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Strategic institutional choice: Voters, states, and congressional term limits

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Abstract States' choices on term limits are quantified as a multiple-categorical variable capturing variation in the type of limits passed. Measures of relative political influence in Congress explain much of this variation. Using 1992 data on the American states, the model controls for unobserved heterogeneity due to voter access to direct democracy in some states. At 2002 values for congressional tenure and federal spending, the model predicts approximately eight to ten additional states would choose to limit their own members' terms but cannot under a Supreme Court ruling. We discuss implications for institutional federalism and the potential passage of similar political institutions across the states.

Keywords Term limits · Political institutions · Federalism · Political economy

JEL Classifications: D72, H7

1 Introduction

This study analyzes the empirical determinants of states' decisions whether to limit the terms of their congressional delegations. The primary result is that congressional term limits are more likely to be adopted, and with greater strictness, in states whose delegations have less tenure and influence in Congress. Results also indicate that states' choices on congressional term limits depend systematically on federal spending, voters' access to direct democracy, and demographic variables from theoretical models of tenure and term limitation.

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The results of this paper offer clues for evaluating the political-economic determinants of a class of institutional reforms. Term limits are unlikely to be imposed on Congress in the future because the Supreme Court's 1995 ruling in *U.S. Term Limits, Inc. v. Thorton* deemed state-imposed limits unconstitutional.[1] However, similar to other proposals such as national recall, balanced budget amendment, and certain types of campaign restrictions, term limitation works to constrain the power of representatives. Moreover, when states limit congressional terms there are spillover effects on other states. This is because congressional term limits would alter the distribution of political power within Congress (Friedman & Wittman, 1995), which in turn would alter the redistribution of wealth among the states (Dick & Lott, 1993; Dixit & Londregan, 1998). These results suggest the types of strategic motivations that would exist behind voter support for proposals that limit representative democracy, particularly where the change would alter states' relative advantages in redistributive politics.

Because congressional term limits are characterized by spillover effects as well as a tension between representative and direct democracy, there are natural implications for institutional federalism. Throughout this paper, we invoke a common distinction between *policy* outcomes and political *institutions*. Variables such as environmental and fiscal policy are generally decided through legislative and bureaucratic representatives. In contrast, political institutions are generally decided by popular votes (i.e., initiatives and referendums to amend state constitutions). Unlike policy changes, which alter the incentives of economic agents, institutional changes alter the incentives of policymakers themselves (Landes & Posner, 1975). A wealth of theory and evidence indicates that state officials strategically set policy variables. Game theoretic models of interjurisdictional competition delineate how states react to other states in setting policy (e.g., Wilson, 1996; Wildasin, 1988). Empirical evidence reveals strategic policymaking with regard to environmental and fiscal policy. For example, List, Bulte, and Shogren (2002) provide evidence for free-riding among states in endangered species expenditures following Reagan-era devolution of environmental regulation. In addition, Case, Hines, and Rosen (1993) show that state budgets react systematically to spending in other similarly situated states. States are especially prone to strategic interaction on policies that have pronounced spillover effects.[2] Based on this work, many scholars have turned to studying the welfare implications of federalized versus centralized policy choice.[3]

In contrast, relatively little work has explored the merits of centralized versus decentralized institutional choice. The issue is increasingly relevant, as a wave of Supreme Court decisions have recently impacted states' rights. *Thorton* centralized a mode of institutional choice that was important and popular among voters, thus strengthening the power of representative

[1] *U.S. Term Limits, Inc. v. Thorton*, 514 U.S. 779 (1995). Barring a reversal on *Thorton*, amending the U.S. Constitution is the only remaining mechanism for limiting congressional terms. Bills failed in Congress in 1995 and 1997, and a constitutional convention (as under Article V of the Constitution) is unlikely in the extreme. However, Congress reformed certain internal institutions after *Thorton*, such as term limits on committee chairs.

[2] As noted above, states free-ride on endangered species expenditures. In contrast, List and Gerking (2000) find no strategic interaction (no "race to the bottom") in air-quality control, manufacturing pollution abatement, or certain chemical emissions. List, Bulte and Shogren (2002) note that these contrasting results suggest that strategic competition is more prevalent with environmental goods whose values have spillover effects into other states. Coates and Munger (1995) offer additional evidence in this regard, demonstrating strategic regional interaction among eight southern states in negotiating where to locate a toxic waste facility.

[3] Gordon (1983) presents a theory for states to "race to the bottom" in tax policy. In contrast, Oates and Schwab (1988) demonstrate conditions (e.g., more homogeneity of voters) under which decentralized policy leads to efficient allocations. Wilson (1996) outlines conditions under which environmental policy can race to the bottom or race to the top. Oates (1999) provides a review and discussion. Laband (2000) discusses the role that tax havens play in limiting high taxation in other jurisdictions.

democracy relative to direct democracy. In addition, a 1997 Supreme Court ruling struck down part of Louisiana's unique open primary system (which frequently resulted in congressional candidates being elected during October) explicitly to avoid adverse consequences of spillover effects.[4] Where states are free to select institutions, there are often no spillover effects: states can choose methods of judicial selection such as the merit plan (Hanssen, 2002) and impose contribution limits and ballot access restrictions on state offices (Stratmann, 2004), but states generally cannot impose similar restrictions on federal offices.[5] Term limitation offers the opportunity to examine how states evaluate political institutions in the presence of spillover effects, toward a richer perspective on institutional federalism.

These results also address an important gap in the large empirical literature on term limitation. The most fruitful area of this work is in specifying the *consequences of* term limits on a variety of political and economic variables. For example, state legislative term limits in the early 1990s caused turnover rates to increase and average tenure to decrease, even before most legislators reached their limits, since many ran for higher office in anticipation of being forced out (Francis & Kenny, 1997; Francis, Kenny, & Anderson, 2000). Gubernatorial limits appear to reduce the incentive for politicians to maintain reputation, resulting in higher sales taxes, income taxes, and per-capita spending while decreasing the minimum wage (Besley & Case, 1995). Crain and Johnson (2004) echo these results for term limits on chief executives in OECD countries. In contrast, relatively little evidence exists to help understand the *causes for* term limits – i.e., the political-economic conditions under which a polity will be likely to decide to limit the number of terms its representatives may serve.[6] Important exceptions are Friedman and Wittman (1995) and Donovan and Snipp (1996), which indicate that voter partisanship is a primary determinant of a district supporting state-legislative and presidential term limits. Yet no study has successfully specified the empirical determinants of states' support for *congressional* term limits, which are distinguishable from limits on other offices in two regards. First, senators and representatives are the holders of elective office in the United States not subject to term limits. Second, compared to limits on local or state offices, congressional term limits are more likely to introduce spillover effects through the impact on redistributive politics.

2 Congressional term limits: Background and previous work

An upsurge of populism in the early 1990s created a period of institutional change across the states.[7] Most visible among these changes were limits on congressional terms. Colorado voters were first, voting in 1990 to limit House and Senate members to three and two,

[4] Louisiana's system, dating to 1978, declared candidates who won 50 percent of the votes in a contested primary the winner without a general election. The Court ruled that this conflicts with federal statutes establishing a uniform election day with the purpose of preventing earlier elections from influencing later voters. *Love v Foster* 522 U.S. 67 (1997).

[5] However, states' decisions on redistricting and apportionment are often superceded by the federal government's interest in protecting racial equity under the Voting Rights Act (*Shaw v. Reno* 509 U.S. 630, 1993). Crain (1999) and (2001) provides empirical evidence on the fiscal consequences and constitutionality of racial gerrymandering.

[6] Similarly, while most research on item-veto considers its effects, de Figueiredo (2003) empirically models the conditions under which states will adopt the item-veto; i.e., the choice of institution is the dependent variable.

[7] Between 1990 and 1996, 21 states enacted state legislative limits. Nine states enacted stricter gubernatorial term limits in 1992 and 1994. Mississippi enacted a statewide initiative in 1992.

respectively, consecutive terms in office. In 1992 fourteen states limited congressional terms, and another seven states followed suit in 1994. In April 1995 New Hampshire became the twenty-third and final state to pass congressional term limits. More states might have followed if not for *Thorton* in May 1995. Thus, unlike other populist institutions that spread across the states over long periods of time (e.g., gubernatorial limits or citizen initiatives), congressional term limits occurred mostly during the 1992 and 1994 general elections.

Among the states there is a strong correspondence between the presence of direct democracy and the choice to limit congressional terms. For example, voters can use the citizen initiative in 24 states, and 22 of those states voted to limit congressional terms (Illinois and Mississippi being the exceptions). Also, 21 of the 23 states passed congressional term limits via the citizen initiative (Utah and New Hampshire did so via state legislatures). Thus, direct democracy was the main institutional vehicle for states passing congressional term limits.

The issue of term limitation continues to motivate a large literature, of which several studies empirically estimate voting support for term limits.[8] As noted, Friedman and Wittman (1995) and Donovan and Snipp (1996) find that partisanship was a primary determinant of a district supporting term limits; however, their studies focus on state-legislative and presidential limits. López (2002) estimates support for congressional limits, but focuses on how legislators voted when the issue was defeated in Congress. Tabarrok (1996), López (1996), and Clain and Mao (2003) all estimate support by voters for congressional term limits. However, Clain and Mao (2003) exclude direct democracy from the model and Tabarrok's (1996) sample excludes the last eight states that limited terms. Moreover, these previous attempts to specify state support for congressional term limits were plagued by endogeneity between term limits and voters' access to direct democracy. There is a near one-to-one correspondence between states that passed term limits and states where voters can use the citizen initiative. Understandably, in previous results the direct democracy variable dominated explanatory power. This is troublesome because it leaves only the inference that a state would pass term limits simply if it had the initiative process, a conclusion at odds with a wealth of theoretical scholarship on term limitation.

Our approach to this estimation problem is twofold. First, we recognize that states passed term limits laws of varying types: some states were strict, others lenient. So we quantify the decision on term limits as a multiple-category dependent variable, unlike previous studies that used a dichotomous coding. Second, we control for unobserved heterogeneity by estimating a two-stage model that instruments for direct democracy to control for upward bias in its coefficient estimate. The results from this model indicate that states' choices on congressional term limits depend systematically on measures of relative influence in Congress – specifically tenure, leadership position, and federal spending by state – as well as other variables predicted by theoretical models of tenure and term limitation. Direct democracy plays an important empirical role in our estimations, but does not dominate explanatory power.

3 Theory: Why states would unilaterally choose congressional term limits

Term limitation is an institutional mechanism that affects the tenure-turnover tradeoff facing voters when choosing candidates for elective office. Voters benefit in certain ways from low turnover among elected officials. A state governor with considerable time in office has gained valuable job experience, has become adept as the ambassador of the state, and has

[8] López (2003) provides a survey of the term limits literature.

familiarity to voters in times of crisis. A U.S. senator, as she accumulates tenure in office, can more effectively represent the state's interests in Congress. With more experience, better committee assignments, and seniority (tenure relative to other senators) she can direct more benefits to the state – for example, greater transfers net of taxes.

Voters can also be harmed by high tenure. The governor, senator, or other elected official with longer tenure may accumulate skills at dissuading effective challengers, and use this electoral security to enrich their own and favored parochial interests. If political entry barriers become sufficiently high, voters may discover that removing their highly-tenured politicians is problematic; the incumbent advantage is too great, even in the face of poor job performance, shirking, scandal, or simply the presence of an attractive challenger.

The optimal tenure of elected officials occurs where the marginal tenure cost equals the marginal turnover cost (Adams & Kenny, 1986). Term limitation is a mechanism for approaching optimal tenure in polities that experience or anticipate excessive tenure costs. In 38 states, for example, voters have decided to impose some form of term limit on governors. Voters in these states implicitly volunteered for the costs of higher turnover in exchange for lower tenure costs.

A similar type of voter calculus presumably underlies the unilateral decisions by 23 states to limit congressional terms. However, unlike gubernatorial or state-legislative limits, states' decisions on congressional limits are interdependent, in that the tenure and turnover costs of one state depend systematically on the tenure and turnover costs in other states. Consider a state whose congressional delegation has accumulated relatively high average tenure. Members from this state hold leadership positions, rank high on valuable committees, and bargain strongly in logrolling. This delegation has a high degree of representative capital relative to members from other states. Consider instead a state with many junior members occupying the rank and file of the seniority system. This delegation has relatively low representative capital. States with relatively high representative capital are advantaged in acquiring net transfers. Thus, term limits would impose greater turnover costs on the state with relatively high representative capital. In short, voters face a strategic decision when considering optimal tenure and term limits for congressional office.

Universal term limits – which would occur if all states agreed to limit, or if Congress term-limited itself – would be welfare enhancing if tenure costs exceed the collective optimum for all states. In a transfer society, excessive tenure exacerbates the political shirking problem, the magnitude of government spending, and the associated deadweight losses created in the economy.[9] Voters could agree to keep tenure from becoming excessive. However, the election mechanism fails to enforce the agreement: voters are better off reelecting their own incumbent to protect relative tenure and free ride off voters who replace their delegation. Universal term limits are a means to solve the dilemma (Dick & Lott, 1993).

The free-rider explanation fits state-legislative term limits, which are universal. The extension to congressional term limits is less clear because states act unilaterally. When a state unilaterally limits the terms of its own representatives, it loses relative representative capital and net transfers. The decision is similar to choosing the low-payoff in an off-diagonal cell of a prisoners' dilemma game. Several possible explanations exist. Voters could have been signaling voters in other states their willingness to cooperate, perhaps engaging in Stackelberg leadership in hopes that other states would subsequently limit their delegations. The first movers were states where voters have access to direct democracy mechanisms (e.g., citizen

[9] The empirical evidence indicates at best a weak relationship between tenure and spending. See Sobel and Wagner (1998); Aka et al. (1996); and Reed et al.(1998). On this basis, López (2003) critiques the efficiency argument for term limits.

initiative). When nearly all states with citizen initiative had passed term limits, the Supreme Court ruled them unconstitutional. *Thorton* halted what may have become a trend toward states without the initiative process choosing to limit terms: New Hampshire was the last state to limit congressional terms (just weeks prior to the Supreme Court ruling) and only one of two states to do so by a vote in the state legislature. Without observing which additional states would subsequently limit terms, the cooperative signaling explanation cannot be tested.[10]

Other explanations present testable alternatives. First, risk-averse voters in states with highly diverse populations may benefit from term limits, not for the reduction in tenure, but for the increased frequency of open-seat elections (Glaeser, 1997; Tabarrok, 1994, 1996). A risk-averse group prefers being in power half the time to a one-half chance of never being in power. In more polarized states, a set of interests will experience greater disutility from any other set of interests being in power. Thus, groups in more diverse populations could be more likely to agree to limit their own terms, if only as a means to ensure that other groups are also limited.

Second, term limits may complement rational ignorance. Information costs about incumbents and candidates are approximately equal across the states, but voters' incentives to gather information, or their ability to process a quantity of information toward rational choices, can vary with education levels. By this reasoning, a more educated electorate is less in need of a substitute for elections and, therefore, less likely to choose term limits (Adams & Kenny, 1986).

Third, voters' ideological or demographic characteristics may influence their preferences for congressional term limits. Since term limits were widely expected to bring about a smaller government, more conservative or populist electorates would have been more likely to pass term limits. Voter ideology may be measurable with roll call voting records of a state's House members, or party identification such as presidential vote shares.

Fourth, voters' decisions on term limits may depend on extant transfers and representative capital. States with greater federal spending would have more to lose if term limits decrease the size of government. On the other hand, voters preferring limited government may perceive high rates of federal spending as indication of the deadweight loss associated with tenure costs. Measures of a state's transfer portfolio could include highway spending, defense spending, and/or federal funds in general. Similarly, states with high relative representative capital have more to lose with term limits, particularly when unilaterally chosen. An electorate whose representatives have higher average tenure, or more leadership positions, should be less likely to pass term limits.

Finally, direct democracy played a key institutional role in states' choices on term limits, which we turn to in the next section.

4 Empirical model

4.1 Citizen initiative

Since the citizen initiative (henceforth "*CI*") played a key institutional role, it is necessary that the estimation of the determinants of term limits (henceforth "*TL*") includes this variable. However, including *CI* as an explanatory variable may introduce endogeneity bias into the estimation if *CI* (or another measure of direct democracy) is determined by many of the same

[10] The empirical application below is used to predict by simulation which states would have passed term limits.

factors that contribute to a state's probability of having term limits. As long as these factors are observable to the researcher, there will be no estimation bias. However, if some of these factors are unobservable, then biases can arise. For example, assume the researcher wishes to run a simple regression of *TL* on the existence of *CI* by state. Also assume that another variable exists, e.g., "*preference by the inhabitants of a state for public control of the state's political institutions*," which has a positive effect on *CI* in that state and also has a positive impact on *TL*. If this factor is unobserved, then the coefficient on *CI* in this regression will be biased upward, since this coefficient will also capture the influence on *TL* of the unobserved factor.[11]

In fact, states adopted *CI* primarily through statewide popular votes (Matsusaka, 2004). Since *CI* was determined by the same political mechanisms and social processes that shape *TL*, there is a theoretical basis for expecting *CI* and *TL* to be determined endogenously (Shvetsova, 2003). Examining results in previous studies reveals also an empirical basis for endogeneity. Earlier work had difficulty calculating reliable estimates because the *CI* variable would swamp the effects of the other explanatory variables, leaving room only for the interpretation that states with direct democracy would be the states with congressional term limits. This conclusion disregards the theoretical motivations for term limits discussed earlier in this paper. We interpret the dominance of the *CI* variable as reported in other work as upward bias of its coefficient estimate. Our approach is to control for this endogeneity, allowing for more theoretically and empirically plausible findings based on unbiased estimates. To accomplish this, we need to construct a consistent estimate of *CI* that is purged of endogeneity bias for use as an explanatory variable in a regression of term limits on its determinants.

One obvious approach to controlling for endogeneity bias is an instrumental-variables-type estimation. In such an approach, an instrument, correlated with *CI* but uncorrelated with the unobserved factor(s), is used in place of *CI* in the term limit regression. To obtain such an instrument for *CI*, we estimate the logistic regression model:

$$\log\left[CI_{i0}/CI_{i1}\right] = \gamma_0 + \gamma_i X_i + \theta_i Z_i + \mu_i. \tag{1}$$

The dependent variable is the log odds of state i having the citizen initiative versus not having it; γ_0, the vector γ_i, and the vector θ_i are parameters to be estimated. X_i is a vector of independent variables, and the vector Z_i represents the variables to be used as instruments. The components of Z_i will also serve as exclusion restrictions to identify Equation (1) from the term limit regression discussed below; these instruments/restrictions will be discussed in the next section.[12] The term μ_i represents the error term, containing all unobserved determinants of *CI*, and is assumed to have a logistic distribution. Equation (1) is a reduced-form expression which can be used to generate predicted probabilities for *CI*. These predicted probabilities will be purged of endogeneity bias and can be used as exogenous regressors in the term limit regression. Specifically, the predicted value for CI_i (denoted $\hat{CI}_i$) equals the predicted probability that state i will have term limits computed using the estimates from Equation (1) and will be bounded by 0 and 1.

[11] This type of endogeneity is referred to as "unobserved heterogeneity."

[12] Notice that Equation (1) does not attempt to model the process by which a state adopts *CI* (an issue that is beyond the scope of this paper). The first stage merely attempts to obtain an instrument for the second stage.

4.2 Dependent variable

The most direct way to measure states' choices on congressional term limits would be a binary variable coded 1 for states that passed term limits and zero otherwise. However, such binary coding would ignore important variations in the types of the laws as passed. Nevada's law, for example, was relatively aggressive, allowing just three House terms and two Senate terms for life. In contrast, Utah's law was relatively lax, allowing multiple stays of six consecutive terms in the House and two consecutive terms in the Senate, following a one-term absence from office. Previous estimates of states' decisions on congressional term limits have used a binary dependent variable, ignoring variations in the types of the laws passed (López, 1996; Tabarrok, 1996; Clain & Mao, 2003).

Table 1 lists the length and waiting period required under each state's term limits law. The states naturally fall into three groups. Type 1 term limits allow relatively long stays in office and let individuals return to office after sitting out just one term. Type 2 limits allow individuals to return to office after a waiting period roughly equal to the term allowed in office. And Type 3 limits typically consist of six and twelve years in House and Senate, respectively, with no opportunity to return to office after a waiting period (i.e., lifetime limits).

Table 1 Types of state term limits laws

State	House term limits	Wait period	Senate term limits	Wait period	Type of term limit
Arizona	3 Terms	1 Term	2 Terms	1 Term	1
Colorado	3 Terms	1 Term	2 Terms	1 Term	1
Nebraska	3 Terms	1 Term	2 Terms	1 Term	1
Florida	4 Terms	1 Term	2 Terms	1 Term	1
Massachusetts	4 Terms	1 Term	2 Terms	1 Term	1
Ohio	4 Terms	1 Term	2 Terms	1 Term	1
South Dakota	6 Terms	1 Term	2 Terms	1 Term	1
Utah	6 Terms	1 Term	2 Terms	1 Term	1
Alaska	6 Years	6 Years	12 Years	12 Years	2
Montana	6 Years	6 Years	12 Years	12 Years	2
Wyoming	6 Years	6 Years	12 Years	12 Years	2
Idaho	6 Years	5 Years	12 Years	11 Years	2
Michigan	6 Years	5 Years	12 Years	11 Years	2
Washington	6 Years	6 Years	12 Years	6 Years	2
California	6 Years	5 Years	12 Years	5 Years	2
Maine	6 Years	5 Years	12 Years	5 Years	2
Arkansas	3 Terms	Lifetime	2 Terms	Lifetime	3
Oklahoma	3 Terms	Lifetime	2 Terms	Lifetime	3
Oregon	3 Terms	Lifetime	2 Terms	Lifetime	3
Nevada	3 Terms	Lifetime	2 Terms	Lifetime	3
Missouri	4 Terms	Lifetime	2 Terms	Lifetime	3
New Hampshire	3 Terms	Lifetime	3 Terms	Lifetime	3
North Dakota	12 Years	Lifetime	12 Years	Lifetime	3

Notes. The state laws express term limits either in years or in terms. Under most circumstances, a House term is equivalent to two years and a Senate term to six years. However, some of the state laws count any portion of a term served as a full term against an individual's limit. Thus, in some cases (such as members entering office at mid-term under special elections or gubernatorial appointment), a House/Senate term is less than two/six years. All states not listed have no term limits

This grouping suggests a multiple-category dependent variable, abbreviated *TL*, which is coded as follows:

TL	= 3	for Type 3 term limits	($n = 7$);
	= 2	for Type 2 term limits	($n = 8$);
	= 1	for Type 1 term limits	($n = 8$);
	= 0	for states with no term limits	($n = 27$).

We estimate the determinants of *TL* in the multinomial logit regression below:

$$\log\left[TL_{ij}/TL_{i0}\right] = \beta_{0j} + \beta_{1j}\hat{C}I_i + \beta_{ij}X_i + \varepsilon\beta_{ij}. \tag{2}$$

The dependent variable is the log odds that state i will choose term limit option j ($j = 1, 2$ and 3 defined above) relative to no term limits.[13] β_{0j}, β_{1j}, and the vector β_{ij} are parameters to be estimated and X_i is a vector of independent variables discussed in the next section. Note that the estimated coefficients will vary across term limit options. $\hat{CI}_i$ is the predicted probability of state i having term limits generated by the results from Equation (1) discussed above. The error terms, ε_{ij}, represent the unobserved determinants of term limit choice and are assumed to be independently and identically distributed as log Weibull.

4.3 Specification

Twenty-four states have adopted the citizen initiative, mostly in the late nineteenth century. The initiative's origins are tied closely to the Populist and Progressive movements, yet there is little scholarly evidence on the empirical determinants of *CI*.[14] Using longitudinal data, Oakley (1994) estimates the probability of adoption, by state, in each biennium, from 1882 through 1990. Dynamically estimating the determinants of states adopting *CI* is beyond the scope of this study. Our interest is in generating a statistical instrument for *CI*. To that end, we select the Z_i variables such that they are theoretically plausible determinants of *CI* but are not expected to affect a state's decision on congressional term limits:

- *STATESIZE*, geographic area of state in square miles;
- *SALARY*, salary per annum of state representatives; and
- *GOVERNOR*, binary variable coded 1 for states with gubernatorial term limits.

Voters in geographically larger states may have a stronger preference for *CI* due to greater costs of monitoring legislative activities or because larger states are generally in the west. States with higher paid legislatures may feature more constitutional constraints on legislative power such as *CI*. Finally, states with gubernatorial term limits will likely consist of voters

[13] It may appear that this categorization of *TL* is ordinal in the length of term limits passed or in the waiting period required, in essence measuring "stringency of term limits." In this case, it could be argued that the appropriate estimator is ordered logit. However, whether a polychotomous dependent variable should be treated as ordered or unordered depends on the underlying assumptions of the model as well as any intrinsic ordering of the dependent variable. Specifically, to treat *TL* as an ordered limited-dependent variable, we must make two assumptions: (1) our categorization correctly indicates the range of weak to strong term limits; and (2) utility is unambiguously increasing (or decreasing) in the stringency of term limits. Assumption (1) may not hold since the categorization is made based on two criteria, length of term and waiting period. Assumption (2) will not hold since utility is not unambiguously increasing (or decreasing) in the choice of term limit stringency. Instead, we only assume that states choose the type of term limits that maximize utility given the options Type 1, Type 2, Type 3, and no limits.

[14] Most research considers the fiscal impact of direct democracy; e.g., Matsusaka (2000), Feld and Matsusaka (2003), and Crain and Oakley (1995). Additional work focuses on voter competence and the influence of money on the vote outcomes in initiatives and referendums (e.g., Matsusaka, 2004).

more in favor of *CI*. Note that these variables are independent from the strategic interaction of states in seeking net transfers from Congress. Thus, these variables can be used to estimate *CI* and are not expected to have an effect on *TL*.

The exogenous X_j variables are chosen to measure the theoretical explanations for *TL* discussed earlier: voter heterogeneity; voter education; voter ideology; the strategic costs of term limits; and direct democracy.

- *POPDENSITY*, a state's 1992 population per square mile of land area; proxies voter heterogeneity.
- *EDUCATION*, percent of state population with college degree; proxies voter education.
- *NOMINATE*, mean of House members' DW-NOMINATE (Poole & Rosenthal, 1997) scores by state; proxies voter ideology.
- *PEROT*, percent of presidential vote share going to H. Ross Perot in the 1992 election; proxies voter ideology and, since Perot voters were widely cited as favoring congressional term limits, also measures a stronger preference for *TL*.
- *FEDERALFUNDS*, federal spending per capita; the extent to which a state's delegation brings in federal spending.[15]
- *TENURE*, average tenure of state U.S. House and Senate delegations; measures relative seniority held by a state's delegation.
- *LEADERS*, index of leadership positions held by a state's delegation; measures relative parliamentary power held by a state.[16]
- $\hat{CI}$, predicted values from Equation (1).

Definitions, sources, and descriptive statistics for all variables appear in Table 2.

5 Results

Table 3 presents the results from an estimate of Equation (2).[17] Multinomial logit calculates a set of parameter estimates for each category of the dependent variable. Each coefficient

[15] This is a broad measure of federal spending. According to the Economics and Statistics Administration, which reports the data, *FEDERALFUNDS* "includes Federal Government expenditures for grants to state and local governments, salaries and wages [to federal employees], procurement, direct payments for individuals, and other programs for which data are available by state and outlying area."

[16] In House, *LEADERS* $= 0 + 1$ for each committee chairman or ranking minority member; $+2$ for a whip position; $+3$ for minority leader; $+4$ for speaker or majority leader. In Senate, *LEADERS* $= 0 + 1$ for each committee chairman or ranking minority member; $+2$ for a whip position; $+3$ for minority leader; $+4$ for majority leader. Notice that this construction of the variable restricts the relative values of the various types of leadership positions (e.g., a majority leader is valued at four times a committee chair). This assumption could be avoided by using separate variables measuring the number of each type of leadership position in a state's delegation. However, due to small sample, we are limited by degrees of freedom in the model. Alternatively, we could simply count the number of leadership positions by delegation, thus imposing the opposite restriction that all leadership positions (majority leader, committee chair, etc.) are of equal value. What *LEADERS* really attempts to measure is the value of the state's congressional delegation. Toward that objective, we instill some variation that approximates well known, if tacit, rankings of types of leadership positions.

[17] The first-stage results appear in Appendix A. The instrumental variables are jointly and individually significant. The pseudo-R^2 is a healthy 0.560 and the correlation coefficient between *CI* and $\hat{CI}$ is 0.796. As required for identification, the exclusion restrictions are jointly significant in the first-stage estimation of Equation (1). Further evidence of the appropriateness of the exclusion restrictions involves the testing of over-identifying restrictions by comparing the estimates of Equation (2) to estimates of a reduced-form version of Equation (2) including all but one of the exclusion restrictions. Under the null hypothesis of valid exclusion restrictions, the two estimates of Equation (2) will not be statistically different. Testing the null hypothesis involves a chi2-test on the additional variables. The chi^2-statistic is 2.84, and we cannot reject the null at any conventional level

Table 2 Variable names, definitions and descriptive statistics

Variable Name	Description (Source)	Mean	Std. Dev.	Min	Max
TL	Multi-category dependent variable (See Table 1 and discussion.)	0.9	1.13	0	3
CI	Binary coded 1 for citizen initiative (Council of State Governments, 1995)	0.48	0.51	0	1
POPDENSITY	Thousands of persons per square mile, 1992 (U.S. Census, 1994, Table 27)	169.8	236.9	1.1	1062
EDUCATION	Percent population with any college degree, 1992 (Barone & Ujifusa, 1994)	45.42	7.09	29	64
NOMINATE	Mean of House members' voting record, 1992 (Poole & Rosenthal, 1997)	−3.82	19.81	−47.1	49.2
PEROT	Percent of 1992 presidential vote to H. Ross Perot (Barone & Ujifusa, 1994)	20.54	5.69	9	32
FEDERALFUNDS	Federal expenditures per capita, 1992, in thousands of 1992 dollars (U.S. Census, 1994, Table 518)	4.82	0.86	3.67	7.69
FEDFUNDS_2002	Federal expenditures per capita, 2002, in thousands of 1992 dollars (U.S. Census, 2003, Table 487)	5.69	1.15	4.11	9.77
TENURE	Mean years of tenure for House and Senate members, 1992 (Barone & Ujifusa, 1994)	22.36	8.12	6	44.33
TENURE_2002	Mean years of tenure for House and Senate members, 2002 (Barone & Ujifusa, 2004)	12.6	4.53	5.71	28.67
LEADERS	Index of leadership positions by delegation, 1992 (authors' calculations)	10.38	5.95	1	30
STATESIZE	Geographic area, thousands of square miles (U.S. Census, 1994, Table 351)	89.05	114.6	1.54	663.3
SALARY	Annual salary to state legislators, 1992, in thousands of 1992 dollars (Council of State Governments, 1995)	21.42	14.64	0.1	57.5
GOVERNOR	Binary coded 1 for gubernatorial term limits (Council of State Governments, 1995)	0.8	0.40	0	1

Table 3 Second-stage multinomial logit

	Coefficient estimate	Standard error
	$TL = 1$	
POPDENSITY	.010*	.006
EDUCATION	−1.18**	.584
NOMINATE	.17*	.089
PEROT	2.42**	1.22
FEDERALFUNDS	6.84*	3.59
TENURE	−.97*	.523
LEADERS	−2.34**	1.15
CI-Instrument	.71**	.333
Constant	−18.02	15.6
	$TL = 2$	
POPDENSITY	−.016	.021
EDUCATION	−1.25**	.616
NOMINATE	.18*	.103
PEROT	2.71**	1.24
FEDERALFUNDS	7.57**	3.69
TENURE	−1.05*	.561
LEADERS	−1.71*	.998
CI-Instrument	.77**	.354
Constant	−33.23*	17.4
	$TL = 3$	
POPDENSITY	−.014	.016
EDUCATION	−1.25**	.601
NOMINATE	.099	.075
PEROT	2.30**	1.13
FEDERALFUNDS	5.26	3.31
TENURE	−1.00*	.524
LEADERS	−2.31**	1.12
CI-Instrument	.64**	.329
Constant	3.44	9.68
Log likelihood	−21.18	
Chi-square (24)	77.07	
Prob > Chi-square	0.00	
Pseudo-R^2	0.64	

Notes. Dependent variable is *TL* as discussed in text, where $TL = 0$ is the base (comparison) category. Sample is the 50 U.S. states. Explanatory variable *CI-Instrument* is predicted value from first-stage logit (Appendix A).
See Table 2 for definitions and descriptive statistics of all variables
** Significant with 95% confidence
*Significant with 90% confidence

reports the effect of the independent variable on the log-odds that the dependent variable takes the value of the relevant category relative to the base category of 0 or "no term limits."[18] *POPDENS*, for example, exerts a positive and statistically significant influence on states choosing Type 1 term limits relative to no term limits; however, for Types 2 and 3 the effect is insignificant with a negative sign. This suggests that states that tend to have the citizen initiative also tend to choose Type 1 term limits rather than other types or none. Similarly,

of significance. Thus, the exclusion restrictions have statistical support in addition to the theoretical support discussed above.

[18] The standard errors of the second-stage estimation should be corrected for the inclusion of a predicted value (instrument) as a regressor. We attempt this correction via bootstrapping. However, possibly due to the small sample size, the bootstrapped standard errors are extremely large (on average 100 times as large as those from the mlogit), resulting in no statistical significance in the model. As a consequence, we report the standard errors from the second-stage estimation; they should be viewed with this in mind.

the ideology of the electorate, as measured by *NOMINATE*, significantly affects Types 1 and 2 relative to no limits but not Type 3 term limits.

Four variables explain a state's selecting term limits of any type. *EDUCATION* is negative and significant in all categories, confirming Adams and Kenny's (1986) prediction that more educated electorates have less need for term limits. *PEROT* is positive and significant for all categories, which is as expected given that Perot voters supported limits on congressional terms. *TENURE* is negative and significant for all categories, indicating that voters in states whose congressional delegations had high seniority were less likely to want any form of term limits. The empirical role of *TENURE* supports the theory that states' tenure and turnover costs are interdependent; i.e., that the decision whether to limit terms is strategic. This property is also reflected in the estimates on *LEADERS*: states with more leadership positions are less likely to want term limits, confirming the expectation that states protect their relative representative capital.

In contrast, states with greater *FEDERALFUNDS* are *more* likely to pass Type 1 or Type 2 term limits relative to no limits. This goes against the hypothesis that states protect the extent of wealth transfers being received, but instead indicates that voters wanting smaller government may view high government spending as indication of the deadweight loss associated with high tenure costs.[19] *FEDERALFUNDS* is insignificant in the choice of Type 3 limits, however, indicating that voters' willingness to constrain representatives does not extend to lifetime term limits. Lastly, the predicted values from the first-stage logit are positive and significant for all categories.

Overall the estimation shows that direct democracy is important to the passage of term limits, but that *CI* is not the entire story to be told. Constructing the dependent variable as multi-categorical to capture variations in the types of term limits, plus controlling statistically for the presence of direct democracy, allows for more substantive findings to unfold. In particular, the results in Table 3 highlight an awareness at the state level of the strategic costs of making institutional changes that affect policies and wealth transfers made at the national level.

5.1 Simulations

The multinomial logit coefficient estimates reported in Table 3 only indicate the direction of the effect relative to the base category. These coefficients tell us little about magnitude of the effect or the direction of effects relative to non-base categories. To measure marginal effects of the independent variables, we could evaluate the impact of changes in the independent variables at the sample means. Alternatively, one could simulate changes in these variables for each state and then compute the aggregate effect on the entire sample. Simulating marginal effects is more appealing than measuring the effects at sample means because a simulation incorporates information on the entire distribution of outcomes instead of just the mean. To provide a clearer sense of the magnitude with which the X_j variables influence *TL*, Table 4 presents results from simulated ceteris paribus changes in each of the independent variables.

[19] Evidence from the literature indicates that lower population states receive a disproportionately high share of federal spending, partly due to small states having more than proportional representation in the U.S. Senate (Atlas et al., 1995) and partly because senators from small states tend to be more focused on parochial interests (Atlas et al., 1997). Estimating the tenure-weighted impact of per capita representation on federal spending by state, Matthews, Shughart, and Stevenson (2006) find that high population states do not compensate for the small state spending bias by accumulating more tenure among their congressional delegations. If low population states have stronger preferences for term limits as well as receive more federal spending, this would also explain the positive sign on *FEDERALFUNDS*.

Table 4 Simulated changes in predicted probabilities and frequencies by category of *TL*

	Baseline predictions by category of *TL*			
	No limits	Type 1	Type 2	Type 3
Predicted probability (states in category)	.540 (27)	.160 (8)	.160 (8)	.140 (7)
	Simulated probabilities for 10% Increase in Indep. Variable (change in number of states relative to base category)			
POPDENSITY	0.537 (−0.13)	0.177 (+0.85)	0.151 (−0.47)	0.135 (−0.25)
EDUCATION	0.695 (+7.73)	0.130 (−1.48)	0.110 (−2.52)	0.065 (−3.73)
NOMINATE	0.547 (+0.34)	0.154 (−0.31)	0.160 (−0.01)	0.140 (−0.02)
PEROT	0.412 (−6.42)	0.181 (+1.04)	0.233 (+3.63)	0.175 (+1.74)
FEDERALFUNDS	0.428 (−5.62)	0.208 (+2.39)	0.215 (+2.73)	0.150 (+0.49)
TENURE	0.632 (+4.60)	0.132 (−1.41)	0.132 (−1.42)	0.105 (−1.77)
LEADERS	0.642 (+5.12)	0.106 (−2.70)	0.150 (−0.52)	0.102 (−1.89)
CI-Instrument	0.426 (−5.71)	0.189 (+1.45)	0.225 (+3.24)	0.161 (+1.03)
	Simulated probabilities at 2002 values (change in number of states in category			
FEDERALFUNDS_2002	0.382 (−7.88)	0.232 (+3.59)	0.231 (+3.57)	0.155 (+0.73)
TENURE_2002	0.339 (−10.03)	0.177 (+0.84)	0.228 (+3.39)	0.256 (+5.81)

Notes. Baseline probabilities are derived from second-stage mlogit estimates in Table 3.
Simulated probabilities are calculated for a 10 per cent increase, ceteris paribus, in the independent variable

In the top section of Table 4, we report the mean "baseline" probabilities for each category of *TL*, as predicted by the results in Table 3.[20] The baseline probabilities are computed in the following manner. First, from the estimated coefficients presented in Table 3, we calculate the predicted probabilities that each state falls into each category of *TL*. Second, we take these predicted individual probabilities and compute a mean for the entire sample. To compute the simulated changes, we modify a particular explanatory variable and then recalculate the predicted probabilities of *TL* for each category and state. From the new predicted probabilities, we compute the new fifty-state mean for each category. In the middle section of Table 4, we report the predicted mean probabilities after simulating a 10 percent increase in each independent variable. In parentheses appears the change in the number of states in each category as compared to the baseline probabilities.

For example, if *POPDENSITY* were to increase by 10 percent, the mean predicted probability that $TL = 0$ changes from 0.540 to 0.537, which decreases by 0.13 the expected number of states with no term limits.[21] This decrease is exactly offset by the combined expected changes to categories 1 (increase of 0.85 states), 2 (decrease of 0.47), and 3 (decrease of 0.25). Thus, if *POPDENSITY* increased by 10 percent, the model predicts some movement into Type 1 term limits and away from all other categories. With the exception of *NOMINATE*, the magnitude of *POPDENSITY* is small relative to the other variables in the model. Consider *EDUCATION*, for example. A 10 percent increase would result in 7.7 more states predicted to choose no term limits, with the largest change occurring in category 3 where only 3.27 states would remain with lifetime limits. The other exogenous variables influence *TL* with similarly large magnitudes, although some in different directions.

[20] The predicted baseline probabilities for each state appear in Appendix B.

[21] Although actual changes in the number of states would have to take on discrete values, we report the changes to two decimal places to facilitate comparisons among variables.

As a means of evaluating the effect of changing *CI*, we also simulate a 10 percent change in the $\hat{CI}$ instrument, even though a ceteris paribus change would only take place through a change in Z_i. The results corroborate our earlier conclusion that *CI* is important empirically, but its marginal impact does not swamp that of the exogenous variables.

The variables *FEDERALFUNDS* and *TENURE* are of special interest because of their policy relevance. By our 10 percent simulations, an increase in federal spending would result in fewer states expected to choose category 0; whereas *more* states would choose category 0 if there were an increase in congressional tenure. To complement the 10 percent simulations, we also simulate results at the actual 2002 values for *FEDERALFUNDS* and *TENURE*. These results are listed in the bottom section of Table 4. The descriptive statistics in Table 2 indicate that average real federal funds *increased* from \$4.82 to \$5.69 billion, and the tenure of the average delegation *decreased* from 22.36 to 12.60 years. At these 2002 levels, the model predicts relatively large movements out of category 0. At 2002 values of *FEDERALFUNDS*, a predicted 7.9 additional states would have some form of term limits. Most of these would choose Type 1 or Type 2, indicating that voters would be willing to constrain representatives but not to the extreme of lifetime term limits. At 2002 values of *TENURE*, an additional 10 states are predicted to limit terms, with most of these expected to choose Type 3 term limits. Thus, given the changes in federal funds and congressional tenure in the decade since term limits began to spread across the states, the model predicts that even more states would have been expected to limit congressional terms had the Supreme Court not prevented them from doing so.[22]

Our simulation procedure can also be used to suggest which specific states might have chosen to limit terms. We cannot say with certainty that any individual state will fall in a particular category, but the simulations can give a clear picture of how the probabilities will change in response to given ceteris paribus changes. Table 5 presents the baseline and simulated probabilities for selected states valued at *TENURE_2002* and *FEDERALFUNDS_2002*. The states are ranked by the magnitude of the change in probability of choosing $TL = 0$. Table 5 lists the top ten states most likely to switch from no term limits to limits of Type 1, 2, or 3.

In the top panel are the simulated probabilities at *TENURE_2002* values. West Virginia experiences the largest change in category 0: its predicted probability of choosing Type 3 term limits increases from 0.001 to 99.93 percent. Rhode Island has a much higher predicted probability of Type 1 term limits (0 to 99.89 percent), and Texas now appears most likely to choose Type 2 limits (90.63 percent). Again, these results cannot tell us with certainty whether a state would choose term limits, or the type of the limits that would be chosen, but such large changes in predicted probability are noteworthy. Since Ohio already had Type 1 limits, and is predicted by the simulations still to choose Type 1 limits, this top panel of Table 5 lists the ten states most likely to enact term limits under the 2002 congressional tenure profile, ceteris paribus. Of these ten states, two would most likely choose Type 1 limits (Rhode Island and

[22] It is possible that voters reacted to the Supreme Court's decision by sorting politicians more frequently. Intuitively, if voters want greater turnover but are not allowed to achieve this through term limits, they will do so at the voting booth. If this is the case, then the Court's decision would have partly determined the decrease in average congressional tenure, and the model would be picking up some of this in the simulated movements out of Category 0. We cannot test for this potential endogeneity; however, it seems implausible given that the largest decrease in tenure occurred in the 1994 elections, which took place before the Court's ruling. Nonetheless, the *TENURE_2002* simulations must be received with this in mind.

Table 5 Baseline and simulated probabilities by state and category for selected states

Rank	State	0		1		2		3		Actual
		Baseline	Simulated	Baseline	Simulated	Baseline	Simulated	Baseline	Simulated	*TL*
					Simulated at *TENURE_2002*					
1.	W. Virginia	.9995	.0001	.0000	.0007	.0000	.0000	.0005	.9993	0
2.	Rh. Island	1	.0011	.0000	.9989	.0000	.0000	.0000	.0000	0
3.	Alabama	.9772	.0038	.0002	.0077	.0000	.0000	.0226	.9885	0
4.	Mississippi	.9583	.0000	.0022	.0246	.0000	.0002	.0395	.9751	0
5.	Kansas	.8616	.0000	.0290	.0651	.0103	.2888	.0991	.6460	0
6.	Connecticut	.6646	.0019	.3353	.9980	.0000	.0000	.0000	.0001	0
7.	Ohio	.6627	.0000	.2944	.7741	.0053	.0524	.0376	.1734	1
8.	Texas	.6549	.0012	.0055	.0089	.3000	.9063	.0397	.0836	0
9.	Minnesota	.6432	.0007	.0015	.0031	.0000	.0001	.3553	.9961	0
10.	Georgia	.9996	.3890	.0000	.0039	.0000	.0001	.0004	.6071	0
11.	Hawaii	.9060	.3283	.0259	.1707	.0000	.0000	.0680	.5008	0
					Simulated at *FEDERALFUNDS_2002*					
1.	Alabama	.9772	.0412	.0002	.0747	.0000	.0007	.0226	.8841	0
2.	Mississippi	.9583	.0275	.0022	.2684	.0000	.0006	.0395	.7034	0
3.	Hawaii	.9060	.0268	.0259	.6230	.0000	.0003	.0680	.3502	0
4.	Kansas	.8616	.1041	.0290	.3216	.0103	.1856	.0991	.3886	0
5.	Connecticut	.6646	.0012	.3353	.9988	.0000	.0000	.0000	.0003	0
6.	Ohio	.6627	.0228	.2944	.9095	.0053	.0264	.0376	.0413	1
7.	Texas	.6549	.0341	.0055	.0114	.3000	.9194	.0397	.0351	0
8.	Minnesota	.6432	.1179	.0015	.0078	.0000	.0001	.3553	.8741	0
9.	Missouri	.3535	.0096	.0946	.1705	.2063	.5826	.3456	.2373	3
10.	W. Virginia	.9995	.7354	.0000	.0028	.0000	.0001	.0005	.2617	0

Notes. States are ranked by the magnitude of the change in predicted probability of choosing $TL = 0$; i.e. the change in probability of choosing no term limits. Complete rankings are available on request.
An entry of .0000 indicates a probability less than 10^{-7}

Connecticut), one would most likely choose Type 2 (Texas), Kansas would likely choose Type 2 or Type 3, and the six others would likely choose Type 3.

The bottom panel of Table 5 reports the simulations at *FEDERALFUNDS_2002*. These estimates reflect patterns similar to the tenure simulations. Alabama is likely to choose Type 3 limits, Texas Type 2, and Connecticut Type 1. Ohio is again likely to stay with Type 1 limits. Notice that Missouri, which is not ranked in the top panel's tenure simulations, has a higher predicted probability of choosing Type 2 rather than Type 3 limits. Thus, this panel lists the eight states without term limits that would be most likely to enact term limits under 2002 federal spending patterns, ceteris paribus. Finally, notice that eight states appear in both the top and bottom panels of Table 5. Taken together, the *TENURE_2002* and *FEDERALFUNDS_2002* simulations suggest that these eight states would be the most likely to enact term limits as of 2002 were the *Thorton* decision not an inhibiting factor.[23]

[23] We calculated simulations at both *TENURE_2002* and *FEDERALFUNDS_2002* (Appendix C). We hesitate to infer too much from the simulation in Appendix C as it stretches the ceteris paribus conditions on which the simulated calculations rely.

As a final note on the simulation results, consider the greater preference for term limits as average tenure declines. One point of view would suggest that this result is backwards. Specifically, if voters desire term limits because tenure is excessive, then the preference for term limits should weaken as tenure goes down. In other words, lower tenure should serve as a substitute for term limits. This view ignores the interdependence of states' tenure and turnover costs. When a state loses tenure, its tenure costs decrease but it also faces decreased turnover costs of choosing to enact term limits. Empirically, the above results indicate that the decreased turnover costs dominate the lower tenure costs, such that the mean preference for term limits increases (as it does, according to our results, for most individual states). Lower average tenure may be a substitute for the desire to enact *universal* term limits. However, for unilateral decisions, our results underscore the strategic nature of states' choices on congressional term limits.

6 Conclusion

Using a multi-categorical approach to states' choices on term limits, in a model that controls for endogeneity of direct democracy, this paper demonstrates empirical evidence that states interacted strategically in deciding whether to limit congressional terms. The Supreme Court decided in *U.S. Term Limits, Inc. v. Thorton* that unilateral term limits are unconstitutional. Absent *Thorton*, our simulations suggest that approximately eight to ten additional states may have passed similar laws.

These results suggest a natural extension to issues regarding institutional federalism. In particular, our findings suggest avenues for normative research on the welfare implications of centralized versus decentralized institutional choice. Some of these normative issues are framed by ongoing debates in states' rights and institutional federalism. For example, federal law currently prohibits states from adopting certain political institutions that have spillover effects, but allows states to choose many institutions that do not affect other states. Recent theoretical work has shown that limiting the power of direct democracy within multiple jurisdictions can result in greater centralization of *policy* choice (Redoano and Scharf, 2004), and the welfare effects of such policy centralization will depend on the intensity of voters' preferences for constraining representatives (Caplan, 2001). Our results provide some evidence on the intensity of such preferences, but invite a similar efficiency analysis of centralizing *institutional* choice. Moreover, de Figueiredo (2003) has demonstrated empirically that political factors as well as social efficiency motivate states' selection of political institutions. Our results underscore that states will interact strategically in choosing institutions that influence redistributive politics. The welfare implications of term limits aside, what are the gains and losses of centralizing the authority to impose term limits? The question of strategic institutional choice among the U.S. States presents a fruitful continuation of this area of research.

Our results also suggest the potential for strategic interaction among the states in reforming other institutions that have spillover effects, such as election reform, informed voter laws, national recall, and some types of gerrymandering. While the Court makes no reference in *Thorton* to interjurisdictional consequences, our evidence supports assigning a more prominent role to potential spillover effects in evaluating future state-level institutional reforms.

Appendix A: First-stage logit

	Coefficient estimate	Standard error
POPDENSITY	−.003	.003
EDUCATION	.046	.097
NOMINATE	−.028	.028
PEROT	.356**	.159
FEDERALFUNDS	−.162	.775
TENURE	−.323**	.138
LEADERS	−.067	.129
STATESIZE	.035***	.013
SALARY	.139**	.065
GOVERNOR	6.19**	2.55
Constant	−11.26	5.59
Log likelihood	−15.22	
Chi-square (11)	38.80	
Prob > Chi-square	0.00	
Pseudo-R^2	0.56	
N	50	

Notes: Dependent variable is $CI = 1$ for states that have citizen initiative process.
Sample is the 50 U.S. states
See Table 2 for definitions and descriptive statistics of all variables
** Significant with 95% confidence ***Significant with 99% confidence

Appendix B: Predicted baseline probabilities and actual category by state

	Predicted Probability that $TL =$				
State	0	1	2	3	Actual $TL =$
Alabama	0.9772	0.0002	0.0000	0.0226	0
Alaska	0.0000	0.0423	0.9577	0.0000	2
Arizona	0.0000	0.3416	0.6334	0.0250	1
Arkansas	0.2461	0.0002	0.0000	0.7537	3
California	0.2998	0.0001	0.7001	0.0000	2
Colorado	0.0000	0.5664	0.3891	0.0446	1
Connecticut	0.6646	0.3353	0.0000	0.0000	0
Delaware	1	0.0000	0.0000	0.0000	0
Florida	0.0000	0.9035	0.0908	0.0057	1
Georgia	0.9996	0.0000	0.0000	0.0004	0
Hawaii	0.9060	0.0259	0.0000	0.0680	0
Kansas	0.8616	0.0290	0.0103	0.0991	0
Kentucky	1	0.0000	0.0000	0.0000	0
Louisiana	1	0.0000	0.0000	0.0000	0
Maryland	1	0.0000	0.0000	0.0000	0
Massachusetts	0.2434	0.7566	0.0000	0.0000	1
Michigan	0.0269	0.3773	0.3216	0.2742	2
Minnesota	0.6432	0.0015	0.0000	0.3553	0

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State	Predicted Probability that $TL =$ 0	1	2	3	Actual $TL =$
Mississippi	0.9583	0.0022	0.0000	0.0395	0
Missouri	0.3535	0.0946	0.2063	0.3456	3
Montana	0.0000	0.1756	0.7777	0.0466	2
Nebraska	0.0000	0.4390	0.0579	0.5031	1
Nevada	0.0000	0.1861	0.3907	0.4232	3
New Hampshire	0.2130	0.0244	0.0000	0.7626	3
New Jersey	1	0.0000	0.0000	0.0000	0
New Mexico	1	0.0000	0.0000	0.0000	0
New York	1	0.0000	0.0000	0.0000	0
North Carolina	1	0.0000	0.0000	0.0000	0
North Dakota	0.0000	0.2879	0.0168	0.6953	3
Ohio	0.6627	0.2944	0.0053	0.0376	1
Oklahoma	0.0000	0.4765	0.1762	0.3474	3
Oregon	0.0360	0.3195	0.0026	0.6420	3
Pennsylvania	0.2552	0.3436	0.3823	0.0189	0
Rhode Island	1	0.0000	0.0000	0.0000	0
South Carolina	1	0.0000	0.0000	0.0000	0
South Dakota	0.0000	0.0396	0.0032	0.9572	1
Tennessee	1	0.0000	0.0000	0.0000	0
Texas	0.6549	0.0055	0.3000	0.0397	0
Utah	0.0000	0.3533	0.6139	0.0328	1
Vermont	1	0.0000	0.0000	0.0000	0
Virginia	1	0.0000	0.0000	0.0000	0
Washington	0.0000	0.3312	0.5161	0.1528	2
West Virginia	0.9995	0.0000	0.0000	0.0005	0
Wisconsin	0.9986	0.0000	0.0000	0.0014	0
Wyoming	0.0000	0.6806	0.1222	0.1972	2

Notes: Baseline predictions are from second-stage mlogit estimates (Table 3). An entry of .0000 means $< 10^{-7}$

Appendix C: Baseline and simulated probabilities at *TENURE_2002* and *FEDERALFUNDS_2002*

	0		1		2		3		
	Baseline	Simulated	Baseline	Simulated	Baseline	Simulated	Baseline	Simulated	Actual *TL*
Rhode Island	1	0.0000	0.0000	1	0.0000	0.0000	0.0000	0.0000	0
West Virginia	0.9995	0.0000	0.0000	0.0049	0.0000	0.0000	0.0005	0.9951	0
Delaware	1	0.0163	0.0000	0.8323	0.0000	0.0000	0.0000	0.1513	0
Georgia	0.9996	0.0223	0.0000	0.0166	0.0000	0.0000	0.0004	0.9610	0
Alabama	0.9772	0.0000	0.0002	0.0563	0.0000	0.0000	0.0226	0.9437	0
Mississippi	0.9583	0.0000	0.0022	0.1469	0.0000	0.0023	0.0395	0.8508	0
Vermont	1	0.0760	0.0000	0.0009	0.0000	0.0000	0.0000	0.9231	0
Hawaii	0.9060	0.0015	0.0259	0.6130	0.0000	0.0000	0.0680	0.3855	0

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	0		1		2		3		Actual
	Baseline	Simulated	Baseline	Simulated	Baseline	Simulated	Baseline	Simulated	*TL*
Kansas	0.8616	0.0000	0.0290	0.0854	0.0103	0.6148	0.0991	0.2998	0
New Mexico	1	0.1522	0.0000	0.0694	0.0000	0.0014	0.0000	0.7770	0

Notes. States are ranked by the magnitude of the change in predicted probability of choosing $TL = 0$ – i.e., the change in probability of choosing no term limits. Complete rankings are available on request. An entry of .0000 indicates a probability less than 10^{-7}

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