30th Anniversary Essay

Creating an Infrastructure for Comparative Policy Analysis

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The study of comparative public policy has changed dramatically in the 30 years since *Governance* has been in operation, and the journal has played an important role in fostering these shifts. While the next generation will hopefully see continued progress, in this short reflection I want to draw attention to some of the important steps that we have made toward the creation of a truly integrated literature on comparative public policy, in particular as it relates to studies of the policy process, my own area of research. While we should not be complacent about where we stand—many unresolved puzzles remain—neither should we underestimate our progress. And we should recognize the importance of this particular journal in helping to create it. To my mind, it has helped eliminate many artificial or epiphenomenal sources of difference, so that we can focus on the more important areas: theoretical difference in what explains public policy.

The Old Days

My first involvement with the journal was before it had published its first issue, as many of my mentors were involved in its creation. Joel Aberbach, Bert Rockman, John Creighton Campbell, and Jack L. Walker, Jr., were directly involved or else just one step removed. They were part of the intellectual movement that generated the IPSA Research Committee on the Structure and Organization of Government (SOG), as described in the recent essay by fellow travelers Guy Peters and Colin Campbell (2016). Volume 2, issue 1 of the new journal consisted of a special issue on "policy communities as global phenomena" organized by John Creighton Campbell and with an introduction by Jack L. Walker, Jr. (see Baskin 1989; Baumgartner 1989; Campbell 1989; Campbell et al. 1989; Halpern 1989; Walker 1989). We collectively explored the concept of policy communities, suggesting that this was a fruitful analytical framework for comparative research on the policy process, with contributions focusing on the United States, France, Japan, Yugoslavia, and China.

Several things made *Governance* the logical place for our work, in particular in retrospect. First and most importantly, other outlets were not necessarily interested in a global analysis of "the crucial relationship between knowledge and power" (Walker 1989, 4), particularly one that put such diverse political systems as we included in our collected essays. While Hugh Heclo's work (1974, 1978) deeply affected us and others (see, e.g., Hall 1993, 1986, or discussion in Baumgartner 2013), it was more common in those days to explore the peculiarities, not the similarities, of bureaucratic systems. Certainly there were few attempts to look at the bureaucratic or policymaking process

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within Chinese or other communist systems and suggest there were similarities to how things work in Western democracies.

Even more than being divided between those studying Western countries and others, each national community of scholars had developed its own set of compelling research questions (or questions that seemed compelling to them, in any case). For example, French scholars focused (with good reason) on the particular training systems, social recruitment, and powers of the elite civil service *grands corps de l'État* (e.g., see Suleiman 1974). Perhaps the dominant way of discussing state–group relations, or policymaking in general, was the "pluralism–corporatism" debate, suggesting that each political system could be characterized by a national style of group–state relations, and that these established patterns determined the structure of the policy process in each state (see Lijphart 1968, 1969; Richardson 1982; Schmitter 1974, Schmitter and Lehmbruch 1979). Woe to the young scholar who might suggest that there are more commonalities than differences, that in each political system there must be a mechanism for the translation of knowledge to power, for the crucial role of bureaucratic elites in this process, for the informal mechanisms by which those "inside and around government" (see Kingdon 1984) with expert knowledge about the details of this or that public policy communicate with each other. The style of research more common at the time emphasized the peculiarities of each national style of policymaking, and we learned much from these works, of course. But each nation was its own intellectual island.

If geography constituted one dimension of limited generalizability, issue focus was another. Even those who were explicitly comparative in their research (i.e., looking at multiple countries) most often were focused on just one policy domain. And a large percentage of the comparative policy work that was done focused on just a few prominent areas central to the "welfare state." For example, a number of comparative scholars focused on the differential development of the welfare state in various countries, on the consequences of the different scales of the welfare state systems, or their different organizational structures. (For a smattering of such studies see Arts and Gelissen 2010; Esping-Andersen 1990; Hall and Soskice 2001; Katzenstein 1985). Similarly, important work was often done comparing single issues across different countries, as with Wilsford's (1991) study of the medical profession in two states, Immergut's broader study of health-care systems (1992), or such areas as the comparative study of pension systems (see Immergut, Anderson, and Schulze, 2007); of course this list could go on as there are many domain-specific comparisons or entire research communities surrounding immigration, energy, heath, transport, and other policies too numerous to mention.

The field of public policy within the U.S. academic discipline was also deeply divided by the tendency of scholars to invest in the "local knowledge" necessary to be a true expert on any given public policy. It makes no sense, however, to have a theory of energy policy that cannot apply to transport, with still a third needed for immigration or drug control (see Baumgartner and Jones 1993, Appendix B; 2015; Jones and Baumgartner 2005). While there is much to be learned from the deep knowledge of the development and histories of individual public policies in particular countries, those who were present at the creation of *Governance* were seeking a different level of theory building and conceptualization.

The search for common patterns, as opposed to intellectual silos buttressed by particularistic communities of scholars each focused on a different set of policy questions taken for granted within their own systems (be these national or policy specific within a country), but untested out of it, was perhaps what *Governance* represented more than anything. Similar to its parent organization, the SOG, those involved in the

journal were fundamentally interested in going outside their own national comfort zones to explore international patterns and to explain why the policy process, or policy outcomes, differed, and to what effect. Within each national political science community, perhaps nowhere more so than in the United States, attention stopped at the water's edge. SOG was the home for those seeking greater generalization.

Whereas the U.S. literature on policy communities had developed with reference to the "cozy relations" between individuals outside of government and those with control of institutional levers inside of government (see, e.g., Bentley 1908; Cater 1964; Freeman 1955; Griffith 1939; Lowi 1964, 1969), this later developed into Heclo's (1978) concept of an issue network. By this point, a new literature was developing pointing to the similarities in certain elements of the policy process, emphasizing the informal norms and immutable factors of human relations that might be in common across systems (such as the difference between policy experts—those with detailed or technical knowledge—and political leaders), and the common dynamics in their relations no matter what formal institutions might structure their relations. So, for example, Aberbach, Putnam, and Rockman (1981) surveyed high civil servants as well as elected officials in many countries, finding important differences to be sure but also many common elements in the nature of their interactions. The key innovation here was to ask common questions and to implement cross-national research projects that would allow (but not force) commonalities to be discovered. And if there were differences, perhaps they could be explained with reference to systematic patterns rather than merely enumerated as a reminder of how peculiar a given country or policy area might be.

It seems quaint to suggest that the search for theoretically driven difference should motivate our research projects. But *Governance* was created to help foster and promote those types of research projects that did just this. It filled an important gap in the literature. In the next section I use some of my own experiences to illustrate this point.

From the PAP to the CAP

When Bryan Jones and I completed *Agendas and Instability* (Baumgartner and Jones 1993), we felt satisfied with an important book, but interested to know whether what we had found through our longitudinal studies of nine different public policies would generalize to other policy domains or could be the basis of a general theory of U.S. politics or politics more generally. In an appendix to that book we explored some of the dangers of statistical modeling of the time-series dynamics associated with agenda dynamics and policy change: a model that describes well the dynamics for domain A is virtually useless when applied in domain B. To explore this puzzle, we decided to embark on a huge data collection project, the Policy Agendas Project, or PAP (www.policyagendas.org). Today, this project makes available over a million observations relating to the policy objects of attention in the U.S. government from 1947 to somewhere near the current year. (Unfortunately, we are never quite up to date.) From these efforts, a number of interesting patterns have emerged, which we have explored in various books (most recently Baumgartner and Jones 2015).

We think we have discovered some important commonalities across all areas of U.S. politics. However, in creating our database, we have also had another goal: to create an infrastructure. That is, by generating the databases associated with the PAP, or now the CAP, we have sought to offer a "subsidy" to our fellow policy scholars who may have no interest in following our particular theoretical interests. Any scholar who wants to trace the history of U.S. government attention to any particular topic might do well to start with a perusal of our Web site, since it shows, after a few

mouse clicks, the trace of hearings, bills, executive orders, or other factors across any or all the topics of public policy where the U.S. government has been active. No matter what the topic, we can easily see when it became the object of significant government concern; while our interest has been in general patterns, the site is more often used for case studies. The "open access" nature of our policy databases, and our desire to encourage those who might be from different intellectual traditions to use them, comes from our hope that more progress would be possible in the literature if each boat did not rest on its own bottom. That is, if we have differences, but our studies are founded in the same empirical points of reference, then at least we will know that our differences are real. But in the past too often it seemed that those following different intellectual traditions also used a completely different vocabulary. While we certainly have not solved the vocabulary problem, the wide availability and use of our databases allow a set of common empirical reference points.

The U.S. project has continued to expand over time and into more and more areas of U.S. government activity. While we started with a focus on Congress, looking at hearings and laws, we have now made considerable progress in looking at the judiciary and the executive branches, with the help of many collaborators. But the most important extensions to the PAP have been overseas. The Comparative Agendas Project, or CAP (www.comparativeagendas.net), is more of a network or a set of franchised operations than it is a single project.

With generous support from many national science agencies as well as the European Science Foundation, this network has created databases reflecting government activities across all areas of public policy in over a dozen different countries, with the list growing each year. Our databases share a common method of classifying the topics of public policy activity (so that a Spanish Parliamentary question about fisheries production is coded the same as a similar question in Denmark, Hungary, Brazil, Hong Kong, or Canada), but make use of nation-specific indicators of government activity and cover different time periods in each country. The first goal, however, has been achieved: to create a common set of databases reflecting the most important indicators of government activities (laws, parliamentary activities, executive branch actions), media attention, and budgetary allocations over long periods of time for a wide variety of countries. With a common topic codebook, we can quickly and easily assess how much attention a given country has paid to a given policy issue, and we can compare across countries, across issues, across time, or across the institutions of public policy.

The CAP does not represent the end of any single research project; rather, it should represent the beginning. Like a large-scale telescope made possible by the collaboration of many national efforts, it enables a scholar to study a new topic previously inaccessible. It does not tell the scholar what theoretical perspective to adopt or what question to ask. But it does provide data that can provide the first few steps in the research process. Our goal is that this will encourage a new, more ambitious, and large-scale-type of comparison of public policy.

Our experience in helping to encourage the growth of the CAP has illustrated the vast differences that have affected the field of comparative public policy in the years since *Governance* has been in operation. To create the CAP would have been impossible in the 1980s. In doing it over the past 10 years, however, we found a set of scholars trained in many countries outside the United States who were conversant with the same methodological traditions that we knew. We found a tremendous desire to be integrated into a single international network of scholars, not to be isolated into a national political science community. We found that the results of many years of ECPR and EU policies had truly created a pan-European and certainly a

cross-national research tradition in every country, including the larger countries such as Germany, France, and Spain, which previously had been home to such large national political science communities that there had been less need, desire, or experience with international and systematic cross-national comparison. Scholars from Asia, Latin America, the Middle East, former Soviet states in eastern, and central Europe have joined in. So the community of scholars associated with the CAP, like that associated with *Governance*, is trending toward the global, rather than focusing only on a narrow segment of advanced industrial countries. As we think of the purpose of theory testing and generalization, these trends could not be more welcome.

Governance emerged from a research group of the IPSA at a time when the substantive topics of research, methodological traditions and approaches, publication outlets, and theoretical perspectives most common, say, within the American Political Science Association, the British Political Science Association, the French AFSP (*Association Française de Science Politique*), or in other national conventions (or in their associated professional journals) were distinct. While there was some overlap, each national group had a powerful identity, and that identity carried over into forms of training, theoretical questions, the most prominent touchstones in the literature, and other elements of scholarship. Today, we see a completely different world. The APSA is no longer just American; indeed, the editors of its flagship journal, the *American Political Science Review*, are based in Europe as of January 2017. The ECPR is not only European, and the national organizations are no longer so focused only on their own country's politics, or their own country's political science. *Governance* has been a large part of this. The ECPR and the European Science Foundation have probably done more than any other set of institutions to encourage, if not mandate, that research questions in political science, and research teams seeking funding or recognition, address issues that bring together, rather than keep apart, scholars of various countries. With these changes, intellectual traditions have been revised as well. To an extent unimagined at the founding of *Governance*, there is something much closer to a single community of scholars looking at public policy and public administration through common theoretical perspectives today. Our creation of the CAP has been a product of these changes; it could not have happened without them.

From Division to Commonality

Thirty years in, those who helped create and nuture *Governance* can take some credit for the transition of a field that was once distinguished by considerable division into one that now can claim a significant amount of commonality. Those divisions were national, paradigmatic, based on policy domains, and often simply idiosyncratic. Today, some of these divisions remain (perhaps most strongly by policy domain). But the strong paradigmatic differences and distinct theoretical and empirical traditions that separated policy scholars working in different national contexts have been significantly chipped away. Today we see no theoretical utopia, to be sure, but still we can recognize much more of a common focus on understanding the basic commonalities of the policy process and administrative behavior. We collectively are much more often engaged in exploring the reasons for difference as well as the impact of different institutional, cultural, or political factors on important policy outcomes. The SOG and *Governance* have played an important role in fostering a common language and encouraging a broad, diverse, international, and leaderless network of scholars to coordinate their efforts and energies on a set of ideas that can be studied in common, breaking down the substantial barriers that previously generated silos of intellectual

knowledge where the first priority was always to focus on the peculiarities of one's geographical or domain-specific focus of inquiry.

Some of this agenda remains aspirational, we must admit. But *Governance* has been at the center of the substantial progress that has been made in creating a truly integrated cross-national community of scholars. The CAP has been an explicit effort by a large network of scholars across many countries to create an infrastructure for the systematic comparisons of public policy outcomes, processes, and institutional relations. By itself, it cannot succeed. We hope, however, that it can be a catalyst for a highly productive new generation of scholarship. *Governance* has done the same over the first 30 years of publication.

What is the unfinished agenda? The most important progress that has been made in the past is the creation of an integrated intellectual community across national borders. There remain, of course, many different intellectual traditions, as the study of comparative public policy has no single paradigmatic theoretical tradition that guides all of our work. This may or may not arrive in the distant future; certainly it is not on the horizon. Without a single paradigm, scholars inevitably will be asking different questions and investigating them following different research traditions. However, we might distinguish between theoretically driven differences and those that have simply accreted due to particular national or literature-based traditions and vocabularies. One is a short-term barrier to comparison, since those from different theoretical traditions may occasionally speak past each other. However, these theoretical differences may eventually be confronted with test cases and empirical facts that cannot be pushed aside, and one theoretical tradition may show its value over another, or new theories may arise perhaps combining features of the old ones. The other types of barriers, those coming from mere national or literature-based tradition, however, have no particular redeeming value. As we develop a literature ever more deeply integrated across different traditions, we will drop these artificial distinctions. And that will leave us with our true theoretical differences. At that point, our attention will be where it should be: on a limited number of fundamentally important issues of theoretical concern. The micro will have given way to the macro. And we will be better off for it.

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