

CHAPTER 28

THE U.S. SUPREME
COURT

LEE EPSTEIN

THE editors assigned me a near-daunting task: to write 8,000 words or so on ‘central issues on the current research agenda’ of students of the U.S. Supreme Court. In the first place, the sheer amount of research on the Court is overwhelming. A search in J-Stor retrieves over 3,245 articles with the *Supreme Court* in the title alone. Our library at Northwestern lists more than 600 books on the Court—and, again, that’s just a title search.

Even more daunting is the lack of a singular research agenda. To the study of the Court, social scientists bring diverse substantive interests, competing theoretical frameworks, and methodological approaches ranging from deep study of a single case to sophisticated statistical analyses of several thousand.

Which all goes to say that possible topics and schemata for organizing them abound. Believing, however, that most research ought begin with interesting questions I have chosen to go that route here. Specifically, I focus on three substantive issues of interest to scholars in the field: Appointments, Agenda Setting, and Decision Making. For each, I summarize the state of our knowledge but, perhaps more importantly, I highlight areas requiring further attention. Indeed, after reviewing the extensive literature, I have come to believe while we have made great strides in our quest to explain various features of the Court we still have some distance to travel. The gaps in our understanding may be narrowing, but they nonetheless remain. Moreover, the task of advancing our knowledge, as I suggest below, will profit immensely from a range of theoretical and methodological approaches.

One, perhaps needless, *caveat emptor*: in what follows, I have only skimmed the surface. I did not cover many strains of research in the three areas I chose to cover; and I left out many areas altogether. Of the latter, I particularly regret lacking the space to discuss the impact of courts—a subject that is of considerable interest to political scientists, and one that has generated deep debate in recent years. Readers can take some solace in the fact that Charles Epp does an excellent job in developing one line of inquiry in this literature: law as an instrument of social reform.

1 APPOINTMENTS TO THE COURT

Of all the difficult choices confronting societies when they go about designing legal systems, among the most controversial are those pertaining to the selection and retention of their judges. Some of the most fervent constitutional debates—whether in Philadelphia in 1787 or in Moscow in 1993–4—over the institutional design of the judicial branch implicated not its power or competencies; they involved who would select and retain its members. It is thus hardly surprising to find an immense amount of scholarship on judicial selection and retention, ranging from the primarily normative (e.g. Carter 1994; Cramton and Carrington 2006) to the chiefly empirical (e.g. Segal et al. 1992; Yalof 1999), to work falling between the two (e.g. Davis 2005; Choi and Gulati 2002).

In the context of the U.S. Supreme Court, recent scholarship has shed considerable light on two aspects of the appointments process: presidential nomination and senate confirmation. On the other hand, our knowledge of the process's triggering mechanism, a vacancy on the Court, remains sketchy.

Turning first to vacancies, although the U.S. Constitution implies that the process of appointing justices starts with the President, it in fact begins with a vacant seat on the Court. In three ways can that void arise: (1) the creation of a new seat, (2) the impeachment and removal or (3) departure of a sitting justice. None has received sufficient attention from political scientists.

The lack of attention to (1) and (2) is understandable. Congress has not altered the size of the Court since 1869; and no justice since Samuel Chase has been impeached and none convicted. Yet, these mechanisms raise too many interesting questions to delegate them to historians. A few suffice to make the point: why did the early Congress establish a norm against (political) impeachments (Knight and Epstein 1996*b*) or, for that matter, any rule that would require justices of one party to resign when a different party came to power, and why have these norms persisted?

That we have paid so little attention to (3), the departure of a sitting justice, is less explicable. Of course it is true that a handful of studies have attempted to model exits from the bench (see e.g. Hagle 1993; Squire 1988) but answers to even basic questions are hardly in abundance. So, for example, while we know from Yoon's (2006) important work that the overall turnover rate for U.S. district court judges jumps to 39 percent in the year they qualify for retirement benefits, that figure is only 13 percent for Supreme Court justices. What other forces are at work? Many commentators posit that justices strategically time their departures to coincide with presidents (and perhaps Senates) who share their partisanship or ideology. But systematic evidence, to the extent it exists, is far from conclusive.

Part of the problem lies in difficult questions of measurement. Thurgood Marshall tried to remain on the Court so that the conservative Ronald Reagan wouldn't replace him; Marshall said as much. But age, health, and the Democratic party's electoral failure conspired against him. Some might not categorize Marshall's departure (during the George H. W. Bush administration) as 'politically timed' but surely his decision to remain was strategic. How to capture that calculus is a challenging enterprise, but one hardly so daunting to render it Quixotic.

In contrast to our lack of knowledge about vacancies come the mounds of literature on the next phases of the appointments process, nomination and confirmation. Making use of Goldman's (1997) classic framework, Segal and I (2005) argue that presidents pursue a variety of goals when making Supreme Court appointments but almost all fall under the rubric of politics. In some instances, politics has centered largely on partisan aims—the President attempts to exploit judicial appointments to promote his or his party's interest. In other, perhaps most, cases, politics has been primarily about policy—the President seeks to nominate justices who share his ideological preferences. (Of course, the two are sometimes difficult to separate.)

At least in political science circles the claim that presidents pursue political goals is relatively uncontroversial. More debate arises over the extent to which the Senate acts as a constraint on the President. To be sure, data suggest that presidents are relatively unfettered: fewer than 20 percent of their nominations have failed to gain Senate approval. In an important paper, however, Moraski and Shipan (1999) argue that the low rate of rejection merely shores up the effectiveness of the 'advice and consent' clause. When confronted with a hostile Senate, presidents typically modulate their appointments, moving to the right or left as necessary.

The President's inclination to attend to the Senate may well explain why most nominations pass muster. Even so, the Moraski and Shipan result speaks most directly to the question of success within the Senate and not to success with individual senators. In 1993, Ruth Bader Ginsburg's candidacy generated only three nay votes but a decade or so later, Samuel A. Alito Jr. rather narrowly escaped defeat by a vote of 58–42.

In light of this degree of variation, accounting for the votes of senators has become something of a disciplinary cottage industry, though surely almost all

contemporary work takes its cues from Cameron et al. (1990)—the first to elaborate and systematically assess a theoretical account of confirmation politics in the United States. Briefly, Cameron and his colleagues assume that electorally oriented senators vote on the basis of their constituents' 'principle concerns in the nomination politics' (Cameron et al. 1990, 528). Those concerns primarily center on whether a candidate for the U.S. Supreme Court is (1) qualified for office and (2) sufficiently proximate to the senator (and his constituents) in ideological space. An analysis of data drawn from the votes of individual senators over the twenty-two nominations between 1953 and 1987 supports the account.

Despite the seminal contributions of both the Moraski/Shipan and Cameron et al., papers, it is hardly time to fold up our tents. Many questions remain, and important questions at that. Moraski and Shipan, for example, do not account for the filibuster. That the President may, in practice, need the votes of sixty senators, rather than a simple majority, suggests the possibility of an even greater role for the Senate (but see Johnson and Roberts 2005). By the same token, the Cameron et al. study pays little heed to the changing dynamics of Senate voting over Supreme Court nominees, be it the greater participation of interest groups and the media (see e.g. Davis 2005; Caldeira 1989; Caldeira and Wright 1998) or the increasingly important role of ideology (e.g. Epstein et al. 2006; Yalof 1999).

2 AGENDA SETTING

Just as institutions governing the selection of Supreme Court justices can structure the decisions of presidents and senators, institutions pertaining to access can constrain the choices justices make. As a general matter, justices can only decide issues that come to Court in accordance with jurisdictional rules in the Constitution or in statutes. Yet, even then, the decision-making process does not automatically go into operation. Judges and justices, in the United States and elsewhere, have imposed a series of informal barriers or developed norms that act as barriers to their courtrooms.

The use of formal and informal norms has given rise to many questions, ranging from whether they work to the advantage of particular litigants, to what effect they and other forces may have on the agendas of courts (see e.g. Pacelle 1991; Epp 1998). Here I consider yet another, actually perhaps the most persistent, question about the Supreme Court—what factors, whether norms or others, influence how the justices make decisions over which disputes to hear and resolve and which to reject; that is, how do they go about setting their agenda? This question has fascinated generations of judicial specialists (e.g. Boucher and Segal 1995; Caldeira and Wright

1988; Schubert 1959; Tanenhaus 1963; Ulmer 1972; Caldeira and Wright 1990; Perry 1991a)—as well it should. Agenda setting is one of the most important activities undertaken by political actors (e.g. Riker 1993)—and the justices are no exception. Actually, the Court's workload seems truly monumental—over 8,000 requests for review each term—and its discretion equally as high—these days, the justices typically hear and decide fewer than ninety cases per term.

No wonder analyses of agenda setting have burgeoned. And yet—despite immense interest in the subject (see e.g. Perry 1991b), the deployment of clever research strategies (e.g. Brenner 1979), and the employment of sophisticated technologies (e.g. Caldeira et al. 1999)—commentators do not agree on why Supreme Court justices make the case-selection decisions that they do. Some offer *legal or jurisprudential* models (Perry 1991a; Provine 1980); others see agenda setting as a clear example of *sincere voting to further policy goals* (e.g. Krol and Brenner 1990); still a third set suggest that it is laden with *strategic calculations* (e.g. Caldeira et al. 1999). Finally, there are those who point to the *litigants* themselves (e.g. Ulmer 1978). This disagreement, I should stress, is not a mere matter of emphasis. It is fundamental and it is an impediment to the development of an understanding of a crucial part of the justices' work—the establishment of their institutional agenda. It also impinges on our ability to think more generally about the role of the Court in American society. I return to these points later in the section. For now, let us consider the four basic perspectives with the acknowledgment that while all are influential, no one dominates.

Scholars adhering to a legal or jurisprudential account hold that judges seek to reach principled decisions at the agenda-setting stage—those based largely on various rules governing their review process. For Supreme Court justices, that would be Rule 10, which suggests that the Court prioritizes cases that have generated conflict in the lower courts or that have come into conflict with their own precedents. When justices follow this rule, so the argument goes, they are engaging in principled agenda setting because the rules themselves are impartial as to the possible result over a particular petition. If justices considered only whether conflict existed, their agenda-setting decisions would not reflect their own policy preferences over the substantive consequences of a case but, rather, those of the dictates of the rule itself.

Support for this claim abounds. Based on interviews with U.S. Supreme Court justices and their clerks, Perry (1991a, 127) concludes: 'All [the justices] are disposed to resolve conflicts when they exist and want to know if a particular case poses a conflict.' Flemming's (2004, 99) research reinforces the general importance of 'impartial' rules from beyond the borders of the United States. Based on a painstakingly detailed analysis of the Canadian Supreme Court's agenda-setting decisions, they claim that a 'jurisprudential account,' grounded in the country's 'public importance' rule, 'offers a persuasive story of how Canada's justice's set their agenda.'

No doubt virtually all scholars who study case selection setting attach some importance to the rules governing the process. But equally true is that data raise questions about whether rules provide the sole or even best explanation of agenda-setting decisions. During the 1989 term, for example, the justices declined to review more than 200 petitions that in one way or another possessed real conflict (Baum 2001); likewise, of the 184 cases it agreed to decide during its 1981 term, only 47 (25 percent) met the explicit criteria identified in Rule 10 (O'Brien 2000).

It is thus hardly surprising that Caldeira and Wright (1988) claim Rule 10 provides little aid in understanding 'how the Court makes gatekeeping decisions.' Or, to put it more charitably, the legal considerations listed in the Rule may act as constraints on the justices' behavior—the Supreme Court might reject petitions lacking genuine conflict—but they do not necessarily further our understanding of what occurs in cases meeting the criteria.

That is why scholars have looked elsewhere, specifically to the justices' policy preferences. Accounts of this sort come in two flavors: sincere (or reversal) and strategic policy models. On the first, justices have policy goals at the review stage—they would like to see the final opinion of the Court reflect their preferred position—and they achieve them by voting sincerely. In operational terms, justices will vote to grant those petitions in which the lower court reached a decision they disliked, such that right-of-center justices will vote to hear cases decided in the liberal direction below and liberal justices will prefer to review those decided conservatively. Why? Segal and Spaeth (2002, 253) provide the conventional answer: 'Given a finite number of cases that can be reviewed in a given term, the Court must decide how to utilize its time, the Court's most scarce resources. Certainly, overturning unfavorable lower court decision has more of an impact—if only to the parties to the litigation—than affirming favorable ones. Thus, the justices should hear more cases with which they disagree, other things being equal.'

A good deal of support exists for this proposition, some of which once again comes from the justices themselves. One told Perry (1991a, 270) that the certiorari vote is a preliminary vote on the merits in 'a majority of cases' and that '[g]enerally when people vote to grant, they feel that it is because [the case was] wrongly decided [below].' More support comes from data showing that, in fact, the Court typically reverses the lower court decisions it reviews—as many as seven out of ten in recent terms—as well as from systematic studies. Krol and Brenner's crosstabular analysis of the aggregated votes of members of the Vinson Court (1946–1952), for example, indicates that the justices simply voted against hearing cases with lower court decisions that they liked (ideologically speaking) and voted for hearing those cases with lower court decisions that they disliked. More recent confirmation, though from lower down on the judicial hierarchy, comes in George's (1999) analysis of the determinants of the decision to grant en banc review by U.S. Courts of Appeals. Her data show that 'extremely conservative courts of appeals . . . are far more likely to rehear a liberal decision en banc than a conservative one.'

In light of these findings, many scholars have come to accept the view that justices are policy-oriented and actively make choices to advance that goal at the review stage and, for that matter, on the merits of cases (Caldeira et al. 1999; Epstein and Knight 1998; Eskridge 1991; Maltzman et al. 2000). Where questions arise is over whether they pursue their policy goals sincerely or with some consideration of the preferences and likely actions of their colleagues. A strategic policy account suggests they do the latter: in deciding whether to review a case, justices consider the likelihood of prevailing at the merits stage. After all, proponents of this account ask, why would policy-oriented justices vote to review a case if they did not think their side could muster sufficient support on the merits?

Does the evidence support this strategic view? Yes but it is mixed. As early as 1959, Schubert relied on inferences from patterns of data (rather than actual certiorari votes) to argue that during the 1940s, liberals on the U.S. Supreme Court chose to grant Federal Employees' Liability Act (FELA) cases in which the lower court had decided against the worker and in which the worker would have a good chance of winning on the merits. In contemporary parlance, justices 'defensively deny' (when they decline to review cases that they would like to hear because they believe they will not prevail at the merits stage) but they do not 'aggressively grant' (when they take a case that 'may not warrant review because they have calculated that it has certain characteristics that would make it particularly good for developing a doctrine in a certain way, and the characteristics make it more likely to win on their merits') (Perry 1991a, 208). Boucher and Segal (1995), however, claim that justices pay heed to probable outcomes when they wish to affirm (an aggressive grant strategy) but not when they desire the Court to reverse, while Caldeira and his colleagues (1999) find evidence of both aggressive grants and defensive denials.

Yet a fourth perspective on agenda setting emphasizes the role of particular litigants or their attorneys, whether 'repeat players' or 'one-shotters,' 'upperdogs' or 'underdogs.' McGuire and Caldeira (1993), for example, show that the U.S. Supreme Court is more likely to grant review when an experienced attorney represents the appellant; and Ulmer (1978) demonstrates that upperdogs, under certain conditions, have a clear advantage in the American high court. Along similar lines, numerous studies have concluded that when the U.S. government is a petitioner to a suit, the Court is significantly more likely to grant review (Armstrong and Johnson 1982; Caldeira and Wright 1988; Tanenhaus 1963; Ulmer 1984).

Why the United States (as represented by the Solicitor General), other repeat players, and upperdogs are so successful is open to speculation. What we do know, and what I hope readers can gather from this short review, is why some scholars have deemed the agenda-setting literature a 'mess' (Boucher and Segal 1995). Even those who agree on the basic motivation of justices at the review stage—the pursuit of policy—disagree over whether justices advance that goal by always voting sincerely or by making strategic calculations about the eventual outcome at the merits stage.

Compounding matters even further is the existence of other plausible explanations that scholars have yet to consider in any systematic fashion. It is possible, for example, that justices do engage in strategic case selection but not with regard to one another. Rather, they may be attentive to the preferences and likely actions of other relevant actors—such as executives and legislatures—when they go about their agenda-setting task, and this may explain why the government enjoys extraordinary success both at the review stage and on the merits of cases. Consider this comment, not from a U.S. jurist but a justice on the Russian Constitutional Court:

When in December 1995, before the [parliamentary] elections and in the very heat of the electoral campaign, we received a petition signed by a group of deputies concerning the constitutional validity of the five percent barrier for party lists. We refused to consider it. I opposed considering this request, because I believe that the Court should not be itching for a political fight... The Court must avoid getting involved in current political affairs, such as partisan struggles. (Nikitinsky 1997)

Though this hardly provides proof positive of the existence of yet another explanation of agenda-setting decisions (for additional, though also inconclusive evidence, see Epstein et al., 2002), it is suggestive: This crucial line of inquiry may be in even greater disarray than many scholars think, and the consequences even more disturbing. Surely, the messy state of the literature impinges on our ability to reach a precise understanding of the agenda-setting process, to generate clear-cut predictions. Likewise, to the extent that case selection has implications for the role the Court plays in American society, we are unable to assess it. When the justices emphasize conflict—whether among lower courts, between state and federal courts, or with the Court's own previously decided cases—they convey important information to their judicial inferiors. A desire to reinforce their role as key players in the larger U.S. political system, on the other hand, may manifest itself in an abundance of particularly salient cases on the docket.

What can be done to clean up this 'mess'? First, scholars ought refrain from taking certain short cuts that are not just potentially problematic but also may explain the mixed findings in this area. Selection on the dependent variable is particularly rampant: In a non-trivial fraction of published studies the authors analyze only those cases to which the Court granted review rather than the full set of petitions—grants and denials. I understand why scholars invoke this strategy—they may lack the time, resources, or both to take another route. But we also must acknowledge that it is replete with potential pitfalls, the most important being the introduction of bias. Since we know that cases granted review are not representative of the universe of petitions—in fact, a great deal of research has demonstrated that they vary systematically from non-granted cases—it is inappropriate to draw inferences about the way in which justices select cases to review by considering only those petitions they grant.

Second, tricky problems of measurement require creative solutions. Consider jurisprudential approaches. Because the question is not whether conflict is an

important consideration but, rather, just how important it is, all studies must incorporate this variable. But how ought they do so? Some rely on the parties' briefs; and others on a project undertaken by law students at New York University in the early 1980s (Estreicher and Sexton 1986, 1984). Neither, for various reasons, is ideal, but alternatives have been difficult to develop. Until now. With the release of Harry A. Blackmun's judicial papers, researchers may be able to make use of the clerk's preliminary (cert.) memos to generate more reliable and valid indicators of conflict.

Finally, as scholars develop better measures and data sources they could make an important addition: expand the reach of their studies to include discretionary courts abroad. As the author of one of the few comparative analyses in this area (Flemming 1997, 1) put it: 'Very little is known about agenda setting by courts of final appeal in countries other than the United States. As a consequence, we do not know if the large and well-developed American literature on this topic can be generalized beyond the U.S. Supreme Court.' Having just undertaken an extensive review of the relevant literature, I am in complete agreement. Then again, I am not sure we know as much about the American context as Flemming suggests, thereby ensuring that work outside the United States would illuminate practices here.

3 DECISION MAKING

So far I have repeatedly made the claim that 'this area has generated an immense amount of research.' This is certainly true with regard to appointments and case selection but it is doubly so for judicial decision making. Over the past six decades, specialists have produced scores of papers and books aimed at explaining Supreme Court decisions on the merits. The result is a vast literature that approaches the subject from normative and positivist perspectives, with theories adopted and adapted from the social sciences and humanities, and with data that are qualitative and quantitative.

Once again no one essay in this volume, much less a section of one, could consider all this scholarship. And I do not try. Rather, let me highlight a point of agreement, several of departure, and finally some ideas for the future.

The point of consensus is easy to identify: the vast majority of contemporary political scientists hold that justices are by and large motivated to pursue policy; that is, members of the Court want to move the substantive content of the law as close as possible to their preferred political position.

This was not always how political scientists viewed the justices. Much of their early writing on the Court was so highly doctrinal that it was virtually indistinguishable

from legal analyses produced by law professors. Indeed, some political scientists who studied the Court went so far as to explicitly reject politics. Cushman's examination of the 1936–37 term, one of the most volatile in history, is exemplary. After acknowledging that the '1936 term . . . will probably be rated as notable, he enumerated some of the facts . . . one should bear in mind'—Roosevelt had won a landslide reelection and had submitted his Court-packing plan. Rather than demonstrate how those 'facts' might have affected Court decisions, however, Cushman simply noted 'no suggestion is made as to what inferences, if any, might be drawn from them' (Cushman 1938, 278).

Through the efforts of Pritchett (1941, 1948) and, later, Schubert (1965), Murphy (1964), and Spaeth (1964), among others, political scientists began to move away from purely doctrinal analyses of the Court and toward the more political explanations that characterize today's work (for an interesting statement about the development of the field, see Whittington, 2000). Surely, it was peculiar in Pritchett's day—at least among political scientists, though not legal realists—to write that justices are 'motivated by their own preferences' as Pritchett did in 1948 (pp. xii–xiii). But I dare say that nary a political scientist blinked an eye when, five decades later, George and I (1992, 325) characterized justices as 'single-minded seekers of legal policy.' Nor was the community any more shocked when Gillman (2001) concluded that *Bush v. Gore* (2000) was a partisan decision.

Where more disagreement arises is over whether analyses should incorporate more than the justices' ideological preferences. A juxtaposition of two competing approaches, the attitudinal model and strategic accounts (relatives of the sincere and strategic policy models of case selection), brings this debate into relief. On the first, we need not venture too far beyond the justices' sincerely held ideological responses to cases before them. Or, as two prominent attitudinalists put it, '[Scalia] votes the way he does because he is extremely conservative; Marshall voted the way he did because he was extremely liberal' (Segal and Spaeth 2002, 86). Freeing justices from considerations other than ideology, according to attitudinalists, is the lack of electoral accountability and ambition for higher office, the control justices enjoy over their agenda, and the dearth of judicial superiors (Segal and Spaeth 2002). In stark juxtaposition are strategic accounts, which belong to a class of non-parametric rational choice models as they assume that goal-directed actors operate in strategic or interdependent decision making context. Specifically, these accounts suggest that: (1) justices make choices in order to achieve certain goals (typically but not necessarily policy goals), (2) justices act strategically in the sense that their choices depend on their expectations about the choices of other actors, and (3) these choices are structured by the institutional setting in which they are made (Epstein and Knight 1998).

The distinctions between the two models are many, with two worthy of note here: *interdependency* and *institutions*. Beginning with the role of interdependent choice, on strategic accounts, goal-oriented justices take into account the preferences and likely actions of actors who are in position to thwart or advance their goals—

be it their colleagues, elected officials, or the public. On the attitudinal model, attentiveness to these forces is unnecessary; justices always behave in accord with their sincere preferences.

Both sides, it is worth noting, have developed substantial support for their positions. In a highly influential article (Segal 1997) and later in his book with Spaeth (Segal and Spaeth 2002, 348), Segal is blunt: 'the Court's reaction to the... revelation of congressional preferences is a collective yawn.' Armed with equally impressive evidence, Bergara et al. (2003) refute Segal's conclusion asserting that when the Court interprets statutes it in fact 'adjusts its decisions to Presidential and congressional preferences.' And now several scholars have added fuel to fire by arguing that the justices are not only attentive to the President and Congress in the statutory context but also in the constitutional realm (e.g. Epstein et al. 2001; Harvey and Friedman 2006; Rosenberg 1992)

A second crucial distinction between attitudinal and strategic accounts centers on how they treat institutions. Only a few come into play for attitudinalists, and then only those that allow the justices to vote as they so desire—such as the lack of an electoral connection. By contrast, strategic accounts emphasize the range of institutions but perhaps none more controversial than precedent. To many strategic analysts precedent may not determine judicial outcomes but neither is it, as some attitudinalists contend, completely irrelevant; rather it can serve as a constraint on justices from acting on their personal preferences. In other words, justices have a preferred rule that they would like to establish in the case before them but they strategically modify their position to attend to a normative constraint—a norm favoring *stare decisis*.

Why would justices follow precedent in those situations in which they would prefer to create a different rule? Strategic accounts supply at least two answers: prudential and normative. As to the first, *stare decisis* is one way in which the Court respects the established expectations of a community. To the extent that members of a community base their future expectations on the belief that others will follow existing rules, the Court has an interest in minimizing the disruptive effects of overturning them. If the Court makes a radical change the community may be unable or unwilling to adapt, resulting in a decision that fails to produce an efficacious rule. As to normative reasons why justices may adhere to precedent rather than their own preferences, the logic is this: if a community holds a fundamental belief that the 'rule of law' requires the Court to be constrained by precedent, the justices may follow the belief even if they do not personally accept it. The constraint follows from the justices' understanding that the community's belief affects its willingness to accept and comply with the Court's decisions. If the members of the community believe that the legitimate judicial function requires adherence to precedent, then they will reject as normatively illegitimate decisions that regularly and systematically violate precedent. To the extent justices are concerned with establishing rules that the community will accept, they will keep in mind the

fact that the community must regard these rules as legitimate. In this way, a norm of *stare decisis* can constrain the actions of even those justices who do not share the view that they should be constrained by past decisions.

Once again, both attitudinalists and strategic analysts have marshaled considerable support for their claims about *stare decisis*. Segal and Spaeth (1996), for example, hypothesize that if precedent matters, it ought to affect the subsequent decisions of members of the Court. In operational terms, if a justice has dissented from a decision establishing a particular precedent, she or he should still feel bound by it and not dissent from its subsequent application. But the data, they argue, suggest otherwise. Of the eighteen justices included in their study, only two occasionally subjugated their preferences to precedent (see also Spaeth and Segal 1999). In a critique of Segal and Spaeth's work, however, Knight and I (1996a), offer substantial evidence of behavior that is consistent with the existence of a norm of respect for precedent. That evidence ranged from the use of precedent in attorneys' briefs, to appeals to precedent made by Supreme Court justices in Conference, to citations to precedent in judicial decisions (see also Brisbin 1996). Newer studies purport to find even more direct evidence of the effect of 'law' on the Court's decisions. Richards and Kritzer's (2002, 308) analysis of free expression cases, for example, emphasizes the importance of jurisprudential regimes, or 'a key precedent, or set of related precedents, that structures the way in which the Supreme Court justices evaluate key elements of cases in arriving at decisions in a particular legal area.'

These and related debates about judging are positive developments. As short as a decade ago, the relationship between the Court and other political actors or even the role of precedent was barely a blip on the radar of political scientists. Today at least we are actively engaged in solving these intellectual puzzles; and, in fact, we are making substantial progress on all fronts. But if the goal is develop a fuller picture of the Court's decisions we can do better. I understand the complicating factors. It is quite difficult, for example, to document the (potential) constraint imposed by legislatures and executives: when justices rule in favor of, say, the existing regime, they may do so because they share the regime's preferences (sincere)—and not because they are attempting to appease that regime (sophisticated). Distinguishing these forms of behavior—sincere and sophisticated—turns out to be no easy task, though recent work has advanced the project. Along these lines, I am particularly taken by analyses of courts abroad (e.g. Helmke 2004; Vanberg 2004). From theoretical, methodological, and substantive perspectives, I have learned a great deal from these studies, and I suspect other Americanists would as well.

Assessing the impact of precedent and other 'legal' variables turns out to be equally difficult. Again, I see substantial progress, though, ironically enough, not necessarily in studies of the Supreme Court (e.g. Staudt 2004; Baldez et al. 2006). One exception is an innovative paper by McGuire and Vanberg (2005). Using new technology for mapping texts into policy space, they estimate the relative polarity

of particular opinions (e.g. *Lee v. Weisman* registers as more liberal than *Wallace v. Jaffree*). Although the method itself is not flawless (see e.g. Monroe and Maeda 2004) and their application only one among many possibilities, I am taken with the general motivation behind the McGuire and Vanberg (2005) study: to move away from an exclusive focus on the (typically dichotomous) bottom line of a judicial decision (e.g. reverse/affirm, liberal/conservative, winner/loser, uphold/strike down) and to an approach that exploits the entire opinion but eliminates researcher judgment. Indeed, it strikes me as powerful method for investigating positions in decisions, whether over policy or, possibly, method (e.g. text-, intent-based approaches to interpretation). Ideal point estimation for individual justices based on this method is also possible and just as likely to be informative. Turning to the dynamics of the decision-making process, we may well be able to determine the extent to which areas of the law are interrelated and the degree to which legal decision making is gradual (as many would argue), abrupt, or both.

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