George C. Edwards III Symposium

Introduction

Justin S. Vaughn, *Boise State University*

This past January, during the SPSA meeting in Puerto Rico, a roomful of scholars gathered for a discussion of how presidency studies has evolved and, specifically, the role George C. Edwards III played (and continues to play) in shaping that evolution. Audience members were treated to thoughtful commentaries on the topic from august members of our field such as Will Howell, Karen Hult, and Rick Waterman.

As chair of the presidency and executive politics section of that meeting, and the individual who organized this particular discussion, I owe these participants a great deal of gratitude for not only their time but their intellectual energy. As one of George's last graduate students, I have benefitted from not only his mentorship, but also a redirected generosity from colleagues who repaid their intellectual debt to George in part by showering others and myself with professional kindnesses. Without those kindnesses, I would not have been in a position to organize what I hope will go down in history as the San Juan Summit. Without the great fortune of having George as my teacher, mentor, and now friend, however, I would not be a political scientist at all.

As a result, the comments that follow are particularly meaningful for me, though even those few in the field who have not had the opportunity to interact with and learn from George will find a great deal of valuable insight in them. This series of brief reflections starts with one from George himself, as he takes us on a first-person journey through the analytical revolution that he pioneered in the field of presidential studies, one that began while he was a graduate student at the University of Wisconsin and continued through his landmark study of presidential prestige and success in Congress that was published in the *APSR* in 1976, until today. His concluding observations should prove as profitable as they are astute for the younger generations of scholars.

Subsequent commentary by Karen Hult, Richard Waterman, and William Howell underscores what George introduces. Karen traces the influence George's work has had on existing work and goes on to suggest ways we can approach the future of presidential studies that reflect the rigor and discipline introduced by George decades ago. Rick celebrates the humane contributions George has made, acknowledges the number of members of our community he has mentored directly at Texas A&M University, and the dozens of others for whom he provided both individual support and an intellectual home at *Presidential Studies Quarterly*, which he resurrected and turned into the leading journal focused on executive politics. Finally, Will reinforces the effect George and others had on what precisely constituted presidency research, the effects of their intervention still seen today in the form of greater attention to conceptualization and measurement, theory-driven investigations of causal relationships, and an inescapable dedication to evidence and logic.

I expect that as you read these pieces, many of you will have your own recollections of both specific instances where George made an impact on your career and more general observations of how had he and others not taken the pioneering steps they did, your work today would be impossible. I encourage you to share those observations, whether with me, with George, or perhaps best of all, with the students you now mentor.

THE REVOLUTION IN PRESIDENCY STUDIES

George C. Edwards III, *Texas A&M University*

One hundred and ten years ago, in the first article in the first issue of the *American Political Science Review*, A. Maurice Low criticized scholars who had complained of "executive usurpation" of power from Congress for "relying upon their rhetoric rather than their facts" (Low 1906). In fact, he argued, if there had been usurpation, it was on the part of the Senate, not the president. Thus the critics had not gotten their facts straight and completely missed the real pattern of behavior. More than a century later, political scientists are still exploring presidential leadership, yet for most of the decades since Low wrote, the study of the presidency was a backwater of the discipline.

The Revolution Begins

In the fall of 1973, I faced a dilemma. A newly-minted PhD, I was assigned to teach a graduate seminar on the presidency at Tulane. I looked forward to the class, but what readings would I assign the students, aside from Dick Neustadt's *Presidential Power?* Edwards Corwin's magisterial *The President: Office and Powers* was considerably out of date (Corwin died in 1963), and

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I wanted to move beyond the formal powers of the presidency anyway. Some scholars in international relations such as Alexander George and Graham Allison were doing interesting work on decision making, and James David Barber was just publishing his wildly successful but deeply flawed *The Presidential Character*. Yet there was very little work on the presidency by presidential scholars. There was especially a dearth of work with an analytical bent that focused on explaining the causes and consequences of presidential behavior rather than describing that behavior or the development of the institutions of the presidency.

I could not turn to the subfield for advice, as there was no organization of presidency scholars. Indeed, there was nothing that could sensibly be termed a field of presidential studies. I could not review syllabi from the leading departments of political science, as most of them (including the one I attended) did not offer a graduate course on the presidency. The absence of rigorous research on the presidency had encouraged these departments to simply avoid the topic in training graduate students.

It was a frustrating time to be a scholar of the presidency. As I declared, perhaps impertinently, a few years later, the presidency subfield had failed to adopt the basic norms of social science. I argued that research on the presidency too often failed 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 1981, 146).

Scholars needed to think theoretically to develop falsifiable propositions about presidential leadership and test these systematically with relevant data and appropriate econometric techniques. They should seek to discover generalizations about behavior rather than produce discrete, ad hoc analyses, repeat colorful anecdotes from presidential press clippings, or reach facile conclusions about Lyndon Johnson's skill at swaying members of Congress or Ronald Reagan's ability to mobilize the public behind his proposals. Thus, the research on the presidency presented a striking irony: The single most important and powerful institution in American politics was the one that political scientists understood the least.

This was the context in which I wrote "Presidential Influence in the House: Presidential Prestige as a Source of Presidential Power" (I submitted the piece in the fall of 1973 and it was accepted in May 1974. There was such a long backlog in the *APSR* that it did not appear in print until March 1976). I knew that scholars had not employed quantitative methods or empirical theory before in writing about presidential leadership, but I did not set out to revolutionize presidential studies. I was just trying to do research as best as I could.

The piece and others I wrote at about the same time made a splash. On the one hand, a number of readers thought that there was little future of quantitative work in the presidency. Much like the old baseball scouts in *Moneyball*, they did not feel that matters such as leadership could be quantified. They knew it when they saw it, and that was good enough for them. There was a brighter side, however. Many younger scholars responded favorably to my approach and began doing their own work in the more modern vein.

Within a decade, there was an identifiable literature on presidential-congressional relations and one that I called the "public presidency." Scholars were also making strides in researching other important aspects of the presidency. Before long, we formed a section on presidential studies within APSA. At last there was a real presidency field.

The Revolution Continues

Although we have made great progress in increasing our understanding of the presidency, it is natural that scholars, including myself, have focused on the areas of presidential politics most accessible to research, especially those dimensions most easily lending themselves to quantitative measurement. For example, presidential approval was the hot topic for a while. It offered a long time series and allowed the application of the latest times series techniques. Yet the theory behind these studies was often primitive and the results frequently indecipherable.

I fear that we have often missed the key questions about the presidency. For example, in 1992 I chaired a panel of distinguished scholars that reviewed the presidency of George H. W. Bush at the APSA annual meeting. Dave Barber asked a very good question from the floor: What should we be looking for in a president, aside from policy aspirations? We did not offer insightful answers. Some years later, I commissioned a special issue of *Presidential Studies Quarterly*, asking leading scholars what questions we should be asking about the presidency. Once again, the effort was not a great success.

I find it convenient to organize presidential studies into four broad areas: 1.) Leading the public. 2.) Leading Congress. 3.) Making decisions. 4.) Implementing policy. These wide categories incorporate agenda setting, media relations, legal issues, and other important topics.

We have learned a lot about leading the public and Congress. There is much more to learn, of course, but I think we have a clear understanding of the basic parameters of the relationships here.

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It seems to me that we need to focus on the important issues of decision making and implementation. I am not suggesting we know nothing about them or that there has been no useful research on the topics, only that we have made much less progress in these areas. There exists a body of theory about decision making, but we typically ignore it altogether. Similarly, presidency scholars rarely exploit broader work on policy implementation. And just what should we be looking for in a president?

Like scholars in other areas, we tend toward the easy work. For example, it is fine to study signing statements, but what difference do they make for actual policymaking? Similarly, our analyses of presidential decision making are generally no better than those by journalists, and these are not especially astute.

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Accessibility to decision making and implementation is always a problem in studying the presidency. Nevertheless, if we do not ask the right questions, we are unlikely to arrive at the right answers. There is nothing more important to study than decision making. Aside from their consequences for the nation, decisions are what make presidents unique.

The field is wide open, and I envy the possibilities available to younger scholars.

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REFLECTIONS ON EDWARDS

Karen M. Hult, *Virginia Tech*

It is an honor to have been invited to reflect on George Edwards's contributions to presidency scholarship and to our field more generally. Like many, I have benefited from George's mentorship, intellectual challenges, and professional guidance. He is a treasured colleague, role model, and friend.

I welcomed the excuse to return to the *APSR* article with which we were asked to begin – "Who Influences Whom? The President, Congress and the Media?" (1999). It has the hallmarks of all of George's works: the research and analysis are careful, clear, systematic, nuanced, and insightful. The work reminds us as well of the changes that have followed its publication: parties and politics have polarized, party coalitions have transformed, and the links between mass and elites have frayed and altered. Even so, its variables (if not its values and specific relationships) and analysis are sturdy.

Like virtually all of George's scholarship, this article has been influential: A Google scholar search (using the keywords "presidential influence," "Congress," and "presidential prestige") produced 99 cites from 1977 through 2015 in multiple scholarly journals (including *APSR, AJPS, JOP, PRQ, APR, Presidential Studies Quarterly* and *Congress and the Presidency*); excluded were the numerous books that also draw on the article. Meanwhile, how he (and we) study the presidency and the political system in which it is embedded and that it reflects, and shapes, has evolved. We use different measures and statistical techniques, more systematically incorporate qualitative evidence, and include other policy areas and political systems.

More generally, George's 1976 article heralded what has become a broadening and deepening of scholarship on the US presidency. First, we have more cumulative research. Among the myriad examples are work on the dynamics, generalizations, and lessons of presidential transitions and on the patterns of legislative, presidential and executive branch relationships at multiple points of the policy process and across policy arenas.

Second, presidency scholars are among those exploring influences on and the implications of change and stability. Works on the "unilateral presidency," for instance, have traced its evolution to the founding, probed contemporary presidents' strategic employment of specific tools (e.g., Obama's shift to "quasi-proclamations"), compared influences of its use and achievement of presidential objectives, and raised concerns about its frequency, constitutionality, and lasting effects. Others examine how presidencies try to cope with increasingly diversifying and fragmenting media, their growing reliance on social media, and the seemingly relentless contraction of space in which presidents can be heard, much less try to influence others. As George has highlighted, the primary narrative is one of presidential facilitation, not direction, and of tactical adjustments.

Third, since the 1970s, presidency research has been enriched by access to greater amounts and more diverse types of data. Archival materials continue to be digitized; the American Presidency Project, the Miller Center, and NARA remain scholarly treasure troves; social media archives and associated "Big Data" analytics point to promising opportunities. Of course, like other social scientists we wrestle with ongoing challenges of measurement validity and of shifts in use and meaning (e.g., "executive orders," "dual vetoes").

Fourth, presidency research now relies on a broader range of research designs and methodological approaches, including survey experiments, process tracing, and comparative case analyses. Meanwhile, scholars employ increasingly sophisticated statistical techniques and game theoretic and other formal modeling.

Where do we go from here? Like Ian Shapiro, I value problem-driven scholarship. That said, paying attention to theoretical and methodological foci clearly is critical as well. Theoretically, it appears to me to be useful to begin as Edwards and Terry Moe do, with a view of presidents as strategic, purposive actors. Deserving consideration as well, though, is how such an actor (viewed variously as an individual and as an institution) is embedded in and influenced by complex, dynamic, and overlapping networks.

This in turn underscores a need to self-consciously model specific types of context. Key contexts might encompass several important, if not as fully studied areas. These evidently include intergovernmental arenas in the US (e.g., presidential direct action in granting waivers, policy dynamics when states and nonprofits are contractors in areas of presidential priority). Researchers also might continue to explore relationships inside the federal executive branch much as Rudalevige (2002) and

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