

On the Future of State Courts Research

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The future of research on state courts is, in many ways, not too different from the future of most other political-science topics or subfields. It faces some of the same challenges and pitfalls as others, as well as the same potential for intriguing new insights and breakthroughs. Yet, in some ways, it is unique. Some aspects of its distinctive path in the academy have been due to customs and norms within the field of political science, and some are attributable to the innate challenges of this research endeavor. Accordingly, I will touch on matters that are perhaps common to research in other fields as well as matters that might be considered special to state courts research.

KEEPING IT INTERESTING

Will state courts research essentially run out of novel topics or new ideas? I think that this is no more problematic for state courts than it is for any other field or research topic. It is easy for a field to become stagnant in its research endeavors, and no field is immune from this possibility. To avoid stagnation of research in state courts, we need to carefully consider what types of research questions and methods are worthwhile. Although this point may seem somewhat abstract, it defines itself in very practical ways through peer-review decisions and the judgments of editors, conference section heads, award committees, and, ultimately, readers.

If the peer-review process can be fairly accused of something, it is that it provides authors with incentives to be a bit too cautious and narrow in the range of questions that they ask and how those questions are approached. In state courts research there are a multitude of intriguing questions and phenomena to be analyzed. However, if we are engaging in rational self-preservation (most especially as untenured faculty), then we tend toward addressing incremental improvements to rather old questions. This is not to say that such an approach does not have its place in the discipline—it does—because refinement of traditional scholarship has merit. But it should not drive the discipline. Of course, this problem is often simply one of degree; there is research that is incremental in its contribution, and then there is research that is essentially derivative—yet both are frequently published. In contrast, some studies take on novel questions or engage literatures that are not commonplace in political science—and often seem to pay the price in peer review and editorial decisions. The fact that a study asks questions that are not steeped in the dominant literature or does not follow conventional approaches does not necessarily mean that the existing literature is being shunned or that the research fails to ask “big” or “important” questions; it may simply mean that new paths of interest are developing. If the discipline is to avoid stagnation and remain vibrant, then it must welcome research that pushes the boundaries of the field and probes new insights.

CLAIMING AN IDENTITY

This brings us to a challenge that faces law and courts study more generally. If the field is to flourish, then it cannot be all things to all people within political science. It must find, and stake claim to, its own identity. While we should always consider courts in broader context, this does not mean that state courts research should be considered as a mere subset of American politics research. This really gets to the point of what questions are to be asked, rather than whether context should be considered; clearly, context within the broader milieu of American politics matters, and proper controls should always be incorporated in any study. However, not all questions that are compelling and worthwhile in law and courts, and especially in state courts, involve questions about how courts or legal actors interact with, or are influenced by, other major political actors (e.g., Congress, state legislatures, executives, etc). Interesting and important questions on state courts research may focus on matters that do not necessarily lend themselves to traditionally popular questions from American politics research. For instance, while judicial elections may draw valuable theories and insights from traditional studies of legislative elections, the election of judges is unique in a number of ways and may present inimitable research questions that should still be considered important and worthy of publication. In similar fashion, analyses of state courts can draw from fields outside of political science and, in turn, make important contributions to fields that are not always readily associated with traditional American politics study—criminal justice, sociology, and psychology, for example.

PRACTICAL CHALLENGES

State politics research, generally, presents a number of practical research challenges. One concerns the scope of a given study. Ideally, our studies would involve fifty states over a significant time span. Of course, this is not always feasible. This does not mean that studies that do not provide this coverage are not worthwhile.

In state courts research we must balance the generalizability of the study against the theoretical leverage or insight that the study offers. Some studies may involve only a few states (or perhaps just one), but if they provide particularly compelling insights that larger-scale studies have not been able to offer, then they are certainly worthwhile. In law and courts, efforts by some scholars such as Paul Brace and Melinda Gann Hall have given us valuable, large-scale databases,¹ and this has helped promote the advancement of research in the field a great deal.² While such extensive efforts

¹ See <http://www.ruf.rice.edu/%7Epbrace/statecourt/index.html> and <http://dvn.iq.harvard.edu/dvn/dv/pbrace/faces/study/StudyPage.xhtml?studyId=18808>. Related publications include Brace and Butler (2002) and Brace and Hall (2002).

² Such efforts are typically highly labor intensive in state courts research due to the inherent challenges involved in collecting raw data from different justice systems. These endeavors are laudable and present us with an interesting question—how do we compensate such high-investment data-collection efforts and protect what is essentially intellectual property while at the same time ensuring access to data to promote and advance our knowledge of state courts? This is a difficult question because its answer necessarily involves judgments on the value of data-collection efforts versus other ways that valuable time might be expended (e.g., writing articles using existing data).

should be encouraged, the advancement of the field will likely depend more on our ability to strike the proper balance between rewarding coverage and theoretical leverage. Regardless of the approach a study employs, its ultimate worth regarding publication should be based on this question—what is the value added? In other words, did this research tell us something that we did not already know and, if so, do these new insights advance our understanding of the given subject in an important way?

Another practical research challenge lies in the nature of the data that we often employ. The questions that we address quite often involve time-series cross-sectional (TSCS) data, and this choice is typically accompanied by a host of concerns about the reliability and validity of our estimates and, therefore, our findings and methodology. Particularly troublesome is the fact that we often seek to assess the impact of state institutional characteristics on legal outcomes, and these institutional characteristics often may not change over time (time-invariant data) or, in the more typical case, change very little over time (nearly time-invariant data). In the past this has posed quite a challenge because time-invariant variables just do not work well in conjunction with state-level fixed effects (as opposed to using random effects). Recent developments in the estimation of TSCS data (e.g., Plumper and Troeger, 2005; Bartels, 2008) have been helpful, but this will likely continue to pose a challenge for state court researchers who are interested in institutional (or other) characteristics that primarily vary by state rather than over time.

ACCESSIBILITY AND RELEVANCE

On a fundamental level, one concern affects the future of not only state court research, but also all political-science research: accessibility. While different approaches to researching law and courts—quantitative approaches, formal theory approaches, and neo-institutional approaches—have engaged in (largely) polite jostling over their respective places in the field, all three have been quite guilty of failing on the dimension of accessibility of scholarship. This is not a call for a “dumbing down” of scholarship. At present, it is questionable whether a significant portion of the scholarship found in the most highly ranked political-science journals is really that accessible to even the average political scientist, not to mention the average layperson.

Most political scientists, including myself, have failed, to one degree or another, in making their work appropriately accessible to readers. This is not to say that we should abandon advanced methodological techniques or that we should oversimplify complex concepts to promote broad appeal. It simply means that straightforward writing, proper organization, transparency, and clarity in discussion of methods and results are paramount to our research mission and should be given their proper due. This applies not only to our writing, but also to how we handle our peer-review responsibilities and what we choose to reward in the discipline. Some authors almost seem to play “hide the ball” and write in a convoluted style using complicated jargon. Reviewers and editors should not reward this type of scholarship, and their decisions on what is to be published should reflect the values of clarity and accessibility. Certainly, the

page-length limits that most journals impose on authors can sometimes make expanded and accessible explanation of approaches and methods difficult. However, the use of Web-based storage for extended appendixes and auxiliary materials can facilitate the development of more-reader-friendly manuscripts in which much of the requisite background material and mechanical aspects of the manuscript can be relegated to accompanying online resource pages.

In short, authors should say what they mean and mean what they say. It is not the responsibility of the peer reviewer (or reader) to “get” the author who pays little attention to the qualities of accessibility outlined above—it is the responsibility of the writer to present their argument in a coherent and readable manner. If we are to remain viable as a profession and have our work taken seriously by people beyond the ivory tower, then this consideration must be taken seriously in all that we do.

A related issue is the relevance of scholarship. This issue can be dicey because there is obviously a subjective aspect to whether one considers a given piece of scholarship to be relevant. This being said, we can at least set forth some basic ideas about how we might strive to make our research more relevant to those within the profession and to a broader audience. First, while academics are sometimes reluctant to weigh in directly on some of the more salient and contested normative questions of the day (e.g., whether a given policy is good or bad), they possess the analytical tools to produce scholarship that may help inform the views and decisions of policy makers and the public. Thus, without acting as a political pundit, state courts scholars can improve the political debate by providing expert analysis of how government and politics actually work and why they work that way. For instance, there currently exists some political strife over the appropriateness and utility of electing state judges to the bench over other possible selection mechanisms, such as gubernatorial-appointment or merit-selection systems. Among the points in this controversy is the age-old question of how to balance judicial independence and judicial accountability. In assessing this question, existing state courts research could prove to be helpful. Scholars working on state judicial-selection systems have provided useful insights on the dynamics of state judicial elections (e.g., Hall and Bonneau, 2008), the impact of such elections on judicial policy making (e.g., Brace and Hall, 1997; Cann, 2002, 2007), and the influence of judicial-selection mechanisms on citizens’ support for their state courts (e.g., Cann and Yates, 2008). By laying a reliable empirical groundwork for claims regarding how judicial-selection mechanisms actually work and what effects they have, we can inform and, perhaps, even elevate the policy debate.

Another way to think about relevance is in terms of the breadth of topics that we address and how we might make our work appealing to a broader audience. In this vein, I suggest that we might consider referring to our subfield not as state courts research, but rather as state law and courts research. The range of topics considered to be relevant and worthwhile should include more than just judicial decision making or even judges for that matter. There is an abundance of interesting topics in matters concerning state law and courts beyond judicial behavior or elections. Such topics

have included the decision making of state enforcement agents, such as police and prosecutors (e.g., Gordon and Huber, 2002), citizen legal mobilization and litigant behavior in state courts (e.g., Yates, Davis, and Glick, 2001), and state regulation of attorneys (e.g., Howard, 1998), just to name a few. By keeping our minds open to new questions and topics for investigation, we welcome the type of creativity and diversity of ideas that are necessary to help sustain energy and vitality within the study of state law and courts. **jsj**

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