

Courts and Judges

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Edited by

Lee Epstein

Washington University, USA

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Series Preface

Introduction

The Selection Criteria

Compiling a volume of twenty influential essays on courts and judges is an enterprise fraught with peril. No two scholars toiling in this field are likely to agree on a list of the top fifty, much less the top twenty. Worse still, I am not sure that I – the compiler of the list of essays included here – could replicate my own decisions.

The problem, if I dare call it that, is an embarrassment of riches. Over the past six decades or so, scholars have produced study after study that has contributed to our understanding of judges and courts. Sometimes that contribution is methodological, sometimes it is theoretical, many times it is substantive but almost always it is some combination of the three. So choosing among the hundreds, perhaps even thousands, of outstanding essays is quite a task.

I have tried to minimize it by adhering to the following criteria.

1. *Essays Only*. My contract with Ashgate Publishing states that, ‘the essays should be drawn from relevant periodicals. . . . Chapters from books, even if the books are collections of essays, should not be included’. Of course I followed this rule but I do wish to point out that many of the most important contributions to the study of judges and courts come in the form of books and, to a lesser extent, chapters in edited volumes and unpublished papers. I think here of Pritchett (1941); Schubert (1965); Danelski (1979); Murphy (1964); Rosenberg (1991); Perry (1991); Tanenhaus (1963); Segal and Spaeth (1993), to name just a few. In some instances, the author(s) wrote an essay before or after publication of the book or chapter, and I was able to incorporate those (e.g. Pritchett, 1941; Rosenberg, 1999). But for many that was not the case (e.g. Perry, 1991; Segal and Spaeth, 1993), and I regret that I cannot include them here.
2. *A Focus on Courts and Judges*. The title of this volume is Courts and Judges. Since I was able to identify more than an ample number of seminal essays directly on this topic, I eliminated pieces that were even remotely peripheral. This criterion led to the exclusion of many more than a handful of papers, including two of my personal favorites: the extremely influential Priest and Klein (1984) and Galanter (1974). While both cover courts, their emphasis, it seems to me, is on litigants. Along similar lines, I excluded even landmark papers on juries, interest groups, and attorneys.¹
3. *A Concentration on Positive, Rather Than Normative, Studies*. At the risk of greatly overgeneralizing, two types of scholars study law and courts: one that chiefly focuses on describing and analyzing observations drawn from the legal world; and the other, primarily on (normatively) theorizing about that world, offering prescriptions to change it, or both.² In light of the emphasis of this series, as well as my own inclination,³ I limited my selections (with a few exceptions) to work falling into the former category. I thus excluded essays asking, for example, how judges *should* interpret statutes or whether the practice of judicial review fits compatibly with democratic principles, or were

otherwise motivated largely by normative concerns. Since many of the most influential of these works appear in book form (e.g., Scalia, 1994; Eskridge, 1994; Posner, 1985; Ely, 1980; Dworkin, 1977; Bickel, 1962), this was a less daunting task than it might appear.⁴

4. *The 'Best' Research*. When Austin Sarat, the general editor of the Law and Society series, asked me to edit this volume, he described my job as one of selecting 'twenty of the best already published essays'. It should go without saying – although I already have said as much – that I cannot imagine many scholars agreeing on a definition of 'best'. So, in the interest of full disclosure I should reveal mine. For purposes of making selections for this volume, I define 'best' as studies that changed the way we (or perhaps more accurately I) think about courts and judges.

At the very least, this was my 'separating the men from the boys' criterion. Scores and scores of works make important contributions to the literature but far fewer are sufficiently powerful to alter perspectives about entire areas of study. Sometimes that alteration comes about by settling old and important debates but more likely – judging by the essays in this volume – it happens from opening new ones.

These are the criteria I employed but I am certain others structured my thinking as well. Those primarily come in the form of my own biases, two of which seem particularly relevant. First, while I have a joint appointment in a law school and a political science department, I am a political scientist by training. Accordingly, I am far more familiar with literature in that discipline than in virtually all others, including economics, sociology, anthropology, and psychology. No doubt my selections reflect that familiarity. Another bias is methodological in nature. I have, to be sure, made use of qualitative data and methods but I have a stronger taste for the quantitative. Again with little doubt my selections shore up that preference, though (perhaps ironically enough) the one piece I selected from my own *oeuvre* (Walker, Epstein and Dixon, 1988) relies less on numerical evidence and statistical analysis than most others I have included.

The Volume's Organization and Contents

If there is one word that characterizes the study of courts and judges it is diversity – diversity in the kinds of questions scholars raise, the theories they invoke, and the methodologies they use to assess the implications of their theories. Given this mix, it would be, on the one hand, a nearly Quixotic task to incorporate even the 'best' articles representative of the range of research in a single book, that would necessitate a much larger volume, perhaps even two or three. On the other hand, despite the multiplicity of specific research questions, theories, and methodologies, analysts of courts and judges have coalesced around perhaps a half dozen general substantive concerns. For purposes of organizing this volume, I fold them into four: judicial selection and retention, judicial decision making, constraints on judicial power, and the role of courts in democracies.

In what follows, I briefly describe each topic, and offer some rationale for the essays I selected to represent them. Along the way, I also provide citations to related work that space limitations prevent me from including but nonetheless may be of interest.

The Selection and Retention of Judges

Of all the difficult choices confronting societies when they go about designing legal systems, among the most controversial are those pertaining to judicial selection and retention: how ought a nation select its judges and for how long should those jurists serve?⁵ Indeed, some of the most fervent constitutional debates – whether they transpired in Philadelphia in 1787 (Farber and Sherry, 1990) or in Moscow in 1993–4 (Hausmaninger, 1995) – over the institutional design of the judicial branch implicate not its power or competencies; they involve who would select and retain its members. It is thus hardly surprising to find an immense amount of scholarship on questions pertaining to judicial selection and retention, ranging from the primarily normative (e.g., Garrow, 2000; Oliver, 1986) to the chiefly empirical (e.g., Segal, Cameron and Cover, 1992; Martinek, Kemper and Van Winkle, 2002), to work falling between the two (e.g., Davis, 2005; Epstein et al., 2005; Choi and Gulati, 2002).

In choosing among the many contenders for inclusion here, I limited myself mainly to the empirical (though with the occasional normative implication) and then to two central substantive questions: what explains the selection (and rejection) of judges and what effect(s) do rules governing selection and retention have on the types of men and women who will serve and, in turn, the decisions they, as judges, will make? As to the first, my choice of Cameron, Cover and Segal (1990) is, I think, an obvious one. It may not be the earliest study to attempt to explain why the American Senate confirms some Supreme Court nominees and rejects others but it is the first to elaborate and systematically assess a theoretical account of confirmation politics in the United States. Briefly, it operates under the assumption that electorally oriented senators vote on the basis of their constituents' 'principle concerns in the nomination politics' (Cameron, Cover and Segal, 1990, 528). Those concerns primarily (though not exclusively) center on whether a candidate for the U.S. Supreme Court is (1) qualified for office and (2) sufficiently proximate to the senator in ideological space. An analysis of data drawn from the votes of individual senators over the twenty-two nominations between 1953 and 1987 supports the account.

To say that Cameron and his colleagues made a critical contribution to a line of inquiry of concern to multiple fields and disciplines is hardly to overstate the case. Scholars studying other executive appointments have liberally drawn on its insights (e.g., Routh, 2004; Nixon, 2001; Krutz, Fleisher and Bond, 1998; King and Riddlesperger, 1996; Hammond and Hill, 1993); secondary accounts in the judicial and legislative fields regularly report its results (e.g., Baum, 2004; Smith et al., 2003); and, it has been a centerpiece of normative debates, particularly in the legal literature, about the confirmation process.

And, yet, however important the Cameron et al. effort, it never explicitly addresses the question of why so many appointments to the Supreme Court are relatively uncontroversial. That task falls to Moraski and Shipan (1999), which I also incorporate in pages to follow. These scholars point to two possible answers: the Senate simply defers to the President or the President, knowing that the Senate must confirm his choice, takes senators' preferences into account when he makes nominations. The first is unlikely in light of the Cameron et al. study and, in fact, it is the second that the data tend to reflect: When the President and the Senate share preferences over the future direction of the judiciary Moraski and Shipan show that the President is relatively free to appoint a nominee of his choice; but when they are distant, as Moraski and Shipan also demonstrate, the President must move toward the Senate if he wants to see his nominee confirmed.

Seen in this way, Moraski and Shipan (1999) is a bridge between the two questions of primary concern here: it speaks to why candidates for judgeships are (un)successful and to how the rules affect choices made by political actors on whom to nominate to the Court; after all, without the constitutional mandate of senatorial ‘advise and consent’, the President would be free to appoint anyone he deemed desirable to the Court. But it is Hall (1987a), the final paper included in this section, that addresses directly the link between rules and decisions eventually made by judges. What she explores is the intersection between the votes jurists cast and the methods used to choose them in the individual U.S. States, most of which elect their judges.⁶

Hall’s findings, though limited to one court, may merely seem to confirm what many analysts had long suspected: that elected judges act in sophisticated fashion, making decisions that echo popular sentiment and not necessarily their own policy or jurisprudential preferences (see, e.g., Gryski, Main and Dixon, 1986; Vines, 1962; Watson and Downing, 1969). On the other hand, the Hall study went some distance toward reinvigorating this area of inquiry, prompting both its author and others to conduct deeper and more rigorous investigations into the effect of selection and retention rules on the choices judges make (e.g., Brace and Hall, 1993, 1997; Croly, 1995; Hall, 1987b, 1992; Hall and Brace, 1992; Pinello, 1995; Tabarrok and Helland, 1999).

Judicial Decision Making

That formal rules work to structure judicial decisions is certainly a promising avenue for future research – especially for work outside the United States, where variation in the mechanisms for the selection and retention of judges abound.⁷ But truth be told most empirical studies of decision making have emphasized other factors that may effect judicial choices – though no one factor more so than judges’ ideology or policy preferences.

Perhaps the emphasis on ideology reflects the genesis of modern-day inquiries into judicial decision making: C. Herman Pritchett’s work on the the justices of the (Roosevelt) U.S. Supreme Court (Pritchett, 1941, 1948). As readers will see, Pritchett asked deceptively simple questions: If judges were merely ‘declaring’ the law rather than making it, why did they so often disagree?⁸ How, in interpreting the same legal provisions, could they consistently reach different conclusions on important questions of law? When Pritchett concluded that the usual explanations – primarily those rooted in the primacy of precedent – could not provide intellectually acceptable responses, he turned to the answer offered by Jerome Frank and the other legal realists (e.g. Rank, 1930; Llewellyn, 1951): judges are ‘motivated by their own preferences’. Pritchett, however, did not see preferences as mere whims but as opinions, often as deeply thought out as they were felt.

Pritchett’s work marked a new beginning, and not an end, to social-scientific inquiry into the legal process. Today, social scientists invoke a range of approaches to the study of judicial decision making – both in the United States and abroad. Some reflect Pritchett’s emphasis on ideology, while others are rather distinct in their theoretical underpinnings.

Among scholars following in Pritchett’s footsteps, few are more prominent than Glendon Schubert. Just as virtually all students of courts and judges have read Pritchett’s *The Roosevelt Court* (1948), many if not all are familiar with Schubert’s (1965) *The Judicial Mind*. Since I cannot include this classic book here, I take the next best step and incorporate a related, classic essay, Schubert (1958). This is a mainstay on graduate syllabi at least in part, I suspect, because

it deepens Pritchett's emphasis on political preferences in ways theoretical, substantive, and methodological. So, for example, while Pritchett (at least to my knowledge) did not write much on how courts set their agendas, Schubert devotes attention to this crucial topic – or what he calls the Certiorari Game. This part of his 1958 paper is a precursor to the more sophisticated analysis conducted by Caldeira, Wright and Zorn (1999), which I include here as well. A multitude of scholars may have written on the subject of agenda setting between Schubert and Caldeira et al.,⁹ but it is Caldeira, Wright and Zorn (1999), I believe, that represents the final word on the subject – at least for now.

Methodologically too Schubert's work advanced Pritchett's project. While Pritchett made use of data in his research (see, e.g., the work I include here, Pritchett, 1941), Schubert went one step further developing innovative methods for analyzing that data. Of course some of those techniques no longer figure prominently in contemporary work but Schubert's willingness to experiment encouraged others to do the same. Of these subsequent efforts, I can imagine no single one more influential than Segal's (1984) 'Predicting Supreme Court Decisions Probabilistically: The Search and Seizure Cases'. While this paper built on ideas in the Schubert (1958) article, as well as in work by Kort (1957) on the role of facts in judicial decisions, it more than pushed them along; it brought to the study of courts and judges a now-ubiquitous class of statistical tools (those enabling researchers to model the outcomes of courts cases), as well.

Pritchett and Schubert – and later Segal (1984) and Segal and Spaeth (2002) – generally confined their work to the U.S. Supreme Court and then to explaining judicial decisions vis-à-vis the political ideologies of judges. Other early students of courts and judges, however, turned to different tribunals and, as I just noted, to different (albeit related) theories of judging. Sheldon Goldman (1966; 1973; 1975), for example, was among the first to study federal intermediate appellate courts.¹⁰ Taking his cues largely from Pritchett, Schubert, and a handful of others, Goldman too explored the extent to which judges' ideology affected their votes but he went further, investigating whether judges' demographic or background characteristics also infiltrate their decisions. As readers will see, he concluded that 'party affiliation was . . . associated with voting behavior' but that 'other demographic variables, such as religion, socio-economic origins, education, and age' were not.

The Goldman (1966) study thus makes a central contribution to the study of judging for any number of reasons but two deserve emphasis here. First, it was among the first to move beyond the Supreme Court. And while I cannot say that its effect was immediate – another three decades or so elapsed before scholars began studying the federal circuit courts in force (see, e.g., Revesz, 1997; Klein and Morrisroe, 1999; Segal, Cameron and Songer, 1995; Merritt, 2001; Sheehan and Mishler, 1992; Songer, Davis and Haire, 1994; Songer, Segal and Cameron, 1994; Spriggs and Wahlbeck, 1995) – it stands as a landmark in the field.

Second, Goldman was one of only a handful of works at the time to contemplate the importance of judges' backgrounds on their decisions.¹¹ Goldman, of course, found little effect but that did not prevent others from continuing the hunt (e.g., Tate, 1981; Tate and Handberg, 1991; Ulmer, 1986; Brudney, Schiavoni and Merritt, 1999). Since I can hardly include all of the many efforts along these lines I selected just two (Giles and Walker, 1975; Sherry, 1986), and for two different reasons at that. One is a famous paper by Giles and Walker (1975),¹² which attempts to 'explain the policy choices made by Southern federal judges in race relations cases' (p. 919) – a subject of considerable interest in the United States. As part of that exercise, the authors

examined a range of social background variables but found only one – whether a judge went to school in the South – to hold any explanatory power.

Giles and Walker's then is not a study that lends much credence to theories emphasizing a judge's personal attributes. But the same cannot be said of Sherry's 'Civic Virtue and the Feminine Voice in Constitutional Adjudication'. Written in 1986, 'Civic Virtue' brought to light a background characteristic that did not – owing to a lack of women on the bench – figure into much earlier work: a judge's sex.

When Sherry suggested that female judges may speak in 'a different voice', she created quite a stir. Scores of scholars undertook follow-up studies (e.g., Segal, 1997; Aliotta, 1995; Davis, Haire and Songer, 1993; Walker and Barrow, 1985; Gruhl, Spolm and Welch, 1981; Allen and Wall, 1993); and even Sandra Day O'Connor, the first woman appointed to the U.S. Supreme Court, chimed in. O'Connor took issue with Sherry's conclusions, as did some subsequent papers (compare, e.g., Gruhl, Spolm, and Welch 1981 and Allen and Wall 1993). But Sherry's work nonetheless stands as a path-breaking, albeit controversial, study if only because it renewed scholarly interest in the analysis of judges' backgrounds and attributes (see, e.g., Ashenfelter, Eisenberg and Schwab, 1995; Brudney, Schiavoni and Merritt, 1999; Schneider, 2001; Sisk, Heise and Morriss, 1998).

No less controversial is yet another approach to judging – and one that has developed a growing following in recent years: strategic analysis (Epstein and Knight, 1998; Gely and Spiller, 1990; Spiller and Gely, 1992; Maltzman, Spriggs and Wahlbeck, 2000; Maltzman and Wahlbeck, 1996; Cross and Tiller, 1998). While shades of this account appear in both Pritchett (1961) and Schubert (1958) it remained for Walter F. Murphy, a student of Pritchett's, to bring it to prominence. Murphy (1962; 1964) begins with the same general premise as did Pritchett and Schubert – justices are single-minded seekers of policy – but he added nuance to that rather stark (and perhaps crude) idea. Specifically, to Murphy, if jurists truly care about etching their policy preferences into law, then they may be willing to modulate their views to avoid an extreme reaction from either their colleagues or external actors. Judges, on this account, would rather hand down a ruling that comes close to, but may not exactly reflect, their preferences than, in the long run, see other actors completely override their decisions.

Unfortunately, I cannot include Murphy's most important work along these lines here: *Elements of Judicial Strategy* is a book, not an essay. What I do instead is incorporate three modern day manifestations of his strategic analysis. One is the Caldeira, Wright and Zorn (1999) essay I mentioned earlier. The second is a classic paper by Richard Posner (1993), which asserts that 'Judges are rational, and they pursue instrumental and consumption goals of the same general kind and in the same general way that private persons do' (p. 39). The third, by political scientists Forrest Maltzman and Paul J. Wahlbeck (1996), runs along similar lines but focuses explicitly on judges as policy maximizers. Following Murphy's (1964) and later Epstein and Knight's (1998) lead, Maltzman and Wahlbeck argue that judges can only achieve their policy goals by attending to the preferences of their colleagues and the actions they expect them to take.

Constraints on Judicial Power

These last three works but especially Maltzman and Wahlbeck (1996) and Caldeira, Wright and Zorn (1999) focus attention on how judges interact with their colleagues. What strategic

approaches also suggest, as I imply above, is that judges who wish to make efficacious decisions must take into account – in addition to the preferences and likely actions of their colleagues – various limitations or constraints imposed by forces external to their tribunals (e.g., Gely and Spiller, 1990; Spiller and Gely, 1992; Vanberg, 1998; Eskridge, 1991b,a; Epstein, Knight and Martin, 2001; Helmke, 291–303).¹³

Those external forces take many different forms: politicians in the states, for example, can refuse to implement judicial decisions, thereby rendering them inefficacious; and ‘the public’ too may play a role in limiting judicial power. But scholars toiling in this area tend to focus on two others: the extent to which higher courts can impose limits on lower courts (the ‘hierarchy of justice’) and the degree to which the checks and balances inherent in systems of shared power can constrain judges (‘separation of powers’).

Beginning with the first, while it seems to be the case that lower court judges are no less interested in etching their values into law than those on higher courts (see, e.g., Sunstein, Schkade and Ellman, 2004), they face a substantial constraint in their quest to do so – the possibility of sanctioning from a higher court. To the extent that supreme courts cannot hire, fire, promote, demote, financially reward, or penalize members of trial or intermediate courts, that sanction can take only one form: reversal. But such is apparently sufficient to restrain judges of lower courts from acting on their sincere preferences – or so argue Cross and Tiller (1998). Their highly influential study, which I include here, asserts that the presence of a ‘whistleblower’ on the court – a judge ‘whose policy preferences differ from the majority’s and who will expose the majority’s manipulation or disregard of the applicable legal doctrine’ to a higher court – can constrain his or her colleagues from behaving in accord with their own preferences.

Hence, in the Cross and Tiller study the hierarchical structure imposes limits on lower courts from tribunals above them (see also, e.g., Segal, Cameron and Songer, 1995; Songer, Segal and Cameron, 1994; Cameron, Segal and Songer, 2000; Sunstein, Schkade and Ellman, 2004). But that same structure can also work the other way, as Murphy’s 1959 seminal paper, which too appears in the page to follow, indicates: Lower-court judges can hamper the commands of higher courts by avoiding, limiting, or even defying them – as many did with the U.S. Supreme Court’s desegregation decisions. Or, as one observer noted in 1941: ‘[Many] precedents have been rejected through the stratagem of distinguishment; others have been the subject of conscious judicial oversight. As a consequence, judicial discretion among inferior judges is not so confined and limited as legal theorists would have it’ (Comment, 1941, 1448–1449). That these words continue to resonate today seems beyond doubt. Indeed, it was only a few short years ago that a lower court (in *Hopwood v. Texas*, 1996) took the dramatic step of defying the U.S. Supreme Court’s landmark decision in the affirmative action case of *Regents of the University of California v. Bakke* (1978).

Seen in this way, the limitation imposed by the hierarchy of justice ‘comes full circle’, as Murphy (1959, 1031) wrote. ‘The Supreme Court must take into account the reaction of inferior judges’, he continued ‘and lower courts must attempt to divine the counter-reaction of the Supreme Court.’ We might say the same of the second major constraint – that imposed by the separation-of-powers system. Although some scholars belie the importance of this limit – Segal and Spaeth (1993), for example, claim that, under certain institutional conditions (e.g., the existence of life tenure, the lack of superiors in the judicial hierarchy, and the dearth of political ambition) judges on high or constitutional courts will be free to ignore the desires of elected actors – most scholars believe otherwise. Just as Murphy (1961) and Pritchett (1962) argued

some thirty years ago, contemporary analysts assert that judges must take into account the preferences of legislators if they are to achieve their goals. That is because legislators and executives can take many steps to punish ‘errant’ courts, thereby making it difficult for them not just to achieve policy goals but to develop or maintain some level of legitimacy, as well. Certainly this is true in the American statutory context in which Congress and the President can overturn judicial interpretations of laws (see the important paper in this volume by Eskridge, 1991a).

But what of constitutional interpretation? While legislatures and executives typically cannot pass legislation to overturn decisions reached by courts on constitutional grounds, they can (and have) used the Senate’s confirmation power to select certain types of judges, enacted constitutional amendments to reverse decisions or change Court structure or procedure, impeached judges, withdrawn Court jurisdiction over certain subjects, altering the selection and removal process, required extraordinary majorities for declarations of unconstitutionality, allowed appeal from the Supreme Court to a more ‘representative’ tribunal, removed the power of judicial review, slashed the budget, and altered the size of the Court.¹⁴

This list of weapons pertains directly to the American context. Even more radical steps have been taken elsewhere, as Helmke (291–303, 292) points out in fascinating insights on the Argentine judiciary:

In many parts of the developing world, judges face threats far greater than simply having their decisions overturned. In such contexts, sanctions range from impeachment, removal, and court-packing to criminal indictment, physical violence, and even death. Compared to American justices, who serve an average of 16.3 years on the bench, in Argentina in the post-Perón period, the average length of tenure has been a mere 5.6 years. Although judges stepped down for a variety of reasons throughout each of the three governments, multiple resignations clustered at the end of both the military and the first democratic government of Alfonsín suggest that incoming governments in Argentina routinely get rid of their predecessors’ judges despite constitutional guarantees.

Helmke proceeds to show, as readers will see, that Argentine judges respond to these potential threats by ‘strategically defecting’, that is, by ruling against the existing regime once it begins to lose power.

The Roles of Courts in Democracies

As the discussion thus far suggests, scholars have spilt no shortage of ink investigating the correlates of judicial decisions and the constraints on the ability of judges to make them – so much ink in fact that they often neglect to ask questions about the impact of judicial decisions or, even more broadly, about the the role(s) courts play in democratic societies.

The three pieces I include in this section – Dahl (1957), Rosenberg (1999), and Franklin and Kosaki (1989) – remind us why these are important, if not critical, questions to ask. For even though they tackle different substantive dimensions of the problem, intriguingly enough they reach the same general conclusion: Courts may not be wholly ineffective organizations within their societies but neither are they as influential as some observers claim.

The first of the trio, Robert Dahl’s ‘Decision Making in a Democracy’, draws attention to Bickel’s (1962) ‘countermajoritarian difficulty’: Given America’s fundamental commitment to a representative form of government, why should its citizens allow a group of unelected

officials – federal judges – to override the wishes of the people, as expressed by their elected officials? Dahl attempts to address this question via the ‘ruling regime’ thesis: Because the political preferences of Supreme Court justices will never be substantially out of line with those of the existing lawmaking majorities, the justices will usually reach decisions consistent with preferences of the elected branches. The primary reason for this, on Dahl’s account, is quite simple: Vacancies occur on the Court on the average once every twenty-two months, giving the president ample opportunity to restaff and reshape the bench. Exceptional circumstances do occur; but, Dahl claims, judges usually cannot – or will not – run counter to the policies of the ruling coalition. They can only fashion specific policies within the general framework of the dominant coalition’s goals or, at considerable risk, make new policy when the alliance has not yet arrived at a consensus.

To be sure, Dahl’s analysis has its fair share of critics. Helmke’s (2002) study of Argentina suggests that it does not fit courts there. And Casper (1976) has argued that Dahl’s conclusion fails to capture accurately certain eras in American history (see also Epstein, Knight and Martin, 2001). But Dahl also has his supporters, of which we might count Gerald Rosenberg as one.

Like Dahl, Rosenberg is interested in assessing the role courts – but especially the American Supreme Court – play in a democratic society. Rather than asking, ‘Are courts majoritarian or counter-majoritarian branches of government’, however, Rosenberg centers his research on what may be the heart of the matter: ‘Are courts effective policy makers?’, ‘Can courts bring about social change?’ His answer, first explicated in the landmark *The Hollow Hope* (1991) and later in the paper I include here, is generally no: courts cannot generate large-scale social change unless the ruling regime supports them. So, for example, Rosenberg claims that *Brown v. Board* (1954) produced little integration in public schools in the South until Congress, at the insistent urging of the President and the broader civil rights movement, had enacted the Civil Rights Act of 1964. This statute put the federal spending power and criminal laws behind desegregation. Then the Voting Rights Act of 1965 utilized federal authority to allow blacks to use their right to vote to retire state and federal officials who wished to continue governmentally imposed racial discrimination. It was these laws, according to Rosenberg, and not *Brown*, which furthered the cause of civil rights.

Rosenberg’s conclusion came as something of a surprise to scholars of the day – many of whom long believed that courts could generate meaningful social change and, in fact, held *Brown*, in particular, as a prime exemplar.¹⁵ But perhaps Rosenberg’s finding should not have been so startling. Long ago, students of the presidency concluded that the White House can seldom bring about lasting changes in public policy.¹⁶ Success typically requires not only strong presidential action, but also new legislation – including, very importantly, appropriation – from Congress, enthusiastic enforcement from administrative agencies, sympathetic treatment from courts, and, not least, the active support of politically skilled interest groups. Public policy is almost always the product of a process, a series of actions and reactions, not of a single decision by a legislative, executive, or judicial institution. And this interactive process starts – or sometimes even stops – with the action of an interest group or one of the formal political institutions.

It is for similar reasons that Franklin and Kosaki’s (1989) findings also fall short of surprising. These researchers too are interested in the role of courts in democratic societies but their emphasis is on public opinion. Specifically, they ask whether judges can act as ‘republican

schoolmasters', conferring legitimacy on the position they favor. The answer, as it turns out, is that they generally cannot. Rather than move the public toward their decisions, Ranklin and Kosaki report that courts have a polarizing effect. So, for example, citizens who were pro-choice prior to the Supreme Court's decision in *Roe v. Wade* (1973), which legalized abortions, grew even more supportive of the right to choose; citizens who were pro-life before *Roe* too became more resolute in their views.

Franklin and Kosaki's study focused on abortion but follow-up research on the death penalty reaches much the same conclusion: both supporters and opponents became even more adamant in their respective views after the U.S. Supreme Court's decision in *Furman v. Georgia* (1972), which held that existing death penalty laws in the United States violated the constitution. In other words, rather than unifying the public *against* capital punishment, the Court's decision further divided Americans (Johnson and Martin, 1998). (Worth noting is that after *Furman* many state legislatures rewrote their laws in an effort to conform to the decision. Four years later, in *Gregg v. Georgia*, the justices upheld most of these new laws but *Gregg* generated virtually no change in public opinion: Americans became no more or less likely to support the death penalty, nor did they grow more polarized.)

At the end of the day, then, the readings in this section combine to show that a judicial ruling, like an executive decision or legislative act, is only one element in a complex battle that often begins in the private sphere, is waged in various political arenas, and will return to those arenas – if, indeed, it ever completely leaves them – after judges have had their say. Furthermore, it is likely that most important issues will also reappear in the courts for fresh decisions, and do so not once but several times before they are finally settled, forgotten, or, what is most likely, superseded by new problems of public policy.

Notes

- 1 Caldeira, Wright and Zorn (1999), which I do include, incorporate interest groups into the analysis but in the context of judicial decision making.
- 2 This is, in fact, a gross generalization. Most empirical work proceeds from theory; and many more than a few scholars move between the empirical and the prescriptive. I think here, to take but one example, of Tiller and Cross (1999), in which the authors analyze voting on the U.S. Court of Appeals for the District of Columbia. Based on that investigation, Tiller and Cross offer a 'modest proposal': eradicating the practice of randomly assigning judges to circuit court panels and instead 'assigning panels with reference to the party of the appointing President' (p. 228).
- 3 This is just one bias that influenced my decisions; I discuss others in the text.
- 4 There are, of course, exceptions – including Frankfurter (1947), Bork (1971), Wechsler (1959) – to cite just a few.
- 5 I adapt this and the next few sentences from Epstein and Knight (2004).
- 6 By the American Judicature Society's tally, in 21 states judges serving on courts of last resort are initially elected to office either on a partisan (8 states) or non-partisan (13) ballot; in all 21 judges must appear on a ballot of one form or another to retain their position. Six states enable their governor (4) or their legislature (2) to appoint their high court judges; and, in many instances to reappoint them. Just 1 of the 6 (New Hampshire) gives its justices life tenure but only until they reach the age of 70. The remaining 23 states employ some version of the 'Missouri' or merit plan. These plans differ from state to state – and substantially differ in some instances – but usually they call for an election after a year or two of service, in which the name of each new judge is put on the ballot with the question whether he or she should be retained in office. If the voters reject an incumbent, another

- 'merit' candidate replaces him or her. If elected, the judge then serves a set term, at the end of which he or she is eligible for reelection.
- 7 While many nations, typically those using the civil law system, have developed similar methods for training and choosing ordinary judges, they depart from one another rather dramatically when it comes to the selection of constitutional court justices. In Germany, for example, justices are selected by Parliament, though six of the 16 must be chosen from among professional judges. In Bulgaria, one-third of the justices are selected by Parliament, one-third by the President, and one-third by judges sitting on other courts. Moreover, in some countries with centralized judicial review, justices serve for a limited period of time. In South Africa, for instance, they hold office for a single 12-year term, in Italy a single nine-year term. In others, including the Czech and Korean Republics, justices serve for a set, albeit renewable, term.
 - 8 Indeed, Pritchett's early work actually derives from a simple empirical observation; namely, that beginning in the 1930s and 1940s dissents accompanied many Supreme Court decisions. For an attempt to explain why dissent rates skyrocketed during this period, see Walker, Epstein and Dixon (1988), also included in this volume. The authors provide a graph illustrating the marked increase that Pritchett observed.
 - 9 A small sample of the extant literature includes Armstrong and Johnson (1982); Baum (1979); Boucher and Segal (1995); Brenner (1979); Brenner and Krol (1989); Caldeira and Wright (1988, 1990); Caldeira, Wright and Zorn (1999); Krol and Brenner (1990); McGuire and Caldeira (1993); Palmer (1982, 2001); Perry (1991); Provine (1980); Schubert (1962, 1959); Smith (1999); Songer (1979); Tanenhaus (1963); Teger and Kosinski (1980); Ulmer (1972); Ulmer, Hintze and Kirklosky (1972); Ulmer (1983, 1984); Lawless and Murray (1997); Estreicher and Sexton (1984); Linzer (1979); Estreicher and Sexton (1986); Berlage (1984); Hartnett (2000); Hellman (1985); Long (1984); Sturley (1989); Tiberi (1993); Baker (1984); Cordray and Cordray (2001).
 - 10 Howard (1977, 1981) too was another important scholar to focus on courts of appeals early on.
 - 11 Other early works include, for example, Nagel (1961); Ulmer (1970, 1973); Tate (1972).
 - 12 The author's name is spelled incorrectly on the paper included here. His name is Micheal Giles.
 - 13 This general idea, as I suggest in the text, too flows from work by Murphy (1962), as well Pritchett (1961), both of whom tell a tale of shrewd judges – judges who anticipate the reactions of *other relevant actors* and take those reactions into account in their decision making.
 - 14 I adopt this list from Rosenberg (1992).
 - 15 Rosenberg's work on the Court, like Dahl's, has its share of critics and supporters. For an example of the latter, see McCann (1994). For recent work also critiquing *Brown*, see Klarman (2004); Ogletree (2004); Bell (2004).
 - 16 I adopt the next few sentences from Murphy et al. (2005).

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Part I
The Selection and Retention of Judges

Part II
Judicial Decision-Making

Part III
Constraints on Judicial Power

Part IV
The Role of Courts in Democracies