

**Toward a Strategic Revolution in Judicial Politics: A Look Back, A Look Ahead**



Lee Epstein; Jack Knight

*Political Research Quarterly*, Vol. 53, No. 3 (Sep., 2000), 625-661.

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*Political Research Quarterly* is currently published by University of Utah.

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FIELD ESSAY



# Toward a Strategic Revolution in Judicial Politics: A Look Back, A Look Ahead

LEE EPSTEIN AND JACK KNIGHT, WASHINGTON UNIVERSITY

As papers presented at recent disciplinary conferences and articles published in major political science journals reveal, the field of judicial politics is undergoing a sea change. Variants of the social-psychological paradigm, which have long dominated thinking about law and courts, are giving way to approaches grounded in assumptions in rationality. More to the point, ever-growing numbers of scholars are now invoking the strategic account to understand judicial politics. In what follows, we investigate this “strategic revolution.” We begin by providing an intellectual history of the field, with special emphasis on why judicial specialists resisted strategic analysis for so long and why they are now (re)turning to it in ever-increasing numbers. Next, we consider the ways that analysts have begun to put the strategic account to work. This is an important task, for debates are already emerging over the “best” way to invoke the account to study judicial politics. We take the position that there is no one “right” way but rather four different approaches—all of which have the potential to provide us with important insights into law and courts.

If papers at recent conferences and articles in the major journals reveal anything, it is that the field of judicial politics is undergoing a sea change.<sup>1</sup> Variants

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<sup>1</sup> At the 1994 meeting of the American Political Science Association, about 19 percent (8 of 43) of the papers presented on law and courts panels were attentive to variants of the rational choice paradigm; five years later, that figure was 40 percent. Of the 14 articles on judicial politics, published in the *American Journal of Political Science*, *American Political Science Review*, *Journal of Politics*, and *Political Research Quarterly* in 1995, 36 percent drew on rational choice approaches; by 1999 the percentage (62.5) was appreciably higher.

NOTE: We are grateful to the National Science Foundation for supporting our work on strategic decision making (SBR-9320284, SBR-9614130). We also thank Gregory A. Caldeira, Charles Cameron, Micheal Giles, Melinda Gann Hall, Jeffrey A. Segal, Harold J. Spaeth, Joseph Stewart, Jr., and Thomas G. Walker for their insightful comments.

*Political Research Quarterly*, Vol. 53, No. 3 (September, 2000): pp. 625-661

of the social-psychological paradigm, which have dominated thinking about law and courts for nearly three decades, are giving way to approaches grounded in assumptions in rationality. More to the point, ever-growing numbers of scholars are now invoking the strategic account to understand judicial politics.<sup>2</sup> On this account (1) social actors make choices in order to achieve certain goals; (2) social actors act strategically in the sense that their choices depend on their expectations about the choices of other actors; and (3) these choices are structured by the institutional setting in which they are made (see, generally, Elster 1986).

In what follows, we investigate this “strategic revolution” (Cameron 1997)—a revolution that may represent the most important development in the law and courts field since behavioralism took hold in the 1950s. We begin by offering an intellectual history of the field, with emphasis on why judicial specialists resisted strategic analysis for so long and why many are now (re)turning to it. These seem to us important enterprises for two reasons. First, it is too often the case that attempts to reconstruct major shifts in our thinking come only after they have transpired, rather than while they are occurring. By providing some context for the current change, we hope to create a record for future generations of scholars and their students that preserves some of the details inevitably lost in retrospectives. Second, many contemporary scholars who invoke strategic accounts are entirely unaware of related work done by political scientists decades ago. By reviewing these vintage studies, we hope to make connections—build bridges, really—among work of the past, present, and (in all likelihood) future.

Next, we consider the ways that analysts have begun to put the strategic account to work. This is also an important task because, even though the revolution is only in its infancy, debates are already emerging over the “best” way to invoke the account to study judicial politics. We take the position that there is no one “right” way but rather four different approaches, all of which have the potential to provide us with important insights into law and courts.

#### THE RISE AND DEMISE OF THE RATIONAL CHOICE PARADIGM IN THE STUDY OF JUDICIAL POLITICS

As more and more judicial specialists have come to invoke the strategic account, some have tried to identify “the” study that paved the way for the massive change the field is now experiencing. At least according to a group of (mainly) law and business school professors<sup>3</sup> known as positive political theorists

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<sup>2</sup> The strategic account belongs to a class of non-parametric rational choice models as it assumes that goal-directed actors operate in a *strategic* or *interdependent* decision making context. Virtually all rational choice models scholars have invoked to study judicial politics are non-parametric.

<sup>3</sup> This group includes Cross (Cross and Tiller 1998; Tiller and Cross 1999); Eskridge (e.g., 1991a, 1991b), Farber (Farber and Frickey 1991), Rodriguez (e.g., 1994), Spiller (e.g., Spiller and Gely

(PPT),<sup>4</sup> that study was a 1989 dissertation written by Marks, a student of economics at Washington University.<sup>5</sup>

And, yet, thirty years before Marks produced his “locus classicus” (Cameron 1993), political scientists—indeed some of the founders of the modern-day study of law and courts—adopted economic perspectives to study judicial decision making. Still, we can hardly fault the PPT group for claiming this particular piece of turf: The early research grounded in assumptions of rationality quickly gave way to studies lodged in a socio-psychological tradition.

Why? To address this question, as well as to begin to understand why the tide is turning yet again, we describe the evolution of rational choice theories in the study of judicial politics.

### *The Rise of Rational Choice Theory*

Brian Marks may be the starting point for modern-day PPTheorists but it was Glendon Schubert, more typically associated with social-psychological theories of judicial decision making (see, e.g., Schubert 1965), who was one of the first political scientists to apply rational choice theory to political problems. In a 1958 review of the public law field Schubert included a section called “game analysis,” in which he asserted that “[t]he judicial process is tailor-made for investigation by the theory of games. . .” (p. 1022). He went on to invoke game theory to study the voting behavior of two Supreme Court justices—Hughes and Roberts—during a crucial historical period, the New Deal (the “Hughberts” game).<sup>6</sup> In so doing, he showed that the justices were strategic decision makers: Only by recognizing their interdependency could they maximize their preferences.

Schubert’s application may have been crude but it was important in two regards. First, it demonstrated that approaches based on assumptions of rationality could

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992), Spitzer (e.g., Cohen and Spitzer 1994), and Tiller (e.g., Spiller and Tiller 1993), to name just a few. These business/law schools professors are joined by a handful of political scientists, many of whom developed their reputations as students of Congress or the Executive (e.g., Cameron 1993, 1994; Ferejohn and Weingast 1992a, 1992b; McNollgast 1995).

<sup>4</sup> A survey of positive political theorists (conducted by Farber and Frickey 1992) makes clear that “considerable” disagreement exists over the meaning of the term PPT. Farber and Frickey (1992: 461) set out the following definition: “PPT consists of non-normative, rational-choice theories of political institutions.” For us, the key point is that PPTheorists typically adopt the three key assumptions of the strategic choice account.

<sup>5</sup> As Segal (1997: 29) notes, Eskridge (1991a: 643) called Marks’ study the “most notable paper,” while Rodriguez (1994: 91) termed it “the seminal contribution.” Bergara and his colleagues (1999: 1) recently deemed it the first “modern version” of the “strategic school,” despite the fact (as they acknowledge) that Marks treated the Court as exogenous and non-strategic.

<sup>6</sup> Game theory provides a potent set of tools to examine social situations involving strategic behavior, that is, situations in which the social outcome depends on the product of the interdependent choice of at least two actors.

be applied to important political problems.<sup>7</sup> Although this is obviously something scholars working in most fields of political science now take for granted, it was not so clear in 1958. Second, Schubert's work generated interest in other applications of the rational choice paradigm to legal questions. Or, at the very least, it encouraged specialists to think about the interdependent nature of judicial decision making. Two of the most proximate exemplars were Pritchett's (1961) *Congress versus the Supreme Court* and Murphy's (1962) *Congress and the Supreme Court*.

Prior to his work on Congress, Pritchett had published a seminal book, *The Roosevelt Court* (1948), which moved legal realism from the sole province of law school professors to political scientists. Like Holmes, Brandeis, and later adapters of socio-legal jurisprudence (e.g., Frank 1930; Llewellyn 1951), Pritchett argued that justices are "motivated by their own preferences," with rules based on precedent nothing more than smokescreens behind which they hide their values. Or, to put it in today's parlance, he was the first political scientist to view justices as "single-minded seekers of legal policy" (George and Epstein 1992: 325).

This intuition about the goals of justices served as the basis for Pritchett's (1961) and Murphy's (1962) books on Congress-Court interactions but, in those works, they pushed it one step further. Relying on interview data, Court cases, information collected from manuscript collections, congressional hearings, and the like, Murphy and Pritchett showed that if justices are single-minded seekers of policy, they necessarily care about the "law," broadly defined. And if justices care about the ultimate state of the law, then they may be willing to modulate their views to avoid an extreme reaction from Congress and the President. Murphy and Pritchett, in other words, tell a tale of shrewd justices, who anticipate the reactions of the other institutions and take those reactions into account in their decision making. The justices they depict would rather hand down a ruling that comes close to, but may not exactly reflect, their preferences than, in the long run, see Congress completely override their decisions.<sup>8</sup>

Pritchett's and Murphy's works on Congress and the Court, thus, adopted, however implicitly, the interdependent assumption of Schubert's "Hugberts" game, and, at the same time, underscored the importance of policy goals brought to light in *The Roosevelt Court*. But it was Murphy's (1964) *Elements of Judicial Strategy*, which came on the heels of the Congress-Court books, that most fully embraced the notion of strategic behavior and that explicitly found its grounding in rational choice theory.

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<sup>7</sup> Other articles published around the same time (e.g., Shapley and Shubik 1954) also pointed to the promise of game theory in political science.

<sup>8</sup> To put it in contemporary terms, Murphy and Pritchett used the intuitions of variants of the social-psychological paradigm (especially the attitudinal model, which we discuss later in the text) to study the relationship between Congress and the Court but they extended those premises and demonstrated that the resulting behavior may differ from what adherents of that paradigm might postulate.

The core arguments of *Elements* are the same ones that Schubert, Pritchett, and Murphy himself had advanced earlier: (1) justices are policy oriented; (2) they act strategically to further their goals; and (3) their interactions are structured by institutions. Moreover, the methodology mirrored his (and Pritchett's) earlier work: examination of Court memoranda and opinion drafts found in the private papers of several justices. The key contribution, instead, came in the blending of Schubert's earlier focus on internal decisionmaking, with Pritchett's (and his own) stress on the external constraints placed on the Court by Congress and the President: Under the *Elements* framework, not only does strategic interaction exist between the Court and the other branches of government but among the justices as well. As Murphy explained it (Murphy and Tanenhaus 1972: 25), *Elements* "took as its point of departure the individual Supreme Court justice and tried to show how, given his power as one of nine judges and operating within a web of institutional and ideological restraints, he could maximize his influence on public policy."

That Murphy arrived at these views is not all too surprising. By 1964, the rational choice paradigm was beginning to take hold in the political science literature, what with publication of Downs' (1957) classic work on political parties and Riker's (1962) on coalitions. And it is quite clear that Murphy was heavily influenced by some of this thinking. In the preface to *Elements*, he wrote (1964: x): "Almost as jarring to some readers as quotations from private papers will be my use of terms which are familiar to economic reasoning and the theory of games but which are alien in the public law literature." Alien perhaps but not completely unknown. The strategic assumption, as we have already documented, formed the basis of Schubert's Hughberts game.

### *The Demise of Rational Choice Theory*

Despite the fact that *Elements* made no attempt to analyze systematically the information it mined from manuscript collections<sup>9</sup> and, thus, lacked conclusive evidence to support a general claim about the predominance of strategic behavior, scholars found aspects of the work attractive. Or at least attractive enough to continue in its path. Particularly noteworthy was Howard's (1968: 44) examination of "fluidity," which attempted to provide more systematic support for one of Murphy's key observations: judges "work" changes in their votes and "permit their opinions to be conduits for the ideas of others" through "internal bargaining." Howard's methodology was similar to Murphy's in its reliance on a small number of important cases, but he cast his argument in general terms: "it may come as some surprise to political scientists how commonplace, rather than aberrational, judicial flux actually is." He further claimed that "hardly any major

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<sup>9</sup> Rather, *Elements* consists of stylized stories that in one way or another reinforced the central thesis.

decision [is] free from significant alteration of vote and language before announcement to the public.”

Howard's article was not the last of the post-*Elements* pieces. Into the next decade, analysts applied theories grounded in assumptions of rationality (especially game theory) to study opinion coalition formation (Rohde 1972) and jury selection (Brams and Davis 1976). In fact, by the 1970s, there had been enough work invoking game-theoretic analysis, in particular, that Brenner (1979) wrote a bibliographic essay devoted exclusively to the subject.

Perusal of the works on Brenner's list, however, reveals that most were not explicit applications of game theory or were conducted in the late 1960s. We do not have to search too long to explain this trend away from approaches that assume rationality. Scholars spurned strategic analysis in favor of four “determinants” of judicial behavior drawn from the social-psychological paradigm:<sup>10</sup> social backgrounds or personal attributes,<sup>11</sup> policy-oriented values and attitudes,<sup>12</sup> roles,<sup>13</sup> and small group influences.<sup>14</sup> To be sure, these approaches differ from one another at the margins. But, because they draw from the same paradigm (social-psychological), they are complimentary in their core beliefs about the way people make decisions. As Grossman and Tanenhaus (1969: 10-11) put it: “these hypothesized determinants can be traced back to the simple-action stimulus response model. . . . This S-R model, of which there are now several variants,

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<sup>10</sup> These were not the only theories of the 1960s but, according to writers of the day, they were the dominant ones. See, for example, Grossman and Tanenhaus (1969: 10) who write: “Hypothesized explanations of current interest for why judges act as they do fall into four broad categories: policy-oriented values, perceptions of role, background characteristics, and small group interaction” (see also Goldman and Jahnige 1976).

<sup>11</sup> This view holds that a range of political, socioeconomic, family, and professional background characteristics account for judicial behavior or, at the very least, help to explain the formation of particular attitudes (see, e.g., Nagel 1961; Schmidhauser 1962; Vines 1964; Ulmer 1970, 1973, 1986; Tate 1981; Tate and Handberg 1991).

<sup>12</sup> On this account, political attitudes toward issues raised in cases explain judicial votes (see, e.g., Pritchett 1948; Schubert 1962, 1965; Spaeth and Parker 1969; Goldman 1966, 1973; Rohde and Spaeth 1976). Schubert's version of this theory came from the psychometric research of Coombs and Kao 1960 and Guilford 1961. Later versions find their grounding in both the social-psychological and economic literatures (see, e.g., Rohde and Spaeth 1976, Chapter 4.).

<sup>13</sup> This perspective suggests that that normative beliefs held by judges about what they are expected to do either act as a constraint on judicial attitudes or directly affect judicial behavior (Becker 1966; Grossman 1968; James 1968; Glick and Vines 1969; Vines 1969; Carp and Wheeler 1972; Unga and Baas 1972; Wold 1974; Howard 1977; Jaros and Mendelsohn 1977; Gibson 1978). Judicial specialists adopted this theory from the work of Campbell 1963 and Rokeach 1968 (see Gibson 1978).

<sup>14</sup> Small group theorists assert that the “need to interact in a face-to-face context” affects the behavior of judges on collegial courts (Grossman and Tanenhaus 1969, 15; see, e.g., Snyder 1958; Danelski 1960; Ulmer 1965, 1971; Atkins 1973; Walker 1973). This hypothesis draws heavily on the work of experimental social psychologists on conformity, deviance, and leadership in small groups (see Ulmer 1971; Goldman and Sarat 1978: 491).

conceptualized the votes of judges as responses to stimuli provided by cases presented to them for decision” (see also Gibson 1978: 917).

Conceptualized in this way, the social-psychological paradigm is quite distinct from the strategic approach offered in *Elements*: While Murphy’s justices are preference maximizers who make decisions to further their goals with regard to the preferences and likely actions of other relevant actors and the institutional context, the S-R justices are policy seekers who further their goals with reference to their own normative and policy-based attitudes (see, generally, Barry 1978).<sup>15</sup> Even small group approaches, which seem to have more in common with strategic analysis than the others, lack clear-cut notions of interdependent interaction. More to the point, most scholars invoking this approach in their empirical work rely less on the rational choice model and more on variants of the social-psychological paradigm. For example, they note that judges occasionally conform to the behavior of their colleagues but not necessarily to further their goals; rather the mechanism seems to lie in the desire to retain friendly relations with colleagues, to “get along.”

In pointing out these differences, we do not mean to imply that strategic and social-psychological accounts of judicial decisions lack any commonalities. Both acknowledge the importance of goals<sup>16</sup> and institutions,<sup>17</sup> and small group theory is obviously concerned with group context. But the fact that many social-psychological approaches do not acknowledge a strategic component to decision-

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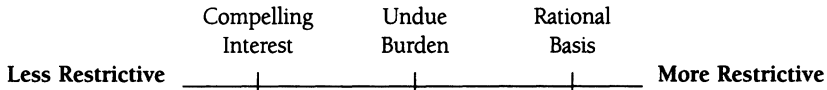
<sup>15</sup> It is interesting to note that most scholars of the 1960s and 1970s would not make this distinction. As we describe later in the text, back then key debates were between “traditionalists” and “behavioralists.” And while *Elements* fits neatly into neither of those camps it was the behavioralists who claimed the work. They classified it with a then-developing body of literature invoking socio-psychological theories of small group interaction, despite the book’s obvious strategic orientation. In so doing, behavioralists could then assert that *Elements* and the writings of other “small group” theorists were “compatible with the attitudinal approach and, in a general sense, provide additional evidence supporting the attitudinal thesis” (Goldman and Jahnige 1976: 192). Pritchett alone seemed to understand the theoretical underpinnings and implications of Murphy’s argument. In two essays, reviewing the field of public law, he (1968, 1969) listed Murphy’s work in a section called “Influence and Judicial Strategy,” and placed the discussion after a paragraph on game theory.

<sup>16</sup> Most rational choice scholarship on the judicial process has adopted the insight of the attitudinal model—that justices are “single-minded seekers of policy.” *But that need not be the case. When a researcher invokes a rational choice account, it is up to the researcher to specify the content of actors’ goals so as to give meaning to the assumption that people are “utility maximizers.”* This thus allows for the possibility that justices are motivated to further certain interpretive principles (the dominant assumption of most jurisprudential analyses of the Court), but it adds to it the proposition that justices may pursue those principles in a strategic way (see Ferejohn and Weingast 1992b). For more on the goals of justices, see Baum 1994, 1997.

<sup>17</sup> This is obvious for role accounts, which stress that various institutions (such as *stare decisis*) may constrain judges from acting on their preferences. But it is also true for contemporary attitudinalists who argue that life tenure (among other institutions) frees justices to vote their sincere preferences (see Segal and Spaeth 1993: 69; Segal 1997).



≡ FIGURE 1  
CHOICES OF LEGAL STANDARD IN ABORTION CASES



making is a point of distinction between the two and one that we cannot stress enough, for it can lead to very different predictions about judicial behavior.

Figure 1, which depicts three choices confronting a justice over which standard of review to apply in abortion cases, shores up this claim. Suppose justice X was to select among the three possible alternatives; further suppose that she sincerely prefers “compelling interest” to “undue burden” to “rational basis.” *Theoretically speaking*, if justice X is motivated in the way assumed by, say, those personal attribute models that suggest a direct connection between background factors and voting, the prediction is simple enough: She would always choose “compelling,” regardless of the positions of her colleagues.<sup>18</sup> That is because she makes decisions that are in accord with her background characteristics—characteristics that do not change after she has ascended to the bench. The strategic account, on the other hand, supposes that justice X might choose “undue burden” if, depending on the preferences of the other players (e.g., her colleagues), that would allow her to avoid “rational basis,” her least preferred outcome.

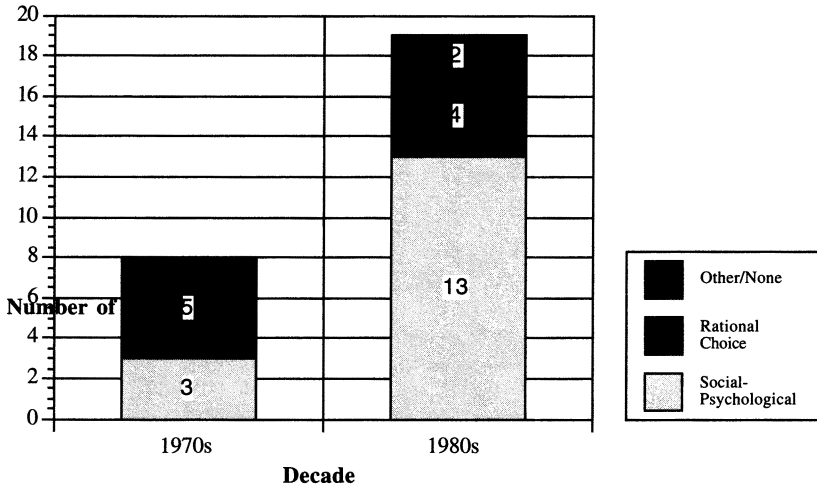
### *Explaining the Demise*

If there are any doubts that it was predictions from variants of the social-psychological model that predominated thinking about law and courts by the 1980s, Figure 2 should dispel them. There we display data on the number of judicial articles published between 1970 and 1989 in the *American Political Science Review* that invoked the social-psychological paradigm and others. The results are clear: Within 30 years of publication of *The Roosevelt Court*, work adopting variants of that paradigm was pervasive, accounting for fully 16 of the 27 articles published in the discipline’s flagship journal. During that same period, only two essays attentive to choice approaches appeared—one of which was a critical assessment (Smith 1988).

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<sup>18</sup> Advocates of the personal attribute model might argue that this statement misrepresents their perspective because it makes a prediction about what justices confronted with three choices would do, when their approach speaks only about the decisions to reverse or affirm. This claim has some merit in the sense that their *empirical* work has focused exclusively on predicting the dichotomous choice. But, from a theoretical vantage point, our statement accurately captures the spirit of the model—at least as it was articulated by some of its developers and of the social-psychological studies on which it is based.

≡ FIGURE 2  
 MAJOR THEORIES INVOKED IN ARTICLES PUBLISHED IN THE  
 AMERICAN POLITICAL SCIENCE REVIEW, 1970-1989



In all of this, the question remains: Why did scholars so fully embrace the social-psychological paradigm and so fully spurn the sort of strategic analysis Murphy conducted in *Elements*? Two answers come to mind. The first is from Schwartz (1997) who contends that the primary explanation lies in the notion of equilibrium predictions: “Murphy,” he claims, “only identifies strategies that *might* be pursued under some circumstances. Often such a pronouncement is immediately followed by a disclaimer that the contrary strategy might be more appropriate in other circumstances. The problem is that he derives no tight predictions about exactly when we should expect to see certain behaviors as opposed to others.”

Certainly there is some merit to this view. In direct contrast to other early advocates of rational choice theory, such as Downs (1957) and Riker (1962), Murphy did not write down any models and derive equilibria that others could go out and test—as did a multitude of scholars with the predictions contained in *An Economic Theory of Democracy* and *The Theory of Political Coalitions*. Even more to the point, Murphy’s “hypotheses” were a good deal more ambiguous than those offered by early adherents of social-psychological approaches.<sup>19</sup>

<sup>19</sup> Compare, for example, a Murphy hypothesis with one offered by Schubert:

Murphy (1964): “When a new Justice comes to the Court, an older colleague *might* try to charm his junior brother.”

Schubert (1960: 91): “In accordance with modern psychometric theory, which generalizes the

And, yet, scholars were able to glean predictions from Murphy's work and did attempt to test them. Surely this was true of work on vote fluidity (see Howard 1968; Brenner 1980) and, we might add, it is true of the current crop of strategic work, much of which explicitly identifies *Elements* as its starting point (see, e.g., Epstein and Knight 1998; Bergara, Richman, and Spiller 1999). In other words, Murphy may not have laid out predictions as boldly as did the other rational choice theorists or those who advocated variants of the social-psychological model. But within his work were sufficient intuitions of judicial behavior that other scholars could in turn "write down" models, solve them, develop behavioral predictions, and assess those predictions against data.

If it was not the lack of expectations that led scholars to snub the strategic account, what then? We think the explanation lies in the very nature of those tests and in the results they yielded—an explanation, we believe, that is a more faithful representation of the tenor of the times. For, during the 1960s, the great battles in the field of judicial politics were not between proponents of rational choice and of social-psychological approaches but between "traditionalists" and "behavioralists"; between those who believed that social scientists should develop generalizable explanations of social behavior and those who did not; and, increasingly, between those who believed scholars could quantify behavior and those who believed they could not (see Walker 1994). Given the dominance of the behavioralist perspective, to be a scientist in the world of judicial politics by the 1970s was to value data and to believe in the power of statistics. It is thus hardly surprising why scholars working in the social-psychological tradition triumphed over their strategically minded counterparts: Beginning with Pritchett (1948) and culminating with Segal and Spaeth (1993), they claim to have gathered a tremendous amount of systematic support for their theory. That is, unlike say Murphy or Howard, they typically refrain from detailed analyses of particular litigation (the *modus operandi* of the "traditionalists") and instead focus on large samples of Court cases—the dispositions of which they claim to predict with a good deal of success.

But there is more: Just as scholars were asserting that the key premises of variants of the social-psychological model held up against systematic, data-intensive investigations, they were also arguing that the Murphy-Howard strategic view did not withstand similar scrutiny. A critical work here is by Brenner (1980),

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basic stimulus-response point relationship, Supreme Court cases are treated as raw psychological data. . . . Each case before the Court for decision is conceptualized as being represented by a stimulus (*j*) point. . . . The combination of the attitudes of each justice toward these same issues also may be represented by an ideal (*i*) point...Obviously how the case will be decided will depend upon whether a majority or minority of the *i*-points dominate the *j*-point. If a majority of *i*-points dominate, then the value or values raised in the case will be upheld or supported by the decision 'of the court'; and if, to the contrary, the *j*-point dominates a majority of *i*-points, then the value or values raised will be rejected. . . ."

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which reassessed Howard's contention; namely, voting fluidity was rampant on the Court. Brenner compared votes cast in conference with those in the published records. Although he found minimal change in case disposition (about 15 percent), his results for vote shifts were rather dramatic: In over 50 percent of the cases did at least one justice switch his vote. Still, Brenner concluded that Howard was incorrect—that, in fact, considerable stability exists in voting. And it is this interpretation of Brenner's work that became the prevailing wisdom among judicial specialists (e.g., Goldman and Sarat 1989: 466; but see Baum 1995: 174). Thus—given Brenner's own rendition of his study, given the massive amounts of data analysts had gathered to support the social-psychological model, and given the significance political scientists in this field attached to large-scale statistical studies—it is easy to understand why decisionmaking theories grounded in assumptions of rationality failed to make any substantial showing in the political science journals of the 1970s and 1980s.

#### THE (RE)EMERGENCE OF THE RATIONAL CHOICE PARADIGM

We would be loathe to write that the tide has fully turned; surely that is not the case. Just five years ago, scholars were still claiming that “the attitudinal model is a, if not *the*, predominant view of Supreme Court decision making” (Segal et al. 1995: 812). But just as surely a change is in the wind, with Murphy's more strategically oriented approach beginning to take hold. The signs are everywhere. At the onset, we noted the existence of a growing and influential group of predominately law and business school professors, which advocates positive political theory (PPT) to study everything from relations among the branches of government (e.g., Eskridge 1991a, 1991b) to internal decision making (e.g., Kornhauser 1992a, 1992b; Stearns 2000). And the approach is now emerging in the political science journals and at conferences (see note 1).

Why the change? Why, after decades of invoking variants of the social-psychological paradigm, are political scientists who study courts now gravitating toward strategic analysis? We believe there are four answers to this question.

#### *Planting the Seeds of the “Revolution”*

The PPT theorists of today, as we have already mentioned, locate the onset of the “strategization” of judicial politics with Marks (1989). Of course, Murphy preceded him by 25 years but it was Marks who galvanized the PPT scholars, and it was they who galvanized political scientists who study courts and laws. They did so by:

- (1) Holding conferences to which they invited mainstream political scientists who study courts (read: advocates of the social-psychological model). E.g., the 1993 Conference on the Political Economy of Public Law, held at the University of Rochester.

- (2) “Shaming” those same mainstream scholars, for the PPT group includes not only law school professors but also several political scientists (e.g., Charles Cameron, John Ferejohn, and Barry Weingast) who rose to prominence as students of Congress or the Executive—*rather than of courts*. Shapiro (1995: 1) makes this point when he writes:

As a glance at the job listings will show, many of us in the [Law and Courts] section are employed today in slots labeled “American politics. We will consider persons with specializations in Congress, parties, elections, the Presidency, or judicial politics.” Public choice has become a prominent, and in some departments dominant, movement in the American politics field. In the past law and courts people have typically come very late to new methodologies that have swept other fields of political science. Now that for many of us, whether we like it or not, American politics is “our field,” it behooves us to at least take a good look at a dominant method of “our field.”

- (3) Publishing articles in prominent law reviews (read: ones that political scientists peruse) (e.g., the *Harvard Law Review* in which the 1993 foreword to the annual feature on the Supreme Court, titled “Law as Equilibrium,” was written by Eskridge and Frickey [1994]—two leaders of the PPT movement); penning books, published with influential university presses such as Harvard (Baird, Gertner, and Picker 1994), Yale (Mashaw 1997), and Chicago (Farber and Frickey 1991), which did not require a background of math or economics to read and appreciate; and, producing edited volumes that bring together much of the seminal work (e.g., Stearns 1997).

Of course, such influence is not unprecedented: Political scientists who study law and courts have often looked to lawyers for their theoretical grounding. When law schools were advocating positivist (or analytical) jurisprudence, our writings followed suit (see, e.g., Cushman 1929). When the legal realists of the 1920s and 1930s rejected positivism for sociological jurisprudence, many political scientists (beginning with Pritchett) too abandoned analytical approaches and began to develop more “realistic” models of judicial decision making. Indeed, Segal and Spaeth (1993) trace—and rightfully so—the attitudinal model back to the legal realists of the 1920s and 1930s, most of whom were lawyers.

In some sense, then, history seems to be repeating itself. Just as positivism and sociological jurisprudence emerged from the halls of the nation’s law schools, so too has PPT. And just as social scientists adapted the premises of those lines of thinking to their work, so too are they attending to the lessons of strategic analyses.

#### *Providing a Unified Framework*

When describing what he liked about choice accounts, Shapiro (1995: 1) had this to say:

What interests me about public choice is not its methodology, which I can take or leave, but its subject matter when it turns to law and politics. Public choice people are asking the right questions, questions that very few people in the law and courts field seem to be asking. . . . I must confess to some degree of angry frustration that people not in the law and courts field, who in fact are now madly scrambling to learn the law we have known all along, are doing some of the most interesting work in law and politics—are asking the questions we ought to be asking and seldom do.

We might put it another way: The strategic account not only raises interesting questions about the range of judicial behavior (and not just the vote). It also provides a unified framework, a set of tools really, for addressing them.

In making this claim, we and other advocates of the strategic account are not saying that scholars have ignored the external (e.g., the interactions between courts and executives) and internal (e.g., the assignment of the majority opinion) contexts of judicial politics. Clearly they have not. What we are instead contending is this: When scholars adapted variants of the social-psychological account (especially the attitudinal model) to their empirical work, they were interested in explaining only the judicial vote. Accordingly, political scientists fashioned a range of mid-level explanations to account for other choices that jurists make—explanations that typically emanated from the findings of previous empirical investigations, rather than from any unified theoretical underpinning. Consider, for example, the long line of work demonstrating that phenomenal success of the United States (as represented by the Solicitor General) at the Supreme Court's case selection stage. Provine (1980: 85) shows that during the 1947-1957 terms the federal government submitted 554 petitions for review; the justices granted fully 66 percent—a figure well-above their overall grant percentage for the period (9.6). Caldeira and Wright's (1988) research, which took into account a range of explanations for the certiorari decision including whether or not the United States was a petitioner, reaches a similar conclusion: Even if a government petition presents no evidence of real conflict, the probability of Court review is a staggering 37 percent—compared with an average review rate of 8 percent for the term Caldeira and Wright examined.

Since these findings do not sit comfortably with social-psychological approaches,<sup>20</sup> scholars have offered various and, occasionally, idiosyncratic explanations. Provine (1980: 82) writes that "Because of their quality, the clerks and justices probably read [U.S.] petitions with special care", Caldeira and Wright (1988: 1121) suggest that the "solicitor general's expertise is evidently highly respected by the justices."

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<sup>20</sup> The same can be said of results showing the remarkable success of the Solicitor General on the merits of cases (see Segal and Spaeth 1993: 237-38).

The strategic account, in contrast, has the benefit of providing us with the following intuition for making sense of the “SG” puzzle: Just as jurists are loathe to take cases that may lead to the creation of policy that they dislike or to collisions with Congress and the President (Epstein and Segal 1997), they are equally loathe to ignore disputes that the government wants them to resolve, even if they believe that they would be unable to set policy on their ideal points. For avoiding such cases might generate a backlash equally as great as deciding cases against the interests of other governmental actors. This proposition follows from the fact that perceptions that the Court is dodging its responsibilities may generate attacks on its institutional authority—attacks that, in turn, may effect the ability of policy-oriented justices to achieve their goals. Hence, in an effort to maximize their preferences, justices have good reason to pay some attention to the SG’s request to resolve certain disputes.<sup>21</sup>

This is but one example. Our broader claim is that the strategic account not only helps us to elucidate the “right” questions but it also provides a methodology to help scholars answer them, to understand the range of choices that contribute to the development of law.

#### *Documenting Strategic Interaction on Courts*

While we believe that the PPT group has served as a catalyst for the injection of choice theory into the study of judicial politics and has raised the “right” questions, we also believe that the PPT theorists would have had virtually no sway over mainstream judicial specialists in the absence of empirical evidence. To see this point, we only need ask whether the legal realists of the 1920s would have influenced the course of political science work on courts had Pritchett been unable to provide systematic support for their claims. We think the answer is no.

The need for empirical support for the plausibility of the strategic account may have been even more important because at least one of the assumptions of that account—that jurists are strategic actors—is not a part of the social-psychological paradigm. Hence the question emerged: Since vast amounts of data support the predictions of variants of the social-psychological model, how can an assumption that is at the very least not part of that model and on many accounts competes with that model be plausible? Compounding matters was that strategic advocates (e.g., Murphy 1964; Howard 1968) did not do much to make the assumption of interdependent interaction more palatable. They offered little in the way of systematic support for this underlying premise, and instead either (1)

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<sup>21</sup> We should note that the other reasons scholars have offered for the SG’s success at the certiorari stage do not preclude the possibility that fear of retaliation plays some role in explaining the Court’s unusual willingness to hear cases the SG wants it to resolve. Even more to the point, they provide support for it. After all, why would the justices read U.S. petitions with “special care” if they were not concerned with the response a denial might engender?

assumed the existence of strategic interaction and developed their models accordingly or (2) invoked highly stylized facts to demonstrate its empirical plausibility.<sup>22</sup> Such was insufficient for many analysts who study courts. In light of the dominant methodological standards in contemporary political science analyses of the judicial process, these scholars required more than mere statements that judges are strategic; they needed evidence of the widespread existence of strategic decisionmaking.

As Table 1 shows, that has now occurred. A growing body of research, conducted over the past few years, has provided documentation of American judicial behavior that is *consistent* with the assumption of strategic interaction. That a healthy proportion focuses on the U.S. Supreme Court is not all too surprising given the emphasis of the law and courts field for the last 50 years. But neither has research been limited to the Nation's highest Court. And we suspect that scholars will conduct even more strategic analyses of circuit court judges, what with the release of the Songer U.S. Court of Appeals Data Base and, eventually, of state supreme court justices, what with the preparation of the Hall/Brace public data base.

The more general point, though, is this: Taken collectively, these studies show that interdependent interaction is a fundamental part of the process by which jurists reach decisions. They relay to scholars that they can feel confident in the empirical basis of the strategic assumption and, thus, can invoke it to answer a range of important substantive questions about the nature of court decision making.<sup>23</sup>

### *Bridging Gaps among the Fields*

Given the importance political scientists who study courts attach to systematic evidence, we have little doubt that the documentation of judicial behavior

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<sup>22</sup> Important exceptions here were two early works by Hall (1987, 1992), which provided systematic evidence of sophisticated behavior on the part of state supreme court justices in death penalty cases. See also Spiller and Gely 1992.

<sup>23</sup> A clear exception here is trial courts to which few, if any, scholars have applied the strategic account. While scholars have invoked PPT-type approaches to study juries, parties, and attorneys (see, e.g., Schwartz and Schwartz 1995; Baird et al. 1994; Reinganum 1988; and cites contained in Songer, Cameron, and Segal 1995: 1120-21) they have yet to turn their attention to the choices made by trial court judges. This is unfortunate since we believe that the strategic account could be of enormous value in helping us to understand politics on these courts.

We should also note the existence of one, albeit important study, that fails to find evidence of strategic behavior on the part of justices—Segal 1997. This research provides a good deal of empirical evidence to show that “the institutional protections granted the Court mean that with respect to Congress and the presidency” the justices almost never need to vote other than sincerely. Whether Segal's conclusion will hold as scholars continue to produce research on this important topic (see, for example, Martin 1997; Bergara, Richman, and Spiller 1999), we cannot say at this point. But, as Table 1 depicts, the great bulk of the research to date surely supports the plausibility of strategic behavior on the part of judges.



≡ TABLE 1  
STUDIES OF AMERICAN COURTS PROVIDING EMPIRICAL SUPPORT FOR THE  
PLAUSIBILITY OF THE ASSUMPTION OF STRATEGIC INTERACTION

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Studies of the U.S. Supreme Court	Studies of the U.S. Courts of Appeals	Studies of State Supreme Courts
Eskridge 1991a, 1991b	Songer, Segal, and Cameron	Hall 1987, 1992
Spiller and Gely 1992	1994	Hall and Brace 1989, 1992
Cohen and Spitzer 1994	Van Winkle 1996, 1997	Brace, Hall, and Langer
Boucher and Segal 1995	Smith and Tiller 1997	1996
Epstein and Knight 1995, 1998	Cross and Tiller 1998	Spiller and VanderBergh
Maltzman and Wahlbeck 1996a, 1996b	George 1998	1997
Schwartz 1996		Langer 1997
Martin 1997		
Spriggs, Wahlbeck, and Maltzman 1997		
Wahlbeck, Spriggs, and Maltzman 1998		
Epstein, Martin, Caldeira, and Segal 1998		
Bergara, Richman, and Spiller 1999		
Caldeira, Wright, and Zorn 1999		

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consistent with strategic interaction played an important role in the sea change. Perhaps even more important is that scholars have moved beyond the documentation stage; they are now arguing that strategic analysis has substantial benefits. Certainly some of those come from providing us with answers to the “right” questions. But another centers on bridging gaps between the fields.

Shapiro makes this point about the field of American politics when he notes that, for the first time in recent memory, students of legislatures and executives are now incorporating the judiciary into their analyses.<sup>24</sup> That this is occurring at the same time as the movement toward strategic analysis in the law/courts field is no coincidence. For years, judicial specialists have tried to entice general Americanists to integrate courts into their studies. But their calls went unheeded, we suspect, because these scholars did not view the existing social-psychological approaches as all that relevant to *their* branches of government. (At least since Mayhew [1974], not many Congress specialists argue that legislators make

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<sup>24</sup> Of course, some of these scholars were among the first to advocate the injection of strategic analysis into studies of courts (see note 3).

decisions solely on the basis of their own ideological preferences or background characteristics.) But with the movement toward the strategic account, the field of judicial politics, as Shapiro suggests, has entered a terrain that could not be more familiar to most Americanists.<sup>25</sup> To mix metaphors, perhaps for the first time in contemporary political science, scholars of the courts and their legislative and executive counterparts are beginning to speak the same language.

Quite a similar phenomenon is occurring in the field of comparative politics. For decades, court specialists have complained about the dearth of comparative judicial research, with many urging their counterparts in comparative politics to join in efforts to fill the void. And for years they went ignored at least in part because the comparative field (just as American politics) went in a much different direction than did law and courts. As judicial specialists continued to work within the social-psychological paradigm, many of their comparative colleagues formed commitments to strategic analysis.<sup>26</sup>

The “revolution” within the judicial field has radically changed this situation. Once again, it is not a coincidence that comparativists, for the first time in any one’s memory, are beginning to study courts—and to do so through a rational choice lens. Indeed, of the eight comparative papers presented on law and courts panels at a recent meeting of the American Political Science Association, four invoked the logic of strategic analysis. Much of this work, we hasten to note, is being produced by newly minted Ph.D.s (see, e.g., Helmke 1999; Vanberg 1999) rather than their more senior counterparts.

#### WHAT’S NEXT?

However promising the future is for injection of rational choice into the study of judicial politics, the move toward the “strategization” of the field will not be without its share of debates. Already one is developing over the question of “how to do strategic work.” On the one side are analysts who are translating the strategic intuition into variables that they include in their statistical models of judicial behavior (e.g., Spriggs, Wahlbeck, and Maltzman 1997). Mostly these are scholars who were not trained as formal theorists but, instead, were schooled in the judicial politics literature. On the other side are those scholars, largely trained as formal theorists, who take the position, in its strongest form, that rational choice work must embody formal “equilibrium” analysis; rational choice work, in other words, is not rational choice work unless the analyst has written down and solved a formal model (Schwartz 1997).

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<sup>25</sup> Over 50 percent of the 130 articles on American politics (excluding judicial politics), published in the *American Political Science Review*, *American Journal of Political Science*, and *Journal of Politics* between 1994 and 1995, invoked rational choice accounts.

<sup>26</sup> See, e.g., Bates 1997: 699, asserting that “the rational choice approach has indeed invaded the field [of comparative].”

As each side plays to its own competitive advantage, we seem to be reaching an “either or” state. We do not see it that way. To us, there are a number of ways to “do” strategic analysis. Here we consider four: (1) incorporating the logic of strategic action into interpretive-historical research, (2) invoking the strategic account to construct conceptions of judicial decision making; (3) using micro-economic theories to reason by analogy; and (4) undertaking formal equilibrium analysis.<sup>27</sup> Below we briefly review them and, along the way, incorporate some substantive suggestions for future research programs. But the basic point we wish to make is as follows: The degree of formalization of the analysis depends on the nature of the explanation that is desired. While formalization is often crucial to explanations of judicial behavior, we want to emphasize the basic idea that strategic behavior is a broader and more extensive phenomenon than that which is captured by formal equilibrium analysis.

#### *Incorporating the Logic of Strategic Action into Interpretive-Historical Research*

Strategic analysis, as we imply above, is not synonymous with formalization; various forms of strategic behavior can be fruitfully analyzed without a formal model. Works including Schelling’s (1960) *The Strategy of Conflict* and North’s (1990) *Institutions, Institutional Change and Economic Performance* are two obvious and seminal illustrations. Many different sorts of studies of courts, therefore, can benefit from the mere incorporation of the logic of strategic action and can, in so doing, significantly enhance our understanding of judicial decision making.

Interpretive historical research is an obvious example, although its practitioners occasionally deny this. Such was Gillman’s (1996-97) reaction to our essay on *Marbury v. Madison* (1803), which analyzed the relationship between President Thomas Jefferson and Chief Justice John Marshall in strategic terms (Knight and Epstein 1996). To Gillman, our work simply confirmed a view of the case advocated by “interpretive scholars for as long as we can remember” (1996-97: 9). We disagree, believing instead that this study (and similar ones; see, e.g., Epstein and Walker 1995) demonstrates the importance of detailed and systematic analyses of the strategic interactions in which justices engage. In fact, in terms of the *Marbury* case, it seems clear that most prior studies misunderstood the strategic

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<sup>27</sup> As we note above, some suggest that research translating the strategic intuition into variables—that is, research of the sort produced by Wahlbeck and his colleagues (Wahlbeck, Spriggs, and Maltzman 1998; Maltzman and Wahlbeck 1996a, 1996b; Spriggs, Maltzman and Wahlbeck 1997)—is another way to do strategic analysis. To the extent that this work represents a transitional bridge between traditional behavioral research and strategic analysis, we agree. To the extent that future scholars might undertake this kind of research in an effort to provide a response to a standard criticism of rational choice research—namely, the empirical predictions of formal models are not subjected to sufficient systematic testing (Green and Shapiro 1994)—we disagree. Investigators framing their work in these terms should derive their hypotheses as implications of formal models.

problem facing Marshall. He was not, as many maintain, a justice shrewder than his presidential opponent; rather, he was merely a strategic political actor who accommodated a political environment that was strongly weighted against him. But, without close attention to the strategic implications of the beliefs that both Marshall and Jefferson held about the possible consequences of their actions, the strength of that conclusion would have been difficult to demonstrate.

More generally, we believe that at least two areas of interpretive-historical research would benefit from such an explicit incorporation of the logic of strategic action. The first is work centering on preference formation. Interpretive-historical scholars argue that their approach is especially well suited to answering questions on the evolution of ideological values and preferences. Since formal models of strategic decision making do not generally address preference formation, there is considerable merit to this claim. But this does not mean that strategic behavior has no role to play in the process of preference formation. To the extent that preferences affect choice and, thus, judicial outcomes, good reasons exist to believe that political actors will attempt strategically to influence preferences. Thus, researchers who employ interpretive-historical methods to explain the evolution of ideology and preferences would benefit from considering the strategic dimensions of this political process.

This basic point about the relationship between politics and strategic behavior is important for a second area in which we can beneficially combine the two approaches. The interpretive-historical approach has been a rich source of studies on the long-term development of legal doctrine. What is striking about many of these accounts is the *implicit* use of strategic behavior to trace the development of the content of law (e.g., Alfange 1994). We think that an *explicit* acknowledgement of the strategic dimension of this historical process would enable these scholars to refine their claims—both about the motivations of political actors and the implications of the strategic interactions that influence judicial outcomes. Such refinements would enhance the persuasiveness of these accounts, not to mention their generalizability.

Some analysts committed to interpretive-historical research, we realize, will continue to resist this argument but others are already embracing it—with Stearns (2000) the most recent exemplar. In *Constitutional Process*, Stearns wields, with great skill, the tools of social choice theory to illuminate doctrinal anomalies that many constitutional law scholars do not typically acknowledge, much less attempt to address (e.g., how majorities on the Court occasionally “empower” minorities to define the direction of constitutional doctrine, with *Planned Parenthood of South-eastern Pennsylvania v. Casey* [1992] an illustration). At the end of the day, Stearns provides substantial support for one of his (and our) key claims: If we want to develop a full understanding of constitutional doctrine, we avoid social choice theory at our own peril, for we cannot understand outcomes unless we understand the collective nature of the decision making process that undergirds them.

*Using the Strategic Account to Construct Conceptions of Judicial Decision Making*

Quite apart from studies that seek to explain particular cases or historical events are those that attempt to develop a general picture of judicial choices, to outline a conception of the mechanisms of strategic behavior that characterize decision making on courts. If these are the tasks of the research—to analyze the basic logic of strategic action, identify the ways in which it is manifest in the choices of judges, and provide a framework for understanding its implications for research on the courts—then an understanding of the mechanisms through which strategic action influences collective outcomes is essential. Such an understanding, we believe, does not require formal equilibrium analysis; rather it can be generated by using the strategic assumption as a starting point for the research.

Work along these lines abounds in the legislative field—with Mayhew's *The Electoral Connection* a prime example. In that seminal study, Mayhew (1974: 5-6) sought to “conjure up a vision of United States congressmen as single-minded seekers of reelection, see what kinds of activity that goal implies, and then speculate how congressmen so motivated are likely to go about building and sustaining legislative institutions and making policy.” Accomplishing these tasks did not require the development of formal models but, instead, begged for an exercise in logic, in creative thinking. Much the same thing could be said of Fenno's (1973) classic *Congressmen and Committees*.

Within the judicial field, we would point to our work, *The Choices Justices Make*, which attempts to follow the examples set by *The Electoral Connection*, *Congressmen and Committees* and, of course, *Elements of Judicial Strategy*: We develop a picture of justices as strategic seekers of legal policy and explore how justices so motivated go about making choices. In so doing, we (like Mayhew and Fenno) do not rely on formal models, which may surprise (disappoint?) scholars who push for this form of analysis. But, again, such surprise would reflect a misunderstanding of the nature of our enterprise. Because the stated goal was to develop a picture of judicial choice, we have accomplished it if *Choices* provided a basis for incorporating strategic choice into various approaches to studying courts.

Finally, note our use of the word “courts” rather than “the Court.” This reflects our belief that while *Choices* focuses on the U.S. Supreme Court justices, this mode of analysis could easily be applied to other jurists. In fact, because we possess so little in the way of generalizable knowledge about trial court judges, we think it would especially fruitful to begin investigations of their choices in the same sort of way that Murphy and we went about exploring Supreme Court justices or that Mayhew studied members of Congress: by invoking the strategic assumption as a launching pad for the research.

Much the same applies to the study of courts elsewhere. While scholars are finally beginning to pay some attention to these tribunals, systematic efforts remain far and few between (see Hull 1999). Judicial specialists attribute this persistent and stubborn bias against judiciaries abroad to many factors, not the

least of which is the lack of “a widely accepted paradigm [with] which to model the relationship between law, courts, and politics in a cross-national context” (Jacob 1996: 2). The strategic account, provides such a paradigm or, at the very least, a set of theoretical tools that comparativists, as we noted earlier, find highly appealing and that can serve to connect their work and ours—with the development of conceptualizations of judicial choice a logical starting point.

*Invoking Micro-Economic Theories to Reason by Analogy*

Reasoning by analogy presents yet a third way to conduct strategic analyses. This approach is common in the field of international relations, where scholars have related international politics to microeconomic models.<sup>28</sup> They construct the analogy by postulating a number of empirical correspondences:

economic marketplace ↔ international system  
firm ↔ nation-state  
firms maximize profits ↔ states maximize survival  
oligopolists ↔ great powers  
market concentration ↔ concentration of power  
price wars ↔ military wars  
  
both are self-help systems  
firms and states both act strategically

From these, scholars draw inferences from economic theory to international politics. For example, since economic theories suggest that oligopolitical market concentration leads to market stability and fewer price wars, we might reason that a concentration of power in the international system leads to system stability and less international conflict.

Judicial specialists can also use this approach to develop new understandings of courts and law; in fact, they are already so doing, with work by Songer, Segal, and Cameron (1994) exemplary (see also Van Winkle 1997). Here the researchers develop an analogy between principal-agent theory and the judicial hierarchy based on the following empirical correspondences:

economic marketplace ↔ judicial system  
principal ↔ U.S. Supreme Court  
agent ↔ U.S. Courts of Appeals  
utility maximizing economic actors ↔ policy maximizing judges  
  
economic actors and judges both act strategically

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<sup>28</sup> We adopt this discussion from Snidal 1986: 31-32.

With the analogy in hand, Songer and his colleagues develop a number of interesting propositions. To wit: Since principal-agent theory suggests that monitoring by the principal should influence the behavior of the agent, we might infer that monitoring by the Supreme Court enhances the responsiveness of the Courts of Appeals (1994: 688-89).

Seen in this way, the “inference” becomes something of a hypothesis; indeed, Songer et al. go on to operationalize their “inference” and test it against data. Herein, though, lies a potential problem: While the status of the inference may be that of a hypothesis, it is of a hypothesis whose plausibility depends entirely on whether the empirical correspondences are valid. In other words, whether we think the Songer et al. prediction a compelling one depends entirely on whether we think the analogy on which the researchers base it is a strong one.

This is not to say that reasoning by analogy is an inappropriate method for developing an understanding of judicial politics. The Songer work certainly suggests otherwise: The analogy it offers is sensible, the inferences it draws are quite interesting, and the empirical evidence strong. Moreover, since there are so many well-developed theories that assume strategic behavior on the part of actors—with at least some of them applicable to judicial problems—scholars can reason by analogy to study a range of behavior. What we are instead saying is that analysts ought to use this approach with due care. For reasoning by analogy to advance our understanding of courts and law, the correspondence between the underlying model and judicial situation must be a tight one. If it is not, errors of inference will abound.

#### *Undertaking Formal Equilibrium Analysis*

Thus far, we have stressed that strategic analysis is not synonymous with formal modeling. But that does not diminish the importance of formal analysis for many central issues related to courts. If scholars want to explain a particular line of decisions or a substantive body of law as the *equilibrium* outcome of the interdependent choices of the judges and other actors, they must demonstrate why the choices are in equilibrium, and a formal model is an essential feature of such a demonstration.

Given space limitations, we cannot provide even a cursory lesson on formal modeling; besides, there are already many good books on the subject (e.g., Kreps 1990; Gibbons 1992; Morrow 1994). Let us instead describe two kinds of modeling enterprises that may be useful to scholars of judicial politics: game theory as a descriptive tool and as an analytical tool.<sup>29</sup>

*Game Theory as a Descriptive Tool.* The first enterprise centers on the use of game theory as a descriptive tool—typically to study individual interactions of

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<sup>29</sup> We adopt these distinctions from Snidal (1986: 27).

≡ TABLE 2

BRAMS' OUTCOME AND PAYOFF MATRICES OF THE WHITE HOUSE TAPES GAME

**2a. Outcome Matrix**

		Nixon	
		Comply with Court	Defy Court
<b>Burger and Blackmun</b>  Decide for President; Create a "weak" 6-2 decision	I. Constitutional crisis averted; Nixon not impeached for non-compliance; majority-rule principle preserved.	II. Constitutional crisis; Nixon impeached but conviction uncertain.	
	Decide against President; create a unanimous 8-0 decision	III. Constitutional crisis; Nixon impeached and conviction certain.	
IV. Constitutional crisis averted; Nixon not impeached for non-compliance; majority-rule principle possible weakened.			

**2b. Payoff Matrix**

		Nixon			
		Comply (C) Regardless	Defy (D) Regardless	C if F, D if A	D if F, C if A
<b>Burger and Blackmun</b>  Decide for President (F)		(4,3)	(1,4)	(4,3)	(1,4)
	Decide against President (A)	(3,2)	(2,1)	(2,1)	(3,2)

Note: (x,y) = (Burger/Blackmun and Nixon); 4 = best; 3 = next best; 2 = next worst; 1 = worst.

Source: Brams 1990, 181, 183.

historical interest. Brams' (1990) investigation of the Watergate case, *United States v. Nixon* (1974), provides an interesting illustration.

In constructing the "White House Tapes Game," Brams relies on a detailed historical analysis of events surrounding Nixon. This leads him to the "outcome" and "payoff" matrices depicted in Table 2. (We direct readers to Brams 1990: 175-87 for his interpretation of the historical events and the specifics of the game. Suffice it to note here that Brams believes that Burger and Blackmun [B&B] sincerely preferred to vote in favor of President Nixon because they voted against granting certiorari in the case.)

Note that Brams' game-theoretic analysis not only seems to replicate history but it provides additional insights as well. On this account, Burger and Blackmun voted against the President—not because it was their most preferred position—



but because they anticipated Nixon's choice. Realizing that Nixon would choose to defy the Court if they decided in his favor, B&B selected A to maximize their payoff (3 rather than 1). But also note that the justices (and Nixon) could have done better had they believed that the President would have complied with a Court decision. If that were the case, B&B would have cast sincere votes (yielding a payoff of 4 rather than 3). But Nixon's failure to induce such behavior led to a Pareto-inferior Nash equilibrium ([3,2] rather than [4,3]).

Hence, Brams' analysis overcomes a major criticism of this sort of modeling exercise: that replications of history are largely descriptive enterprises that do not tell us very much more than we already know. Yet it cannot overcome others. For one thing, because Brams relies on the "totality" of the case (including its outcome) to create the model, his use of game theory does not produce empirically falsifiable predictions. For another, while Brams attempts a careful analysis of the historical "facts" surrounding the case, he gets them wrong. From Justice William J. Brennan's dockets, we now know that it was Blackmun and White (not Burger) who voted against certiorari. Following Brams' logic, it was thus White and Blackmun who sincerely preferred to rule in favor of the President—an assumption that many scholars (at least for White) would find hard to swallow and one that does not square with Brennan's conference records.

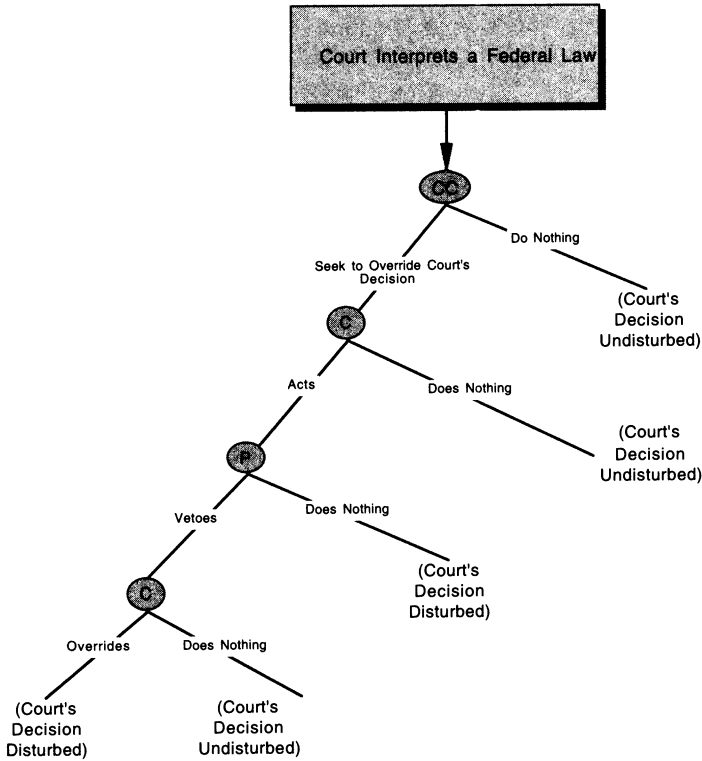
These problems noted, we do not wish to discourage scholars from invoking this kind of analysis. As we mention above, using game theory in this way can lead to interesting insights into important historical problems. Moreover, at least some of the criticisms of this approach are not indictments of game theory per se but rather of the ability of the researcher. That Brams got the facts wrong, for example, is only a measure of his skills in reconstructing history and not a test of strategic analysis.

*Game Theory as an Analytic Tool.* The use of game theory to "reconstruct" individual interactions has, to be sure, its benefits. But we would agree with those scholars who argue that game-theoretic analysis is at its most potent when investigators use it to generate new and more general understandings (and empirically testable behavioral predictions) of politics.

Many fields in political science have benefited from this sort of modeling enterprise but it has been slow to take effect in the world of judicial politics. Indeed, to our knowledge, there have been only a handful of attempts (e.g., Caldeira, Wright, and Zorn 1999; Eskridge 1991a, 1991b; Schwartz 1992, 1996; Ferejohn and Weingast 1992a; Tiller and Spiller 1999). But those that do exist well illustrate the power of the approach.

Take Eskridge's separation of power (SoP) games. In considering the course of civil rights policy in the United States, Eskridge identified many Supreme Court decisions that would be difficult to explain if the justices voted solely on the basis of their own policy preferences (e.g., instances of the relatively conservative Burger Court reaching liberal results). Hence, the question emerged: If not

≡ FIGURE 3  
 ESKRIDGE'S VERSION OF THE SEPARATION OF POWERS GAME



Note: CC = Congressional Committees; C = Congress; P = President.

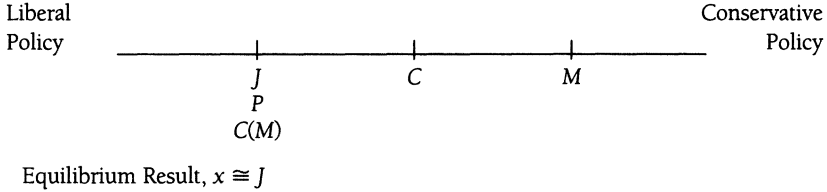
Adapted from: Eskridge 1991, 644.

straight preferences, then what? Eskridge's intuition, based on the work of Marks (1989) and others, is that the SoP system induces strategic decisionmaking on the part of Supreme Court justices. In other words, if the goal of justices is to establish policy for the nation that is as close as possible to their ideal points, they must take into account the preferences of other relevant actors (here, Congress and the President) and the actions these others are likely to take. Justices who do not make such calculations risk congressional overrides and, thus, of seeing their least preferred policy become law.

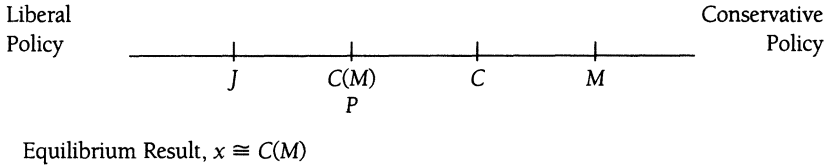
Eskridge formalized this intuition in his Court/Congress/President game (a/k/a SoP game) that unfolds on a one-dimensional policy space over which the

≡ FIGURE 4  
HYPOTHETICAL DISTRIBUTIONS OF PREFERENCES

**Figure 4a. Unconstrained Court**



**Figure 4b. Constrained Court**



*Note:* J is the justices' preferred position based on the attitudes of the median member of the Court; M and P denote, respectively, the most preferred positions of the median member of Congress and the president; C is the preferred position of the key committees in Congress that make the decision of whether or not to propose legislation to their respective houses; and C(M) represents the committees' indifference point (between their preferred position and that desired by M).

*Adopted from:* Eskridge 1991b.

relevant actors have single-peaked utility functions. All these actors, Eskridge assumes, have perfect and complete information about the preferences of the other actors and about the sequence of play. As Figure 3 depicts, the Court begins the game by interpreting federal laws. In the second stage, legislative gatekeepers (congressional committees/leaders) can introduce legislation to override the Court's decision; if they do, Congress must act by adopting the committees' recommendation, enacting a different version of it, or rejecting it. If Congress takes action, then the President has the option of vetoing the law. In this depiction, the last "move" rests again with Congress, which must decide whether to override the President's veto.

By invoking simple spatial models, Eskridge notes the existence of two different regimes with regard to the Court (illustrated in Figure 4): one in which the Court is not constrained by one or more political actors and one in which it is. Based on the ideal points depicted in Figure 4a, the equilibrium result is  $x \cong j$ .

In other words, the Court is free to read its sincere preferences into law.<sup>30</sup> Figure 4b yields a very different expectation. Because the Court's preferences are now to the left of  $C(M)$ , it would vote in a sophisticated fashion to avoid a congressional override; the equilibrium result is  $x \cong C(M)$ .

These regimes, at least according to Eskridge, not only provide us with insight into some important and seemingly anomalous Burger Court decisions in the area of civil rights but they also hold important lessons about the Court's decisionmaking process. For example, in interpreting legislation, we learn that the intent of the enacting Congress is far less important to preference-maximizing justices than are the preferences and likely actions of the current one. Do these results hold over larger samples of cases? Many scholars suggest that they do (e.g., Spiller and Gely 1992; Bergara, Richman, and Spiller 1999) but at least one, Segal (1997), argues that they do not (see note 23).

Segal's study certainly raises interesting questions about whether justices are strategic with regard to Congress. Yet, to reject SoP games based on his research would be a mistake. That is, Segal's findings may be less of a death knell for the strategic account and more of an indictment of Eskridge's particular modeling approach. Perhaps it is too much to believe that Supreme Court justices have perfect and complete information about the preferences and likely actions of members of Congress or the President. If that is so, then we should not discard the model altogether but write down one with more realistic assumptions. And the very fact that scholars can do so points to an important benefit of this sort of modeling enterprise: Because it forces analysts to make clear the assumptions under which they are operating, others can evaluate and, if necessary, revise them. That has already occurred with the SoP games: Martin's (1997) research reveals that when the assumption of complete and perfect information is relaxed, neither Segal nor Eskridge gets it exactly right.

#### DISCUSSION

In a 1995 paper, we wondered whether political scientists would take heed of the call of the positive political theorists. Now, writing just five short years later, the answer is patently obvious: Yes. But simply because judicial specialists are beginning to invoke the strategic account does not mean that the transition from the theories of our predecessors will or, even, should be a smooth one. Undoubtedly debates will ensue. Some, which we have not addressed in this essay, will be of a general nature, taking the form of arguments for and against the various strains of approaches that are grounded in assumptions of rationality; these have

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<sup>30</sup> Of course, variants of social-psychological theory (especially the attitudinal model) would make the same prediction. The difference between the two approaches is seen in Figure 4b, where the attitudinal model would still predict  $x \cong j$ .

occurred in many other fields of our discipline and, thus, seem inevitable in ours.<sup>31</sup> Another debate, the one we have highlighted, is over the “right” way to do rational choice work. But this too represents just the tip of the iceberg. As more scholars incorporate the logic of strategic action into their analyses, construct conceptions of judicial decision making, reason by analogy, and write down models, others will be standing ready to assess their results against data. Whether they will meet these tests, whether they will improve our understanding of judicial politics, remains to be seen.

What does not remain to be seen, at least to our minds, is that strategic analysis can help to illuminate many important, but under-explored, questions of paramount interest to judicial specialists. Throughout this essay, we have provided examples, most of which centered on judges. Our emphasis was hardly accidental; it was designed to reflect the dominating concern of the field for nearly 50 years (see, e.g., Baum 1997). Nonetheless, we do wish to make clear that the strategic account is in no way limited to studies of these elite actors. To the contrary, this approach, as work by other scholars makes clear, is perfectly suited to research ranging from the choices parties make, be they the selection of arguments (e.g., Epstein and Kobylka 1992) or the decision to appeal (e.g., de Figueiredo 1998); to the decisions made by interest groups to participate in judicial confirmation proceedings (e.g., Caldeira and Wright 1995) or in court cases (e.g., Caldeira and Wright 1988); to the willingness of the public and political actors to comply with Court decisions (e.g., Spriggs 1996); and, to just about any other substantive question that centers on a social situation—one in which the outcome is the product of the interdependent choice of at least two actors and, thus, one that aptly describes the targets of virtually all socio-legal research.

With this, we cannot help but end our essay along the same lines as we started it: There is little doubt that the field of judicial politics is undergoing a sea change that has the potential to transform the way we think about law and courts in the United States and elsewhere. For we believe that the strategic account not only stocks a set of tools to illuminate a wide-range of enduring and new questions. It also can contribute to the unification of judicial specialists and their counterparts in other fields of political science, as well as of those within the law and

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<sup>31</sup> The debate in the comparative field, for one, has received wide disciplinary coverage, having been played out in the pages of *PS* and in *APSA-CP*, the newsletter of the APSA's Organized Section in Comparative Politics (available at: <http://emma.sscnet.ucla.edu/apsacp/>). For an interesting critique of features of rational choice, see Yee (1997) and cites contained therein. Chiming in on these sorts of debates is beyond the scope of our set of concerns but suffice it to note that, in all our advocacy of strategic analysis, we recognize that other approaches may be more suitable for the examination of particular kinds of political problems. For example, while we are unwilling to say that the strategic account has nothing to add to questions concerning preference formation, we would have no hesitation in writing that other approaches certainly do—including interpretive-historical accounts and, of course, variants of the social-psychological model.

courts area—especially scholars who study legal doctrine and those who focus on judicial behavior. While these analysts agree on the fundamental importance of law, they have talked passed each other for decades, exhibiting fundamental disagreements over how best to study judicial decisions. To be sure, strategic rationality cannot resolve all of these debates. But, as we have implied throughout and noted elsewhere (Epstein and Knight 1998), it can provide a mechanism for addressing at least one question of core concern to both groups: How law evolves from judicial action.

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Received:

Accepted for Publication: March 27, 2000

epstein@artsci.wustl.edu

knight@artsci.wustl.edu