Controlling for District Court Judges' Preferences[1]

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Manuscript Dated: 04/15/2010
Version 2.08

ABSTRACT

This study invokes the common space scores of executives and senators to generate a number of alternative preference point positions for U.S. District Court judges. Tests of these continuous measures against a null case fact specification suggest that the legal model always proves an effective predictor of decisions, but that ideological influences have incrementally grown throughout the last century. Continuous preference measures that assume a traditional norm of senatorial courtesy tend to be robust in limited samples of more recent outcomes. However, measures that account for cyclical changes in inter-branch appointment relationships are more effective for temporally lengthy large N samples. The magnitude of these ideological effects is modest, but not unsubstantial. During the recent era of independent executive vetting practices, the likelihood of a conservative decision is approximately 78 to 85% for Democratic appointees, and 85 to 90% for Republican appointees.

The adoption of continuous representations of judges' ideological preferences is an empirical issue that increasingly crosses disciplinary boundaries, including those that exist between political science, criminology, sociology, and the legal academy. The transmission of knowledge and best practices amongst these fields often stems from methodological critiques, with Epstein and King's *The Rules of Inference* (2002a) acting as an exemplar. At the heart of Epstein and King's (2002a, 87) assessment of empirical legal research is the question of how best to control for the ideological preferences of judges and justices. Because this topic is central to the longstanding debate over theories of judicial decision making, such as the legal, attitudinal (Segal and Spaeth 2002), and strategic (Epstein and Knight 1998) models, it understandably evoked a fervent discourse – immediate reactions included Cross, Heise, and Sisk (2002), Goldsmith and Vermeule (2002), Revesz (2002), Epstein and King's own response (2002b), and more recently Sisk and Heise (2005). Of these, Revesz (2002, 180) and Sisk and Heise (2005) most directly address the issue of preference measurement with a respective focus on the lack of measures and the magnitude of ideological influence.[1] Thus in current context, it seems that this debate no longer centers upon whether ideological preferences in fact matter, but rather how much more they contribute over traditional legal explanations.

The development and incorporation of valid, exogenous, and continuous measures of judges' preferences potentially allows socio-legal scholars to make inroads on this important question. Epstein and King (2002a) clearly advocate the use of such measures, which over time have become widely available. At the Supreme Court level, Segal and Cover (1989), Bailey and Chang (2001), and Martin and Quinn (2002) offer competing ideal point estimates. Giles, Hettinger and Peppers (2001) have generated continuous representations of U.S. Court of Appeals judges' preferences that are based upon Poole and Rosenthal's (1997) common space scores. Those inter-

ested in state supreme courts can take advantage of Brace, Langer and Hall's (2000) party adjusted measures.

An obvious gap in alternatives to dichotomous controls for judges' ideology can be found for those courts that are primarily concerned with matters of original jurisdiction (e.g., U.S. District Courts), where one reasonably can hypothesize that case facts and precedents should predominate and that the effects of ideology should be limited in scope.[2] A skeptic might point out that a lack of continuous measures at this level merely reflects the weakness of attitudinal explanations within the vast majority of cases heard by American courts. This does not entirely appear to be the case, since studies of U.S. District Court decision making (Carp and Rowland 1983; Rowland and Carp 1996) provide an array of evidence supporting ideological influence. Such criticism, however, would be very much on point, because we do not have reasonable estimates of how much ideology contributes to the prediction of case outcomes versus legal aspects.

With this in mind, the purpose of this analysis is to generate a number of continuous alternatives for District Court judge's preferences and to evaluate their robustness versus a null model specification of fact based controls. Like the Giles, Hettinger, and Peppers (2001) scores, these alternative measures invoke the Poole and Rosenthal (1997) common space positions of executives and senators, but are developed in light of the results of a lengthy analysis of the selection and confirmation of District Court judges (Hendershot 2010). This approach provides leverage not only on the relative predictive improvement that continuous preference controls garner, but also offers insight into the advantages of considering temporal changes within the federal judicial appointment process (e.g., Goldman 1997).

EXECUTIVE AND SENATOR INFLUENCE ON DECISION MAKING

From the perspective of many legal scholars, reluctance to incorporate continuous measures of lower court judges' preferences may be quite understandable. The studies that have emerged from the field of political science offer considerable variance in sample, dependent variable and independent variable construction, and recent replications (Sisk and Heise 2005) suggest that the substantive improvement these measures offer may be rather small.

For example, some analyses of U.S. Courts of Appeals decision making depend upon samples that are relatively short bifurcated periods (Songer 1982). Others capture lengthy samples of decisions, but model judges' career liberalism scores (Songer and Humphries Ginn 2002; Kuersten and Songer 2003), that result in relatively small numbers of observations. Studies of dichotomous case level outcomes (Giles, Hettinger and Peppers 2001; Songer and Humphries Ginn 2002) focus on specific case types (i.e., civil liberties and rights) or only look at those decisions with a dissenting position, which may limit the generalizablity of resulting conclusions.

While these limitations are a function of the time intensive demands of gathering data and the difficulty of the task at hand, the segregation of variables designed to test ideological relationships also may diminish the usefulness of empirical findings. The trend has been to separately control for the preferences of executives, home state senators, and home state elites, and measurement strategies similarly have been inconsistent. Executive preferences have been captured with ordinal Guttman scaling techniques (Songer 1982), Poole and Rosenthal's (1997) common space scores (Giles, Hettinger, and Peppers 2001; Songer and Humphries Ginn 2002), and surveys of political science professors' perceptions (Segal, Timpone, and Howard 2000; Kuersten and Songer 2003). Senators' positions have been controlled by dichotomous identifiers of traditional courtesy rights (Kuersten and Songer 2003), the mean common space scores for senators

of the president's party (Giles, Hettinger and Peppers 2001) and differences between senators and their party's mean position (Songer and Humphries Ginn 2002). Differences attributable to those judges appointed to vacancies with two opposing party senators have been modeled by split sample estimations (Songer and Humphries Ginn 2002) and adaptation of Berry, Ringquist, Fording and Hanson (1998) estimates of citizen ideology (e.g., Giles, Hettinger, and Peppers 2001; Songer and Humphries Ginn 2002).

Despite the considerable variance in research design, these studies do provide fairly consistent results. Executive preferences show a discernable relationship with Court of Appeals decision making. Home state senator influence operates through the traditional norm of senatorial courtesy. The influence of home state elites for vacancies with opposing party senators is either insignificant or minute. Of these studies, perhaps the most relevant is Giles, Hettinger, and Peppers (2001), which subsequently has been used to generate single ideal point estimates for Courts of Appeals judges. These ideal points have been constructed with parsimonious formula[3] and have been widely adopted (Giles 2008). Thus a single point estimate is now available for empirical studies focused upon Courts of Appeals decision making.

Comparatively, studies of the influence of ideology at the District Court level are less prevalent. Carp and Rowland (1983) and Rowland and Carp's (1996) analyses of decision making depends upon an extensive database of published opinions. Through a rigorous categorization strategy, a sound case for the influence of executive and home state senators has been made across a number of issue areas. More recently, this data has been used to test the effects of continuous measures of executive, home state senator, and home state elite preferences. Johnson and Songer (2002) and Songer, Johnson, and Stidham (2003) estimate variance in judges' career liberalism scores versus executive preferences (Poole and Rosenthal 1997; Segal, Timpone, and

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Howard 2000), and senators' preferences relative to the executive position and party mean. Like the Courts of Appeals literature, relationships for the executive and traditional courtesy norm are found, but the influence of courtesy rights was less robust and results tended to vary by the issue type being considered.

One omission from the District Court research is the availability of judge specific ideal point estimates that are akin to Giles, Hettenger, and Peppers (2001) scores for the Courts of Appeals level. Still, the lack of control variables associated with traditional legal explanations also creates some uncertainty about the relative effects of ideology on District Court decision making. The use of judges' career liberalism scores, even when samples are segregated into particular issue types (e.g., civil liberties and rights, economic, and criminal justice) indirectly captures the effects of legal criteria. Therefore efforts to create judge specific ideal point estimates and tests of these measures against a null fact based model would provide additional leverage on the pre-dictive improvement that ideological explanations afford.

RECONCILING MEAUREMENT AND JUDICIAL APPOINTMENT LITERATURES

A separate issue with these lower court studies of preferences and decision making is incon-sistency relative to judicial appointment research. Early studies (Harris 1953; Chase 1972) em-phasize the role of patronage politics, which operating through senatorial courtesy rights acted as the foundation for the appointment of federal judges. Patronage, of course, can take on a number of different meanings (e.g., the selection of trusted friends or the influence of machine politics) and it is not clear whether this is innately ideological. Current studies (Scherer 2005) point to the politicization of the process in the post-World War II era, when the emergence of divisive civil liberties and rights disputes altered the selection function for lower court judges to greater focus

on ideology. This transition may be important, since existing measurement strategies (Giles, Hettinger, and Peppers 2001) may be less effective for the earlier patronage politics era of appointments.

In addition to matters of patronage, Goldman's (1997) exhaustive analysis of lower court appointments offers us a political realignment hypothesis. According to Goldman, temporal differences exist in the underlying balance of power between the executive and Senate that is similar to Dahl's (1957) view of the Supreme Court appointment process. Both suggest that times of wholesale political change alter the executive vetting process for judicial nominees; the goal being to counter time worn preferences on the bench. For the lower courts, Goldman (1997, 361) finds evidence of independent executive vetting during Franklin Roosevelt's New Deal and also in the interstice between Presidents Carter and Reagan.

This realignment hypothesis may have substantive implications for the construction of judge specific preference points, since it would suggest that the executive position would be preferable to a traditional courtesy based calculation in some periods. However, available evidence from the above decision making studies does not corroborate temporal change – Giles, Hettinger and Peppers (2001) reject this hypothesis and other studies including controls for Goldman's (1997) categorization scheme are insignificant (Kuersten and Songer 2003).

This debate is not yet exhausted and recent analysis of the lower court appointment process tends to confirm the changes identified by Goldman (1997). Hendershot (2010) models the duration of selection and confirmation events for District Court nominations between 1901 and 2006. The goal of the study was to conceptualize the prevailing executive-Senate relationship as either an *Advice and Consent* (i.e., when senators exert influence over the selection of nominees) or a simple *Consent* process (i.e., when senators respond to independent executive choices at the con-

firmation stage). The operative determination was based upon the presence of senatorial constraint within selection and confirmation activity and it was tested with measures of ideological distance (Poole and Rosenthal 1997) related to the three stages of appointment (i.e., senatorial courtesy, committee consideration, and floor procedures).

Temporal variance in appointment events was evaluated and used to segregate the sample into four distinct appointment regimes: 1) 1901-1942; 2) 1943-1976; 3) 1977-1994; and 4) 1995-2006.[4] Significant constraint relationships revealed a repetitive cycle of senate influence. Ideology was absent in the selection process of Regime 1 (i.e., a *Consent* process that could be a function of patronage politics). The balance of power was altered in Regime 2, when active bargaining relationships could be found during the selection of nominees (i.e., *Advice and Consent*). The last pair of regimes emulated this transition. Beginning in the Carter Administration, relationships for selection activity were marginalized (i.e. a return to a *Consent* process of independent executive selection), followed by a period of conflict with constraint found both during selection and confirmation (i.e., *Advice and Consent*).

Therefore, in addition to the primary goal of determining how continuous measures of District Court judges' preferences perform relative to fact based models of decision making, this analysis also considers whether these cyclical constraint relationships can be invoked to improve upon existing traditional courtesy based measures of judges' preferences.

SAMPLE DERIVATION AND RESEARCH DESIGN

In order to conduct this analysis, a sizeable sample of District Court outcomes that extends as far back as the first regime of appointments is necessary. The best available alternative[5] is a combination of the original and updated United States Courts of Appeals Databases.[6] The com-

bined database represents a stratified random sample (i.e., stratified across circuits, and random within each circuit year) of published opinions for the U.S. Courts of Appeals from 1925 to 2002. Although these data primarily have been adopted for studies of appellate court outcomes, this data set was designed to be utilized in studies of lower court compliance and therefore it is possible to obtain the liberal/conservative dichotomous position of the District Court outcome. Songer and Humphries Ginn (2002, 312) include such a measure as an independent variable within their analysis of Courts of Appeals decision making, so precedent exists for its use.

From the aggregate database, those cases that originated from one of the fifty-state District Courts, were decided by a three judge panel, resulted in a distinct affirm or reversal, and contained a single issue outcome were selected (see supplemental Table 1). This specification provided a potential sample of 5,707 observations. After separate coding checks based upon judge/state identifiers, and years of service, the final sample contained 5,680 cases distributed across the four appointment regimes.[7]

The use of appellate level data places obvious limitations for generalizeablity to all aspects of District Court decision making and the exclusion of cases appealed from other venues erodes some aspects of the stratified random sample. However, the primary purpose of this study is to evaluate alternative measures of judges' preferences. The lower court compliance literature (Songer, Segal and Cameron 1994; Cross and Tiller 1998) would suggest that appealed decisions represent fertile areas to find ideological influence. Each preference measure is tested on the same sample of decisions and estimate results should prove sufficient to draw inferences about measurement performance.

The use of these data also generates beneficial aspects for the research design, such as a case level unit of analysis that can readily be associated with threshold questions and the inclusion of

large numbers of unpublished District Court outcomes.[8] In fact, disparity between published and unpublished decisions is a topic of some dispute.   Keele, Malmsheimer, Floyd, and Zhang (2009) recently have found that unpublished opinions do not exhibit ideological influence (*but see,* Songer 1988). In this instance, a super majority of unpublished opinions exists within the sample and estimations should therefore represent a more difficult test for preference measures.

Dependent Variable Calculation and Estimation Strategy

The dependent variable for this study is a dichotomous variable that identifies the liberal (0) or conservative (1) disposition of the District Court outcome.[9] It is calculated with two variables from the U.S. Courts of Appeals Database (see supplemental Table 2 for calculation matrix). Decisions that were subsequently affirmed by the appellate court were coded with the same liberal/conservative direction. Decisions that were reversed by the appellate court were inverted – liberal appellate decisions were coded as conservative District Court positions, and conservative appellate decisions were coded as liberal District Court positions. This dichotomous dependent variable is estimated with the logit model. To preserve as much as possible the favorable aspects of the stratified random sample design, these maximum likelihood estimates are weighted via the circuit/year strategy suggested by the U.S. Courts of Appeals Database. Unweighted estimations tend to be more robust, so the following results can be considered conservative in a methodological sense. To parse as much variance as possible, separate estimations are presented for judges appointed in the first three appointment regimes. The lack of observations for the most recent appointment regime prevents a separate estimation, but some information can be gathered from composite model results for the entire sample.

Controlling for Case Facts

The U.S. Courts of Appeals Database provides a wide assortment of threshold issue coding variables that can be employed to construct a null legal model specification. This analysis initially cast a wide net and considered the 52 issue coding variables associated with criminal, civil law, civil law-government, and diversity issues.[10] The coding[11] of these variables is a direct reflection of the dichotomous liberal/conservative dependent variable, so each was recoded into dichotomous identifiers of whether the issue, or question, was present in the case outcome.

The segregated regime estimation strategy prevents the inclusion of all 52 issue control variables for each of the four estimations, since some of these variables are perfectly correlated with the dependent variable (e.g., the identifier was entirely associated with conservative outcomes) or one issue control was a perfect linear combination of one or more of the other control variables. To construct a single series of issue controls applicable to each estimation, the 52 variables were tested in the three regime subsamples and in the composite specification. Controls exhibiting perfect collinearity in one or more of the estimations were excluded, leaving 26 potential legal control variables. From these, an issue control that showed at least a moderate statistical relationship ($\rho < .20$) in one of the five estimations was retained.[12] The final series of controls thus comprises 22 variables distributed across the general issue areas.[13]

This series of controls offers a good alternative to split-sample estimations by issue type, since they do exhibit overlap. For the entire sample of 5,680 observations, 24.5% of the sample is unidentified by one of the issue variables and thus represents the null category of outcomes. The remaining 75.5% of the sample is identified by one or more of the issue controls – 43.3% of observations are associated with a single issue, 23.4% with two issues, 7.3% with three, and the remaining 1.5% is associated with as many as seven issue controls. The null category observa-

tions tend to be slightly skewed toward criminal case types, but the underlying differences in the distribution of case types are minor and do not appear problematic (see supplemental Table 3). Estimations incorporating these controls represent the null legal model specification and the relative improvement that continuous preference measures offer will be evaluated through tests of significances for the ideal point estimates, general improvements in predictive ability of ideological specifications, and likelihood ratio tests between the alternative model specifications.

Calculation of Baseline and Alternative Preference Measures

This analysis evaluates four unique ideal point measures for District Court judges – two baseline measures, and two alternatives constructed with evidence of constraint during the selection and confirmation of nominees. The first of the baseline measures corresponds to the nominating president. The executive point is the DW Nominate (Poole and Rosenthal 1997) value of the president first associated with the District Court nominee. Because the executive value is not available prior to the Eisenhower administration, the executive point for early administrations (1901-1952) is identified by the median of the president's party in the Senate at the time of the nomination.[14]

The second baseline measure[15] is a function of the traditional norm of senatorial courtesy that suggests rights of consultation are only present for senators of the president's party. When two senators of the president's party were associated with the nominee, the traditional courtesy point is the mean position[16] of the executive and more distant senator. When senators of both parties were present, the measure is the mean position of the president and senator of the same party. Vacancies with two opposing senators were identified by the executive position, as the traditional norm assumes no consultation rights for these vacancies.

The two baseline measures for those District Court judges confirmed between 1901 and 2006 are presented in Figure 1, which reveals a distribution of the traditional courtesy point around the sequence of executive point values. It should be noted that some variation in the executive position can be found for nominees that were not confirmed in the final year of an executive term and were subsequently renominated by the succeeding president.

[INSERT FIGURE 1 ABOUT HERE]

The first alternative measure is the selection point[17] measure which is created with the evidence of significant inter-branch constraint (Hendershot 2010) between executive and senatorial actors. These values are regime specific and are based upon the executive-Senate relationships that existed at the time the nomination was first submitted to the Senate. Inter-branch constraint was absent in Regime 1 (1901-1942) and therefore the judge's ideal point simply corresponds to the executive point measure. In Regime 2 (1943-1976), significant constraint relationships for each senatorial courtesy diad and committee consideration could be found. Because two veto points were active in this period, an absolute distance, or gridlock, interval was calculated for senatorial courtesy and the committee relationship. For those observations where senatorial courtesy yielded the largest gridlock interval, the selection point is represented by the mean position of the executive and appropriate senator.[18] For those nominations where the committee gridlock interval was greatest, the selection point is the mean position of the president and judiciary chairman. Regime 3 (1977-1994) revealed a cyclical decline in inter-branch bargaining and constraint could only be found for those nominees associated with two senators of the president's party. The selection point for these observations again is the mean position of the executive and senator; all other observations are represented by the executive point. Regime 4 (1995-

2006) showed a return to inter-branch constraint. Senatorial courtesy and committee relation-ships were active and thus the calculation of the measure corresponds to that of Regime 2.[19]

The second alternative measure is the confirmation point[20] measure which is based upon the executive-Senate relationships that existed at the time the District Court nominee was successful-ly confirmed by the Senate. The confirmation model of Regime 1 provided evidence of con-straint for each senatorial courtesy diad. The confirmation point for this period is the mean value of the executive and appropriate senator. Regime 2 and 3 yielded a single constraint relationship for committee consideration and thus identifies the mean position of the executive and judiciary chairman. Regime 4, however, showed significant constraint relationships related to senatorial courtesy (i.e., observations with two opposing senators), committee consideration, and floor pro-cedures. A gridlock interval was calculated for each of the three stages of confirmation, and the measure captures the mean distance between the executive, the most distant opposing party sena-tor, judiciary chairman, or opposite party median.

The selection and confirmation points are presented in Figure 2, where distinct patterns of dispersion for the two alternative measures can be found according to appointment regime. The confirmation point offers greater amounts of variance in Regime 1, but then coalesces with the committee constraint relationships found in Regimes 2 and 3. Active senatorial courtesy bar-gaining relationships create a more diffuse selection point measure during Regime 2, which be-comes more limited in Regime 3. However, the recent conflict over appointments in Regime 4 exhibits more concentrated patterns for both alternatives. Divided committee relationships con-strain the selection point measure during the Clinton administration, but then diffuse during uni-fied government situations of the George W. Bush administration. Opposing party resistance on

the floor of the Senate (i.e., the use of legislative holds and filibusters) acts to limit ideological outliers, since the floor stage almost always represented the largest gridlock interval.

[INSERT FIGURE 2 ABOUT HERE]

In addition to tests of these four continuous preference measures, a final independent variable attempts to control for any temporal disparities in decision making. Because the judge's length of service on the bench incrementally declines with each appointment regime,[21] a natural log transformation of years of service was included in each model specification.

ESTIMATION RESULTS BY APPOINTMENT REGIME

The weighted logit estimates for District Court judges appointed in Regime 1 (1901-1942) can be found in Table 1,[22] where the substantive conclusion is an absence of ideological influence. The standard error for each preference measure is greater than the corresponding parameter value (i.e., none of the measurement strategies close to meeting traditional standards of significance). Although models of confirmation events (Hendershot 2010) show that inter-branch constraint operated through the traditional courtesy norm, these estimates indicate that this senatorial resistance did not have a strong ideological component that carried over to case outcomes.

The most likely explanation of these null results is the role of patronage politics within the judicial appointment process. Confirmation contests may have existed with home state senators, but in the end, these contests seem to be related to the appointment of political insiders, or merely a function of machine politics. Still, this particular finding is not entirely unexpected. Songer, Sheehan, and Haire (1999; 2000, 129) similarly find a lack of ideological decision making for Courts of Appeals judges of this era.

[INSERT TABLE 1 ABOUT HERE]

Systematic relationships in decision making are not entirely absent, however, and the model controlling only for case facts (i.e., Model 1) does yield an improvement in predictive ability. This estimation is strongly significant (i.e., the Wald statistic) and the Percent Reduction in Error (PRE)[23] is 8%, so a rudimentary legal model specification is informative. The traditional courtesy specification (i.e., Model 3) offers a very slight improvement in the PRE value, but likelihood ratio tests of the baseline and alternative specifications versus the null legal model are all insignificantly different. The decision making process of District Court judges appointed in Regime 1 is consistent with traditional legal model perspective and does not tend to support the presence of systematic attitudinal influence. These results do not necessarily imply that this process was temporally static, since some evidence of change related to years of service is evident. In general, the likelihood of conservative outcome grew as these judges remained active on the bench.

While attitudinal influences cannot be found in Regime 1, the decision making process of judges appointed in Regime 2 (1943-1976) does contain a significant ideological component (see Table 2). Each of the baseline and alternative preference point parameters is significantly different and in the expected positive direction. Likelihood ratio tests versus the legal model specification are all significant allowing the rejection of the null hypothesis that case facts offer the best explanation of District Court outcomes.

[INSERT TABLE 2 ABOUT HERE]

More importantly, the models support the presence of inter-branch constraint or an *Advice and Consent* appointment process. Continuous measures accounting for the preferences of senatorial actors are preferable to the simple executive point (i.e., the likelihood ratio tests versus the executive point specification). Of these, the traditional and selection point measures prove to be most robust, offering the best PRE values of approximately 9%. The selection point measure

minimizes the log likelihood value and thus offers the best fitting preference measure. However, the relative improvement is not significantly different in a likelihood ratio test versus the traditional point estimation. The selection point measure, which captures relationships with two opposing senators and the judiciary chairman, offers a slightly better, but not substantially large improvement over a measure constructed with the traditional courtesy assumption.

Some care is necessary with respect to the magnitude of ideological effects. The traditional and selection point measures provide about a 1.4% increase in PRE versus a legal model specification. Given that the modal category of a conservative outcome is high, 78.0%, this improvement is not inconsequential, but the legal model still provides a strong foundation for understanding District Court decision making. Temporal disparity that coincides with time on the bench is once again a factor in Regime 2, but the direction of this parameter is negative meaning that these judges tended to be slightly more liberal with longer tenures.

District Court judges appointed in Regime 3 (1977-1994) continue to show an attitudinal component within decision making (see Table 3), but the underlying effects are somewhat marginalized when compared to the earlier period. Parameters related to each of the continuous measures are significantly different by traditional standards, but do not meet the most stringent $\rho<.001$ standard. Significant differences relative to the null legal model specification are once again found, but the case for inter-branch constraint within the appointment process is less clear cut. Neither the traditional, selection, nor confirmation point measure offers a marked improvement over the executive point specification when evaluated with likelihood ratio tests. The lack of such findings would be consistent with a *Consent* view of the appointment process, but the traditional point estimation does minimize the likelihood function and offer the greatest predictive ability (7% versus 6% for the executive point). Ideological controls continue to add to our

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knowledge of District Court outcomes, with as much as a 1.6% improvement in PRE over the null legal specification, but once again case facts provide a relatively strong basis for explaining case outcomes. The left censoring of the years of service variable is evident in temporally stable patterns of decision making for Regime 3 appointed judges.

[INSERT TABLE 3 ABOUT HERE]

From these split-sample estimations of District Court outcomes, the empirical evidence supports a handful of initial conclusions. First, and perhaps most relevant, the legal model is a good predictor of decision making outcomes throughout and is the only relevant perspective for those judges appointed in the first half of the twentieth century. Second, significant attitudinal effects arise later and the case for senatorial influence is strongest within the second appointment regime. The attitudinal component of decision making for Regime 3 judges appears to be somewhat in flux. The traditional point provides a marginally better predictor of outcomes, but does not prove to be substantively better when considering model fit versus the executive measure. Together, this would suggest that preference measures based upon the assumption of the traditional courtesy norm are reasonably sound proxies for modern judges' preferences.

COMPOSITE ESTIMATION RESULTS AND EVIDENCE OF TEMPORAL CHANGE

The small N sample for Regime 4 (1995-2006) judges prevents a separate estimation, but the transition to a composite model specification for those judges appointed in all four regimes does offer further refinement of these initial conclusions. Table 4 provides estimation results and support for the presence of attitudinal influence within District Court outcomes. Each of the executive, traditional, selection and confirmation point parameters is significantly different and corresponding models are preferred to the null legal model specification via likelihood ratio tests

of model fit. Some evidence of senatorial influence can be found, since the traditional point measure is preferred to a simple executive point specification.

[INSERT TABLE 4 ABOUT HERE]

Nonetheless, other criteria would suggest that these ideological relationships are lacking and point to underlying problems in model specification. PRE values for the executive, traditional courtesy, selection, and confirmation point estimations offer no improvement versus the null legal model specification. The resulting conclusion could be that ideological preferences make no contribution to the prediction of District Court decision outcomes, but a more likely explanation is that a single ideal point measurement strategy is not effective when considering temporally long large N samples. If this is in fact the case, then cyclical adjustment may be necessary to more effectively leverage the existence of ideological effects. For instance, the inclusion of a continuous measure for judges appointed in Regime 1, where ideological controls were insignificant, might actually impair predictive ability. The alteration of senatorial influence between the second and third appointment regimes similarly may yield poor performing estimations.

To assess this possibility, three cyclical preference points were tested that control for regime specific appointment relationships (see Models 6, 7, and 8). These three measures leave the preferences of judges appointed in Regime 1 uncontrolled (i.e., judges' ideal points were set to 0), adopt one of the senatorial preference measures (i.e., ether the traditional, selection, or confirmation point) in Regime 2, reverts to the executive point in Regime 3 when Goldman (1997) hypothesizes independent executive selection practices, and again utilizes one of the senatorial measures in Regime 4.

Tests of these cyclical measures are encouraging and show that cyclically constructed measures are preferred to single ideal point assumptions. Parameters controlling for the traditional,

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selection, and confirmation cycle measures are significant at the highest $\rho<.001$ level. Likelihood ratio tests for the three specifications are preferred to the null legal model, the simple executive point, and traditional point measure. More important, however, is that each of these three measures offers improvement in predictive ability relative to the null legal model specification.

In this final set of estimates, the confirmation cycle measure is preferred both in terms of model fit and PRE – Model 8 minimizes the log likelihood function and provides a 1% increase in predictive ability versus the modal category of a conservative outcome. Some residual evidence of temporal variance in decision making can be found in the independent variable for years of service, which is negative and significantly different. Thus, in addition to variance attributable to the altered appointment process for District Court judges, longer tenure on the bench is associated with a slight liberal trend.

The combination of split sample and composite estimations generates a refined set of conclusions for this analysis. First, if one is working with District Court decisions in the first half of the twentieth century, controls for judges' preferences do not appear necessary, and actually may impair model performance. Second, when attempting to control for judges' preferences within limited samples of more recent outcomes, a continuous measure based upon the assumption of a traditional courtesy norm is relatively robust and captures an ideological component within the decision making calculus. However when considering large N samples that traverse changes within the judicial appointment process, preference measures that control for the repeating cycle of senatorial influence tend to be the best performing proxies of judges' preferences. Continuous measures that account for judiciary chairman obstruction and independent executive selection of nominees (Goldman 1997), offer a substantive improvement over statically constructed measures. Considering the relatively small sample of judges appointed under recent conflict condi-

tions, it perhaps is too early to say that this cycle again is altered after the switch to Republican control of the Senate (104th Congress), but initial evidence is somewhat supportive.

With the above conclusions in hand, the final task is to assess the relative effect of ideological influence within District Court decision making, which has been a central consideration of the debate over empirical studies of decision making (e.g., Sisk and Heise 2005, 793). The most effective strategy to tap into the significant ideological effects found above is to take advantage of King, Tomz, and Wittenberg's (2000) Clarify simulation software. This approach allows for the generation of predicted probability values that can be presented in the form of standard 95% confidence intervals. A simulation was run for each case outcome in an unweighted[24] estimation containing the confirmation cycle measure (Model 8 in Table 4 above), where the 22 issue controls and the years of service variable were set to their mean values. The confirmation cycle measure was then set to 0 in Regime 1, the mean confirmation point for each president of Regime 2, the executive point of each president in Regime 3, and again the mean confirmation point for Clinton appointed judges in Regime 4.

These simulations can be found in Figure 3, which presents a useful account of the growth of ideological influence over time. First and foremost, District Courts can hardly be viewed as bastions of liberal decision making – the mean probability of a conservative outcome is very high and just shy of 85%. When simulating on the basis of legal criteria alone, judges appointed by the executives of Regime 1 offer consistent distributions, along with some minimal variance that can be attributed to changing issue types. The case for substantively important ideological effects begins with Eisenhower appointees, but even at this point simulated distributions remain firmly within a 95% confidence interval. Nixon and Ford appointees tend to be decidedly more conservative, but the effects of more independent executive vetting become quite clear in Re-

gime 3. The disparities between Democratic and Republican appointees breach the confidence interval, even if relative differences are not overwhelmingly large. The outer tails of Carter and Clinton appointees would represent a 6% increase in likelihood of a liberal outcome, whereas the outer tail for Reagan and Bush appointees would indicate at most a 5% increase in the likelihood of a conservative decision. The small number of observations for Clinton appointees in Regime 4 prohibits strong conclusions, but the simulation does imply that recent appointment conflict may have the beneficial effect of constraining ideological influence within District Court decision making.

[INSERT FIGURE 3 ABOUT HERE]

THE ROLE OF IDEOLOGY WITHIN THE DISTRICT COURTS

This study has developed a number of continuous measures of District Court judges' preferences and tested them against an arguably simple legal model specification. Because the above estimates are specific to appealed cases, they are not generalizeable to all aspects of District Court decision making; future work on non-appealed samples clearly is necessary and now possible. The underlying focus on appealed decisions does, however, offer a means to evaluate ideological influence in a broad sample of unpublished decisions (see Keele, Malmsheimer, Floyd and Zhang 2009) and results show that ideological influence is present in this context.

The answer to the question of how much ideology contributes to our knowledge of District Court decision making ultimately depends upon the period of study. Attitudinal effects tend to incrementally increase over time, but case facts and traditional legal criteria continue to act as a robust foundation for the prediction of case outcomes throughout. Controlling for judges preferences does not appear to be productive for much of the first half of the twentieth century, but

does begin to make a substantive contribution in the post-New Deal era, and these effects are exacerbated with the advent of independent executive vetting strategies (Goldman 1997). The magnitude of such relationships is somewhat modest, but not inconsiderable. For example, confidence intervals for the likelihood conservative outcomes expand from 83-86% for Regime 1 judges, to 81-88% in Regime 2, and are as large as 77-90% in Regime 3.

The incorporation of continuous preference measures for more recent appointees clearly is advisable, but the resulting issue is which measurement strategy should be adopted. This conclusion also depends upon the period of study. Empirical scholars interested in relatively short samples of decisions should be able to utilize a traditional courtesy based measure with reasonable assurance of validity. On the other hand, studies of extended samples of decisions may improve model performance with cyclical measures that account for the repetitive cycle of senatorial influence within the judicial appointment process.

The cyclical effects of inter-branch constraint upon judges' preferences previously had not been found, so this analysis makes a material contribution in this regard. In fact, one of the more interesting aspects of the above results is how they speak to recent conflict within the judicial appointment process. From the perspective of this analysis, the conflict that has emerged over lower court appointments can be grounded within the growing influence of ideology in the era of independent executive selection strategies (e.g., Regime 3). In the preceding regime, senatorial influence over the selection and confirmation of judicial nominees had acted to moderate such attitudinal effects. The results associated with the small number of Clinton appointees facing a Republican controlled Senate suggest that this recent conflict may actually be beneficial from traditional legal perspective, as it may once again have placed limits on the ideological component of District Court decision making.

[1] For example, see Sisk and Heise's (2005, 793) discussion of this position – "Does ideology matter in the selection and decisionmaking of federal judges? Unquestionably. The expanding literature of empirical research on the subject, including our own, makes it starkly clear that the partisan or political dimension is, among others, and inescapable element in understanding judicial behavior. But, at least at the lower federal court level, ideology explains only part of judicial behavior and tends to emerge in certain narrowly defined sets of cases in studies designed to tease out those marginal effects."

[2] Principal-agent theories of lower court compliance (e.g., Songer, Segal, and Cameron 1994; Cross and Tiller 1998) suggest that attitudinal influence should be less likely in lower court outcomes, since these courts are subject to the threat of reversal.

[3] Giles, Hettinger, and Peppers (2001) scores either capture the common space score for the single senator of the presidents party, the mean of two senators from the presidents party, or the executive position for judges associated with two opposing party senators.

[4] Temporal variance was evaluated with a cumulative sum of squares test (CUSUM) of the standard deviation of the confirmation period for each Congress between 1901 and 2006. The CUSUM test evaluates subsample variance and the resulting 95% confidence interval can be used to identify cut points. In this case, subsample variance exceeds the confidence interval in the Congress ending in 1944 and then shows a return to equilibrium, before exceeding the interval in the Congress ending in 1978 and again returning to equilibrium at the Congress ending in 1996. The regimes are thus segregated at the end of the preceding Congress (i.e., 1942, 1976, 1994).

[5] The Rowland and Carp (1996) database represented one alternative, but this database is in the process of being updated and currently is not available to the public.

[6] Donald R. Songer, *The United States Courts of Appeals Database*. National Science Founda-

tion Grant SES-8912678. Ashlyn K. Kuersten and Susan B. Haire, *The United States Courts of Appeals Database Update*. National Science Foundation Grant SES-9911284.

[7] 1,169 observations are associated with 239 judges appointed in Regime 1. 2,916 observations are associated with 594 judges appointed in Regime 2. 1,592 observations are associated with 555 judges appointed in Regime 3. The most recent Regime 4 provided a small N sample of 66 observations attributable to 49 judges. The sample of decisions contains at least one case outcome for 72.7% of the 1,976 District Court judges that were successfully confirmed between 1901 and 2001 (one year prior to the end of the sample of decisions). The mean and maximum numbers of observations per judge are: 4.9 and 34; 4.9 and 29; 2.8 and 14; and 1.4 and 3, respectively. The reversal rate of the District Court outcome is consistent across the four regimes – 29%, 26%, 22%, and 26% of observations.

[8] Unpublished opinions account for a super majority of observations (82%), which is consistent across appointment regimes (79%, 83%, 83%, and 83% respectively).

[9] Conservative decisions are identified as 1 to correspond with the Poole and Rosenthal (1997) scale that runs from liberal to conservative (i.e., -1 to 1). Conservative decisions also represent the modal category for the sample of decisions.

[10] These 52 variables start with a control for prejudicial conduct (PREJUD / field 100) and end with a control for which state's law should be binding (WHLAWs / field 152).

[11] The variables are coded: 0 – issue not discussed, 1 – no, the court answered question negatively, 2 – yes, court answered question in affirmative, 3 – yes, but error was harmless, 9 – court gave a mixed answer to question.

[12] This strategy was designed to limit problems associated with over specification of the models. Only 4 threshold issues were excluded due to the probability standard. Threshold issues asso-

ciated with erroneous standards, arbitrary and capricious standards, diversity of the parties, and

questions involving which state law should control the outcome did not exhibit significant rela-

tionships in any of the specifications.

[13] Issue controls related to criminal disputes include questions involving jury instructions, sen-

tencing, defective indictment, search and seizure, and other inadmissible factors. Civil law va-

riables include identifiers for questions related to due process, executive orders, state policies or

laws, weight of evidence, pre-trial procedure, trial procedure, post-trial procedure, attorney fees,

judge's discretion, alternative dispute resolution, injunction, summary judgment, federal vesti-

ture, or other civil issues. Civil law-government variables include identifiers for substantial evi-

dence, agency discretion, and appropriate general standing.

[14] The use of the presidential party median corresponds to the District Court appointment analy-

sis (Hendershot 2010). The Pearson correlation coefficient between the executive and party me-

dian measures is .98 for periods when both are available. In the earlier study, model results for

the party median yielded consistent appointment relationships for those periods when the presi-

dential measure was also available. The following estimates show a lack of ideological relation-

ships for the earliest District Court appointees and the lack of specific executive position could

be a possible explanation. However, an absence of attitudinal influence in this period can also be

found in the extant literature for appellate judges (e.g., Songer, Sheehan, and Haire 1999; 2000).

[15] Calculation matrices for the two baseline measures can be found in supplemental Table 4.

[16] The use of the mean common space value between executive and legislative branch actors

represents a divergence from the Giles, Hettinger, and Peppers (2001) calculation and is based

upon (Hendershot 2010). The goal of that analysis was to model inter-branch bargaining and

resistance and corresponds to Chase's (1972, 40) view of appointment. As Chase describes it,

the selection process is a product of delay strategies on behalf of executive branch actors and senators with courtesy ties that ultimately results in a consensus or independent executive choice. If the traditional courtesy norm is relevant, then the ideological preferences of District Court nominees should be a function both of executive and senator preferences, which can readily be captured by the use of an inter-branch mean calculation.

[17] A calculation matrix for the selection point measure can be found in supplemental Table 5.

[18] For nominees associated with two senators of the same or opposite party, the most distant senator was used. For nominees associated with a senator from both parties, the senator of the same party was used.

[19] The difference for Regime 4 is evidence of a non-traditional, or ideological, courtesy norm. For nominees associated with a senator from both parties, the senator of the opposite party is invoked for the final regime calculation.

[20] A calculation matrix for the confirmation point measure can be found in supplemental Table 6.

[21] For the sample of decisions, the mean and maximum years of service by appointment regime are: 13.9 and 45; 10.6 and 41; 8.6 and 23; and 3.8 and 7, respectively. The date of the District Court decision was not available, so this measure reflects the elapsed time from the judge's year of commission and the year of the appellate outcome.

[22] To conserve printing space, model results with the 22 issue control estimates are presented in Tables 7 through 10 of the supplemental web appendix.

[23] The PRE value is a ratio of the correctly predicted number of outcomes for the model versus the modal category of outcomes, which in this case always is a conservative decision.

[24] A Clarify simulation was not possible when also incorporating the recommended circuit/year weights.

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Figure 1: Baseline ideal point measures – executive and traditional courtesy point values

U.S. District Court Judges Appointed – 1901 to 2006

Note: See supplemental Table 4 for calculation matrix. Both measures capture executive and / or home state senator positions at the time of the initial nomination to the Senate.
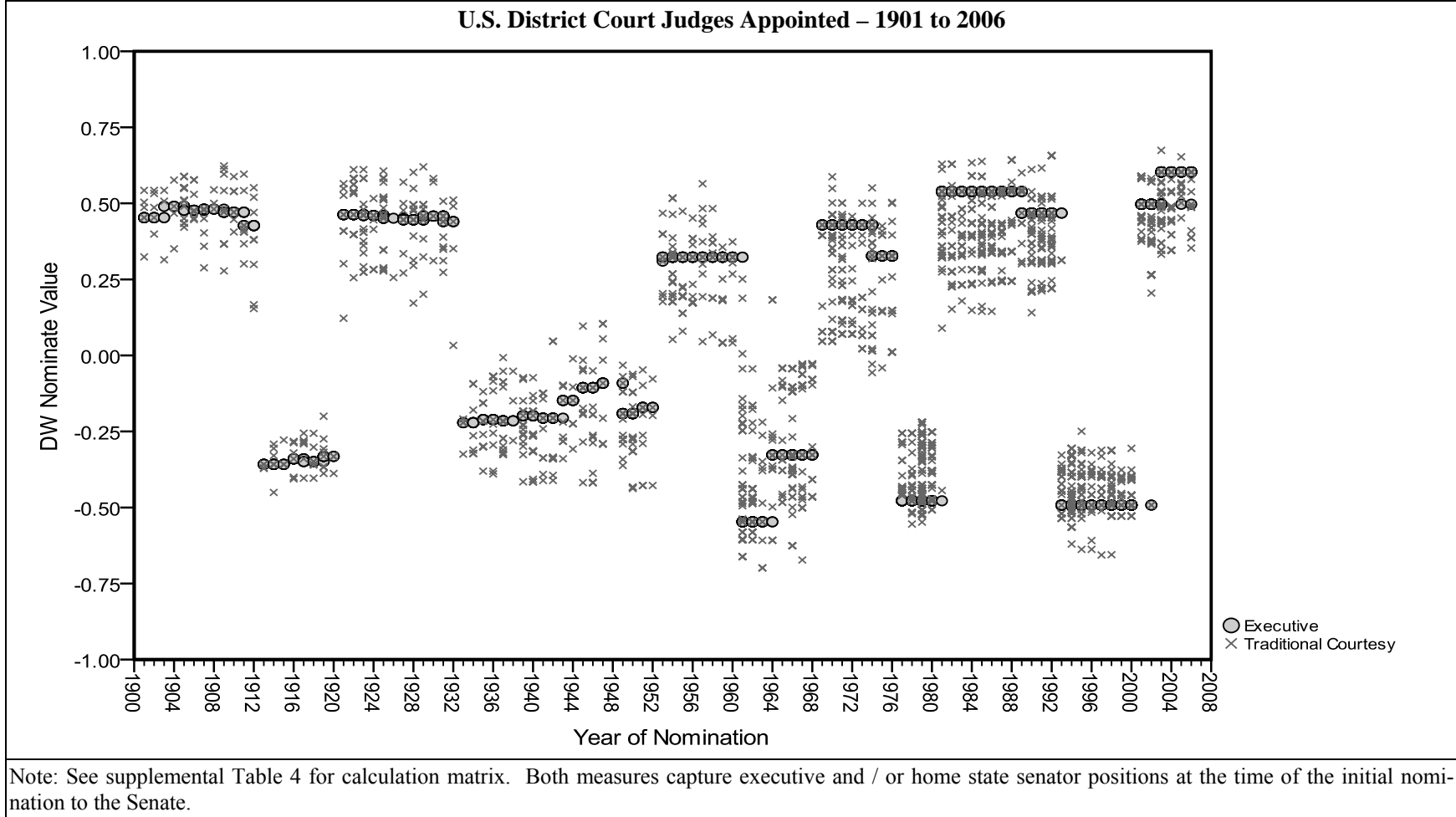
Figure 2: Alternative ideal point measures - selection and confirmation points

U.S. District Court Judges Appointed – 1901 to 2006

Note: See supplemental Tables 5 and 6 for calculation matrices. Selection measure captures the executive, home state senator or committee chairman positions at the time of the initial nomination. Confirmation measure captures the executive, home state senator, committee chairman, or opposite party median at the time of confirmation.
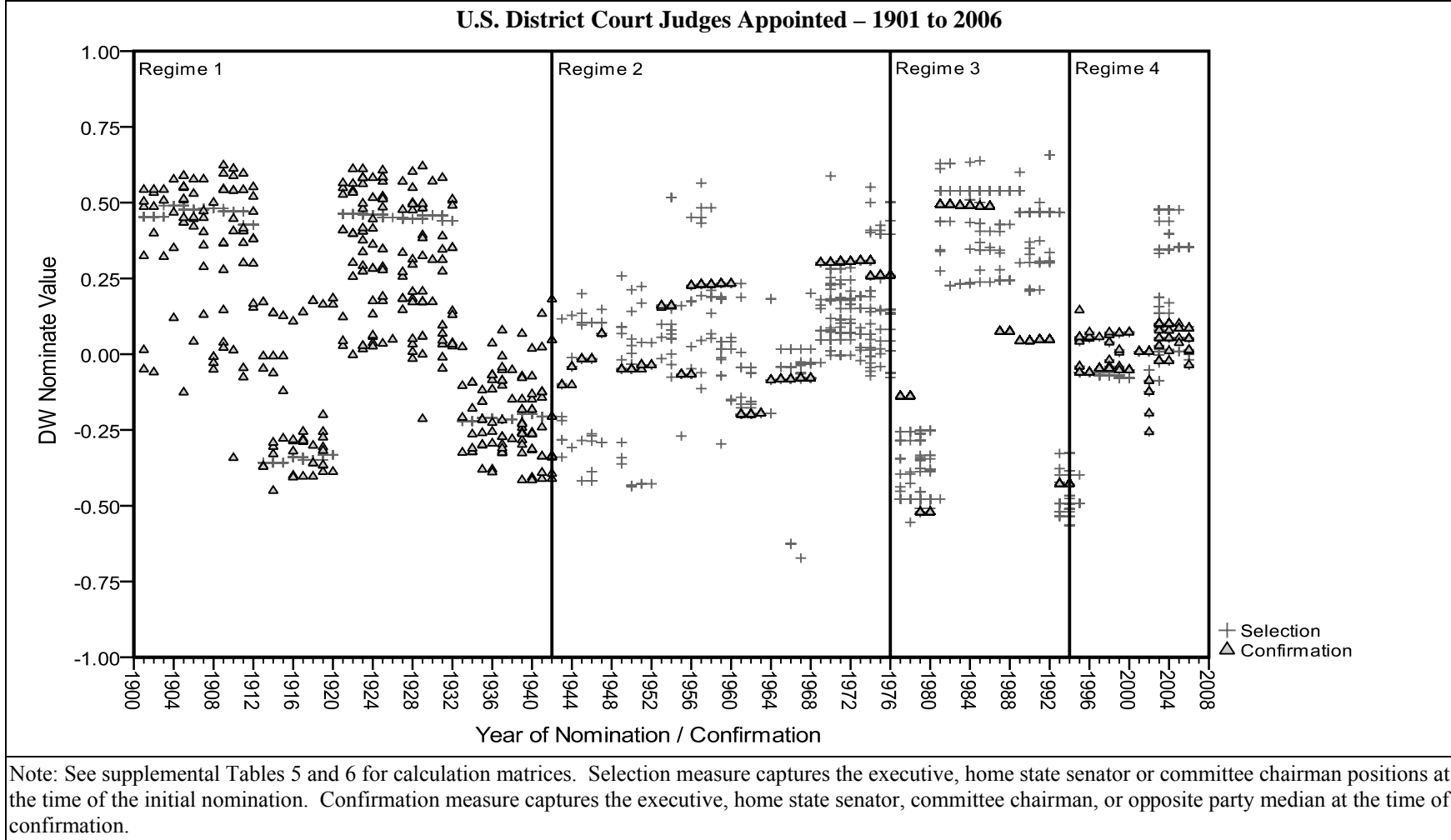
Figure 3: Predicted probability distributions for the confirmation cycle model
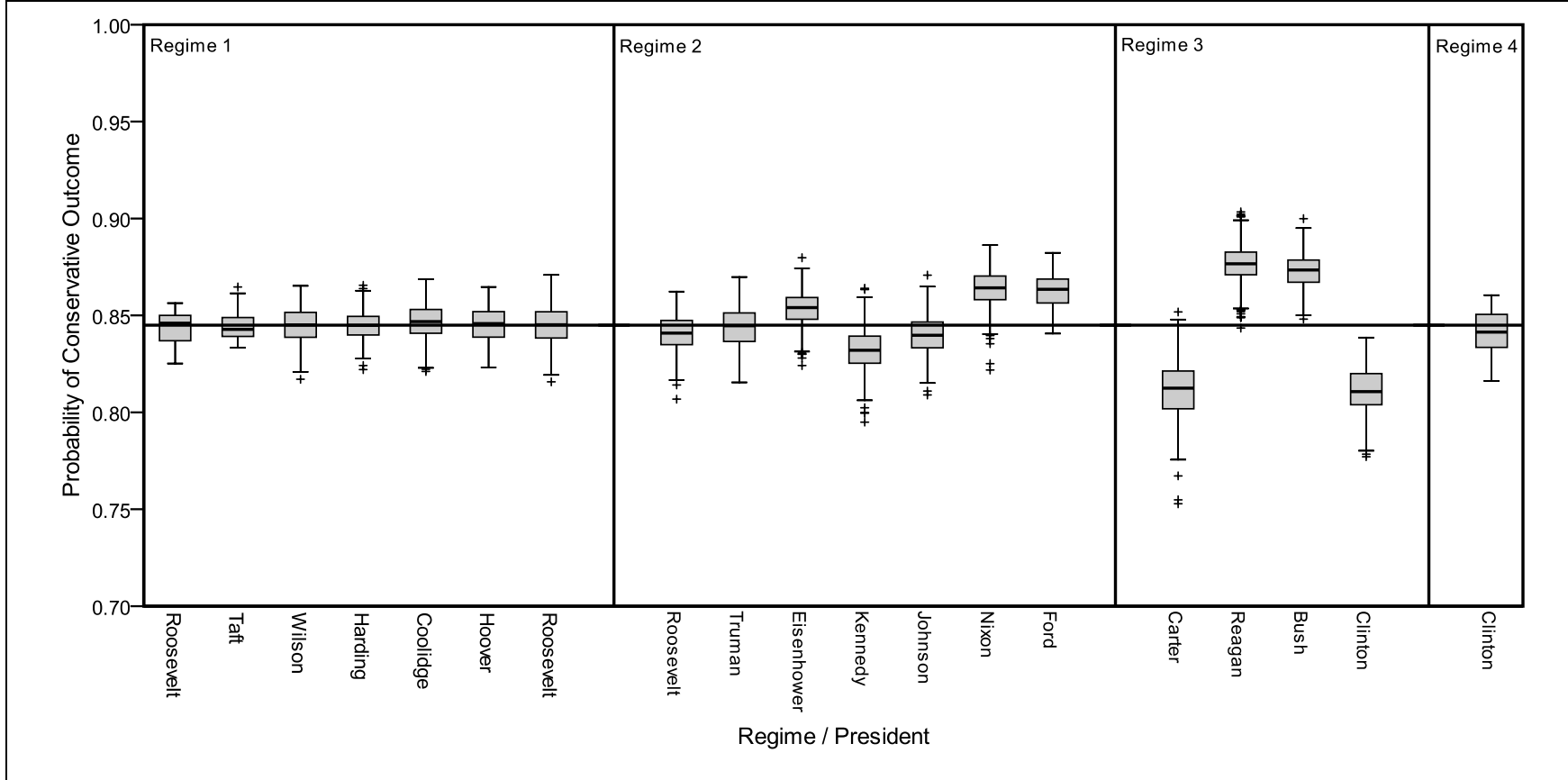
Note: Probabilities generated with King, Tomz, and Wittenberg's (2000) Clarify software. Estimated model is an unweighted specification of the confirmation cycle ideal point (Model 8 in Table 4). In Regime 1, the ideal point represents a null value (i.e., 0). In Regime 2 and 4, the ideal point represents the mean value of the confirmation point measure for each president. In Regime 3, the ideal point is the executive point for each president. The horizontal reference line is the mean predicted probability (84.5) for all observations with the preference measure set to 0. The outer tails of the box plot represent a 95% confidence interval.

Table 1: Weighted Logit Estimates of the Likelihood of a Conservative Vote
U.S. District Court Judges Appointed in Regime 1 – 1901 to 1942

	Null Configuration Model 1	Executive Point Model 2	Traditional Courtesy Model 3	Selection Point Model 4	Confirmation Point Model 5
Ideal Point		.06 (.21)	.10 (.20)	*N/A*	-.08 (.23)
Years of Service (natural log)	.18 * (.08)	.18 * (.08)	.18 * (.08)		.18 * (.08)
Issue Controls Omitted	___	___	___		___
Constant Value	.40 (.23)	.39 (.23)	.39 (.23)		.41 (.23)
N	1169	1169	1169		1169
Wald ~ χ^2	76.90 ***	76.93 ***	76.79 ***		77.89 ***
Log-Likelihood	-672.19	-672.15	-672.04		-672.12
Correct Predictions	784	785	787		780
PRE	8.33	8.57	9.05		7.38
LR Test (Null)		.09	.31		.14
LR Test (Exec)			.22		.05
LR Test (Trad)					*-.17*

Note: Source of data is the original and updated U.S. Courts of Appeals Database. Full table with issue control parameters can be found in supplemental Table 7. The sample includes at least one case outcome for 239 judges or 64.1% of the 373 judges appointed during the regime. The mean and maximum numbers of observations per judge are 4.9 and 34. Estimation is weighted by the recommended circuit/year values. Robust standard errors are in parentheses. * $\rho<.05$, ** $\rho<.01$, *** $\rho<.001$, two tailed tests.

Table 2: Weighted Logit Estimates of the Likelihood of a Conservative Vote
U.S. District Court Judges Appointed in Regime 2 – 1943 to 1976

	Null Configuration Model 1	Executive Point Model 2	Traditional Courtesy Model 3	Selection Point Model 4	Confirmation Point Model 5
Ideal Point		.47 ***	.64 ***	1.18 ***	1.14 ***
		(.15)	(.16)	(.29)	(.29)
Years of Service (natural log)	-.19 **	-.21 **	-.20 **	-.21 **	-.20 **
	(.07)	(.07)	(.07)	(.07)	(.07)
Issue Controls Omitted	___	___	___	___	___
Constant Value	1.91 ***	1.96 ***	1.96 ***	1.95 ***	1.90 ***
	(.17)	(.17)	(.17)	(.17)	(.17)
N	2916	2916	2916	2916	2916
Wald ~ χ^2	227.34 ***	230.79 ***	232.31 ***	236.00 ***	234.87 ***
Log-Likelihood	-1236.11	-1229.95	-1226.84	-1226.04	-1227.15
Correct Predictions	2322	2323	2331	2331	2326
PRE	7.33	7.49	8.73	8.73	7.96
LR Test (Null)		12.31 ***	18.54 ***	20.14 ***	17.93 ***
LR Test (Exec)			6.23 *	7.83 **	5.61 *
LR Test (Trad)				1.60	*-.61*

Note: Source of data is the original and updated U.S. Courts of Appeals Database. Full table with issue control parameters can be found in supplemental Table 8. The sample includes at least one case outcome for 594 judges or 88.8% of the 669 judges appointed during the regime. The mean and maximum numbers of observations per judge are 4.9 and 29. Estimation is weighted by the recommended circuit/year values. Robust standard errors are in parentheses. * $\rho<.05$, ** $\rho<.01$, *** $\rho<.001$, two tailed tests.

Table 3: Weighted Logit Estimates of the Likelihood of a Conservative Vote
U.S. District Court Judges Appointed in Regime 3 – 1977 to 1994

	Null Configuration Model 1	Executive Point Model 2	Traditional Courtesy Model 3	Selection Point Model 4	Confirmation Point Model 5
Ideal Point		.37 *	.47 **	.37 *	.40 *
		(.15)	(.18)	(.16)	(.19)
Years of Service	.00	.02	.02	.02	.01
(natural log)	(.11)	(.11)	(.11)	(.11)	(.11)
Issue Controls Omitted	___	___	___	___	___
Constant Value	1.42 ***	1.39 ***	1.39 ***	1.39 ***	1.43 ***
	(.27)	(.27)	(.27)	(.27)	(.27)
N	1529	1529	1529	1529	1529
Wald ~ χ^2	128.48 ***	134.02 ***	135.07 ***	133.70 ***	134.14 ***
Log-Likelihood	-607.16	-603.84	-603.39	-604.29	-604.45
Correct Predictions	1245	1247	1250	1249	1246
PRE	5.65	6.31	7.31	6.97	5.98
LR Test (Null)		6.65 **	7.55 **	5.75 *	5.43 *
LR Test (Exec)			.90	*-.90*	*-1.22*
LR Test (Trad)				*-.1.80*	*-2.12*

Note: Source of data is the original and updated U.S. Courts of Appeals Database. Full table with issue control parameters can be found in supplemental Table 9. The sample includes at least one case outcome for 555 judges or 77.0% of the 721 judges appointed during the regime. The mean and maximum numbers of observations per judge are 2.8 and 14. Estimation is weighted by the recommended circuit/year values. Robust standard errors are in parentheses. * $\rho<.05$, ** $\rho<.01$, *** $\rho<.001$, two tailed tests.

	Null Configuration Model 1	Executive Point Model 2	Traditional Courtesy Model 3	Selection Point Model 4	Confirmation Point Model 5	Traditional Cycle Model 6	Selection Cycle Model 7	Confirmation Cycle Model 8
Table 4: Weighted Logit Estimates of the Likelihood of a Conservative Vote **U.S. District Court Judges Appointed in Regimes 1, 2, 3, and 4 – 1901 to 2000**								
Ideal Point		.25 ** (.09)	.34 *** (.10)	.33 ** (.12)	.40 ** (.13)	.43 *** (.10)	.56 *** (.13)	.56 *** (.13)
Years of Service (natural log)	-.10 * (.05)	-.10 * (.05)	-.10 * (.05)	-.10 * (.05)	-.10 * (.05)	-.10 * (.05)	-.10 * (.05)	-.10 * (.05)
Issue Controls Omitted	___	___	___	___	___	___	___	___
Constant Value	1.51 *** (.12)	1.52 *** (.12)	1.52 *** (.12)	1.50 *** (.12)	1.51 *** (.12)	1.53 *** (.12)	1.51 *** (.12)	1.50 *** (.12)
N	5680	5680	5680	5680	5680	5680	5680	5680
Wald ~ χ^2	382.63 ***	387.94 ***	389.80 ***	389.12 ***	392.49 ***	392.64 ***	395.07 ***	397.10 ***
Log-Likelihood	-2584.51	-2580.08	-2577.79	-2579.90	-2578.92	-2574.82	-2573.96	-2573.79
Correct Predictions	4364	4359	4364	4363	4360	4372	4372	4379
PRE	4.15	3.79	4.15	4.08	3.86	4.74	4.74	5.25
LR Test (Null)		8.88 **	13.44 ***	9.22 **	11.19 ***	19.39 ***	21.10 ***	21.47 ***
LR Test (Exec)			4.08 *	.35	2.31	10.51 **	12.22 ***	12.59 ***
LR Test (Trad)				-4.22	-2.25	5.95 *	7.66 **	8.03 **

Note: Source of data is the original and updated U.S. Courts of Appeals Database. Full table with issue control parameters can be found in supplemental Table 10. The sample includes at least one case outcome for 1,437 judges or 72.7% of the 1,976 judges appointed from 1901 to 2001 (a year prior to the end of the sample of decisions). The mean and maximum numbers of observations per judge are 4.0 and 34. Estimation is weighted by the recommended circuit/year values. Robust standard errors are in parentheses. * $\rho<.05$, ** $\rho<.01$, *** $\rho<.001$, two tailed tests.