Smith 1993; Yashar 1997), forum or venue shopping (Baumgartner and Jones 1991; Berry 1993) and shopping venues (Lund 2006; von Benda-Beckmann 1981), the politics of scale (Lebel et al. 2005), and institutional reinterpretation or conversion and layering (Thelen 2003).

Governing the Commons has also been criticized for its relative inattention to broader supra-local dynamics. Although the case narratives address market pressures, demographic changes, and interventions by national-level politicians and governments, the framework for analysis and design principles focus attention on local conditions. Of the eight design principles, only the last two—external recognition of local rights to organize and nested enterprises—address external factors, and both of these concern political (authority) relations. Market forces and demographic changes enter the analysis as background conditions that influence the heterogeneity of interests among

resource users. Other macro factors, such as globalization, economic development, and historical patterns of social organization, receive little to no attention. Even if the property systems are not driven simply by shifts in relative prices as suggested by earlier scholarship (e.g. Alchian and Demsetz 1973; North and Thomas 1973), macro conditions and dynamics influence local conditions and dynamics and thus the prospects for self-governance of common pool resources. Since the various dimensions of social and economic development can be expected to influence the prospects for collective action in contradictory ways (Rudel 2011; Richards 1997), any analysis should consider these factors explicitly (see also Agrawal 2001).

If Ostrom's relative inattention to conflict and supra-local conditions makes her seem overly optimistic about the prospects for collective action, she may be overly pessimistic about the role of the state in fostering sustainable local management of shared natural resources. The final chapter of *Governing the Commons* examines the role of the state more closely, distinguishing among situations where the external regime is neglectful or absent, present and supportive of local initiatives, or present and actively taking initiative. Where the external state is active and taking initiative, it is likely to crowd out local initiatives and encourage lobbying, regardless of whether state actors are honest or corrupt. Ostrom implicitly assumes that central initiative means uniformity, or at least a strong tendency toward uniformity. Consequently, variation in local institutions occurs only when there is no central authority, where the central authority supports local initiatives (rather than taking initiative itself), or takes initiative but is corrupt and thus allows exceptions in response to lobbying. Thus, the best situation for self-governance involves the presence of an external regime that supports local initiatives, as with the development of water boards in California.

State interventions frequently undermine or destroy local systems of natural resource management, creating rather than solving tragedies. *Governing the Commons* attacks conventional models that encourage these sorts of destructive policy interventions. Given the emphasis on demonstrating the potential for self-governance, the relative inattention to the potential positive role of the state makes sense. But does central initiative have to result in uniformity? Even officially uniform state policies are rarely uniform in practice as state actors respond to diverse local conditions (Boone 2003). And do all state actions clash with spatially variable ecological conditions, systems of production, or social organization? States can play a more active role not just in allowing local initiative but by promoting it (Anthony and Campbell 2011). Further, the state can counter the undesirable effects of conflict (e.g. exclusion and inequality) by ensuring broader participation in decision-making (Knight 1992). The potentially positive role of the state in countering local oppression and promoting more equitable outcomes also appears as a recurring theme in research on decentralization (e.g. Bardhan 2002; Gibson 2005; Mohan and Stokke 2000; Turner 2002). Just as self-governance is possible but not inevitable, a positive role for the state is possible but obviously not inevitable. Nonetheless, scholars should acknowledge and seek to account for both possibilities. As Bardhan (2002) notes, there is no theoretical reason to expect local actors to be less corrupt or power-hungry than actors at the national level.

BROADER CONTRIBUTIONS

Beyond the design principles, *Governing the Commons* contributes to more general debates about collective action, institutions, and methodological practice. In some respects, it presents an emblematic contribution to the new institutionalism at its high tide in the early 1990s. *Governing the Commons* pushes the boundaries of institutionalism, however, particularly in its rational choice incarnation. Like others within this tradition, Ostrom emphasizes the importance of institutions in overcoming collective action problems and seeks to understand institutional origins and change (North 1990; Shepsle 1989; see reviews in Hall and Taylor 1996; Immergut 1998). Ostrom depicts institutional change as incremental, endogenous, and characterized by path dependency. Many other scholars, particularly within the historical and sociological institutionalist traditions, would agree with these points, at least in the abstract. Ostrom goes further to argue that there are *always* institutions. Her deontological approach distinguishes three types of rules: “may,” “must,” and “must not.” If “may” is recognized as a rule, institutions exist even in the absence of rules specifying what one “must” or “must not” do. If there are always institutions, there is no meaningful break between institutional origins and institutional change. These processes are incremental and sequential, involving small steps that often are not so costly.

Like Douglas North but more than the typical contributors to this research tradition, Ostrom gives considerable attention to mental models and psychology, as reflected in her conceptualization of rules as shared understandings of acceptable behavior, whether written or informal, and her identification of shared understandings of management problems and the development of trust as critical for the development of enduring institutions of self-governance. While the importance of reputation as developed through repeated interactions is a leitmotif in rational choice scholarship (Axelrod 1984; Kreps et al. 1982), Ostrom’s conceptualization of social trust has much in common with the notion of social capital as developed by Putnam (1993) and others.

Governing the Commons also departed from mainstream approaches to social science research in adopting an explicitly inductive strategy for analysis and developing a framework rather than a theory or model to explain varied outcomes. Ostrom and colleagues distinguish frameworks from theories and models based on the degree of specificity in the hypothesized relationships (see also Schlager 2007). A framework identifies the important factors to be considered in the analysis of some phenomenon without specifying the nature of the relationships among those factors or making precise predictions. Theories are more specific; they offer logically coherent hypotheses about the nature of relations among key variables and offer general predictions. Models depict specific situations based upon a particular set of conditions and assumptions.

As a framework for analysis rather than a theory or model, the design principles identify factors associated with enduring institutions for self-governance of shared natural resources, but that operate in varied ways, depending on the characteristics

of particular situations. Ostrom does not depict the principles as a probabilistic and additive model of successful collective action, nor does she suggest that they represent a set of necessary or sufficient conditions. Instead, insisting that there is no panacea, Ostrom argues that there are many ways to achieve sustained collective action. In essence, Ostrom rejects the pursuit of universal covering laws on pragmatist grounds—they do not work in a complex world—and argues forcefully for an analytical approach that is consistent with multiple causal accounts. In this regard, *Governing the Commons* was revolutionary, predating broad acceptance of causal complexity within political science by a decade or more (Brady and Collier 2010; Braumoeller 2003; see also Ragin 1987).

NOTE

1. While meta-analysis offers a useful gauge of the external validity of the design principles, the one-by-one assessment of principles in Cox et al. (2010) is based on an additive model that is inconsistent with the logic of Ostrom's explanation. Assessments based on the logic of an additive model may be just as misleading when applied to the design principles as they are when applied to explanations involving necessary or sufficient conditions (Dion 1998; Rudel 2008).

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CHAPTER 37

CHRISTOPHER HOOD, “A PUBLIC MANAGEMENT FOR ALL SEASONS?”

PER LÆGREID

INTRODUCTION

THE issue of state reform has been a recurring theme in the study of public policy and administration. With his 1991 seminal article “A Public Management for All Seasons?” Christopher Hood coined the term used to describe the reforms that had been around in the public sector in many countries, especially English-speaking countries, since the early 1980s and thereafter became known as “New Public Management” (NPM).¹ New Public Management introduced private sector management tools and organization forms into the public sector and had a special focus on efficiency. With 6,207 citations on Google Scholar it is also the most-cited article in the history of the long-standing journal *Public Administration* and also by far the most cited of Christopher Hood’s many scholarly publications. In addition, it is one of the articles most frequently referenced in public administration and public management research over the last 20 years. The number of citations peaked in 1998 but the article is still mentioned in a substantial number of publications every year. The article, which made a significant contribution to setting the research agenda in administrative reform in the 1990s and beyond, delineates the essence of NPM and its impact on the theoretical landscape of public administration. The article is a skeptical argument that no single approach to organization and management could satisfy all administrative values in all circumstances (Hood 2011).

Hood’s concept related to ideas that were on the agenda from the mid-1980s, revolving around the questions of what is new and different about public management in contrast to public administration (Overman 1984). Perry and Kraemer (1983) claimed that public management is a distinct and integrative approach merging the normative

oriented tradition of traditional public management and the instrumental orientation of generic management. Gunn (1987) challenged their politics and administration distinction and argued that management processes affect political processes and vice versa. Hood's position is closer to the latter, questioning the idea of generic management as well as the politics-administration divide and he is, like Aucoin (1990) and Pollitt (1990), more preoccupied with the paradoxes and pendulums in the public management paradigm.

Hood discusses the doctrine of NPM and the reasons why it has been criticized. He identifies seven different doctrinal components of NPM: hands-on professional management, explicit standards and measures of performance, greater emphasis on output control, disaggregation of units, greater competition, private sector styles of management, and discipline and parsimony in resource use. He shows that NPM is not a coherent set of ideas but a marriage between different theoretical streams—on the one hand, economic organization theory, which underlines competition, user choice, transparency, contracts, and incentives (“make the manager manage”); on the other hand, management theory, which focuses on flexibility, autonomy, and discretionary power (“let the manager manage”). The new set of doctrines combined managerialism with economic rationalism. Thus, as a “marriage of opposites” NPM contains a lot of inbuilt tensions (Hood 1991: 5).

Hood debates the justification for different administrative doctrines and asks why some doctrines are accepted and others are not. He argues that there is no single explanation for why NPM found favor in specific countries at a specific point of time. Skeptical explanations focusing on the importance of administrative fashions and NPM as a ritual “cargo cult” might be part of the answer. But it can also be interpreted as a “synthesis of opposites.” According to Hood, the most promising interpretation, however, is to see NPM as a response to the special social conditions that had developed out of peaceful economic growth in developed countries since World War II.

He pays particular attention to the claim that NPM is a panacea for poor or inefficient public service provision. NPM was presented by those who adopted it as an “all purpose garment” and launched as a generic and holistic “catch-all” framework. The claim that NPM was universally applicable and offered the one best approach regardless of context was followed by ideas of transferability and diffusion. The same set of doctrines was supposed to be applicable in different countries, levels of government, and policy areas. It was also presented as a politically neutral or apolitical framework that could pursue different values and goals set by the political executive.

Hood discusses four main criticisms of NPM. The first one is that NPM is like the Emperor's New Clothes—all hype and no substance. In other words, NPM has changed the talk and the language but underneath the old problems remain. The second criticism is that NPM has damaged public services because it has not been able to deliver on its central claim of cost reduction and more efficient public services. Creative accounting, a whole new industry of performance indicators and reporting, and playing the system have destabilized the bureaucracy and weakened essential components at the front line. The third criticism is that NPM is a vehicle for particularistic advantage,

self-interest, individualization, and aggregative behavior that tend to undermine the public ethos, the public good, and integrative behavior in public bureaucracies. The fourth line of criticism, to which Hood pays most attention, is NPM's claim of universality. The main incompatibility argument is that different administrative values have different implications for administrative design. Hood argues that NPM has been most commonly criticized because of its alleged contradiction between "equity" and "efficiency" values.

A main feature of the paper, however, is its discussion of possible conflicts between three core sets of administrative values. First, sigma-type values: *lean and purposeful*, focusing on matching resources and defined tasks, efficiency, economy and parsimony, fixed and single goals, money, output control, performance management, tight coupling, and low slack. Second, theta-type values: *honest and fair*, addressing mutuality, impartiality, neutrality, trust, incompatible goals, process control, anti-corruption, medium coupling, and medium slack. Third, lambda-type values: *robust and resilient*, focusing on security, reliability, survival and adaptivity, emergent and multiple goals, input control, redundancy, diversity, loose coupling, and high slack. The main argument is that these different types of administrative values have different implications for administrative design. While there is some overlap, it will be hard to satisfy all three sets of values using the same administrative design dimensions. NPM's focus on economy and frugality, for instance, might undermine the values of honesty and resilience.

The implication is that NPM is primarily an expression of sigma-type values, focusing on cutting costs and doing more for less. There is, however, little reliable evidence of such effects on efficiency and cost-reduction. The question of whether such effects can be obtained without negative side-effects on honesty and fairness and on security and resilience has yet to be investigated, and it remains to be seen whether dismantling administrative tools and measures instituted to enhance honesty, impartiality, and neutrality will tend to weaken such traditional values and the ethical capital of public service. It also remains to be seen whether contracting out, corporatization, cost-cutting, and fragmentation are compatible with the need for safety, security, and resilience.

The publication can be seen as part of Hood's interest in the evolution and contestability of administrative doctrines and how specific ideas become popular at different points in time (Hood and Jackson 1991). He focuses on how different doctrines shape strategies, administrative tools, and techniques but he also warns against simplistic assumptions about design. He emphasizes the unintended consequences and paradoxes inherent in NPM (Hesse et al. 2003). The conclusion is that the value of research on NPM has chiefly been to identify the key conceptual questions raised by NPM rather than to answer them. Hood asks for more tests of the limits of NPM in terms of different administrative values.

Hood's article represents an early approach to NPM that later came to be refined by more systematic empirical and comparative work (Barzelay 2000; Pollitt and Bouckaert 2011). In the remainder of this chapter I will address some of the main issues

raised by Hood in his 1991 article and discuss how they have been dealt with in subsequent research.

WHAT IS NPM? AN EXPANDING AND LOOSE CONCEPT

As indicated by Hood, NPM is a loose and multi-faceted doctrine that encompasses a range of different administrative tools. He claims that administrative reform has become a theological issue in politics with “religious wars” over terminology and content (Hood 2005). It is characterized as a “shopping basket,” meaning that it is a collection of different means and measures not all pointing in the same direction and not all included in all baskets (Pollitt 1995). In addition to the components identified by Hood, there are other elements such as decentralization of management authority within public agencies, separating the functions of public service provision and purchase, contract management, service quality, and customer responsiveness (Pollitt 1995). Other characteristics, such as separating political decision-making from direct management and community governance, are often added to this list (McLaughlin et al. 2002; Kettl 2006). Over recent years regulation, auditing, and evaluation have become an integrated part of NPM, a trend recognized by Hood and colleagues already in 1998 (Hood et al. 1998). Thus one can distinguish between “hard” NPM tools, which address accounting, auditing, and performance measurement; and “soft” NPM tools, which include things like human factors, user-orientation, quality improvement, and individual development. Ewan Ferlie (1996) distinguishes between four different NPM models: the efficiency drive, downsizing and decentralization, the search for excellence, and public service orientation. NPM promises to integrate these themes, linking efficiency and accountability. One can also distinguish between NPM as a big general model and its specific tools and measures (Pollitt and Bouckaert 2011). Not all tools can be exclusively associated with one model and thus there is no one-to-one relationship between specific techniques and the general model. Thus, in subsequent literature new characteristics and tools have been added to the original list of NPM features in Hoods 1991 article. One can discuss if the term has become so loose as to lose meaning (Hood 2005).

The main components of NPM are, however, disaggregation, competition, and use of incentives (Dunleavy et al. 2005). One primary characteristic of NPM is the adoption by public organizations of the management and organizational forms used by private companies. This challenges two traditional doctrines of public administration (Dunleavy and Hood 1993): that public sector organizations are “insulated” from the private sector in terms of personnel, structure, and business methods; and that they operate in accordance with a precise set of rules limiting the discretion of public officials. In contrast the NPM movement subscribes to the generic principle that the

formal organization of the public and the private sectors should be similar and that managers of public sector organizations should have enough leeway in their daily work to be able to make efficient use of allocated resources.

WHY NPM? MULTIPLE DRIVING FORCES

As underlined by Hood (1991), there is no single-factor explanation for the emergence and popularity of NPM. Hood's article helped to shape the independent variables that other scholars looked at and it had a major impact on the comparative public administration field. Broad convergent pressures have led to largely similar reform programs. However, differences remain in the character, style, pace, and timing of the reforms. These differences may be explained by analysing the nature of the pressures, the size and scope of the traditional public sector, the ambitions and durability of governments, as well as their ideological commitment, and the political opportunity structure (Wright 1994).

Hood's account of the triggers of NPM has infiltrated later empirical works. The institutional dynamics of the reforms can be interpreted as a complex mixture of environmental pressure, polity features, and historical-institutional context (Christensen and Lægreid 2001, 2007a). These factors can both enhance and obstruct reforms and define how much leeway political leaders have in making choices about them.

One school of thought points to the fact that different countries have different *polity features and political-administrative structures* and contends that these factors go some way to explaining how they handle national problems and reform processes. From an instrumental point of view the reforms may generally be seen as conscious organizational design. This perspective is based on the assumption that political and administrative leaders use the structural design of public entities as an instrument to fulfill public goals. Two aspects are important in instrumental decision-making processes: social or political control and rational calculation or the quality of organizational thinking (Dahl and Lindblom 1953).

Another view holds that reforms are primarily a product of the *national historical-institutional context*. Different countries have different historical-cultural traditions and their reforms are "path dependent," meaning that national reforms have unique features. Thus, the cultural context of reform is important (Verhoest 2011). The cultural features of public organizations develop gradually in institutional processes, giving institutionalized organizations a distinct character. How successfully a reform wave like NPM is applied in a public organization has a lot to do with cultural compatibility (Brunsson and Olsen 1993). The greater the consistency between the values underlying the reforms and the values on which the existing administrative system is based, the more likely the reforms are to be implemented.

Historical traditions in the state and administration constrain the reform trajectory. Whether countries have a legalistic *Rechtsstaat* tradition, a politicized tradition,

a consensual and corporatist tradition, an Anglo-American, Napoleonic, Germanic, or Scandinavian administrative tradition matters for the reform paths they choose. Traditions are important, but they do not determine reform choices and they need to be understood as one of several factors affecting the way administrative reforms develop (Painter and Peters 2010).

A third view regards NPM primarily as a response to *external pressure* which can be of two kinds: either institutional or technical (Meyer and Rowan 1977). In the first instance a country may adopt internationally based norms about how a civil service system should be organized simply because these have become the dominant doctrine diffused all over the world. Hood labeled such fashions and ritual a “cargo cult” phenomenon. This diffusion process implies isomorphic elements, creating pressure for similar reforms in many countries and representing a kind of “taken-for-grantedness” concerning which organizational forms are appropriate. They function as “window-dressing,” enhancing legitimacy without actually affecting practice (Brunsson 1989).

In the second instance, NPM may be seen as the optimal solution to widespread technical problems—i.e. it is adopted to solve problems created by a lack of instrumental performance, by economic competition, or by changing socio-technical system and demography, also emphasized by Hood (1991). In this instance NPM reforms are adopted not because of their ideological hegemony but because of their technical efficiency. Quite often, NPM reforms are a response to the technical environment, such as an economic crisis or changing political or administrative pressure.

Summing up, external reform programs are filtered, interpreted, and modified by a combination of two nationally based processes. One is a country’s political-administrative history, culture, and traditions. The other is national polity features, as expressed in constitutional and structural factors. Within these constraints political and managerial executives have varying leeway to launch NPM reforms via an active administrative policy. The process of reform has not been the same everywhere. In some countries there might be a strong element of diffusion of NPM ideas from outside, whereas in others the reform process might be more a result of national or local initiatives that have subsequently acquired an NPM label.

Studies of NPM reform processes around the world reflect many of the theoretical points outlined by Hood (Christensen and Lægreid 2001, 2007a; Pollitt and Bouckaert 2011; Verhoest et al. 2010). NPM processes seem to pose great challenges concerning rational calculation. The complexity of different and changing environments for reform, different cultures and structures, multiple goals, intentions, interests, problems, and solutions makes organizational thinking problematic. Political and administrative leaders’ ability to control the processes is constrained by negotiations, cultural resistance, and pressure from the technical and institutional environment. One factor that Hood discusses is changing politics, but not many scholars have followed up on this driving force.

HAS NPM WON? DIVERGENCE MORE THAN CONVERGENCE

A main research topic following Hood's 1991 article was the generic question of whether NPM is about convergence or divergence. Although NPM represents a global change of paradigm, this convergence thesis is contested (Pollitt 2001). Undoubtedly, NPM has left its mark. Many of the ideas and practices referred to in "A Public Management for All Seasons?" have moved from fashion to mainstream (Hood 2011). Yet measured against its self-proclaimed universal relevance, NPM clearly has *not* become the predominant public management paradigm. NPM has led to major changes in the public sector in many countries (Pollitt and Bouckaert 2011). Yet the spread of NPM is a complex process, going through different stages and packaged in different ways in different countries, with each country following its own reform trajectory within a broader framework (Bouckaert et al. 2010). For example NPM has not become the only public management paradigm in developing countries even if it has had some transformative power. Hierarchical bureaucracies have not been replaced with NPM features (Manning 2001). In Europe there might have been a development from an active to an enabling state, but it has been diverse and not followed a straight path in different countries (Page and Wright 2007). In his work on "Public Sector Bargains", on "Regulation inside Government," and on "the Art of the State" Hood has also questioned whether NPM has won or led to convergence (Hood et al. 1998; Hood 1998, 2002).

There is no clear convergence toward one single organizational form (Pollitt and Bouckaert 2011). What we see is a complex combination of old public administration, New Public Management, and post-NPM features, often containing elements that point in different directions (Christensen and Læg Reid 2007b). NPM ideas have been implemented to different degrees, at different paces, and with differing emphases in different countries and sectors. A general finding is that the degree of variation between countries and also between policy areas increases when we move away from the world of ideas, talk, and policy programs and look at specific decisions, and even more so when we consider the implementation and impact of the reforms (Pollitt 2001). One can argue about whether NPM has led to a convergence of administrative systems in different countries, yet there is much to suggest that ideas and policy programs resemble one another more than the corresponding practices do.

Some major NPM reform ideas have spread around the world quite easily, while the more specific reform measures have shown patterns of divergence. One main reason for this may be national variations in polity features, different political and administrative cultures, and different environments. The combination of factors furthering NPM reforms is more likely to occur in Anglo-Saxon countries where there is elective dictatorship, cultural compatibility, and often strong environmental pressure for NPM reform (Christensen and Læg Reid 2001). At the other extreme, weaker control of processes, cultural incompatibility, and low environmental pressure may lead to reforms

that are less NPM-oriented, as has traditionally happened in continental Europe (Hood 1996). A wide variety of countries fall between these extremes, however.

Although different countries present their reforms in similar terms and support some of the same general administrative doctrines, closer scrutiny reveals considerable variation (Christensen and Lægreid 2012). Pollitt and Bouckaert (2011) distinguish between two main models. First, the fast-pace NPM marketizers—Australia, New Zealand, and the UK, which they label the core NPM group. The second group are slow-moving systems consisting of the continental European and Scandinavian modernizers. They claim that the latter group of countries represents a distinct reform model labeled the neo-Weberian state. They share a more positive attitude toward the state and a less positive attitude toward private sector models, and underline the role of representative democracy and administrative law. Compared to traditional bureaucracies they are more focused on citizens' needs, citizens' participation, performance and results, and professionalization of public service. Citizen orientation and participation are more characteristic of the Northern countries than of France, Italy, and Belgium, which are more managerially oriented modernizers. Compared with the Northern countries, the Southern ones, whose state traditions are based on the Napoleonic legacy, were latecomers to reforms (Ongaro 2009).

There are also differences between administrative levels. The United States, Belgium, and Germany have been more reluctant to introduce NPM reforms at the federal level than at lower administrative levels. Another way to look at convergence and divergence is to examine the political salience of different tasks. The more politically important tasks are, the less NPM measures tend to be used, because the political leadership will want to have hands-on political control. There are also variations between policy areas in the implementation of NPM measures. It seems to be easier to implement the reforms in more technical and economic policy areas than in the "softer" welfare state areas which often represent more mixed and hybrid reform models (Gregory 2003). In areas like welfare, education, health, and environment it is more difficult to quantify and measure goal achievement than in transportation and garbage collection where efficiency measurement might be easier to conduct.

Thus there is no consistent movement toward a new isomorphic model of civil service systems. Variations in reform practice from one country to another and between policy areas are the rule rather than the exception. Different countries and governments face different contexts, risks, and problems and start out with different values and norms. They have different starting points, are at different stages of reform, and face different external and internal constraints (Wright 1994). Most governments still share some main elements of the traditional system of public administration, but some strong common modernization trends have emerged in public services across groups of countries. One of these trends has been a reduction in the differences between the public and private sectors. Nevertheless, the story is not only one of convergence; neither, however, is it a story only of divergence. Instead, what we are seeing is a complex mixture of robustness and flexibility (Lægreid and Wise 2014) and of reform paths that can hardly be explained by using a single-perspective approach.

DID IT WORK? AMBIGUOUS, INCONCLUSIVE, AND CONTESTED EFFECTS

NPM has been around for 25 years. Yet there have been few comprehensive evaluations. Especially lacking are empirical studies of changes in the role of government and citizens as a result of NPM reforms (Van de Walle and Hammerschmid 2011). We know more about changes in processes and activities than about the effects on output and very little about the impact on outcomes (Pollitt and Dan 2011). But generally the scholarly attention has moved somewhat from descriptive mapping and a priori critiques to the analyses of paradoxes and unintended side-effects of NPM-reforms (Hood and Peters 2004) where "soft theory" meets "hard cases" (Hesse et al. 2003). This can be seen as a follow-up on the tensions and trade-offs between different values which is a central feature of the 1991 article. And generally there has been a trend toward collecting more systematical data on how NPM works in practice (Hood 2005).

One lesson is that the effects of NPM are context-dependent rather than general. The main hypothesis of NPM reforms is that they will lead to increased efficiency without having negative side-effects on other goals and concerns. So far this hypothesis has yet to be confirmed as evidence-based fact. While it may be correct under specific conditions, it cannot be said to apply generally to NPM reforms everywhere, in all policy areas, and at all times. Effects are often assumed or promised, but there are few systematic and reliable studies of whether they actually happen, so hard evidence is often lacking. Attention tends to be more focused on strategies, plans, and selective success stories than on systematic analyses of results (Pollitt and Bouckaert 2011).

The evidence-based knowledge about the effects and implications of different administrative reforms is still rather patchy and contested. Means-end knowledge and the ability to engage in ex ante rational calculation of impacts and effects of different organizational forms are rather weak among reform agents. Fast-pace reforms are often symbol-ridden. Different organizational forms matter and affect the way public organizations operate and work in practice. But normally there is no one-to-one relationship between organizational design and performance. This has to do with the fact that context matters to a great extent.

One implication is that there is a need to go beyond the narrow concept of performance measured in terms of economy and efficiency and to include in the equation the broader democratic implications for power relations, trust, accountability, and legitimacy. In most democratic systems, values such as impartiality, predictability, rule of law, political loyalty, political control, participation, responsiveness, professional competence and equity are also important elements of performance (Christensen and Lægread 2007a). Not only effects on main goals but also side-effects and dysfunctions have to be taken into account (Hesse et al. 2003).

One international lesson is that most governments do not learn sufficiently from previous administrative reforms in their own country or in other countries. Alleged

successes often have more influence than elements of reform failure. Therefore, there are a lot of ambiguities in learning from experiences of administrative reforms. And politicians are generally more interested in launching new reforms than learning from previous ones, partly because reforms look more attractive *ex ante* than *ex post* (Brunsson and Olsen 2003).

An important implication is that one cannot just graft private sector management tools, organizational forms, and steering mechanisms onto public sector organizations and expect successful implementation and results. Policy-makers may be well advised not to simply copy new reform solutions like agency models but instead to adapt them to local contexts. One lesson is that holistic or generic models have clear limitations. One of the big flaws of NPM was probably the claim that there was a clear dividing line between policy-making and formulation on the one hand and policy implementation on the other (Kettl 2006).

One conclusion to be drawn is that the considerable variation in the design of NPM reforms between countries, tasks, sectors, and administrative levels will have differentiated consequences for effects studies. In some countries, such as those in Southern Europe, administrative reforms hardly ever seem to have had significant effects (Kickert 2011). The implication is that discussions of the effects of reform must strive for exceedingly precise terminology and must not be conducted at a general level.

In sum, it is hard to say unequivocally what the effects of NPM reforms are, and they are often disputed and uncertain. After several decades of NPM we know surprisingly little about its effects (Christensen and Læg Reid 2011). One reason for this is that the reform movement has made before-and-after evaluations difficult. Reliable cross-national and longitudinal data are often missing. One way to measure efficiency gains is to look at the major macroeconomic performance of a country. However, it is not easy to establish whether improvements in performance are the result of NPM, since there are many other factors that play a role. Nevertheless, few studies have demonstrated a favorable macroeconomic effect for NPM (Pollitt and Bouckaert 2011).

Thus, there is little hard evidence about the effect on NPM's main goal of cost cuts and improved efficiency (Pollitt and Dan 2011) and the studies done on cost-cutting effects of NPM do not find convincing positive results (Hood and Dixon 2013). NPM approaches to boost organizational productivity has not been a success (Dunleavy and Carrera 2013). A meta-analysis of 18 studies of public sector efficiency reveals that there might be some improvement in technical efficiency as a result of agencification, performance management, competition, public-private partnerships, or consumerism, but almost nothing is known about effects on allocative and distributive efficiency (Andrews 2011). Privatization and contracting out might have some positive efficiency effects in the short run in some policy areas, but there might also be negative side-effects on equity (Hodge 2000; Boyne et al. 2003). Studies of the effects of agencification are also inconclusive (James and van Thiel 2011; Læg Reid and Verhoest 2010). The same can be said about the effects on citizens' satisfaction and trust, which is a very complex issue (van de Walle 2011). There also seem to be quite a few perverse gaming effects (Hood 2006). One claim of the NPM reform was that it would strengthen political control, but

the bulk of comparative studies of the effects of NPM reforms seem to stress that the control of the political executive has decreased (Christensen and Lægreid 2001, 2007a; Pollitt and Bouckaert 2011). The demand for independent, apolitical bodies did not work as expected and calls for democratic accountability have become stronger.

Pollitt and Bouckaert (2011) underline that there are multiple difficulties in assessing the results of administrative reforms in general, with government effectiveness especially difficult to evaluate. Some of the most significant effects may actually have been in the way we talk about public sector organizations. The reforms have produced a new discourse and reform climate, changing attitudes, activities, and procedures more than outcomes. The general conclusion reached by many studies is that major reforms are often launched with little or no attention to evaluation and the paradox is that "the international management reform movement has not needed results to fuel its onward march" (Pollitt and Bouckaert 2011: 58).

NPM, Accountability, and Performance: A Contested Relationship

The quest for stronger accountability was a driver of many NPM reforms. A key premise was that with effective vertical managerial accountability better performance would follow (Boston and Gill 2011). Various NPM initiatives were based on the assumption that enhanced accountability would improve performance. But the empirical evidence that this has happened is inconclusive (Lægreid and Verhoest 2010), and evidence of whether performance measurement leads to better accountability is scarce (van Dooren et al. 2010). The relationship between accountability and performance is characterized by tensions, ambiguities, and contradictions, and more responsibility for performance does not lead to more accountability for performance (Bouckaert and Halligan 2008). Pollitt (2011) examines critically the contested proposition that performance management systems will improve agency accountability to citizens and political representatives and concludes that the measurement of performance has not enhanced political accountability. The relationship between accountability and performance is contested and it is becoming increasingly clear that we have to operate with a multi-dimensional accountability concept going beyond hierarchical accountability (Lægreid 2014). This is especially the case in unsettled situations that often characterize NPM reform periods (Olsen 2014).

The NPM reforms have complicated the already broad notion of accountability in the public sector (Mulgan 2003). Rather than reducing or increasing accountability, NPM implies a transfer from one set of accountability relationships to another (Olsen 2010). NPM reforms tend to concentrate more on individual accountability and less on collective accountability and have tended to change the accountability focus from processes and compliance with rules and input to output and results. NPM supplements the vertical mandatory accountability relationship with more voluntary horizontal

accountability arrangements such as social accountability to customers and users of public services.

NPM reforms imply a lot of tensions, dilemmas, and ambiguities related to accountability issues. The role of political accountability has been reduced and the exposure of the manager has been increased. NPM reforms have generated a renewed tension between flexibility and political accountability. How to guarantee political accountability when politically sensitive questions are left to experts in autonomous agencies is a matter of concern. Generally, managerial accountability works best in the least political, or politically salient, areas of public service. Political accountability and traditional *Rechtstaat* accountability relationships are still important but have now been supplemented by newer NPM accountability relations. Rather than asking whether government officials are more accountable or less after NPM reforms, one should focus on what kind of accountability the participants perceive as appropriate (Romzek 2000). Emphasizing outcomes and outputs at the expense of input and processes does not necessarily mean more or less accountability. Rather it means that different accountability relationships should be addressed. Accountability in a multi-functional public sector means to be responsible for the achievement of multiple and often ambiguous objectives, as Hood reveals when distinguishing between *Eta*, *Lambda*, and *Theta* values. NPM argues for more business efficiency and accountability for performance without paying much attention to political accountability and accountability for fairness, resilience, and robustness (Behn 2001).

The emergence of NPM reforms thus seems to have made accountability a more ambiguous and complex issue. By highlighting the importance of people as consumers NPM has introduced the dual accountability of civil servants to politicians and users. It represents a shift in focus concerning accountability, from a broadly defined public interest to a more narrowly defined set of personal interests. A political theory is needed that can explain how applying customer service techniques and tools to government and giving civil servants more authority to make policy decisions about the results they produce and how they produce them is consistent with democratic accountability (Behn 2001).

BEYOND NPM: WHOLE OF GOVERNMENT AND “WICKED” ISSUES

NPM has first of all been preoccupied with the issue of vertical coordination and control. It has led to fragmentation of public sector by assuming that each and every organization has its own objectives and targets that can be assessed by organization-specific performance indicators. This works well as long as the tasks follow the boundaries of public sector organizations, but fails for cross-border tasks that transcend organizations, policy areas, and administrative levels. Tasks like internal security, climate

change, poverty, unemployment, and immigration can only be handled by organizations working together. NPM has little to offer when it comes to horizontal interorganizational coordination. This is why there has been a growing need for a more integrated administrative policy in recent years.

Reforms that have emerged more recently have been variously labeled as post-NPM, whole-of-government, joined-up government, new public governance, etc. A central question concerns what happens when the different reform waves meet each other. Will NPM prevail, be modified and pushed back, or combined with newer reform measures? Currently a main question is whether NPM has peaked, thus requiring us to look beyond NPM (Christensen and Lægread 2007b).

One view is that NPM is still alive and kicking (Pollitt 2003) and will continue to be a major force in the near future, especially in a period of strong pressure for cost-savings; another is that NPM is dead (Dunleavy et al. 2005). A third view is that new reforms complement or supplement NPM rather than replacing it (Christensen and Lægread 2011). The reasons why post-NPM reforms have emerged seems to be, first, a reaction to a loss of political control, second, NPM's failure to deliver on economic measures, and, third, the "fear factor"—i.e. terrorism, pandemics, tsunamis, climate threats, and financial recession, all of which have created a greater need for control. There have been strong demands for more integration and central cross-boundary coordinating capacity, which is reflected in various features of the new post-NPM reforms.

A main finding in the research in this area is that administrative reforms have not taken place along a single dimension. In practice we face mixed models and increased complexity. It is fair to say that NPM is still very much alive in many countries, and NPM reforms have normally not been replaced by new reforms but rather revised or supplemented. It is too early to conclude that the old public administration model is unsustainable. It has considerable capacity to adapt and is both robust and flexible, even after a long period of NPM reforms and emerging post-NPM reforms.

The post-NPM generation of reforms of the last decade has used formal structural instruments to modify vertical and horizontal specialization. In contrast to the NPM reforms espousing organizational disaggregation, the recent reforms have been characterized by aggregation and joined-up government (Halligan 2011). Vertical control and levers of control are increasingly being applied, while a "whole-of-government" approach uses new coordination instruments and cross-sector programs and projects to modify horizontal fragmentation.

Whole-of-government initiatives are important for handling the "wicked issues" that transcend the boundaries between organizational policy areas or administrative levels (Christensen and Lægread 2007b). NPM reforms primarily addressed the principal-agent issues of how superior bodies can control subordinate organizations within the same ministerial area, but have little to offer when it comes to the more pressing question of how to handle problems and tasks that straddle organizational boundaries. The challenge is to find organizational arrangements that can enhance cross-border collaboration and horizontal coordination.

Rather than purifying a single model we need a repertoire of models for political-administrative institutions to face the future challenges of public management, administration, and governance. Designing a holistic and integrated public administration is not easy and may not be a good idea either, as research shows that public administration systems actually are a “mixed” order of partly overlapping, partly contradictory complementary and competing organizational forms and are hence necessarily compound in nature (Olsen 2010). The public administration is multi-functional and multi-tasked and has to balance different administrative values. Complex political-administrative problems require complex solutions. The main challenge is how these different elements can be combined and balanced in such a way as to supplement or complement one another. Today we encounter no dominant model. Instead, several key concepts such as NPM, New Public Governance, and the neo-Weberian state are on the agenda (Pollitt and Bouckaert 2011). Summing up, we would tend to subscribe to the argument that reform movements are characterized by combining, complexity, layering and hybridization, rather than by a linear process towards more NPM or a pendulum swing (Christensen and Lægheid 2011). Public administration faces increasingly complex conditions, reflected in multi-functional organizational forms, and the administrative reforms in the public sector can be understood as compound reforms that combine different organizational principles based on multiple factors working together in a complex mix. Compound administrative reforms are multi-dimensional and represent “mixed” orders and combinations of competing, inconsistent, and contradictory organizational principles and structures that coexist and balance interests and values (Olsen 2010). It is not a question of NPM or post-NPM but of how the mixtures of these forms change and how the trade-off between central capacity-building and autonomization is altered.

CONCLUSION

One important lesson from the comparative administrative reform movement is the difficulty of drawing general policy recommendations that are valid across countries, policy areas, and over time. While most of the discussion has revolved around differences between countries, differences between policy areas might also be significant. Owing to the contextual variations, variations in tasks, and different historical-institutional legacies that different countries face, the holistic and generic approach has many limitations. One policy recommendation is that reformers should be preoccupied not only with the steering capacity and capability of public sector organizations but also with steering representativeness, legitimacy, and trust relations. Gradual reorganization and reform of a more limited scope will more easily allow the broader participation of different stakeholders and potentially increase the legitimacy of the reforms. The main challenge is to find organizational forms that enhance both the representativeness and the capacity of governance. Often there is a trade-off

between the two. Reforms intended to enhance one aspect tend to harm the other. The big question is whether it is possible to design administrative reforms in a way that strengthens both representativeness and capacity.

Twenty years after Hood's 1991 article, NPM research is still focusing more on identifying rather than answering the key conceptual questions raised by NPM. Nevertheless, we have seen a movement from descriptive mapping and normative and analytical critique toward analyses of the paradoxes of NPM (Hood and Peters 2004). We still need more tests of the limits of NPM in terms of different administrative values. What we probably need more than anything else are reliable empirical data on the effects and implications of NPM across countries, policy areas, administrative levels, and over time, both regarding the effects on the main goals of efficiency and economy and the effects on democratic legitimacy, fairness, accountability, and resilience which are the most important ones in democratic governance.

NOTE

1. Actually Hood first used the term New Public Management in 1989 (Hood 1989).

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CHAPTER 38

IAN AYRES AND JOHN BRAITHWAITE, *RESPONSIVE REGULATION: TRANSCENDING THE DEREGULATION DEBATE*

MARTIN LODGE

THE CLASSIC

ENFORCEMENT-RELATED questions are at the heart of political science and public policy. Enforcement deals with questions regarding the exercise of state power, the relationship between state and corporate power, the way in which institutions looking after vulnerable people are controlled, and how individual and corporate actors respond to threats of sanctions. Furthermore, in an age of privatized public services and concern with social and environmental policies, the oversight function of the state has become increasingly critical: implementation failure has largely become a failure of oversight over private (privatized) actors.

Within the regulation field (and beyond), Ian Ayres and John Braithwaite's book *Responsive Regulation* represents a classic in at least three ways (Ayres and Braithwaite 1992). First and foremost, the book is associated with a normative model that provides for a dynamic and gradual sanctioning regime (the "enforcement pyramid"). This regime is meant to encourage mutually beneficial cooperative relationships between regulators and regulatees. *Responsive Regulation* also favors the inclusion of third-party actors. These central ideas have been adopted in various social science disciplines and in a range of policy fields, such as regulatory and tax enforcement, and criminal justice systems (V. Braithwaite 2007).

Second, *Responsive Regulation* is a classic in that it represents the convergence of ideas in game theory-inspired law and economics and socio-legal studies where these

fields arrived at similar conclusions about the (in)effectiveness of different enforcement strategies. In particular, ideas regarding the “enforcement pyramid” offered an alternative to the diagnosed shortcomings of rigid and hierarchical punishment systems. It further accommodated the empirical fact that enforcement was largely about negotiation and mediation. Enforcement-related studies also continued the central concern of implementation research by being interested in the disconnect between organizational practices and regulatory requirements and, more broadly, the limits of top-down, forward-looking, and hierarchical interventions.

Third, *Responsive Regulation* is a classic in that it represents a focusing event for the subsequent literature at the interdisciplinary interface between two social science areas. It therefore did not represent a “breakthrough” classic in the sense of developing ideas that previously had not been formulated. Instead, it is a classic in the sense of bringing together evolving and converging ideas from different disciplinary homes in one source. If therefore there is a debate to be had whether classics emerge at the heart of a discipline or at the interdisciplinary periphery, then *Responsive Regulation* supports the latter argument.

The central place of *Responsive Regulation* among the classics in public policy and administration is furthermore justified by its prominence in textbooks, handbooks, and international journals in the field of regulation (Gunningham 2010; Baldwin et al. 2012; Lodge and Wegrich 2012; Morgan and Yeung 2007; Parker 2013).

The following sets out the background to and central argument of *Responsive Regulation* and then considers the book’s overall contribution. In particular, the evolving debates around enforcement foreshadowed the “discovery” of behavioral aspects of policy under the mantra of “nudge” (Thaler and Sunstein 2008). For example, the literature on enforcement and different motivations argued on the same lines about what was later popularized as a distinction between “knights” and “knaves” in the public policy literature on the welfare state (LeGrand 2006). More broadly, *Responsive Regulation* has been a classic in areas interested in the power of street-level bureaucrats and actual enforcement “at the coalface,” whereas wider (especially non-English-speaking) fields in political science have shown less interest. Thus, *Responsive Regulation* represents a classic whose language has not entered the discourse of all areas of public policy research.

INTELLECTUAL BACKGROUND AND CENTRAL ARGUMENT

Enforcement is about “making law happen.” This “making law happen” relates to the prosecution of those found to be in breach of legal rules. More importantly, it is about understanding and explaining a whole array of ways in which regulators seek to modify behaviors below the formal prosecution level. Thus, enforcement-oriented studies are

interested in the way in which state power is exercised, how resources are utilized (or not), and how interactions between regulators and regulatees evolve (Hutter 1989: 153).

Responsive Regulation was published in a context in which a variety of debates centered on regulation. On the one hand, especially in Western Europe, regulation had emerged as a central policy tool as part of wider public sector reforms (Majone 1997). Wider discussions focused on regulatory reform as part of “deregulation,” especially regarding the role of administrative bodies that were tasked with enforcing social and environmental regulation. On the other hand, and more immediately related to *Responsive Regulation*, regulatory activity had become increasingly contested. This affected those areas of regulation that had witnessed growth in the late 1960s and 1970s, namely social and environmental regulation. These regimes had emerged in opposition to concentrated business interests (and in favor of dispersed consumer interests) and had been designed in formal and adversarial ways to resist “capture.” Continued business opposition, and the diagnosed “burden” and ineffectiveness of “command and control” regulation, gave rise to calls that advocated greater flexibility (see Bardach and Kagan 1982).

Debates about regulatory enforcement were also evolving in the law and economics and socio-legal literatures. These two literatures offered accounts as to why enforcement could hardly be “perfect,” but for different reasons. Both literatures moved towards an emphasis on mixed strategies that replaced an earlier emphasis that focused on the dichotomy between deterrence and persuasion (see Reiss 1984; Black 1976). Traditionally, the deterrence view sees all regulation as a battle between rational actors, calculating the costs and benefits of regulatory activities. Regulatory enforcement, therefore, becomes an issue of the costs and benefits of complying with rules by enhancing the probability of detection and sanctioning and the penalty itself. Enforcement, according to this literature, represents a strategic choice in terms of resource allocation (see Becker 1968; Stigler 1970; Becker and Stigler 1974), involving also private enforcement regimes (Becker and Stigler 1974; Ehrlich 1973).

In contrast, traditional persuasion-based accounts start from the empirical observation that deterrence is highly problematic, prosecution the exception rather than the norm, and that inspectors operate by appearing to carry “tough” penalties in their armory. Explaining a reluctance to prosecute is not about capture by business or political strategies that emasculate regulatory enforcement agencies by starving them of resources, or about dominant elite consensus. Instead, enforcement is shaped by the inherent tensions that applied to the regulators’ individual and organizational task. These tensions emerge due to inspection taking place in the cross-fire between competing constituencies and having to cope with different types of visible and non-visible harm. Moreover, prosecution represents a public exposure and admission by regulatory staff that they have failed to prevent wrongdoing in the first place (Hawkins 1984: 7–15). Motivations across regulatees differ, and personal relationships between enforcers and regulatees matter for explaining regulatory “style” (as illustrated in the concept of “relational distance,” Black 1976). Thus, by drawing and building on the literature on implementation and street-level bureaucracy (Lipsky 1980; Wilson 1968),

regulatory enforcement, according to this literature, is about negotiation, appearance, and mediation. As a result, enforcement, and the achievement of regulatory objectives, is not about deterring rational actors, but about persuading “ignorant” and resource-poor regulatees.

Responsive Regulation's success can partly be attributed to its integration of both law and economics and socio-legal studies. It did so by acknowledging the limitations of the traditional dichotomy between deterrence- and persuasion-based accounts, as just outlined. From the law and economics side, it showed that rational actors were likely to cooperate under certain conditions, even when assuming opportunistic rule-evading. This argument was particularly emphasized in the seminal work by John Scholz and collaborators. John Scholz and Feng Heng Wei noted how regulatory enforcement varied across state and task environments (Scholz and Wei 1986), thereby linking the regulatory enforcement literature to wider accounts that stressed how bureaucracies were responsive to their environment (Wood and Waterman 1991; Atlas 2007). Furthermore, and critical for the argument in *Responsive Regulation*, Scholz (1984, 1991) showed how tit-for-tat game-theoretical insights were useful in illustrating how self-interested actors would comply with enforcement requirements as cooperative strategies were advantageous to both regulators and the regulated. This cooperative approach would concentrate on serious violations and repeat offenders (Scholz 1991: 132). Actors would cooperate as long as there was a credible threat of punishment in case of non-cooperation. This established the case for a dynamic sanctioning regime as long as iterative games between regulators and regulatees could be played.

From the other side, socio-legal research was primarily motivated by emphasizing the importance of administrative discretion and the implementation factor in order to move beyond the description of formal law (Richardson et al. 1982: p. vii). A second key motivation was to extend criminological insights into policing. This socio-legal tradition drew on the literature on bureaucracy that pointed to bounded rationality, power relationships within organizations, and interaction between prescribed and discretionary behavior by enforcement officials that was contingent on the particular setting (following the work by Gouldner, Blau, Lipsky, and Wilson; see Page, Chapter 4, this volume). In general, the socio-legal literature emphasized three themes that were central to *Responsive Regulation*. These were the role of inspectors and their enforcement strategies, the importance of discretionary decision-making, and the role of regulatee motivations. The following paragraphs consider these three themes in turn.

First, regarding inspectors and their enforcement strategies, the criminology literature on the role of the police officer interpreted this role as that of a gatekeeper who decided on whether individuals “deserved” access to institutional treatment. Studies of inspectorates in other fields pointed to similar patterns (despite the considerable differences across inspector types, for example in terms of visibility, uniform, or “target population”). These studies concluded that enforcement was constituted by varied sanctioning strategies: in some cases, inspectors immediately “deterred,” in other cases they relied on informal warnings. Thus, the empirical reality required a model of enforcement that saw deterrence and persuasion as a continuum, not as a dichotomy

(Hutter 1986, 1988, 1997; Richardson et al. 1982: 23–6; Carson 1970; Cranston 1979). Similarly, Braithwaite and colleagues pointed to diverse enforcement strategies across regulatory agencies in Australia (Braithwaite et al. 1987).

One of the key explanations for these varied enforcement strategies was reputation and appearance, given the ambivalence of any enforcement action (Hawkins 1984). Reputation-related accounts were inspired by Goffmann (1959) and by Selznick (regarding the TVA) that stressed the importance of the interaction between organizational actions and their reputational implications given their varied audiences (Selznick 1966: 10). In enforcement, business chose to accept and comply with enforcement standards to suppress competition and to enhance their own reputation and efficiency.

This analysis of widespread mediation and only reluctant prosecution was condemned by some scholars as the acceptance by empiricists of captured regulation (whereas deterrence via extensive persecution would accordingly reflect a “standing-up to business,” see Pearce and Tombs 1990). In response, Hawkins and others pointed to the inherent resource limitations and reputational bases that shaped the work of enforcement agencies. It was also argued that the analysis of “mediated” enforcement patterns did not represent an endorsement of such patterns. In sum, enforcement action therefore was interpreted as an act that was shaped by moral and political background conditions, perceptions of motivations of regulatees, and by individual assessments of the severity of observed rule violations (Hawkins 1984: 191–207; Richardson et al. 1982: 195).

The second key theme was discretion. While some public administration literature appeared attached to the idea of “clearly enforceable” rules that left no scope for discretion, other accounts pointed to the inherent normative importance and prevalence of discretion in administrative and regulatory behavior (Lowi 1979; Kagan 1978; Mashaw 1983). Bardach and Kagan (1982) argued that non-discretionary behavior was often likely to destroy the inherent goals of regulatory regimes. Similar conclusions emerged from John Braithwaite’s work on nursing homes which contrasted formalistic “box-ticking” regimes in the United States with more mediated and “principle-based” regimes in Australia (for work on coal mines, see Braithwaite 1985, 2002a). Discretionary regulation was seen to encourage regulatees to reflect on their operations, whereas formalistic enforcement practices encouraged unreflective (or creative) compliance with stated provisions.

The third theme was a focus on regulatee motivation. Enforcement-interested accounts pointed to “creative compliance” as one way in which corporations responded to regulatory requirements (McBarnet and Whelan 1991). This interest in different “gaming” and “cheating” strategies suggested that an emphasis on deterrence was unlikely to achieve intended compliance behaviors (for subsequent discussion, see Bevan and Hood 2006). Furthermore, firms’ compliance patterns also highlighted the importance of mixed motivations. Regulated entities were found not to be calculating the cost and benefits when deciding on whether to comply or not, rather they were seen to be reactive to actual incidents. Thus, instead of viewing all regulatees

as opportunists and amoral calculators (in the law and economics tradition), or as “resource-poor incompetent” (as the persuasion-based literature initially assumed), a mixed set of motivations and capacities was observable, ranging from the opportunistic, the incompetent, to principled dissenting (on the basis of normatively or practically objecting to the existence of a rule). Indeed, later work highlighted the importance of viewing firms as internally fragmented and divided (Gunningham and Sinclair 2009). In short, seeing organizations as rational corporate actors whose compliance could be understood as well-informed cost-benefit calculators was challenged, without, however, necessarily dismissing the importance of deterrence as one method to change organizational conduct in some cases.

These three themes from the socio-legal literature as well as the emerging findings from game theory on the advantages of cooperative strategies formed the background that influenced *Responsive Regulation*. Gradual and interactive sanctioning regimes (based on tit-for-tat) were seen to be superior to either pure deterrence- or persuasion-based strategies, regulatees’ actions could not universally be said to be opportunistic, while regulatory discretion and negotiation were at the heart of enforcement. Based on these themes, *Responsive Regulation* explored three central ideas that reflected a bringing together of these dispersed insights: the “benign big gun” and the “enforcement pyramid,” the idea of “enforced self-regulation,” and the role of “public interest groups” (PIGs).

The ideas of the “benign big gun” and the “enforcement pyramid” are the most well-known components of *Responsive Regulation* (set out in chapter 2). It displays both the influence of the law and economics and the socio-legal traditions in that it favors a gradual sanctioning regime that offers cooperation first and punishment only after continued non-compliance has been diagnosed (“walk softly while carrying a very big stick”: Braithwaite et al. 1987: 336). The foundation of this argument was Scholz’s work on iterative games based on tit-for-tat (also Axelrod 1984), and Braithwaite’s work on regulatory enforcement in various domains. The presence of a “big gun” was essential as it signalled sufficient deterrence to give firms incentives to cooperate. This also reflected the view that the threat of deterrence did, at times, have intended effects on corporate approaches to safety. Firms were seen as cooperative if there was a credible sanction; in the absence of a credible “big gun” firms would have little to fear from being non-cooperative. The idea of the “pyramid” proposed, first, a dynamic sanctioning regime that started with advice and warnings and only gradually moved towards criminal sanctions. Second, it suggested that most diagnosed non-compliance could be rectified at the bottom (via persuasion and warnings) and only a few cases would make it to the very top (thereby reducing the amount of formal processes to revoke licenses, presenting a substantial reduction in resource-deployment for regulatory enforcement agencies).

The notion of “enforced self-regulation” was a logical consequence of a strategy that was based on the “benign big gun.” Ayres and Braithwaite (as set out in chapter 4 of *Responsive Regulation*, also Braithwaite 1982) argued that regulatees should be seen and encouraged to act as responsible actors. Firms were to write their own self-policing

rules and have these agreed, verified, and checked by regulatory agencies. This not only reduced information asymmetry problems in that firms were better informed about their processes than regulators. In addition, it encouraged cooperation rather than adversarial relationships as firms were offered discretion rather than (possibly ill-informed) prescription to amend their ways. Enforced self-regulation treated regulatees as “responsible,” thereby encouraging them to pay attention to operational detail rather than just seeking to blindly apply prescribed provisions.

Moreover, *Responsive Regulation* also drew on the literature on tripartism (and neo-corporatism) by arguing that the role of non-state actors should be enhanced (as set out in chapter 3). Ayres and Braithwaite argued that such public interest groups (PIGs) could be utilized as information-gatherers. They argued in favor of regular tournaments to allocate the temporary right to be a PIG. While their ideas regarding “auctioning” the right to represent the public interest have not been taken further, the idea of using non-state actors in the enforcement of regulatory regimes foreshadowed developments that have emphasized the limits of focusing on state-based regulatory regimes (and international state-based treaties) and pointed to the importance and potential effectiveness of non-state-based regimes (such as the Forest Stewardship Council). It also linked to the wider regulation literature that stressed the deconcentrated nature of regulatory authority (associated with the ideas of “regulatory space” (Hancher and Moran 1989) and of “decentered regulation” (Black 2002)).

In sum, *Responsive Regulation* is a model of regulatory enforcement. It is a prescriptive model that points to the advantages of particular institutional arrangements and strategies. It is an empirical model that formalizes both deterring and persuasive enforcement styles and that explains limited prosecution action. It is a predictive model that claims that its prescriptions will lead to particular enforcement outcomes. Furthermore, it is also a normative model. The normative ideal of “civic republican” regulation (with its underlying principle of “freedom from domination”) was openly acknowledged: Ayres and Braithwaite advocate a regulatory ideal where government seeks to balance interests, where non-organized interests are empowered, where competing ideas about regulation are reconciled, and where participation in public life is encouraged. Regulatees are to consider their obligations and accept their responsibility for regulating themselves in a way that conforms with broad legal principles (Ayres and Braithwaite 1992: 18; also Braithwaite 2007, 2013).

Despite *Responsive Regulation*'s own ideational underpinnings, select elements appeal to different research and practitioner audiences that cannot be seen as usual bedfellows of “civil republicanism” (Tombs and Whyte 2013; Mascini 2013). The ability of others (especially practitioners) to utilize the idea of an enforcement pyramid without having to buy into the normative agenda of civic republicanism has arguably been part of the book's success (even if this was unintentional). Thus, the concentration on the “benign big gun” and the “pyramid” points to the possibility of strategic regulatory enforcement activities. Similarly, *Responsive Regulation* could be seen as mostly about professional judgment and the inclusion of third parties (the PIGs). A further contrasting view would see the key advantages in granting flexibility to business under

enforced self-regulation. Finally, *Responsive Regulation*'s key appeal could be seen as its emphasis on openness and the importance of "shaking up" established power relationships. In other words, *Responsive Regulation* is also a classic in that (or exactly because) it successfully manages to appeal to different audiences for different reasons.

WHERE ARE WE NOW?

The ideas of *Responsive Regulation* have moved to further fields, such as tax administration and civil wars (see Braithwaite 2013). "Enforced self-regulation" has been an important empirical trend, whether in the development of hazard-based regulatory approaches (such as in food safety) or in the oversight of (English) local government or teaching quality oversight for UK universities (see Hood et al. 2001; Haines 1997; Braithwaite 2003; V. Braithwaite 2007). Despite the popularity, a number of criticisms have emerged.

Most criticisms and subsequent developments have focused on the assumed existence of a clearly identifiable regulator capable of enacting the "benign big gun," and the well-established relationship between a regulated target and the regulator. As a result, one criticism has been that *Responsive Regulation* is too state-centric and therefore fails to acknowledge the importance of non-state actors, the dispersed nature of contemporary regulation (and thus the difficulty of regulators to mobilize when regulatory authority is dispersed), and, especially, the transnational nature of contemporary regulation (Baldwin and Black 2008; Bartley 2007; Abbott and Snidal 2013). In such a fragmented system, any notion of an enforcement pyramid is arguably unrealistic and therefore problematic. At minimum, it requires a refocusing towards the idea of regulatory capacity being situated outside the state (Abbott and Snidal 2013).

A second criticism has been based on the inherently inconsistent approach across companies that *Responsive Regulation* advocates. After all, one implication of the enforcement pyramid is that the same infringement by different regulatees earns different types of sanctions, as these depend on the diagnosed degree of cooperation by the regulatee. Such an endorsement of explicitly inconsistent law application could be seen as problematic by some (especially some lawyers), others would argue that what matters is the consistency of the republican principle ("freedom from domination") being applied and not legal formalism (Braithwaite 2013; Westerman 2013). Further critical attention has been paid to the need for iterative interactions between regulators and regulated. Such iterative games are less likely to occur where regulators lack the resources to maintain an institutional memory and where the mobility of regulatees turns any enforcement action into a one-shot game (Gunningham 2010). Furthermore, it is questionable whether all regulatory enforcement should always start at the bottom and then be gradually escalated or whether there are violations that require immediate escalated sanctioning. This possibility of a "targeted" (although not automatized)

approach that involves both reward and punishment was never denied in *Responsive Regulation* and later work (Braithwaite 2002b, 2008).

A third criticism has focused on administrative prerequisites. *Responsive Regulation* and later elaborations take on the tone of a technocratic “fix-it” that pays little attention to the administrative processes required. The raising and lowering of sanctions according to the “benign big gun” is little discussed and is arguably highly problematic (see Haines 1997). First of all, the organizational constraints that are likely to inhibit moving up and down the pyramid are not explored. Little work has been done to connect these demands on regulators to organization-theoretical insights that would point to selective attention or internal resistance to escalation. Similarly, little attention has been paid to the detection or information-gathering side; after all, the conditions under which regulators operate are not just to identify wrongful conduct, but also to understand the underlying motivation and the capacity of regulatees to correct their conduct. Furthermore, once escalation has taken place, it is likely that cooperative relationships will require some time to “heal,” and it is questionable whether “cooperative relationships” will remain on course. Moreover, the politics of escalation and de-escalation, the organizational processes that facilitate (or not) information-gathering and behavior-modification, as well as the political and moral context to operate the pyramid continue to be underspecified.

There has been a growing awareness that “enforced self-regulation” places considerable demands on regulated entities and regulators alike (Gilad 2010). In particular, responsive strategies require a sustained supportive environment, or otherwise decisions will remain contested and distrusted (Parker 2006). More generally, little attention has been paid to the unintended consequences that a regime based on principles outlined in *Responsive Regulation* might trigger, one of which is both parties’ interest in avoiding discretionary judgment and moving towards protocolization and prescription. One pressure potentially challenging the enforcement pyramid is that regulatees demand to be told what should be done. Another pressure encouraging prescriptive regulatory approaches comes from the side of inspectors. For inspectors, discretion leads to more visibility and thus more scope for potential blame. This fear of blame leads to risk-averse demands for reduced discretionary judgment.

Finally, *Responsive Regulation* cannot explain cross-sectoral or cross-national variation. Admittedly, the original purpose was not to explain such variation, but to develop potentially testable claims about the observable impact of responsive enforcement strategies in terms of enhancing cooperative attitudes, greater regulatee trust in regulators, and, above all, advanced compliance (for such a test, see Nielsen and Parker 2009). Different enforcement styles are explained by the type of business, the broader political, moral, and economic setting (giving rise to “adversarial legalism” in the US, according to Kagan 2003), the legal design, the task environment, the costs of complying, and the resources and legitimacy of the regulatory entity (see McAllister 2008, 2010).

These four criticisms and immediate responses have shaped later developments. Among the various intellectual avenues that directly build on and respond to

Responsive Regulation, four strains can be distinguished that further develop the field of studies in regulatory enforcement.

One strain is “smart regulation” (Gunningham and Grabosky 1998). Building on Ayres and Braithwaite, this strain stresses the importance of dynamic and iterative enforcement relationships. However, “smart regulation” also emphasizes the role of third parties as information-gatherers and behavior-modifiers, thus moving the focus beyond the regulator–regulatee relationship towards the range of different regulatory instruments and the importance of non-state actors (Grabosky 1995, 2013). Examples of such regimes include the Forest Stewardship Council (as an example of a private certification regime), and the self-regulatory Institute of Nuclear Power Operators which hands over reluctant laggards to state regulators (Gunningham 2010: 132–3; Rees 1994).

A second strain is the work on “open corporations” and “meta-regulation.” This approach is a direct elaboration of “enforced self-regulation” and is directly associated with Braithwaite’s work. It emphasizes the importance of decentralized decision-making within the context of broad principles that require consent and verification from state regulators (Parker 2002). Similarly, Coglianese and Lazar (2003) and Gilad (2010) note the rise of “management-based” regulation, namely the requirement for private corporations to develop their own safety case (i.e. the identification of particularly risky or hazardous aspects and of strategies to address these areas) and to be able to justify its robustness vis-à-vis state regulators. Meta-regulation is therefore not about the abandonment of state-based regulation, but rather about the reduced prescriptiveness of so-called command and control regulation and a reshaping of state power toward a more advisory and arguably strategic role. At the same time, meta-regulation entrusts regulatees with being capable and motivated to develop credible self-regulatory systems and requires regulators to be able to detect the workings of such decentralized regimes. Both of these requirements arguably are highly demanding and, as yet, have received only limited empirical research (Nielsen and Parker 2009; Gunningham et al. 2003).

A third strain has explored the rise of risk-based enforcement techniques (Black 2005). Arguably, this regulatory policy trend has mostly emerged in the world of practice. Regulators are called upon to calculate the likelihood of a particular harm occurring and the severity of that harm. Consequently, regulators are required to focus resources on calculated “systemic” or high-risk areas. This approach is supposedly about allocating resources and signaling that not all risks can be followed up. It admits that failure cannot be avoided. However, while it is fair to suggest that even under responsive regulation a risk-based approach would shape decisions in terms of allocation of resources, more generally, a risk-based approach offers a direct challenge: it takes away the dynamic nature of responsive regulation and removes the possibilities of regulators to explore underlying conditions in “low-risk” areas. Apart from all else, a risk-based approach places great trust in regulators’ ability to identify risks (and not fail to detect risks as they emerge; for study on selective identification of risks in nuclear installations, see Manning 1989) and is therefore similarly, if not even more, open to the criticism that it is based on a limited consideration of the administrative prerequisites

of such a strategy. Furthermore, the financial meltdown of the late 2000s that occurred on the watch of supposedly risk-based enforcement regimes offers little confidence that risk-based approaches offer advantages over responsive regulation-based ones.

A fourth strain continues the interest in comparing and explaining enforcement styles (Nielsen and Parker 2009). This work is partly influenced by implementation-based approaches, as noted (Kagan 1978; Bardach and Kagan 1982; Gormley 1998; May and Burby 1998; May and Wood 2003). More broadly, enforcement studies contrast the “legalistic adversarial” pattern in the United States with more consensual styles across European countries (Kelman 1981; Kagan 1989). More recently, comparative studies have moved into industrializing economies, such as China and Brazil (McAllister et al. 2010). Elsewhere, ideas about the enforcement pyramid and enforced self-regulation shaped the work by Hood and colleagues on the cross-domain and cross-national analysis of regulation *inside* government (Hood et al. 1999, 2004). In this work, the key theoretical mileage was derived from the application of cultural theory to the (comparative) study of regulatory regimes. This approach offered further insights into the prerequisites and conditions under which particular variations of enforcement styles (and “big guns”) emerge and change, especially in an age of supposed managerialism in public services.

In short, *Responsive Regulation* has received considerable critical attention that has triggered responses, extensions, and incorporations into other frameworks. Some of the criticisms point to the limitations of a state-centric approach, others to the implications regarding prerequisites and outcomes, while others are interested in different research questions. None of these criticisms and responses have, on their own or together, amounted to a killer blow to the key ideas incorporated in *Responsive Regulation*; in contrast, they largely constitute part of an ongoing conversation about key themes.

ONGOING EXPLORATIONS

Apart from the ongoing work that responds directly to the agendas that emerged from *Responsive Regulation*, the literature on enforcement is evolving in a variety of ways (see Mascini 2013). These lines of enquiry are following particular socio-legal inspired themes, whereas the law and economics inheritance of *Responsive Regulation* has arguably not been taken much further (Ayres 2013). However, “behavior economics” (with its focus on bounded rationality and motivation) is centrally concerned with key themes explored in the traditional enforcement literature. Three areas can be identified in particular. One area draws directly on *Responsive Regulation* (“deepened exploration”), another on similar interests to *Responsive Regulation*, but without directly drawing on its intellectual legacy (“rediscovery”), and a final area does consider compliance-related questions, but without paying attention to *Responsive Regulation*, or related concerns (“terra incognita”).

In terms of “deepened exploration,” and going beyond the strains already noted, there are three evolving themes that build directly on *Responsive Regulation*. These themes relate to the role of the enforcement officer (or inspector) (Pautz and Wamsley 2012). Across domains, this role has witnessed considerable change. A number of causes for this change can be noted: different control regimes and logics (due to the rise of “managerialist” doctrines in public management), growing heterogenization of the regulatee population (due to increased internationalization of business activities following market liberalization), and a growing internationalization of different oversight regimes (Lodge and Wegrich 2011; Hutter 2011; Moran 2003). A second, related, theme focuses more narrowly on how relational distance in enforcement has changed and what the effects of this potential change might be. A third theme explores in more detail (and with more theoretical sophistication) the underlying prerequisites and the side-effects of the application of a “benign big gun”-based enforcement pyramid and of enforced self-regulation-type regimes.

As in many areas of social science, it is not surprising that many themes in the enforcement literature are forgotten and then rediscovered. One key rediscovery in the area of enforcement is the emphasis on reputation. As noted, the early socio-legal literature highlighted how inspectors were shaped in their work by reputational considerations, and by concerns about their appearance. The concern with reputation has recently been taken up in work on licensing in pharmaceutical and in other areas where the behavior of agencies (less of individuals) is explored (Carpenter 2010; Maor 2007, 2009; Gilad 2012). A subsequent step considers how reputational concerns influence the co-working within dispersed systems of regulation rather than merely at the level of the individual enforcement officer or agency.

A second rediscovery has been the interest in creative compliance and other gaming activities. This work on the strategic responses to regulatory demands has put different motivations back on the agenda. For example, the “nudge” (behavioral economics) agenda assumes that regulatees (or any targeted population) is willing to cooperate but ignorant. It does not consider those populations whose behavior could be identified as principled objection or as opportunism. Similarly, Bevan and Hood (2006) note how the key challenge in performance management is to ensure that “honest triers” are not turned into “rational gamers” due to the imposed regime. These questions are at the heart of the enforcement pyramid and of enforced self-regulation with its emphasis on ensuring cooperative attitudes and discouraging opportunism. Cass Sunstein represents the direct connection between regulation and this later work on “nudging” and “behavioral economics.” However, it is noticeable how the insights of the enforcement literature have had little traction in the contemporary literature that focuses on incentive regimes (it is e.g. not mentioned in LeGrand 2006).

Finally, what about those areas in which there is a natural linkage between the themes represented in *Responsive Regulation* and other fields, but no direct interaction? Those interested in “compliance” in international relations, global governance, and relations between EU member states have shown little interest in the insights of *Responsive Regulation*. This literature has been largely interested in the way in which

states acted on international agreements and how transnational regimes, often of a private nature, impact on national and local laws. Despite the considerable attention on third-party actors in private regulatory regimes (at the domestic and the international level), questions as to how international regimes are being verified by NGOs and other third parties have not been fully explored. Debates about state compliance with international commitments are largely restricted to studying legal incorporation, transposition, and infringement actions. Such an approach that uses formal law may, in part, be excused by reason of the complexity of studying compliance in a multi-state setting and by a reluctance to engage with street-level bureaucracies. As such, therefore, international relations (and European Union) oriented studies rely on formal policy change, institutional arrangements, and formal infringement proceedings rather than actual enforcement practices (see Knill et al. 2009: 524; Börzel 2001; Börzel et al. 2012; Falkner et al. 2004: 453–5). Some emerging work is displaying an awareness of the themes of the wider enforcement literature, especially the importance of gradual sanctioning systems that emphasize “management” (persuasion) and “enforcement” (deterrence) (this distinction is used by Tallberg 2002; see also Jensen 2007; Versluis and Tarr 2013).

Similarly, the latter-day remnants of “bottom-up implementation studies,” whether in the form of “participatory” or “collaborative” governance have focused largely on co-production and coordination issues rather than on the questions relating to the enacting of policy at the coalface. In doing so, they reflect a tradition of governance studies that is more interested in participation and the normative ideas of “bottom-up” implementation than the governance tradition that emphasizes questions regarding steering (and thus enforcement).

CONCLUSION

Responsive Regulation, as noted, represents a classic whose innovation lay in the bringing together of two disciplines interested in the same phenomena and showed how these different disciplines converged on views about enforcement. It contributes a “republican” conception of regulation that seeks to move beyond (or “transcend” as the subtitle of the book suggested) so-called “deregulation” and “pro-regulation” perspectives. Instead, it developed a perspective that relied on enlightened self-interest of regulatees and smart, non-adversarial regulators who engaged in sophisticated and differentiated regulatory enforcement activities. Its ongoing relevance is evident in the type of questions that encourage research, despite the somewhat limited extent to which responsive regulation has been explored empirically (for an exception, see Nielsen and Parker 2009). More broadly, the growing interest in different motivations and behaviors reflects core insights from the enforcement literature. In the “real world” *Responsive Regulation* has also witnessed continued, if not heightened relevance: regulation has increasingly moved toward ideas of enforced self-regulation (meta-regulation and management-based regulation). More broadly, *Responsive*

Regulation offers a model that reduces regulatory inspection intensity, grants business discretion to regulate itself, and supposedly facilitates cooperative relationships between regulators and regulatees.

The relevance of *Responsive Regulation* is further enhanced by contemporary discussions about regulatory change in the light of the financial crisis. One, so far unresolved, question is whether the financial crisis was a result of enforcement failure or a lack of regulation, and whether responsibility can be laid on the inherent ideas regarding responsive and/or risk-based regulation said to underlie actual regulatory practice prior to 2008. Regardless, the financial crisis has raised questions as to the prerequisites for “modern” enforcement techniques to work, namely the ways in which firms are, if at all, capable *and* motivated to control their own conduct, and how regulatory capacity has to be developed to be able to detect those cases where firms do not display sufficient motivation and capability to comply with regulatory intentions. The financial crisis also raises issues regarding the normative “civic republican” implications of *Responsive Regulation*. As pyramid-selling schemes have collapsed, investment banks have bitten the dust, and retail banks have ended up in the rescuing arms of the state, calls for a deterrence-type approach toward enforcement have increased. *Responsive Regulation* offers a useful check on these calls for more deterrence. The normative idea of “freedom from domination” does not deny the appropriateness of using the “big gun”: in fact, it advocates its presence. However, it questions the usefulness of a regulatory approach that encourages adversarial legalism and creative compliance.

Furthermore, the financial crisis, and the subsequent sovereign debt crisis, led to budget cutbacks and a reduction in the resources available to enforcement agencies. The enforcement pyramid offered a vision in which agencies were avoiding resource-intensive deterrence activities by relying on persuasion in the first instance. However, the importance of the “relational” in *Responsive Regulation* is challenged when regulators have not got the resources to deal with growing demands and reduced capacities to maintain iterative relationships with regulatees.

In other words, *Responsive Regulation* is a classic of continued significance; it has shaped the research agenda in enforcement, and it offers scope for further probing into key aspects of enforcement regimes, namely administrative prerequisites and a greater insight into the way in which firms are capable and motivated to comply. It also addresses bigger questions that shape the world of public policy more broadly, namely how discretionary state power can be constrained, how often vulnerable individuals can be protected from abusive treatment, and how corporate power can be best contained from posing a risk to the viability of political systems.

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CHAPTER 39

FRANK R. BAUMGARTNER AND BRYAN D. JONES, *AGENDAS AND INSTABILITY IN AMERICAN POLITICS*

PETER JOHN

EVERY student of public policy finds out about Baumgartner and Jones's *Agendas and Instability in American Politics* (1993). In fact, most students probably do not know very much about many other classics in public policy, particularly those written before the 1990s. The book's current dominance, alongside John Kingdon's (1984) *Agendas, Alternatives, and Public Policies* and Sabatier and Jenkins-Smith's *An Advocacy Coalition Framework of Policy Change and the Role of Policy-Oriented Learning Therein* (1993), is partly due to the distinctive framework Baumgartner and Jones elaborated—that of punctuated equilibrium—but it also derives from the way in which they built on previous studies in the field and integrated them into a wide-ranging and comprehensive account of agenda-setting and public decision-making. As a result, the book appeals to a wide range of scholars working in public policy, even those who come from contrary intellectual traditions. It is eminently worthy of inclusion in this handbook.

The 20 or so years since its first publication and a new edition to assess alongside the original (Baumgartner and Jones 2009) present a timely opportunity to review the classic status of the book as it moves from being an innovative approach in public policy to one that is introduced as part of the canon. To achieve this aim in this chapter, I elaborate the factors that made the book so important at the time it originally came out in print, in particular the way Baumgartner and Jones responded and reacted to the contemporary intellectual agenda, and also the readiness of scholars for a book that pushed forward the study of agenda-setting. After presenting a summary of the book's arguments and empirical work, I assess the value of the book for students and scholars of public policy reading it in any period.

ORIGINS AND INTELLECTUAL HERITAGE

Baumgartner and Jones criticize contemporary studies that predict stability and equilibrium in decision-making. In the view of many authors, policy-makers have strong incentives not to change policies that are stabilized by the power of elites and the powerful routines of political institutions. This tendency toward equilibrium is not necessarily an incorrect conclusion for scholars to draw, but Baumgartner and Jones reject stability as a general claim—hence the term “instability” in the title of the book. They claim that stability could be disrupted by agenda-setting by key participants in the policy process, but only when conditions are propitious.

Baumgartner and Jones were reacting against several kinds of academic study. One was by rational choice scholars who stressed equilibrium in decision-making, in particular the role of institutions in stabilizing decision-making cycles (Shepsle 1979). Baumgartner and Jones use other formal work to make the opposite point, enlisting Riker's (1982) account of manipulation and agenda change, a dynamic approach that is more in sympathy with their view. As the book unfolds, it is clear that Baumgartner and Jones are very interested in equilibrium, but more in the way in which it can be disrupted and then re-established, and in the role of institutions in that process. In fact, Shepsle's concept of structure-induced equilibrium makes several appearances in the book (e.g. p. 38) as it assists their account of the establishment of equilibrium and its maintenance.

Baumgartner and Jones also responded to the classic works in mainstream of the study of public policy, in particular incrementalism, the claim the policy proceeds in small steps, moderated by limited searches for new options, the exigencies of the interest group process, and the cross-pressures of bureaucratic politics. The classic works on incrementalism (e.g. Lindblom 1959; Wildavsky 1975) have dominated the study of public policy; but Baumgartner and Jones do not entirely believe the inferences that its advocates make. This counterclaim might sound surprising as Baumgartner and Jones consider that decision-makers do not have the capacity to consider the full range of options, that they have bounded rationality just as Simon and the incrementalists argued. But Baumgartner and Jones want to demonstrate that bounded rationality implies *both* instability and stability.

Baumgartner and Jones are steeped in work on American subgovernments, that is in the accounts of strong networks or iron triangles that can dominate policy sectors, such as agriculture or defence; but they believe that these networks could be disrupted by the entry of new political participants, and they acknowledge work that observed the changes in the interest group world in the 1970s and the fragmentation of the world of subgovernments (Hecl 1978). In a similar way, Baumgartner and Jones were inspired by traditional theories of elites and US policy-making, such as by Schattschneider (1960), and draw upon his account of agenda expansion, wherein participants of a conflict can manipulate the message the audience receives so as to gain support; but

Baumgartner and Jones wanted to part company from elite theorists to claim that policy does not always reflect the interests of powerful, and they show that the agenda can at times shift away from representing established interests.

Baumgartner and Jones are also critical of key assumptions about American politics, in particular that governments can do nothing, and that Congress and the Presidency, as well as other interests, had blocked each other to prevent creative policy-making, an outcome that is often called gridlock (though this term does not appear in the book). Such a concern does not appear until the concluding chapter; yet throughout the text, especially in the case studies, Baumgartner and Jones offer a paean about what is possible in politics and provide a rebuff to the pessimists.

As well as wishing to distinguish themselves from extant approaches in political science and public policy, Baumgartner and Jones elaborate and find inspiration from other writing in these fields, in particular from studies of agenda-setting. The work of Schattschneider is critical, as is that of John Kingdon (1984) who is cited at regular intervals in the book, as Kingdon is interested in many of the same processes as Baumgartner and Jones, in particular the way in which participants in politics can sometimes interact to change the public agenda rapidly. They also discuss the work of Downs (1972) who pointed to the way in which the agenda shifts in response to public problems (see pp. 86–8); but they do not share Downs's skepticism that once elites and publics realize the costs of policy change a topic would slide off the agenda back to the relative lack of attention it had received before, even though Baumgartner and Jones admit that cycling does happen for some topics.

Another important source of inspiration was the growing body of work on ideas and public policy, partly reflecting the constructivist turn in social science at the time—sometimes called the new policy analysis—in which ideas are seen as primary: political actors seek to contest ideas as much to broker and bargain between themselves. The work of Deborah Stone (1989) is considered to be important in this respect: it is given a fair amount of treatment in the volume, and sustains the account of policy images (to be discussed further in this chapter). The use of such a work prompts the thought that the underlying intellectual orientation of *Agendas and Instability* is ideational in the sense that ideas matter crucially for policy and they can influence the array of interests at any one time, helping ensure that the ideas constitute politics rather than the other way round. However, Baumgartner and Jones make careful reference to work in rational choice and on the impact of institutional structures as well as to agenda-setting theory. In the end, they adopt a broad approach to theory and display a capacious intellectual sensibility, which ensures their treatment of the policy agenda is compatible with a number of intellectual traditions, whether rational choice, ideational, institutionalist, or just plain empiricist.

So it is possible to understand some aspects of the intellectual environment that shaped the thinking of *Agendas and Instability*, which Baumgartner and Jones were either reacting against in wanting to question some of the conventional wisdoms, or as they were engaging with contemporary debates. It is instructive to observe how they

brought key insights from contemporary work on agenda-setting into their framework so they could elaborate a more comprehensive and integrated approach to this topic.

SUMMARY OF THE FRAMEWORK

Baumgartner and Jones set out a framework for understanding decision-making in American politics, and it is also applicable to other political systems even though that was not their aim at the time of writing the book. The key claim is that decision-making exhibits long periods of stability that are punctuated by short bursts of instability and policy change. In what they termed a policy monopoly, a limited number of interest group leaders, politicians, and bureaucrats can exclusively govern a sector of public policy using similar ideas and approaches to decision-making, sometimes unchallenged for many decades. The process of negative feedback—that is limited attention to a range of issues—can help lock in decision-making for many decades in some circumstances. This equilibrium can be disrupted by new ideas about policy problems and debates about the alternative solutions that could be applied, which often come from decision-makers in the media and from other parts of the political system, as well as from specialists and experts, such as scientists. Ideas catch on through the process of positive feedback whereby attention to the issue expands across different decision-making venues, the various jurisdictions for decision-making. The feedback ensures that, as more people get involved with a debate, they attract still more participants in their immediate networks by creating an upswing of interest, a surge in attention that accelerates in a non-linear fashion. The policy topic then opens up to general debate and discussion right across the political system and in full public view. The momentum for a change in policy soon becomes unstoppable and a punctuation or significant policy change can occur whereby decisions lurch from an old to a new consensus.

Essential for the framework is the limited attention capacity of individuals, which at first causes them to screen out new information that does not fit into the routines and assumptions of the policy monopoly; but once the agenda starts to move, people shift their attention onto the new topic with equally close engagement and filtering as the old topic had received. A parallel set of movements in the agenda across different venues of decision-making creates a surge in attention as new actors get drawn into the debate. Once this rise in attention is established there is a potential for a new policy monopoly to get established, based on the new ideas that have been introduced into political debate, which are advocated by cognate interest groups and their bureaucratic allies. After the period when positive feedback opens up the policy process, negative feedback can help ensure that stability returns by limiting the range of debate and reducing the numbers of participants in it. Rather like the pigs in George Orwell's *Animal Farm*, the same interest group leaders and critics who wanted the policy change become the

beneficiaries of the new order and they dominate the subgovernment in the same way as the previous masters did.

The key idea in the book is punctuated equilibrium, which describes the large shifts in attention that can occur after long periods of stability. This idea draws from work in biology, notably that of Gould, about the progress of evolution, and the observation that there are long periods when there is a slow rate of adaptation, and then rapid periods of accretion of new species. Baumgartner and Jones acknowledge the intellectual debt (p. 17), but they make no explicit claims for an evolutionary mechanism, such as a gene pool of policies and competition, which has figured in work by others (e.g. John 1998) and also is tentatively explored in Jones's other books (Jones 1994, 2001). Theirs is a metaphor.

An important element to their framework is the concept of the policy image that represents how policy is perceived, and which can be contested by participants in the policy process and represented in different ways. Images may be positive or negative or represent different conceptions about how a technology or set of policy instruments work. They can represent different accounts of a problem, alternative problem definitions, which is where the authors directly use Stone's (1989) work, as well as other works on agenda-setting (Cobb and Elder 1972). Although politicians and interest group representatives are important in conveying a policy image, the media are central to the analysis as they can help shift the image and thereby affect the stances of other participants. They encourage positive feedback and contribute to the undermining of policy monopolies. It is no surprise that the media play a central role in the empirical chapters of the book.

The other important term is venue: an arena of decision-making. In the US political system it is often thought to be Congressional committees and their jurisdictions, but it can be other points of the decision-making process where authoritative decisions are made. The venue can be associated with an image in that particular assumptions about public policy may be adopted and followed by key decision-makers located there. This association of venue with image produces what Baumgartner and Jones call a policy niche, which supports a stable subgovernment and reinforces policy monopolies. But the venue may be the place where decision-makers alter their attention to a new issue, especially when in competition with other venues, thus aiding positive feedback and contributing to policy punctuations. Politicians are very much aware of the relationship between venues and images and they may manipulate which committee has a jurisdiction in the knowledge that this allocation might affect agenda-setting and how policy evolves over time. Interest groups that are promoting a policy image are also aware of the powerful role of the venue and will seek to promote policy in their preferred one, engaging in what Baumgartner and Jones call venue shopping, seeking venues for decision-makers that are—or can be persuaded to be—sympathetic to a desired image. Also important are policy entrepreneurs. These are advocates of policies who seek to get benefit from their successful adoption, such as political influence or office. Entrepreneurs operate by brokering new coalitions based on an image they have been championing. In this complex set of interactions, the porous institutions of American

government play their role, particularly in Congress where issues may arise by diffusing across committee jurisdictions, which of course raises the question as to what extent the framework can transport to other countries that have a different kinds and numbers of venues, and less variegated and accessible institutions.

EMPIRICAL APPLICATIONS AND EXTENSIONS

As well as elaborating their framework, Baumgartner and Jones devote the bulk of *Agendas and Instability* to examples of punctuated equilibrium and agenda-setting. As such, they provide models for public policy researchers and inspire an approach to the measurement of the policy agenda. In chapter 3 they criticize the limitations of extant studies in public policy that rely on cross-sectional designs, or just comparison across policy sectors. They make the case both for longitudinal studies that collect reliable data on the policy agenda, where it is possible to observe how a policy image changes over long periods of time, such as in the way it is represented in the media, but also for studies of other public documents, such as the Congressional record to determine the extent of interest group access, and then for comparative studies that can examine different cases. The central chapters of *Agendas and Instability* are based on case studies that illustrate the empirical strategy as well as exploring the punctuated equilibrium framework in the various contexts and stages of policy-making. The aim is to show that there are similar patterns of change in these policy sectors, which acts as a confirmation of the theoretical propositions set out in earlier chapters. The main variation they introduce is the extent to which partisan interests are at work, something the book is relatively silent on up until this point. But the implication of this partisan variation is to show how the shape of policy-making is similar whether it is something the parties fight about or not, which is a familiar theme in agenda-setting studies: the production of ideas is endogenous to the policy sector and they are not introduced as part of some larger ideological debate across a range of topics in public policy.

The first and most detailed case study is about the change in image of nuclear power. This policy area had been positively valued but became increasingly criticized in public debates, with the result that the generation of power by nuclear plants virtually stopped in the United States. It is an example of the destruction of a policy monopoly, which occurred in the manner the theory indicates, through a shift in the image and its spread across different venues. They attribute the change to science reporting that began in 1968 (long before the Three Mile Island disaster propelled the industry into its final crisis), which shifted toward a more critical stance focused on the risks and potential dangers of nuclear power. As the tone changed, so attention increased. Also regulations from Atomic Energy Commission rose in number; then the topic and image appeared in Congress and its oversight committees. The power of their approach is to show how the tone can alter rapidly, such as the striking lines in figure 4.3 on p. 73 indicating the large rise in negative coverage and the stability of positive coverage. Baumgartner

and Jones discuss how the economics behind the industry also changed, which made it unviable, which introduces an interesting question as to whether the agenda change was influenced by other structural changes—had the nuclear option remained competitive would the private sector have been able to control the agenda much more with the resources at its disposal? The authors resolve this attribution of influence by showing that the agenda changed before the investment values declined, suggesting that the agenda changes drove the economics rather than the other way round, revealing the importance of agenda-setting. In a similar way, public opinion followed the change in elite opinion rather than the other way round.

Of course, it is hard to make causal inferences even when the data are ordered in this way as it is not possible to observe anticipated reactions or a strategic decisions by power holders to use or not to use the resources they hold, and these decisions are not part of the public record described in *Agendas and Instability*. Baumgartner and Jones question the dominance of a countervailing interest, but they cannot indicate that the exercise of power may be determined prior to the agenda unfolding. In spite of the importance of the media in this and other case studies, it is not possible to make a causal inference about its importance, though a plausible claim can be made from the data.

One key argument in the book is that the representation of an image can affect policy change, in particular whether it is enthusiastic or critical. The former leads to the establishment of a policy monopoly; the latter can assist its dissolution. These ideas are illustrated in two further two case studies on tobacco and pesticides, which are cases where bad news stories destroyed the monopoly. They show the focusing role of the media, and in particular the way in which media attention can amplify a problem and cause attention to it to grow, and also the manner in which conflict over images can get resolved in favor of one image. But not all policy topics proceed in this way, and their example of automobile safety shows cycles of activity rather than the escalation of interest.

Agendas can rise and fall, even in a disaggregated policy area like urban policy, which shows how a new policy interest can be generated from public alarm, such as from the urban riots of the 1960s (also see John 2006a), which promoted public interest, then legislative hearings and public spending on urban policy. But nothing is stable in public policy and such a policy area can easily become a victim of changing political trends, such as the new approach to federalism and public spending in the 1970s. The rise and fall in attention can happen at great speed, which is a feature of the punctuated character of policy change. In this case, the fate of the policy is also tied up with partisan conflict about the preferred type of government intervention.

Attention lurches can occur in valence policy areas where there is not so much partisan conflict, and Baumgartner and Jones review the examples of drugs, alcohol, and child abuse to show the rises in attention to these issues, which take a particular form because of strong norms, such as enforcement for drugs, but less demand for action on alcohol. In these sectors, there is a preference for certain kinds of solutions and also selective readings of the evidence and statistics that are available.

In part three of the book, Baumgartner and Jones elaborate their account with attention to the context and the institutional structures of American politics. This section of book focuses on interest groups and they show how their number and activity change over time, thereby affecting the level of attention overall. They take the example of environmental groups and show how their rise can change the content and tone of agenda. They provide more detail about the changes in Congressional organization and its shifting jurisdictional boundaries that have affected agenda-setting. They give examples about how the success of an image can be affected by the jurisdiction of committees. In the penultimate chapter they outline how the federal system acts as a source for complexity and venue access in the American system, which can hold up policy change, but also promote it.

THE INFLUENCE AND LEGACY OF AGENDAS AND INSTABILITY

Agendas and Instability has had a massive impact in the study of public policy, partly because it is a comprehensive study of agenda-setting and also because it moved the field forward by using new ideas and concepts. The book has dominated public policy studies ever since its publication, leaving other fields such as the study of implementation to languish by comparison. The book is extensively cited (3,796 on Google Scholar), though not as much as Kingdon's. The second edition reproduces most of the text and data of the first, and includes a revised introduction, which extols the comparative work being done with the policy agendas framework. At the end of the book, there is a newly written chapter that updates the empirical case studies.

The book has helped move forward the longitudinal study of public policy by using data sources that extend over many decades. This achievement in the book has also been assisted by the subsequent work of the authors in refining their ideas and units of measures in a series of research-funded projects, called the Policy Agendas Project (www.policyagendas.org). Whereas the data in *Agendas and Instability* were based on selective examples, collected for the case studies in the book, its successor studies produced comprehensive data on agenda-setting for over a century covering most of the key venues in US politics, amounting to 260,000 observations (or over 500,000 when including Congressional bills), and which have generated a slew of publications (e.g. Baumgartner and Jones 2002; Jones and Baumgartner 2004; Baumgartner et al. 2008). In recent years, comparative endeavors have come forth, which were brought together in the Comparative Agendas Project (<http://www.comparativeagendas.info>), and this too has produced its own rash of outputs (e.g. Jones and Baumgartner 2004; Baumgartner et al. 2006, 2011)—all of which have their origin in *Agendas and Instability*. They show how the punctuated pattern of agenda-setting occurs in many political systems including those with very different institutional arrangements to

the US (see John 2006b, for a review). Nor should it be forgotten that a large amount of qualitative work on agenda-setting uses insights from *Agendas and Instability* (e.g. Pralle 2003).

Finally, the concept of punctuated equilibrium has taken off in public policy studies and political science, partly as a result of the book being used to drive the work of the Policy Agendas Project and Comparative Agendas Project. This has been encouraged by the work of Baumgartner and Jones in a number of subsequent publications and in further theoretical work (e.g. Jones and Baumgartner 2007). Similarly, punctuated equilibrium has been subject to a number of tests in recent years (e.g. John and Margetts 2003; Mortensen 2005; Robinson et al. 2007; John and Jennings 2010; John and Bevan 2012a). Moreover, after the publication of *Agendas and Instability*, work on punctuated equilibrium started to appear in historical sociology, with its focus on path dependence and increasing returns, with policy change appearing from time to time (e.g. Pierson 2000; Bridges 2000).

Of course, like any path-breaking book, there are critics of the approach, particularly of the punctuated equilibrium model. The main criticism is the predictable one that it is not a fair representation of work from evolutionary biology, and is essentially a metaphor without the causal mechanisms of evolution in the natural world. This is the approach of Prindle (2006), whose critique also appears in an edited special issue of the *Policy Studies Journal* on the concept (Prindle 2012). Howlett's examination of policy-making in Canada throws some doubt on the prevalence of punctuations and suggests the existence of different kinds of large policy changes, such as of a stepped kind (Howlett 1997). Such an argument also appears in John et al.'s (2013) application of the policy agendas approach to changes in British politics, where they find policy punctuations, issue-attention cycles, and incremental changes. In this book and in John and Bevan (2012b), the standard method of measuring policy punctuations through aggregate methods and examining the extreme points in a frequency distribution are criticized as identifying too many large changes that are not ground-breaking shifts of the punctuated kind. To deal with this problem, John and his coauthors develop the concept of focused adaption as a better way to represent policy changes, which draws on the idea that change may be latent for a number of years before a shift in direction may come about. This concept reintroduces the steering capacity of the state into agenda-setting studies, and highlights the practice of statecraft in guiding the policy agenda. In *Agendas and Instability* politics and politicians often take the backseat when faced with the rise and fall of issues; but in fact they have powerful institutionally entrenched positions from which to determine the course of the agenda, while at the same time responding to pressures from the media and public opinion. Yet, in spite of these criticisms, there is no doubt that the punctuated equilibrium concept plays an important role in understanding policy change, even if not all scholars believe that stochastic methods are the sole means of measuring it. In this way, with both advocates and critics in play, there are ongoing and important research programs that have come from or are linked to this classic book on agenda-setting.

CONCLUSIONS

The potency of *Agendas and Instability* comes from the clarity of its opening claims, its novel use of terms, the strength of the case studies, and its attention to the detail of policy-making in the US. It might have been possible to have used more robust methods and forms of analysis, such as electronic collection of data (as the authors suggest in the second edition), or more advanced statistical methods, or even a more purposive and less selective strategy for case selection; but it is hard to question the quality of the empirical work and careful way the authors derive insights from the temporal ordering of the data series. Of course, it is hard to make causal inferences in the absence of counterfactuals, and the agendas data do not provide these. In a similar way, it is important to remember that punctuated equilibrium is essentially a metaphor to represent the policy process and does not have a causal status in Baumgartner and Jones's account. It is not possible to use the framework to explain why the policy agenda changes; it is better to use it to claim agenda-setting takes a particular course when it does start to change. But this distinction alerts the reader to one of the strengths of the book: Baumgartner and Jones are careful in their causal claims and their book is more about the how than the why of politics and policy. In this way, *Agendas and Instability* remains as an original and highly valuable description of the character and path of agenda-setting in modern democratic political systems.

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CHAPTER 40

ROBERT D. PUTNAM, *BOWLING ALONE: THE COLLAPSE AND REVIVAL OF AMERICAN COMMUNITY*

*Empirical Foundations, Causal Mechanisms,
and Policy Implications*

MELISSA J. MARSCHALL

WHILE the focus of Robert Putnam's writing and research has spanned a number of fields, including comparative political elites, Italian politics, and diplomacy, he always seems to tackle the most pressing questions in the social sciences. For example, in his 1993 book, *Making Democracy Work*, he addressed the age-old question of why some democratic governments succeed while others fail. The findings from this project, which focused on regional governments in Italy, centered on the importance of civil society and civic engagement for the development of strong and effective political institutions. Building on these insights, Putnam very quickly published his now famous article "Bowling Alone: America's Declining Social Capital," in the *Journal of Democracy*. This article not only created an incredible stir among policy-makers, pundits, and political elites, but also provided the impetus for the 2000 book, *Bowling Alone: The Collapse and Revival of American Community*. Because the ultimate question in *Bowling Alone* is essentially a matter of public policy—What can be done to revive community and the stock of social capital in America?—it is this piece of Putnam's collective works that will serve as the central focus of this chapter.

Building on his findings in *Making Democracy Work*, which connected civic virtue and the cumulative effects of choir groups and soccer leagues to the development of trust, civic engagement, and cooperation among Italian citizens, in *Bowling Alone*, Putnam shifts his attention to the United States and the question of what happened

to civic and social life in our local communities over the past several decades. He wonders: “Is life in communities as we enter the twenty-first century really so different after all from the reality of American communities in the 1950s and 1960s” (p. 25)? Though Putnam brings more data to bear on this question than he did in the *Journal of Democracy* article or other similar essays,¹ he arrives at the same answer in *Bowling Alone*: Yes! Indeed, bringing together an impressive array of indicators and evidence, Putnam devotes section II of the book (chapters 2–9) to documenting the waxing and waning of Americans’ social capital and civic engagement.² While not all of Putnam’s graphs include trends for the entire twentieth century, those that do tend to show the same basic pattern (depicted in Figure 40.1): rising from a low baseline in the early 1900s and continuing until around 1930, dipping slightly during the Great Depression, climbing steeply through the 1940s and 1950s to a peak around 1960, and then declining gradually and steadily through the last decades of the twentieth century.³

In *Bowling Alone*, Putnam devotes considerable time and energy not only to documenting this trend, but also to explaining its causes and sources. In section III of the book he addresses the questions of how and why the US metamorphosed from a model of civic virtue and social connectedness to a nation of non-voters and non-joiners. While television is still part of the answer, the expanded set of data sources employed in *Bowling Alone* and Putnam’s more rigorous criteria for evaluating causal hypotheses in this research lead him to attribute only about 25 percent of the decline to television. He estimates that “suburbanization, commuting and sprawl” and the “pressures of time and money, including the special pressures on two-career families” each account for another 10 percent of the decline in social capital and that “generational change”—what Putnam defines as “the slow, steady, and ineluctable replacement of the long civic generation by their less involved children and grandchildren” (p. 283)—explains the lion’s share of the decline (more than 50 percent).

These findings, as well as Putnam’s meticulous documentation of the changing character of American society over the past several decades, are stimulating and provide much food for thought. However, it is really the second half of the book, sections IV and V, where Putnam addresses the critical policy questions of whether social capital matters for individuals and communities and what if anything, we can do to get it back.

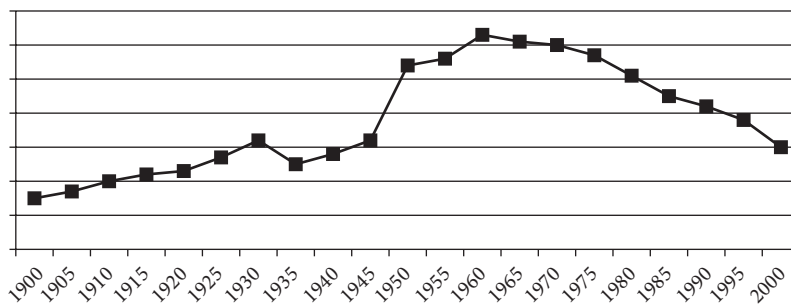


FIG. 40.1 Waxing and waning of social capital and civic engagement (illustration)

For this reason, my chapter focuses on how Putnam goes about addressing these policy questions and identifies what I see as some of the main strengths and weaknesses of his approach and analysis. As a disclaimer, I should note that Putnam's tendency to focus on aggregate-level measures and macro-level processes and explanations is a bit at odds with my own approach, which is much more localized and micro-focused. Though I will put aside issues of local context and how variation in local conditions, legal and political frameworks, and cultural norms and expectations shape social processes and outcomes for now, in this chapter I do pay considerable attention to conceptual, theoretical, and empirical issues that I believe must be examined more critically at the micro-level. Indeed, throughout this chapter I will make the case for why a micro-foundation to the study and understanding of social capital is so essential, particularly for those of us interested in public policy and administration.

SOCIAL CAPITAL: OUTCOMES, RELATIONSHIPS, AND MECHANISMS

Having documented the decline in civic engagement, social connectedness, and Americans' sense of community more broadly, in section IV of *Bowling Alone*, Putnam sets out to document just why these trends are so devastating for America and Americans. He selects five "illustrative" fields—child welfare and education, public safety and neighborhood organization, labor-market outcomes and economic performance, health and happiness, and democracy and democracy values—and summarizes the literature and findings that examine the relationship between "social capital" and a variety of indicators measuring these outcomes. He draws on studies and findings that examine these relationships at the individual and aggregate levels, where aggregates vary from neighborhoods and schools, to states and even nations.

Across most of these fields, the research investigating the effects of social capital is relatively advanced. Thus, it is to be expected that his review of this research is neither systematic nor comprehensive. While in places, he devotes more time to articulating the causal mechanisms and specific components of social capital that underlie the substantive outcome of interest, his purpose appears to be to illustrate the broad array of outcomes that are linked to social capital and to highlight just how pervasive this concept is across all of the social sciences. A clear strength of this section is the way it uses the concept of social capital as a vehicle to demonstrate the interconnectedness of social science disciplines. For example, researchers in sociology, economics, public health, psychology, and political science, use indicators of social capital to explain outcomes such as SAT scores, the Kids Count Index of Child Welfare, violent crime rates, the employment status of inner-city black youths, housing values, life expectancy, tax compliance, to name just a few. Though we infrequently attempt to speak to audiences outside of our subfields or contemplate how our own work might contribute to larger

processes or phenomena that span multiple subfields and even disciplines, Putnam shows us just how valuable these exercises could be.

Indeed, tackling the big questions and problems—as Putnam does in *Bowling Alone*—requires that scholars and researchers move outside their comfort zones and engage in more meaningful cross-disciplinary research and writing.⁴ Given the nature of our work, policy scholars are more apt to already be doing this, or to at least realize the value of this type of cross-fertilization. However, it remains more difficult (and less rewarded) than it ought to be, particularly given the pay-offs in terms of moving knowledge forward, increasing our capacity to tackle the biggest and most pressing questions, and ultimately providing critical evidence that could aid policy-makers and other practitioners as they devise programs and solutions to these problems.

While Putnam's section IV is likely of most interest to policy scholars, it is the shortest and most superficial section in the book. Apart from summarizing many well-established areas of inquiry and highlighting some key findings, he does not engage this literature very rigorously, or offer much in the way of a plan for how future inquiry might proceed. The main purpose appears to be to answer the "So what?" question. Why should we care about social capital and its decline? Answer: Because it matters for practically everything important to us as humans and human societies!

The downside in this broad and relatively superficial approach to the concept of social capital in *Bowling Alone* is that it is so all encompassing that it tends to muddy and confuse the waters rather than add clarity and precision. This is a problem for section IV and the book more generally, since Putnam tends to lump myriad indicators of social capital—ranging from behaviors to attitudes to social structures—into a single construct and simplify what are in reality, complicated processes and relationships. Policy scholars and practitioners will likely find this approach limiting since they are especially interested in both identifying precisely which indicators of social capital are linked to which outcomes and understanding what mechanism(s) are at work. This level of specificity would enhance policy makers' ability to concentrate on the things that really matter, protecting and expanding programs and policies that appear to be working, eliminating or changing others that are not, and devising new ones to address problem areas or exploit new opportunities.

Given that the ultimate objective of the *Bowling Alone* seems to be in section V, where Putnam tackles the biggest question of all, "What is to be done?" there is indeed, an imperative to be more precise about indicators, relationships, and mechanisms. Otherwise, how can Putnam or the researchers and practitioners interested in policy change develop a set of tangible and doable steps for what individuals and communities can do to reverse the disturbing decline of social capital in America? For this reason, in the next section of this chapter I look more critically at both the dimensions and indicators of social capital and the relationships and mechanisms Putnam identifies and describes. This will lay the foundation for understanding

whether and how Putnam's answers to the "What is to be done?" question are appropriate and capable of addressing our social capital problem and thus what solutions might be developed for stemming or even reversing the depletion of social capital in American communities.

What is Social Capital and How Does it Work?

As the first sections of the book amply illustrate, Putnam conceptualizes social capital as multi-dimensional. By his account, it includes a wide range of social and political behaviors (voting, volunteering, joining, donating, talking, bowling) as well as attitudes and beliefs (trust, efficacy, tolerance). However, it also includes social structure (networks) and social resources (norms, obligations, information). In fact, Putnam emphasizes the role of social structure in the definition of social capital he provides in the opening pages of the book:

[S]ocial capital refers to connections among individuals—social networks and the norms of reciprocity and trustworthiness that rise from them. In that sense social capital is closely related to what some have called "civic virtue." The difference is that "social capital" calls attention to the fact that civic virtue is most powerful when embedded in a dense network of reciprocal relations. A society of many virtuous but isolated individuals is not necessarily rich in social capital. (Putnam 2000: 19)

Social capital is not simply a multi-dimensional concept, but a multi-level one as well. The broad range of individual-level attitudes and behaviors can be and customarily are, aggregated to create measures of social capital at the neighborhood, city, state, or even national level. Yet, according to Putnam's own definition, this measurement approach might not be enough to capture the stock of social capital among groups, communities, and societies. In particular, because social structures and resources embody what Przeworski and Teune (1970) called "settings," they are properties of collectives that cannot be observed at the level of individual at all. Indeed, social structures and resources are the most elusive component of the social capital story. They appear as indicators, mechanisms, and outcomes of social capital, depending on the question or context of Putnam's writing. Yet, they are rarely operationalized and almost never measured. In short, we know almost nothing about them, despite the central role they play in Putnam's description and analysis of social capital.

The complexity of the "social capital" construct raises significant problems from both a conceptual and empirical perspective. For example, how do we specify and test social science theory if we do not have a clear understanding of concepts and measures? How can we identify potential solutions and alter outcomes if we have not first clearly defined social capital's component parts and specified how they are related to each other?

Table 40.1 lists the four major dimensions of social capital referenced by Putnam and provides examples of specific indicators of each. Putnam goes to extraordinary length when it comes to measuring the individual-level indicators over time. Yet as noted previously, when it comes to explaining what causes the waxing and waning of these indicators, Putnam's focus is on macro-level factors rather than the specification of a micro-level theory.⁵ This is not to say however, that Putnam does not imply, and in many instances formally hypothesize about the relationships among and between micro-level phenomena. And though he may state that "The causal arrows among civic involvement, reciprocity, honesty, and social trust are as tangled as well-tossed spaghetti" (Putnam 2000: 137), the claims he makes in *Bowling Alone* and elsewhere suggest that some causal relations and processes are more favored than others.

For starters, it is clear in many of Putnam's earlier articles that associational membership is the arena where he believes much of the social capital activity and formation takes place. According to Putnam, membership in voluntary associations fosters face-to-face interactions between members and creates a setting for the development of trust. While the process does not necessarily begin with joining and participating, Putnam repeatedly asserts that civic attitudes are developed and strengthened through social interactions that take place in formal organizations and settings:

- "Internally, associations instill in their members habits of cooperation, solidarity, and public- spiritedness. ... Participation in civic organizations inculcates skills of cooperation as well as a sense of shared responsibility for collective endeavors" (Putnam 1993: 89).
- "Joiners become more tolerant, less cynical and more empathetic to the misfortunes of others" (Putnam 2000: 288).
- "The theory of social capital presumes that, generally speaking, the more we connect with other people, the more we trust them, and vice versa" (Putnam, 1995b: 665).

It is not only formal involvement that matters however. Informal social interactions play a critical role in Putnam's argument since they foster the development of

Table 40.1 Components of the Social Capital Construct

Behaviors	Attitudes	Social Structure	Social Resources
joining	trust	strong ties	norms
voting	efficacy	weak ties	obligations
talking	tolerance	intergenerational closure	information

attitudes— especially generalized trust—and the formation and maintenance of social networks and structures:

- “Frequent interaction among a diverse set of people tends to produce a norm of generalized reciprocity” (Putnam 2000: 21).
- “People who trust others are all-around good citizens, and those more engaged in community life are both more trusting and more trustworthy” (Putnam 2000: 137).
- “[I]nformal connections generally do not build civic skills in the ways that involvement in a club, a political group, a union, or a church can, but informal connections are very important in sustaining social networks” (Putnam 2000: 95).
- “Dense networks of interaction probably broaden the participants’ sense of self, developing the ‘I’ into the ‘we,’ or (in the language of rational-choice theorists) enhancing the participants’ ‘taste’ for collective benefits” (Putnam 1995a: 67).
- “People who have active and trusting connections to others—whether family members, friends or fellow bowlers—develop or maintain character traits that are good for the rest of the society” (Putnam 2000: 288).

Though Putnam never devotes much time or attention to the literature on social networks, they figure prominently in his causal thinking. Networks exist and are activated when two or more individuals engage in social behavior. In *Bowling Alone* he develops a stronger distinction between two particular types of networks: bridging (inclusive) and bonding (exclusive). These parallel Granovetter’s (1973) weak and strong ties and also incorporate in some respects, Coleman’s (1988) concept of closure. In any case, Putnam typically treats networks and social structure as the mechanisms that translate individual behaviors and attitudes into social resources and outcomes:

- “Networks of community engagement foster sturdy norms of reciprocity” (Putnam 2000: 20).
- “Networks of civic engagement foster sturdy norms of generalized reciprocity and encourage the emergence of social trust. Such networks facilitate coordination and communication, amplify reputations, and thus allow dilemmas of collective action to be resolved” (Putnam 1995a: 67).
- “When people lack connections to others, they are unable to test the veracity of their own views, whether in the give-and-take of casual conversation or in more formal deliberation. Without such an opportunity people are more likely to be swayed by their worst impulses” (Putnam 2000: 288–9).
- “Bonding social capital is good for undergirding specific reciprocity and mobilizing solidarity. . . . Bridging networks, by contrast, are better for linkage to external assets and for information diffusion” (Putnam 2000: 22).
- “Bonding social capital constitutes a kind of sociological superglue, whereas bridging social capital provides a sociological WD-40. Bonding social capital, by creating strong in-group loyalty, may also create strong out-group antagonism” (Putnam 2000: 23).

While the causal relationships depicted have been examined, empirically scrutinized, verified, and refuted by scholars across many subfields and disciplines, a unified causal model capturing each of the four dimensions in Table 40.1 has not been fully specified or tested (for more on this, see Theiss-Morse and Hibbing 2005; Delhey and Newton 2003; Lin 1999). So while there is ample empirical evidence to support causal connections between each of these dimensions of social capital, there is also conflicting evidence, counter-arguments, and alternative theorizing. Putnam's objective was not to engage this line of inquiry very rigorously. Yet not doing so leaves readers unsatisfied and still uncertain of how social capital works. I will highlight several areas where scholars have fruitfully engaged or challenged Putnam's theoretical contribution, again keeping an eye on the fact that Putnam's ultimate question in *Bowling Alone* is essentially a matter of public policy: "What is to be done?"

Challenges, Extensions, and Future Directions

First, much was written in the years between the publication of Putnam's initial piece on social capital (Putnam 1995a) and *Bowling Alone*, critiquing Putnam's argument that civic engagement and social interactions are solely or even primarily responsible for trust production.⁶ This criticism continues. For example, some scholars posit that sources of social interaction outside of associational life (e.g. school, family, work, and community) are equally, if not more important (Newton 1997; Mutz and Mondak 2006; Schneider et al. 1997; Delhey and Newton 2003). Others point to institutional and state sources of trust and cooperation (Berman 1997; Levi 1996; Tarrow 1996) or broader experiences and societal conditions (Whiteley 1999; Newton 1999; Inglehart 1999), while still others believe that social trust is primarily a personality trait learned early in life and only marginally subject to change from outside forces (Uslaner 1999; see also social psychology literature, e.g. Allport 1961). Thus, if we are to devise policies and devote public (or private) resources to stimulating the development of social capital, it is important to have a more accurate understanding of which interactions are most productive, the conditions under which they produce constructive attitudes such as social trust, and what the magnitude of these effects are under different conditions. Otherwise, policy-makers run the risk of investing scarce resources in civic organizations and programs to bolster membership and civic engagement to little or no effect.

Second, there is the sticky problem of the causal arrow. To be sure, people who are joiners and participators also generally trust others more. Does this greater trust lead them to participate? Does trust develop primarily as a result of involvement in civic and social organizations? Or is it some combination of both? While Stolle (1998) makes a rather strong case for self-selection (see also Rothstein and Stolle 2008; Stolle and Rochon 1998), others have specified and tested a reciprocal relationship between trusting and joining, finding mixed evidence (e.g. Claibourn and Martin 2000). For example, empirical findings from Brehm and Rahn (1997) support Putnam's argument that building or maintaining social capital involves a cyclical process. However, the

tight reciprocal relationship between civic engagement and interpersonal trust uncovered in their analysis is an asymmetric one. Specifically, they find the effect of civic engagement on interpersonal trust much stronger than the reverse effect. As they put it, participation is more efficient in bolstering positive impressions of others, and an ecology of trusting people may not be necessary to initiate the virtuous cycle (Brehm and Rahn 1997: 1017). From a policy perspective this finding is optimistic. Not only are many of the tools and mechanisms necessary for fostering civic engagement relatively well defined by political scientists (see e.g. Verba et al. 1995), but they are also better understood than those required for developing trusting attitudes among community members.

A third and related point concerns the nature of associational groups and memberships. As Rothstein and Stolle (2008) aptly note, even if the link between civic engagement and trust exists, not all associations serve a normatively desirable purpose. Indeed, Putnam's initial failure to acknowledge this point—that groups are just as likely to promote intolerance and undemocratic values and thus are quite capable of creating “unsocial” capital (Levi 1996)—likely prompted him to devote an entire chapter in *Bowling Alone* to the “Dark Side of Social Capital.” Indeed, we do not need to search very hard to find examples of such groups in the United States. From cults to gangs to neo-Nazi or al-Qaeda groups and the KKK, there are many associations that nurture an inward-looking, segregating culture that is frequently characterized by intolerance and norms of obedience (see also Armony 2004; Theiss-Morse and Hibbing 2005). Therefore, this causal link in Putnam's model must be given considerably more care.

Associations cannot be treated as black boxes that automatically produce positive externalities in the form of civic attitudes, democratic norms, or collective benefits. As an early advocate of this concern, Stolle (1998) cautioned that little was known about whether and how voluntary associations make their members more trusting and cooperative, whether trust and cooperative attitudes increase linearly with the length of time spent in any type of association, or whether these civic attitudes are a function of a particular type of involvement or a special type of group. While Putnam's work has certainly inspired researchers to tackle these questions with renewed enthusiasm, a priority should be the advancement of micro-theory of social capital capable of explaining the role of membership in voluntary associations with respect to trust, norms of reciprocity, and collective attitudes.

The fourth point focuses on the connections among civic attitudes, social structures and resources. Putnam describes resources as collective assets that are embedded in social networks. He views them as the core element of social capital. However, with the exception of his delineation of bridging (weak) and bonding (strong) ties, he never pursues this piece of the puzzle. Nowhere in *Bowling Alone* does Putnam truly engage extant work on social networks and he offers little when it comes to the issue of how to measure the social structures that connect behaviors and attitudes to social resources and ultimately outcomes. What are these resources and how can we operationalize and measure them? Who possesses these resources and how are individuals within

and outside the network situated vis-à-vis the individuals who possess them? How might the social networks of Americans have changed over the decades that Putnam has recorded declines in behavioral and attitudinal indicators of social capital? And what do the social networks of communities and societies look like in places that score high on Putnam's Social Capital Index?⁷

In his article, "Building a Network Theory of Social Capital," Lin (1999) addresses these questions in considerable detail, enumerating and describing the array of indicators that have been used by network scholars, summarizing the perspectives and controversies in the literature, and tackling the difficult issue of how to model social capital. Lin not only articulates how resources are embedded in social structure, but also explains what mechanisms are at work to enable individuals situated in networks with different forms, compositions, and sizes to access, utilize, and contribute to these resources. Lin's work helps us to specify both theoretically and empirically how we might devise programs and policies to target social capital creation. In addition, Lin provides a much stronger framework for addressing Putnam's vexing question about how contemporary modes of social interaction, namely social media, fit into the social capital debate. For example, in response to Putnam's arguments regarding the decline of social capital in the US, Lin states that:

There are a number of conceptual (tautological) and measurement (what associations are relational) flaws one can find in this [Putnam's] research program. In view of the dramatic growth of cybernetworks, a fundamental question can be raised: do cyber-networks carry social capital? If so, there is strong evidence that the declining thesis is false. I suggest that indeed we are witnessing a revolutionary rise of social capital, as represented by cybernetworks. In fact, we are witnessing a new era where social capital will soon supersede personal capital in significance and effect. (Lin 1999: 45)

In short, research on social networks has much to say about and contribute to the ideas and claims Putnam makes in *Bowling Alone*. There seems to me a grave danger in leaving this critical component of the social capital story unspecified, as Putnam is so prone to do. Without anchoring the concept of social capital more clearly and unambiguously in social networks and embedded resources, its utility is questionable and, as Lin predicts, it is increasingly likely to fade away as an intellectual enterprise (Lin 1999: 48).

PUTNAM'S "WHAT'S TO BE DONE?" A POLICY PERSPECTIVE

So, what is to be done? This is the ultimate question Putnam poses in *Bowling Alone*, and the one policy and public administration scholars are likely most anxious to finally get to. Putnam does not provide many hints about what this section will contain,

and indeed, it comes largely as a surprise (at least to this reader!). It includes a rather lengthy chapter that uses the historical period of the Gilded Age (1870–1900) and the Progressive Era (1900–15) to illustrate how a similar national crisis during this period was overcome by the “great civic generation” through their inspired grassroots activism, national leadership, social inventiveness, political reform, and practical civic enthusiasm (Putnam 2000: 368). Having told the story of this “exceptional epoch,” Putnam concludes with a final chapter, “Toward an Agenda for Social Capitalists,” where he outlines six key spheres in which Americans—collectively and individually—can begin developing more concrete ideas and plans for how to move forward and restore American community for the twenty-first century. As he explains, these suggestions were informed by the Saguaro Seminar: Civic Engagement in America, which brought together 33 accomplished thinkers and doers from all over the country (including Barack Obama), to deliberate about the ways in which our actions impinge daily upon social capital and how we might discover and invent new ways to connect and engage with each other (Putnam 2000: 404). As a spoiler alert to those who have not yet read the book, these are not policy recommendations! Instead, they are a set of milestones that Putnam believes Americans can and should achieve over the next decade. For example, he calls for us to find ways to ensure that by 2010:

[T]he level of civic engagement among Americans then coming of age in all parts of our society will match that of their grandparents when they were that same age, and that at the same time bridging social capital will be substantially greater than it was in their grandparents’ era. (p. 404)

America’s workplace will be substantially more family friendly and community-congenial, so that American workers will be enabled to replenish our stocks of social capital both within and outside of the workplace. (p. 406)

Americans will spend less time traveling and more time connecting with our neighbors than we do today, that we will live in more integrated and pedestrian-friendly areas, and that the design of our communities and the availability of public space will encourage more casual socializing with friends and neighbors. (p. 408)

To be sure, Putnam’s milestones are lofty and laudable. They reminded me of President Bush’s 2001 No Child Left Behind (NCLB) legislation, which among other things, sought to have all American students achieve proficiency or better in reading and math by 2013–14 and all American students taught by highly qualified teachers by 2005–6. Like NCLB Putnam’s milestones have proved unrealistic and ultimately unobtainable. Writing this chapter in 2013, it is evident that we have achieved none of them. While we have certainly made progress in some areas, we have also lost ground in others. So, what happened? And what do we do now?

Stepping back for a moment, Putnam’s original essay in the *Journal of Democracy* definitely played an important agenda-setting role. Akin to Kingdon’s (1984) focusing event, the paper focused attention in a very compelling and simple way on a problem

with which most people could identify. It generated considerable enthusiasm among academics, foundations, and public figures. In particular, there was widespread attention paid to renewing involvement in associational life. As Theiss-Morse and Hibbing (2005: 228) note, not only did foundations and institutes pour hundreds of millions of dollars into the study of civic participation, but academic communities also supported it through scholarships, required volunteering, and service-learning programs.

Unfortunately, *Bowling Alone* appears to have done very little to move the process along. The issue has receded from the public agenda and is not as fashionable among academics and foundations as it was a decade or so ago. In addition to the points I have made here, the contents of other chapters in this volume are sure to offer many valuable insights. Using the language of policy scholars, there are clear limitations in the way *Bowling Alone* addresses the problem definition and issue framing stages of the policy process. Putnam's macro-level focus in diagnosing the causes of social capital decline not only makes the problem seem unapproachable and insurmountable, but the possible policy solutions often seem at odds with American's core values of liberty and the pursuit of happiness. For example, are there government programs or policies that could alter Americans' TV viewing habits, make us (especially women) work less, or encourage some of us to earn or accumulate less wealth? Perhaps, but imagining how such policies could be framed is not easy. How about policies that alter our residential patterns so that we live closer to work or school or that redesign our cities and suburbs in ways that make them denser, less dependent on automobiles, and more friendly toward pedestrians? Or most obtusely, what about programs that would help us become more like our grandparents or great grandparents?

In short, framed as it is in *Bowling Alone*, the issue of American's declining social capital is too big, too elusive, and frankly, too controversial. Instead of gaining momentum, it faded from public view. One might argue that this is at least in part due to the fact that the terms for policy debate were not concretely or explicitly established. In addition, Putnam did not offer any specific policy proposals in developing his agenda for social capitalists. Finally, he did not seem to have delegated any action items to his Segura Seminar participants. Unaware of what actions could or should be taken, stakeholders appear not to have taken up Putnam's call to arms.

It is interesting that Putnam borrows the title from Lenin's famous political pamphlet, *What is to be Done?* for the final section of his book. In Lenin's version, however, strategy and action are the drivers for political and social change. Lenin views the creation of a unified, political party that would provide the necessary leadership and organization as essential to his revolutionary movement. Had Putnam's agenda provided more strategy and specific action items for his social capitalists, perhaps an advocacy coalition (Sabatier 1988) or a set of issue networks (Hecló 1978) might have emerged and began to lobby more effectively for policy change. With the timeline Putnam set for the achievement of his milestones, it does appear that he was envisioning something more revolutionary (a punctuated equilibrium?)⁸ and less incremental. In any case, insights from other chapters in this volume will certainly provide more thorough answers as to

why Putnam's milestones have not yet been achieved and what he might have done differently in his attempt at agenda-setting.

SUMMARY AND CONCLUDING REMARKS

At the heart of Putnam's *Bowling Alone* is the quest to address the pressing problem of the declining stock of social capital in America. As Putnam illustrated, this problem has the potential to adversely affect all facets of our lives—economic, political, social, psychological, physiological. Yet the limitations in Putnam's approach to measuring and modeling social capital are a major stumbling block for Putnam's project. In particular, he does not go far or deep enough into the matter of how the different dimensions of social capital are related to one another or which mechanisms are principally responsible for turning social connections and resources into socially desirable outcomes. For policy analysts and public administration scholars, this is a serious shortcoming. To formulate and evaluate public policies and programs it is imperative that we understand not only the problem, but also its causal dynamics, pathways, and mechanisms. If we are to devise solutions to the decline and disappearance of social capital in America, the micro-foundations of social capital development must be better articulated and empirically demonstrated.

At the same time, the concept of social capital has been critically important in getting scholars and policy-makers to pay more attention to the social processes and relationships that shape so many of the outcomes that matter to Americans and America. Putnam deserves a great deal of the credit for this. Through his steadfast commitment to the *Bowling Alone* project and his intellectual exuberance, his work has influenced social and even natural scientists of nearly every stripe. He has single-handedly fostered truly meaningful cross-disciplinary work and he has gotten us all thinking about the questions that truly matter. As policy and public administration scholars and practitioners continue to do the hard work of addressing Putnam's question of "What is to be done?" we can draw on the ever expanding body of research that spans many disciplines and subfields. Indeed, thanks to Putnam and the cross-fertilization his work has inspired, we now have better tools and a broader view of just how important and urgent the work of social scientists and policy scholars is.

NOTES

1. e.g. Putnam 1995(b).
2. e.g. membership rates in national chapter-based associations (p. 54), parent-teacher associations (p. 57), organizational involvement (p. 60), club meeting attendance (p. 61), union membership (p. 81), professional association memberships (p. 84), social visiting (p. 99), family dinners (p. 101), card-playing and other leisure activities (p. 105), "neighboring" (p. 106), informal socializing (p. 108), and, of course, bowling leagues (p. 112).

3. There are a few exceptions. For example, Putnam's data show an increase in altruistic and charitable/volunteer activity as well as activity by evangelical conservative groups, self-help support groups, and other new small groups (for more on the implications of these counter-trends, see Shapiro 2001; Quesenberry 2002).
4. This comfort zone also encapsulates the incentive system of our profession, which tends to encourage more narrow and specialized research and publication outlets.
5. Specifically, he attributes the decline in social capital to Americans' TV viewing habits, work habits and income levels, residential patterns, and the fact that the attitudes and behaviors of the "great civic generation" have not been exhibited at the same rate by subsequent generations of Americans.
6. Putnam acknowledges the "lively debate" in a footnote (ch. 8 n. 15), but notes only that it is important, yet complicated both theoretically and empirically, and only tangential to his concerns (p. 466).
7. e.g. figures 81–3, Putnam finds a linear relationship between this state-level index and the Kids Count Index (1995), an index of educational performance (1990–6) and TV watching by fourth and eighth graders (1990–4) (negative in this case). Presumably the networks of residents in these high-scoring states (e.g. SD, ND, VT, MN, MT, NE, IA) are different than those of residents in low-scoring states (e.g. MS, LA, AL, GA), but Putnam never pursues this line of inquiry.
8. See Baumgartner and Jones (1993) for more details.

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CHAPTER 41

PETER A. HALL AND DAVID SOSKICE, *VARIETIES OF CAPITALISM: THE INSTITUTIONAL FOUNDATIONS OF COMPARATIVE ADVANTAGE*

BERNHARD EBBINGHAUS

INTRODUCTION

CAPITALISM won the Cold War in 1989/90, but is there only one form of capitalism? Rarely has one edited volume received as much academic attention as has the manifesto of a new political economy approach named “Varieties of Capitalism” (Hall and Soskice 2001b), which investigates the cross-national institutional variations of market economies. This influential multi-authored edited volume combines contributions by younger and more senior scholars and has led to innumerable subsequent studies published as conference papers, journal articles, and books. In their Introduction (2001a), political scientist Peter Hall and economist David Soskice lay out an extensive analytical framework, the need to think capitalism not only in big letters but also in the plural. Challenging neoliberal currents, this approach proclaimed that there were two ideal types of capitalism: the Liberal Market Economy (LME) model of neoclassical economics, and the Coordinated Market Economy (CME) with a different institutional “infrastructure.”

This heterodox economic view was already advanced by earlier political economists like Andrew Shonfield (1965), who studied Europe’s post-war “mixed” economies, the cross-cultural comparison of British and Japanese firms by Ronald Dore (Dore

1973), and the popular pamphlet by French economist Michel Albert of the struggle between Anglo-Saxon and “Rhenish” capitalism (Albert 1993). By disciples and critics, the Varieties of Capitalism approach has been labeled “VoC,” which ironically resembles the abbreviation of the Dutch East Indian Company (Vereenigde Oostindische Compagnie), the first globally operating shareholder company. The VoC’s claim of persistent capitalist variety challenged dominant expectations about global convergence since the neoliberal turn of the 1980s and the unbridled success of market capitalism after the fall of Communism in the 1990s. For public policy scholars, the VoC approach provides a fresh institutionalist and firm-centered view on the interdependence of economic and other policy spheres. Most importantly, it claims that a specific set of inter-related institutions determine economic performance due to comparative institutional advantages in today’s global economy.

VoC’s broad-brush typology and comparative institutionalism may be compared to earlier waves of comparative public policy analyses, such as Esping-Andersen’s best-selling monograph on *Three Worlds of Welfare Capitalism* (1990) and Philippe C. Schmitter’s manifesto article “Still the Century of Corporatism?” (1974). VoC shares with these efforts a comparative research program that presaged a paradigm shift in conceptually understanding and empirically studying the durable cross-national diversity of advanced economies. Earlier or parallel undertakings in studying the diversity and convergence of “social systems of production” include several edited volumes (Crouch and Streeck 1997; Hollingsworth and Boyer 1997; Kitschelt et al. 1999) to which the VoC editors and their collaborators have contributed over the years.

Building upon these comparative economic studies at the interface of political science, economic sociology, and institutional economics, Hall and Soskice provided a theoretical framework to rally earlier classificatory attempts. As editors they were able to inspire a cadre of young American and European scholars to systematically analyze the varieties of market economies and their consequences. David Soskice, a British economist then working at Social Science Centre Berlin (WZB) in Germany, had contributed to the neo-corporatist analyses of wage determination in Europe (Flanagan et al. 1983; Soskice 1990) and had already developed binary comparisons of liberal and coordinated market economies (Soskice 1991, 1999). Peter A. Hall, a Canadian political scientist at Harvard University, was well known for his earlier comparative work comparing French and British public administration policies and economic policy (Hall 1986), the ideational turn from Keynesianism to neoliberalism (Hall 1989), and different stages of policy change seen from an institutionalist perspective (Hall 1993; Hall and Taylor 1996).

The VoC’s epicenter is still the extensive Introduction (68 pages) that summarizes the approach not only of a remarkable edited volume combining analytical elaborations, comparative analyses, and case studies of particular sectors and countries. Google Scholar counts above 6,000 citations in text documents on the internet (mid-January 2014), while ISI-citation index reports 2,156 citations for the Introduction by Hall and Soskice to the *Varieties of Capitalism* volume (of which 22 percent during the first five years) in peer-reviewed (largely English) journal articles. This chapter will

introduce the VoC approach as developed in the edited book, summarize the main contributions to that volume along the different institutional spheres, and then discuss applications of its typology before finally criticizing its initially rather static and apolitical approach. This review will go beyond the volume by referring to recent extensions and critical revisions in the comparative political economy literature.

THE VoC APPROACH

Contributing to its success as major reference point, object of critical debates and inspiration for subsequent research, the “Introduction” of the VoC volume set the main stage. Peter Hall and David Soskice as editors lay out the main theoretical framework and major claims of the VoC approach. Given their workplaces it may not be a surprise that many examples of key institutional differences are based on Germany and the United States. The VoC approach provides several claims as part of its theoretical framework: an institutionalist perspective on national market economies, a micro-foundational view of firms’ coordination strategies, and a focus on the conditions and consequences of the institutional infrastructure for an economy and society.

Following Douglass C. North (1990), the editors adopt an *institutionalist* perspective of market economies, defining institutions as “set of rules, formal or informal, that actors generally follow, whether for normative, cognitive or material reasons” (Hall and Soskice 2001a: 9). Similar to Mark Granovetter’s theory of institutional embeddedness (1985), they conceive economic activities as relational between actors who are embedded in social relations. Following the neo-institutionalist conception of a social organization of the economy (Zukin and DiMaggio 1990), the VoC approach considers markets, whether liberal or coordinated, as embedded in social institutions that shape economic preferences, expectations, and actions. Thus, the type of market economy in a country results from the particular institutions present in the different spheres of the economy, and from their impact on actors’ strategies and relations. The most important claim of the VoC approach is that there are institutional complementarities between particular social arrangements across socioeconomic spheres, and that under global competition these can provide specific comparative advantages for economic performance and thus potentially also for social outcomes (Estévez-Abe et al. 2001; Hall and Gingerich 2009).

A major analytical-methodological claim of VoC is its *firm-centered* approach, bringing the corporation as an economic actor back into the analysis of political economy (Hall and Soskice 2001a: 6). While corporatist analyses concentrated on organized labor and capital, in particular on trade unions and employers, VoC focuses first of all on firms as strategic actors. The preferences of firms are not given, but derive from the institutional infrastructure in shaping these relations. Building upon earlier political economic concepts (Hollingsworth and Boyer 1997; Streeck and Schmitter 1985), the importance of different *governance* modes to coordination problems are at the

core of the explanatory framework: in a competitive environment, firms rely on market mechanisms, but firms can also overcome coordination problem by means of hierarchy (solving transaction problems within a company), trust-based networks with other firms (e.g. between a producer and its suppliers), or associative modes in interest groupings (e.g. employer organizations).

VoC advances a *relational* view of the firm by mapping the interaction of the firm with its owners, suppliers, customers, employees, unions, public agencies, and other “stakeholders” in society. Whether economic actors are capable of coordination is crucial, its capacity and style determining how interests can be aligned. Coordination in pure markets allows only exchange in which prices are set by supply and demand between buyers and providers of goods or services. In addition to the market, hierarchy could solve coordination problems through power relations such as in economies with state intervention. To have more long-time, positive-sum coordination, trust in other actors is needed, but this depends on social relations between actors.

A fundamental thesis is that of *institutional complementarity* that “arises when there are interdependencies across domains” (Aoki 2001: 87). In their Introduction, Hall and Soskice define institutional complementarity in a rather functionalist way as institutions in which “the presence (or efficiency) of one increases the returns (or efficiency) of the other” (Hall and Soskice 2001a: 17). This implies coexistence (“presence”) of complementary institutions, thus empirical studies investigate cross-national correlations between institutions. Second, it assumes an efficiency-enhancing relation between spheres, i.e. institutions support each other. Third, this conceptualization leaves many questions open: why did these institutions arise, how do they support each other, and why does this lead to positive returns? The concept of institutional complementarities resonates with Max Weber’s concept of elective affinities, but it remains an open question whether similarity or opposition attracts. Many VoC scholars assume that complementarities can only be between similar institutions, while others have a wider understanding: see the debate in *Socio-Economic Review* following Höpner’s paper (2005a, 2005b) in Crouch et al. (2005). In empirical studies, the cross-national correlation of similar measures across different institutional spheres is seen as a test of such institutional coherence (Hall and Gingerich 2009; Kenworthy 2006).

Further, VoC scholars claim that institutional complementarities, both of LME and CME, provide specific *comparative advantages* in global markets. LME can compete thanks to profit-seeking capital markets with radical innovations or price-competitive mass production due to flexible employment relations. In contrast, CME firms specialize in more incremental technological progress with long-term patient capital and high-quality production, thanks to a skilled labor force and stable employment relations. VoC’s Introduction provides some comparative evidence for sectoral specialization and particular innovation patterns of firms. A subsequent quantitative study indicates that institutional coherence between spheres produces positive economic outcomes (Hall and Gingerich 2009), but other studies have found only mixed support for positive economic outcomes of institutional coherence (Kenworthy 2006; Schneider and Paunescu 2012).

THE INSTITUTIONAL SPHERES

As an institutionalist approach, VoC provides an important framework to study institutional interdependencies within modern political economies, claiming that such diverse spheres as corporate financing, vocational training systems, social policy, and innovation strategies display institutional affinities, if not functional complementarities, as well as potential comparative advantages. The volume's contributions provide comparative cross-national studies on different areas with potential coordination problems. However, the coverage of countries varies, providing a patchwork of case studies examining varying sets of countries selected as exemplars for the liberal model (the UK or USA being the prime examples) and/or coordinated model (Germany being the most frequent choice).

In their Introduction, Hall and Soskice (2001a: 6–7) distinguish different spheres with particular coordination problems of firms: industrial relations, vocational education and training, corporate governance, interfirm relations, and firm–employee relations. Because no exact rationale is given for the selection of these five spheres, it remains unclear whether the list is comprehensive. Indeed, some chapters and later work add further spheres, from the financial system and the welfare state to innovation policy. Thus, the financial system, the relationship between different financial institutions (banks, financial markets) and companies, has been distinguished from the corporate governance of these companies (Jackson and Deeg 2006). Beyond the spheres listed in the Introduction, two VoC contributions also include social and labor policies that go beyond intra-firm employee relations (Estévez-Abe et al. 2001; Mares 2001). Further studies have noted an overlap between VoC and Esping-Andersen's welfare regimes (Ebbinghaus 2006; Ebbinghaus and Manow 2001; Schröder 2013), though there may be advantages to keeping political economy and welfare state typologies separate. Innovation could well be a further area, extending the focus on the vocational training sphere.

According to the VoC framework, *corporate governance* is an important causal factor for explaining differences in political economies. Corporate governance entails the institutions shaping the ownership structure and rules for controlling companies. VoC distinguishes two ideal-typical governance modes: shareholder versus stakeholder models, for instance, the British equity-driven capitalism and the German co-determination model (Vitols 2001). In liberal market economies (LMEs), shareholder value-orientation seeks to align the interests of management to the profit-interests of its owners (following principal-agent theory). Coordinated market economies (CMEs), however, are characterized by a stakeholder governance model in which companies must take into account the interests of its owners, management, employees, suppliers, and clients as well as the wider public. An example for an institutional complementarity between corporate governance and labor relations is the German co-determination law that stipulates not only information rights at workplace level, but also the equal representation of labor on bipartite supervisory boards of stock-listed companies.

Related to corporate governance, the *financial system* provides different incentive structures for firms with respect to their short-term or long-term goals. In the case of LMEs, larger firms mainly derive their financing through equities and financial markets are driven by institutional investors seeking high returns that reinforce the orientation towards short-term profits. As a consequence of more limited state pensions, private pension funds reinforce the importance of institutional investors in these countries (Jackson and Vitols 2001). On the other hand, credit provided by banks with time-honored ties to companies (*Hausbanken*) and self-financed growth strategies matter more in CMEs, allowing a more long-term investment strategy. In Germany, for example, larger firms pledge occupational pensions for their long-term employees that are financed by book reserves, thus reinvesting profits into the company (Ebbinghaus 2006).

Applying a relational view to *interfirm cooperation*, VoC argues that the efficiency with which firms use labor and capital is dependent on how well they coordinate with others, for instance in research and development. In LMEs, strong market competition and antitrust laws have limited business coordination, whereas a long tradition of business coordination through larger trusts, interfirm relations, and business associations exists in CMEs. Cross-ownership and interlocking directorates have also been a major hallmark of the German and Japanese business models, while long-term producer–supplier relationships extend cooperation beyond conglomerates. Financial institutions have played a broker role in such networks, though this led to a dramatic disintegration of the “Deutschland AG” in the “disentanglement” of the interlocking German corporations in the early 2000s, assisted by tax advantages provided by the red-green government (Beyer and Höpner 2003).

Building upon neo-corporatist studies, the VoC approach highlights important differences between two modes of *labor relations* that shape employer–employee intermediation of interests. In LME countries, conflictual labor relations with short-term zero-sum bargaining over wages and working conditions prevail. While management prefers to keep unions in check and maximize profits for shareholders, trade unions are more fragmented and their organizing capacity depends on market power through the strike weapon. As consequence of asymmetric power relations, larger wage inequality and less wage growth prevail. In contrast, labor relations in CMEs are planned long-term and consensual, particularly when positive-sum bargains are possible. As corporatist theory stipulates, this requires both sides to be well organized and encompassing—and the state to be supportive of their self-governing role. As a consequence of corporatist labor relations, wages should be more compressed, less numerical but with internal flexibility. Such differences between pluralist versus corporatist labor relations reflect the long-term historical trajectories and political compromises despite globalization pressures (Thelen 2001). In contrast to corporatist studies, Nordic corporatism has not been the main focus of VoC, but rather coordinated systems such as Germany, with its leading sectoral or regional negotiations that set wage norms (Soskice 1990).

Among VoC’s particular contributions (with parallel efforts by economic and educational sociologists) is the focus on the importance of *skill formation*, building on

studies of the diversity of skill regimes (Crouch et al. 1999) and with more attention recently in comparative studies that also reflect the influence of supranational governance in Europe (Busemeyer and Trampusch 2012). Iversen and Stephens (2008) distinguish three “worlds of human capital formation” by combining welfare regime and VoC perspectives: a social-democratic redistribution with public education investment, a Christian-democratic social insurance and vocational training policy, and a liberal one relying on modest public education and significant private investment in general skills. Germany, relying on its dual vocational training model that combines school-based theoretical education with firm-based practical training, is seen as a particularly “successful skill machine” (Culpepper and Finegold 1999) based on a historical trajectory of craft-related apprenticeships (Thelen 2004) in which small and medium-sized firms contribute to occupation-based skill formation from which larger companies also profit. Advantages include smoother transitions from school to work and lower youth unemployment. However, not all CMEs have adopted German-style training systems and the Japanese on-the-job training focuses on firm-related skills only (Thelen 2004).

For a long-term growth perspective, *innovation* strategies matter. VoC claims a major difference between LMEs that foster radical innovation and CMEs that are more inclined towards incremental innovation (Hall and Soskice 2001a). In LMEs, entrepreneurial experimentation, private–public partnerships in research, and risky investments through venture capital of start-ups provide more opportunities for radical innovation. In contrast, innovation in CMEs remains more incremental, long-term, and improvement-oriented, due to long-established public research infrastructure, in-house research and development, and patient capital. When discussing comparative advantages, Hall and Soskice (2001a: 38–44) provide graphs of patent specialization, indicating the cross-national differences across sectors: the US specializes in radical innovation (such as in biotechnology), Germany in more incremental innovation (such as in the machine tool industry). However, there have been considerable debates on whether national or even regional innovation systems can be adequately captured by this binary (Lundvall 2010; Ortiz 2013). There may also be complementary insights between VoC’s concern for firms’ skill formation and the focus on knowledge production in the national innovation system perspective (Herrmann and Peine 2011).

Production models differ not only in skill formation, but also in terms of *employee relations*: in LMEs, flexible hiring and firing provides much less stable employment relations than in CME firms in which long tenure is common. Thus, internal labor markets providing advancement within firms (or conglomerates) play an important role in both Germany and Japan. Employment tenure and seniority wages, however, also lead to problems at career end. For example, until recently German employers relied on early retirement financed by public social policies to rejuvenate their workforce, while Japanese employers provided secondary jobs for their older-career workers in other firms within their network (Ebbinghaus 2006). The production models have unintended consequences for the gender biases in wage gaps and employment segregation

given the specific skills biases for male breadwinners in CMEs in comparison to the more flexible labor market in LMEs (Estévez-Abe 2006).

Finally, analysis of *social protection* within the VoC approach as well as the employer perspective in social policy studies have gained in importance. Three contributions in the VoC volume explicitly addressed the production–protection nexus. In contrast to Esping-Andersen’s welfare regime analysis, VoC focuses on the firm’s preferences for particular solutions to social policy problems. One study explains differences in employers’ positions on social policy reforms using game-theoretical models (Mares 2001), while a British–German comparative case study explores employer interests in labor market policy (Wood 2001). A highly influential contribution is the comparative analysis of the welfare–employment nexus (Estévez-Abe et al. 2001) based on an asset theory of social policy preferences (Iversen and Soskice 2001). The main argument here is that employers in CMEs have an interest in fostering long-term investment in occupational or firm-specific skills by employees through regulated employment protection, generous unemployment insurance, and collective wage formation. Individuals are only willing to invest in non-transferable skills if they can trust that they will be sheltered against income losses. Empirical evidence shows some correlation between LMEs and rather flexible labor markets, low unemployment protection, and lower collective wage growth. However, the relationship is more complicated among CMEs, such as in firm-specific training for tenured employees in Japan, industry-specific skills in smaller Danish firms, or occupation-based dual vocational training in Germany (Estévez-Abe et al. 2001).

THE TYPOLOGY BUSINESS

The most widely referred to aspect of the VoC approach is its binary typology of capitalist systems: the distinction between LMEs and CMEs based on their coordination capacity. This typology has been widely used as a reference point for classifying capitalist systems even beyond the OECD countries initially covered. After the collapse of Communist regimes, this typology of intra-capitalist variety seemed to address the need to contrast different conceptions of market economies. VoC provided a micro-theoretical base and macro-empirical mapping for the existence of two institutional equilibria that were locked in and had their comparative advantages (Hall and Soskice 2001a). However, the claim of a stable binary typology also gave rise to many empirical studies and conceptual attempts to partially revise or completely refute this scheme (for a review see Jackson and Deeg 2006). The original VoC volume did not provide any empirical test and the Introduction showed only illustrative bivariate evidence and ad hoc references, largely covering Germany/Japan versus Britain/USA. Thus, VoC has been criticized for being “limited by its methodological nationalism, a tendency towards static analysis and latent institutional functionalism, and by an inability to adequately balance national specificity and path-dependency on the one hand with

common underlying tendencies in capitalist restructuring on the other” (Peck and Theodore 2007: 731).

A widely cited comparative analysis by Hall and Gingerich (2009) tested the VoC “congruence” across institutional spheres for a set of 20 OECD countries. Particularly indicative is one graph that shows the correlations between the six spheres: labor relations, corporate governance, firm strategy, interfirm relations, training systems, social protection, and production market regulation. This cross-national comparison has been interpreted as a test of institutional complementarities, though any correlation would merely indicate institutional coherence across the differentially classified countries. Interestingly, the empirical test included a third type situated between the poles of LME and CME, namely a hybrid Mediterranean Market Economy (MME) including France and some Southern European countries that have less fortunate economic performance.

Later empirical replications found more mixed results and much less support for the thesis that coherence within LME or CME institutional infrastructures is associated with positive economic output, in particular with employment growth (Kenworthy 2006). A major comparative study based on a wide range of indicators that suggested five clusters of capitalism across OECD countries was presented by French economist Bruno Amable (2003): an Anglophone market-based, a Nordic social-democratic, a continental social market, a Mediterranean mixed, and an Asian model. Such a differentiated scheme reflects not only varieties of production systems, but also its “elective affinities” with different labor relations and welfare regimes (see also Ebbinghaus 2006; Schröder 2013). A recent comparative time-series study (Schneider and Paunescu 2012) finds mixed evidence for VoC among 26 old and new OECD countries, in particular finding considerable liberalization among some of the prominent CME show-cases, such as Nordic countries and the Netherlands. Similarly, an econometric study of OECD countries derived four clusters that matter less for economic growth than for distributional outcomes, though these clusters became unstable after the fall of the Berlin wall (Pryor 2005).

VoC’s binary categorization (as well as its empirical test of three real types) has been criticized on conceptual, methodological, and empirical grounds. Conceptually, the ideal-type model is more strongly grounded in the liberal than in its coordinated counterpart, which was largely based on an eclectic summary of features from real cases such as Germany and Japan—a criticism advanced by Crouch (2005) and others. Indeed, various authors have emphasized different aspects of coordination and there seems to be a wide variety of institutional arrangements that are subsumed as “non-liberal”. Thus, the choice of a primary example will shift the analytical focus to institutional combinations more specific to this region, while comparative efforts to extend the typologies beyond these will indicate more complex patterns. Already in VoC’s initial empirical typology (Hall and Gingerich 2009), the pragmatist ad hoc use of a third type (MME) may reflect the importance of state intervention in France, Southern Europe, and Latin America that resonates with criticism from other scholars about state-led capitalism that has evolved over recent years (Guillén 2001; Schmidt 1996).

The role of the state in promoting economic development and a more evolutionary concept of business development has been noted in South Korea and other Asian emerging economies (Carney et al. 2009). Whether finer distinctions need to be introduced to classify real variations and to what degree hybridization undermines the CME ideal type remains much debated in comparative political economy.

The VoC approach and parallel efforts by others has led to a cottage industry of comparative or historical case studies, indicating more complex variations and contingent paths than VoC suggests. Streeck and colleagues explored the historical commonalities and subtle differences of two “non-liberal” capitalisms, Germany and Japan, and their future prospects in a global economy (Streeck and Yamamura 2001; Yamamura and Streeck 2001). Although Germany and Japan had overcome their authoritarian past by a post-war economic miracle, they developed strong export-oriented economies with coordinated governance. Today, they need to adapt these non-liberal institutions, given global competition and political trends of liberalization. There was a more delayed reaction in Scandinavia as the former academic interest in the social-democratic welfare states and Nordic corporatist centralization eroded, while the studies on Nordic economies indicated rather hybrid models of export-oriented small economies (Campbell et al. 2006; Campbell and Pedersen 2007; Mjøset 2011).

More recent comparative studies aim beyond the usual VoC showcases and emphasize global changes, thus asking “where are national capitalisms now?” (Perraton and Clift 2004). Case studies on the long-term institutional change of capitalism across Europe show the rather contingent outcome of socio-political compromises (Jackson and Deeg 2012). With the transition to market economies and European Union membership since 2005 or later, the Central and Eastern European countries have gained attention, though these studies sought to develop more adapted concepts for the changing and hybrid institutional mixes (Bohle and Greskovits 2009, 2012; Myant and Drahokoupil 2010; Nölke and Vliegthart 2009). Further extensions on other Asian economies (Carney et al. 2009) and on the Global South, in particular Latin America (Schneider 2009) have pointed to the importance of different national trajectories, state developmental policies, and the role of foreign direct investment. The VoC conceptual scheme has thus been a starting point to develop more differentiated maps of institutional variation, showing the importance of historical contingencies, global economic location, and variant political conditions for capitalist development.

INSTITUTIONAL CHANGE AND POLITICS

The widespread reception of VoC and its influence on debates in political economy, political science, and economic sociology as well as other social sciences (education, geography, developmental studies) has also led to conceptual clarification and substantial empirical revisions. As a contemporary classic in public policy, several of these challenges to the original VoC volume are of particular relevance. First, there are

conceptual critiques from a historical institutionalist perspective about the VoC conceptualization of institutional change, which may assume too much path dependence and lock-in. Second, there is a debate in political science on whether and how political forces (partisan effects, class conflict, and state traditions) have shaped the evolution of VoC and political coalitions and power relations explain the institutional changes in the interrelated institutional spheres. Third, the recent financial and economic crisis has deepened the controversy over the fate of coordinated market economies subject to institutional changes resulting from intensified global economic pressures and transnational diffusion of liberalization.

The largest scope for debate and the most fundamental challenge has focused on VoC's arguably rather ahistorical, apolitical, and static-functionalist view of institutions (Blyth 2003; Howell 2003). VoC assumes a rather path-dependent and static equilibrium perspective of institutional complementarities (Crouch et al. 2005). At the time of publication in 2001, VoC scholars wanted to stress the stability of institutional diversity despite the popular and at that time omnipresent expectation of cross-national convergence due to deindustrialization, liberalization, and globalization (Kitschelt et al. 1999). Despite such pressures, the authors pointed out the lock-in effects of institutional complementarities and comparative advantages of given institutional infrastructures for continuously export-oriented and highly productive economies such as Germany and Japan. After publication of the path-breaking VoC volume, its editors themselves have addressed many criticisms (Hall and Soskice 2003) and in particular Hall has further discussed the need to study institutional change (Hall 2007; Hall and Thelen 2009).

Although the VoC approach considered itself to adopt a political economy perspective, the *political* constitution of market economies has been less at the center of scholarly attention. The editors noted that one "of the objects of the volume was to highlight the importance of organized capital, as well as organized labor, to the political economy" (Hall and Soskice 2003: 249). In contrast to power resource theory (Korpi 2006) applied to welfare regimes, most prominently by Esping-Andersen (1990), VoC did not systematically consider the strength and unity of organized labor and the electoral and governmental power of left political parties as paramount factors, but focused on the interests of employers shaped by institutional constraints (see also Culpepper 2010). Only the chapters on labor by Thelen (2001), labor policy by Wood (2001) and on social policy by Mares (2001) dealt in depth with political interests, while the Introduction remained relatively silent on the political forces that shaped the known varieties of capitalism. In recent years, scholars have developed more explicit considerations of VoC politics (Hancké et al. 2007), for instance, when explaining education and social policies for different skill formation regimes (Iversen and Stephens 2008).

From a historical institutionalist perspective, the VoC approach has been criticized for its static typology (Howell 2003) with its strong path-dependent, functional equilibrium assumption, and strong lock-in through institutional complementarities. Given VoC's assumption that institutional change would either be externally

induced or follow shifts in firms' interests along VoC's asset theory of preferences (Iversen and Soskice 2001), politics would not matter much. Importantly, Kathleen Thelen and Wolfgang Streeck (Streeck and Thelen 2005) provided an alternative view that stressed how institutions were the result of historical contingency and that gradual institutional change occurs even if politicians do not act (Streeck and Thelen 2005). Indeed, Hall (in collaboration with Thelen) has developed a more evolutionary perspective on institutional change, stressing that contestation over distributive results could lead to "defection, reinterpretation and reform" (Hall and Thelen 2009).

Many observers have pointed to multiple institutional changes of advanced capitalist economies propelled by globalization and liberalization. All market economies are seen to be subject to exogenously induced changes through global competition, financialization, and technological change. For instance, responding to external pressures set by globalization and Europeanization, German finance has changed substantially since the 1990s, but remodeled its stakeholder corporate governance structure to adapt to the demands of financial markets (Deeg 2005; Lütz 2005). The financial and economic crisis following the Lehman Brothers bankruptcy in 2008 implicated all economies and nation states face mounting public debt as a consequence of their bank bailouts, while their success in regulating banks and financial markets to prevent such crises in the future have thus far been limited.

Liberalization has been a politically advanced project of deregulation, privatization, and flexibilization, though there are differences across countries as to its scope and form (Hall and Thelen 2009). Nevertheless, critics of VoC have pointed to the effects of gradual institutional change in Germany, for instance, the crumbling corporatist pillars (Streeck and Hassel 2003), the erosion of collective bargaining (Hassel 2002) and the disintegration of corporate governance (Beyer and Höpner 2003) as a move toward a liberal model (Streeck 2009). In terms of flexibilization, policy-makers in conservative welfare states have opted for political reasons toward dualization, maintaining the rights of tenured employees while flexibilizing the employment conditions for labor-market outsiders (Emmenegger et al. 2012).

As Kathleen Thelen has pointed out, VoC, by focusing on coordination problems between economic actors, focused mainly on one of two dimensions in modern economies, while others have emphasized problems of social solidarity (Thelen 2012). For the politics of redistribution, the balance of power between capital and labor as well as between conservative and progressive political parties matters. Iversen and Soskice go even further in claiming that center-left governments redistribute more and that they are more likely to rule under proportional representation than under majoritarian systems (Iversen and Soskice 2006). In their view, capitalist diversity derives from different historical roots of electoral systems—or hard-wired politics. Hence, the politics of VoC may thus be related to other public policies, such as social protection and labor relations, in intricate ways (Ebbinghaus and Manow 2001). While it has been crucial to strengthen our understanding of firms' preferences in economic and social policies, we should also seek to analyze the political and social forces reshaping the economic

and social institutions upon which capitalism is built. The conditions for the survival of CMEs seem to depend not only on institutional changes needed to adapt to changing economic conditions, but also on the political consensus needed to legitimate the institutional changes necessary to maintain a distinct non-liberal path in CMEs. Whether such a model can survive global economic challenges and endogenous social and political transformations will have to be seen.

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