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On the Study of Judicial Behaviors: Of Law, Politics, Science and Humility

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Abstract

In this paper, which was prepared to help set the stage at an interdisciplinary conference held at the University of Indiana (Bloomington) in March, I first briefly review what I take to be the key events and developments in the history of the study of judicial behavior in legal scholarship, with attention to corresponding developments in political science. I identify obstacles to cooperation in the past – such as indifference, professional self-interest and methodological imperialism -- as well as precedents for cross-fertilization in the future. Second, drawing on extensive reading in the political science and legal literatures concerning judicial behavior, I seek to identify the most important lessons that we have learned, or should have learned, to date, as the springboard for progress in the future. The first lesson is that the relationship between law and judicial politics (as I define them) is not monolithic; it varies among courts and, even on the same court, among cases. As a result, we should speak of “judicial behaviors” rather than “judicial behavior.” The second lesson is that there is no dichotomy between law and judicial politics; they are complements, each needing (or relying on) the other. The third lesson is that the mix of law and judicial politics on any given court does or should result from institutional design decisions that reflect what the polity wants from that court. Finally, I argue that, because the relevance of the enterprise in which we are engaged and its stakes transcend the world of scholarship, scholars who work in this area bear a special burden of responsibility.

I. Introduction

It is fitting that I have been asked to help set the stage for this conference by appearing with Professor Segal on a panel that compares legal and political science models of judicial behavior and the light they shed on what judges do. I am not only a law professor; I am an old law professor. Moreover, a legal scholar commenting on an article that I published in 1997 called it the work of a “committed formalist,” which was not intended as a compliment (Neuborne 1997, 2094). Who better to channel William Blackstone than an aged “committed formalist?”

What had I done to deserve the pejorative? In a festschrift honoring Jack Weinstein, I had argued that, although Professor Weinstein’s brilliance and creativity

accompanied him from Columbia Law School to the bench, so also did his conception of professional autonomy and his ideology. A federal trial judge, I argued, does not have, and cannot properly assert, the intellectual independence of a member of the United States Supreme Court, let alone of a tenured law professor. I also argued that ideology in the sense of a set of policy preferences that holds sway “with such power as to be impervious to adjudicative facts, competing policies, or the governing law as generally understood” is “revealed as the enemy of judicial independence” (1997, 1999).¹

If those views mark me as a “committed formalist,” so be it. In fact, I agree with Professor Schauer that “[f]ormalism is about power” (1988, 543), and that it “ought to be seen as a tool to be used in some parts of the legal system and not in others” (1988, 547). Sensible commitment to rules and the values of predictability, even-handed treatment and stability that such commitment can advance is an important feature – by no means, however, the only important feature -- of a well-functioning legal system. It is for that reason an important part of what it means, or should mean, to be a judge on some courts, in some types of cases, at some times. I have even ventured the view that “all members of the [Supreme] Court, including its strong ideologues, are imbued to some extent with Rule of Law values, and that almost all of them understand the importance of adherence to, or rational explanations for departures from, precedent” (2007, 141; see also Gerhardt 2008).

As this truncated statement of views suggests, I do not champion the monolithic and mechanistic account of judicial behavior that Professor Segal and his co-author, Harold Spaeth, represent as “the legal model” (2002).² Thus, in writing about Judge Weinstein, I acknowledged that “[j]udges are bound to have beliefs about both the appropriate role of, and appropriate policies or goals for, government, some of which they are bound to translate into law” (1997, 1999).³ To provide richer context for the views about law and politics that I express here, it may be useful at the start briefly to elaborate my scholarly background and orientation. Since I have not deeply engaged the political science literature on judicial behavior until recently, this can serve as evidence of my “Bayesian priors” (Posner 2008, 67) on the questions before us at this conference.

It would be hard to find any lawyer, law professor, or judge who did not believe that on some questions governed by law – whether deadlines in procedure or default rules in contract -- the existence of a clear and determinate rule is more important than the content of that rule. In addition, some legal doctrine such as preclusion (*res judicata*) (Burbank 1986), although hardly devoid of policy, is so complicated that lawyers and judges need all the help they can get.⁴ Even though, wearing his scholar's hat, Richard Posner proclaimed the “epistemic shallowness” of doctrinal scholarship (1995, 88), as a judge he has come to bemoan its threatened disappearance from the legal academy (2008, 209-10). In any event, while mastering highly technical procedural doctrine, I learned through close attention to history that the struggle for federal procedural reform in the early twentieth century was a struggle for power (1982a), and I came to realize that, in the practical world of procedural lawmaking, empiricism is perhaps the best way to test claims of objectivity or neutrality, as well as to identify problems of lawmaking legitimacy (1989). More generally, I came to understand that, although the capacity for technical reasoning is a necessary condition for good scholarship in procedure, it is not a sufficient condition (2002a).⁵

My early work in procedure resulted in recruitment to write the procedural rules for the Third Circuit implementing the Judicial Councils Reform and Judicial Conduct and Disability Act of 1980 (1982b). That work, in turn, led to service on the National Commission on Judicial Discipline and Removal. With Judge (former law school professor and dean) Jay Plager, I was able to shape the Commission's research program so that its recommendations could be informed by studies using multiple research strategies, including multiple qualitative and quantitative empirical methods (Burbank & Plager 1993).

Service on the Commission, together with work for the American Judicature Society (AJS), caused me to explore questions of judicial independence and accountability in depth, work that included my first systematic exposure to political science scholarship (1999). That experience, nurtured by conversations with my friend,

Barry Friedman, who was also actively involved in the work of AJS, resulted in our joint perception that law professors and political scientists had been talking past one another about judicial independence. As a result we organized an interdisciplinary conference attended by some thirty scholars spanning four disciplines, an enterprise that yielded an edited volume advocating, and suggesting the promise of, an interdisciplinary approach (Burbank & Friedman 2002a).

Returning to work in procedure, I realized that, as a scholar whose main message had been the power of procedure, I should be conversant with, and deploy the insights of, the discipline that systematically studies power in government. Thus, in recent years I have sought to enrich my procedural work with the fruits of research in political science, all the while holding to the view that the best scholarship is that which can leverage the perspectives and techniques of as many relevant disciplines as possible (2004a).

In sum, I am an old law professor who aspires not to be old-fashioned. I have done a great deal of analytical and normative scholarship, a great deal of work exploring doctrinal and institutional history, and some work deploying quantitative and qualitative data, including data that I have generated for the projects (1989; 2004b). The quantitative work that I have done is primitive, and I am not competent to assess many questions of statistical method. I have had occasion, however, to pay close attention to questions concerning inferences that can properly be drawn from quantitative data (2004c).

In seeking to help set the stage for this conference, I first briefly review what I take to be the key events and developments in the history of the study of judicial behavior in legal scholarship, with attention to corresponding developments in political science. I identify obstacles to cooperation in the past – such as indifference, professional self-interest and methodological imperialism -- as well as precedents for cross-fertilization in the future.

Second, drawing on extensive reading in the political science and legal literatures concerning judicial behavior and the wonderful experience of teaching a seminar on the

subject with my colleague, Ted Ruger, that regularly brought in scholars working in the area to discuss their work-in-progress, I seek to identify the most important lessons that we have learned, or should have learned, to date, as the springboard for progress in the future. The first lesson is that the relationship between law and judicial politics (as I define them) is not monolithic; it varies among courts and, even on the same court, among cases. As a result, we should speak of “judicial behaviors” rather than “judicial behavior.” The second lesson is that there is no dichotomy between law and judicial politics; they are complements, each needing (or relying on) the other. The third lesson is that the mix of law and judicial politics on any given court does or should result from institutional design decisions that reflect what the polity wants from that court.

Finally, I argue that, because the relevance of the enterprise in which we are engaged and its stakes transcend the world of scholarship, scholars who work in this area bear a special burden of responsibility.

II. A Troubled History

When I was a student at the Harvard Law School in the late 1960’s and early 1970’s, one might not have known that political scientists at the University studied the courts (or more precisely the Supreme Court). I knew it, because as a Harvard undergraduate, I had been privileged to take Government 154 from Robert McCloskey, vignettes from whose brilliant lectures about the Court and its history have remained with me over forty years. I recall for instance his lecture on the formative influences on a Supreme Court Justice, the exemplar being that “old, bald-headed son of a bitch,” Stephen Field (Aitken & Aitken 2008, 156). Nor can I think of Morrison Waite -- not that I think very often about Morrison Waite -- without hearing McCloskey describe his opinions as “turgid and incomprehensible.” Someone, perhaps Paul Freund, told me that McCloskey was the exception to the rule that Harvard Law School did not welcome political scientists. Whether or not that was true, precious few of McCloskey’s insights, or of those of other political scientists, made it into the courses I took or the course materials I studied.

My courses with Paul Freund *did* benefit from the influence that Thomas Reed Powell, his teacher and later colleague, had on that great constitutional scholar. But Powell was both a lawyer and a political scientist. His influence on Freund was that of a legal realist with acute analytical skills who paid close attention to judicial decisions, believed that ideas matter, and was on record that “[t]hose who see law as judicial whim or fiat are partly right, but only partly. Those who see law as *only* this or *only* that see but narrowly” (Freund 1956, 800).

In retrospect, the indifference to political science in that era is not a surprise, because Harvard Law School was still in the grip of the legal process approach (Kalman 1986). This was an effort to navigate between two strands of legal realism: its de(con)structive project of exposing the indeterminacy of law that (as some legal realists would have it) permits judges to pursue their own values while hiding behind the myth that the law made them do it, and its constructive project of seeking to develop policy through a value-neutral assembly and analysis of empirical data (facts).⁶ The latter (constructive) strand fell out of favor quickly, as law professors became discouraged by the time and money required to gather data, and by the seemingly slight payoffs, and with the effects of the Depression on funding (Kalman 1986). The former (de(con)structive) strand, always living uneasily in Langdell Hall,⁷ came increasingly under pressure with attacks on the Warren Court in the 1950’s and the quest for neutral principles such attacks elicited (Wechsler 1959).⁸ Long before that, however, both strands of legal realism had been caught up in controversy about scientific naturalism during what Edward Purcell describes as “the crisis of democratic theory” that was caused by the rise of totalitarian governments on the left and the right in the 1920’s and 1930’s.

As with their colleagues in philosophy and the social sciences, however, the realists’ motives could not explain away the intellectual problems they generated. Intentionally or not, their theoretical position raised two basic questions about traditional democratic theory. First, how could the idea of the subjectivity of judicial decision be squared with the doctrine that free men should be subject only to known and established law, one of the hallmarks of republican as opposed to despotic

government? Second, if the acts of government officials were the only real law, on what basis could anyone evaluate or criticize those acts? What, in other words, was the moral basis of the legal system in particular and of democratic government in general? Most revealingly, Felix Cohen, who alone among the realists attempted an elaborate analysis of ethical theory, admitted that his conclusions remained “in the shadow” of doubt and bordered on failure. That was an unsatisfactory solution to almost everyone after the middle thirties (1973, 94).

I have taught at law schools in one capacity or another since 1976. For many years thereafter, the study of judicial behavior in legal scholarship essentially did not exist. Even after technology facilitated, and funds became available for, empirical research, reducing obstacles that had discouraged legal realists in the 1930's, such research was not valued at most elite law schools.⁹ Rather, law professors advanced *conceptions of judicial behavior*, and for a decade or more the most prominent such conceptions were advanced by scholars of polar opposite policy preferences and, usually, political persuasions.

On one side there were the adherents of Critical Legal Studies, latter day realists with an attitude, if not a social agenda, who tended to confuse the question of what judges *can do* with the question of what they *ought to do* and who, in any event, did not systematically explore what judges *in fact do*.¹⁰ On the other side there were the adherents of Law and Economics, whose agenda was to explain law through micro-economic theory and, to the extent they were concerned about judicial behavior, to explain it through an economic theory of human behavior (Posner 1995). Richer than the conception of judicial behavior held by most adherents of Critical Legal Studies, for many years this too was, nevertheless, a theory undisciplined by facts.

Finally, in that area of legal scholarship most likely to overlap with, and be informed by, political science – constitutional law – judges “behaved” either in a world of doctrine or in a world of theory. In neither sphere, with few exceptions, was the scholarship of law professors informed by quantitative analysis or by the insights of

traditional political science, perhaps because the legal scholars in question “were more interested in shaping the law than explaining it” (Keck 2007, 511; see also Friedman 2006, 263).

Most of this is, I suspect, old news even to political scientists. Certainly, in the period chronicled here – that is, until quite recently -- members of that discipline had good reason for the complaint that law professors ignored their work. To their great credit, the same was not true in the other direction. Legal realism is acknowledged as one of the intellectual inspirations of the attitudinal model of judicial behavior (Segal & Spaeth 2002). In addition, although rational choice (strategic) theory was evident in the law and courts subfield as early as the 1950’s, political scientists have generously credited law professors, economists and business school professors with reviving it by demonstrating its potential (Epstein and Knight 2003; Brenner 2003).

Admirable as the willingness of political scientists to follow a scholarly Golden Rule has been, it renders more puzzling the certitude that some of them have occasionally displayed in their dismissive treatment of the notion that law may play a role in the behavior of the Justices of the United States Supreme Court (Segal & Spaeth 2002; see Keck 2008).¹¹ For one who is conversant with the intellectual (and moral) dilemmas that scientific naturalism was thought to pose in the 1930’s, Thomas Reed Powell’s observation about those who “see but narrowly” is wise counsel in favor of humility. One would have thought the same of the cautions expressed by C. Herman Pritchett, “the first pioneer in what became the leading body of research on judicial behavior” (Baum 2003, 71). Pritchett observed that “political scientists, who have done so much to put the ‘political’ in ‘political jurisprudence’ need to emphasize that it is still ‘jurisprudence.’ It is judging in a political context, but it is still judging; and judging is still different from legislating or administering. ... Any accurate analysis of judicial behavior must have as a major purpose a full clarification of the unique limiting conditions under which judicial policy making proceeds” (1969, 42).¹²

It is tempting for a law professor to regard political scientists' dismissals of law from the landscape of judicial behavior as payback for decades of neglect by better paid, higher status colleagues across campus. Yet, law professors have not been the only, and probably not the primary, targets of criticism by those who deny the influence of law. After all, Pritchett's caution was a response to what Lawrence Baum has termed "a degree of intolerance across methodological divides [within political science], especially directed by quantitative scholars toward those who do qualitative research" (2003, 69).¹³ Consider in that regard Epstein and Knight's answer to the question "why scholars so fully embraced the social-psychological [attitudinal] paradigm and so fully spurned the sort of strategic analysis [political scientist Walter] Murphy conducted in [*The*] *Elements of Judicial Strategy*]" (2003, 208):

During the 1960s, as Murphy made clear to us, the great battles in the field of judicial politics were not between proponents of the rational choice and social-psychological models but between traditionalists and behavioralists; between those who believed that social scientists should develop realistic and generalizable explanations of social behavior and those who did not; and, increasingly, between those who believed that scholars could quantify behavior and those who did not share such beliefs ... 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 that scholars working in the social-psychological tradition triumphed over their strategically minded counterparts. Beginning with Pritchett's *The Roosevelt Court* (1948) and culminating with Segal and Spaeth's *The Supreme Court and the Attitudinal Model* (1993), such scholars have claimed to gather a tremendous amount of systematic support for their theory. Unlike Murphy, they typically refrained from detailed analyses of particular litigation (the *modus operandi* of the traditionalists) and instead focused on large samples of Court cases, claiming to predict their dispositions with a good deal of success (2003, 210).¹⁴

Indeed, perhaps the real intended audience for criticisms of "outdated immersion in legal rules and legal doctrine" were "mainstream" political scientists, since, "[b]y demonstrating that the study of judicial behavior was amenable to statistical analyses,

[quantitative law and courts scholars] could thereby prove [their] bona fides as card-carrying social *scientists*” (Scheingold 2008, 748).¹⁵

III. The Present Situation

That was then. This is now. Now is better. Even if still viewed with suspicion at some law schools – and hence, a risky scholarly path for the untenured -- empirical research on legal institutions, including courts,¹⁶ seems poised to fulfill the promise that eluded the legal realists, albeit without the pretense of value-agnostic pragmatism that Robert Hutchins unmasked.¹⁷ More to the point of this conference, for close to two decades scholars teaching at law (and business) schools have been making important contributions to the literature on judicial behavior, particularly (as discussed above) by developing and testing rational choice models of judicial behavior (Eskridge 1991; Revesz 1997; Cross & Tiller 1998; Friedman & Harvey 2006; Cross 2007; Posner 2008; Landes & Posner 2008; Kim 2009). Others, whether or not themselves conducting original empirical research, have begun regularly to consult the political science literature on courts and judicial behavior, and, unencumbered by turf wars, to find nourishment “across methodological divides” (Friedman 2006).

An overwhelming majority of judges are probably ignorant of the political science literature on judicial behavior. Some judges read law reviews, however, and because those teaching in law schools tend to publish in law reviews, some of their articles on judicial behavior came to the attention of judges and elicited responses (Edwards 1998; Wald 1999). Although at times reminiscent of methodological intolerance in political science, more recently these interventions have suggested a dialogue holding the possibility for all participants to learn (Edwards 2003).

The same, I hope and would like to believe, is true of the relationship between law professors and quantitatively oriented political scientists.¹⁸ Although some political scientists abandoned “the scholarly Golden Rule” in the throes of a disciplinary turf war, they have regained (or can regain) the high road and, in any event, clearly are more

willing to engage -- to participate in a dialogue with -- law professors who are interested in the study of judicial behavior. Witness their presence at this conference. There are doubtless many reasons for these welcome developments (or prospects). I will suggest three.

First, it cannot hurt that, after decades of neglect which, after a while, must have seemed malignant rather than benign, law professors and law schools are paying attention to the work of judicial behavior scholars in political science. The attention paid comes in many forms: citations in the legal literature, invitations to present work in seminars for law school faculty and/or students, invitations to co-author with law faculty, increasing opportunities to publish in law reviews,¹⁹ and, perhaps most important for the interdisciplinary enterprise, appointments to law school faculties. “Ignorance [and] self-interest” may still prompt “skepticism of the empirical literature on judicial ideology” (Fischman & Law 2008, 3) among some law professors, but their ranks are thinning.

Second, it is hard to ignore the energy, creativity and research results of those scholars, including law professors, who have taken up the challenge by quantitative political scientists to demonstrate by acceptable (that is, of course, quantitative) methods that law matters, including on the Supreme Court, and how it matters (Richards & Kritzer 2002; Kritzer & Richards 2003, 2005; Kestelec & Lax 2008; Cross 2007; Johnson, Spriggs & Wahlbeck 2007; Bailey & Maltzman 2008). To the extent that such efforts are deemed successful, the narrowly instrumental theory that the Justices (or judges more generally) are autonomous, uni-focal and uni-dimensional personal-policy-preference machines²⁰ that underlies the attitudinal model and, without the assumption of complete autonomy, the work of some rational choice scholars,²¹ has lost any claim to monopoly power. “Justices are not simply life-tenured policy maximizers” (Bailey & Maltzman 2008, 282).

Third, at the same time that some scholars have sought to meet the challenge to model law, other scholars have begun to take a close look at the “science” undergirding the work of those issuing the challenge. The number and variety of fundamental

assumptions, operating principles, and concrete choices that have been questioned, particularly within the last few years, are impressive (Kritzer 1996; Gillman 2001; Friedman 2006; Shapiro 2009; Fischman & Law 2008; Landes & Posner 2008; Kim 2009; Kastellec & Lax 2008; Braman 2008). Many of the demonstrated problems can be fixed. Yet, now that there is good reason, on their own terms, to question quantitative political scientists' monopoly of knowledge about the wellsprings of judicial behavior,²² other problems in the specification and testing of their models and with their data -- such as behavioral or observational equivalence (or collinear variables) (Segal 1984; Friedman 2006; Stras 2006; Bailey & Maltzman 2008; Fischman & Law 2008),²³ inability to accommodate cases presenting multiple issues (Young 2002; Braman 2008; Shapiro 2009), selection bias (Kastellec & Lax 2008), coding bias (Harvey 2008; Fischman & Law 2008) and systematic coding errors (Stras 2006; Landes & Posner 2008, Shapiro 2009)²⁴ -- may prompt greater humility, perhaps helping to bridge the methodological divide.²⁵

What, then, do I see as the state of current knowledge about judicial behavior? The framework I have chosen to describe it is drawn from my work on judicial independence and accountability, particularly the interdisciplinary work with Barry Friedman to which I have referred (1999; Burbank & Friedman 2002a; Burbank & Friedman 2002b). Accountability to law is an important source of constraint (or self-restraint, Ferejohn & Kramer 2006) posited by those who resist claims that judges are completely independent to decide as they wish. A putative dichotomy between independence and accountability thus maps well on to a putative dichotomy between "judicial politics," *defined for this purpose as the pursuit of a judge's preferences on matters of policy relevant in litigation*, and "law," *defined for this purpose as known and established (but not necessarily determinate) law*. This way of framing the inquiry recalls one of the realists' dilemmas described by Purcell (1973). It also recalls the turf war in the law and courts subfield of political science. Imagine my surprise when, having learned from one group of political scientists that Supreme Court Justices are accountable to elected politicians (Dahl 1957; McCloskey 2005) and thus that judicial independence is a myth (Rosenberg 1991; Jacob 1962), I learned from another group of political

scientists that Supreme Court Justices are wholly independent, and thus that judicial accountability is a myth (Segal & Spaeth 2002).

A. For Contextualism

The first lesson that I take from a review of the judicial behavior literatures is that, just as it is an error to treat judicial independence (and accountability) as a monolith (Burbank & Friedman 2002b), so, in describing judicial behavior, *it is an error to assert or assume that the relationship between “judicial politics” and “law” is or should be the same with respect to every judge in a particular judicial system, or indeed that it is or should be the same even for judges on the same court in every type of case.*²⁶ In sum, we should speak not of “judicial behavior” but of “judicial behaviors” (Friedman, 2006).

For many years these points were obscured as a result of the long-time virtual monopoly that studies of the United States Supreme Court held in the public law subfield of political science.

By the 1950s “public law” political science had pretty much reduced itself to the study of the constitutional law decisions of the U.S. Supreme Court. So when the behavioral revolution started to revolt against the “public law” subfield of political science, it revolted against what was there, namely, the study of the Supreme Court’s constitutional law opinions, by proclaiming that what ought to be studied was not the formal prescriptions of the justices’ written opinions but the real political behavior of the justices; that is, their votes in the constitutional cases. The rather grandiose title was “judicial behavior.” The real study was the voting behavior of nine of the thousands of judges in the U.S. and even then only their votes in a small subset of cases they actually voted on (Shapiro 2008, 769).

Even today the variousness of the relationship between “law” and “judicial politics” may be obscured by the occasional failure of scholars to confine descriptions of their results, and their claims, to the judges, courts and cases in fact studied and/or their apparent reluctance to acknowledge that judges on different courts are influenced in different ways

and to different degrees, including by “law,” even after that should have been clear (Segal & Spaeth 2002, 10, 92-93, 235; Spaeth 2008, 753).²⁷

Those promoting the attitudinal model have never satisfactorily explained unanimous decisions of the Supreme Court, which in recent years have accounted for thirty to forty percent of the Court’s output. And these percentages are “misleading” because they “ignore the petitions for certiorari that the Justices turn down because they are not minded to disturb a precedent for which they would not have voted in the first place, and the petitions that are never filed because the Court would be sure to deny them on the basis of established precedent or clear constitutional or statutory language” (Posner 2008, 50; see also Gerhardt 2008).²⁸ Even if an explanation for unanimous decisions other than a shared view of what fidelity to law requires were plausible, such an explanation presumably would contemplate behavior different from that evinced in 5-4 decisions. Moreover, even if one were willing to accept the test of adherence to precedent used by Segal and Spaeth in their attempt quantitatively to falsify the operation of the legal model on the Supreme Court (2002), that would tell us nothing about the role of precedent on the lower federal (or state) courts. In fact, as many scholars have pointed out, their test confuses judicial roles, neglecting the fact that the legal norms concerning precedent that govern Supreme Court Justices are different from those that govern the judges of other courts (Gillman 2001; Gerhardt 2003; Friedman 2006).²⁹ Moreover, recently other scholars have provided “evidence that non-policy factors [including precedent] influence Supreme Court justices and that the extent of such influence varies across individual justices in interesting ways” (Bailey & Maltzman 2008, 369).

The attitudinal model requires that the Justices not be constrained by Congress (or by the Executive Branch), which is the most obvious reason why its proponents have resisted contrary findings by those testing rational choice models designed to determine whether the Justices act strategically in anticipation of responses by Congress (Segal & Spaeth 2002, 106-09, 346). In unpublished work perhaps stimulated by Harvey and Friedman’s study that yielded “fairly robust support, across different models of the legislative process, for the hypothesis of a constitutionally constrained Court” (2006,

553), Professor Segal proves (again) that his commitment to scholarship is stronger than his commitment to the attitudinal model (Segal, Westerland & Lindquist 2007). He reports statistical results leading him to conclude that, although the Court does not act strategically in its statutory rulings, there is an “institutional maintenance effect” discernable in its constitutional decisions, one that is attributable to concern not about the fate of a particular decision but about the Court’s ongoing ability to function independently (recalling the distinction in the political science literature on legitimacy between specific support and diffuse support) (Caldeira & Gibson 1992).

A less obvious reason why adherents of the attitudinal model may feel invested in falsifying strategic accounts relates to a defense they have deployed in response to their critics. There is something deeply unsettling about an account of judicial behavior, even if confined to the Justices of the Supreme Court, that consigns, if not all of the effort of the lawyers briefing and arguing cases before them, then the opinions that fill the U.S. Reports, to the category of window-dressing.³⁰ The attitudinalists’ response to criticisms reflecting that discomfort has been the assurance that they are not claiming conscious dissembling, coupled with invocation of the phenomenon of motivated reasoning, about which, however, they profess agnosticism (Segal & Spaeth 2002, 433).³¹ If, however, the Justices are acting strategically, modifying their behavior in anticipation of, or response to, other actors, it is not clear that there is any room for motivated reasoning. That helps to explain why some strategic accounts, as for instance of *stare decisis* on the Supreme Court, are so controversial in certain quarters (Bloom 2001; Edwards 2003).

Increasingly over the past thirty years, political scientists and law professors have studied judicial behavior on courts other than the United States Supreme Court. The necessary conditions for the operation of the attitudinal model do not exist on those courts, but the failure satisfactorily to deal with unanimous opinions has also been a problem in many of the studies of their judges (Piniello 1999). More recently, by exposing the influence of precedent and of law more generally on lower court judges, scholars have confirmed that judicial behavior is not monolithic (Cross 2007; Sisk & Heise 2005). It has not been clear, however, whether findings of widespread obedience to

Supreme Court precedent on the courts of appeals – findings acknowledged by Segal and Spaeth (2002, 96) --- reflect the influence of law or simply strategic behavior to avoid reversal. The latter explanation became increasingly implausible as the Supreme Court’s appetite for work became more anorexic. Moreover, as noted by Professor Cross, “the studies to date have shown only very limited evidence of hierarchical strategic decision making by circuit courts” (2007, 103). He continued:

The possibility of legal model decision making somewhat complicates any test of the strategic model. Even if the Supreme Court justices themselves decide cases ideologically, those decisions then become the law that subsequent circuit court decisions are supposed to follow ... Most of the previous studies on strategic decision making have not segregated the theories because the researchers did not consider the possible separate effects of legal model precedent on decision making (2007, 103-04).

Using a “proxy for following the precedents of previous Supreme Courts,” Cross obtained results that are “striking and directly opposite of what the strategic theories of compliance with the Supreme Court would suggest,” leading him to conclude that “circuit court judges do not anticipatorily repudiate old precedents but instead aggressively follow old precedents that are presumptively unattractive to the current Supreme Court” (2007, 104-05).

Forthcoming work by Professor Kim confirms the existence of panel effects – different voting behavior by court of appeals judges on ideologically heterogeneous than on ideologically homogeneous panels – in sex discrimination cases (2009). She too finds no evidence that such behavior (on the part of those in the ideological majority) reflects a strategic response to possible reversal by the Supreme Court, and for that and other reasons she rejects the “whistleblowing” hypothesis posited by Cross and Tiller (1998). She does find, however, that such behavior is correlated with the proximity of the minority panel member’s preferences to those of the circuit median judge, which is consistent with strategic behavior to avoid reversal by the en banc court of appeals.

To her credit, noting statistics indicating that en banc review is about as (un)likely as review by the Supreme Court, Professor Kim discusses possible alternative explanations for her findings, in particular deliberative accounts that focus on the internal dynamics of a panel. One such account is the “intuitive–override theory” (Guthrie, Rachlinski & Wistrich 2007) according to which the “intuitive judgments of court of appeals judges might entail quick decisions that tend to align with their policy preferences – judgments that may yield if subjected to more deliberative processes. The presence of a judge with a different ideological orientation might induce such a deliberative process on the part of the majority judges” (2009,).

Professor Kim’s distinction between “the internal dynamics of panel deliberation” and “interaction with other actors in the judicial system” is potentially misleading. That courts of appeals accomplish (almost all of their) decisional work through panels of three judges should not cause us to forget that the court as a whole bears responsibility for the law of the circuit. That the judges take their responsibility very seriously is suggested by rules forbidding one panel from overruling the legal ruling of another and by pre- and post-release opinion dissemination practices that permit all judges to see what their colleagues are doing (or proposing to do) without having to rely on disappointed litigants.

These features of circuit practice probably mean that, pace Professor Kim, the shadow of en banc review is longer than that of Supreme Court review. But they also raise the question whether we should conceive of the deliberations or dialogue occurring when panel members have different views as restricted to the panel. Conversely, of course, strategic behavior need not be confined to maximizing a judge’s preferences as to policy. As often pointed out, rational choice models can accommodate a preference for law (Epstein & Knight 2003; Posner 2008).

Professor Kim’s admirable acknowledgment of the problem of behavioral or observational equivalence causes her to be appropriately cautious in reporting her findings and in making claims. My point here is only that consideration of her posited deliberative/strategic dichotomy underscores the extent to which both the attitudinal

model and rational choice models based solely on a preference for policy deny any consequential role on a plural court for dialogue about law (Edwards 2003; Whittington 2000). Particularly if motivated reasoning is not an available refuge, but in any event, one can only wonder why judges, including the Justices, spend so much time talking, and talking about, law, not just publicly (as at oral argument or in opinions), but to each other.³² In believing that such dialogue can make a difference, at least some of the time, I feel less old-fashioned having read the report of a recent study finding that, controlling for ideology, the “Justices’ votes in a case depend substantially on the relative quality of the lawyers appearing before the Court” (Johnson, Spriggs & Wahlbeck 2007, 524).

All of this suggests that just because parsimony is *useful* in modeling for purposes of statistical analysis does not mean that the results of those analyses are *sufficient* for thinking about human behavior, about law, or for that matter about politics.

Human beings are indeed charming and perverse and altogether fascinating creatures, and the study of ourselves is among the richest of intellectual endeavors. We ought to give ourselves more credit than the revolutionists of the social sciences extend to us: we pursue many goals at the same time, and we do so in all kinds of predictable and unpredictable ways (Wolfe 2008, 55).

Unlike the attitudinal model, a strategic model that is narrowly focused on pursuit of personal policy preferences at least acknowledges that judicial behavior deemed political because not constrained by “law” is not fundamentally different from the political behavior of other government actors in at least one respect: it is shaped or constrained by institutional context.³³ Neither model, however, leaves room for other influences -- such as, for instance, insecurity (Burbank 2007a) -- that might complicate the judicial utility function with elements that do not lead invariably to pursuit of a judge’s personal policy preferences. Yet, whatever the merits of the psychological theories that informed the attitudinal model of judicial behavior (Segal & Spaeth 2002; Segal 2003), scholarship in the intervening decades has provided accounts of human behavior that, in their

complexity, accord far better with the sense that most of us have about what motivates (or, more precisely, can motivate) human beings (Posner 2008).

In this light, it is not surprising to learn that, even on that most political of all American courts, the Supreme Court of the United States, the Justices respond to a variety of audiences, with the result that their behavior cannot be explained in a single dimension (Baum 2006; see also Peretti & Rozzi, 2008), or that “observed changes in judges’ voting positions on final votes result from factors other than changes in preferences and issue content” (Meinke & Scott 2007, 933). Other scholars, studying court of appeals chief judges, have found that “the judicial utility function includes non-policy as well as policy concerns” (George & Yoon 2008, 8). Finally, studies have indicated that, *on certain courts and in certain types of cases*, personal characteristics such as religion (Sisk, Heise & Morriss 2004) and sex (Boyd, Epstein & Martin 2007) may influence judicial behavior, and that judges are subject to unconscious bias (Rachlinski, Johnson, Wistrich & Guthrie 2007). In sum,

[r]inging changes on the “political” might seem to exhaust the possible nonlegalist factors in adjudication. It does not begin to. The possible other factors (call them “personal”) include personality traits, or temperament (and thus emotionality at one end of the temperament spectrum and emotional detachment at the other end), which are more or less innate personal characteristics. They include personal *background* characteristics, such as race and sex, and also personal and professional *experience*. ... Also figuring in judicial decisions are *strategic* considerations ... *Institutional* factors – such as how clear or unclear the law is, salary and workload, and the structure of judicial promotion – also influence judicial behavior” (Posner 2008, 10).

We can all agree that “social scientists should develop realistic and generalizable explanations of social behavior” (Epstein & Knight 2003, 210). Science has told us where human beings come from (Shubin 2008). It has not yet been able to tell us very much about the human brain (Morse 2006, 2008).³⁴ Even when confined to the Supreme Court, the attitudinal model is implausible (*not* “realistic”) as a self-sufficient theory of judicial behavior, not just because it strips judging of “law” and institutional context. Both it and

narrowly instrumental strategic models are implausible because they strip judges of their humanity, reducing human behavior to the single-minded pursuit of a narrow set of goals.³⁵ As with an external critique of criminal responsibility founded in the supposed implications of neuroscience that “is conceivably demonstrable by scientific findings,” because such models “fundamentally den[y] our ordinary understanding of ourselves,” they are “unlikely to undermine” that understanding until and unless such demonstration occurs (Morse 2006, 402). That day will never come. Recent studies contradict the narrow supposition of these models’ proponents, suggesting that, to the contrary, it is not just “law” that must be considered in the mix with “judicial politics” and that, as with “law,” whether and when other influences affect judicial behavior is context-dependent.

B. Against Dichotomies

The second lesson I take from a review of the judicial behavior literatures is that, just as it is an error to posit a dichotomy between judicial independence and judicial accountability (Burbank & Friedman 2002b), so, *in describing judicial behavior, it is an error to posit a dichotomy between “law” and “judicial politics.” Instead, like judicial independence and accountability, “law” and “judicial politics” are different sides of the same coin. They are not opposites but rather complements.*

This proposition may be logically anterior to the first, and it is implicit in the way in which I framed some of that discussion, in particular by referring to “the relationship between ‘judicial politics’ ... and ‘law’” and the “mix of ‘law’ and ‘judicial politics.’” It is also suggested by Thomas Reed Powell’s caution that “[t]hose who see law as *only* this or *only* that see but narrowly” (Freund 1956, 800). I chose this order instead, because the evidence adduced above suggesting that “law” matters on all courts at least some of the time should make it clear that a “law”/“judicial politics” dichotomy is untenable with respect to those courts and may make it easier to rethink the relationship between them more generally. On the view I take here, the answer to the question at the center of this conference -- What’s law got to do with it? -- depends in important measure on how one defines law.³⁶

Those who deny judicial independence have tended to think in absolute terms (i.e., independence *or* accountability, not independence *and* accountability), to neglect the role that dialogic processes play in a system of separated but interdependent lawmakers, and to confuse influence with control (Burbank 1999). Those who deny that “law” matters (judicial accountability) have tended to model and code in absolute, uni-dimensional terms, to deny (because, for some, their theory demands it) the influence of dialogic processes, and to confuse the results of statistical tests confirming the influence of one variable in an impoverished model with proof of their prophecy.

My proposition is not, however, just that different models of judicial behavior should be regarded as complements because “each model accurately captures some of what every judge does some of the time, and ... no single model is likely to describe any judge all of the time” (Robbennolt, MacCoun, and Darley 2008, 1-2). Judge Posner has warned that “one must be careful about dividing judicial decisions (or judges) into legalist and political, or, what is closely related, asserting a Manichean dualism between law and politics. The dualism only works when ‘law’ is equated to legalism, and that is too narrow” (2008, 8; see also 47). Or again,

[t]he middle ground is not the idea that adjudication is part “law” and part “ideology.” When the authors of a study of political voting by federal appellate judges, finding areas of law in which the judges’ presumed political leanings do *not* seem to influence their votes, conclude that “perhaps in those areas the law is effectively controlling,” they are defining “law” too narrowly. Law is suffused with ideology (2008, 43).

Other scholars have also observed that “law” and “judicial politics” “are not, in fact, mutually exclusive categories: the ‘law’ may explicitly give room for a judge’s ‘ideology’ to operate” (Fischman & Law 2008, 6). In that regard, consideration of the many roles that, without apology or disguise, judicial discretion plays in “law” suggests both that the territory where “judicial politics” are part of (cannot usefully be distinguished from) “law” is substantial (Gillman 2001), and that, like decisions about

formalism (Schauer 1988), decisions about discretion (and hence about the proper role for “judicial politics” in “law”) have to do with power.

Positive political theorists have been skeptical that legal norms in themselves motivate judges, in part because they find formalistic accounts of law implausible. The law, however, need not be fully determinative in order to have a binding quality. Jurisprudential accounts recognize that law can be both binding on judges *and* permit them to exercise discretion in certain contexts. The discretion exists not only because legal rules will inevitably be indeterminate at some point, but also because social needs demand some measure of flexibility in the application of legal rules, and because institutional values argue for allocating different types of power between different levels of the judiciary (Kim 2007, 442).³⁷

Obscuring this relationship between (not opposition of) “law” and “judicial politics” were, at one time, claims made by lawyers, scholars and judges concerned to establish, preserve or augment their professional power by minimizing judicial agency in (or, as opposed to) “law” (including even, or especially, the common law), and more recently, anachronistic insistence on such claims by political scientists concerned to establish, preserve or augment disciplinary power by maximizing judicial agency in (or as opposed to) “law.”³⁸ Critical to acceptance of this proposition by both groups is a willingness to renounce reductionist thinking (Clayton 2003, 308), even at the risk of losing professional power. Like a “[m]onolithic brain explanation of complex behavior,” a monolithic social science explanation of judicial behavior is almost always “radically incomplete” (Morse 2006, 464).

The quotation above from Professor Kim’s valuable discussion of judicial discretion has both positive and normative dimensions. Confident that she, Judge Posner (2008), and others in law and political science (e.g., Gillman 2001) are correct as a descriptive matter when they insist on a more nuanced view of law that recognizes the “interpenetration of law and [judicial] politics” (Whittington 2000, 631), I will not pause long over the objection that such accounts are non-falsifiable (Segal & Spaeth 2002, 48 n.12, 433).

One response to that objection is a reminder that a person whose only tool is a hammer is prone to see only nails. Another is to recall the reasons for humility, canvassed above, given how the science of modeling and statistics has fared even with models not at all nuanced in this respect. A more hopeful response is that the relevant science (which, after all, has come a long way since the realists' forays into the empirical world) may one day catch up to the complexity of the enterprise, and that in the meanwhile, as recent work suggests, some law can be modeled and its influence tested through statistical manipulation. For the rest, and given that, as I discuss below, the stakes are not limited to the world of scholarship, I prefer the messiness of lived experience to the tidiness of unrealistically parsimonious models.³⁹

Like those responsible for legal processes, legislative and judicial, scholars who study those processes and the human beings who make them work should seek enlightenment from science. At the end of the day, however, just as “it may be a mistake to let science furnish not only evidence with which we adjudicate controversies but the standards for deciding whether evidence can be considered” (Burbank 1996; see also Lempert 2009), so too may some judicial behavior elude the scientific techniques by which we seek to pin down phenomena in the natural world (Edwards 2003).

In thus rejecting the belief of some realists “that truth [is] wholly dependent on empirically established facts,” I do not side with the rational absolutists' belief “that human reason [can] discover certain universal principles of justice by philosophical analysis of the nature of reality” (Purcell 1973, 176). I simply opt for an epistemology that seems appropriate in light of how little we know about the causal mechanisms of human behavior, mindful that

[i]n the long history of humankind, the social sciences were developed only recently, but we have been trying to figure ourselves out since we first began to think. It defies the imagination that one new methodology or theoretical assumption is going to topple all previous efforts to understand the human condition (Wolfe 2008, 55).

This is not the occasion to delve deeply into the normative dimensions of Professor Kim's account of judicial discretion. Yet, some consideration of those dimensions is appropriate, if only as a way to anticipate questions likely to be raised by the proposition that "law" and "judicial politics" are complements or different sides of the same coin. I have already indicated that complementarity means more than existing side-by-side. But if it is true that "law" is "suffused with" "judicial politics" (Posner 2008, 43), do we not confront again the dilemma that bedeviled the realists: "how could the idea of the subjectivity of judicial decision be squared with the doctrine that free men should be subject only to known and established law" (Purcell, 1973, 94)?

Professor Kim posits that judicial discretion exists in part because "social needs demand some measure of flexibility in the application of legal rules" (2007, 442). She thus helps us to understand that "judicial politics" are a complement to "law" not only as the necessary price, in a human construct, of filling interstices and resolving indeterminacies. "Judicial politics" are also a complement to "law," because "law" needs their mediating influence (Scheppel 2002). It is equally true, however, that for reasons that span the legal landscape – from the predictability and even-handedness that a well-functioning market economy requires, to the respect for other lawmaking institutions that constitutional democracy requires – "judicial politics" need the restraining influence of "law." *In other words, known and established (but not necessarily determinate) law and the pursuit of a judge's preferences on matters of policy relevant in litigation are complements in the sense that, like judicial independence and accountability, they need (or at least must rely on) each other.* To acknowledge that normative views about the proper balance between them as to any particular judge or court (see the first proposition above) may differ or that, even on the same normative view and as to the same judge or court, the proper balance may change over time, is not to deny that, on some courts and in some cases, (1) "law" without "judicial politics" would be weak and feeble, or (2) "judicial politics" without "law" would be dangerous.⁴⁰ *Moreover, it is not to deny that the two in combination can constitute law in the colloquial sense.*

Judge Posner argues that

the reasons for the legislative character of much American judging lie so deep in our political and legal systems and our culture that no feasible reforms could alter it, and furthermore that the character of our legal system is not such a terrible thing. The falsest of false dawns is the belief that our system can be placed on the path to reform by a judicial commitment to legalism – to conceiving the judicial role as exhausted in applying rules laid down by statutes and constitutions or in using analytic methods that enable judges to confine their attention to orthodox legal materials and have no truck with policy (2008, 15).⁴¹

One useful perspective on the historical influences to which he alludes is provided by the history of equity. That American lawyers, scholars and judges in the nineteenth and early twentieth centuries could with straight faces have denied the role of judicial agency in (or as opposed to) law is, from a historical perspective, hardly surprising. Apart from the advantages of such a position as a means to establish, preserve or augment professional power, the common law looked relatively determinate and quite free of “judicial politics” when compared with the system of equity with which it had long been in competition and remnants of which survived the Revolution, reshaped in its image. As described by Professor Subrin:

By the sixteenth century, the development of common law jurisprudence thus reflected a very different legal consciousness from equity. Common law was the more confining, rigid, and predictable system; equity was more flexible, discretionary, and individualized. Just as the common law procedural rules and the growth of common law rights were related, so too were the wide-open equity procedures related to the scope of the Chancellor’s discretion and his ability to create new legal principles. In equity, the Chancellor was required to look at more parties, issues, documents, and potential remedies, but he was less bound by precedent and was permitted to determine both questions of facts and law. The equity approach distinctly differed from the writ-dominated system. Judges were given more power by being released from confinement to a single writ, a single form of action, and a single issue, ...[they were not] as bound by precedent; and they did not share power with lay juries (1987, 920).

Much changed in the relationship, both in England and in the United States. In England, common law and equity were separate systems (of substantive rules, procedures, courts and judges).

In the 1600s there were titanic struggles with the common law and Parliament. The spirit of their attack on equity and the Crown can be partly sensed in the sneer of lawyer and historian John Selden around 1650 that “Equity is a Roguish thing: for Law we have a measure, know what to trust to; Equity is according to the Conscience of him that is Chancellor, and as that is larger or narrower, so is Equity. ‘Tis all one as if they should make the Standard for the measure we call a Foot, a Chancellor’s Foot; what an uncertain Measure would be this.” ...

Chancery prevailed in these struggles. But to do so, it surrendered its rationale of exercising the royal prerogative. And it recognized an obligation to treat like cases alike. It was no longer the king’s delegate extraordinarily dispensing justice case-by-case, but a regular court of constant resort separately applying its general principles called maxims and even applying rules that it largely adopted from the common law. As a consequence, Chancery received a flood of cases. A reporting system started to embody the output. Equity no longer simply “did equity,” but instead acted only within its fixed jurisdictional bounds. Equity was even following *stare decisis* by 1700 (Field, Kaplan & Clermont 2007, 1072).

The price of equity’s survival thus involved becoming more like the common law. Still, in part because it reposed so much power in judges (unchecked by juries), equity had a chilly reception in some colonies and states of the new United States.

The merger of common law and equity was one of the hallmarks of state procedural reform in the nineteenth century, and it was one of the major accomplishments of the federal Rules Enabling Act of 1934 and the 1938 Federal Rules of Civil Procedure (Burbank 1982a). As Professor Subrin (1987) has demonstrated, the federal merger gave us procedure influenced much more by equity than by common law.⁴² More important for present purposes, however, is Subrin’s description of the relationship between common law and equity. Following Maitland’s insight that equity presupposed the existence of common law, Subrin observed:

In assessing the place of equity practice in the overall legal system, it is critical to realize the extent to which the common law system operated as a brake. One could not turn to equity if there was an adequate remedy at law. Equity grew interstitially, to fill in the gaps of substantive common law (such as the absence of law relating to trusts) and to provide a broader array of remedies – specific performance, injunctions, and accounting. Equity thus provided a “gloss” or “appendix” to the more structured common law. An expansive equity practice developed as a necessary companion to common law (1987, 920).

This is one way, as a normative matter, to think about the relationship between “law” and “judicial politics.” *Or at least it is once one recalls that equity was and is a species of law as we colloquially define it.* Again, the balance between them will vary within a judicial system and across the litigation landscape, and it will vary over time.

It is often said that “we are all legal realists now” (Kalman 1986, 229). Certainly, very few if any lawyers believe today that a constitutional case can be decided in the mechanistic way that Justice Owen Roberts described.⁴³ Yet, it is probably true that the lawyers of every generation need to relearn the lessons of legal realism. One such lesson is that, even when a legal provision does not invite the infusion of judicial policy preferences, it may be sufficiently indeterminate that judges so inclined can deploy their power to try to implement their personal policy preferences. Another lesson is that undisciplined pursuit of judicial policy preferences is difficult to square with the assumptions of a system that aspires to democracy under law.⁴⁴

As this discussion suggests, however, the lessons of legal realism are misleading to the extent they imply a dichotomy between “law” and “judicial politics.” The critical concept is that of discipline, and it operates in both directions.

In one direction, cynicism engendered by a monolithic (and dichotomous) conception of both law and politics makes it too easy to ignore the extent to which “law” must rely on “judicial politics” in order to avoid (1) crippling formalism (because the gaps and indeterminacies, planned and unplanned, that result from avoiding it will be

filled by human beings who have policy preferences), and (2) socially destabilizing unfairness (because, as Hamilton observed, independent judges are needed to provide relief from, by mitigating the severity and confining the operation of, “unjust and partial laws”) (Hamilton 1961; Burbank 1999). Recall that Judge Posner describes “the legislative character of much American judging” as “not such a terrible thing” (2008, 15).⁴⁵ Professor Ramseyer goes further. In his view, “[t]hat [judges] act politically in political cases simply reflects their essential independence. That politics matters should not embarrass. To the extent judicial independence is a good, it should engender pride” (2008, 3). If this does not assume a dual dichotomy, it invites attention to the constraining influence, promoting accountability, of “law.”

In the other direction, I find persuasive Kritzer and Richards’ argument that “the justices create jurisprudential regimes to provide guidance to other political actors [including other courts] and to themselves,” and that “the goal here is consistency” (2005, 35). Moreover, a legal system disciplines “judicial politics” not only when it provides guidance through law that is determinate, but also by means of practices, norms, and customs that have accreted around the judicial office,⁴⁶ of which adherence to precedent is the most important. Such practices, norms and customs can themselves be regarded as instantiations of a penchant for self-discipline (Ferejohn & Kramer 2006) that acknowledges the sources of, and constraints on, institutional power in a system of separate but interdependent lawmaking institutions (Cross 2007; McCloskey 2005). In this too, the relationship between “law” and “judicial politics” is akin to the relationship between judicial independence and judicial accountability (Geyh, 2006; Burbank & Friedman 2002b).

C. Institutional Design

The third lesson I take from a review of the judicial behavior literatures is that, as with judicial independence and judicial accountability, *the quantum and quality, or mix, of “law” and “judicial politics” depends, or should depend, on what a particular polity wants from its courts.*

This proposition can be viewed as a normative corollary to, or partial restatement of, the proposition that it is an error to think that the relationship between “judicial politics” and “law” is or should be the same with respect to every judge (court) in a particular judicial system, or that, even with respect to judges on the same court, it is or should be the same in every type of case. It echoes Professor Schauer’s suggestion that formalism (or what Judge Posner calls “legalism”) “ought to be seen as a tool to be used in some parts of the legal system and not in others” (1988, 547), as it does, conversely, Professor Kim’s argument that judicial discretion exists in part “because institutional values argue for allocating different types of power between different levels of the judiciary” (2007, 442).

As Alan Wolfe has observed, the “Social Sciences are not just empirical; they are normative, too. ... [D]emocratic debate is not well served by pretending that the empirical findings of a single controversial approach in a single academic discipline contain definitive answers to [normative] questions” (2008, 50). The fact that quantitative judicial behavior scholarship, standing alone, cannot answer such questions of institutional design is, of course, no reproach. Properly conceived and properly implemented, such research could be of enormous benefit to those responsible for institutional design who are interested, or can be persuaded to take an interest, in knowing what judges in fact do and why they do it. In order to be “properly conceived” for these purposes, the research will have to reflect attention to Professor Friedman’s concern that quantitative scholarship too often has lacked “normative bite” (2006, 262), a concern recently echoed in Wolfe’s observation that “[t]echnique comes first in the new [behavioral] economics, just as it did in the old, and conclusions follow” (2008, 53). It may also be true that, before research of this type and with this potential is undertaken, those capable of conducting it will have to broaden their (research) horizons to include questions of “[p]ower and authority” (Whittington 2000, 631) that they have hitherto neglected.

On the assumption that future quantitative research will be designed to address institutional questions of import, the failure or inability of some theories or models of judicial behavior to acknowledge or accommodate the documented phenomenon of preference change (or “drift”) (Epstein, Martin, Quinn & Segal 2007; Ruger 2005; Burbank 2007a) will limit their ability to inform wise public policy even if their other assumptions and methodologies are accepted. Of greater concern, the apparent reluctance of some quantitative scholars to embrace and move on from evidence that judicial behavior is not monolithic, even on the same court, and even if that court is the United States Supreme Court, is antithetical to the development of wise public policy.

At least until recently, quantitative research on judicial behavior by political scientists was not likely to come to the attention of those responsible for the development and implementation of public policy affecting the judiciary. That may also be true even of the non-quantitative work of political scientists whose primary scholarly interest in courts relates to institutional dynamics. Meanwhile, the work of those in academia most likely to be consulted – law professors – has too often been relentlessly normative or woodenly quantitative, eschewing relevant insights of both history and (other) qualitative social science (Burbank 2006). Perhaps even more distressing has been the latter day promotion by law professors of “scholarship in the aid of reform” (Burbank 1982a, 1186), this time with the bells, whistles, and apparent invulnerability of statistics (Schkade & Sunstein 2003; see Sisk & Heise 2005).

There are many examples of reforms of the judiciary that have not turned out the way those responsible for them intended or have turned out in ways they could never have imagined. Thus, although we may assume that Congress created the federal courts of appeals in 1891 in order to lessen the docket burden of the Supreme Court, it is not clear that the legislation would have passed but for the perception that district courts (judges) lacked adequate legal accountability (Geyh 2006). As a result of a number of other alterations in the relevant landscape, however, district courts have been re-empowered (Yeazell 1994).

The Congress that in 1925 acceded to the Justices' request to make the Court's docket almost entirely discretionary surely did not imagine the effects of that legislation. As recounted by Professor Hartnett (2000), their putative surprise could be explained in part by the failure of subsequent Courts to keep almost all of the promises that the Justices made to Congress when seeking the legislation. More consequential by far, Hartnett suggests (following a number of political scientists (Provine 1980; Pacelle, 1991)), has been the doctrinal freedom that release from the obligation to police its consequences has afforded, coupled with the power that the Court seized by unilaterally "claim[ing] the authority to issue limited grants of certiorari, that is, to decide only a particular issue in a case, ignoring the other issues" (2000, 1705). As to the former, Hartnett asks:

But would the Supreme Court have incorporated the Fourth, Fifth, Sixth, and Eighth Amendments if it were obligated to review every state judgment that upheld a criminal conviction or sentence over a defendant's objection based on one of these Amendments? And if it did, is it remotely possible that it would have spun out such elaborate doctrinal requirements if it were required to apply and enforce them in every such case (2000, 1732)?

It is not clear which, if any, of the institutional design decisions, by Congress or the judiciary, that these examples reflect would have been changed if those making them had available relevant quantitative and qualitative political science research (of the sophistication and quality that can be expected today) and/or legal scholarship informed by the fruits of such research. One can, however, profit from interdisciplinary work of the sort that Hartnett's article represents when comparable questions of institutional design are presented in the future. Thus, for instance, both quantitative and qualitative scholarship and institutional design decisions about the federal courts of appeals should reflect the fact that, just as the Supreme Court "became essentially impervious to caseload pressures" after 1925,

so, we now know, are the courts of appeals, at least so long as they have an adequate supply of judicial surrogates with whose help they can create functionally discretionary dockets through tiered decisionmaking, leaving Article III judges freer to search for lawmaking opportunities. In both cases, the

ability of the courts to set their own agenda permits them “to ‘shed the long-standing image of a neutral arbiter and an interpreter of policy’ and emerge ‘as an active participant in making policy’” (Burbank 2005, 23).

One is not left entirely to speculation, however, about the possible impact of such scholarship on the assessment of current proposals affecting the institutional design of the judiciary. In order to reach an informed judgment about the claims made by those promoting a non-renewable eighteen-year term for Supreme Court Justices (Cramton & Carrington 2006), recourse to history and to both quantitative and qualitative research in political science is essential. The relevant research concerns not just the Court itself, but what the public knows about the Court and how (as bearing on legitimacy), and the behavior of interest groups (Burbank 2006).

IV. Conclusion

We have made great progress in the study of judicial behaviors, and law professors interested in that subject owe a large debt to scholars in political science. Their work, quantitative and qualitative, has enriched our knowledge and provided us with new tools and perspectives. In devoting attention to the more extreme claims of some quantitative political scientists, I do not mean to diminish either their contributions to this body of knowledge or the potential that their work holds to enable additional advances. Just as it is a mistake to banish law and other influences from the behavioral landscape, even at the Supreme Court of the United States, so is it a mistake to neglect the numerous insights that the attitudinalists and other like-minded (as to judicial preferences) scholars have provided, and continue to provide, that shed light on an extraordinarily complex subject.

I have argued that the record to date counsels humility about our capacity definitively to pin down the causal mechanisms of judicial behaviors and that, for this and other reasons, it also counsels embrace of methodological pluralism. These are not, however, the only reasons for scholars to abandon their intra- and inter-disciplinary turf

wars, to resolve to help, and learn from, each other, and to pay close attention to the ways in which they present their findings and state their claims.

Three years ago, I described the state of interbranch relations affecting the judiciary as follows:

Recent years have witnessed attacks on the courts, federal and state, that have been notable for both their frequency and their stridency. Many of these attacks have been part of strategies calculated to create and sustain an impression of judges that makes courts fodder for electoral politics. The strategies reflect a theory of judicial agency, the idea that judges are a means to an end and that it is appropriate to pursue chosen ends through the selection of judges who are committed or will commit in advance to pursue those ends on the bench. The impression sought to be created is that not only are courts part of the political system; they and the judges who make them up are part of ordinary politics.

At the federal level, pursuit of these strategies prompts politicians to curry favor by promising to hold courts accountable: staffing them (or ensuring that they are staffed) with reliable judges, monitoring them through “oversight,” and, when they stray, reining them in through the instruments of politics, ordinary or extraordinary (impeachment). At both the federal and state levels, these strategies enable interest groups to wield influence by framing judicial selection in terms of the supposed causal influence of a vote in favor of or against a judicial nominee or candidate on results in high salience cases such as those involving the death penalty or abortion.

....

Given what we know about public knowledge of and attitudes towards courts and about the incentives and tactics of the interest groups that are involved in judicial selection, there is reason to fear that the distinction between support for courts irrespective of the decisions they make (“diffuse support”) and support depending on those decisions (“specific support”) will disappear. If that were to occur, the people would ask of the judiciary only “what have you done for me lately?,” and, at least in systems where they lacked the ability to express their preferences directly (i.e., by voting in an election or retention election), would expect elected officials to help them secure the desired results by holding courts and judges “accountable” when they failed to produce them. Politicians whose strategies had encouraged viewing judges as policy agents

would have no incentive to resist. Those inclined to resist in the public interest might find it very difficult to do so.

In such a system, law itself would be seen as nothing more than ordinary politics, and it would become increasingly difficult to appoint (elect, or retain) people with the qualities necessary for judicial independence, because the actors involved would be preoccupied with a degraded notion of judicial accountability. At the end of the day, judicial independence would become a junior partner to judicial accountability, or the partnership would be dissolved (Burbank 2007b, 910, 916).

It may be that these risks have diminished at the federal level with the loss of power of those primarily responsible for the strategies I identified and the attacks on courts that were designed to implement them. Even if so, however, recrudescence is possible, and, in any event, the federal courts are responsible for only a very small portion of the judicial business in this country. It thus remains important that scholars not contribute to the process of assimilating the law made by judges to ordinary politics unless they are confident, and have adequate reason to be confident, about their findings and the inferences they draw from them (Sisk & Heise 2005)..

Professor Gillman has cautioned judicial behavior scholars to consider “the potential real world consequences of this debate [between thinking of law as behavioral uniformity and thinking of it as good faith deliberation], especially as it moves beyond the insulated borders of political science,” and “to be especially careful of what we mean by legal and political decision making” (2001, 497). Judge Posner has observed that the “attitudinalists’ traditional preoccupation with politically charged cases decided by the Supreme Court creates an exaggerated impression of the permeation of American judging by politics” since “[m]ost cases decided by American courts are neither politically charged nor decided in the Supreme Court” (2008, 27-28). He has also pointed out that the trend toward selecting “judges who will be politically dependable ... is the triumph of the attitudinal school” (2008, 169).

This is not a plea that “valid social research should not be undertaken in order to protect cherished myths” (Segal & Spaeth 2002, 429). There is a difference between the merit of a research topic and of a model used to explore it, as there is between the merit of a model and the methods by which, and the data with which, it is tested. In any event, one need not be a mythologist to believe that, the more obvious the real world implications of research, the more important it is that researchers attend carefully not just to their models, methodology and data, but also to their findings and the claims that those findings reasonably support. The ability of policymakers who are so inclined *not* to “see but narrowly” (Freund, 1956, 802) when considering institutional design (and other) questions affecting the judiciary depends critically on the willingness and ability of scholars to abjure methodological imperialism and to recognize the value of pluralism in perspective as well as methodology.

[I]t is possible to understand us [human beings] – slowly, patiently, in fits and starts, and with due respect for those who have been studying us for so long ... In both the social scientific and humanistic studies of human existence, it is not revolutions that we need. What we need are observations and suggestions and ideas, collected one at a time, by different people, from different disciplines, with different methodologies. That is not sexy, but neither is it easy (Wolfe 2008, 55).

¹ Nine years previously I had observed that Judge Weinstein’s professed indifference to the Federal Rules of Civil Procedure might be shared by “federal judges who do not share his politics,” leading to the concern that “as judges of Jack Weinstein’s sympathies are replaced by those whose interests lie elsewhere, ‘the personality of our legal system’ he describes will ... seem as remote as the history of the Rules Enabling Act” (Burbank 1988, 33).

² The phenomenon is not restricted to attitudinalists. Professor Whittington notes that “rational choice models have no mechanism for dealing with ambiguity. As a result,

scholars in this tradition have tended to produce highly stylized models of judicial behavior with unclear relevance to actual decision making” (2000, 627).

³ I continued: “That is not only because judges are human. It results as well from the fact that in our system ‘the line between law and policy ... is blurred [because] [m]any cases cannot be decided by reasoning from conventional legal materials [and] [s]uch cases require the judge to exercise a legislative judgment’” (Burbank 1997, 1999).

⁴ So, apparently, do political scientists. Segal and Spaeth’s description of the law of preclusion is a disaster (2002, 236-37).

⁵ “Without such knowledge or perspective, we may be prisoners of the unarticulated agendas, conscious or unconscious, of those responsible for making and administering the rules, and of those with the power to bring about change in their content” (Burbank 2002a, 343). The “knowledge or perspective” needed, I argued was that “necessary to get behind the words of a case or rule and to evaluate by some method more scientific than navel-gazing (which is how I would uncharitably characterize many discussions of ‘policy’ in procedure or other fields) and more socially productive than ideology (which is how I would uncharitably characterize many works of critical legal scholarship) the fruits of technical reasoning, however acute” (Burbank 2002a, 343).

⁶ Robert Hutchins, who adopted a realist approach while a professor at Yale Law School, rejected its tenets after he became President of the University of Chicago. As described by Purcell, Hutchins came to believe that “the search for facts to discover how the law operated had failed in its purpose, for without some basic ideas of the true nature of law, legal researchers did not know what facts to look for nor what to do with them once they found them. The resulting research ‘did not help us to understand the law, the social order, or the relation between the two.’ The whole concept of the ‘fact situation,’ Hutchins concluded, threatened the legal profession ‘with a *reductio ad absurdum*.’” Hutchins also came to believe that his colleagues “had been forced to surreptitiously bring in their own personal value judgments in order to make the pragmatic criteria viable” (1973, 146).

⁷ “Introduced originally into American legal education in 1871 by Christopher C. Langdell, the dean of Harvard Law School, the case method rested on the assumption that a close study of past judicial decisions would reveal the basic principles and rules of the law that had led to the various decisions” (Purcell 1973, 75).

⁸ “But saluting these content-free, technocratic-seeming precepts is to adjudicating as spring training is to the baseball season. The precepts are warm-up measures. Closure requires agreement on substance. Without that, the choice of neutral principles is up in the air. No more than legal realism could legal process offer a substitute for legalism on the one hand and politics and emotion on the other” (Posner 2008, 236).

⁹ Whether one deems empirical work by scholars of law and society (Mather 2008) an exception to this proposition may depend on one’s views about “elite law schools.”

¹⁰ It is true that procedural rules are never neutral in their effects, if not their purposes. It is also likely that there has been more systematic misrepresentation about the value-free nature of procedural rules than about any other category in the traditional lexicon. But what does it mean to say that procedural choices are “political”? To some it may mean either that procedural choices are driven by an individual’s own substantive

values or that they should be. One holding that view may be suspicious of the purposes of every judge or law reformer who has a choice in the application or formulation of doctrine. There is, however, a difference between purposes and effects. Neither judges nor procedural reformers have a general charter to reform society, and broad-scale social reform would be necessary to eradicate the non-neutral effects of many, and perhaps most, procedural rules.

If procedural rules are not neutral and judges and reformers should not use procedural rules to advance their own substantive values, we confront a paradox: neutral transmission of the substantive law, if possible, would itself be a political act because it would reinforce the status quo. The paradox disappears to the extent that one can distinguish individuals' values from the values that inform the rules of substantive law. The reminder that there is no bright line between procedure and substantive law has been a refuge of procedural reformers for fifty years. But the existence of "under-determinacy" is no reason to wipe the slate clean (Burbank 1987, 1472-73).

¹¹ It cannot have helped that the "whistleblowing" rhetoric of some of those writing in law reviews whose work inspired a renaissance of the rational choice model in political science not only dismissed law but left no room even for motivated reasoning (Cross & Tiller 1998; Kim 2009).

¹² Professor Spaeth has recently reiterated his insistence that Pritchett's "assertion that judging is different from the free choice of congresspersons or administrators is simply false" (Spaeth 2008, 753).

¹³ Professor Spaeth's recent description of "a vehement antibehavioral onslaught" (2008, 757) is a useful reminder that the intolerance within political science has run in both directions. Gillman refers to a "symmetry of frustration" (2001, 484). "Almost fifty years after these seminal accounts were published, the degree to which contemporary political scientists acknowledge the possibility of legal influence on judicial behavior appears to correlate with the degree to which they use interpretive methods" (Keck 2007, 515).

¹⁴ "Like Newton, academics stand on the shoulders of giants – but they stand on them to bury them" (Wolfe 2008, 55).

¹⁵ Compare Professor Hilbink's review of the sections of the Oxford Handbook of Law and Politics dealing with comparative and international/supranational perspectives:

In striving to establish our legitimacy as comparatavists interloping in a traditionally Americanist domain, however, I fear we are, perhaps unwittingly, reproducing some of the existing pathologies of the law and courts subfield. ... I was struck by three things: first, the almost exclusive focus on high courts and constitutional decision-making in the countries of study; second, a tendency to discount the relevance of factors in any way internal to law or legal institutions; and third, a limited and limiting view of politics as narrowly instrumental (Hilbink 2008, 1098).

¹⁶ The empirical legal studies (ELS) movement is broader than portrayed by Keck, who described it “as the new law school equivalent of what political scientists usually call ‘judicial behavior’ research” (2007, 512). Moreover, the account in the text does not include either economists or psychologists who do empirical work and who have been hired by law schools in increasing numbers.

¹⁷ See *supra* note 6. “Legal pragmatism is disciplined by a structure of norms and doctrine, commonly expressed in standards such as negligence, good faith, and freedom of speech, that tells judges what consequences they can consider and how (in what relation to each other, for example). Take away the framework and what judges do does not merit the word ‘law’” (Posner 2008, 362).

¹⁸ Dialogue about judicial behavior among legal scholars and qualitative political scientists is well-established (Keck 2007).

¹⁹ Those aware that law reviews are not peer-reviewed may ask “What is second prize?” As the earlier reference to the debate (and ultimately dialogue) between scholars teaching in law schools and judges suggests, the benefit is the greater opportunity that publication in law reviews affords actually to reach and affect those being studied (and others with whom judges interact).

²⁰ “That the justices use their attitudes to decide cases still does not tell us why they might do so ... Indeed, theory is probably Spaeth’s weakest area” (Benesh 2003, 125).

²¹ “Epstein and Knight [1998] reject this possibility [that adherence to norms stems from the justices “professional (and perhaps ideological) commitment to the substantive content of those norms”], and it is not entirely clear why, although one suspects that it has to do with their assumption that policymaking is the primary objective of the justices....But one result of this assumption is that, at times, some of the explanations of the book feel a bit forced” (Bloom 2001, 223).

²² As discussed below, those willing to indulge the possibility of truth without formal statistical proof, have other reasons to doubt that the only “attitudes” driving the behavior of judges, including Supreme Court Justices, are their attitudes as to policy (Baum 2006; Kim 2007; Posner 2008).

²³ Segal and Spaeth assert that “[w]hen prior preferences and precedents are the same, it is not meaningful to speak of decisions being determined by precedent” (2002, 290). The obverse is equally true. “In this formulation, what is important is not whether the justices are acting on the basis of law or politics; that question is likely to be difficult to answer other than by fiat – naming certain commitments ‘legal’ and other ‘political’” (Keck 2007, 550). Compare Segal (1984) with Segal & Spaeth (2002).

²⁴ “Variables may also not bear the meaning attributed to them because of coding conventions or coding errors. Readers of empirical work should always look behind the language in which results are presented for information about how a concept has been *operationalized*, which is to say instantiated in the data” (Lempert 2009, 244).

²⁵ Methodological humility seems appropriate for the anterior reason that, once one recognizes that most judges, most of the time, seek to advance a variety of preferences,

one confronts the fact that “it is difficult to test formal models when multiple goals are being pursued” (Brenner 2003, 280).

²⁶ Compare Professor Kritzer’s description of Martin Shapiro’s anticipation of “what we today call the new institutionalism” (2003, 387):

The thrust of Shapiro’s analysis is that the Supreme Court’s role in the policy process varies substantially from area to area. The variations depend on factors such as the need for technical expertise, the level of detail involved in the area, the constitutional or statutory nature of the issues, and the nature of the policy implementation process (2003, 390).

For the view that “[e]mpirical and normative studies of circuit courts must take into account their unique historical, geographic and legal characteristics,” see George & Yoon, 2008, 50.

²⁷ Similarly, Paul Frymer has recently written that “[w]ere Law and APD [American Political Development] scholars to examine courts more broadly as institutions, they would see the multitude of roles judges (Supreme Court, federal, state, and administrative), lawyers, appellants, and institutional procedures and rules play in both the development and current functioning of modern government” (2008, 782).

²⁸ “Focusing on non-unanimous cases or on controversial issue areas, as some of Spaeth’s work does, misses these easy cases, thereby underestimating the strength of the legal model” (Benesh 2003, 124). Compare in that regard the explanation given for the view that the Court’s control over its docket “is a requisite for” the Justices voting their policy preferences: “Many meritless cases undoubtedly exist that no self-respecting judge would decide solely on the basis of his or her policy preferences” (Segal & Spaeth 2002, 93). Professor Spaeth has recently sought to explain unanimous decisions as caused by “indifference.” Thus, “[a]t the Supreme Court level, decisions of limited applicability, such as those involving narrow tax questions, matters of arcane civil procedure, or the preemptive effect of federal legislation (*sic*), may also engender indifference” (2008, 762).

²⁹ Professor David Shapiro has provided a good recent summary of the empirical literature on the role of precedent on the Supreme Court (2008, 938-40).

³⁰ Two things fatally undermined legal realism in the eyes of the professional legal community and later killed off critical legal studies, legal realism’s radical grandchild. The first was that the realists exaggerated the open area, sometimes implying that all cases are indeterminate. The second was that the noisier realists imputed willfulness, whether in the form of politics or prejudice or sheer orneriness, to judges” (Posner 2008, 112).

³¹ “How could a judge think himself a good judge if he thought his decisions seasoned with politics or personality? One answer is that he might be sophisticated enough to realize that this just is the nature of American judging. But a more interesting answer is that the nonlegalist influences on a judge are likely to operate subliminally” (Posner 2008, 65).

³² Jack Knight and Lee Epstein note that *stare decisis* serves as an important norm constraining Justices from being “motivated by their own preferences” because, *inter alia*, attorneys rely primarily on precedents, the Justices invoke

them at conference and in their opinions, and cases overruled amount to only a miniscule percentage of those available for overruling. At the very least, then, precedent shapes strategic behavior by advocates and judges – even those who may put little stock in the doctrine. Presumably they do so not because they are all engaged in a collective effort of self-deception but because some significant percentage of those who decide do consider precedent an important factor in reaching a result (Shapiro 2008, 939-40).

³³ Strategic models typically neglect the fact that “law, like other institutions, is created by actors (justices) with political goals (attitudes) whose subsequent decisions are then in turn influenced but not determined by the institutional structure they have created” (Kritzer & Richards 2005, 35).

³⁴ “Absent the ability to peer inside a judge’s mind and observe a thing called ‘ideology’ at work, the only way to measure ‘ideology’ is to focus upon some observable trait or behavior that is correlated with, or indicative of, ideology” (Fischman & Law 2008, 8-9).

³⁵ Compare Alan Wolfe’s conclusions about behavioral economics (or economic psychology). Having observed that neoclassical economics “remains a vulnerable approach, stuck in unrealistic assumptions about human behavior” (2008, 54), he states that “[w]hat began as a movement marked by a curious, if not actually humanistic, sensibility has transformed itself into a one-dimensional vision of human nature in which the perfect rationality of neoclassical economics is replaced by an equally simple-minded conception of human beings as either happiness maximizers or perfect fools” (2008, 55).

³⁶ Observe that, in the absence of a stipulated definition of “judicial politics,” one might as well say that the answer depends on how one defines “politics.”

But I remind the reader that partisan politics is not the only politics, and politics shades into ideology, which in turn shades into common sense, moral insights, notions of sound policy, and other common and ineradicable elements of judicial decision making. Politics in these extended senses is the core of the attitudinal model, sensibly construed... . (Posner 2008, 73).

³⁷ “The amount of legislating that a judge does depends on the breadth of his ‘zone of reasonableness’ – the area within which he has discretion to decide a case either way without disgracing himself. The zone varies from judiciary to judiciary and from judge to judge. Among institutional factors that influence the breadth of the zone is the judge’s rank in the judicial hierarchy” (Posner 2008, 86).

³⁸ Attitudinalists and legalists disagree about the extent of political judging rather than about its existence. One source of that disagreement is that attitudinalists, who mostly are political scientists rather than lawyers, are positive theorists, while most legalists are lawyers, who – inveterately normative as most lawyers are – very much want, and are predisposed by their training and by the mores and understandings of the profession to expect, judges to conform to the legalist conception ... In contrast, since the subject of political science is politics, political scientists expect judging to be imbued with politics – and even want it to be, as demonstrating the power of political science to illuminate behavior (Posner 2008, 47).

³⁹ Courts, not judges, have judicial power, at least in the federal court system. This suggests that the atomistic assumptions of attitudinalism and many rational choice models are also discordant with basic assumptions of the founding.

⁴⁰ “An accountable judiciary without any independence is weak and feeble. An independent judiciary without any accountability is dangerous” (Burbank 2003, 325).

⁴¹ Results of a laboratory experiment suggest that “given a particular policy preference, judgments about a law’s constitutionality depend, in part, on the law’s policy implications” (Furgeson, Babcock & Shane 2008, 225)..

⁴² This (coupled with the recognition of the power of procedure) helps to explain why, more than sixty years after the merger, and in a case involving the provisional remedies available in an action for breach of contract, the opportunity to minimize the powers of a court of equity appealed to Justice Scalia, who had bigger fish to fry (Burbank 2002b).

⁴³ “When an act of Congress is appropriately challenged in the courts as not conforming to the constitutional mandate the judicial branch of the Government has only one duty,—to lay the article of the Constitution which is invoked beside the statute which is challenged and to decide whether the latter squares with the former” (United States v. Butler, 62).

⁴⁴ Another, made by Purcell (1973) and again thirty-five years later by Judge Posner (2008), is that neither in scholarship nor in judging can pragmatism be a value-neutral enterprise.

⁴⁵ Compare the legal realist, Max Radin’s view:

But in that great mass of transactions which will not fit readily or quickly into established types, or will fit into one just as easily as another, the judge ought to be a free agent. We need not fear arbitrariness. Our Cokes and Mansfields and Eldons derive their physical and spiritual nourishment from the same sources that we do. They will find good what we find good, if we will let them (1993, 198).

⁴⁶ “The ‘constraining rules’ operating on judicial actors may not be directly legible off the legal text. Indeed, there may be formal rules and informal norms internal and particular to judicial institutions that shape and constrain judicial conduct by providing, as historical institutionalists put it, ‘the content of the identities, preferences, and interests that actors [can] embrace and express’” (Hilbink 2008, 1099-1100).

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