

# New Directions in State Court Research

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It is important periodically to take a step back from one's research to assess the sub-discipline of which one is a part and to determine the directions that field should be going. This symposium, thus, has been a welcome opportunity for those of us who study state courts to take a more comprehensive view. As academics and practitioners, we need to assess candidly the successes and failures of past research. We should also consider what important questions have been ignored in the past or have emerged recently and require our attention. Finally, having identified those key questions, we must determine the most effective strategies for addressing them. In undertaking this review, we should not restrict our attention to state court research but should consult as well the research agendas in complementary fields in political science and law. This broadened focus may reveal issues that scholars of state courts ought to consider, offer findings that are pertinent to state court research, or identify approaches that might fruitfully be used in studying state courts.

Of course, inviting state court scholars to take a broader perspective on their research does not guarantee a congruence of views. The perspectives of academics and practitioners often differ, and even within those groups, views vary. For researchers, there is always the temptation to look in the mirror and think that we are viewing the scholarly landscape, to insist that what the field needs is simply more of whatever we are already doing. Certainly, the participants in this symposium have differed as to the phenomena we find most interesting and as to the approaches we employ in studying them. But that is not a problem—it may even reflect the vibrancy of the field. And, in fact, the differences in our perspectives may be more fruitful than our commonalities, stimulating dialogue about where we have been and where we should be going. To encourage such dialogue, let me offer four provocations.

First, I believe that students of state courts should strive to speak not just to each other but to a broader scholarly community. This hardly sounds revolutionary, but the audience for our research on state courts often does not extend beyond other state court scholars. Yet as political scientists looking at one state institution, we should engage with research on state politics and policy, and our research should be of interest to those studying state politics more generally. To me this means that we should focus more on how state courts contribute to governance in the states and on how they interact with other governmental and nongovernmental institutions. In the past, some state court scholars have used a comparative-case-study approach to investigate how state courts participate in the political life of their states. Scholars such as Matthew Bosworth and Douglas Reed, for example, employed that approach effectively in studying school-finance litigation in the states, and Mary Cornelia Porter and I used this same approach in describing more generally how state supreme courts inter-

act with governmental institutions in their states and beyond their borders (Bosworth, 2001; Reed, 2001; Tarr and Porter, 1988). Yet the crucial consideration is not the approach but the substantive focus. Having identified courts as political institutions and judges as political actors, we should analyze how they participate in politics and what effects their participation has.

Because the study of state courts is a subdiscipline of public law, our research should also take into account the broader research agenda on courts and politics, and it should be both accessible and interesting to judicial politics scholars more generally. Perhaps the most striking development in public law over the last two decades has been the resurgence of interest in historical inquiry, in situating judicial activity within the framework of American political development and in using that framework to understand the range of choices available to judges at various points in time and the effects of the choices that they have made (see, e.g., Whittington, 2007; Kersch, 2004; Keck, 2003; Gillman, 1993). Public-law scholars have analyzed *The Supreme Court and American Political Development* (Kahn and Kersch, 2006), but there is nothing remotely comparable looking at state supreme courts. Future research should repair that gap. While research on federal courts may provide us with a useful starting point, in some instances the development of state courts has diverged from that of the federal courts. For example, whereas judicial selection and tenure have been settled issues at the federal level for more than two centuries, they remain contested in the states (Geyh, 2006; Bybee, 2007). Insofar as there are differences between federal institutions and practices and their state counterparts, as well as among the institutions and practices of the various states, we should be examining why they have developed and what effects they have produced.

My second point is implicit in my first. I believe that the unit of analysis for state court research should primarily be the court or the state's judiciary as a whole, rather than the individual judge. I am not suggesting that the study of state judicial decision making has not been useful—note, for example, Larry Baum's comment in *Judges and Their Audiences* about how scholars studying state courts anticipated some of his ideas (Baum, 2006). But if we are to understand not merely the internal dynamics of courts but also their role in state politics, then we must examine how those courts act and are acted upon, how they affect the policy of the state, and how other institutions respond to their policy pronouncements.

Take as an example tort law. The study of state courts opens up all sorts of opportunities for comparative analysis. What differences are there among state courts in the changes in tort law that they have introduced? Why have some states become "judicial hellholes," according to the defendants' bar, whereas others have not? What sorts of "tort reform" initiatives have state legislatures introduced in response? How have various state supreme courts responded to the tort reform legislation—have they accepted and applied it, or have they invalidated it on state constitutional grounds? What effect have tort reform statutes had on the agendas of state supreme courts and on their rulings? What role have interest groups played in attempting to affect the

composition of the state bench, and why have they been active and effective in some states but not in others? These are all questions that state court scholars should be addressing.

Third, we should draw more heavily upon parallel research in other fields of political science to understand state judicial politics. One contributor to this symposium, Melinda Gann Hall, has demonstrated the rewards of this broadened focus in her research on state judicial elections (Hall 2001, 2007). Critics of judicial elections have long insisted that they do not provide the opportunity for the meaningful electoral choice characteristic of other elections (See, e.g., Geyh, 2001). Yet in comparing state supreme court elections with elections for the federal House of Representatives, “the quintessential representative institution,” Hall discovered far more similarities than differences (Hall, 2001:321). Contrary to what might have been expected, although elections for state supreme court seats were less likely to be contested than House races, they were roughly as competitive (as measured by the margin of victory), and more likely to result in the defeat of incumbents, at least when judicial candidates ran on partisan ballots. In sum, by establishing a baseline derived from the study of other elections, Hall was able to offer a more informed judgment about the quality of accountability in state judicial elections.

Hall’s intriguing findings should encourage other state court scholars to follow her example, to look to bodies of research outside the public-law field to understand state courts better. Building upon Hall’s pioneering work on judicial elections, several questions immediately suggest themselves. Where do voters in judicial elections get the information on which they base their electoral choices, and is this the same as for voters in other low-salience elections? What are the implications for assessing state judicial elections of the literature on how low-information voters choose (see, e.g., Lupia and McCubbins, 1998; Ferejohn and Kuklinski, 1990)? Do turnout and voter drop-off in judicial elections parallel that in other low-salience elections, or if not, what accounts for the differences? What do studies of negative campaigning and its effects in other elections have to say about the effects of negative campaigning in judicial elections (see, e.g., Geer, 2006)? More broadly, how can studies of independence and accountability in other public institutions inform discussions of judicial independence and accountability in the states (see, e.g., Burbank and Friedman, 2002)?

Finally, tempting though it may be to do so, especially given the availability of data sets pertaining to state appellate courts, we should not ignore state trial courts. During the 1970s, many political scientists studied the operation and rulings of state trial courts, and although the LEAA funding that supported much of this research long ago disappeared, the justifications for the research have not (see, e.g., Feeley, 1979; Eisenstein and Jacob, 1977; Levin, 1977). State trial courts play an important, if often invisible role in making public policy. Most of the cases these courts decide announce no novel legal principles and involve disputes that are of concern only to the disputants. Nevertheless, state trial judges exercise considerable discretion in resolving these relatively routine cases, and their resolution may in the aggregate embody impor-

tant policy choices. Sentencing in criminal cases illustrates this sort of cumulative policy-making, and so do child-custody awards based on “the best interests of the child” in contested divorce cases. Beyond that, state trial courts are where most persons encounter courts and law, and they form their impressions of state courts and state judges largely based on those contacts. When Justice William Brennan, who served on both state and federal courts, observed that “the composite work of the courts in the fifty states probably has greater significance [than that of the U.S. Supreme Court] in measuring how well America attains the ideal of equal justice for all,” doubtless he was thinking of the work of state trial courts (Brennan, 1966:236).

Thus, as state court scholars, we must address the administration of justice in the states more generally. Here it seems to me that an awareness of the practical problems confronting those courts is crucial. Let me mention two areas that are crying out for scholarly engagement. The first is so-called therapeutic justice. Early in the 1990s, convinced that their courts were failing to deal effectively with large numbers of drug offenders, states began to create so-called drug courts to provide an alternative to incarceration and recidivism. Buoyed by what they deem the success of this initiative, states have created other specialized “problem-solving” courts (Berman and Feinblatt, 2005). Scholars not associated with the therapeutic justice movement need to address the success or failure of these specialized courts, the contexts in which they are likely to be effective, and the possible negative aspects of the therapeutic justice model.

The other example is legal representation in civil cases. State judges report an epidemic of pro se litigation and are perplexed at how to ensure justice is served. State court scholars should not leave such access-to-justice issues to legal academics (e.g., Rhode, 2004). Rather, we should take the initiative in exploring how best to provide equal justice for all in state trial courts. **jsj**

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