



A Letter from the Section Chair:

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The cancellation of the 2012 annual meeting of the American Political Science Association in New Orleans due to Hurricane Isaac was disappointing for many reasons, not the least of which is that the Law and Courts Section had much to celebrate. The Section still has much to celebrate, however, and chief among them are the Lifetime Achievement Award recipient for 2011-12 and the upcoming inaugural issue of the *Journal of Law and Courts*.



As I am sure you are all aware, Professor Robert Kagan was the recipient of the Lifetime Achievement Award, the Section's most prestigious award. This is terrific choice on the part of the award committee—Susan E. Lawrence (chair), Lori Hausegger, John Kilwein, Jeffrey Staton, and Mary Volcansek—for so many reasons. In the words of the committee itself:

Professor Kagan's elegant blend of legal, socio-legal, political, historical, and comparative analysis in path-breaking scholarship, teaching, and academic service has redefined the boundaries of the law and courts field. Finding big theoretical questions in careful empirical analysis of specific settings, Kagan's work integrates comparative perspectives and the "law and society" dimension into a deeply political account of law, regulation, courts, and adjudication. A founding member of the law and society movement, Kagan directed Berkeley's Center for the Study of Law and Society. A gifted teacher and a careful, cogent, and inquisitive critic, Kagan has mentored several generations of award-winning scholars of law and courts/law and society.

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Announcements

Rorie Spill Sohlberg as new Editor of *Judicature*

Judicature, a peer-reviewed journal, strives to present the best scholarship and commentary on the administration of justice, both civil and criminal, and judicial politics, broadly speaking.

Judicature publishes work from across the social sciences, as well as scholarship and commentary from attorneys, judges, and research organizations. While the journal focuses primarily on the administration of justice in the United States—manuscripts that are global or international in scope, as well as work that is national, local, or examines connections between these levels are welcome. The content of the journal is not subject to restrictions based upon the policy positions of the *American Judicature Society*. *Judicature* is interdisciplinary in focus.

The editor is open to a wide range of analytic approaches including interpretive, historical, quantitative, and multi-method analyses, among others. Applied research on the administration of justice is particularly suited for publication in *Judicature*. *Judicature* also welcomes shorter pieces adapted from works larger in scope. The readership includes practitioners as well as scholars; contributors should keep this broad audience in mind when crafting their manuscripts for the journal.

Judicature is indexed in *Index to Legal Periodicals*, *Current Law Index*, *Legal Resource Index*, *Criminal Justice Periodical Index*, and *PAIS Bulletin* and is available on-line on the WESTLAW service.

LSAC Research Grants

The Law School Admission Council (LSAC) Research Grant Program funds research on a wide variety of topics related to the mission of LSAC. Specifically included in the program's scope are projects investigating precursors to legal training, selection into law schools, legal education, and the legal profession. To be eligible for funding, a research project must inform either the process of selecting law students or legal education itself in a demonstrable way.

The program welcomes proposals for research proceeding from any of a variety of methodologies, a potentially broad range of topics, and varying time frames. Proposals will be judged on the importance of the questions addressed, their relevance to the mission of LSAC, the quality of the research designs, and the capacity of the researchers to carry out the project.

Application deadlines are February 1 and September 1.

For more details, go to <http://www.lsacnet.org/LSACResources/Grants/lvac-legal-education-grant-program.asp>.

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General Information

Law and Courts publishes articles, notes, news items, announcements, commentaries, and features of interest to members of the Law and Courts Section of the APSA. **Law and Courts** is published three times a year in Winter, Summer, and Fall. Deadlines for submission of materials are: February 1 (Winter), May 1 (Spring), and October 1 (Fall). Contributions to **Law and Courts** should be sent to the editor:

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Articles, Notes, and Commentary

We will be glad to consider articles and notes concerning matters of interest to readers of **Law and Courts**. Research findings, teaching innovations, or commentary on developments in the field are encouraged.

Footnote and reference style should follow that of the *American Political Science Review*. Please submit your manuscript electronically in MS Word (.doc) or Rich Text Format (.rtf). Contact the editor or assistant editor if you wish to submit in a different format. Graphics are best submitted as separate files. In addition to bibliography and notes, a listing of website addresses cited in the article with the accompanying page number should be included.

Symposia

Collections of related articles or notes are especially welcome. Please contact the Editor if you have ideas for symposia or if you are interested in editing a collection of common articles. Symposia submissions should follow the guidelines for other manuscripts.

Announcements

Announcements and section news will be included in **Law and Courts**, as well as information regarding upcoming conferences. Organizers of panels are encouraged to inform the Editor so that papers and participants may be reported. Developments in the field such as fellowships, grants, and awards will be announced when possible. Finally, authors should notify **BOOKS TO WATCH FOR EDITOR**:

Drew Lanier, of publication of manuscripts or works soon to be completed.

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The cancellation of the annual meeting meant, of course, that the Lifetime Achievement Panel scheduled to commemorate Bob's remarkable contributions to the study of law and courts was also cancelled. Stay tuned, however, as plans are underway to have this panel rescheduled for the 2013 annual meeting in Chicago. (Note that all award recipients from 2011-12 and 2012-13 will be recognized at the Law and Courts Section's business meeting at the 2013 annual meeting.)

On a related note, the Executive Board has decided to forego a Lifetime Achievement Award recipient for 2012-13. This accomplishes two goals. First, and most importantly, it allows us to focus on one Lifetime Achievement Award recipient at a time. As even a casual perusal of the list of past recipients makes clear, the Section is fortunate to have so many illustrious scholars included in our ranks. Their remarkable careers are truly causes for celebration by the Section, and this is certainly the case with Bob's career. Skipping a 2012-13 recipient means that we can give our full and undivided attention to celebrating his achievements. And, second, the panel allotments for Section 26 Law and Courts and Section 27 Constitutional Law and Jurisprudence are never sufficient to permit all of the high quality work in our sub-field to appear on the program. The jobs of the section chairs are always made more challenging by the need to carve out some of their limited panel space for the Lifetime Achievement Award Panel. Skipping a 2012-13 recipient means that Sara Benesh (Section 26 Chair) and Corey Brettschneider (Section 27 Chair) will not face an even greater challenge in having to carve out space for two Lifetime Achievement Award Panels. (Though I hasten to add, there is no question that the Lifetime Achievement Award Panel is always worth any such trade-off.)

Special thanks are due to Rorie Spill, the editor of *Judicature*, for another item related to the Section's Lifetime Achievement Award. She reached out to Tom Burke, who was to chair the Lifetime Achievement Panel at the cancelled 2012 meeting, to inquire about putting together a special piece for *Judicature* about Bob's career and achievements. This focus piece is tentatively anticipated to appear in one of the Spring 2013 issues of the journal. Rorie is also willing to make this a regular feature to highlight the recipient of the Lifetime Achievement Award each year. This is a marvelous idea for so many reasons, not the least of which is that it will provide one more venue for showcasing our Lifetime Achievement Award recipients and serve as a mechanism for further promoting the fine scholarship of our members to even broader audiences.

Also cause for celebration is the Section's new journal, *Journal of Law and Courts*, which is set to make its debut shortly in Spring 2013. Under the very able direction of Dave Klein, the *Journal's* editor, and the accomplished set of scholars who serve on the *Journal's* editorial board, the inaugural issue includes six compelling articles. In that issue, Gillian Hadfield and Barry Weingast examine the coordination of decentralized collective punishment and, in the process, draw attention to the emergence, stability, and function of law in fostering economic growth. Ryan Owens, Justin Wedeking, and Patrick Wohlforth consider how ambiguity in the language of Supreme Court opinions can be used by the justices to protect their policy choices from a politically hostile Congress. Alex Stone Sweet and Thomas Brunell investigate how the courts of ECHR, the EU, and the WTO engage in majoritarian activism; i.e., producing laws that enjoy a high degree of state consensus but would be unlikely to be adopted under unanimity decision rules. Lawrence Baum uses the Takings Clause as the setting for unraveling the linkage of issues and ideology in the Supreme Court. Ward Farnsworth, Dustin Guzior, and Anup Malani explore the nature of legal ambiguity in their analysis of how policy preferences shape legal interpretation. And, finally, Peter Nardulli, Buddy Peyton, and Joseph Bajjalieh take a close look at the meaning of the rule of law and develop a measure based on a country's formal commitment to the rule of law and the presence of infrastructure necessary to meet that commitment.

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To date, submissions have come primarily from political science but a non-trivial number have also come from other disciplines as well as the legal academy. Under the agreement between the Section and the publisher (the University of Chicago Press), the *Journal* will publish two issues per year for the time being. The *Journal* will transition to publishing four issues a year as soon as practicable given the volume and quantity of submissions. The underlying message here is that how quickly the *Journal* moves from two to four issues per year is up to us. If we increase the volume of our high quality work that we submit for consideration at the *Journal*, that transition will come sooner. We are a diverse section, to be sure; there is nothing novel in that observation. And our diversity means that our scholarship finds a home in an equally diverse set of publication outlets. There is value in that but there is also real value (and I would argue even more value) in having some of our best work appear in an outlet common to the Section's membership. We rightly extol the virtues of the diversity of our membership but it seems to be more and more difficult to keep abreast of even the most exciting work outside of our individual niches in law and courts. By publishing work that represents the diversity in the Section, the *Journal* provides a means for helping the members (not to mention those outside of law and courts) to truly understand and appreciate that diversity.

I am sure that the academic year will bring much more in the way of things to celebrate in the Section. We do seem to be fortunate in that regard. In the meantime, I hope that you are finding yourselves either just embarking on or enmeshed in fulfilling and satisfying professional careers.

Symposium: The Contributions of Psychology to Law and Courts Research

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Political scientists have long incorporated theories and methods from psychology into their studies of law and courts. Such analyses have greatly improved our understanding of a wide range of topics, including decision making, attitudes toward the legal system, and the impact of judicial decisions. That political scientists have devoted so much attention to understanding psychological aspects of the legal system should not be surprising. After all, judges, jurors, litigants, attorneys, and a bevy of other legal actors are, above all, human beings. As such, their judgments and decisions are potentially subject to the same psychological influences that weigh on all of us.

While incorporating psychological theories and methods into law and courts research offers a great deal of leverage over the behavior of legal actors, doing so also presents an assortment of unique challenges. As the Section Chair for Judicial Politics at the 2012 annual meeting of the Midwest Political Science Association, I had the opportunity to put together an exceptional roundtable addressing the contributions of psychology to judicial politics research. Participants included Larry Baum, Eileen Braman, Brandon Bartels, Dave Klein, and Wendy Martinek. Each of these esteemed scholars has a great deal of experience applying psychological insights to the study of law and courts. As such, this symposium, which builds on some of the issues addressed at the roundtable, is sure to shed light on how political scientists

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studying judicial politics think about psychology, including both the benefits and limitations of incorporating psychology into our research. As a means of familiarizing readers with how psychology has informed the study of law and courts, below I present an abridged look into some of the dominant themes in this vein of scholarship.¹

PSYCHOLOGY AND LAW AND COURTS RESEARCH: A VERY BRIEF OVERVIEW

It is doubtful that psychology has influenced any line of research more so than the study of judicial voting behavior, particularly with regard to the U.S. Supreme Court. Indeed, many of the seminal early studies of judicial decision making were heavily shaped by psychology in arguing that judges are motivated by policy goals (e.g., Frank 1930; Pritchett 1948; Schubert 1965). Moreover, several of these studies adopted methods from psychological research, such as Guttman scaling (e.g., Schubert 1959; Spaeth 1965). In building on these pioneering efforts, later scholars turned to understanding attitude formation (e.g., Ulmer 1973; Tate 1981), heuristics (e.g., Segal 1986), judicial roles (e.g., Gibson 1978; Grossman 1968), personality traits (e.g., Atkins, Alpert, and Ziller 1980), and small group influences (e.g., Atkins 1973; Ulmer 1971). By the time Segal and Spaeth (1993) articulated the clearest vision of the attitudinal model to date, it was apparent that psychological approaches were a mainstay in law and courts research. In more recent years, political scientists have sought to understand how audiences (e.g., Baum 2006; Yates, Levey, and Moeller 2010), birth order (e.g., McGuire n.d.), cognitive styles (e.g., Owens and Wedeking 2012), context (e.g., Bartels 2011; Baum 1997), preference choice (e.g., Braman 2006), and reasoning processes (e.g., Bartels 2010; Braman 2009; Collins 2008a; Collins and Martinek 2011; Rowland and Carp 1996; Ostberg and Wetstein 1998) shape judicial decision making.

Of course, scholars have incorporated psychological approaches to study behavior outside of the judicial vote. For example, psychological theories have informed our understanding of why judges author separate opinions (e.g., Collins 2011; Rathjen 1974) and the cognitive complexity of those opinions (e.g., Owens and Wedeking 2011). Beyond the behavior of judges, psychological approaches have enhanced our comprehension of the legitimacy of judicial institutions and decisions (e.g., Gibson and Caldeira 2009; Hoekstra and Segal 1996; Mondak 1990), the implementation and impact of those decisions (e.g., Canon and Johnson 1999; Johnson 1967; Muir 1967), and media framing of court procedures (e.g., Baird and Gangl 2006).²

TEMPORAL CHANGES

While this brief review makes it evident that psychological approaches have long been employed in a wide array of research agendas, I wanted to get a sense of whether there have been any temporal changes with regard to the use of psychology in law and courts scholarship. In addition, I sought to bring some data to bear on my suspicion that psychological research on law and courts was being supplanted by strategic research. My sense is that, in the past two decades or so, there has been increased attention to strategic theories and that this increased attention has resulted in fewer political scientists incorporating insights from psychology into their studies of laws and courts, or training their graduate students to

¹ For much more thorough reviews of psychological approaches to law and courts research, particularly with regard to judicial decision making, see Klein and Mitchell (2010), Guthrie, Rachlinski, and Wistrich (2001), and Wrightsman (2006).

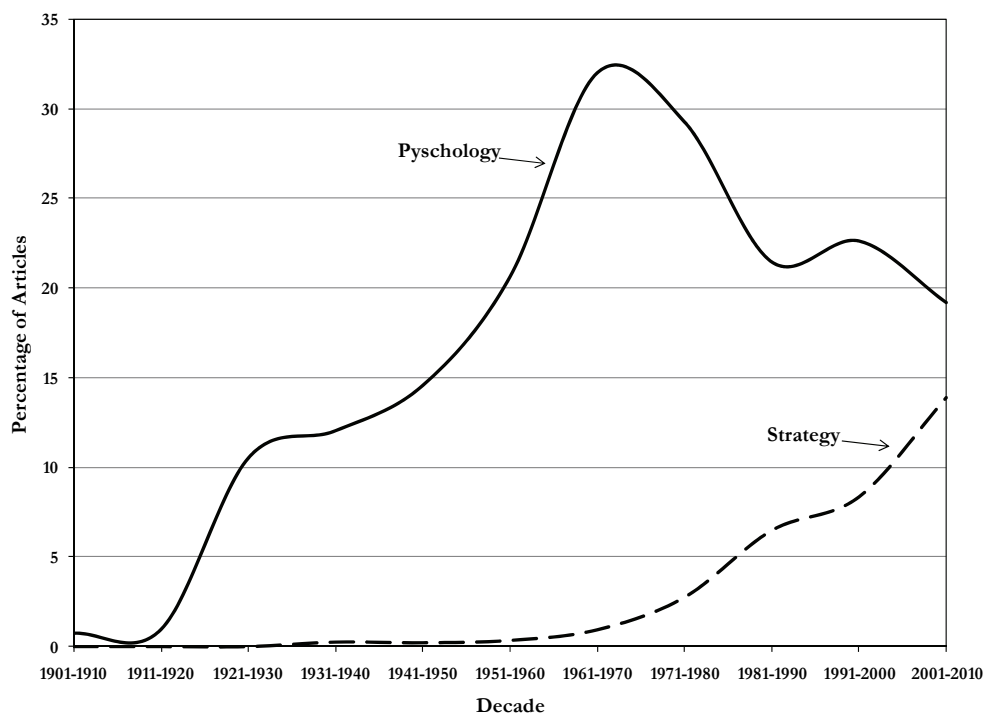
² One of the areas in which political scientists have devoted little attention – from psychological perspectives or otherwise – is jury decision making, the study of which tends to be dominated by psychologists (e.g., Vidmar and Hans 2007). This is somewhat surprising given that juries play a major role in determining who gets what, when, and how (Lasswell 1936).

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do so (e.g., Bonneau 2008; Epstein and Knight 2000). To be clear, this is not a dig on strategic approaches to law and courts. I have no doubt that the “second wave” of strategic studies of judicial politics has contributed a great deal to our understanding of the legal world. I simply sought to examine whether my suspicion stands up to empirical scrutiny.

To do this, I examined courts-related research articles published in four major political science journals: *American Journal of Political Science*, *American Political Science Review*, *Journal of Politics*, and *Political Research Quarterly*.³ Using JSTOR, I performed full text searches for the terms “court” AND “psycholog*” (psychology); and “court” AND “strateg*” (strategic), where the asterisk is a wild card.⁴ I searched by decade from 1901 to 2010. To establish a baseline to compute the percentage of courts-related articles referencing psychology and strategy, I also searched for the word “court” in the full text of research articles. While this is obviously a crude search strategy, it nonetheless should shed some light on how often political science research on law and courts works from psychological and strategic perspectives.

Figure 1. The Percentage of Courts-Related Articles Referring to Psychology and Strategy in Four Political Science Journals



³ While there are certainly many other fine outlets, these journals are well regarded in the law and courts community and overwhelmingly publish work written by political scientists, who make up the vast majority of section members (Martinek 2011; see also Curry 2009; Kuersten 1998). I also recognize that, by excluding books from consideration, I have missed a number of important contributions to both the psychological and strategic research traditions, including those made by several members of the roundtable on which this symposium is based (e.g., Baum 2006; Braman 2009; Collins 2008a; Hettinger, Lindquist, and Martinek 2006; Klein and Mitchell 2010).

⁴ Note that these categories are not mutually exclusive. For example, an article that incorporates insights from both psychological and strategic theory (e.g., Collins 2008b; Wedeking 2010) would appear in both categories.

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Figure 1 displays the percentage of courts-related articles referring to psychology and strategy in the four political science journals for each decade. The solid black line is the smoothed percentage of articles referencing psychology, while the dashed black line is the smoothed percentage of articles referencing strategy. Turning first to psychology, it is evident that there was a fairly steady increase in psychological approaches to law and courts, up until it reached its pinnacle in the 1960s and 70s. During that era, about 30% of all courts-related research referred to psychology. Since 1980, there has been a slow decline in attention to psychology. Today, about one in five articles published in these journals references psychology.

As psychological research abated in the 1980s, there has been a rather sharp increase in strategic law and courts research. For example, during the 1980s only about 6% of articles referred to strategy, compared to about 14% in the modern era. Thus, it is clear that, among research in the journals under analysis, law and courts scholars are increasingly turning to strategic theories, with fewer and fewer articles utilizing intuitions from psychology. If these trends hold true for future scholarship, it is plausible that the next decade or so will see strategic research supplant psychological approaches in the study of law and courts.

It is consequently an opportune time to engage in a broader conversation about the costs and benefits of applying psychological theories to better understand the behavior of legal actors and to discuss directions for future research on psychological approaches to the law, which is the purpose of this symposium. Larry Baum begins with a consideration of cognition and judicial choice that speaks to the value of viewing judicial behavior from a cognitive, rather than motivational perspective. Next, Eileen Braman addresses decision making in multi-dimensional cases, including the theoretical and methodological leverage psychological approaches provide to the analysis of the complexity of decision making. Following this, Wendy Martinek focuses on the psychology of small group decision making, both in terms of group cognition and group members' identities. Brandon Bartels wraps things up by offering his insights into the cognitive processes of judging, in addition to addressing the challenges of applying psychological approaches for understanding, explaining, and empirically analyzing judicial decision making.

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Cognition and Judicial Behavior

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The study of judicial behavior by political scientists and legal scholars has taken many theoretical perspectives and methodological forms, but it has been unified by a focus on judges' motivations. The long-standing debate about the relative importance of legal and policy considerations in judges' choices has been framed primarily in terms of what judges want to accomplish. The growing importance of strategic conceptions of judging rests on an implicit assumption that judges care about securing the adoption of their preferred policies rather than about expressing those preferences directly.

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There is no doubt about the value of a motivational focus for an understanding of judicial behavior. Analyses with this focus have taught us a great deal about the bases for judicial decisions, and the impressive contributions of the current wave of research in the field make clear how much can be learned by thinking rigorously about what judges want to accomplish.

But the dominant motivational approach also has significant limitations. Most broadly, it tends to foster a conception of decision making as a direct path from goals to choices. The actual process of decision making is probed only in limited ways, even though that process shapes and complicates the linkage between motives and outcomes. Similarly, the potentially powerful role of emotions in shaping choices is left aside. Thus there is value in seeking alternative perspectives from which to analyze decision making.

One alternative to analytic approaches that focus on motivation is the study of judges' personalities. Political psychologists have used personality-based approaches to probe the behavior of political elites, and their theoretical and methodological approaches could be applied to judges. Indeed, Harold Lasswell (1948) employed a psychoanalytic approach to gain an understanding of the behavior of trial judges, and Harry Hirsch (1981) did so in his study of Justice Felix Frankfurter. Other studies have made use of personality-based approaches, probing issues such as the impact of self-esteem on judging (Atkins, Alpert, and Ziller 1980; Gibson 1981).

Even more promising, because of the breadth of the issues they can address, are cognitive approaches. These approaches, which are central to the study of judgment and decision making in social psychology, hone in on the decision-making process itself. Motivation comes into play as one factor that shapes this process. Cognitive approaches direct attention to the difficulty of working from goals to decisions, they take into account the roles of emotions in decision making, and they facilitate consideration of the deeper motives that underlie proximate goals such as the judicial goals of making good law and good policy.

Students of judicial behavior have given attention to cognition as an element of the decision making process. Along with the research discussed later in this essay, several works merit note. C. K. Rowland and Robert Carp (1996, Ch. 7) laid out a broad cognitive approach to judicial decision making. Psychologists have probed integrative complexity as an element of Supreme Court decision making (Tetlock, Bernzweig, and Gallant 1985; Gruenfeld 1995; Gruenfeld and Preston 2000). Ryan Owens and Justin Wedeking (2011, 2012) have further explored the effects of cognitive complexity (as well as cognitive consistency) on decision making by Supreme Court justices.

Perhaps the most important way in which attention to psychological theory can enhance our understanding of judging is by underlining the potential for variation in the ways that judges behave. Research on motivation and on personality points to interpersonal attributes that can lead to different patterns of judicial behavior across individuals. Rather than treating all judges as acting on the same considerations, we can take into account differences in needs and motives that lead them to approach the same cases differently. Even on the Supreme Court, whose members have considerable similarity in background and training, different justices may approach their work quite in different ways.

Cognitive psychology can also help in understanding interpersonal differences, but it is especially valuable in identifying relevant differences among situations that affect judicial behavior. Three strands of theory illustrate this contribution.

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The first is the concept of motivated reasoning or motivated cognition. In the leading formulation, by Ziva Kunda (1990), people's reasoning to reach a conclusion about some matter is affected by the motive "to arrive at an accurate conclusion" and the motive "to arrive at a particular, directional conclusion" (480). Kunda emphasized the capacity of people to reach the conclusion they favored, based on their directional goals, but she also emphasized that this capacity has limits: "The biasing role of goals is thus constrained by one's ability to construct a justification for the desired conclusion: People will come to believe what they want to believe only to the extent that reason permits" (483).

Kunda's formulation seems to fit the debate over the weight of legal and policy considerations in judicial decisions quite well. Getting the law right can be considered judges' accuracy goal. Advancing their policy preferences can be considered a directional goal—at least under some circumstances, the dominant directional goal. Some students of the Supreme Court adopted a position that paralleled Kunda's formulation long before she wrote, arguing that the ambiguity of the law in the cases that the Court hears frees the justices to reach positions consistent with their policy preferences (Rohde and Spaeth 1976, 72-74). Several scholars have explicitly linked motivated reasoning to judicial decision making (Baum 1997, 65-66, 82; Segal and Spaeth 2002, 433; Wrightsman 2006, 119-20, 174-75; Kahhan 2011, 19-27). Eileen Braman (2009; Braman and Nelson 2007) has made the most extensive contribution with her theoretical analysis and experimental research on motivated reasoning in legal decision making.

The use of motivated reasoning to explain perceived limits on the role of law in Supreme Court decision making is quite appropriate, but the concept may be even more useful in explaining variation in the balance between law and policy. As many scholars have noted, legal considerations play a larger role in courts that lack discretionary jurisdiction, because those courts hear so many "easy" cases in which only one conclusion is easily justified under existing legal principles.

Even within a single court, cases differ in ways that relate to the process of motivated reasoning. If all the cases that the Supreme Court hears on the merits are relatively "hard," they still differ in the extent to which the law constraints the justices. More important is a second difference that relates to a less-noticed implication of the theory of motivated reasoning: the justices care about the outcome of some cases considerably more than they do about others, and where their directional goals are relatively weak, their accuracy goal (and other relevant goals) can have a greater impact. One possible partial explanation for the high percentage of unanimous decisions in the Court is that there are some cases in which the justices' reading of the law and their interest in achieving consensus (and in limiting their own workloads) outweigh their interest in the policy issues that arise in the case.

A second strand concerns what are called dual-process theories of decision making (Chaiken and Trope 1999), including the elaboration likelihood model (Petty and Cacioppo 1981) and the MODE model of linkages between attitudes and behavior (Fazio 1999). These theories rest on the assumption that people reach decisions in different ways under different conditions.

One example is the heuristic-systematic model that Shelly Chaiken and her collaborators have developed (Chaiken, Liberman, and Eagly 1989; Chaiken, Giner-Sorolla, and Chen 1996). This model distinguishes between a heuristic mode of information processing that is "relatively effortless" and "characterized by the application of simple decision rules" and a systematic mode that is "more effortful and analytic" (Chaiken, Giner-Sorolla, and Chen 1996, 553). People engage in systematic processing when that mode seems necessary to meet their needs; otherwise, they take the easier heuristic route.

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As applied to the courts, dual-process theories raise the possibility that judges take quite different routes to decide different kinds of cases. Such a dichotomy may seem unlikely, but courts sometimes divide cases systematically into one group that they gave greater attention than another. It is now common in intermediate appellate courts for staff members to assign cases to one category in which it appears that a decision will be easy to reach and another in which decisions seems likely to be more difficult, and judges then give less attention to the cases in the easy category (Symposium 2002). The U.S. Supreme Court and at least one state supreme court (California) divide petitions for hearings into two categories, those that will be discussed collectively by the justices and those that will be denied without discussion, based primarily on recommendations by law clerks or staff members.

With exceptions such as these, courts do not formally differentiate between classes of cases in the decision-making process. Still, it seems almost inevitable that judges analyze some cases more closely than others, whether that difference takes the form of a dichotomy or something more complex. That possibility merits more attention in both our theories of decision making and empirical analyses.

A final strand of theory concerns the cognitive limitations of decision makers. An array of scholars in psychology and other disciplines have done research documenting these limitations and their impact on people's choices. Most prominent is the work of Daniel Kahneman, Amos Tversky, and their collaborators (Kahneman, Slovic, and Tversky 1982; Gilovich, Griffin, and Kahneman 2002). This research has extended into economics, leading to the development of behavioral economics as a field (Thaler 1991). Concepts from this scholarship, especially the role of heuristics in decision making, are widely known and used.

Research on cognitive limitations presents a challenge to models of rational decision making. The scholarship in this area indicates that individuals can have great difficulty in identifying the choices that will best advance their goals. It also indicates that even collective groups (such as investors) generally do not converge toward the best decisions, because errors in decision making are not distributed randomly around the choice that maximizes utility. Recognizing these complications, political scientists have participated in the effort to develop the implications of cognitive limitations for rational theories of behavior (Bendor 2010).

Judges certainly are subject to the same kinds of cognitive limitations that beset other people (Guthrie, Rachlinski, and Wistrich 2001). The scholarship on cognitive limitations is relevant to all of the motivation-based theories of judicial decision making (see Segal 1986), but it is most directly applicable to strategic models that draw from rational choice approaches. Strategic accounts tend to assume that judges can identify the choices that maximize achievement of their policy goals. That assumption is not fully justified for any form or arena of judicial strategy, and for some it seems heroic. The Supreme Court justice who seeks to determine the ultimate impact of a prospective decision on the incidence of crime or the state of the health care system would not seem to be standing on very firm ground.

This does not mean that strategic behavior by judges is futile. It does suggest that judges' strategic capabilities differ across situations. For Supreme Court justices, strategy aimed at achieving a favorable collective decision in the Court seems far easier to carry out than strategy that requires taking into account the behavior of people and institutions outside the judiciary. The justices come to know each other's preferences and behavior well, so they are in a relatively good position to identify appropriate strategic choices within the Court. Prediction of how members of Congress or state officials will respond to a decision is considerably more difficult. And judges may err systematically in their strategic choices. For instance, risk aversion may cause some Supreme Court justices to overestimate threats to the Court's legitimacy and thus the need to adjust their decisions to the state of public opinion. It may be as well that

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justices eschew some forms of strategy altogether because they understand the difficulty of making good strategic choices in those contexts. (For long-term strategy, the tendency to discount the future may also come into play.)

Each of these strands highlights the potential for variation in the ways that judges reach decisions, based on their perceptions of cases. Although none of the major theoretical approaches in the judicial behavior field incorporate that potential, several scholars have probed the possibility that Supreme Court justices respond to cases differentially based on relevant attributes, especially their perceived salience or importance, and they have provided significant evidence that these attributes make a difference (Unah and Hancock 2006; McAtee and McGuire 2007; Bartels 2011; see Bartels 2010). If that is true within the select sets of cases that the Court accepts, it seems even more likely to be true in cases that hear a more heterogeneous range of cases.

Such variation is only one of the implications and lessons that can be drawn from scholarship in cognitive psychology. Of course, there is a need for caution in importing theory from a quite different context. In particular, research based primarily on experimental research with ordinary people (weighted heavily toward college students) does not automatically apply to decisions by judges that have real consequences. But I think that research on cognition firmly establishes the need to take cognitive processes into account. Doing so is perhaps the single most useful step that we can now take toward gaining a richer sense of judicial behavior.

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Embracing Complexity in Law and Courts: A Psychological Approach

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Legal decision-making is complex decision making. First, there are esoteric rules jurists are trained to follow to promote fairness, objectivity and predictability in the rule of law. Whether or not judicial scholars actually believe such norms typically constrain decision makers, we must deal with the fact that those rules structure the way lawyers argue cases and how judges justify their decisions to external audiences, other judges, and (perhaps even) themselves. Second, most cases do not, as many psychological and strategic studies assume, involve one single dimension. Cases arise from messy real-world circumstances and advocates are trained to present all arguments that could conceivably help their client. As a result, litigation often involves multiple issues where competing values vie for recognition in ways that are not always ideologically consistent or strategically coherent.

Legal decision makers are also complex. As Lawrence Baum's contribution here reminds us, judges often have competing goals and motivations acting upon them when making decisions. These may be triggered by internally experienced identities or interested external audiences (Baum 2006). Moreover, one thing that psychological (and judicial) research tells us, is that individuals are notoriously ill-equipped to recognize, let alone accurately report, influences in their own cognitive decision processes; so alternative techniques must often be utilized to discover the role of factors that judges are either unable, or unwilling, to admit to in their decisions. This is part of the brilliance of introducing behavioral techniques to study judicial decisions (Pritchett 1948; Schubert 1965). But such methods are often accompanied by simplifying assumptions that students of judicial behavior have, perhaps, become too comfortable with. Judges are commonly represented as single-minded seekers of policy and cases as unambiguous avenues in which to achieve those ends. Neither of these assertions has much basis in the realm of real world complexity.

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It is time for those interested in studying legal decision-making to embrace the complexity inherent in the endeavor. As I look at topics that are of increasing interest to law and courts scholars, it appears we are chomping at the bit to do so; indeed, one could argue that complexity represents the ‘Next Frontier’ in understandings of judicial decision making phenomena. Here I argue that psychological approaches should remain highly relevant in understandings of law and courts phenomena because they give us theoretical and methodological purchase on addressing complexity in decision-making.

Complexity Represents the ‘Next Frontier’ in Understandings of Judicial Behavior

Although most scholars have their own views about what predominates in the minds of judges, no one believes that judicial decision-making is exclusively driven by legal criteria, or attitudes, or strategy without being tempered, to at least some degree, by other considerations. Once we admit to such nuance – that judges may want to achieve both legal accuracy and policy goals – or that they care about institutional legitimacy as well as outcomes they have been strategically working towards through a line of doctrine – context becomes extremely important. As Brandon Bartels (2011) has effectively argued, given this more nuanced understanding of judicial behavior, the question is less about which influence dominates in the minds of decision makers, and more about “the conditions under which” each might be influential in shaping decisions. Recognizing that judges have multiple motivations, such that different types of considerations may be triggered under different conditions, is an important step to embracing complexity in our research.

Although judicial scholars have been pretty good about looking at the role of context plays in varying the influence of legal, policy, and strategic considerations across distinct levels of the judicial system (Segal and Spaeth 1993; Hettinger et al. 2006; Rowland and Carp 1996), we have been less inclined to consider the role of context plays for triggering alternative considerations across judges on similar courts. Studies that interact case and judge characteristics come closest to discovering whether there is something “different” about how women or/and minorities, for instance, decide cases in particular issue areas (e.g. Songer et al. 1994). But observational hypotheses are necessarily vague where data involves dichotomous case votes, often amounting to predictions that certain types of judges will be more (or less) ideological in particular issue areas. Even where “differences” between judges are discovered they cannot tell much about is going on in the minds of decision makers to produce such results. Still, these studies embrace the sort of complexity that is the subject of this essay because they posit that different case stimuli call to mind distinct considerations in the minds of judges in unique, but predictable, ways.

Besides recognizing that the balance of legal, attitudinal, and strategic considerations can vary depending on the context, students of legal decision-making should also acknowledge that cases are often more complex than we assume. Litigation commonly involves more than one legal question. Procedural issues, for instance can raise a host of “judicial values” that ordinary citizens might not think a great deal about. These technical questions can implicate judges’ feelings about things like court access, fair notice, and concerns about the adequacy of evidence in litigation. Such institution-specific values may not fit neatly across any ideological spectrum, but they can be important in how judges think about “threshold issues” that accompany substantive disputes in complex cases.

Moreover, cases can raise multiple substantive legal questions. Consider the recent case where the Affordable Health Care Act was justified by the Justice Department using Congress’ commerce power and its taxing authority. The case provides an especially interesting example of complexity in judicial behavior where Court watchers have argued that context, particularly Roberts’ role as Chief Justice, elevated institutional considerations over policy concerns in his reasoning, resulting in the rather surprising choice of determinative grounds to uphold a statute that he personally did not support. Prior statements,

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and perhaps Robert's own desire for consistency, would have made it difficult to achieve this goal using commerce clause doctrine. The taxation argument provided an alternative ground on which to base his decision; although some see the logic as strained, by using the mandate-as-tax saving construction Roberts was able to craft a legal opinion justifying his vote to uphold the Health Care Act. One cannot fully appreciate the mandate-as-tax construction adopted by Chief Justice Roberts by simply looking at the justices' votes along any single dimension in the case. The *combination* of arguments in complex cases structures the decision choice providing obstacles and opportunities to decision makers to achieve alternative goals. For scholars interested in the *process* of decision-making, understanding how decision makers navigate alternative grounds can be just as important as understanding how they vote. Obviously this is one rather idiosyncratic example, and we can't definitively know what was going through the mind of Justice Roberts at a distance. But it does suggest how context can impact goals and how complexity can structure choice in a particularly salient instance.

Even cases involving a single legal issue can raise competing dimensions for judges. The sub-field's growing interest federalism judgments (Pickerill and Clayton 2004; Solberg and Lindquist 2006; Collins 2007; Parker 2012) provides an excellent example of a decisional field where this is often the case. Specifically, federalism cases involve a "states rights" and a "substantive policy" dimension that are potentially in conflict with respect to whether a particular government program should be upheld. Studies based on observational data commonly investigate which dimension is 'driving' judicial case votes in this area. Sometimes the question is raised as whether judges are pursuing and principled states rights agenda or using federalism doctrine to achieve more immediate policy goals. Although there has been some excellent statistical work on this interesting question (Parker 2011) – yielding as one might predict, complex results about decisional behavior across justices – it is an area that could particularly benefit from theoretical insights in the field of psychology and the control afforded by experimental techniques.

Psychology gives us Theoretical and Methodological Purchase on Complexity

Scholars have written at length about the usefulness of psychological theory and methods to address questions of interests to law and courts scholars (See generally, Klein and Mitchell 2010). Here I highlight three areas that seem particularly relevant to understanding how decision makers deal with complexity that all fall under the general rubric of "Value Pluralism:" research on value conflict, the negotiation of competing identities, and accountability cross pressures.

Cases that involve multiple issues or competing dimensions involve value conflict because decision makers must often choose one determinative issue or avenue to pursue over available alternatives. Abelson (1968) wrote about strategies decision makers may use to deal with such conflict. Moreover there is a large body of research on cognitive consistency and strategies decisions makers will use to avoid uncomfortable states of "cognitive dissonance" resulting from the acknowledgement of conflicting beliefs (Festinger 1957). In the federalism context such insights may help us to think more carefully about how decision makers experience conflict, whether they subjectively acknowledge or deny tension across states rights and policy dimensions. It may also help us to understand *why* some decision makers chose to bolster states rights considerations over more immediate policy concerns while others do the opposite. Are differences driven by individual differences across judges (such as their level of commitment to federalism ideals) or more contextual factors (like the policy area at issue in a given case)?

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Another interesting question psychological literature can help us consider is how individual decision makers think about themselves in the context of competing identities. All judges are legal professionals, and have some conception of their appropriate “judicial role.” But judges are also citizens who might strongly identify with political parties who played a part in their ascension to the bench. They also have subgroup and relational identities that can influence the balance of goals and considerations relevant to their decisional behavior. Psychological literature indicates that our thinking about ourselves is somewhat fluid depending on aspects of the environment that make one identity particularly salient over others (Brewer 2001). While all judges have some choric awareness of their “judicial role” when adjudicating cases, there could be contextual triggers (case facts or arguments) that change the balance of goals and/or considerations for distinct types of judges because they make alternative identities salient. Understanding the literature on strategies for negotiating identity conflict (Tajfel and Turner 1985) should be just as important as identifying those triggers for predicting when those situations are likely to result in observable effects and what those effects might be.

Related to thinking about competing identities is literature on “accountability cross pressures” judges may feel in crafting decisions to appease different external audiences. Green, Visser and Tetlock (2000) have written about the psychological pushes and pulls that can act on individuals in complex decisional environments where observers want different outcomes. As with the other literature I mention, they identify specific strategies for dealing with such conflict that may help us theorize about when judges may choose to court one constituency at the expense of another – or try to take a more balanced approach that appears to take all relevant concerns into account. Complex cases with more than one issue may provide a special opportunity for judges to take a “mixed strategy” with competing audiences.

Methodologically, experiments should be part of our analytical tool kit in exploring how legal decision makers deal with complexity. Obviously there are problems in studying judges using experimental techniques and generalizing from other available populations (Braman 2009). But such problems are not totally insurmountable (Guthrie, Rachlinski and Wistrich 2001). Moreover, even where proxies are utilized, the benefit of using experimental methods for uncovering cognitive phenomena can outweigh the costs. They are uniquely effective at isolating aspects of context that are so important in understanding “the conditions under which” alternative goals and considerations will be more (or less) important in the minds of decision makers. Moreover, experiments are especially well suited to test and explore complex interactions between variables that can reveal different effects across situations or different types of decision makers.

Going back to the federalism context, one could write a legal brief with identical authority and arguments in two conditions involving a similar policy question in the context of states’ rights. In one condition the policy could have a liberal effect, in another a conservative effect. Such a design would allow us to see how decision makers treat the same federalism authority where the policy effect is consistent with their attitudes AND where it is not. Thus, we would be able to see if decision makers faithfully apply states rights principles in both circumstances or if the ideological content of the policy influences their decision-making under alternative conditions. If decision makers are more likely to uphold policies they agreed with and less likely to do so where they disagreed, notwithstanding the fact that the legal authority and arguments in both conditions was identical, it would be good evidence that they were using federalism doctrine to achieve more immediate policy goals. If they act consistently in alternate conditions, it would be evidence of a more principled approach to federalism doctrine.

Of course, differences across judges uncovered in previous research (Parker e.g. 2012) suggests decision makers may use different tactics in federalism cases; the proposed design can also give us leverage to explain *why this is the case*. It makes sense to think decision makers may “bolster” (Abelson

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1968) the issue dimension over the states' rights concerns in litigation involving policy areas they especially care about. By measuring the how important the policy issue is to participants, we could see if there is an interaction between personal salience and the likelihood of elevating policy concerns over states' rights principles when the two are in conflict. One might test a similar hypothesis using existing observational measures of issue salience – like whether a case was mentioned on the front page of the *New York Times* – but such measures assume public attentiveness works in the same way to make issues salient across all decision makers. This may or may not be the case in our complex world where issues raised in litigation can be important to individual decision makers for a variety of different reasons.

Experiments give us an additional avenue to investigate complexity where we can be confident about causal inferences. Psychological theory points to the variables and interactions we should be considering in our inquiries about how different judges navigate complicated legal terrain. Of course none of this is easy, in exploring complexity researchers will need to be very careful about identifying contexts where different goals or considerations may be activated in the minds of decision makers and specifying the observable implications of distinct strategies or considerations in their hypotheses. But I am confident judicial scholars are up to this worthwhile task.

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Appellate Courts as Small Groups

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Virtually all appellate courts are multi-member courts. Whether they rely on *en banc* approach in which the full set of judges on the court participates simultaneously in rendering a decision (e.g., the U.S. Supreme Court) or use a panel mechanism in which some subset of the judges renders a decision (e.g.,

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the U.S. Supreme Court) or use a panel mechanism in which some subset of the judges renders a decision (e.g., the South African Appellate Division), appellate courts produce decisions that are collective rather than individual in nature. In normative terms, the motivation for using groups of judges for appellate court decision making is to enhance the quality of the adjudicative process and maximize the probability of the appellate court arriving at the correct decision (Kornhauser and Sager 1986: 97-100). From an empirical perspective, students of judicial behavior have (implicitly if not always explicitly) considered the collegial nature of appellate court decision making. Even when focusing on the individual preferences of a judge as the animating force of judicial behavior (as in the attitudinal model), scholars have recognized that the formal force and effect of any appellate decision lies in its identity as a decision of the court, not of an individual judge on that court. As Kornhauser and Sager (1993) observe, “Appellate judges, almost always sitting in panels of three or more, are expected to behave as colleagues. They are expected to adjust their formal and informal behavior as judges to the fact that they have power to adjudicate cases not individually, but only collectively as a court” (1-2). Scholars who have adopted the strategic model of judicial decision making have, of course, been very explicit in taking into account the collegial nature of appellate court decision making.

Rooted in neo-classical economic theory, the appeal of the strategic account lies both in its parsimony and in the impressive body of evidence that has been amassed in support of the notion that appellate court judges are forward-thinking individuals who pay attention to the interdependent nature of their decisions. Though in real terms this scholarship does take into account the small group context of appellate court decision making, it does so with a singular focus on policy goals and a narrow understanding of the collegial court context as a venue for strategic calculations. Policy surely does matter to judges and collegial courts surely do offer the opportunity for strategic decision making. But judges have multiple goals they wish to achieve (Baum 2006) and collegial courts are settings in which more than purely strategic interactions transpire (Bowie and Songer 2009). Paying greater attention to the nature of small group decision making, and its non-strategic dimensions, offers unique opportunities to extend our understanding of decision making on collegial courts. The argument is not that attitudes or strategy (or the law, for that matter) are unimportant. Rather, the point is that we can advance our understanding of appellate court decision making even further by considering the psychology of small groups.

Scholarship on small groups can be found in a wide range of disciplines, including psychology, sociology, public policy, communications, social work, anthropology, education, information science, political science, urban planning, management, and organizational behavior.¹ Theories of small groups have been applied to subjects as diverse as workforce efficiency, criminal reentry, leadership development, political socialization, alternative dispute resolution, organizational demography, adolescent risk-taking behavior, and emergency response. Though precisely what is considered to constitute a small group varies across disciplines and even across researchers within the same discipline, the definition offered by Levine and Moreland is useful because it incorporates four elements that are most common across disciplines: A small group is a group of individuals who “interact on a regular basis, have affective ties with one another, share a common frame of reference, and are behaviorally interdependent” (1994: 3).² Appellate courts evidence each of these characteristics.

¹ The edited collection by Poole and Hollingshead (2005) is a particularly nice (and relatively recent) volume that illuminates the various disciplinary perspectives on small groups and offers some suggestions as to how to better integrate theories of small groups across disciplines.

² Much of the early work on small groups included face-to-face interactions among individuals as a prerequisite for identifying a collection of individuals a small group (e.g., Verba 1961). “Given the growth of virtual communication, however, one might question whether face-to-face interaction is essential for the existence of a small group” (Harrington and Fine 2000: 313). I find myself among those who eschew face-to-face interaction as a necessary condition for a small group to be said to exist.

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First, some appellate court judges may interact with one another more often than others but all the judges on a given court have some regular interaction. For those on courts with discretionary dockets, they must interact to make decisions as to which cases are to be granted review. They may also interact in conferences to make preliminary decisions as to case dispositions and opinion assignments. In addition, they must regularly come together for oral arguments. Further, the judges interact regularly (though perhaps only virtually through electronic or other media) in the drafting and circulation of opinions. Second, both anecdotal (e.g., Cooper 1995) and systematic (e.g., Cohen 2002) evidence demonstrates that the relationships between and among appellate court judges are not limited to dispassionate professional associations. The affective component may be positive (think about the relationship between Justices Brennan and Marshall) or negative (think about the relationship between Justices Burger and Blackmun). But, even when personal affect is absent, the judges share affective ties for the court on which they serve. Third, the institutional environment of any given appellate court provides a common frame of reference for the judges who populate that court. Most obviously, both the informal norms and the formal rules create a shared frame of reference for the judges of a court. Moreover, the common body of law the judges on any particular appellate court are charged with interpreting contributes to that shared frame of reference. And, fourth, notwithstanding variations in status, experience, expertise, or any other source of influence, no single member of an appellate court can determine the outcome of a case or the legal rule articulated. Rather, the judges (at least a majority) must coordinate as to both outcome and legal rule and, hence, they are definitionally interdependent in terms of their behavior.

One line of small group research that is promising for its potential to inform our understanding of appellate court decision making centers on group cognition. In its broadest sense, cognition encompasses the complete set of conscious and unconscious reasoning processes in the mind. More specifically, “[c]ognition refers to what we know, how we come to know it, and how we use what we know to judge or act” (Lepgold and Lamborn 2001: 1). Group cognition (i.e., social cognition) arises in settings in which the interactions between and among group members result in the sharing of individual cognitive frameworks with the consequence that both the individual’s cognitive understanding and the group’s shared cognitive understanding are affected. In fact, the interaction can change not only the content but also the structure of both individual and group cognition. Group cognition, then, is more than the sum of its parts (Allard-Poesi 1998). The social exchanges that transpire among group members “produce shared cognitive products, including memories, norms, scripts, schemas, and interpretations of shared events and activities” (Gruenfeld and Hollingshead 1993: 384).

Shared mental models (i.e., shared understandings among group members) represent one way in which group cognition can be more than the sum of the individual cognitions of group members. A group with a shared mental model have a common understanding of the task before it, the members of the group, and the tools and processes by which the task is to be accomplished. In the context of appellate court judging, shared mental models may lead members of an appellate court to rely more heavily on conventional legal factors in adjudicating a case. This assumes, of course, that part of those shared mental models emphasize the reliance on legal factors. This is probably not a terribly heroic assumption to make given that, from the beginning of their legal education and throughout their socialization into the legal world, judges are inculcated with the norm that judicial decision making should be governed by legal factors. Also implicit in the notion of social exchanges yielding shared cognitive understandings is the idea that reliance on legal versus non-legal factors may become self-reinforcing as a set of judges continues to decide cases together. A natural question, then, is what is required for legal versus non-legal factors to drive group decision making at the outset. Is a single judge focusing on legal factors sufficient or is a critical mass of judges on the court necessary? Theories of group cognition can provide useful guidance.

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Another line of small group research that is appealing for the leverage it can afford us in understanding appellate court decision making focuses on organizational diversity.³ This area of research seeks to untangle the relationship between group composition and such things as group performance, group cohesion, and social interaction among group members. Also falling within the ambit of this line of research is the analysis of the effect of group composition on group members' individual performance, commitment to group goals, and satisfaction. There are multiple dimensions of diversity that this line of research has considered, including race, gender, educational background, values, and attitudes. Regardless of the dimension of diversity in question, one common question has been about the relationship of diversity to team reflexivity (and, ultimately, team performance). "Team reflexivity refers to the team's careful consideration and discussion of its functioning and is proposed to result in team learning and improved team performance" [citations omitted] (Van Knippenberg and Schippers 2007: 527). The argument for diversifying a group (i.e., including more people with different backgrounds and experiences) often rests directly on the notion that it will lead to different perspectives being brought to bear in the deliberations of that group (and perhaps different outcomes). This has natural applications to appellate court decision making and, indeed, law and courts scholars have been sensitive to how the composition of an appellate court panel in terms of race (e.g. Farhang and Wawro 2004), gender (e.g., Boyd, Epstein, and Martin 2010), ideology (e.g., Sunstein 2003) may affect the decisions made. What the extant body of small group research has to offer in this context is insight into why we observe the differences that we see, insights grounded in theories of human behavior that are explicit in their consideration of the immediate social context in which that behavior takes place.

A small group perspective can also be helpful in directing attention to a different kind of group composition; specifically, to how the roles and identities of various members of an appellate court bench might matter. Small group theorists have investigated the manner in which a group member's external status (based on, e.g., race, gender, educational background, prior experience) shapes other group members' perceptions as to his or her legitimacy (e.g., Ridgeway and Berger 1986; Ridgeway, Johnson, and Diekema 1994). Since those who occupy formal leadership roles on appellate courts (e.g., chief judges) have typically very few tools at their disposal to induce their colleague to comply with their wishes, legitimacy is a potent resource on which they can draw. Accordingly, understanding how and when an appellate court leader can exercise influence can be substantially improved by taking into account what small group theory scholars can tell us about leadership in groups. Likewise, expectation states theory can aid our understanding of how new judges acclimate to the bench. Expectation states theory focuses on the expectations that group members have for themselves and for other members of the group in terms of what they can contribute toward meeting the group's responsibilities and fulfilling its tasks (e.g., Berger, Wagner, and Zelditch 1985). Paying attention to how different characteristics (including race, gender, educational background, prior experience) matter for shaping the expectations of new judges for themselves as well as the expectations their colleagues have of them may be key for understanding the so-called freshman effect.

These examples are intended for illustrative purposes only and certainly do not constitute an exhaustive inventory of the kinds of questions about appellate court behavior that could be better understood with the application of a small group approach. To be clear, I am certainly not the first to suggest the utility of applying theories of small groups to appellate court decision making. Ulmer (1971), for example, was an early proponent and work by Walker (1973) illustrates the leverage that can be gained by taking this approach. Further, my purpose in encouraging scholars to draw from the small group

³ Knippenberg and Schippers (2007) offer an excellent review of the field of organizational diversity with a particular emphasis on diversity as a group characteristic.

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literature to enrich their theorizing about appellate court decision making is not intended to suggest that it can or should supplant other approaches. It is complementary in nature. Perhaps the best analogy is to behavioral economics. Behavioral economics modifies the unrealistic traits of human beings embedded in classic economic models (e.g., unbounded rationality) and takes into account the realities of human cognition (e.g., anchoring, loss aversion). It does not jettison the whole of classic economics but, instead, reworks it to more accurately reflect the human nature of human decision making. Thinking about appellate court decision making as a form of small group behavior (with more than just strategic dimensions) can likewise more accurately reflect the human nature of judicial decision making.

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Psychological Approaches to Judicial Behavior: Opportunities and Challenges

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Adaptations of psychological theories to the political science study of judicial decision making offer numerous opportunities but also some serious challenges and constraints that researchers need to consider. In this essay, I consider these issues in the context of psychological research on the cognitive processes underlying judicial decision making, which borrows from theories in social psychology (e.g., Bartels 2010; Baum 1997, 2006; Braman 2009; Klein and Mitchell 2010; Rowland and Carp 1996; Wedeking 2010). Discussions of these issues have implications as to what possibilities exist for building an influential and enduring literature in psychology and judging.

Opportunities

The primary goal of applying psychological theories to judicial behavior is to provide a systematic understanding of the cognitive processes of judging. The ultimate value of this enterprise is gaining significant theoretical and empirical leverage on distinguishing the relative influence of law versus ideology and politics on judges' choices. A cognitive approach can achieve this goal in a more nuanced way than competing theories (e.g., the attitudinal model or strategic perspectives), with a focus on the conditions under which each consideration matters more or less. Outside of the explicitly cognitive approach, though alluding to such work, I have sought to achieve these goals (to varying extents) in prior work (Bartels 2009, 2011).

An aspect of the cognitive processes of decision making that I find most compelling is the extent of "biased processing" that occurs in decision making (Bartels 2010). First, what are judges' motivations in a given context? Are they motivated by policy considerations, legal considerations, a desire to get the "right answer" (accuracy), or accountability to external actors and audiences? While attitudinal and strategic approaches assume that judges are motivated by policy/ideological goals (e.g., Epstein and Knight 1998; Maltzman, Spriggs, and Wahlbeck 2000; Segal and Spaeth 2002), psychological approaches often focus on a multitude of goals that may be operative depending on the context in which an actor is deciding (e.g., Fazio and Towles-Schwen 1999; Fiske and Taylor 1991; Kunda 1990). Baum's (1997, 2006) multiple goals framework reflects this perspective, with a focus on the individual and contextual characteristics that shape judges' goals and motivations.

Next, depending on what motivation is operative, how do those motivations drive the reasoning process one undertakes? Adopting insights from social psychology, these reasoning processes can be placed on a spectrum ranging from top-down to bottom-up processing (Bartels 2010). This spectrum captures variation in the extent to which individuals' predispositions (e.g., policy preferences or ideology) completely bias how they process facts and information (pure top-down) versus individuals objectively scrutinizing facts and information and fully suppressing predispositional biases (pure bottom-up). In a mixed, or controlled, process, predispositions are activated, thus facilitating the capacity for biased reasoning, but one may be motivated to control, to an extent, an inclination to engage in biased processing due to some accuracy or accountability motivation. A top-down process can be thought of as roughly the cognitive analogue of the attitudinal model, whereas a bottom-up model is consistent with a strong legal model. New and more nuanced explanations can be gained by further probing these "controlled" reasoning processes. As mentioned, these types of approaches are capable of providing enormous value to the study of judicial decision making and understanding the processes by which and conditions under which law and ideology influence judges' choices. Moreover, such approaches are capable of producing new knowledge and inferences distinct from insights produced by the "big three" models in the literature

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(attitudinal, legal, and strategic perspectives).

Challenges and Constraints

While there are benefits of a psychological approach for theory building purposes, a significant challenge centers on fully and adequately testing these theories using the types of observational data that judicial decision making analysts typically rely on. More specifically, we can test the theory's empirical implications but we cannot test the mechanisms underlying the theory, which are crucial to defining and distinguishing cognitive approaches. Of course, experimental research is ideal for testing the types of cognitive mechanisms underlying decision making and drawing causal inferences in general. Experimental research dominates social psychological work that is sometimes referenced and adopted by judicial politics scholars. There are obvious obstacles to implementing experimental design into our empirical study of judging, but they are not insurmountable, as experimental work by Braman (2009) and Guthrie, Rachlinski, and Wistrich (2001) can attest.

The broader question is: To what extent should we employ psychological frameworks if we cannot adequately draw empirical inferences about psychological mechanisms? The most significant problem with using observational data to test predictions (but not mechanisms) from a psychologically-based theory is behavioral equivalence. That is, we may generate predictions from a psychologically-based theory that have some degree of overlap with, say, predictions that could also be derived from a strategic approach, yet the mechanisms underlying those predictions are theorized to be quite different (e.g., a strategic approach may suggest a policy-motivated rationale for a certain prediction, while a psychological approach suggests an accuracy or accountability rationale).

Once again, I do not think this obstacle is insurmountable. One response is that we need to be systematic and transparent about discussing alternative theories that could have produced the same predictions, and work to make clear distinctions about what we would expect from each theory, given the assumptions and posited reasoning processes. Another response is to generate as many empirical implications as possible for competing theories (e.g., King, Keohane, and Verba 1994), which serves (at least) two purposes. First, the more empirical implications one produces, the better able one is to evaluate the theory more generally. Second, the more empirical implications we produce, the more likely we are to uncover competing empirical implications between competing theories, which can aid us in distinguishing mechanisms underlying the different theories. Related to the prior two points, I think it is within reason to think that, even with observational data, we can find ways to attribute empirical findings to a particular psychological mechanism and be able to rule out alternative theories that may generate the same predictions but via a different mechanism. Of course, we will still want to make clear the degree of uncertainty associated with such inferences.

To return to the gold standard for distinguishing mechanisms, I think the power of *survey experiments* could be wielded more prominently. Retired judges may make good "subjects;" they may be more prone to fill out a survey if asked, compared to active judges. They may also feel that they have less of a stake in projecting an image of pure legality and objectivity. Regardless of this issue, though, one could still design a survey experiment capable of drawing broad causal inferences and isolating mechanisms underlying decision making. Moreover, interviewing judges is a means of digging into mechanisms more systematically. Retired judges may make ideal interview subjects for reasons of "image projection" discussed above. Certainly, other work has made use of interviews, which are capable of enhancing the types of inferences made using observational data and even experimental design. Of course, multi-method studies employing observational data, interviews, and experimental design are ideal if they can be executed.

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Conclusion

One could safely say that the future is wide open with respect to psychological approaches to judicial decision making. The success of this literature for influencing how scholars think about judging turns on, first and foremost, strong theoretical development and specifically, making concepts and theories from social psychology directly germane to judging. It is fine for political scientists to adopt social psychological insights—just as rational choice theorists have adopted insights from economics—but it is crucial that we “make it our own” and transform those insights into the political science realm focusing on the political context of judging. As for empirical testing, we should fully confront the challenges and constraints—namely, the problem of empirically evaluating psychological mechanisms—that exist but remember that the obstacles we face are not insurmountable, as work employing experimental design and interviews already demonstrates. Moreover, even with observational data, careful research design and empirical analysis—e.g., producing as many empirical implications as possible and trying to draw out competing expectations—may facilitate the ability to distinguish mechanisms and thereby elude the behavioral equivalence problem.

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Symposium: New Directions in Comparative Public Law*

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Conducting fieldwork in South Sudan in the aftermath of what was Africa's longest civil war, one author of this article arrived at the office of a newly appointed Supreme Court justice. The desk was sparse. A new computer was switched off. And the court's docket was empty, with no case making its way through the state's nascent system following decades of political violence. If courts are effectively silent (and a new legislature only beginning to draft bills) where is state law in these places, at these moments?

Cognizant of the controversial politics of judicial review of immigration policies in the U.S., the other author went to France to study immigration in a civil-law system. While French appellate judges reviewed immigration-agency discretion, they hardly concerned themselves with the role of courts in an administrative state. Rather, judges sought to settle the legal meaning of foreigners' substantive rights, a subject left relatively untouched by their U.S. counterparts. If immigration law has taken such distinct trajectories in the U.S. and France, what are the broader implications for studying the judicialization of politics?

* We owe special thanks to Michael McCann for a thoughtful review of an earlier version of this article.

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Important questions emerge, then, for scholars of public law about how culturally significant norms, routines, and institutional relations inform legal practices. A research strategy that fully attends to the distinct settings in which the politics of law is enacted would require significant research time abroad and typically substantial foreign-language skills. But the investment comes with appreciable rewards: Tackling comparative legal politics in a sophisticated and richly contextualized manner has the potential to extend public law scholarship in original and exciting directions.

Despite the pioneering work of Shapiro (1981), only during the past decade has consideration of law and courts outside of the United States developed into a significant current within public law. Based on the important insight that courts and constitutional rights have come to play a meaningful and growing role in politics, studies of non-U.S. legal contexts have emphasized the broadly shared features of this global “judicialization” of politics. With an eye towards developing generalizable models of how judicialization takes place, public law scholars have examined judicial empowerment in Europe (Cichowski 2007), Latin America (Helmke and Rios-Figueuroa 2011), and East Asia (Ginsburg 2003). Innovative new work on authoritarian regimes has similarly brought attention to courts and the sources of judicial empowerment in these complex settings (Ghias 2010; Ginsburg and Moustafa 2008; Moustafa 2007).

These studies of the politics of courts are important in shedding light on institutional relationships within national government and the relative power of judiciaries. But judicialization is just the tip of the iceberg. Now is the time to deepen the comparative public law agenda to address broader questions about the inter-relationship of legal processes with the diverse ideas and practices that constitute social life. For those considering embarking on a comparative public law research project and who may be looking for ideas and encouragement, we suggest three directions for fruitful research in largely uncharted waters.

First, consider diversifying the focus from the *politics of courts* to the *politics of law*. In many parts of the global South, many people have little ability or need to take their grievances to state courts. Studying judicial empowerment in these places may disconnect scholars from the daily realities faced by those with real disputes. That is, there are varieties of sites where the politics of law is generated and its impact is mediated, including non-governmental organizations, refugee camps, and the home. And even in robust industrialized democracies with well-functioning legal systems, the politics of rights may be largely enacted outside the judicial realm. Yet the fact that politics occurs through other mechanisms than formal adjudication does not mean that the politics of law is absent. As studies of rights-based politics in the U.S. have shown, the symbolic power of rights may catalyze political struggles “even where the shadows of official legal institutions are dim or largely disregarded” (McCann 1994). Inductive and empirical studies that acknowledge individual and collective experiences of law will ultimately shape how scholars theorize its practice. It may also expose law’s multiple faces—promoting but also retarding the goals of the actors who use it for their benefit (Massoud 2013).

Second, pay attention to the *discursive and symbolic dimensions* of law. As studies of disputing in the U.S. have shown, culturally embedded norms, routines, and institutional relations inform the range of issues and legal arguments that are understood as suitable for litigation (Harrington and Merry 1988; Mather and Yngvesson 1980). And engagement with a particular juridical community’s shared discursive repertoire lies at the heart of the adjudication process (Feeley and Rubin 1998; Gillman 2006). What are the specific cultural forms, then, that configure law and rights in non-U.S. contexts? Applying legal anthropology may be helpful in this regard—to illuminate, for instance, how international ideas of rights translate into local practice or take local meaning (Merry 2006).

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Third, be mindful of the *dynamic, multidimensional, and uneven* nature of worldwide engagement with rights. Comparative public law scholarship has generally framed the “judicialization” of politics as uni-directional and uni-dimensional: At some cognizable point, courts enter the game of politics and change the rules by which it is played. Recent work has offered a sophisticated picture of the combination of ideas and institutions that lie behind this judicial engagement with rights (Woods and Hilbink 2009). Pushing further ahead, what would happen if we reframed the “rights revolution” not as an accomplishment but rather as an ongoing process in which legal change is frequently neither linear nor continuous? Are there systematic differences in the register and temporality of judicial engagements with rights across national contexts? “Constitutional ethnography” makes an important contribution in this direction—bringing an ethnographic sensibility to comparative public law by privileging the “lived detail of the politico-legal landscape” (Scheppelle 2004).

These three directions for research are, we hope, promising starting points for extending the frontier of comparative law-and-courts scholarship. Scholars in this section have demystified the process of litigation and how it transforms “facts” about the social world into new, culturally embedded formations of legal knowledge. But law is also a practice of politics, as the process of adjudication exerts a powerful influence over the orientations and actions of litigants. Lawyers and judges must adjust their strategies to the culturally meaningful register of the particular legal field in which they operate. In the U.S., the prominence of public interest law has been understood as a product of this dynamic, as legal professionals seek out symbolic judicial validation for their claims (McCann 1986; Trubek et al. 1994). Different strategies might be available to legal professionals who engage in reform-oriented litigation in different cultural settings (Kawar 2011). How are the contours of judicial engagements with rights shaped by these differential registers of adjudication?

Adopting a political and cultural approach to law also allows comparative scholars to understand both the power of rights and its consequences (see Couso, Huneeus, and Sieder 2010). On the one hand, framing politics in terms of rights reproduces the coercive power of the state. Rights have the discursive power to constitute the identities and grievances of political actors (Brigham 1996). On the other hand, rights are embedded in cultural meanings lending them symbolic power, which in specific contexts may catalyze fundamental rearrangements of politics. By illuminating what happens to rights and what happens to people who use notions of rights in a variety of settings, we can assess how law’s discursive power is enacted in diverse forms within national or subnational systems of cultural meaning (Massoud 2011).

Political scientists who care about law and courts are well suited to conducting this kind of examination into the politics of law. Many of us see law as embedded in—shaping and shaped by—political processes. We do not take legal rules on their face value, and we find rules existing in multiple sites. For example, in post-conflict areas where courts or regulatory frameworks are weak, a political science graduate student or scholar hoping to study law may be more suited to examining the legalistic operations within United Nations agencies than speaking with a group of freshly appointed Supreme Court justices who have yet to hear a case.

Examining the inter-relationship between cultural meaning and legal practice, both inside and outside the formal justice system, does require going beyond the conceptual categories of American law. This research agenda, then, would require an openness to rethinking what the fundamental questions may be in comparative public law. In studies that focus on adjudication outside the U.S. context, the global rights revolution cannot be fully appreciated if it is measured according to a rubric generated purely in the U.S. legal system and then exported to other national contexts. A more sensitive approach is required, in which the analytical framework encompasses the full range of juridical practices and actors that are present in another country’s diverse legal cultures.

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Our purpose here has been to put forward promising research trajectories that would build upon and enrich public law scholarship with insights drawn from local legal contexts outside the U.S. Our community of scholars has examined the wider politics produced by the deployment of juridical categories in much greater depth in the U.S. than anywhere else in the world. Studies in comparative public law of judicialization and constitutional rights have now called our attention to the global scope of legal politics. But we have only begun to scratch the surface of the full range of cultural meanings associated with the experience and practice of law.

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Law Here, There, and Everywhere: A Note on the Complexities of the Challenge

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It can be inspiring to hear the voices, insights, and commitments of a new generation of scholars. That is the case with the relatively short essay by Professors Kwar and Massoud. I thus urge all of my colleagues to welcome and take serious their challenging statement. My comments here aspire to be welcoming in all regards. I begin with a close reading that elaborates a bit on the multiple, often understated or implied substantial challenges that I identify in the article by Kwar and Massoud. My aim is to show how their proposals are more ambitious, complex, and most likely to be controversial than they suggest. I then reflect briefly on the implications of accepting these challenges, especially for scholars like myself whose research previously has focused on the US.

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The Challenges of Taking Off in New Directions

Kawar and Massoud point us in a variety of “new directions.” The essay begins with a seemingly simple invocation about “where” we focus our study, about the geographic dimension of new directions that they urge. Specifically, they urge Law & Courts scholars to devote more attention to looking “outside of” and “beyond” the United States. They challenge us to “deepen the comparative law agenda,” and they mention a number of times the goal of expanding our vision to the “global scope of legal politics.” Of course, while these various commitments are in some ways interrelated, they also are distinct. After all, one may study law beyond the U.S.—say, in Mexico, South Africa, Pakistan, or Germany—but do so in relatively insular, non-comparative manner, without systematic, theoretically informed comparative reference to similarities and differences relative to other settings. Since the demands and challenges of systematically comparative study are not elaborated much in the essay, I would urge that we take seriously comparative analysis as an analytical commitment. Comparison—whether in the form of large data sets, a small N of two to four comparative cases, or a single case study that engages comparative theorization and empirical research conducted by others—is essential to how we, with our limited capacities for understanding, attempt to make sense of things.

At the same time, comparative study requires us to scrutinize what we are comparing, i.e., the choices of units that we are analyzing comparatively. In particular, must we make the institutions or policies of different nation states as the most relevant or even only units for comparative study, as suggested by my examples above and is so common among political scientists? A long tradition of research had demonstrated that that what happens at the formal or official state level of law production does not determine or provide much insight into what happens on the ground in those societies. Indeed, the authors’ focus on the global dimension pushes us to recognize the dynamic interdependencies and complex convergence of forces that bind actors and bend institutional practices in different sites within as well as beyond state rule. For example, I have been teaching for some years a class in Rome comparing legal culture in the U.S. and Italy. It has become quite clear to me that Italy does not have a unitary, discrete, static legal system that can be compared easily to the U.S. legal system. Rather, Italian law is a dynamic, ever changing hybrid of codified Roman law, the Napoleonic civil code, transplanted elements of U.S. law (especially in the criminal system), European Law, and international commercial law and human rights conventions of many sorts. This intricate fusion of legal elements becomes even more complex when we shift from “law on the books” to actual legal practices in different geographic and institutional sites. For example, recent changes in the legal norms and practices of criminal justice processes—including incorporation of more American-style adversarial elements, which has only compounded the endemic problems of delay—diverge substantially from the processes and practices of civil disputing in Italy. At the same time, the administration of immigration law and the functioning of criminal courts are very different in parts of the northern regions relative to the southern regions. Indeed, in many nations today, law is an ever changing, often regionally variable mosaic of mixed traditions and evolving legal fusions that further contradict assumptions about coherent legal systems. And many nations have developed little in the way of unified legal system at all, either because of civil war, other deep historical or group divisions, or other disruptions. To study law in the contemporary age thus requires attentiveness to how the multiple traditions and forces of law around the world increasingly interact as well as to the local contexts where different legal, social, religious, political, and economic forces converge (or diverge) to shape or constrain law, often variably within as well as beyond nation states.

I suspect that these understandings inform the other key challenges identified by Kawar and Massoud about where and how we change directions in the study of law. They identify three types of changes,

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each of which invites a bit of elaboration. First they urge studies that are attentive to the “politics of law” and rights at work outside of official judicial institutions. This does not require ignoring courts so much, I think, as giving due attention to lower as well as high courts and “decentering” both, treating them as actual and potential actors or sites in socio-legal interactions that largely take place distant from courtrooms, judges, even lawyers. In this view, the authors seem to embrace the position that “law is all over” and “in” society, permeating social interaction at multiple levels and in complex ways. This focus would seem to suggest, in answer to my earlier query, that comparing official legal systems or institutions of nation states is an inadequate focus of comparative study. We need to study local contexts or “sites” of law in comparative fashion; such sites may be within a nation (e.g., workplaces, interactions with police in street or station, civil disputing among ordinary people, administrative offices, etc.), between states, or beyond the nation state at transnational level. And such projects again exacerbate all the traditional challenges of choosing what constitutes cases (of what?) for comparative study. It is worth pointing out that comparative study of civil disputing, of rights claiming, of professional legal practice, of social reform movements, of defining crimes, of apprehending and processing alleged “criminals,” and much more are rich legacies of comparative socio-legal study.

The second challenge is perhaps even more substantial. The authors urge rethinking law itself, attending not just to official actors, institutions, and legal rules but to the “discursive and symbolic dimensions of law,” which is to say to the variable, dynamic, and contested *meanings* of legal discourse and *knowledge* as practical activity, or legal practices.¹ This seems a sensible position for anyone who studies contexts that require different (non-English) languages and frameworks of understanding. After all, we may be interested in studying how different legal systems deal with the challenges of limiting and structuring legal discretion, but comparative study must attend to the fact that “discretion” can have different meanings and pose different dangers and promises in different social traditions and among differently situated legal subjects. The same could be said for the concept of “corruption” or judicial independence and its key features, each of which carries different meanings and connotations in different places. Even more vexing, attention to the elusiveness, contingency, and polyvalence of discursive meaning requires not only understanding differences in legal practice, but the challenge of using common analytical concepts for analyzing such different practices and sites of practice variously associated with the analytical category.

The authors recognize the previous points in their exhortation for scholarly sensitivity to the “dynamic, multidimensional, and uneven worldwide engagement with rights.” After all, rights may be increasingly a familiar and important part of public discourse in many parts of the world, but a great deal of study suggests not only that different rights are valued differently in various places, but that the very meaning of rights as a general concept as well as their various attendant constructions varies substantially. What the authors do not openly recognize, however, perhaps for strategic reasons, is that this focus highlights the value of interpretive or culturalist modes of study and perhaps complex process-based models of change while pointing away from linear causal analysis of discrete independent and dependent variables. Their framework also would seem to privilege small N or even single case studies informed by comparative theory and invocation of work by other on other contexts. This epistemological and analytical move is familiar among socio-legal scholars, many of whom are political scientists, but it is likely to stir more resistance from other scholars more wedded to positivist traditions of behavioral and institutional analysis. If more scholars shift toward comparative socio-legal analysis, as the authors urge, it is likely that epistemological issues will become more rather than less a matter of discussion and debate.

¹ I do not know if the authors exhortation to study meaning and practice is a shift away from behavior, a reconceptualization of what once was considered behavior, or a rejection of the concept. These variable positions are worth attention in considering this essay.

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In my view, open debate and puzzling about such challenges is good for our scholarly quests.

Professional Considerations for Law and Courts Scholarship

As a long time socio-legal scholar who reads, teaches, and researches increasingly about rights contestation and legal practices in varying contexts, I am generally supportive of the argument by Kavar and Massoud. But I have also attempted to show very briefly how there is a great deal at stake in their argument, and taking their exhortations seriously opens up a great number of difficult issues and potential controversies about how and what we study. Rather than try to answer all of these questions, I focus instead on one key challenge. In short, the type of research that they propose requires developing a great deal of knowledge about the contexts beyond the US that we want to study. To undertake sophisticated analysis of local meanings, practices, institutional relations, and the like would seem to require that the research develop extensive knowledge of language (often multiple languages and dialects), history, economic organization, politics, culture, and the like. As I noted earlier, this becomes increasingly complex and challenging in a global world where multiple traditions of law from distant sources converge in various sites of practice. Scholars focusing on colonial and post-colonial legal systems and practices have known this for a long time. It now is a fact of comparative socio-legal analysis in nearly every context. And hence the challenge: How can scholars like me, who traditionally have focused on the US, even consider undertaking such modes of study for which we are so thoroughly unprepared?

The simple answer is that we should just turn this project over to the younger generation that is preparing itself for such study and wish them well, offering whatever support we can. But we can do more, I think. I am a graying scholar who traditionally has studied U.S. legal institutions and rights-based practices by merging interdisciplinary socio-legal perspectives with distinctively political science understandings and analytical tools. While lacking extensive language skills and regional knowledge beyond the U.S., I have found a number of ways to become more comparative, more informed, more global. The **first** way has been through recruiting and working with graduate students from around the world. While I do not know a great deal about Turkey, Israel, Korea, or the Congo, I have learned a great deal working with students who are from or very familiar with these places. In fact, around 75% of dissertation supervisory committees on which I serve these days focus on politics and legal practice beyond the U.S. Many of these graduate students are international, but some are U.S.-based. One effect of this has been to sensitize me to the growing importance on pushing American English-speaking students to study languages and local context early in their graduate careers; this can extend graduate training, but it is critical to the types of research discussed here. And this in turn has pushed me to work much harder to help them find financial support for language study and immersion-based research abroad.

Other options exist as well, including for those many colleagues who have fewer opportunities to recruit and mentor great graduate students. This includes my **second** gambit of increasingly communicating and collaborating with scholars in various parts of the world, from Europe to the Global South, as well as with local U.S. scholars who are specialists in sites beyond the U.S. Collaboration is a promising way to overcome one's limitations. **Finally**, teaching about law and politics beyond the U.S. is another way to expand geographic knowledge and theoretical insights. Both my graduate and undergraduate courses now include a great deal of comparative method and non-U.S. materials. Moreover, I routinely teach a course on "Comparative Legal Culture" course abroad, *in Italy*, nearly every year as well as travel for conferences to places of interest several times a year.

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I still do not consider myself an expert on a regional context of legal policy and practice beyond the U.S. But I have become far more attentive to changes beyond the U.S. and to global interdependencies and complexities. Perhaps most important, this commitment to looking “outward and downward,” as Martin Shapiro once aptly put it, has pushed me to see law and legal practices in the U.S. in increasingly different and more critical ways. I am presently researching a book (with colleague George Lovell) on the legacy of U.S. colonial rule in the U.S. and over Philippine national subjects who migrated to the U.S. metropole over the twentieth century. This research has altered how I view the development of the American state, which is even more fundamentally bloody, imperial, and repressive than I had fully anticipated, and about how (not so) liberal rights “work,” for ill as well as good, among migrant workers over the last century. Viewing the development of the U.S. legal order through comparisons and contrasts with both European colonial legacies and post-colonial dealings with former subjects has been most enlightening. I began that research in the mid-1990s, and I dropped it for a dozen years to learn more about colonial, post-colonial, imperial, and neoliberal legacies around the world. It is the most demanding but also the most exciting and gratifying research I have conducted in my academic career. I heartily recommend taking our intellectual pursuits in new directions, outward and downward, and I thank Drs. Kwar and Massoud for inviting others to take such study seriously.

Institutional Constraints on the Comparative Study of Law and Courts

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The appearance of Kwar and Massoud’s précis of comparative public law developments is one of a number of signs of a major new movement shared by “public law” or “law and courts” political scientists and comparative politics specialists. It is in the very nature of scholarly disciplines and their academic departments to create and maintain boundaries and enforce scientific practices. That is why they are called disciplines. Almost inevitably then, new practices that breach traditional boundaries encounter resistance from disciplinary establishments, often unconscious or unintended or merely inertial. Young scholars, engaged in new practices, but depending on the establishment for hiring, promotion and tenure, must consider not only the life of the mind, as are Kwar and Massoud in their piece, but also where the next meal is coming from.

For a long time, although called “public law,” the subfield consisted almost exclusively of the study of the constitutional law decisions of the U.S. Supreme Court. As a result, at the undergraduate teaching level the subfield generated a yearlong course in constitutional law and little else. It followed that most political science departments had multiple specialists in most subfields but only one or two in “public” law.

This situation resulted in an anomalous public law recruitment situation. While American politics, comparative politics, international relations and public administration openings were filled by departmental search committees composed of a number of faculty all of whom typically were in the same subfield as the opening, public law slots were filled by committees that typically were composed almost entirely of specialists in American politics. Given that the opening was usually created by the departure of the department’s sole public law specialist, and the subfield itself concerned itself almost exclusively with matters American, such search committees were natural and almost inevitable. There were after all either

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no or only one public law specialist left in the department to be on the committee, and only Americanists were likely to be knowledgeable enough about American constitutional law, or the Supreme Court, to be on the committee.

Indeed over time in many departments public law was reduced from an independent subfield to a sub-subfield of American politics. While some job openings continued to be advertised in a separate public law category, many began to show up in the American politics listings in entries that read "The department seeks to fill an American politics opening and will consider specialists in Congress, The Presidency, parties and elections, judicial politics or bureaucratic politics."

The bottom line then is a situation in which any non-Americanist law and courts person is defined out of the search, or at least that the search is almost totally controlled by the department's Americanists who are naturally inclined to use the public law slot to augment department's strength in American politics. Indeed from the point of view of most search committee members, the best public law recruit is one who, besides good old con law, will also help out teaching the introductory American politics courses that demand so much of their teaching time.

So if we are now producing graduate students who basically are concerned with law and courts other than American constitutional law and the U.S. Supreme Court, what do we do with them?

One answer is to finesse the institutional problems. Law and courts graduate students, even if their specialized interests are elsewhere, ought to do enough about U.S. Supreme Court constitutional decisions, either by quantitative or non quantitative studies or both, to at least be able to claim competence in and enthusiasm for teaching the good old con law course. In addition, or alternatively, Ph.D. candidates basically interested in non-American courts, ought to prepare themselves as country or area specialists of whatever place or places' law and courts interest them so that they can apply not for law and courts jobs but for particular country or area slots in comparative politics. So long as they can teach the comparative politics courses needed, no one will care that they are most interested in the law and courts rather than some other aspect of those places. Finally law and courts graduate students fundamentally interested in administrative or regulatory or trans-national law and courts may be able to present themselves as public administration or international relations specialists.

One of the most difficult institutional taboos to evade, however, may be God's prohibition on any Americanist employing comparative methods or any comparativist using the U.S. as one of the countries to be compared. Only glimmers of a "trans-Atlantic" study of politics have so far emerged. It may be better until after getting a job not to admit that you want to mix meat and milk.

If not finesse, how about institutional change? Departments that have subsumed law and courts under American politics or fill their law and courts search committees with Americanists should understand that they are declaring that the study of non-U.S. courts is not an appropriate part of political science. "Public law" openings should be searched separately and the search committee should have strong representation from the other subfields of political science besides American politics.

It may even be time to question whether good old con law ought to be the sole or central undergraduate course offered in the law and courts area by political science departments. I hope that I have the authority, after spending over half my career on a law faculty, to say that law schools do not care at all whether their applicants have had the undergraduate constitutional law course. Even from a prelaw perspective, the best single political science undergraduate course would be one on the law-making process showing the interrelation between lobbyist, legislative, administrative and judicial law making. From a

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political science perspective, the question ought to be: If the political science major were only to take one course about law and courts, what should be in that course? There may be many different answers to that question, but surely the answer is not that they should study only one very atypical body of law created by one very atypical court in one country. The good old con law course is a wonderful liberal arts course melding the most fundamental value issues with practical, nitty gritty real world situations. But it should not be allowed to define the entire political science study of law and courts. And for many departments, either consciously or unconsciously, that is what it is doing.

Finally the institutional answer is not that the Americanists and the comparativists should each have their “own” law and courts specialist. The Kavar-Massoud paper signals both opportunities and dangers. It shows that the law and courts subfield have now come a long way “outward and downward” from the exclusive study of the U.S. Supreme Court’s constitutional law. The very existence of a separate law-and-courts subfield now creates a natural place to bring Americanist and comparativist studies together.

In a sense it has been only natural that as the American practice of individual rights-oriented constitutional judicial review has spread around the world, public law specialists overly preoccupied with American constitutional judicial review should feel their attention pulled outward from U.S. con law to constitutional judicial review elsewhere. Simply in biographical terms that is what happened in earlier generations. And the unintended consequence of those biographies was two fold. One, because the scholars involved had begun in American and moved to foreign, they naturally tended to compare U.S. to foreign experience, bridging the gap between American and comparative that existed in the rest of political science. Two, because they had begun in U.S. con law, they were naturally attracted to the new non-U.S. con law, and so they preserved the traditional political science excessive preoccupation with constitutional law to the exclusion of other kinds of law and with highest courts to the exclusion of other courts.

Kavar and Massoud show us a new generation, many of whom began as comparativists – often as country or area specialists, especially concerned with courts. This generation presents the danger that precisely because they began abroad rather than moving from American to foreign studies, they will lose the bridge and go back to thinking that as comparativists they ought to study only things foreign and are forbidden to use the U.S. courts as one of their comparative data points. And precisely because it was the dramatic and new impact of constitutional judicial review in their countries or areas that led many of the new generation to want to study law and courts, the Kavar and Massoud bibliography, with a number of important exceptions, as a whole shows the same excessive preoccupation with constitutional law in the present generation that has burdened past generations. Yet in much of the world the protection of individual rights will be far more a matter of administrative law than constitutional, the most politically important law will be regulatory law and the most rapidly growing law and court regimes will arise in connection with intellectual property law and commercial contract arbitration.² As we move from U.S. to foreign law and courts, it is important not to repeat abroad the total preoccupation with highest court constitutional law that has so burdened us at home. Of course each individual scholar is entitled to pursue whatever particular research he or she pleases, including exclusively work on matters constitutional, but it is important that the new movement be thought of not as “comparative constitutional law” but as comparative law and courts, all law and courts.

² I do not wish to re-ride here my old hobby horse about the “public” in “public law,” but I continue to insist that private law, and the vast areas of regulatory law that are not strictly speaking public law, ought to be within the domain of political science studies.

Marketing Comparative Law and Legal Institutions to a Broad Audience

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Leila Kavar and Mark Massoud set out an interesting and important agenda for the study of law and courts in comparative contexts, identifying a number of questions that matter in both institutionalized and under-institutionalized environments. Although I can imagine interesting research emerging from the proposed agenda, as an agenda the project is a degree of separation from outcomes and important political phenomenon. My friendly amendment would reframe the agenda to more clearly address general issues of politics.

I say this because I think it is important that law and courts scholars connect with a broader audience. Although the larger part of my scholarly career has been devoted to studying comparative law and courts, I was never interested in law and courts per se, nor was anyone around me (perhaps because I went to graduate school at MIT). Honestly, it was dumb luck more than crafty design that the topics I work on have gained a broader appeal. But I also know that the strategy I used to deal with the adversity I faced has contributed to the great fortune I have had in my career.

When I started studying the reception of European law by national judges, there were few comparative politics scholars studying law and courts. My dissertation chair was pretty much completely uninterested in my topic. It was hard to find literature to answer questions that were helpful to me. Most legal scholars thought I was clueless, and that my research question was long settled since clearly the supremacy of European law made such good sense. When I used to submit my work to conferences, I would end up on what I called “law orphan” panels where the only thing the papers had in common was that they studied some legal dimension of European integration. The audience was thin. The papers didn't speak to each other. And European lawyers were actually rather hostile to my political science efforts to understand a domain that they claimed for themselves. I feared I would end up in a law ghetto, a prospect that was especially worrying because I had met my husband in graduate school. We needed jobs for both of us, in the same city.

My dissertation advisor's skepticism forced me to redouble my efforts. The onus was on me to convince my colleagues that they should care about my work. This meant that I had to insert law and courts into broader debates of European politics and international relations. After one or two law orphan panels, I started to construct my own panel proposals in which I invited senior scholars and situated my interests as part of general debates. Usually I was the only person on the panel talking about law and courts. I guess I could have tried to connect with American politics scholars interested in legal institutions, but I wasn't interested in legal institutions per se. Also, the methods, data and questions of American politics scholars studying judicial institutions did not easily travel to the parts of the world and the institutions that interested me. Instead, I worked to convince general international relations and comparative politics scholars that my research answered questions that they cared about. I did not write for the American dominated field of law and politics, and thus my tenure case surely did not require their approval. Instead I published in mainstream comparative politics and international relations journals, and made sure I maintained a European politics audience. But I also framed my work in general political science terms. Of course I hoped that scholars of law and courts would also find the work of interest, but I constructed my own audience.

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In my view, the strategy I pursued by necessity is still the best strategy. Political science departments have few scholars who study law and courts. Especially if one looks for compatriots in law schools, chances are good that law-oriented colleagues will be more interested in American legal institutions than in comparative politics or comparative legal institutions. I also hear that jobs and interest in public law as a topic is declining. In my eleven years at Northwestern, we have done one half-search—for half a line—in public law. One of my American politics colleagues even made the case that half a line made sense for the studying of judicial institutions—a statement that astounds me to this day. Last I checked, the judiciary was one of the three branches of American government. More fundamentally, law is one of the central tools of politics—American, comparative, and international.

I don't agree that 'half a public law line' is about right, but it is a fact of life that most political science departments do not look at 'law and courts' as a topic in its own right. Perhaps it is a mistake, but I have to admit that I don't really care to argue in defense of law and courts even if undergraduate pre-law students will flock to these courses. I think a better approach is to make the very easy case that law and courts are fundamentally defining of many if not most of the central issues that interest political scientists.

So now I return to Kwar & Massoud's contribution that defines law and courts as an object of study. On the one hand, I agree that one must also focus on law as distinct from courts, that one can fruitfully study discursive and symbolic dimensions of the law and adjudication, and that things we might call 'law' and 'courts' will operate very differently in different contexts. But in the end of the day it is the political question that interests me.

If something big and real is at stake, then comparative politics, political theorists, international relations and even American politics scholars should be fairly easily convinced that an extra-institutional focus on law and legal mechanisms makes sense. A lot of social ordering is promoted through law in ways that never involve courts. So scholars interested in social order need to understand law's influence independent of courts. A lot of what looks like 'law' and 'courts', that walks, talks and quacks like 'law' and 'courts' is a form of political control that is quite removed from what Americans and West European promoters of liberal democracy and the rule of law have in mind. So of course there is much to learn by thinking about the discursive and symbolic ways in which law and adjudication are used to send political messages to subjects and foreigners alike. And every polity has domains where public law does not operate as such—whether it is gang/tribe norms rather than the state's legal norms shaping political life, whether the central state's authority counts for little because local patronage politics matter more, or whether charismatic leaders form their own domains of control and order within a geographic territory, the uneven reach of legal institutions is par for the course.

My fear then is that Kwar and Massoud are laying out an insider's agenda. I read Kwar and Massoud's essay as trying to counter what is perhaps an overly institution focus on courts, so as to create space to study law and courts in ways that interest them (and of course interest us all). But in my view, as long as the topic is important, the space is already there.

It takes work to push one's way into broader debates. Since I never thought of myself as a law and courts scholar, I did not know that there was a choir that might want to join. For those who are part of the law and politics choir, the risky path of least resistance is to join the choir and perhaps change it from within. But in my view it every scholar, and especially junior scholars, should explain their topic in general terms of broad interests. This goes for issues that already have broad audiences and for more

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esoteric topics like law and politics in the Andean community and the Economic Community of West African States. To this end, it is useful to shed terminology that is exclusionary (such as “public law”) and to bring in anecdotes, teasers and examples that are broader than a focus on law and courts per se.

It crushed me when my dissertation advisor said to me—in an effort to show comfort— “I used to think your topic was boring, but now I think it is interesting.” But I learned from her that a good puzzle is all it takes. A compelling question, a good method, and an interesting finding will draw readers to you. No matter the discipline, substantive area of focus, or geographic domain, curious minds will want to know your answer. As for the minds that are not curious, life is short and there are plenty of other kids in the sandbox to play with.

I started studying law and courts to better understand European integration. I was interested in European integration because I wanted to know how Europeans constructed successful liberal democracies from the ashes of shattered fascist regimes. I think most political scientists are like me—as undergraduates they had questions about politics that led them down a rabbit hole of specialization. Underneath the specialization resides the larger compelling interest in politics. Connecting to this interest broadens the appeal of one’s work. We must of course convince the choir that our research is sound, and that we can sing in tune. Maybe the subfield is stuck singing various versions of the top pop tunes over and over. But then just go off and do your own thing. We write for the larger audience, and this audience cares about politics in all its forms.

Public Law, Comparative Public Law, and Comparative Politics

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This special edition of the Law and Courts Newsletter that is initiated by Kavar and Massoud is welcomed. It is clear that Public Law, as a field, will continue to evolve into comparative public law. This evolution is not because the interesting and important questions that motivated American Public Law scholars are exhausted. Rather, the interesting and important questions raised by American Public Law scholars demand comparative analysis. In this light, Martin Shapiro (2012) argues that Law and Courts’ graduate students who are interested in Comparative Public Law must ground their training in one of four areas: 1) American Public Law; 2) Comparative Politics; 3) Public Administration; or 4) International Relations. I will further develop Shapiro’s argument and assert that Comparative Public Law, as a field, will evolve most fully as a convergence of traditional American Public Law and Comparative Politics. To this end, Kavar and Massoud’s (2012) conceptualization of Comparative Public Law as the “*politics of law*” is essential for understanding the development of Comparative Public Law.

In making this argument, I essentially marginalize Comparative Public Law as grounded in International Relations or Public Administration. My point is not to completely exclude the important contributions that International Relations and Public Administration scholars make to the Public Law field. It is more the fact that Public Law operates at the country level. This means even given the diffusion of norms and conventions from international law, these norms and conventions must be internalized by domestic political systems for these norms and conventions to exert authority on domestic political outcomes.

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The diffusion of human rights law is a case in point. In my field research in Central America, I have found that human rights (particularly in post-conflict states) are a principal concern of judges on Central American high courts. That said, the pressures that Central American high court judges feel to advance human rights practices do not come from civil society, as Epp's (1998) "rights revolution" argument would suggest. It is tempting to ascribe the actions of these judges to their attempts to comply with international human rights regimes. However, in post-conflict environments, the actions of these judges are relatively unobserved by both domestic and international actors. Consequently, the pressures that judges feel to advance human rights are generally self-imposed. These societal elites view human rights as fundamental rights.

Meanwhile, citizens in these countries are interested in advancing human rights. They, however, choose to advance their human-rights agenda through the legislative arena. As Kwar and Massoud (2012) point out "the politics of rights may be largely enacted outside the judicial realm." Does this then mean that the "judicialization" of politics is less pronounced in these Latin American countries and developing democracies in general? Of course, the answer to this question is no. Once a country adopts a law the judiciary becomes the center of administration and enforcement of the legislative provision. Democratizing societies are important environments in which to study the development of the judicial power largely because there are many moving parts. This is why comparative politics and comparative formal institutional work are great training areas for graduate students who are interested in Comparative Public Law.

The study of comparative politics ensures that comparative judicial scholars have the necessary knowledge to place the behavior of judges in its respective institutional environment. Particularly in presidential democracies, students of comparative judicial politics find separation of power approaches to be quite useful in understanding how judges use their discretionary power. But the institutional relationship between the formal institutions of government in developing countries is only part of the scholarship that Comparative Public Law scholars produce. Kwar and Massoud's contention that the focus of Comparative Public Law should shift to *the politics of law* is very appropriate. In democratizing societies, this focus on the *politics of law* is particularly important.

Scholars like Guillermo O'Donnell (1999) demonstrate the centrality of the rule of law in the consolidation of democratic governance. These arguments validate the importance of the judiciary in the rule of law but also demonstrate the necessity of placing the law, and law construction itself, into the structure of the political system. This placement again establishes the need for comparative training as there are discursive and symbolic dimensions of the law that are embedded in country-specific norms, routines and institutions. At the same time, the role of the law in creating a functioning rule of law encourages cross-national and cross-regional comparison. Again, comparative politics is a fertile training ground for comparative public law scholars.

It is here that I also stress the importance of field research and letting individuals, who are involved in law and courts domestically, speak for themselves. Judges and legal experts have a particular way of thinking about the world in which they live. In a study of attitudes about the fairness of the justice system, Gill and Walker (2005) find that Nicaraguan judges produced estimates of fairness that were distinctive from the estimates of other Nicaraguan societal elites, who were more ideological in their assessments. This distinctiveness is important because this means these underlying attitudes likely color judges decision-making processes. This distinctiveness is also important because it highlights the fact that comparative public law scholars must continue to search for measures that more adequately

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account for the preferences of judicial elites. Asking judges directly, while crude, is still an important component of comparative judicial research, even if we believe that the responses that we elicit may be suspect. The suspect and probabilistic nature of data makes country-specific knowledge even more important.

Comparative large-N analyses are increasingly visible in comparative public law research. Nevertheless, most large-N scholars tend to augment their large-N findings with detailed case-level analysis. These case-level analyses demand country-specific knowledge. While scholars can do good work without spending time in the field, good field research produces richer contextual analysis of the phenomenon of interest.

The case for a comparative *politics of law* is compelling. Training in American public law is very useful for comparative public law scholars. Nearly all of the theoretical frameworks that scholars associate with comparative judicial politics and judicial decision making are derived from American public law. That said, Shapiro (2012) is correct that there is a feedback process. As comparative public law scholars test American public law theories in comparative perspective, American public law scholars have the ability to re-test these American public law theories in new subnational or new political contexts. New political contexts include separation-of-powers contexts that democratizing or parliamentary contexts expose.

Shapiro (2012) points out that comparative public law scholars should not become “comparative constitutional law” scholars. I would agree that comparative public law scholars have shown a bias toward constitutional law. Again, this is because American public law scholars influence comparative public law scholars. It is clear that comparative public law scholars will increasingly deviate from privileging primarily the examination of constitutional law. This deviation is because high courts in other countries are often structurally different from the United States’ Supreme Court. Because many high courts are divided into chambers with specific competencies, important public law issues are also adjudicated in these chambers. In turn, these non-constitutional chambers are increasingly important to how the rule of law gets done. At the same time, constitutional chambers are able to impact politics without ruling on constitutionality. Again, the *politics of law* requires that comparative public law scholars examine how courts create areas of uncontested competencies as much as how courts contest policies against the executive and legislative branches.

Furthermore, the focus on constitutional law emphasizes dissension rather than consensus. In post-conflict and democratizing societies, the process by which courts form consensus may be more important than how judges behave in the context of dissension. Moreover, courts in democratizing societies may seek to enhance their legitimacy through unanimous voting rather than attitudinal voting, or unanimous voting actually reflects an attitude on a different dimension. In democratizing societies, this tendency to vote unanimously is perhaps more likely in areas where the court, as a body, attempts to build its competency and control. While law is written by the legislature, consistent enforcement of the law enhances the reputation of courts. The evolution of comparative public law is likely to continue toward the study of the *politics of law*.

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BOOKS TO WATCH FOR

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Christopher B. Banks (Kent State University) and **John C. Blakeman** (University of Wisconsin-Stevens Point) have published *The U.S. Supreme Court and New Federalism: From the Rehnquist to the Roberts Court* (Roman & Littlefield, 978-0-7425-3504-6). They offer the most current and the first book-length study of the U.S. Supreme Court's "new federalism" begun by the Rehnquist Court and now flourishing under Chief Justice John Roberts. Using descriptive and empirical methods in political science and legal scholarship, and informed by diverse approaches to judicial ideology, from historical to new institutionalist, they investigate how the U.S. Supreme Court rulings have shaped the political principle of federalism. While the Rehnquist Court reinvigorated new federalism by protecting state sovereignty and set new constitutional limits on federal power, Banks and Blakeman show that in the Roberts Court new federalism continues to evolve in a docket increasingly attentive to statutory construction, preemption, and business litigation. In addition, they analyze areas of federalism not normally studied by scholars such as religious liberty and foreign affairs.

James L. Gibson (Washington University St. Louis) has published *Electing Judges: The Surprising Effects of Campaigning on Judicial Legitimacy* (University of Chicago Press, 978-0226291086). Gibson responds to the growing chorus of critics who fear that the politics of running for office undermine judicial independence and even the rule of law. While many people have opinions on the topic, few have supported them with actual empirical evidence. Gibson rectifies this situation, offering the most systematic and comprehensive study to date of the impact of campaigns on public perceptions of fairness, impartiality, and the legitimacy of elected state courts—and his findings are both counterintuitive and controversial. In what is the most surprising finding of the book's analysis, Gibson argues persuasively that elections are ultimately beneficial in boosting the institutional legitimacy of courts, despite the negative effects of some campaign activities. This is because judicial elections, ipso facto, boost the legitimacy of courts—and do so to a degree that overpowers whatever negative reactions citizens have to campaign actions. Thus, the net effect, Gibson argues, of electing judges is to inject states courts with enhanced institutional legitimacy.

Donald P. Kommers (University of Notre Dame Law School) and **Russell A. Miller** (Washington and Lee University School of Law) have published the third edition of *The Constitutional Jurisprudence of the Federal Republic of Germany* (Duke University Press, 978-0822352488). Justice Ruth Ginsburg has written the foreword to the book. "The third edition of this renowned English-language reference has now been fully updated and significantly expanded to incorporate both previously omitted topics and recent decisions of the German Federal Constitutional Court. As in previous editions, Kommers and Miller's discussions of key developments in German constitutional law are augmented by elegantly translated excerpts from more than one hundred German judicial decisions. Compared to previous editions, this third edition more closely tracks Germany's Basic Law and, therefore, the systematic approach reflected in the most-respected German constitutional law commentaries. Entirely new chapters address the relationship

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between German law and European and international law; social and economic rights, including the property and occupational rights cases that have emerged from Reunification; jurisprudence related to issues of equality, particularly gender equality; and the tension between Germany's counterterrorism efforts and its constitutional guarantees of liberty.”

George I. Lovell (University of Washington Seattle) has published *This is Not Civil Rights: Discovering Rights Talk in 1939 America* (University of Chicago Press, 978-0226494036). Since the earliest days of the American republic, observers have claimed that Americans have an unusual tendency to use the language of rights when expressing dissatisfaction with political and social conditions. Today, this tradition continues as Americans confront a complicated set of twenty-first-century problems by arguing over the meaning of constitutional language and symbolic events of the 1700s. Drawing on a remarkable cache of Depression-era complaint letters written by ordinary Americans to the federal government, Lovell challenges these common claims. Although the letters were written prior to the emergence of the modern civil rights movement—which people often assume is the source of contemporary rights talk—many contain novel legal arguments demanding new rights entitlements that went beyond what authorities regarded as legitimate or required by law. Lovell demonstrates that rights talk is more malleable and less constraining than is generally believed. Americans, he shows, are capable of deploying idealized legal claims as a rhetorical tool for expressing their aspirations for a more just society while retaining a realistic understanding that the law often falls short of its own ideals.

Sean Wilson (Wright State University) has written *The Flexible Constitution* (Lexington Books, 978-0739178157). Influenced by the views of Ludwig Wittgenstein, Wilson tackles the problem of how a judge can obey a document written in ordinary, flexible language. He argues that whether something is “constitutional” is not an historical fact, but is an artisan judgment. Criteria are set forth showing why some judgments represent superior connoisseurship and why others do not. Along the way, Wilson offers a potent critique of originalism. He not only explains this belief system, but shows why it is inherently incompatible with the American legal system. His conclusion is that originalism can only be understood as a legal ideology, not a meaningful contribution to philosophy of law. The ways of thinking about constitutional interpretation provided in the book end up challenging the scholarship of Ronald Dworkin and numerous law professors. And the findings also challenge the way that professors of politics often think about whether a judge has “followed law.”

Howard Gillman (University of Southern California), **Mark A. Graber** (University of Maryland), and **Keith E. Whittington** (Princeton University) have published *American Constitutionalism, volume 2: Rights and Liberties* (Oxford University Press, 978-0-19-975135-8). This book of cases and materials for the teaching of constitutional law is the second of two volumes. Constitutionalism in the United States is not determined solely by decisions made by the U.S. Supreme Court, and this book reflects that broader process of how the constitutional system actually works while providing materials for interesting theoretical, political and legal discussions. *American Constitutionalism* offers a number of useful features. It covers all important debates in American constitutionalism (not just those that have been recently litigated before the Supreme Court), organized by historical era. It incorporates readings from all the prominent participants in those debates. It clearly lays out the political and legal contexts of those materials. It integrates a wide range of documents and cases, including decisions made by elected officials and state courts. The book offers numerous pedagogical features, including topical sections within each historical chapter, explanatory headnotes for the readings, questions on court cases, tables and figures, and suggested readings. The text is supported by a supplementary website for instructors and students.

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Reginald S. Sheehan (Michigan State University), **Rebecca D. Gill** (University of Nevada-Las Vegas), and **Kirk A. Randazzo** (University of South Carolina) have published *Judicialization of Politics: The Interplay of Institutional Structure, Legal Doctrine, and Politics on the High Court of Australia* (Carolina Academic Press, 978-1-61163-207-1). In this book the authors argue that it is the interplay of institutional structures, a growing concern for individual rights, and the willingness of the justices to engage in purposive policymaking that lead the court to engage in judicial politics. The High Court of Australia underwent a significant structural change in its jurisdiction at about the same time that it was also experiencing a shift away from strict legalism. Segments of the Australian population began to lose faith in the ability of Parliament to right societal wrongs and protect the rights of individuals. The result was a period of time in which the decision-making of the High Court was under scrutiny because the Court seemed to be engaging in policymaking. The findings suggest that justices can be constrained by institutional structures and the acceptance of restrictive legal doctrines. Changes in those conditions are necessary for judicialization of politics to occur in a court. This book will be of interest to a wide range of scholars who are interested in the phenomenon of the judicialization of politics.