Explaining the Incidence and Timing of Congressional Responses to the U.S. Supreme Court

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Abstract

Sparked by interest in game-theoretic representations of the separation-of-powers, empirical work examining congressional overrides of Supreme Court statutory decisions has burgeoned in recent years. Much of this work has been hampered, however, by the relative rarity of such events; as has long been noted, congressional attention to the Court is limited, and most Court decisions represent the last word on statutory interpretation. With this fact foremost in our minds, we examine empirically a number of theories regarding such reversals. We apply a split-population duration model to the survival of Supreme Court statutory interpretation decisions. This approach allows us to separate the factors which lead to the event itself (i.e., the presence or absence of an override in a particular case) from those which influence the timing of the event. We find that case-specific factors relating to the salience of a case are an important influence in the incidence of overrides, while Congress- and Court-specific political influences dominate the timing at which those overrides occur. By separating the incidence and timing of overrides, our results yield a more accurate and nuanced understanding of this aspect of the separation-of-powers system.
1. Introduction

With the rise of the modern regulatory state, the interaction of the legislative and judicial branches has become among the most important for defining U.S. public policy. Particularly regarding issues of statutory interpretation, courts, and most notably the U.S. Supreme Court, have come to occupy a central role in shaping the rules and procedures by which government operates. And in what one scholar has called a “continuing colloquy” (Paschal 1992), the Court and Congress have, at times, engaged in a dynamic and repeated dialogue over the meaning of the law. As a result, the importance of statutory construction and interpretation in the legislative and judicial branches has risen considerably.

One result of this increased significance has been a renewed interest in outlining the relationship between the branches, particularly in those instances where their goals regarding policy do not coincide. Central among these have been studies, both theoretical and empirical, of congressional responses to Supreme Court opinions regarding statutory interpretation. The theoretical contours of this relationship are well developed, both regarding the incidence of such responses (see, e.g., Eskridge 1991a; Eskridge and Ferejohn 1992; Ferejohn and Weingast 1992; McCubbins, Noll, and Weingast 1992; Spiller and Spitzer 1995), and the extent to which the Court considers, and is constrained by, those responses in its decision making (e.g. Gely and Spiller 1990; Martin 2000; Schwartz, Spiller, and Urbiztondo 1992; Spiller and Gely 1990). By contrast, empirical studies of congressional overrides, while plentiful, have often reached contradictory conclusions regarding the incidence, causes, and effects of such actions (e.g. Eskridge 1991a; Henschen 1983; Ignagni and Meernik 1994; Ignagni, Meernik, and King 1998; Meernik and Ignagni 1997; Stumpf 1965).
Widely overlooked in these studies is the empirical fact that, as a general matter, most of the Court’s cases receive no attention by Congress, and thus are not even candidates for a potential override. Here, we distinguish, both conceptually and statistically, between cases which are unlikely ever to be overridden and those which are. This distinction allows us to test for separate effects of a range of covariates on whether an override will ever occur, as well as their influence on when such an event happens. This model represents a substantial improvement over previous work; most importantly, it allows us to distinguish between different theories of the causes of such overrides, and to recover reliable and accurate estimates of override probabilities even in the presence of very few actual responses by Congress. Drawing upon a wide assortment of perspectives on the question of overrides, we provide an integrated test of the relative influence of the whole range of factors important to such overrides.

We begin by reviewing theoretical explanations for congressional responses to the Court and discuss the contrasting expectations these models provide for the incidence of those responses. We then formulate a model which allows us to assess the relative impact of factors relating to congressional and interest group signals, case-specific characteristics, and political factors derived from spatial models of Congress-Court interaction on both the probability and timing of those responses. We go on to address issues of data and operationalization, and continue with an analysis of the incidence of congressional overrides of Supreme Court statutory decisions during the Warren, Burger and Rehnquist eras. Adopting a split-population event history approach allows us to distinguish the effects of covariates on the probability of an override from its timing. We conclude with a discussion of the implications of our research.
2. Congressional Overrides: Models and Influences

The relationship between Congress and the Supreme Court is a classic theme in the study of American political institutions (e.g. Murphy 1964; Pritchett 1961; Warren 1935). More recently, this study has been advanced by a theoretical approach that incorporates the preferences of multiple actors in the several branches of government into a statutory decision-making game (Eskridge 1991a, Eskridge 1991b; Ferejohn and Shaposhnikov 1990; Ferejohn and Weingast 1992; Gely and Spiller 1990; Segal 1997). Separation-of-powers games typically involve a series of moves by various political actors. The key to the expected moves is a spatially conceived range that encompasses the policy preferences of select decision-makers. We refer to this range of preferences as the set of Pareto optimals.¹ Within the set of Pareto optimals, “there does not exist an alternative point that makes everyone else at least as well off” (Krehbiel 1988, 271). That is, Congress cannot revise the policy outcome of a decision that falls inside this set without making some key members of Congress worse off. Once the set of Pareto optimals is defined, the policy making actions of the Supreme Court can be examined to determine where they fall relative to that range. If the outcome of the Court decision falls outside this set, Congress can revise the policy outcome and (at least one of) the members Congress that define the set of Pareto optimals will be better off than if the policy is not changed. In such models, then, the expectation is that any Congress will pass new legislation to override any Supreme Court decision that falls outside the set of Pareto optimals.

¹This range has also been referred to as the “constraint set” (Schiavoni and Groseclose 2001, 132), the “set of non-reversible decisions” (Spiller and Gely 1992, 471), the “set of feasible equilibria” or the “contract set” (Spiller and Gely 1992, 467), the “Pareto set” (Spiller and Tiller 1996, 509) and the “set of politically viable interpretations” (Ferejohn and Weingast 1992, 268).
In every instance, theoretical models of the separation-of-powers require that, in an instance where Congress is unhappy with a decision of the Court, it will act to overturn the offending decision. Moreover, the common assumption that all actors possess complete and perfect information implies an absence of overrides in equilibrium; instead, “the Court will push legislation as close to its ideal point as possible without getting overturned by Congress (Segal 1997, 30).” This general expectation comports well with empirical research on congressional overrides, which has consistently shown that the incidence of such overrides is very small. The earliest of such studies, for example, noted that congressional overrides were “rare” (Note 1958, 1336), and Henschen’s (1983) study of labor and antitrust decisions found that only 12 percent of all such decisions provoked any kind of congressional response. Similar figures are reported for more recent research as well (e.g. Eskridge 1991a; Solimine and Walker 1992).

Thus, one potential explanation for the relative rarity of overrides is that the Court, by taking account of the preferences of Congress in its decision making, only rarely renders decisions which the Congress finds objectionable. In this way, Congress may exert influence over the decisions of the Court without ever having to exercise its power to override statutory decisions (cf. Weingast and Moran 1983). Under such a model, the rare instances of actual overrides will be little more than unsystematic “noise.” While this perspective is the one generally favored by game-theoretic approaches to the separation of power system, it is inconsistent with the work of Segal (1997) and others, who find little or no influence of congressional preferences on Supreme Court decision making.

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Another important implication of such studies is whether the justices anticipate such congressional action and modify the outcome of the decision in order to avoid an override (e.g. Segal 1997). We do not explore that question here, but focus instead on congressional reactions to Supreme Court decisions.
Alternatively, overrides may also occur if the Congress only addresses particularly important or egregious cases of statutory misinterpretation. That is, while a “sincere” Court may issue numerous rulings which contravene the preferences of the Congress, only a fraction of those cases may rise to the level of importance necessary to prompt a congressional response. Under this explanation, we would expect that the Court will freely rule against the desires of Congress, and that some subset of the cases in which it does so subsequently will be overridden. At the same time, a large number of less important statutory interpretations will never be reconsidered by the Congress; whether because of their scope, subject matter, or other characteristics, such cases will not be targets for override attempts. In such a model, contravention of the congressional will is a necessary but insufficient cause for an override to occur; decisions consistent with congressional intent will stand no chance of override, and only a fraction of those which are inconsistent with congressional intent will be the target of such actions.

Empirically, these two approaches offer fundamentally different views of the process driving the occurrence of statutory overrides. Implicit in the separation-of-powers view is the notion that all cases are subject to congressional response, conditional on their location in the policy space. By contrast, our case salience model suggests that only particularly important cases will be likely to provoke congressional responses, and that a significant portion of cases will, by virtue of their relative lack of importance, be essentially “immune” from congressional review. Here, we adopt a modeling strategy that allows us to evaluate these competing perspectives.

3. Hypotheses, Data and Methodology

While much of the extensive theoretical work on separation-of-powers games involves congressional overrides of Supreme Court decisions, empirical evaluations of these
theories are less common.\(^3\) Two studies that provide a more or less systematic assessment of some aspects of the separation-of-powers approach are those of Spiller and Gely (1992) and Segal (1997). The former focuses on how the preference alignments between Congress and the Court as a whole influence Supreme Court decision making, showing that the Court defers to the preferences of Congress. By contrast, Segal (1997) examines the responsiveness of individual justices' behavior to the preferences of the Congress and concludes that the justices do not defer to congressional preferences. In neither case, however, are the specific congressional decisions to override the statutory decisions of the Supreme Court the primary focus of inquiry.

We examine congressional overrides of Supreme Court statutory decisions using cases selected from the *United States Supreme Court Judicial Database* (Spaeth 1997). We restrict our analysis to those cases "in which the Court hears oral argument and which it decides by a signed opinion. These are the Court's so-called formally decided full opinion cases" (Spaeth 1997, 78). Decrees, judgements of the Court, memorandum decisions, and per curiam decisions are therefore excluded. Furthermore, because we are interested only in cases involving federal statutory interpretation, we include in our analysis only cases in which statutory interpretation was the basis for the decision. These include cases that deal with interpretation of a federal statute, treaty, or court rule, and those involving interpretation of a federal executive order, or an administrative order or rule. Records

\(^3\) The bulk of the empirical work in this vein involves interpretive case studies. Gely and Spiller (1990), for example, outline the legislative proposals and changes in congressional membership that led to overrides of two Supreme Court statutory interpretation decisions. Epstein and Walker (1995) turn away from statutory interpretation and consider the habeas corpus cases that followed the Civil War. While providing some supportive evidence, these studies fall short of a systematic test of the separation-of-powers approach.
citing judicial review, supervision of lower courts, diversity jurisdiction, or federal common
law are therefore excluded. In order to ensure that our cases are among those examined by
Eskridge (1991a), we further limit our analysis to decisions on civil rights or civil liberties
issues, and to those decided during the years 1967-1989. The result is a sample of 609
cases distributed over 23 years.

Our primary variable of interest is the presence and timing of congressional
overrides of the Court’s decisions, coded from the list provided by Eskridge (1991a).
Eskridge defines overrides as “statutes that ‘modified’ the holding or reasoning of a federal
statutory decision. The statute does not have to have entirely nullified the court’s statutory
decision for me to have counted it as an override” (1991a, 419). We analyze the duration of
time, in years, between the decision of the Court and the subsequent override by Congress.
Of the 609 decisions we examine, 42 (or 6.9 percent) were overridden by Congress during
the period examined; we address the treatment of censored (i.e., non-override) cases
below.

The central factor driving overrides in all separation-of-powers models is the relative
policy location of the case outcome vis-a-vis the relevant congressional actors: cases which
fall within the congressional set of Pareto optimals will not be overridden, while those
outside that range will. Alternative specifications of the set of Pareto optimals have been
derived from the positive theories of congressional decision making. Two such models are
the distributive (or committee control) model (Shepsle 1979, Shepsle and Weingast 1987),

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4 Though the term begins in October, the vast majority of opinions are issued in the
part of the term that takes place in the next calendar year. Decisions from the 1967 term
are therefore treated as 1968 decisions and matched with 1968 congressional preferences,
and so forth.

5 We also rely on footnote 4 in Eskridge (1991a) to identify the decisions involved in
the 1991 Civil Rights Act.
and the partisan (or majority party) model (Cox and McCubbins 1993). The former takes as central the role of committees as gatekeepers, and defines the set of Pareto optimals as a function of committee and floor preferences. In the partisan model, by contrast, the key players are presumed to be certain members of the majority party, whose control the agenda of legislation by virtue of their majority status. In addition, Segal (1997) proposed the multiple-veto model. This specification of this set of Pareto optimals includes the preferences of the Rules Committee and both Judiciary committees, the chairs of those committee, and party leadership in both chambers and represents the broadest range of potentially critical actors of the three models.\footnote{See Segal (1997) for an introduction to these models.}

To evaluate the separation-of-powers thesis, we code the policy outcome of each case in our data, according to its location relative to the set of congressional preferences, under the assumption of a multiple veto model. This policy outcome is equal to the mean of the justices' preference scores for the justices who joined the majority opinion.\footnote{Justices' preference scores are calculated using each justice's percentage of liberal votes in constitutional civil rights and civil liberties cases during the years 1953-1991 (Spaeth 1997). The additional years capture votes by Hugo Black and Harold Burton; we include these two justices because both served as members of Congress. The scores for these two justices are then regressed on their Poole (1998) common space scores for congressional preferences, and the resulting linear equation is then applied to transform the preference scores of all justices onto the same scale as the Poole common space scores.}

That score is compared to the set of Pareto optimals derived from the multiple veto model (e.g. Segal 1997, 1998).

The multiple veto set of Pareto optimals is derived from the preferences of: the median member of the Judiciary committees in the House ($J^h_{\text{CC}}$) and Senate ($J^s_{\text{CC}}$), the chairs of the Judiciary committees in the House ($J^h_{c\text{CC}}$) and Senate ($J^s_{c\text{CC}}$), the median member ($R_{\text{CC}}$) and chair of the Rules committee in the House ($R_{c\text{CC}}$) the
median member of the House (H) and Senate (S), the Speaker of the House (Sp(CC)), the Senate Majority Leader (SML(CC)), and the members of each chamber associated with overriding a presidential veto \((H_{33}, \& S_{33})\) or \((H_{67}, \& S_{67})\).\(^8\)

In addition, the set of Pareto optimals includes an indifference point for all actors listed above except the presidential veto points and the chamber medians. The indifference points are included because we assume that these actors have gatekeeping power and changes to the legislation are possible once it passes beyond the control of that gatekeeper. This assumes the final legislative outcome will come from a conference committee: \(CC = (H+S)/2\). The indifference points are based on the corrected formula in Segal (1998). The general form for deriving the indifference points is \(2G-CC\) (where \(G\) stands for the particular gatekeeper). Thus, the Pareto optimal sets are defined as

\[
\begin{align*}
\text{min} & \quad [H_{33}, S_{33}, J^{H}(CC), J^{S}(CC), J^{C}_{H}(CC), J^{C}_{S}(CC), R(CC), R_{c}(CC), \text{Sp}(CC), \text{SML}(CC)], \\
\text{max} & \quad [H, S, J^{H}(CC), J^{S}(CC), J^{C}_{H}(CC), J^{C}_{S}(CC), R(CC), R_{c}(CC), \text{Sp}(CC), \text{SML}(CC)],
\end{align*}
\]

when the president is a Democrat, and

\[
\begin{align*}
\text{min} & \quad [H, S, J^{H}(CC), J^{S}(CC), J^{C}_{H}(CC), J^{C}_{S}(CC), R(CC), R_{c}(CC), \text{Sp}(CC), \text{SML}(CC)], \\
\text{max} & \quad [H_{67}, S_{67}, J^{H}(CC), J^{S}(CC), J^{C}_{H}(CC), J^{C}_{S}(CC), R(CC), R_{c}(CC), \text{Sp}(CC), \text{SML}(CC)],
\end{align*}
\]

when the president is a Republican.\(^9\) In addition, following Segal (1998) and others (e.g. Schiavoni and Groseclose 2001), we incorporate the effect of a possible presidential veto. Our variable thus takes a value of one when the decision outcome falls outside the set of

\(^8\) The congressional preference scores are based on Poole's first dimension common space scores (1998).

\(^9\) The veto override points associated with the party of the president are reversed from those stated in Segal (1997, 1998) and Schiavoni and Groseclose (2001) because the Poole common space scores are scaled opposite the ADA scores that these authors use.
Pareto optimals, and zero otherwise; separation-of-powers models predict that a decision outside this set will have a significantly higher probability of congressional override.

In addition to theories stemming from separation-of-powers models, research on congressional overrides of the Supreme Court has also investigated factors other than institutional preferences that correlate with congressional reversals of Supreme Court decisions (Henschen 1983; Ignagni and Meernik 1994; Meernik and Ignagni 1997; Note 1958; Solimine and Walker 1992; Stumpf 1965). Drawing on insights obtained from these studies, we address a number of variables which we believe to be influential on both the probability of an eventual override and on its timing. A key factor unifying these variables is their influence on case salience: the likelihood that Congress will take up action in response to the decision. Summary statistics for the data are presented in Table 1.

[Table 1 about here]

*Amicus curiae briefs.* Interest group participation as amicus curiae is frequently explored as a correlate of subsequent congressional reversal. The earliest such research on the subject emphasized the importance of group pressures, noting that "nearly all (reversals) enjoyed the almost unanimous support of the politically articulate groups affected by the Court's decision" (Note 1958, 1336; see also Stumpf 1965). More recently, Banks and Weingast (1992) have argued that interest groups serve an important function in overcoming the informational asymmetries that exist between the bureaucracy and Congress (see also Epstein and O'Halloran 1995; Hopenhayn and Lohmann 1996). The fact that interest groups serve a similar informational function for Congress following a Supreme Court decision is supported by the widespread finding that the presence or
frequency of amicus participants in a given statutory decision is positively correlated with the occurrence of congressional reversal (Ignagni and Meernik 1994; Meernik and Ignagni 1997; Solimine and Walker 1992).

Data on interest group participation before the Court are available from the United States Supreme Court Judicial Database, Phase II (Gibson 1997) for the years 1965-1986. We obtained data for the 1987-1988 terms from the list of amicus participants appearing in the opinions as reported on Lexis. The data in the Phase II database represent the total number of “amicus curiae briefs filed in the case by non-individual interests” (Gibson 1997, 236), including interest groups, corporations, and governments. Records in the Phase II database reflect docket numbers rather than individual citations. For citations with multiple dockets, we eliminated from the count the filers that appear in more than one docket. Each group appears only once in the count of filers for a citation, even if that group filed an amicus brief that applied to more than one docket number. The number of briefs per case ranges from zero to 28; because we expect diminishing returns with additional amici, we use the natural logarithm of this variable in our analyses.

Congressional amicus briefs. In addition to interest groups and other organizations, members of Congress occasionally file briefs amicus curiae in cases before the Court. To the extent that filing such briefs is a costly endeavor, both in terms of time and of money (Caldeira and Wright 1990), the presence of such briefs should indicate a higher level of congressional interest in the case at hand. We include in our analysis a variable indicating the presence of such a brief, coded one where such participation was noted and zero for cases without such amici. The search for congressional amicus curiae briefs begins with the list provided by Epstein, Segal, Spaeth, and Walker (1994, Table 7-2). A Lexis search for variations of the term “amicus briefs” within the same sentence as variations on “Senate,”
"House," "Speaker," "Representative," and "Congress" identified additional congressional amicus briefs. Together, these two sources identified one or more amicus curiae briefs filed by members of Congress in 17 cases in our data.

U.S. loses case. In addition to amicus participants, litigants, especially the losing party, may trigger the reversal process. Litigants dissatisfied with the ruling of the Court may, in statutory cases, take their case to the Congress in an effort to have the Court's decision overridden (Note 1958). Furthermore, because of its unique position as both a frequent litigant before the Supreme Court and as the "consummate repeat player" vis-à-vis the law (Galanter 1974), the United States federal government is uniquely positioned to adopt such a strategy.  Eskridge (1991a), for example, finds that, though the United States lost in only nine percent of all cases included in his study, it was the losing litigant in almost 28 percent of those which were subsequently overridden by Congress.10

To examine the effect of the U.S. as a losing litigant we include a variable that takes a value of zero when the United States does not appear as a litigant, or when the United States is the winning litigant, and equals one when the United States is listed as a litigant but loses the case on the merits. Three categories of parties described in the Supreme Court Judicial Database are used to flag the case as one in which the United States participated: when the party is listed as the United States, when the case involves an administrative agency, or when the case has the Attorney General as a party to the litigation. If the executive branch is successful in counteracting its losses in the legislative branch, we would expect both a high probability of an override, and a more rapid congressional response, in such cases.

10 These figures are derived from his Table 9 (Eskridge 1991a, 351).
Multiple legal provisions. A related issue of information lies in the nature of the case being decided. Statutory interpretation cases vary widely in their degree of complexity: some present a single, relatively narrow issue, while others address multiple legal issues. On one hand, we might expect that those cases involving several distinct matters of law would have a correspondingly wider impact, and would thus receive greater attention by groups, making them more likely subjects for overrides. Counterbalancing this, however, is the fact that complex cases, presenting a slew of claims and issues, may not be good “vehicles” for achieving policy goals in comparison to simpler cases. In addition, in a somewhat different context, Vanberg (2001) presents evidence that legislative bodies may find it easier to respond negatively to Court decisions (in his case, to evade their ruling) in instances where the subject matter in question is highly complex. As an indicator of case complexity, we include an indicator for cases in which multiple legal claims were addressed by the Court. This variable is also coded from the Database, and takes on a value of zero for cases concerning a single legal issue, and one if more than one legal provision was addressed.

Unanimous decision. Scholars have also studied the effects of factors relating to the Court’s decision-making and opinion writing processes. For example, a unanimous decision is frequently considered as a correlate of congressional responses. Because of the maximal institutional support for such decisions, most studies hypothesize that unanimity decreases the likelihood of a negative congressional response, and many find some support for that hypothesis (Henschen 1983; Hettinger 1999; Nagel 1969; Solimine and Walker 1992). In contrast, Ignagni and Meernik (1994) argue and find support for the claim that, because such decisions represent a threat to congressional power, unanimity actually increases the likelihood of a congressional override. Here, we include a variable indicating that the
decision of the Court was made without dissent. Our unanimity variable is derived from
the vote variable in the Supreme Court Judicial Database, which “pertains to the number of
justices who agree with the disposition made by the majority...and not to the justices' vote
on any particular issue in the case” (Spaeth 1997, 85). We code this variable one if the
decision was rendered unanimously, and zero otherwise.

Lower court reversal. A similar case-specific variable relates to the disposition of the
case by the Supreme Court. Cases in which the Supreme Court reverses the decision of the
lower court reflect a greater extent of disagreement over the meaning of the law than those
which result in an affirmance; as a result, such cases may be particularly attractive
candidates for congressional overrides, for at least two reasons. First, a reversal by the
Supreme Court may indicate a stronger case, from the perspective of the losing party, than
does an affirmance, thus increasing their perceived chances of securing a congressional
override. This is because the presence of a reversal indicates that at least two lower court
judges came to the opposite conclusion of the Court in this case; conversely, affirmance by
the Supreme Court indicates that the losing party's argument was also unable to carry the
day in a lower court. Second, the presence of the lower court decision provides a ready-
made policy alternative to the decision of the Supreme Court.11 Accordingly, we include a
variable coded one if the Supreme Court's decision on the merits reversed the decision of
the court it reviewed, and zero if the Supreme Court's decision resulted in an affirmance,
with the expectation that such reversals will have higher odds of an override.

11 The Harvard Law Review Note makes a similar point, noting that “nearly all these
reversals involved a return to a 'common understanding' which had been disrupted by the
Court's decision” (1958, 1336).
To separate the effects of our independent variables on the probability of an eventual override from their influence on the duration until that override, we adopt the split-population survival technique of Schmidt and Witte (1989). These models are also known as “cure models” in the biometrics literature, and have both a long history (e.g. Boag 1949; Berkson and Gage 1952) and widespread applications in recent years (e.g. Farewell 1982; Aalen 1992; Kuk and Chen 1992; Longini and Halloran 1996; Maller and Zhou 1996; Tsodikov 1998). The intuition behind these models is that, while standard duration models require a proper distribution for the density which makes up the hazard (i.e., one in which all subjects in the study eventually fail), cure models allow for a subpopulation which never experiences the event of interest. This is typically accomplished through a mixture of a standard hazard density and a point mass at zero.\[12]

Cure models offer the potential for substantial improvements in the manner in which political scientists study duration data. In many, if not most, cases in the social sciences, assuming that all observations will eventually fail is unrealistic. Schmidt and Witte’s (1989) study of criminal recidivism, for example, notes that typical long-term studies place long-range eventual recidivism at around 50%, rather than the 100% implied by most duration models. In our example, the use of a split-population model allows us to assess whether some factors render certain cases effectively “immune” from congressional override. In particular, it allows us to determine whether factors relating to case salience lower the probability of an override ever occurring, and to evaluate whether allowing for a “cured” fraction of cases provides a significantly better fit to the data on such overrides.

\[12\] See the Appendix for a derivation of the parametric cure model used here.
4. Results and Analysis

Our dependent variable is the length of time between the Court's decision and its subsequent override by Congress. To model this duration, we adopt a log-logistic duration model. This assumes that \( \ln(t) \) follows a logistic distribution, with mean \(-\ln(\lambda)\) and variance \( \pi^2/(3p^2) \). This yields the hazard function:

\[
h(t) = \frac{\lambda p(\lambda t)^{p-1}}{1 + (\lambda t)^p}
\]

We specify \( \lambda_i = \exp(-X_i \beta) \) and \( p = 1/\sigma \) as the scale parameter (Greene 1997, 989). This model is "even more flexible than the Weibull and Gompertz distributions" (Blossfeld and Rohwer 1995, 183); in particular, the log-logistic allows for nonmonotonic hazards. Because some minimal amount of time is required to pass a law overturning a decision of the Court, we expect that the hazard of an override will first rise rapidly, then decline over time. This intuition is borne out in Figure 1, which indicates that the highest numbers of overrides occurred one, two and six years after the decision was handed down by the Court.

[Figure 1 about here]

Estimation results for standard and split-population duration models of congressional overrides are presented in Table 2.\(^{13}\) Examining first the standard log-logistic model, we note that the model is a significant improvement over the null (intercept-__________

\(^{13}\) Models were estimated using the SURVIVAL routine in LIMDEP 7.0.
only) model, and that a number of our hypotheses are supported by the data. Both the total number of amicus briefs and the presence of congressional amici lead to significantly shorter durations, while unanimous decisions take substantially longer to be overridden. The coefficient for the United States as a losing party is in the expected direction, but fails to reach traditional levels of statistical significance \((p = 0.064,\) one-tailed). Conversely, cases with multiple legal provisions and decisions outside the set of Pareto optimals both exhibit longer durations, findings contrary to our expectations, but also fail to reach traditional levels of statistical significance. Additionally, no significant difference is found in the hazards of cases involving a reversal and those resulting in an affirmance.

[Table 2 about here]

While a number of the variables behave as expected, the model is also clearly a suboptimal fit to the data. With all independent variables at their means, the estimated value of \(\lambda\) is 0.0009, which translates to a median survival time of 1066 years. The low estimated hazards and long expected durations are a result of the model's assumption that all decisions will eventually be overridden, irrespective of how unlikely this is in practice.

Accordingly, we next turn to the estimates derived from the split-population model. Note immediately that the fit of the model to the data is substantially better than that for the logistic model alone. A likelihood-ratio test of the two models yields a value of 39.42 \((\sim \chi^2(9), p < 0.001)\), indicating a significantly better fit for the latter model; moreover, the model's predictions are also significantly more accurate. For example, the mean probability of a case ever being overridden, fixed at 1.0 in the previous model, is now estimated at 0.09, close to the proportion of cases in the sample in which an override was observed (0.07).
Similarly, conditional on a case being overridden, the median survival time is now 2.01 years, a value that falls very close to the observed median age for overridden cases in the data. Moreover, because the hazard and survival functions are now conditional on the case being (eventually) overridden, there is substantially greater variation in these functions for the split-population models than for the standard model.

These differences are illustrated graphically in Figures 2 and 3, which present the estimated mean hazard and survival functions, respectively, for the standard and split-population models estimated in Table 2. Two results are immediately apparent from Figure 2. First, the estimated hazards for the standard model are excessively low, and exhibit very little variation over time, as compared to those from the cure model. Second, the latter model's estimates display the non-monotonicity also evident in Figure 1; the hazard of a congressional override rises to a peak 2 to 3 years after the decision is handed down, then declines monotonically after that. While the pattern of decay is obscured by the scale in the figure, the estimated hazard for the standard logistic model displays no such property.\(^\text{14}\) Similarly, Figure 3 illustrates the associated survival estimates over a 40 year period, again with data set at their mean values. While the survival estimates for the standard model begin and remain high, declining only very slowly, those for the split-population model decline rapidly. Thus, while the standard model estimates a 0.96 probability of a case surviving override beyond its tenth year, the corresponding probability for the split model (conditional on an observation's eventual override) is a mere 0.05. This represents an important difference in substantive terms: while the standard model would have us believe that most cases are overridden only after a long time has passed, the split

\(^{14}\) The log-logistic hazard is nonmonotonic when \(p\) is greater than one, and monotonically declining for \(p < 1\).
model instead demonstrates that the average "age" of an overridden decision is relatively low.

[Figures 2 and 3 about here]

Turning to the effects of the individual variables in the split-population model, we note both similarities and differences from the results obtained using the standard logistic model. The coefficient estimate for the spatial variable is again counter to expectations in the probability of override model and in the expected direction in the duration model, but again, neither even approaches statistical significance, and their substantive magnitude is very small. Examining further the variables for amicus participation, we see that the observed effect of amicus participation is to increase the hazards for those observations subject to override, while failing to influence the probability that an override will be forthcoming. Conversely, the presence of one or more congressional amici has a large positive influence on the probability of an override, but little or no effect on the duration until such an override occurs; such amici increase the probability of an eventual override by 0.43 with other variables at their median levels.\footnote{Note that the estimated baseline probability of an override, holding variables at their median levels, is 0.075.} These results suggest, then, that congressional amici are qualitatively different from others: while group pressure may act to speed along an override once one has been initiated, groups appear to have little influence in getting Congress to address the issue in the first place. By contrast, congressional amici appear to play a key role in the likelihood of an eventual override, presumably because they indicate the extent to which Congress is cognizant of the decision in question. Previous
studies have shown that this awareness is a key factor in the likelihood of congressional action following a statutory interpretation decision of the Court (e.g. Hettinger 1999).

Our understanding of the influence of the federal government in cases in which it is the losing party is not enhanced by the split-population model. We see no effect of the U.S. losing a case on the conditional duration to override and that variable does not significantly increase the probability that an override will be forthcoming. In contrast, the influence of unanimity is seen to operate only on the probability of an override; the impact of unanimity is to decrease appreciably the probability of a case ever being overridden, but has little or no effect on the duration until such an override occurs. This finding is interesting, for it suggests that the Congress does, in fact, give greater weight to unanimous decisions of the Court, counter to the explanation that it simply waits for membership turnover on the Court to ensure no further responses before overriding a law with which it disagrees.

Also interesting is the compound effect of multiple legal provisions. In contrast to the standard model, here we see that this variable both decreases the probability that a congressional response will occur (at median levels, by 0.11) but also significantly shortens the duration until that override. In this sense, the variable appears to work at cross-purposes, a fact which likely explains the insignificant estimate obtained using the standard model alone. We believe that this finding is due to the dual character of such decisions: while their complexity makes them less attractive targets for a response, we suspect that the opportunity for a “partial” override of only one part of the larger decisions makes them occur more rapidly once they are so targeted.

The effect observed for decisions reversing that of the lower court is notably different than in the standard model: such cases experience marginally lower (though not significant) probability of an eventual override as well as more rapid congressional response
when such overrides occur. The year in which the decision was handed down by the Court does not significantly influence the probability of an override.\textsuperscript{16} Finally, more recent decisions are correspondingly more likely to be the target of an override than are older ones though the estimate fails to reach traditional levels of significance ($p = 0.06$).

In summary, our analysis is consistent with earlier work (e.g. Segal 1997) which found little evidence of separation-of-powers influences on Congress-Court interactions.\textsuperscript{17} In general terms, our more nuanced investigation suggests that the primary determinants of Congress’ response to the Court’s decisions lie in characteristics of the case itself, signals relating to the case’s significance, and the institutional position of the Court vis-à-vis the decision, rather than factors related to simple policy congruence.

5. Conclusion

Our study reexamines and extends previous work on the interplay of Congress and the Supreme Court, as well as offering new insights into legislative-judicial interaction in the American context. Our chief contribution, we believe, is to present a more empirically accurate model of congressional overrides of Supreme Court statutory decisions, one which allows us to assess the underpinnings of a number of widely-cited separation-of-powers models of that process. The fact that most such decisions face a very small risk of reversal by Congress, we show, has important implications for modeling the incidence of those overrides. In particular, we find that interest group signals, in the form of the presence of

\textsuperscript{16} Because time dependence in the duration model is captured by the estimate of $\sigma$, we omit the year of decision from that part of the model.

\textsuperscript{17} We should also note that, by choosing the multiple-veto model for our analysis, the cases which fall outside the set of Pareto optimals will be the most extreme ones. These results therefore constitute a relatively weak test of the null hypothesis, and make the absence of any effects all the more persuasive.
amici curiae in the case in question, may exert pressure to speed along congressional overrides but appear to do little to increase the congressional awareness which leads to such overrides in the first instance. Conversely, congressional amici appear to increase the probability of an override while having little or no effect on the duration until that override occurs. And a number of variables which, in conventional duration analyses, exhibit no influence on overrides are in fact shown to operate at cross-purposes: factors relating to legal complexity and Supreme Court reversals of prior decisions both decrease the probability of an override occurring and increase the conditional hazards of such action.

Our example also points out the potential usefulness of split-population models to scholars studying political duration data more generally. Social scientists often encounter data for which the assumption that all observations will eventually fail is unrealistic. Cure models offer a more realistic alternative to modeling these data, as well as the potential for providing new insights into the data generating process in question. These applications also suggest future directions for the current research. Chief among these is extending the models to handle time-varying covariates; the ability to assess the influences of such variables is one of the main advantages of event history models, both in the current context (e.g. Hettinger 1999) and more generally (Box-Steffensmeier and Jones 1997). Such an extension would allow us to consider the impact of variables which change over the “life-span” of a Court decision, most notably variations in the composition of the Congress.
Appendix: Derivation of the Split-Population Duration Model\textsuperscript{18}

We begin our discussion with a standard parametric model for continuous-time duration data, where the duration of interest \( t \) is assumed to have a distribution function \( f(t, \theta) \), with \( \theta \) a parameter vector to be estimated. Define \( F(t, \theta) = \Pr(T \leq t), \ t > 0 \) as the corresponding cumulative density, where \( T \) represents the duration defined by the end of the observation period. The associated survival function (defined as the probability of survival to time \( t \) and denoted \( S(t, \theta) \)) is then equal to \( 1 - F(t, \theta) \). If \( f(t, \theta) \) is the density function of \( F(t, \theta) \), then we can write the hazard rate:

\[
h(t, \theta) = \frac{f(t, \theta)}{S(t, \theta)}
\]

This value is akin to the conditional probability of a failure at time \( t \) given that no failure has occurred prior to \( t \) (e.g. Box-Steefensmeier and Jones 1997).

We consider a model for the duration \( t \) which splits the sample into two groups, one of which will eventually experience the event of interest (i.e., “fail”) and the other which will not. We define the latent variable \( Y \) such that \( Y_i = 1 \) for those who will eventually fail and \( Y_i = 0 \) for those who will not; define \( \Pr(Y_i = 1) = \delta_i \). The conditional density and distribution functions are defined as:

\[
f(t_i | Y_i = 1) = g(t, \theta) \\
F(t_i | Y_i = 1) = G(t, \theta),
\]

\textsuperscript{18} This section draws extensively on the presentation of these models in Schmidt and Witte (1989).
while leaving \( f(t \mid Y_i = 0) \) and \( F(t \mid Y_i = 0) \) undefined.\(^9\) Let \( R_i \) be the observable indicator of failure, such that \( R_i = 1 \) when failure is observed and \( R_i = 0 \) otherwise. For those observations which fail during the observation period, we observe \( R_i = 1 \) and their duration. Since these observations also necessarily have \( Y_i = 1 \), we can write the unconditional density for these observations as:

\[
\Pr(Y_i=1)Pr(t_i \leq T_i \mid Y_i=1) = \delta_i g(t_i, \theta)
\]

where \( T_i \) indicates the censoring time. In contrast, for those observations in which we do not observe a failure (\( R_i = 0 \)), this may be due either because \( Y_i = 0 \) (the observation will never fail) or because \( t_i > T_i \) (the observation is censored). The unconditional density for observations with \( R_i = 0 \) is therefore:

\[
\Pr(Y_i=0) + \Pr(Y_i=1)Pr(t_i > T_i \mid Y_i=1) = (1-\delta_i) + \delta_i G(T_i, \theta).
\]

Combining these values for each of the respective sets of observations, and assuming independence across observations, the likelihood function is:

\[
L = \prod_{i=1}^{N} \delta_i g(t_i, \theta)^{R_i} [1 - \delta_i + \delta_i G(T_i, \theta)]^{1-R_i} \tag{A.1}
\]

The probability \( \delta_i \) is typically modeled as a logit, and can include explanatory variables:\(^{20}\)

\(^9\) Because \( Y_i = 0 \) implies that the observation will never fail (and thus the duration will never be observed), the probabilities for \( g(t_i \mid Y_i=0) \) and \( G(t_i \mid Y_i=0) \) cannot be defined.

\(^{20}\) Note that this model is identified even when the variables in \( \delta_i \) are identical to those in the model of duration. This means that one can test for the effects of the same set
\[ \delta_i = \frac{\exp(Z_i \gamma)}{1 + \exp(Z_i \gamma)} \]

Note that when \( \delta = 1 \) (i.e. when all observations will eventually fail), the equation reduces to the standard general duration model with censoring.

______________________________

of variables on both the incidence of failure and the duration associated with it (Schmidt and Witte 1989).
References


<table>
<thead>
<tr>
<th>Variable</th>
<th>Mean</th>
<th>Standard Deviation</th>
<th>Minimum</th>
<th>Maximum</th>
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</thead>
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<td><strong>Duration</strong></td>
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<tr>
<td>Censored (N = 567)</td>
<td>12.81</td>
<td>6.15</td>
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<td>0</td>
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<td>(\ln(\text{Amici}))</td>
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<td>1.02</td>
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<td>3.33</td>
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<tr>
<td>Multiple Legal Provisions</td>
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<td>0.47</td>
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<tr>
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<td>Lower Court Reversal</td>
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<td>79.32</td>
<td>6.21</td>
<td>67</td>
<td>89</td>
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Note: N = 609; see text for details of coding.
### Table 2


<table>
<thead>
<tr>
<th>Variables</th>
<th>Standard Duration Model</th>
<th>Split-Population Model</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Pr(No Override)</td>
<td>Duration to Override</td>
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<tr>
<td>(Constant)</td>
<td>6.079**</td>
<td>7.147*</td>
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<td>optimals</td>
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<td>ln(Amici)</td>
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<td>(1.002)</td>
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</table>

Note: N = 609. Numbers in parentheses are standard errors. Asterisks indicate $p < .05$ (one-tailed). Estimates reflect variables influence on ln(duration); i.e., positive coefficients correspond to longer durations at higher values of the variable in question. Models are log-normal in duration and logit for Pr(No Override).
Figure 1

Figure 2

Standard and Split-Population Hazard Estimates

Note: Figure plots estimated hazards at data means/medians for $t \in [1,40]$. Split-population estimates are conditional on $Y_i = 1$ (i.e., the observation's eventual "failure"). See text for details.
Figure 3

Standard and Split-Population Survival Estimates

Note: Figure plots estimated survival probabilities at data means/medians for $t \in [1,40]$. Split-population estimates are conditional on $Y_i = 1$ (i.e., the observation's eventual "failure"). See text for details.