

The Revolution in Presidential Studies

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Until very recently, the field of presidential studies had long suffered from an inferiority complex—a self concept that arose not from weaknesses that were imagined, but from weaknesses that were real. The classic criticisms came from presidency scholars themselves, who, as the rest of political science raced ahead during the 1970s, saw their own field falling behind.

King (1975, p.173) complained that “the existing literature is mainly descriptive and atheoretical: general hypotheses are almost never advanced, and, when advanced, almost never tested.” Heclro (1977, p.5) found little in the field to praise, arguing that “beneath the extensive veneer of presidential literature, there are immense gaps and deficiencies.” The criticisms continued into the 1980s, and there was much pressure for making the study of the presidency more quantitative and theoretical (Edwards, 1981; Wayne, 1983). Yet even with the dawning of the 1990s, leaders of the field were still remarking on its lowly status (Edwards, Kessel, and Rockman, 1993, p.485):

Presidency scholars sometimes feel humbled by the company they keep. Like the comedian Groucho Marx, they wonder whether any club (subfield) willing to accept them is not already too inclusive. Many of the currents influencing research in American political science have been perceived by scholars both inside and outside the presidency subfield as having passed it by or, at best, only lightly grazed over it...[F]rom within the political science community, questions have been raised as to whether there is a legitimate field of presidency research.

Today, I doubt that anyone familiar with the field would characterize it this way. Over the past decade or so, there has been a revolution in the study of the presidency. Part of this revolution is that quantitative studies are much more common than in the past, and there is far greater attention to

hypothesis testing, measurement issues, research design, and other ingredients of scientific methodology. In these respects, the field's internal critics have gotten what they asked for. Yet as important as these developments are, and as integral to the revolution, they are not the essence of it. Quantitative analysis, hypothesis testing, and all the rest have been staples of this literature for some time—in research on presidential success in Congress (Edwards, 1989), for example, and on presidential popularity (Mueller, 1973; Ostrom and Simon, 1985). Now there is more of this kind of work, and it is more sophisticated. But it is also of a different character, owing to a revolution that has occurred on other grounds.

Most fundamentally, this has been a revolution in *theory*. In just a few short years, a field mired in isolation and traditionalism has been catapulted into a new scientific realm through a seismic shift in the scope, power, and analytical rigor of its theories—a shift that has put an end to the Era of Inferiority, modernized and invigorated the way the presidency is thought about, and integrated the field much more fully and productively into the mainstream of political science.

The mechanism of this transformation has been rational choice theory, which has become the dominant (but not the only) analytic approach among the cutting-edge works of greatest influence in the field. The triumph of rational choice, it's fair to say, is *not* what most critics were asking for. Years earlier, I and a few others (Moe, 1993; Sullivan, 1988, 1990; Miller, 1993) had argued for rational choice theories of the presidency, but without much support or near-term impact. What it took was a new generation of scholars—unwedded to the past, broadly institutional in their perspective, well trained in rational choice. And when the change came, it came suddenly: as these new talents took on key substantive issues long central to the presidency, turned presidential studies into an exciting arena of innovative theory, and created an edifice of work that connected the field to the rest of the discipline.

One of my purposes here is to describe what has happened, and to offer some perspective on how the revolution came about, what it consists of, and why it is on balance a very good thing. I am not, however, writing to crow about the triumph of rational choice, nor to announce that we as a field have finally reached the promised land. For I also want to argue that, while rational choice is destined to be the prime vehicle of theoretical progress in the near future (emphasis on “near”), it is not destined to drive out other approaches to presidential studies even in the short term—and it is likely to lose its dominance over the over the longer haul, both in presidential studies and in political science more generally, to competitors that are far more in keeping with the concerns of its critics.

These are big issues, perhaps too big for a short paper. My intention is not to offer a definitive analysis, but to encourage debate and discussion—and progress.

Traditional Weakness

If we look back on the field of presidential studies, what immediately stands out is that, for some forty years running, its intellectual tradition was profoundly shaped by one book: Neustadt’s (1960) *Presidential Power*. Other fields have their seminal books and articles as well. But in view of all the developments in theory and methodology that fueled political science during this period, it is unusual in the extreme—indeed, it is totally unique by comparison to other fields—for one contribution to remain so completely dominant for so long.

Why did this happen? The conventional wisdom is that it was simply a great book, filled with timeless insights that rightly served as the basis for theory and research. A more revealing answer, I believe, is that the book had unequalled staying power because the field itself was not participating in the analytic progress sweeping the rest of the discipline. If the field had been caught up in these swift currents of change, *Presidential Power* would have been superseded long ago, experiencing the normal fate of classic works as their fields move ahead.

In this case, two basic problems needed addressing: one substantive, the other analytic. The substantive problem was that *Presidential Power* was out of sync with the modern presidency. Neustadt's view of the presidency was explicitly personal and informal. Yet even as the book was being written, the presidency was becoming a larger, more complex, more formal institution—and this very institutionalization was and is at the core of the modern presidency. Neustadt's personalization of the presidency, along with his discounting of formal structure and formal power, was exactly the wrong theoretical move for the times.

The analytic problem was that Neustadt did not offer a coherent, well developed theory anyway, but rather a loose set of ideas—about reputation and prestige, vantage points, the power to persuade, and the like—that were not formulated with much precision. Moreover, they did not derive from broader bodies of theory and research and did not generalize beyond the field. The theory was a stand-alone that was largely isolated from the rest of political science.

Neustadt's book was not the sum total of the presidency field, of course. This was a field that contained diverse lines of research, expressed a genuine interest in institutions, and sought to build better theories. But the book's ideas and approach were enormously influential. They led the field to pay much more attention to the personal and the informal than it otherwise would have, especially once the new institutionalism hit with full force in the larger discipline; and new editions of the book, expanded and updated (the latest appearing in 1990), did nothing to change that, sticking to the same basic themes and arguments as the original. Owing to its out-sized influence, the field was inclined to pursue theories that were inward-looking, lacking in rigor, and unconnected to developments in other fields.

In "Presidents, Institutions, and Theory" (Moe, 1993), written more than thirty years after *Presidential Power* first appeared, I discussed these problems—which were still quite debilitating to the

field even then, and obviously deeply rooted—and outlined what I considered a promising way of dealing with them. It involved two basic elements: an institutional approach to the presidency and an analytic reliance on rational choice.

An institutional approach is not unique to the presidency, but can readily be applied to it (and now almost always is, without any controversy, and indeed without any recognition that a major analytic move has been made). Its perspective is entirely *impersonal*, based on conceptual building blocks—structure, authority, incentive, and other institutional variables—that treat presidents and other actors as generic types rooted in an institutional system. Presidents are not individual people, by this reckoning. They are actor-types occupying an office whose powers and incentives are institutionally determined, and it is by means of their institutional commonalities that we understand them.

Such an approach is well suited to a theory dealing with the Executive Office of the President, the White House Office, politicization, centralization, and other aspects of the institutional presidency. But its scope is actually much broader than that. It may be used to explore any aspect of institutional politics—having to do with legislatures, bureaucracies, courts, elections, whatever—and any sets of institutional actors. This generality, which is the norm throughout institutional analysis, makes it possible to build theories that involve presidents but are not exclusively presidential—and connect to virtually all parts of the political system.

An institutional approach offers great advantages, but it is not sufficient in itself to do the job. For it may still give rise to analyses that—depending on the methodology that guides them—are overly complicated, lack clear logical structures, generate little deductive power, and otherwise fail to provide an effective basis for theory. In fact, this has long been the likely outcome, because the traditional methodology within the presidency field—indeed, within the social sciences generally—has been to

seek explanation by proliferating the number of relevant variables, pushing for comprehensiveness, and embracing complexity. The result has been analytic weakness. And weak theory.

Rational choice corrects for these problems. As a methodology, it puts the emphasis on simplicity, clarity, logical rigor, and deductive power. And it pursues them by purposely not seeking to include all relevant variables or to be comprehensive. The aim instead is to capture just the essence of the phenomenon being explained. This tradeoff with realism was intensely controversial in the past, when more historical and personal approaches reigned and a rich empiricism ruled the roost; but much of the controversy remains today, and these continuing concerns about the excessive simplifications of formal modeling have garnered rational choice many detractors throughout political science. It is dominant, but much despised. The criticisms are not without merit, as I will discuss later. But the insistence on a thoroughgoing realism, however reasonable it may sound on the surface, has done much to weigh the presidency field down over the years, burying it in detail and driving out theory.

Prior to the 1970s, rational choice rarely dealt with institutions. But with the emergence of the new institutionalism during that decade, economists crafted powerful new tools for explaining the emergence and structure of business firms (Williamson, 1985; Moe, 1984). And during the 1980s, political scientists appropriated those tools to begin building a theory of political institutions—generating a research program that has become perhaps the most successful in the history of the discipline. It has created a body of institutional theory that is not only rigorous, clear, and deductively powerful, but is built around a common conceptual and methodological core that, as the theory has spread to all major areas of the discipline—beginning in American politics, then moving to international relations and comparative politics—has literally knit the discipline together. Whether scholars are studying the U.S. Congress or the European Union or the World Bank, they can turn to a shared body of theory, speak the same language, think in the same terms—about collective action problems,

information asymmetries, defection, compliance, commitment, self-enforcing equilibria, and other analytic ingredients that have now become standard equipment for understanding political institutions (Weingast and Wittman, 2006).

This discipline-wide revolution did not spread with the same force or timing to each and every field. But what made it possible (where it happened) was the joining of two elements: an institutional approach and the methodology of rational choice. The challenge was to bring this same joining of elements to presidential studies.

Prelude

During the 1980s and 90s, when the field was undergoing much soul-searching, there was considerable support for a more quantitative approach to research. But this support did not extend to rational choice. There were at that point almost no rational choice theorists who thought of themselves as presidency scholars. There was almost no rational choice work on the presidency. And the methodology of rational choice was flatly inconsistent with the way most presidency scholars thought the presidency should be studied. There *was* support for studying the institutional presidency. But this was based on the substantive importance of the topic, and paired with a recognition that the personal aspects of the presidency were equally deserving of study. The idea that institutionalism was an approach, and that it meant eliminating the personal dimension altogether, was neither popular nor well understood.

Changes were brewing outside the field. As rational choice institutionalism took hold in American politics, theory building began with the internal structure of the U.S. Congress, but then quickly moved to larger questions—notably, issues of delegation and bureaucratic control—that connected Congress to other institutions. Especially in the early going, these works tended to ignore presidents or give them short shrift (Weingast and Moran, 1983; McCubbins, 1985; McCubbins, Noll,

and Weingast, 1987). But as the literature developed, presidents were more often taken into account, as were the courts, and the aim was increasingly one of capturing the logic of separation of powers and the inherent struggles among its key institutional actors (Calvert, McCubbins, and Weingast, 1987; Hammond and Miller, 1987; Moe, 1989, 1990; Ferejohn and Shipan, 1990; Kiewiet and McCubbins, 1991; Eskridge and Ferejohn, 1992).

In effect, a theory of the presidency was being created, but not by presidency scholars. It was being created by scholars who saw themselves as institutionalists. And the theory was one that understood the presidency not by showing it special attention, but by integrating it into the larger institutional system. It was a theory that, in one package, was a theory of the presidency, a theory of Congress, a theory of the courts, a theory of the bureaucracy—a theory of the system and its interdependence. From an analytical standpoint, and from the standpoint of the discipline as a whole, this was a very good thing. But it also meant that presidency scholars were losing control of their own body of theory, and sitting on the sidelines as “outsiders” developed it for them.

During this time, a few institutionalists with anchorings in rational choice began carrying out work that focused more centrally on the presidency itself. In an early effort, “The Politicized Presidency” (Moe, 1985), I attempted to explain important features of the institutional presidency—politicization and centralization—by arguing that they have nothing to do with presidents as individuals, but are driven by institutional incentives and opportunities that are largely shared by all modern presidents and rationally acted upon in their pursuit of strong leadership and bureaucratic control. In subsequent work, I argued that presidents rationally respond to their underpowered position in the separation of powers system by seeking to build the institutional presidency, design and control the bureaucracy, take unilateral action, and increase their power at the expense of the other branches (Moe, 1993; Moe and Wilson, 1994). Sullivan (1988, 1990) made an early case for approaching the president-

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Congress bargaining relationship in game theoretic terms, and showed how information and expectations shape the way presidents use concessions in building winning coalitions. And Miller (1993) showed how presidents can gain influence by helping to solve the legislature's coordination problems and social dilemmas—via the provision of focal points, for example. He also used spatial models to show how the presidential veto interacts with Congress's bicameralism and its committee system to determine legislative outcomes.

These works, by focusing their analytic attention on the presidency, were self-consciously trying to promote a body of presidential theory that was more than just a by-product of the broader institutionalist literature—and that allowed the field, in effect, to take control over its own theory. But they were just the first stirrings, and they were not fully applying the kinds of analytic tools that were increasingly at the cutting edge of institutional theory discipline-wide. For presidential studies to take advantage of these developments, something big had to happen.

Revolution

And it did. A number of young institutional scholars, well trained in rational choice, broke ranks with the mainstream of formal theorists—who, in American politics, had long proceeded as though Congress was the center of the political universe—and trained their energies on the presidency. True to type, they thought of themselves as institutionalists, not simply as presidency scholars. But they had genuine interests in the substance of the field, and took on the insider challenge of tackling some of its most central issues. In short order, their work transformed presidential studies into a hotbed of theoretical innovation.

If one event symbolizes the onset of the post-Neustadt revolution, it is the publication of Charles Cameron's (2000) *Veto Bargaining*, which was "the first book-length study of the presidency to adopt an explicit, formal, and sustained rational choice perspective" (p.3). Who said formal modeling has to

be boring and impenetrable? This book is a work of science, but it is also a work of art: beautifully written, clearly developed, engaging, clever, and eye-opening informative. In it, Cameron explores a quintessentially Neustadtian topic, the bargaining between president and Congress. His analysis, however, is entirely impersonal and structural, focusing on their formal legislative roles under separation of powers—Congress proposes, the president can veto, Congress can override—and the strategies and outcomes they produce.

A standard criticism of rational choice models is that they demonstrate the obvious. Yet that is hardly the case here, as Cameron takes a familiar aspect of presidential politics, lays bare its logical underpinnings, and generates new insights. Most fundamentally, his formal models highlight the key role of uncertainty, showing how it opens the door to presidential vetoes, “creates rich opportunities for presidents to engage in strategic behavior” (p.19), gives presidents incentives to make veto threats and build reputations (another Neustadt concept, here rigorously developed), and allows them to wrest concessions from Congress. An elaboration of this core logic spells out the conditions under which vetoes and concessions occur—showing, for example, that vetoes should be greater under divided government and for “significant” legislation, and that they should be less common at the end of the presidential term. It also shows that, when vetoes do not occur, presidents may still have considerable influence—for legislation may be freighted with concessions as Congress anticipates what it needs to do to avoid a veto.

Rational modeling is also criticized for being unconcerned about data and empirical tests. But Cameron’s book is intensely empirical, and indeed begins with a detailed discussion of historical patterns in presidential veto politics that is hugely informative on descriptive grounds alone. It reveals, among other things, that “significant” bills are vetoed at relatively high rates (20%) and that most vetoes occur during divided government (71%)—and in general, it fills a big gap in our basic knowledge about

Sampling?

presidential vetoes. Cameron uses this factual background to set up his theoretical work: the challenge, he says, is to develop a theory of veto politics that can explain these empirical patterns—and once his models are set out, he shows that they can indeed account for them, engaging in a data analysis that is sophisticated, careful, and sensitive to nuances in history and meaning.

If there is another scholar who, along with Cameron, has played a pivotal role in initiating and propelling this revolution, it is Nolan McCarty. From the mid-1990s on, McCarty has generated a stream of articles pioneering the application of rational choice models to the presidency. Much of this work has also been on the veto. He began, in a 1995 piece, by imposing much-needed coherence on existing theoretical arguments related to the veto and subjecting them to empirical test (McCarty and Poole, 1995). He went on to explore presidential reputation-building in veto politics (McCarty, 1997), the impact of the veto on pork-barrel politics (McCarty, 2000), and the “blame game” in which Congress purposely passes bills it knows will be vetoed—making the president appear extreme to the public and undermining his popularity (Groseclose and McCarty, 2001). In addition, McCarty has done innovative work on presidential nominations and appointments, exploring the impact of Senate rules and other aspects of institutional politics on the delay and confirmation of presidential nominations (McCarty and Razaghian, 1999), and the “appointments dilemma”—and bargaining inefficiencies with Congress—that can result because presidents have both the power to appoint and the power to remove (McCarty, 1994).

With Cameron and McCarty at the leading edge, what came next was a stunning surge of new books that brought the revolution to fruition. Often, these books were accompanied by spin-off articles, some in the discipline’s top journals—a development that was a transformation in itself, given how rarely articles about the presidency had made it into these journals in the past. The difference was analytic sophistication. Studies of the presidency, for the first time, were at the cutting edge of political science theory.

The new books share the general virtues of Cameron's Veto Politics. All develop rational choice theories of the presidency—usually, but not always, via formal models—focusing on substantive issues of longstanding importance to the field. All are institutional in approach. All are well written and accessible to a broad range of readers. And all are major empirical projects that not only test theoretical implications, but present vast arrays of original data that are genuinely fascinating and revealing.

On this last characteristic: it is precisely these empirical components—which involve sophisticated quantitative analysis, and cover topics long crying out for insightful research—that may prompt some in the field to think that the essence of the field's revolution has been the rise of quantitative empirical work. Yet what is really new is not the quantitative work itself, but the fact that it flows from rigorously developed theories—theories with analytic connections to the broader corpus of institutional theories in political science generally—and that it has important consequences for their modification and progress over time.

For reasons of space, I cannot discuss all these new works on the presidency here or give them the separate attention they deserve. But by briefly acknowledging several of them, I hope to suggest just how giant a step forward they have collectively taken.

- **Unilateral Action.** Howell's *Power without Persuasion* (2003) takes on Neustadt directly, arguing that modern presidents often do not need to bargain, but can shift policy through unilateral action. He develops a formal model to explore when presidents will choose to do that, and when Congress and the courts will take action to stop him. He follows up with an empirical analysis, based on original data, showing for the first time that presidential actions are only infrequently overturned by Congress or the courts, and he relates these struggles to divided government, congressional fragmentation, and other basic aspects of politics. (See also Moe and Howell, 1999)

- **Leadership and the Public.** Canes-Wrone moves innovatively along two related fronts in *Who Leads Whom?* (2006). She develops a formal model of “going public” that expands creatively upon Kernell’s (1986) classic work, leading to new and more precise expectations—e.g., that presidents will tend to go public on issues that are already popular. She also develops an insightful model of leadership and pandering, shedding light on when presidents will pursue policies that are unpopular but good for society (acting as leaders) and when they will pursue policies that are popular but bad for society (thus pandering). Both theories are subjected to thorough empirical test. (See also Canes-Wrone, 2001a, b; Canes-Wrone, Herron, and Shotts, 2001; Canes-Wrone and Shotts, 2004.)
- **Bureaucratic Organization.** With the discipline’s theory of delegation and control almost entirely Congress-centered, Lewis’s *Presidents and the Politics of Agency Design* (2003) puts the spotlight on presidents: developing a rational choice perspective on how presidents have used their discretion and strategic advantages to bring about the creation of agencies whose structures are more amenable to (and less insulated from) presidential control than Congress might like. He tests the theory using an original data set on all federal agencies created between 1946 and 1997, developing an empirical analysis that not only confirms his theoretical notions, but provides a wealth of new information on what presidents have done to get control of the bureaucracy. (Howell and Lewis, 2002; Lewis, 2004)
- **Centralization.** In *Managing the President’s Program* (2002), Rudalevige uses transaction cost analysis to develop a theory of “contingent centralization,” arguing that presidents will not relentlessly centralize policy making in the interests of greater control, but rather will find it beneficial to centralize only under certain conditions—e.g., when the issues are new or cut across departments, or when the issues are not so complex that they require considerable

bureaucratic expertise. Employing original data on some 400 legislative issues over the period 1949-1996, he goes on to test the theory in the first large-scale, longitudinal study of this key component of the institutional presidency.

- **Politicization.** Lewis's *Politicizing Administration* (2008) is a work of great scope and a major accomplishment. In it, he develops a formal theory that points to the conditions under which presidents can be expected to politicize, and—using multiple data sets and case studies—shows that the theory stands up empirically. He also shows that politicization has a negative impact on agency performance. Along the way, he provides an overview and history of the entire appointments process, flush with facts about the various types of political appointees, how many are in which agencies, at what levels they are located, and how things have changed over time. This is a theoretical work, but it also fills a big gap in our knowledge. (See also Lewis, 2005, 2007)

These and other rational choice contributions (see especially Conley, 2001, on presidential mandates and Larocca, 2006, on presidential agendas), together with the pioneering work of Cameron and McCarty, have transformed the imbalance of analytic power that has for so long relegated presidential studies to the second echelons. Meantime, rational choice institutionalists have continued to generate more broadly based work that, while not focused on the presidency or designed to explore it in depth, sheds new light on its connection to other institutions.

Krehbiel's (1998) gridlock model, for instance, provides insight into the president's integral involvement—albeit one dimensional, as a veto player—in the checks and balances that structure and often immobilize the American legislative process. The delegation literature has grown considerably over the last decade, and presidents have been given more explicit attention—again, as a veto player (sometimes accompanied by the assumption that bureaucrats share the president's policy preferences)—

although the focus is still on the legislature and its delegation decisions (Huber and Shipan, 2002; Volden, 2002). There is also a lively and growing literature that seeks to explain presidential nominations, mainly to the courts; but this work—as befits its separation of powers moorings—is just as concerned with Congress and the courts as it is with presidents (Moraski and Shipan, 1999; Rhode and Shepsle, 2007; Krehbiel, 2007).

The difference from ten years ago, however, is that these institutionalist works are no longer the sum total of the rational choice theory of the presidency. What they contribute is valuable and an integral part of presidential theory. But control over the development of that theory has now shifted, and is mainly in the hands of people who—while institutionalists well connected to (and participating in) the larger separation of powers literature—are dedicated to an in-depth understanding of the presidency itself.

The Downside of Rational Choice

A revolution is defined by the dramatic change it brings, not by whether it is good or bad. In this case, the revolution is mostly very good. The status quo ante was a field mired in traditionalism, overly influenced by Neustadtian concerns for the personal and the informal, and struggling to join the mainstream of modern political science. The revolution has changed all that, and rational choice institutionalism has made it happen: shifting the foundations of theory from the personal to the impersonal, from the informal to the structural, from loose argument to logical rigor and deductive power. These are profoundly consequential developments. They have already paid off handsomely. And they bode well for the future of the field—for its theoretical progress, and for the enlightening empirical work that goes along with it.

But rational choice also has serious weaknesses—weaknesses that, while sometimes more egregious because of the way formal modeling is applied in particular cases, are basically inherent in the

methodology itself. In fact, looking across political science as a whole, I think there is much truth to what its critics have to say about its more formal variants—particularly game theory, which, after taking economics by storm, is now the dominant mode of formal analysis in our own discipline. The criticisms are probably familiar to most readers, but this doesn't make them any less valid or important (see, e.g., Green and Shapiro, 1995; Walt, 1999). Among the main drawbacks, I view three as perhaps at the top of the list.

First, the players in these formal models are optimizers whose assumed capacities for calculation and information processing are typically light years beyond those of real people. Most of the time, no attempt is made to justify such extreme caricatures of individual decision making, as the analytic technology is so widely accepted among those in the modeling community that assumptions wildly at variance with human cognitive abilities are routinely accepted. When justifications are provided, they often rely on Friedman-type methodological claims, contending that only predictive accuracy matters and that people behave “as if” the underlying assumptions are true. But the fact is that these assumptions imply—and thus the models imply—all sorts of sophisticated, strategic behaviors that fly in the face of decades of carefully accumulated evidence on human choice, much of it derived from controlled experiments. In important respects, people do not behave “as if” the assumptions are true. (I will discuss this literature below.)

Second, the solutions to these models take the form of equilibria, usually Nash equilibria. “Change” is explored, and prediction and explanation pursued, through comparative statics: by investigating how exogenous shifts in the model's parameters might lead to different equilibrium outcomes. An equilibrium, however, is an arrangement from which none of the players has an incentive to defect or depart—if indeed they happen to find themselves in such an arrangement—but there is no guarantee that any given equilibrium can actually be reached by the players at all. And when there are

multiple equilibria, which is common, the theory cannot say which equilibrium is the likely outcome or whether any of them is actually reachable. The underlying problem is that the actual dynamics of change—the process by which the players get from A to B—cannot readily be explored given the methodology, and in practice is simply not part of the theory or its explanation. Because much of what political scientists want to know in the realm of institutions involves interactions (and contextual effects) that work themselves out over time, often through tortuous processes whose specifics are crucial to their outcomes, there is a knowledge gap here that rational choice's formal models cannot fill. This gap is all the more yawning because the common notion that political and social processes (aside from multi-dimensional voting) typically lead to equilibria may in fact be quite wrong. Most of the behaviors and events that are truly important in politics and society may actually be out of equilibrium much or even all of the time.

Third, while simplification is necessary for successful theory building, and while rational choice has done political science a great service by cutting through the crush of excessive detail, its models nonetheless tend to be overly simplified—meaning that they often do *not* capture the essentials, but go too far in stripping away much of what is necessary to an adequate understanding. The heroic assumptions about calculation and information are part of this problem. So are the analytics of equilibria and the associated ignorance of dynamics. But more generally, rational choice models are not equipped to handle complexity—even when complexity appears to be quite essential to an explanation. In effect, simplification is not just a modeling strategy that is employed when appropriate: it is required by the methodology *whether it is appropriate or not.* The reason these models cannot handle complexity is that, when an attempt is made to model large numbers of actors who truly interact with one another over time and who are embedded in various sorts of groups and structures—which is the typical situation throughout politics and society—the mathematics quickly breaks down or is insoluble.

It becomes impossible to derive equilibria, if indeed there are any. The only models that “work” are those that are extremely simple—sometimes misleadingly, unjustifiably simple. Theorists want to avoid this downside, of course, but they are heavily constrained: they must radically simplify whether the situation calls for it or not. Complexity is not an option.

These are important weaknesses, and they limit on what rational choice can ultimately contribute to our understanding of the presidency, as well as politics and political institutions more generally. Were I intent on doing so, or if others were, it would surely be possible to dig more deeply into the formal models on the presidency that I lauded in the previous section—by Cameron, McCarty, Canes-Wrone, Howell—and take them to task on various grounds. Much as they contribute, they all make heroic assumptions about the calculating and information processing capacities of the players. All focus on equilibria, and are unable to explore processes of change and adaptation over time. All simplify away complexities—the sources and full, debilitating extent of Congress’s collective action problems, for instance—that are probably essential to an understanding of what is being studied.

But criticism is easy. The key point to be emphasized, I believe, is not that these works may leave something to be desired, but rather that we need to have perspective on what they are and what they have done. We need to judge rational choice in the context of its times, and thus relative to a baseline of the theory and research that made up the discipline and presidential studies before it arrived on the scene. By these yardsticks, rational choice has done an extraordinary job of fueling scientific progress: putting the emphasis on clear, logically rigorous, deductive theory, creating an edifice of theory that is logically linked to developments in other subfields, and generating reams of sophisticated, enlightening research. These are revolutionary developments.

The Near Future

In the near term, rational choice will continue to stand out as the most powerful force in the analytic arena, led by its game-theoretic formal models. Even so, its preeminence will be exercised in an intellectual environment that also supports considerable diversity and eclecticism, and its “march of science” will fall far short of the kind of top-to-bottom formalization that it has achieved in most of economics.

In part, this is because formalization is not the only way that rational choice theory is productively expressed. Much of it is developed verbally, without the math and the radical simplification. There are good reasons for approaching theory in this way, and thus good reasons for it to continue. While informal analysis lacks some of the rigor and deductive power of formal models, it also has more flexibility in its elaboration of ideas, and can be more wide-ranging and creative—serving as a basis for formalization, as well as a source of theory in its own right. The fact is, some of the most influential and enduring work on politics and institutions is of this type, successful because of its ideas and not its math. Think of Olson’s work on (1965) on collective action, Schelling’s (1960) on strategic interaction, Williamson’s (1985) and North’s (1990) on institutions. Within the presidency field, my own work has been of this type, and some of the revolution-forging books I discussed earlier—by Lewis (2003) and Rudalevige (2002)—are exercises in informal rational choice as well.

The contributions of rational choice, then, are not due to formalization alone; and indeed, formal models actually benefit from having a body of more free-wheeling analysis to feed upon. Similar complementarities, moreover, arise from lines of analysis and study that are outside the rational choice literature. The fact is, rational choice needs this sort of intellectual diversity for its own success. Its total dominance would not only be bad for the field—because rational choice itself is inherently

limited—but also bad for rational choice. Consider, for instance, the synergies involved in the generation of facts and ideas.

Presidency scholars have always been good at generating facts. And they are even better at doing so now that sophisticated empirical methods have made their way into the field. Rational choice theorists obviously need a good storehouse of information if they are to understand their subject matter, have insight into it, and be able to model it productively. Most of what they know about the presidency has always come, and will continue to come, from scholars who are not modelers. A theory of politicization cannot help but be informed by Nathan's (1983) work on the administrative presidency or Weko's (1995) study of how presidents have institutionalized their personnel decisions. A theory of unilateral action can only be better for the empirical research of Cooper (2002) and Mayer (2002). And so it goes. In the future, the relationship between modelers and empirical researchers will be more reciprocal, because rational choice will have a productive influence on what facts get collected and what studies get carried out. But it will work the other way too: for rational choice is heavily dependent on what empirical studies uncover, whether the research is rooted in rational choice or not.

Much the same is true in the realm of ideas. Rational choice is an idea machine, with an analytic core that readily suggests promising avenues of inquiry. This is a major plus for the field as a whole. When Cameron shows how uncertainty affects the politics of presidential vetoes, or when Howell shows how presidents can move policy unilaterally in the face of congressional and judicial opposition, the community of scholars is enlightened whatever their own analytic traditions happen to be. But rational choice is not now the sole source of good theoretical ideas, and it won't be in the future. Ideas can come from anywhere, and good ones can be hit upon by historians, journalists, and empirical researchers just as well as by rational choice theorists—and sometimes better, because they are less constrained by the technical and conceptual apparatus of rational choice. When Kernell wrote his classic book about

presidential strategies of “going public,” he was not proceeding from rational choice foundations. But his work clearly served as an intellectual springboard for Canes-Wrone in her path-breaking moves to develop formal theories of presidential leadership. A diversity of ideas is enormously productive, and its influence works both ways.

Part of the reason that game theory and other formal models from rational choice are not going to dominate the field, then, has to do with the inherent benefits of division of labor. There is more, however. The dominion of rational choice is also limited because it has an important competitor that is angling for influence and working to expand its terrain. This competitor goes by the name historical institutionalism, and in the context of American politics is often referred to as the study of American Political Development. To avoid confusion, I will simply use the former term in reference to both.

The best known study of the presidency to come out of this tradition is Stephen Skowronek’s (1993) *The Politics Presidents Make*, which in the eyes of many is the most impressive book on the American presidency since Neustadt’s (1960) *Presidential Power*. At the heart of this analysis is the idea of “political time,” an analytic stroke of brilliance referring to the temporal recurrence of distinctive structural “regimes”—defined in terms of the “established commitments of ideology and interest” prevailing in a given era that surround, constrain, and (sometimes) empower individual presidents as they endeavor to exercise leadership. Whether presidents are likely to be great, marginally successful, or dismal failures is heavily determined by the regimes they happen to inherit on coming into office—structures that are shaped and transformed through history, that regularly recur in more modern guises, and that individual presidents do not themselves control. Presidents are compelled by their own structure-induced incentives to disrupt and create and lead, and they are the driving forces of political history. But they are also the prisoners of political history, conditioned by what its structures allow

them to do; and their success in office can only be understood from that perspective. It is a structural phenomenon. An institutional phenomenon.

Like rational choice, historical institutionalism casts a wide net. As a distinctive school of theory and research, it is interested in explaining the development, operation, and effects of political institutions generally, and over the last two decades or so has grown into a sizeable body of literature with highly respected leaders and proponents (Pierson and Skocpol, 2002; Orren and Skowronek, 2006). Much of the work is comparative, studying institutions cross-nationally or in non-American settings. And of the studies that focus on American political institutions, much of it concerns bureaucracy and the rise of state capacity, but has typically not been centrally focused on the presidency as an institution, on presidential power or leadership, or other topics long at the heart of presidential studies. Skowronek's work on the presidency is something of an exception (yet see James, 2000, 2005). This is changing, however—partly because Skowronek's analysis has begun to create a new line of research, and partly because historical institutionalism is continuing to grow and attract talented minds (Skowronek, 2008; James, forthcoming).

One of the great strengths of historical institutionalism is that it does what rational choice is not well equipped to do, and attempts to fill in the knowledge gaps that are so clearly in need of systematic scholarly attention. Its perspective on institutions is, as the name would imply, historical: concerned with the often drawn-out dynamics of institutional origins, the determinants of institutional stability and change, the central roles of power and ongoing struggle, and the often profound implications of chance and contingency—a constellation of elements that, when pulled together into a coherent perspective on institutions, is well beyond what the formal models of rational choice can handle. In the process, this school has self-consciously developed (or in some respects borrowed) a distinctive set of analytic tools for gaining leverage and insight into these subjects—understanding aspects of institutions and history by

reference to critical junctures, policy feedback, path dependence, and other concepts that are now emblematic of its approach.

Yet there is also a debilitating downside. Although the concept of path dependence is certainly amenable to clear, analytically hard-edged use—it was originally introduced, explored, and (in certain economic applications) formalized by economists (see David, 1985; Arthur, 1994), and has been insightfully extended to political institutions by Paul Pierson (2004)—many of the concepts at the heart of historical institutionalism are very fuzzy indeed. To say, as Skowronek does, that a regime is defined by “established commitments of ideology and interest” leaves that concept virtually undefined, and the theory difficult to operationalize and test. Much the same is true for many of the terms of trade that make this school what it is: the ideas are sensible in the abstract and seem to have potential for theory, but their meanings are not precise enough to allow for rigorous analysis, and the aggregate effect is stultifying to a science of institutions. Rather than moving toward clarity and rigor, moreover, historical institutionalists have often moved in the other direction: inventing a language of their own—about “multiple institutional orders,” institutions “colliding,” and the like—that is so obscure and ponderous that this school sometimes seems to be running away from science.

What historical institutionalism needs for analytic success are the very qualities that have fueled the success of rational choice: clarity, precision, rigor, and deductive power. On the other hand, the mission of historical institutionalism, which is to bring social science to the study of political history, and to bring the great value of history—and its study of dynamics, timing, and contingency—to an understanding of institutions, targets a whole realm of theory and substance that rational choice has been woefully unsuccessful at dealing with, and indeed is simply not built to handle very well. From a scientific standpoint, then, there appears to be a definite need for what historical institutionalism is trying to do.

So this, too, is a reason why rational choice has not dominated to the exclusion of other approaches—and why it won't in the future. It has a competitor that is clearly onto something: something that strikes right at rational choice's own weaknesses. How is this situation going to work itself out, then, going forward?

On the surface, it might appear that there is some sort of convergence in the works. Historical institutionalism is nothing if not eclectic, and its contributors have long taken advantage of important concepts within the rational choice tradition that they think are useful for understanding institutions. Rational choice is far less eclectic, because it has a hard technology at its core that constrains how liberally it can borrow from other approaches. Even so, in recent years rational choice theorists have been involved in high-profile efforts that recognize the importance of studying institutional dynamics over time. Some projects have attempted to show that game-theoretic models, while not really dynamic and not really designed to explain out-of-equilibrium behavior, can still be used to provide theoretically informed and analytically structured approaches to the study of history (Bates et al., 1998; Grief and Laitin, 2004; Greif, 2006). In other efforts, rational choice theorists have explicitly sought to build bridges to historical institutionalism through joint projects, attempting to show that the “dissimilar strengths of these ‘schools’ can advance each other’s agendas, some aspects of which have been converging” (Katznelson and Weingast, 2005, p.1).

The coming years will doubtless yield more of both: more imperialism by rational choice in the realm of historical analysis, accompanied by heightened attempts to build bridges to historical institutionalism. But neither addresses the key problems that stand in the way of theoretical progress. Rational choice is the dominant mode of theory building in political science, as well as in the study of the presidency, because it possesses far greater analytical strengths than any of the alternatives, including historical institutionalism. Yet it also has serious weaknesses, and these weaknesses are not

overcome by trying to adapt game theory to the study of history. The weaknesses are built in—and they extend well beyond rational choice’s difficulties in dealing with history.

The progress of theory in future years, as well as the prominence and centrality of rational choice, will turn on how these weaknesses get addressed.

The Longer Run

And they *are* being addressed. In social science more broadly, rational choice is actually in the process of fighting for its own survival. This struggle is still in its early stages, and it has barely shown up so far within political science, where rational choice is probably still on an upward trajectory. But the forces of contention are building, and they will soon spill over into our own discipline as well.

Rational choice is being challenged by two powerful bodies of scholarship that have been gathering steam year by year. One targets the microfoundations of rational choice. It traces its roots to Herbert Simon, who argued that cognitive limitations hold the key to human choice, and that these limitations can be studied, discovered—and modeled. Simon’s ideas gave rise to a lively research program that, through the use of controlled experiments, has been extraordinarily successful over the decades in building an empirically grounded science of individual choice (Bendor, 2003).

Propelled by the pioneering experiments of Kahneman and Tversky on the psychology of choice (e.g., Kahneman, Slovic, and Tversky, 1982), this literature has done far more than show that people fail to meet the stringent optimization and calculation requirements of standard rational choice models—which is rather like shooting fish in a barrel. It has also shown that people depart from classical assumptions in systematic ways: ways that allow them, given their cognitive limitations, to cope with complex environments (and that may be due, more fundamentally, to the way humans have been wired via evolution to survive—see, e.g., Buss, 2007). While the science has a way to go, the findings strongly indicate that humans are adaptive decision makers who follow simple rules (heuristics), engage

in simple forms of learning (such as trial and error and imitation), are given to certain biases (due to the framing of issues, for instance), place greater weight on losses than gains, and embrace values (a belief in fairness, most notably) that incline them toward cooperation (Thaler, 2000; Hastie and Dawes, 2001).

This research movement is now huge and diverse, and is in the process of taking on the analytic core of rational choice via an insurgency that is increasingly pressing its fight within economics itself. Today, “behavioral economics” is attracting some of the best, most creative minds in that discipline (Camerer, Loewenstein, and Rabin, 2003)—and they are on their way, eventually, to a radical restructuring of economic theory. Political scientists won’t be far behind.

But how to get from micro to macro? This is where the second research movement comes into play. A great strength of traditional rational choice is that its extreme assumptions allow for the mathematical derivation of solutions, in the form of equilibria that represent social outcomes. But once these extreme assumptions are replaced by more realistic alternatives, and especially if actors are allowed to interact with one another and to learn and adapt over time, the traditional method of paper-and-pencil modeling essentially breaks down. The problems get too complex, the math impossible.

But not for computers. It is no accident that Simon himself became a computer scientist later in his career, exploring ways to harness the power of computers in modeling individual choice (Newell and Simon, 1972). Early organization theorists, embracing Simon’s basic understanding of choice, blazed the same path in building the first computer models of formal organizations (e.g., Cyert and March, 1963; Cohen, March, and Olsen, 1972). In the decades since, enormous effort has been invested in putting computers to use in modeling social behavior, and the research literature has grown tremendously in scope, sophistication, and genuine influence.

The specifics can vary from analysis to analysis, because computers are so powerful and so flexible that they can incorporate almost any theoretical structure, however complicated, and work out

the implications. Yet the literature, as it has developed, reflects a great deal of coherence. Its micro foundations are taken directly from the Simon-inspired research literature we just discussed: actors are adaptive, learn in simple ways, and so on. And the analytic world they investigate is characteristically complex (compared to rational choice) and dynamic: inhabited by large but finite numbers of these adaptive actors who interact with one another over time according to certain rules, generating aggregate social phenomena—patterns, structures, outcomes—that may or may not represent equilibria. Indeed, what is often most interesting and instructive about these models is not the equilibria themselves (if they exist), but the process of getting from A to B—and the ability to understand why people go to B rather than C. Unlike rational choice, the process matters and is fundamental to the theory and its explanation (Epstein, 2006; Miller and Page, 2007; Kollman and Page, 2006).

Variations on this approach have been used to study a broad range of important topics: cooperation, social networks, segregation, ethnocentrism, Tiebout competition, social norms, civil violence, social classes, employment, voter turnout, party competition—the list could easily go on—as well as standard topics in economics, such as financial markets and labor markets (see, e.g., Hammond and Axelrod, 2006; Kollman, Miller, and Page, 1997; Bendor, Diermeier, and Ting, 2003; Laver, 2005; Tesfatsion and Judd, 2006). Despite the already extensive applications, these “agent-based” models usually begin (as I’ve noted) with individuals as their units of analysis and thus move from the mass level to higher-order social outcomes; and this being so, their standard set-up is currently not designed to address topics that many students of political institutions want to know about, which have to do with existing formal institutions—the presidency, the Congress, the bureaucracy, the courts—and the dynamics and consequences of their interactions. Skeptics might even argue that the Simon microfoundations are somehow less necessary or appropriate here, as the “actors” actually represent collectivities. Yet the analytics of computational modeling can easily be adapted to explore these

existing institutions and their dynamics (see, e.g., Bendor and Moe, 1985). And the same microfoundations are still warranted: for the decisions of collective “actors” still arise from the decisions of real human beings (via internal processes that can themselves be modeled computationally)—and it is clear that these collective “actors,” too, are imperfectly informed, adapt their behavior over time, and learn from their experiences.

Especially when it comes to the analysis of formal political institutions, this is a movement that is still in its early stages. But its potential is truly impressive, and it is destined to be unleashed with increasing force as more and more scholars recognize its power and get on board (e.g., Buchanan, 2007). The fact is, it represents a full-blown analytic alternative to traditional rational choice. It is rigorous. It is deductive. It is mathematical. It generates testable implications. It is amazingly flexible. And it brings all these scientific properties to the analysis of the kinds of complexity and dynamics that are fundamental features of society—but that rational choice cannot deal with very well. It is strong where rational choice is weak.

For those who might think that rational choice as we know it will somehow be saved by “evolutionary game theory”: think again. While the latter does bring the logic of game theory to bear on dynamic interactions—a very important objective—it actually had its origins in the study of biology and Darwinian evolution, not standard economics; it has been extended to humans through the use of Simon-type microfoundations; and it relies heavily on computational modeling to generate outcomes and explanations. So evolutionary game theory saves and extends essential aspects of the logic of rational choice, but it does so by jettisoning the formal strictures that make standard rational choice what it is, and by embracing the new analytics we’ve just discussed (see, e.g., *Stanford Encyclopedia of Philosophy*, 2003; see also Axelrod, 1984, 1997).

Rational choice as we know it is unlikely to die out. It has major strengths, and in future years there will no doubt continue to be domains of behavior that it explains reasonably well. But dark clouds are already gathering, and they are in the process of unleashing the perfect storm: two imposing challenges to its analytic superiority. One provides microfoundations grounded in research and reality. The other provides a formal, computational methodology for moving from micro to macro. And the two are entirely complementary, fitting together hand in glove to provide a coherent, empirically warranted, analytically powerful alternative—one that, with further development over the next decade or two, is likely to become the dominant approach to theory in much of the social sciences. Including political science. And presidential studies.

Conclusion

When I say that there has been a revolution in presidential studies, then, I am not saying that rational choice has finally triumphed, and that it will only expand its dominion in the years to come. Far from it. Rational choice has served as an *agent of change*. That has been its key role here, and it is a very important and positive one. Presidential studies was for decades mired in traditionalism, a theoretical backwater isolated from the rest of political science and from cutting-edge developments in the study of political institutions. Rational choice has literally transformed the field, vaulting it into the scientific mainstream and making it one of the most innovative areas of inquiry in the entire discipline.

What presidential studies needed, most fundamentally, was stronger analytics: a methodology that put the emphasis on clarity, precision, deductive power, and other essentials of genuine theory. Rational choice provided that. Its analytics were simply superior to what had prevailed in years past—approaches that buried the field in facts and detail—and the superiority paid off in results: with its young theorists generating a spate of new studies on substantive issues long central to the field, from the presidential veto to unilateral action to the administrative presidency to going public. Its analytics were

also superior to those of historical institutionalism, its prime competitor in the larger realm of institutional theory. Because despite the insightful ideas that historical institutionalists have produced over the years, and despite their concern for scientific principles, their research program remains analytically weak. Much weaker than that of rational choice.

But this imbalance is destined to change. New analytics are barreling down the tracks, and they are going to trump the very strengths that have allowed rational choice to attain its position of dominance. The new approach has virtually all the advantageous scientific properties that have propelled rational choice's success. But it is rooted in the science of how people really do think and make decisions, rather than in fabrications that are essentially just mathematical conveniences. And its computer-based technology is far more flexible in the social problems it can handle, in the "virtual experiments" it makes possible, and in its capacity for modeling social complexity, exploring over-time dynamics, and investigating out-of-equilibrium behavior. In a host of ways, it allows political scientists to explore and explain substantive aspects of politics and society that are simply beyond rational choice.

Needless to say, these new analytics stand to empower historical institutionalism. Up to now, its methodology has looked a lot more like history than an engine for rigorous, deductive theory. But that engine is suddenly at hand: a set of analytic tools just as powerful as rational choice, but perfectly suited to the exploration of critical junctures, path dependence, policy feedback, and other historical dynamics—including Skowronek's concept of political time—that appear central to an understanding of the stability and change of political institutions. It may well be that most of today's historical institutionalists are not very interested in formalization. But the technology exists, and in time it will surely be put to use—perhaps by a new class of recruits—to bring about a much more scientific body of theory on the dynamics of political institutions. One that rational choice could never have produced.

The larger value of these analytics, however, is not that they empower historical institutionalism per se. It is that they empower political scientists of diverse theoretical stripes, almost whatever they might be—including those embracing a reconstructed version of rational choice—to model their ideas, pursue them in new rigorous ways, and conduct their inquiries within a common analytical framework in which basic components are shared. This is the convergence that is destined to be the theoretical wave of the future. And it is very different from, and much more powerful than, the kind of convergence that rational choice theorists and historical institutionalists are now experimenting with.

Years ago, presidential studies was not much affected by what was happening in economics or cognitive psychology, or even what was happening in the rest of political science. But that is not true anymore. The field is now an integral part of theoretical developments throughout social science, and it cannot help but be shaped by the new analytics as they come online. Rational choice theory is responsible for the dawning of this era, and it deserves the credit. But its methodology is not the future of presidential studies. The future of the field is more expansive, more diverse, and more eclectic than the strictures of rational choice could ever allow—and better suited to scientific inquiry and progress.

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