

Formal Theory in Comparative Judicial Politics

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Late last fall, the Law and Courts discussion list took up a pointed query concerning whether the field had learned anything unique from formal theory approaches. At first blush, the question appears eminently fair. We might reasonably ask whether a framework for model building has actually taught us something we did not know already. And I would readily agree that we do not need mathematical statements about what judges want, believe and can do in order to conclude that judicial decision-making is sometimes prudent, as formal theory skeptics commonly note. The thread died after Jeffrey Lax posted a three-page list of “things we’ve learned from formal theory,” which seemed to suggest that, at the very least, you would have to do a good deal of reading before you could claim with much certainty that we have learned nothing. Yet, strictly speaking, the question remained open, closed perhaps only by a collective disinterest in checking the intimidating citation list against prior knowledge.

Nevertheless, I would like to suggest that whereas the question is reasonable, in so far as it seeks intellectual property than can be attributed uniquely to formal theory, it is not the right question to ask. It is not that one cannot identify unique substantive contributions from scholars that use formal methods, as Lax does, but rather that such claims and the corresponding debate over whether alleged contributions are genuinely unique mischaracterize the construction of knowledge in our field and obscure how precisely formal theory contributes to this process. If we must ask what we have learned from formal theory, we will do better to consider how formal theory, as a method of argumentation, contributes to the clarity and internal consistency of particular arguments. Instead of attempting to identify the big ideas, we should be highlighting the process by which scholars get there. Formal theory’s chief contribution lies in this process. In what follows, I would like to put some flesh on these general claims. Then I will provide a few examples of how formal theory can enrich research by discussing two well-known projects in comparative judicial politics.

Formal Theory and Law and Courts Research

The search for unique substantive knowledge produced by formal theory models of law and courts is not particularly productive two core reasons. Primarily, it obscures the most likely contributions formal theory can make to the field. It is critical to stress that formal theory is a method, designed to structure and clarify the claims we make about the world and to ensure their internal consistency. It is a tool. In this sense, asking how formal theory has informed substantive debates in our field is like asking how archival methods or regression analysis have informed debates in the field. Both methods offer advantages, but to identify how precisely they have added value to a literature requires an understanding of the tool and the research context in which it is applied. A description of the many types of formal theory is well beyond the scope of this essay; however, in order to suggest the ways in which it can be useful, it is helpful to summarize the main components of one of its branches. I will focus here on non-cooperative game theory, which has come to be the dominant method of choice in judicial politics.ⁱ

A non-cooperative game theory model includes five components. It explicitly identifies the actors whose behavior will be analyzed and it describes from what and how these actors derive value—their preferences. In addition, a model describes what the actors can do and when they can do these things. It also specifies what the actors know about themselves, the other actors in the model and exogenous features of the world in which they live. Readers are no doubt aware that individuals in game theory models are assumed to choose actions that offer them the highest value according to their preferences; however, when the best action depends on what others choose and features of the world about which an actor might be uncertain, it is not entirely clear what the best action is. Thus, the final component of a model is a theory, referred to as a solution concept, about how actors will behave jointly in light of the information they have and their expectations about what the other actors will do. Critically, the solution concept provides explicit rules for the analyst about how to put all of the preceding information together in order to make behavioral predictions.ⁱⁱ

This, in general, is the structure of a non-cooperative game theory model. In the simplest sense, it offers rules for

constructing arguments about how people behave together in light of what they want, what they can do, what they know and their expectations of others' behavior. This strikes me as a common feature of many (though surely not all) arguments in law and courts, even when scholars do not invoke explicitly each component of a game theory model. When scholars proceed with non-formal strategic arguments, they typically identify the actors under analysis, give some rough account of their goals, suggest behavioral alternatives available to them and may even discuss their beliefs about the world. But what always goes unstated in non-formal arguments is how precisely theoretical claims are made about joint behavior in light of the other components of the model. In other words, solution concepts are left implicit. This is not to suggest that there are no solution concepts in operation in these cases. There are. Indeed, there must be, otherwise there would be no argument at all. This does not mean that scholars are drawing invalid logical inferences. It does mean that scholars are opting out of a framework this is designed to ensure the validity of these inferences.

No doubt, there are some arguments that are straightforward enough that theoretical expectations about joint behavior can be derived easily without a solution concept. But this is not always true. Indeed, some of the most interesting research questions (see Bueno de Mesquita and Stephenson 2002 below) involve strategic settings that are quite difficult to reason through without the benefit of rules for drawing deductive inferences. And some of the most interesting theoretical claims derive from models in which what "makes sense" for an actor to do is highly conditional. Identifying these conditions is a core element of good theorizing and game theory models are designed to do this. Assumptions are laid bare and we can readily identify the impact of different conceptual choices by making a change in the model and applying the solution concept again. For this reason, game theory models are particularly helpful when considering the empirical implications of small changes in key features of an argument (i.e. for developing testable hypotheses).

Certainly, not every paper in law and courts could benefit from, much less needs, a formal theory model. Arguments can be stated clearly without appealing to payoff functions and information structures. Whittington (2005) and Gillman (2002) immediately come to mind. And it is surely possible to obscure a relatively straightforward strategic argument through formalization. I merely wish to suggest that the key components of a game theory model are common to theoretical arguments generally. The advantage game theory offers lies in the rules that structure arguments about strategic interaction, rules that force scholars to provide behavioral predictions according to well-known procedures. It is the structure that formal theory places on the process of developing arguments that marks its contribution to the field.

The second reason that the search for unique formal theory-driven knowledge is unproductive is that it encourages us to view formal scholarship as constituting an entirely separate, arcane subfield. Yet, obviously scholars that use formal theory are neither all researching the same questions nor are they speaking exclusively to other scholars that use formal theory. It would be difficult to conclude that Pérez-Liñán, Ames and Seligson (2005), Vanberg (2005) and Lax (2007) are writing about the same thing, even if their models share common assumptions about human cognition; and, it would be highly unfair to suggest that key insights in these works should be, or worse can be, knowable only by a tiny set of social scientists. These scholars are writing about important questions in well-developed substantive literatures.ⁱⁱⁱ They are engaging theoretical answers to these questions and generating new questions to ask, much like every other scholar in the field.

It would be difficult to appreciate fully Bueno de Mesquita and Stephenson's (2002) ideas about the doctrine of *stare decisis* without being familiar with the general (non-formal contributions included) debate over whether precedent does or should bind a court to its own prior decisions. Their ideas nestle comfortably within this tradition. Does the paper provide what we might think of as unique insights? In my view, it does in so far as it identifies clearly a trade-off inherent in the choice to break a line of cases between the substance and the accuracy of doctrine communicated within a judicial hierarchy; and, it also identifies the precise conditions under which judges might opt for one side of the tradeoff over the other. But one might quarrel with whether these are unique contributions. Regardless, the key point is that these insights build on and respond to ideas in Segal and Spaeth (1996), Epstein and Knight (1996), Nelson (2001), and Kornhauser (1989), papers that display significant methodological variance yet advance the conversation. Thinking about matters in this way underscores a point that often gets lost in debates about the usefulness of formal theory in particular literatures. If we force ourselves to seek out information unique to formal theory, we set ourselves up to ignore the ways in which scholars of all methodological stripes have interactively produced knowledge.

By so doing, we risk being simultaneously unfair to formal and non-formal scholarship.

Formal Theory and Comparative Politics

Many of the core questions in comparative judicial politics involve strategic interaction. Comparative scholars have considered the choice of a state to bind itself formally to constitutional limits enforced by the judiciary (Finkel 2005; Ginsburg 2003; Hirschl 2004) and to the ability of courts to effectively constrain states to higher law obligations (Carrubba 2003; Epstein, Knight, and Shvetsova 2001; Staton 2006; Vanberg 2005). They have considered the development of judicial independence from external and internal forces (Hilbink 2007; Ramseyer and Rasmussen 1997; Rios Figueroa 2007), and, they have addressed the implications of judicial independence for economic development and human rights (Cross 1999; La Porta *et al* 2004; North and Weingast 1989). And, of course, comparative scholarship has explored well-trodden questions of judicial decision-making in institutional contexts very much different from that of the United States (Herron and Randazzo 2003; Iaryczower, Spiller, Tommasi 2002). Some of the scholars I cite here operate explicitly by way of a formal argument. Others do not. Still, the arguments in these works all contain strains of strategic interaction, whether between judges at different levels of a hierarchy, across branches of government or between elected officials considering judicial reform. What is more, key concepts in theories of institutional design or judicial independence vary considerably across states (e.g. political competition, public support, formal institutions that supposedly insulate judges, doctrinal norms of deference, access rules, etc.). Since formal theory provides precise rules for considering how changes theoretical concepts influence predicted outcomes and since a comparative research design is capable of constructing the counterfactual conditions necessary to test these claims, formal theory has been and likely will continue to be an important tool in comparative judicial politics. With this frame in mind, I would like to give two quick examples of ways in which it can enrich arguments in the field. I will focus on two recent and well-known projects.

Helmke (2002) asks why Argentine high court judges, who enjoy few *de facto* protections from political interference, have sometimes challenged the authority of the very officials who appointed them. Unsurprisingly, existing theories of judicial behavior offer no compelling explanation for such choices. Indeed, on the standard account, strategic decisions are inherently deferential, designed to minimize confrontation with current members of government. So why would a judge seemingly invite conflict with a sitting government? Helmke's intriguing theoretical claim is that where formal rules governing judicial tenure are not respected and political instability is significant, judges may begin "defecting" from their appointers in an effort to keep their positions after a government or regime failure. This simple yet important innovation links elegantly the judicial politics literature on strategic judicial decision-making with a rich tradition in comparative politics on regime transitions. The argument is clear and persuasive and the data analysis is largely supportive of its key empirical implication.

Ginsburg (2003) pursues two familiar questions in law and courts. Why would a state delegate powers of constitutional review to the judiciary and simultaneously build institutions that insulate it from political interference? Similarly, what accounts for the expansion of judicial authority over time among courts with powers of judicial review? In answering the first question, Ginsburg develops a model of political insurance, in which political coalitions construct independent judiciaries to "lock-in" preferred policies lest they lose power. With respect to the second question, Ginsburg suggests that new courts help construct a norm of compliance over time by carefully exercising their jurisdiction in their institutional infancy and only expanding their formal powers once compliance becomes expected. Like Helmke, Ginsburg provides considerable empirical support for empirical implications of these arguments.

There is a great deal to admire in these projects. That said, both arguments suggest a role for an explicit formal model. Consider Helmke. The logic of the argument depends on the expectations judges have about what will happen to them after a transition. It only makes sense to begin defecting once it appears that a transition is likely – otherwise, a judge faces the wrath of the sitting government. Waiting until something like a regime tipping point allows a judge to avoid immediate retribution and take advantage of the good graces of the new government. But here is the rub. Why would a defecting judge expect that a new government would perceive her to be loyal to the new coalition and not conclude that she had changed positions merely to keep her job? Why would a new government not want to purge such a judge and replace her with a copartisan? These expectations are not modeled in the informal account Helmke gives, yet it is precisely the kind of thing that a game theory model of the process would require. It is not that this piece of the argument could not be modeled informally, but rather that there is an entire class of game theory models designed to provide Helmke with a precise answer to the question of what beliefs must be held in order for the logic of strategic de-

fection to hold. Interestingly, Helmke clarifies this point herself in her 2005 monograph *Courts under Constraints*, where she applies a standard signaling model to the same problem addressed in Helmke (2002). By doing so, she identifies how exactly a judge can successfully exploit the uncertainty of the incoming government to avoid a purge prior to and following a regime transition. Many of the implications of that model have yet to be tested, and so not only does Helmke (2005) clarify Helmke (2002), but she expands the set of empirical implications we can test.

Like Helmke, Ginsburg tackles a difficult, normatively important question. There are a variety of reasons why we might like a judiciary to enforce constitutional limits on power yet world history is riddled with failed efforts to rein in the state. Yet, just as Helmke struggles with the strategic implications of her argument across regimes, Ginsburg struggles with these implications across the periods of institutional design and institutional growth. There are two key questions. If courts in their institutional infancy require clever deferential strategies (say like Marshall's in *Marbury*) until a compliance norm can be established, how is it that that new courts constitute insurance for governing coalitions against the loss of power? Ginsburg recognizes this tension, and suggests that constitutional review makes attacking minority interests more costly, all things equal (p. 75); however, if the argument about the growth in institutional strength is right, then the best we can say seems to be that constitutional review makes attacking minority interests no less costly, all things equal. And, perhaps this claim itself is subject to qualification. Might it be easier to attack minority interests when you can expect a pliant constitutional court to approve of your choices? Either way, the point a formal model would render explicit the conditions under which these claims can be supported.

Regarding the second part of the Ginsburg argument, we might wonder about how precisely the norm of compliance is developed? Ginsburg suggests that losers have to be sufficiently likely to win in the future in order to comply in the present (pp. 73-74). In so far as this is true, a pattern of compliant behavior emerges. This is persuasive for sure; however, if a weak court is behaving per the Ginsburg argument, then it is not asking for compliance when it would be sufficiently costly for the government (or some other party) to comply. In so far as this is true, why might not another norm develop, say a norm in which the judiciary is expected to challenge a government only in low salience political conflicts? This was the general understanding of the Mexican Supreme Court during the period of one-party rule (Fix-Fierro 1998, 202). If it challenges government in a salient area, non-compliance is acceptable. In part, Carrubba (n.d.) addresses this second question with a non-cooperative game theory model of endogenous institutional change, which explains how an institutionally weak court can become institutionally strong. Yet, Carrubba does not deal with competing norms of compliance. Moreover, the first question is left entirely open. It is not obvious that a formal model is needed to answer how a new constitutional court can serve as insurance in light of the ensuing enforcement problem; however, it is precisely the kind of question that a formal model could be fruitfully used to answer.

Conclusion

I raise these questions about Ginsburg and Helmke not to criticize their scholarship. On the contrary, I believe that these scholars have produced some of the best recent work in our field. Yet, both projects underscore the ways in which formal and non-formal scholarship can interactively produce knowledge and they both suggest that formal theory can help pin down strategic arguments. Helmke (2002) and Ginsburg (2003) build on ideas of scholars that made use of formal and non-formal methods. And importantly, their work has been clarified and extended in key ways by formal versions of the original arguments. Carrubba answers some key questions in Ginsburg, and Helmke answers key questions of her own. A healthy research agenda produces answers to existing theoretical questions and also suggests new questions that can be modeled subsequently. Sometimes these new questions will be answered by the scholar that raises them, but it is even better when we tackle collectively our theoretical challenges. Formal theory is contributing to this process in our field, and by so doing helping to maintain its intellectual health. Formal models of law and courts ultimately will be evaluated as we evaluate all models. Do they illuminate important theoretical questions? Are they pregnant with empirical implications? Do we find support for their expectations in data? I am enough of an empiricist to welcome that analysis wholeheartedly, and I believe that formal theories can be defended on those grounds. That said, in so far as we turn to that analysis without considering the ways in which formal modeling influences the process by which arguments are developed, I believe that we miss the approach's key contribution to our field.

(Notes)

ⁱ For a discussion of why this has come to pass, especially in research on institutions, see Diermeyer and Krehbiel (2003).

ⁱⁱ Scholars that are sufficiently persuaded that classical rationality assumptions simply do not describe well how people think need not abandon the effort to formalize their theoretical claims. Indeed, behavioral game theory models are designed exactly to capture features of human cognition that seem to depart from classical assumptions (For a review of various modeling approaches, see Camerer 2003).

ⁱⁱⁱ Pérez-Liñán, Ames and Seligson write about how careerism and hierarchy influence judicial choice, whereas Vanberg (2005) considers the conditions under which public support can induce judicial power and Lax (2007) investigates the possibility of constructing intelligible legal doctrine on a collegial court.

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