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Ducking Trouble:

Congressionally-Induced Selection Bias in the Supreme Court's Agenda

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Abstract

Existing studies of congressional influence on Supreme Court decision-making have largely failed to recognize the fact that the Court has a discretionary docket. We model the effects of congressional preferences on the certiorari decision, and find strong evidence that the Court's constitutional agenda is systematically influenced by Congress. The Court's docket is significantly less likely to contain cases wherein there are large congressionally-induced deviations between what the Court would *like* to do, and what it *can* do in its final rulings. This selection bias in the Court's docket can lead to considerable uncertainty in estimating the effects of congressional constraint on the Court's final decisions, including a failure to properly reject the null hypothesis of no constraint.

Over the last two decades many scholars have studied whether the Supreme Court moderates its rulings when faced with an ideologically hostile Congress (Gely and Spiller 1990, Eskridge 1991, Spiller and Gely 1992, Ferejohn and Weingast 1992, Segal 1997, Hansford and Damore 2000, Epstein, Knight and Martin 2001, Spriggs and Hanford 2001, Segal and Spaeth 2002, Bergara et al 2003, Friedman and Harvey 2003, Sala and Spriggs 2004, Martin 2006, Harvey and Friedman 2006). These studies have typically failed to find evidence that such moderation exists (but see Bergara et al 2003, Friedman and Harvey 2003, and Harvey and Friedman 2006).

The focus of these studies has been exclusively on the Court's final judgments, however. As a result analysts may have failed to find evidence of congressional influence on the Court despite the existence of such influence. Because the Supreme Court's jurisdiction is discretionary, it is entirely possible that the Court refrains from hearing cases wherein it can anticipate a hostile congressional response to its most preferred ruling on the merits. This selection bias might then diminish the observability of judicial deference to Congress.

Despite the importance and obviousness of this point, only one study of which we are aware has extended the analysis of interbranch interaction to the agenda setting or certiorari stage of the Court's decisionmaking (Epstein et al 2002a). However, there is reason to question the Epstein et al results.

In this paper we look for congressional influence on the Court's agenda by estimating the probability that the constitutionality of a congressional statute will be reviewed by the Court, conditional on the expected utility gains to the median Justice from the final ruling on the merits. Under a model of congressional constraint, the predicted ideological location of the Court's opinion is in part a function of congressional preferences, while under a model of judicial

independence from Congress, congressional preferences play no role in the location of that opinion. We estimate these models using two different assumptions about the median's Justice's utility function and a variety of models of congressional constraint, for both generic and "landmark" statutes.

Using this research design we find strong evidence that the Court's constitutional agenda is systematically influenced by congressional preferences, under both approaches to the median Justice's utility function, across a range of models of congressional constraint, and for both generic and landmark statutes. The Court is significantly less likely to review statutes when there are large congressionally-induced deviations between what the Court would *like* to do, and what it *can* do in its final rulings. Instead, the Court's docket disproportionately comprises cases wherein the Court will face little congressional constraint on its most preferred decisions.

For example, using our first approach to the median Justice's utility function, and a floor median model of congressional constraint, we find that the likelihood that the Rehnquist Court during its 1994-2001 terms would review a generic liberal statute enacted between 1987 and 1994 increased by 123% as a result of the rightward turn of Congress after 1994, and by 500% for landmark liberal statutes. Similarly, we find that the sample of liberal statutes that the Court is predicted to review *before* that rightward congressional turn is reduced by 64% for generic statutes, and by 83% for landmark statutes, relative to the sample the Court would have reviewed had Congress been as conservative as it was in the latter half of the 1990s.

This paper thus provides the first estimates of which we are aware of the magnitude of the congressionally-induced selection bias in the Court's constitutional docket. As a result of this selection bias, the sample of cases heard by the Court contains significantly less variation in the degree of congressional constraint than does the universe of possible cases. As we show, this can

lead to considerable uncertainty in estimating the effects of congressional constraint on the Court's final decisions, including a failure to properly reject the null hypothesis of no constraint.

1 Congressional Influence on the Certiorari Decision

Previous empirical work on the decision to grant certiorari largely has neglected the influence of separation of powers constraints. Scholars have looked at case specific factors that influence the grant of certiorari (Caldeira and Wright 1988, McGuire and Caldeira 1993), whether a Justice's vote to grant certiorari is conditioned on the likely outcome of the final merits decision (Boucher and Segal 1995, Caldeira et al 1999, Benesh et al 2002), and whether cert is used strategically by the Supreme Court to induce lower courts to adhere to the Court's preferences (Boucher and Segal 1995, Caldeira and Wright 1988, Caldeira et al 1999, Cameron et al 2000).

With only one exception, however, scholars have not examined the possible influence of congressional and presidential preferences on the certiorari decision. The exception is Epstein et al (2002a), who suggest that the proportion of cases reviewed by the Court that involve statutory interpretation, as opposed to constitutional interpretation, should be responsive to the ideological composition of the political branches.[1] Their argument follows from the median Justice's incentives with respect to the Court's final decisions on the merits in cases involving statutory interpretation. If there is an interpretation of a statute that the pivotal members of Congress prefer to the Court's interpretation, they can enact that interpretation following the Court's ruling. There thus may well be situations in which the Court is better off issuing a statutory interpretation that is not the preferred interpretation of the Court's median Justice, but is both unlikely to be overturned by a sitting Congress, and is preferable to the interpretation which

would be enacted by that Congress were it to act (Gely and Spiller 1990, Eskridge 1991, Spiller and Gely 1992, Ferejohn and Weingast 1992, Bergara et al 2003).

Extending this logic to the agenda setting stage, the Court may very well be less likely to grant certiorari to cases involving statutory interpretation when it faces an ideologically hostile Congress, given the deference to congressional preferences it may then be forced to display. Epstein et al thus expect the proportion of rulings issued by the Court involving constitutional interpretation to increase as the ideological distance between the Court and the current Congress increases.

Even in constitutional cases, however, there may be means by which the Congress can influence the Court in its constitutional decisions. The Congress can alter the Court's appellate jurisdiction, manipulate the number and composition of the lower federal courts, control the Court's budget, change the size and composition (via impeachment) of the Court, and refuse to implement Court decisions (Epstein et al 2002a: 414, see also Nagel 1965, Landes and Posner 1975, Stone et al 1991, Rosenberg 1991, Rosenberg 1992, Fisher 1993, Mcnollgast 1995, Epstein et al 2001, Cross and Nelson 2001, Segal and Spaeth 2002, and Martin 2006).[2]

What makes most congressional responses to the Court particularly threatening is that the Justices cannot simply respond to them by invalidating congressional actions, like they could an ordinary statute. Impeachment threats, budget-cutting, angry oversight hearings and threats to pack the Supreme Court or otherwise examine its internal procedures are not subject to judicial review at all. Indeed, the only congressional weapon that most likely is subject to review is jurisdiction-stripping, and the only firm precedent here unequivocally upheld congressional jurisdiction-stripping authority (See *Ex parte McCardle*, 74 U.S. 506 (1868)).

These opportunities for congressional action would seem to provide relatively powerful incentives for the Justices to be mindful of congressional policy preferences, even in constitutional cases. And while earlier analyses had largely failed to find evidence in support of this claim (Spriggs and Hansford 2001, Sala and Spriggs 2004, Martin 2006), Friedman and Harvey (2003) and Harvey and Friedman (2006) report strong evidence that in fact the Court is quite sensitive to the preferences of elected officials even in constitutional cases.[3]

The possibility that the Court's final rulings may be conditioned on congressional preferences has important implications for the Court's docket. The Court chooses the cases it wishes to review through the decision to grant a writ of certiorari. Presumably the median Justice prefers to review cases which promise her the greatest utility gains. However, the structure of congressional preferences, in conjunction with congressional control of policy instruments that may be used to punish the Court, may prevent the median Justice from issuing her most preferred rulings in some cases (thus decreasing the utility she receives from those rulings). Consideration of congressional preferences may thus affect the extent to which the median Justice supports granting cert in any given case.

However, estimating the effects of congressional preferences on the Court's certiorari decisions is not straightforward. Litigants may be strategic about their appeals to the Court. If litigants can anticipate which cases the Court is less likely to take because of congressional hostility, then they should be less likely to appeal those cases in the first place. One would then be unlikely to observe the Court's responsiveness to congressional preferences in the sample of cases for which writs of certiorari are requested. Testing a model of congressional constraint on the Court's docket by using a sample of certiorari petitions thus may be an ill-advised strategy.

We take account of the possible selection bias in the pool of certiorari petitions by eschewing an approach that looks only to cases, instead utilizing a "statute-based" approach that follows congressional statutes enacted between 1987 and 2001. Because we include all statutes enacted by Congress during this period in our sample, we eliminate the possibility that strategic action by litigants will bias the sample.[4]

We further restrict our attention to constitutional challenges to these statutes as a means of looking for congressional constraint in what is arguably the least likely place to find it. We estimate the probability that a statute is reviewed by the Court on constitutional grounds, as a function of the Court's predicted ruling on the merits, under two approaches to the median Justice's utility function, across a range of models of congressional constraint, and for both the full sample of statutes and a subset of "landmark" statutes. We find considerable evidence that the Court's constitutional docket, whether because of the Court's certiorari decisions, or because of the actions of litigants who anticipate those decisions, is in fact affected by congressional preferences.

2 Modeling the Merits Decision

We assume that at the time of the certiorari decision, the Justices vote based on their expectations about the outcome of the merits decision. We thus begin by modeling the merits decision, then address the certiorari decision.

We follow Epstein et al (2002a) in modeling the final merits decision as a choice in continuous rather than dichotomous space. We assume that both Justices and members of Congress have symmetric single-peaked preferences over a common left-right policy continuum.

We further assume that each congressional law L embodies some standard of constitutionality preferred by the enacting Congress, which may be expressed as a point on this

policy continuum. For example, when the Congress enacts a statute regulating gun possession on school grounds, we assume that this statute embodies the view of the pivotal members of that enacting Congress that it is within congressional authority, most obviously under the Interstate Commerce Clause, to regulate gun possession on school property, and that this standard of constitutionality may be represented by a point, L, on a policy continuum.

Such statutes, once enacted, are subject to constitutional challenge in the courts, including recourse to the Supreme Court.[5] Should a statute be reviewed by the Court, the Court's median Justice selects a point on the policy continuum (L') as a standard of constitutionality against which the law will be judged. We assume that the median Justice's utility from L' is a decreasing function of the distance between it and her ideal point (C): $U(L') = -|C - L'|$. Absent constraint, the Justice will set L' at her ideal point.[6]

Should the Court review our hypothetical statute regulating gun possession on school grounds, then, we assume that the median Justice, via the majority opinion, sets a standard of constitutionality which is the same as that embodied in the statute, which endorses a view of congressional authority that is even more expansive than that embodied in the statute, or which sets a constitutional standard for congressional authority that is more restrictive than that embodied in the statute. Absent institutional constraints, our median Justice would simply set this standard at her own ideal point.

If the Court faces institutional constraints, however, L' may not equal C. In this case, after the Court chooses L', the Congress could choose to punish the Court with retributive legislation. We assume that the Congress will do so only if all pivotal legislators prefer L to L'. Should at least one pivotal legislator be closer to (or equidistant from) the Court's ruling than to the original law, that member will choose either not to introduce, or to block, legislation

disciplining the Court. However, if the pivotal members are all closer to the law than to the Court's chosen constitutional standard, those members will act to ensure passage of punitive legislation.

In this constrained case, we hypothesize that the Court acts so as to avoid Court-punishing legislation. That is, the median Justice will set *L'* as close to her ideal point as possible, while yet forestalling punitive congressional action.[7] Under some configurations of ideal points and for some laws, the median Justice of the Court will be constrained in her rulings of constitutionality by congressional preferences. Under other configurations, she will not be constrained, and will simply use her ideal point as a standard of constitutionality.[8]

3 Modeling the Certiorari Decision

We hypothesize that the Justices look ahead to the merits decision at the time of voting to grant review. In modeling the Justices' certiorari behavior we assume that the median Justice is also the pivotal voter at the certiorari stage, and will be more likely to take cases which promise larger utility gains for her in the merits decision. This is not technically true, as the Supreme Court requires only 4 votes out of 9 in order to hear a case. However, Lax (2003) demonstrates that the effect of the Rule of Four in a one dimensional spatial model is primarily to lower the threshold for the magnitude of utility gains that must be realized by the median Justice before the Court agrees to hear a case. Therefore, it will still be the case under the Rule of Four that cases which promise larger utility gains for the median Justice should be more likely to be granted cert than cases which promise smaller utility gains for the median Justice.

At the time of the certiorari decision, then, the Justices can look forward to the merits decision and predict the likely location of *L'* for any congressional statute that they might review.

We model two different ways in which the Justices might prioritize granting review to some cert petitions above others.

First, we assume that the utility of granting certiorari for the median Justice is decreasing in the distance between her ideal point and the congressionally-induced constitutional standard she will be able to set in the majority opinion:[9]

$$U(\textit{Grant Certiorari}) = -|C - L'| \qquad (1)$$

If the Court is institutionally constrained, then the median Justice will prefer to take cases wherein she can set L' as close to her own ideal point as possible. Cases wherein congressional preferences will force L' to be located at some distance from the median Justice's ideal point generate less utility for the median, and are less attractive certiorari choices. We should thus find a negative relationship between $|C - L'|$ and the probability of review.[10]

If the Court is not institutionally constrained, on the other hand, then this expression collapses to zero and provides no information about which cases the Court will take. When the median Justice can locate all opinions at her own ideal point without fear of congressional reprisal, then congressional preferences should give us no insight into the Court's certiorari decisions.

However, the median Justice may also care about the utility gains she is able to achieve in a ruling, *relative to the status quo* represented by the statute under review. In our second approach to modeling the median Justice's utility function, the median Justice compares the utility gains she realizes from setting a new constitutional standard to those she realizes from leaving the statute alone:[11]

$$U(\textit{Grant Certiorari}) = -|C - L'| - (-|C - L|) = |L' - L| \qquad (2)$$

If the Court is institutionally constrained, then the utility gains for the median Justice from granting certiorari are increasing in the distance between the statute being reviewed and the standard which the Court can set; the median Justice prefers to hear the cases wherein she can move the status quo the furthest toward her own ideal point.[12] We should thus find a positive relationship between $|L' - L|$ and the probability of review.

If the Court is not institutionally constrained, then the first part of this expression collapses to zero and the utility to the Court from granting cert is equivalent to $|C - L|$, or the ideological distance between the Court and the statute being reviewed. The unconstrained median Justice prefers to review the statutes that are the most distant from her own ideal point; she can move these status quos the furthest toward her own ideal point. We should then find a positive relationship between $|C - L|$ and the probability of review.

4 The Statute-Based Test of Strategic Certiorari Behavior

We examine the question of congressional constraint using a "statute-based" approach. Rather than focusing solely on the cases presented to or heard by the Justices, we ask about the probability of an enacted congressional statute being reviewed by the Justices at some point during its life. We do this to avoid the inherent problems of selection bias in using cases as the units of analysis.[13]

Our statute-centered analysis begins with the 100th Congress, elected in 1986, and continues through the first year of the 107th Congress (2001). We track two pools of statutes through this period. The first pool consists of all public laws enacted by Congress. The public laws enacted by the 100th Congress were first available to be reviewed by the Supreme Court sitting in October of 1987, the second term of the Rehnquist Court.[14] We follow the fate of these laws, and all public laws enacted through 2001, through the Rehnquist Court's 2001 term. Table

1 reports the numbers of public laws enacted in each congressional year, and the number of years that the laws are followed for each group.[15] In total we follow the fate of 3,833 laws over a range of 1 to 15 years. An observation thus consists of law *i* observed in year *t*; we have 33,588 observations in this first pool.

The second pool of statutes consists of those statutes designated as "landmark" statutes by Stephen Stathis of the Congressional Research Service (Stathis 2003). This pool addresses the likelihood that most statutes in the first pool may be sufficiently inconsequential to be not worth litigating to the Supreme Court. The full sample of ordinary statutes is thus likely to be dominated by laws that are never challenged and thus never reviewed by the Court. Presumably looking only at landmark statutes should give us a clearer picture of the contours of congressional constraint, should it exist.

Table 1 also reports the numbers of landmark statutes enacted in each congressional year between 1987-2001, and the number of years that these statutes are followed in our analysis. In all we follow 141 landmark statutes over 1-15 years for a total of 1,210 observations in this second pool.

6.1 Dependent Variable

Because we are interested in whether (and when) these laws are reviewed by the Court, our dependent variable is dichotomous: a law is either reviewed and coded 1 in a given year, or is not reviewed and is coded 0 for that year. Struck laws are coded as 1 in the Term in which they are struck down and are then removed from the dataset; upheld laws are coded as 1 in the Term in which they are reviewed and then revert to 0 in subsequent Terms, giving the Court an opportunity to change its mind.[16]

In order to determine which laws were reviewed by the Court during this period, we used the United States Supreme Court Judicial Database (USSCJD) to identify all cases involving a federal statute in which the authority of the decision was cited as judicial review. We then read all cases to identify those in which a clear ruling of constitutionality (uphold/strike) was issued.[17] The cases involving struck laws were then checked against alternative sources, identifying some errors in the USSCJD (Zeppos 1993, Congressional Research Service 2001, Epstein 2002b).[18]

For each case we then identified the Congress which enacted the statute or part of a statute whose constitutionality was at issue. These statutes have been frequently amended. In the face of these frequent amendments, we adopted the following decision rule for the first pool of statutes (consisting of all public laws). First, we identified the specific section or sections of the statute actually being reviewed by the Court. We then looked at both the original enacting date and all reenactments of or amendments to this section or sections. As long as the challenged language of the statute remained substantially intact through all amendments and/or reenactments, we adopted the most recent reenacting or amending Congress as the enacting Congress.[19] In all, 42 of the 3833 laws in the first pool were reviewed by the Court on constitutional grounds between 1987 and 2001.

For the second pool of statutes, that consisting only of "landmark" statutes, we identified enactment date somewhat differently. First, we ignored minor amendments to landmark bills, i.e. amendments which themselves were not entitled to "landmark" status. Similarly, if a landmark bill incidentally amended an existing statute without changing the substance of that statute, and if that latter statute was then reviewed by the Court, we did not count that as judicial review of a landmark bill. But if a bill identified as a landmark bill included as a subsection a separate act, which was then reviewed by the Court, we did treat that as judicial review of a landmark bill. In

all, 16 of the 141 landmark statutes in the second pool were reviewed by the Court on constitutional grounds between 1987 and 2001.[20]

6.2 Independent Variables

Our empirical model requires us to compute measures of ideological distance between the Court and various pivotal members of Congress. Typically, researchers testing separation of powers models use measures of ideology which are not scaled in the same institutional "space" (Segal 1997, Segal and Spaeth 2002, Epstein et al 2002a). Here we use the measures developed by Michael Bailey, which use bridging observations to scale the Court, the Congress, and the President in the same space (Bailey 2007). The Bailey measures are available through 2002; because we model an October Term Court as looking ahead to the Congress which will be sitting as of January of the following year, we are able to observe the Court only through its 2001 Term.

Our empirical model also requires some assumptions about the legislative process. We here employ five prominent theories of congressional pivotality (Krehbiel (1998)). Three theories imply that legislation, should it reach the floor, will lie somewhere between the chamber medians (the Floor Median Model, the Committee Gatekeeping Model, and the Majority Party Gatekeeping Model). For these three models we take the midpoint between the enacting chamber medians as the measure of the ideology of a statute L. For the Majority Party Median and Veto-Filibuster Models, we measure statute ideology as the ideological midpoint of the set of pivotal legislators in an enacting Congress.[21]

For each Congress, we also identify the pivotal legislators' indifference points with respect to each statute L which may be reviewed by the Court. These indifference points, along with the ideological locations of each set of statutes, are then used to construct congressional constraint sets for each Court term.

For each of the congressional constraint sets that we define below, if the Court median's own ideal point lies within this set, then the Court is unconstrained and *L'* will be set at the Court median. If the Court median lies to the left (right) of the leftmost (rightmost) boundary of this set, then the Court is constrained and will set *L'* at this leftmost (rightmost) boundary. The constraint sets are as follows:[22]

The Floor Median Model: L, $I_H(L)$, $I_S(L)$

The Committee Gatekeeping Model: L, $I_H(L)$, $I_S(L)$, $I_{HJ}(L)$, $I_{SJ}(L)$

The Majority Party Gatekeeping Model: L, $I_H(L)$, $I_S(L)$, $I_{HP}(L)$, $I_{SP}(L)$

The Majority Party Median Model: L, $I_{HP}(L)$, $I_{SP}(L)$

The Veto-Filibuster Model, Democratic President: L, $I_H(L)$, $I_S(L)$, $I_{H146}(L)$, $I_{S34}(L)$, $I_{S60}(L)$

The Veto-Filibuster Model, Republican President: L, $I_H(L)$, $I_S(L)$, $I_{H290}(L)$, $I_{S67}(L)$, $I_{S41}(L)$

For the purposes of illustration, Figure 1 displays the leftmost and rightmost endpoints of the constraint sets for 1987 statutes under all five models, as well as the ideal point of the median Justice. This figure illustrates a phenomenon common to all constraint sets for pre-1994 statutes except for those generated by the Veto-Filibuster Model, namely that the Court lay to the right of the rightmost boundaries of all these constraint sets prior to the 1994 term.[23] For these statutes in these years, there is a considerable gap between what the Court would *like* to do and what Congress will *permit* it to do. Were the Court to review one of these liberal statutes during the 1987-1993 terms, it would be forced to locate its opinion at the rightmost boundary of the relevant congressional constraint set, a point which typically lies at some distance from the Court median's own ideal point (the distance being more precisely measured as $|C - L'|$).

As a result of the 1994 elections, the rightmost boundaries of all the constraint sets displayed in Figure 1 shift dramatically rightward to include the Court's median, through the 2001 term. This phenomenon is also common to the constraint sets for all other pre-1995 statutes.[24] After the 1994 elections, then, the Court was completely unconstrained in its rulings on these liberal statutes according to every legislative model. There is no longer a gap between what the Court would like to do and what Congress will permit.[25]

These constraint sets suggest that, if the Court is indeed constrained by the elected branches, prior to the 1994 elections the Rehnquist Court should have been hesitant to grant certiorari to cases challenging the constitutionality of the liberal statutes enacted by the Congresses sitting between 1987 and 1993. By a significant margin, those liberal Congresses preferred the existing liberal statutes to the rulings that the Rehnquist Court would have liked to have handed down. Were the Court to issue its most preferred rulings, it would have faced a substantial threat of reprisals from the Democratic Congresses. To avoid such reprisals, the Court would have had to have issued rulings considerably distant from its most preferred rulings. As a result, the Court would have received little utility from granting certiorari in these cases.

However, after the 1994 congressional elections, the utility gains to the Court from taking cases challenging the constitutionality of these liberal statutes would have jumped considerably. Now, the Court would have been liberated to decide those cases essentially at will. As a result the Court should have been significantly more likely to review liberal statutes during the 1994-2001 terms.

This pattern indeed appears to be reflected in the Court's docket. Between 1987 and 1993, 2163 total statutes were enacted by liberal Democratic Congresses and were available for review by the Court prior to its 1994 term. Taking into account the fact that most of these

statutes were available for review during multiple terms, we have 8991 liberal statute/year observations prior to the 1994 term. The Court reviewed 8 of these liberal statutes during this period, for an average rate of review of .0009. Between 1994 and 2001, there were 2418 liberal statutes available for review by the Court (including the additional 255 statutes enacted during the 1994 congressional session). These statutes were all available for review during each of the Court's 8 terms during this period, resulting in 19344 liberal statute/year observations during the 1994-2001 terms. The Court reviewed 23 of these liberal statutes during this period, resulting in an average rate of review of .0012. In other words, even after taking into account the fact that the Court had many more opportunities to review recently enacted liberal statutes after the 1994 congressional elections, we still find that the Court had a higher rate of review of those statutes after the 1994 elections than during the prior period.

However, these frequencies may in part be the product of other factors, including the temporal dependence of the rate of review. Perhaps statutes simply become more likely to be reviewed as they age, due in part to the fact that it takes time for legislation to make its way through the litigation process to the Supreme Court. In order to test the hypothesis of congressional influence more systematically, we require a more sophisticated approach.

9 Empirical Specifications and Results

Our data consist of individual laws observed over discrete units of time (years). In any given year, a law may be reviewed by the Court (and generate a value of 1), or not (and generate a value of 0). Laws that are reviewed and then struck are dropped from the dataset for subsequent years. We observe the laws for only a limited period of time, ending our period of observation with the close of 2001. Finally, there is some possibility that laws are more likely to be reviewed the longer they survive.[26] We thus require an empirical method which takes into account the facts

that we have a binary dependent variable, data which are "right censored," and potential temporal dependence. The appropriate method for analyzing this kind of data is grouped event history analysis (also known as duration, hazard or survival analysis) (Beck et al 1998). Grouped event history models are derived from continuous time event history models, which estimate the probability of an event occurring as a function of both the set of independent variables, and a baseline "hazard" rate.

We here apply the grouped version of the most common continuous time event history model, namely the Cox (1975) proportional hazards model. The Cox continuous time model, reported in Equation 3, is widely used because it allows the estimation of a baseline hazard rate which is unknown and possibly time varying.

$$h(t \mid \mathbf{X}_{i,t}) = h_0(t)e^{\mathbf{X}_{i,t}\beta} \tag{3}$$

In Equation 3, the instantaneous hazard or probability of an event occurring (h) as a function of the time t and the vector of independent variables measured for unit i at time t ($\mathbf{X}_{i,t}$) depends both on the latter (through the $e^{\mathbf{X}_{i,t}\beta}$ term) and on the possibly time varying baseline hazard ($h_0(t)$). Its grouped version is reported in Equation 4.

$$P(y_{i,t} = 1 \mid \mathbf{X}_{i,t}) = h(t \mid \mathbf{X}_{i,t}) = 1 - \exp(-e^{\mathbf{X}_{i,t}\beta + \kappa_t}) \tag{4}$$

In Equation 4, $y_{i,t}$ is the binary indicator of whether an event occurred to unit i within year t, $\mathbf{X}_{i,t}$ represents the observed values of the independent variables for the entire year t, and κ_t is a dummy variable marking the length of time the unit has been "at risk."

The model reported in Equation 4 is identical to a binary dependent variable estimated using a Poisson link function, with duration dummy variables included (Beck et al 1998). Following Beck (1998), we first included duration dummies in each estimation to capture

potential nonlinearities in the baseline hazard rate. We then tested these initial models against ones which included simpler linear trend terms.[27]

For all estimates we report below, likelihood ratio tests failed to reject the null hypothesis that the duration dummy variables were no better than linear trend terms to capture the effects of time on the baseline hazard rate. We thus report the results of estimations including that linear term (*Age of Law*), whose values range from 1-15. The standard errors are Huber (1967) robust standard errors, clustered by year of enactment. Finally, Poisson goodness-of-fit statistics confirm the appropriateness of a Poisson model for all estimations reported in Tables 2-5.[28]

The estimations reported in Table 2 test the hypothesis of a Court constrained in its cert decisions using the full sample, under our first approach to the median Justice's utility function. Column 1 reports the results from the Floor Median Model, whose predicted standard of constitutionality is represented by L_1'; Column 2 reports the estimates from the Committee Gatekeeping and Majority Party Gatekeeping models, whose predicted standards of constitutionality are represented by L_2' and L_3', respectively; Column 3 reports the results from the Majority Party Median Model, whose predicted standard of constitutionality is represented by L_4', and Column 4 reports the estimates from the Veto-Filibuster Model, whose predicted standard of constitutionality is represented by L_5'.

These tests provide strong support for the hypothesis of a constrained Court. In all four estimations, the coefficient on the variable $|C - L'|$ is in the predicted negative direction and is significant at conventional levels. The Court appears to be less likely to review the constitutionality of congressional laws when the standard it will be forced to set in its rulings is more distant from the median Justice's ideal point. If the Court were not constrained by congressional preferences, then we would expect the median Justice to set the standard of

constitutionality at her own ideal point in every case. We then would not expect to gain any insight into the certiorari decision by looking at *L'*, or the equilibrium standard of constitutionality which should result from the Court considering the preferences of the sitting Congress. Finally, in all four estimations the coefficient on the trend variable *Age of Law* is negative and significant, indicating that the Court is actually less likely to review the constitutionality of older laws.

The results from the analysis of landmark statutes, reported in Table 3, produce even larger coefficients for the congressional constraint variables, although we are unable to estimate the Veto-Filibuster Model with the landmark data due to insufficient variation in the degree of predicted congressional constraint. For the four legislative models that we are able to estimate, the coefficients on the variable $|C - L'|$ are again in the predicted negative direction, are larger than their counterparts for the full sample, and are significant at conventional levels. Relative to ordinary legislation, the Court appears to be even less likely to review the constitutionality of landmark statutes when the standard it will be forced to set in its rulings is more distant from the median Justice's ideal point. This makes intuitive sense; the Court should exhibit greater deference to congressional preferences when it is considering landmark statutes, relative to ordinary statutes. The coefficient on the trend variable *Age of Law* is again consistently negative, although it falls short of conventional levels of significance.

The estimates reported in Tables 4 and 5 test the hypothesis of a Court constrained in its certiorari decisions using our second approach to the median Justice's utility function, namely that the Court selects cases which promise the greatest utility gains for the median Justice relative to the status quo of the statute being reviewed. The first three columns of Table 4 directly test the hypothesis of an unconstrained Court, using the full sample of statutes. In these

estimations the probability that a law is reviewed by the Court is modeled as a function of $|C - L|$, where C is the Court median and L represents the law being reviewed by the Court. These estimations assume that congressional preferences play no role in the certiorari decision. If this assumption is correct, then we would expect to see a positive relationship between this variable and the probability of review: the Court should prefer to review statutes which are most ideologically distant from the median Justice's ideal point.

The estimation in Column 1 corresponds to the theoretical predictions made by the Floor Median Model, the Committee Gatekeeping Model, and the Majority Party Gatekeeping Model, all of which assume that a law's ideological position may be represented by the midpoint between the floor medians of the two enacting houses (L_1). The estimation in Column 2 corresponds to the predictions made by the Majority Party Median Model, which assumes that a law's ideological position may be represented by the midpoint between the majority party medians of the two enacting houses (L_2). The estimation in Column 3 corresponds to the predictions made by the Veto-Filibuster Model, which assumes that a law represents the midpoint between the senatorial filibuster pivot and the most distant veto pivot (L_3).

The results are not promising for the hypothesis of an unconstrained Court. The coefficients on the variables measuring ideological distance are all in the opposite direction of that predicted, although none reach conventional levels of statistical significance. All three estimations provide extremely poor fits to the data.

Columns 4-7 of Table 4 instead model the probability that a law is reviewed by the Court as a function of $|L' - L|$, where L' represents the predicted point at which the Court will set its standard of constitutionality, given the constraint exercised by the sitting Congress, and L again represents the law at issue. These estimations assume that the Court considers congressional

preferences in its certiorari decision. Column 4 reports the results from the Floor Median Model, whose predicted standard of constitutionality is represented by L_1'; Column 5 reports the estimates from the Committee Gatekeeping and Majority Party Gatekeeping models, whose predicted standards of constitutionality are represented by L_2' and L_3', respectively; Column 6 reports the results from the Majority Party Median Model, whose predicted standard of constitutionality is represented by L_4', and Column 7 reports the estimates from the Veto-Filibuster Model, whose predicted standard of constitutionality is represented by L_5'.

In all four estimations, the coefficients on the $|L' - L|$ variable are now in the predicted (positive) direction and are significant at conventional levels. For these models of the legislative process, the hypothesis of a Court constrained by congressional preferences clearly outperforms the rival hypothesis.[29] The trend variable measuring the age of a law is consistently in the predicted negative direction and is significant at conventional levels.

Table 5 replicates these tests for the sample of landmark statutes. There is again no evidence to support the hypothesis of a Court unconstrained by Congress; the coefficients on the variables for unconstrained ideological distance are still in the opposite direction of that predicted, although short of statistical significance, and all three models provide very poor fits to the data.

The coefficients on the models of congressional constraint are all in the predicted direction, however, and with the exception of the Majority Party Model, are all at conventional levels of significance. Because the Majority Party Model provides a poor fit to the landmark data for both the unconstrained and constrained models, we cannot use it to discriminate between these hypotheses. But the results from the other legislative models show a clear pattern of support for the hypothesis of a Court constrained by congressional preferences. The variable

measuring the age of a law is again consistently negative and is significant in the Floor Median and gatekeeping models.

We can also convert these Poisson estimates into more meaningful quantities, namely the predicted probabilities that a congressional law will be reviewed in any given term as a function of both the Court's preferences over the law and the relevant congressional constraint. We hold the age of the law at the sample means. Figures 2 and 3 display the predicted probabilities of review for 1987 statutes, for the full sample and for landmark statutes only, using the constrained Floor Median Model estimates reported in Tables 2 and 3, along with their 95% confidence intervals. Quite evident in both figures are the large and sharp jumps in the predicted probabilities that a liberal statute would be reviewed by the Rehnquist Court as a function of the 1994 congressional elections. The predicted probability of review for a generic 1987 statute increases by 114% between the Court’s 1993 and 1994 terms, and by 400% for landmark statutes; there is no overlap in the 95 percent confidence intervals for these point predictions. Since the estimated ideal point of the Court’s median Justice (Justice O’Connor) does not change between the 1993 and 1994 terms, we know that the leap in the probability of review for these statutes must be solely due to the electorally induced change in congressional preferences.

The results for the other legislative models estimated in Tables 2 and 3 are very similar, generating increases in the predicted probability that a 1987 statute would be reviewed between the 1993 and 1994 terms of 100 to 117 percent for ordinary statutes, and 350 to 1800 percent for landmark statutes.

We can also aggregate the predicted probabilities of review for all the liberal statutes in our samples enacted prior to the 1994 congressional elections, again using the constrained Floor Median Model estimates reported in Tables 2 and 3. We isolate the effect of changing

congressional preferences after the 1994 elections on these predicted probabilities of review by holding congressional preferences constant at their 1994 values, while allowing the Court's preferences to vary. We then compare the results from this simulation to those we obtain using actual congressional preferences. Statute age is again held at the sample mean.

For the full sample, during the 8 terms occurring after the 1994 elections, the Court is predicted to review 29 of the 2,418 liberal statutes enacted between 1987-1994. But if Congress is held to its 1994 ideological values, the Court is predicted to review only 13 of the liberal pre-1995 statutes during the 1994-2001 terms.[30] This means that a predicted 16 additional liberal statutes, representing an increase of 123 percent, are reviewed by the Court during these terms *solely* as a result of the dramatic shift of Congress to the right in the mid-1990s.

For the pool of landmark statutes, we find that the Court is predicted to review 12 of the 85 landmark statutes enacted between 1987 and 1994 during its 1994-2001 terms. However, if we hold Congress to its 1994 ideological values, this number drops to 2 statutes.[31] That is, the probability that the Court would review one of these liberal landmark bills during the 1994-2001 terms increases by 500% solely as a result of the rightward turn in Congress.

These predicted probabilities convey the perhaps surprising magnitude of the effects that congressional preferences have on the composition of the Court's docket. It is worth bearing in mind that during its 1994-2001 terms, the Court struck 83 percent of the generic statutes it reviewed that were enacted by the Congresses sitting between 1987-1994, and 91 percent of the landmark liberal statutes.[32] The strikes included both substantively important laws, such as the Brady Handgun Violence Prevention Act (*Printz v. U.S.* (1997)), the Violence Against Women Act (*U.S. v. Morrison* (2000)), and the Americans With Disabilities Act (*Board of Trustees of the University of Alabama v. Garrett* (2001)), and cases in which the Court established novel and far

reaching interpretations of the Interstate Commerce Clause (*U.S. v. Lopez* (1995), limiting the ability of Congress to regulate activity "noneconomic" in character), Section 5 of the 14th Amendment (*City of Boerne v. Flores* (1997), rescinding the ability of Congress to expand the Court's interpretations of prohibited conduct under the 14th Amendment), and the 11th Amendment (*Seminole Tribe of Florida v. Florida* (1996), prohibiting Congress from abrogating states' sovereign immunity from suit in federal courts under the Indian or Interstate Commerce Clauses). If our analysis is correct, then most of these struck statutes never would have made it onto the Court's docket in the first place had congressional preferences remained relatively liberal.

These predicted probabilities also strongly suggest that analyses which rely solely on the pool of laws actually reviewed by the Court in order to test claims about the effect of congressional preferences on the Court's final decisions may well fail to reject the null hypothesis (e.g. Segal 1997, Segal and Spaeth 2002). If the hypothesis of congressional influence on the Court's final rulings is correct, that is, then presumably we would expect the magnitude and direction of the gap between the median Justice's ideal point and *L',* or the predicted opinion location conditional on congressional preferences, to have an effect on the ideological direction of the Court's judgments (Segal 1997). The more a conservative court is constrained by a liberal sitting Congress, for instance, the greater a propensity it should have to uphold liberal statutes (and vice versa).

However, we now know that the Court will also be much less likely to review ideologically distant statutes when it faces an ideologically distant Congress. It is precisely when the gap between what the Court would *like* to do, and what it *can* do, increases that cases begin to disappear from the Court's docket. This selection bias will systematically remove from the

Court's docket cases that have large positive and negative values on the key explanatory variable of congressional constraint, leaving primarily those cases where there are very small or nonexistent predicted effects of congressional preferences on the Court's decisions. We will thus have much less variation on the main explanatory variable in our sample than exists in the population. As a result we will incur much more estimation uncertainty.

In the case of our data, we can estimate the degree of this selection bias by simulating how many of the liberal statutes the Court would have reviewed during the 1987-1993 terms, had Congress taken an earlier conservative turn. For these simulations, we give to the 1988-1994 Congresses the ideological values of their 1995-2001 counterparts. Using the estimates reported in Table 2 for the Floor Median Model, if the pre-1994 Congresses had been as conservative as the post-1994 Congresses, we predict that the Court would have reviewed 14 of the total 2,163 liberal statutes during its 1987-1993 terms. But under the Democratic Congresses that the Court actually faced, we predict that the Court would review only 5 of those statutes (a decrease of 64%).[33]

For the landmark statutes, if we give to the 1988-1994 Congresses the conservative ideological values of their 1995-2001 counterparts, using the estimates reported in Table 3 we predict that the Court would have reviewed 6 of the 78 liberal landmark statutes during its 1987-1993 terms. However, the Court is predicted to review only 1 of these statutes while it faced the actual liberal Congresses of 1988-1994 (a decrease of 83%).[34]

In our data, then, we find that the number of liberal statutes reviewed by the Rehnquist Court during the 1987-1993 terms is reduced considerably as a result of the Court facing an oppositional Congress. The analyst who looks just to the statutes reviewed by the Court will have lost a significant quantity of data due to the selection bias in the Court's docket. Now, that

analyst will be trying to estimate the Court's propensity to strike ideologically distant statutes when it faces an ideologically distant Congress with only a very small number of statutes. Given that we are not talking about large numbers of statutes in the first place, the substantial drops in sample sizes when the Court is facing ideologically hostile Congresses could easily lead to significant estimation uncertainty for models of the Court's rulings on the merits.

10 Conclusion

Our results may shed some light on the failure of most empirical studies to find any effects of congressional preferences on the Court's final rulings on the merits (Segal 1997, Hansford and Damore 2000, Spriggs and Hanford 2001, Segal and Spaeth 2002, Howard and Segal 2004, Sala and Spriggs 2004, Martin 2006). These studies have often been cited as evidence that the Court is truly independent from the Congress (Segal 1997, Segal and Spaeth 2002, Hettinger and Zorn 2005). However, another possibility, and one which receives strong support in our data, is that the selection mechanism by which cases appear on the Court's docket is itself conditional on congressional and/or presidential policy preferences. Whether because of the Court's certiorari decisions, or because of the actions of litigants who anticipate those decisions, the Court is less likely to review cases involving constitutional challenges to congressional statutes when it will have to defer significantly to congressional preferences in its final rulings. Cases that could systematically demonstrate the effects of congressional constraints on the Court are likely then to be weeded out of the Court's docket. Studies which fail to take into account the fact that the selection bias in the Court' docket permits it to duck congressional trouble may then fail to observe the extent to which the Court is in fact dependent on the political branches.

Moreover, while our findings specifically concern the responsiveness of the Court's docket to congressional preferences over constitutional challenges to congressional statutes, there is no reason to think that the endogeneity of the Court's docket is not a broader problem for studies of the Justices' decisionmaking (Kastellec and Lax 2008). Our findings thus recommend a much greater sensitivity to issues of research design that take this endogeneity into account.

Table 1
Public Laws, 1987-2001

Congress	Years	Total number of public laws enacted	Number of years laws are followed	Number of observations	Number of "landmark" laws enacted	Number of "landmark" observations
100	1987	240	15	3600	5	75
	1988	473	14	6622	18	252
101	1989	240	13	3120	7	91
	1990	410	12	4920	13	156
102	1991	243	11	2673	12	132
	1992	347	10	3470	9	90
103	1993	210	9	1890	14	126
	1994	255	8	2040	7	56
104	1995	88	7	616	6	42
	1996	245	6	1470	12	72
105	1997	153	5	765	8	40
	1998	241	4	964	11	44
106	1999	170	3	510	3	9
	2000	410	2	820	9	18
107	2001	108	1	108	7	7
Total	1987-2001	3833	1-15	33588	141	1210

Table 2
Grouped Event History Estimations of All Statutes, 1987-2001
Poisson Link Function with Linear Time Term
Dependent Variable: Supreme Court Review of Constitutionality of Federal Statute (0/1)
U(*Grant Certiorari*) = -|*C* - *L'*|

	Floor Median Model	Committee Gatekeeping/Party Gatekeeping Models	Majority Party Model	Veto-Filibuster Model
$\|C - L_1'\|$	-3.05*** (1.16)			
$\|C - L_2'\|$		-2.84*** (1.17)		
$\|C - L_3'\|$				
$\|C - L_4'\|$			-1.13*** (0.41)	
$\|C - L_5'\|$				-4.26* (2.41)
Age of Law	-0.15*** (0.04)	-0.14*** (0.05)	-0.15*** (0.05)	-0.10** (0.05)
Constant	-5.79*** (0.20)	-5.87*** (0.22)	-5.79*** (0.24)	-6.14*** (0.22)
N	33470	33470	33470	33470
Wald Chi^2	21.38***	18.45***	18.48***	10.95***
Poisson GOF	0.74	0.90	0.99	0.40

Note: *α≤.10; **α≤.05; ***α≤.01 (all two-tailed tests). Huber (1967) robust standard errors clustered by year of enactment reported in parentheses. Poisson GOF reports the significance of the Pearson statistic used to test the hypothesis that our data are not Poisson distributed.

Table 3
Grouped Event History Estimations of Landmark Statutes, 1987-2001
Poisson Link Function with Linear Time Term
Dependent Variable: Supreme Court Review of Constitutionality of Federal Statute (0/1)
U(*Grant Certiorari*) = -|*C* - *L'*|

	Floor Median Model	Committee Gatekeeping/Party Gatekeeping Models	Majority Party Model
$\|C - L_1'\|$	-6.85** (2.96)		
$\|C - L_2'\|$		-6.29*** (2.55)	
$\|C - L_3'\|$			
$\|C - L_4'\|$			-2.09** (1.07)
$\|C - L_5'\|$			
Age of Law	-0.11 (0.08)	-0.10 (0.08)	-0.13 (0.08)
Constant	-3.41*** (0.45)	-3.55*** (0.44)	-3.34*** (0.47)
N	1145	1145	1145
Wald Chi^2	6.32**	7.40**	5.28*
Poisson GOF	0.97	0.98	0.98

Note: *α≤.10; **α≤.05; ***α≤.01 (all two-tailed tests). Huber (1967) robust standard errors clustered by year of enactment reported in parentheses. Poisson GOF reports the significance of the Pearson statistic used to test the hypothesis that our data are not Poisson distributed. The Veto-Filibuster Model cannot be estimated with the landmark legislation data.

Table 4
Grouped Event History Estimations of All Statutes, 1987-2001
Poisson Link Function with Linear Time Term
Dependent Variable: Supreme Court Review of Constitutionality of Federal Statute (0/1)
U(*Grant Certiorari*) = $|L' - L|$

	Unconstrained Floor Median/ Committee Gatekeeping/Party Gatekeeping Models	Unconstrained Majority Party Model	Unconstrained Veto-Filibuster Model	Constrained Floor Median Model	Constrained Committee Gatekeeping/ Party Gatekeeping Models	Constrained Majority Party Model	Constrained Veto-Filibuster Model
$\|C-L_1\|$	-1.72						
	(1.34)						
$\|C-L_2\|$		-1.78					
		(1.29)					
$\|C-L_3\|$			-0.27				
			(1.71)				
$\|L_1'-L_1\|$				2.31***			
$\|L_2'-L_1\|$				(0.92)			
$\|L_3'-L_1\|$					2.62***		
					(0.77)		
$\|L_4'-L_2\|$						0.84**	
						(0.43)	
$\|L_5'-L_3\|$							1.56**
							(0.82)
Age of Law	-0.08	-0.09*	-0.09*	-0.15***	-0.15***	-0.14***	-0.09*
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Constant	-5.83***	-5.10***	-6.20***	-6.49***	-6.56***	-6.47***	-6.69***
	(0.42)	(0.84)	(0.55)	(0.31)	(0.32)	(0.34)	(0.35)
N	33470	33470	33470	33470	33470	33470	33470
Wald Chi^2	5.79*	5.80*	3.20	13.30***	24.35***	14.01***	10.58***
Poisson GOF	0.95	0.80	0.91	0.81	0.88	0.99	0.95

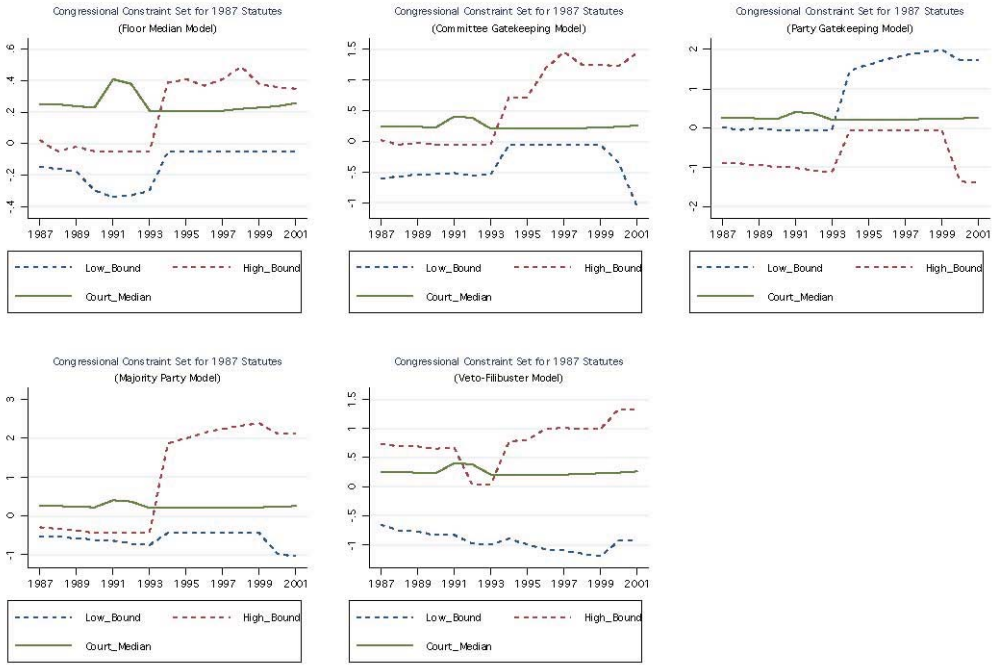
Note: *α≤.10; **α≤.05; ***α≤.01 (all two-tailed tests). Huber (1967) robust standard errors clustered by year of enactment reported in parentheses. Poisson GOF reports the significance of the Pearson statistic used to test the hypothesis that our data are not Poisson distributed.

Table 5
Grouped Event History Estimations of Landmark Statutes, 1987-2001
Poisson Link Function with Linear Time Term
Dependent Variable: Supreme Court Review of Constitutionality of Federal Statute (0/1)
U(*Grant Certiorari*) = $|L' - L|$

	Unconstrained Floor Median/ Committee Gatekeeping/Party Gatekeeping Models	Unconstrained Majority Party Model	Unconstrained Veto-Filibuster Model	Constrained Floor Median Model	Constrained Committee Gatekeeping/ Party Gatekeeping Models	Constrained Majority Party Model	Constrained Veto-Filibuster Model
$\|C\text{-}L_1\|$	-1.22						
	(1.84)						
$\|C\text{-}L_2\|$		-1.73					
		(1.61)					
$\|C\text{-}L_3\|$			-0.62				
			(1.62)				
$\|L_1'\text{-}L_1\|$				4.73***			
$\|L_2'\text{-}L_1\|$				(1.78)			
$\|L_3'\text{-}L_1\|$					5.22***		
					(1.75)		
$\|L_4'\text{-}L_2\|$						1.50	
						(1.11)	
$\|L_5'\text{-}L_3\|$							1.76**
							(0.73)
Age of Law	-0.03	-0.03	-0.04	-0.14*	-0.14*	-0.11	-0.03
	(0.07)	(0.07)	(0.08)	(0.08)	(0.08)	(0.08)	(0.07)
Constant	-3.80***	-3.01***	-3.93***	-4.73***	-4.86***	-4.51***	-4.60***
	(0.72)	(1.22)	(0.82)	(0.57)	(0.62)	(0.73)	(0.59)
N	1145	1145	1145	1145	1145	1145	1145
Wald Chi^2	0.53	0.55	0.26	7.71**	10.36***	3.87	9.52***
Poisson GOF	0.65	0.64	0.62	0.65	0.89	0.76	0.73

Note: *α≤.10; **α≤.05; ***α≤.01 (all two-tailed tests). Huber (1967) robust standard errors clustered by year of enactment reported in parentheses. Poisson GOF reports the significance of the Pearson statistic used to test the hypothesis that our data are not Poisson distributed. The Veto-Filibuster Model cannot be estimated with the landmark legislation data.

Figure 1: Congressional Constraint Sets for 1987 Statutes



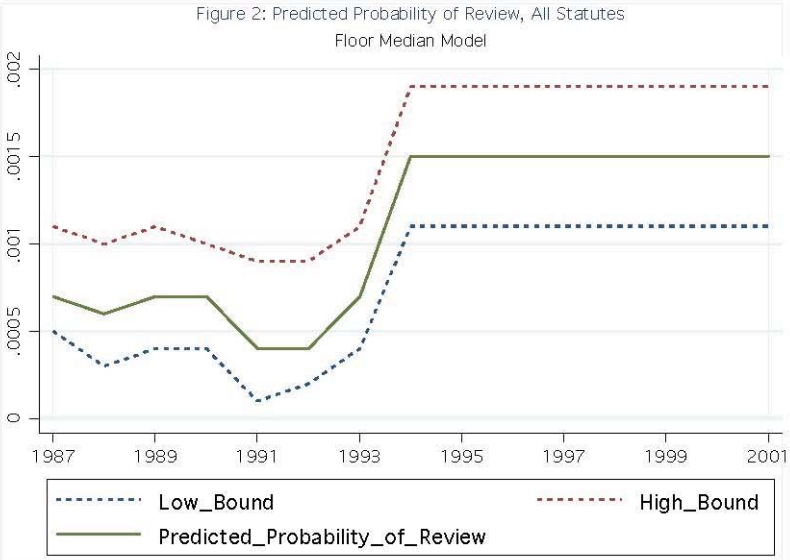


Figure 2: Predicted Probability of Review, All Statutes
Floor Median Model
0
.0005
.001
.0015
.002
1987
1989
1991
1993
1995
1997
1999
2001
Low_Bound
High_Bound
Predicted_Probability_of_Review

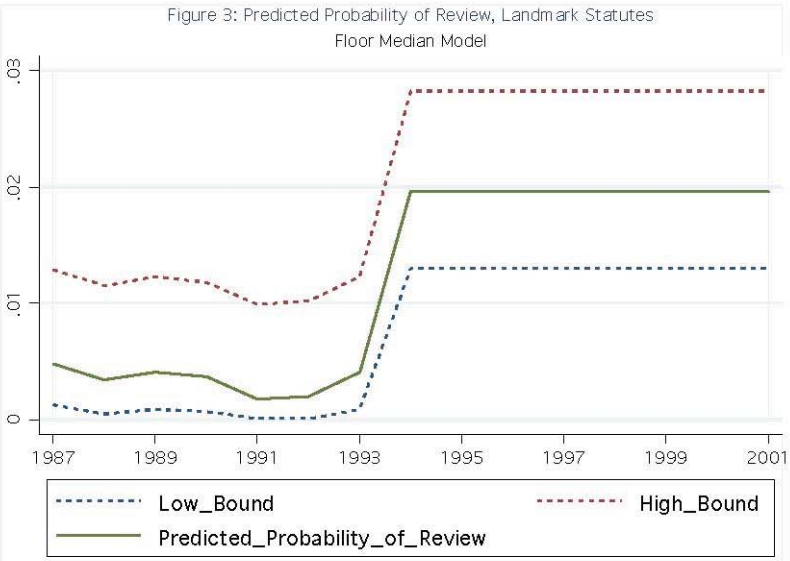


Figure 3: Predicted Probability of Review, Landmark Statutes
Floor Median Model
0
.01
.02
.03
1987
1989
1991
1993
1995
1997
1999
2001
Low_Bound
High_Bound
Predicted_Probability_of_Review

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[1] Also see Provine 1980: 54-62, Epstein and Knight 1998: 83, and Cross and Nelson 2001: 1476.

[2] Others have also suggested that at least some Justices may have a normatively derived "preference for deference" to the democratically elected Congress (cf. Howard and Segal 2004).

[3] The possibility of congressional constraint even in constitutional cases would lead us to expect that we would not find evidence of that constraint by looking at the mix of statutory and constitutional rulings issued by the Court in any given term. Reestimating the Epstein et al models with ideal point estimates generated from a large sample of interbranch "bridge" observations (Bailey 2007), we find no evidence that the proportion of constitutional rulings issued by the Court is responsive to congressional preferences. This analysis is available from the authors upon request; we thank Lee Epstein for making all data used in the 2002a article available at http://epstein.wustl.edu/research/dynamic.html.

[4] We restrict our claims to the possibility of selection bias; we are agnostic as to the probability of such bias.

[5] This is true even for laws that have a finite time horizon, such as an annual appropriations bill. Such a bill may still be litigated to the Supreme Court even after its formal authority has expired.

[6] Some recent papers suggest that the ideological location of the Court's majority opinion is also influenced by the preferences of the opinion author and possibly potential dissenting Justices (Bonneau et al 2007, Lax and Cameron 2007). While recognizing the merit of these papers, our research design requires us to model judicial utility functions even for constitutional challenges to congressional statutes that never materialize. Because we cannot accurately predict the opinion author for these potential cases, we rely on the Court's median Justice as a reasonable alternative.

[7] In equilibrium, then, we would not expect to see Court-punishing legislation, precisely because the Court is perfectly responsive to congressional preferences. This feature of models of congressional constraint undermines the claim that the Court must be fully independent of Congress, because we so rarely see Congress enacting Court-punishing statutes (e.g. Segal and Spaeth 2002).

[8] While we do not directly model the decision to strike or uphold a statute here, we implicitly assume that the probability that a statute is struck is an increasing function of $|L'-L|$. Whether a statute is upheld or struck, the resulting policy is assumed to be that set by the Court, namely L'.

[9] Assuming a quadratic utility function makes little difference in the results.

[10] Sala and Spriggs (2004) point out that the observational equivalence between the constrained and unconstrained hypotheses for the observations for which the Court is predicted to be unconstrained will dampen the results for the constrained model. Our estimation is thus a conservative test of the constrained model.

[11] Equation (2) will be true as long as $|C - L'| \leq |C - L|$. Because the median prefers to set L' as close to her own ideal point as possible, this condition will always be met.

[12] Bonneau et al (2007) model the status quo as the ideological location of the ruling in the lower court, which is measured using the ideal points of those Justices voting for and against certiorari. Such a research design would be inappropriate here, where we need to designate a status quo for every *potential* case involving a constitutional challenge to a congressional statute, irrespective of whether those challenges actually occur.

[13] As a result of this research design, our empirical tests can at best provide only indirect evidence of a constrained Court. That is, if we in fact find that the probability that a congressional law is reviewed is responsive to congressional preferences, then we will have

evidence that either the Court, or actors who anticipated the Court's likely actions, was/were constrained by anticipated congressional preferences in constitutional cases. However, we will not be able to distinguish between these two possibilities.

[14] Although it usually takes several years for a challenge to a law to work its way to the Supreme Court, it can happen quite quickly. Examples from our dataset include a November 18, 1988 amendment to the Communications Act of 1934, struck by the 1988 OT Court, the Flag Protection Act of October 28, 1989, struck by the 1989 OT Court, and provisions of the Communications Decency Act of February 8, 1996, struck by the 1996 OT Court.

[15] The data on public laws in Table 1 were obtained from the Congressional Record's summaries of legislative activity, available from *http://thomas.loc.gov/home/resume.html*. The Congress also enacts an increasingly small number of "private" bills every legislative session; these bills concern topics of very narrow interest to individual members. Private bills are not included in our dataset.

[16] Removing upheld laws from the dataset after they are reviewed by the Court makes no appreciable differences to the results.

[17] During this period there are 4 cases reported by the USSCJD as involving judicial review of a federal law, in which the Court does not actually reach a decision on constitutional grounds (*Austin v. U.S.* (1993), *TBS v. FCC et al* (1994), *Miller v. Albright* (1998), and *Friends of the Earth v. Laidlaw Environmental Services* (2000)). Including or excluding these cases does not make any substantive difference to the results reported below; we report results excluding these cases.

[18] The USSCJD both includes cases that do not strike congressional laws, *e.g.*, *Saenz v. Roe*, 143 L.Ed.2d 689 (1999) (involving the constitutionality of a California statute limiting the maximum

welfare benefits available to newly arrived residents), and excludes cases that do strike congressional laws, *e.g., Bartnicki et al v. Vopper et al,* 532 U.S. 514 (2001) (involving a provision of federal wiretapping law found to violate the First Amendment).

[19] For example, in *Greater New Orleans Broadcasting Assn. v. United States* (1999), the Supreme Court invalidated a 1934 congressional law prohibiting the advertisement of casinos. A 1988 amendment to the section of this law at issue before the Court had added the words "or television," thus including television broadcasting as a medium through which casino advertising was prohibited. The original prohibition on casino advertising remained intact through this amendment, clearly signaling congressional support for the thrust of the original law. The later Congress was thus designated as the enacting Congress.

[20] The full list of reviewed statutes (and cases) for both pools is available from the authors upon request.

[21] Sala and Spriggs (2004) measure the ideology of a congressional statute with the roll call coordinates estimated using the DW-NOMINATE algorithm (Poole and Rosenthal 1997). However, as the bill location estimated by this procedure is widely known to be highly dependent upon the algorithm's assumptions, we eschew this approach here.

[22] $I_H(L)$ and $I_S(L)$ represent the indifference points of the House and Senate medians, respectively, with respect to L; $I_{HJ}(L)$ and $I_{SJ}(L)$ represent the indifference points of the House and Senate Judiciary Committee medians, respectively, with respect to L; $I_{HP}(L)$ and $I_{SP}(L)$ represent the indifference points of the House and Senate majority party medians, respectively, with respect to L; $I_{H146}(L)$, $I_{S34}(L)$, $I_{H290}(L)$, and $I_{S67}(L)$ represent the indifference points of the veto pivots in the House and Senate for Democratic and Republican presidents, respectively,

with respect to L; and $I_{S60}(L)$ and $I_{S41}(L)$ represent the indifference points of the filibuster pivots in the Senate for Democratic and Republican presidents with respect to L.

[23] The constraint set generated by the Veto-Filibuster Model is qualitatively different from those generated by the other four models. This model predicts that, for all laws enacted prior to the 1994 congressional session, the Court was constrained by Congress and the President only during the 103d Congress, or that sitting during 1993 and 1994. During this period the Democratic Congress was joined by a Democratic president, and the Court lay to the right of the rightmost boundary of the constraint set generated by the relevant pivotal actors.

[24]The Majority Party Gatekeeping Model generates a rightmost boundary for the congressional constraint set which is identical to that of the Committee Gatekeeping Model between 1987-1993. Its predictions are thus observationally equivalent to those of the Committee Gatekeeping Model.

[25] With only a few exceptions, the Court also lies within all predicted constraint sets for all laws enacted by the post-1994 Congresses, implying that the Court would have been largely free to rule as it wished on these laws as well.

[26] There is also the possibility of a nonlinear temporal relationship; Justices may prefer that recently enacted statutes "percolate" in the lower courts until they are "ripe for review" (Perry 1991: 230-232).

[27] Beck (1998) and Beck et al (1998) also recommend a natural cubic spline as a way to capture nonlinearities in the baseline hazard rate without using up as many degrees of freedom as required for the duration dummy variables. However, our data show no evidence of nonlinear baseline hazard rates.

[28] Beck *et al* (1998) report that Equation 4 may also be estimated using the more familiar probit or logit link functions as long as the probability of an event occurring remains less than 50%. Since in our datasets, the probability of a law being reviewed is considerably below this threshold, we also used the probit link function to estimate Equation 4. These estimates were qualitatively identical to those reported in Tables 2-5. We also reestimated all models in Tables 2-5 using both the Rare Events Logistic Regression (RELOGIT) estimator developed by Michael Tomz et al (Michael Tomz, Gary King, and Langche Zeng, Version 1.1, Cambridge, MA: Harvard University, available at http://gking.harvard.edu/), and a random effects or "frailty" Poisson model with the frailties assumed to operate at both an individual statute level and clustered by the enacting Congress. We found only negligible differences in the estimated coefficients and standard errors for these alternative estimators.

[29] We also estimated Poisson regressions in which we included both the $|C - L|$ and the $|L' - L|$ variables on the right hand side, for each legislative model in Tables 4 and 5. All coefficients on the $|C - L|$ variables continued to be in the wrong direction, and were even significantly in the wrong direction for the Median and Majority Party Median models using the full sample of statutes. All coefficients on the $|L' - L|$ variables continued to be positive and strongly significant.

[30] The 95% confidence intervals are 21.5-37 and 8-20 statutes, respectively.

[31] The 95% confidence intervals are 8-18 and .5-7.5, respectively.

[32] The Court reviewed the constitutionality of 23 of the generic liberal statutes during the 1994-2001 terms, striking 19 of them, and reviewed 11 of the landmark liberal statutes, striking 10 of them.

[33] The 95% confidence intervals for these predictions are 10-17 and 2.5-8.5, respectively. The Court actually reviewed 8 of these statutes during its 1987-1993 terms.

[34] The 95% confidence intervals for these point predictions are 4-8 and 0-3, respectively. The Court actually reviewed 1 of the liberal landmark statutes during its 1987-1993 terms.