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APD and Rational Choice

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[–] Abstract and Keywords

Rational choice and American political development (APD) both emerged as responses to (perceived) limitations with the dominant behavioral tradition. While their critiques were based on very different research traditions, similarities were also present; in particular, both rational choice and APD approaches focused on the importance of *institutions* for studying political outcomes. Over time, rational choice and APD research has converged to a significant degree, as scholars in both traditions have increasingly been exposed to different theoretical and methodological perspectives and thus become consumers of each other's work. This chapter documents how and why rational choice research has moved in an APD direction.

Keywords: rational choice, historical institutionalism, anti-behavioralism, equilibrium institutions, structures, rules, procedures, processes, defining American political development, methodological pluralism

It was not so long ago that “rational choice (RC)” and “American political development (APD)” were almost wholly separate research enterprises in political science. Interestingly, both emerged as reactions to the dominant “behavioral paradigm” of the mid-twentieth century. Both RC and APD scholars believed that behavioral research, which grew out of the sociological and social-psychological traditions and assumed that individuals responded reactively to larger forces and pressures around them (and thus assumed that political outcomes were the result of social roles, psychological cues, and/or group-based norms), left out key ingredients that were important for understanding the political world. RC scholars in the 1960s and 1970s focused on individual agency as the missing ingredient and recast political actors as proactive (or purposive) agents, with distinct preferences, beliefs about the political world, and the ability to act on those preferences and beliefs toward some chosen goal(s). By the late 1970s, RC scholars identified another missing component in behavioral research—the lack of a role for *institutions* in the production of political outcomes. The focus on institutions (formal/informal structures and processes) would come to play a privileged role in RC scholarship and, in short order, transform the field (Aldrich 1994; Shepsle 1989). APD scholars of the 1980s and 1990s, on the other hand, focused on a different set of (perceived) limitations in behavioral research. The first was the lack of attention paid to history, and the ways in which the historical political context or historical power relationships in society affected outcomes (and were shaped by outcomes). The second, which was related, was the absence of any reference to (or awareness of) the “state”; APD scholars believed that policy processes and outputs were inextricably linked to the macro-political-economic environment and the constellation of political and economic forces in society (that developed and evolved in an historical way). In this way, APD scholars also pointed to institutions as crucial for a fuller understanding of politics (Evans, Reuschmeyer, and Skocpol 1985; Orren and Skowronek 2004).

While RC and APD scholars had the same behavioral foil, and seemed to share an affinity for institutions, they did not regularly engage one another. Much of this was likely due to methodological differences (see Swift and Brady 1994). Rational choice scholarship descended from operations research and economics; research was deductive in nature, often employing formal (mathematical) models, and hypotheses were derived and tested with large-*N*

datasets using advanced statistical techniques. APD scholarship, by contrast, descended from history, sociology, and democratic theory; research was inductive in nature, often employing qualitative techniques like thick description and process tracing in a narrative-based, non-hypothesis-testing format. In effect, RC and APD scholars spoke different languages. And since both were the new kids on the block, and relative adolescents as intellectual traditions, they did not always play well with those unlike them. Over time, RC scholars, the larger of the two groups, sought to vie with behavioralists for disciplinary hegemony. And while they succeeded in various colonization efforts, they could not fully vanquish their behavioral foes; today, RC and behavioral scholars maintain a rough equality in the halls of research universities in the United States. APD scholars, while considerably smaller in number, have grown steadily over time; moreover, many APD scholars (especially those from the first generation) have embraced their outsider position and happily viewed the APD enterprise as an intellectual “insurgency” (see Bensel 2003).

While pride of intellectual tradition is still strong today, and tensions between the camps still exist,¹ communication between RC and APD scholars has improved considerably. Some of this is due to generational replacement, as younger scholars are often exposed to both types of work in their graduate school curricula (while also receiving broad methodological training). Some is also probably due to top scholars in both traditions paying homage to research endeavors in the other camp (see, e.g., Orren and Skowronek 2002; Swift and Brady 1994). Finally, and related to the prior two points, the intellectual “culture wars,” for all intents and purposes, are over. The “pathologies” of rational choice have been aired and disputed (Green and Shapiro 1994; Friedman 1996), the *Perestroika* movement has come and (largely) gone (see Monroe 2005 for the last set of organized thoughts), and a period of relative calm has ensued. Does everyone agree that the right level of “methodological pluralism” has been achieved? Probably not. But the feverish days of intellectual battles seem to be behind us (at least for now). In my view, and this is purely impressionistic, there seems to be more tolerance today than a decade or two ago.

Part of that tolerance, I believe, has come from a willingness to learn across boundaries, which has fostered a healthy respect for those with alternative ideas and approaches. A case in point, in my own experience, has been the Congress and History Conference (CHC), the brainchild of Ira Katznelson. Now a decade old, the CHC brings together congressional scholars from the rational choice, APD, and historical traditions. I have had the pleasure of attending each CHC, and the opportunity to learn from scholars like Katznelson, Richard Bensel, Rick Valelly, and Elizabeth Sanders has helped shape the way that I approach research questions. In short, engagement and communication with APD scholars has broadened my intellectual perspective and made me a better scholar.

I will have more to say about the shrinking gap between RC and APD scholarship later in this chapter. My primary focus in the following sections will be to detail the inroads that RC scholars have made into the study of APD, which I will initially define as the “inquiry into temporal aspects of governance” (Orren and Skowronek 2002, 722). Two decades ago, there was almost no RC research with APD overtones. Now there is a considerable amount, and the trajectory of RC-based APD work is quite positive. Part of this growth, as I note above, is due to the blurring of boundaries, thanks in part to better communication and active engagement. But for such communication and engagement to take place, an initial foundation of RC-based APD research had to emerge. Once a credible commitment to elements of the APD agenda (sensitivity to historical context, respect for theoretical/methodological concepts like timing and sequence, etc.) was made, the basis for common ground was created, and a serious and fruitful dialogue could then begin.

In describing how RC scholarship moved in an APD direction, I will mostly discuss examples from the Congress and parties literatures, simply because this is where RC-based APD work has had its greatest purchase.² But before doing so, a preliminary step is necessary; specifically, to understand the RC move toward APD, we must first understand the evolution of RC as a field.

Rational Choice and the “Discovery” of Institutions

In this section, I provide a short overview of the evolution of the RC field. As the RC literature is more than a half-century old, my coverage will be obviously (perhaps embarrassingly) brief.³ But my main goal here is not to provide a comprehensive intellectual history, but rather to lay out how (some) RC scholars gravitated toward APD. The answer will be through the (re)discovery of institutions.

As mentioned previously, RC had its origins in the economics and operations research literatures. The RC

approach was in many ways the polar opposite of the prevailing behavioral approach of the time. While both approaches took the individual as the base unit of analysis (or theoretical building block), the RC approach replaced behavioralism's "passive man," nudged by his surroundings toward some outcome, with "active man," who possessed well-defined preferences and beliefs about the world around him, and selected strategies that maximized his likelihood of achieving his goals (most-preferred outcomes).⁴ Regarding preferences, RC theory assumed that an individual was self-interested and purposive (and sometimes a "maximizer"); "self-interest" did not necessarily mean "selfish," however, as an individual could greatly value benefits that accrued to those beyond him (i.e., social benefits). More important was that basic self-interest in a given study be clearly identified—in congressional studies, for example, a common RC assumption has been that a member of Congress seeks to maximize his chance of reelection (Mayhew 1974).

RC theory differed from behavioralism in another important way: it was deductive rather than inductive. RC theory was built abstractly, using premises and assumptions, and hypotheses were derived from the theory (and often tested). A theoretical outcome was said to be an *equilibrium* if it was stable, which typically meant that no decisive coalition—often a simple majority in most decision settings—preferred a different outcome. While much RC scholarship over the years has been informal (i.e., logic-based), the deductive nature of the RC enterprise led to various formal (mathematical) representations. Decision- and game-theoretic models became popular, while the spatial model of decision making—where, in its simplest form, actors (voters, legislators) and alternatives (candidates, policies) are arrayed from left to right along a line—emerged as the "workhorse" model in RC studies.

Early RC scholars focused considerable attention on the dynamics of democratic decision making. At issue was an early axiomatic result by Arrow (1951) that suggested that no democratic voting method (no method of aggregating preferences) was "fair"—that individual rationality did not necessarily lead to group rationality.⁵ In response, Black (1948, 1958) and Downs (1957) showed that if the number of decision makers were odd, decision makers' preferences were well ordered ("single peaked"), and decisions were made along *one* dimension of choice, then the median voter's most-preferred policy (his "ideal point") was an equilibrium outcome in a majority-rule setting—it was a "Condorcet winner," in that it could beat *any* other alternative in pair-wise voting. The median voter result would, in time, transcend academia and become part of the public lexicon, as politicians and journalists would discuss how important it was for political candidates to "move to the center" or "capture those in the middle" in advance of an election.

By the 1960s and 1970s, formal theorists working on democratic decision making (i.e., social choice theorists) sought to move beyond the single-dimensional world of Black and Downs. The belief was that democratic politics could not typically be reduced to one dimension of policy choice, and that decision-making models—to be realistic—needed to incorporate additional (higher) dimensions. When a second dimension was added, the median voter result effectively vanished. Except under very rare circumstances (Plott 1967), outcomes in two dimensions exhibited no stability—no equilibrium existed, as any majority-preferred outcome was susceptible to being overridden by a new majority (composed of, in part, losers from the prior round). Moreover, as McKelvey (1976) showed, any outcome in the choice space could potentially be reached. The possibility of "chaos" in majority-rule decision making was real, as conditions that allowed for instability, agenda manipulation, and cycling were pervasive.

The problem with the social choice theorists' work was summed up in the title of a paper by Tullock (1981): "why so much stability?" In comparing the academic results to real world voting bodies, Tullock saw a disjuncture: legislatures were able to pass policies into law, and once passed, those laws were quite stable. None of the procedural dysfunctionality and endless cycling that McKelvey's theorem warned about was apparent. Something seemed to be missing from the social choice theorists' work.

Shepsle (1979) provided an answer: the work of Plott, McKelvey, and others had abstracted away too much of the real world's complexity. In their stripped-down models, where political actors were deciding (voting) in a largely structure-free environment, the lack of an equilibrium in preferences alone was indeed the result. But the real world had structure. The real world had *institutions*. With this in mind, Shepsle developed a model of a legislature with a committee system, based loosely on the US House of Representatives. The key features revolved around rules of jurisdiction and agenda control—if individual policies (or individual parts of complex policies) were assigned to individual committees and only those amendments germane to the policy dimension in question were allowed, then an equilibrium could be generated. Institutions (a committee system with jurisdictional arrangements and

amendment restrictions) could *induce* stability and thus help legislators achieve their goals. Shepsle called this *structure-induced equilibrium* (SIE) in contrast to the *preference-induced equilibrium* (PIE) of the largely institution-free social choice research.⁶ Later, other RC scholars turned to a different institution—political parties—as the structural solution to various legislative choice problems (Cox and McCubbins 1993; Aldrich 1995). This RC trend of studying preference choice within a particular institutional context—where the institutions constrain and shape the goals and strategies of political actors—became known as the “new institutionalism.”

This “discovery” of institutions by Shepsle and others was, of course, little more than a rediscovery. In the late nineteenth and early twentieth centuries, an “old institutionalism” was central to the political science discipline. Predicated on constitutional law and constitutional history, this “legal-formal analysis,” practiced by such luminaries as Woodrow Wilson and James Bryce, survived through the early 1950s, when behavioralism took root and reduced its predecessor to marginal status. This old institutionalism had much in common with contemporary APD, as “state-based” structural forms played a major role in its intellectual agenda. Old institutional work was generally expository, however, as scholars were content mostly to describe institutions in detail and not pursue deeper theoretical or empirical treatments (see Peters 1996; Rhodes 2006).⁷

Thus, thanks to Shepsle’s pioneering efforts, institutions came to be viewed as a solution to collective and social choice problems in stylized decision-making bodies. But were they *the* solution to similar problems in real world legislatures? That is, were institutions *chosen* by political actors to help them to achieve their goals? For Shepsle (1986, 1989), then, the question became: how were institutions selected and, once selected, how were they maintained? He called this the search for *equilibrium institutions*. In this endeavor, Shepsle and others were not served by more mathematical equations on a blackboard; rather, they needed to find empirical evidence. A detailed search through history was needed to determine if intellectual conjecture had a basis in reality, that is, whether there were grounds for believing that political actors in the past *purposively* turned to institutions to solve problems of democratic decision making.

Institutional Choice: Structures

Determining the origins of institutions thus became RC scholars’ entrée into APD. Their only other alternative, given the direction that their work had taken them, was to resort to functionalism, i.e., (a) majority-rule decision making was unstable in more than one dimension, (b) institutions were shown to be a solution to this problem, and (c) thus institutions emerged. And that, of course, was not an intellectually satisfying alternative.

In this section, I discuss one form that RC-based institutional-choice research took: structural choice. Indeed, the first major APD forays made by RC scholars focused on the structural makeup of Congress, specifically the emergence of standing committees and institutional political parties. Other questions of institutional choice, like the selection of rules and procedures, will be discussed in the following section.

Gamm and Shepsle (1989) were the first study to explore the equilibrium institutions idea empirically. Not surprisingly, given the structural component of Shepsle’s earlier institutional-equilibrium work, the focus in Gamm and Shepsle (1989) was the emergence of a standing committee system in Congress. While they were unable to find a “smoking gun” (in the form of a letter, diary entry, etc.) in their historical search, Gamm and Shepsle argued that a RC account, revolving primarily around the machinations of House Speaker Henry Clay, provided a plausible explanation for the sudden shift from select-committee government to standing-committee government in the years between 1810 and 1825. Several years later, Jenkins (1998) presented a similar story, but with more specific hypotheses and critical tests.

In short, the Gamm–Shepsle–Jenkins (GSJ) account rested on changing external factors that drove entrepreneurial innovation. Clay became Speaker at a time when war with Britain was in the air; he took advantage of ideological homogeneity within his Republican Party to establish a stable policy agenda, which he used to maintain control of the speakership. When the war ended and the First Party System collapsed, however, Clay was unable to construct a new policy agenda to hold his coalition together; without the Federalists as a viable foil, the Republicans lost cohesion and started to focus on regional issues. This parochialism led to voting (decision making) instability that, in effect, mirrored the multidimensional chaos of the social choice theorists’ world. To solve this problem, Clay reorganized the House’s internal structure by jettisoning the existing select committee system and creating a system whereby standing committees, with strong jurisdictional controls and property rights of assignment, would

handle the chamber's business. With this shift to standing committees, Clay in effect created electorally valuable "turf" in the House, which gave members "quasi-permanent influence on those issues that most concerned them" (Aldrich and Shepsle 2000, 36). Clay parceled out influence in this way to help him achieve his goals: maintaining control of the speakership in an unstable environment, which (among other things) kept him in the running for the presidency in 1824. While, again, GSJ uncovered no smoking-gun validation, Jenkins (1998) did find indirect empirical support (using data on bill referrals, committee assignments, and speakership votes) for all the main elements of this RC account.

Underlying the GSJ account was an assumption that national politics was stable *before* the collapse of the First Party System. Aldrich (1995) entered here to make a case for this assumption; he argued that institutional political parties (i.e., parties in Congress) were created to solve coordination and collective choice problems that hampered decision making in the early Congresses. Specifically, Aldrich contended that the Federalists, led by Alexander Hamilton, recognized that they had a majority of support for the "Great Principle" of the day—how strong the Federal government should be—but were unable to translate that support into policy outcomes. This was because the Republicans, led by Thomas Jefferson and James Madison, were able to inject regional (or secondary) dimensions into debates and votes; this, in effect, made the choice space multidimensional, which the Republicans used to their advantage in stymieing the Federalists. According to Aldrich, Hamilton recognized the problem early on, and set about creating informal structures (caucuses, floor leaders, and whip systems) that would convince Federalists to focus on the Great Principle dimension and ignore attempts to add complexity to vote decisions. Very quickly, Hamilton saw his goal achieved, as the Federalists began winning more often.

For Aldrich, Hamilton did not "create" parties so much as he did "intuit" them. Revolutionary leaders understood (or came to understand) the importance of institutions. It was clear, for example, that the weak institutions inherent in the Confederal system led to its demise; as Jillson and Wilson (1994) documented, colonial representatives were unwilling to cede much authority in the creation of the Continental Congress, and this led to weak institutional structures that could not effectively coordinate shared interests or help overcome collective choice problems. As a result, the Founders built much stronger institutions into the new Federal system. However, with regard to Congress, no mechanism was included to aggregate preferences in the face of potential instability. As a result, a new extralegal institution was developed, as the separate informal structures devised by Hamilton would form the core of an institutional political party.⁸ The Republicans quickly copied these features, in response to the Federalists' newfound legislative success, and an institutional party system was born. Aldrich found support for this story by looking at key votes associated with Federalist fiscal and foreign policy issues—over time, between the First and Third Congresses, party was responsible for structuring more individual vote choices and reducing the influence of non-Great Principle dimensions.

Aldrich (1995) also documented the emergence of mass political parties; here the entrepreneur was Martin Van Buren, who sought to create a new party (and party system) built around Andrew Jackson and based on Jeffersonian principles rather than personal allegiance to elites. Van Buren's goal was to maximize party members' likelihood of retaining office. His tactics included building a national party organization around Jackson, by linking numerous state and local party organizations, and using that national organization to mobilize the electorate behind candidates on the Jackson ticket. Election and re-election meant a steady stream of policy and political spoils. Organization and mobilization were thus the collective action problems that Van Buren solved through the construction of a mass party. Aldrich uncovered evidence to support this RC story by looking at party organizational and turnout data in a variety of ways.

Jenkins and Stewart (2013) also placed Van Buren in an entrepreneurial role a decade after his successful creation of the Jacksonian Party. In this case, Van Buren came to recognize that House officer positions—principally the Speaker, but also the Printer and Clerk—controlled resources that could be used in pursuit of policy and patronage; if secured consistently, these positions could provide substantial benefits for the majority party. The problem was that officer elections exhibited the same type of instability that plagued voting in the early Federal Congresses; in the late 1830s, for example, dissident majority-party members sometimes voted with the minority for ideological reasons, which on several occasions resulted in the majority party losing the officer election. As a result, Van Buren, now president, pushed for the adoption of a public ballot (as the secret ballot had governed House officer elections to that point) and the creation of a legislative party caucus, based on an institution that he used effectively in New York as part of the Albany Regency. With the public ballot in place,⁹ the party caucus would be the venue for intra-party coordination on officer elections. The expectation was that decisions made in caucus

would be honored by all party members on the floor. This was enforced by a system of carrots and sticks: “losers” in caucus were compensated with various benefits (policy, committee assignments), while the threat of being expelled from the party—for refusing to toe the line on the floor—was repeatedly underscored. After some initial successes, the Van Burenites were unable to make the party caucus operate effectively, as slavery became too strong a regional pressure for the party bond to withstand. The seed was planted, however. As Jenkins and Stewart showed, once slavery was no longer an issue, beginning during the Civil War, the Republicans and then later the Democrats used the party caucus to effectively structure officer nominations and floor elections. By the late nineteenth century, the binding party caucus on organizational matters had fully developed into an equilibrium institution.

Institutional Choice: Rules, Procedures, and Processes

As RC research on structural emergence and development took off, a parallel RC literature on institutional choice dealing with rules, procedures, and processes also commenced. This literature was dominated by RC scholars who began taking the historical development of Congress seriously. An assortment of such RC studies has appeared since the late 1980s; I focus below on some of the most important ones.

Around the same time that Gamm/Shepsle were focusing on standing committee emergence in Congress, Stewart (1988; 1989) sought to apply a RC framework to the study of budget reform politics in Congress between 1865 and 1921. Unlike Gamm/Shepsle, though, who focused on committee development as an empirical analogue to the formal literature on institutions, Stewart followed Mayhew (1974) in applying a basic RC assumption of re-election-seeking behavior on the part of members of Congress to an earlier time period. In doing so, Stewart was aware that applying a RC framework to the historical study of Congress was “bound to be controversial” (1989, 9). Thus, apart from his analysis of changes in the House’s appropriations process, Stewart also made an important contribution in explicating why an assumption of goal-directed behavior was tenable in earlier congressional periods.

Substantively, in a set of empirical case analyses, Stewart showed that moments of fragmentation and centralization in the House budget process followed from the interaction between the decision context and members’ spending preferences. When the economy was flush and the existing committee system overworked, legislators with a high demand for particularistic policy (as a way to meet constituents’ interests) sought fragmentation, to better increase spending and expand policy outputs. This was the case in the 1870s and 1880s, when the Appropriations Committee was stripped of most of its spending jurisdictions. When the economy was stagnant and deficits existed, legislators with a low demand for particularistic policy sought centralization, to better control spending and limit policy outputs. This was the case right after World War I, when the Budget and Accounting Act was considered and eventually passed. In creating a “marriage” of RC theory and detailed historical analysis, Stewart set a standard for the study of institutional choice (and change) in Congress.

Binder (1996) shifted the focus away from process by examining procedural choice in Congress from 1789 through 1823. Building off the purposive approach of Gamm/Shepsle, she outlined how a majority party that was unable to secure its preferences directly might seek to change the rules—in this case, by restricting minority rights—to achieve its goals. In looking at votes on the previous question motion (which allowed a majority to cut off debate) in the House, Binder found that partisanship, and not “increased workload” (which was tied to an older sociological literature), was a significant determinant—specifically, the previous question motion was altered when parties were homogeneous and polarized from one another. Binder (1997) expanded this line of reasoning, both in terms of articulating when minority rights generally should be suppressed (when majority party strength was high and a short-term advantage could be achieved) and across a longer swath of congressional history (200 years). Moreover, she argued that inherited rules were also important to consider as a contextual factor in studying institutional change; for example, the strong minority rights in the Senate resulted from particular decisions (like the elimination of the previous question rule) made early in that chamber’s history.

Dion (1997) covered similar ground as Binder, by looking at minority rights restrictions from the 1830s through the 1890s. Using newly collected data, Dion tested formally derived hypotheses—in both the traditional statistical sense, as well as through detailed case studies—and found that cohesive majorities were more likely to provoke obstruction from the minority, and accordingly, cohesive majorities were then more likely to push for procedural change to limit the minority’s right to obstruct. Where Dion differed from Binder was on how the size of the majority

correlated with procedural restrictions; for Binder, larger majorities sought to restrict minority rights, while for Dion it was smaller majorities. Binder's finding (using a longer time series) was purely empirical, while Dion's empirical finding was consistent with formally-derived hypotheses. Beyond his careful formal and empirical RC analyses, Dion actively reached out to the APD community, stating that he "[tried] hard to take the historical side of the enterprise as seriously as the formal side" (xii–xiii).

Adler (2002) tackled the issue of institutional choice in a different way by looking at *failed* changes. In doing so, he explored a different element of the equilibrium institutions question: why institutions maintain themselves (in the face of reform efforts) over time. Specifically, Adler investigated why the basic structure of the House committee system remained largely intact since World War II despite significant attempts at reform in the 1940s, 1970s, and 1990s. Adler's thesis, like Stewart's, revolved around the electoral connection; he argued that since reelection-seeking members of Congress had learned how to serve the needs of their constituents through the committee system, they were unwilling to support any efforts to shake up the current arrangement. Using a variety of socioeconomic and financial data, Adler showed that distributive benefits and committee membership were indeed linked. And using case studies, he showed that members of Congress were risk averse when it came to reform, willing to accept a suboptimal status quo system rather than incur the costs of learning a new, potentially better system (with only two years between election cycles). Adler thus argued that uncertainty (and the potential costs therein) led to institutional stasis.

Cox and McCubbins (2005) investigated the emergence of the Reed Rules in the early-1890s House. At issue was the chamber's outdated system of procedural rights, which the minority used effectively in the 1870s and 1880s to thwart the legislative momentum of the majority. Cox and McCubbins referred to this period as a "dual veto system," as both the majority and minority parties ostensibly possessed agenda vetoes, and majority gains often came only with minority concessions. Thomas Reed, after becoming House Speaker, reformed the system by remaking the rules—minority rights were restricted and majority rights were enhanced. This destroyed the prevailing dual veto system, and the majority party was quickly transformed from a procedural lightweight into a procedural heavyweight. And as Cox and McCubbins showed in a time-series analysis, the majority party's ability to control the agenda in a *negative* way, by preventing legislative change that a majority of its members would find distasteful, spiked with Reed's innovations and has remained strong (regardless of other procedural changes made over the years) through the present day.

Wawro and Schickler (2006) revisited the earlier procedural debate, by exploring the lawmaking environment in the Senate across time. At issue was how the Senate was able to legislate in the nineteenth and early twentieth centuries when there was no rule in place to cut off debate. They argued that lawmaking during this time was akin to a game-theoretic "war of attrition," whereby minorities and majorities would convey information and "preference intensity" during debate. Except in rare situations, majorities (even narrow ones) would eventually work their will because the threat that a majority could change the rules—directly in a formal way or indirectly through precedent—and weaken or destroy minority rights was very real. Eventually the growing size of the Senate and its increasing workload made continued operation under the existing informal system too costly, and a formal rule to invoke cloture (shut off debate) was adopted in 1917. To support their argument, Wawro and Schickler used a combination of spatial models, quantitative techniques, and qualitative case studies. And in arguing that Senate rules were in effect governed by majority choice, they took a provocative stance, opposing Binder (1997) and others who emphasized path dependence and inherited rules that enabled Senate minorities to prevent rules changes preferred by majorities.

Moving the Goal Posts? Redefining APD

Recall that I had initially adopted Orren and Skowronek's (2002, 722) definition of APD as the "inquiry into temporal aspects of governance." This definition had an inclusive quality, and allowed for a fairly seamless discovery of RC-based work with an APD flavor. More recently, Orren and Skowronek (2004) proposed a more exclusive definition of APD. This was due in part to their desire to establish (or perhaps better clarify) APD as a unique intellectual enterprise and subfield. In responding to various critiques of APD, Orren and Skowronek (2004, 121) noted that "in APD research today, there is more at stake than political change in the past and strategic interactions in historical context."¹⁰ As such, they shifted gears and redefined political development as

a durable shift in governing authority. By ‘governing authority’ we mean the exercise of control over persons or things that is designated and enforceable by the state. By ‘shift’ we have in mind a change in the locus or direction of control, resulting in a new distribution of authority among persons or organizations within the polity at large or between them and their counterparts outside.

(Orren and Skowronek 2004, 123, emphasis added)

Some might contend that this shift is akin to “moving the goal posts.” Based on this narrower definition, for example, many of the RC studies of intra-institutional change that I described earlier would probably not be recognized as “APD research.” Indeed, RC work is not naturally suited to the “sweeping impact” that seems necessary under Orren and Skowronek’s new definition—because RC research is often theoretically tied to formal concepts like “equilibrium” and empirically tied to establishing clear causal relationships, its scope (or frame) is often limited (or modestly drawn). That said, and apart from other concerns relating to the narrowing of the definition, my goal in this section is to ask simply: does RC work that satisfies this more exclusive APD definition exist? The answer is “yes,” and I provide two examples below.

The first example is a substantive one, and deals with how the Republicans were able to protect the policies they enacted during the Civil War and Reconstruction as the Democrats reemerged as a major player on the national stage in the 1870s. Stewart and Weingast (1992) couched their analysis in the growing institutional literature of the time, but emphasized the role of *electoral institutions* (rather than legislative institutions of the SIE revolution). They argued that the Republicans, beginning in the Civil War era, used “statehood politics”—or the process of bringing new states into the Union—in a strategic (partisan) way to increase their share of congressional representation. This “artificial representation” in Congress allowed the Republicans to counter the Democrats’ growing influence in the nation (which was also partially artificial due to efforts to disenfranchise African Americans in the South). Engstrom (2006) extended the Stewart/Weingast argument by detailing how Republicans’ strategic decisions at *the state level*—notably gerrymandering efforts in the redistricting process—were designed to provide a partisan bias and increase the GOP’s representation in the late-nineteenth-century House. Finally, Jenkins (2007) described how the Republicans in the late-nineteenth-century House used “disputed” elections (based on alleged voting irregularities) to strategically flip Democratic seats into Republican seats, thereby increasing their working majority in the chamber. These contested (disputed) election cases also served as an “equalizer” to combat the Democrats’ use of fraud, corruption, and violence in Southern elections and helped the Republicans maintain a foothold in the former Confederacy.

The second example is related to Congress’s impact on other facets of the state. In particular, a literature has emerged to investigate how and why Congress has helped to develop the Executive branch over time. The path of such development has often been tied to members’ goals. For example, Johnson and Libecap (1994) argued that members of Congress first passed civil service reform (via the Pendleton Act of 1883) as a strategic initiative to facilitate their re-election efforts—with skilled politicians in the civil service, members of Congress could better manage their remaining political appointees and ensure that the needs of their constituents were met. Theriault (2003) built on Johnson and Libecap by recasting the role of the electoral connection; he found that civil-service reform was driven as much by direct public pressure (following President Garfield’s assassination by a disgruntled bureaucratic office seeker) as by the strategic machinations of members of Congress. Kernell and McDonald (1999) also weaved an electoral connection story in discussing the transformation of the U.S. Post Office from patronage to service in the late nineteenth century—fourth class post offices were dismantled and replaced with “rural free delivery” as a way for members to meet the needs of their constituents and curry electoral favor. Gailmard and Patty (2013), on the other hand, focused on members’ *policy* goals to explain why Congress has delegated substantial authority to the Executive branch over time (and created “the institutional presidency”). Because the president and federal bureaucrats possess informational advantages, Congress has had an incentive to ensure that Executive branch officials acquire expertise so that “good” public policies (consistent with Congress’s preferences) are produced.

What Does the Future Hold?

Regardless of how APD is defined, RC scholars will almost certainly continue taking history seriously—both in general and as a methodological concept. Thus, the sorts of work that I have discussed on institutional choice will

only increase in number. And the continued efforts of RC scholars to produce work that might speak to “a durable shift in governing authority” will only increase as well. RC-based APD work, in whatever form, is here to stay.

But the larger and more important point, in my mind, is that the gap between RC scholarship and traditional APD scholarship is shrinking. As I noted previously, whereas scholars of institutions a generation or two ago might have sorted pretty seamlessly into RC or APD camps, today the story is different. Students now receive broad training almost as a matter of course, and are exposed to a range of theoretical and methodological perspectives.

This “narrowing of the gap” between RC and traditional APD can be illustrated in two ways. First, while this chapter has documented the movement of RC in an APD direction, the reverse has also been true. Some of the best APD work in recent years, while firmly ensconced in historical-institutionalist or historical-interpretivist traditions, has also embraced some RC tenets as well. For example, the acceptance of individual agency, or purposive action on the part of political actors, is pretty standard now—and can be found in James’s (2000) work on presidents and the politics of regulatory choice, Schickler’s (2001) work on the institutional development of Congress, Carpenter’s (2001) work on bureaucratic autonomy, Orren and Skowronek’s (2004) foundational APD work, Valelly’s (2004) work on black enfranchisement across time, Galvin’s (2009) work on presidents and national parties, and Crowe’s (2012) work on the development of the federal judiciary.

Second, RC and traditional APD scholars are often working on the same questions, and increasingly engage each other in their research. For example, just as RC scholars like Gamm/Shepsle, Aldrich, and Jenkins/Stewart have focused on “political entrepreneurship” to help explain institutional development over time, so too have traditional APD scholars like Sheingate (2003), Strahan (2007), and Crowe (2007), who have explored the topic in depth. Indeed, it is fair to say that APD scholars are further along than RC scholars in the development of a *theory* of political entrepreneurship. In addition, just as Shepsle/Weingast, Engstrom, and Jenkins have focused their efforts on a particular shift in postbellum governing authority, so too have APD scholars like James and Lawson (1999) and Gillman (2000), who have examined the Republicans’ strategic deployment of Federal election officials and deputy marshals and strategic expansion and staffing of the federal court system, respectively, in the late nineteenth century. The James/Lawson and Gillman analyses nicely complement the RC analyses and help provide a fuller view of the GOP’s strategic initiatives in the decades after Civil War and Reconstruction.

In sum, the future for RC-based APD work is bright. However, I also believe that we are not far away from dropping the adjectives that adorn our substantive interests. That is, while we may currently employ labels like “rational-choice institutionalists,” “historical institutionalists,” and “institutionally-focused APD scholars,” I envision a time when we simply call ourselves “institutionalists.”¹¹ And, as institutionalists, the fact that we possess historical sensibilities will be assumed.

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Notes:

⁽¹⁾ One such tension is in the way political actors' preferences have been treated by RC and APD scholars. APD scholars have criticized some RC scholars for treating political actors' preferences as given (or imputed) rather than as historically situated and constructed; likewise, RC scholars have criticized some APD scholars for downplaying the role of political actors' preferences in favor of (outsized) attention to macro-level tracing of history

and process. Katznelson and Weingast (2005, 6) acknowledge this tension but also contend that the aforementioned criticisms are “out of date,” as scholars in both traditions have increasingly converged thanks to a greater focus on institutions.

(²) RC-based APD work in other fields clearly exists, such as Moe’s (1985) work on the development of the institutional presidency, Harvey’s (1998) work on the development of organized/electoral interests, and Cameron’s (2005) work on judicial state building.

(³) Short overviews can be found in Riker (1990), Ordeshook (1990), and Aldrich (1994). For a detailed, accessible primer on RC theory, complete with substantive applications, see Shepsle (2010).

(⁴) Shepsle (1989) refers to these individuals as “sociological man” and “economic man.”

(⁵) This is obviously a very simple description of Arrow’s Theorem.

(⁶) See also Shepsle and Weingast (1981).

(⁷) For a RC take on the “old institutionalism,” and its relation to the “new institutionalism,” see Aldrich and Shepsle (2000).

(⁸) In a different RC treatment, Jenkins (1999) looked at the flipside of party emergence: party destruction. Specifically, he examined why Democrat–Whig divisions, which had formed the basis of the Second Party System and existed at the state-level in the South through 1860–61, vanished in the Confederacy. Looking at roll-call votes in the Confederate Constitutional Convention, Jenkins found that Democrats—who comprised a majority of convention delegates—voted to prohibit two of the Whigs’ major issues: protective tariffs and federal funding for internal improvements. This eliminated these issues from the legislative agenda, and thus erased the source of Democrat–Whig divisions. As a result, the Confederacy began its existence without parties, and it did not survive long enough for a new party system to emerge around some different set of issues.

(⁹) The public ballot solved the “hidden shirking” problem by allowing leaders to identify partisan recalcitrants and (potentially) punish them. The problem was that *constituents* could also now observe individual votes in officer elections. Consequently, as pressure was building over slavery, House members often found themselves caught between party and region.

(¹⁰) On these general points, see also Skowronek (2003).

(¹¹) While quite optimistic about “points of intersection and overlap” between historical and RC institutionalists, Katznelson and Weingast (2005, 20–1) are less sanguine about the full convergence of the institutional approaches: “There is little danger that communication and collaboration will produce methodological uniformity.”

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