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CHAPTER 16

EXECUTIVES—THE AMERICAN PRESIDENCY

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In the early 1980s, George Edwards took the presidency sub-field to task for its failure to adopt basic norms of social science. While scholars who contributed to the various other sub-fields of American politics constructed hard theory that furnished clear predictions that, in turn, were tested using original data-sets and the latest econometric techniques, too many presidency scholars, it seemed to Edwards, insisted on wading through a bog of anecdotes and poorly justified prescriptions. Unlike their would-be closest kin, congressional scholars, presidency scholars tended to prefer complexity to simplicity, nuance to generality, stories to data. Consequentially, Edwards noted, "Research on the presidency too often fails to meet the standards of contemporary political science, including the careful definition and measurement of concepts, the rigorous specification and testing of propositions, the employment of appropriate quantitative methods, and the use of empirical theory to develop hypotheses and explain findings" (Edwards 1983, 100). If the sub-field hoped to rejoin the rest of the discipline and enter the modern era of political science, it would need to nurture and reward scholars conducting quantitative research.

Edwards did not sit alone with such sentiments. In a damning report to the Ford Foundation, Hugh Heclo summarized the state of the presidency literature circa

1977 as follows: "Political observers have written excellent interpretations of the Presidency. Important questions about Presidential power have been raised. But considering the amount of such writing in relation to the base of original empirical research behind it, the field is as shallow as it is luxuriant. To a great extent, presidential studies have coasted on the reputations of a few rightfully respected classics on the presidency and on secondary literature and anecdotes produced by former participants" (Heclo 1977, 30). By recycling over and over again a handful of old chestnuts and witticisms, Heclo observed, scholars had failed to establish even the most basic empirical facts about the presidency.

In the years that followed, others delivered similar lamentations. According to Stephen Wayne, the presidency field languished for lack of clearly defined concepts and standards of measurement. As he put it, "By concentrating on personalities, on dramatic situations, and on controversial decisions and extraordinary events, students of the presidency have reduced the applicability of social science techniques" (Wayne 1983, 6). A decade later, Gary King bemoaned the fact that "Presidency research is one of the last bastions of historical, non-quantitative research in American politics" (King 1993, 388). And jumping yet another decade in time, Matthew Dickinson observed that "American presidency research is often described as the political science discipline's poor stepchild. Compared, for example, to election or congressional studies, presidency research is frequently deemed less clearly conceptualized, more qualitative and descriptive, overly focused on the personal at the expense of the institution, and too prone to prescribing reforms based on uncertain inferences" (Dickson 2004, 99).

Of course, not everyone agreed that more, and better, quantitative research constituted the solution to this dispiriting state of affairs. A variety of scholars made powerful cases for the value of legal analysis (Fisher 2002), carefully constructed case studies (Thomas 1983), and theoretically informed historical research (Skowronek 2002). And they plainly had cause to do so. Some of the best insights and most theoretically informed treatises on the American presidency come through biographical, historical, and case study research;[1] and there are many questions about the presidency that simply are not amenable to quantitative research. Hence, no one now, or then, could plausibly argue that quantitative research should wholly supplant any of the more qualitative modes of research.

Still, Edwards spoke for many when he recommended that presidency scholars direct greater investments towards more systematic data collection efforts and the development of statistical skills needed to conduct quantitative research. For the presidency sub-field to recover its rightful stature in the discipline, a genuine science of politics would need to take hold among presidency scholars; and to do so, clear, falsifiable theory and systematic data collection efforts would need to replace the subfield's preoccupation with personalities, case studies, reflective essays, and biographical accounts. Hence, by the early 1980s, one observer would later reflect, "observation, data collection, quantification, verification, conceptual clarification, hypothesis testing, and theory building [became] the order of the day" (Hart 1998, 383).

[1] Many of the most influential books ever written on the American presidency do not contain any quantitative analysis of any sort. Prominent examples include: Corwin 1948; Rossiter 1956; Barber 1972; Schlesinger 1973; Greenstein 1982; Neustadt 1990; Skowronek 1993.

This chapter surveys the state of quantitative research on the presidency a quarter-century after Edwards issued his original entreaty. After briefly documenting publication trends on quantitative research on the presidency in a variety of professional journals, it reviews the substantive contributions of selected quantitative studies to long-standing debates about the centralization of presidential authority, public appeals, and presidential policy-making. Though hardly an exhaustive account of all the quantitative work being conducted, this chapter pays particular attention to the ways in which recent scholarship addresses methodological issues that regularly plague studies of the organization of political institutions, their interactions with the public, and their influence in systems of separated powers.

1 Publication Trends on the American Presidency

Though numerous scholars have complained about the arrested state of quantitative research on the American presidency, none, ironically, has actually assembled the data needed to answer some basic empirical questions: What proportion of articles in the field journal for presidency scholars is quantitative in nature? Has this proportion increased or decreased over the past several decades? Are articles published in this journal more or less likely to contain a quantitative component than are articles on the presidency that are published in the top professional journals? And how does the literature on the presidency compare to that on other political institutions, notably Congress? This section provides answers to these questions.

In a survey of publication trends during the past several decades, I identified almost 500 articles on the American presidency published in prominent, mainstream American politics journals, as well as another 800 articles published in the flagship sub-field journal for presidency scholars.[2] Among articles on the

[2] I counted all articles with the words "presidency," "presidential," or "president" in the title or abstract and discussing the US president somewhere in its text; articles had to be published between 1980 and 2004 in the field journal for presidency scholars, *Presidential Studies Quarterly* (*PSQ*), or

American presidency, I then identified those that were quantitative in nature.[3] The differences could not be more striking. Whereas the top journals in American politics published almost exclusively quantitative articles on the American presidency, the field journal for presidency scholars published them only sporadically. In a typical year, the proportion of presidency articles published in mainstream outlets was nine times as high as the proportion of presidency articles published in the sub-field journal. And though some over-time trends are observed in these publication rates, in every year the differences across these various journals are both substantively and statistically significant. Nor are such differences simply a function of the publication trends of mainstream and sub-field journals. When writing for their respective sub-field journals, congressional scholars were seven times more likely to write articles with a quantitative component than were presidency scholars.

Who wrote the presidency articles that appeared in these various journals? For the most part, contributors came from very different circles. A very small percentage of scholars who contributed presidency articles to the top, mainstream journals also wrote for the sub-field journal; and an even smaller percentage of scholars who contributed to the sub-field journal also wrote for the mainstream journals. The following, however, may be the most disturbing fact about recent publication trends: of the 1,155 scholars who contributed research on the presidency to one of these journals during the past twenty-five years, only 51 published articles on the presidency in both the sub-field journal and the mainstream American politics journals.

Unavoidably, such comparisons raise all kinds of questions about the appropriate standards of academic excellence, the biases of review processes, and the value of methodological pluralism. For the moment, though, let us put aside the larger epistemological issues of whether the top journals in political science are right to primarily accept quantitative articles on the presidency; whether the sub-field journal for presidency scholars is right to provide a venue for research that does not follow these methodological orientations; or whether congressional scholars are right to incorporate these basic norms into the research that fills their own sub-field's journal. I cannot possibly settle such issues here. From the vantage point of a graduate student or young professor intent on assembling a record that will secure employment and tenure at a major research university, the more practical conclusions to draw from these data could not be clearer: if you intend to publish research on the American presidency in one of the field's top journals, you would do well to assemble and analyze data. Though purely theoretical essays and case study research may gain entrée into the presidency sub-field's premier journal, they appear to offer substantially fewer rewards in the discipline more generally.

If a sub-field's alienation from the broader discipline is appropriately measured by the regularity with which its scholars publish in both top mainstream journals and their chosen sub-field journal, then we have obvious cause for concern. For most of this period, few bridges could be found between the main publication outlet designated expressly for presidency scholars and the best journals in American politics. Indeed, if contributing to a sub-field's journal constitutes a prerequisite for membership, then the vast majority of scholars assembling the literature on the presidency in the top journals cannot, themselves, be considered presidency scholars. With some notable exceptions, meanwhile, those who can lay claim to the title of presidency scholar, at least by this criterion, do not appear to be contributing very much to the most influential journals in American politics.

one of the top three professional journals in American politics more generally: *American Political Science Review* (*APSR*), *American Journal of Political Science* (*AJPS*), and *Journal of Politics* (*JOP*). Excluded were: articles written by undergraduates, articles that were fewer than five manuscript pages (not including references) or that were submitted to symposia, transcripts of speeches, rejoinders, responses, research notes, comments, editorials, updates, corrections, and book reviews. In total, 799 articles meeting these criteria were published in *PSQ*, 155 in *APSR*, 165 in *AJPS*, and 160 in *JOP*. I gratefully acknowledge the research assistance of Ben Sedrish and Charlie Griffin.

[3] To count, an article had to subject actual data to some kind of statistical analysis, however rudimentary. Articles were identified as quantitative if they reported the results of any kind of regression, Bayesian inference, data reduction technique, natural or laboratory experiment, or even a simple statistical test of difference of means. Hence, an article that reported an occasional public opinion rating, or even one that tracked trends in public opinion in a figure or table, was excluded; however, an article that analyzed the determinants of public opinion, that tested for structural shifts in public opinion, or that decomposed measures of public opinion was appropriately counted as quantitative. Case studies, first-person narratives, and biographies, though certainly drawing upon empirical evidence, were not counted as quantitative; and neither were game theoretic models or simulations.

2 A Literature's Maturation

Not all the news is bad. For starters, a slight shift in the methodological underpinnings of presidency research can be observed. The proportion of quantitative work on the American presidency has increased rather notably of late.[4] And an increasingly wide spectrum of scholars is now contributing to the presidency sub-field's journal.[5] In both the mainstream and sub-field journals, there exists a

[4] Between 1980 and 1984, 30 percent of articles on the presidency published in the four journals examined in this chapter had a quantitative component; between 2000 and 2004, 46 percent did so. The percentage of quantitative articles published in *PSQ* alone since 2000, the first full year that George Edwards served as the journal's editor, nearly tripled.

[5] Of those scholars who wrote on the presidency in both mainstream and sub-field journals between 1980 and 2004, fully 65 percent contributed an article to *PSQ* during the first five years of Edwards' editorship.

considerably richer body of quantitative research on the American presidency than was available as little as a decade ago.

Obviously, disciplinary progress should not be measured only by reference to the number of articles amassed, no matter what their methodological tendencies might be. The mere addition of quantitative articles on the American presidency does not ensure that students today know anything more about the office than did their immediate or more distant predecessors. Fortunately, though, recent developments in the presidency literature provide additional cause for optimism. By attending to a host of standard problems of research design and causal inference, problems endemic to quantitative research throughout the social sciences, scholars have materially enhanced the quality of research conducted on the American presidency, just as they have gained fresh insights into the institution itself. This section reviews some of the ways in which scholars have grappled with a host of methodological challenges in order to make fresh contributions to ongoing debates about the political control over the bureaucracy, public appeals, and presidential power.

2.1 Political Control of the Bureaucracy

In a series of highly influential articles in the 1980s and early 1990s, Terry Moe spelled out a political rationale for presidents to politicize the appointment process and centralize authority within the Executive Office of the President (Moe 1985, 1987, 1990; Moe and Wilson 1994). Moe observed that in an increasingly volatile political world, one wherein opportunities to effect change are fleeting, power is always contested, and opposing factions stand mobilized at every turn, presidents and their immediate advisers have a strong incentive to hunker down, formulate policy themselves, and fill administrative agencies with people who can be counted on to do their bidding faithfully. Neutral competence and bureaucratic independence, Moe observed, does not always suit the president's political needs. Rather than rely upon the expertise of a distant cadre of civil servants, presidents, for reasons built into the design of a political system of separated powers, have considerable cause to surround themselves with individuals who are responsive, loyal, and like-minded.

By focusing explicitly on institutional incentives and resources, and by dispensing with the normative considerations that then pervaded much of the public administration work on bureaucratic design and oversight, Moe's research had a huge impact on the ways in which scholars thought about presidential power. The theory that Moe postulated, however, lacked the dynamic components needed to identify when, precisely, presidents would centralize or politicize authority and when they would not—that is, Moe's work did not generate any clear comparative statics. Moreover, Moe's empirical analysis resembled the existing literature's at the time. Evidence of centralization and politicization consisted of selected case studies of individual agencies and a handful of policies they helped write, and little else.

Fortunately, subsequent scholars picked up where Moe left off. Consider, for instance, Andrew Rudalevige's recent book, *Managing the President's Program* (2002).[6] Using the *Public Papers of President*, Rudalevige tabulated some 2,796 messages from the president to Congress on 6,926 proposals. He then drew a random sample of 400 proposals and examined their legislative "pre-histories." Specifically, Rudalevige identified whether each presidential proposal was the product of cabinet departments and/or executive agencies; of mixed White House/departmental origin, with department taking the lead role; of mixed White House/departmental origin, with White House taking the lead role; of centralized staff outside the White House Office, such as Office of Management and Budget or Council of Economic Advisors; or of staffers within the White House itself. So doing, Rudalevige constructed a unique data-set that allowed him systematically to investigate the regularity with which presidents centralized the policy-making process within the EOP.[7]

Notably, Rudalevige discovered that many of the proposals that presidents submit to Congress are formulated outside of the confines of his immediate control. Only 13 percent of the proposals Rudalevige examined originated in the White House itself; and just 11 percent more originated in the EOP. Cabinet departments and executive agencies drafted almost half of all the president's legislative proposals. Moreover, Rudalevige found, the occurrence of "centralization" did not appear to be increasing over time. Though the proportion of proposals that originated within the EOP fluctuated rather dramatically from year to year, the overall trend line remained basically flat for most of the postwar period. Rudalevige did not find any evidence that presidents were centralizing authority with rising frequency.

The real contribution of Rudalevige's book, however, lay in its exploration of the political forces that encouraged presidents to centralize. Positing a "contingent theory of centralization," Rudalevige identified the basic trade-off that all presidents face when constructing a legislative agenda: by relying upon their closest advisers and staff, they can be sure that policy will reflect their most important goals and principles; but when policy is especially complex, the costs of assembling the needed information to formulate policy can be astronomical. Though Moe correctly claimed that centralization can aid the president, Rudalevige cautioned that the strategy will only be employed for certain kinds of policies aimed at certain kinds of reforms.

[6] For other recent quantitative works that examine presidential control over the bureaucracy, see Wood and Waterman 1991; Waterman and Rouse 1999; Dickinson 2003; Lewis 2003.

[7] Testing various dimensions of Moe's claims about politicization, a growing quantitative literature also examines presidential appointments. See, for example, Cameron, Cover, et al. 1990; McCarty and Razaghian 1999; Binder and Maltzmann 2002.

To demonstrate as much, Rudalevige estimated a series of statistical models that predicted where within the executive branch presidents turned to formulate different policies. His findings are fascinating. Policies that involved multiple issues, that presented new policy innovations, and that required the reorganization of existing bureaucratic structures were more likely to be centralized; while those that involved complex issues were less likely to be. For the most part, the partisan leanings of an agency, divided government, and temporal indicators appeared unrelated to the location of policy formation. Whether presidents centralized, it would seem, varied from issue to issue, justifying Rudalevige's emphasis on "contingency."

Rudalevige's work makes two important contributions. First, and most obviously, he extends Moe's theoretical claims about the organizational structure of the executive branch. Rudalevige goes beyond recognizing that presidents have cause to centralize authority in order to explore the precise conditions under which presidents are most likely to do so. Though the microfoundations of his own theory need further refinement, and the statistical tests might better account for the fact that presidents decide where to formulate policy with a mind to whether the policy will actually be enacted, Rudalevige deftly shifts the debate onto even more productive ground from where Moe had left it.

Second, Rudalevige demonstrates how one might go about testing, using quantitative data, a theory that previously had strictly been the province of archival research. Before Rudalevige, no one had figured out how one might actually measure centralization, had determined what kinds of policies might be subject to centralization, or had identified and then collected data on the key determinants of centralization. No one, that is, had done the work needed to assemble an actual database that could be used to test Moe's claims. Plainly, future research on centralization will (and should) continue to rely upon case studies—there is much about centralization that Rudalevige's data cannot address. But residing in the background of Rudalevige's work is gentle encouragement to expand not only the number of data-sets assembled on the US presidency, but also the kind.

2.2 Public Appeals

In another influential book, *Going Public: New Strategies of Presidential Leadership*, Samuel Kernell (1997) recognized the rising propensity of presidents to bypass Congress and issue public appeals on behalf of their legislative agendas. To explain why presidents often abandon the softer, subtler tactics of negotiation and bargaining, the supposed mainstays of presidential influence during the modern

era (Neustadt 1990),[8] Kernell emphasized the transformation of the nation's polity, beginning in the early 1970s, from a system of "institutionalized" to "individualized" pluralism. Under institutional pluralism, Kernell explained, "political elites, and for the most part only elites, matter[ed]" (Kernell 1997, 12). Insulated from public opinion, presidents had only to negotiate with a handful of "protocoalition" leaders in Congress. But under the new individualized pluralist system, opportunities for bargaining dwindled. The devolution of power to subcommittees, the weakening of parties, and the profusion of interest groups greatly expanded the number of political actors with whom presidents would have to negotiate; and compounded with the rise of divided government, such developments made compromise virtually impossible. Facing an increasingly volatile and divisive political terrain, Kernell argued, presidents have clear incentives to circumvent formal political channels and speak directly to the people.

But just as Moe did not posit a theory that specified when presidents would (and would not) centralize authority, Kernell did not identify the precise conditions under which presidents would issue public appeals. Kernell offered powerful reasons why presidents in the 1980s and 1990s went public more often than their predecessors in the 1950s or 1960s. But his book did not generate especially strong expectations about whether presidents holding office during either of these periods would be more or less likely to issue public appeals on one issue versus another. Additionally, Kernell did not identify the precise conditions under which such appeals augment presidential influence, and when they do not.

During the last decade a number of scholars, very much including Kernell himself, have extended the analyses and insights found in *Going Public*. Two areas of research have been especially prodigious. The first examines how changes in the media environment, especially the rise of cable television, have complicated the president's efforts to reach his constituents (Groeling and Kernell 1998; Baum and Kernell 1999). Whereas presidents once could count on the few existing television networks to broadcast their public appeals to a broad cross-section of the American public, now they must navigate a highly competitive and diffuse media environment, one that caters to the individual interests of an increasingly fickle citizenry. Hence, while structural changes to the American polity in the 1970s may have encouraged presidents to go public with greater frequency, more recent changes to the media environment have limited the president's ability to rally the public behind a chosen cause.

It should not come as much of a surprise, then, that public appeals do not always change the content of public opinion, which constitutes the second body of quantitative research spawned by Kernell's work (Cohen 1998; Edwards 2003; Barrett 2004). Though it may raise the salience of particular issues, presidential

[8] With over a million copies sold, Neustadt's book remains far and away the most influential treatise on presidential power. And as does any classic, Neustadt's book has attracted a fair measure of controversy. For selected critiques, see Sperlich 1969; Moe 1993; Howell 2005.

speeches typically do not materially alter citizens' views about particular policies, especially those that involve domestic issues. Either because an increasingly narrow portion of the American public actually receives presidential appeals, or because these appeals are transmitted by an increasingly critical and politicized media, or both, presidential endorsements of specific policies fail to resonate broadly.

Brandice Canes-Wrone has also examined the conditions under which presidents will issue public appeals; and given its methodological innovations, her research warrants discussing at some length (Canes-Wrone 2001, 2005; Canes-Wrone and Shotts 2004). By increasing the salience of policies that already enjoy broad-based support, Canes-Wrone argues, plebiscitary presidents can pressure members of Congress to respond to the (otherwise latent) preferences of their constituents. Further recognizing the limited attention spans of average citizens and the diminishing returns of public appeals, Canes-Wrone argues that presidents will only go public when there are clear policy rewards associated with doing so. Then, by building a unique database that links presidential appeals to budgetary outlays over the past several decades, Canes-Wrone shows how such appeals, under specified conditions, augment presidential influence over public policy.

Two methodological features of Canes-Wrone's work deserve special note, as they address fundamental problems that scholars regularly confront when conducting quantitative research on the presidency. First, by comparing presidential budget proposals with final appropriations, Canes-Wrone introduces a novel metric that defines the proximity of final legislation with presidential preferences. This is no small feat. When conducting quantitative research, scholars often have a difficult time discerning presidential preferences, and an even more difficult time figuring out the extent to which different laws reflect these preferences. The challenge, though, does not negate the need. If scholars are to gauge presidential influence over the legislative process, they need some way of identifying just how well presidents have fared in a public policy debate.

Prior solutions to the problem—focusing on presidential proposals or accounting for what presidents say or do at the end of the legislative process—have clear limitations. Just because Congress enacts a presidential initiative does not mean that the final law looks anything like the original proposal made; and just because another law is enacted over a presidential veto does not mean that every provision of the bill represents an obvious defeat for the president. Moreover, even when such ambiguities can be resolved, it often remains unclear how observers would compare the "success" observed on one policy with the "success" claimed on another.

By measuring the differences between proposed and final appropriations, Canes-Wrone secures a readily interpreted basis for comparing relative presidential successes and failures across different policy domains. Now of course, the proposals that presidents themselves issue may be endogenous—that is, they are constructed

with some mind to how Congress is likely to respond—and hence not perfectly indicative of their sincere preferences. But for Canes-Wrone's analyses to yield biased results, presidents must adjust their proposals in anticipation of Congress's responses in different ways depending upon whether or not they issue public appeals. This is possible, perhaps, but the most likely scenario under which it is to occur would actually depress the probability that Canes-Wrone would find significant effects. If presidents systematically propose more extreme budgetary allotments when they plan to go public, anticipating a boost in public support from doing so, then Canes-Wrone may actually underestimate the influence garnered from public appeals.

Budgetary appropriations provide a second benefit as well. Because presidents must issue budget proposals every year, Canes-Wrone skirts many of the selection biases that often arise in quantitative studies of the legislative process. The problem is this: the sample of bills that presidents introduce and Congress subsequently votes on, which then become the focus of scholarly inquiry, are a subset of all bills that presidents might actually like to see enacted. And because presidents are unlikely to introduce bills that they know Congress will subsequently reject, the sample of roll calls that scholars analyze invariably constitutes a non-random draw from the president's legislative agenda.

Without accounting for those bills that presidents choose not to introduce, two kinds of biases emerge. First, when tracking congressional votes on presidential initiatives, scholars tend to overstate presidential success. Hence, because Congress never voted on the policy centerpiece of Bush's second term, social security reform, the president's failure to rally sufficient support to warrant formal consideration of the initiative did not count against him in the various success scores that *Congressional Quarterly* and other outlets assembled. And second, analyses of how public opinion, the state of the economy, the partisan composition of Congress, or any other factor influences presidential success may themselves be biased. Without explicitly modeling the selection process itself, estimates from regressions that posit presidential success, however measured, against a set of covariates are likely to be biased.

Unfortunately, no formal record exists of all the policies that presidents might like to enact, making it virtually impossible to diagnose, much less fix, the selection biases that emerge from most analyses of roll call votes. But because presidents must propose, and Congress must pass, a budget every year, Canes-Wrone avoids these sample selection problems. In her statistical analyses, Canes-Wrone does not need to model a selection stage because neither the president nor Congress has the option of tabling appropriations. Every year, the two branches square off against one another to settle the terms of a federal budget; and without the option to retreat, we, as observers, have a unique opportunity to call winners and losers fairly in the exchange.

2.3 Policy Influence beyond Legislation

Outside of elections and public opinion, the most common type of quantitative research conducted on the presidency has concerned the legislative process. Scholars have examined how different political alignments contributed to (or detracted from) the enactment of presidential initiatives (Wayne 1978; Edwards 1989; Bond and Fleisher 1990; Peterson 1990; Mayhew 1991; Edwards, Barrett, et al. 1997; Coleman 1999; Bond and Fleisher 2000; Howell, Adler, et al. 2000; Peake 2002). Following on from Aaron Wildavsky's famous claim that there exist two presidencies—one foreign, the other domestic—scholars have assembled a wide range of measures on presidential success in different policy domains (Wildavsky 1966; LeLoup and Shull 1979; Sigelman 1979; Edwards 1986; Fleisher and Bond 1988; Wildavsky 1989). Scholars have critically examined the president's capacity to set Congress's legislative agenda (Edwards and Wood 1999; Edwards and Barrett 2000). And a number of scholars have paid renewed attention to presidential vetoes (Cameron 1999; Gilmour 2002; Conley 2003; Cameron and McCarty 2004). Given the sheer amount of attention paid to the legislative process, one might justifiably conclude that policy influence depends almost entirely upon the president's capacity to influence affairs occurring within Congress, either by convincing members to vote on his behalf or by establishing roadblocks that halt the enactment of objectionable bills.

Recently, however, scholars have begun to take systematic account of the powers that presidents wield outside of the legislative arena. Building on the insights of legal scholars and political scientists who first recognized and wrote about the president's "unilateral" or "prerogative" powers (Cash 1963; Morgan 1970; Hebe 1972; Schlesinger 1973; Fleishman and Aufses 1976; Pious 1991), scholars recently have built well-defined theories of unilateral action and then assembled original data-sets of executive orders, executive agreements, proclamations, and other sorts of directives to test them. In the past several years, fully five books have focused exclusively on the president's unilateral powers (Mayer 2001; Cooper 2002; Howell 2003; Warber 2006; Shull forthcoming), complemented by a bevy of quantitative articles (Krause and Cohen 1997; Deering and Maltzman 1999; Mayer 1999; Krause and Cohen 2000; Howell and Lewis 2002; Mayer and Price 2002; Howell 2005; Lewis 2005; Martin 2005).

Collectively, the emerging quantitative literature on unilateral powers makes two main contributions to our substantive understanding of presidential power. First, and most obviously, it expands the scope of scholarly inquiry to account for the broader array of mechanisms that presidents utilize to influence the content of public policy. Rather than struggling to convince individual members of Congress to endorse publicly a bill and then cast sympathetic votes, presidents often can seize the initiative, issue new policies by fiat, and leave it to others to revise the new political landscape. Rather than dally at the margins of the policy-making process, presidents regularly issue directives that Congress, left to its own devices, would not enact. So doing, they manage to leave a plain, though too often ignored, imprint on the corpus of law.

Second, the literature highlights the ways in which adjoining branches of government effectively check presidential power. After all, should the president proceed without statutory or constitutional authority, the courts stand to overturn his actions, just as Congress can amend them, cut funding for their operations, or eliminate them outright. And in this regard, the president's relationship with Congress and the courts is very different from the one described in the existing quantitative literature on the legislative process. When unilateral powers are exercised, legislators, judges, and the president do not work cooperatively to effect meaningful policy change. Opportunities for change, in this instance, do not depend upon the willingness and capacity of different branches of government to coordinate with one another, as traditional models of bargaining would indicate. Instead, when presidents issue unilateral directives, they struggle to protect the integrity of orders given and to undermine the efforts of adjoining branches of government to amend or overturn actions already taken. Rather than being a potential boon to presidential success, Congress and the courts represent genuine threats. For presidents, the trick is to figure out when legislators and judges are likely to dismantle a unilateral action taken, when they are not, and then to seize upon those latter occasions to issue public policies that look quite different from those that would emerge in a purely legislative setting.

Some of the more innovative quantitative work conducted on unilateral powers highlights the differences between policies issued as laws versus executive orders. In his study of administrative design, for instance, David Lewis shows that modern agencies created through legislation tend to live longer than those created by executive decree (Lewis 2003). But what presidents lose in terms of longevity they tend to gain back in terms of control. By Lewis's calculations, between 1946 and 1997, fully 67 percent of administrative agencies created by executive order and 84 percent created by departmental order were placed either within the Executive Office of the President or the cabinet, as compared to only 57 percent of agencies created legislatively. Independent boards and commissions, which further dilute presidential control, governed only 13 percent of agencies created unilaterally, as compared to 44 percent of those created through legislation. And 40 percent of agencies created through legislation had some form of restrictions on the kinds of appointees presidents can make, as compared to only 8 percent of agencies created unilaterally.

In another study of the trade-offs between legislative and unilateral strategies, I show that the institutional configurations that promote the enactment of laws impede the production of executive orders, and vice versa (Howell 2003). Just as large and cohesive legislative majorities within Congress facilitate the enactment of legislation, they create disincentives for presidents to issue executive orders. Meanwhile, when gridlock prevails in Congress, presidents have strong incentives to deploy their unilateral powers, not least because their chance of building the coalitions needed to pass laws is relatively small. The trade-offs observed between unilateral and legislative policy-making are hardly coincidental, for ultimately, it is the checks that Congress and the courts place on the president that define his (someday her) capacity to change public policy by fiat.

Quantitative work on the president's unilateral powers is beginning to take systematic account for unilateral directives other than executive orders and departmental reorganizations—most importantly, perhaps, those regarding military operations conducted abroad. Presidency scholars have already poured considerable ink on matters involving war. Until recently, however, quantitative work on the subject resided exclusively in other fields within the discipline. Encouragingly, a number of presidency scholars have begun to test theories of unilateral powers and interbranch relations that have been developed within American politics using data-sets that were assembled within international relations (Howell and Pevehouse 2005, forthcoming; Kriner 2006; Shull forthcoming). Just as previous scholarship examined how different institutional configurations (divided government, the partisan composition of Congress) affected the number of executive orders issued in any given quarter or year, this research examines how such factors influence the number of military deployments that presidents initiate, the timing of these deployments, and their duration. Though still in its infancy, this research challenges presidency scholars to take an even more expansive view of presidential power, while also bridging long-needed connections with scholars in other fields who have much to say about how, and when, heads of state wield authority.

3 Concluding Thoughts

This very brief survey offers mixed assessments of the quantitative literature on the US presidency. On the one hand, the publication rates of quantitative presidency research have been rather dismal. In the last twenty-five years, only one in ten research articles published in the sub-field's premier journal had a quantitative component. By contrast, in the top American politics journals, almost nine in ten articles on the presidency did so. Additionally, the scholars who wrote about the presidency in top mainstream journals almost never contributed to the presidency sub-field's premier journal, while those who contributed to the sub-field's journal almost never wrote about the presidency in the top mainstream journals. Of the 1,000 plus authors who wrote about the American presidency in the four journals

surveyed in this chapter, a minuscule 4 percent contributed to both the mainstream and the sub-field outlets.

Signs, however, suggest that change is afoot. In the last several years, the presidency sub-field's journal has published a greater proportion of quantitative studies, written by a wider assortment of scholars. And the more recent quantitative work being conducted on the presidency makes a variety of substantive and methodological contributions to the sub-field. The literatures on bureaucratic control, public appeals, and unilateral policy-making have made considerable advances in the past several years in large part because of the efforts of scholars to assemble original data-sets and to test a variety of competing claims. On each of the topics considered here, quantitative analyses did considerably more than merely dress up the extant presidency literature—indeed, they stood at the core of the enterprise and constituted the key reason that learning occurred.

Moving forward, quantitative research on the US presidency confronts a number of challenges. Three, in my mind, stand out. First, much quantitative research on the presidency, as with quantitative research on political institutions generally, lacks strong theoretical footings. When conducting such research, scholars all too often proceed through the following three steps: (a) collect data on some outcome of interest, such as whether a proposal succeeds, a war is waged, an order is issued, or a public appeal is delivered; (b) haul out the standard list of covariates (public opinion, divided government, the state of the economy, etc.) that are used to predict the things that presidents say and do; and (c) estimate a statistical model that shows how well each covariate influences the outcome of interest, offering a paragraph or two on why each of the observed relationships does or does not conform to expectations. Though occasionally a useful exercise, this formulaic approach to quantitative analysis ultimately is unsustainable. Without theory, we cannot ascertain the covariates' appropriate functional forms; whether other important covariates have been omitted; whether some of the explanatory variables ought to be interacted with others; whether endogeneity is a concern, and how it might be addressed. And without theory to furnish answers to such issues, the reader has little grounds for assessing whether or not the results can actually be believed. Rote empiricism, moreover, is no substitute for theory. For when different results emerge from equally defensible statistical models, theory is ultimately needed to adjudicate the dispute.

Second, greater attention needs to be paid to the ways in which adjoining branches of government (Congress and the courts), international actors (foreign states and international governing agencies), and the public shape presidential calculations, and hence presidential actions. At one level, this claim seems obvious. Ours, after all, is hardly a system of governance that permits presidents to impose their will whenever, and however, they choose (Jones 1994). The trouble, though, lies in the difficulty of discerning institutional constraints—and here, I suggest, there is room for continued improvement. Too often, when trying to assess the extent to which Congress constrains the president, scholars take an inventory of the number of times that vetoes are overridden, investigations are mounted, hearings are held, or bills are killed, either in committee or on the floor. Such lists are helpful, if only because they convey some sense for the variety of ways in which Congress checks presidential power. The deeper constraints on presidential power, however, remain hidden, as presidents anticipate the political responses that different actions are likely to evoke and adjust accordingly.[9] To assess congressional checks on presidential war powers, for instance, it will not do to simply count the number of times that Congress has invoked the War Powers Resolution or has demanded the cessation of an ongoing military venture. One must, instead, develop a theory that identifies when Congress is especially likely to limit the presidential use of force, and then assemble data that identify when presidents delay some actions and forgo others in anticipation of congressional opposition—opposition, it is worth noting, that we may never observe. The best quantitative research on the presidency recognizes the logic of anticipated response and formulates statistical tests that account for it.

Finally, scholars too often rely exclusively on those data that are most easily acquired, which typically involves samplings of presidential orders, speeches, and proposals issued during the modern era. But as Stephen Skowronek (1993) rightly insists, much is to be learned from presidents who held office before 1945, the usual starting point for presidential time series. Early changes in political parties, the organizational structure of Congress and the courts, media coverage of the federal government, and public opinion have had huge implications for the developments of the presidency. And, as Skowronek demonstrates, the similarities between modern and premodern presidents can be just as striking as the differences between presidents holding office since Roosevelt. When searching around for one's keys, it makes perfect sense to begin where the proverbial street lamp shines brightest. Eventually, though, scholars will need to hone their sights on darker corners; and, in this instance, commit the resources required to build additional data-sets of presidential activities during the nineteenth and early twentieth centuries.

It remains to be seen whether scholars can build a vibrant and robust body of quantitative scholarship on the presidency. To be sure, some trends are encouraging. Important advances have been made. But until the literature is better integrated into the discipline, and until quantitative research addresses some of the problems outlined above, there will be continued cause to revisit and reiterate the simple pleas that George Edwards issued a quarter-century ago.

[9] For a survey of the recent game theoretic research that accounts for these interbranch dynamics, see de Figueiredo, Jacobi, et al. 2006.

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CHAPTER 17

EXECUTIVES IN PARLIAMENTARY GOVERNMENT

R. A. W. RHODES

1 Introduction: Mapping the Field

The literature on executive government in parliamentary systems can often be more fun to read because it is *not* written by political scientists. There are the popular biographies of individual prime ministers with varying degrees of lurid detail about their private lives. There are psycho-biographies probing childhood and other formative experiences. There are the journalists recording the comings and goings of our leaders, with an eye for a story that is never discomforted by an inconvenient fact. There are novels. But where are the theories, the models, and the typologies of executive government in parliamentary systems that distinguish political scientists from their more racy rivals? In fact, the academic political science literature is limited—more so than readers might expect or the importance of the subject warrants.

It is limited in part by the continuing need to break free of worn-out debates, especially in the analysis of Westminster systems. Instead of the tired debate about the power of the prime minister, the study of executives in parliamentary

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