

Agenda Setting in Comparative Perspective

Frank R. Baumgartner

Agenda Setting, Policies, and Political Systems: A Comparative Approach. Edited by Christoffer Green-Pedersen and Stefaan Walgrave. Chicago: University of Chicago Press, 2014. 272p. \$85.00 cloth, \$27.50 paper.

It has been more than 50 years since the discipline was split by the difficulty of studying “power” and “influence.” Writers such as E. E. Schattschneider (1960) and Peter Bachrach and Morton Baratz (1962) brought our attention to the scope of conflict and those issues kept off the agenda as fundamental strategies of political control. Schattschneider wrote that the struggle over what to fight about was the most fundamental political conflict of all. Bachrach and Baratz noted that if political debate could be restricted only to those issues that were comparatively innocuous to those in power, we could have the appearance of democratic debate and pluralism inside an elitist structure.

Overall, our profession responded to these challenges by bifurcating. One group took the critique as an argument not to do large-scale empirical work on decision-making processes at all, essentially eschewing behavioralism and empiricism as a valid methodology. Another responded with narrower but more careful work designed to clearly answer some questions while leaving these bigger issues unexplored. Perhaps they were interesting questions, but they just could not be addressed in a scientifically valid manner, so better to focus more narrowly on what we could indeed accomplish. Mancur Olson (1965) was the most influential within one group, bringing interest-group studies away from the study of influence, policymaking, and lobbying and toward the more tractable question of who joins a group. Anthony Downs (1957) provided the model for an even larger group, reducing the political struggle to one played out on a strategic platform based on a single dimension of conflict relating to larger or smaller government.

One thing that disappeared in all of this was the study of what governments actually do. *Agenda Setting, Policies, and Political Systems* proposes a more complete political science, one that accepts the challenge of the critics of pluralism rather than flees from it as an insurmountable obstacle. In fact, its contributors argue that we cannot have a theory of politics without a theory of public policy.

Jacob Hacker and Paul Pierson (2014) have recently commented on the decline of the Downsian perspective and the rise of a new, policy-focused perspective within political science. Whereas the Downsian approach emphasizes elections and the role of parties in seeking election to office by positioning themselves strategically on an ideological left–right scale, “in the policy-focused approach, pride of place is given not to elections but to policies—to the exercise of government authority for particular purposes” (Hacker and Pierson 2014, 644). They argue that “the waning of the Downsian era has the potential to open up new opportunities for constructive engagement between electorally-oriented studies and those that place public policy and organized groups at the heart of analysis” (p. 656), and conclude their essay by stating: “We need to bring policy back in, not just to better understand what government does, but also to better understand why” (p. 657).

In the edited volume under review, Christoffer Green-Pedersen and Stefaan Walgrave, working with 27 collaborators and presenting chapters on 11 different countries, illustrate the potential of a policy-focused approach. The project is as much intellectual as it is methodological. Intellectually it is focused on integrating the study of public policy with the study of political conflict. Methodologically it is focused on a large empirical sweep that takes advantage of collaboration, openness, and shared data infrastructure, encouraging large scientific teams to work together and in a way that fosters replication and collaboration rather than in isolated research projects where each scholar must develop a novel theoretical approach as well as a distinctive (but necessarily smaller scale) set of evidence to test it. Further, the

Frank R. Baumgartner (fbaum@email.unc.edu) is the Richard J. Richardson Distinguished Professor of Political Science at The University of North Carolina at Chapel Hill.

method is completely open and should be useful to those who disagree with or are uninterested in the intellectual agenda shared by most members of the research network.

Each of the 11 country chapters is based on databases collected from official documents or other authoritative sources and made freely available over the Internet. There is or should be no sense of property associated with the data sets, though collectively more than a million euros have been spent in their compilation. Soon, a single Website will allow not only their download but interactive analyses as well. Additionally, international comparability is a fundamental driving force, and all data collection has been done according to a common methodology. (That is, what is called water pollution in one country cannot be called infrastructure development in another; the data are comparable not only within country but across countries as well.) Third, the databases are comprehensive rather than focused, as has been common in the literature until now, on any single policy domain. A data set on oral questions in parliament will contain all such questions; one on executive speeches will have every speech. Nothing is limited by policy domain, encouraging encompassing tests of theories rather than a focus on welfare state development, health, the environment, energy, defense, immigration, or any single issue area. Fourth, the databases encourage a dynamic approach to the study of the development of public policies over time by covering many decades of recent political history. (The exact time coverage depends on the country but sometimes goes back many decades, not just a few.) To say the least, the participants in this research collaboration have huge ambitions. The goal is to change how we study public policy, both domestically and comparatively. It is to lay a foundation of easily available data sets so that scholars will use them as a starting point, leading to larger, richer, and empirically more ambitious studies of public policy. As such, this volume only scratches the surface of what the comparative policy agendas network hopes to make possible.

As a graduate student at the University of Michigan, it was clear to me that Phil Converse, Warren Miller, and others interested in elections, voting, and public attitudes never suffered from the public availability of the National Election Study. Like a big telescope for astronomers, the availability of that data resource made possible the work of hundreds of political scientists. When the data sets became completely public, so that the principal investigators no longer had any privileged access to them before they were released to others, there was little effect on the central position that Miller and his colleagues had within a huge and growing research network. By making these data resources available to the general community, large national surveys also drew scholars to the study of elections, voting, and attitudes; it was free data, and of the highest quality. So, rather than suffer from the free

availability of the data sets that they struggled to compile, the leaders of the early survey research movement became leaders in a disciplinary shift that lasted decades.

Green-Pedersen, Walgrave, and those involved in this volume are part of the Comparative Agendas Project (CAP) (<http://www.comparativeagendas.net>), extending much of the work of the U.S. Policy Agendas Project (<http://www.policyagendas.org>) for other political systems. (Full disclosure: I was one of the creators of the U.S. project and have been closely involved in CAP as well, though I did not contribute to the volume being reviewed here.) How does this volume illustrate the potential of the agendas approach, and what are its limitations? Can it change how we study public policy?

Chapter 1, by the editors, introduces the project and the volume, beginning in the very first sentence with citations to Bachrach and Baratz (1962) and Schattschneider (1960). Clearly, the intellectual goal here is to return to some lost roots in the discipline: a policy-centered focus. They note that policy attention is scarce and that tracing attention to various policy priorities over time is therefore a potent way to assess not only policy but also politics more generally. Further, they argue, we can gain new insights by looking at information, preferences, and institutions. So the volume certainly has no aim to ignore the Downsian focus on preferences, but, as Hacker and Pierson recommend, to generate a dialogue about the relative impact of preferences on political processes and outcomes. The introductory chapter explains the nature of the agendas project, the approach of which it is a part, and the structure of the volume.

The chapters focused on a single country are linked together only by the fact that all use a similar methodological approach; each looks at a different element of policy dynamics and concentrates on a narrow research question rather than giving an overview of the entire data-rich project. The volume has no cross-national chapters, but seeks rather to illustrate how the agendas approach can help explain domestic politics in each country analyzed. This work is a scratch on the surface of the comparative agendas project, not its final word.

Six chapters make up Part I, on the impact of parties and elections on policy priorities. Peter John, Shaun Bevan, and Will Jennings are concerned with party effects in the issue priorities of UK governments, looking at the period of 1946 through 2008, in particular at Speeches from the Throne and Acts of Parliament; their systematic comparison of Labour versus Conservative rules shows significant differences in attention in about one-third of the policy domains evaluated. Bryan Jones and Michelle Whyman develop measures of lawmaking priorities in the United States from 1948 through 2010, similarly showing that lawmaking intensity is driven by a general downward trend unreflective of partisan control; that more important lawmaking activities are indeed affected

by partisan control and public opinion; and that the topics of policymaking activity cannot be explained by partisan control but, rather, reflect longer-term trends such as government growth, international events, and technological change.

Chapter 4, by Sylvain Brouard, Emiliano Grossman, and Isabelle Guinaudeau, assesses the issue priorities of the major (and some smaller) French political parties from 1981 through 2007, focusing on the parties' electoral manifestos, and showing that the parties compete not only by their positions on issues but also by their differential emphases on one or another topic. Further, the mainstream parties "take up" the issues of the greens, far-right, and other "niche" parties in order to seek electoral advantage.

Denmark is the focus of Green-Pedersen in Chapter 5; he documents the transformation of the Danish political agenda from 1953 to 2006 as a greater variety of issues emerge in a steady trend that shows little to no impact of elections or partisan control of government. However, he notes that while the substantive focus of Danish politics has been transformed over two generations, the partisan cleavage structure has not been transformed. Thus, he suggests that the rise of new issues tied, for example, to globalization need not change the domestic party system; incumbents can incorporate these shifts, and a stable cleavage structure may be maintained even as the issues that originally defined that structure are replaced by others.

Arco Timmermans and Gerard Breeman analyze party coalition agreements in the Netherlands from 1963 through 2010 in Chapter 6, comparing these lengthy and heavily negotiated documents that determine the priorities and limits of the multiparty coalition governments that emerge from Dutch elections with the content of the bills that are introduced in the subsequent legislature. They find a moderate but imperfect correspondence between stated plans and actual legislative work, and a cyclical pattern of greater attention to the initial agreement the longer a coalition government lasts; they also show that some policies generate more lawmaking activities than attention in the agreement, and vice versa. In sum, they assess the coalition agreement as a credible sign of government priorities and find a complicated and partial confirmation that the governments do, to some extent, feel constrained by what they promise each other on entering the Cabinet.

Frédéric Varone, Isabelle Engeli, Pascal Sciarini, and Roy Gava complete Part I with an evaluation of issue dynamics associated with the rise of the Swiss People's Party from 1979 through 2007 by assessing party manifestos, parliamentary motions, and popular initiatives. They show the impact of one new player in a multiparty system, looking at shifting agendas in three domains of politics and focusing on the dramatic changes that separate

the periods before and after 1991. The substantive topics of Swiss politics clearly shifted, and some of this shift can be said to be due to the actions of this newcomer.

If each of the chapters in the first part of the volume focuses on the possible effects of elections on issue priorities, Part II, consisting of five chapters, calls attention to the links between issue priorities and institutional change. Christian Breunig looks at legislative agendas in Germany; Walgrave, Brandon Zicha, Anne Hardy, Jeroen Joly, and Tobias Van Assche concentrate on Belgian political parties; Enrico Borghetto, Marcello Carammia, and Francesco Zucchini on the lawmaking agenda in "First" and "Second" Republic Italy (1983–2006); Laura Chaquès-Bonafont, Anna Palau, and Luz Muñoz Marquez on the links between policy promises and legislative activities by Spanish governments from 1982 to 2008; and Martial Foucault and Eric Montpetit on the diffusion of ideas from the Canadian government to and from the provinces through an analysis of speeches from the throne from 1960 to 2008.

The editors attempt to summarize this rich collection of diverse single-country studies in Chapter 13, focusing on the issue of "What It Takes to Turn Agenda Setting from an Approach to a Theory." Reviewing the first six empirical chapters, they assess the impact of traditional party-related variables in explaining the content of the various agendas studied, summarizing each chapter in a simple table. Effects are "small," "hardly any effect," "weak," "probably the other way around," and in just one case "considerable." Such a set of minimal-effects findings

challenges the core idea of party government and mandate politics. It also challenges the idea that issue competition among political parties is just a matter of parties focusing on their preferred issues or the issues they own. This has been the typical point of departure for the idea of party competition. . . . In terms of issues addressed by government, the party in power does not seem to make much difference. . . . [Governments] are constrained both because their competitors also want their preferred issues to dominate the agenda and because real-world developments matter. (p. 226)

Turning to the second part of the volume, Green-Pedersen and Walgrave similarly note that institutional design or changes in institutional structures do not determine issue priorities: "Together with the above observation that actor preferences are only part of the story of issue priorities, these results suggest, again, that changing real-world circumstances are crucial drivers of political change, especially in the long term. Institutions and preferences are often stable, but even when institutions change they do not affect to a defining extent the issues a political system processes. The moving target of politics is real-world problems" (pp. 227–28). They add that "[f]indings about devolution in Belgium, reunification in Germany, major landmark elections in Denmark, party government in the UK Westminster system, the

limited success of the Swiss People's Party, change in the Italian electoral system, the absence of issue ownership in France, and the like all contradict conventional wisdom about those countries" (p. 228).

Why is this approach so contrarian? The editors go back to Bachrach and Baratz (1962) for the distinction between the first and second face of power. In the agendas project, the focus is on what is being discussed, an operationalization of the second face of power. Before debating how immigration should be handled in a given country, immigration itself must be discussed. By looking at what is being discussed, the authors in this network assess the second face of power, and with great consistency they find that factors widely found to contribute to the first face of power (decision making) are surprisingly unhelpful in understanding the second face (agenda setting). The editors conclude that for this research paradigm to move from being an approach to the study of politics to proposing a clear theory of political change, attention and preferences will both have to be integrated.

Political science previously considered the possibility of integrating the second face of power, and most rejected the idea. Rather, empirical, theory-focused, and statistically oriented scholars moved with the behavioral revolution and formal theory to focus on that which could be measured, decision making, and how institutional structures influence outcomes. Interpretive scholars used Bachrach and Baratz's insights to suggest that decision-making studies are trivial, as they miss the larger point. (Or see Gaventa 1980 on the "third face" of power: a Gramscian sense of paradigmatic acceptance or "hegemony.")

The authors in this edited volume are betting that political science, with big data, can square the agenda-setting and decision-making circle. It is a big bet. Here, they have laid down a set of puzzles, challenges, maybe embarrassments. In Thomas Kuhn's terms, they have noted an awful lot of anomalies. There are two responses to these anomalies. One, perhaps the most common, and certainly the easiest, is to say that they are irrelevant because theories of decision making have never been designed to shed light on agenda setting. As the authors here are studying issue priorities, not policy outcomes or goals, the findings they reach are simply of no interest to those with a concern about decision making or outcomes. This is fair enough as far as it goes, but it comes at a heavy price if also followed by others. The more difficult approach is to take up the challenge. If we all agree that government actions are worth explaining, then we need an integrated theory of how governments set their agendas, and then how they make decisions once the agendas are set. The collection of studies here make clear that variables widely found to help in the second part of that process seem surprisingly unrelated to the first stage in the process.

Should political science integrate agendas into decision making? Or is it an impossible task best left aside in the

interest of tractability? Hugh Hecló (1974) suggested that policymakers inside and outside of government were engaged not only in self-interested pushing and pulling in order to gain more benefits, but also in a more cerebral "collective puzzlement on society's behalf." Consistently in studies of agenda setting, scholars find that governments, like it or not, are forced to "puzzle" over issues they might prefer to avoid. What forces them? Opposition parties, journalists, real-world facts and developments, the shooting of a police officer or unarmed motorist, the arrival of a hurricane, the action of a rival political leader, the statement of an interest-group leader: the impetus can come from many sources. Of course, none of these sources necessarily drives government attention, but a view of political change and decision making that treats the first stage of the process (paying attention to this, rather than to that) as a given cannot possibly help us understand how decisions are made. Now, at the same time, an exclusive focus on attention will not generate much insight into how decisions are made, once attention is focused. Green-Pedersen and Walgrave conclude with a call for integration and note that neither approach to politics can be a true theory of politics without integration of the other. A big challenge is laid down here, and a big opportunity. We can all hope that the next generation of political scientists will accept the challenge that two previous generations have rejected as intractable or impossible.

One thing that should be a consequence of the creation of these databases is that where they can be used, there will be little excuse for small-scale tests of theories. We can begin to demand that theories of policy change, election effects, or executive power be tested across multiple policy domains, multiple countries, and across long time horizons. If a theory is useful in one domain, let us shift focus to finding its limits and treat that limitation as variance to be explained, building a more general theory to encompass the conditions under which various existing theories or approaches make sense, and where they do not. If collectively we take seriously the idea that generalizations should be in proportion to the scope of one's evidence rather than inversely proportionate to the strictness of one's assumptions, we will all benefit. So the work illustrated here is a large step in creating a new generation of studies that will take measurement, sampling, and generalization as seriously as theory building.

The databases that are the common elements of the policy agendas project will not by themselves change much of anything. It depends on how they are used, what is added to them by individual scholars who use them as a starting point, and what theories they are used to test or topics to explore. Framing, counterframing, lobbying, social movements, and the actions of individual political leaders are all elements that are not systematically measured in the CAP. But they can be added to the

baseline of information that the CAP provides, allowing studies that start from a much larger empirical base than previously would have been possible.

The project has the potential to draw us back to the core issues of political science posed by Bachrach and Baratz and Schattschneider. The data sets that make up the CAP cannot solve the inherent problems of observing influence, but they can push us a long way. Merely by multiplying the number of issues than can easily be studied and the number of countries where we can study them, and by lengthening the time frame of these studies, we are sure to see more, never less, variance on key dimensions of discussion. And they may reveal a paradox not recognized by Bachrach and Baratz, who wrote of agenda denial as an active strategy or one based on expected reactions. This is undoubtedly true. But a more important reason for the lack of consideration of most policy proposals is not an Oz-like process by which unseen elites keep issues from public debate. Rather, it is the crush of other issues, the never-ending stream running like a torrent over the political system: more social problems than we have time to confront.

We are only at the beginnings of understanding how issues rise to the top of the agenda, and with what consequence. The number of puzzles and unanswered questions listed in the conclusions to the chapters in this volume illustrates the state of this new literature. The authors lay bare many puzzles that become apparent from a new approach, but we are far from proposing, yet, a full explanation of why governments pay attention to X rather than to Y. These essays do show that this question matters, and they invite the profession's collective

puzzlement about how we can combine the approach illustrated here with other approaches already strongly represented throughout the profession in order to move to the next level of complexity. Rather than take the agenda as a given, and explain choice, let us take Schattschneider and Bachrach and Baratz seriously and attack this more difficult question of where the agenda comes from in the first place, then moving on to decision making and finally assessing, in a single framework, how policy choices are made. This is a research agenda for an entire generation.

References

- Bachrach, Peter and Morton Baratz. 1962. "The Two Faces of Power." *American Political Science Review* 56: 947–52.
- Downs, Anthony. 1957. *An Economic Theory of Democracy*. New York: Harper.
- Gaventa, John. 1980. *Power and Powerlessness: Quiescence and Rebellion in an Appalachian Valley*. Urbana: University of Illinois Press.
- Hacker, Jacob S. and Paul Pierson. 2014. "After the 'Master Theory': Downs, Schattschneider, and the Rebirth of Policy-Focused Analysis." *Perspectives on Politics* 12(3): 643–62.
- Hecl, Hugh. 1974. *Modern Social Policies in Britain and Sweden: From Relief to Income Maintenance*. New Haven: Yale University Press.
- Olson, Mancur Jr. 1965. *The Logic of Collective Action*. Cambridge, MA: Harvard University Press.
- Schattschneider, E. E. 1960. *The Semi-Sovereign People*. New York: Holt, Rinehart and Winston.