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Politics and the Judiciary: The Influence of Judicial Background on Case Outcomes

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ABSTRACT

It is widely believed that the background and worldview of judges influence their decisions. This article uses the fact that judges are assigned their cases randomly to assess the effect of judicial background on the outcome of cases from the day-to-day docket in three federal trial courts. Unlike the political science findings of ideological influence in published opinions, we find little evidence that judges differ in their decisions with respect to the mass of case outcomes. Characteristics of the judges or the political party of the judge's appointing president are not significant predictors of judicial decisions.

"CLINTON May Use Diversity Pledge to Remake Courts," the *New York Times* headline reads.[1] The headline echoes a widely shared view about politics and case outcomes. Since the rise of legal realism, it has been axiomatic that the background and worldview of judges influence cases. Concerns over the impact of conservative judicial ideology intensified in light of efforts by Presidents Reagan and Bush to appoint conservative judges.[2] President Clinton's promise of a more diverse judiciary is seen as

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[1] Stephen Labaton, Clinton May Use Diversity Pledge to Remake Courts, N.Y. Times, March 8, 1993, at A1.

[2] Many observers believe the Reagan administration was more concerned than other administrations about the worldviews of federal judicial appointees. See Edwin Meese Interview: Reagan Seeks Judges with Traditional Approach, U.S. News & World Rep.,

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not just foreshadowing more female and minority appointments but also as promising more liberal adjudication.

Concerns about the power of presidents and their appointees to affect case outcomes predate Presidents Clinton, Bush, and Reagan. An extensive political science literature empirically examines the influence of ideology, using such measures as the judge's political party, appointing president, experience on the bench, and other attributes. Nearly all existing studies of ideological influence, however, are limited to cases in which the court publishes an opinion, and most focus on appellate opinions.

This article likewise assesses the effect of judicial background on the outcome of cases. It employs a data source ignored in the judicial background literature, the day-to-day docket of federal trial courts. The goal is to see whether the political science findings of ideological influence in published opinions apply to the mass of filed cases. In shifting the focus to trial courts, it is critical to consider the possible influence of judges on settlements. After all, the majority of cases filed in the federal courts are not decided by court judgment but rather by settlement of the parties. Scholars have recently begun to examine the increasingly innovative role of trial judges in promoting settlement.[3] This is the first study to look

October 14, 1985, at 67; Note, All the President's Men? A Study of Ronald Reagan's Appointments to the U.S. Court of Appeals, 87 Colum. L. Rev. 766 (1987); Stuart Taylor, The One-Pronged Test for Federal Judges: Reagan Puts Ideology First in Filling Vacancies, N.Y. Times, April 22, 1984, at E5; Stuart Taylor, Study Puts Bork to Right of Other Judges Named by Reagan, N.Y. Times, July 28, 1987, at A15, col. 1; Aric Press, Reagan Justice: The Reshaping of the Courts May Be His Most Enduring Legacy, Newsweek, June 30, 1986, at 4; Bernard Weinraub, Reagan Says He'll Use Vacancies to Discourage Activism, N.Y. Times, October 22, 1985, at A1, A29; Stephen Wermiel, Reagan Choices Alter the Makeup and Views of the Federal Courts, Wall St. J., February 1, 1988, at 1, col. 1; Stephen Wermiel, U.S. Appeals Court Judge Draws Fire for Stance on Civil Rights: Praise for Intellect and Manner, Wall St. J., February 18, 1988, at 46, col. 1; Stephen Wermiel, State Supreme Courts Are Feeling Their Oats about Civil Liberties, Wall St. J., June 15, 1988, at 1, col. 1; Finding the Face That Fits, Economist, March 27, 1993, at 25. But see David Whitman, Reagan's Court Revolution Comes Up Short, U.S. News & World Rep., February 2, 1987, at 27. For the view that President Bush continued the Reagan judicial appointments agenda, see Neil A. Lewis, Bush Picking the Kind of Judges Reagan Favored, N.Y. Times, April 10, 1990, at A1.

[3] See, for example, Wayne D. Brazil, Settling Civil Suits: Litigators' Views about Appropriate Roles and Effective Techniques for Federal Judges (1985); D. Marie Provine, Settlement Strategies for Federal District Judges (Federal Judicial Center 1986); D. Marie Provine & Carroll Seron, Innovation and Reform in Courts: A Cross-Cultural Perspective, 13 Just. Sys. J. 158 (1988–89); James A. Wall, Jr. & Dale E. Rude, Judicial Involvement in Settlement: How Judges and Lawyers View It, 72 Judicature 175 (1988). Some scholars and judges criticize this trend for compromising the rights of litigants; see Howard Bedlin & Paul Nejelski, Unsettling Issues about Settling Civil Litigation: Examining "Doomsday Machines," "Quick Looks" and Other Modest Proposals, 68 Judicature 9 (1984); Owen Fiss, Against Settlement, 93 Yale L. J. 1073 (1984); Laura Macklin, Promoting Settlement, Foregoing the Facts, 14 N.Y.U. Rev. L. & Soc. Change 575 (1986); Judith Resnik, Managerial Judges, 96 Harv. L. Rev. 374 (1982); H. Lee Sarokin, Justice Rushed Is Justice Ruined,

systematically at judges' ideology as a factor influencing whether cases settle.

A major problem haunting studies of judicial decisions in published opinions is selection bias: because published opinions are not a representative sample of all cases, the fact that plaintiffs win, say, half of the published opinions of a certain type of case tells us nothing about the legal system's effect on all cases filed in the system (much less on all disputes out there). To be concrete, suppose we are interested in whether the legal system treats contract plaintiffs more favorably than tort plaintiffs (or medical malpractice plaintiffs more favorably than other tort plaintiffs, or sex discrimination victims more favorably than race discrimination victims). We observe that contract plaintiffs win half the court judgments, while tort plaintiffs win only 30 percent, a statistically significant difference. The problem is that one cannot tell from this whether the court is treating these cases differently. The litigants themselves can decide to settle a case or present it to the court for judgment. In making this decision, the cases for court judgment are unlikely to be a random sample of cases.[4]

To correct for selection bias, the researcher normally must use a more or less elaborate two-stage statistical procedure. For example, one must first estimate the likelihood that a case with particular characteristics will settle and then estimate the likelihood, given that the case has not settled, that the court will rule favorably.

In this article we avoid problems of selection bias in studies of published opinions by taking advantage of the fact that in the federal courts cases are usually assigned randomly ("from the wheel") to judges. Random assignment guarantees that, apart from statistical sampling error, judges receive cases with the same characteristics. Thus, differences in success rates across judges must be due to differences in judicial behavior[5] and cannot be attributed to unseen characteristics of litigated cases.

38 Rutgers L. Rev. 431 (1986), while others defend the practice on its merits or as a necessary response to a perceived litigation explosion. See Steven Flanders, Blind Umpires: A Response to Professor Resnik, 35 Hastings L. J. 505 (1984); Thomas D. Lambros, The Federal Rules of Civil Procedure: A New Adversarial Model for a New Era, 50 U. Pitt. L. Rev. 789 (1989); Andrew W. McThenia & Thomas L. Shaffer, For Reconciliation, 94 Yale L. J. 1660 (1985).

[4] In a well-known model of the selection effect, George Priest and Benjamin Klein suggest a tendency for plaintiffs to win 50 percent of litigated cases, regardless of the legal standard or judges' attitudes toward the cases. See George L. Priest & Benjamin Klein, The Selection of Disputes for Litigation, 13 J. Legal Stud. 1 (1984).

[5] Orley Ashenfelter & Joel Waldfogel, Bargaining in the Shadow of the Judge: Empirical Tests (paper presented at the meeting of the American Association for the Advancement of Science, Boston, February 11–16, 1993); Shari Seidman Diamond, The Assessment of Sentencing Choices through Triangulation: A Response to Walker, 17 Law & Soc. Inquiry 115, 118 (1992).

Section I reviews prior studies of judicial background and discusses the data used in the analysis. Section II explains the statistical technique we use to determine whether individual judges affect the settlement of cases and the success of cases not settled. Section III seeks to explain any individual judge effects in light of the judges' backgrounds. To preview our findings, we find surprisingly little evidence that the identity of the judge hearing a particular case influences the case's outcome. To the extent judges do appear to influence case outcomes, the characteristics of judges, including their political party and the party of their appointing president, provide little help in explaining the interjudge variance in case outcomes. We interpret these results as evidence that the individual judge has more modest influence on the outcome of the mass of cases than on the subset of cases leading to published opinions.

I. Existing Studies of Judicial Background

A. Prior Findings

Prior studies of judicial influence on case outcomes help shape this work. Many appellate-level studies find Democratic judges to be more liberal than Republican judges, tending to vote for the civil rights claimant, the labor union, or the governmental agency in a business regulation case.[6] Some scholars find no clear correlation between political party and

[6] Jilda M. Aliotta, Combining Judges' Attributes and Case Characteristics: An Alternative Approach to Explaining Supreme Court Decisionmaking, 71 Judicature 277, 280 (1988) ("being a Democrat is associated with casting votes in favor of equal protection claims"); Donald R. Songer, The Policy Consequences of Senate Involvement in the Selection of Judges in the United States Courts of Appeals, 35 W. Pol. Q. 107–19 (1982); C. Neal Tate, Personal Attribute Models of the Voting Behavior of U.S. Supreme Court Justices: Liberalism in Civil Liberties and Economics Decisions, 1946–1978, 75 Am. Pol. Sci. Rev. 355 (1981); Sheldon Goldman, Voting Behavior on the United States Courts of Appeals Revisited, 69 Am. Pol. Sci. Rev. 491, 496 (1975); Sheldon Goldman, Voting Behavior on the United States Courts of Appeals, 1961–1964, 60 Am. Pol. Sci. Rev. 374 (1966); Joel B. Grossman, Social Backgrounds and Judicial Decisionmaking, 79 Harv. L. Rev. 1551 (1966); William E. Kovacic, Reagan's Judicial Appointees and Antitrust in the 1990s, 60 Fordham L. Rev. 49 (1991) (Reagan appointees are more conservative than Carter appointees in antitrust cases); John R. Schmidhauser, Stare Decisis, Dissent, and the Background of Justices of the Supreme Court of the United States, 14 U. Toronto L. J. 194 (1962); S. Sidney Ulmer, The Political Party Variable in the Michigan Supreme Court, 11 J. Pub. L. 352 (1962); Stuart S. Nagel, Political Party Affiliation and Judges' Decisions, 55 Am. Pol. Sci. Rev. 843 (1961); Stuart S. Nagel, Unequal Party Representation on the State Supreme Courts, 45 Judicature 62 (1961); Glendon A. Schubert, Quantitative Analysis of Judicial Behavior (1959); Vicki Schultz & Stephen Petterson, Race, Gender, Work, and Choice: An Empirical Study of the Lack of Interest Defense in Title VII Cases Challenging Job Segregation, 59 U. Chi. L. Rev. 1073, 1167–81 (1992) (finding significant correlations between political party and appointing president and outcomes of employment discrimination cases).

judicial decision[7] or find a correlation in some classes of cases but not others.[8] On balance, a pattern emerges of Democratic judges being more liberal than Republican judges.[9]

Evidence also exists of a correlation between the president appointing a judge and case outcomes. A study of the Supreme Court finds significant correlations between the appointing president and liberalism in civil rights cases.[10] A study of federal courts of appeals reports that in voting on

[7] See David W. Adamany, The Party Variable in Judges' Voting: Conceptual Notes and a Case Study, 63 Am. Pol. Sci. Rev. 57 (1969) (a study of the Wisconsin Supreme Court); Theodore Eisenberg & Sheri Lynn Johnson, The Effects of Intent: Do We Know How Legal Standards Work? 76 Cornell L. Rev. 1151, 1190 (1991) (finding no significant relationship between judge's party or appointing president and outcome of race-based equal protection cases).

[8] Goldman, Voting Behavior on the United States Courts of Appeals Revisited, *supra* note 6; Nagel, Political Party Affiliation and Judges' Decisions, *supra* note 6, at 844; Adamany, *supra* note 7; Don Bowen, The Explanation of Judicial Voting Behavior from Sociological Characteristics of Judges (unpublished Ph.D. dissertation, Yale Univ. 1965); J. Woodford Howard, Jr., Courts of Appeals in the Federal Judicial System (1981); Kenneth N. Vines, Federal District Judges and Race Relations Cases in the South, 26 J. Pol. 337–57 (1964); Thomas G. Walker, A Note Concerning Partisan Influences on Trial-Judge Decision Making, 6 Law & Soc'y Rev. 645–49 (May 1972).

[9] Robert A. Carp & C. K. Rowland, Policymaking and Politics in the Federal District Courts 7 (1983). The judge's age, sex, religion, and prior political and judicial experience have been found worthy of study but, in general, show less clear correlations with voting record in most types of cases. See Howard, *supra* note 8; Herbert M. Kritzer & Thomas M. Uhlman, Sisterhood in the Courtroom: Sex of Judge and Defendant in Criminal Case Disposition, 14 Soc. Sci. J. 77, 86 (April 1977); Goldman, Voting Behavior on the United States Courts of Appeals Revisited, *supra* note 6, at 505; Nagel, Political Party Affiliation and Judges' Decisions, *supra* note 6, at 849. In studying southern federal district judges in race cases, Vines, *supra* note 8, reports insignificant correlations between treatment of race cases and (1) place of birth, (2) location of law school, and (3) location of law practice, as well as significant correlations between such treatment and (1) religious affiliation and (2) prior political experience. But see Goldman, Voting Behavior on the United States Courts of Appeals Revisited, *supra* note 6, at 501, 503 (age appears to be the single most important background variable for civil liberties issues and religion the most important for the injured persons category). Tate reports a correlation between Supreme Court justices' treatment of civil liberties cases and (1) extent of judicial experience and (2) type of prosecutorial experience and provides a useful summary of the research (Tate, *supra* note 6). More stable correlations exist between judges' characteristics or identity and their treatment of isolated issues such as criminal sentencing. See Susan Welch, Cassia Spohn, & John Gruhl, Convicting and Sentencing Differences among Black, Hispanic, and White Males in Six Localities, 2 Just. Q. 67–77 (1985); James L. Gibson, Judges' Role Orientations, Attitudes, and Decisions: An Interactive Model, 72 Am. Pol. Sci. Rev. 911 (1978). But see Kritzer & Uhlman, *supra* (female judges behave no differently than their male colleagues in sentencing criminal defendants). An interesting recent approach looks to judges' professional incentives in assessing their behavior. For example, see Mark A. Cohen, The Motives of Judges: Empirical Evidence from Antitrust Sentencing, 12 Int'l Rev. L. & Econ. 13 (1992).

[10] Tate, *supra* note 6. See also Jeffrey A. Segal & Albert D. Cover, Ideological Values and the Votes of U.S. Supreme Court Justices, 83 Am. Pol. Sci. Rev. 557 (1989) (justices' ideological values are reflected in their votes).

four civil rights issues and three economic issues, "Reagan's appointees are not clearly or dramatically more conservative than Nixon's or Ford's appointees, although they *are* clearly more conservative than appointees of the Carter, Kennedy and Johnson administrations."[11] Other characteristics of the judge—such as age, sex, religion, and experience in politics—have a weaker correlation with voting record in most types of cases.[12]

At the district court level, early work failed to find relationships between background characteristics and trial judge decisions.[13] More recent and comprehensive work supports the appellate findings of greater Democratic liberality and suggests that the most important background correlate of judicial judgments is the appointing president. Nixon appointees are more conservative than Kennedy appointees, who in turn are more conservative than Johnson appointees.[14] Reagan's appointees have been

[11] Jon Gottschall, Reagan's Appointments to the U.S. Courts of Appeals: The Continuation of a Judicial Revolution, 70 Judicature 48, 54 (1986); C. K. Rowland, Donald Songer, & Robert A. Carp, Presidential Effects on Criminal Justice Policy in the Lower Federal Courts: The Reagan Judges, 22 Law & Soc'y Rev. 191 (1988). See also Jon Gottschall, Nixon's Appointees to the United States Courts of Appeals: Attitudes, Cohesion and the Influence of Senatorial Courtesy (unpublished manuscript, State University of New York, Plattsburgh, Department of Political Science, n.d.) (study of courts of appeals opinions in criminal cases finds Nixon appointees to be most conservative, non-Nixon Democrats the most liberal, and non-Nixon Republicans in the middle); Jon Gottschall, Nixon's Appointees to the United States Courts of Appeals: The Impact of the Law and Order Issue on the Rights of the Accused (unpublished manuscript, State University of New York, Plattsburgh, Department of Political Science, n.d.).

[12] Aliotta, *supra* note 6; Kritzer & Uhlman, *supra* note 9, at 77, 86; Melinda G. Hall, An Examination of Voting Behavior in the Louisiana Supreme Court, 71 Judicature 40 (1987) (finding little evidence that individual justice's preferences shape voting); Goldman, Voting Behavior on the United States Courts of Appeals Revisited, *supra* note 6, at 505. But see Goldman, *supra*, at 501, 503 (age appears to be single most important background variable for civil liberties issues and religion most important for injured persons category); Eisenberg & Johnson, *supra* note 7, at 1190 (judges with prior prosecutorial experience, judges with prior judicial experience, and older judges all treat racial equal protection cases more favorably than judges lacking such features); S. Sidney Ulmer, Social Backgrounds as an Indicator to the Votes of Supreme Court Justices in Criminal Cases, 1947–1956 Terms, 17 Am. J. Pol. Sci. 622–30 (1973). But see Thomas G. Walker & Deborah J. Barrow, The Diversification of the Federal Bench: Policy and Process Ramifications, 47 J. Pol. 596–617 (1985) (finding few differences between black and white judges; female judges tend to be less supportive of personal rights claims and minority policy positions than male judges; female judges deferred to positions taken by government).

[13] Kenneth M. Dolbeare, Trial Courts in the Urban Politics (1967); C. K. Rowland, Robert A. Carp, & Donald Songer, The Effect of Presidential Appointment, Group Identification and Fact-Law Ambiguity on Lower Federal Judges' Policy Judgments: The Case of the Reagan and Carter Appointees 1 (paper presented at the meeting of the American Political Science Ass'n, New Orleans 1985).

[14] Carp & Rowland, *supra* note 9, at 34–36, 51–83, 150–52; Rowland, Carp, & Songer, *supra* note 13, at 2.

found to be more conservative than any of his predecessors and Bush's more conservative than Reagan's.[15]

B. Existing Studies in Perspective

In assessing work on the influence of judicial background, two views of the effect of the judge's background on decision making are worth separating. The first view, and the one implicitly dominating empirical research to date, posits that judicial background influences "close" or opinion-worthy cases.[16] The issue is the influence of party or other factors in cases at the margin—those in which appellate judges publicly disagree by dissenting or in which the judge deemed a published opinion to be advisable. A finding of statistical significance in such cases suggests less than it first seems to show. In close cases, something must make a difference. It could be random fluctuation, what the judge ate for breakfast, the judge's background, or other less obvious factors. It is not self-evidently disturbing when the judge's worldview (as revealed by party affiliation and other variables) dominates over some competing sources of decision.

An alternate view of the role of judicial background hypothesizes a more drastic and disturbing relationship between background and case outcome. It is that in the routine bulk of litigation or in important subclasses of cases, and not necessarily just in close cases, the judge's background affects the outcome. In examining the bulk of litigation, one must examine cases decided without published opinion.[17]

Most studies examine appellate decisions, and virtually all are limited to published opinions. For at least three reasons, these studies may be seeking legal realism's effect in the wrong place. First, appellate cases may not be the most likely place for judicial discretion. Appellate judges decide cases in panels of three or more. Multiple decision makers may check judicial discretion. If one's fellow judges evaluate the legal arguments a certain way, concern for collegiality and respect pushes one to conform to these views, regardless of whether the result is liberal or conservative, unless the judge can make plausible counter legal arguments. Even in the rarefied

[15] Robert A. Carp, Donald Songer, C. K. Rowland, Ronald Stidham, & Lisa Richey-Tracy, The Voting Behavior of Judges Appointed by President Bush, 76 Judicature 298, 299 (1993).

[16] Indeed, some important studies have limited themselves to published appellate opinions that produce a dissent. For example, see Goldman, Voting Behavior on the United States Courts of Appeals Revisited, *supra* note 6.

[17] To some the likelihood of finding notable judicial influence in the bulk of filed cases is so unlikely as to pretermit inquiry. See the anonymous review of National Science Foundation proposal SES 89-22086 (1988), on file with us.

atmosphere of published opinions it has been suggested that the bulk of decisions are unaffected by politics or personal views.[18]

This does not mean that appellate judges are uninfluenced by the "politics" of the decision or that district judges, who sit alone, are not bound by appellate authority. On many issues, however, district judge discretion governs, and on nearly all issues reversal is the exception, not the rule. In their isolation, district judges may feel less constrained by legal doctrine[19] and thus be more likely to follow political inclinations, precisely because district judges decide alone and often without publishing an opinion. In short, if judges' characteristics at the appellate levels influence their decision making, some believe that such characteristics should be even more influential at the lower court level.[20]

Second, published opinions may not be the best place to search for evidence of judicial discretion. The constraints on judicial decision making are more likely to apply when the judge must articulate reasons for the decision, reasons that can be read and criticized by legal scholars and the practicing bar.[21] Judges themselves decide whether to write and publish an opinion to accompany their decision. A seat-of-the-pants decision, based on the judge's perception of individual justice in the case, is unlikely to be accompanied by a published opinion. And the decision-to-publish filter may make published opinions a nonrepresentative sample of all court judgments.[22]

Third, the judge's political philosophy can affect the result of a case in some areas of the law far more easily than in other areas. Contract cases are thought to be less openly ideological than civil rights cases. Scholars sampling all cases may find the judicial discretion present in some cases masked by the constraints all judges face in other cases.

Because we want to explain judicial influence on the bulk of cases, an additional challenge is to consider cases that settle. Judges influence settlements both directly and indirectly. Some judges directly promote settlement. Indeed, scholars express concern about the heavy-handed methods of judges in coercing settlements in certain kinds of cases.[23] The

[18] Harry T. Edwards, The Judicial Function and the Elusive Goal of Principled Decisionmaking, 1991 Wis. L. Rev. 837, 838.

[19] David L. Shapiro, Federal Habeas Corpus: A Study in Massachusetts, 87 Harv. L. Rev. 321, 340–42 (1973); Finding the Face That Fits, *supra* note 2, at 26.

[20] Stuart S. Nagel, Testing Relations between Judicial Characteristics and Judicial Decision-Making, 15 W. Pol. Q. 425–37 (1962).

[21] *Id.* at 427.

[22] See Theodore Eisenberg & Stewart J. Schwab, What Shapes Perceptions of the Federal Court System? 56 U. Chi. L. Rev. 501–39 (1989).

[23] Fiss, *supra* note 3; Resnik, *supra* note 3.

judges' perception of the purpose of courts (is it primarily to resolve disputes or perhaps to provide a participatory forum for individuals to seek justice?) might influence whether cases settle or go to court judgment. In addition to examining whether judicial characteristics affect win rates, then, we also examine their effect on settlement.

C. *The Data*

The 2,258 cases analyzed here constitute nearly every federal civil rights and prisoner case filed in three federal districts (the Central District of California, the Eastern District of Pennsylvania, and the Northern District of Georgia) in fiscal 1981. These districts include, for 1980–81, 8.1 percent of all federal nonbankruptcy civil filings, 7.9 percent of all nonprisoner civil rights filings, and 4.2 percent of all prisoner civil rights filings.[24] Although the cases were filed in 1980–81, the most contested of them continued for several years, well into the 1980s. The 47 district judges included in the study constituted nearly 10 percent of the 516 full-time district judgeships existing in 1980–81.[25] Because of the ongoing political sensitivity of civil rights cases, these cases are more likely than most to be ones in which judicial background would affect the outcome.[26]

Methodological details of the data gathering are presented elsewhere,[27] but some are worth recounting here. From Administrative Office of the U.S. Courts computer tapes, we obtained a list of the docket numbers of every civil rights case filed in the three districts in fiscal 1981. Law student research assistants then examined the courthouse records, in each case recording, among other variables, the type of civil rights case, whether it set-

[24] Administrative Office of the U.S. Courts, 1981 Annual Report of the Director 369–73 (1981); Eisenberg & Schwab, *supra* note 22, at 506–7. These figures are for a year slightly different than that used in the present study.

[25] Administrative Office of the U.S. Courts, Management Statistics for United States Courts (1981). The actual number of judges sitting on cases should be increased by retired judges who may hear cases and courts of appeals judges assigned to district courts. For purposes of this study, however, the universe of judges should be limited to those for whom case assignments are random. This eliminates many senior judges and some judges in regional branches of district courts. Such judges may not see the same mix of cases as judges in more central locations. In this study, for example, the Eastern District of Pennsylvania case assignments appeared extremely nonrandom until judges in the Eastern District but outside of Philadelphia were excluded.

[26] Gottschall, Reagan's Appointments to the U.S. Courts of Appeals: The Continuation of a Judicial Revolution, *supra* note 11, at 54.

[27] See Theodore Eisenberg & Stewart Schwab, The Reality of Constitutional Tort Litigation, 72 Cornell L. Rev. 641–95 (1987); Stewart J. Schwab & Theodore Eisenberg, Explaining Constitutional Tort Litigation: The Influence of the Attorney Fees Statute and the Government as Defendant, 73 Cornell L. Rev. 719, 721 (1988).

tled, who prevailed if it did not settle, and the judge assigned to the case.[28] Standard biographical dictionaries and other sources supplied the political party, sex, race, religion, law school, and age of each judge, as well as whether the judge had prior judicial or prosecutorial experience.

II. Random Assignment and Judge Effects

Analysis proceeds in two parts. In this section we explore whether judges influence case results. The analysis of this part in turn has two parts. In part *A*, we check whether judges in fact are assigned cases randomly. Then in part *B*, to the extent we are satisfied with the randomness assumption, we assess whether individual judges affect case results. As we will discuss, to the extent the randomness assumption is violated, our methodology remains affected by selection bias. In Section III we then attempt to explain the differences between judges on the basis of their individual characteristics such as political party, age, and experience.

A. Random Assignment of Cases

Since random assignment is a critical assumption in our analysis, we initially test whether cases are randomly assigned to judges in each of the three districts. For each district, we test whether one can reject the hypothesis of similar distributions of cases being assigned to each judge.

In particular, our concern is that, if assignments are not random, some judges might receive cases in which plaintiffs are disproportionately likely to win. We therefore construct case-characteristic variables that measure the difficulty of the case from the plaintiff's perspective. To do this, we assign a "difficulty" dummy variable for each kind of case. Four levels of difficulty exist, based on our knowledge of the law of each area.

Level of Difficulty 1, DIFFICULTY1, applies to prisoner habeas corpus cases and represents the class of cases inherently most difficult to win. A habeas corpus case, by definition, contests a prior judicial finding that has become final. The legal system rarely upsets such findings, particularly when the result would be to release or force retrial of a convicted felon. All studies of habeas corpus litigation confirm that petitioners

[28] In general, federal civil dockets are on an individual judge assignment system. See U.S. Dist. Ct. E.D. Pa., R. Civ. P. 3(b) (effective Aug. 1, 1980); U.S. Dist. Ct. N.D. Ga., Civ. Case Assignment P., at 1 ("Cases will be assigned to a judge as they are received"). Thus, as a general rule, a single judge presides over a case from time of filing to final disposition. When the judge responsible for a case did change, we used the judge in charge of the case at the time the case terminated. In districts with branch offices, all cases from a specified geographical area may be assigned to the district judge associated with that area. See U.S. Dist. Ct. N.D. Ga., Civ. Case Assignment P., at 7.

rarely prevail.[29] A judge receiving a disproportionately large number of habeas corpus cases might be expected to have a relatively low proportion of settled or successful cases.

Level of Difficulty 2, DIFFICULTY2, applies to all prisoner cases other than habeas cases. Most are prisoner civil rights cases, including constitutional tort cases. These cases do not necessarily attack the conviction underlying the prisoner's incarceration. They often involve attacks on prison conditions, rules, or treatment by guards. Thus, they do not necessarily involve attacks on prior final judicial determinations and could fare better than prisoner habeas corpus actions.

Level of Difficulty 3, DIFFICULTY3, covers nonprisoner constitutional tort cases and certain other nonprisoner civil rights actions.[30] Constitutional tort cases are actions brought against state or federal officials alleging constitutional misbehavior and seeking damages or injunctive relief, usually brought under 42 U.S.C. § 1983.[31] Most lawyers would expect these cases to be easier to win than cases brought by prisoners.

Level of Difficulty 4, DIFFICULTY4, applies to civil rights categories in which, as of 1980, plaintiffs could sometimes establish a prima facie case without having to show wrongful intent on the part of the defendant. Many commentators regard a requirement that intentional discrimination be shown as a significant obstacle to successful litigation.[32] Cases in which intent need not be shown are mostly employment discrimination cases brought under Title VII of the Civil Rights Act of 1964.[33] Because our data do not distinguish between cases brought under an impact or

[29] Eisenberg & Schwab, *supra* note 27, at 643 n.3 (collecting studies).

[30] DIFFICULTY3 covers cases brought under § 1983, constitutional tort actions against federal officials, actions alleging conspiracies to violate constitutional rights under 42 U.S.C. § 1985 or § 1986, other civil rights actions brought by nonprisoners, civil rights actions brought by nonprisoners that do not rely on a stated statute, and nonprisoner immigration cases.

[31] Eisenberg & Schwab, *supra* note 27, at 643.

[32] Riordan v. Kempiners, 831 F.2d 690, 697–98 (7th Cir. 1987) (Posner, J.); Kenneth L. Karst, The Costs of Motive-Centered Inquiry, 15 San Diego L. Rev. 1163 (1978). The perceived difficulty of proving intent has led to much criticism of the Supreme Court's decisions requiring a showing of discriminatory intent in equal protection cases. See, for example, David A. Strauss, Discriminatory Intent and the Taming of *Brown,* 56 U. Chi. L. Rev. 935, 937 n.4 (1989).

[33] Other cases assigned DIFFICULTY4 are those brought under the Fair Housing Act, the Equal Pay Act, Title VI of the Civil Rights Act of 1964, cases based on 42 U.S.C. §§ 1981, 1982, Age Discrimination in Employment Act cases (Leftwich v. Harris-Stowe State College, 702 F.2d 686 (8th Cir. 1983) (suggests disparate impact standard)), and Age Discrimination Act cases. The Supreme Court later held that cases based on §§ 1981 & 1982 require proof of intent. See General Building Contractors Ass'n, Inc. v. Pennsylvania, 458 U.S. 375 (1982).

intent theory (at the complaint stage, it is common to allege both), we classify as DIFFICULTY4 all cases that could allege an impact claim.

We further check for randomness by distinguishing within Title VII cases between the two most numerous kinds of employment discrimination actions, actions alleging race discrimination and sex discrimination. The dummy variables RACE and SEX (equaling 1 when plaintiffs raise that claim) will allow us to determine whether judges receive random proportions of race discrimination and sex discrimination cases. Similarly, within section 1983 cases we create the dummy variable POLICE to examine whether judges receive a random assignment of actions against the police. These actions are the most successful large category of section 1983 cases.[34]

The test for randomness is a straightforward comparison of means.[35] For each district, we determine the proportion of the docket comprised of each type of case. In California, for example, DIFFICULTY1 cases constitute 36.2 percent of the docket. We then calculate the proportion of each judge's docket[36] composed of each type of case. For example, DIFFICULTY1 cases constitute 24.5 percent of JUDGE1's docket. An *F*-test reveals whether the individual judge proportions differ significantly from one another for each type of case.

Panel A of Table 1 summarizes the results of this randomness test. It shows for each district and type of case the probability that we would see assignments like this if cases were truly assigned randomly. The first two columns present results for the Eastern District of Pennsylvania, the third for the Northern District of Georgia, and the fourth for the Central District of California. We present two Pennsylvania columns because the assignment system and results in that district highlight the importance of the random assignment assumption to studying the mass of case outcomes and the dangers inherent in studies that do not rely on a random mix of cases.

[34] Schwab & Eisenberg, *supra* note 27, at 735. We have also checked for random assignment for other detailed case characteristics. We find that we cannot reject, using the *F*-test procedure detailed below, the random assignment hypothesis for § 1981 cases, Fair Housing Act cases, and Age Discrimination in Employment Act cases. As the case categories are made finer, the small number of cases would make it more difficult to detect nonrandomness even if it exists. For this reason, we emphasize in the text the broader grouping of cases by difficulty, which generally provides a more meaningful test for nonrandomness.

[35] The lengthy output showing the means for each judge for each kind of case, case outcome, and district is available from us.

[36] The number of judge dummy variables is less than the total number of judges hearing cases in the three districts during the period covered. This is because the study excludes cases heard by senior judges, for whom the random assignment assumption may not hold, and cases heard by judges who do not sit regularly in the district.

TABLE 1

F-TEST PROBABILITIES OF RANDOM ASSIGNMENT AND JUDICIAL INFLUENCE, TESTING EACH GROUP OF JUDGES LOCALLY
(Number of Judges Is in Parentheses)

	EASTERN DISTRICT OF PENNSYLVANIA		NORTHERN DISTRICT OF GEORGIA	CENTRAL DISTRICT OF CALIFORNIA
	Nonrandom (21)	Random (18)	(12)	(17)
A. Prejudge case characteristics:				
DIFFICULTY1	.056	.212	.134	.621
DIFFICULTY2	.047	.466	.339	.288
DIFFICULTY3	.497	.483	.915	.589
DIFFICULTY4	.032	.032	.085	.740
Combined difficulty probabilities	.014	.039	.318	.178
RACE case	.137	.105	.287	.468
SEX case	.526	.687	.839	.014
POLICE case	.686	.586	.037	.943
B. Postjudge outcomes:				
SETTLE	.055	.222	.324	.576
WIN	.452	.802	.037	.722
WIN OR SETTLE	.018	.107	.120	.429
COUNSEL	.000	.000	.051	.648
DISCOVERY EVENT OCCURRED	.015	.006	.000	.326
REFER CASE TO MAGISTRATE	.000	.000	.790	.000
C. Test judge coefficients = 0:				
SETTLE	.134	.470	.590	.820*
WIN	.495*	.848*	.040*	.797*
WIN OR SETTLE	.050	.306	.159	.793*
COUNSEL	.001	.003	.000	.727*
DISCOVERY EVENT OCCURRED	.227	.136	.000*	.356*
REFER CASE TO MAGISTRATE	.000*	.000*	.476*	.000*

SOURCES.—Civil rights and prisoner cases filed in E.D. Pa., N.D. Ga., and C.D. Ca., during fiscal 1980–81.

* Ordinary least squares regression has been used; otherwise, logistic regression has been used for panel C.

For the Northern District of Georgia and the Central District of California, we cannot reject the hypothesis of random assignment in any category except for California DIFFICULTY4 cases that are based on sex discrimination and Georgia police cases.[37] Considering the four difficulty levels simultaneously, the combined probability of random assignment is

[37] The only borderline result is the assignment of DIFFICULTY4 cases in Georgia, where the differences between judges are significant at the .085 level. Of course, one must remember that as one repeats tests of significance over many samples, one expects at, say, the .05 level to see 1 sample in 20 to be significant even if assignments are truly random.

given in the fifth numerical line in Table 1. For both districts the random assignment assumption holds up well across difficulty levels.

For Pennsylvania, consider first Table 1's "Nonrandom" column. It suggests, for DIFFICULTY levels 1, 2, and 4, rejection of the random assignment hypothesis. Three of the 21 active judges in our Pennsylvania sample were stationed outside of Philadelphia and, under local assignment rules,[38] were not part of the overall random assignment pool. The "Random" Pennsylvania column in Table 1 shows the effect of removing these three judges from our sample. Without the three non-Philadelphia judges, the assignments of prisoner habeas and civil rights cases appear random, but the assignment of DIFFICULTY4 cases (largely nonprisoner employment discrimination cases) still suggests rejecting the random assignment hypothesis. There is no apparent reason for this departure.[39] It appears that some Philadelphia judges receive significantly more DIFFICULTY4 cases (largely employment discrimination cases) than other judges do. As suggested by the insignificant judge differences in Pennsylvania RACE, SEX, and POLICE cases, however, within the broader categories judicial assignment of cases does appear random.

B. Judge Effects on Case Outcomes

Having concluded (with the caveats noted above) that judges are randomly assigned the cases in our sample, we then test whether judges affect the outcome of cases. Given random assignment, we simply compare means by judge. We need not control for characteristics of cases coming before judges. If our sample is large enough, we can attribute all systematic difference in outcomes to differences in the judge, because other influences should wash out with the random assignment.

We examine the effect of judges on six measures of case outcome. First is whether a case settles. The variable SETTLE equals 1 when the case settles and 0 otherwise. Second is whether a plaintiff wins a court judgment. The variable WIN equals 1 when that occurs, 0 otherwise.[40] Third, we combine SETTLE and WIN by asking whether a plaintiff re-

[38] E.D. Pa. R. 3(b). The local rules have recently been changed to give districtwide assignments for prisoner civil rights and habeas cases (telephone interview with Michael E. Kunz, Clerk of Court for the Eastern District of Pennsylvania (June 2, 1993)).

[39] The Clerk of Court stated that all active judges in the Eastern District of Pennsylvania are on the same "wheel" for civil rights cases. He suggested that cases consolidated after filing might explain a deviation from randomness, at least over a short period such as a year (telephone interview with Michael E. Kunz, Clerk of Court for the Eastern District of Pennsylvania (June 2, 1993)).

[40] The variable WIN is not limited to tried cases. Thus, WIN is coded as 0 for pretrial dismissals.

ceives either a settlement or a favorable court judgment. The variable WIN OR SETTLE equals the sum of WIN and SETTLE. Viewed another way, when WIN OR SETTLE is 0, the case was dismissed by the judge on pretrial motion or lost at trial. The motivation for combining these two variables is to get an overall measure of successful cases. By not characterizing settlements as victories one would ignore most of plaintiffs' true victories.[41]

The final three variables evaluate procedural aspects of the case that the judge can influence, which in turn could affect outcomes. First is whether the judge refers the case to a magistrate for a substantive decision (REFER CASE TO MAGISTRATE). While referral to a magistrate does not necessarily affect the final outcome of the case, a suspicion is that judges hostile to civil rights litigants will simply dismiss the case without a referral. Second is whether the plaintiff was represented by a lawyer. Clearly, the plaintiff has substantial control over the existence of counsel. But the judge to whom the case is assigned likewise influences whether counsel is present, particularly in prisoner cases. The judge can authorize assigned counsel or insist on representation by counsel. This decision, in turn, should affect case outcomes. Uncounseled litigants rarely prevail. Third, the judge can influence the outcome of the case by allowing liberal discovery. We account for crude differences in discovery by coding a discovery variable as 1 in cases in which any discovery event occurred.

The procedure for determining judicial influence is again a straightforward comparison of means.[42] Panel B of Table 1 presents the summary of the *F*-tests.

Comparing the two Pennsylvania columns in panel B shows the critical importance of randomization. The "Nonrandom" Pennsylvania column shows significant or near significant judge effects for both SETTLE and WIN OR SETTLE. These effects fade when the sample is limited to judges who are part of a true random assignment process, as revealed by the "Random" Pennsylvania column. Thus, one studying Eastern District of Pennsylvania judges, but failing to account for nonrandom assignment, might conclude that judges treat cases differently, when most of the difference is explained by the different pattern of cases that the judges are assigned.

[41] See Schwab & Eisenberg, *supra* note 27, at 726–27. The difficulty with this approach is that we cannot look behind the settlements to test which are "truly" successful as measured by size of recovery and/or dollars and time invested in securing the recovery.

[42] The sample of cases for case outcomes differs slightly from the sample of cases used to assess case characteristics before the judge acts. We have excluded from the case outcome sample cases with unclear dispositions, such as those that were pending at the conclusion of the study or were transferred to another district. This removed 121 cases from the sample.

If we limit Pennsylvania to those judges who are part of a true random assignment system, none of the three districts shows significant judges' effects on SETTLE or WIN OR SETTLE. In Georgia, there are significant judge effects on WIN. In California and Pennsylvania, judges significantly vary in how often they refer a case to a magistrate. In Georgia and Pennsylvania, judges appear to vary significantly or nearly significantly in ensuring that plaintiffs are represented by counsel and in the rate of discovery.

As a check on our simple test of comparing mean settlement rates, win rates, discovery rates, and so forth, by judge, we also ran multiple regressions that simultaneously controlled for case characteristics and the judge.[43] If our "random in, all effects out are judge effects" procedure is correct, the conclusions from a multiple regression procedure should not vary much from our initial conclusions, as long as random assignment prevails. For panel C of Table 1, we regress our six measures of case outcome as a function of case difficulty dummy variables, RACE, SEX, POLICE, and the judge dummy variables. We then test the hypothesis that the judge dummy variables in each equation are jointly 0. The probability of their jointly being 0 is reported in panel C. In the random assignment columns, the conclusions almost uniformly are the same as in panel B, which used a simple means test. Every nonsignificant finding in panel B remains nonsignificant in panel C, and with one exception every significant or nearly significant finding in panel B remains significant in panel C. The one exception is whether Philadelphia judges significantly influence discovery, which appears to be so under the means test of panel B but appears not to be so under the logistic regression approach of panel C. By contrast, when the randomness assumption is violated, the means test and multiple regressions often support different conclusions. Looking at the first column (nonrandom assignment in the Eastern District of Pennsylvania), conclusions differ for WIN OR SETTLE and DISCOVERY and are quite different for SETTLE as well.

In sum, judges do influence procedural aspects of a case, such as whether the plaintiff will have (perhaps assigned) counsel, whether discovery will occur, and whether the judge will refer the case to a magistrate. As is plausible, these influences vary by judicial district. On the more substantive decisions, however, we generally cannot reject the hy-

[43] As explained earlier, a single regression is not the ideal way to control for case characteristics because it ignores the selection effect that litigants have on which cases reach the judge. A preferred but complex procedure would be a series of bivariate probit regressions. For further details on this procedure, see Theodore Eisenberg & Stewart Schwab, The Influence of Judges on Settlement and Trials in Civil Rights Cases: A Bivariate Probit Approach (mimeographed, Cornell Law School 1990).

pothesis that the identity of a particular judge assigned the case has no influence on whether a case settles or whether the plaintiff wins.

III. Explaining Judge Effects

Section II shows that judges significantly affect case procedures and have more minor effects on case outcomes. These interjudge differences provide a quantitative measure of the influence of the judge on the particular outcome or procedure. This section seeks to explain these differences. We use two different sets of regression analyses. For each method, we limit the Pennsylvania part of our sample to the Pennsylvania judges who are part of the fully random assignment process.

In the first regression analysis, the proportion of each judge's docket that settled, won, won-or-settled, had counsel, had a magistrate, or had a discovery event is the dependent variable. We use biographical and political party data about the judges as independent variables. Our technique is weighted least squares regression analysis, with the weights being determined by the number of cases on each judge's docket.

In the second regression analysis, we use a dependent variable that more precisely accounts for variation in case assignment patterns. Rather than use the simple proportions as dependent variables, we use the individual judge dummy variable coefficients from the regressions used to generate panel C of Table 1. For the same six dependent variables as in the first method, the quantitative measure of the judge's influences becomes a regression coefficient and not a simple mean.

A word about methodology is in order. The two-step process used here—first measuring judge effects and then seeking to explain them—has advantages over alternative methods. A direct regression of the case outcome variables (for example, WIN OR SETTLE) against judicial characteristics ignores the fact that the judges decide more than one case and would have to be estimated by a procedure that accounted for the multiple observations if correct tests of statistical significance were to be performed. Although the data include about 2,300 cases, they cover only 47 judges.

A. Judge Characteristic Variables

Characteristics used by prior scholars examine the effect of judicial background on court decisions. These variables include the judge's party (REPUBLICAN PARTY); the party of the appointing president (REPUBLICAN PRESIDENT); age (JUDGEAGE); experience (JUDGE EXPERIENCE); religion; and prior background as judge (PRIOR JUDGE), prosecutor (PRIOR PROSECUTOR), or elected office holder (ELECTED). Theoretical justification for including each of these charac-

teristics can be found in the substantial political science literature on the subject, so we discuss them here only briefly.

The REPUBLICAN PARTY and REPUBLICAN PRESIDENT variables test the popular and scholarly notion that Republican judges are less favorable to civil rights plaintiffs than Democratic judges and that Republican presidents appoint judges with this Republican bias. The same perception suggests testing separately who appointed the judge. If there is a distinct presidential philosophy at work even within the same party (Kennedy differed from Carter) the individual presidents are as important to test as are the political party variables. When testing for the effect of individual presidents, the appointing president replaces the REPUBLICAN PARTY and REPUBLICAN PRESIDENT variables. In such cases, the presidential dummy variables NIXON, JOHNSON, CARTER, FORD, KENNEDY, and REAGAN are used. CARTER is omitted in the regressions that follow as the reference variable.

Judges' age and prior job background have been explored in the political science literature, and we replicate study of these influences here. If one believes society in general is becoming more conservative, the Kennedy appointee in the early 1960s might differ from the Ford appointee of the late 1970s less because they are from different parties than because they are from different eras. The variable JUDGEAGE is intended to capture this difference. The variable JUDGE EXPERIENCE measures years on the federal bench. Some judges may become jaded to certain kinds of claims (especially high-volume, unsuccessful prisoner claims) over time. Prior prosecutors might be thought especially unlikely to be sympathetic to prisoner civil rights claims. Judges who held prior elected office might be more deferential to the other branches of government often being challenged in civil rights cases. Federal judges with prior state judicial experience might be expected to adapt more quickly to the federal bench than federal appointees with no prior judicial experience. The variable PRIOR JUDGE captures whether the federal judge has prior judicial experience. The variable ELITE SCHOOL accounts for whether the judge graduated from an elite law school.[44]

Dummy variables, JEWISH, CATHOLIC, and PROTESTANT, code the judges' religions. The historical association of Jewish people with liberal causes may suggest that Jewish judges would be more sympathetic to civil rights claims. In the regressions that follow, judges with unknown religions form the reference category.

[44] The elite schools in this study are Harvard, Yale, Stanford, and Chicago. Many strong schools had no graduates in the sample.

To account for senatorial influence on federal court appointments,[45] a variable, SENATEADA, reflects the liberality of the senators in the judge's state at the time of appointment. It is the sum of the two senators' Americans for Democratic Action scores for the year in which the judge was appointed.[46]

The variables SEXJUDGE and MINJUDG code whether a judge is female and whether a judge is a member of a minority group. Popular wisdom might be that such judges are more sympathetic to civil rights claimants than other judges.[47] Women and minority judges have career and personal experiences that are not the same as men's.[48] Like other studies,[49] the sample contains so few minority and female judges that findings with respect to these variables should only be viewed as guides to the behavior of judges in the sample rather than to any larger class of minority and female judges.

Table 2 shows summary statistics for the judge characteristic variables used.

B. Explaining Outcomes Using Judge Characteristics

Panel A of Table 3 shows the regression results for models in which the judge means with respect to SETTLE, WIN, WIN OR SETTLE, COUNSEL, DISCOVERY EVENT, and REFER CASE TO MAGISTRATE are sought to be explained as a function of the judges' characteristics. Panel B of Table 3 shows the results for models in which the coeffi-

[45] See, for example, Rayman L. Solomon, The Politics of Appointment and the Federal Courts' Role in Regulating America: U.S. Courts of Appeals Judgeships from T.R. to F.D.R., 1984 Am. B. Found. Res. J. 285–343; Finding the Face That Fits, *supra* note 2, at 26.

[46] There are many other possible ways to take into account senatorial influence. We have tried several other variables and combinations of variables to monitor senatorial influence of federal judicial appointments. These include whether the President and one or both senators from a state are of the same political party. None of the other variables yields significant results. Another possibility would be to take into account the liberality of the Senate Judiciary Committee or its Chairman. For studies or descriptions of the shifting criteria for judicial appointment, see, for example, Bradley C. Canon, The Impact of Formal Selection Processes on the Characteristics of Judges—Reconsidered, 6 Law & Soc'y Rev. 579–93 (1972); Sheldon Goldman, Reagan's Judicial Legacy: Completing the Puzzle and Summing Up, 72 Judicature 318–27 (1989); Gottschall, Reagan's Appointments to the U.S. Courts of Appeals, *supra* note 11; Elliot E. Slotnick, The U.S. Circuit Judge Nominating Commission, 1 Law & Pol'y Q. 465–96 (1979).

[47] See Elaine Martin, Men and Women on the Bench: Vive La Difference? 73 Judicature 204 (1989–90).

[48] *Id.* (women judges).

[49] Walker & Barrow, The Diversification of the Federal Branch, *supra* note 12; Jon Gottschall, Carter's Judicial Appointments: The Influence of Affirmative Action and Merit Selection on Voting on the U.S. Courts of Appeals, 67 Judicature 165 (1983).

TABLE 2

SUMMARY OF JUDGE CHARACTERISTICS

Variable	Mean	SD	Min	Max
PRIOR JUDGE	.45	.50	0	1
PRIOR PROSECUTOR	.40	.50	0	1
ELECTED	.11	.31	0	1
JUDGEAGE	57.4	8.3	38.8	74.7
JUDGE EXPERIENCE	8.2	5.9	.2	20.8
SENATEADA	88.6	47.2	8	187
SEXJUDGE	.11	.31	0	1
MINJUDGE	.09	.28	0	1
JEWISH	.06	.25	0	1
CATHOLIC	.17	.38	0	1
PROTESTANT	.28	.45	0	1
REPUBLICAN PARTY	.57	.54	0	2*
REPUBLICAN PRESIDENT	.49	.51	0	1
ELITE SCHOOL	.26	.44	0	1
PA	.38	.49	0	1
GA	.26	.44	0	1
CA	.36	.49	0	1

SOURCE.—Various biographical references.
NOTE.—The number of judges = 47.
* One judge's party is missing.

cients for judge dummy variables with respect to SETTLE, WIN, WIN OR SETTLE, COUNSEL, DISCOVERY EVENT, and REFER CASE TO MAGISTRATE are to be explained as a function of the judges' characteristics. The coefficients for the individual judge dummy variables are obtained from preliminary regressions in which the dependent variable is modeled as a function of the individual judges as well as the prejudge case characteristics (combined level of difficulty, RACE case, SEX case, POLICE case) reported in Table 1. Table 3 suggests the individual judge characteristics explain very little of the variation in case outcomes.

For SETTLE, WIN, WIN OR SETTLE, COUNSEL, and DISCOVERY EVENT, one cannot reject the hypothesis of zero coefficients for all or nearly all independent variables. Given the modest judge effects reported in Table 1, this is not surprising. There may be nothing to explain. Of particular interest may be the inability to reject the null hypothesis for REPUBLICAN PARTY and REPUBLICAN PRESIDENT, even when limiting the regression analyses to more parsimonious sets of independent variables (not reported here). Indeed, REPUBLICAN PRESIDENT is in a direction opposite to what most would expect.[50] Cases before judges ap-

[50] One possible concern is whether the variables REPUBLICAN PARTY and REPUBLICAN PRESIDENT are masking each other's effect. We find no evidence of this because each remains insignificant when the other is deleted.

pointed by Republican presidents are more likely to have settled and won or settled than cases before judges appointed by Democratic presidents, although the result is not statistically significant.

For REFER CASE TO MAGISTRATE, some effects do emerge, as one might expect from the much stronger judge effects reported in Table 1. Judges appointed under more liberal senators are more likely to refer cases to magistrates than judges appointed under more conservative senators. Catholic judges are less likely to do so than judges whose religions are not known, but the effect is not significant. The political party variables, REPUBLICAN PARTY and REPUBLICAN PRESIDENT, again do not allow rejection of the null hypothesis.

These findings do not change substantially when one replaces the political party variables (REPUBLICAN PRESIDENT and REPUBLICAN PARTY) with dummy variables representing the appointing presidents, with Carter as the reference category. Table 4 reports these results for the model based on the judge means only (corresponding to panel A in Table 3). The Catholic variable in the REFER CASE TO MAGISTRATE equation is now significant.

The president SETTLE and WIN OR SETTLE equations reveal no significant correlation between the appointing president and case outcomes. In both equations, one cannot reject the hypothesis of zero coefficients for all the presidential variables. Only Carter (the reference category), with 15 appointees, Nixon (with 17), and Johnson (with 11) have enough appointees in the sample to support any inferences.[51] There is little evidence confirming the popular notion that Nixon appointed many more conservative judges than Johnson or Carter. In the SETTLE and WIN OR SETTLE equations, the NIXON coefficient is a smaller negative number than the JOHNSON coefficient and is not noticeably different than the CARTER reference category.

IV. Conclusion

Our task is to detect and explain whether the outcome and procedures in the mass of civil rights cases filed in federal district court are affected by the judge to whom the case is assigned. Normally, explaining case outcomes by their characteristics is made difficult because of selection bias. But here, we exploit the fact that judges are assigned randomly to

[51] Reagan appointed two, Ford three, and Kennedy four of the judges in the sample. The total district court appointees in the careers of the presidents covered here are as follows: Reagan 224 (as of 1987), Carter 202, Ford 52, Nixon 179, and Johnson 122. See Sheldon Goldman, Reagan's Second Term Judicial Appointments: The Battle at Midway, 70 Judicature 324, 328 (1987).

TABLE 3

RELATIONSHIP BETWEEN OUTCOME AND JUDGE CHARACTERISTICS
(Columns Show Regression Coefficients)

	DEPENDENT VARIABLES					
COEFFICIENTS FOR JUDGE CHARACTERISTICS	SETTLE	WIN	WIN OR SETTLE	COUNSEL	DISCOVERY EVENT	REFER CASE TO MAGISTRATE
A. Using judge means:[a]						
PRIOR JUDGE	−.007	−.007	−.009	−.019	−.046	.004
PRIOR PROSECUTOR	−.002	.010	.002	−.018	−.002	.041
ELECTED	−.045	.010	−.034	−.047	−.082	−.020
JUDGEAGE	.001	.001	.002	.001	.000	.002
JUDGE EXPERIENCE	−.003	−.002	−.005	−.003	−.003	−.007*
SENATEADA	.000	−.000	.000	.000	.001	.001**
SEXJUDGE	.010	.010	.025	.070	.011	.055
MINJUDGE	−.009	−.023	−.049	−.056	.049	−.031
JEWISH	.089	.003	.107	−.089	−.143	.060
CATHOLIC	−.020	−.003	−.007	−.064	−.060	−.086
PROTESTANT	.011	.003	.020	−.007	−.026	.007
REPUBLICAN PARTY	−.039	−.010	−.058	−.084	−.040	.007
REPUBLICAN PRESIDENT	.028	.009	.045	.079	.023	.041
PA	.010	.017	.022	.070	.168	−.250***
GA	−.015	.008	−.006	−.009	.050	−.159**
ELITE	.101	.003	.020	.031	.002**	.056
Constant	.173	−.001	.171	.438*	.278	.065
F-test	.492	.532	.485	.272	.204	.000***
Adjusted R^2	−.004	−.020	−.001	.091	.128	.638

B. Using judge dummy variables:[b]						
PRIOR JUDGE	−.009	−.005	−.013	−.021	−.046	.022
PRIOR PROSECUTOR	−.011	.002	−.008	−.023	−.016	.047
ELECTED	−.038	.002	−.030	−.002	−.061	−.033
JUDGEAGE	.001	.001	.002	.000	−.001	.002
JUDGE EXPERIENCE	−.003	−.002	−.005	−.001	−.003	−.006
SENATEADA	.000	.000	.000	.000	.000	.001**
SEXJUDGE	.000	.014	.017	.032	−.009	.083
MINJUDGE	−.007	−.030*	−.044	−.065	.053	−.040
JEWISH	.104	.009	.116	−.100	−.087	.052
CATHOLIC	.000	.000	.009	−.065	−.017	−.079
PROTESTANT	.006	.006	.013	−.003	−.020	.017
REPUBLICAN PARTY	−.038	−.015	−.055	−.070	−.036	.008
REPUBLICAN PRESIDENT	.037	.013	.052	.074	.039	.018
PA	.024	.008	.041	.141**	.171***	−.273***
GA	.017	.013	.039	.079	.073	−.175**
ELITE	.006	.000	.011	−.001	.011	.039
Constant	.316*	.020	.326*	.591**	.456*	.149
F-test	.587	.288	.440	.191	.135	.000***
Adjusted R^2	−.041	.083	.017	.140	.173	.747

SOURCES.—Civil rights and prisoner cases filed in E.D. Pa., N.D. Ga., and C.D. Ca., during fiscal 1980–81; various biographical references.

NOTE.—Number of observations = 46.

[a] For example, if 25 percent of JUDGE1's cases settled, then the value of the dependent variable SETTLE for JUDGE1 is 0.25. The dependent variable values are similarly constructed for each judge and each dependent variable.

[b] Obtained from preliminary regressions in which the dependent variable is modeled as a function of the individual judges as well as the prejudge case characteristics reported in Table 1.

* Significant at the .05 level.

** Significant at the .01 level.

*** Significant at the .001 level.

TABLE 4

RELATIONSHIP BETWEEN OUTCOME AND JUDGE CHARACTERISTICS

	DEPENDENT VARIABLES					
COEFFICIENTS FOR JUDGE CHARACTERISTICS	SETTLE	WIN	WIN OR SETTLE	COUNSEL	DISCOVERY EVENT	REFER CASE TO MAGISTRATE
PRIOR JUDGE	−.022	−.011	−.032	−.052	−.079*	.020
PRIOR PROSECUTOR	−.009	.011	−.002	−.013	−.002	.029
ELECTED	−.024	.012	−.020	−.042	−.083	.011
SENATEADA	.000	.000	.000	.001	.000	.001*
SEXJUDGE	−.010	.006	−.000	.042	−.038	.041
MINJUDGE	.012	−.019	−.017	−.013	.092	−.045
JEWISH	.090	−.002	.095	−.109	−.137	.099
CATHOLIC	−.029	−.006	−.017	−.079	−.059	−.129*
PROTESTANT	.030	.012	.052	.035	.009	.002
KENNEDY	.055	−.009	.016	.010	−.014	.131
JOHNSON	−.045	−.011	−.053	−.047	−.054	−.109**
NIXON	−.025	−.014	−.045	−.047	−.073	.018
FORD	−.028	.000	−.026	.002	−.071	.014
REAGAN	.145	.054	.200*	.217*	.350	−.022
PA	−.005	.015	.009	.065	.157***	−.308***
GA	−.059	.003	−.046	−.038	.012	−.237***
ELITE	.004	.002	.011	.018	−.027	.066
Constant	.265	.029	.285***	.480	.313***	.212***
F-test	.250	.526	.280	.135	.009**	.000***
Adjusted R^2	.108	−.017	.092	.100	.392	.759

SOURCES.—Civil rights and prisoner cases filed in E.D. Pa., N.D. Ga., and C.D. Ca., during fiscal 1980–81; various biographical references.
NOTE.—Number of observations = 47.
* Significant at the .05 level.
** Significant at the .01 level.
*** Significant at the .001 level.

cases. This allows us to use a simple and intuitive statistical procedure—comparing means between judges—to assess the effect of the judge. The simplicity is in itself a great advantage in the approach. And the set of Pennsylvania results in Table 1 shows the critical effect of nonrandom assignment on studies of judicial influence.

We find that judges influence the procedures within civil rights cases but have relatively little effect on whether cases settle or win. Further, judicial characteristics such as political party cannot explain what few effects we see.

Many will be surprised that we cannot find that Republican judges differ from Democratic judges in their treatment of civil rights cases. The religion and gender of the judge had larger but still modest effects. One can always question the data or model, and we are careful not to accept our null hypothesis that there are no differences. But their failure to emerge in a reasonably sized fraction of all civil rights filings in a year (about 8 percent of the national total for nonprisoner cases) is evidence that individual judge characteristics cannot be assumed to influence substantially the mass of cases.

Our initially surprising results can be reconciled with prior findings. In the mass of cases that are filed, even civil rights and prisoner cases, the law—not the judge—dominates the outcomes. Judges may treat most cases as ones in which political interests are irrelevant or cannot change the outcome. In the select few cases that are appealed or lead to published opinions, individual judges have a greater role in shaping outcomes. In such close cases, this may not be disturbing. What should shape the outcome of indeterminate cases? Of course, close cases often make policy, both for the courts and for the society at large. Our findings neither undermine that received wisdom nor suggest the unimportance of careful judicial selection. But the political aspects of that selection process may not filter down to the mass of litigation.[52]

The study also suggests a difficulty presidents face in trying to "stack" the federal judiciary. We could not explain the admittedly small differences among judges on the basis of standard biographical characteristics. Whether more detailed knowledge of individual candidates could do so is unknown.

[52] There is a way to test whether trial court judge influence increases in close cases. One could study whether judicial influence emerges more strongly in cases that reach trial, a rough indicator of a case being "close." In our data, so few cases actually reach trial before each judge that the effect, if any, might not emerge. At roughly 50 cases per judge and an approximate 10 percent trial rate, our sample could produce only about five trials per judge. A realistic test of observing such a "close-case" effect may require a larger sample of tried cases.