



Law & Courts

NEWSLETTER OF THE LAW & COURTS SECTION OF
THE AMERICAN POLITICAL SCIENCE ASSOCIATION

A Letter from the Section Chair

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The Law and Courts Section begins the new year with a robust membership (ranking sixth among all APSA sections at 766 members), two new executive committee members, and a new treasurer. We welcome James Stoner and Chad Westerland to the executive committee and Isaac Unah as our new treasurer. We are also grateful to the former Chair, Howard Gillman, for his remarkable energy and attention to section business even as he juggled the demands of his Deanship, and our former Treasurer, Gordon Silverstein, for his careful stewardship of our finances. We also thank outgoing executive committee members Doris Marie Provine and Tim Johnson. I would also like to thank those many members of the section who have agreed to chair or serve on Section committees in the coming year.

In addition, I think everyone will also agree that this year's conference was a great success. Panels were well attended—not a surprise given the high quality of research that was presented. We had 232 paper submissions and 32 panel submissions; we ended up with 113 papers given on 19 panels with 58 discussants and chairs, and an average panel attendance of 22 persons.

At the executive committee meeting in August, we discussed several new items that will shape our agenda for the coming year. One of those involves the search for two key positions: Editor of the Law and Courts Book Review and a Webmaster for the Law and Courts Webpage. Wayne MacIntosh has served tirelessly in the former position for years; as Howard Gillman noted, Wayne is surely one of our hardest workers on behalf of the Section and we are extremely grateful for his commitment to the Book Review. As for the latter position, Christine Harrington has maintained the Section's website for years as well and we thank her for her outstanding work in archiving the Section's materials for our benefit. It is now time for these positions to move to new stewardship.

Among the most prominent policy matters we discussed involved consideration of whether the Section should sponsor its own journal. I requested this item be added to the meeting's

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Inside this issue:

Malcolm Feeley,
Lisa Hilbink,
Thomas Keck, Wendy
Martinek, Jeffrey
Staton, James
Gibson—plus “Law
and Courts Section
Awards,” “Books to
Watch For” and
“Upcoming
Conferences.”

continued on page 4

Table of Contents

<i>Letter from the Chair</i>	<i>pages 1 & 4</i>
<i>Symposium: “The Oxford Handbook of Law and Politics”</i>	
<i>Whither Public Law Scholarship? An Assessment of an Assessment of the Field</i> by Malcolm M. Feeley	<i>pages 4—7</i>
<i>From Comparative Judicial Politics to Comparative Law and Politics</i> by Lisa Hilbink	<i>pages 8—10</i>
<i>Law, Politics, and Political Science</i> by Thomas M. Keck	<i>pages 11—15</i>
<i>Interdisciplinarity in Legal Scholarship</i> by Wendy L. Martinek	<i>pages 16—18</i>
<i>Shaping the Field? A Review of The Oxford Handbook of Law and Politics</i> by Jeffrey K. Staton	<i>pages 19—22</i>
<i>Research Spotlight: Nastier, Noisier, Costlier — And Better: Why Letting Judges Speak Out During Political Campaigns Enhances Democracy And Serves Justice</i> by James L. Gibson	<i>pages 23—26</i>
<i>Annual Law & Courts Section Award Winners</i>	<i>pages 26—30</i>
<i>Books to Watch For</i>	<i>pages 31—32</i>
<i>Upcoming Conferences and Announcements</i>	<i>page 33</i>

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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, Spring, and Summer. Deadlines for submission of materials are: November 1 (Winter), March 1 (Spring), and July 1 (Summer).

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**: Bruce Peabody, bgpeabody@msn.com of publication of manuscripts or works soon to be completed.

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(Chair's Column, continued from Page 1)

agenda because of my continuing concern that court scholarship is sometimes hard to place in mainstream political science journals. Many of us, I am sure, have seen reviews of our work suggesting that it be placed in a "specialized" journal for judicial politics. I continue to puzzle over why judicial scholars (who are presumably the peers reviewing our submitted manuscripts) would suggest such a course of action; in my view the judiciary is the third branch of government and in that sense is not a "specialized" subject in political science. It *is* political science. This problem is most acute for junior scholars in our field, of course, who are often counseled that their work must appear in the top political science journals for purposes of tenure review. Hence my interest in the possibility of a section journal.

To be sure, we are the beneficiaries of a number of peer-reviewed journals that feature legal scholarship, such as *Judicature*, *The Journal of Empirical Legal Studies*, *Justice System Journal*, *Law and Society Review*, and *The Journal of Law and Politics*. It may be that these journals serve our needs sufficiently. Nevertheless, the idea of a Section journal has been fielded on several different occasions in the past and the members of the Executive Committee thought it would be useful to explore the idea more systematically. To that end, I appointed a committee chaired by Larry Baum of Ohio State University to consider the costs and benefits of such an endeavor. In the coming year, Larry and his committee will evaluate those costs and benefits in consultation with Section members, and formulate a recommendation for the Section to consider at the next Business meeting.

Symposium: *The Oxford Handbook of Law and Politics*

Editors Note:—The study of law and politics is a cornerstone of the discipline of political science, and it has been one of the productive areas of cross-fertilization between the various subfields of political science and between political science and other cognate disciplines. *The Oxford Handbook of Law and Politics*, (New York: Oxford University Press, 2008), edited by Keith E. Whittington, R. Daniel Kelemen, and Gregory A. Caldeira, seeks to provide a comprehensive survey of the field of law and politics in all its diversity, ranging from such traditional subjects as theories of jurisprudence, constitutionalism, judicial politics and law and society to such re-emerging subjects as comparative judicial politics, international law, and democratization. The volume gathers together leading scholars in the field to assess key literatures shaping the discipline today and to help set the direction of research in the decade ahead. The contributions to this symposium discuss the Handbook and the state of the field more generally.

Whither Public Law Scholarship? An Assessment of an Assessment of the Field

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At the University of Wisconsin, it was the policy—at least in the 1970s when I was there—for all members of the political science department to read each others' annual professional activities reports as part of an exercise to determine salary increases. I was always awed at how productive my colleagues were, but I must confess that for a while I tended to be respectfully dismissive of encyclopedia and handbook entries. This changed markedly after I'd written my first such entry. I gained a new respect for the genre. Encyclopedia and handbook entries constitute an art form requiring much thought and much discipline. Consider: authors are charged with summarizing and assessing work within an area of specialization. They must identify the core features of that specialization, succinctly examine the most salient issues,

present the current debates, and point to future issues. And they must do all this within strict word limits. This poses immense challenges: one must do more than summarize the conventional wisdom. One cannot criticize the conventional wisdom without first explicating it. Similarly, one cannot merely report on latest developments in the field without first providing an overview of its core. The entry is not like a set of comments prepared for a roundtable at one of our professional meetings, where like-minded scholars come together to talk about their latest research. It is different than all these things. While authors need not aim to write only for the “general reader” or for the intensely interested specialist, nevertheless they do write for both of them, as well as others. Further, while they do not need to write for the ages, they must keep in mind that someone will be reading their entries and judging them some years hence--they cannot write the academic equivalent of today’s headlines. And again keep in mind that at most they only have a few pages in which to do their job.

In short, a good entry in an encyclopedia or handbook must be a gem. It must have several facets and each must sparkle. Some handbooks and encyclopedias are renowned for this quality. My personal favorite is the 1968 edition of the *Encyclopedia of the Social Sciences*, which I consult as often as I do the more recent edition.

The *Oxford Handbook of Law and Politics* compares favorably with the best of this genre. Keith Whittington, Daniel Keleman and Gregory Caldeira are to be congratulated for assembling a first rate group of contributors who in combination have put together a wonderful volume. It is a weighty project in every sense of the word. The book weighs several pounds. It has forty five separate entries. And as I’ve said, the contributors are an all star cast. The headings, subheadings and titles of entries are still further evidence of the thought and care that went into the volume. But of course the real test is the quality of the entries themselves. I must confess I have not read all 774 pages and all forty five entries (excluding index and front matter). But I have skimmed and dipped in here and there—reading most of the essays that are both closest to and farthest away from my areas of interest, and reading as well the volume’s four introductory and three concluding essays. I feel moderately confident in assaying the *Handbook* as a whole, and in underscoring my conclusion: the editors and authors have done well for the field. We are in their debt.

But the question is, how should we approach such an enterprise? Below is my effort.

The entries in this handbook can serve several quite different functions for different readers. First, some of them provide a quick survey into areas that are on the periphery of the reader’s interests; they provide a convenient way for catching up, at least casually, in areas where we don’t read the journals. In a discipline and a field that has no obvious methodological or theoretical core, and draws heavily on work in other disciplines and fields, this is valuable. Thus for instance, I found Tom Tyler’s entry on law and psychology, and Chris Tomlin’s article on law and history, and still others’ entries interesting because they cover areas I don’t keep up with, but which have relevance to some of my concerns and some of the central issues in the field.

Second, some of the entries will provide instructors with materials for class comments, and for me especially in my undergraduate survey course. Robert George’s essay on natural law is as good a succinct treatment of the subject as I have ever read. Indeed it is especially valuable since it is explicitly directed at a law and politics audience. And of course it has to prick the consciences of the normative selves in all of us who should search for greater values coherence in our divided selves. Similarly, although Frank Cross’ discussion of judicial independence is frustrating in its failure to pin down the subject, nevertheless it is helpful in cataloguing the many and at times conflicting dimensions of the concept of judicial independence. I am likely to steal from sections of his discussion in response to questions about judicial independence. Similarly, Bryant Garth’s essay on law and globalization will probably be useful in my teaching in ways I cannot yet even begin to anticipate. The same can be said for many of the other entries. Christine Harrington’s discussion of informal justice provides a framework for thinking about all types of legal orderings.

A number of entries are likely to be useful in work with students at opposite ends of their educational experience: new graduate students (and advanced undergraduates) at the one end, and advanced graduate students preparing for qualifying exams at the other. The same essay can serve different functions for both these quite different groups, an introduction to students unfamiliar with the field, and synthesis for advanced students. A student new to the topic can read an essay to get his or her bearings before diving in to the field in depth, beginning in all likelihood with some of the materials cited in the essay itself. An advanced student can profit not so much by gaining new knowledge but by learning how to synthesize, how to break down a topic into constituent parts, how to relate it to allied issues, and how to identify some

of the major continuing issues in the field. Indeed, some of the Handbook's essays can serve as model answers to preliminary questions.

As I said, I've not read all the essays in the volume. But I did read the editors' long introduction in Part I, the three essays in Part II (Approaches), and the three essays in Part X (Old and New). Since they frame the contributions in the large middle section of the volume, I want to offer some brief comments on these bracketing essays. Whittington, Keleman, and Caldeira do a wonderful job of summarizing the range of concerns in the volume, and thus the field. They recognize that our field is eclectic and needs to be tolerant as to both scope and methods. Indeed, they embrace an expansive view of the field, and identify contributors and topics that I suspect many others, less imaginative or gracious, would not have included in the *Handbook*. (One indicator of this: by my rough count over a third of the contributors have no training in political science and have never held appointments in political science departments. Nevertheless, their entries are strong and clearly speak to our interests, as measured by the range of courses we teach and the range of articles and books we publish.) This expansiveness speaks well of the editors' judgments. Their decision to invite such a range of contributors also reflects two other factors: the "law and..." enterprise has had a significant impact not only on the periphery of our field but on its core as well; and it reveals that at long last law professors are paying greater attention to the work of political science public law scholars. Nice.

All three of the essays, in Part II, "Approaches," are models of coverage and clarity. Jeffrey Segal does a wonderful job explaining the main tenants of the attitudinal model. Furthermore, he engages meaningfully with the strategic model as it applies to the U.S. Supreme Court in a way that advances understanding and scholarly debate. Similarly Pablo Spiller and Raphael Gely summarize the main features of the newer strategic approach based on rational choice models of the judicial process. Although they too focus on the U.S. Supreme Court, they move beyond it to consider courts elsewhere, and indeed it may be that the strategic model has more power elsewhere, where high courts are not so well institutionalized and secure. Indeed, the essays by Segal and Spiller and Gely nicely engage each other. Anyone wanting an introduction to the two areas they discuss would be well served if they were directed to these essays. The excellence of these essays clearly reflects the erudition of the authors, but I suspect the mutual engagement also reflects encouragement by the editors as well. (One small criticism of the volume: it would have been nice to see more mutual engagement in other contributions. But this would have required authors to submit their contributions well before deadline. Heaven forbid.)

Rogers Smith's "Approaches" essay is in a special class. More than any other single person in recent years, he is responsible for creating the new and important sub-subfield in the law and courts area, historical institutionalism and the law. He has connected the dots among constitutional history, doctrine, the Supreme Court, American political history, and the revived interest in institutions (the new institutionalism) in political science and sociology more generally. This essay is likely to place this new subfield on an even more secure footing. Indeed, it reveals the constitutive power of a label; give an inchoate set of ideas a name, and it becomes a "project." Subsume a group of diverse writing under the label and you have a subfield. This is a wonderful development. It returns the study of law and politics to its roots. The study of law, legal institutions, legal regimes, and legal systems has traditionally been anchored in historical and institutional and comparative analysis that seeks to address big social science issues—variation across time and across location. This development is a nice supplement to judicial behavior studies, which has in recent years occupied a great deal of the field's attention. Although judicial behaviorists claim to be markedly different than the legalistic law professors whose approach they ridicule, nevertheless much of their work is something of a reflection of conventional legal scholarship. Like their law professor counterparts, judicial behaviorists focus, almost exclusively, on the handful of split decisions in relatively recent U.S. Supreme Court cases in order to divine their own form of explanation. Of course this is a legitimate enterprise; social science ought to be about accounting for variation, and in my view judicial behaviorists do a convincing job of explaining why Supreme Court justices vote as they do. But the US Supreme Court is a highly atypical "court," and legal decision making is everywhere. Historical institutional analysis promises to be much more expansive. Further, it too is interested in accounting for variation, but of a much more dramatic sort—variation across time and regime (and comparative historical analysis promises even more), and how issues emerge before a court, as opposed to how judges on one court vote once issues get to their doorstep. Smith's essay points the way towards a more theoretically rich, if methodologically messier, agenda for public law scholarship. Although framed in terms of *current* developments in American political science and sociology, its vision is certainly not limited to this. Indeed, in scope and method it harkens back to work in sociological jurisprudence and some of the classical concerns of nineteenth and early twentieth continental and English legal history and comparative law, and to Weberian-inspired social science more generally.

The three “approaches” essays are interesting for still other reasons. Despite all the good things I’ve just said, in some ways there is a disconnect between the “approach” chapters and the remaining thirty nine topical essays. Many don’t fit comfortably within any of the approaches. This suggests that the scholars in our field have not waited for theoretical “approaches” to be developed before striking out on their own agendas. And of course it reflects the eclectic nature of our field. Still it is puzzling that there is not more of a connection. One reason may be the lack of breadth of the approaches chapters. All the references cited in Segal’s Approach essay focus exclusively on American courts, and most of them on one court, the U.S. Supreme Court. This suggests that this approach is successfully used to explore some of the behavior of a handful of justices on a particularly distinctive—unique?—institution. Yet none of the subsequent essays, except Spaeth’s, really builds on or elaborates this approach in any sustained way.

As the newest subfield, it is not surprising that only a handful of the subsequent essays in the volume build in any explicit way on the historical institutional approach. Still the themes of several of the essays are compatible with it (among them, the essays by Ginsburg, Hirschl, Alter, and most especially Whittington, Graber, Gillman, and Harrington). If this approach has the potential that I think it does, one would expect that entries informed by a historical institutionalist approach will feature prominently in the next *Handbook of Law and Politics*. Also, almost all of the works cited in Smith’s ambitious essay deal with American subjects, yet as I suggested above, this approach has obvious implications for comparative research, as the author himself acknowledges.

Interestingly, it is Spiller and Gely’s discussion of rational choice models, which are often accused of being overly narrow, that is most cosmopolitan. Although they too draw mostly on work about American institutions, their essay has a higher proportion of references to research on non-American institutions (clearly a growth area of our field) than do the other two “Approaches” essays. Also, by my count some appreciation of the strategic approach informs the discussions in a high proportion of the entries in this volume (roughly one third), and especially of those entries that extend coverage beyond law and politics, see e.g., Shapiro, Chavez, Ginsburg, Vanberg, Hirschl, Scheppele, Rodriquez, Garrett, Yaloff, Epstein, Haire, Cross, Rose-Ackerman, and Kornhauser.

Still, for the long term I would bank on historical institutionalism and the law, especially since it has begun to find ways to incorporate some of the more provocative insights of rational choice models of the sort suggested by the works of John Ferejohn, Barry Weingast, Oliver Williamson, Douglass North, Mark Ramseyer, Martin Shapiro, Lee Epstein, Keith Whittington, and still others, which are discussed in various different places in the volume.

Before concluding let me say a few words about the three final essays. They are all written by grand old men in the field (the next edition of the *Handbook* most certainly will have a grand old lady). Both Scheingold’s and Spaeth’s essays offer views of the profession from their many many years in the profession and their distinctive (and distinctively different) perspectives. Graduate students should eat them up. They get two smart people looking back and reflecting on their careers and the intellectual passions that animated them. They should be so lucky as to have such engagement in their work after long careers. The essays are all the more valuable because they are personal reflections. Good stuff. Martin Shapiro’s essay is more didactic. Always the one to look for the teaching moment, Shapiro takes advantage of the forum to point out that if in our field, we claim to be interested in “law,” there is a lot more law in a lot more places than we choose to look. Similarly, he says, if we claim to be interested in courts, we have made a great mistake to focus so much of our attention on the most distinctive if not unique forms of courts and ignored other important courts, and especially court-like institutions that are part of the administrative process. Still, Shapiro must be pleased with the entries in the *Handbook*. It seems to me that the range of entries nicely reflects his concerns about the breadth of the field.

I feel obliged to offer a conclusion, some sort of wrap-up, to this rambling essay, so here it is—my suggestion as to what do with the volume: Get your department to order the volume—or if you can afford it, order it yourself. When it arrives, place it on departmental reserve and invite public law graduate students and would-be public law graduate students to read it. Arrange a schedule; select two or three essays at a time, and meet in the student lounge to discuss them. Eventually work your way through most if not all of the volume. It should be a rewarding experience not only for the students but for you as well. What you can do in this process is what the authors of the entries did not do enough of—engage the different entries with each other. You might be able to come up with some speculations that could lead to some nifty student paper topics if not research projects. What happens when the attitudinal model meets trial courts? Non-American courts? Courts with no tradition of dissenting? What happens when the strategic model meets historical institutionalism? What happens when American concerns with race, gender and courts meet non-American courts or their

alternatives? What if any are the implications of the dramatic recent shifts in the roles of constitutional courts in Eastern Europe and the former Soviet Union for the understanding of American judicial institutions? What does the study of Latin American supreme courts suggest about the study of the American Supreme Court? After reading the entries of Bryant Garth and Christine Harrington, wither courts? The combination and permutation of the various ideas in the entries invite a host of interesting possibilities.

You get the idea.

From Comparative Judicial Politics to Comparative Law and Politics

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Among the many merits of the new Oxford Handbook of Law and Politics (OHLP) is its inclusion of two full sections (ten chapters totaling 200 pages) on law and politics issues from comparative and international/supranational perspectives. Had the volume been published a decade earlier, this would certainly not have been the case. Indeed, many of the authors of these chapters were, like myself, still graduate students ten years ago, toiling to find a place in a subfield long dominated by scholars of a single court in a single country—namely the U.S. Supreme Court. Both the quantity and the quality of their contributions to this handbook are a testament to our shared success in opening the subfield to new cases and new questions. The study of law and courts outside the U.S. has come a long way in a short time.

In striving to establish our legitimacy as comparativists interloping in a traditionally Americanist domain, however, I fear we are, perhaps unwittingly, reproducing some of the existing pathologies of the law and courts subfield. Reading the chapters in the “Comparative Judicial Politics” section of the OHLP, I was struck by three things: first, the almost exclusive focus on high courts and constitutional decision-making in the countries of study; second, a tendency to discount the relevance of factors in any way internal to law or legal institutions; and third, a limited and limiting view of politics as narrowly instrumental.

The focus on high courts and constitutional decision making is easy enough to understand and arguably the least problematic of these three issues. If Americanists who study law and courts at levels other than SCOTUS have faced an uphill battle for professional attention and recognition, imagine the position that comparativists find themselves in, having to demonstrate that the phenomena they want to study are empirically significant and theoretically relevant, even if they aren’t likely to make even the back pages of U.S. newspapers or be the subject of major debate on U.S.-based blogs and list-servs. To be sure, the proliferation of high courts with constitutional review powers (Ginsburg, OHLP) and the increased involvement of old and new courts in “mega-politics” (Hirschl, OHLP) are phenomena that cry out for explanation and are thus natural targets for comparative analysis. However, if (at least part of) the goal is to understand when, where, how, and why law and legal institutions matter, or come to matter, for governance,—that is, for the way that governments provide (or fail to provide) basic public goods, such as physical and economic security or social services—, then a narrow focus on the highest court, and often on a subset of decisions of that court, is definitely not sufficient. Not only do we need more studies of lower court decision-making in different countries (and different provinces), but we also need more research into the role of police, prosecutors, public defenders, ombudsmen offices, comptrollers, and other agencies charged with upholding the law and/or monitoring powerful actors, as well as the interactions between them. Brinks’s (2008) book on the judicial response to police killings in five different Latin American cities offers an excellent model for such work.

We could also benefit from far more studies of the role that actors in civil society, be they inside or outside the legal profession, play in advancing or undermining the rule of law at any level. Chavez (OHLP) surveys some of the work that has been done in this vein, but it is clear that the various propositions in the literature are in serious need of systematic comparative testing and refining. One noteworthy effort in this regard is the Halliday, Karpik, and Feeley (2007) volume, which seeks to illuminate the role that actors in the “legal complex” (judges, lawyers, and legal academics) play in constructing or eroding political liberalism. Many of the contributions to that work, covering sixteen countries on four continents, discuss high courts, but treat them as only one piece of a larger picture in struggles for core rights of

citizenship, a moderate state, and associational autonomy. The analyses do not all point in the same causal direction, but they offer a rich trove of empirical material and a number of propositions that might be further tested in future research.

Perhaps turning the focus away from high courts in this way would help mitigate the second tendency in the literature on comparative law and courts, evident in several of the chapters of the OHLP, to caricature and dismiss arguments that highlight factors internal to law or legal institutions. I can think of no political scientist specializing in judicial behavior outside the U.S. (or inside, for that matter) who accepts the legal formalist myth that judges decide cases uniquely or mechanically on the basis of legal text or other legal rules. But even if we all proceed on a legal realist/political jurisprudential premise, we need not conclude that judicial behavior is determined by “conditions outside of the courts and the law” (Chavez, OHLP: 75). To paraphrase Martin Shapiro in the closing chapter of the OHLP, law, by definition, puts limits on discretionary decision making. Where political decisions are purely discretionary, as in autocratic regimes, there is really nothing for law and politics scholar to study. And while democratic legislators may have relatively few constraints on their discretion, the discretion of administrators and judges (particularly in lower courts) is often highly constrained, and therefore, “the law and politics scholar working on such secondary rulemaking...is warned to pay as much or more attention to the constraining rules as to the discretion” (Shapiro, OHLP: 773).

To this I would add that the “constraining rules” operating on judicial actors may not be directly legible off the legal text. Indeed, there may be formal rules and informal norms internal and particular to judicial institutions that shape and constrain judicial conduct by providing, as historical institutionalists put it, “the content of the identities, preferences, and interests that actors [can] embrace and express” (Smith OHLP: 47-8). In my work on Chile, I demonstrate that there was a clear pattern of judicial behavior in civil and political rights cases across time (despite administration and regime change), and argue that this persistence is best explained by institutional factors that served both to constitute and constrain judicial preferences (Hilbink 2007). This is not a crude argument about judges’ “political culture,” as Chavez (OHLP: 70-1) portrays it, but rather an analysis that takes seriously the ways that the institutional setting can (and in the Chilean case, does) affect the way judges understand what they want to do, what they think they ought to do, and what they believe they can do (Gibson 1986: 150). As Baum argues, both consciously and subconsciously, judges are motivated by a desire for respect and approval from their reference groups or audiences (2006: 43–48), and among the audiences that will be most salient for judges are professional colleagues and superiors (2006: 171). While calculations about the way the other branches will respond to judicial decisions (the “separation of powers” model) are no doubt at work to varying degrees in different courts and at different times, even high court judges “face a wide array of incentives based on personal preference, professional ethos, and the institutional environment in which they operate” (Halberstam OHLP: 151). Scholars of comparative judicial politics should thus take care not to mischaracterize or write off analyses that give explanatory weight to legal and judicial (that is, “internal” (Chavez OHLP: 75)) variables. Not only is it incorrect to equate such analyses with traditional legal formalism or crude cultural arguments, but rejecting them may prevent us from identifying when and how legal rules and norms actually matter, or when they matter more or less and why.

This brings me to my third and final observation, which is that there appears to be an unfortunate tendency among scholars of comparative law and courts to define and portray the “political” in narrow instrumental (or “realist”) terms. Because of the strong influence of rational choice approaches in the discipline, much theorizing on judicial empowerment and judicial behavior has proceeded on several core assumptions. The first is that interests are the driving force behind political decision making, and that interests are independent of ideas, which are simply tools that political actors invoke, ignore, or attack in order to advance their pre-existing interests. The second is that the primary interest of political actors, be they dressed in suits, robes, or uniforms, is the maximization of their personal, partisan, or institutional power. And the third is that outcomes, such as the establishment or maintenance of judicial independence (Chavez OHLP; Vanberg OHLP), or the expansion of the judicial role (Ginsburg OHLP; Hirschl OHLP) are the product of coldly rational and strategic calculations to advance this primary interest.

To be sure, arguments based on these assumptions are a welcome corrective to overly functionalist or naively idealistic accounts that leave power considerations out of the picture. I do not wish to detract from or dismiss the very valuable contributions to the literature that have been based on these assumptions. However, in their zeal to “bring the politics back in,” comparative judicial theorists have tended to go too far in the opposite direction, reducing everything to “exogenously specified” (Ginsburg OHLP: 91) raw power calculations. Identities, norms, and social aspirations and aversions (Scheppelle 2003) have been treated as (at best) interesting but politically, and hence causally, irrelevant. Yet an increasing number of “microstudies” (Ginsburg OHLP: 93) reveal that the dichotomy between ideas and interests

in judicial politics is a false one, and that arguments based on rational-strategic/realist assumptions are unable to account for the timing, nature, and outcome of the establishment and exercise of judicial independence and the rule of law. The aforementioned volume by Halliday, Karpik, Feeley (2007) contains several chapters, including one by Ginsburg, that highlight the importance of ideas, and the mechanisms through which ideas are transmitted and implemented in fights for political liberalism. And in a collective work in progress, Patricia Woods and I are bringing together analyses of judicial empowerment in seven countries that highlight the ways in which ideational factors, social processes, and historical trajectories drive the way that political actors, in and out of the courts, perceive their interests and construct their strategies, and, thereby, shape the timing and nature of outcomes.

Ultimately, what we should strive for in comparative judicial studies is to transcend the old, stale binary of law qua naïve idealism and politics qua pure instrumentalism and get on with analyses that treat both law and politics more generously. We should allow for the possibility (but never assume) that law limits the discretion of decision makers, and never assume (even if the possibility is much greater) that discretionary decision makers seek, whenever possible, to avoid or subvert law. In her chapter in the OHLP on the European Court of Justice (ECJ), Karen Alter describes how a debate previously polarized between a “heroic” legalist narrative about the ECJ role in European integration and a more hard-nosed political account casting the ECJ as a mere “agent” of individual states has given way to a “view that both legal and political considerations influence ECJ jurisprudence.” The convergence around what she calls a “comparative politics narrative,” which treats ECJ decision making as only one step in a more complex chain, represents in her view “the maturation of European law scholarship” (Alter OHLP: 217). It is my hope that work on comparative law and courts can and will undergo a similar maturation, the hallmark of which would be a recognition that while there may be politics in all decisions interpreting and applying law, this does not necessarily render law irrelevant, and, moreover, that while all law involves politics, only certain kinds of politics involve law. Pursuing research that seeks to identify the conditions under which law matters in public decision making (by judges or other actors), how it matters, and when it ceases to matter across time and space would allow us to claim the title of true comparative “law and politics” scholars.

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Law, Politics, and Political Science

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The Oxford Handbook of Law and Politics is a fabulous collection of essays. Its forty-five chapters include something for everyone in the law-and-courts community. In fact, despite my repeated efforts to prepare for this essay by focusing on the chapters most relevant to my own research on the Supreme Court and American constitutionalism, I kept getting distracted by excellent chapters on comparative and international law, particularly the contributions by Kim Lane Scheppele (on extralegal emergencies), Karen Alter (on the European Court of Justice), Tom Ginsburg (on the global spread of judicial review), and Ran Hirschl (on the worldwide judicialization of politics).

Other readers will rightly praise different aspects of the collection, but I would like to emphasize several chapters that, taken together, make a strong case for the independent significance of law and legal institutions in shaping American politics. Distilling the key contributions of a wide range of literatures, these chapters make clear, first, that courts play an influential role in policy change and political development, and second, that their performance of this role is shaped in important ways by distinctive legal norms and commitments. Put another way, these chapters emphasize the multiple ways in which law influences the decisions of judges, the actions of other lawmakers, the outcomes of policy processes, and the trajectory of political conflicts. This research paradigm has the distinct advantage, from my perspective, of accurately characterizing the significance of legal phenomena that have sometimes been neglected or misunderstood. In addition to its substantive merits, this research agenda has the potential to encourage greater attention from the broader discipline of political science to legal ideas, legal actors, and legal institutions. But this potential will only be realized if the broader discipline is aware of this research, and on this count, the Oxford Handbook series is a mixed bag.

One of the great strengths of the law and politics handbook is its broad representation of each of the three principal paradigms of empirical research in contemporary political science: large-*n* behaviorism, rational choice modeling, and historical institutionalism. After an opening essay by the editors, a section on “Approaches” presents three chapters, each of which surveys one of these three paradigms as it has been deployed in the field of law and courts. And in the forty-one more narrowly focused chapters that follow, each of the three paradigms shows up repeatedly. But in the other volumes of the Oxford Handbook series—many, if not all, of which cover territory that is of interest to (some) law-and-courts scholars, the subfield has been less than fully represented.

Consider, first, several chapters from the law and politics volume. In “Historical Institutionalism and the Study of Law”—one of the broad paradigm chapters that opens the book—Rogers Smith briefly surveys the disciplinary origins of, and recent theoretical advances in, the historical institutionalist wing of political science and the distinctive ways in which law-and-courts scholars have made use of this approach. Smith makes a strong pitch for continued attention to the distinctive role of legal ideas in shaping judicial behavior in particular and political development more broadly. Among other things, he notes, the failure to attend to such relatively autonomous “normative orders” risks “undue minimization of the role of courts in politics” (p. 55). In a closely related chapter on “Law and Political Ideologies,” Julie Novkov examines “the relationship between law and ideas in the historical process of constitutional and legal change” (p. 627). Drawing on recent historical institutionalist accounts of several noteworthy lines of nineteenth- and twentieth-century constitutional development, Novkov argues that extant political “ideology[ies] shape[] the way courts make decisions, providing boundaries or frameworks within which decisions are made.” In addition, with the causal arrows running the other direction, she emphasizes that “the language of judicial decisions themselves can generate ideological change” (p. 628).

In a chapter on “Courts and the Politics of Partisan Coalitions,” Howard Gillman surveys the contemporary literature on regime politics and the courts. The primary theme of this literature is that governing coalitions use judicial institutions to promote their own partisan and policy ends, but as Gillman makes clear, the distinctive features of courts shape both the nature and degree of the coalitions’ success in this regard. Indeed, the regime politics literature has documented a variety

of “circumstances or conditions that would allow judges to promote unexpected agendas”—for example, when they are addressing an issue that “central decision-makers in the regime care little about,” one on which regime leaders are genuinely divided, or one on which “judges [have] arrive[d] at some independence by virtue of internalizing their legal training and taking seriously the distinctive institutional ‘mission’ of the judiciary” (pp. 656-7). Mark Graber explores similar themes in “Constitutional Law and American Politics,” emphasizing the multifarious ways in which constitutional norms structure political and legal conflicts, without dictating the specific outcome of those conflicts. Citing a range of contemporary literature on constitutional development, Graber notes that “[i]deology often explains the differences between persons engaged in a constitutional debate, but law usually explains the debate they are having” (p. 310). He argues that judges and legislators alike “act on the basis of constitutional visions that structure their policy choices” (p. 317) and that “institutional affiliations influence constitutional decision-making” by fostering particular constitutional visions among judges that are less prevalent among legislators (and vice versa) (pp. 314-15). He ends by calling for greater scholarly attention to the nature and value of the distinctive constitutional principles that tend to be disproportionately held by judges.

In addition to the Smith-Gillman-Graber-Novkov school of constitutional development, the scholarly tradition of legal mobilization is also well represented in the volume. Among political scientists, this interdisciplinary tradition is associated most closely with Michael McCann, and his chapter provides a wonderfully clear and concise survey of its 35-year history. Among the many lessons of this research, McCann notes, is the observation that ordinary citizens, left to their own devices, “rarely mobilize law,” usually responding to legally cognizable injuries by deciding to “lump it” rather than to litigate (p. 531). But despite this widespread and realistic sense that the formal legal system is stacked against ordinary folks, legal mobilization scholars have repeatedly found circumstances in which “subaltern groups and relatively powerless citizens [have the capacity] to mobilize the law against more powerful groups and status quo relations,” particularly when the ordinary citizens are effectively mobilized by organized interest groups (p. 525). Surveying this same line of research in a separate chapter on “Law as an Instrument of Social Reform,” Charles Epp notes that legal mobilization scholars have often shown that “judicial decisions (and law more broadly) [are] potentially effective in contributing to long-term social changes” (p. 603). As a result, “court-structured law may have a broader social-reform impact than once thought” (p. 597). Exploring some related themes, Scott Barclay and Susan Silbey mine the literature on legal consciousness to develop an original account of the impact of legal mobilization in prompting otherwise unresponsive legislative institutions to enact policies supported by the public. Most of the scholars cited by McCann, Epp, Barclay and Silbey would identify themselves as “law and society” scholars rather than historical institutionalists, but the two communities share overlapping memberships and a number of methodological and substantive commitments, and McCann ends his chapter by calling for greater integration between the two approaches (p. 535).

All of the chapters I have mentioned here assert the broad political significance of legal ideas, legal actors, and legal institutions. Graber argues that “[c]onstitutions and constitutional law consistently secure agreements where agreements would otherwise not exist” (p. 305). Smith notes that judges often have “some relative autonomy from broader political forces, indeed real power to affect political results” (p. 48). McCann insists that “legal mobilization is one of the most important but least studied models of citizen participation in the U.S. political system and perhaps around the world” (p. 527). And Hirschl suggests that “[t]he judicialization of politics—the reliance on courts and judicial means for addressing core moral predicaments, public policy questions, and political controversies—is arguably one of the most significant phenomena of late twentieth and early twenty-first century government” (p. 199). If these claims are right, then the literatures surveyed in these chapters are relevant not just to the law-and-courts community but to the broader discipline of political science. Gillman makes this point most explicitly, opening his chapter by noting that “[o]ne of the most promising ways of integrating the study of law and courts into the mainstream of political analysis is to focus more attention on how legal institutions figure into the broader interests and agendas of other power holders,” and closing it by calling for “scholars across various fields . . . to reintegrate courts into the broader analysis of political systems” (pp. 644, 658).

Unfortunately, the law and politics handbook is likely to draw attention only from specialists, and law-and-courts research is not well-represented in the other volumes in the Oxford Handbook series. As summarized on Oxford’s website, *The Oxford Handbooks of Political Science* is a ten-volume series that purports to serve as “the essential guide to the state of political science today.” Readers of *Law and Courts* should all be pleased that one of these ten volumes has been devoted to law and politics, and that the editors of this volume have so ably assembled a collection that captures the full range of the law-and-courts subfield. But we might well be concerned about the representation of the subfield in the

other nine volumes.

The handbooks of political behavior, public policy, and contextual political analysis have no entries from the field of law and courts. The handbook of comparative politics has one chapter (out of 38 total) on “Comparative Judicial Politics”; the handbook of international relations has one (out of 44) on “International Law”; and the handbook of political methodology has, by my count, just one chapter (out of 37) that is authored by a law and courts scholar. The other volumes do somewhat better in covering our subfield, but not much. The handbook of political economy has one chapter on “The Judiciary,” as well as several chapters on constitutional theory. The handbook of political theory has an entry on “Constitutionalism and the Rule of Law” as well as several chapters on topics that are related to the field of law and courts, including rights, liberty, and equality. And the handbook of political institutions has two chapters on constitutions and two others on judicial institutions. It is hard to say whether these numbers represent an adequate allocation. Given the ambitious scope of these volumes, their editors faced a daunting task, and I’m confident that every member of the discipline could identify important topics that were left under-examined. More important than the number of chapters devoted to law and courts, however, is the limited range of research that these chapters have covered.

Consider *The Oxford Handbook of Political Institutions*, published in 2006. The editors of this volume present its 38 chapters as a collective effort to survey “the origins, evolution, and impact of institutions on politics and policy alike” (p. xvi), but the research paradigm I have surveyed here—the large body of literature relying principally on interpretive and historical methods to document the independent political significance of law and legal institutions—goes almost entirely unmentioned in the volume.

In a chapter on “Historical Institutionalism,” Elizabeth Sanders includes one brief (and favorable) reference to contemporary studies of constitutional development, citing Ken Kersch’s award-winning *Constructing Civil Liberties*. In a chapter on “Analyzing Constitutions,” Peter M. Shane likewise offers praise for contemporary historical institutionalists who seek “to show how the attitudes of legal actors, especially judges, are shaped not only by individual preference, but also by the institutions through which these actors operate and the relationship of those institutions to others” (2006, 192). But Sanders focuses almost entirely on historical institutionalist studies of legislative and executive institutions (and the social movements that have shaped them) and Shane focuses almost entirely on conventional legal scholarship. Neither devotes more than a paragraph to historical institutionalist studies of constitutional development. Josep M. Colomer’s chapter on “Comparative Constitutions” does not mention such work at all.

Two chapters on judicial institutions devote sustained attention to the central questions that are present throughout the law and politics volume, but they too largely ignore the research traditions that I have emphasized here. In a chapter on “The Judicial Process and Public Policy,” Kevin McGuire (2006, 536) examines more or less the same question as Epp—“Are judges capable of actually producing changes within society?”—but presents a strikingly different picture of the state of contemporary research on this question. Relying extensively on Donald Horowitz’s 1977 *The Courts and Social Policy* and Gerald Rosenberg’s 1991 *The Hollow Hope*, McGuire emphasizes the limited ability of courts to implement their decisions. On his account, “[t]he very nature of adjudication . . . serves as a serious limitation on the extent to which courts can generate meaningful legal change” (2006, 539). These constraints include the fact that “adjudication tends to focus on a limited range of policy alternatives”—that is, those presented by the two litigants—and that judges are generally presented with “only limited amounts of information upon which to base decisions.” To make matters worse, judges “have little capacity to summon additional information” that might help, and they have “no formal mechanism by which [they] can examine the ongoing impact of their policies.” In addition, “policy-making through adjudication requires that judges be presented with a genuine legal controversy that plainly presents the issues that judges wish to address. Stated differently, courts do not speak until spoken to” (2006, 540-2).

As is common with this sort of judicial impact scholarship, McGuire contrasts this cramped reading of judicial capacity with an expansive vision of legislative capacity: “When Congress seeks to develop new policies in telecommunications or agriculture or foreign policy, it gathers information, conducts committee hearings, and considers testimony for various affected interests.” Unlike judges, elected legislators “routine[ly] . . . seek to gather as much information and analysis as they deem useful on the impact of various policy alternatives.” They are “free to consider what policies they regard as most sensible, even if those policies constitute major departures from the status quo.” They commonly define the terms of art used in their policies, thereby “reduc[ing] ambiguity and allow[ing] for a common understanding of the meaning of policy enactments,” and they “need nothing beyond their own initiative to stimulate policy change. They may promote

reform whenever they see fit” (2006, 540-1). As McGuire himself acknowledges at the end of the chapter, this is an overly romantic account of the legislative process in the U.S. Congress (2006, 550). As a result, McGuire’s conclusion that “courts require considerable cooperation and support from other actors as a condition for effective policy-making” is not wrong, but neither does it distinguish courts from other institutions (2006, 546).

Toward the end of the chapter, McGuire notes in passing “that the role of courts around the world is . . . expanding, with judges assuming an ever increasing scope of influence” (2006, 550), and James Gibson opens his chapter on “Judicial Institutions” by noting that “[l]egal institutions throughout the world have become increasingly powerful” (2006, 514). Gibson closes with an even stronger assertion that “[l]aw and courts are not marginal to politics; they are central, and this is increasingly being understood by the entire discipline of political science” (2006, 531). But neither McGuire nor Gibson offer any examination of the mechanisms of judicial influence on policy and politics. The central thrust of McGuire’s chapter is to show that judges face daunting “institutional constraints that limit their policy ambitions” (2006, 536), and Gibson’s chapter focuses primarily on the question of judicial decision-making. On Gibson’s account, such decision-making is primarily a function of judges’ ideological preferences, tempered by their views of appropriate judicial behavior and by their understandings of other significant actors’ views about appropriate judicial behavior. He notes that “the single-minded pursuit of policy goals may on occasion threaten the legitimacy of a court, and therefore judges will act to protect the institution rather than maximize policy preferences” (2006, 522).

Despite its clear relevance to this argument, Gibson does not engage with the Smith-Gillman-Graber-Novkov line of research, apparently because of a fundamental methodological divide. (He notes that “the empirical question of how . . . beliefs about proper behavior influence actual decision-making can only be resolved through careful empirical analysis, based mainly on positivist methods” (2006, 521).) Likewise, the authors of the one chapter on law and courts in *The Oxford Handbook of Comparative Politics*—John Ferejohn, Frances Rosenbluth, and Charles Shipan—attend almost solely to research using formal or statistical methods, despite the clear relevance of a variety of interpretive and historical works to their central thesis that political fragmentation increases judicial independence. This methodological divide—which James Mahoney and Gary Goertz (2006) have characterized as a tale of two distinct cultures—shows up in the law and politics handbook as well, with chapters by Jeffrey Segal, Lee Epstein, and others dismissing certain qualitative and interpretive lines of research out of hand.

One of the great virtues of the volume, as I noted at the outset, is the extent to which its forty-five essays collectively represent the diverse range of research that makes up the subfield. In addition to the constitutional development and legal mobilization scholarship that I have emphasized here, the research summarized elsewhere by McGuire, Gibson, Ferejohn, Rosenbluth, and Shipan is also well represented in the law and politics handbook, as well it should be. But the research summarized by Smith, Novkov, Gillman, Graber, McCann, Epp, Barclay, and Silbey is not well represented in the other handbooks. In short, the editors of *The Oxford Handbook of Law and Politics* have done an admirable job of surveying, summarizing, and (by implication) praising the methodological pluralism of the subfield, but the message may not be getting out.

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Interdisciplinarity in Legal Scholarship

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It is surely a Sisyphean task to identify and elucidate the various strands of scholarship that fall under the rubric of law and politics. The research that can be reasonably gathered under this organizational heading continues to proliferate, producing an ever more diverse literature such that there will undoubtedly be many “new” areas that could merit their own chapters in any future edition of *The Oxford Handbook of Law and Politics*. The kind of synthesis – or, more accurately, syntheses – represented by the chapters in *The Oxford Handbook* reflect the careful thinking and writing of a collection of the finest scholars working in the law and politics métier. Of special interest to me is what the entries in this book – individually and collectively – tell us about the interdisciplinary nature of law and politics scholarship.

There is certainly an allure to the term “interdisciplinary.” It sounds intellectually weighty and broad-minded. (And, who wouldn’t want to be considered broad-minded?) What exactly constitutes interdisciplinary scholarship, however, is rarely made clear in discussions about the relative merits of such work compared to work that is grounded in a particular discipline (such as political science).¹ As a general description, interdisciplinary research might be characterized as scholarship that draws from two or more disciplines to examine a phenomenon of interest. To be truly interdisciplinary, presumably the work must integrate the theories and/or tools of each of the relevant disciplines. In the absence of a true synthesis, the work might be described as multidisciplinary but surely not as interdisciplinary. But “if there is too much overlap in examined questions and methodology, then the multiple disciplinary perspectives become redundant and no new insights can be gained from the exchange” (Dau-Schmidt 2004: 200). In other words, valuable interdisciplinary work will integrate complementary but distinct aspects of two or more disciplines.

A hidden-in-plain sight complication is determining what constitutes a separate discipline. Exactly how distinct must the two or more disciplines (from which the purported interdisciplinary scholarship is drawing) be for the research in question to be considered interdisciplinary and not intradisciplinary?² On its face, a collaboration between an artist and a biologist on the form and function of bird wings may be considered interdisciplinary.³ But, for example, are social psychology and psychological sociology separate fields or simply subfields of social psychology (Sewell 1989)? No doubt those who labor in the field of social psychology have strong opinions on this and, not being either a social psychologist or a psychological sociologist, I would defer to them on this point. But such thoughts do lead me to wonder if, indeed, law is a discipline separate from other disciplines that include the study of law, legal processes, legal actors, etc.

Even a casual reader of the chapters on law and economics (Kornhauser), law and psychology (Tyler), and law and history (Tomlins) – all of which appear in Part IX “Interdisciplinary Approaches to Law and Politics” – will come away with some sense of what might reasonably be seen as an economic or psychological or historical approach to the study of some aspect or aspects of law. But we can consider these interdisciplinary approaches only by assuming that law is a separate discipline.⁴ Setting aside the study of and training in the actual practice of law, however, I respectfully submit that law is not a separate discipline unto itself. There is no unique theoretical focus or particular methodological tool that demarcates law from other disciplines.⁵ And scholarship that examines law through the lens of only one discipline – no matter how finely crafted and insightful – cannot be properly understood as interdisciplinary.⁶

When I began work on this short contribution to the symposium devoted to *The Oxford Handbook of Law and Politics*, I initially thought I would be writing a piece that extolled the virtues and contributions of interdisciplinary legal scholarship, rife with examples drawn from the extant literature. While I am quite convinced that interdisciplinary work (if executed well) is virtuous and that it can make important contributions, I have come to the conclusion that only a small proportion of the work devoted to the law and legal phenomenon that is commonly labeled as interdisciplinary truly is so.⁷ The more I read purported examples of interdisciplinary work, the more I found that the work was really examining the law from one or another disciplinary basis. And it was difficult to discern what was uniquely from the “discipline of law.” There is, of course, interdisciplinary legal work;⁸ just not as much of it as I originally thought there was. This is a pity since, as Scheingold asserts, “In the academic world, disciplines are very decidedly the ties that bind – and often that blind” (2008: 2). And that is exactly why the paucity of real interdisciplinary legal scholarship is unfortunate.

As Dau-Schmidt observes:

In making abstractions from reality that allow insight into a problem, all social scientists need to be mindful of the limitations of their analysis and to adapt that analysis when the essential features of the problem exceed the capacity of their model. Talking to scholars in other disciplines, or reading their work, is one of the best ways to learn about the limitations of your own analysis and to find ways to address the problem (2004: 205).

Interdisciplinary work, if done well, has the very practical and salutary effects of improving the quality of our theorizing and enhancing the models we construct, thereby advancing the state of our knowledge. Encouraging it, then, is a good thing.

Beneficent effects notwithstanding, interdisciplinary work is notoriously difficult to sustain as an intellectual program over the long term (see, e.g., Harty and Modell 1991; Sewell 1989).⁹ Others have catalogued a plethora of reasons for this (e.g., Scheingold 2008; Sewell 1989). These reasons include professional rivalries, with members of various subfields jealously guarding their turf. Institutional organization also matters, as most universities are structured primarily as collections of separate disciplines.¹⁰ And, of course, there is the simple matter of developing the necessary human capital.¹¹ It is not enough to merely bring together a group of people from different disciplines who share common interests. There must be a willingness on the part of scholars not only to bring their vocabulary, theories, and methods to the table but also to consume the vocabulary, theories, and methods brought to the table by scholars from other disciplines. As Kramer asserts, interdisciplinary research “is perfectly possible, but one cannot get it just by mixing the different people [from different disciplines] together. One must mix the disciplines together in one human brain, so to speak” (1959: 565). And this is not an easy thing to do since the academic reward structure is not a clean fit with the structure of interdisciplinary research. For example, with notable exceptions such as *Law & Society Review*, most journals are discipline specific. But the difficulty of fostering a sustained interdisciplinary research agenda is not a reason to avoid making the effort. Doing so successfully necessitates, I suspect, being quite a bit clearer about what we mean by interdisciplinary legal research.

There is one additional point that *The Oxford Handbook of Law and Politics* brought to mind and that I think is worth mentioning here. Political scientists studying law and legal processes are often wont to don a hair shirt and cilice regarding our failure to make our scholarship relevant to those outside of our “narrow” little field. But even a casual perusal of the chapters in this book should make two things clear. First, the field of law and politics is anything but narrow. There may be, as Martin Shapiro argues in his contribution to this volume, a disproportionate emphasis on law and courts in the law and politics field. But the breadth of actors, institutions, products, and processes examined by law and politics scholars is astounding. Second, much of the law and politics research is self-evidently relevant to those who identify themselves as legislative specialists, interest group analysts, social movement scholars, etc. If it is true, as it undoubtedly is, that law and politics scholars could do more to make this relevance clear, their duty to do this is no greater than the duty of scholars in other subfields of political science to make the relevance of their scholarship clear to others (including to law and politics scholars). And it is far from self-evident that law and politics scholars currently do any better or worse at this than non-law and politics scholars. And, so, wearing a hair shirt or a cilice rather than a hair shirt and calice might well suffice.

In one of the last chapters in this volume, Stuart Scheingold recalls his days as a graduate student at Berkeley as a means of communicating a sense of what “public law” entailed at that time. Scheingold reports thinking about “public law” then as “that forbiddingly opaque and never elucidated designation” (739). No doubt many graduate students in many fields still struggle with understanding the nature and substance of the intellectual endeavor that characterizes their respective disciplines. Students of law and courts now have an advantage in this regard. Though it may not be able to elucidate the designation of “law and politics” to everyone’s satisfaction, *The Oxford Handbook of Law and Politics* does much to make the study of law and politics a great deal less opaque.

Notes

¹ A notable exception is Lynn Mather’s contribution to this volume regarding the law and society field.

² This is a lot like asking how substantial the effect of purely intrastate commerce must be on interstate commerce for its regulation to fall under federal Commerce Clause authority.

³ Or perhaps not if they collaborate on the collection of bird photographs but the artist goes on to use them for an art installation and the biologist goes on to use them to create a forensic record. There is scant synthesis there.

⁴ Mather's chapter is a bit different in this regard. As Mather notes, "the law and society field lacks clear boundaries to separate its interdisciplinary perspective from the other disciplines" (692) but the work that she cites as examples of law and society scholarship all draw on two or more distinct disciplines, making law and society research perhaps some of the most truly interdisciplinary work of all the scholarship that claims that title.

⁵ Tomlins (2000) offers a diametrically opposed view that is worth reading for both the substance of his argument and the intellectual history it provides. See, also, Martin Shapiro's contribution to *The Oxford Handbook*.

⁶ This is akin to referring to work that examines the political institutions or behavior in one country, as long as it is not the United States, as comparative politics scholarship.

⁷ Interestingly, some of the work that is really interdisciplinary, such as economic analyses of the political decision making of judges that takes into account the cognitive limitations of decision makers, is only rarely referred to as interdisciplinary.

⁸ An excellent example is the line of research that draws from both economic theory and psychology (behavioral economics) to understand the decision making of juries.

⁹ This does not mean that it is impossible, as the longevity and robustness of the law and society field attests (Scheingold 2008).

¹⁰ Interdisciplinary programs abound but they remain a minority in a sea of discipline-based departments.

¹¹ Another difficulty sometimes identified is the lack of financial support for interdisciplinary work. That difficulty is substantially ameliorated by the Law and Social Science Program of the National Science Foundation since this program's mission explicitly includes fostering interdisciplinary – or, at least, multidisciplinary – research on the law.

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Shaping the Field? A Review of *The Oxford Handbook of Law and Politics*

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I strongly recommend purchasing *The Oxford Handbook of Law and Politics*, edited by Keith Whittington, Daniel Kelemen and Gregory Caldeira. For me, the handbook has already been a useful guide to less familiar literatures and a fine summary of those in which I write. If this is a fair snapshot of our field, we have a lot to be excited about. The essays the editors have amassed not only review a solid base of knowledge but they also identify new puzzles to pursue and possibilities for collaboration. Before I address the material directly, however, I thought I might say a word or two about process.

The key question is this. On what grounds does one review an Oxford Handbook? I have struggled with this question for more time than I care to admit. It is not a summary of a particular study or a proposal for future research, so the normal guidelines for manuscript review do not necessarily apply. An edited volume presents similar troubles, but at least it is usually a collection of research papers, which gives you the opportunity to review a literature in light of the volume's theme. A handbook, in contrast, is a series of review essays. So, what we are talking about here is reviewing a collection of reviews. How this should be done was not immediately obvious to me. After burning through a couple of legal pads and my colleagues' patience, I decided to lean a little bit on the volume's fine editors. Rather than invent a standard, which frankly was not coming to me, I decided to evaluate *The Oxford Handbook of Law and Politics* according to their goals.

The overarching goal of the Oxford series, stated succinctly on the book jacket, is "to shape the discipline [of political science]." The more modest goals of the volume editors are twofold. As we would expect, the editors hope to offer a useful conceptual map of the field. They define law and politics as the political analysis of law and courts. The goal of law and politics is to see what leverage we can gain in our understanding of law and legal institutions from models that are explicitly political (p. 4). With this frame in mind, the editors construct a reasonable partition of literatures, which draws on existing scholarly labels: Jurisprudence and the Philosophy of Law, Constitutional Law, Politics and Theory; Judicial Politics; Law and Society; and, Comparative and International Law and Courts. The review essays do not perfectly follow the map they develop; but there is wide coverage, and there are plenty of new ideas to consider.

The real success of the volume, in my view, lies in how the review essays might spark new inquiry. This is how disciplines are shaped. Toward this end, the editors' second goal is to start productive conversations among scholars in the various subfields. Although the essay writers do not really engage each other directly, I think the best way to view the handbook is as an invitation to readers to flesh out implicit conversations in the essays. I divide the rest of this note as follows. In the next section, I raise two areas of common ground in law and courts on which scholars might engage each other across the subfields. Admittedly, my training in comparative political institutions limits the dialogues that I see. I have targeted issues that influence, in the broadest sense, concerns over the rule of law. No doubt, I will have missed many important points of concern. Yet if the number of unresolved issues that I see in the literatures that I know relatively well is a reflection of these issues in the general field, I expect that scholars outside comparative politics will find much to stimulate them. In the conclusion, I return to the editor's first goal, the conceptual map, and consider it with respect to Oxford's interest in shaping the discipline. While I appreciate the editor's framework, the field definition risks failing to articulate the importance of law and politics scholarship for political science, and the volume's organization reinforces this issue. It is not that big implications are omitted, but rather that they could be highlighted better.

Avenues of Engagement

Some of the most intellectually stimulating moments on the job occur at faculty workshops or job talks where the research subject is outside your area. Learning that someone else thinks about a particular research problem in roughly the same way as you think about an analogous yet distinct research problem is exciting and reassuring. But it is even better

when you see someone work through a problem in a way that provides material assistance to your own struggles. In large part, the handbook serves this purpose. I am anxious to get to work on my own research in light of what I have read from other scholars. I am also excited about the opportunity to reach out to people outside my subfield in an effort to collaboratively advance shared research questions. In what follows, I suggest two possibilities for collaboration.

Procedural Fairness and Compliance

One avenue for fruitful inter-subfield engagement involves placing the law and psychology literature summarized by Tom Tyler in direct dialogue with the literatures on the rule of law and international law, summarized by Rebecca Chavez and Beth Simmons, respectively. The most conspicuous issue involves compliance. The approach Tyler describes affords significant causal weight to the internalization of beliefs in the legitimacy of authorities, the basis of which lies in perceptions of procedural fairness. We obey the law not when we believe compliance is in our immediate or even long-term interests, but because we believe that the procedures authorities use are essentially fair. This perception induces a belief in the legitimacy of those authorities, which compels compliance as an appropriate moral choice.

The concept of compliance is essential to the rule of law. There can be no meaningful legal constraints on the state, no real judicial remedies for illegal state actions, if political figures refuse to comply with unfavorable decisions. Yet, the models Chavez summarizes largely ignore the procedural fairness approach to compliance in psychology. Instead, causal mechanisms turn on expectations about substantive outcomes. Like much of the rule of law literature, popular accounts of compliance in international relations draw on rationalist models of state behavior, which do not make use of the legitimacy concept (e.g. Morrow 2002; Simmons 2002). On the other hand, a rich constructivist tradition in international relations suggests that compliance flows from an internalization of norms of system legitimacy. Nevertheless, it is not clear that the procedural fairness mechanism has penetrated constructivist theories of compliance. So, there is reason to believe that a fruitful conversation awaits.

Consider the rule of law literature first. A preliminary step will involve thinking through an obvious conceptual difference between its research subjects and those in law and psychology. Whereas the psychology literature deals with mass compliance in society, the rule of law literature is concerned mostly with compliance among elites, power holders in particular. We might ask whether this difference in research subjects is material. Can we gain leverage in understanding elite compliance by appealing to models of procedural fairness? What special problems emerge if we do? For example, is it the perceptions of procedural fairness among democratic elites that matters, or should we be focusing on beliefs in the electorate? The latter approach is broadly consistent with the public support models of judicial power summarized in Vanberg's contribution, but perhaps we should be focusing on elites themselves.

A dialogue between the international relations literature and the literature on law and psychology confronts the same research subject problem. If that problem could be resolved satisfactorily, we might suspect that the constructivist literature would be a natural place to begin the conversation. The most exciting possibility is that the procedural fairness approach could give more precise shape to the process by which international norms are internalized. Rather than focusing on the special substantive qualities of the legal norms with which states are supposed to comply (as in Hawkins 2004 or Keck and Sikkink 1998), perhaps the focus should be on the process by which these norms are adopted and/or subsequently vindicated.

Turning our attention to the law and psychology literature itself, we might wonder if the rule of law models Chavez summarizes can inform the law and psychology understanding of compliance. It is one thing to know that beliefs about procedures drive compliance. It is quite another thing to know from where these beliefs in fair procedures derive. And it seems quite possible that perceptions could be tied to the interactions between courts and governments around which the rule of law literature revolves. At the very least, it seems plausible that individuals in a society characterized by high levels of institutionalized corruption might not believe that objectively fair procedures are genuinely fair.

Reconciling Models of Judicial Independence

Another subject of shared interest deals with questions of judicial empowerment and the subsequent use of independent judicial power. Why do politicians delegate political authority to judicial institutions? Why do courts exercise their

powers independently? These questions are front and center in the essays on judicial independence in law and comparative judicial politics, authored by Frank Cross and Georg Vanberg, and in Tom Ginsburg's essay on constitutional review. Also, the concept of judicial independence is a key element of the judicialization of politics story as told by Ran Hirschl. Even the story Beth Simmons tells about states' compliance with their international law obligations involves assumptions about judicial independence. The possibility for fruitful collaboration is obvious, and due in no small part to an existing interdisciplinary commitment to these research questions.

Over the past three decades, we have made considerable progress resolving key conceptual issues related to judicial independence. Definitional ambiguity was once severe, but we now largely have a shared understanding of judicial independence. Authors have one of two concepts in mind. They either wish to describe a world in which judges are free from undue interference in their decision-making process, so that they can be the "authors of their opinions" (Kornhauser 2002); or, they wish to describe a world in which judges are not only autonomous but able to definitively resolve policy conflicts – they are powerful (Cameron 2002). Further, we also largely agree that there is a tradeoff between judicial independence and judicial accountability. For this reason, more judicial independence is not always desirable. Beyond these conceptual issues, the field also has produced numerous theoretical explanations for the empowerment of courts and for their exercise of independent authority. In fact, so much ink has been devoted to judicial independence over the past three decades, one is inclined to call the subject closed and move on to issues that have received less attention. My reading of the handbook suggests that this would be a significant error.

There are a number of challenges to meet, but in the interests of space, I will focus on one. We are in serious need of real theoretical integration, as Vanberg suggests in his essay. Specifically, the empowerment stories are not easily reconciled with the models of independence. For example, consider the standard credible commitment argument for judicial empowerment. A powerful actor that is essentially unconstrained by competing political rivals empowers a court to solve its inability to credibly commit to respecting property (or other) rights. This argument must anticipate a future world in which the newly empowered court is independent in the Cameron sense. Otherwise, the court's formal empowerment would not induce the credibility for which the state is looking.

As Chavez and Vanberg note, the well-known political fragmentation argument about judicial independence suggests that independence increases in the number of veto players. In so far as the credible commitment story operates most persuasively in a single veto player world, the fragmentation argument suggests that the newly empowered court is unlikely to be powerful. If this is true, then the central logic of the credible commitment empowerment story is undermined. It is worth asking whether these two arguments can be reconciled. There are many other puzzles of this sort that emerge if we place the various arguments Vanberg summarizes up against each other. Until we sort out these puzzles effectively, I would not recommend moving on to different subjects. The good news is that we have numerous scholars who seem to be interested in the subject. For this reason, I am hopeful that the process of integration will be fruitful.

Law and Politics and Political Science

The primary goal of the Oxford series is to shape the discipline of political science. By launching new conversations among law and courts scholars, this handbook will serve that cause well. If I could change one thing about the volume, however, it would be this. I would have liked the editors to make a stronger case in the introduction for the critical role of law and courts scholarship in the larger discipline. Practitioners surely see multiple reasons why understanding law and courts is useful for explaining broader political phenomena, and many of those reasons are found in the volume's contributions. So it is not that the editors do not expose us to important implications. They do. The issue I am identifying is about where, when and how they emerge.

Consider the first three chapters. In the introduction, the editors suggest that in the field of law and politics, political models of human behavior help enlighten our understanding of law. In this sense, law and politics is analogous to law and economics or law and psychology. As Martin Shapiro notes in his delightful final essay on boundary problems in law and courts, the intellectual flow runs from economics to law and from psychology to law in those fields and not the other way around. So, if law and politics is like law and economics, then it is political science theory that is informing our understanding of law and not vice versa. I do not believe that law and politics is limited in this way or should be limited in this way. I doubt that the editors would disagree. Nevertheless, the impression suggests itself as one reads the first part of the handbook.

The second and third chapters, which review models of judicial decision-making, reinforce the “law and politics” as “law and economics” frame. The first, written by Jeffrey Segal, and the second, written by Pablo Spiller and Rafael Gely, provide clear and succinct summaries of their subjects. In a fundamental sense, they are excellent review essays. What they do not do, however, is articulate why it matters whether judges are guided by their role perception or their ideology or whether they are strategically prudent on occasion. If our field is really about law only, then I think it is perfectly defensible to develop good models of judicial decision-making and call it a day. But this is not what our field is about, and it is not why we model decision-making.

We want good models of judicial decision-making because we want to answer broad questions in political science. Our models of judicial behavior matter because they inform the answers we want to give to questions about whether law can produce social change, whether courts can help governing coalitions manage political instability, whether judges can help create conditions for order and economic development, and many other inquiries of major political relevance. These are big questions in our field and our scholarship is critical to answering them.

Later in his essay, Shapiro reminds us that the scope of law and politics can be and probably should be broader than what the editors’ definition might suggest. Indeed, he argues that most scholars who include themselves in the law and politics camp joined because they thought that their understanding of the law would help enlighten their understanding of politics. One is tempted to conclude that where Whittington, Kelemen and Caldeira see the political study of law, Shapiro sees a field in which this endeavor is paired with the legal study of politics.¹ I suspect that the editors would not quarrel with the broader view of the field, and certainly not with the relevance of law and politics to political science generally. Their own excellent research suggests otherwise. And as I say, the volume is brimming with big implications. Still, I would like to see them framed more clearly.

For me, the bottom line is this. I am more excited about our field than I was before reading the handbook. I am anxious to get back to work on the problems the handbook addresses. I would strongly recommend purchasing it and using it in your classes. I surely will.

Notes

¹ Shapiro sees to overlapping subfields of law and politics. One, law and politics, deals with political decision making that is constrained significantly by legal rules. This field might include studies of agency decision-making or detailed analysis of congressional statutes in addition to the more constrained areas of judicial behavior. The second, law and courts, largely deals with relatively judicial behavior, especially in the context of judicial lawmaking.

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Nastier, Noisier, Costlier — And Better: Why Letting Judges Speak Out During Political Campaigns Enhances Democracy And Serves Justice

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In the not-so-distant past, state-level judicial elections were decent, docile and dirt-cheap affairs, even if drab and dull, and scarcely deserving of being called democratic. Today, they are, as professor [Roy A. Schotland](#) of Georgetown Law Center says, nastier, noisier and costlier, meaning that attack ads have become more commonplace, campaign activity of every sort has increased dramatically and, consequently, the need for campaign contributions to finance candidates for judicial office has increased exponentially. Few observers believe that this trend toward increasingly politicized campaigns will abate anytime soon.

Three major developments have contributed to the growing politicization of judicial elections. First, state courts have become vastly more important within the American legal system. With the U.S. Supreme Court now deciding fewer than 100 cases per year, the "court of last resort" for many legal disputes is a state court. These courts are making extremely important decisions on matters ranging from massive, class-action torts to the use of the death penalty to the state's use of eminent domain. State courts have always been important in the American legal scheme; today, not only are they highly influential, but they are recognized as such by nearly all legal observers.

Because state-level courts are -- and are seen as -- important, interest groups have become vastly more involved with efforts to "reform" judicial selection and retention systems. Some groups are ideologically motivated, as in the Federalist Society's ongoing efforts to gain more influence for right-wing interests. Other groups, such as the [U.S. Chamber of Commerce](#), are primarily motivated by economic interests. And some groups, including left-leaning trial lawyers, are motivated by both ideology and economics. Direct interest-group involvement in judicial elections -- via, for instance, advertising in electoral campaigns -- is certainly a new feature of state judicial elections.

Added to this volatile political brew is the U.S. Supreme Court's 2002 decision in [Republican Party of Minnesota v. White](#). At one level, this decision is very simple: It declared that candidates for judicial office, including incumbent judges running for re-election, are covered by the [First Amendment to the U.S. Constitution](#). This simple declaration has myriad consequences, however. Once judicial candidates have free speech rights, it becomes vastly more difficult to regulate the content of their speeches, including speeches in which candidates announce their policy views on important legal issues. In the past, judicial candidates were largely forbidden from discussing anything remotely related to how they might rule on issues and cases that might come before their court in the future.

While the limits of judicial speech are still being litigated -- few believe, for instance, that judicial speech rights will be extended to such statements as "If elected, I will vote to overturn the multibillion-dollar judgment a trial court awarded to smokers in their case against the tobacco companies" -- at present, candidates for judicial office are free to announce their general views on important issues of legal policy. Constitutional protection now clearly protects such speech as "I believe the Second Amendment was designed to protect the right of individual citizens to bear arms" or "I believe that the emanations and penumbras of the Bill of Rights establish a right to privacy, a right that extends to private sexual behavior and having an abortion."

In short, judicial elections have become much more like other state elections. Candidates increasingly campaign on the basis of their policy views; they seek campaign contributions from citizens and groups to get their messages across to the voters; and they produce tawdry advertisements in which they attack the record and/or integrity of their opponents. Judicial elections have become ... elections.

What consequences flow from this new style of judicial election? Based on research I conducted in 2007, I contend that

voters are emphatically not put off by policy talk from judicial candidates. Many legal scholars, judges and interest groups, however, argue quite the contrary .

Indeed, these developments have set off a flurry of complaints and concerns emanating from a variety of legal actors and groups, including [Sandra Day O'Connor](#), the former Supreme Court justice who cast the deciding vote extending speech rights to judicial candidates (and who also cast the deciding vote in the 2000 presidential election -- in [Bush v. Gore](#)). These observers believe that politicized judicial campaigns pose a serious, if not mortal, threat to the legitimacy of state judicial elections.

Those who fear that increased politicization of judicial elections threatens the legitimacy of the courts argue something like this:

Courts are inherently weak political institutions, famously lacking the power of the purse and the sword. Because courts cannot tax and spend, as legislatures can, they cannot buy the support of their constituents. Because they do not command the coercive state apparatus (i.e., the police and the military), they cannot mobilize force to ensure compliance with their decisions. All political institutions face the difficulty of getting citizens and organizations to comply with their decisions. But of all institutions, courts are the most vulnerable; judicial power is the least powerful form of power.

Because courts are weak, they require institutional legitimacy, the belief that an institution has the right to make binding decisions for a constituency and that such decisions must be complied with. Legal observers from the framers of the American Constitution onward have extolled the necessity of courts having a store of legitimacy. In many respects, legitimacy is more efficacious than purses and swords because legitimacy provides a standing presumption in favor of compliance. At the same time, however, legitimacy is far from automatic; it is contingent, and it is fragile.

A key source of legitimacy in the American judiciary, so the argument continues, is the perception of judicial impartiality. Because citizens view courts and judges as disinterested and principled decision makers, their decisions are generally accepted as legitimate. Earmarked legislation passed by Congress is perceived as fair and impartial by practically no one (except perhaps the direct beneficiaries of this duplicitous largess). Judicial decisions are different. Because judges have no stake in the outcome, they are free to decide legal issues on the merits of the case, not on the politics of the litigants, and because the decisions are principled and disinterested, they are legitimate.

The direction of this argument is now undoubtedly obvious: Politicized judicial campaigns are thought by many to impugn judicial impartiality, thereby undermining the bedrock of legitimacy, making compliance with judicial decisions less likely and more costly, and even threatening the very existence of this third branch of government. The pathway from judicial speech rights to the destruction of the judiciary is a long and tortured one, to be sure, but many see American state courts as traveling headlong down this road to ruin.

The above forecast of judicial woe and despair turns on a variety of crucial empirical assertions, the first and simplest of which is that policy pronouncements by candidates for judicial office are offensive to the American people because they view them as indicative of a loss of impartiality on the part of the judge. This is the argument of the state of Minnesota, which sought to ban policy speech by judicial candidates, and it is the argument of the dissenters in *Republican Party of Minnesota v. White* as well.

Is policy talk by candidates for judicial office off-putting to the American people? Remarkably little rigorous empirical research has addressed this issue, so it is perhaps useful to begin by sketching a logic by which such policy talk is *not* offensive.

Assume for a moment that people view state courts of last resort primarily as responsible for not just implementing but *making* legal policy (the view, by the way, shared by virtually every political scientist in the land who specializes in judicial politics). Of course, implementation is a conventional view of judging: Legislatures pass laws, stated in general and to some degree abstract terms, and judges apply those laws to individual disputes. The driving force in this style of judging is the syllogism: a major premise (the law), a minor premise (the facts of the case) and a deduction (the decision in the case). In this model, a good judge is a well-trained legal logician. And, of course, when it comes to deduction, Democrats and Republicans, liberals and conservatives, and even men and women act identically, so these factors are of

little relevance when it comes to selecting good judges.

An alternative view, however, denies the deductive structure of judicial decision making. The policy-making argument goes something like this:

Many legal controversies have no definitive solution (which, by the way, is one reason they are litigated). What is the "correct" answer to the question of whether the death penalty is cruel and/or unusual punishment? Obviously, those who wrote the Eighth Amendment to the U.S. Constitution believed they were not outlawing the death penalty. But also as obviously, "cruel" and even "unusual" are concepts determined by context. (At one point, even being drawn and quartered was not thought to be cruel.) So what do we take from the words written in the Eighth Amendment -- the concrete intent of the framers or their abstract assertions of guiding principles?

Moreover, it is not just constitutional interpretation that grants judges so much discretion. The fundamental value commitment of the American common law system is to justice, and especially justice when it conflicts with legality. To the extent that we expect our judges to "do justice" in their decisions, we grant them the right to make policy decisions.

The central point of the policy-making perspective is that legal disputes, especially at the appellate level, are indeterminate and not capable of being resolved via deductions. Indeed, a more revealing imagery is that of the pan balance, perhaps even the one that Lady Justice so famously holds in her left hand. In this conception, judicial controversies represent clashes of values: order versus liberty, privacy versus accountability and equality versus individuality. The process of judging is therefore one of weighing in on the relative weight one attaches to the contending values. To use a more concrete example, when a private newspaper publishes its employment want ads segregated by gender ("Help Wanted -- Men") and an equal-opportunity government agency seeks to prohibit the publication of such ads, a value conflict is generated between free press rights and equality rights. Such disputes can only be decided by calculating the relative value of the conflicting and contending rights. Relative value, unlike deductive logic, does indeed depend upon whether one is liberal or conservative.

If lawsuits require that judges make public policy -- indeed, even that they apply their own values in deciding cases -- then the legitimate criteria for selecting judges broaden significantly. The best legal training in the world, coupled with the most Solomon-like judicial temperament, cannot provide an answer to the question of whether women have the right to an abortion. Instead, such judgments turn inevitably on judicial ideologies and philosophies. The key and crucial question of judicial elections is this: If judges must (not can, but *must*) rely on their own pre-existing judicial attitudes in making decisions on the bench, do voters have the right to know about these attitudes and to base their voting decisions on policy agreement with the candidates for judicial office?

It appears that voters themselves do indeed believe that they have a right to hear the policy views of candidates for judicial office before they give them their votes. In a national survey conducted in 2007, I showed that voters in states electing judges do not equate policy pronouncements with partiality and that judges who make such policy statements are nonetheless believed to be able to serve as fair and impartial arbiters if they are awarded a seat on a state court of last resort. Though that research did not specifically address the matter, it is even conceivable that voters believe not just that there are no negative consequences of disclosing policy positions but that failure to disclose may be inappropriate for judicial candidates. Many legal elites seem to equate policy pronouncements with partiality and bias, but it appears that most of the American people do not.

This does not mean, however, that all aspects of judicial elections are acceptable to the American people. This same 2007 survey revealed that most Americans believe that the current system of interest groups making campaign contributions to those seeking public office -- judicial, legislative and executive -- is corrosive because it seems to create a *quid pro quo* relationship between interest groups and office holders. But campaign contributions and judicial speech should not be conflated, as they seem not to be in the minds of most Americans. My research indicates that the former does indeed pose a threat to judicial legitimacy; the latter clearly does not.

Many who observe judicial elections complain that campaigns have also become nastier, by which they mean that the rough-and-tumble campaign advertisements so common in races for other public office are becoming commonplace in judicial races. The available empirical evidence, however, suggests that nastiness, by itself, does little to undermine

judicial impartiality and legitimacy. Moreover, if judges are policy makers, making value judgments when deciding cases, to assert that judges ought to be immune from (or even legally protected from) criticism is illogical in a democratic polity. There are surely limits to the perceived appropriateness of negative and attack ads -- and in judicial elections as elsewhere, perhaps the antidote to bad speech is more not less speech -- but in general those who believe that criticism of judges and their decisions impugns fairness and impartiality appear to be mistaken, at least when it comes to the American people.

I would certainly be the first to concede that available empirical evidence is entirely insufficient for drawing firm conclusions about all of the effects of judicial campaign activity on perceptions of judicial impartiality and the legitimacy of the third branch. We suspect but do not know, for instance, that nonpartisan electoral systems, generally bereft of crucial information about candidates for judicial office, exacerbate the effects of attack ads. Social scientists are only in the early days of figuring out the multitude of consequences of campaign activity.

But the void in our knowledge should not be filled by supposition, assumption or ideological deduction -- and especially not by hasty and even stealthy efforts to "reform" systems of selecting and retaining judges in the U.S. Perhaps most important, a mythical view of judging, which proposes that judges are nothing more than legal technicians, should not be allowed to structure our thinking about the methods we use to select and retain judges. Nor should special influence be ceded to organized interest groups (e.g., the [American Bar Association](#)) when it comes to structuring judicial selection systems.

Judges are not simply "politicians in robes." But they are politicians, and states that have decided to elect their judges must protect the sanctity of democratic elections and not allow them to become sham elections like the ones so obvious in many parts of the world today. In the end, Justice Thurgood Marshall was correct when he opined in [Renne v. Geary](#): "(T)he greater power to dispense with elections altogether does not include the lesser power to conduct elections under conditions of state-imposed voter ignorance. If the State chooses to tap the energy and the legitimizing power of the democratic process, it must accord the participants in that process . . . the First Amendment rights that attach to their roles."

Annual Law And Courts Section Award Winners

The CQ Press Award :

Tom S. Clark

PhD Candidate, Princeton University

“The Separation of Powers, Court-Curbing and Judicial Legitimacy”

Crossing the boundaries of several different research literatures, this paper asks whether the U.S. Supreme Court’s use of judicial review is conditioned by congressional and public attitudes towards the Court. The paper develops its hypotheses from research on the separation of powers, and it posits that the justices are motivated by a desire to preserve one of the Court’s most important assets: its legitimacy.

Through painstaking efforts in data collection, Clark identifies all congressional legislation introduced from 1877 to 2006 whose purpose was to limit judicial power, and through various time-series



models he shows the significant impact that this legislation has had on the Court's decision making. He likewise makes effective use of available data on public opinion and tests its impact on more recent periods.

The effects that the paper uncovers not only confirm the importance of the system of constitutional checks but also reveal the complexities of its operations. As Clark demonstrates, the justices have historically been quite attentive to threats to their autonomy. So, when legislation aimed at limiting the Court's independence begins to percolate on Capitol Hill, the justices exercise restraint and reduce their level of confrontation with the Congress. At the same time, the Court is willing to invalidate acts of Congress, as long as it perceives that doing so will not come at the cost of its public support. When the justices sense that they may lose legitimacy in the eyes of the public, they check their ambitions, regardless of whether Congress sends hostile signals to the Court.

Equally impressive, Clark formulates his theory through an ambitious series of elite interviews with both members of the Supreme Court and law clerks. So, the paper is grounded upon scholarly perspectives and the first-hand observations of the participants in the process. Clark's paper invites scholars to think more deeply about the separation of powers, and it will doubtless influence how other researchers analyze the Supreme Court. It is a compelling analysis that makes an important contribution to our understanding of the Court and its role in the political system

The 2008 CQ Press Award Committee

Kevin McGuire (chair), University of North Carolina at Chapel Hill

Justin Crowe, Pomona College

Mariah Zeisberg, University of Michigan

The 2008 American Judicature Society Award

Jeffrey R. Lax & Kelly Rader

Columbia University

“Tactical Opinion Assignment and Voting in the Supreme Court”

The authors investigate an old and important question: Does opinion authorship—and, by extension, the assignment of an opinion to a particular justice—affect the voting decisions of Supreme Court justices? Combining sophisticated theoretical analysis and an innovative research design, Lax and Rader present compelling evidence that opinion assignment has significant effects on voting behavior and the stability of minority and majority coalitions. The paper represents a significant contribution to the literature on opinion assignment, and has broad implications for the importance of opinion content, which is closely linked to opinion authorship, for decision-making on the Supreme Court.

Honorable mention goes to Tom Clark for his paper "The Separation of Powers, Court-Curbing, and Judicial Legitimacy." In an outstanding example of combining rigorous theoretical analysis with rich empirical analysis, Clark demonstrates that the possibility of congressional court-curbing has a significant effect on the Supreme Court's use of the power of judicial review, suggesting that the Supreme Court is politically constrained by the preferences of other political institutions.

The 2008 American Judicature Society Award Committee

Georg Vanberg (chair), University of North Carolina at Chapel Hill

Gretchen Helmke, University of Rochester

Patricia Woods, University of Florida

The 2008 Houghton-Mifflin Award

Thomas Keck
Syracuse University

“Party, Policy, or Duty: Why Does the Supreme Court Invalidate Federal Statutes?” *American Political Science Review*, Volume 101, May 2007

Thomas Keck’s article, “Party, Policy, or Duty: Why Does the Supreme Court Invalidate Federal Statutes,” explores hotly contested accounts of judicial review by comparing the enacting coalitions in Congress and invalidating coalitions on the Supreme Court in decisions on the constitutionality of fifty-three statutes enacted over the last quarter century.

Focusing on distinctive implications of the attitudinal model versus historical-institutional approaches, Keck examines a partisan hypothesis, a policy hypothesis, and an institutional hypothesis. The partisan hypothesis maintains that Supreme Court justices will strike down statutes enacted by legislators of the opposite party; the policy hypothesis maintains that justices will strike down statutes enacted by legislators of the opposite ideological orientation; and the institutional hypothesis maintains that justices will strike down statutes based on institutional-legal commitments apart from ideology and partisanship.

Keck pairs a close reading of the cases with an analysis of the enacting and judicial coalitions. He concludes that the data “fit uneasily, at best, within the party and policy frameworks that have dominated empirical studies of judicial review.” Adding that the upshot of his analysis “is not to focus on institutional motivations instead of partisan and policy commitments, but to examine the interaction of these competing pulls for judicial loyalty.”

The 2008 Houghton-Mifflin Award Committee

James R. Rogers (chair), Texas A&M University
Lisa Hilbink, University of Minnesota
J. Mitchell Pickerill, Washington State University

The 2008 C. Herman Pritchett Award

Keith E. Whittington
Princeton University

Political Foundations of Judicial Supremacy: The Presidency, the Supreme Court, and Constitutional Leadership in U.S. History
(Princeton University Press)

Keith Whittington’s *Political Foundations of Judicial Supremacy: The Presidency, the Supreme Court, and Constitutional Leadership in U.S. History* is the most sophisticated analysis we have as to why the so-called “political branches,” especially the Executive, tolerates significant “constitutional leadership”—and even on occasion “judicial supremacy”—with regard to assigning meaning to the Constitution. Not only was there nothing inevitable about the role that the Supreme Court has come to play, but it is also the case, as Whittington demonstrates, that the Court is recurrently vulnerable to claims of “reconstructive presidents” who have their own distinctive constitutional agendas. We can do no better than adopt Mark Graber’s comments in a forthcoming review-essay: Whittington’s “is the rare work likely to be considered a seminal study for at least three important and growing research agendas in law, political science, and history. Whittington in less than three-hundred pages brings conceptual order to the burgeoning literature on the constitution outside of the court, identifies crucial patterns in the political construction of judicial review, and brilliantly elucidates the growth of judicial power in the United States.”

Honorable mention goes to Lisa Hilbink’s *Judges beyond Politics in Democracy and Dictatorship: Lessons from Chile*. This book is a truly outstanding, and quite surprising, analysis of the reasons for the Chilean judiciary’s consistent passivity over all of its history, including, most dramatically, the years of the Pinochet dictatorship. It should fascinate anyone interested in the relationship between “law and politics.”

The 2008 C. Herman Pritchett Award Committee

Sanford Levinson (chair), University of Texas, Austin
Stephen Bragaw, Sweet Briar College
Thomas M. Keck, Syracuse University

The 2008 Law & Courts Section Lasting Contribution Award

Mark A. Graber

University of Maryland at College Park

“The Nonmajoritarian Difficulty: Legislative Deference to the Judiciary”
Studies in American Political Development, Volume 7, Spring 1993

Now fifteen years since its original publication, Mark Graber's article is a part of the canon of the public law field because it provides an historical and institutional perspective for why the Supreme Court has moments of surprising authority through the use of judicial review. Unlike some of the academic literature that paints the power of judicial review in black and white terms—either the court has the power or it effectively has no power to have any influence—Graber argues that when understood historically, we find that the Supreme Court has specific moments of opportunities that enable authoritative assertion. These historical moments are themselves drawn from an understanding of politics and law that places the judiciary in a broader institutional context, one in which other political actors who seemingly have more direct institutional authority to act—such as the powers of Congress over money and the powers of the Executive over militias—will at times find it politically opportune to give the Court more authority. By understanding the Supreme Court as part of a broader theory of regime politics, Graber argues that elected officials will often find it opportunistic and strategic to rely on court authority.



Graber's work has been widely cited and has influenced a wealth of scholarship, particularly in the field of law and American Political Development. In the last five years, several APSR articles by prominent political scientists were directly influenced by Graber's argument. Graber's work is also evident in notable books by both political scientists and law school scholars. Because of Graber's article, no one can argue that judicial review is either powerful or powerless—it is contingent on other political actors in a regime. In turn, it makes clear the fallacy of thinking of any institution as all powerful or not—all political actors have weapons and weaknesses and the best understanding of American politics is one that sees these different actors interacting, asserting, and recanting in an institutional context.

The 2008 Law & Courts Section Lasting Contribution Award Committee

Gary J. Jacobsohn (chair), University of Texas, Austin

James F. Spriggs II, Washington University, St. Louis

Paul Frymer, University of California, San Diego

The 2008 Law & Courts Section Teaching & Mentoring Award

Jeffrey A. Segal

Stony Brook University (SUNY)

Jeffrey Segal has taught at Stony Brook since 1982. An active member of our section, he has established himself as a leading scholar in our discipline and earned the rank of Distinguished Professor at Stony Brook in 2004. Along with his distinguished research record, Segal is an excellent teacher. He has won his department's Distinguished Teaching Award and his teaching and mentoring is reflected in the careers of his former graduate students who have become important scholars in the field. However, Segal's excellence as a teacher reaches beyond Stony Brook and enriches the education of students at other universities. This award recognizes him for the vehicle of that influence his undergraduate course in Supreme Court Decision Making. This course meets very well the criterion for this award: “innovative teaching and instructional methods and materials in law and courts.”

The course is designed to provide students with a deep understanding of the Supreme Court and to enhance their research and analytical skills in an empirical methodology used in our discipline. Weekly sessions in the computer lab allow students to learn research techniques and apply them to the study of the Court. Homework assignments give students a chance to hone their skills, and a term paper requires them to test hypotheses about the Court and present their tests in papers that follow the format of scholarly articles. The result is an extraordinary experience that teaches students to be political scientists. As a former student wrote, the class “truly gave the student a venue to practice and try skills and theory we had only learned and heard about thus far.”

Segal has disseminated information about the course through articles in *Law & Courts* and *P.S.* In these articles, Segal also explains how the course has evolved over time in response to changing technology and the students' comfort level with computers. Colleagues and former students have taught versions of the course themselves, and they have attested to the value of the course for their students. Their doing so has extended the reach of Segal's instruction so that he has had an impact on students of political science across the nation. The insights and commitment reflected in his development of the course make Jeffrey Segal a very deserving winner of the Section's Teaching & Mentoring Award.

The 2008 Law & Courts Section Teaching & Mentoring Award Committee

Lawrence Baum (chair), Ohio State University

Lisa M. Holmes, University of Vermont

Joseph F. Kobyłka, Southern Methodist University

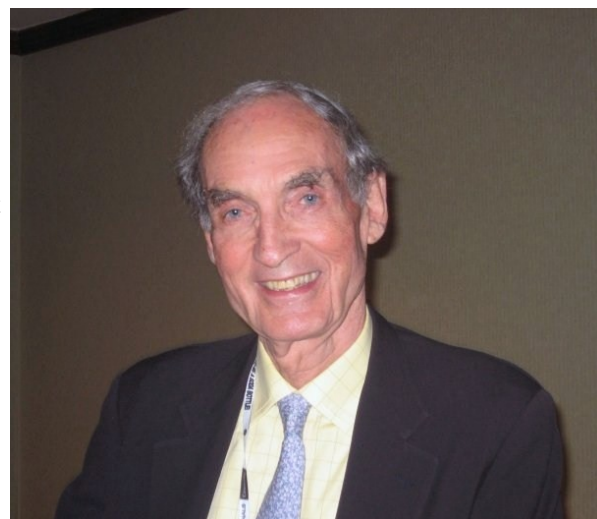
The 2008 Law & Courts Section Lifetime Achievement Award

J. Woodford Howard

Johns Hopkins University

The winner of this year's Lifetime Achievement Award for the APSA's Law and Courts Section is J. Woodford Howard, Emeritus Professor of Political Science at Johns Hopkins University. Professor Howard has made outstanding contributions to the field in both scholarship and teaching.

As a noteworthy teacher, Professor Howard has taught inspiring seminars on law and judicial process, and has directed the work of many future legal scholars. As a testament to his pedagogical influence, three of his graduate students have won the APSA's Edward S. Corwin Award for the best dissertation in constitutional and public law.



Professor Howard's research has been equally outstanding, setting high standards for inquiry in the field. His noteworthy works include *Mr. Justice Murphy: A*

Political Biography (Princeton University Press, 1968), which brilliantly combined biographical analysis with political science logic. Howard's article "On the Fluidity of Judicial Choice" (APSR, 1968) is widely considered one of the most important works ever written on the significance of roles and pathways in the process of judicial decision-making. In 2001, this work won the Law and Courts Section's Harcourt College Publishers Award for a book or article, 10 years or older, that has made a lasting impression in the field. And Howard's 1981 book, *Courts of Appeals in the Federal Judicial System* (Princeton University Press) blazed the path for serious study of appellate court decision-making by superbly blending quantitative and qualitative methods with incisive analysis.

His distinguished contributions in teaching and scholarship make Professor J. Woodford Howard a worthy recipient of the Lifetime Achievement Award.

The 2008 Law & Courts Section Lifetime Achievement Award Committee

Donald Downs (chair), University of Wisconsin, Madison

Lee Epstein, Northwestern University

Scott D. Gerber, Ohio Northern University

Julie Novkov, University at Albany (SUNY)

James Stoner, Louisiana State University

BOOKS TO WATCH FOR

Bruce Peabody
Fairleigh Dickinson University
bgpeabody@msn.com

In **From Words to Worlds: Exploring Constitutional Functionality** (Johns Hopkins University Press), **Beau Breslin** (Skidmore College) examines the essential functions that a modern, written constitution must incorporate in order to serve as a nation's fundamental law. These elements include creating a new citizenry, structuring the institutions of government, regulating conflict between layers and branches of government, and limiting the power of the sovereign. The author also discusses the theoretical concepts behind the fundamentals of written constitutions, examines in depth some of the most important constitutional charters from around the world, and draws distinctions between constitutional texts and constitutional practice.

Is a judge legally obligated to enforce an unjust law? **Douglas Edlin** (Dickinson College) addresses this question in **Judges and Unjust Laws: Common Law Constitutionalism and the Foundations of Judicial Review** (University of Michigan Press). Employing case law analysis, legal theory, constitutional history, and political philosophy, the author argues that the common law tradition gives judges a dual mandate: to apply and develop law. He also contends that there is no conflict between jurists' official duties and moral responsibilities. Consequently, judges have the authority, perhaps even the obligation, to refuse to enforce laws they determine to be unjust. Exploring the problems posed by these laws helps illuminate the institutional role and responsibilities of common law judges.

Donald W. Jackson (Texas Christian University) **Michael C. Tolley** (Northeastern University) and **Mary L. Volcansek** (Texas Christian University) are the editors of **Globalizing Justice: Critical Perspectives on Transnational Law and the Cross-Border Migration of Legal Norms** (Albany, New York: State University of New York Press, 2009). The book examines how various aspects of legal globalization today have advanced freedom, rule of law, and democracy around the globe. Among other phenomena, the authors consider the transnational spread of constitutional and human rights norms, the rise of universal criminal jurisdiction, and the role of both international nongovernmental organizations and domestic human rights commissions.

In **The Perils of Federalism: Race, Poverty and the Politics of Crime Control** (Oxford University Press) **Lisa Miller** (Rutgers University) explores how distinctive aspects of U.S. federalism shape the politics of punishment and serve to marginalize the interests of racial minorities and the poor. The author's focus is on the specific political institutions and actors that drive policymaking and their relationship to the politics of race and poverty. The book provides a detailed analysis of the narrow and often parochial nature of national and state crime politics, in contrast to the active and intense local political mobilization on crime by racial minorities and the urban poor. The work also considers how the absence of blacks as well as poor groups from the political process promotes political and legal frames skewed in favor of police, prosecutors and narrow citizen interests. Ultimately the author argues that the politics of crime control contributes to our understanding of group theory, political mobilization, and the continuing marginalization of black interests.

Gordon Silverstein (University of California, Berkeley) has written **Law's Allure: How Law Shapes, Constrains, Saves and Kills Politics** (Cambridge University Press). He contends that by the time John Roberts and Samuel Alito joined the Supreme Court, the link between judicial and political power seemed more important and more pervasive than ever before. The author argues that from war powers to abortion, from tobacco to integration, from the environment to campaign finance, Americans have been increasingly turning away from the political tools of negotiation, bargaining, and persuasion to embrace what they have come to believe is a more effective, more efficient, and even more just world of formal rules, automated procedures, litigation, and judicial decision-making. Through numerous policy case studies, this work analyzes the motives and incentives that encourage this legalization of the political process and American public policy, as well as the risks and rewards this effort can generate.

Laurence Tribe (Harvard University) argues in **The Invisible Constitution** (Oxford University Press) that there is an unseen constitution accompanying the text or “parchment” version. According to the author, some of our most cherished and widely held beliefs about constitutional rights are not part of the written document, but can only be deduced from this visible text. Through a variety of historical episodes and key constitutional cases, this book discusses the content, evolution, and operation of this “invisible constitution.”

John Vile (Middle Tennessee State University), **David Hudson** (Vanderbilt University), and **David Schultz** (Hamline University) are the editors of **The Encyclopedia of the First Amendment** (CQ Press). With more than 1,400 entries and over 200 authors, this two volume set discusses the political, historical, and cultural significance of the First Amendment, including its free speech, press, assembly, petition, and religion protections. This work traces themes like expressive rights in American political and legal history, in American political thought and social movements, in political and popular culture, and in the arts, in addition to examining the classic tensions between freedom of the individual and maintenance of political order. The set also features a chronology, seven introductory essays covering the core rights and liberties, a bibliography, and subject and case indexes. Additional tables of content give readers ways to find entries by topic or case.

Upcoming Conferences

Conferences

Southern Political Science Association 80th Annual Meeting

www.spsa.net/joomla/index.php?option=com_content&task=view&id=29&Itemid=31

Dates: January 8–10, 2009

Location: New Orleans, Louisiana

Call For Proposals Deadline: July 25, 2008

2009 APSA Conference for Chairs

http://www.apsa.com/section_236.cfm

Dates: February 5–6, 2009

Location: Baltimore, MD

2009 APSA Teaching and Learning Conference

http://www.apsanet.org/content_36794.cfm

Dates: February 6–8, 2009

Location: Baltimore, MD

Western Political Science Association 2009 Annual Meeting

<http://www.csus.edu/ORG/WPSA/mtgs.stm>

Dates: March 18–20, 2009

Location: Vancouver, BC, Canada

Submissions Deadline: September 19, 2008

Midwest Political Science Association National Conference

www.mpsanet.org

Dates: April 2–5, 2009

Location: Chicago, IL

Submission Deadline: October 10, 2008.

Southwestern Political Science Association Annual Meeting

www.swpsa.org

Dates: April 8–11, 2009

Location: Denver, Colorado

Proposal Deadline: September 29, 2008

2009 APSA Annual Meeting

http://www.apsanet.org/content_2665.cfm

Dates: September 3–6, 2009

Location: Toronto, ON, Canada

Submission Deadline: December 15, 2008