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Congressional voting on term limits

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Abstract. Between 1990 and 1995, twenty-three states unilaterally imposed term limits on their own delegations to Congress. In 1995 the House of Representatives defeated a constitutional amendment that would have limited the terms for all of Congress. Only weeks later, the Supreme Court struck down the individual state laws. In 1997 the House again brought the issue to a vote, which also failed. This paper models congressional voting on term limits with a simple game within an interest-group theory with legislators as imperfect agents of constituents. The game foremost predicts that members from term-limited states would be more likely to support term limits in the first vote but no more likely on the second vote. The empirical section employs probit, multinomial logit, and ordered probit maximum likelihood estimations to confirm the stated hypotheses. Among other results, in particular both the joint and conditional probability of a 'yea' on the first vote and a subsequent 'nay' on the second vote is higher for members from states that had unilaterally self-imposed term limits. The results are robust to model specification, estimator, and alternative sampling. Implications are proposed in the concluding comments.

> We shall never find two thirds of a Congress voting for anything which shall derogate from their own authority and importance…
>
> George Byron, "An Old Whig",
> Independent Gazetter, October 12, 1787

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1. Introduction

Why would even a single member of Congress vote to limit his own tenure in office? Perhaps his constituents demand it with the credible threat of electing someone to replace him – three more terms is presumably better than none at all. Or perhaps the member follows his ideology – a conservative, for example, could vote for term limits because he esteems Jefferson, Mason and other Founding Fathers who advocated mandatory rotation. Other explanations are more Downsian, attributing responsibility for congressional voting on term limits to constituent preferences. The political debate over Congressional term limits was dominated by the assumption that term limits would reduce the size and/or growth of government.[1] A congressional district whose constituents are heavily taxed, therefore, would rather its members vote in favor of term limits, and a district that receives disproportionate federal spending would be in opposition. Similarly, Friedman and Wittman (1995) suggest term limits would shift the extant balance of political power. A relatively low-tenured member might, through the passage of congressional term limits, increase her relative power to provide her district with net transfers, and therefore favor term limits. More broadly, it could simply be that term limits are good for society as a whole; but two thirds of a Congress would still have to agree. The plausible reasons for a member supporting term limits are many, and not mutually exclusive. Since the House recently voted twice on congressional term limits, the data are available to investigate these questions and more in detail. Broadly speaking, evidence in this paper shows that economic interests and members' own characteristics were central to the votes, as expected. But there is an uncommon twist to the story. Before the first vote reached the House floor, 23 states individually voted to limit the terms of *their own* delegations to Congress. Moreover, *between* the two votes, the Supreme Court struck these state laws down.[2] The purpose of this paper is to take advantage of these events to conduct a sort of natural experiment in which states' rights, congressional politics, and the Supreme Court decision can all be controlled for. I begin by estimating a single equation model to explain the two votes separately. I then illustrate the occurrence of dramatic shifts in member voting between the first and second votes, in response to changed incentives resulting from states and Court institutional changes. The results suggest that, in addition to constituent interests, strategic interaction among members to improve or maintain their relative political power determined the outcome on term limits in Congress. I propose some additional conclusions at the end of the paper.

1.1. *Background*

It is instructive, at the outset, to establish some of the important facts about congressional term limits. Though tenure laws pepper the history of self-government (most prominently in the Articles of Confederation), their widespread adoption is a very recent phenomenon. Between 1990 and 1995, 23 states unilaterally voted to limit the terms of their congressional delegation. All but one of these laws was passed using referenda, typically by 2-to-1 and as high as 3-to-1 margins. These election results and polling data combined to form compelling evidence of widespread popular support for term limits. For example, for the three years prior to the 1994 elections, polling data indicate support for term limits between 61 and 67 percent (Moore et al. 1994). In the 1994 general election campaigns, the Republican minority promised to bring term limits to a floor vote. This was conditional on their becoming the majority party, of course, and it was partly responsible for their success in doing so. Early in the 104th Congress, House Republican leaders shuffled a bill through committee, passed rules for debate, brought the issue to the floor, and saw the matter fall well short of receiving the necessary two-thirds majority, 227–204 (henceforth “VOTE1”).[3] Within weeks, the Supreme Court’s 5–4 decision in *U.S. Term Limits v. Thorton* struck down the 23 state laws. House Republicans promised again in the 1996 elections another vote on term limits, and made it their first major agenda item in the 105th Congress. The second attempt failed badly, barely acquiring the simple majority of 217–211 (henceforth “VOTE2”). For a more detailed background on term limits and a survey of the literature, see Lopez (2000).

2. Conceptual framework

In the theory that underlies my empirical approach, Congress functions as a broker of wealth transfers among private groups in the polity, where each group receives transfers net of taxes as a function of its collective action costs (Stigler 1971, Peltzman 1976, McCormick and Tollison 1981). Constituents and their representatives interact in an information-deficient agency model with non-trivial monitoring costs. Constituents elect representatives to maximize net transfers to the district. Representatives seek to maximize political support, either by dutifully serving constituent interests or by acquiring a stock of reputational capital (name and platform recognition, campaign warchests, etc.) that acts as a barrier to entry to effective challengers in future elections. Generally, the more slack in the agency relationship or the stronger the representative’s preference for shirking, the more political shirking chosen by the representative. Otherwise, the representative serves constituent interests. To emphasize tenure in the model, let "representative capital" be defined as the vector of legislator characteristics determining the member's productivity in procuring net transfers for his district. Examples of such characteristics would include (but not be limited to) the value of the member's committee seats, logrolling and lobbying contacts, law-making acumen, and political wile – all of which increase with tenure.[4] Since all districts receive gross wealth transfers and pay gross taxes, it is *relative* representative capital that is crucial to maximizing net transfers. Therefore, it is relative tenure that matters.

Within this framework, Dick and Lott (1993) model the passage of legislative term limits as a free rider problem. States or districts that continue to reelect their incumbents are free riding on those that vote for more frequent rotation. Since all states recognize this, all states will continue to reelect their incumbents, ceteris paribus. Term limits provide a solution to this free rider problem. As Dick and Lott explain:

> If all voters could agree simultaneously to limit the tenure of incumbents, ... [this would] ... lower the cost incurred by any district that removes its incumbent, because term limits place an upper bound on the decline in relative political experience that a district suffers when replacing its incumbent with a challenger.
>
> (Dick and Lott 1993: 4)

Hence, voters support universal passage of term limits because they expect more efficient representation due to the fact that the incentive to free ride is eliminated. But there is the rub: not all states passed term limits laws. Dick and Lott assume voters support term limits exclusively as a means of obtaining more efficient representation for themselves. This is perhaps only half the story. In a wealth transferring polity, voters care about the representation from *other* districts as well. When some states unilaterally self-impose term limits while others do not, new asymmetries in the market for wealth transfers result. A state that unilaterally self-imposes reduces the average tenure of its delegation relative to other states, which reduces relative representative capital and places the state's delegation at a disadvantage in acquiring net transfers. I refer to the 23 states that made this choice as "disadvantaged" in acquiring net transfers.[5] The 27 other states are likewise "advantaged." VOTE1 (104th Congress) took place with 23 "disadvantaged" and 27 "advantaged" delegations. The Supreme Court's *Thorton* decision then restored the pre-existing, non-term-limited status quo. Subsequently, VOTE2 (105th Congress) took place without these asymmetries.

This sequence of events altered the voting incentives of both advantaged and disadvantaged members between VOTE1 and VOTE2. Consider

Member from State without Term limits ("Advantaged")		Member from State with term limits ("Disadvantaged"): Limit	Member from State with term limits ("Disadvantaged"): Don't Limit
	Limit	(2, 2)	(1, 4)
	Don't Limit	(4, 1)	(3, 3)

Notes: All values are ordinal.

Figure 1. Payoffs to congressional voting on term limits

the simple game in Figure 1, which models the payoffs to members for different outcomes on term limits. The Nash equilibrium is for neither player to limit terms (cell 4). However, the term-limited member was constrained by the referendum passed in his state to unilaterally self-impose. Caught in the low payoff of the off-diagonal prior to *Thorton*, members from the 23 term-limited states would have tried to eliminate their disadvantage. The first best solution would have been to repeal only their own state law, moving them to the high payoff in the off-diagonal (i.e., making them one of the advantaged while leaving the remaining 22 disadvantaged). But this was not within their power as members of Congress. A second-best solution would have been to repeal the laws in all of the 23 disadvantaged states, thus returning to the pre-existing absolute and relative status quo (the Nash equilibrium). Again, however, this was not in their control. Finally, a third-best solution would be to impose term limits on all states at once, including the 27 advantaged, thus re-establishing the pre-existing relative (though not absolute) status quo (cell 1). In contrast, after *Thorton* this game disappears because members are no longer constrained by unilaterally self-imposed limits. The Court effectively granted the disadvantaged members their second-best solution and the Nash equilibrium.

The game predicts how members of Congress would vote on universal term limits. Districts that either possess greater representative capital and/or receive greater net transfers have more to lose and should vote against term limits, ceteris paribus. Supporters of term limits, on the other hand, would be districts with low representative capital and low net transfers. Members may pursue their own gain if agency slack is sufficient. In Section 3 below, I estimate a model that lends support to this prediction.

The model can also be used to predict *changes* in member voting from VOTE1 to VOTE2. Though VOTE1 and VOTE2 obtained similar majorities (53 and 51 percent, respectively), not all members voted the same way on both

		Vote Before *Thorton* (VOTE1) Yea	Vote Before *Thorton* (VOTE1) Nay
Vote After *Thorton* (VOTE2)	Yea	Y|Y	Y|N
	Nay	N|Y	N|N

Figure 2a. Categories of each member's voting on both votes.

votes. Of the 352 members who voted in both roll calls, 24 changed their vote. Voting 'nay' on both were 159 members, and the remaining 169 voted 'yea' both times (see Table 2, discussed further below). With these four possible outcomes, each member can be placed into the categories shown in Figure 2A. According to the game in Figure 1, the probability of a member falling into a given category in Figure 2A should be systematically related to whether he was from a term-limited state. *Thorton* granted the disadvantaged members their second-best solution, but not until *after* VOTE1. Prior to *Thorton*, the disadvantaged members could only pursue their third-best solution: vote for universal term limits. Thus, the game directly suggests specific testable hypotheses. Term-limited members should be more likely to vote yea on VOTE1 than their non-term-limited colleagues, ceteris paribus. However, *after* the *Thorton* decision, members from these 23 states should vote no differently on VOTE2 from any others, ceteris paribus. By the reverse reasoning, members from non-term-limited states would be more likely to vote nay on VOTE1, but not systematically differently on VOTE2, ceteris paribus. Figure 2B lists these predicted hypotheses between disadvantaged and advantaged members in terms of their conditional probabilities for the four possible outcomes.[6] In particular, voting nay on VOTE2, conditional on having voted yea on VOTE1, should be higher for members from term-limited states. In Section 4 I estimate multivariate models to test these predictions.

3. Empirical model of the House floor votes

In this section I specify a single equation probit model to estimate the votes on term limits in the House.[7] The dependent variables are VOTE1 (the 104th vote) and VOTE2 (the 105th vote). Both are coded 1 for 'yea' and 0 for 'nay.' To incorporate the relevant aspects of the underlying agency model, the empirical specification of the reduced form will be organized to include the following explanatory factors: a) legislator characteristics to proxy their own

Member from State with Term Limits ("Disadvantaged")		Member from State without Term Limits ("Advantaged")
$P_{N\|N}=1-P_{Y\|N}$	>	$P_{N\|N}=1-P_{Y\|N}$
$P_{Y\|N}$	<	$P_{Y\|N}$
$P_{N\|Y}=1-P_{Y\|Y}$	>	$P_{N\|Y}=1-P_{Y\|Y}$
$P_{Y\|Y}$	<	$P_{Y\|Y}$

Figure 2b. Testable hypotheses on change-of-vote behavior from VOTE1 to VOTE2 pairwise comparisons of conditional probabilities.

preferences; b) district characteristics to proxy constituent preferences (net transfers received); and c) measures of monitoring costs (i.e., agency slack). In interpreting preferences, I observe that during the term limits debate there was widespread expectation that term limits would reduce federal spending. This relationship is not necessarily upheld in the literature (Lopez, 2000); however, it is what the relevant political agents involved expected.[8] In general it is predicted that groups who rely on government spending oppose term limits, while groups who bear the burden of government spending favor term limits.

3.1. *Variables*

All variables used in this study are listed in the Data Appendix, which includes brief definitions, sources, expected signs, and descriptive statistics. The legislator-specific characteristics to proxy economic and ideological positioning consist of party affiliation (REPUBLICAN), "representative capital" as defined above (TENURE), gender (FEMALE), expected time horizon which I proxy with the member's age (AGEREP), and electoral security which I proxy with the margin of victory in the member's previous election (MARGVICT). The constituent-specific characteristics to proxy net transfers consist of economic, geographic, and fiscal variables (PUBEMP, WELFARE, FARMPOP, MEDINCM). Agency slack on the specific issue of term limits is captured by constituents' preferences for term limits, which I proxy with Perot voters (PEROT92). And, finally, disadvantaged members are indicated by a dummy variable set to 1 for members from the 23 states that unilaterally self-imposed term limits (STATELAW).

3.2. *Results*

The probit maximum likelihood estimation results appear in Table 1. To facilitate easy interpretation of the results, I report the marginal probabilities for each independent variable rather than the associated beta coefficient estimates. (The marginal probability is the partial derivative of that variable on the dependent variable, transformed into a probability, while holding all other regressors at their means.) In Table 1, there are four separate specifications for each of the two votes. Models 1 and 2 use the variable PUBEMP, while Models 3 and 4 use MEDINCOME, and Models 2 and 4 examine TENURE-squared.

The legislator-specific variables typically perform as expected. REPUBLICAN is positive and significant with a large coefficient increasing the probability of a yea vote by nearly .60. This is unsurprising because the vote on term limits was highly partisan: 81.5 percent of Republicans voted in favor of term limits, while 82.3 percent of Democrats voted against.[9] Given this partisanship and the performance of REPUBLICAN in the model, one criticism could be that party affiliation is sufficient for explaining voting on term limits. As a first response to this argument, I estimated a probit model with only a constant term and PARTY.[10] In both House votes, the overall fit of this specification was good, but this is to be expected given the strong correlations. Moreover, a likelihood ratio test strongly rejects the joint hypothesis that other legislator and constituent interest variables do not belong in the model.[11] The apparent conclusion is that REPUBLICAN is an important explanatory variable, but simply attributing the votes on term limits to party affiliation is incorrect (as well as uninteresting).

The negative and significant impact of TENURE is also as expected. The longer in office, the more representative capital there is at stake with term limits. In both samples, each year of TENURE reduces the probability of voting yea by approximately .03 with strong statistical significance. AGEREP, on the other hand, is positive as hypothesized, but only significant in the 104th vote. I was concerned that TENURE may jointly determine the vote along with AGEREP, FEMALE, and MARGVICT.[12] I examined the potential problem this introduces by piecemeal removing AGEREP, FEMALE, and MARGVICT from the base model. In all of these alternative specifications, TENURE is stable as negative and significant.[13] I am persuaded by the data that each of the legislator characteristics included in the model exert distinct marginal influences on the term limits vote. Also, whereas TENURE appears to be convex in the first vote (positive sign on TENURE-squared), it then appears to become concave in the second. But neither case is statistically significant.

Table 1. Univariate probit estimation of house votes on term limits – Dependent variables are the individual member's vote ('Yea' coded 1)

	Vote in 104th Congress (Dependent variable is VOTE1)				Vote in 105th Congress (Dependent variable is VOTE2)			
	Model 1	Model 2	Model 3	Model 4	Model 1	Model 2	Model 3	Model 4
Republican†	.599***	.589***	.617***	.607***	.568***	.567***	.566***	.566***
	(9.73)	(9.30)	(9.71)	(9.24)	(10.14)	(10.17)	(10.08)	(10.10)
Tenure	–.031***	–.049***	–.031***	–.047***	–.026***	–.017	–.025***	–.019
	(–5.27)	(–3.38)	(–5.33)	(–3.16)	(–4.64)	(–1.04)	(–4.65)	(–1.16)
Tenursq		.001		.001		000.	–	000.
		(1.33)		(1.15)		(–0.5 7)		(–0.45)
Female†	–.230***	–.236***	–.212**	–.219**	–.128	–.130	–.118	–.118
	(–2.60)	(–2.68)	(–2.37)	(–2.46)	(–1.38)	(–1.40)	(–1.24)	(–1.25)
AgeRep	.009**	.009**	.009**	.009**	.000	.000	–.000	.000
	(2.24)	(2.25)	(2.31)	(2.31)	(0.07)	(0.12)	(–0.02)	(0.02)
MargVict	–.001	–.001	–.002	–.001	.002	.001	.002	.001
	(–1.09)	(–0.73)	(–1.19)	(–0.85)	(1.07)	(0.90)	(0.98)	(0.84)
StateLaw†	.227***	.222***	.227***	.223***	–.064	–.063	–.062	–.061
	(3.12)	(3.04)	(3.10)	(3.03)	(–0.85)	(–0.83)	(–0.81)	(–0.80)
Perot92	.003	.004	.005	.006	.022***	.022***	.023***	.023***
	(0.46)	(0.63)	(0.78)	(0.90)	(3.03)	(3.00)	(3.09)	(3.08)
PubEmp	–.004	–.003			–.009*	–.009*		
	(–0.83)	(–0.69)			(–1.67)	(–1.71)		

Table 1. Continued.

	Vote in 104th Congress (Dependent variable is VOTE1)				Vote in 105th Congress (Dependent variable is VOTE2)			
	Model 1	Model 2	Model 3	Model 4	Model 1	Model 2	Model 3	Model 4
MedIncome			–.007	–.006			–.001	–.001
			(–1.52)	(–1.34)			(–0.26)	(–0.30)
Welfare	–.003***	–.003***	–.003***	–.003***	–.001**	–.001**	–.001**	–.001
	(–2.88)	(–2.87)	(–3.03)	(–2.99)	(–1.53)	(–1.54)	(–1.45)	(–1.46)
FarmPop	.007***	.007***	.006*	.006*	.003	.003	.003	.003
	(2.73)	(2.75)	(1.87)	(1.94)	(0.98)	(0.99)	(0.90)	(0.89)
Constant	–1.730	–1.5554	–0.9852	–0.8961	–0.9647	–1.3253	–1.3625	–1.6613
	(1.1532)	(1.1753)	(1.3509)	(1.3701)	(1.0444)	(1.0871)	(1.1449)	(1.1824)
Log likelihood	–164.84	–163.95	–163.93	–163.24	–181.41	–181.26	–183.00	–182.90
Chi^2 (10)	166.22	166.91	168.23	168.91	169.77	173.38	164.95	168.96
Prob > Chi^2	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Pseudo R^2	0.451	0.454	0.454	0.456	0.387	0.388	0.382	0.382
N	434	434	434	434	427	427	427	427

Notes. Marginal probability estimates (change in probability of voting yea for a continuous increase in the independent variable) appear on the line with the variable name. These are transformed, for ease of interpretation, from the associated probit coefficient estimates.
T-statistics are based on robust standard errors and are reported in parentheses. †In the case of dummy variables, the marginal probability is for discrete change from 0 to 1. *** Significant with 99% confidence. ** Significant with 95% confidence. * Significant with 90% confidence.

Female members appear to oppose term limits (though only marginally in the second vote). The data used by Schansberg (1994) indicate that women have lower average tenure than men do. This is also true of both my samples: men have average tenure of 10 years, while women have 6.5. Schansberg hypothesizes that women members have more to gain from term limits and should prefer them more, ceteris paribus. My regressions do not support this point of view. One alternative explanation lies in females' preferences for greater spending. Sobel and Wagner (1998) use a detailed data set to generate results on bill sponsorship and floor voting on spending bills. In all of their empirical estimations, females are more likely to sponsor and to vote for more spending, with female Democrats even more inclined to do so. Given the assumption that political agents expected term limits to reduce federal spending, this result would support the hypothesis that female members should oppose term limits. Additional support for the negative sign on FEMALE can be found in the partisan alignment of females' constituencies. Fox (1998) finds that between 1954 and 1992, female Democrats are elected in overwhelmingly Democrat districts, whereas female Republicans are elected in moderate or mildly Democrat districts. A plausible inference – although I am unaware of specific supporting data – would be that females would vote more frequently with Democrats, and therefore oppose term limits. The validity of this inference would depend on whether Fox's result still held into the 104th and 105th Congresses.[14]

Constituent variables did not generally perform as well as member variables. WELFARE is negative and significant, providing some support for the argument that groups who rely on government expenditures did oppose term limits. And FARMPOP is consistently positive and significant. This is a sensible result even though farmers are an interest group that receives government transfers. Agriculture policies do not depend on spending so much as price and entry regulation. I interpret the FARMPOP variable to embody the rural preference for term limits as in Adams and Kenny (1986). But the coefficients on PUBEMP, MEDINCOME, and MARGVICT are somewhat surprising. Neither public employment nor income in the district exerted significant influence on the member's vote. It can be argued that PUBEMP incorporates all government workers, including state and local, whose well being is not tied directly to federal spending. But it is somewhat puzzling why income would not be a significant predictor. With the t-ratio on MARGVICT consistently close to 1, it appears that electoral security – as measured by margin of victory in the prior election – did not affect the decision to vote on term limits. Coates (1995) argues electoral security enters the voting calculus as a conditioning variable on constituent preferences. Put simply, we should expect that a more electorally secure member will be less responsive to constituents' wishes, such that the influence of a given vector of constituent interest measures will vary with electoral security. I tested this by interacting MARGVICT with each of the constituent interest variables in Model 1. In a similar test, Coates finds strong support for the hypothesis. The vote on term limits, however, did not follow suit: the interaction model performs worse, overall, than Model 1, and none of the interaction terms are significant, with the exception of welfare expenditures.[15] Based on this result, I conclude that electoral security was not a significant factor in this particular vote, and I am inclined to favor Model 1 over more complicated specifications.

The most important constituent interest variables, PEROT92 and STATELAW, provide the most interesting results of the model. PEROT92 is statistically zero in the first vote, but positive and significant in the second. At the same time, STATELAW is positive and significant in the first vote, but statistically zero in the second. The explanatory role of these two variables is where the interesting aspects of congressional voting on term limits can be found. I investigate this in detail in Section 4 below.

Looking at the models overall, with the exception of FEMALE, PUBEMP, MEDINCOME, and MARGVICT, all the variables in the equation perform as hypothesized. Model fit is also consistently good. Against alternative specifications, Model 1 performs comparably well and is simpler. Voting on term limits appears to have adhered to an underlying wealth transfer calculus. Furthermore, it appears that constituent interest variables matter more, and legislator characteristics less, in the second vote. Both REPUBLICAN and TENURE reveal smaller marginal probability estimates in the second vote.

4. Changes in voting behavior

In this section I use multivariate extensions of the regression model above to estimate the *changes* in voting behavior that were brought about by the altered incentive structure imposed by *Thorton*. In observing change-of-vote behavior on term limits, selecting the sample is ambiguous: should one observe only those *members* who cast votes in both roll calls (N=352), or observe all congressional *districts* whose members voted twice even if they replaced their members after VOTE1 (N=427[16])? In part, the appropriate sample depends on the underlying question of whether the *member* or the *district* is the true unit of choice. To the extent that members are good agents of their constituents' interests, then the district would be the unit of choice. But insofar as members are able to shirk or pursue their own preferences in office, then the returning member would be the unit of choice.

Table 2 provides the change-of-vote data for both samples, and is further broken down by "advantaged" and "disadvantaged" members. Of the 352 members voting twice, 24 changed their vote. Of the 427 districts whose members cast votes twice, 58 changed their vote. In this section, I use the probit model above as a baseline to model this change-of-vote behavior vis-à-vis the game in Figure 1. I first closely examine the pre- and post-*Thorton* marginal effects of the variable STATELAW. I then use joint and conditional probabilistic frameworks to directly test the hypotheses as predicted in Figure 2B.

4.1. *Marginal effects in univariate probit*

In an important way the regression results in Table 1 already support the hypotheses in Figure 2A: the variable STATELAW is positive and significant before *Thorton* but afterward it becomes statistically zero. This suggests members from these states preferred term limits more strongly prior to *Thorton*. Such a conclusion warrants a closer look at the behavior of this variable.

The (continuous) marginal effects of a binary regressor such as STATELAW are more intuitive using the logit model, which estimates the log odds ratio of the dependent variable:

$$\ln\left[\frac{\mathrm{Pr}(\mathrm{V}_{\mathrm{in}}=1)}{1-\mathrm{Pr}(\mathrm{V}_{\mathrm{in}}=1)}\right]=\alpha+\beta'\mathbf{x}+\mu_{\mathrm{i}} \tag{1}$$

where V_{in} is the ith member's vote on the nth term limits bill (1=yes), $\mathbf{x}$ is the member's regressor vector and β' is the vector of coefficient estimates. The model generates $\ln[\bullet]=\mathrm{k}_{\mathrm{in}}$ for the ith member on the nth vote such that k_{in} can be converted into an estimated probability by the underlying:

$$\mathrm{P}_{\mathrm{r}}(\mathrm{V}_{\mathrm{in}}=1=\left[\frac{\mathrm{e}^{\mathrm{k}_{\mathrm{in}}}}{(1+\mathrm{e}^{\mathrm{k}_{\mathrm{in}}})}\right] \tag{2}$$

Let $\mathbf{x_1}$ be a vector containing the elements $\mathrm{STATELAW}_{\mathrm{in}}$ and $\mathrm{TENURE}_{\mathrm{in}}$. Let $\mathbf{x_2}$ be a vector consisting of the remainder of the regressors. Therefore, $\mathbf{x}_1\cup\mathbf{x_2}=\mathbf{x}$. I now isolate the marginal effect of STATELAW by calculating equation (2) at the individual elements of $\mathbf{x_1}$ and the mean values of $\mathbf{x_2}$ using k_{in} and β' as estimated by logit maximum likelihood in (1) (i.e., Model 1 from Table 1).[17] The results are presented graphically in Figure 3.

In general, the monotonic decline in all four panels of Figure 3 shows the effect that increased tenure has on the probability of voting for term limits. More striking, however, is the ceteris paribus impact of STATELAW on VOTE1 apparent in Figure 3A. At every tenure, the probability of a yes vote is higher for disadvantaged members. At tenure of 10 years STATELAW increases the chances of a yes vote by 24.88 probability points, ceteris paribus

Table 2. Actual change-of-vote behavior.

	For returning members only (N=352)			For all districts (N=427)		
Member voted	Member with term limits ("Disadvantaged")	Member without term limits ("Advantaged")	Totals	Member with term limits ("Disadvantaged")	Member without term limits ("Advantaged")	Totals
Nay then nay	55	104	159	63	114	177
Nay then yea	2	7	9	8	12	25
Yea then nay	8	7	15	21	114	33
Yea then yea	81	88	169	89	17	192
Totals	146	206	352	181	103	427

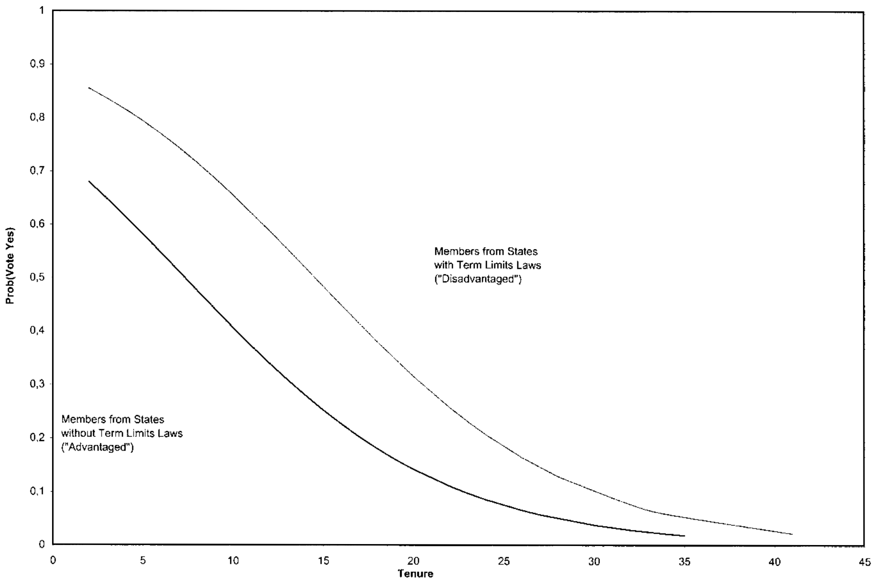

Figure 3a. Marginal effect of state law in 104th (before Thorton).

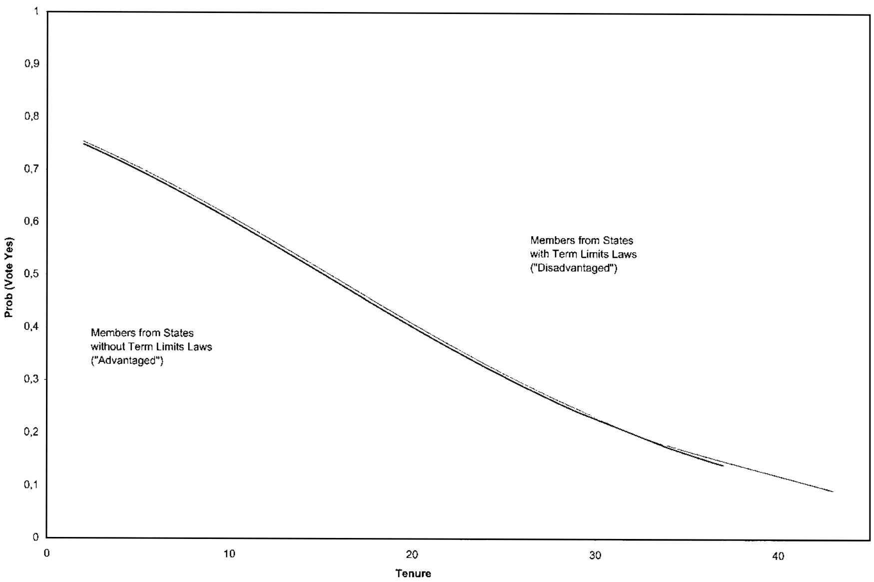

Figure 3b. Marginal effect of state law in 105th (after Thorton).

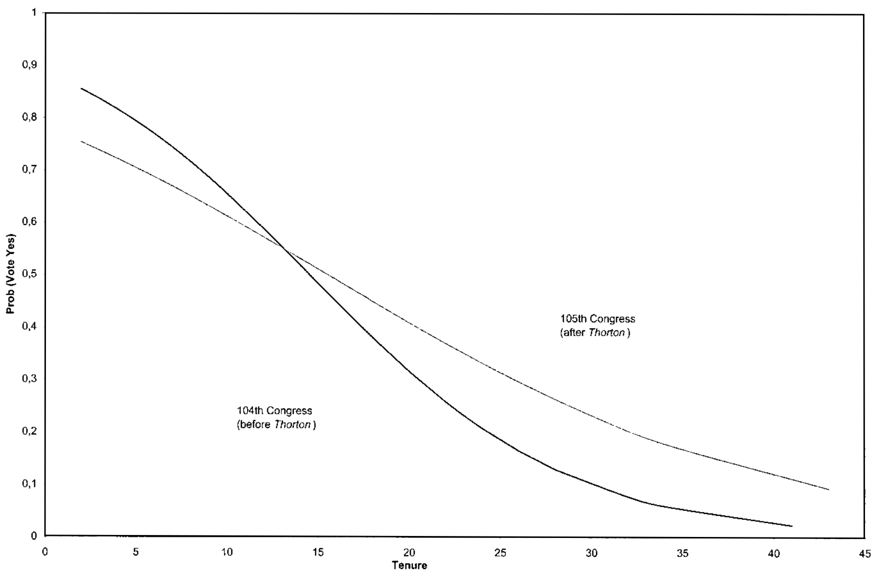

Figure 3c. Only members from states with term limits laws.

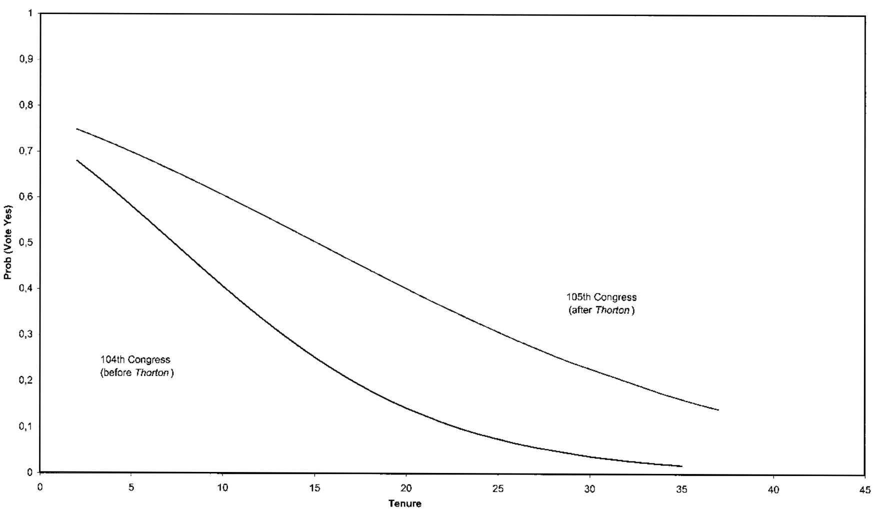

Figure 3d. Only members from states without term limits laws.

(i.e., holding all other independent variables at their means). Overall the two functions are statistically different at every tenure with 95% confidence (2-tailed t-statistic=–13.02 with df=23). In the context of Figure 1, disadvantaged members prior to *Thorton* are clearly opting for the third-best strategy – the only option available to them at that time.

On VOTE2, after *Thorton*, the marginal impact of STATELAW *completely disappears*, as can be seen in Figure 3B. There is no difference between the votes of members from states that had passed term limits laws and those that had not (the greatest difference in the two functions is .68 probability points at a tenure of 14 years). Overall the contribution of STATELAW to VOTE2 is statistically equal to zero. In the game in Figure 1, members from term-limited states no longer perceive themselves at a disadvantage. *Thorton* restored the pre-existing status quo, and all members are "free" to pursue whichever cell is optimal, given their own and constituent preferences. This explains why constituent interests are more statistically significant in the VOTE2 model.

Did members within each group tend to vote differently? Figures 3C and 3D look at the two votes for states with and without term limits. Consider Figure 3C first. Aside from the monotonic decrease in the function with tenure as pointed out, the most striking feature of this comparison is the intersection of the two curves. At approximately 12.8 years of tenure, the probabilities for both votes are .55. In both samples, mean TENURE is 9.6 – which is three-fourths of 12.8 – and 12.8 is at (or very near) the 70th percentile. These curves indicate that prior to *Thorton*, members beneath 1 and 1/3 of the mean (i.e., up to the 70th percentile) were *strongly* in favor of term limits, while those above that tenure were opposed. After *Thorton*, however, the separation between low and high tenure did not matter as much – the curve has flattened out considerably. Again, this points to the suggestion that members' characteristics, most notably tenure, were relatively less important in the second vote.

Figure 3D presents another interesting result. Members from states *without* term limits changed their vote *even more* than their colleagues from term limited states. The average difference between functions in 3C is 20 percent, whereas the average difference for 3D is 44 percent. Just as formerly term-limited members no longer perceived their disadvantage following *Thorton*, members from non-term limited states no longer perceived an advantage – they no longer obtained a differential payoff from voting against universal term limits.

While Figure 3 demonstrates that both disadvantaged and advantaged members exhibited strategic behavior consistent with the game in Figure 1, the marginal effects analysis does not directly test the hypotheses in Figure 2B. This suggests examining change of voting in a multivariate model so as to estimate joint and conditional probabilities and directly test the game-theoretic predictions.

4.2. *Aggregate change of vote behavior: A multinomial logit approach*

One way to test the predictions in Figure 2B is to use a multinomial logit in which the dependent variable (call it V′ for change-of-vote) is coded {0, 1, 2, 3} for members voting {N|N, Y|N, N|Y, Y|Y}. This is an *unordered*, categorical variable that accounts for the four possibilities given in Figure 2. The appropriate estimator is multinomial logit maximum likelihood, which I used to estimate V′ with the same set of independent variables as in Model 1 of Table 1. M-logit computes a complete set of parameter estimates for each value that the dependent variable can take. Since these parameters require much space to report, while adding no direct interpretive value, I do not report the estimation results here.[18] However, transforming the parameter estimates into information that is useful for present purposes – predicted probabilities – is a convenient procedure. The estimated probability that V′ takes on the value "j" for the ith member (a joint probability) is computed by evaluating the logistic CDF at the means of each independent variable:

$$\hat{\Pr}(\mathbf{V}' = \mathrm{j}) = \left[\frac{\mathbf{exp}(\hat{\mathbf{V}}'_{\mathrm{i}} \,|_{\mathrm{V}'=\mathrm{j}})}{\sum_{\mathrm{m}=0}^{3} \mathbf{exp}(\hat{\mathbf{V}}'_{\mathrm{i}} \,|_{\mathrm{V}'=\mathrm{m}})} \right] \tag{3}$$

The estimated simple probabilities of voting 'yea' or 'nay' on VOTE1 can then be computed from the estimates provided by (3). These are reported in Table 3A for both advantaged and disadvantaged members, and for both returning members (N=352) and all districts (N=427).

As expected, disadvantaged members are more likely to vote 'yea' on VOTE1 (which holds in both samples), reaffirming the earlier probit results. On VOTE2, however, disadvantaged are less likely to vote 'yea' in the member sample but more likely in the district sample. This is not inconsistent with the probit results; nor can it be compared to the analysis in Figure 3 (doing so would seem to present contradictory results) because these simple probabilities are not marginal effects. In fact, the simple probabilities in Table 3A do not have much useful interpretive value, but I report them because I next use them in calculating the conditional probabilities to test Figure 2B.

The joint probabilities estimated by (3) are reported, also for both samples, in Table 3B. As can be seen in the table, the results are always consistent with the stated hypotheses for members/districts that changed their vote. In particular, the probability of switching from a 'nay' to a 'yea' is higher for disadvantaged members than for advantaged. And this result is obtained

Table 3a. Estimated change-of-vote behavior – Multinomial logit estimated simple probabilities of member voting on each vote (as predicted by multinomial logit of model 1 from Table 1)

For returning members only (N=352)		For all districts (N=427)	
Member with term limits ("disadvantaged")	Member without term limits ("advantaged")	Member with term limits ("disadvantaged")	Member without term limits ("advantaged")
$P_{(Y1)}$=.887	$P_{(Y1)}$=.794	$P_{(Y1)}$=.606	$P_{(Y1)}$=.458
$P_{(N1)}$=.113	$P_{(N1)}$=.206	$P_{(N1)}$=.394	$P_{(N1)}$=.542
$P_{(Y2)}$=.805	$P_{(Y2)}$=.874	$P_{(Y2)}$=.566	$P_{(Y2)}$=.458
$P_{(N2)}$=.195	$P_{(N2)}$=.153	$P_{(N2)}$=.434	$P_{(N2)}$=.542

Notes. "$P_{(Y1)}$" denotes the probability of voting 'yea' on VOTE1. The other terms are defined accordingly. The dependent variable is 0,1,2,3 corresponding to N-N,Y-N,N-Y,Y-Y where "N-Y" indicates switching from yea on VOTE1 to nay on VOTE2. M-logit estimation results are not shown for brevity.

for both samples. However, the results do not always support Figure 2B for members and districts that did *not* switch their vote. Because these are joint probabilities, however, they present only an indirect test of the stated hypotheses.

The conditional probabilities are easy to calculate given Tables 3A and 3B. For example, the probability of voting 'nay' on VOTE2, conditional on having voted 'yea' on VOTE1, is simply: Pr(N|Y)=Pr(N,Y)/Pr(VOTE1=1). I calculated these conditional probabilities for all four categories, and report them in Table 3B, again for both the member and district samples. As can be seen in the table, all the hypotheses are confirmed in the data. In particular, disadvantaged members are more likely to vote 'nay' on VOTE2 if they cast a 'yea' on VOTE1. And advantaged members are more likely to vote 'yea' on VOTE2 having voted 'nay' on VOTE1. These are precisely the predictions from the game developed earlier (Figure 1) and explicitly stated in Figure 2B.

4.3. *Aggregate change of vote behavior: An ordered probit approach*

An alternative approach to testing Figure 2B would be to model the vote-switching decision with an *ordered* categorical variable. For example, if instead as defined above we have V'=VOTE1–VOTE2, then it is interpreted according to:

Table 3b. Estimated change-of-vote behavior – Multinomial logit estimated joint probabilities of member falling into categories in Figure 2A (as predicted by multinomial logit on model 1 from Table 1)

For returning members only (N=352)				For all districts (N=427)			
Member with term limits ("disadvantaged")		Member without term limits ("advantaged")	As pred-icted?	Member with term limits ("disadvantaged")		Member without term limits ("advantaged")	As pred-icted?
$P_{(N,N)}$=.090	>	$P_{(N,N)}$=.072	Yes	$P_{(N,N)}$=.380	>	$P_{(N,N)}$=.508	No
$P_{(Y,N)}$=.023	<	$P_{(Y,N)}$=.134	Yes	$P_{(Y,N)}$=.014	<	$P_{(Y,N)}$=.034	Yes
$P_{(N,Y)}$=.105	>	$P_{(N,Y)}$=.081	Yes	$P_{(N,Y)}$=.054	>	$P_{(N,Y)}$=.034	Yes
$P_{(Y,Y)}$=.782	<	$P_{(Y,Y)}$=.713	No	$P_{(Y,Y)}$=.552	<	$P_{(Y,Y)}$=.424	No

Notes. The dependent variable is defined as in Table 3A, thus, "$P_{(N,Y)}$" denotes the *joint* probability of voting nay on VOTE2 and yea on VOTE1. The other "$P_{(i,j)}$" terms are defined accordingly. M-logit estimation results are not shown for brevity. Predictions derived from Figure 2B appear as > or < in center columns.

Table 3c. Estimated change-of-vote behavior – Multinomial logit estimated conditional probabilities of vote-switching (calculated from Tables 3A and 3B)

For returning members only (N=352)				For all districts (N=427)			
Member with term limits ("disadvantaged")		Member without term limits ("advantaged")	As pred-icted?	Member with term limits ("disadvantaged")		Member without term limits ("advantaged")	As pred-icted?
$P_{N\|N}$=.796	>	$P_{N\|N}$=.349	Yes	$P_{N\|N}$=.964	<	$P_{N\|N}$=.937	Yes
$P_{Y\|N}$=.204	<	$P_{Y\|N}$=.651	Yes	$P_{Y\|N}$=.036	<	$P_{Y\|N}$=.063	Yes
$P_{N\|Y}$=.118	>	$P_{N\|Y}$=.102	Yes	$P_{N\|Y}$=.911	>	$P_{N\|Y}$=.074	Yes
$P_{Y\|Y}$=.882	<	$P_{Y\|Y}$=.898	Yes	$P_{Y\|Y}$=.089	>	$P_{Y\|Y}$=.926	Yes

Notes. "$P_{N|Y}$" denotes the probability of voting nay on VOTE2 *conditional* on having voted yea on VOTE1. The other "$P_{i|j}$" terms are defined accordingly. Calculated from Tables 3A and 3B above using $P_{i|j}$=P(i,j)/P(j1). Predictions derived from Figure 2B appear as > or < in center columns.

if $V' = -1$	then	Y|N	i.e., switch from 'nay' to 'yea';
if $V' = 0$	then either	N|N or Y|Y	i.e., no switch;
if $V' = 1$	then	N|Y	i.e., switch from 'yea' to 'nay'.

Table 4. Ordered probit estimated probabilities of vote-switching (calculated from ordered probit of model 1 from Table 1)

For returning members only (N=352)				For all districts (N=427)			
Member with term limits ("disadvantaged")		Member without term limits ("advantaged")	As pred-icted?	Member with term limits ("disadvantaged")		Member without term limits ("advantaged")	As pred-icted?
$P_{(V'=-1)}$=.016	<	$P_{(V'=-1)}$=.032	Yes	$P_{(N,N)}$=.037	<	$P_{(N,N)}$=.077	Yes
$P_{(V'=0)}$=.927		$P_{(V'=0)}$=.935	N/A	$P_{(Y,N)}$=.859		$P_{(Y,N)}$=.867	N/A
$P_{(V'=1)}$=.057	>	$P_{(V'=1)}$=.033	Yes	$P_{(N,Y)}$=.103	>	$P_{(N,Y)}$=.056	Yes

Notes. The dependent variable is V′, an ordered categorical defined as $\{--1, 0, 1\}$ for a member/district voting (Y|N, N|N or Y|Y, N|Y. Thus, a value of 0 indicates the member/district did not switch votes, while a –1 indicates switching from 'nay' on VOTE1 to 'yea' on VOTE2 and a 1 indicates the opposite switch. Predictions derived from Figure 2B appear as > or < in center columns.

For an ordered categorical variable, the appropriate estimator is ordered probit maximum likelihood. Ordered probit calculates a single parameter estimate for each independent variable, as well as threshold values, or "cut points," to split the estimation of the independent variable along the standard normal distribution. In this case there are two cut points because V′ – as now defined – takes three possible categories. The parameter estimates can then be used to calculate the probability that the dependent variable would fall in a given category as defined by the cuts. Specifically, the probability that V′ takes a category is computed by evaluating the standard normal CDF at the ordered probit estimates, given cut points and values for the independent variables. If k_1 and k_2 are used to denote the cut points, the estimated probabilities are:

$$
\begin{aligned}
&\hat{\Pr}(V' = -1) = \Pr(\hat{V}' < k_1) = F(k_1 - \hat{V}') \\
&\hat{\Pr}(V' = 0) = \Pr(k_1 < \hat{V}' < k_2) = F(k_2 - \hat{V}') - F(k_1 - \hat{V}') \\
&\hat{\Pr}(\hat{V}' = 1) = \Pr(\hat{V}' > k_2) = F(k_2 - \hat{V}')
\end{aligned} \tag{4}
$$

I estimated an ordered probit on V′ using the same independent variables as Model 1 of Table1 on both the returning-member and all-district samples (the same models as the multinomial logit above). Like the multinomial logit, the coefficient estimates in the ordered probit are not directly useful, so I do not report them here.[19] But they are used in my calculation of the probabilities in (4), which are reported in Table 4 for both samples.

This estimation is able to test the hypotheses for observations that switched votes, but not for those that voted the same (N|N and Y|Y are in the same category, $V' = 0$). As can be seen in Table 4, the probability of switching from 'yea' to 'nay' is higher for disadvantaged members in both samples, while the probability of switching from 'nay' to 'yea' is higher for advantaged members in both samples. This reinforces the earlier multinomial logit results, and demonstrates that the results are robust not only to sampling but to alternative estimators.[20]

5. Conclusion

The experience of congressional term limits provides a sort of natural experiment in which we can control for states' rights, congressional politics, and Supreme Court rulings. This affords us with an uncommon opportunity to observe both the outcome on term limits and patterns in congressional voting. In the 104th Congress, members from states that had unilaterally imposed term limits were at a strategic disadvantage, and they wanted to impose term limits on all other members as well. In contrast, members from states without term limits were at an advantage, and strongly opposed term limits for all members. The Supreme Court decision in *Thorton* restored the pre-existing status quo, and the data clearly show that this caused *both* types of members to vote systematically differently the next time around. The asymmetric advantages *completely* disappeared, and voting could no longer be distinguished by whether the member came from a state with term limits. *Thorton* took away the strategic imperative, leaving constituent interests to then play a larger role in explaining the second vote.

Because term limits would alter the distribution of parliamentary rights, the results in this paper are consistent with unilateral term limitation being irrational; however, that is far from clearly demonstrated in these data. More clearly demonstrated is the result that a member will vote to limit his own terms because his constituents want him to, but he will also vote to protect and maintain his own and his constituents' position vis-à-vis other districts. More broadly, this result would be consistent with legislators maximizing their net political support by satisfying constituent interests while also investing in their own representative capital.

Notes

1. The proponents of term limits were primarily responsible for advancing the argument that congressional term limits would reduce federal spending. This is understandable given the

laissez-faire character of political debates during that time: an opponent's most resonant argument would not have been that federal spending would remain high. See, for examples of arguments against, Schrag (1995) and Kesler (1992), neither of which directly discusses the impact of term limitation on spending. In contrast, proponents such as Moore and Steelman (1994), Coyne and Fund (1992), and Rotunda (1995) all explicitly argue that term limits will reduce the size of government. The popular press also framed the issue in terms of government spending, e.g. Henderson (1996), Miller (1991), Cannon (1995), and Star Tribune (1995). Finally, public opinion reflected the assumption that term limits would reduce the size of government (Moore et al. 1994, Saad 1994).

2. *U.S. Term Limits, Inc.* v. *Thorton*, 514 U.S. 779 (1995). *Thorton* only explicitly struck down the Arkansas statute, but made litigation to uphold any of the other 22 state laws certainly futile. Hence, the decision effectively ruled against all states' term limits laws. It was a 5–4 decision.
3. Senate leaders held contemporaneous hearings early in the 104th Congress. After delays in committee because the chairman opposed the bill, the Senate held floor debate in the spring of 1996 where the bill failed a cloture motion 58–42 and was permanently tabled. Empirical examination of the Senate vote is left to other research. Though see endnote 7 below.
4. See Groesclose and Stewart (1998) on committee portfolio values increasing monotonically (though diminishingly) with tenure. Empirical studies of logrolling, of which there are very few, do not explicitly examine the relationship between tenure and logrolling proficiency. However, key to a successful logroll coalition is its stability, which depends on relationships, tacit agreements, and reputations that build over time. Stratmann (1992, 1995) gives a good overview and the only prominent empirical treatment of logrolling. Finally, assumptions about learning-by-doing are common in research about legislator experience and tenure. E.g., Lott (1989) and Reed and Schansberg (1995).
5. It is not necessarily irrational for a locale to unilaterally self-impose. Generally speaking, this is because decreased relative representative capital is not the only effect of unilaterally self-imposing. See Adams and Kenny (1986), Tabarrok (1996), Glaeser (1997), Konrad and Torvsik (1997) and the summary discussion in Lopez (2000).
6. Special thanks are due to an anonymous referee at *Public Choice* for suggesting this exposition.
7. I also tested the Senate votes using this model, acquiring similar results overall (results available from the author). The Senate model, though similar, is not suited to testing the main theme of this paper because the Senate only conducted one vote (in the 104th Congress). For this reason, I leave close examination of the Senate model to other research.
8. See endnote 1 above.
9. Voting in the 104th and 105th Congresses was becoming increasingly partisan. Party line votes increased to 73.2 percent in the House and 68.8 percent in the Senate. This is all part of a trend toward increased partisan voting that began in the early 1980s (Carey 1996).
10. VOTE1. Beta-estimates and robust standard errors are: Constant: –1.528 (.184); REPUBLICAN: 3.072 (.252). Log likelihood=–203.07. $Chi^2(1)$=195.88. Pseudo R^2=.3254. VOTE2. Beta-estimates and robust standard errors are: Constant: –1.127 (.168); REPUBLICAN: 2.115 (.223). Log likelihood=–244.78. $Chi^2(1)$=103.54. Pseudo R^2=.1746.
11. Model 1 is the unrestricted model and the bivariate REPUBLICAN model is the restricted. Degrees of freedom are 9 and 9 in both tests. Chi^2 test statistics are 77.39 and 60.08 for tests on VOTE1 and VOTE2 respectively.

12. The Spearman rho statistics between TENURE and AGE are .593 and .364 in the respective samples. Between TENURE and MARGVICT, the rho's are .309 and .420. Between TENURE and FEMALE, they are –.135 and –.125. These are all statistically significant with at least 90% confidence.
13. I did not conduct tests for endogeneity because data limitations prevented the construction of a statistically sound instrument for TENURE. Given the stability of TENURE with variations in specification centered on other legislator characteristics, I expect an endogeneity problem is not likely to be severe.
14. Fox's sample consists of females elected between 1954 and 1992. My sample is 1995 to 1998. In the 104th House, there were a total of 47 female legislators, only 18 of which (38%) were elected in or prior to 1992. Likewise, in the 105th Congress only 15 of 49 (31%) were also in Fox's sample.
15. For the interested reader, these results are published on my web page: *www.econ.unt.edu/elopez*
16. The sample is reduced to 427 from the 435 House seats as follows. Six returning members did not cast votes for VOTE2. The representative from Texas district 28, Frank Tejeda, died late in the 104th Congress and was not yet replaced by the time of VOTE2. The representative from New Mexico district 03, Bill Richardson, was appointed to higher office and was not yet replaced either.
17. This is similar to the suggestion in Amemiya (1981) for interpreting the effect of qualitative regressors in a probit model. Greene (2000: 817–818) provides a similar example.
18. For the interested reader, these results are published on my web page: *www.econ.unt.edu/elopez*
19. These results are also published on my web page: *www.econ.unt.edu/elopez*
20. In fact, the same results are found if V' is redefined to include all four categories (results available from author), even though this is ordinally ambiguous because it is unclear whether a N|N should be ranked higher or lower than a Y|Y.

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Data Appendix 1. Variable names, definitions, sources, and descriptive statistics

Variable (E[sign])	Description and source	Units	Obs	Mean	σ	Skew	Min	Max
REPUBLICAN (+)	Dummy=1 if member is Republican. Barone (1994)	{0, 1}	434	0.537	0.249	–0.148	0	1
			427	0.529	0.250	–0.116	0	1
Tenure (-)	Member's time in office in 1994. Barone (1994)	Years	434	9.618	62.02	1.144	2	41
			427	9.529	58.32	1.3	2	43
TENURESQ (?)	Tenure*Tenure	Years	434	154.378	54905	2.570	4	1681
			427	148.988	54497	2.908	4	1849
AGEREP (–)	Member' age in 1994. Barone (1994)	Years	434	52.961	98.34	0.313	29	87
			427	52.741	98.30	0.273	26	87
FEMALE (?)	Dummy = 1 if member is female. Barone (1994)	{0, 1}	434	0.108	0.097	2.530	0	1
			427	0.115	0.102	2.415	0	1
MARGVICT (+)	Member's vote share less primary challenger's in last general election. FEC (1995)	Whole percent	434	33.714	721.32	1.009	0	100
			427	30.566	489.38	1.065	0	100
PUBEMP (–)	Total public sector employees in the district. US Census (1994)	1,000's of People	434	12.589	47.16	3.402	3.588	63.12
			427	12.578	47.28	3.403	3.588	63.12
WELFARE (–)	Welfare expenditures in the district. US Census (1994)	$1,000's	434	64.886	1601	2.251	13.57	308.23
			427	64.904	1597	2.262	15.34	308.23
FARMPOP (?)	Population living on farms within the district. US Census (1994)	1,000's of People	434	8.902	171.77	2.527	0	79.09
			427	8.906	172.37	2.518	0	79.09

Data Appendix 1. Continued.

Variable (E[sign])	Description and source	Units	Obs	Mean	σ	Skew	Min	Max
MEDINCM (–)	Median income of constituents.	$1,000's	434	30.741	70.06	0.860	15.06	57.22
	US Census (1994)		427	30.752	69.16	0.872	15.06	57.22
PEROT92 (+)	Percent of district voting for Ross	Whole Percent	434	18.412	36.47	–0.438	3	33
	Perot in 1992. Barone (1994)		427	18.395	35.67	–0.474	3	33
STATELAW (+)	Dummy=1 if member's state had	{0, 1}	434	0.424	0.245	0.309	0	1
	self-imposed term limits. Author is source.		427	0.425	0.245	0.305	0	1

Notes. For descriptive statistics, the first row corresponds to the 104th Congress and the second to the 105th Congress.