JUDICIAL RETIREMENT STRATEGIES
The Judge’s Role in Influencing Party Control of the Appellate Courts

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If judges are politically strategic, they may try to retire at times that maximize the chances that an ideologically compatible successor will be appointed. Using biographical data on all appellate judges who have retired since 1892, a heteroscedastic panel probit model is used to examine retirement timing as a function of personal and political factors. We determine whether retirement from the bench can be explained exclusively by personal factors such as salary, pension, and workload, or if political considerations enter into the decision. The data reveal that retirement decisions are affected primarily by nonpolitical considerations, but presidential elections may factor into a judge’s decision. The only important strategic political consideration in evidence is whether a judge contemplating retirement faces an opposing party president and how far off that president’s next election is.

By constitutional design, a federal judge is appointed for life to ensure impartiality in, and thus remove politics from, the judicial branch. Hamilton’s (1961) argument in Federalist 78 is that lifetime tenure with good behavior creates a branch independent from the executive and, more important, from the legislature. Political neutrality in the judiciary, however, is not the reality. The partisan composition of the bench rapidly changes with the election of a president of a different party (Barrow, Zuk, & Gryski, 1996). These significant shifts are depicted in Figure 1. Since the appellate bench was created in 1892, the Democratic share of judgeships has ranged from just 22% to more than 83%. Partisanship in our third branch of government results from such factors as the political appointment process and congressional increases in the number of judges on the bench.

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Judges, often politically active long before an appointment to the federal bench, do not leave politics behind when they don their robes. Prior research on judicial decision making in the U.S. Supreme Court, the U.S. Courts of Appeals, and the Federal District Courts indicates that judicial opinions are influenced by a judge or justice’s ideology. C. Herman Pritchett’s finding that attitude heavily influences judicial decision making on the U.S. Supreme Court is a dominant explanation in the judicial decision-making literature (Pritchett, 1941, 1948; Rohde & Spaeth, 1976; Segal & Spaeth, 1993; Shubert, 1965). Examination of the political nature of judicial voting behavior has also been conducted in both the U.S. Courts of Appeals and the Federal District Courts, and a similar conclusion has been reached—partisan or ideo-
logical attitudes influence judicial decisions (Carp & Rowland, 1983; Goldman, 1966, 1975; Howard, 1977). It is safe to say that politics and ideology influence judges and justices throughout their careers.

Active participation in politics is a likely avenue for the lawyer aspiring to sit on the federal bench. Of the 107 Supreme Court justices, 89 of them, or 83%, had participated somehow in the political arena (Epstein & Knight, 1998). In a letter to Henry Cabot Lodge, Theodore Roosevelt expressed the importance of party, stating that “A judge is not fitted for the position unless he is a party man . . .” (Murphy & Pritchett, 1979, p. 135). A speech to the Chicago Bar Association by then U.S. District Court Judge Joseph Samuel Perry described the political nature of attaining his seat. Perry recognized that “to begin with, if you want to be appointed to that office in Illinois, you almost have to be a Democrat” (Murphy & Pritchett, 1979, p. 137).

There is more than anecdotal evidence suggesting that it is to the advantage of one aspiring to a federal judgeship to be aligned with the political party in power. It is certainly in a president’s interest to select a judge with a clear and predictable partisan viewpoint. The number of federal judicial appointments coming from the sitting president’s own party rarely falls below 90%. Franklin Roosevelt’s same party appointment rate was 96.4% and Reagan’s was 94.4%. Ford’s same party appointment rate of only 81.2% was the lowest percentage among modern presidents (Abraham, 1993).

The confirmation process is also highly political. Although not as well publicized as recent controversial Supreme Court appointments, the process in the lower federal courts is often awash in political controversy. Without the media carnival of a “Borking,” political squabbling has led to too many vacancies on the federal bench. In his 1997 year end report on the federal judiciary, Chief Justice Rehnquist expressed his concerns concerning the number of appointees on hold by the Senate Judiciary Committee. He urged the president, when vacancies occur, to “nominate candidates with reasonable promptness” and asked that the Senate act on the appointments within a “reasonable time.” Due to a reluctance to act on vacancies, only 36 judges were confirmed in 1997 and only 17 in the previous year; this was well under the 101 federal judges approved in 1994 (Rehnquist, 1998, p. 3).

The last decision a judge makes is whether or not to retire. Partisan or policy-motivated judges behave strategically throughout their
careers (Murphy, 1964). It is therefore plausible that their final decisions are also influenced by politics. Perhaps the best way to continue one’s own policy legacy is to ensure an ideologically similar replacement. We hypothesize that federal appeals court judges are more likely to retire when the president is from the same party so that the president can nominate a younger judge with a similar ideology. Consistent with this line of reasoning, it is most advantageous to the party when the judge can retire at a time when the president and the Senate are of the same party to which the judge identifies.

**PRIOR RESEARCH**

Early judicial retirement research focused on the Supreme Court. It was argued that politics played a role in whether a Supreme Court justice would retire. Activist justices, those involved in the development of the law, were found less likely to retire due to age (Schmidhauser, 1962). Further anecdotal studies supported the conclusion that politics is often a consideration in a justice’s decision on whether or not to retire (Atkinson, 1976; Danelski, 1965). Recent judicial retirement studies have reached different conclusions and have extended the area of inquiry from the Supreme Court level to lower federal courts.

Squire challenges the assumption that political factors affect a justice’s resignation decision. His examination of the retirement of individual Supreme Court justices emphasizes nonpolitical factors (Squire, 1988). Specifically, he looks at the sitting president’s political party, age, infirmity, pay, history, pension eligibility, workload, and whether or not it is the chief justice. Squire finds that political factors do not matter as much as infirmity, pension eligibility, and workload. The model finds no support for political effects on retirement of Supreme Court justices. Using a probit model, the author only tests one political factor. Spriggs and Wahlbeck (1994) rightly point out that Squire’s findings, or lack thereof, “may be attributed to his model specification and research design” (p. 575). Subsequent research has investigated a much richer set of strategic political factors that may affect judicial retirements, and this article is an extension of those efforts. However, we continue along the path established by Squire by sticking to an individual-level approach to retirement. Both statisti-
ally and theoretically, this approach has a number of advantages over the aggregate approach more common in the literature.

King’s study of Supreme Court appointments from 1789 to 1984 suggests that justices retire due to political turmoil and shifts in the electorate. The aggregate annual number of appointments, resulting from resignations plus deaths in office was attributed to electoral change and military conflict (King, 1986). However, the author makes no distinction between deaths and retirement. Nearly half of the justices leaving the bench during this period did so involuntarily—they died (Hagle, 1993). A further weakness is that no controls for nonpolitical, or “corporate,” factors, such as pension benefits, are included in King’s analysis.

Hagle’s (1993) study is more inclusive of all factors, political and nonpolitical, that affect the rate of retirement on the Supreme Court. The author concludes that five political factors have influenced retirement decisions. Whether it is the president’s first or second term in office and the year within that term are both important. Retirements are more likely in the early years of a president’s second term. Hagle assumes that strategic retirement will be less important in the first term because presidents are likely to be elected to second terms. Justices of the same party hold out until early in a president’s second term because they wish to assist the president by decreasing the likelihood of a controversial appointment. A justice who is of a different party than the president waits until early in a president’s second term in hopes of a change in party control of the White House. Third, the years prior to a loss of seats in the Senate for the party in a majority on the bench are years exhibiting larger numbers of retirements. Fourth, the justices’ past political experience is an important factor. Justices with more political experience are more likely to leave the bench voluntarily. Presumably, their decisions are more likely to be politically motivated. More broadly, Court stability since Marbury v. Madison has decreased judicial retirement rates (Hagle, 1993). After this landmark decision, the increase in power and prestige of the Court made it less likely that a justice would leave for another position.

Overall, Hagle’s description of the political factors affecting retirements is couched in terms of strategic timing with respect to presidential elections. Hagle’s examination captures politically strategic factors and, to some degree, aging and illness, but he does not explore other
nonpolitical factors such as workload, changes in real salary, or pension issues. In addition, part of Hagle’s findings depict nonpurposive behavior on the part of justices facing a same-party president. According to Hagle, such justices wait until after a president’s reelection bid to retire. By doing so, they run a substantial risk of losing an opportunity to ensure an ideologically compatible replacement. Furthermore, the guarantee that a president would serve only two terms was not constitutionally established for most of the period Hagle examines. Finally, Hagle’s use of a poisson model without proper controls for heterogeneity means that overdispersion in the data may significantly bias his significance tests, encouraging Type I errors (inferring significant effects where none actually exist) (King, 1989).

To expand the range of inquiry and avoid the pitfalls of low-n data, several judicial scholars have turned to the appeals courts. Recent systematic studies of retirement from lower courts have also led to contradictory conclusions. Explanations for retirement range from personal factors to a variety of political factors. Although it may be less likely that a lower court judge retires for political reasons than a Supreme Court justice, it is probable that, to some degree, political factors enter into the equation. The appeals courts provide a better opportunity to examine the effect of partisanship and other factors on retirement decisions empirically, due to a significantly larger number of judgeships.

Barrow and Zuk (1990) offer a model incorporating both political and nonpolitical factors on appellate retirement. They include presidential elections, salary and retirement benefit changes, workload, and effects of landmark Supreme Court decisions. The authors test separate models of Democratic and Republican retirements using multivariate time series analyses. They find that the election of presidents of the same party increases judicial retirements. Therefore, it is likely that judges “stick it out” until an ideologically similar president is elected. In addition, an increase in retirement benefits increases retirements, at least for Democratic judges. Republican judges are more likely to retire when there is an increase in caseload.

Zuk, Gryski, and Barrow (1993) use a time series model to evaluate institutional and political factors affecting the president’s ability to appoint same-party members to the federal courts. In this model, the authors not only analyze presidential appointments but also voluntary departures. They examine the effects of the political environment on
same-party appointment rates, including the role played by the Senate. Various individual and political factors are explored. The authors find that the partisanship of the institution changes quickly, although it was created to be independent through lifetime appointments. The partisan balance in the judiciary changes rapidly due to presidential elections, politically timed departures by judges, unified partisan control of Congress and the White House, and judgeship bills (Zuk et al., 1993).

Spriggs and Wahlbeck (1994) tested an array of variables affecting voluntary departures including strategic timing, aging, salary, benefits, and workload. The authors find that voluntary departures from the appellate bench are politically strategic. Although nonpolitical factors also affect retirement, presidential elections play a major role in a judge’s calculations about whether or not to retire. Spriggs and Wahlbeck confirm the findings of prior researchers. They conclude that a variety of political and nonpolitical factors influence retirement. Barrow and Zuk’s study attributed a judge’s retirement decision to the party of the president and judicial salaries (Barrow & Zuk, 1990). Spriggs and Wahlbeck agree that these factors influence retirement but also confirm Squire’s finding that pension eligibility is significantly related to judicial retirements. Building on Hagle’s work, they find that retirements are also influenced by the president’s term and the confirmation environment in the Senate.

MODEL OF VOLUNTARY RETIREMENT

With the exception of Squire’s (1988) analysis of Supreme Court retirements, all of the literature examining retirements has been at the aggregate longitudinal level. The approach, although instructive in some respects and probably adequate as a first pass at the problem, may be misleading. A longitudinal analysis, like any aggregate analysis, may lead to ecological fallacies in that the factors predicting aggregate outcomes are not necessarily the same as those determining individual behavior. Because voluntary retirement arguments are explicitly based on individual strategic calculations, the longitudinal analyses of retirements in the literature may not accurately reflect individual calculations of judges.
For example, what if judges with opportunities to retire early and help their party are less political in their calculations than judges facing the necessity of hurting their party if they retire? The estimated aggregate effect of political factors would be muted in an aggregate analysis and would mask substantial variability that cannot be uncovered except by disaggregating and examining individual-level decisions. We hypothesize that there are qualitative differences in judicial retirement decisions across varying decision contexts. Specifically, potential “hurters” and “helpers” are likely driven by different factors. Prior researchers have aggregated these unique decision contexts together. We examine them separately.

We adopted Squire’s individual-level model design to explain voluntary retirements from the appellate bench, by collecting an observation for each month for each judge serving on one of the Circuit Courts of Appeals who eventually left office voluntarily. The data was compiled from Zuk, Barrow, and Gryski’s (1997) appellate biographical database. This is a panel probit design wherein the predicted probability of a judge retiring in any given month is quite low, although the aggregate predicted probability over a longer time frame is larger and potentially more accurate than that derived by aggregate indicators.

Consider the “utility” calculation for judge $i$ choosing to retire or not at time $t$, which is a function of some observable costs and benefits, $X_{it}$, as well as some unobserved factors, $\varepsilon_{it}$:

$$U_{it} = X_{it}b + \varepsilon_{it}.$$  

$X_{it}$ are a variety of personal, financial, and political factors that have associated weighted costs and/or benefits, $\beta$, for judge $i$ at time $t$. $\varepsilon_{it}$ represents every unquantified factor affecting the judge’s calculations that we do not explicitly measure in our model. If $\varepsilon_{it}$ can be thought of as a linear combination of a large number of independent factors, the Central Limit Theorem suggests that $\varepsilon_{it}$ is normally distributed. Because $\varepsilon_{it}$ contains every unquantified idiosyncratic factor affecting a judge’s retirement decision, it very likely is normally distributed.

Suppose that we observe only the outcome of the judge’s cost-benefit analysis:

$$Y_{it} = 0 \text{ if } U_{it} < 0$$
Y* = 1 if U* > 0.

As long as ε* is normally distributed, the probit framework is the appropriate one for examination of this cost-benefit calculation.6

The literature has clearly indicated that a number of personal and financial considerations have an effect, at the aggregate level, on judicial retirements. For each judge-month, we measured the judge’s age,7 workload (defined as the number of case filings per judge8), inflation-adjusted salary,9 and pension eligibility.10 Based on previous research, we expect positive relationships between a willingness to retire and age, workload, and pension eligibility, regardless of the political consequences of the choice. Age probably controls for a number of health issues that cannot be objectively quantified. We expect a negative relationship between salary and willingness to retire. That is, all other things equal, a larger salary seems to lead to a larger number of retirements. Ultimately, a larger salary results in larger retirement payments.

In conjunction with personal and financial considerations, various political factors have been identified as predictors of judicial retirements. For each judge-month, we measured a number of factors plausibly related to a judge’s cost-benefit analysis. First, a judge is unlikely to retire in the first few months of tenure on the bench. The log of the months since the judge’s appointment allows an estimate of a substantial disinclination to retire immediately after starting service on the bench, and the effect will decline and eventually become negligible after a few months of service. This logarithmic effect is probably an accurate reflection of the dynamic.11

In addition, the inclusion of a measure of the time since the observations for a judge began serves as an important statistical control for duration dependence. Beck, Katz, and Tucker (1998) emphasize that control for duration dependence is a must in order to arrive at valid statistical tests for the independent variables. They suggest inclusion of nuisance dummies or splines but point out that “a theoretically based specification of this duration dependence would be best” (p. 1283). Estimation of the tenure effect is precisely such a theoretically based control.

Whereas specifying a tenure effect has obvious practical import, it also has a strategic interpretation. Among those judges with an oppor-
tunity to help their party by retiring immediately after taking office and allowing a same-party replacement, the benefit to the party is negligible, relative to the costs of renominating and reconfirming another judge. In this context, the effect should be positive. Conversely, an extremely early resignation that hurts a judge’s party may be mediated by the fact that the benefit to the opposing party is offset, in part, by the costs of the appointive process. In this context, the effect should be less strongly positive.

Second, when the president is of an opposing party from the judge’s own, the decision to stick it out and hope for a same-party president may be related to the size of the judge’s party caucus in the Senate. Conversely, when the president is the same party as the judge, the decision to retire early and avoid the risk of an opposing president filling the slot may be related to the strength of the judge’s party contingent in the Senate. That is, a judge may be more willing to hurt his or her party when his or her party has greater strength in the Senate. A judge may be more willing to help his or her party when his or her party has greater strength in the Senate. Thus, in either context, the strength of a judge’s partisan contingent in the Senate is likely to be positively related to the judge’s likelihood of retirement. For each judge-month, we measured the strength of that judge’s partisan brothers in the Senate.12

Third, the strength of the judge’s party on individual circuits also plausibly factors into retirement decisions. All other things being equal, a judge may be more willing to retire if his or her party has a firm majority on that particular circuit. Based on the Zuk data, we constructed a measure of the partisan balance on each circuit for each month since 1892. Our measure is the proportion of co-partisans on the relevant circuit for each judge-month.

Fourth, if the judge was originally a cross-party nomination, which may indicate party mavericks or ideological moderates, the judge’s efforts to time retirement to his or her party’s advantage may be muted. That is, cross-party nominated judges may be more willing to hurt or less willing to help their party because partisan loyalty is either weak or nonexistent.13

Finally, the literature suggests that judicial retirement decisions are sensitive to presidential election cycles (Hagle, 1993; Spriggs & Wahlbeck, 1994). We established a set of 16 quarterly dummy variables
spanning a presidential cycle. Due to increased uncertainty about the party likely to control the White House when a judge’s vacancy is filled, judges should be substantially less likely to retire in the final months of a presidential cycle if that retirement will hurt their party. They should be substantially more likely to retire in the final months of a presidential cycle if that retirement will help their party. We included all but the last dummy variable, indicating judge-months falling in August, September, or October of a presidential election year, which serves as the baseline for comparison. Because the previous literature has suggested that there is less uncertainty about the outcome of a presidential election following a first term than following a second (Hagle, 1993), we also included a dummy measure of whether the justice-month fell in a president’s second term. Politically motivated judges should be less likely to retire under an opposing party president when it is the president’s second term. Similarly, judges may be more likely to retire under a same party president when it is the president’s second term.

In summary, we examined two distinct decision contexts separately. One decision involved a judge’s willingness to retire when that retirement would benefit his party (i.e., when that judge’s party controlled the White House). This context was relevant for 25,813 judge-months. A second decision involved a judge’s willingness to retire when that retirement would hurt his party (i.e., when that judge’s party did not control the White House). This was the context for the remaining 23,170 judge-months. For each month, the judge’s choice of whether or not to retire was modeled as a function of the following:

- age\(_t\) (in years)
- workload\(_t\) (in hundreds of case filings per judge)
- salary\(_t\) (in thousands of 1983 dollars)
- pension eligibility\(_t\) (0 = no, 1 = yes)
- log(time since appointment)\(_t\) (in years)
- Senate strength of own party\(_t\) (as a proportion of members in the chamber)
- strength of own party on own circuit\(_t\) (as a proportion of judges on the circuit)
- same party appointees\(_t\) (0 = originally a cross-party nominee, 1 = originally nominated by president of same party as self)
presidential term, (0 = first presidential term, 1 = second presidential term)
• quarterly dummies, (15 dummies excluding using Quarter 16, the election quarter, to be used as the baseline for comparison)

It should be noted that both probit and logit coefficient estimates are inconsistent in the presence of heterogeneity in $\varepsilon$. Some heterogeneity controls exist for panel data, but they are currently not possible to calculate for this large-N, large-T data. As an alternative, we have estimated a heteroscedastic probit model, which is consistent even in the presence of heterogeneous errors, as long as the error variances are adequately modeled. In tandem with the estimation of the independent variable effects on the dependent variable, a heteroscedastic probit estimates the effects of some specified variables on the error variances. That is, the error variances can be modeled as a function of some plausible explanatory factors, $Z_t$, as in (2):

$$E(\varepsilon_t, \varepsilon_t') = [\exp(Z_t \gamma)]^{\varepsilon_t} \tag{2}$$

$$\varepsilon_t | Z_t \sim N(0,1).$$

As long as the additional parameters, $\gamma$, are estimated, and the specification of factors affecting the errors is reasonably complete, the coefficient and standard error estimates for $\beta$ from (1) are consistent.

We investigated a number of specifications for the variance. One plausible type of heterogeneity is partisan. That is, Republican retirements may be substantially more difficult to predict, in the same way that they are more difficult to predict for legislators, because typical career paths for Democrats may present fewer extralegal opportunities than for Republicans. Both Spriggs and Wahlbeck (1994) and Barrow and Zuk (1990) estimate Democratic and Republican retirements separately and find that the judges of different parties differ in their retirement behavior. If this is true, estimated errors for Republicans may be larger than for Democrats.

Another plausible type of heterogeneity may be related to changes in the pension plan for appeals courts judges. There was no senior status retirement option prior to 1919. As a result, the earliest retirement choices were more significant, from both lifestyle and financial standpoints. In addition, senior status benefits were upgraded in 1954 to
allow judges to continue to receive salary increases after their retirement. If these factors are important, estimated errors may “jump” between the pre-1919, 1919-1954, and post-1954 periods. We included dummy variables for the two earliest periods and excluded the post-1954 dummy. Thus, the coefficients should be interpreted as indicating the magnitude of the errors relative to the modern period.

Another plausible type of heterogeneity is related to political regimes. For example, retirements may be substantially more difficult to predict during eras of divided government. In such situations, not only is the strength of one’s own party in the Senate larger, there are potentially unique factors in retirement decisions at work because of the dynamics associated with legislative-executive bargaining difficulties. There may also be heterogeneity across different circuits, and there may be heterogeneity related to the age of the judge. In our final model (Model 2, described in the following section), we reestimated the model as a heteroscedastic probit, in which the error variances are a function of the following:

- senior service dummies, (pre-1919 and 1919-1954 period dummies, excluding the modern period, to be used as the baseline for comparison)
- party of judge, (1 = Democrat, 2 = Republican)
- divided government, (0 = White House and both congressional chambers controlled by same party, 1 = otherwise)

RESULTS

We subset our analyses between potential helpers and potential hurters to examine variation in the effects of the independent variables. That is, as noted earlier, we examine separately the qualitatively distinct contexts of willingness to hurt one’s own party and willingness to help one’s own party.

Based on the simple probit examination in Model 1 (see Table 1), which assumes homogeneous errors and excludes the presidential election cycle, personal and economic considerations are related to retirement decisions in the expected direction. The sign for salary is positive for both decision contexts, indicating that as a judge’s real wages drop, he or she will be less likely to retire. Instead, it is prefera-
TABLE 1
Individual-Level Model of Voluntary Retirements and Resignations, 1892-1995

<table>
<thead>
<tr>
<th>Independent Variable</th>
<th>Hurt Own Party by Retiring, (n = 23170) $\beta$(se)</th>
<th>Help Own Party by Retiring, (n = 25813) $\beta$(se)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Model 1</td>
<td>Model 2</td>
</tr>
<tr>
<td>Constant</td>
<td>-4.69 (.627)**</td>
<td>-6.72 (1.16)**</td>
</tr>
<tr>
<td></td>
<td>.0209 (.00787)**</td>
<td>.0258 (.00976)*</td>
</tr>
<tr>
<td>Age</td>
<td>.263 (.0456)**</td>
<td>.399 (.0804)*</td>
</tr>
<tr>
<td></td>
<td>.0367 (.0353)</td>
<td>.0547 (.0621)</td>
</tr>
<tr>
<td>Workload</td>
<td>.844 (.105)**</td>
<td>1.03 (.150)**</td>
</tr>
<tr>
<td>Salary</td>
<td>-.0561 (.0698)</td>
<td>-.0680 (.100)</td>
</tr>
<tr>
<td>Pension eligibility</td>
<td>.346 (.178)$†$</td>
<td>.384 (.230)$†$</td>
</tr>
<tr>
<td>Log[years since</td>
<td>-.0970 (.128)</td>
<td>-.0975 (.175)</td>
</tr>
<tr>
<td>appointed]</td>
<td>.0504 (.0810)</td>
<td>.120 (.115)</td>
</tr>
<tr>
<td>Own party Senate</td>
<td>-.555 (.390)</td>
<td>-.535 (.918)</td>
</tr>
<tr>
<td>strength</td>
<td>.346 (.178)$†$</td>
<td>.384 (.230)$†$</td>
</tr>
<tr>
<td>Own party circuit</td>
<td>-.0970 (.128)</td>
<td>-.0975 (.175)</td>
</tr>
<tr>
<td>strength</td>
<td>.0504 (.0810)</td>
<td>.120 (.115)</td>
</tr>
<tr>
<td>Same party nominees</td>
<td>.177 (.345)</td>
<td>.314 (.749)</td>
</tr>
<tr>
<td>Second presidential</td>
<td>-.155 (.156)</td>
<td>-.168 (.203)</td>
</tr>
<tr>
<td>term</td>
<td>.261 (.117)$*$</td>
<td>.307 (.144)$*$</td>
</tr>
<tr>
<td>15 quarterly election</td>
<td>.0841 (.0761)</td>
<td>.104 (.0984)</td>
</tr>
<tr>
<td>cycle dummys</td>
<td>see Figure 2</td>
<td>see Figure 3</td>
</tr>
<tr>
<td>Variance parameters—</td>
<td>$\gamma$(se)</td>
<td></td>
</tr>
<tr>
<td>Pre-1919 dummy</td>
<td>.0861 (.0387)</td>
<td>.0500 (.0541)</td>
</tr>
<tr>
<td>1917-1954 dummy</td>
<td>.0980 (.0683)</td>
<td>.0500 (.0548)</td>
</tr>
<tr>
<td>Divided government</td>
<td>.0686 (.0525)</td>
<td>.0478 (.0423)</td>
</tr>
<tr>
<td>Party</td>
<td>.164 (.0813)*</td>
<td>.153 (.0656)*</td>
</tr>
<tr>
<td>Likelihood ratio tests—LogL (restricted LogL)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Restriction: $\beta = 0$</td>
<td>633.3 (746.0)**</td>
<td>615.9 (746.0)**</td>
</tr>
<tr>
<td>Restriction: $\beta$ (election cycle) = 0</td>
<td>615.9 (633.9)**</td>
<td>801.5 (962.7)**</td>
</tr>
<tr>
<td>Restriction: $\gamma = 0$</td>
<td>610.9 (615.9)$†$</td>
<td>797.0 (801.5)$†$</td>
</tr>
</tbody>
</table>

$p < .05$, one-tailed. $*p < .05$, two-tailed. $**p < .01$, two-tailed.
ble to wait until a salary adjustment is enacted because this directly translates into higher pension payments. However, this factor is not statistically significant, taking into account the other variables in this model. The insignificance of salary to retirement decisions should not be surprising because the potential private sector salaries for appellate judges far exceed their federal salary (Posner, 1996). Thus, wages are not likely to affect retirements because they do not clearly affect acceptance of nominations in the first place.

Whereas the sign of the coefficient for age is as expected for both helpers and hurters, age is a significant consideration only for those who are facing the necessity of hurting their party if they retire. This may be explained by the many anecdotal accounts depicting federal judges as unusually likely to stick it out through even severe physical limitations. After all, about one in four of their colleagues die in office and, thus, do not officially retire. As expected, pension eligibility and workload are significant contributors to a judge’s willingness to retire irrespective of the political implications of that retirement. This finding is consistent with previous literature.

On the political front, the implications are mixed and the evidence is less clear. In Model 1, the length of time since the judge was appointed has different effects in the two political contexts, in the directions expected by our strategic interpretation. However, the effect is significant only for those judges faced with an opportunity to help their party. The strength of a judge’s party in the Senate seems to play no significant role in the judge’s retirement decision. In neither decision context is the coefficient significantly different from zero.

Judges considering retirement who are from the same party as the president are less likely to hurt their party when the opportunity presents itself. However, the effect is not significant for hurters, even using a one-tailed test. In addition, the strength of a judge’s party contingent on his or her own circuit is positively related to a willingness to retire only for those situations where the retirement will hurt one’s party. If the retirement will help one’s party, the effect is negative and not significant.

In contrast to Hagle’s findings for Supreme Court retirements, appeals court judges do not significantly alter their retirement strategy during a second presidential term (although the signs of the coefficients are consistent with the strategic hypothesis).
The election cycle coefficients were estimated only for our final model (Model 2), which controls for heteroscedasticity. The individual-level model we present in this article allows controls for heterogeneity and allows a very finely parsed examination of retirements across a presidential election cycle. Thus, if Model 2 is a statistical improvement over Model 1, it indicates the utility of our approach. Differences in findings between the models also demonstrate that the choice between an individual and longitudinal approach has substantive import.

As previously discussed, there may be substantial heterogeneity in the data, which biases the results of Model 1. Heteroscedasticity in the probit model is an especially serious concern because it leads to inconsistent coefficient estimates, which may lead to biased predictions, in addition to biased inferences about the significance of the effects of the independent variables. The results of a heteroscedastic probit estimation are presented in Table 1 as Model 2. A likelihood ratio (LR) test of whether the variance components are necessary is significant for helpers ($\chi^2_{(4)} = 10.4, p < .05$) but not for hurters ($\chi^2_{(4)} = 8.97, n.s.$), although it should be noted that both tests would be significant if the insignificant divided government and party variables were dropped.

Based on the coefficient estimates in the variance component of Model 2, it appears that retirement decisions in the pre-1919 era were significantly less predictable than they have been since 1954. The variance coefficient for the intermediate period is also positive, but less strongly so, indicating that retirement decisions between 1919 and 1954 were also less predictable than in the modern period, although the effect is not statistically significant. This situation probably arises because of the lack of a senior status retirement option prior to 1919 and the lack of postretirement salary adjustments prior to 1954. The same results could arise because of increasing politicization of the bench. If that is the case, we see no evidence of acceleration in politicization, as indicated by smaller errors, since 1954 (see Note 13). There is scant evidence that the model performs less poorly for Democrats than for Republicans. The variance coefficient is positive but is substantively and statistically insignificant. Likewise, there is no evidence of heteroscedasticity related to divided government.

For our data, it does appear that the bias in the coefficients reported in Model 1 is substantial for some independent variables. For exam-
ple, the coefficient for the salary impact for potential helpers is more than twice as large after controlling for heteroscedasticity, raising the coefficient to statistical significance. With the exception of the salary factor, the statistical inferences remain essentially the same between Model 1 and Model 2 for either decision context. However, only the Model 2 coefficient estimates are reliable because the LR test indicates significant heterogeneity, and Model 2 controls for heterogeneity. Therefore, we will rely on Model 2 point estimates in describing the magnitude of the effects of the independent variables on the probability of retirement.

In both decision contexts, we estimated coefficients for 15 quarterly dummy variables to examine the timing of retirements across a presidential election cycle. That is, for each judge-month observation, we coded a set of 16 dummy variables, indicating in which quarter of a 4-year presidential administration the observation fell. The final quarterly dummy, which is 1 for all observations in August, September, and October of a presidential election year, was excluded from the regression and serves as the baseline for comparison (i.e., \( \beta_{16} = 0 \)). An LR test based on the log-likelihoods in Table 1 indicates that the set of 15 variables is jointly significant for potential hurters \( (\chi^2_{15} = 34.8, p < .01) \) but not for potential helpers \( (\chi^2_{15} = 13.6, \text{n.s.}) \).

Rather than presenting the results in Table 1, Figures 2 and 3 graphically depict the coefficients for hurters and helpers, respectively. A distance-weighted smoothing function is superimposed for each figure to clarify the overall pattern of coefficients. In addition, the coefficients for each decision context are depicted on the same scale to highlight the differences in magnitude between the coefficients in the two decision contexts.

For potential helpers, none of the quarterly dummy variables differ significantly from the election quarter or from one another. The standard errors for all the quarterly dummy coefficients were in the range of .2 to .25, so roughly a .5 difference between any two quarterly coefficients is statistically significant. Overall, there is little relationship between presidential election cycle and judicial retirement decisions when the judge is of the same party as the president. What little pattern is evident cannot be presented as confirmation of our hypotheses.

However, among potential hurters, the election quarter is significantly different from all but the previous (15th) quarter, and none of
the remaining quarterly dummy variables differ significantly from one another. The standard errors for all the quarterly dummy coefficients were in the range of .35 to .38, so roughly a .75 difference between any two quarterly coefficients is statistically significant. There is a significant drop-off in the likelihood of a judge retiring during the final days of a presidential election season. The estimated decline seems to begin during the summer of a presidential election year and accelerates for the last quarter prior to the election. This is a statistically significant and substantively dramatic effect, is consistent with a strategic account of retirement decisions, and will be described in more detail in the following section.

Figure 2: Retirements Hurting the Judge’s Party
The probability that a judge retires in any given month is quite low. The average probability is .00566. Retirements occur in only 0.5% of our observations. As a result, the predicted probabilities never rise to a .5 prediction criterion—in none of the observations is the dependent variable predicted to be 1 by the .5 prediction criterion. This is perfectly logical because the chances that we could predict the precise month in which any judge retired is virtually nil. Nevertheless, the LR tests indicate that the model is hugely significant in either decision context ($\chi^2_{(28)} = 270.2, p < .01$ for hurters, $\chi^2_{(28)} = 331.3, p < .01$ for helpers). That is, the independent variables do significantly distinguish between more and less likely retirement dates.

Figure 3: Retirements Helping the Judge’s Party
In addition to statistical criterion, it is important to consider the substantive magnitude of the effects. Some of the plausible variation in independent variables does dramatically affect the dependent variable. A judge with a typical profile (average workload, average salary, average Senate and circuit strength, a same-party nominee, in the 14th quarter of a presidential administration) who is 70 years old, has served 9 years and 11 months, and is facing an opposing party president has a predicted probability of retiring of .0002. If the judge were suddenly eligible for pension that month, his retirement probability jumps 3000% to .006. Two quarters later, during the final three months of the presidential election campaign, the judge’s retirement probability drops precipitously to .0004, despite being eligible for pension. Had the judge been facing a same party president, his retirement probability would have remained high through the election and even increased slightly, consistent with our strategic expectations. Two quarters later, during the beginning months of the new administration, the same pension-eligible judge returns to his relatively nonstrategic retirement probability of approximately .008.

It turns out that variation in the strength of a judge’s party in the Senate or party on the circuit has only minor estimated effects. As well, the kinds of salary adjustments witnessed over the past century have had only a bit more than a negligible effect on retirement decisions. This bolsters our earlier claims, based on t tests, that these variables are either not significant or only marginally significant determinants of judicial retirements.

Perhaps most importantly, our model design allows a direct test of the most fundamental strategic retirement question: Ignoring the short-run election quarter phenomenon, are judges more likely to retire when they are of the same party as the president? This is one rare instance where the constant in a regression is substantively meaningful. Indeed, a comparison of the constants between the two decision contexts is implicitly a test of strategic behavior because the only thing that distinguishes the two contexts is that the presiding president is the same party as the judge for potential helpers and is the opposite party for potential hurters. A strategic account suggests that the constant for helpers will be more positive than for hurters. We find, indeed, that
this is the case. The difference between the two contexts is 1.62, which is probably not significant in light of the estimated standard errors.

To illustrate the magnitude of the direct and long-term ideological effect, consider the same typical judge described earlier (average workload, average salary, average Senate and circuit strength, a same-party nominee, in the 14th quarter of a presidential administration) who is 70 years old and facing an opposing party president but is not yet eligible for pension. He has a predicted probability of retiring of .0002. If the president were the same party as the judge, the retirement probability is .0005—more than twice as large. The same logic applies if the judge were eligible for pension. His retirement probability was .006 when facing a president of a different party, but is .011 if the president is of the same party.

There is, then, both long-run and short-run strategic behavior in evidence in judicial retirements, although it is almost exclusively oriented around presidents rather than other plausible factors, such as bench partisanship or Senate control. Furthermore, it must be reiterated that the most significant factors in retirement timing, by far, are nonpolitical factors such as workload, pension eligibility, age, and salary.

As a more concrete illustration of the potential and limits of strategic judicial retirement behavior, consider the retirements of Thomas Meskill and Thomas Tang. Tang, a Democrat, was appointed by Carter to the Ninth Circuit at the age of 55 in October 1977. He had not previously served as a federal district court judge and was thus eligible for pension beginning in October 1992, after having served 15 years on the appellate bench. Based on our model estimates, Judge Tang’s predicted retirement probability was low throughout his career. When he became eligible to retire in October of that year, we would normally expect his retirement probability to skyrocket if not for the fact that October is the last month of the last election quarter and he faced a president of the opposing party. Thus, his predicted probability of retirement remained quite low for October at .014. The very next month, following the election, Tang’s predicted retirement probability did grow, dramatically, to .07. By January 1993, his predicted retirement probability had reached a plateau at a very high level of .12, where it remained until he retired later that year. Tang was eligible for
retirement prior to the presidential election but chose to forestall his departure in a rational fashion, in hopes of presenting a vacancy opportunity to a president from the same party. The wait paid off.

Meskill, a Republican, was appointed by Ford to the Second Circuit at the age of 47 in April 1975. He had not previously served as a federal district court judge and was thus not eligible for pension until after 15 years on the bench, ending in April 1993. Our model estimates demonstrate that Meskill’s predicted retirement probability was also low throughout most of his career. Unlike Tang, Meskill faced a president of the same party throughout the 1988-1992 period and therefore had a strategic incentive to retire early and be certain the vacancy opportunity was presented to an ideological compatriot. In fact, Meskill’s retirement probability, although very low throughout 1992, remained about 20% higher than Tang’s because, as a Republican under a Republican president and therefore a potential helper, he had a slightly higher long-run estimated retirement probability. Not surprisingly, Meskill did not choose to retire before the election—he waited until April when he was eligible for pension. His predicted retirement probability inched up only very slightly near the election but grew dramatically to more than .15 by the summer of 1993, at which point he retired.

LONGITUDINAL PERSPECTIVE

The very low probabilities of retirement in any given month in the individual-level model, of course, have a cumulative effect. Consider a judge who has a probability of retirement of .03 each month. His probability of retirement over 3 months is $1 - (.97)^3 = .08$. Over 6 months, it is $1 - (.97)^6 = .167$, and over a full year it is $1 - (.97)^{12} = .306$. If the predicted retirement probability is higher (which we do observe in this data), say .05, the predicted retirement probabilities are .143, .265, and .460 over 3, 6, and 12 months, respectively. These probabilities, aggregated annually, are considerably more accurate, on predictive grounds, and they have the benefit of having been constructed from individual-level data with explicit controls for both event dependence and residual heterogeneity.
The predicted number of retirements over any particular number of months is the sum of the individual predicted probabilities for all judges serving in that interval:

\[
\text{Total Predicted Voluntary Retirements} = \lambda_t = \sum_{t=0}^{\text{month}_{\text{last}}} \sum_{i} \hat{\pi}_{it} \]

We use this fact to generate annual predicted voluntary retirements, depicted in Figure 4.

To directly assess the value of our individual-level approach, versus a longitudinal event-count approach, we constructed an event-count time series model in which the predictors are directly comparable. That is, we derived the corresponding annual measures of total retirements, average age of judges on the bench, and so on, as described in our previous model specification. The event count model estimates appear in Table 2.

A comparison of the longitudinal predictions with the individual-level predictions indicates that the models have essentially identical statistical power. The Pseudo-$R^2$ for the individual-level model predictions, .494, is practically indistinguishable from that of the longitudinal model, .493. Event count coefficients and $t$ tests are notoriously unstable in the presence of ill-behaved errors, but in neither model is there indication of significant overdispersion or serial correlation of errors. For the longitudinal model, separate regressions for the pre-1919 and pre-1954 periods were run, and there is no indication of model instability across changes in the senior status retirement option.

Consistent with the findings from our individual-level analysis, the longitudinal estimates indicate that personal factors such as workload and pension eligibility are the most substantively and statistically significant factors affecting aggregate retirements. However, salary considerations appear important in the longitudinal analysis, which the individual model estimates suggest is true only for judges contemplating retirement during the administration of a president from the same party. In addition, the proportion of same-party nominees appears not significant in the longitudinal model, despite the fact that we did observe a significant effect at the individual level—again, only for same party judges, or helpers. We again find no statistical evidence to support the
contention that there are more appellate court retirements during a president’s second term, although the coefficient is in the hypothesized direction.

Finally, the longitudinal model indicates that partisan composition of the bench and the Senate are significant factors affecting retirements. Based on the longitudinal model estimates, a stronger majority on the bench results in fewer retirements. This finding is inconsistent with the individual-level strategic hypothesis we suggested at the outset and is statistically significant at the .05 level only for a one-tailed
test (which would not be theoretically justified in the context of our hypothesis). On the other hand, the longitudinal model estimates confirm the Spriggs and Wahlbeck finding that a stronger Senate contingent results in more retirements.

These relationships, although perhaps coincident at an aggregate level, are unsupported by analysis of individual retirement decisions. To illustrate, Republicans were in the majority on the appellate bench in 1994, and Republicans occupied only 43% of the seats in the Senate that year. If Republicans had held 53% of the seats that year, the longi-
The longitudinal model predicts a 17% greater retirement rate, or approximately 2.5 additional retirements. Spriggs and Wahlbeck (1994) arrived at a similar finding in their longitudinal analysis, in that the corresponding shift (from 43% to 53% Republican) would result in a 20% increase in Republican retirements. By contrast, using the individual model and estimates to calculate the 1994 retirement predictions in the presence of a hypothetical 53% Senate Republican strength raises the prediction by a mere 0.23 retirements.

The longitudinal model suggests that Senate control is an important causal factor in retirements in the direction consistent with an individual strategic account. Researchers relying on a longitudinal-level approach might improperly infer that judges incorporate strategic considerations about Senate control into their retirement choices. They may improperly infer that judges retire more often when their party is weakest on the bench. Our individual model demonstrates that they do not behave in this fashion, irrespective of their own party or the party control of the White House. Instead, we believe Senate control and bench partisanship are spuriously related to aggregate retirements, even though an extremely rigorous analysis of the errors provides no statistically significant evidence of a problem. Perhaps subsequent analysis of out-of-sample predictions will vindicate this claim.

At this point, it may seem that a great deal of effort has been expended to arrive at a predictive model that could have been constructed with aggregate data and a longitudinal event count model, as in the previous literature. However, it is clear that both the predictions and the substantive inferences researchers derive about judicial retirements vary dramatically between the two models. It is ironic that a longitudinal model run through every conceivable specification test and estimated in the most conservative manner possible finds evidence of relationships unsupported by a cross-sectional model with more than 48,000 observations and adopting a probit specification, which is statistically more prone to Type I errors.

The advantage of our approach is that we have modeled individual decisions and can present a more finely parsed description of strategic calculations that may be misrepresented by aggregate data. It is clear that choices that hurt a judge’s party and choices that help a judge’s party are both qualitatively and quantitatively distinct. That distinction is impossible to examine with only longitudinal data. It is clear
that presidential election cycles do affect the timing of judges’ retirements under limited circumstances. Even longitudinal analyses have suggested as much (Hagle, 1993; Spriggs & Wahlbeck, 1994). The individual analysis presented here provides a more precise depiction of the effects. It is further clear that some strategic components appear to be significant determinants of aggregate retirements, but on closer inspection they clearly are not important factors to individual judges. Conversely, aggregate retirement patterns fail to provide evidence for effects that, on closer inspection, are clearly important for some judges. Finally, and most important, our individual-level analysis allows us to unequivocally support the contention that, over the long haul, judges are more likely to retire when an ideologically more similar person occupies the White House. The effect varies, depending on the judge’s circumstances, but we have presented several plausible scenarios in which the likelihood of retiring is roughly 10% higher if the president is of the same party as the judge.

CONCLUSION

Barrow and Zuk’s most recent work in this area examines the factors involved in making partisan change occur so rapidly in the judiciary through changes in the other institutions. The role of the president through the appointment process, the role of judges through strategic voluntary retirement, the role of the Senate through the confirmation process—including Senatorial courtesy in the lower courts, and the role of Congress through bench expansion are all explanations for such rapid change. They find that the institution has become increasingly politicized due to the constant recomposition of the bench (Barrow & Zuk, 1990). The importance of voluntary retirements is readily apparent when viewed from an institutional perspective.

It turns out that there is evidence of significant political complications in appellate judge retirements, but the statement must be tempered by noting that nonpolitical factors are even more important, and most of the plausible strategic factors we examined, such as bench composition and Senate partisanship, are not important to judges. To the extent that politics matters, it is primarily in the efforts of judges to delay retirement in the waning months of an opposing party presi-
dent’s administration, hoping for a change in White House control. Judges of the same party as the sitting president exhibit no significant inclination to retire early, just prior to a presidential election, out of concern that their party will lose control of the White House.

Our findings partially support the proposition that presidential election cycles are important to voluntary federal appellate court retirements. They cast doubt on the proposition that retirement decisions are so perfectly Machiavellian as to be affected by more subtle political factors, such as Senate or circuit partisan composition. Instead, although perhaps still slightly persuaded by the political climate, the judge’s decision to retire is largely a personal one.

NOTES

1. We refer here only to voluntary retirements and resignations. We exclude, in this analysis, judges who die in office and the small number of judges who have been removed from office through the impeachment process. Further, a few judges have left the Courts of Appeals only to be elevated to the Supreme Court, and these judges are also excluded. Finally, we excluded that relatively small proportion of judges who were neither Democrat nor Republican and thus had unclear partisan loyalties.

2. Legislation controlling the size of the judiciary is important, for it has dramatic effects on the partisan balance of the institution.

3. A more complicated model design, such as a competing risks framework, to explain both retirement decisions and death in office is possible, but we chose to ignore death in office as a risk and exclude judges who died in office. A competing risks framework is estimated in Box-Steinensmeier and Jones (1997), but they too ignore death in office as an exit option. This is sensible because the timing of death in office is certainly unrelated to political and personal factors (other than age, perhaps) and thus can be treated as “white noise.”

4. The choice of a time frame is, in some sense, an arbitrary one, which may have consequences for the results of an analysis. Squire assembled his data annually (1,651 Supreme Court Justice years). We could have followed suit or could have assembled quarterly data. We found that appellate court retirement strategy with respect to presidential elections cannot be observed on an annual basis but only at a quarterly or monthly basis. We chose to split the data by the smallest practical time frame to reduce the inference biases resulting from aggregation and to more accurately model the aggregate annual outcomes. The result is that there were 48,983 judge-months for those Democrat and Republican judges who voluntarily retired between May 1892 and December 1995.

5. In the language of Beck et al. (1998), this is binary time series cross-sectional (BTSCS) data, or a grouped duration model. Box-Steinensmeier and Jones (1997) refer to it as a discrete time duration model.

6. The choice between probit and logit is quite difficult and important in this context of a very large imbalance between observed 1s and 0s. Hagle (1993) claims that such an imbalance may “result in inflated standard errors for the estimated coefficients [of a probit model]” (p. 27),...
and his assertion is repeated in Spriggs and Wahlbeck (1994, p. 575), with a citation to Hagle. We are not aware of any basis for this statement in the statistics literature. Rather, the problem of imbalanced dichotomous dependent data is that the coefficient standard errors (although not the coefficients themselves) are unusually sensitive to assumptions about the distribution of the errors. The two dichotomous choice models—logit and probit—result in substantially different statistical inferences in the data of this article. From a practical standpoint, logit is more conservative because the tails of a logistic distribution are thicker than a normal distribution. Logit is an “extreme value” statistic that presumes there are frequent data outliers. The probit model, on the other hand, has the virtue of a more solid theoretical grounding because the Central Limit Theorem suggests that the aggregation of many independent factors, as the errors in predictions of retirements probably are, is almost surely normally distributed. This choice is all the more vexing because no empirical test for normality of the errors is possible. We have adopted the probit framework for the aforementioned theoretical reasons, although it is important to note that a logit framework results in substantially lower \( t \) statistics for our models. We take special care, as a result, to describe the magnitude of the coefficients (which are not sensitive to the choice between probit and logit) to verify that statistically significant effects are substantively significant as well.

7. Our age measure is linear, which follows Squire (1988). The only other author to have examined age as a retirement factor used a linear measure in conjunction with an “over 80” dummy (Hagle, 1993). Our subsequent analysis (inclusion of a squared term) confirms that a linear operationalization is sufficient (\( \chi^2 = 0.14 \), n.s.).

8. Case filings per sitting judge follow the convention used by every study of judicial retirement that considered workload (Barrow & Zuk, 1990; Spriggs & Wahlbeck, 1994; Zuk et al., 1993). Unfortunately, only annual case filings are available. However, because our workload measure is partly based on the number of sitting judges, and that number is measured monthly, it is probably a very good approximation to true monthly workload. Alternative measures of workload are possible, such as the percentage change in per-judge case filings from the previous year. Our subsequent analysis indicates that such a measure is almost totally unrelated to retirement decisions. An LR test in favor of the measure we settled on is hugely significant. That is, case filings per judge are significantly related to retirement decisions, whereas annual change in case filings per judge is not.

9. Exact monthly salary figures were provided by the Administrative Office of the U.S. Courts. Monthly CPI figures were obtained from the Bureau of Labor Statistics. CPI data for 1892 to 1913 are estimates.

10. Retirement provisions were established in the Act of April 10, 1869. Judges were eligible to retire at the age of 70, provided they had served 10 years (Zuk et al., 1993). After 1954, judges could retire at the age of 65 with a minimum of 15 years on the bench or at 70 with 10 years on the bench. Since 1984, judges may retire at 65, provided the sum of age and years of service is equal to 80 (Spriggs & Wahlbeck, 1994). Credit for prior service as a District Court judge has always been granted for pension eligibility. Fortunately, the Zuk et al. (1997) data include a measure of time served as a district judge prior to appointment, so our eligibility measure properly takes into account prior service.

11. An alternative, linear operationalization for this variable is possible. We found the logarithmic measure to be a significant improvement, based on a likelihood ratio (LR) test. In addition, we examined various spline dummies over the course of a judge’s tenure, as recommended by Beck et al. (1998). The estimates indicated that a logarithmic specification captures virtually all of the residual duration dependence in the data.

12. For the most part, Senate strength does not vary monthly. However, on occasions where death, appointment to higher office, or other factors have changed the partisan balance in the
Senate midsession, our measure accounts for this accurately. Our Senate strength variable is,
then, measured monthly, regardless of whether it actually varies monthly.

13. A more direct measure of ideological moderation would be preferable. The pioneering
work in this area developed a civil liberties ideology measure for Supreme Court justices, based
on content analysis of newspaper articles (Segal & Cover, 1989). A similar measure of ideology
for appeals courts judges has been proposed (Songer, Segal, & Cameron, 1994) that features the
predicted direction of a judge’s search-and-seizure vote (liberal or conservative, as defined in the
Songer dataset) as a dependent variable and party of the appointing president and other measures
as independent predictors. However, no general measure of ideology for appeals courts judges or
even Supreme Court justices has been presented in the literature, and construction of such a mea-
sure is a project, if not an entire research agenda in itself. Epstein and Mershon (1996) have spe-
cifically cautioned against use of the existing ideology measures for any research other than pre-
dicting judicial votes. An alternative ideology measure that has been occasionally used (Rohde
& Spaeth, 1976; Segal & Spaeth, 1993), the percentage of cases on which the judge voted in the
liberal direction, can be constructed from the Songer database, and it would likely correlate rea-
sonably well with a more sophisticated measure because the propensity to vote in a liberal (or
conservative) direction is, ultimately, the variable being measured. Despite some qualms about
constructing an overall appellate judge ideology measure de novo, we constructed a measure of
liberalism for as many judges as possible (444 of the 596 judges appointed between 1892 and
1995). We then included each judge’s absolute distance from the mean as a measure of ideologi-
cal extremism. This might be seen as an alternative to a measure of same-party appointees as
ideologically consistent. Surprisingly, ideological extremists, as defined by our crude measure,
are significantly more likely to retire, regardless of who controls the White House. In addition,
after controlling for this “impulsiveness effect,” salary appears to be a significantly more impor-
tant factor in retirement decisions. These are puzzling findings, the measure of ideology is crude,
and the effort at constructing a judicial ideology measure runs contrary to the suggestions of
Epstein and Mershon (1996), so we are unwilling to defend it at this time. The approach so far,
although preliminary, provides no additional evidence that judges retire strategically in pursuit
of an ideological agenda beyond that indicated by other factors.

14. Following Hagle (1993) as well as possible with our monthly data, Franklin Roosevelt’s
third term was coded as a second term. November 1944 through April 1945 were coded as
another second term for FDR, whereas the remainder of the presidential term was coded as Tru-
man’s first.

15. We examined a number of alternative variance specifications, investigating whether
there was heterogeneity related to age, circuit, party, divided government regimes, and several
more complicated operationalizations of time. None of these more complex variance specifica-
tions is a statistically significant improvement over the parsimonious specification presented in
Table 1, based on LR tests. We are confident that the variance specification in Model 2 is a rela-
tively complete control for heteroscedasticity in the data.

16. There is some indication that the effect of salary on leaving office is dependent on the exit
options available. Ever since the creation of the pension program for circuit court judges, there
has been an important financial distinction between “resigning”—leaving office before one is
eligible for pension—and “retiring”—leaving office after one is eligible. Inclusion of an interac-
tion term between pension eligibility and salary is a statistical improvement for potential helpers,
based on an LR test. This indicates that there is a significant difference between the salary-
resignation option and the salary-retirement option. The coefficients suggest that salary has a
slight but nonsignificant negative effect on pre-pension resignations and a significant positive
effect on post-pension retirements. Thus, the finding that salary is an important spur to leaving
office for potential helpers is really true only for pension-eligible judges. More generally, the
effects of the independent variables could vary, depending on whether the option is resignation
or retirement. Subsequent analysis of interaction terms between pension eligibility and the pri-
mary independent variables turned up no significant improvements, based on LR tests. That is,
with the exception of salary for potentially helpful exits, the effects of the independent variables
reported in Table 1 are not significantly different for pre-pension or post-pension exit decisions.

17. For identification reasons, the 10th quarterly dummy must be omitted from the model
specification for potential hurters because no judges have ever retired during the 10th quarter
under an opposite-party president. This restriction is for identification purposes and has no sub-
stantive import, other than that we cannot say for certain whether retirements during the 10th
quarter are consistent with the overall pattern. Given the clear cycle in our results, we are reason-
ably certain that retirements are not unusually likely (or unlikely) at this particular point in the
election cycle. This serves as a warning that quarterly analysis of the election cycle is probably
the lower feasible limit of parsing the data.

18. π may be summed because the predicted probabilities, conditional on X and Z, are
independent.

19. The t tests on α indicate no significant overdispersion, in which case poisson estimates
are asymptotically equivalent to negative binomial estimates. Indeed, poisson coefficients for
the longitudinal model are virtually identical to negative binomial estimates. However, we chose
to report negative binomial estimates because the t tests for independent variables tend to be
more conservative than poisson estimates even when the overdispersion parameter is not
significant.

20. An LR test against inclusion of a lagged dependent variable mirrors the approach in
Spriggs and Wahlbeck (1994).

21. In keeping with the heteroscedasticity tests in the individual model, we tested for model
instability between unified and divided government regimes and across several more compli-
cated time subsets. No Chow tests, which are the approach used by Spriggs and Wahlbeck
(1994), indicated significant error problems.

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