

University of Chicago Law School

## Chicago Unbound

---

Coase-Sandor Working Paper Series in Law and Economics Coase-Sandor Institute for Law and Economics

---

2008

### Rational Judicial Behavior: A Statistical Study

Richard A. Posner

Richard.Posner@chicagounbound.edu

William M. Landes

William.Landes@chicagounbound.edu

Follow this and additional works at: [https://chicagounbound.uchicago.edu/law\\_and\\_economics](https://chicagounbound.uchicago.edu/law_and_economics)



Part of the **Law Commons**

---

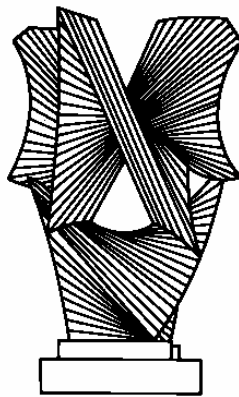
#### Recommended Citation

Richard A. Posner & William M. Landes, "Rational Judicial Behavior: A Statistical Study" (John M. Olin Program in Law and Economics Working Paper No. 404, 2008).

This Working Paper is brought to you for free and open access by the Coase-Sandor Institute for Law and Economics at Chicago Unbound. It has been accepted for inclusion in Coase-Sandor Working Paper Series in Law and Economics by an authorized administrator of Chicago Unbound. For more information, please contact [unbound@law.uchicago.edu](mailto:unbound@law.uchicago.edu).

# CHICAGO

JOHN M. OLIN LAW & ECONOMICS WORKING PAPER NO. 404  
(2D SERIES)



## Rational Judicial Behavior: A Statistical Study

*William M. Landes and Richard A. Posner*

THE LAW SCHOOL  
THE UNIVERSITY OF CHICAGO

April 2008

This paper can be downloaded without charge at:  
The Chicago Working Paper Series Index: <http://www.law.uchicago.edu/Lawecon/index.html>  
and at the Social Science Research Network Electronic Paper Collection:  
<http://ssrn.com/abstract=1126403>.

April 14, 2008

## **RATIONAL JUDICIAL BEHAVIOR: A STATISTICAL STUDY**

**William M. Landes and Richard A. Posner<sup>1</sup>**

### I. INTRODUCTION

A large literature, mainly in political science, uses statistical techniques to explain various aspects of judicial behavior, with particular emphasis on federal appellate judges—circuit (that is, court of appeals) judges and Supreme Court Justices. Legal writers have tended to ignore this literature despite its richness,<sup>2</sup> in part because its vocabulary and empirical methodology are unfamiliar and in part because, unlike economic analysis of law, it does not have clear implications for the reform of legal doctrine and cannot readily be integrated into the teaching of the major law school courses. We believe, however, that it has a great deal to offer to the understanding of judicial behavior—a subject of theoretical interest to economists as well as to other social scientists and to academic lawyers and of practical significance to lawyers and judges.<sup>3</sup> We try to make a distinctive contribution to the literature in this paper.

A number of the previous studies, listed in Appendix A to this paper, are based, as is ours, on one of two large databases (or both)—a court of appeals database called the “Songer” da-

---

<sup>1</sup> The authors thank Alicia Beyer, Allison Handy, and Tara Kadioglu for their very helpful research assistance. In addition, Handy did the first draft of the appendices. We thank Lee Epstein, Emerson Tiller, and other participants in the Political Economy Colloquium of Northwestern University School of Law, where we gave an earlier version of this paper on September 24, 2007, for their helpful comments; participants in the University of Chicago Workshop on Rational Models in the Social Sciences, where an earlier version of the paper was given on October 30, 2007, for their helpful comments; and the John M. Olin Program in Law and Economics for support.

<sup>2</sup> A richness exemplified by James L. Gibson, “From Simplicity to Complexity: The Development of Theory in the Study of Judicial Behavior,” 5 *Political Behavior* 7 (1983).

<sup>3</sup> Richard A. Posner, *How Judges Think* (Harvard University Press, forthcoming 2008).

tabase<sup>4</sup> and a Supreme Court database called the “Spaeth” database.<sup>5</sup> These databases record data on a large sample of court of appeals cases decided since 1925 and Supreme Court cases decided since 1937. (Appendix B describes the databases.) Many of the data collected about each case—such as the date of the case, the main issue in it, and its disposition by the court—are straightforward or nearly so. But a critical datum is not. It is the classification of the vote of each judge or Justice as being “liberal,” “conservative,” “mixed,” or “other.” The “mixed” and “other” categories are found only in the court of appeals database. “Mixed” means that the judge voted for an intermediate outcome, for example to affirm a criminal conviction but reduce the sentence—in other words, he cast a liberal vote on one issue and a conservative vote on another in the same case. “Other” means that the vote had no political valence—usually because the opposing sides could not be classified as “liberal” and “conservative.” The ideological classifications of votes are dependent variables in studies that seek to explain judicial behavior by reference to judges’ characteristics, such as (the particular interest of political scientists who study the courts) whether a judge is “liberal” or “conservative.” That characteristic is usually proxied by the party of the President who appointed the judge—if it was the Democratic Party the judge is deemed “liberal” and if the Republican Party “conservative.” Other proxies are sometimes used, however.

---

<sup>4</sup> The “U.S. Courts of Appeals Database” was originally compiled by Donald R. Songer, and updated by Ashlyn K. Kuersten and Susan B. Haire. It is archived at the S. Sidney Ulmer Project for Research in Law and Judicial Politics, available at [www.as.uky.edu/polisci/ulmerproject](http://www.as.uky.edu/polisci/ulmerproject). For data about the attributes of the judges, we used “The Attributes of Federal Court Judges Database,” originally compiled by Gary Zuk, Deborah J. Barrow, and Gerard S. Gryski, also archived at the S. Sidney Ulmer Project home page, and sometimes referred to as the “Auburn” database.

<sup>5</sup> The U.S. Supreme Court database was compiled by Harold J. Spaeth (for the 1953–2000 terms), and by Lee Epstein and Jeffrey A. Segal (for the 1937–1952 and 2001–2006 terms). The justice-centered databases we used (“The Justice-Centered Warren Court Database,” “The Justice-Centered Burger Court Database,” and “The Justice-Centered Rehnquist Database”) were created from the original database by Sara C. Benesh. The databases are archived at the S. Sidney Ulmer Project for Research in Law and Judicial Politics, note 4 above. For ? Finish statement or get rid of “For”

Of course it is possible to question the assumption that all judges appointed by Democratic Presidents are liberal and all judges appointed by Republican Presidents are conservative. But for some purposes the realism of the assumption is irrelevant. If the question, for example, is whether Democratic Presidents appoint more liberal judges than Republican Presidents do, the classification of the votes supplies the answer: If judges appointed by Democratic Presidents vote more often for liberal outcomes than judges appointed by Republican Presidents, it doesn't matter whether a particular judge, when appointed, would have been considered liberal.

But the classification of judges' votes is problematic in the two databases. A problem limited to the court of appeals database is that the coders who classified decisions as liberal or conservative (or mixed or other) apparently had trouble classifying older cases. A spot check of 40 cases, 10 from each approximately 20-year period in the database (which, remember, covers the 77 years from 1925 to 2002), reveals a high error rate in cases decided before 1960.<sup>6</sup> Second—and again this is a more serious problem with the court of appeals database—a number of the systematic classification decisions that the coders made are erroneous, such as classifying all votes for plaintiffs in intellectual-property cases as liberal. We have reviewed and corrected the systematic classifications, as explained in Appendix C to this paper. But we have not reread enough of the actual decisions to be able to correct the misclassification of individual cases as distinct from categories such as intellectual property.

The databases corrected in the manner just indicated are the source of the data in our statistical analysis, so let us see just how significant our corrections are. Table 1 compares the number of liberal, conservative, and unclassified votes in the Supreme Court database, with and without our corrections. Table 2 makes the same comparison for the court of appeals database but with the addition of "mixed" votes. The principal effect of the corrections is to increase the number of decisions that are not classified as ideological. The corrections are not

---

<sup>6</sup> In the first sample, consisting of cases decided between 1925 and 1940, the error rate is 40 percent; in the second sample, 1940 to 1959, it is 20 percent; but for the period from 1960 to 2002 it is only 10 percent.

major in the Supreme Court database but do lead to substantial changes in the court of appeals database.

[Insert Tables 1 and 2 here]

Applying statistical methodology to the corrected databases, we explore a range of empirical questions, such as whether a judge's political voting behavior changes over his term of office and whether it depends on the ideological make-up of the other judges on the court—that is, whether social influence or group effects play a role in judicial decision making. We are especially though not exclusively interested in testing hypotheses derived from a rational-choice (economic) approach to judicial behavior. We do not propose a formal economic model of judicial behavior, but in the next part we sketch an informal such model to guide our empirical analysis.

Before turning to that, however, we note one more methodological innovation. Social scientists have become very interested in recent years in group effects, such as group polarization, but most of their empirical work on such effects utilize students as experimental subjects. Judicial voting at the appellate level (trial judges sit by themselves) provides an opportunity to observe the behavior (in the form of votes) of actual rather than experimental groups, and thus avoids criticisms of extrapolating from experimental to real social situations.

## II. RATIONAL JUDICIAL BEHAVIOR

Our analysis in this paper is limited to federal judges (Supreme Court Justices and federal circuit—i.e., court of appeals—judges). Federal judges who are appointed under Article III of the Constitution (as Supreme Court Justices and federal district and circuit judges are, but other federal judicial officers, such as magistrate judges, bankruptcy judges, and administrative law judges, are not) have an unusual career structure and employment conditions. The federal judiciary is primarily a lateral-entry system, judges being appointed in their 40s or 50s after a career in another branch of the legal profession. Promotion from one tier of the judiciary to another is un-

usual, so that for most federal judges there is no promotion carrot to motivate them. “External” promotion (appointment to a higher-paying job in the private sector) is rare; the judicial appointment normally is terminal. Nor can federal judges receive bonuses or raises for exceptional performance. Nor can their pay be docked for substandard performance, and the removal of judges from office is virtually impossible unless they engage in criminal behavior. Their outside income is strictly limited, and of course they are not permitted to hear cases in which they might have a direct or indirect pecuniary stake.

With the ordinary motivations and constraints that are designed to minimize agency costs being absent, it is difficult to model judicial behavior in rational-choice terms. And no doubt emotional and other psychological factors play a larger role in judicial behavior than in that of normal employees. But we believe that most judicial behavior is rational and hence that there is a judicial utility function.<sup>7</sup> We would expect that leisure would be a major argument in the judicial utility function, as in that of any rational individual with secure tenure; also self-expression, for example of political preferences, since there are no (or very weak) penalties for basing judicial decisions on such preferences. Another argument in the judicial utility function is likely to be esteem (prestige, reputation, etc.), which in turn is likely to make judges averse to being reversed. With these arguments, we will be able to suggest explanations for a number of the findings in our statistical analysis.

### III. ANALYSIS OF SUPREME COURT VOTING: 1937–2006

Our Supreme Court sample contains 43 Justices (including eight who were appointed prior to 1937) and 637 observations. As suggested by Andrew Martin and Kevin Quinn,<sup>8</sup> we exclude from most of our analysis 9–0 decisions on the ground that they are unlikely to involve the kind of ideological issues that divide judges. Differently stated, the fact that all the Justices agree

---

<sup>7</sup> See references in Posner, note 3 above, ch. 1.

<sup>8</sup> See Andrew D. Martin and Kevin M. Quinn, “Dynamic Ideal Point Estimation via Markov Chain Monte Carlo for the U.S. Supreme Court, 1953–1999,” 10 *Political Analysis* 134, 137 n. 3 (2002).

on a particular outcome suggests that ideological considerations play a negligible role in the vote on the case. We do, however, present a separate analysis of 9–0 decisions to see if the recorded ideological direction of unanimous decisions is related to the Justices’ ideological make-up.

#### *A. Judicial Ideology Rankings.*

Table 3 ranks the Justices in our sample from most to least conservative on the basis of their judicial votes. Rehnquist and Thomas rank as the most conservative Justices, while Thomas, Roberts, and Alito are the most conservative in economic cases (economic regulation, labor, and tax).<sup>9</sup> At the other end of the ideological spectrum, Justices Marshall, Douglas, Murphy, and Rutledge are the most liberal, although Black is the most liberal in the economic-regulation category. We present results in two other subject-matter categories as well: civil liberties (all cases minus economic-regulation, labor, and tax cases) and adjusted civil liberties (which excludes from the civil liberties category federalism and judicial-power cases). The two civil liberties categories track all cases closely because civil liberties cases account for 67 percent of all votes in nonunanimous cases. Notice the drop in the fraction of conservative votes of the most liberal Justices (except for Marshall) in the adjusted compared to the unadjusted civil liberties category. Apparently issues of federalism and judicial power tend to be less ideological than issues involving personal liberty.

[Insert Table 3]

Our ideological rankings are generally consistent with what everyone knows to be the ideological differences among Supreme Court Justices—the Justices at the top are indeed more conservative than those at the bottom—but some of the specific rankings cannot be taken seriously. For example, Kennedy is more conservative than O’Connor, Ginsburg more conservative than Blackmun, McReynolds more conservative than Powell. And Justices who served 70 years ago are difficult to

---

<sup>9</sup> It should be noted, however, that the calculations for Roberts and Alito are based on votes in only two terms.



place on the same ideological scale as current Justices, because the meaning of liberal and conservative have changed over this period.

Table 3 includes three other ideology measures. One, labeled “S/C score,” is based on a content analysis by Jeffrey A. Segal and Albert D. Cover of newspaper editorials published prior to the Justice’s confirmation but limited to Justices appointed after 1945. The Segal/Cover scores range from 0 (most liberal) to 1 (most conservative).<sup>10</sup> The two remaining measures are based, like ours, on judicial votes. Martin and Quinn derive ideology scores from votes in nonunanimous cases, using the uncorrected Spaeth Supreme Court database, while Epstein et al. calculate the fraction of conservative and liberal votes in the adjusted civil liberties category on the basis of an expansion of the Spaeth database to cover the 1946 to 2004 terms, but the expanded database, like the original one, does not correct for erroneous ideological classifications. Table 4 is a correlation matrix of the various ideology measures.

[Insert Table 4 here]

It is no surprise that the measures based on actual votes are more highly correlated with each other than the Segal/Cover scores, which are based on newspaper editorials that in effect predict the Justice’s judicial voting. In contrast, though unsurprising in view of the fact that our adjustments to the Supreme Court database have only slight effects overall, the correlations between our data and Epstein et al.’s in Table 3 are above .95 except for the correlation between their civil-liberties category and our economic-regulation category. Nevertheless the high positive correlations between the Segal/Cover scores and the fraction of conservative votes suggest that newspaper editorials prior to appointment are surprisingly good predictors. Indeed, Segal/Cover scores will turn out to be

---

<sup>10</sup> We have transformed them to 0 for most liberal and to 1 for most conservative in order to make them easier to compare to our fraction of conservative votes ranking. The Segal/Cover scores are reproduced in Table 6–1 in Lee Epstein et al., *The Supreme Court Compendium: Data, Decisions & Developments* (4th ed. 2007).

highly significant predictors in our regression analysis of judicial voting.

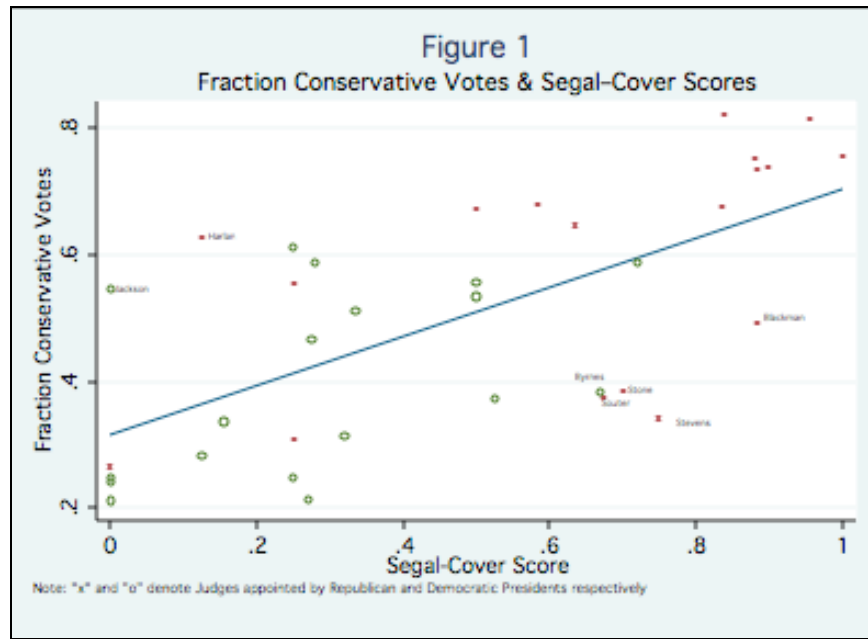


Figure 1 depicts the relation between Segal-Cover scores and the fraction of conservative votes in all categories for the 36 Justices whose Segal-Cover scores are available.<sup>11</sup> As expected, Justices appointed by Republican Presidents (denoted by the symbol "x") tend to vote more conservatively and have higher Segal-Cover scores, while judges appointed by Democratic Presidents ("o") tend to vote less conservatively and have lower Segal-Cover scores. Notice that the positive relation between the fraction of conservative votes and Segal-Cover scores is similar for Justices appointed by Republican and Democratic Presidents.<sup>12</sup>

Several outliers in Figure 1 should be noted. Jackson and (the second) Harlan (also Vinson and Stewart, but less so)

<sup>11</sup> The straight line in Figure 1 depicts the regression (t-statistics in parentheses)  $Y = .315 (7.29) + .390 (5.10)X$  where  $Y$  and  $X$  denote the fraction of conservative votes and the Segal-Cover scores respectively.

<sup>12</sup> There was no significant difference between the regression coefficients when we estimated separate regressions for Republican and Democratic appointees only. (Alternatively, can have "is" with "estimate" for consistent verb tense)

voted more conservatively than predicted by their Segal/Cover scores, while Stevens, Souter, Byrnes, Stone, and Blackmun voted more liberally than expected. These discrepancies suggest that Presidents sometimes do not have good public and private information concerning the ideological proclivities of possible Supreme Court candidates. Nevertheless, Table 5 reveals a strong correlation between the political party of the appointing President and the voting behavior of the Justices appointed by a President of that party. In each of the 11 subject-matter categories (excluding a miscellaneous category in which there is only 1 nonunanimous vote), the fraction of conservative votes cast by Justices appointed by a Republican President is greater than that cast by Justices appointed by a Democratic President—and significantly so except in the privacy category, which has the fewest votes.

This finding, which is consistent with a large empirical literature in political science, supports the hypothesis of a self-expression argument in the judicial utility function. Justices tend to have political views similar to those of the President who appoints them. The freedom of federal judges from the usual sticks and carrots of an employment situation enables them to express those views, even though it may make them unfaithful agents of Congress (when they are interpreting federal statutes) or the framers and ratifiers of constitutional provisions (when they are interpreting the Constitution).

This is not to suggest that Justices appointed by Republican Presidents *always* cast a conservative vote or Justices appointed by Democratic presidents always a liberal one. The former vote liberal more than conservative in 3 of the 11 categories in Table 5, while the latter vote liberal more than conservative in 9 categories. “Privacy” and “Judicial Power” are the two categories in which both types of appointee vote most conservatively. The biggest difference is found in “Civil Rights,” “Due Process” and “Unions.” In these categories, Justices appointed by a Republican President are more than 50 percent more likely than those appointed by a Democratic President to vote conservatively.

[Insert Table 5 here]

Martin and Quinn, and Epstein et al., suggest that a Justice's judicial ideology may vary over his tenure, depending on strategic considerations, changes in preferences, and changes in the composition of cases before the court.<sup>13</sup> We tested this hypothesis by estimating separate regressions for each Justice who served 15 or more terms. The dependent variable is the fraction of the Justice's conservative votes ( $y$ ) and the independent variable is the length of time that he served (or has served, if he is a current Justice) on the Supreme Court (where  $x$  equals 1 for his first term, 2 for his second term, and so on). We make the simplifying assumption that the Justice's judicial ideology either is constant over time (the regression coefficient is statistically insignificant) or changes linearly (the Justice becomes more or less conservative at a constant rate, as shown by whether the regression coefficient is significantly positive or significantly negative). The changing-ideology hypothesis is supported for 11 of the 21 Justices who served a minimum of 15 terms.<sup>14</sup> We find statistically significant negative coefficients for Blackmun, Brennan, Douglas, Marshall, O'Connor, Rehnquist, Stevens, and Souter and statistically significant positive coefficients for Frankfurter, Reed and White.<sup>15</sup> That is, all of these Justices became either more liberal or more conservative in their judicial voting over the course of their career on the Court. Of the eight Justices who became more liberal, six were appointed by Republican Presidents; the three who became more conservative had all been appointed by Democratic Presidents. This is further support for a self-expression argument in the judicial utility function. The Justices are not faithful agents of their appointing President because there is noth-

---

<sup>13</sup> See *id.*; Epstein et al., note 9 above.

<sup>14</sup> We used 15 terms as the cut-off to increase the reliability of our estimates. If we estimate regressions for all 43 Justices, we also find significant negative coefficients for Ginsburg (14 terms), Murphy (10 terms), Rutledge (7 terms), and Souter (11 terms), and significant positive coefficients for Jackson (12 terms), Owen Roberts (8 terms), and Whittaker (6 terms).

<sup>15</sup> There is substantial overlap between our findings and those in Martin and Quinn and in Epstein et al. Martin and Quinn find that Justices Black, Frankfurter, Thomas, and White became more conservative while Justices Blackmun, Brennan, Marshall, Stevens, and Souter became more liberal. Epstein et al. find no trends for Marshall and Brennan but liberal trends for Warren, Clark, and Powell.

ing the President can do (even before the President leaves office) to affect their welfare.

The moderation of Rehnquist's conservative stance seems related to his becoming Chief Justice in 1986. If we add a dummy variable for the period of his chief justiceship, the regression coefficient is negative (indicating about a 10 percent decline in the fraction of conservative votes) and nearly significant, while the coefficient on the tenure variable becomes insignificant.

### *B. Unanimous Votes.*

Figure 2 shows that about 30 percent of the Supreme Court cases in our database were decided unanimously (defined as 9–0 votes). Somewhat surprisingly in light of the belief that ideological differences among the Justices have been growing, the fraction of unanimous decisions has been trending upward and is now in the 40 percent range. This means we have excluded a substantial fraction of Supreme Court decisions by limiting our analysis to nonunanimous decisions. In a court made up of liberal, moderate, and conservative justices, ideology is unlikely to be an important consideration in a unanimous decision. But since many of the unanimous decisions are coded as conservative or liberal, we might expect the ideological direction of these decisions to be related to the Justices' ideology.

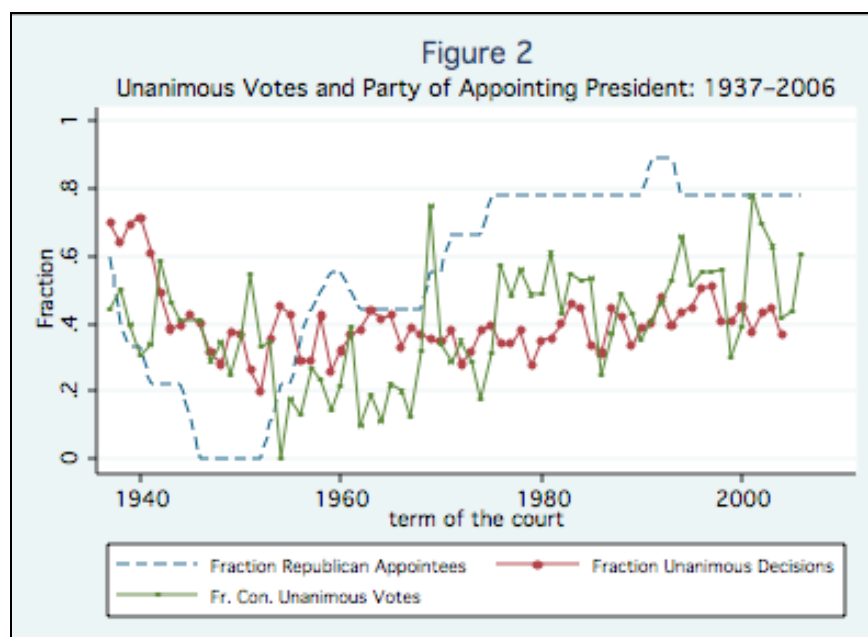


Figure 2 shows that the higher the fraction of Justices appointed by Republican Presidents, the higher the fraction of unanimous decisions that are conservative. And regressing the fraction of conservative votes in unanimous decisions against the fraction of Justices appointed by Republican Presidents yields a positive regression coefficient of .240 and a t-statistic of 3.90 (significant at the .01 level). However, if we add a linear time trend variable to the regression neither the trend nor the fraction of Justices appointed by Republican Presidents is statistically significant because the two variables are highly correlated (.84). In short, there appears to be a positive relationship between the fraction of conservative unanimous votes and the fraction of Justices appointed by Republican Presidents but we cannot reject the hypothesis that this effect vanishes once we account for the positive time trend in the fraction of conservative unanimous decisions.<sup>16</sup>

<sup>16</sup> The fraction of Justices appointed by Republican Presidents is only a proxy for the ideological makeup of the Justices because Presidents have sometimes appointed Judges who turn out to have ideologies that differ from the appointing President. (Some well-known examples are Eisenhower's appointment of Brennan, Ford's appointment of Stevens and Bush I's appointment of Souter.) As an alternative measure of judicial ideology, we classified the 43 Justices into one of three categories: conservative

### C. Regression Analysis of Nonunanimous Votes.

Our regression analysis seeks to explain the percentage of conservative votes in nonunanimous decisions by the Supreme Court as a function of a set of variables that seem likely to influence the ideological direction of a Justice's vote.<sup>17</sup> Table 6 defines the variables in the analysis and Table 7 presents the regressions.

[Insert Table 6 here]

We base our analysis on two regression equations:

$$(1) \quad ID_i = \alpha_0 + \alpha_1 X_i + u_i$$

$$(2) \quad FrCon_{ij} = \beta_0 + \beta_1 X_i + \beta_2 u_i + \beta_3 Y_{ij} + w$$

In equation (1),  $ID_i$  denotes the  $i^{th}$  Justice's ideology prior to his appointment (as proxied by his Segal/Cover score),<sup>18</sup>  $X_i$  is a set of factors likely to predict his ideology (such as the party of the appointing President, the fraction of senators who are Republicans, the year or first term of the judge, and prior experience

---

(14 Justices), moderate (11) and liberal (18), and then re-estimated the regression substituting the number of conservative and the number of liberal Justices each term (the left-out variable is the number of moderate judges). As in the regressions in which we use the fraction of Justices appointed by Republican Presidents, we find no significant effects of the conservative/liberal categories on the fraction of conservative unanimous decisions when we also include a time-trend variable.

<sup>17</sup> The percentage of liberal votes is simply 1 minus the percentage of conservative votes, since the Supreme Court database does not contain "mixed" or "other" categories but only "conservative" and "liberal."

<sup>18</sup> See Lee Epstein and Jeffrey A. Segal, *Advice and Consent: The Politics of Judicial Appointments* 108–113 (2005), for a concise description of how ideology scores are computed from editorials. Segal and Cover first coded each paragraph in editorials written about the candidate's likely views as describing the candidate as conservative, liberal, or moderate, and then subtracted the fraction coded liberal from the fraction coded conservative and divided by the total number of paragraphs. The scores range from +1 for pure liberal and –1 for pure conservative. These scores are reproduced in Epstein et al., note 9 above, tab. 4–17, for Justices nominated between 1937 and 2006.

as a federal court of appeals judge) and  $u_i$  is the residual, that is, the difference between the Justice's actual and predicted voting-ideology score. In equation (2), the dependent variable ( $FrCon_{ij}$ ) is the fraction of conservative votes that each Justice cast per term from 1937 to 2006, and the independent variables include  $X_i$ ,  $u_i$ , and such other factors ( $Y$ ) as prior judicial experience, years on the Supreme Court, a time trend, and a variable that we call "group effects" or "social influence," which estimates the influence of other members of the Court on Justice  $i$ 's votes. A positive (negative)  $u_i$  in equation (1) indicates that the Justice's conservative ideology score is higher (lower) than the  $X_i$  variables predict. In other words, the Justice is even more conservative than one could have predicted. This implies that he will vote more conservatively in equation (2) than a Justice with a lower  $u_i$  would. Similarly, the larger a Justice's negative  $u_i$  the more likely he is to vote liberally.

The group-effects variable tests whether the ideological leanings of *other* members of the Court influence the ideological cast of a Justice's votes and, if so, in what direction. Three group effects should be distinguished. One is conformity: wanting to conform to the majority. We do not interpret this as a psychological phenomenon, although social psychologists discuss it in those terms; instead, we relate it to (rational) dissent aversion.

The second group effect or social influence that we consider is group polarization: deliberation among persons who lean in one direction is likely to make them lean even farther in that direction. The economic interpretation (though again there is also a psychological one) is that a person who takes an extreme view, bucking the majority, is likely to be the best informed, and so it is rational for the other members of the group to be persuaded by him). If conformity dominates, an increase in the fraction of Justices appointed by Republican Presidents should lead a Justice to vote more conservatively, whether a Republican or a Democratic President appointed him. In contrast, group polarization would lead a Justice appointed by a Republican President to vote more conservatively as the fraction of Justices appointed by Republican Presidents increase but would not affect the voting of Justices appointed by a Democratic President.



In a different sense, polarization could refer to two interacting groups' growing farther apart, as when political scientists speak of the growing "polarization" of the American electorate. Here an increase in the fraction of Justices appointed by Republican Presidents could lead Justices appointed by a Republican President to vote more conservatively and Justices appointed by a Democratic President to vote more liberally. We shall call this third social influence "political polarization," but we do not have an economic interpretation for it.

[Insert Tables 7 here]

Now to the results: The variables in the first regression equation explain about 43 percent of the variance in Segal/Cover scores ( $ID_i$ ). Two of the four variables in equation (1) are statistically significant. Justices appointed by Republican Presidents have significantly higher conservative scores than those appointed by Democratic Presidents, as we predict. And holding constant the party of the appointing President's, more recent appointees have significantly higher ideology scores (that is, they tend to be more conservative). But our main interest in equation (1) is in supplying an ideological variable for use in regressions based on equation (2). These are regressions (2) through (4) in Table 7. In regression (2) the dependent variable is the fraction of conservative votes by Justices whether they are appointed by Republican or Democratic Presidents. In regressions (3) and (4) the dependent variable also is the fraction of conservative votes but regression (3) is limited to Justices appointed by Republican Presidents, and regression (4) to Justices appointed by Democratic Presidents.

Regression (2) reveals as expected that Justices appointed by Republican Presidents tend to vote conservative in a higher fraction of cases (about 15 percent higher across all categories) than Justices appointed by Democratic Presidents. The political composition of the Senate at the time of the Justice's appointment also has a separate and significant effect on how ideological a Justice turns out to be. For example, a change in the lineup in the Senate from 53 to 47 Republicans reduces the fraction of conservative votes of a Justice appointed by a Republican President by .076 ( $= .06 \times 1.27$  in regression (3)) and of

a Justice appointed by a Democratic President by .040 ( $= .06 \times .67$  in regression (4)). In contrast, regression (1) indicates that the composition of the Senate does not have a significant effect on a Justice's observed pre-appointment ideology score. The combination of the Senate having no impact on observed pre-confirmation ideology but a significant impact on how a Justice's later votes suggests that Senators have private information about the ideological leanings of Supreme Court candidates that is not publicly available as evidenced by the content of newspaper editorials (for remember that Segal/Cover scores are based on newspaper editorials about the candidates). This implies further that make up of the Senate also influences the President's choice of whom he appoints to the Court for the greater the fraction of Republican senators, holding constant the observed ideology of the appointee and the President's party, the more conservative the Justice turns out to be.

The most significant variable in regressions (2) through (4) is the residual ( $u$ ) from equation (1)—the difference between the Segal/Cover score and the predicted ideology score. As expected, the fraction of conservative votes significantly increases as  $u$  increases. Consider Justice Brennan, who was appointed by a Republican President (Eisenhower) and confirmed by a Senate equally divided between Republicans and Democrats. Although Brennan's Segal/Cover score is 0 (the maximum liberal score), his predicted ideology score in regression (1) is .577, which gives Brennan the highest unexplained liberal score (negative residual). In regression (3) this implies that Brennan would vote liberally on average in 65 percent of the cases compared to 45 percent if one did not know Brennan's Segal/Cover score but knew the party of the appointing President and the make-up of the Senate at the time of Brennan's appointment...

Here are two examples at the other extreme. Roosevelt appointed James Byrnes in 1941, when Democrats outnumbered Republican Senators by more than 2 to 1. Byrnes's predicted ideology score from regression (1) was .231, yet his Segal/Cover score was a relatively conservative .67. Similarly, in 1945 Truman appointed Burton, whose Segal/Cover score was .72 but whose predicted ideology score was .260. Byrnes and Burton have the highest unexplained conservative scores (.439 and .459 respectively) of any Justice in our sample, and this results

in about 16 percent more conservative votes by Byrnes and Burton than would be predicted from regression (1).

The *SCRep* variable tests whether a Justice's colleagues influence his vote. The significance of social influence on individual behavior is well documented, and it would hardly be surprising to find that judges are not immune from it. The conformity hypothesis predicts a positive sign of the *SCRep* variable in regressions (2)–(4)—that is, that the larger the fraction of Justices appointed by Republican Presidents, the more conservatively each Justice will vote. We find the opposite—negative signs that are highly significant in two of the three regression equations. Our results thus suggest that there is no conformity effect, or, equivalently, “dissent aversion,” in the Supreme Court. We offer an economic explanation later, when we discuss our finding that there is dissent aversion in the courts of appeals.

Our *SCRep* variable does not allow us to test the group-polarization effect directly (that is, the tendency of an in-group to take a more extreme position than the average member of the group, owing to the influence of its most extreme members<sup>19</sup>) because we do not know whether an increase (decrease) in *SCRep* implies that a new Justice is more (less) conservative than the Justices that were previously appointed by Republican (Democratic) Presidents. However, the larger the relative size of the in-group, the likelier there is to be a group-polarization effect because the likelier there is to be a member with more extreme views than in a smaller group, for the smaller the group, the lower the probability that there will be a member in the tail of the distribution in which the most extreme views are found. Also, there will be fewer opponents. Hence the *SCRep* variable provides an indirect test of the hypothesis. But our results fail to support it. We find a negative (not positive) though insignificant effect of *SCRep* in the Republican-only regression (regression (3)) and a highly significant negative effect in the Democratic-only regression (regression (4)). The implication is that Justices appointed by Democratic Presidents become more liberal as they become more out-

---

<sup>19</sup> Alice H. Eagly and Shelly Chaiken, *The Psychology of Attitudes* 655–659 (1993).

numbered. This is consistent with conservative Justices' tending to be ideologically more committed—their views are less affected by the views of liberal Justices, whereas the liberals are not roused to assert their full liberalism until pushed into a corner by a growing conservative bloc.

The third social influence is what we called “political polarization,” and leads to the prediction that if one bloc of Justices grows at the expense of another the result will be to push a Justice in the ideological direction of the group to which he belongs. Thus, if the Supreme Court becomes more dominated by, say, Justices appointed by Republican Presidents (an increase in *SCRep*), those Justices will vote more conservatively than before but the smaller number of Justices appointed by Democratic Presidents will vote more liberally. As just noted, we find this latter effect. In regression (4), if the Court shifts from a 5–4 majority of Justices appointed by a Democratic President to a 5–4 majority of Justices appointed by a Republican President, a Justice appointed by a Democratic President can be expected to vote more liberally in about 3 percent of the cases ( $=.244 \times 1/8$  as *SCRep* increases from 4/8 to 5/8). But, as also noted, we find no similar effect when the parties are reversed.

An additional and puzzling point about the social influence variable is that if we divide the sample into civil liberties cases and economic cases and re-estimate the regressions in Table 7, we find a significant negative effect of the *SCRep* variable in the second category but not the first. That is, the tendency of Justices appointed by Democratic Presidents to vote more liberally as their number shrinks shows up only in economic cases. We have no explanation for this finding.

Our regression analysis yields the following additional results:

(1) Justices appointed more recently (*YrAppt*) are more likely to vote conservative. But although highly significant overall (equation (2)), this result appears limited to Justices appointed by Republican Presidents (equation (3)).

(2) *Term* (i.e., time-trend) is negative, and is significant only in the Republican-appointee regressions. The effect is small—about a .024 increase in the fraction of liberal votes every three years. The explanation for the effect and its small

size may be that what is moving the Court in a conservative direction is the ideology of the appointees. That effect is picked up in *YrApt*, so that the small negative effect of *Term* may be reflecting the less conservative drift of society than the more conservative drift of judges appointed by Republican presidents. The former may have an independent effect on the Justices because they do not want to get too far out of step with public opinion.

(3) Supreme Court Justices appointed from the federal courts of appeals vote more liberally than other Justices. The magnitude of this effect is large and significant in all the regressions. The fraction of conservative votes falls by .07, and since the mean of the fraction of conservative votes in all categories is .47, a decline of .07 translates into a percentage decline of 15 percent. A possible explanation is that judges socialized into the judicial role by prior appellate experience on a lower court that is bound to conform to Supreme Court precedent become more respectful of *stare decisis* (decision making based on precedent), and the most controversial precedents are the liberal decisions of the Warren Court and, to a lesser extent, of the Burger Court.

### C. Other Behavior of Supreme Court Justices.

We now regress such outcome variables as fractions of dissents, concurrences, one-vote decisions, and reversals on independent variables that include the fraction of nonunanimous conservative votes of the median Justice (a measure of the median Justice's ideology), the difference between the Justice with the maximum and the Justice with the minimum of the fraction of nonunanimous conservative votes among Justices in a given term (a proxy for the range of ideological differences among Justices), the number of new Justices and Justices' length of tenure, the number of cases per term of Court, and a time-trend variables. The data cover the 1937 to 2004 period. Table 8 explains the variables and reports their means.

[Insert Table 8 here]

Although we do not have a complete model of judicial decision-making, we suspect that any such model would predict, for

example, that the greater the ideological differences among Justices, the greater the fraction of close decisions.<sup>20</sup> A more ideologically divided court is less likely to be able to coalesce around a single opinion in each case.

Table 9 presents the regression results. The dependent variables are the fraction of dissents (regressions (1) and (2)—the dependent variable is the same in both regressions but (2) has an additional independent variable), the fraction of concurrences (regression (3)), the fraction of cases decided by one vote ((4)), and the fraction of reversals ((5)) in the 1937 to 2004 terms (although data on reversals begin in 1946).

[Insert Table 9 here]

Regressions (1) and (2) reveal a positive and statistically significant correlation between the fraction of cases in which there is a dissent and the fraction in which there is a concurrence. Causation, however, is unclear. One might have expected concurrences and dissents to be substitutes; we find them to be complements. The explanation may be that ideological differences generate not only dissents but also concurrences, the former reflecting disagreement over the outcome, and latter disagreement over the grounds for the outcome. Absence of dissent aversion may also be a factor that reduces the substitutability of a concurrence for a dissent.

Other results of the regression analysis in Table 9 are as follows:

(1) The ideology of the ideologically median Justice has a significant effect on the fraction of decisions with dissenting opinions (regressions (1)) and the fraction of reversals (regression (4)). We find that the more conservative the median Justice, the greater the fraction of cases with dissenting opinions but the lower the fraction of reversals. This suggests that lower-court judges tend to be conservative, since the reversal rate is a measure of the disagreement between the Supreme Court and the courts whose decisions it is reviewing. We shall

---

<sup>20</sup> Although in a case decided by one vote there necessarily is at least one dissent, the mean proportion of cases with dissenting opinions is 58 percent, whereas the mean proportion of cases decided by one vote is only 15 percent.

suggest an explanation for this result when we discuss the court of appeals cases.

(2) The greater the range of potential disagreement among Justices (as proxied by the difference between the maximum and minimum conservative votes per term), the greater the fraction of dissents and of cases decided by one vote. These results are highly significant in regressions (1) and (3) and are in the predicted direction. The range variable has no significant effect on reversals (regression (4)), but this is not too surprising because why should the reversal rate be related not to the ideology of the median judge but to the amount of ideological disagreement on the Court? But it is a little surprising because a polarized Court may be quite unpredictable. That is the situation today, with pretty solid blocs of four liberals and four conservatives and a moderate Justice (Kennedy) who swings between them, though more often to the conservative side.

(3) Contrary to what one might have expected, there is a *negative* time trend in the fraction of dissents (regression (1)), implying a decline in disagreement among the Justices, holding ideological differences constant (the range variable). But the fraction of concurrences has increased significantly, suggesting an increase in disagreements over the reasoning in the majority opinion as distinct from disagreements over outcome. Since we are holding ideological differences constant, the implication is that even ideological allies are finding it more difficult to agree on the grounds of decision.

(4) The number of cases per term is positively and significantly related to the fraction both of concurrences and of reversals but not to the fraction of dissents. The correlation with reversals makes economic sense. The more disagreement there is between the Supreme Court and lower courts, the more cases the Court has to take (its jurisdiction is discretionary) in order to enforce its views on those courts. A related point is that most Supreme Court decisions are reversals of the lower court, and many of the affirmances are in cases in which there is a circuit split, meaning that in affirming one decision the Supreme Court is disapproving (and thus in effect overruling) a decision or decisions in another circuit or other circuits.

This is an illustration of management by exception, a practice emphasized in the literature on the economics of organiza-

tions. To economize on skilled employees, routine problems are left to subordinate employees to resolve, but the nonroutine are bounced up to higher levels. When the Supreme Court reverses a lower-court decision, it in effect “corrects” the mistakes made by a subordinate. The more “mistakes” the subordinates make, the more frequently the superiors must intervene; hence the positive correlation between cases heard by the Supreme Court and percentage of reversals. In this analysis the Supreme Court Justices are principals and the lower-court judges are their agents, who have to be kept in line to minimize agency costs.

The effect of the number of cases that the Court hears on the number of concurrences may seem puzzling because the more cases there are, the less time the Justices have to write separate opinions. But the increase in the number of law clerks may have offset this effect. More interesting, an increase in concurrences may reflect a desire to provide more guidance to lower courts, since concurring opinions by the Supreme Court often are influential with the lower courts. In effect, concurrences act as multipliers of number of cases heard by the Court and so help to minimize agency costs.

(5) We find no significant effects of number of new Justices or of average tenure on the outcome variables.

#### IV. ANALYSIS OF COURT OF APPEALS VOTING: 1925–2002

##### *A. Data Summary.*

The court of appeals (Songer) database contains random samples of 15 decisions from each federal court of appeals annually from 1925 through 1960, and of 30 decisions annually from 1961 through 2002.<sup>21</sup> Our corrected database includes 536 judges and 48,161 votes—an average of 90 votes per judge. These votes are classified in four ideological categories (conservative, liberal, mixed, and other) and eight subject-matter categories (criminal, civil rights, first amendment, due process, privacy, economic regulation, labor, and—a residual category—miscellaneous). Recall that the “other” category consists

---

<sup>21</sup> The data are limited to the 12 regional circuits. The Federal Circuit, created in 1982 with a semi-specialized jurisdiction, is omitted.



of votes that can be classified by subject matter but that have no ascertainable ideological hue. Although we exclude “other” votes from the regression analysis, it is useful to note its magnitude since many of our corrections to the court of appeals database involved shifting votes into that category. Table 10 shows that about 25 percent of the votes in the database are in the “other” class.

[Insert Table 10 here]

The average of 90 votes per judge hides the fact that there are fewer votes for those judges appointed before 1925, the first sample year, and for those judges appointed not long before 2002, because both sets of judges cast relatively few votes in the period covered by the database. Thus, of the 58 judges with fewer than 20 votes (including 3 with zero votes), 10 were appointed before 1925 and 27 after 1990. Figure 3 shows the distribution of votes by the year in which the judge was appointed.

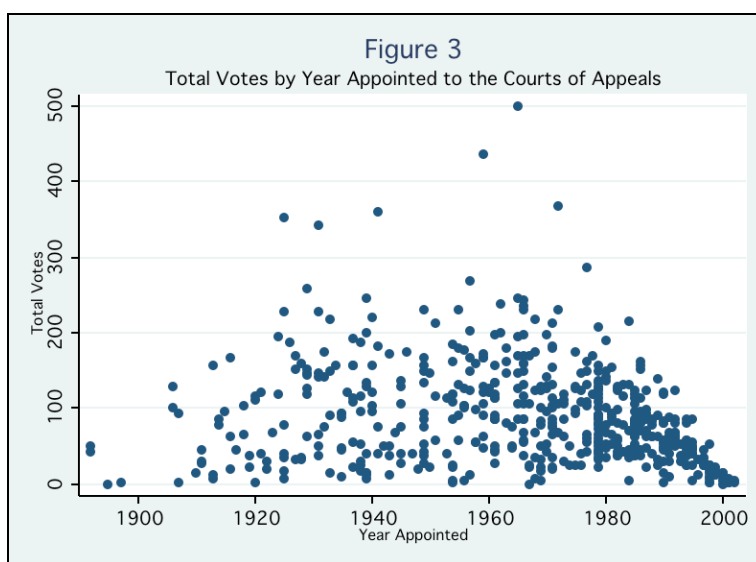


Table 11 relates the percentage of judges’ votes in the different subject-matter and ideological categories to the party of the appointing President. We see that judges appointed by Republican Presidents are indeed more likely to vote conservative, with the imbalance being greater among judges appointed by the most recent Republican Presidents—Reagan and the two

Bushes.<sup>22</sup> The data in Table 11 also reveal a positive time trend in the fraction of conservative votes regardless of the party of the appointing President, though this finding must be taken with caution because of the high error rate in the ideological classification of votes in older cases.<sup>23</sup> The time trend is observed even if we exclude criminal appeals, which yield a high proportion of conservative votes for both Republican and Democratic appointees. The overwhelming majority of criminal appeals are by criminal defendants whose appeals are financed by the government, and with the cost of appealing thus being zero to most criminal defendants, there is a high percentage of groundless criminal appeals.

[Insert Table 11 here]

We cannot meaningfully compare the fraction of conservative votes in all cases in the courts of appeals sample with the corresponding fraction in the Supreme Court because the two judiciaries have a different mixture of cases. We might hope at least to compare the constitutional cases in the two databases, since they are such a large part of the Supreme Court caseload, and a significant part of the court of appeals caseload. We cannot do that exactly, because the categories in the two databases are different, but a satisfactory approximation is to compare votes in the civil liberties category that we used for the Supreme Court (see Table 5) to votes in the constitutional category (civil rights, First Amendment, due process, and privacy) for the court of appeals. The result is striking: The fraction of conservative votes is significantly higher for the court of appeals judges, whether appointed by Republican or Democratic Presidents, than for Supreme Court Justices. The ratios of conservative to liberal votes in the constitutional category in the courts of appeal are 1.84 to 1 for judges appointed by Republican Presidents and 1.24 to 1 for judges appointed by Democ-

---

<sup>22</sup> The sum of the fraction of conservative and liberal votes is less than one because of mixed votes, which account for between 5 and 10 percent of the votes.

<sup>23</sup> Recall from note 6 above that a spot check of appellate cases found a 40 percent error rate in classifications of cases decided between 1925 and 1940, compared to only a 10 percent rate for cases decided since 1960.

rat Presidents, while the corresponding ratios of conservative to liberal votes in the Supreme Court are 1.33 to 1 for judges appointed by Republican Presidents and .82 to 1 for judges appointed by Democratic Presidents.

The likeliest reason for the difference is a selection effect similar to the one that explains the tendency of liberals to cast conservative votes in criminal cases. The liberal side of a civil case is generally the plaintiff's side. Most cases are losers—for example, most employment discrimination cases (a major category of civil rights cases) are dismissed and the dismissal affirmed. The affirmance of a civil rights case that has been dismissed, as of most civil cases in which the defendant won in the district court, is classified as a conservative vote. So, since most dismissals of civil rights cases would be affirmed by any judge who was not ultraliberal, even liberals vote conservative in a large fraction of civil cases. The situation in the Supreme Court is different. Any litigant who loses in the district court can appeal to the court of appeals, but the Supreme Court's jurisdiction is discretionary and the Court agrees to hear only a tiny percentage (currently only slightly more than one-tenth of 1 percent<sup>24</sup>) of the cases decided by the courts of appeals. The cases the Court accepts for review tend to be the more meritorious cases (one indication of this is that the Court reverses more than three-fourths of the cases it decides; the reversal rates in the courts of appeals are much lower), since if the court of appeals had affirmed an obvious loser, the Supreme Court would rarely bother to take the case. Being more meritorious on average, the civil rights cases decided by the Supreme Court receive a higher fraction of liberal votes than the average court of appeals case does.

### *B. Regression Analysis.*

Most of our independent variables are the same as in Table 6. But some are new, including the dummy variables denoting the judge's circuit (*Cir*), gender (*Gend*), race (*Black*), and prior experience as a district judge (*Dist*).<sup>25</sup> There have been too few

---

<sup>24</sup> Posner, note 3 above, ch. 10.

<sup>25</sup> We include 11 *CIR* dummy variables for the 12 circuits (the left out variable is the D.C. circuit); *GEND* equals 1 for a male and 0 for a female judge;

black and female Supreme Court Justices (just two of each) to enable meaningful comparisons with other Justices (and virtually no Justices who had been district judges), whereas 8.4 percent of our court of appeals sample (45) consists of female judges and 4.4 percent (24) of black judges, along with a substantial percentage—42.7 percent—of former district court judges. Over the period embraced by the sample, the percentage of court of appeals judges who are former district judges has declined from 46 percent to 32 percent. This decline is not, as one might have thought, due to an increase in the number of female and black judges, who are appointed from a smaller and presumably younger (because until relatively recently there were few black or female federal judges) pool of eligible candidates and so are less likely, one might think, to have been district judges. Of the 41 black or female court of appeals judges serving currently, 37 percent had been district judges, compared to only 30 percent of the white male court of appeals judges.

There are several differences between the Supreme Court and court of appeals regressions. They are due to differences between the two databases that have required us to tabulate our court of appeals vote data in the form of votes per judge over the sample period rather than votes per year and have prevented us from distinguishing between majority and dissenting votes. Also, because the data are sampled (15 cases per court of appeals through 1960 and 30 per court of appeals thereafter), there may be no or only a few cases per judge in any year, which makes it difficult to study the time path of a judge's votes within subject-matter categories.<sup>26</sup> And while an increase in the fraction of conservative votes in the Supreme Court translates into an identical decrease in the fraction of liberal votes because there is no "mixed" vote category in the Supreme Court database, in the court of appeals database such an increase can lead to a decrease in the fraction of mixed votes, of liberal votes, or of both. The biggest effect, however, is

---

*BLACK* equals 1 for black and 0 otherwise; and *DIST* equals 1 if the judge had been a district court judge and 0 otherwise.

<sup>26</sup> Notwithstanding the sampling method for the court of appeals, we plan to do a follow up study that disaggregates the court of appeals data to allow us to tabulate a judge's vote each year.

a decrease in the fraction of liberal votes because, as Table 11 shows, the fraction of mixed votes is only 7.2 percent of all votes (excluding “other” votes).<sup>27</sup>

Table 12 presents our regression results for the courts of appeals. Regressions (1) through (3) are limited to civil cases (*CIV*) and (4) through (6) to criminal cases (*CRIM*). We include independent variables for the fraction of the judge’s votes (excluding criminal) in economic and labor cases (*ECON*), and in the miscellaneous case category (*MIS*), in order to account for differences in the mixture of cases across circuits. We estimated separate regressions for criminal cases because conservative votes in this category (that is, votes against the defendant) are more likely to be a reflection of the lack of merit of these cases than of a judge’s ideological bent. We did not report a similar division in the Supreme Court because the Court’s selectivity means that all categories of cases that it decides are likely to be difficult. Consistent with this suggestion, the fraction of conservative votes in the Supreme Court’s criminal cases is only slightly higher than the fraction of civil cases—for example, the fraction of conservative votes in the criminal-procedure category, .535, is near the midpoint of the range of the other categories, which is .41 to .59 (excluding the tax category, where 34 percent of the votes are conservative).

[Insert Table 12 here]

(1) Consistently with our findings for the Supreme Court, judges appointed by Republican Presidents (*Pres*) are significantly more likely to vote for conservative than for liberal outcomes in both the civil and criminal regressions. This is not surprising because federal court of appeals judges have the same secure tenure as Supreme Court Justices and so we can expect their utility function to contain a self-expression argument. We find no effects of the President’s party on the mixed or other vote categories (regressions (3) and (6)).

(2) As in the Supreme Court regressions, we find that the greater the fraction of Republican senators (*SenRep*) at the

---

<sup>27</sup> To verify this assumption, we estimated separate regressions for the fraction of conservative, mixed, and liberal votes.

time of a judge's confirmation, the more likely the judge is to cast conservative votes and the less likely he is to cast liberal ones. This is true in both the civil and criminal regressions, but the effects are statistically significant only in the former.

(3) The judge-specific variables (*Gend*, *Black*, and *Dist*) are not significant in any regression, with the exception that black appellate judges are significantly less likely to vote conservatively in criminal cases (regression (4)).

(4) Recall that the civil regressions add two variables to account for possible voting differences in constitutional, economic-regulation, and miscellaneous cases. We find a significantly higher fraction of conservative votes in the miscellaneous class as a result of a decline in the fraction of liberal votes, but no change in the fraction of mixed votes, and a significantly higher fraction of liberal votes in the economic-regulation class that is offset by a decline in the fraction of mixed economic votes.

We re-estimated the regressions in Table 12 restricting the sample to judges appointed since 1960. We did this in order to verify the results from the full sample, because there are fewer classification errors in the post-1960 period. We do not reproduce these regressions because they do not differ greatly from the regressions for the full sample. As expected, we are able to explain more of the variance in the data for the later period. Thus, in the regression of the fraction of conservative votes in civil cases, the  $R^2$  is .34 in the 1960–2002 period, compared to .21 for the full sample. The corresponding figures for the criminal cases are .29 in the later period compared to .22 for the full sample. In addition, all t-ratios for the *PRES* variable in the fraction-conservative and fraction-liberal regressions exceed 3 in the 1960–2005 regressions but only one exceeds 3 in the full sample.

The results in the two sets of regressions differ significantly in three respects:

(1) There is no significant effect of the fraction of Republican senators variable in the 1960–2002 regressions, although all coefficients have the predicted positive sign.

(2) In 1960–2002 regressions the coefficient for the year-of-appointment variable is positive and highly significant in civil (though not in criminal) cases, whether the dependent variable

is fraction of conservative votes, fraction of liberal votes, or fraction of mixed votes. In other words, judges appointed more recently (among judges appointed since 1960) are more likely to cast conservative and mixed votes and less likely to cast liberal ones. These results may reflect an increase in the number of ideologically committed conservative judges appointed by Republican Presidents beginning with Reagan. He, followed by his Republican successors, decided to give less weight to the preferences of home-state Senators, whose recommendations often reflect patronage rather than ideological concerns.

To test this hypothesis, we separated the 1960–2002 sample into judges appointed by Republican Presidents (179 judges) and judges appointed by Democratic Presidents (172). The “more committed conservative” hypothesis implies a positive and significant effect of the appointment year in the Republican but not the Democratic sample. Consistent with this hypothesis, the regression coefficient on the appointment variable is positive and highly significant (a t-ratio of 3.88) in the Republican sample but insignificant (a t-ratio of .84) in the Democratic sample.

(3) In contrast to finding (5) in the civil case regression for the full sample, we find no significant effects for the two subject-matter variables in the 1960–2002 regressions. The explanation for this difference may lie in the greater number of classification errors in the full sample. If the errors in classifying the ideological direction of votes in the earlier cases are systematically related to the subject areas, this could produce significant coefficients for the economic and miscellaneous variables in the 1925–2002 but not 1960–2002 regressions.

### *C. Group Effects (Social Influence)*

The circuit dummy variables are jointly significant in all regressions in Table 12, including the re-estimated regressions limited to the 1960–2002 subsample, indicating that there are significant (unexplained) differences among some circuits in the fraction of conservative, liberal, and mixed votes. These results are summarized in Table 13 for both the 1925–2002 and 1960–2002 regressions, where + indicates a positive and significant correlation between the circuit and the fraction of con-

servative (or liberal or mixed) votes and – indicates a negative and significant correlation.

[Insert Table 13]

Although there is no significant circuit effect in Table 13 for most circuits (for example, column (1) indicates that there are no significant differences among the Second, Third, Fourth, Sixth, Ninth, Tenth, Eleventh, and D.C. circuits in the fraction of conservative votes case in civil cases), there is a pronounced effect in the Seventh and Eighth Circuits, and smaller effects in three others (the First, Second, and Fifth).<sup>28</sup> In all five circuits in the civil regressions, the circuit effect makes the circuit more conservative than one would expect on the basis of the judges' ideology as proxied by the party of the President who appointed them and by the fraction of Republican senators at the time of confirmation. Thus, it is not that these are the five most conservative circuits, but that they are more conservative than they "should be" on the basis of the presumed ideology of their judges, a factor that the regression corrects for. We have no explanation for this result.

Another reason to observe circuit effects is social influence, which we discussed with reference to the Supreme Court. We tested three possible social-influence hypotheses—a conformity hypothesis, a group-polarization hypothesis, and political-polarization effect—and found that only the third was supported by the data. We now test these hypotheses in the courts of appeals by examining the voting behavior of the current (as of 2002) active court of appeals judges, plus those who have taken senior status, retired, or resigned since 2000, for they would have interacted extensively with the current active judges. Our sample contains 154 judges, 141 appointed since 1980, with 81 appointed by Republican Presidents, and 75 appointed by Democratic Presidents. The former group has an average tenure of 14.8 years and the latter 10.5 years. Re-

---

<sup>28</sup> Technically, the circuit dummies indicate the difference between each circuit and the D.C. Circuit, which is the excluded dummy variable. This implies that if seven circuits are not significantly different than the D.C. circuit whereas four are, then the latter four are significantly different than the eight other circuits.



stricting the analysis to this sample provides a more powerful test of our two social-influence hypotheses than either the 1925–2002 or 1960–2002 samples. Judges currently active in a circuit (plus other judges appointed after 1960 who were active as of 2000 but not 2002) by definition interact with each other, whereas many of the judges in the same circuit in the 1925–2002 and 1960–2002 samples did not overlap, given the length of time covered by each sample.

Table 14 re-estimates the regressions in Table 12 but is limited to current judges and contains two alternative independent variables for social influence.<sup>29</sup> One is the fraction of judges in each circuit who were appointed by Republican Presidents (*FrRep*); the other is that fraction weighted by the number of years of a judge's service through 2002 (*FrRep\_wtd*). Because the court of appeals judges almost always sit in panels of three judges randomly selected from the judges on the court (though a complicating factor, which we ignore, is that a number of the courts of appeals make extensive use of visiting judges, mainly senior judges), an increase in *FrRep* implies a higher probability that a panel will include two or more judges appointed by Republican Presidents.

Thus, of the 11 judges in the Seventh Circuit in the sample of current judges, Republican Presidents appointed eight and Democratic Presidents three. This implies that the probability that a judge appointed by a Republican President will sit on a panel with at least one or two other judges appointed by a Republican President is .933, while the probability that he will sit on a panel of two judges appointed by Democratic Presidents is only .063. In comparison, a judge appointed by a Democratic President in the seventh circuit has a .62 probability that he

---

<sup>29</sup> With regard to the other variables in Table 14, that is, variables carried over from Table 12, the results are similar to those for the 1960–2005 sample in that table. Thus, judges appointed by Republican Presidents are significantly more likely to vote conservatively; judges appointed by Republican Presidents in their second term are less likely to vote conservatively in civil cases than those appointed in the first term; and the fraction of conservative votes is significantly lower in economic-regulation than in other civil cases. None of the other variables in Table 14 that were carried over from Table 12 is statistically significant, except the second-term Democratic variable in the criminal regression, which has a positive and (barely) significant effect.

will sit on a panel with one or two other judges appointed by a Democratic President.<sup>30</sup> Group effects in the court of appeals thus are panel-composition effects.<sup>31</sup>

The conformity hypothesis is that an increase in the proportion of judges appointed by Republican Presidents will increase the likelihood that any judge in the circuit will cast a conservative vote and that a decrease in that proportion will increase the likelihood of a liberal vote. The group-polarization hypothesis is that judges appointed by Republican Presidents will vote more conservatively in response to an increase in the fraction of the judges on their court appointed by Republican Presidents but that judges appointed by Democratic Presidents will not. The political-polarization hypothesis is that an increase in the size of one of the blocs relative to another will cause the second to vote more antagonistically to the first. We test the first hypothesis by estimating a regression for all 154 judges in the sample. We test the second and third hypotheses by estimating separate regressions for judges appointed by Republican Presidents and judges appointed by Democratic Presidents.

[Insert Table 14 here]

The dependent variable in the first two regressions in Table 14 is the fraction of conservative votes in civil cases and the dependent variable in the third and fourth regressions is the fraction of conservative votes in criminal cases. In contrast to

---

<sup>30</sup> More generally, we can calculate the probabilities of various panel compositions for a given judge as follows. Let N=the number of judges in the circuit; R=the number appointed by Republican Presidents and D=the number appointed by Democratic Presidents.

<i>Panel Make-Up</i>	<i>Judge Appointed by Republican President</i>	<i>Judge Appointed by Democratic President</i>
All R	$(R-1)(R-2)/(N-1)(N-2)$	0
2R & 1D	$[(R-1)(R-2) + 2(R-1)D]/(N-1)(N-2)$	$R(R-1)/(N-1)(N-2)$
1R & 2D	$D(D-1)/(N-1)(N-2)$	$[(D-1)(D-2) + 2(D-1)R]/(N-1)(N-2)$
All D	0	$(D-1)(D-2)/(N-1)(N-2)$

<sup>31</sup> See Cass R. Sunstein et al., *Are Judges Political? An Empirical Analysis of the Federal Judiciary* (2006)

our finding with respect to the Supreme Court, we find in our court of appeals sample a significant increase in the likelihood that a judge will vote conservatively in both civil and criminal cases the greater the fraction of judges appointed by Republican Presidents, regardless of whether the judge in question was appointed by a Republican or a Democratic President.<sup>32</sup> So the conformity hypothesis is supported. The effect is greater in the case of judges appointed by Democratic Presidents than in the case of judges appointed by Republican Presidents. For in the separate civil regressions (not reported in Table 14), the coefficients (t-ratios in parentheses) are .202 (2.68) and .285 (3.08) for judges appointed by Republican and by Democratic Presidents respectively, and in the separate criminal regressions they are .339 (3.20) and .546 (5.11). These results are merely suggestive, however, because we cannot reject the null hypothesis that the effects are equal.

We can estimate the magnitude of the effect of marginally changing the fraction of court of appeals judges appointed by Republican and Democratic Presidents. Suppose in a circuit that has 6 judges appointed by Republican Presidents and 6 appointed by Democratic Presidents, one of the judges that had been appointed by a Democratic President is replaced by a judge appointed by a Republican President—i.e., the *FrRep* variable changes from .5 (6/12) to .5833 (7/12). The mean value of the fraction of conservative votes in civil cases for the average judge will increase from .52 to .54 (.221  $_$  (.5833  $-$  .5)) in civil cases and from .74 to .78 in criminal cases (.438  $_$  (.5833  $-$  .5)). In a ten-year period, assuming an equal number of civil and criminal cases, the average judge would thus cast 20 more conservative votes in civil cases and 40 more conservative votes in criminal cases.

There is also some evidence of group polarization. Remember that an increase in the fraction of judges appointed by a Republican President increases the conservative voting of those

---

<sup>32</sup> Recall that votes in the court of appeals are classified as conservative, mixed or liberal so that an increase in conservative votes does not necessarily imply a decrease in liberal votes. We re-estimated the regressions in Table 14 using the fraction of liberal votes as the dependent variable and found that the greater *FrRep* and *FrRep\_wtd*, the lower the fraction of liberal votes.

judges, not just of the judges appointed by a Democratic President. In other words, the in-group becomes more extreme. There is thus a triple effect of a change in the ideological composition of a court when a member of the minority bloc on the court (say judges appointed by Democratic Presidents) is replaced by a member of the majority bloc: The majority becomes larger and therefore the court becomes more conservative irrespective of any group effects; the members of the majority become more conservative than they were when there were fewer of them; and the minority becomes more docile—more likely to go along with the majority than before. This triple whammy suggests that judicial confirmation battles will be most intense when the appointment would result in enlarging the majority bloc on the court of appeals at the expense of the minority.

So the conformity hypothesis is supported, but so is the group-polarization hypothesis, because the size of the “in group” (judges appointed by Republican Presidents, or judges appointed by Democratic Presidents) has an independent effect on the fraction of conservative (or liberal, as the case may be) votes.

The political-polarization effect, which we found supported by the Supreme Court data, is not supported by the court of appeals data. This is implied by the finding of a conformity effect in the court of appeals but not in the Supreme Court. Given the conformity effect, when one bloc of judges grows at the expense of the other (that is, holding the size of the court constant), the entire court is pushed in the ideological direction of the group that has grown, at least if that group is in the majority. We offer the following possible economic explanation for the difference between the Supreme Court and the courts of appeals regarding the conformity effect. The workload of the courts of appeals is heavier than that of the Supreme Court, because the Supreme Court has a discretionary jurisdiction, which enables it to limit its caseload; the jurisdiction of the courts of appeals is almost entirely mandatory. Especially given leisure preference, the heavier workload in the courts of appeals makes the cost of a dissent greater for court of appeals judges than for Supreme Court Justices. The heavier workload also increases the benefits of decision according to precedent, which greatly reduces the time and effort involved in a deci-

sion; instead of having to analyze the case from the ground up, the court looks for a very similar previous case and decides the new case the same way. So we can expect decision in accordance with precedent to be more valued in the courts of appeals. That reduces the value of a dissent, because the majority vote will establish the precedent and the dissent will usually have no influence on the law. Because decision in accordance with precedent is less important in the Supreme Court, dissents have more influence because future Justices are less inhibited than courts of appeals judges would be about departing from a precedent in favor of a dissent. In addition, self-expression is a more powerful motivator in the Supreme Court because the Court is more powerful than a court of appeals.<sup>33</sup>

Given that a Justice's cost-benefit analysis is likely to favor dissenting more than a court of appeals judge's does, we can expect, as we find, a conformity effect in the courts of appeals but not in the Supreme Court. When a court of appeals is closely divided between liberal and conservative blocs, panels tend to be balanced and a draft of a dissent may swing one of the other judges over to the dissenter's side; and published dissents may signal support for like-minded judges not on the particular panel and provide ideas that may fructify in subsequent cases. The more one-sided the court, the less the value of dissenting. In the limit, in a court of say 10 conservative judges and 1 liberal one, the liberal's dissents would have virtually no impact on the law. Moreover, if he stuck to his liberal guns, consistency might require him to dissent so often that he would be overloaded with work. Hence the conformity effect.

---

<sup>33</sup> "The cases the Court hears tend to arouse strong emotions. And the Justices have a lighter workload than lower-court judges, are more in the public eye and therefore more concerned with projecting a coherent judicial philosophy, and are more likely to influence the law even when dissenting, because of the instability of Supreme Court precedent as a consequence of the greater stakes in the cases that the Court decides and the absence of review by a higher court." Posner, note 3 above, ch. 1. See also James F. Spriggs and Thomas G. Hansford, "Explaining the Overruling of U.S. Supreme Court Precedent," 63 *Journal of Politics* 1091 (2001), finding that the Supreme Court is more likely to overrule a prior nonunanimous decision than a prior unanimous one.

## IV. CONCLUSION

The principal methodological contribution of this paper is the correction of a number of systematic errors in the ideological classification of Supreme Court and court of appeals decisions—errors such as classifying all judicial votes for the plaintiff in an intellectual property case as a liberal outcome—in the Spaeth (1953 to 2000, Supreme Court) and Songer (1925 to 2002, courts of appeals) databases; in addition we identified errors in the Songer database in ideological classification of individual outcomes of cases decided before 1960. The two databases have been used in a large number of previous studies (see Appendix A); our corrections enable more accurate statistical measurements and analyses. The corrections are limited to the systematic errors, but our separate analyses of the post-1960 cases enable us to eliminate most of the individual case errors as well. Another methodological contribution is the study of group effects in a real-world group rather than a group contrived for experimental purposes.

We analyzed the Supreme Court and court of appeals data separately, using where feasible an informal economic model to explain some of our results. With regard to the Supreme Court we found that our ideology measure (the corrected version of the ideological measure in the Spaeth database) corresponds closely though not identically to what “everyone knows” is the ideological rank order of the Justices who served between 1953 and 2002. We also found, consistent with many other studies, that Justices appointed by Republican Presidents vote more conservatively than Justices appointed by Democratic Presidents, with the difference being most pronounced in civil-rights cases and least pronounced in privacy and judicial-power cases. We related to this finding to the “self-expression” argument that we posit in the federal judicial utility function.

We used regression analysis to try to isolate the causes of various aspects of the judicial behavior of Supreme Court Justices, beginning with their ideological voting. We found, for example, that some though by no means all Justices become more conservative and others more liberal during their time on the Court. This “ideology drift” is consistent with the correlation between the appointing President’s party and a Justice’s ideology, because over time issues and party ideologies change. A

challenge for further research is to determine whether it is the Justice's ideology that changes over time or that of the party of the appointing President, or perhaps the ideological character of the cases.

We find no dissent aversion on the part of Supreme Court Justices and therefore no tendency for members of a liberal or conservative minority on the Court to go along with the majority the larger that majority is. That is, we find no conformity effect. Nor do we find a group-polarization effect, though it is notable that we find a political-polarization effect among Justices appointed by Democratic but not by Republican Presidents: The fewer of them there are, the more liberally they vote. This finding is consistent with the proposition that there is more ideological intensity or commitment among conservatives than among liberals, for the more committed a person is to a particular view the less likely he is to be influenced by persons holding other views. (An alternative interpretation, however, is that as the Court becomes more conservative, the majority produces more decisions that the liberal minority disagrees with.) There is further evidence of the greater ideological commitment of presumptively conservative judges in our finding that an increase in the fraction of Republican Senators results in more conservative voting by Justices appointed by Democratic Presidents but that there is no parallel effect of Democratic Senators on Justices appointed by Republican Presidents.

Another finding is that, other things being equal, Justices appointed from the federal courts of appeals vote more liberally than other Justices. We speculate that this is because these Justices have been socialized by their lower-court experience to be respectful of precedent, and the most controversial Supreme Court precedents are those created in the liberal Warren Court era.

Examining nonideological aspects of the Supreme Court's behavior, we found among other things that the number of cases the Court decides is increasing in the fraction of the Court's decisions that reverse the lower court. This makes sense because the greater the disagreement between lower courts and Supreme Court, the more cases the Court must hear in order to enforce its views on the lower courts. This is an ef-

fect of what organizational economists refer to as “management by exception.”

Regarding court of appeals judges, we found among other things that the fraction of conservative votes cast is much higher in the courts of appeals than in the Supreme Court, even for judges appointed by Democratic Presidents. We attribute the difference to a selection effect—the courts of appeals, which have a mandatory rather than a discretionary jurisdiction, decide a great many one-sided cases; the Supreme Court decides more evenly balanced cases because the one-sided ones tend not to present significant issues and the Court’s decisional capacity is very limited relative to the number of lower-court decisions. The difference is especially pronounced in criminal cases. Most criminal appeals are subsidized and lack merit, so that even liberal judges usually vote to affirm; hence the study of ideological influences in the federal courts of appeals is better focused on civil than on all appeals. But the Supreme Court only agrees to hear criminal appeals that have substantial merit.

Perhaps the most interesting finding in our court of appeals regressions is of both a conformity effect and group polarization. Thus there is a triple effect when, holding the size of a court of appeals constant, a judge appointed by a President of one party is replaced by a judge appointed by a President of the other party and the newly appointed judge is part of the majority bloc on the court. If, for example, the majority consists of judges appointed by Republican Presidents, a more conservative judge will replace a less conservative one, the members of the majority bloc will vote more conservatively than when there were fewer of them, and the members of the minority will vote more conservatively than when there were more of them.

We speculate that the difference in conformist behavior between the Supreme Court and the courts of appeals is due to the stronger commitment of the courts of appeals to *stare decisis*, as a result of which a dissent (say by a liberal on a conservative panel) has less effect in those courts on the precedential effect of a decision. With fewer dissents, a conservative (liberal) minority will tend to vote more with the liberal (conservative) majority, or in other words to conform to the majority, which is the conformity effect that we find in the courts of



appeals but not in the Supreme Court. We explain the different role of precedent in the two judicial tiers by reference to differences in costs and benefits resulting mainly from differences in workload pressures.

There is much additional work that could be done to refine our analysis. We suggest just two projects, in closing: The first would be to identify from media accounts court of appeals judges who have had good prospects for promotion to the Supreme Court, based on media speculation, and see whether they dissent more than their peers, or otherwise behave differently, in order to attract attention or otherwise enhance their promotion prospects. The second project would be to use number of amicus curiae briefs filed in Supreme Court cases as proxies for the importance of case, which could be used as a variable to attempt to explain the likelihood of dissent, on the theory that the value of judicial self-expression through dissenting from a decision with which he disagrees is greater, the more important the case.

## APPENDICES

APPENDIX A—PREVIOUS STUDIES BASED ON THE  
SPAETH AND SONGER DATABASES

Stefanie A. Lindquist and Frank B. Cross, "Empirically Testing Dworkin's Chain Novel Theory: Studying the Path of Precedent," 80 *New York University Law Review* 1156 (2005); Susan B. Haire, Stefanie A. Lindquist, and Donald R. Songer, "Appellate Court Supervision in the Federal Judiciary: a Hierarchical Perspective," 37 *Law & Society Review* 143 (2003); Kevin M. Scott, "Supreme Court Reversals of the Ninth Circuit," 48 *Arizona Law Review* 341 (2006); Sara C. Benesh, "The Contribution of 'Extra' Judges," 48 *Arizona Law Review* 301 (2006); Stefanie A. Lindquist, Wendy L. Martinek, and Virginia A. Hettinger, "Splitting the Difference: Modeling Appellate Court Decisions with Mixed Outcomes," 41 *Law & Society Review* 429 (2007); Virginia A. Hettinger, Stefanie A. Lindquist, and Wendy L. Martinek, "Comparing Attitudinal and Strategic Accounts of Dissenting Behavior on the U.S. Court of Appeals," 48 *American Journal of Political Science* 123 (2004); John Szmer, Susan W. Johnson, and Tammy A. Sarver, "Does the Lawyer Matter? Influencing Outcomes on the Supreme Court of Canada," 41 *Law & Society Review* 279 (2007); Susan B. Haire, "Judicial Selection and Decision-making in the Ninth Circuit," 48 *Arizona Law Review* 267 (2006); Rorie Spill Solberg, Jolly A. Emrey, and Susan B. Haire, "Inter-Court Dynamics and the Development of Legal Policy: Citation Patterns in the Decisions of the U.S. Courts of Appeals," 34 *Policy Studies Journal* 277 (2006); Stefanie A. Lindquist, Susan B. Haire, and Donald R. Songer, "Supreme Court Auditing of the U.S. Courts of Appeals: An Organizational Perspective," *Journal of Public Administration Research and Theory* (Advance Access, published online Jan. 11, 2007); Sarah A. Binder and Forrest Maltzman, "Senatorial Delay in Confirming Federal Judges 1947–1998," 46 *American Journal of Political Science* 190 (2002); Gregory C. Sisk, Michael Heise, and Andrew P. Morriss, "Searching for the Soul of Judicial Decisionmaking: An Empirical Study of Religious Freedom Decisions," 65 *Ohio State Law Journal* 491 (2004); Emerson H. Tiller and Frank B. Cross, "A Modest Proposal for Improving American Justice," 99 *Columbia Law Review* 215 (1999); Paul J. Wahlbeck, "The Development of a Legal Rule: The Federal Common Law of Public Nuisance," 32 *Law & Society Review* 613 (1998); Ryan Shoen and Paul J. Wahlbeck, "The Discuss List and Agenda-Setting on the Supreme Court" (paper prepared for presentation at the 2006 Annual Meeting of the Southern Political Science Association);. Tajuana Massie, Susan W. Johnson, and Sara Margaret Gubala, "The Impact of Gender and Race in the Decisions of Judges on the United States Courts of Appeal" (paper prepared for

delivery at the 2002 Midwest Political Science Association Annual Meeting); K. M. Scott, "An Exploration of the Motivation of Court of Appeals Judges" (paper prepared for delivery at the 2003 Midwest Political Science Association Annual Meeting); Kirk A. Randazzo, Richard W. Waterman, and Jeffrey A. Fine, "Statutory Constraint and the Federal Judiciary" (paper presented at the 2004 Annual meeting of the American Political Science Association); S. Haire, S. Lindquist and D. R. Songer, "Hierarchical Relationships in the Federal Courts" (paper prepared for delivery at the 2001 Midwest Political Science Association Annual Meeting); Todd C. Peppers, Katherine O'Harra Vigilante, and Christopher J. W. Zorn, "Sitting by Designation: Ideology and Caseload in District Court Judge Participation on the Federal Courts of Appeals" (paper prepared for presentation at the 1998 Annual Meeting of the American Political Science Association).

## APPENDIX B—THE ORIGINAL SPAETH AND SONGER DATABASES

### I. THE SPAETH DATABASE

The United States Supreme Court Judicial Database, compiled by Harold J. Spaeth, contains data for all Supreme Court decisions in the 1953 through 2006 terms in which at least one Justice wrote an opinion. The data include the Justices' votes in each of the cases plus the identification of the case plus the chronology of the litigation information, outcomes and issues, and information concerning opinions (e.g., whether majority, dissenting, or concurring).

There are three databases concerning the attributes of the Justices of, respectively, the Warren, Burger, and Rehnquist Courts, covering the period 1953–2000. There are separate observations for each Justice, indicating for example where he deviates from the majority in identifying issues or authorities.

### II. THE SONGER DATABASE

The data in the U.S. Courts of Appeals Database (the "Songer" database) include the history of a case, the participants, the issues involved, the resolution, the judges who decided the case, and each judge's vote on a maximum of two issues in the case. The database consists, for the years 1925–1960, of a random sample of 15 cases from each court each year and for the years 1961–2002 of a random sample of 30 cases from each court each year.

We merged the Songer database with what is known as the "Auburn" dataset,<sup>34</sup> which contains attribute data for the judges in the Songer database. Although the two databases were intended to be used together, there were some inconsistencies in the judge identi-

---

<sup>34</sup> See note 4 above.

fication codes. We made the corrections suggested by the Songer documentation and made further corrections as we discovered errors while working with the data. (These corrections are separate from those discussed in Appendix B.) We continue to describe the combined databases as the Songer database.

## APPENDIX C—THE CORRECTED DATABASES

### I. THE SPAETH DATABASE

All observations containing the following codes for the variable ISSUE were altered so that all judges' votes were coded as "other" rather than conservative or liberal. We made these changes on the basis of the description of the type of issue from "The Supreme Court Justice-Centered Judicial Databases: the Warren, Burger, and Rehnquist Courts (1953-2000 Terms): Documentation."

All votes in case types 411 and 444 we changed to other. A vote for the plaintiff on an issue of commercial speech (excluding attorneys) (411) had been coded as liberal; also a vote in favor of requiring accountability in campaign spending (444).

On issues pertaining to unions, votes in case types 555, 563, and 581 were changed because in antitrust cases against unions (555) a pro-competition vote had been coded as liberal, though some liberal judges would put union interests ahead of the competition interest enforced by antitrust law. In disputes between a union and a union member (563), a vote against the union member had been coded as liberal, and in disputes in cases involving jurisdictional disputes between (581), a pro-union vote had been coded as liberal. These seem arbitrary classifications.

On issues pertaining to the propriety of federal judicial review of state court or federal administrative actions, we changed all votes in case types 706, 712, 751, 755, 759, 855, 856, 862–864, 868, 869, and 899 from liberal to other. Originally, votes on any of these issues for judicial power or "activism" were coded as liberal. These case types covered judicial power with respect to obscenity (706), comity and civil procedure (712), determination that a writ was improvidently granted (751), remanding a case to determine the basis of a state court decision (755), miscellaneous no-merits votes (759), jurisdiction or authority of the Court of Claims (855), the Supreme Court's original jurisdiction (856), certification (862), resolution of circuit conflicts (863), objections to reason for denial of certiorari or appeal (864), the Act of State doctrine (868), miscellaneous judicial-administration issues (869), and miscellaneous exercises of judicial power (899). Despite the number of changes that we made in the Spaeth database, Table 2 indicates that the aggregate effect on ideological classification of Supreme Court Justices' votes was less than in the courts of appeals.

## II. THE SONGER DATABASE

We made the following changes to the coding of “vote directionality” (i.e., the ideological valence of the judge’s vote) in the U.S. Courts of Appeals database. All votes in case types 114–118, 134–138, and 154–158 were changed from liberal or conservative to “other”—that is, the outcome cannot be coded as liberal or conservative. According to the database documentation, all votes in criminal cases for the defendant were coded as liberal. We changed moral charges (114, 134, and 154) to “other” because they could include child pornography, an issue on which neither liberal nor conservative judges would be likely to be sympathetic to the defendant. The other changes were economic crimes (violations of government regulations of business, other white collar crime, and other crimes), in which a liberal judge would tend to favor the government rather than the defendant.

Votes for the plaintiff in non-employment-related sex discrimination cases brought by a man (235), classified by the original coders as liberal, we changed to “other” because the category could include cases in which the plaintiff was harassed by another man because he (the plaintiff) was homosexual or suspected of being homosexual, and in such a case a liberal judge would tend to favor the plaintiff, perhaps also if the man were alleging harassment by a woman. Suits charging on race or sex discrimination in employment (239) we also changed from liberal to “other” because like the closely related category 235, category 239 is too broad from an ideological standpoint; it could (and doubtless does) include claims of discrimination by whites and men against blacks and women in cases of affirmative action.

Commercial speech cases (301) had been coded so that a vote for the broadest interpretation of First Amendment protection was liberal. We changed this to “other” because businesses typically assert claims of commercial speech. In obscenity cases (307), where again a vote for the broadest interpretation of First Amendment protection had been coded as liberal, we changed the coding to “other” because some liberals (especially feminists) disapprove of obscenity and, as with criminal charges, few judges would have sympathy for a First Amendment claim based on child pornography, which is classified under obscenity.

In the original database, case type 412 was described as a claimed “denial of due process under the ‘taking’ clause of the Fifth Amendment,” and a vote for the plaintiff was coded as liberal. Yet in case type 771—eminent domain disputes with government—a vote for the government is coded as liberal. Due process is not mentioned in the taking clause, so case type 412 is mysterious and we thought it better to shift the votes in it to the “other” category.

Votes for plaintiffs in the general category of labor relations were changed from liberal to other if the parties involved were on one side (1) the executive branch of the government and on the other side (2) a union or an individual. This changed all the votes in case

types 600–699. We made a similar change for all votes in categories 710–713, which cover copyrights, patents, trademarks, trade secrets, and personal intellectual property. All votes for the plaintiffs in such cases had been coded liberal. Yet an intellectual property case often is brought by a large firm against a small firm or an individual, as when a giant pharmaceutical company sues the manufacturer of a generic drug or a large record company sues a file-sharing college student to make an example of him.

We made a similar reclassification of votes in case types 773–774, which involve the government’s seizure of property either as an incident to the enforcement of criminal statutes (773) or in civil cases (774). All votes for the government in these categories had been coded liberal, but because the party whose property is seized could either be wealthy or poor, the ideological classification was overbroad.

Votes in all cases in type 903 we likewise changed from liberal to other. This case type is described only as “attorneys (disbarment, etc.).” There is no reason to expect a liberal judge to favor a lawyer in a disbarment proceeding.

Votes in all cases in types 905, 906, and 920 were changed to other, as the issues in these cases—challenges to the authority of a magistrate or bankruptcy judge, and international law—do not lend themselves to being categorized on ideological lines.

We recoded the cases in type 921, government regulation of immigration, from liberal to conservative. A vote for government regulation had been coded as liberal, but a liberal judge would be likely to support the rights of the immigrant against the government rather than vice versa.

TABLE 1  
VOTES BY SUPREME COURT JUDGES IN NONUNANIMOUS CASES:  
1937–2006

	<i>Civil Liberties</i>	<i>Economic/labor/tax</i>	<i>All</i>
<i>Uncorrected</i>			
<i>Total</i>	41,032	19,438	60,470
<i>Other</i>	184	13	197
<i>Corrected</i>			
<i>Total</i>	39,228	18,936	58,164
<i>Other</i>	2,004	506	2,510

*Notes:* (1) Civil liberties includes criminal procedure, civil rights, first amendment, due process, privacy, attorneys, federalism and judicial power.  
(2) In both the corrected and uncorrected data, we excluded several hundred miscellaneous and interstate relations votes, because, in the former, we could not classify the subject matter, and, in the latter, because the ideological direction of the votes are indeterminate.

TABLE 2  
VOTES BY FEDERAL COURT OF APPEALS JUDGES 1925–2002

	<i>Criminal</i>	<i>Constitutional</i>	<i>Econ/labor</i>	<i>Miscellaneous</i>	<i>All</i>
<i>Uncorrected</i>					
<i>Total</i>	16677	8234	30849	1737	57497
<i>Other</i>	65	117	5081	503	5766
<i>Corrected</i>					
<i>Total</i>	10721	7849	28375	1216	48161
<i>Other</i>	6024	502	7561	1058	15145

*Notes:* (1) Total is net of votes that are not classified ideologically (“other”).  
(2) In the corrected data, we are not able to classify the subject matter for 492 votes.

**TABLE 3**  
**FRACTION OF CONSERVATIVE VOTES IN NONUNANIMOUS CASES: 1937–2006 TERMS**  
**FOR 43 SUPREME COURT JUSTICES RANKED FROM MORE TO LESS CONSERVATIVE**

<i>Justice</i>	<i>Fraction Conservative Votes<sup>1</sup></i>				<i>Mean Votes Per Term</i>	<i>Other Ideology Measures<sup>5</sup></i>		
	<i>All</i>	<i>Civ. Lib</i>	<i>Econ</i>	<i>Adj. Civ Lib.</i>		<i>S/C Score<sup>2</sup></i>	<i>M/Q Score<sup>3</sup></i>	<i>Adj .Civ Lib.<sup>4</sup></i>
Thomas	.822	.841	.751	.884	52.69	.840	3.65	.765
Rehnquist	.815	.864	.630	.891	87.09	.955	2.77	.774
Scalia	.757	.791	.625	.820	65.57	1	2.57	.724
Roberts	.753	.767	.700	.804	46.5	.880	1.54	
Alito	.740	.754	.688	.860	36.5	.900	1.46	
Burger	.735	.771	.607	.790	118.29	1	1.79	.711
O'Connor	.680	.687	.653	.709	83.75	.585	.86	.632
Powell	.677	.694	.609	.700	106.81	.835	.91	.627
Whittaker	.673	.682	.660	.696	79.50	.500	1.22	.562
Kennedy	.647	.671	.556	.707	59.0	.635	.80	.623
Harlan	.628	.656	.560	.649	100.12	.125	1.59	.533
Vinson	.613	.693	.510	.723	83.86	.250	.97	.634
Burton	.587	.669	.482	.673	84.64	.720	1.00	.614
Minton	.587	.710	.412	.717	68.63	.280	1.04	.624
White	.556	.605	.384	.606	109.88	.500	.43	.575
Stewart	.555	.557	.549	.529	115.17	.25	.55	.486
Jackson	.546	.594	.499	.612	87.25	0	.71	.585
Clark	.534	.651	.332	.668	91.11	.500	.47	.562
McReynolds	.520	.550	.505	.463	101.00	-	2.55	-
Frankfurter	.512	.571	.453	.516	92.125	.335	.52	.465
Roberts, O.	.505	.546	.482	.535	112.13	-	1.55	-
Sutherland	.500	.429	.522	.500	30.00	-	1.96	-
Blackmun	.492	.504	.446	.503	102.40	.885	-.03	.470
Butler	.481	.531	.460	.429	134.00	-	1.90	
Reed	.467	.617	.340	.631	92.65	.275	.35	.639
Stone	.384	.508	.316	.451	117.33	.700	-.07	-
Byrnes	.383	.523	.296	.577	115.00	.670	-.18	



**TABLE 3 (CONTINUED)**  
**FRACTION OF CONSERVATIVE VOTES IN NONUNANIMOUS CASES: 1937–2006 TERMS**  
**FOR 43 SUPREME COURT JUSTICES RANKED FROM MORE TO LESS CONSERVATIVE**

<i>Justice</i>	<i>Fraction Conservative Votes<sup>1</sup></i>				<i>Mean Votes Per Term</i>	<i>Other Ideology Measures<sup>5</sup></i>		
	<i>All</i>	<i>Civ. Lib</i>	<i>Econ</i>	<i>Adj. Civ Lib.</i>		<i>S/C Score<sup>2</sup></i>	<i>M/Q Score<sup>3</sup></i>	<i>Adj .Civ Lib.<sup>4</sup></i>
Hughes	.378	.510	.322	.395	120.50	-	.10	-
Souter	.374	.358	.433	.357	54.59	.675	-.82	.371
Brandeis	.373	.492	.323	.412	110.00	-	-.50	-
Breyer	.372	.355	.446	.359	50.31	.525	-1.15	.387
Stevens	.341	.325	.399	.302	79.47	.75	-1.56	.347
Fortas	.336	.335	.341	.195	107	.155	-1.13	.179
Cardozo	.333	.800	.211	.800	24	-	-1.68	-
Ginsburg	.312	.308	.324	.302	51.36	.320	-1.29	.337
Warren	.308	.334	.257	.263	103.63	.25	-1.12	.213
Black	.283	.354	.190	.300	105.09	.125	-1.70	.259
Brennan	.265	.249	.312	.184	113.41	0	-1.87	.203
Goldberg	.248	.209	.341	.110	100.67	.25	-.75	.112
Rutledge	.247	.270	.227	.246	93.29	0	-1.34	.237
Murphy	.241	.292	.203	.195	96.40	0	-1.52	.209
Douglas	.213	.187	.253	.139	98.08	.270	-4.07	.113
Marshall	.211	.186	.305	.133	109.50	0	-2.72	.186

<sup>1</sup> Fraction Conservative Votes are weighted by the number of cases the Justice voted on in each term in each category. Civil Liberties includes criminal procedure, civil rights, first amendment, due process, attorney, federalism and judicial power. Economics includes economic, union and tax cases. Adjusted Civil Liberties category excludes federalism and judicial power from the broader Civil Liberties category.

<sup>2</sup> S/C Perceived ideology of Justices prior to appointment is from Jeffrey Segal and Albert Cover, "Ideological Values and the Votes of Supreme Court Justices," *Amer. Political Science Rev.* 83: 557-565 (1989) and updated in Table 6-1 in Lee Epstein et. al. *The Supreme Court Compendium: 4<sup>th</sup> Edition* (2007).

<sup>3</sup> M/Q Score is yearly average of posterior mean scores from Andrew D. Martin and Kevin M. Quinn, "Dynamic Ideal Point Estimation via Markov Chain Monte Carlo for the U.S. Supreme Court, 1953–1999," 10 *Political Analysis* 134 (2002). The data are available thru 2006 at [mqscores.wustel.edu/index](http://mqscores.wustel.edu/index).

<sup>4</sup> Votes in the adjusted civil liberties category for the 1946–2004 terms are from Lee Epstein et al., *The Supreme Court Compendium* (2007) Table 6–4.

<sup>5</sup> We converted the S/C and Epstein I estimates from liberal to conservative ideologies to facilitate comparison with the estimates presented in the first four columns.



**TABLE 5**  
**FRACTION OF CONSERVATIVE VOTES IN NONUNANIMOUS CASES BY**  
**SUBJECT MATTER AND BY POLITICAL PARTY OF APPOINTING PRESIDENT:**  
**1937–2006 TERMS**

<i>Case Category</i>	<i>Proportion of Conservative Votes</i>			<i>Ratio</i>	<i>No. Votes</i>
	<i>All</i>	<i>Judges Appointed by Republican President</i>	<i>Judges Appointed by Democratic President</i>		
Criminal Procedure	.535	.603***	.436	1.38	12980
Civil Rights	.466	.549***	.338	1.62	8678
First Amendment	.454	.507***	.387	1.31	4522
Due Process	.450	.531***	.346	1.53	2211
Privacy	.578	.589	.545	1.08	583
Attorneys	.469	.509*	.389	1.31	605
Unions	.423	.534***	.337	1.58	2382
Economic Activity	.405	.485***	.337	1.44	13217
Judicial Power	.593	.625***	.558	1.12	7054
Federalism	.445	.480**	.401	1.20	2595
Federal Taxation	.344	.389*	.314	1.24	3337
All Categories	.473	.544***	.391	1.39	58165
Civil Liberties	.510	.570***	.428	1.33	39228
Economic Regulation	.397	.475***	.333	1.43	18936

*Note:* \* significant at .05; \*\*significant at the .01; and \*\*\*significant at the .001 level

**TABLE 6**  
**DEFINITION AND MEANS OF VARIABLES**  
**IN SUPREME COURT REGRESSIONS: 1937–2006**

<i>Variable</i>	<i>Definition</i>	<i>Mean</i>
<i>FrCon</i>	Fraction of conservative votes in all non-unanimous decisions	.472
<i>ID</i>	Segal-Cover perceived ideology from a content analysis of newspaper editorials	.467
<i>Pres</i>	1=Republican appointee; 0=Democratic appointee	.535
<i>SenRep</i>	Fraction of Republican senators at time of initial appointment	.430
<i>Resid</i>	Residual or unexplained ideology	0
<i>Term</i>	Term of court or time trend variable	1971.5
<i>YrAppt</i>	Term or year of Judge's appointment to the Supreme Court	1956
<i>AppCt</i>	1=federal appellate judge prior to appointment; 0 otherwise	.349
<i>SCRep</i>	Fraction of other judges appointed by Republican presidents	.549

*Note:* The mean for *FrCon* is weighted by the number of votes per judge per term in that category and the mean for *SCRep* is weighted by the number of terms of each judge. The means for the remaining variables (except for the term variable which is the midpoint between 1937 and 2006) are the averages for the 43 judges in our sample.

**TABLE 7**  
**REGRESSION ANALYSIS OF SUPREME COURT VOTES IN**  
**NONUNANIMOUS CASES: 1937–2006 TERMS**  
*(t-statistics in parentheses)*

<i>Independent Variables</i>	<i>Dependent Variable</i>			
	<i>Segal-Cover Score</i>	<i>Fraction of Conservative Votes in Non-Unanimous Cases</i>		
	(1)	(2) <i>All judges</i>	(3) <i>Rep. appointed</i>	(4) <i>Dem.-appointed</i>
<i>Pres</i>	.247* (2.15)	.076*** (4.04)	-	-
<i>SenRep</i>	.071 (0.13)	.431*** (4.09)	1.273*** (5.80)	.668*** (4.12)
<i>Residual</i>		.348*** (13.39)	.314*** (9.56)	.323*** (4.51)
<i>Term</i>		-.002* (2.60)	-.007*** (5.48)	.001 (0.95)
<i>YrAppt</i>	.006* (2.38)	.007*** (6.50)	.012*** (9.26)	.001 (0.90)
<i>AppCt</i>	-.040 (0.43)	-.068*** (4.12)	-.040 (1.90)	-.084* (2.43)
<i>SCRep</i>		-.166*** (3.68)	.015 (0.14)	-.194*** (3.52)
<i>Constant</i>	-10.85 (2.37)	-8.115*** (4.59)	-8.959*** (3.57)	-4.900* (2.19)
$R^2$	.43	.46	.45	.39
N	36	614	332	282

*Notes:* (a) Regressions (2)–(4) are weighted regressions where each observation (a judge-term) is weighted by the number of votes the judge casts per term. (b) Since Segal-Cover scores are only available for 36 judges appointed on or after 1937 (except for Stone appointed in 1925), we excluded votes for 7 judges appointed before 1937 who cast votes in the 1937–2000 period. The excluded judges only account for 22 observations because most were no longer on the court after 1940. (c) \*significant at .05; \*\*significant at .01; and \*\*\*significant at .001 level.

**TABLE 8**  
**DEFINITION AND MEANS OF VARIABLES FOR**  
**SUPREME COURT REGRESSIONS; 1937–2004**  
**(All Variables Are Per Court Term)**

<i>Variable</i>	<i>Definition</i>	<i>Mean</i>
<i>Dissent</i>	Fraction of Cases with Dissenting Opinion	.584
<i>Concur</i>	Fraction of Cases with Concurring Opinion	.348
<i>OneVote</i>	Fraction of Cases Decided by a One-Vote Margin	.152
<i>Reverse</i>	Fraction of Cases in which Appellant Pre- vailed	.619
<i>Med_Judge</i>	Fraction of Conservative Votes of Median Justice	.528
<i>Range</i>	Difference between Justice with Maximum and Minimum Fraction of Conservative Votes	.560
<i>New</i>	Number of New Justices	.5
<i>Service</i>	Average years of Service of Justices on the Court	12.04
<i>Term</i>	Term or Time Trend Variable (1937–2004)	1972.5
<i>Cases</i>	Number of Cases Decided by the Court	119.5

**TABLE 9**  
**REGRESSION ANALYSIS OF SUPREME COURT CASES**  
**1937–2004 TERMS**  
*(t-statistics in parentheses)*

*Dependent Variables*

<i>Independent Variables</i>	<i>Dissent (1)</i>	<i>Concur (2)</i>	<i>One-Vote (3)</i>	<i>Reverse (4)</i>
<i>Med_Judge</i>	.243 (2.11)*	-.013 (1.60)	.166** (2.88)	-.468 (6.45)***
<i>Range</i>	.308 (2.84)**	-.024 (0.37)	.235 (5.10)***	.116 (1.37)
<i>New</i>	-.018 (0.89)	-.003 (0.18)	-.005 (0.52)	-.003 (0.22)
<i>Term</i>	-.005 (3.83)***	.005 (6.28)***	.000 (0.08)	.001 (1.30)
<i>Service</i>	-.006 (0.21)	.001 (0.17)	-.0005 (0.19)	.006 (1.27)
<i>Cases</i>	-.001 (2.90)**	.001 (2.98)**	-.000 (0.33)	.001 (1.22)
<i>Concur</i>	.707 (4.79)***	—	—	
<i>Dissent</i>	—	.445 (6.18)***	—	
<i>Constant</i>	9.998*** (4.02)	-9.916*** (6.44)	-.168 (0.15)	-1.783 (0.98)
<i>R<sup>2</sup></i>	.57	.73	.59	.44
<i>N</i>	68	68	68	59

Notes: \*significant at .05; \*\*significant at .01; and \*\*\*significant at .001 level

**TABLE 10**  
**COURT OF APPEALS VOTES BY SUBJECT MATTER AND IDEOLOGY FOR 537**  
**JUDGES: 1925–2002**

	<i>Crim</i>	<i>Civ Rts</i>	<i>First</i>	<i>Due- Proc- ess</i>	<i>Priv</i>	<i>Econ</i>	<i>Labor</i>	<i>Misc</i>	<i>Total</i>
<i>Conservative</i>	7840	3106	604	493	135	10906	1608	585	25277
<i>Liberal</i>	2188	2039	519	226	68	11489	2273	606	19408
<i>Mixed</i>	693	487	101	57	14	1628	471	25	3476
<i>Other</i>	6024	294	115	90	3	7368	193	1058	15145
<i>Total</i>	16745	5926	1339	866	220	31391	4545	2274	63306

*Note:* We have excluded 482 votes the subject matter of which could not be determined.



TABLE 11

Category	Harrison, McKinley, T. Roosevelt & Taft			Wilson			Harding, Coolidge & Hoover			Roosevelt & Truman			Eisenhower					
	M	C	L	M	C	L	M	C	L	M	C	L	M	C	L			
All	.028	.496	.476	.03	.504	.463	.056	.492	.453	.044	.475	.482	.059	.541	.399			
Civil	.019	.462	.519	.032	.478	.490	.054	.462	.484	.044	.431	.526	.064	.472	.465			
Criminal	.091	.742	.167	.041	.732	.228	.064	.747	.188	.043	.638	.218	.046	.766	.188			
Constitutional	0	.684	.316	0	.806	.194	.018	.635	.347	.007	.649	.344	.048	.604	.347			
Econ. & Labor	.021	.439	.540	.033	.458	.509	.058	.447	.495	.047	.415	.538	.069	.448	.484			
Number Tenure	11 26.5			17 17.1			38 15.3			76 14.1			45 12.3					
Category	Kennedy/ Johnson			Nixon/Ford			Carter			Reagan			Bush			Clinton		
	M	C	L	M	C	L	M	C	L	M	C	L	M	C	L	M	C	L
All	.070	.494	.436	.085	.543	.372	.096	.507	.400	.098	.599	.303	.094	.627	.279	.084	.535	.3
Civil	.076	.422	.502	.091	.470	.439	.100	.452	.449	.105	.534	.361	.104	.566	.329	.094	.478	.4
Criminal	.055	.689	.255	.065	.760	.175	.091	.672	.237	.082	.767	.151	.068	.778	.154	.059	.684	.2
Constitutional	.080	.480	.440	.092	.551	.357	.099	.489	.413	.099	.601	.300	.09	.624	.277	.107	.503	.3
Econ. & Labor	.078	.407	.515	.092	.439	.469	.103	.429	.467	.111	.499	.390	.112	.528	.360	.087	.460	.4
Number Tenure	60 14.9			57 13.5			56 16.7			77 14.6			37 12.2			59 8.3		

(3) \*significant at .05; \*\*significant at .01; and \*\*\*significant at .001 level.

**TABLE 13**  
**CIRCUIT EFFECTS ON IDEOLOGY OF JUDGES' VOTES**

	<i>Civil Cases</i>						<i>Criminal Cases</i>					
	<i>Fraction</i>						<i>Fraction</i>					
Circuit	<i>Conser- vative</i>		<i>Liberal</i>		<i>Mixed</i>		<i>Conser- vative.</i>		<i>Liberal</i>		<i>Mixed</i>	
1st	+	+	–	–								
2d			–	–	+	+		–			+	
3d							–	–		+		
4th		+		–				–		+		
5th	+	+	–	–								
6th								–		+		
7th	+	+	–	–	–	–	+	+	–		–	–
8th	+	+	–	–			+	+				–
9th								–		+		
10th						–						
11th												
D.C.												

*Notes:* (1) Shaded columns denote 1925–2002 regressions and unshaded columns denote 1925–2002 regressions.  
(2) The D.C. circuit is the excluded variable.  
(3) A positive (negative) sign denotes a significant positive (negative) effect compared to the D.C. circuit.  
(4) The absence of a sign for a circuit indicates that there is no significant difference between that circuit and the D.C. circuit.

**TABLE 14**  
**REGRESSION ANALYSIS OF APPELLATE COURT VOTES:**  
**CURRENT JUDGES**  
*(t-statistics in parentheses)*

Independent Variables	Dependent Variables			
	Civil Cases		Criminal Cases	
	Fraction Conservative (1)	Fraction Conservative (2)	Fraction Conservative (3)	Fraction Conservative (4)
<i>Pres</i>	.092*** (3.47)	.092** (3.38)	.092** (3.23)	.092** (2.97)
<i>SenRep</i>	-.014 (0.07)	.021 (0.10)	.516* (2.24)	.536* (2.22)
<i>YrAppt</i>	.0004 (0.26)	.0004 (0.25)	-.0003 (0.16)	-.0002 (0.09)
<i>Gender</i>	.025 (0.84)	.030 (0.96)	.018 (0.62)	.027 (0.84)
<i>Black</i>	.001 (0.29)	.015 (0.46)	.003 (0.07)	.009 (0.19)
<i>District Court</i>	-.016 (0.74)	-.015 (0.70)	.022 (0.82)	.024 (0.88)
<i>Fraction Economic</i>	-.266** (2.64)	-.297** (2.95)	-	-
<i>Fraction Miscellaneous</i>	-.378 (0.66)	-.464 (0.79)	-	-
<i>FrRep</i>	.221** (3.32)	-	.438*** (5.54)	-
<i>FrRep_wtd</i>	-	.152* (2.57)	-	.332*** (4.90)
<i>Constant</i>	-.458 (0.13)	-.427 (0.12)	.713 (0.21)	.492 (0.14)
<i>R<sup>2</sup></i>	.24	.21	.36	.31
No. Observations	154	154	151	151
<i>Notes:</i> 1) All regressions are weighted by the judge's total votes in civil cases (equations (1)-(2) or criminal cases (equations (3)-(4)). 2) *Significant at .05; **significant at .01; and ***significant at the .001 level.				

Readers with comments should address them to:

Professor William M. Landes  
University of Chicago Law School  
1111 East 60th Street  
Chicago, IL 60637  
[wlandes@uclaw.uchicago.edu](mailto:wlandes@uclaw.uchicago.edu)

## Chicago Working Papers in Law and Economics (Second Series)

For a listing of papers 1–299 please go to Working Papers at  
<http://www.law.uchicago.edu/Lawecon/index.html>

300. Adam B. Cox, The Temporal Dimension of Voting Rights (July 2006)
301. Adam B. Cox, Designing Redistricting Institutions (July 2006)
302. Cass R. Sunstein, Montreal vs. Kyoto: A Tale of Two Protocols (August 2006)
303. Kenneth W. Dam, Legal Institutions, Legal Origins, and Governance (August 2006)
304. Anup Malani and Eric A. Posner, The Case for For-Profit Charities (September 2006)
305. Douglas Lichtman, Irreparable Benefits (September 2006)
306. M. Todd Henderson, Paying CEOs in Bankruptcy: Executive Compensation when Agency Costs Are Low (September 2006)
307. Michael Abramowicz and M. Todd Henderson, Prediction Markets for Corporate Governance (September 2006)
308. Randal C. Picker, Who Should Regulate Entry into IPTV and Municipal Wireless? (September 2006)
309. Eric A. Posner and Adrian Vermeule, The Credible Executive (September 2006)
310. David Gilo and Ariel Porat, The Unconventional Uses of Transaction Costs (October 2006)
311. Randal C. Picker, Review of Hovenkamp, The Antitrust Enterprise: Principle and Execution (October 2006)
312. Dennis W. Carlton and Randal C. Picker, Antitrust and Regulation (October 2006)
313. Robert Cooter and Ariel Porat, Liability Externalities and Mandatory Choices: Should Doctors Pay Less? (November 2006)
314. Adam B. Cox and Eric A. Posner, The Second-Order Structure of Immigration Law (November 2006)
315. Lior J. Strahilevitz, Wealth without Markets? (November 2006)
316. Ariel Porat, Offsetting Risks (November 2006)
317. Bernard E. Harcourt and Jens Ludwig, Reefer Madness: Broken Windows Policing and Misdemeanor Marijuana Arrests in New York City, 1989–2000 (December 2006)
318. Bernard E. Harcourt, Embracing Chance: Post-Modern Meditations on Punishment (December 2006)
319. Cass R. Sunstein, Second-Order Perfectionism (December 2006)
320. William M. Landes and Richard A. Posner, The Economics of Presidential Pardons and Commutations (January 2007)
321. Cass R. Sunstein, Deliberating Groups versus Prediction Markets (or Hayek's Challenge to Habermas) (January 2007)
322. Cass R. Sunstein, Completely Theorized Agreements in Constitutional Law (January 2007)
323. Albert H. Choi and Eric A. Posner, A Critique of the Odious Debt Doctrine (January 2007)
324. Wayne Hsiung and Cass R. Sunstein, Climate Change and Animals (January 2007)
325. Cass R. Sunstein, Cost-Benefit Analysis without Analyzing Costs or Benefits: Reasonable Accommodation, Balancing and Stigmatic Harms (January 2007)
326. Cass R. Sunstein, Willingness to Pay versus Welfare (January 2007)
327. David A. Weisbach, The Irreducible Complexity of Firm-Level Income Taxes: Theory and Doctrine in the Corporate Tax (January 2007)
328. Randal C. Picker, Of Pirates and Puffy Shirts: A Comments on "The Piracy Paradox: Innovation and Intellectual Property in Fashion Design" (January 2007)
329. Eric A. Posner, Climate Change and International Human Rights Litigation: A Critical Appraisal (January 2007)
330. Randal C. Picker, Pulling a Rabbi Out of His Hat: The Bankruptcy Magic of Dick Posner (February 2007)
331. Bernard E. Harcourt, Judge Richard Posner on Civil Liberties: Pragmatic (Libertarian) Authoritarian (February 2007)

332. Cass R. Sunstein, If People Would Be Outraged by Their Rulings, Should Judges Care? (February 2007)
333. Eugene Kontorovich, What Standing Is Good For (March 2007)
334. Eugene Kontorovich, Inefficient Customs in International Law (March 2007)
335. Bernard E. Harcourt, From the Asylum to the Prison: Rethinking the Incarceration Revolution. Part II: State Level Analysis (March 2007)
336. Cass R. Sunstein, Due Process Traditionalism (March 2007)
337. Adam B. Cox and Thomas J. Miles, Judging the Voting Rights Act (March 2007)
338. M. Todd Henderson, Deconstructing Duff & Phelps (March 2007)
339. Douglas G. Baird and Robert K. Rasmussen, The Prime Directive (April 2007)
340. Cass R. Sunstein, Illusory Losses (May 2007)
341. Anup Malani, Valuing Laws as Local Amenities (June 2007)
342. David A. Weisbach, What Does Happiness Research Tell Us about Taxation? (June 2007)
343. David S. Abrams and Chris Rohlf, Optimal Bail and the Value of Freedom: Evidence from the Philadelphia Bail Experiment (June 2007)
344. Christopher R. Berry and Jacob E. Gersen, The Fiscal Consequences of Electoral Institutions (June 2007)
345. Matthew Adler and Eric A. Posners, Happiness Research and Cost-Benefit Analysis (July 2007)
346. Daniel Kahneman and Cass R. Sunstein, Indignation: Psychology, Politics, Law (July 2007)
347. Jacob E. Gersen and Eric A. Posner, Timing Rules and Legal Institutions (July 2007)
348. Eric A. Posner and Adrian Vermeule, Constitutional Showdowns (July 2007)
349. Lior Jacob Strahilevitz, Privacy versus Antidiscrimination (July 2007)
350. Bernard E. Harcourt, A Reader's Companion to Against Prediction: A Reply to Ariela Gross, Yoram Margalioth, and Yoav Sapir on Economic Modeling, Selective Incapacitation, Governmentality, and Race (July 2007)
351. Lior Jacob Strahilevitz, "Don't Try This at Home": Posner as Political Economist (July 2007)
352. Cass R. Sunstein, The Complex Climate Change Incentives of China and the United States (August 2007)
353. David S. Abrams and Marianne Bertrand, Do Judges Vary in Their Treatment of Race? (August 2007)
354. Eric A. Posner and Cass R. Sunstein, Climate Change Justice (August 2007)
355. David A. Weisbach, A Welfarist Approach to Disabilities (August 2007)
356. David S. Abrams, More Time, Less Crime? Estimating the Deterrent Effect of Incarceration using Sentencing Enhancements (August 2007)
357. Stephen J. Choi, G. Mitu Gulati and Eric A. Posner, Professionals or Politicians: The Uncertain Empirical Case for an Elected Rather than Appointed Judiciary (August 2007)
358. Joseph Bankman and David A. Weisbach, Consumption Taxation Is Still Superior to Income Taxation (September 2007)
359. Douglas G. Baird and M. Todd Henderson, Other People's Money (September 2007)
360. William Meadow and Cass R. Sunstein, Causation in Tort: General Populations vs. Individual Cases (September 2007)
361. Richard McAdams and Janice Nadler, Coordinating in the Shadow of the Law: Two Contextualized Tests of the Focal Point Theory of Legal Compliance (September 2007)
362. Richard McAdams, Reforming Entrapment Doctrine in *United States v. Hollingsworth* (September 2007)
363. M. Todd Henderson, From *Seriatim* to Consensus and Back Again: A Theory of Dissent (October 2007)
364. Timur Kuran and Cass R. Sunstein, Availability Cascades and Risk Regulation (October 2007)
365. David A. Weisbach, The Taxation of Carried Interests in Private Equity (October 2007)
366. Lee Anne Fennell, Homeownership 2.0 (October 2007)
367. Jonathan R. Nash and Rafael I. Pardo, An Empirical Investigation into Appellate Structure and the Perceived Quality of Appellate Review (October 2007)
368. Thomas J. Miles and Cass R. Sunstein, The Real World of Arbitrariness Review (November 2007)
369. Anup Malani, Maciej F. Boni, Abraham Wickelgren, and Ramanan Laxminarayan, Controlling Avian Influenza in Chickens (November 2007)
370. Richard H. McAdams, The Economic Costs of Inequality (November 2007)

371. Lior Jacob Strahilevitz, Reputation Nation: Law in an Era of Ubiquitous Personal Information (November 2007)
372. Thomas J. Miles and Cass R. Sunstein, The New Legal Realism (December 2007)
373. M. Todd Henderson, Everything Old Is New Again: Lessons from *Dodge V. Ford Motor Company* (December 2007)
374. Jonathan Remy Nash, Economic Efficiency versus Public Choice: The Case of Property Rights in Road Traffic (December 2007)
375. Edward L. Glaeser and Cass R. Sunstein, Extremism and Social Learning (December 2007)
376. Stephen J. Choi, G. Mitu Gulati, and Eric A. Posner, Are Judges Overpaid?: A Skeptical Response to the Judicial Salary Debate (December 2007)
377. Eric A. Posner, Does Political Bias in the Judiciary Matter?: Implications of Judicial Bias Studies for Legal and Constitutional Reform (January 2008)
378. Susan Bandes, The Heart Has Its Reasons: Examining the Strange Persistence of the American Death Penalty (January 2008)
379. Susan Bandes, After Innocence: Framing Wrongful Convictions (January 2008)
380. Jacob E. Gersen and Ann Joseph O'Connell, Deadlines in Administrative Law (January 2008)
381. Richard A. Epstein, The Property Rights Movement and Intellectual Property (January 2008)
382. Richard A. Epstein, Some Reflections on Custom in the IP Universe (January 2008)
383. Richard A. Epstein, Decentralized Responses to Good Fortune and Bad Luck (January 2008)
384. Richard A. Epstein, How to Create—or Destroy—Wealth in Real Property (January 2008)
385. Richard A. Epstein, The Human and Economic Dimensions of Altruism: The Case of Organ Transplantation (January 2008)
386. Cass R. Sunstein, Adolescent Risk-Taking and Social Meaning: A Commentary (January 2008)
387. Cass R. Sunstein and Adrian Vermeule, Conspiracy Theories (January 2008)
388. Ariel Porat, Expanding Restitution: Liability for Unrequested Benefits (January 2008)
389. Randal C. Picker, *Twombly*, *Leegin* and the Reshaping of Antitrust (February 2008)
390. Alon Harrel and Ariel Porat, Aggregating Probabilities across Offences in Criminal Law (March 2008)
391. John Bronsteen, Christopher Buccafusco, and Jonathan Masur, Hedonic Adaptation and the Settlement of Civil Lawsuits (March 2008)
392. Randal C. Picker, Fair Use v. Fair Access (March 2008)
393. Jonathan S. Masur, Process as Purpose: Administrative Procedure, Costly Screens and Examination at the Patent Office (March 2008)
394. Eric A. Posner, Human Welfare, Not Human Rights (March 2008)
395. Lee Anne Fennell, Slices and Lumps, 2008 Coase Lecture (March 2008)
396. Eric A. Posner, Fault in Contract Law (March 2008)
397. Cass R. Sunstein, Is OSHA Unconstitutional? (March 2008)
398. Randal C. Picker, Take Two: Stare Decisis in Antitrust/The Per Se Rule against Horizontal Price-Fixing (March 2008)
399. M. Todd Henderson and Anup Malani, Corporate Philanthropy and the Market for Altruism (April 2008)
400. Shyam Balganesh, Foreseeability and Copyright Incentives (April 2008)
401. Cass R. Sunstein and Reid Hastie, Four Failures of Deliberating Groups (April 2008)
402. M. Todd Henderson, Justin Wolfers and Eric Zitzewitz, Predicting Crime (April 2008)
403. Richard A. Epstein, Bell Atlantic v. Twombly: How Motions to Dismiss Become (Disguised) Summary Judgments (April 2008)
404. William M. Landes and Richard A. Posner, Rational Judicial Behavior: A Statistical Study (April 2008)