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Articles

Taking Law Seriously

Barry Friedman

The positive literature on judicial behavior has not received nearly the attention it deserves. That literature has a great deal to offer, both to legal scholars and to those who are concerned about the role of legal institutions generally. For example, the literature has the potential to help shed light on the ability of courts to protect rights and foster economic development. This article argues that the positive literature has failed to see its due in large part because positive scholars often do not take law and legal institutions seriously enough.

The article identifies three specific sets of problems with the positive scholarship, offering detailed suggestions on how positive scholars can avoid them. The first is the problem of normative bite: Too often positive scholars of judicial behavior seem to be trapped in their own disciplinary debates, without pausing to examine why it is that they are studying what courts and legal institutions do. Second, positive scholars need to pay greater attention to the norms of the law, i.e., how law and legal institutions operate. A skeptical stance toward law is fine, but that skepticism should not get in the way of accurately understanding properly the mechanics of law and legal institutions. Finally, empiricists in particular must take great care regarding the data upon which they rely. It is difficult to obtain good data on the workings of legal systems. Data that are readily available often present a distorted picture of the system being studied.

As Karl Popper famously observed, “We are not students of some subject matter, but students of problems. And problems may cut right across the borders of any subject matter or discipline.”¹ As true—and obvious—as Popper’s point is, it presents a very real difficulty for scholars. The perils of interdisciplinary scholarship are apparent in relentless efforts by scholars to patrol the bounds of their discipline. Historians regularly complain about lawyers and legal academics relying on “law office history”² including “the selection of data favorable to the position being advanced without regard to or concern for contradictory data or proper evaluation of the relevance of the data proffered.”³ Lee Epstein and Gary King recently also took legal academics to task for “little awareness of, much less compliance with, the rules of inference that guide empirical research in the social and natural sciences.”⁴ At best, interdisciplinary work, done poorly, teaches us nothing, or nothing of value. At worst, it can lead to more confusion than illumination. Doing good interdisciplinary scholarship is hard work, requiring proficiency in two or more disciplines.

Despite the perils, interdisciplinary scholarship no longer is optional. Because Popper is right, that real-world problems simply do not break down into the neat categories of academic disciplines, solutions require interdisciplinarity. The rise of methodological studies suggests that tools exist that can be brought to bear across disciplines.⁵ The National Academies just published a lengthy report urging institutions to remove barriers and foster interdisciplinary work, which “has delivered much already and promises more.”⁶ In diverse fields, scholars of different disciplines are reaching toward one another in a “dialogue that aims at challenging or developing existing ideas and techniques on questions of common concern to members of both disciplines?”⁷

There are a set of pressing questions about legal institutions that cry out today for sound interdisciplinary study. More nations are turning to courts and judicial review to play an important role in the protection of basic human liberties. Similarly, following Friedrich Hayek, scholars and policy analysts increasingly believe that a well-functioning legal system able to decide cases according to law—and without corruption or other inappropriate influence—is crucial to financial development and economic growth.⁸ Simply put, in courts and the rule of law rest great aspirations for the material well-being and liberty of civil society. But are courts up to the task? Are legal institutions different from political institutions in any meaningful sense? What can legal institutions contribute to a good society? Are judicial independence and the rule of law, so commonly bandied about, even meaningful concepts? In short, does law matter?⁹

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There is a tradition of positive scholarship in political science with the potential to answer these very questions. That scholarship is just now receiving due attention outside the discipline. Normative scholars concerned with human rights and economic development understand they need just the sort of help positive political scientists can offer. Legal scholars increasingly are citing some of the positive scholars' foundational work in judicial behavior.¹⁰ Indeed, legal scholars now are pursuing the same sort of empirical inquiries as positive scholars,¹¹ creating exciting opportunities for true interdisciplinary collaboration. The possibilities for disciplinary cross-fertilization are marked by several recent notable appointments of positive political scientists to law faculties.¹²

Unfortunately, many positive scholars have limited sharply the promise and utility of their own work by not looking beyond their own discipline to the one place most apt: the law. One would surely think that if any interdisciplinary project were appropriate, it would be the marriage of legal theory and positive study of judicial behavior. Yet, reflecting an almost pathological skepticism that law matters, positive scholars of courts and judicial behavior simply fail to take law and legal institutions seriously. This failure accounts for methodological shortcomings that diminish the value of the entire positive endeavor.

This article discusses three common failings of much positive scholarship studying judicial behavior. These are the very sorts of problems with which any interdisciplinary project must grapple, particularly when one of the disciplines is highly methodological. The aim here is not to criticize, but to offer somewhat of an outsider's perspective on where the work presently falls short, with the aim of fostering an immensely important project. Positive political scientists have much to offer. Yet, at present the reach of this work is severely limited by its failure to take account of its Siamese discipline, the law.

The first suggestion is to direct more attention to the issue of "normative bite," i.e., to the question of why the positive scholarship matters. The value of positive scholarship rests in what it can tell us about how judges and legal institutions are likely to behave as they interact with the other institutions of government and society. Too often positive scholars of judicial behavior seem to be trapped in their own disciplinary debates, without pausing to examine why it is that they are studying what courts and legal institutions do. Authors of studies should ask, at the outset of every project, why we, as a society, might care about what is being studied. What is the practical impact of the positive results? Does the positive project contribute to our understanding of courts in some meaningful way?

Second, positive scholars need to pay greater attention to the norms of law, i.e., how law and legal institutions operate. A skeptical stance toward law is fine, but skepticism should not cover for comprehension. Positive scholars who work at the nexus of law and politics need to do a

better job of understanding law itself—its methodology, its substance, and its process.

Finally, empiricists in particular must take great care that the data upon which they rely presents an accurate picture of the legal world they are studying. Collecting data on the judiciary is extremely difficult and time consuming, and the temptation is great to rest on what data is readily available, allowing that to define the questions that are asked and the way in which they are answered. That temptation should be avoided, because it runs the risk of presenting an incomplete and idiosyncratic view of the legal system.

The discussion that follows elaborates upon each of these concerns, using examples from the extant literature to underscore its points. The examples have been chosen with all due humility and recognition of the accomplishments of the authors. There is a risk in singling out any given piece of work in order to demonstrate persistent difficulties in the field, so it is important to be explicit at the outset about the decision to use any particular piece of work as an exemplar here. Each piece of work discussed has been selected because of its overall strength and the justifiable prominence of its authors, despite the difficulties that are highlighted. It is often easiest to see problems that are amiss in the highest quality scholarship, and that is the approach taken here.

Great strides have been made by positive scholars in understanding how judges behave, and what motivates and constrains them. Nonetheless, that work often lacks relevance to those who care normatively about how law and legal institutions operate. There is no reason, a priori, why scholars cannot be interested in normative and positive questions both. Disciplinary rigidity and ideological commitments that caused the initial dichotomy in positive and normative endeavors are breaking down. Now is the time to improve the positive work, so that it has the impact it should.

Normative Bite: Ensuring the Relevance of the Work

Shining interdisciplinary light upon the law and legal institutions has provided a variety of intriguing insights and data points. But it is important to separate an outsider's sightseeing from the insider's focused investigation. If conducted by talented researchers, close study of almost any subject might yield information of some intellectual interest. What matters, at bottom, is whether the positive scholarship has something to teach about how law and legal institutions operate in a way that is pertinent to how they should, and to the aspirations put upon those institutions by society.

Oftentimes positive scholarship seems to be struggling with the normative implications of its work only after the project is complete, if at all. One sees indications of a

“research now, justify later” approach. Empirical projects follow on themselves, rather than originating in the world of law and legal institutions. Normative bite ought to define the problem, not be an afterthought. Falsifiable hypotheses should be about something of consequence. At least some significant part of the positive scholars’ agenda should come from within law itself.

Answering questions of normative concern requires reading widely in scholarship about law and legal institutions. It is apparent from the face of most positive scholarship that this is not being done at present. The scholarship is deeply self-referential, with only glancing citation to what legal scholars are saying. It is impossible to be interdisciplinary without reading in both disciplines.

Some readers of an earlier draft of this piece resisted the suggestion that the positive scholars’ projects should be defined by normative questions of interest in the legal world. They view the endeavors as separate and claim that their role is finding and resolving empirical puzzles. But determining that something is a puzzle worth pursuing depends on some external metric, whether stated explicitly or not. No one studies the hair color of judges, presumably because there is no theory—normative or otherwise—about why it would be relevant. Theory necessarily precedes positive scholarship, and that theory has to come from somewhere.

From Pritchett to the present

The research path of positive scholars has its own internal logic, revealing why the projects of political scientists at times become self-referential, rather than addressing problems of real concern to society. Familiar history recounts how, beginning with Herman Pritchett in the 1940s, political scientists interested in the courts went their separate way from scholars in the legal academy. Legal Realism occasioned the split. Legal Realists recognized that judges, being human, were likely to see legal issues through the lens of pre-existing social and political commitments. Legal scholars, troubled by what Realism spelled for law’s legitimacy, took a normative turn, devoting their efforts almost exclusively to telling judges how they should decide cases. Political scientists, on the other hand, fascinated by Realist claims, chose the positive path, seeking a better understanding of how judges actually decide cases, and why.¹³

The first works by political scientists were attitudinal. Born out of the Legal Realist movement, it was both appropriate and understandable that tremendous effort was devoted to the endeavor of establishing the role of ideology in deciding cases. But the concern for answering this question became obsessive. Today, attitudinalists devote too much effort to demonstrating that ideology is rampant in adjudication, and to fending off claims that something other than attitudes matter.¹⁴

The “strategic revolution,” when it came, was a reaction to the hegemonic claims of attitudinalism. Strategic institutionalists sought to establish that judges did not act only on their own will, but were constrained by their institutional environment.¹⁵ Historical institutionalists challenged both approaches, stressing the importance of context and of law itself to the resolution of legal disputes.¹⁶

Struggles within the discipline have kept political scientists from developing more nuanced projects with clear relevance to how legal institutions matter to society. Questions tend to be asked wholesale—because that is the level of intradisciplinary debate—when the really important issues are ones of retail. Today attitudinalists struggle with strategic institutionalists over whether ideology or institutional constraint influences the decision of cases, when the answer undoubtedly is both, and the important question is how much of each.¹⁷ Historical institutionalists compete with attitudinalists over the claim that law matters, when in some cases it surely does, and the interesting question is in which courts and which cases.¹⁸

Why study collegial courts?

Studies of voting fluidity on the Supreme Court provide a good example of the problem of disciplinary inside baseball. These studies examine whether, and why, votes shift on the Supreme Court between the time of the initial conference on the merits and final decision.¹⁹ Those outside the subfield of law and courts would be justified in scratching their heads wondering precisely why anyone cared, and what it was that students of law or legal institutions should take as a lesson from demonstrations of such fluidity. The studies themselves do not offer sufficient explanation. As best as one can discern, the reason for fluidity studies is that the fact of fluidity suggests that attitudinal claims might be overblown. Thus, as one such study explains, “‘fluidity’ served as a bold challenge to the public law subfield.”²⁰ Bold it may have been, but it is not a challenge calculated to matter much to those beyond the subfield.

Brenner, Hagle, and Spaeth’s work on majority coalitions and defection of marginal justices highlights just how far a question internal to political scientists can stray from what ought to have been the animating project, its significance for law and legal institutions. Following on earlier work,²¹ in a 1989 piece Brenner, Hagle, and Spaeth examine “why [marginal justices] defected from minimum winning original decision coalitions on the Warren Court.”²² They conclude that “less able” justices stray more frequently, and that straying tends to realign a Court along the ideological lines attitudinal studies would have predicted.²³

Though one can perform some mental gymnastics to imagine a normative reason for studying this subject, it

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surely is far from anything that motivated the authors. They tell us we ought to care because:

from the standpoint of group effectiveness the ideal situation is one in which each justice votes “correctly” at the original, or conference, vote. We define voting correctly as voting the same way (i.e., affirm or reverse) at both the original and final votes. Failure to do so imposes costs on the Court, including the possible need to reassign the majority opinion and the recasting of opinions from majority to minority and vice-versa. The time and energy spent to redo such “incorrect” decisions obviously lessens the Court’s productivity.²⁴

Apparently the normative vision animating this project is that the Court, like a factory, does best by producing the most output with the least frequent interruption. This is a normative position regarding the work of a nation’s high constitutional court that requires substantial defense. It may well be that in the lower courts law should work mechanically, to facilitate the rapid resolution of disputes. But it would be difficult to formulate a normative defense of such a conception of Supreme Court decisionmaking.

Many normative scholars believe the purpose of a multi-member court is to foster deliberation, in order to achieve better outcomes.²⁵ Deliberation implies mind-changing, exactly the opposite of what motivates the Brenner, Hagle and Spaeth study. Although Brenner, Hagle and Spaeth might have a very different normative vision of Supreme Court decisionmaking, one gets the distinct impression that the study was the next step down a path carved within the discipline, driven in part by the availability of data.

A helpful contrast is posed by a recent paper by Benesh and Spaeth, which examines the sources of disagreement on a collegial court. The question the authors pose is, when justices disagree, are they reaching opposing conclusions about the same legal issues, or are they disagreeing even about what the issues themselves are. After studying the content of opinions, the authors largely conclude it is the former.

This study goes directly to the question of the extent to which law constrains Supreme Court justices. If the justices reach differing outcomes because they have different takes on what the issues are in a case, nothing certain might be said about law’s determinacy. But finding, as the authors do, that the justices regularly agree on the precise legal issue, and yet still disagree on the conclusion, implies that at least on the Supreme Court law is insufficiently determinate to constrain judges. This fits nicely with attitudinal claims that it is ideology and not law deciding cases. And as such it undermines claims in legal thought about the constraining influence of law.

Even here, however, the authors do not seem fully aware of what they have found, again framing their paper in terms of existing debates about whether an “attitudinal” or “legal” model operates on the Supreme Court. Their own conclusion about their work is that it shows that “attitudinal differences abound” rather than finding any “sup-

port of a ‘legal model’ of decisionmaking.”²⁶ But outside the subfield, most likely there is agreement that attitudes and law both play a role—the question is how much, and more particularly, how much law can constrain. To state this differently, the question is not so much whether law plays a role, as what role it plays.

Why study “strategic” behavior?

The same problem of normative bite is apparent in studies of “strategic” behavior. Lee Epstein and Jack Knight’s *Choices Justices Make* is unquestionably path-breaking work, and deeply commendable for the painstaking labor obviously involved in supporting the authors’ central argument that the assumptions of the attitudinal model are too strong, and that a strategic component to Supreme Court decisionmaking must be recognized. *Choices* represented a great leap forward. If *Choices* has a problem, however, it is that the authors were themselves motivated too much to write against the attitudinal model, thus distracting their attention from the relevance of their work to law and legal institutions.

Attending to internecine struggles, the authors of *Choices* adopted a definition of strategic behavior that is too capacious to be doing sufficient normative work. The *central* question for strategic studies must be the extent to which the Supreme Court is constrained in its decisionmaking by other political actors. This question has enormous normative significance, because it calls into question some of the fundamental tenets of judicial independence, and raises important issues about the function of judicial review. If, for example, the Supreme Court is constrained by political actors, then normative theories that support judicial review as protecting minority rights against a willful majority at least require nuance if not rethinking.²⁷ If the Supreme Court is subject to the political winds, what do we mean when we speak of “judicial independence?”

For the authors of *Choices*, however, “strategic” often means only that the justices are paying attention to one another’s arguments, rather than simply voting their own ideology. This question makes sense if all one is trying to do is disprove the overstated claims of the attitudinal model. Thus, Epstein and Knight meticulously document that the justices send memoranda back and forth to one another regarding draft opinions. “These data,” they say, “convey important information about the nature of the Supreme Court decision making; they indicate that the justices respond to one another’s opinions.”²⁸

Outside the subfield of law and courts, however, does anyone reasonably doubt that the justices take account of one another’s views? It seems apparent from the face of the justices’ written opinions that they respond to one another. To be fair, the study of the justices’ interaction in memoranda itself might be relevant to normative concerns about how deliberation occurs on multimember

courts, but that is not primarily how the authors frame their own questions. Rather, their attention is directed too much to responding to attitudinalists, and as such they do not define their study in ways that might have much broader utility.

Why study the Supreme Court?

This failure to connect the issues being studied to concerns about normative relevance skews what gets studied in the first place. Many of the political science studies focus on the Supreme Court. But if constraint is the issue, all the important action might be in the lower courts. Within the world of law and legal institutions, few seriously believe the Supreme Court justices are constrained in the way rule of law models of a legal system demand. Almost by definition the cases the Court takes should be novel enough that precedent (itself just one interpretation of “law”) will not decide the case.

One really important question is whether law can and does have precedential influence in the run of the mill cases in the lower courts. Studying the lower courts is more difficult than studying the Supreme Court, in no small part because there is less available data. But normative concerns, not difficulty with data, ought to define the research agenda of the scholar interested in judicial behavior. For this reason, some of the most important work in judicial behavior is being done with regard to the lower courts.²⁹

Similarly, studies of strategic behavior turn perplexing when reference is made to the lower courts. The evidence we have suggests less strategic behavior by courts lower than the Supreme Court,³⁰ but there there is still more work to be done—including theorizing. While everyone seems to have some sense of what it means for a Supreme Court justice to act strategically, this is less clear for the lower courts. It is a matter of normative debate, for example, whether lower courts should follow what the Supreme Court has said, or what it is likely to do.³¹ Is it “strategic” or “legal” for a lower court to try to get a decision “right” in a way that avoids reversal?

Undoubtedly there are reasons to study the Supreme Court. It is a central policymaking institution in American government. Yet, for many of the questions being asked by scholars, studying the lower courts may make more sense. At the least, any agenda for studying the Supreme Court ought to focus on pertinent normative questions, such as how constrained it is by other institutions of government, how deliberation affects outcomes, and how the Supreme Court interacts (and controls) the lower courts.

As this discussion, and much of what follows, demonstrates, the positive project has a great deal to offer those who care about the law and legal institutions, and their

impact on society. But political scientists can and should improve the quality of their own project by paying more attention to the issue of normative bite. Foremost should be the concern: what of consequence does this help us understand? The answer must come from the world in which the courts operate and are supposed to matter.

The Norms of the Law

Positive scholarship about courts and judicial behavior can only be as good as the grasp scholars have of the norms of law and the legal system. Most positive scholars work hard to understand the particular aspect of law and the judiciary they are studying. Nonetheless, the existing literature reveals some persistent difficulties, attention to which will serve only to strengthen future work. Among these is a real skepticism that law itself makes much of a difference.

There are three categories of legal norms that require attention. It is common to divide the legal world into substance and process. Substance refers to the legal rules that govern society, process to the means by which the substantive rules are made (including in ordinary litigation). But those who write at the intersection of law and politics must also come to grips with an overarching concern, the methodology of law itself.

Law’s methodology

The methodology of law presents a seeming problem for positive scholars. The rules of positive scholarship require that relevant assertions be falsifiable. In light of persistent disputes about the ability of law to constrain actors, many positive studies try to measure, or control for, law’s influence. To this end, political scientists seek a clear statement of what law is and how it operates, and often are perplexed when there is no such agreed-upon statement in the legal world.³² Similarly, when positive scholars examine the results of legal cases, they find the outcomes varying for no apparent reason other than the identity of the judge.³³ All this leads some political scientists to conclude that law is a chimera, a fig leaf covering up a system of complete indeterminacy, nothing but a set of words used to justify any conclusion.³⁴

It is entirely understandable that the nature of law is perplexing. This is true even for those within the discipline. The reason is that law is not immutable like the laws of nature. Law governs society, and when the members of society have disagreements they contest not only the primary (substantive) rules that govern them, but also the secondary rules of the game themselves—how the law is made and what constitutes it.³⁵ The nature of law is contested and contestable. There are deep philosophical debates within the legal academy itself about what law is.³⁶ Small wonder law cannot be captured easily in an

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empirical equation, and that it is difficult to make claims about law's influence that are readily subject to falsification.

Still, it is quite a leap from the difficulty of describing what law is and how it operates to the conclusion that law is a chimera. Perhaps it is possible that what lawyers, judges, and legal academics spend years learning, practicing, and theorizing about is meaningless, that the legal system stands entirely on wobbly stilts, and that the entire discourse of law is an illusion. But claims of this nature are sufficiently improbable that before coming to such conclusions, it seems important to explore any reasonable alternative. For all law's supposed indeterminacy, many aspects of society seem to run quite fine along legal rails.

The importance of opinions, not outcomes. Political scientists' difficulty with law stems in no small part from their own empirical methodology, which typically casts attention on the outcomes of judicial cases rather than the legal opinions written to explain and support those outcomes.³⁷ This point runs the risk of becoming a cliché, but still many political scientists fail to heed it. In their work on the attitudinal model, Segal and Spaeth say "the opinion of the Court, . . . constitutes the core of the Court's policymaking process."³⁸ Similarly, in their book on *Crafting Law on the Supreme Court*, the authors stress the importance of examining judicial decisions rather than simply outcomes.³⁹ Yet, in neither book is there any systematic evaluation of the content of opinions, and the law itself.

In common law systems, law is found primarily in legal opinions, not divined from the outcomes of cases. (Not to overstate the matter, outcomes can matter too, more so in some cases than others obviously.) In judicial opinions are found the rules that govern the next case, and thus the conduct of institutions and actors in society.⁴⁰ Particularly when it comes to the Supreme Court, which repeatedly tells us by word and act that it is not generally in the business of error correction, it is the opinions that matter most.

At bottom, what law imposes is a requirement of reasoned justification, and reasons are found in the opinion of a court. It is entirely legitimate in law for judges in some circumstances to reach differing answers to the same question; what matters is that judges explain those answers in a plausible and coherent way.⁴¹ They not only must explain why a result is reached in one case; they also must explain how that result squares with the rules of other cases. This requirement of justification is fundamental in common law systems. It is almost impossible to study law in a meaningful way without some attention to the opinions that contain these justifications.

Attention only to the outcomes of cases can present a misimpression of what the courts have done. In a famous debate over competing legal and political science methodologies, Harold Spaeth, a leading proponent of the attitudinal model, drew a distinction between what judges say,

and what they do. He said "I find the key to judicial behavior in what the justices do, Professor Mendelson in what they say. I focus upon their votes, he upon their opinions."⁴² Spaeth's comments reflect a bias for evaluating outcomes over opinions. In law, however, what courts say often spells what it is they have actually done.

Studying the content of opinions empirically is not impossible, though it is labor intensive, and ultimately may require methodological advances in positive scholarship itself. Most studies appear to gravitate to the outcomes of cases (and to the Supreme Court's in particular) because this data is readily available. However, some scholars have conducted empirical studies of the influence of law by looking to the content of opinions, and have concluded that law does have an influence in structuring analysis and organizing outcomes.⁴³ To do so, scholars typically find a factor in a legal test that ought to be determinative and assess changes in outcomes based on the presence or absence of that legal factor in later cases.⁴⁴ Because the nature of the common law is itself evolutionary, scholars have yet to find a way of testing for law's influence in a sustained line of cases.⁴⁵ Still, this sort of empirical approach to the study of law is promising.

How focusing on outcomes leads positive scholars astray. An instructive example of the problem of looking only to judicial outcomes is found in a recent study in which legal and political science academics joined together to run a contest to see if legal scholars or a computer could better predict the outcome of Supreme Court cases.⁴⁶ The authors recognized that predicting outcomes only could tell us so much: "We readily acknowledge the limitations of a study, like ours, that would have treated the most famous case in American history as simply "Marbury loses," without any concern for what John Marshall actually said in reaching that result."⁴⁷ How true this point was becomes evident by comparing two cases in their study—decided the same day—involving the constitutionality of affirmative action in higher education. In one case the Supreme Court upheld an affirmative action program, in the other case it struck one down.⁴⁸ According to the rules of the study, the machine predicted the outcome of one case correctly and the other incorrectly. But as this example itself shows, those conclusions are entirely without meaning.

What the affirmative action cases demonstrate is that looking to outcomes rather than opinions leads to the wrong conclusion of what the court "did." Although it looked as if the Court decided the question of the constitutionality of affirmative action in higher education in two different ways, in fact the Court gave one consistent answer. Justice O'Connor's vote was determinative in the affirmative action cases, and her opinion explaining the different outcomes provided the governing rule for the future. That rule—the Court's legal conclusion—was that affirmative action in higher education is constitutional so

long as it meets certain criteria.⁴⁹ But suppose, as is more ordinarily the situation, that there had only been one affirmative case during the Court's Term, and that in that case the Court struck down the program. The typical empirical single-minded focus on outcomes would lead one to conclude that the Court had invalidated the use of affirmative action in higher education. Reading the opinion, however, would reveal just the opposite—as was so clearly the case in this unusual situation. Thus, the lesson: case outcomes rarely tell us anything about the “rule” in the case and what matters for law especially in appellate courts, is the rule.

By focusing on votes rather than opinions, real differences in judicial ideology are obscured. A comparison of the voting records of the former Chief Justice, William Rehnquist and Justice Thomas would suggest that they are both quite conservative.⁵⁰ But if one reads the decisions authored by these justices, it is apparent that the two are quite different in ways that have great significance for the law. For example, the two often vote the same way as to outcome in Commerce Clause cases, but when one examines their written opinions, the Chief Justice would have reined in Congress's power somewhat, while Justice Thomas would return that power to the limited scope it had at the time of the Founding.⁵¹ When it comes to Congress's constitutional power, a court of nine Rehnquists would look very different than a court of nine Thomases.

Misunderstanding law. By the same token, when positive scholars do turn their attention from outcomes to opinions, they often overstate the justificatory demands of the law, requiring greater determinacy than normative theory about law itself would demand. This is precisely what leads some political scientists to conclude that law does not play any meaningful role. A good example of the problem is found in Harold Spaeth and Jeffrey Segal's *Majority Rule or Minority Will*. In their relentless effort to falsify the impact of the law, Spaeth and Segal performed an exhaustive analysis of Supreme Court justice's fidelity to precedent. Their test of precedential behavior essentially asks whether justices who dissented from a seminal precedent nonetheless followed it in later (progeny) cases. As a normative matter it is not clear that fidelity to precedent requires a justice to bow to the opinion of a majority as opposed to adhering to one's own views of the law.⁵² But even if this were the test, the nature of law is such that the gravitational pull of seminal cases on their “progeny” cases is not felt in the way Spaeth and Segal imagine.⁵³

One example from the Spaeth and Segal project reveals how commonly even positive scholars most seriously committed to studying the law will misunderstand or misstate its demands. One of Spaeth and Segal's seminal cases is *Miranda v. Arizona*, the famous case in which the Supreme

Court required that police read criminal suspects certain rights before any confession resulting from “custodial interrogation” can be admitted into evidence. Spaeth and Segal identify as a progeny case of *Miranda* the decision in *Rhode Island v. Innis*, which adopted a rather narrow view of what sort of “interrogation” was subject to the *Miranda* rule. Spaeth and Segal then code Justice Stewart as acting on his own preferences (albeit “weakly preferential”) rather than precedent because he dissented in *Miranda*, and then wrote the decision in *Innis*, which Spaeth and Segal view as deviating from *Miranda* itself.⁵⁴

This example indicates a misunderstanding of the dictates of precedent. The difficulty with this analysis is that *Miranda* simply did not, as a legal matter, address the question resolved in *Innis*. Scholars might disagree about whether one definition of interrogation is more faithful to *Miranda*'s spirit than another, and it may well be the case that in the minds of some the *Innis* decision was less than fully faithful with that spirit.⁵⁵ But it is simply not correct to label *Innis* “opposite in direction” from *Miranda* as Spaeth and Segal apparently do.⁵⁶ Even if one accepts an obligation of Supreme Court justices to follow precedents with which they disagree (itself contestable), it is unlikely many lawyers would say that a *Miranda* dissenter who joined the *Innis* majority somehow acted unfaithfully to that obligation. *Innis* simply took up a legal question that followed from *Miranda*, and answered it in its own way. The heart of *Miranda* remains, even after *Innis*.

To be fair, attitudinal studies such as Spaeth and Segal's do demonstrate something important about law, which is that—at least on the Supreme Court—law does not constrain in the way that some rule-of-law models might suppose. Although existing studies do not accomplish what they hope in terms of falsifying the influence of law, they do indicate that judges facing the very same legal issues often vote in different ways—ways, in fact, that tend to line up with some proxy for judicial ideology. This evidence does not falsify law's influence because of the problem of behavioral equivalence. We cannot say that a justice's votes are a function of her ideology or her view of the law.⁵⁷ What we can say, however, is that law does not constrain some judges in some circumstances—most notably Supreme Court justices—from voting in patterns that reflect ideology. That is a point not without its importance, though the importance can be vastly overstated.

Law may seem frustrating to political scientists in that, because of the way it works, the actions of legal actors are not so easily coded as they might like. But the difficulty may be as much the result of a failure to understand law's normative commitments from within the discipline of law itself. If positive scholars want to study politics and law, they must understand the methodology of law itself. This means understanding how law works, as well as the normative demands placed upon it.

Law's substance and process

While modeling legal methodology is complicated, understanding the substance and process of the law is a more straightforward matter. In general, positive scholars do a good job of understanding the substance and process of the law that is the subject of their studies. Nonetheless, there are difficulties. Many of these seem to occur at the vague boundary where substance meets process, and it is difficult to say precisely which is at issue. And, for some reason, appellate practice, the subject of many studies, seems to present its own unique confusion. Unfortunately, these particular difficulties infect otherwise illuminating work in deeply problematic ways.

The law-fact distinction. One persistent problem of great significance seems to be telling the difference between what is a “legal” conclusion and what is a “fact”. This is a distinction of great importance in the legal world, because it often defines who the relevant decisionmaker is. When juries are sitting, most questions of fact are resolved by the jury. Whether trial is by judge or jury, appellate courts tend to defer to factual determinations of lower courts. Although the question of what is a fact seems descriptively easy, in reality it is somewhat complicated, perhaps in part because of the consequences that flow from the determination.

Jeffrey Segal’s study of Fourth Amendment decisions provides a useful example of the difficulty, because the project started down the correct path and took a wrong turn. In early work, Segal specified a “legal” model of Supreme Court decisionmaking.⁵⁸ Charting a course that would be followed by other scholars, he identified the legal factors that seemed determinative of Supreme Court Fourth Amendment decisions, and then tested whether those factors predicted outcomes. The factors Segal relied upon included whether there was “probable cause” for a search, whether the search was of the “home” or of an “automobile”, and whether the search was “incident to a lawful arrest”. In his later work, however, Segal came to view the factors he identified as “facts,” and sought to predict outcomes based on those facts, eschewing the conclusion that it was a legal model deciding the cases.⁵⁹

As it happens, Segal had it right in the earlier work. It is easy to see why one might miss this, because factors such as “search incident to a lawful arrest” or “home” as opposed to “automobile” certainly sound in common parlance like facts. This, however, is wrong. They are legal conclusions that follow from underlying facts. Probable cause, for example, is a legal conclusion based on a collection of specific facts observable by a police officer. The legal standard for probable cause asks whether there is enough evidence (specific facts) to warrant a person of reasonable suspicion in believing that a crime is being, or has been, committed.⁶⁰ Probable cause decisions are reviewed *de novo* (afresh) on

appeal,⁶¹ as are legal conclusions generally, though for other reasons the decision of a magistrate to issue a warrant on probable cause receives deference.⁶²

That these are legal conclusions can be seen by examining what quintessentially looks to be a fact: whether a search occurred in a “home” or an “automobile.” Segal and Spaeth code the case of *California v. Carney* as one involving the “fact” of an automobile; yet, an examination of that case in its proper legal context shows that even the automobile-home distinction is really one of law. Under the Fourth Amendment, warrants are required for searches, but there is an exception for searches that occur in “automobiles.” *California v. Carney* is a case in which the question was whether the police violated the warrant requirement when they searched a mobile home without a warrant. Is a mobile home a “home” or an “automobile”? Stating the problem this way helps one to see that it depends on a resolution of a deeper policy question. If the exception exists because automobiles are mobile and obtaining warrants might be difficult, then Carney’s mobile home is within the automobile exception, as the Supreme Court majority held. But if the warrant requirement is applied more stringently to homes because of the greater violation of privacy involved in invading them, then the Carney dissent was correct in treating the mobile home as outside the automobile exception. The justices in Carney did not differ over the “facts,”—they all agreed on what it was that was searched—but whether, as a matter of law, mobile homes were to be treated as within or without the “automobile” exception.

Calling something a fact or a legal issue matters significantly, both within the disciplinary project of positive scholars and outside the discipline as well. It goes to the very heart of what we think judges are doing, and whether law plays any role at all. Segal and his co-author Harold Spaeth have staked much on the claim that judges decide cases based on factual cues, rather than on legal factors.⁶³ Yet, what Segal’s early specification of his model suggests is that judges actually can and do decide based on legal factors, and that by specifying those factors the influence of law can be seen.⁶⁴ Segal’s original approach finds voice in later useful scholarship that tests a legal model of decisionmaking.⁶⁵

The law-fact distinction on appeal. The confusion about the law-fact distinction plays out with particular impact in important studies of appellate decisions. One example of this is in the Segal and Spaeth work itself. The authors state that they read lower court decisions to ascertain the true facts, because the appellate courts might be inclined to distort those facts to reach outcomes they prefer.⁶⁶ As we have seen, this misconceives the law-fact distinction. And because the Segal-Spaeth factors are legal, not factual, review of them thus lie properly within the province

of appellate courts. Indeed, the Segal-Spaeth approach refracted through the proper role allocation between appellate courts and trial courts brings to mind the old saw about whether the umpire properly called a pitch a ball or a strike: One might say of appellate courts “they ain’t nothin’ till we call ‘em.”

Appellate courts will rarely disturb true factual findings on appeal, something that seems elusive in some of the most promising work on appellate court supervision of the lower courts. One of the great problems in judicial politics is how the Supreme Court, with its small docket, can ensure the fidelity of the lower courts, itself an essential element of the rule of law. There are studies that offer interesting explanations, but to the extent these models turn on appellate fact-finding they necessarily miss the mark.⁶⁷

An example of this phenomenon is found in Cameron, Segal, and Songer’s otherwise excellent analysis of how it is that the Supreme Court decides which cases to resolve, and thereby ensure lower court compliance. As positive studies often recognize, the Supreme Court is able to hear only a small fraction of the cases presented to it (let alone those resolved by the lower courts), so some means of screening cases is needed to ensure that the justices take those that are most useful to it in supervising lower courts. Cameron, Segal, and Songer provide a creative “auditing” model by which the Supreme Court sorts through the cases with a simple mechanism to identify those in which there most likely is a need for intervention.⁶⁸ The intuition of the model is that a conservative Court will be most interested in hearing cases by liberal lower courts rendering liberal results. Far less important will be hearing cases involving conservative results rendered by liberal or conservative lower courts, or even liberal outcomes from conservative panels. This unquestionably is one of the most interesting and plausible theories of Supreme Court case selection.

Unfortunately, in developing and testing their model, Cameron, Segal, and Songer wrongly assume that appeals are about facts, rather than law. Their operating assumption is that only upon reviewing the case will the Supreme Court justices get true information about the facts, thus permitting them to decide how to resolve the case. Until that time, the relevant facts simply are unknown to the appellate judges. This unknown information provides for the asymmetry of information that runs their model.

Although the auditing theory that informs their paper is hugely insightful, the model itself necessarily is flawed. It flies in the face of the cardinal rule of appellate procedure, that appeals exist to resolve questions of law, and that the facts as found by the lower court are pretty much fixed in stone for the appeal.⁶⁹ In other words, appellate courts take the lower court findings of fact, which are displayed on the case record for all to see, as a given, absent the most extraordinary circumstances. Even then,

the dispute typically involves conclusions about ultimate facts to be divined from the evidentiary record. Because of the rigidity of this rule, parties’ briefs do not contain facts not found in the lower court record, and such facts provide no basis for rendering decision.

What motivates appeals? Positive scholars similarly often make assumptions about what motivates appeals that, while rational, do not capture the full range of litigant behavior. These assumptions see litigants as strategic actors, but fail to account fully for litigants’ motivating strategic assumptions, or overstate the capacity of litigants to behave strategically in a rational way. People are not always rational in the way scholars assume, and even if they were, other institutional features may compel seemingly irrational behavior.

A recent paper by McGuire, Smith, and Caldeira provides an example. In the paper, the authors argue that studies of judicial behavior should focus solely on reversals by the Supreme Court, ignoring affirmances.⁷⁰ The theory underlying their paper is that litigants often overshoot the mark in estimating the conservatism or liberalism of the Supreme Court. For this reason, affirmances of lower court decisions may not reflect the Court’s ideological center; affirmances are the Court’s response to litigants that pursue appeals thinking the Court is more extreme ideologically than it really is.

McGuire, Smith, and Caldeira fail to account for the fact many appeals are taken—even in the Supreme Court—simply because there is enough at stake that the case must be pursued even if the likelihood of success is small. Animating the authors’ paper is the assumption that litigants are strategic players, trying to make law favorable to them. Under this sort of strategic approach, appeals will not be taken if the probability of success is low, particularly if this will establish a bad precedent. But not all litigants are repeat players, nor do they all care about the direction of the law. Death penalty cases provide a good example. The present Supreme Court is hardly friendly to death-sentenced inmates, but one who is facing execution will pursue every possible appeal, and rationally so. This fact explains the many Supreme Court cases that a rational strategic actor concerned solely about the long-term direction of the law would not pursue. The same may be true of a civil litigant who has a lot on the line. Pursuing fruitless appeals may be all the more common if the litigant is not footing the bill. Many criminal defendants likely pursue long-shot appeals, particularly in the Courts of Appeal, because—as indigents—their fees are being paid by the government.

Trading grounds for decision. Just as studies of strategic interaction must take full account of factors that might influence strategic decisions, they also must pay attention to the bounds law places on strategic conduct. One of the

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most intriguing areas of research into the strategic behavior of courts looks to the ways in which courts can trade the grounds on which they rest decisions in order to maximize institutional preferences. In a seminal piece of work Pablo Spiller and Matt Spitzer make the point that to pursue policy preferences and avoid strategic responses from other branches of government, judges have the option of moving from one ground of decision to another.⁷¹ Thus, Spiller and Spitzer describe the trade off between the relative permanency of constitutional grounds of decision and the more specific instructions to administrative agencies offered by other, nonconstitutional grounds of decision.⁷²

Although this is a valuable insight, scholars applying it must take care to ensure that the alternative grounds for decision really are available. An example of where the trading-grounds analysis can run into trouble involves studies of strategic behavior between courts and legislatures. Scholars testing separation-of-powers models involving the Supreme Court have argued that trading between constitutional and statutory grounds could be useful in situations where courts arguably were restrained by the other branches.⁷³ The idea here is that the Court can be overriden when it comes to statutory but not constitutional grounds. Thus, if the Court is operating in a political environment in which reversal is likely, the Court will trade away from statutory and to constitutional grounds.⁷⁴

The practice of moving from constitutional to statutory grounds plainly occurs, and its occurrence actually does provide insight into strategic separation of powers behavior. Not all constitutional cases involve statutes, but many cases involving the federal government do. When confronted with the question of whether what Congress did was constitutional, the Court will not infrequently interpret the statute in a manner that avoids the constitutional question altogether.⁷⁵ Sometimes these “interpretations” are quite creative, suggesting the Court is bending over backwards to avoid ruling on constitutional grounds. There are normative reasons for this: it is more difficult to overturn constitutional decisions, and the Court might wish to give Congress some room to respond. But there also are strategic grounds, because the Court can avoid trouble by making it seem that Congress itself made the relevant policy determination, and by making it easy for Congress to reverse the Court’s decision if Congress so desires. In these instances, the Court dances around constitutional conflict by trading to statutory grounds.

Trading from a *statutory* to a *constitutional* question, however, is much less common, and often is impossible. First, many cases will not have been brought to the Supreme Court in constitutional terms. There is a general rule that the Court will only resolve questions put to it. The rule is not cast in concrete, and the Court sometimes reaches out to resolve constitutional questions that were not presented. Empirical testing could reveal how often this occurs,

but it seems a poor idea to rely on the practice until we know. Second, and more important, it misconceives of the nature of law to think that any statutory case simply can be turned into a constitutional question. Many if not most questions of statutory interpretation do not present any obvious constitutional issue, and surely not one that can be presented with a straight face.⁷⁶ Those who rely on the possibility of trading from statutory to constitutional grounds bear the burden of demonstrating that this is possible.⁷⁷

Pointing to these examples should not undermine the excellent job most studies do of working to get law right. The points made here easily can escape the notice of those not mired in the norms of the legal profession. Still, such errors undermine otherwise strong work.

Data Collection and Bias

The workings of courts and judges are difficult to subject to empirical analysis. Not only is it laborious to collect data about judicial decisions, but some of the most valuable information simply is unavailable because of the shrouded process of judging. Nonetheless, output is only as good as input. Here there are some very tangible things that political scientists can do to improve the quality of their conclusions.

There are two separate problems discussed here, both of which might lead to a lack of confidence in the results of any given study of courts and judicial behavior. The first is a classic problem of selection bias. Selection bias occurs when researchers find support for a hypothesis about judicial behavior by focusing on a portion of the available data where support is most likely to be found, while neglecting to study a broader set of data that might call those conclusions into question. The second is one of idiosyncrasy. Researchers may reach conclusions based on the data at hand, without sufficient recognition that because of the nature of the judicial process the conclusions they draw may have a scope limited to specific judges or types of cases. The distinction between these two problems is a subtle one, but worthwhile drawing as it does highlight different things that can go wrong.

As will be apparent, many of the sort of selection bias and idiosyncrasy problems discussed here are fairly attributable to difficulties with data collection as much as an overeager desire to obtain confirmation of the researcher’s hypothesis. An obvious difficulty is that enormous effort may go into collecting data on courts, only to have the relevant questions shift beyond what the collected data can support. But bias is bias, whatever the cause. While researchers must draw lines regarding what data they will collect or rely upon it is important in drawing those lines to have an explanation as to why the ultimate findings are not the result of selection bias or idiosyncratic behavior.

Problems arise when the excluded data is very promising as a source of accurate information regarding the matter being studied, and no such explanation is forthcoming.

Selection bias

Merits cases and discretionary review. One potential problem of selection bias occurs when scholars studying Supreme Court decisionmaking focus on cases the Supreme Court decides on the merits, without regard to those the Court has declined to hear. Whether the focus on decided cases presents an actual problem depends on the use to which the data is being put. Because attitudinal studies only are trying to reach a conclusion about what affects the decision of cases actually taken up by the Court, attention to decided cases is just fine. But studies that seek to estimate the constraint the Court faces from the other branches necessarily must look not only to cases the Court does take, but to those it does not as well.

In separation of powers studies, for example, some scholars have concluded that the Court typically does not pull its punches because of constraint imposed by the other branches.⁷⁸ But looking only to cases in which the Court grants merits review potentially provides a biased picture. The Court may decline review in those cases in which it faces constraint; if so, such constraint never would show up in studies that look only to granted cases.⁷⁹

Unpublished decisions. Another possible source of bias appears when scholars rely only on a court's published decisions, without taking account of the unpublished ones. Whether this is a problem depends on how published opinions differ from unpublished ones with regard to the subject being studied. In a recent revealing study of the influence of ideology in court of appeals decisionmaking, Cass Sunstein, David Schkade, and Lisa Ellman look only to published opinions. They reason that because they specifically are looking for evidence of the influence of ideology on judging, they are justified in excluding from their sample unpublished decisions. They explain that "unpublished opinions are widely agreed to be simple and straightforward and to involve no difficult or complex issues of law."⁸⁰ Because they are only looking for evidence of ideological influence, and not purporting to present a complete picture, it is possible that excluding published opinions did not matter their study.

The difficulty with excluding unpublished opinions, however, is that there is some evidence suggesting that these cases are not as straightforward as Sunstein, Schkade, and Ellman state. As a variety of studies—including their own—suggest, ideological voting may in part be a product of appellate panel composition.⁸¹ Panels of three Republican or Democratic judges may see a case as "easy" and unworthy of publication that a mixed panel would see very differently. To the extent this is true, looking

only to published opinions may lead to substantial selection bias.

Settlement effect. Yet another source of bias arises from looking only at decided cases, although this may be less of a problem when studying aspects of judicial behavior. As George Priest and Benjamin Klein explained some time ago, studies of law and judicial behavior often rely heavily on appellate decisions. Yet, because parties have it in their power to settle cases, looking only at decided cases can be troublesome. As Priest and Klein point out, "only a very small fraction of disputes comes to trial and an even smaller fraction is appealed."⁸² Positive scholars need to demonstrate an awareness of whether a settlement effect might be biasing their conclusions.

Civil liberties cases. A more complicated problem of selection bias is in positive scholars' over-reliance on civil liberties cases, particularly in attitudinal studies.⁸³ The difficulty with the use of such cases is that they are among the most fraught in our political system, and therefore the most likely to be the subject of ideological voting. Again, to the extent that one seeks only to show that *sometimes* ideology plays a role, singular attention to these cases is just fine. But at some point demonstrating the same thing over and over becomes old hat, and it becomes necessary to get a more balanced picture of what goes on in the judiciary. This certainly is the case to the extent one cares whether the rule of law can operate in run of the mill cases, such as contract actions or debt collection actions, the sort of thing of great interest to theories of the relationship between an independent judiciary and economic development. Once the goal of the judicial behavior studies is expanded, focus only on the set of cases where ideology is most prevalent presents a not insubstantial problem of selection bias.

Idiosyncrasy

This brings to the fore the problem of idiosyncrasy, a serious one in studies of judicial behavior. As others have observed, conclusions about judicial behavior run the risk of being highly particularistic. Thus, Dan Pinello refers to Bowen's caveat: "any general statement on judicial behavior 'must be qualified to mean that this holds for these judges, in these cases, at this point in time.'"⁸⁴ Thus, conclusions about judicial behavior may vary by court, by judge, and by case, or even by the time period in which decisions are rendered. A good example to make this point are the studies showing an early twentieth-century norm of consensus on the Supreme Court, which has since collapsed.⁸⁵ Those offering snapshots of judicial behavior must remain cognizant of the point that the norms of judging may vary widely, and that it is difficult to generalize from results.

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Whether idiosyncrasy is a problem again depends on the nature of the claims being made, as is apparent in two different uses of similar data by Lee Epstein and co-authors. The data in both cases involved Supreme Court decision-making at a time when Warren Burger was the chief justice. In legal lore Chief Justice Burger is legendary for his exercise of the prerogatives of the Chief's chair, including his habit of passing during the conference vote in order to see where a majority stood, so that he could then cast a vote that allowed him to utilize his opinion assignment authority.⁸⁶ In *Choices*, the authors draw a variety of conclusions about the behavior of the chief justice of the Supreme Court, and those conclusions are offered in a way that makes them seem generalizable. But if Burger's behavior was idiosyncratic, not much of a conclusion can be drawn from it—except, perhaps, about how power can be misused. In contrast, in an interesting paper on “heresthetical maneuvering,” the authors make precisely this point, focusing on what a strategic-minded justice *could* do.⁸⁷ The difference in emphasis is important, because although both pieces of work are extremely revealing of Supreme Court behavior, the latter source limits the claim it is making to what the selected data actually show.

Positive scholars face enormous problems of data collection. In order to get at their data they might have to dig through the private papers of the justices or mine countless cases in which certiorari is denied, or review numerous unpublished decisions. It is reasonable at times to decide not to do this, and to rely on data more readily at hand. But in choosing the latter course, authors of studies of judicial behavior must ensure that the conclusions they reach will not be belied by data that has been neglected.

Conclusion

In some ways, positive scholars of the judiciary are in the catbird seat. For years, they have been seeking answers to questions that now seem of paramount importance. The positive study of courts and judicial behavior provides an opportunity to understand what it is we realistically can expect of courts as institutions of government. The positive project, however, is only as good as its own understanding of law and the legal system. An outsider's eye can be extremely enlightening, but only if the outsider has full knowledge and understanding of the practices being observed.

There are tangible things that positive scholars studying judicial behavior should do. They should expend less of their efforts on internal disciplinary disputes, structuring their projects to address pressing questions about courts and the legal system. Rather than being so quick to dismiss law, positive scholars should do their best to understand it and take the claims made on its behalf seriously, enhancing our ability to model and assess legal decision-making. They should take care that in researching and

drawing conclusions about the legal system and judicial behavior, they properly understand the substance of the law and the legal process. And, in collecting and analyzing data, they should be sensitive to whether their conclusions are based upon a sample that may be idiosyncratic or biased, because they have neglected additional data that—although hard to collect—would cast a very different light on their subject. By taking these steps, positive scholars studying courts can maintain their place at the forefront of an intellectual quest that initiated decades ago.

Finally, it is worth suggesting that now might be the time for interdisciplinary collaboration between legal scholars and positive political scientists. Decades of differing approaches have left a lingering antagonism between the projects. Yet, one sure way of overcoming lack of understanding about a discipline is collaboration. Two heads—or the heads of two disciplinary scholars—may indeed be better than one. As positive scholars and legal scholars increasingly look to one another's projects, collaboration is at least one important route to explore.

Notes

- 1 Popper 1963, 88.
- 2 Kelly 1965, 122.
- 3 Kelly 1965, 122 n.13.
- 4 Epstein and King 2002, 1.
- 5 See for example European Association of Methodology 2005.
- 6 Committee on Science, Engineering, and Public Policy 2004, 1.
- 7 Slaughter, Tulumello, and Wood 1998, 368.
- 8 See Cameron 2002; Mahoney 2001, 504–5, discussing Hayek 1960; Hayek 1973.
- 9 As Dan Klerman and Paul Mahoney recently put it, “the phenomenon of interest is clear—can judges make decisions based on ‘the law’ without fear of reprisal from the executive or legislature?” Klerman and Mahoney 2005, 3.
- 10 For example, a Westlaw search of law reviews and journals for citation references to Segal and Spaeth's *The Supreme Court and the Attitudinal Model*, *The Supreme Court and the Attitudinal Model Revisited*, and *Majority Rule or Minority Will* yields a total of 287 references. Only 144 of those references occurred between the years of 1993 and 2001, but since 2001 legal scholarship has referenced one of these works 161 times.
- 11 Epstein and King 2002, 2.
- 12 Examples include the appointment of Frank Cross to the Texas law faculty, and Emerson Tiller to Northwestern. Stefanie Lindquist is an associate professor in both Vanderbilt University's Department of Political Science and School of Law. Tracey George, a Professor at Vanderbilt Law School, is an

- example of a joint JD/PhD working on law and courts issues in the legal academy.
- 13 Segal 2003; Baum 2003; Whittington 2000.
 - 14 Segal and Spaeth 2002; Whittington 2000.
 - 15 Epstein and Knight 1998; Gillman 2001; Whittington 2000.
 - 16 Gillman 2001; Gillman 2003; Whittington 2000.
 - 17 Segal and Spaeth 2002; Epstein and Knight 1998.
 - 18 Gillman 2001; Peretti 1999; Segal and Spaeth 2002.
 - 19 Brenner 1980; Brenner 1982; Brenner, Hagle, and Spaeth 1989; Brenner and Spaeth 1988; Hagle and Spaeth 1991.
 - 20 Maveety and Maltese 2003, 229.
 - 21 Brenner and Spaeth 1988.
 - 22 Brenner et al. 1989, 409.
 - 23 Ibid, 423.
 - 24 Ibid, 409–10.
 - 25 Kornhauser and Sager 1986; Kornhauser and Sager 1993; Michelman 1986.
 - 26 Benesh and Spaeth 2001, 14.
 - 27 See Friedman 2004.
 - 28 Epstein and Knight 1998, 72.
 - 29 Interestingly, it often is being done with those who have legal training. For example, Staudt 2004; Tiller and Cross 1998; Revesz 2002; George 1998.
 - 30 See Klein 2002, 143; Cross 2003, 1511
 - 31 Caminker 1999, Dorf 1995.
 - 32 Segal and Spaeth 2002; Spaeth and Segal 1999; Peretti 1999.
 - 33 Peretti 1999; Spaeth and Segal 1999; Baum 2003; Segal 2003.
 - 34 Peretti 1999; Segal and Spaeth 2002.
 - 35 Hart 1961.
 - 36 For example Dworkin 1986; Murphy 2000; Raz 1980.
 - 37 Maltzmann, Spriggs and Wahlbeck 2000; Ruger et al. 2004.
 - 38 Segal and Spaeth 2002, 357
 - 39 Maltzmann, Spriggs and Wahlbeck 2000.
 - 40 See Levi 1948.
 - 41 Dworkin 1986, 81–130; Dworkin 1977, 225–75.
 - 42 Ruger et al. 2004, 1152–53, quoting Spaeth 1965, 879.
 - 43 George and Epstein 1992; Kritzer and Richards 2003; Richards and Kritzer 2002.
 - 44 Ibid.; see also Cross 1997.
 - 45 George and Epstein 1992.
 - 46 Ruger et al. 2004.
 - 47 Ibid, 1162–63.
 - 48 *Grutter v. Bollinger*; *Gratz v. Bollinger*.
 - 49 *Grutter v. Bollinger*.
 - 50 Segal and Spaeth 2002.
 - 51 See for example, *United States v. Lopez*; *United States v. Morrison*.
 - 52 Caminker 1999.
 - 53 Songer and Lindquist 1996.
 - 54 Spaeth and Segal 1999.
 - 55 *Rhode Island v. Innis*, 305–7 (Marshall, J., dissenting).
 - 56 Spaeth and Segal 1999, 36 (stating this test for weakly preferential voters).
 - 57 Epstein and Knight 1998.
 - 58 Segal 1984.
 - 59 Although it is unclear why he did so, one possible reason is that predicting cases on the basis of “facts,” lent strength to the claim that decisions were based on attitudes and not on law. Segal 1986; Segal and Spaeth 2002.
 - 60 *Brinegar v. United States*.
 - 61 *Illinois v. Gates*.
 - 62 Ibid.
 - 63 Segal and Spaeth 2002.
 - 64 Segal 1984.
 - 65 George and Epstein 1992; Kritzer and Richards 2003; Richards and Kritzer 2002.
 - 66 Segal and Spaeth 2002.
 - 67 It is not clear whether the confusion represents uncertain understanding about appellate procedure alone, or if the necessities of formal modeling contribute to the problem. Some of the models of appellate decisionmaking rely upon the idea of asymmetric information. The assumption is that litigants or lower court judges may have information about a case that the Supreme Court has to grant review in to obtain. Although this assumption standing alone may be correct, trouble comes when scholars make further assumptions about the nature of this private information.
 - 68 Cameron, Segal, and Songer 2000.
 - 69 Fed. R. Civ. P. 52; 5 Am. Jur. 2d *Appellate Review* § 697.
 - 70 McGuire, Smith, and Caldeira 2004.
 - 71 Spiller and Spitzer 1992.
 - 72 Ibid.
 - 73 See Epstein, Knight and Martin 2001, especially at 595 & n.32, 596; Spiller and Spitzer 1992.
 - 74 Epstein, Knight and Martin 2001, Spiller and Spitzer 1992.
 - 75 *Webster v. Doe*; *Ashwander v. Tennessee Valley Auth.*
 - 76 The reasoning underlying the idea of trading to constitutional arguments is not airtight anyway; if the Court is acting at a time of constrained decision-making, it might do well to avoid constitutional decisions that cannot be overturned and thus will sharpen interbranch conflict.
 - 77 It is possible that the claim of these studies is only that the Court trades in gross, taking more of its constitutional docket when facing a hostile Congress. There are difficulties with this claim, however. First, it is not apparent on the face of the work. See Epstein, Knight, and Martin 2001, Spiller and

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Spitzer 1992. Second, given that the other branches have the power to discipline the Court for decisions they cannot alter, the idea that the Court would trade to constitutional issues to avoid overruling by a hostile Congress is in significant conflict with claims (often by the same authors) that the Court is constrained in constitutional cases by the Congress and the Executive.

- 78 Segal and Spaeth 2002.
 79 Friedman and Harvey 2003.
 80 Sunstein, Schkade, and Ellman 2004, 313.
 81 Songer, Smith, and Sheehan. 1995; Revesz 1999.
 82 Preist and Klein 1984, 2.
 83 Gillman 2003 (Howard Gillman, Separating the Wheat from the Chaff in *The Supreme Court and the Attitudinal Model Revisited*, in *Supreme Court Attitudinal Model Revisited: Authors Meet Critics*, 13 *Law and Courts Newsletter*, Issue 3, at 14 (located at <http://www.law.nyu.edu/lawcourts/pubs/newsletter/summer03.pdf>)).
 84 Pinello 1995, 24.
 85 Epstein, Segal, and Spaeth 2001; Post 2001.
 86 By tradition, the Chief Justice assigns the opinion author when in the majority; otherwise the assignment is for the senior Associate Justice in the majority to make. See Woodward and Armstrong 1979.
 87 Epstein and Shvetsova 2002.

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- 5 Am. Jur. 2d *Appellate Review* § 697 (2004).
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