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LEE EPSTEIN

and

STEFANIE A. LINDQUIST

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CHAPTER 17

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THE ECONOMIC ANALYSIS OF JUDICIAL BEHAVIOR

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LEE EPSTEIN AND JACK KNIGHT

IN the study of judicial behavior, “economics” has multiple meanings. Many scholars view it through a conceptual lens, grounding their studies on the assumption that the judge is a “rational maximizer of his ends in life, his satisfactions—or ... his ‘self-interest’” (Posner 2011: 3).¹ A second group might claim that work exploring economics as a substantive matter—say, a paper on the effect of the economy on judicial decisions—qualifies as an economic study of judging. Still others might emphasize methodology, focusing on whether the research employs the tools of mathematics or econometrics.

We limit our discussion here to research falling into the first group—to studies proceeding from the assumption of rationality, regardless of their methodological approach. By trimming our chapter in this way, we do not mean to deprecate the other two types; ours is a decision of necessity. Because most important studies of judicial behavior fit into at least one of the three categories, including the full range of “economic” work would require not a chapter but an entire Handbook. For the same reason, we exclude work related to but not squarely focused on the behavior of judges such as studies of litigants and lawyers (including the selection of cases for litigation) (e.g., Priest and Klein 1984; Hubbard 2013a) and the large literature on judicial appointments that explores the choices made by rational politicians (e.g., Segal, Cameron, and Cover 1992; Moraski and Shipan 1999).

For a number of reasons, though, our choices aren’t especially limiting. First, rational choice studies of judicial behavior vary in approach. Some are purely theoretical; others are mostly empirical. And even within these categories there is variation. On the theoretical side, there are scholars who explore how the individual judge acts *parametrically* to maximize her preferences. If the judge desires to make more money, she might write a novel assuming she has weighed her own costs (e.g., time away from her judicial work) and benefits; in other words, she is the variable and all others, the constants (Elster 1983). Other studies of judicial behavior—perhaps the great majority—invoke *non-parametric* rational choice models. Strategic accounts belong in this category because they assume

that goal-directed judges operate in a strategic or interdependent decision-making context, regardless of the judges' specific motivation. Then, within the class of strategic accounts there are studies that undertake formal equilibrium analysis (e.g., Anderson and Tahk 2007; Cameron et al. 2000) and there are studies that use economic theories to reason by analogy (e.g., Songer et al. 1994, Westerland et al. 2010). Empirical work too comes in many shades, from large- n studies that invoke complex methods (Epstein et al. 2005; Black and Owens 2016) to largely descriptive or historical case studies (Brams 1990; Knight and Epstein 1996). Exemplars of all theoretical and empirical types appear in this essay, reflecting our belief that there are many ways to incorporate assumptions of rationality into research on the choices judges make (Epstein and Knight 2000).

A second way our focus on rational choice studies of judicial behavior isn't especially limiting is that the targets are many in number. Though the early studies focused on the U.S. Supreme Court (Murphy 1962, 1964; Schubert 1958), modern-day scholars have paid as much if not more attention to lower federal court judges (e.g., Kastellec 2007, Beim, Hirsch, and Kastellec 2014) and judiciaries in the U.S. states (e.g., Huber and Gordon 2007; Berdejo and Yuchtman 2013) and abroad (e.g., Staton 2010; Helmke 2005; Vanberg 2005). Our essay considers all of these developments.

Third, because rational choice accounts of judicial behavior are hardly the sole province of economists, the scholars and their studies vary considerably. Murphy, a political scientist, produced a path-marking book, *Elements of Judicial Strategy* (1964), which portrayed U.S. Supreme Court justices as strategic actors who seek to maximize their policy preferences (see also Schubert 1958). Decades later, Eskridge (1991), a law professor, developed an account of statutory interpretation in which judges are more concerned with the preferences and likely actions of contemporaneous elected actors than with the legislature that enacted the law. The studies we cite in this essay demonstrate this disciplinary diversity, with their authors hailing from business and law schools and from departments of economics, political science, and psychology. They also span the globe, from the United States of course but also Latin America, Europe, and the Middle East. (As an aside, bringing together these literatures in one essay is, we hope, an important contribution in and of itself. Because of the number of disciplines engaged in the study of judicial behavior, the right hand doesn't always know what the left hand is doing, even when they are writing on the same topics.)

Finally, even focusing exclusively on rational choice studies, the range of topics is impressive. There are many ways to splice and dice them but these six stand out: (1) the judge: motivations, careers and performance; (2) selection and retention of judges; (3) opinions and precedent; (4) collegial courts; (5) the hierarchy of justice; and (6) external actors.

In what follows we consider the literature on each, with the caveat that these topics are hardly mutually exclusive. Because this is abundantly clear from the discussion that follows, only a few examples suffice to make the point. Consider, first, Cross and Tiller's (1998: 2156) important study "Judicial Partisanship and Obedience to Legal Doctrine," which argues that the presence of a "whistleblower"—a judge "whose policy preferences differ from the majority's and who will expose the majority's manipulation or disregard

of the applicable legal doctrine” to a higher court—can constrain his or her colleagues from behaving in accord with their own preferences. This study started a now large literature on panel effects, and so we discuss it in the section on collegial courts. Yet because the mechanism triggering the effect is the lower court judges’ fear of reversal by a higher court, the chapter implicates the hierarchy of justice (relations between lower and upper courts) too. Then there is Ramseyer and Rasmussen’s (2001) research on Japanese judges, which suggests that the judges’ careers hinge on deference to the government. This study could fit within the scope of the topic, selection and retention of judges, but it also speaks to the judges’ interest in promotion and other careerist motivations.

With this (long!) windup in mind, let’s turn to the research. For each topic, we discuss the literature and offer some thoughts on future directions. In the last section we are even more specific, emphasizing two promising opportunities for scholars to pursue.

THE JUDGE: MOTIVATIONS, CAREERS, AND PERFORMANCE

As the subheading suggests, studies in this area explore features of individual judges—with a central question being, “What do judges want?” This is a reasonable starting point because rational choice accounts assume that actors make decisions consistent with their goals and interests. We say that judges or any other actors make rational decisions when they choose a course of action that they believe satisfies their desires most efficiently. To give meaning to this assumption—essentially, that judges maximize their preferences—we must identify the judges’ goals. If we do not, our explanations become a tautology because we can always claim that a judge’s goal is to do exactly what we observe her doing (Ordeshook 1992).

Political scientists have long had a ready and singular answer to the question of the judges’ desires: it’s all politics, stupid! That is, on the standard political science account of judicial behavior, judges hope to bring the law in line with their own policy or ideological values.² Because many political science studies continue to analyze the behavior of U.S. Supreme Court justices, we don’t think an emphasis on policy preferences is especially misguided. From Pritchett (1941) to Segal and Spaeth (2002) to Epstein, Landes, and Posner (2013), scholars have offered plausible (although somewhat distinct) reasons and mounds of data for thinking that ideology plays a very substantial role in the choices *justices* make.

It’s also worth pointing out that U.S. Supreme Court justices are hardly unique in this regard. Grendstad et al.’s (2015) wonderful study of the Norwegian Supreme Court demonstrates that justices appointed by social democratic governments are significantly more likely to find for the litigant pursuing a “public economic interest” than are their non-socialist counterparts. Ideology (as measured by the appointing regime) plays a bigger role in these decisions than most any other factor that Grendstad et al.

considered. Hönnige (2009) found that ideology helps predict the votes of judges serving on the French and German Supreme Courts (see also Hanretty 2012). And, in their study of Spanish Constitutional Court judges, Garoupa et al. (2013: 516) discovered that under certain conditions, “the personal ideology of the judges does matter,” which led them to “reject the formalist approach taken by traditional constitutional law scholars in Spain.”

At the same time, though, these very studies demonstrate that ideological (or partisan) motivations have their limits—and not only for judges outside the United States. As so much research has shown, there is hardly a perfect correlation between ideology and voting: far short of 0 percent of the votes cast by liberal justices (however measured) are conservative; and far short of 100 percent of the conservative judges’ votes are conservative (see even Segal and Spaeth 2002). Moreover, once we move down the judicial hierarchy to the U.S. Courts of Appeals and district courts, ideology carries even less weight (see, e.g., Epstein, Landes, and Posner 2013). In other words, however useful ideology is for understanding judicial behavior, it cannot be the only motivation for explanations of the judges’ choices.

Recognition of this fact has led law professors, economists, judges, and even some political scientists to challenge the standard political science approach to judicial motivations. These scholars assert that more realistic conceptions of judicial behavior must take into account the judges’ desire to maximize their preferences over a set of personal factors (while also attending to the political scientists’ emphasis on ideology and the law community’s interest in legal motivations). According to Epstein, Landes, and Posner (2013; see also Epstein and Knight 2013), personal factors (most of which also have implications for ideological and legal goals) include:

1. job satisfaction, which we take to mean the internal satisfaction of feeling that one is doing a good job as well as the more social dimensions of judicial work, such as relations with other judges, clerks, and staff (see Baum 2006; Caldeira 1977; Drahozal 1998; Gulati and McCauliff 1998; Shapiro and Levy 1995)
2. external satisfactions that come from being a judge, including reputation, prestige, power, influence, and celebrity (see Drahozal 1998; Miceli and Cosgel 1994; Schauer 2000; Shapiro and Levy 1995; Whitman 2000)
3. leisure, such that at some point, “the opportunity cost of foregone leisure exceeds the benefits to the judge of additional time spent making decisions” Drahozal (1998: 476; see also Klein and Hume 2003).
4. salary/income, in that all else equal, judges like most of us, prefer more salary, income, and personal comfort to less (see, e.g., Baum 2006)
5. promotion.

We flesh out these possibilities in Epstein and Knight (2013). But let us provide a flavor of the analysis by focusing on the last motivation: promotion. This would seem to be an important factor influencing the personal utility that judges derive from their work. It could be coincident with policy preferences: the higher judges sit in the hierarchy, the

more important the cases they hear and the greater the opportunity to influence the law. Promotion also tends to increase job satisfaction, prestige and reputation, and, of course, salary.

To explore the possible effects of this motivation, Epstein, Landes, and Posner (2013) compared federal judges with some realistic chance of promotion to those without much hope of advancement to determine whether the former “audition” for their next job. Epstein et al. hypothesized that the auditioners would impose harsher sentences on criminal defendants to avoid being tagged as soft on crime. The data supported their hypothesis.

We think these results, along with other studies of promotion effects (e.g., Cohen 1991; Sisk et al. 1998; Taha 2004; Gaille 1997; Ramseyer and Rasmussen 2001; Salzberger and Fenn 1999; Black and Owens 2016), do more than flesh out a particular element in the judge’s personal utility function; they provide sufficient evidence of the importance of personal motivations to pose a serious challenge to the (un)realist(ic) conception of judicial behavior that generations of political scientists have assumed in their work on law and legal institutions. The findings also of course should invite more creative research on the general topic of judicial motivations.

SELECTION AND RETENTION OF JUDGES

Societies have devised an impressive array of institutions to govern the selection and retention of judges—from life tenure to a single nonrenewable term to periodic elections and re-election by the electorate. And a substantial body of work considers the decisions of the actors who select (or retain) the judges (e.g., in the United States, the president and Senate for federal judges, and governors, legislatures, commissions, and voters for state judges; see Gibson 2012; Cameron et al. 1990).

The economic literature on judicial behavior is more concerned with whether and how these various institutions affect the choices judges make—and so is related to important work on *judicial independence*. We (and others) define this term as the ability of judges to behave sincerely without fear of reprisal and with some confidence that political actors will enforce their decisions (see, e.g., Ríos-Figueroa and Staton 2014). So defined, we might expect that forcing judges to face the electorate or the legislature for renewal—relative to providing them with life tenure—would lead to a more dependent judiciary; the opportunity costs for voting on the basis of sincere preferences are higher. A life-tenured system, in contrast, should lead to a more independent judiciary, with judges freer to vote as they desire.

The empirical literature provides strong support for these expectations. Consider, for example, a classic article by two economists, Tabarrok and Helland (1999). Focusing on U.S. states that elect their judges, the authors examined whether tort awards were higher in cases brought by an in-state plaintiff against an out-of-state defendant. They predicted that elected judges would be more likely than appointed judges to rule in favor of plaintiffs because “redistribut[ing] wealth from out-of-state businesses to in-state

plaintiffs” would help them win re-election (p. 157).³ Using a variety of statistical methods, Tabarrok and Helland report that their hypothesis held: “the expected total award in ... elected states with out-of-state defendants is approximately \$240,000 higher than in other states” (p. 186).

Similarly, in the criminal context research shows the judges who face the electorate to retain their jobs are especially tough on defendants, in part because the public (yet again) doesn’t like judges who are softies. Gordon and Huber (2004: 247), for example, found that as retention elections for Pennsylvania trial court judges grew closer, the judges were significantly harsher on defendants. Overall, they “attribute at least 1,818 to 2,705 years of incarceration to the electoral dynamic.” Berdejo and Yuchtman (2013) reached much the same conclusion. Their analysis of sentences imposed by 265 trial court judges in Washington State shows that sentences are significantly longer “at the end of a judges’ political cycle than [at] the beginning.”

This is just the tip of the iceberg. Study after study confirms that elected judges engage in sophisticated behavior designed to please their constituents—whether ruling for in-state plaintiffs or against criminal defendants. (For work on civil cases, see, e.g., Kang and Shepherd 2011; Canes-Wrone, Clark, and Park 2012; Caldarone, Canes-Wrone, and Clark 2009; Helland and Tabarrok 2002. On criminal cases, see, e.g., Gordon and Huber 2007; Hall 1987; Brace and Hall 1997; Brace and Boyea 2008).

These results are not too surprising: when societies subject their judges to reelection, they are presumably trying to induce accountability. But what of life-tenured judges? Oddly, research has shown that U.S. Supreme Court justices tend to vote as their appointing president would—almost as if the president appointed himself to the Court (Epstein and Segal 2005; Segal et al. 2000). Perhaps this recurring pattern reflects some perceived benefit (and minimal costs) of loyalty on the part of life-tenured justices (Epstein and Posner 2015). Or perhaps life-tenured justices feel free to vote sincerely (without fear of reprisal) because they share the preferences of the regime that appointed them.

But therein lies the rub for life-tenured schemes. Although they are designed to induce judicial independence, once the appointing regime changes, the institutional framework can produce the opposite effect, or at least cause substantial trouble for the judges. Some, though limited, evidence (not to mention famous anecdotes)⁴ shows that as a life-tenured Court ages, attacks from the elected branches follow—with the judges ultimately caving to the pressure (Epstein, Knight, and Shvetsova 2002). Seen in this way, nonrenewable terms, which many European countries use for their constitutional court judges, may be a better mechanism than life tenure for inducing judicial independence. But we need far more work, necessarily comparative work, to get a better handle on this.

OPINIONS AND PRECEDENT

In the course of deciding cases, judges face countless choices, including, in many societies, whether to join the majority or write separately and then how to craft their opinions. A great deal of research makes use of rational choice to analyze these decisions. Such

is Epstein, Landes, and Posner's (2011) study, which takes up the puzzle of why judges do not always dissent when they disagree with the majority. The basic argument is that dissents impose substantial collegiality costs on the other judges on the panel by making them work harder (e.g., more work), while the benefits of dissenting (e.g., future citations) are few. Although "dissent aversion" is stronger in the federal circuits than in the U.S. Supreme Court, there is some evidence that it exists there too, especially in cases in which the ideological stakes are low. (But see Beim, Hirsch, and Kastellec 2015, Lindquist et al. 2007, and Black and Owens 2012, all presenting evidence that the benefits may be more substantial than Epstein, Landes, and Posner posited.)

Other studies focus on opinions of the court, and consider how they make use of precedent and other traditional legal materials. In the formal economics literature, there is a debate of long standing over the extent to which reputation- or power-seeking judges ought to be followers (apply existing doctrine) or more avant-garde. Levy (2005) takes the latter position, contending that judges motivated by esteem should be less willing to follow precedent to show that they are more capable than their predecessors. Creating new precedents, on this account, can also generate citations and other accolades. On the other side, Landes and Posner (1976: 273) note that if judges regularly choose to reject existing doctrine, the "precedential value of [their] own decisions would be reduced." Of course, this could lead to rogue judges—those who feel free to disregard all precedents in their quest for power (or policy). But on Landes and Posner's account appellate review keeps the system in check. Judges who are too innovative run the risk of seeing their decisions overturned, which can harm their reputation.

Staton and Vanberg (2008) also analyze majority opinions but are less concerned with the internal context of judging—adhering to precedent or grappling with colleagues—than with external context. They want to know why judges sometimes write vague opinions when they could be more decisive. The answer the researchers derive is interesting: assuming that the costs to implementers of deviating from a clear court decision are higher than the costs of deviating from a vague decision (because non-compliance is easier to detect), a court facing "friendly" implementers will write clear opinions. Clarity increases pressure for—and thus the likelihood of—compliance. But when the probability of opposition from implementers is high, clarity could be costly to the judges; if policymakers were determined to defy even a crystal clear decision, they would highlight the relative lack of judicial power. To soften the anticipated resistance, courts may be purposefully vague.

Staton and Vanberg provide several examples of their theory in action (e.g., *Brown v. Board of Education (Brown II)* (1955)), but more systematic follow-ups now exist. Black et al. (2016) show that justices strategically craft language in their opinions, adjusting the level of clarity to correspond to their assessment of the likelihood of noncompliance by external actors, including federal agencies and the states. Corley and Wedeking (2014) find that lower courts are more likely to follow Supreme Court decisions that are written at higher degrees of certainty. And there is now also work on how lower court judges write opinions to insulate themselves from reversal (Boyd 2015a; Hinkle 2016; Hinkle et al. 2012; Smith and Tiller 2002). In addition to serving as an antidote to the

long-suffering study of judicial implementation and compliance, this new line of work on opinion writing provides a link between the internal and external contexts of this important facet of judging.

COLLEGIAL COURTS

There is a large literature exploring relations among judges who serve on the same court. Many studies focus on so-called panel, collegial, or peer effects, asking whether the case's outcome (or a judge's vote) would have been different had a single judge, and not a panel, decided the case (Kastellec 2007)—and, if so, why? As we noted earlier, the foundational work is Cross and Tiller's (1998) article, which argues that the presence of a “whistleblower” on a judicial panel can constrain his colleagues from voting sincerely. Some studies find evidence of this phenomenon while bringing more nuance to the Cross/Tiller account (e.g., Kastellec 2007, 2011; Beim and Kastellec 2014); others offer different explanations for moderation among judges serving on an ideologically mixed panel—including “dissent aversion.”

Cross and Tiller's study and work following from it focus on the U.S. courts of appeals, where judges decide cases in panels of three (except in those cases heard en banc). Collegial effects, though, are not limited to courts that sit in panels. Consider Caldeira, Wright, and Zorn's (1999) justifiably famous analysis of case selection in the Supreme Court. The authors show that justices are less likely to vote to grant certiorari (agree to hear a case) when they think they will be on the losing side if certiorari is granted, even if they would like to reverse the decision below (sometimes called a “defensive denial”). Caldeira et al. also supply evidence of “aggressive grants”: voting to hear a case when the justice agrees with the lower court's decision because he believes that the majority of the other justices do too.

Work on case selection is ongoing (e.g., Black and Owens 2009; Benesh et al. 2002), as are related studies on the role of threshold requirements. Following from Riker's (1984, 1986) analysis of the “art of manipulation,” scholars have explored whether and to what extent judges raise questions about standing, mootness, and the like to turn around a seemingly foregone loss (e.g., Black et al. 2013; Epstein and Shvetsova 2002; Goelzhauser 2011; Staudt 2004). Much of this work focuses on collegial courts—where, for example, a judge might seek to kick a case on mootness because he thinks his colleagues will affirm when he wants to reverse. But there are also studies of trial court judges exploring why and how they embrace doctrines that limit judicial workloads to maximize leisure (especially if they have heavy dockets). We think here of research assessing the impact of the U.S. Supreme Court's decisions in *Bell Atlantic Corp. v. Twombly* (2007) and *Ashcroft v. Iqbal* (2009) (“*Twiqbal*”), which made it easier for district courts to dismiss cases before any pretrial discovery (for a review of the studies see, Engstrom 2013). Though *Twombly* alone “precipitated no significant change in dismissal rates” (Hubbard 2013b), *Iqbal* did, perhaps because it clarified *Twombly's* reach. In the post-*Iqbal* period

(June 2009 to June 2010), the predicted probability of dismissal increased by somewhere between 35 and 49 percent relative to their pre-*Iqbal* levels (Epstein, Landes, and Posner 2013).

Once judges have agreed to resolve disputes, research has demonstrated that they try to influence one another through memoranda (Maltzman et al. 2000; Epstein and Knight 1998; Spriggs et al. 1999). Other research suggests that group effects also play a role in the Chief Justice's assignment of the majority opinion (Maltzman and Wahlbeck 1996; Lazarus 2015; Wahlbeck 2006). A formal model developed by Lax and Cameron (2007: 293), for example, yields a prediction that the Chief Justice will favor justices more (ideologically) extreme than himself in part because "more extreme writers must invest more heavily in judicial craftsmanship, in order to hold the majority."

All in all, these studies demonstrate that group or collegial effects are hardly limited to panels on circuit courts. Whether voting on certiorari or the merits of cases, bargaining or assigning opinions, they show that a judge's choices might have been different if the outcome depended only on his vote and not on the votes of his other colleagues.

HIERARCHY OF JUSTICE

Research on the hierarchy of justice explores interactions between higher and lower courts and so is related to work on the collegial court. The efficacy of whistleblowing, for example, hinges on the ability of a higher court to reverse the decision of a lower court.

Many of the studies make use of principal-agent theory, which, in this literature, assumes heterogeneous ideological preferences between upper and lower court judges and emphasizes how a higher court can extract conformity from a lower court with different preferences (Westerland et al. 2010: 891–2). More concretely, the typical starting point in these studies is that lower court judges no less than higher court judges are interested in etching their political values into law. But lower court judges face a substantial constraint in their quest to do so: the possibility of sanctioning from a higher court. To the extent that higher courts (at least in the United States) cannot hire, fire, promote, demote, financially reward, or penalize members of trial or intermediate courts, the studies propose various mechanisms for keeping the lower courts honest, including strategic auditing (e.g., Spitzer and Talley 2000; Cameron, Segal, and Cover 2000), implicit tournaments among lower courts (e.g., McNollgast 1995), en banc review (Clark 2009b; Giles et al. 2006, 2007), the articulation of rules rather than standards (Smith and Todd 2015), and, of course, whistleblowing (Lindquist, Haire, and Songer 2007; Black and Owens 2012; Kstellec 2007; Beim, Hirsch, and Kstellec 2015). Much of the empirical work assessing these mechanisms focuses on review by the U.S. Supreme Court (or circuits *en banc*) of decisions reached by appellate panels. But there is a growing literature showing that trial court judges alter their behavior to avoid reversal by intermediate appellate courts (e.g., Boyd 2015b; Randazzo 2008; Epstein, Landes, and Posner 2013).

Other research, though rarer, reverses the usual approach to lower–higher court relations and attends to the fact that lower court judges are fully capable of limiting the commands of higher courts by avoiding, limiting, or even defying them—as many did with the U.S. Supreme Court’s desegregation decisions. In these studies, the limitation imposed by the hierarchy of justice “comes full circle”: higher courts “must take into account the reaction of inferior judges, and lower courts must attempt to divine the counter-reaction” of superior courts (Murphy 1959: 1031). Perhaps this is why lower court criticism of Supreme Court precedents seems associated with the justices’ willingness to depart from them (Hansford and Spriggs 2008; Epstein, Landes, and Liptak 2015). At the very least these studies illustrate the truly interactive nature of judicial hierarchy and highlight yet another important avenue for future research on strategic judicial decision-making.

EXTERNAL ACTORS

Much of the work on the relationship between courts and external actors—especially elected officials— bears a family resemblance to the hierarchy of justice studies in that it too uses a strategic framework to explore their interactions. In this literature, the judges must attend to the preferences and likely actions of the government if they are to achieve their goals (e.g., Eskridge 1991; Ferejohn and Weingast 1992; Gely and Spiller 1990). If the judges do not, they run the risk of retaliation from elected actors, thereby making it difficult for them to establish enduring policy, maintain legitimacy, or whatever else their objectives may be.

Most of the early empirical work assessing this claim focused on cases of statutory interpretation, on the theory that when federal judges interpret statutes, Congress can override their interpretations by enacting new laws. The results assessing this expectation are mixed and debates (apparently) continue (e.g., compare Bergara, Richman, and Spiller 2003; Spiller and Gely 1992; Eskridge 1991; Sala and Spriggs 2004; Segal 1997).

In *The Choices Justices Make* (1998) and elsewhere (Epstein, Knight, and Martin 2001), we extended the argument to cases involving constitutional interpretation. Our contention was (and is) that although Congress cannot pass legislation to overturn constitutional judicial decisions (but see Meernik and Ignagni 1997), it can take aim at courts in other ways: withdrawing their jurisdiction, eradicating judicial review, approving constitutional amendments to overturn decisions, slashing their budget, and impeaching judges (Rosenberg 1992). As a result, we speculated (but only partially tested) the idea that the Court must be equally if not more vigilant in attending to the preferences of elected actors in constitutional cases.

Subsequent work has shown this to be a promising hypothesis. From Clark’s (2009a) important study, we know that congressional threats are not merely theoretical: Members of Congress introduced more than 900 court-curbing proposals between 1877 and 2008. More to the point, Clark’s work, along with an equally important article

by Segal et al. (2011), demonstrates that when Congress threatens the Supreme Court's authority, the Court cowers, either exercising greater judicial self-restraint or reaching decisions closer to congressional preferences. Related research also suggests that under certain circumstances, the Court will duck constitutional cases (Goelzhauser 2011).

These studies focus on the United States. Helmke's (2002, 2005) work on Argentina draws attention to the fact that in many parts of the developing world, governments have taken even more radical steps to tame their courts, with sanctions ranging from impeachment, removal, and court-packing to criminal indictment, physical violence, and even death. Her study and many others running along similar lines show that judges respond to these potential threats by defecting against the old regime (Helmke 2002, 2005), avoiding cases that may contribute to further escalation (Epstein, Knight, and Shvetsova 2002), going public (Gauri et al. 2015; Staton 2010) and passing up posts on apex courts altogether (Basabe-Serrano 2012), among other strategies.

This line of research focuses most directly on the relations between courts and executives/legislatures. There is also a substantial body of work that explores the relationship between courts and the American people—specifically, the extent to which the public constrains judicial decisions. Using qualitative data and historical methods, Friedman (2009) provides an unequivocal answer to the question of whether “we the people” influence the Supreme Court: we do. Many large-*n* studies agree (e.g., Giles et al. 2008; McGuire and Stimson 2004; Mishler and Sheehan 1996). At the risk of generalizing, they tend to find that when the “mood of the public” is liberal (conservative), the Court is significantly more likely to issue liberal (conservative) decisions.

But *why* is the real question. Friedman offers a response straight out of the strategic literature: the justices bend to the will of the people because the Court requires public support to remain an efficacious branch of government. We agree with his response; and the existing quantitative studies could be read to support this view. But even we must admit that the findings are consistent with another mechanism: that “the people” include the justices. On this account, the justices do not respond to public opinion directly but rather respond to the same events or forces that affect the opinion of other members of the public. Or, to quote Cardozo (1921: 168), “the great tides and currents which engulf the rest of men do not turn aside in their course and pass the judges by.” That's why Epstein and Martin (2010) titled their paper (written for a symposium on Friedman's book), “Does Public Opinion Influence the Supreme Court? Possibly Yes (But We're Not Sure Why).”⁵ (By the way, the same problem of behavioral equivalence arises in work on macroevents, e.g., war: Do judges strategically retreat from war-related cases [or otherwise rubber stamp the government] or are they too swept in rally-round-the-flag effects?)

WHAT'S NEXT?

However extensive the economics literature on judging, many more studies will inevitably follow. In what directions are scholars heading (or should they head)? There are

many possibilities but in the interest of space we note two: one mostly theoretical, the other empirical.

Beginning with the theoretical, we have emphasized studies assuming that judges are rational actors. To us, this is a reasonable assumption, or at least it gets us pretty far in understanding judicial behavior. But it will not get us all the way. It is just too late in the day to question the decades' worth of studies showing that in many situations, people rely heavily on their intuitions to make fast decisions without much effort. Social psychologists tell us that these responses are not always wrong or even unhelpful (Wistrich et al. 2015). But we also know that unchecked by deliberative assessments, they can lead to mistakes and biased decisions (Thaler 2015; Kahneman 2011).

Although judges may think they can “suppress or convert” their intuitions, prejudices, sympathies, and the like into rational decisions (Wistrich et al. 2015: 862), this is not so. Experiments conducted on thousands of judges by Rachlinski et al. demonstrate that judges respond more favorably to litigants they like or with whom they sympathize (e.g., Wistrich et al. 2015), are affected by anchors in making numeric estimates (e.g., Rachlinski et al. 2015), and fall prey to hindsight bias when assessing probable cause (e.g., Rachlinski et al. 2015). In short, it turns out that judges are human too (see also Rachlinski et al. 2009, 2011; Guthrie, et al. 2001, 2007, along with work by Braman 2009; Landsman and Rakos 1994; Simon and Scurich 2013).

To the extent that judges are influenced by their emotions, intuitions, and prejudices, it will complicate their ability to make rational decisions. And thus it complicates our efforts to explain their behavior. There is no getting around the fact that these factors can distort purely rational or strategic decision-making. The interesting research questions relate to how much these non-rational factors alter what we would expect to see if we assume that judges are acting rationally. One possibility would be to compare the predictions of strategic analysis with the data on actual judicial decisions. This would give us a better sense of the extent to which these non-rational factors influence the judges' choices.

As should be obvious, we take the experimental evidence quite seriously, but some members of the legal community (especially judges) do not; they complain that the experiments are artificial and do not capture the real courtroom environment. This counsels for observational studies—that is, studies making use of data that the world, not the researchers, have created. These are not easy to do, but neither are they impossible, as Shayo and Zussman's (2011) study of Israeli small claims courts demonstrates. More to the point, we think they are crucial: should the experimental and the observational converge, we can have more confidence in our conclusions.

That's the theoretical point; the more empirical avenue concerns our targets of inquiry. At the outset, we noted that they have expanded from an exclusive focus on the U.S. Supreme Court to lower courts and courts outside the United States. Our discussion of the existing literature provides ample proof of this claim. Today, there is almost no corner of the world whose judges are not under analysis by some scholar somewhere. Through these analyses, we have learned a great deal about the choices of judges in other societies of course, but also about our own judges. For example, to understand the effect

of life tenure on judging, we need points of comparison. Research on the state courts provides some, but moving to a comparative context provides even more. The worldwide variation of judicial selection-retention mechanisms is so high that it is hard to imagine one that does not exist.

The comparative advantage in illuminating judicial behavior is now so well understood that we've already said too much. Worth emphasizing, though, is that the data infrastructure has not kept pace with the interest. Instead of creating modern, reliable, maintainable, and sustainable large-scale public databases with crucial information about world's courts, judges, and judicial decisions, scholars have worked in isolation building one-off (or otherwise limited) data sets designed for particular projects and centered on particular courts, countries, or regions. The "one-off" approach has its benefits of course. But it also has substantial costs, ultimately impeding the march toward knowledge, innovation, and invention. These costs include the massive duplication of effort, inefficiencies, and dated (if not downright unreliable) measures and data.

Moving the field forward requires the development of at least two large-scale and sustainable databases, one focused on the institutional features of courts throughout the world and the other, on case-level data. Sure, we understand that scholars will raise questions about the kinds of institutional detail and case-based information we should collect, just as they did when Spaeth, Songer, and others built their databases and other infrastructure for the analysis of the U.S. federal judiciary. The answer to these questions is simple: public databases are foundational, not comprehensive. By design scholars should be able to adapt and build on them to suit their own needs. Very few studies published on the U.S. Supreme Court, for example, use Spaeth et al.'s U.S. Supreme Court Database "as is"; their authors backdate data, recode categories, and add variables. And so it will be for global databases.

The same answer holds for questions about conceptualization. We know that there is much hand-wringing in the comparative analysis of judicial behavior over how to define and measure even basic concepts (e.g., judicial independence). If the past is any indication, waiting around for scholars to agree will continue to impede innovation and invention. It would be better to focus on the provision of basic data—facts about the world—that scholars can develop in ways they think appropriate. We believe there is widespread agreement on the importance of at least some of these facts. Falling into this category are, among other institutional details, legal family, number of judges, judicial/constitutional review power, tenure, budget, and salaries. As for case-level data, the parties, the year of decision, the judges, separate opinions, and the disposition would seem rudimentary.

Building this infrastructure sooner rather than later will have great payoffs for the analysis (economic or otherwise) of judicial behavior. Not only will it advance knowledge but it also has the potential to bring together diverse communities of scholars worldwide—goals on which we all can agree.

NOTES

1. This chapter draws on some of our other work on judicial behavior, including Epstein (2013), Epstein and Knight (2013), Epstein (2016). Epstein thanks the National Science Foundation, the John Simon Guggenheim Foundation, and Washington University in St. Louis for supporting her research on law, legal institutions, and judicial behavior.
2. The idea here is that judges want the law to reflect their preferred policy positions. To assess empirically the judges' policy positions, political scientists use partisan measures (e.g., the party affiliation of the judge or the appointing president) and ideological measures (e.g., the Segal-Cover [1989] or Martin-Quinn [2002] scores). For a review, see Epstein et al. (2012). This is why the political science literature tends to treat political goals, policy goals, ideological goals, and partisan goals as interchangeable terms.
3. The story is a little more complicated than this because Tabarrok and Helland (1999) compared awards in partisan-elected states (where judges identify their partisan affiliation), nonpartisan states (where judges run for reelection but not on a partisan ballot), and non-elected states.
4. The battle between FDR and the Supreme Court is the most obvious example. For a terrific empirical account, see Ho and Quinn (2010).
5. For a possible answer (supportive of Friedman's argument), see Casillas et al. (2011).

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