

# 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

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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

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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.

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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.<sup>1</sup> 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

<sup>1</sup> 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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