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Publication Date

2001-07-01

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INSULATION:
WHY STATES ADOPT THE ITEM VETO**

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Working Paper 2001-8

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Endogenous Budget Institutions and Political Insulation: Why States Adopt the Item Veto

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First version: January 1997

This version: July 2000

ABSTRACT

Forty-three of the fifty states of the United States have granted item veto authority to their governors as part of state constitutions. In this paper, I test four explanations of why and when a legislature would cede institutional power. Using data from 1865 to 1994, I show that these measures are most likely proposed by fiscal conservatives who fear the loss of power in the future; in order to protect their interests for those periods when they will be in the minority, therefore, they seek to obtain institutions such as the item veto which will limit future, liberal legislatures. The results therefore shed light on two important substantive areas. First, by endogenizing budgetary institutions, it clarifies that they are not necessarily adopted for economic efficiency. Second, it provides evidence in support of theories which have posited that electorally weak groups will heavily 'insulate' policies in periods in which they momentarily hold power (e.g. Moe 1989, de Figueiredo 2000).

1. Introduction

Forty-three of the fifty states of the United States have granted item veto authority to their governors as part of state constitutions. Primarily, these provisions have been proposed by the legislative bodies in those states. In so doing, legislative majorities have relinquished some control over their own fates—their ability to provide legislative pork, obtain programs, and so on—to other institutional actors. This presents a puzzle: why would a majority party in a legislature cede institutional power to its opponents and the executive? One possibility, implied by Cox and McCubbins, is that legislatures allow themselves to be disciplined as a way of overcoming a collective action problem.¹ But in the Cox and McCubbins argument, legislative majorities do not release power to their *opponents* but to their *leaders*. A second plausible theory is that, in the same spirit as Cox and McCubbins, legislators recognize they need to "tie their own hands", and therefore, collectively, they choose an institutional structure that effectively accomplishes such control. Again, however, this argument does not explain why particular *parties* would do this, *after* they have obtained power.²

* Haas School of Business and Department of Political Science, University of California at Berkeley. The author gratefully acknowledges Michael Bailey, Severin Borenstein, Brandice Canes, John de Figueiredo, Brian Gaines, Daniel Kessler, Keith Krehbiel, Jeff Macher, Nolan McCarty, Terry Moe, Douglas Rivers, Pablo Spiller, Rick van den Bergh, Barry Weingast, and seminar participants at Stanford University and the University of California at Davis, for helpful comments and conversations. The author also gratefully acknowledges Brian Gaines, Richard Sylla, John Legler, John Wallis and Martin Colaco, for assistance with the collection, provision, and cataloging, of the data used in this analysis. Special thanks go to Briana Blower and Conor O'Dwyer for excellent research assistance. Finally, the author would like to recognize the United States Department of Education for providing partial funding under the Jacob Javits Fellowship Program.

¹ Gary Cox and Mathew D. McCubbins, *Legislative Leviathan* (University of California: Los Angeles, 1993).

² For similar arguments about institutional methods of overcoming commitment problems, see Barry R. Weingast and William J. Marshall, "The Industrial Organization of Congress; or Why Legislatures, Like Firms, Are Not Organized as Markets," *Journal of Political Economy* 96 (1988), pp. 132-163; Lisa Martin, "Credibility, Costs and Institutions: Cooperation on Economic Sanctions," *World Politics* 45 (April), pp. 406-432; Douglass North and Barry

This paper addresses the question of why legislative majority parties will cede institutional power to governors and their opponents in the legislature over perhaps the most important source of legislative rents: the budget. I analyze four potential explanations for proposal of the line-item veto. One possibility accords with conventional wisdom: fiscal conservatives adopt such veto measures as a way of reducing a state's budget. Second and relatedly, such provisions could be adopted in situations of financial crisis. Finally, I consider two explanations based on a dynamic notion of policy implementation and policy insulation. According to this argument, groups that feel their prospects of retaining power are weak will attempt to "insulate" their policies from future interference.³ I argue that the line-item veto can be considered precisely such an insulation mechanism. Exactly who will have such a use of the item veto, however, depends on the effects of the item veto, over which the existing literature is divided. One view is that the item veto (weakly) reduces state budgets. If we maintain this view, then fiscal conservatives will be most likely to pass it, when they feel their hold on the legislature is tenuous. Alternatively, if as others argue, it does not reduce budgets, but simply shifts power from the legislative majority to the governor and legislative minority, it will be used by both fiscal conservatives and liberals when either's future prospects are weak. Rather than choose a singular assumption about the effects of the item veto, I utilize both potential views in my empirical specification and test.⁴

I test these four explanations in a multivariate context using panel data from 1866 to 1994 on line-item veto adoption dates, partisan compositions of state legislatures and governorships, and state financial data. The analysis provides evidence that the line-item veto is proposed by conservative legislators, irrespective of who is likely to hold the governorship, but only when these conservatives perceive their future prospects of winning the legislature to be weak. The analysis also shows that fiscal strain does not increase the likelihood of adoption. These results have applications in two areas. First, they provide test case evidence in favor of the theory that adoption of costly and durable institutions that shift power to minorities will occur only when the sitting majority perceives its future electoral fortunes as dim. Second, explaining the adoption of budgetary institution is important in its own right as the analysis also speaks to the literature on the institutions of public finance. While the brunt of this literature focuses on the *effects* of the line-item veto and other budget institutions, there is very little scholarship on the conditions under which these institutions are adopted. The results here move the level of explanation back one step further: making the adoption of the institutions themselves the dependent variable. The paper therefore also contributes to the burgeoning literature on "endogenous budgetary institutions".⁵

The paper proceeds as follows. In section 2, I posit the four potential hypotheses to explain the adoption of the item veto mechanism: that conservatives will adopt it; that they are adopted in times of fiscal strain; and that they are adopted as an insulation mechanism. This last, dynamic hypothesis is operationalized two ways, depending on whether we assume that the item veto simply reduces budgets or that instead, it shifts power between parties and institutional actors. The third hypothesis maintains the former and states that fiscal conservatives will be more likely to adopt the item veto, but in contrast to the first hypothesis, only when conservatives have been historically weak. The fourth hypothesis assumes the latter and therefore states that both conservatives and liberals will adopt the line-item veto, but only when their prospects of holding the legislature are weak and the governorship strong. In section 3, I describe the sources and issues for the data I use in the empirical analysis. In section 4, I describe the construction of the measures and econometric methods used to test the hypotheses. In section 5, I describe the results. The evidence supports only one explanation: that fiscal conservatives will adopt the line-item veto when their

R. Weingast, "Constitutions and Commitment: Evolution of Institutions Governing Public Choice," *Journal of Economic History* 49 (1989), pp. 803-832.

³ Rui J. P. de Figueiredo, Jr., "Electoral Competition, Political Uncertainty and Policy Insulation," Unpublished Manuscript, 2000.

⁴ It is important to note that my interest here is not in determining the effects of the item veto. These effects are simply *maintained* assumptions for my current purpose: testing hypothesis of power insulation by weaker parties in legislatures. With *either* maintained hypothesis, the theory can be tested. For this reason, I do not arbitrate between the two competing possible maintained hypotheses. Instead I assume each is conditionally true, and operationalize the theory using both, which provides the most general test of the insulation theory.

⁵ See for example, James M. Poterba, "State Responses to Fiscal Crises: The Effects of Budgetary Institutions and Politics," *Journal of Political Economy* 102 (1994), pp. 799-821; James M. Poterba, "Budget Institutions and Fiscal Policy in the U.S. States," *The American Economic Review* 86 (1996), pp. 395-400.

electoral prospects are dim, providing support for the theory of institutions as insulating mechanisms. Finally, in section 6, I offer some concluding remarks.

2. The Effects of the Line-item Veto and the Implications for Adoption

Prior to any explanation of *why* the line-item veto is adopted, it is necessary to consider what the actual or perceived *effects* of the line-item veto are. Certainly those who adopt the veto would only do so if they felt it would help them achieve their objectives. There is a large body of literature, both theoretical and empirical, on the effects of the line-item veto. Unfortunately, there is disagreement in this literature about the impact of the line-item veto. One view is that the item veto is *budget reducing*. It is this rhetoric that the public proponents of the line-item veto use in arguing its virtues. The veto authority is asymmetric, governors can only reduce budgets with it, so it must have a weakly budget-reducing effect, these scholars argue. Holtz-Eakin finds, for example, that under certain political conditions, the item-veto does reduce aggregate spending, although his results are not conclusive on this point. As Alm and Evers conclude, "The results...suggest that the item veto has a small and negative impact in total."⁶ According to this view, budgetary politics should be seen on a single dimension of more or less spending, and the effect of the item veto is to simultaneously shift power to the governor and reduce budgets.

Despite this view, other studies have found both theoretical and empirical evidence that the line-item veto does not have the large effect on aggregate spending originally assumed. For example, Dearden and Husted propose a spatial model that compares the absolute and item vetoes. They conclude that the line-item veto shifts power from the legislatively strong to the legislatively weak and particularly to the governor, and therefore, can either reduce *or* increase budgets, depending on who holds public office.⁷ Thus, they argue, while the item veto does not drastically reduce aggregate expenditures, it reallocates spending among programs, issues and policies. In other words, they view budgetary politics as multi-dimensional, with the item veto simply shifting the policy location on an iso-spending line. As Dearden and Husted conclude on the executive's power, "Taking into account the socioeconomic, institutional, time-specific and state-specific factors that determine the size of the state expenditure budget, the empirical results support the theory that the line-item veto enhances the governor's ability to obtain a more desirable budget."⁸

These assumptions are utilized to examine the adoption of line-item veto measures as a dependent variable.⁹ Rather than attempt to adjudicate between the two conflicting results on the budgetary effects of the item veto, I operationalize hypotheses based on *both*. I consider four here, which I term the conservative hypothesis, the crisis hypothesis, the conservative insulation hypothesis, and the non-partisan insulation hypothesis. The first of these, the *conservative hypothesis*, holds that it is largely fiscal

⁶ B. A. Abrams and W. R. Dougan, "The Effects of Constitutional Restraints on Government Spending," *Public Choice* 49 (1986), p. 112; C. K. Rowley, W. F. Shughart, and R. D. Tollison, "Interest Groups and Deficits," in J. M. Buchanan, C. K. Rowley, and R. D. Tollison, eds., *Deficits* (New York, 1987), p. 269. James Alm and Mark Evers, "The Item Veto and State Government Expenditures," *Public Choice* 68 (1991), pp. 1-15; Douglas Holtz-Eakin, "The Line-item Veto and Public Sector Budgets," *Journal of Public Economics* 36 (1988), pp. 272-273; Mark W. Crain and James Miller, "Budget Process and Spending Growth," *William and Mary Law Review* (Spring 1990), pp. 1021-1046; John R. Carter and David Schap, "Line Item Veto: Where is Thy Sting?," *Journal of Economic Perspectives* 4 (1990), pp. 103-118.

It is worth noting that this stream of the literature does not maintain that budgets are always reduced, but only under certain conditions. For the purposes here, this is sufficient to act as an incentive (perhaps weak) for adoption.

⁷ James A. Dearden and Thomas A. Husted, "Do Governors Get What They Want?: An Alternative Examination of the Line-Item Veto," *Public Choice* 77 (1993), p. 710.

⁸ Dearden and Husted, p. 717.

⁹ It is important to note that in most cases, the item veto is adopted as an amendment to the constitution. In general, the paths to amendment of state constitutions vary. In this case, for every amendment or full constitution adopted by the states, there has been a two-stage process of *proposal* by the legislature and *approval* by the voters. While it is certainly the case that the legislature will condition their proposal on anticipation of approval, this means that not every opportunity will be taken. Instead, the conditions posited here should be interpreted as necessary for approval, enhancing the probability of adoption. In practice, the constraint placed on legislatures by citizen approval is largely minimized in this case in that in general, most voters prefer to provide for strong budgetary responsibility.

conservatives who want to enact line-item vetoes. According to this argument, weak spending reduction leads fiscal conservatives to adoption of veto measures.

Hypothesis 1 (H1): Legislative control by fiscal conservatives will, on average, increase the probability that the line-item veto will be adopted.

A second, and non-competing hypothesis is that the line-item veto is adopted in periods in which there is significant fiscal crises, which I call the *crisis hypothesis*. One way of looking at this argument is that the line-item veto is more likely to be adopted when there is significant debt or budget deficit in a particular state. As fiscal policies become more extreme, a legislature is more likely to adopt a weakly budget-reducing measure such as the line-item veto. Thus, I have the non-exclusive

Hypothesis 2 (H2): The line-item veto is more likely to be passed when a state's fiscal position becomes increasingly worse.

The two *insulation hypotheses* are based on a formal theory of policy insulation developed elsewhere. According to this theory, when a historically weak party or group gains power, it will be more likely than historically strong parties to adopt mechanisms that trade benefits when they are in power for benefits when they are not in power. The rationale behind this argument is that if insulation is costly, those who are *most likely* to have their programs sabotaged are the most willing to pay those costs.¹⁰ How this type of dynamic thinking will be implemented, however, depends on the effects that the item veto is believed to have.

By definition, if budgetary politics can be reduced to one dimension, conservatives want less spending than liberals. If the item veto is perceived to be an aggregate budget-reducing mechanism in a one-dimensional policy fight, then, it will be more attractive to fiscal conservatives, as noted in *H1*. The question raised based on this maintained hypothesis, however, is why would conservatives be willing to cede power to the governor in order to reduce budgets if they could retain such power themselves? The answer lies in insulation. If conservative legislators feel they can retain power, they have no incentive to allow budgets to be reduced further, since they are already obtaining spending at a level near their ideal points. The item veto will only serve to move policy farther from their optimum. If a sitting conservative legislature feels it will usually be out of power in the future, however, it has a greater incentive to pay the price of ceding budgetary authority for times when they hold the legislature: when they lose control, spending will be lower with the line-item veto than without it, and therefore, closer to conservatives' preferences. No matter which party holds the governorship, conservative legislatures will do better by shifting power away from the legislative majority. Since the item veto, I assume here, simply reduces budgets, conservatives will gain benefits of such a reduction in many periods, losing only on the rare occasions they are in of power.¹¹ This analysis therefore suggests a third hypothesis, which I term the conservative insulation hypothesis:

Hypothesis 3 (H3): The line-item veto is more likely to be passed by fiscal conservatives when they have been historically weak¹².

¹⁰ To be precise, a mechanism will meet this criteria if it has three characteristics: first, it must shift rents from majorities to minorities; second, it must be durable; third, it must carry "policy costs." For a more detailed discussion see de Figueiredo 2000; Moe 1989, 1990.

¹¹ It is useful to clarify why the conservatives will lose when in power. Assuming that legislators of a conservative party will always want spending plus pork, and governors will be interested in reducing pork, since the item veto allows governors to reduce spending, and shifts the budget location weakly away from the policy point under no item veto, the conservatives will be weakly worse off when in power.

¹²Note that this hypothesis is stated in terms of *historical* electoral positions. I use a party's historical electoral results as a way of estimating the party's subjective estimate of its long term electoral prospects. That parties rely on their historical performance to estimate their subjective probabilities is a maintained hypothesis. Alternative maintained assumptions, such as a prospective calculus, are also tested, as I will discuss later.

Also note that H4 is a very sharp prediction for a number of reasons. First, it bases the prediction not only on current considerations but also past and future ones. Second, these conditions obtain relatively rarely. This is true in

Notably, H3 can be contrasted with H1. In the latter case, conservatives uniformly want budget reductions and therefore the item veto. In contrast, under H3, which takes account of a dynamic calculus, proposal of the line-item veto depends on the *interaction* of conservative control *and* their belief that they will be out of power most of the time.

In the previous case the party of the governor did not matter: conservatives gain from the line-item veto, irrespective of whether a fiscal conservative or fiscal liberal holds the governorship. This result was based on an assumption about the uni-dimensionality of budgetary politics. An alternative assumption one could make, however, is that while the item veto does not reduce spending, it shifts allocations from the legislative majority to the governor and legislative minority in a multi-dimensional policy space. In this case, the governor's party does matter. Since governors will be like legislators of the same party, conservatives and liberals alike will only cede such power when they feel the governor will be of their own party. In the context of the line-item veto, this suggests an explanation for when a legislature will propose the line-item veto: a party that considers itself to be traditionally electorally weak will pass a line-item veto if it does gain power *and* that their party will be represented in the governorship. Thus, as an alternative test of the insulation theory, I have the *non-partisan insulation hypothesis*:

Hypothesis 4 (H4): The line-item veto will be passed if:

- (i) there is undivided control by a party of both houses of the legislature in which that party has a large enough majority to pass amendments*
- (ii) that party is historically weak*
- (iii) that party historically holds the governorship.*

I term this the *non-partisan* hypothesis because in contrast to H3, *both* conservatives and liberals will act in similar ways under the conditions posited. In one sense, however, non-partisan is a misnomer, since in fact, if H4 is true, the legislative actors are keenly concerned about the likely partisanship of future public officials.

3. Data Sources and Issues

Data Sources. To test the effects of partisanship on the adoption of the line-item veto by state governments, I use three types of data. The first is the adoption dates for line-item veto provisions in the states. As of 1996, forty-three of the fifty states had adopted some line-item veto provision. These dates were collected from the annotated state constitution for each state and the U.S. House Committee on Rules' *Item Veto: State Experience and Its Application to the Federal Situation. The Book of the States* was used to obtain the proposal and passage requirements for amending each state's constitution.¹³ The information is

part, by definition, since conditions (i) and (ii) require that the current supermajority party must usually be in the minority. Further, conditions (ii) and (iii) require that divided government must have recently occurred. Empirically, there was divided control of the legislature and the governorship for about thirty percent of the state legislative sessions from 1866 to 1994. As an interesting aside, the requirement for (super)majority control and undivided legislatures is *not* very stringent. As a rough estimate, one party had supermajorities large enough in both legislative chambers to propose and/or pass amendments in as many as eighty-six percent of these cases. (Estimates based on data on partisan compositions for non-solid South, in the post civil war period. Estimates are preliminary.)

¹³ Note that Table 1 lists the *current* supermajority requirements necessary for proposal. Data for the requirements in each state over time would be superior, but is not reliably available.

contained in Table 1. Although there is variation in the type of provision—some states allow governors to amend lines rather than just strike them, for example—for the purposes of this analysis, I assume that the critical aspect of the veto is adoption.¹⁴ Two features of these adoption dates are important to note. First, the line-item veto originally appeared in an American institutional context when it was part of the constitution adopted by the Confederacy prior to the Civil War. The first appearance of such a provision at the state level was in 1865, when both Texas and Georgia included it as part of the post-War rewrites of their state constitutions. Second, no state that has adopted the provision has later rescinded it.

The second set of data is the partisan composition of each state legislature and governorship. This data was obtained from 1830 to 1995 from a number of sources. Most importantly, the data was gathered for 1830 to 1985 by Walter Dean Burnham and is contained in a data set available from the Inter-University Consortium for Political and Social Research (ICPSR), titled *Partisan Divisions Among the States, 1830-1985*. This data has been updated—including corrections to errors, elimination of missing data, and inclusion of the years 1986 to 1995—using a number of sources including *The Statistical Abstract of the United States*, *The Tribune Almanac* and *The Book of the States*, from various years.

The third set of data used in the analysis is state government financial data. Again the complete time series has been collected from a number of sources. For the years 1865 to 1915, data was obtained from another ICPSR data set collected by Sylla, Wallis and Legler, titled *Sources and Uses of Funds in State and Local Governments, 1790-1915: United States*. Since 1915, public records have been kept by the United States Bureau of the Census detailing state government finances. The data in this paper were obtained from three sources provided by the Census Bureau, depending on the dates. From 1915 to 1941, the data was obtained from a series called *Financial Statistics of the States*. From 1942 through 1964, the data was obtained from *The Compendium of State Government Finances* series. And for 1965 to 1981, it was obtained from the *State Government Finances Series*. From 1981 to 1995, the data was updated from *The Statistical Abstract of the United States*. Finally, statistics on state populations and national inflation rates were obtained from *Historical Statistics of the United States: Colonial Times to 1970, Volumes 1 and 2*.¹⁵

Missing Data and Measurement Problems. There are a number of issues concerning the data that must be dealt with. First, a number of the veto provisions are implemented as parts of constitutional revisions. This introduces noise into the data, since the veto provisions are agreed upon, presumably, as parts of larger bargains over new constitutional structures. Of course, if anything, this problem will cloud the results by making the conditions for passage less easy to identify. To be safe, however, I deal with this problem by estimating the results for all of the cases and the subset in which the item veto provision was passed by itself, as an amendment. Second, there is a significant proportion of missing data in the financial series. Some years are completely missing (1920, 1932 through 1936, and 1972). In addition, particularly in the ICPSR data for years prior to 1915, there are frequently missing observations. To deal with this problem, in reporting the results I test the models on the full data without the financial measures, and on the subset that includes the financial measures. Third, there are accounting issues that potentially make incomparable the data that is obtained from different sources. In the analysis below, where financial measures have been used to measure changes in financial positions, the first year in which data was obtained from a new source (for example, in 1915, I switch from the ICPSR data on state expenditures to the Census Bureau figures), I

¹⁴ Item veto authority comes in very different forms. The standard conception is that a governor can veto particular items in appropriations legislation. Fisher and Devins, however, point out that a number of states have adopted one of three modifications of the traditional item veto. First, some states have item-reduction vetoes as well, in which a governor does not have to eliminate a proposed expenditure entirely, but can also reduce it. A second modification on the traditional line-item veto is the amendatory veto. In this case, a governor can condition the veto on approval of amendments to be adopted by the state legislature. Finally, some governors, have item veto authority on non-appropriations items within appropriations bills. (Louis Fisher and Neal Devins, "How Successfully Can the States' Item Veto be Transferred to the President," *Georgetown Law Journal* 75 (1986), p. 166)

¹⁵ United States Department of Commerce Bureau of the Census (USBOC), *Financial Statistics of States*, annual serial, 1915-1931, 1937-1941 (Washington); USBOC, *Compendium of State Government Finances*, annual serial, 1942-1964 (Washington), G-SF Series No. 2; USBOC, *State Government Finances*, annual serial, 1965-1981 (Washington), GF Series, No. 3; USBOC, *Historical Statistics of the United States: Colonial Times to 1970, Vols. 1 and 2* (Washington, 1975), Tables A195-209; The Council of State Governments, *The Book of the States, Volumes 1-30* (Lexington, KY); USBOC, *Statistical Abstract of the United States*, annual serial, 1980-1996 (Washington). Note that since state population data was only available for each decade, a linear interpolation was applied to calculate the yearly totals for each state.

omit that year to ensure comparability. For example, the change in spending from 1914 to 1915 is suspect, so these observations are omitted from analysis involving changes in expenditures.¹⁶

Table 1. Adoption Dates of Line-item Veto

<i>State</i>	<i>Year</i>	<i>% Majority Required^d</i>	<i>State</i>	<i>Year</i>	<i>% Majority Required^d</i>
Alabama ^a	1875	60	Montana ^{ab}	1889	67
Alaska ^b	1960	67	Nebraska ^a	1875	60
Arizona ^{ab}	1911	50	Nevada	No provision	50
Arkansas ^a	1874	50	New Hampshire	No provision	60
California ^a	1879	67	New Jersey	1875	50
Colorado ^{ab}	1876	67	New Mexico ^{ab}	1912	50
Connecticut	1924	50	New York	1874	50
Delaware ^a	1897	67	North Carolina	No provision	60
Florida	1875	60	North Dakota ^{ab}	1889	50
Georgia ^a	1865	67	Ohio	1903	60
Hawaii	1960	67	Oklahoma ^{ab}	1907	50
Idaho ^b	1889	67	Oregon	1916	50
Illinois	1884	60	Pennsylvania ^a	1873	50
Indiana	No provision	50	Rhode Island	No provision	50
Iowa	1968	50	South Carolina ^a	1895	67
Kansas	1903	67	South Dakota ^{ab}	1889	50
Kentucky ^a	1891	60	Tennessee	1953	67
Louisiana ^a	1879	67	Texas ^a	1866	67
Maine	No provision	67	Utah ^{ab}	1895	67
Maryland	1891	60	Vermont	No provision	67/50 ^c
Massachusetts	1918	50	Virginia ^a	1902	50
Michigan ^a	1908	67	Washington ^{ab}	1889	67
Minnesota	1876	50	West Virginia ^a	1872	67
Mississippi ^a	1890	67	Wisconsin	1930	50
Missouri ^a	1875	50	Wyoming ^{ab}	1889	67

^a Adopted during a full constitutional revision

^b Adopted within six years of admittance as a state or before 1870

^c Vermont requires a two-thirds majority from the upper house and simple majority from the lower house for first passage

^d Override provisions are as of 1996.

4. Measures and Methods

As noted above, to test the hypotheses of interest it is first necessary to determine the spending patterns of each of the parties at the state level. Both the conservative hypothesis and the conservative insulation hypothesis require that I designate which party is more fiscally conservative. Although in the post-Progressive Era the Republicans are well-known to be more fiscally conservative than the Democrats, prior to that period the evidence is less clear. Therefore, using the above data, I test the hypothesis that Republicans are *higher* spenders in the period from the Civil War until the institution of the income tax in 1913. As documented in Appendix 2, I find that Republican state legislatures were higher spenders in the pre-Progressive period. Using this result, I turn back to the task at hand: explaining the adoption of the line-item veto.

Measures. In order to test potential explanations of line-item veto adoption, I construct a number of measures. A complete summary of the variables used appears in Table 2. First, I use the above dates to

¹⁶ Julie F. Pottorff, "Political Stew: Item Veto Issues Bubbling to the Top in State Court Jurisdictions." *Cornell Law Journal*.

construct a dummy variable for whether the line-item veto was passed in a particular year.¹⁷ I call this variable y_1 . Thus,

$$y_{1it} = \begin{cases} 1 & \text{if line-item veto passed in year } t \\ 0 & \text{otherwise} \end{cases}$$

Note that i indexes a particular state and t indexes a particular year. For each of the forty-three states that has adopted a line-item veto provision, y_1 equals one in the year it was adopted and zero in every other year. For the seven states that have never implemented a line-item veto provision, y_1 is set to zero for every observation. Note that the unit of analysis is a *legislature*.¹⁸

To construct the independent variables, I use two intermediate measures. The first x_1 is the sum of the Republican plurality in the lower and upper houses of each state. The second x_2 is a dummy variable which equals one if there is a Republican governor and zero otherwise.

Using these intermediate variables, I construct an independent variable to capture the notion of fiscal conservatives controlling the legislature. Notably, in many cases, a simple majority is not enough to propose or pass a line-item veto provision. As shown in Table 1, in many states, supermajorities are required. For each state, I call this requirement m_i . Thus, I can construct a dummy variable for conservative control

$$x_{3it} = \begin{cases} 1 & \text{if } \% p > m_i \text{ in upper and lower houses} \\ 0 & \text{otherwise} \end{cases}$$

$$\text{where } p = \begin{cases} \text{Democrat} & \text{if } t \leq 1912 \\ \text{Republican} & \text{if } t > 1912 \end{cases}$$

An analogous variable x_4 is set up for majority liberal control in which the parties in the definition of p are switched.

An additional set of measures captures the historical strength and weakness of the parties. To measure the historical weakness of the conservatives, I take the percentage of times the conservative party had a majority in both houses in the previous *three* sessions of the legislatures, which I term x_5 . An analogous measure x_6 is calculated for historical liberal strength in the legislature.¹⁹

¹⁷ See Appendix 1 for a general description of state constitutional amendment procedures. In every state, while legislatures have the prerogative to *propose* constitutional amendments, they must be *approved* by a referendum. Legislative gatekeeping power means that the conditions that I posit must exist for *proposal*. A potential problem, therefore, is that this process means that y_1 is actually a measure of legislative proposals *conditional on approval*. In other words, I miss two sets of potential proposals: those that are not made because they will not likely pass, and those that are made but do not pass. Although this does introduce a potential bias, I have investigated the histories in a number of states (California, Alabama, New York, Connecticut) and have not yet found a case in which proposals have been made that were not approved.

¹⁸ In practice, this means considering only the even numbered years for most of the legislatures.

¹⁹ The reason to use simple majority rather than supermajority as previously is because *after* the item veto has been adopted, it operates to constrain the majority from achieving the same outcomes it might have in the absence of the veto. In other words, it operates to constrain normal legislative policymaking which occurs under majority rule.

I define a similar measure for historical conservative strength or weakness in the governorship:

$$x_{7it} = \frac{1}{3} \sum_{j=1}^3 x_{2i(t-j)}$$

Similarly x_8 is defined analogously to measure historical liberal governorship strength. All four of these measures reflect the finding of a changepoint in fiscal conservatism after 1912. Two points are worth noting about these partisanship measures. First, x_5 through x_8 implicitly assume that elected officials are *retrospective* in estimating their future chances of winning elections. This would mean that if there is a regime change after a long period of control by one party, both parties would assume that this was an anomaly. An alternative is that elected officials are *prospective*, that their subjective assessment of their electoral prospects is based on a rational expectation. One example of such a process would be if after a change in the partisan composition of the legislature, officials assumed that the change represented a realignment. While I feel that it is more reasonable to assume the retrospective case, I also test the model with a prospective measure and obtain substantively the same results.²⁰ Second, the choice of basing the partisan strength measures on three periods is arbitrary. Notably, I chose this based on what I viewed was a reasonable assumption along with a requirement to include as many observations as possible. As noted earlier, as the period over which I calculate historical party strength increases, I am forced to ignore more and more observations at the beginning of the sample. Particularly because many states adopted line-item veto provisions soon after the Civil War, choosing longer lags would mean a significant loss of data. When the models are tested on the sub-samples with longer lags, the results are substantively stable.

The final measure I construct is to operationalize the idea that fiscal crisis might lead to the adoption of the line-item veto. A natural candidate to use for this would be a measure of state debt. Unfortunately, that data is very limited in its availability in the nineteenth century. Another alternative would be to use the size of a state's budget deficit. This measure, however, is inadequate, since many states have balanced budget amendments, and even those that do not use offsetting revenue programs to counter budget deficits. Thus, based on both availability and validity, I create a final variable x_9 which is a measure of the percentage change in per capita real government expenditures from one session to the next. I lag this variable one period to measure how much a state's expenditures rose in the previous legislative session. H2 would hypothesize that if spending had increased dramatically in the previous period, a legislature would be more likely to pass a line-item veto provision.

²⁰ To preview the results, if I estimate the fully-specified model using a prospective rather than retrospective measure, the coefficient of interest has a sign in the right direction and is significant at all conventional levels.

Table 2. Summary of Measures

<i>Variable</i>	<i>Indicator</i>	<i>Measures</i>
y_1	dummy for adoption of line-item veto	adoption of line-item veto
y_2	logarithm of per capita real expenditures	state fiscal policy
x_1	sum of conservative plurality in legislature	conservative legislative strength
x_2	dummy for conservative governor	conservative governor
x_3	dummy for conservatives have super majority in both houses	conservative party is sufficiently strong in current period to pass line-item veto
x_4	dummy for liberals have super majority in both houses	liberal party is sufficiently strong in current period to pass line-item veto
x_5	% times in 3 previous legislatures there has been conservative simple majority	historical conservative strength
x_6	% times in 3 previous legislatures there has been liberal simple majority	historical liberal strength
x_7	% times conservative party has governorship in previous three sessions	historical conservative party strength over the governorship
x_8	% times liberal party has governorship in previous three sessions	historical liberal party strength over the governorship
x_9	change in per capita expenditures from two sessions to one session previous	growth in government expenditures

Econometric Model. The method of testing the hypotheses concerning the line-item veto is a one-way transition, discrete hazard analysis. I make a number of assumptions about the process of adoption. First, I assume that the factors that drive the decision to adopt are *time-independent*. In other words, none of the factors are functions of t (so t does not appear in the model specification either directly or in any of the covariates). Second, I assume the transition is *one-way*. In other words, once a provision is adopted it will remain. While this is clearly not institutionally necessary, states can reverse the adoption of the line-item veto, a few facts make this a reasonable simplification. First, as indicated in Table 1, it is extremely difficult to reverse the adoption of a constitutional amendment in most cases. Second, no state has ever reversed a line-item veto amendment.²¹ This means that I eliminate all observations that occur *after* the line-item veto has been adopted. Finally, I assume that the *hazard function* can be represented by a standard

²¹ John Palffy, "Line-Item Veto: Trimming the Pork," *The Background* (April 1994), p. 6. It is interesting to consider why this behavior, adoption without reversal, obtains. As noted earlier, since these provisions are uniformly adopted upon proposal by legislatures and approval by voters, adoption and revocation are conditional on support from the voters. Here, a review of the histories in a number of states indicates that voter support is usually in favor of budget restraint mechanisms, therefore making it easier to adopt the measures than to revoke them. Further, if I assume that unlike legislators, governors tend to be more concerned about their personal fortunes—in other words that governors have small discount factors in comparison to legislators, then governors interests are also asymmetric. While governors therefore will throw all of their institutional power in order to see these mechanisms passed, they will do the opposite, fighting revocation proposals. Combined with the incentives of voters, therefore, this asymmetric nature of governors' interests makes revocation very unlikely.

normal cumulative distribution function. Although the final assumption is appropriate and at the same time flexible, as a check for the robustness of the specification, I also present the results when exponential and Cox proportional hazard models are utilized.

These three assumptions mean I can represent the event history model as a standard probit in which I condition on the event not yet having occurred.²² Putting all of these assumptions together, I can construct a formal representation of this model. For a vector of covariates \mathbf{x} , I have

$$P(y_{lit} = 1 | \mathbf{x}_{it}, y_{lis} = 0 \text{ for } s < t) = \Phi(\beta' \mathbf{x}_{it}) \quad (1)$$

which can be estimated by the usual maximum likelihood methods for a probit model. To test the four hypotheses, therefore, the fully specified model is:

²² The probit function I estimate in the above sections can be put into a traditional hazard model framework by transforming the above model into the corresponding probability density function, survivor function and hazard function. To see this, suppose T is a random variable that indicates the time of an event. If $T=t$, then the event has occurred at time t . A *hazard function* $\lambda(\cdot)$ is an instantaneous probability that the event occurs in time t conditional on it not yet having occurred. In the context of the above model, this means I can write the hazard function as

$$\lambda(t | \mathbf{x}, \text{unpassed}) = P(T = t | T \geq t, \mathbf{x}, \text{unpassed}) \quad .$$

Since this is a one-way transition model in which the hazard rate is assumed to be time-independent, I can rewrite the hazard function as

$$\lambda(t | \mathbf{x}, \text{unpassed}) = P(y_{2it} = 1 | \mathbf{x}, y_{2is} = 0 \text{ for } s < t)$$

which is simply the function I use to construct the likelihood in (1). I also express the *probability density function* which is

$$\begin{aligned} f(t_j) &= P(T = t_j | \mathbf{x}) = P(y_{2it} = 1 | \mathbf{x}) \prod_{s=1}^{t-1} P(y_{2is} = 0 | \mathbf{x}) \\ &= \Phi(\beta' \mathbf{x}) \prod_{s=1}^{t-1} (1 - \Phi(\beta' \mathbf{x})) \quad j = 1, 2, \dots \end{aligned}$$

where t_j indicates the j th discrete time point and satisfies $t_1 < t_2 < \dots$. This means that the cumulative distribution function is

$$F(T \leq t_j) = \sum_{k=1}^{t_j} f(t_k) \quad .$$

Now I can represent the *survivor function*, which is the probability of *not* having the event prior to time t , in the familiar way

$$S(t | \mathbf{x}, y_{2is} = 0 \text{ for } s < t) = P(T \geq t | \mathbf{x}, y_{2is} = 0 \text{ for } s < t) = 1 - F(\beta' \mathbf{x}_{it}) \quad .$$

Now I have the traditional set-up for a discrete one-way transition hazard model, although because of the time independence of the dependent variable, it means that the hazard is not a function of time as in most duration models. (Kazuo Yamaguchi, *Event History Analysis* (Newbury Park, CA, 1991); Hans-Peter Blossfeld, Alfred Hamerle, and Karl Ulrich Mayer, *Event History Analysis: Statistical Theory and Application in the Social Sciences* (Hillsdale, NJ, 1989); Ronald Kiefer, "Economic Duration Data and Hazard Functions," *Journal of Economic Literature* 26 (1988), pp. 646-678)

$$\begin{aligned}
P(y_{lit} = 1 | \mathbf{x}_{it}, y_{lis} = 0 \text{ for } s < t) = & \\
& \Phi(\alpha + \beta_3 x_3 + \beta_4 x_4 + \beta_5 (1 - x_5) + \beta_6 (1 - x_6) + \beta_7 x_7 + \beta_8 x_8 + \beta_9 x_9 \\
& + \beta_{10} x_3 (1 - x_5) + \beta_{11} x_3 x_7 + \beta_{12} (1 - x_5) x_7 + \beta_{13} x_4 (1 - x_6) \\
& + \beta_{14} x_4 x_8 + \beta_{15} (1 - x_6) x_8 + \beta_{16} x_3 (1 - x_5) x_7 + \beta_{17} x_4 (1 - x_6) x_8) \quad (2)
\end{aligned}$$

A few points are worth noting about the implementation of this model. First, the panel nature of the data means that there exists the possibility that there is both heteroscedasticity and autocorrelation. Although these problems will not affect the estimates of the coefficients, they will lead, if present, to inconsistent estimates of the standard errors. Therefore, to eliminate this potential problem, I recalculate the standard errors of the estimates using an adaptation of the method suggested by Newey and West. The details of this procedure are outlined in Appendix 3.²³ Second, because of the lagged variables, it is not possible to estimate this model for states which implemented the line-item veto within six years of admission of the state to the Union. Third, the seven states which never passed a line-item veto were included in the pooled sample, although they never have $y_{lit} = 1$. Fourth, because of their nonpartisan state politics, I excluded Nebraska and Minnesota.

For evidence of H1, I would expect $\beta_3 > 0$. This would mean that if there is a sitting conservative supermajority, the probability of adoption increases. For evidence of H2, I would expect $\beta_9 > 0$; as expenditures rise, so does the probability a line-item veto will be adopted. For H3, I would expect that $\beta_{10} > 0$. This means that for confirmation of the conservative insulation hypothesis, I would expect if there is a conservative supermajority in the present legislature and historical conservative weakness in the legislature, the line-item veto is more likely to be adopted. Finally, for H4, I would expect that both $\beta_{16} > 0$ and $\beta_{17} > 0$. This test means that for evidence of the non-partisan insulation hypothesis, I would expect the probability of adoption of the item veto to rise if conservatives have a supermajority in *both* houses, if they have historically been weak, *and* if they have historically controlled the governorship; or the same for the liberals.

5. Results

I estimated five models. Models 1 through three are estimated using the probit specification with the corrected standard errors presented in the previous section. Model 1 includes only the dummy variable for supermajority conservative control x_3 . Model 2 includes the full set of partisan variables. One problem which occurs when including all of the partisanship variables in (2) is multicollinearity. As an example, the auxiliary (OLS) regression of x_3 on the remainder of the explanatory variables yields an *R-squared* of 0.97. To eliminate this problem, I impose the following restrictions on (2): $\beta_6 = \beta_{15} = 0$ and $\beta_{16} = \beta_{17}$. The former restriction is because the historical weakness of the liberals in a legislature is almost perfectly correlated with the historical weakness of conservatives.²⁴ The latter restriction is imposed for theoretical reasons: H4 states that both conservative and liberal parties will behave the same way. A *pre-test* of this restricted model yields a likelihood ratio statistic with a p-value of 0.31, indicating the restrictions are reasonable. Model 3 includes all of the variables for H1, H3, and H4, as well as the financial variable for H2. The results of the estimation of the three models are reported in Table 3. Notably, Model 3 is estimated only for the subset of the data for which the financial data is available. When Model 2 is estimated with this subset of the data, the results are substantively identical to those reported here for Model 2. Finally, Models

²³ Greene, Chapters 13-15 and 21; Halbert White, "A Heteroscedasticity-Consistent Covariance Matrix Estimator and a Direct Test for Heteroscedasticity," *Econometrica* 48 (1980), pp. 817-838; Halbert White and Ian Domowitz, "Nonlinear Regression with Dependent Observations," *Econometrica* 52 (1984), pp. 143-161; Whitney K. Newey and Kenneth D. West, "A Simple, Positive Semi-Definite, Heteroskedasticity and Autocorrelation Consistent Covariance Matrix," *Econometrica* 55 (1987), pp. 703-708.

²⁴ For example, the correlation between x_5 and x_6 is -0.73.

4 and 5 are the same as Model 2, except they use a Cox proportional hazard model and exponential hazard model respectively.²⁵

²⁵ Three points are worth noting about the final two columns. First, in both cases, Model 2 is replicated since missing data in the financial series is problematic for a standard survival analysis with time varying covariates. Second, there is no constant reported for the Cox model as it is eliminated in the specification. Third, there are more observations than in Model 2 because the additional lags required for the autocorrelation correction in the probit structure are *not* required here, saving some observations for analysis.

Table 3. Explaining Line-Item Veto Adoption: Estimates

<i>Dependent Variable: P (y_{jit} = 1)</i>	<i>Model 1</i>	<i>Model 2</i>	<i>Model 3</i>	<i>Model 4</i>	<i>Model 5</i>
α : <i>Constant</i>	-1.826	-16.401	-22.805		-52.116
x_3 : <i>Conservative Party Control</i>	0.184 (0.157)	-1.468 (1.193)	-4.856 (3.444)	-2.573 (2.102)	-2.454 (2.004)
x_4 : <i>Liberal Party Control</i>		7.941 (1.039)	9.248 (1.459)	-2.447 (3.263)	-3.772 (3.364)
$1 - x_5$: <i>Historical Conservative Weakness</i>		-1.116 (0.954)	-3.726 (2.192)	-0.708 (1.545)	-0.835 (1.480)
x_7 : <i>Historically Conservative Governor</i>		6.235 (1.861)	8.754 (4.607)	41.959 (14.6E+3)	47.421 (7.5E+2)
x_8 : <i>Historically Liberal Governor</i>		15.498 (2.444)	24.365 (8.176)	43.675 (14.6E+3)	49.347 (7.5E+2)
$x_3(1 - x_5)$: <i>Conservative Control and Historical Conservative Weakness</i>		2.224 (1.381)	5.360 (3.622)	4.472 (2.612)	4.423 (2.445)
x_3x_7 : <i>Conservative Control and Historically Conservative Governor</i>		9.929 (2.404)	17.520 (7.008)	2.334 (2.685)	2.504 (2.638)
$(1 - x_5)x_7$: <i>Historical Conservative Weakness and Historically Conservative Governor</i>		1.602 (2.003)	7.467 (6.608)	0.844 (2.742)	1.440 (2.663)
$x_4(1 - x_6)$: <i>Liberal Control and Historical Liberal Weakness</i>		0.564 (1.007)	3.882 (4.853)	3.481 (3.295)	5.270 (3.389)
x_4x_8 : <i>Liberal Control and Historically Liberal Governor</i>		-7.962 (1.276)	-8.832 (1.895)	2.864 (3.517)	4.307 (3.620)
$x_3(1 - x_5)x_7 + x_4(1 - x_6)x_8$: <i>Party Control, Historical Weakness, and Historical Control of Governorship</i>		-2.237 (2.034)	-8.391 (7.172)	-4.474 (3.713)	-5.929 (3.721)
x_9 : <i>% Change in Real Per Capita Expenditures in Previous Period</i>			-0.001 (0.001)		
<i>Log Likelihood</i>	-146.04	-104.40	-67.69	-61.99	-61.98
<i>n</i>	870	804	513	965	965

Note: standard errors in parentheses

Models 1 through 3 use a probit specification with standard errors are calculated by method outlined in Appendix 3

Model 4 uses a Cox proportional hazard model

Model 5 uses an exponential hazard model

The results shown in Table 3 strongly confirm H3, the conservative insulation hypothesis. In none of the models does legislative conservatism alone explain the adoption of the line-item veto. The coefficients of the conservative party dummy are insignificant in all of the models ($p = 0.89$ in Model 2).²⁶ In other words, when controlling for other factors, the *main effect* of conservative control is not significant by itself.²⁷ This would lead one to strongly reject H1. Similarly, as the results in Model 3 indicates, the line-item veto is not a measure adopted by states that are facing increasing spending. Although not significant, the sign of the spending variable is *negative* ($p = 0.90$). This allows us to reject H2 as well.

The partisanship measures, however, paint a revealing picture about the dynamic insulation hypotheses H3 and H4. First, in both Models 2 and 3, the interaction of party control, historical legislative weakness and historical control of the governorship is positive is not statistically significant ($p = 0.99$ in Model 2) and negative. This means that when we control for the main effects of the interaction terms, taking account of the governor's position, and assuming both parties act the same, is misguided. The conservative insulation hypothesis H3, however, is confirmed by Models 2 and 3. In both models, the interaction of conservative control and historical conservative weakness is positive. Although the hypothesis tests yield marginally significant *p-values*, they are sufficiently high to be supportive ($p = 0.05$ in Model 2 and 0.07 in Model 3). Further, even when we use an alternative specification, as in Models 4 and 5, the same basic pattern emerges. This provides confirmation of the hypothesis that the line-item veto is used as an insulation mechanism by conservative state legislatures when they perceive their electoral prospects as relatively weak. This statement is true *irrespective of the party of the governor*. Table 4 shows how the predicted probabilities change when historical partisan legislative strength changes given a Democratic and Republican supermajority respectively. These estimates provide further confirmation of H3: the predicted probability of a conservative legislature adopting a line-item veto doubles when moving from one with average to one with weak historical performance in holding the legislature. Similarly, when conservatives have been historically strong, the reaction is the opposite: they are no longer as willing to weaken future legislative majorities.²⁸

Table 4. Predicted Probability of Adopting Line-Item Veto

<i>Historical Position in Legislature</i>	<i>Current Supermajority Control</i>	
	<i>Conservatives</i>	<i>Liberals</i>
<i>Weak</i>	0.06	0.01
<i>Average</i>	0.03	0.01
<i>Strong</i>	0.02	0.02

Note: Predicted probabilities based on Model 2

²⁶ Reported *p-values* are for one-tailed tests.

²⁷ That is not to say that on average, conservatives are less likely. As seen later, on average, the predicted probabilities under conservative control is higher, but this is accounted for by including the interactions.

²⁸ As mentioned previously, a number of alternative empirical specifications were tested in order to address the data issues. First, because there is a possibility that states that adopted the line-item veto with full constitutional revisions were doing so as part of larger bargains or logrolls. This would mean that the conditions I specify might be more difficult to test in these cases. Therefore, I tested the model on the subset of the states in Table 3.1 that *did not* pass their amendment under a full revision. The results, in particular H3's force, remain the same in this smaller model. Second, I also tested the model using *prospective* measures of partisan control. This was to make an alternative maintained hypothesis about the source of expectations for electoral outcomes. In the reported results, I use retrospective measures, assuming officials base their calculations on historical results. If we assume that people have rational expectations or perfect foresight, then future outcomes would be better measures of expectations. When I alter the measures in this way, again the results substantively hold.

Average category based on average value for relevant variable; x_3 and x_4 held at 1 or 0; all other variables held at their means
Weak and strong categories are based on \pm one standard deviation from mean

6. Discussion

Elsewhere, I among others have argued that a critical aspect of policy implementation is the insulation of policies from future interference.²⁹ More importantly, the incentive to bear the costs of insulation are not identical for all actors. In particular, only those officials who feel that they have *weak electoral prospects* are willing to bear the potentially burdensome costs of insulating their programs. If an individual, group, or party, feels that it will be able to retain public authority for a long period, it would be much less willing to incur penalties to insulate programs from future interference, since in all likelihood, they will be the only ones with an opportunity to do so. Alternatively, if a group temporarily gains hold of public authority, it is much more likely to cede control now in order to obtain ongoing benefits in the future, when they are likely to be out of power.

In the context of the line-item veto, this essay provides evidence that confirms this theory. In particular, as others have shown, the line-item can be considered a tool of fiscal conservatism. This means that the line-item veto should be interpreted as an insulation mechanism—when conservatives are usually in the minority they can protect their *future* interests by passing a line-item veto measure, given an opportunity to do so. Further, this insulation mechanism has costs: it means that the legislature must cede institutional power to the governor, and it means that the current majority must cede power to the current minority. Finally, using a multivariate probit model that controls for not only partisanship but also financial viability, I establish empirically that the line-item veto is *much more likely* to be passed under the conditions that are derived, than any other plausible explanation. The results, therefore, provide strong contributions to two literatures. In the first place, as noted, it provides evidence for a rethinking of the traditional theories on how political uncertainty affects policy implementation and governmental inefficiency. Second, it gives us a deeper understanding of the logic of state fiscal policy, and more generally, the legislative behavior of the states. The budgetary institutions of states are not necessarily adopted for economic efficiency—instead they are endogenous to the political institutions and environment.

²⁹ Terry M. Moe, "The Politics of Bureaucratic Structure," in John E. Chubb and Paul E. Peterson, eds., *Can the Government Govern?* (Washington, DC: Brookings, 1989); Terry M. Moe, "The Politics of Structural Choice: Towards a Theory of Public Bureaucracy," in Oliver E. Williamson, ed., *Organization Theory: From Chester Barnard to the Present and Beyond* (University of California: Berkeley, CA, 1990); Murray Horn, "The Political Economy of Public Administration: Organization, Control and Performance of the Public Sector," Harvard University Thesis, 1989.

Appendix 1. State Budget and Amendment Processes

To appropriately match the variables requires that I take account of three state governmental processes: elections, constitutional amendment procedures and budgetary processes. In general, I want to match all of the variables by *legislatures that took the action*.

In most states, elections of state officials are held towards the end of even numbered years. This means that the legislative sessions generally take place at the beginning of odd numbered years. Some states, however, have legislative sessions in even years only.³⁰ In either case, I have coded the partisan composition data to reflect the timing of elections. Further, since I only consider even-year compositions, if a veto amendment was passed in an odd year, I lag the adoption date by one year to appropriately match the legislative session with the provision's passage.

For most states in the period I consider, proposals for constitutional amendments could only be made by a legislative majority or supermajority.³¹ More recently, thirteen states allow initiatives to be proffered if enough of the general electorate sign petitions to place the proposal on the ballot. For the purposes of this analysis, I assume that initiatives are placed on the ballot by legislatures since most of the line-item veto provisions were passed in the post-Civil War nineteenth century or early twentieth century. Following the proposal stage, in every state, the provisions are placed on the ballot for majority approval by eligible voters. In every state, once a proposal has been approved, it is placed on the ballot in the next election. To reflect this process, therefore, I code the passage years based on the year it was approved.

Finally, in almost every case, the state begins its budgeting process in a legislature the year before a budget takes effect. The general process is that budget guidelines are given to agencies anywhere from fifteen to twelve months before a budget is take effect. The budget requests are then processed and undergo a series of steps—hearings, governor's submissions, legislative debate, legislative passage and gubernatorial approval—so that the budget is adopted one to three months in advance of its effective date. All but four states begin their fiscal year in July.³² The data I have obtained concerning state expenditures is based on *fiscal years*. In measuring fiscal crisis, I want to match spending with the legislatures that *passed them*. Thus, I move forward the expenditure figures by one year to match a legislature with its budget. For example, the 1980 budget runs from July 1979 to 1980. Thus, it is the 1979 legislature that passes the 1980 budget.

³⁰ For most of the nineteenth and twentieth centuries, there were four states that held sessions only in even years, all members of the solid South: Kentucky, Louisiana, Mississippi and Virginia. More recently, New Jersey now holds elections for legislative members in odd years. (see *Book of the States, Volume V*, p. 138 and *Volume 29*, p. 269)

³¹ See Frederic Jesup Stimson, *The Law of the Federal and State Constitutions of the United States* (Boston: The Boston Book Company, 1908).

³² New York's fiscal year currently begins in April. Texas begins its fiscal year in September. Alabama and Michigan begin their fiscal years in October. (see *Book of the States, Volume V*, p. 138 and *Volume 29*, p. 351)

Appendix 2. Partisan Fiscal Patterns

To operationalize the hypothesis that conservatives are more likely to pass a line-item veto, it is necessary to designate which party is more fiscally conservative. In the post-Progressive era, that is a relatively straightforward task: the Republican party tends to have lower-spenders than the Democratic party does. This common intuition is confirmed by the scholarly literature. Alt and Lowry, for example, examine state spending patterns in the period from 1967 to 1988. They find that Republicans, on average, want to spend 6.7 percentage points of personal income *less* than Democrats at the state level.³³

Although in the modern era, the spending preferences of the two parties is fairly well understood, in the period after the emergence of the Republican Party until the Progressive Era, there is little documentation about spending patterns at the state level. At the national level, the parties' positions on spending were clear: the Democrats were lower spenders than the Republicans. In the respective party platforms in 1868, for example, while the Democrats emphasized debt reduction, the Republican document advocated extending the national debt repayment period. According to the Democratic national platform, "The Democratic party in National Convention assembled,...does, with the return of peace, demand...[p]ayment of the public debt of the United States as rapidly as practicable. All moneys drawn from the people by taxation, except so much as is requisite for the necessities of the government, economically administered, being honestly applied to such payment, and where the obligations of the government do not expressly state upon their face, or the law under which they were issued does not provide, that they shall be paid in coin, they ought, in right and in justice, to be paid in the lawful money of the United States." In contrast, the Republican national platform read, "The National Union Republican Party...make the following declaration of principles...[that t]he National Debt, contracted as it has been for the preservation of the union for all time to come, should be extended over a fair period of redemption, and it is the duty of Congress to reduce the rate of interest thereon whenever it can be done honestly."³⁴ At the national level, these policies persisted, tied, in part, to the fact that the southern-slanted Democrats were against the tariff, which was the primary source of revenue prior to the implementation of the national income tax. After the institution of the national income tax by the Democrats in 1913, the parties positions' gradually shifted at the national level, until ultimately, with the implementation of the New Deal in the post-Depression era, the Democrats and Republicans took on their more modern guises.³⁵

At the state level, however, it is not possible to conclude simply that in the "extended" nineteenth century, state-level Democrats were low-spenders. Theoretically, one could posit plausible arguments in either direction. For example, the fact that the Democrats wanted more decision-making and implementation to be carried out at the local level would lead one to consider the possibility that they favored reduced national budgets in favor of greater state spending. Alternatively, if in this era of party politics, the parties had to maintain a consistent brand, then the national policies would have to influence behavior in a parallel way at the state level. Further, since the states in the south were the primarily Democratic ones, they had less urbanization to support, and therefore might tend to favor lower spending. Given this indeterminacy concerning the parties' spending patterns in state governments, it is important to test the character of partisan financial policies before proceeding to a test of the hypothesis that conservatives are more likely to adopt the line-item veto.

Measures and Econometric Methods. Fortunately, with the vast data set mentioned above, it is possible for us to test the pattern of state spending. Here I observe that per capita spending is a function of the partisan composition of the state legislature, the governors' party, and a number of other factors that are specific to each state's individual position or each year. These assumptions suggest the use of a cross-sectional times series model. In particular, I use a *two-way error component fixed effects model*. The measures used for the dependent variable is the natural logarithm of per capita spending y_{2it} . The independent variables are the sum of the Republican plurality in the lower and upper houses for each house

³³ James E. Alt and Robert C. Lowry, "Divided Government, Fiscal Institutions, and Budget Deficits: Evidence from the States," *American Political Science Review* 88 (1994), p. 820.

³⁴ Donald Bruce Johnson and Kirk H. Porter, eds., *National Party Platforms, 1840-1972* (Champagne, IL, 1973), pp. 37, 39.

³⁵ The income tax was first adopted in 1861, but was quickly repudiated after the crisis of public finance born by the Civil war had passed. Later, in 1894, Congress passed another income tax, but the Supreme Court struck it down the following year. (R. Douglas Arnold, *The Logic of Congressional Action* (New Haven, 1990), p. 195)

x_{1it} and a dummy variable for a Republican governor, x_{2it} . Note that states are indexed by $i = (1, \dots, N)$ and years are indexed by $t = (1, \dots, T)$. Thus, the model is:

$$y_{2it} = \alpha + \beta_1 x_{1it} + \beta_{21} x_{2it} + u_{it}$$

To capture the fact that there are state and year-specific effects, I must re-specify the error term, so that

$$u_{it} = \mu_i + \lambda_t + v_{it}$$

where the μ_i 's represent state-specific effects, the λ_t 's represent year-specific effects and the v_{it} 's are normally distributed disturbances with mean zero and variance σ^2 . The full model therefore is:

$$y_{2it} = \alpha + \beta_1 x_{1it} + \beta_{21} x_{2it} + \mu_i + \lambda_t + v_{it} \quad \text{with} \quad v_{it} \sim N(0, \sigma^2) \quad (1)$$

I can rewrite this model in matrix notation. First, I stack the observations by unit, so that $\mathbf{y}_i = (\mathbf{y}_{i1}, \mathbf{y}_{i2}, \dots, \mathbf{y}_{iT})'$, and $\mathbf{y} = (\mathbf{y}_1, \mathbf{y}_2, \dots, \mathbf{y}_N)'$. Then let \mathbf{i}_1 be a column vector of N one's and \mathbf{i}_2 a $T \times T$ identity matrix. Then define the following matrices \mathbf{D}_1 and \mathbf{D}_2 :

$$\mathbf{D}_1 = \begin{bmatrix} \mathbf{i}_1 & \mathbf{0} & \dots & \mathbf{0} \\ \mathbf{0} & \mathbf{i}_1 & \dots & \mathbf{0} \\ & & \vdots & \\ \mathbf{0} & \mathbf{0} & \dots & \mathbf{i}_1 \end{bmatrix} \quad \mathbf{D}_2 = \begin{bmatrix} \mathbf{i}_2 \\ \mathbf{i}_2 \\ \vdots \\ \mathbf{i}_2 \end{bmatrix}$$

where \mathbf{D}_1 is $NT \times N$ and \mathbf{D}_2 is $NT \times T$. Further, define the vector $\boldsymbol{\mu} = (\mu_1, \mu_2, \dots, \mu_N)'$, $\boldsymbol{\lambda} = (\lambda_1, \lambda_2, \dots, \lambda_T)'$, and $\boldsymbol{\beta} = (\beta_1, \beta_2)'$. Finally, define the vectors \mathbf{x} and \mathbf{v} in the same way I defined \mathbf{y} . Putting all these definitions together, I can rewrite (1) as:

$$\mathbf{y} = \mathbf{D}_1 \boldsymbol{\mu} + \mathbf{D}_2 \boldsymbol{\lambda} + \mathbf{X} \boldsymbol{\beta} + \mathbf{v} \quad (2)$$

which is simply the *LSDV* model with dummy variables for each state and year. Since the disturbances are well-behaved, this model can be estimated by the typical partitioned inverse method of ordinary least squares (OLS). Notably, this estimation technique is identical to the Within estimator suggested by Wallace and Hussein.³⁶

³⁶ Badi Hani Baltagi, *Econometric Analysis of Panel Data* (Chichester, UK, 1995), Chs. 2 and 3; William H. Greene, *Econometric Analysis* (New York, 1993), Ch. 16. Note that one problem which potentially occurs here is the classic incidental parameters problem. In particular, if I consider the asymptotic properties of the estimators in the direction of either T or N , then the dummy variables which index that dimension will not tend toward an infinite number of observations. In other words, if I consider the properties as say $T \rightarrow \infty$ then the dummy variables for each year will also be increasing (I keep adding additional variables λ_t 's).

Results. Using this method, I can then estimate the model on the post-Civil War observations prior to the posited cutpoint year of 1913. Before getting to the results, it is important to note a few points. First, I exclude the observations from Nebraska, Minnesota, since these states are omitted in the later analysis.³⁷ Second, implicitly, our definition of x_2 imposes a restriction on the model by adding the percentage of Republican seats in the upper and lower houses of each state legislature together. I impose this restriction since the degree of collinearity between the independent variables is extremely high (the correlation between the percentage Republican seats in the lower and upper houses in this period is 0.89). To test the validity of this implicit restriction, I performed a *pre-test* on the model, calculating an F -statistic comparing the restricted and unrestricted models. The p -value for this test was 0.91, meaning that imposing the restriction is reasonable. Third, it is important to understand why I chose a fixed effects model here, as opposed to a random effects one. I had three reasons to do so. First, the fixed effects model is appropriate since I am focusing on a fixed set of states in a fixed time period. Although this means that our inference is restricted to the behavior of these states in this time period, I do not intend to make any out-of-sample predictions.³⁸ Second, since I do not intend to fully model the determinants of spending levels, it is likely that the state and time error components would be *correlated* with the error terms, making use of a random effects model inappropriate. Finally, although the fixed effects model requires the use of a large number (eighty four for the sample I am considering) dummy variables, meaning that I incur a large penalty in terms of degrees of freedom, because both N and T are large in our case (twenty seven and forty six, respectively), I still obtain a large number of degrees of freedom so that I can obtain reasonably small standard errors to our estimates.

Given these caveats, the results of our analysis are reported in Table A1. The first column gives the results for the period of interest, from 1866 to 1912. Note that I do not report the coefficients for the dummy variables since they are not of interest to the present analysis.³⁹ Recall that I am testing for a difference between Republican and Democratic legislative strength on the level of spending by state governments. Thus, our hypotheses are

$$H_0: \beta_1 = 0$$

$$H_A: \beta_1 \neq 0$$

As Table A1 indicates, there are significant differences between Republican and Democratic state legislatures in the Pre-Progressive Era. The coefficient of x_{2it} is highly significant at any conventional level. Further, because it is negative, I can make a more positive statement that is of use in the later analysis: *Republicans were higher spenders at the state level in the period before 1912.* For purposes of validation, I ran a similar regression on the period from 1913 to 1970. Here, I obtain the result that conforms with intuition, that Republicans tend to be lower spenders in the post-Progressive era. This gives credence to the structure of the regression specified above. There is one strange result, however, in the second regression. Namely, Republican governors tend to be *higher* spenders. While I do not have a completely satisfying explanation for this result, our hypothesis is that it has to do with the fact that this model is implicitly one-dimensional. As noted above, while I constrict the analysis in this way to test a competing, and I think less plausible, hypothesis later on, it is probably more appropriate not to think of spending as a uni-dimensional choice problem. I suspect that the positive and significant coefficient on the

While the incidental parameters problem is one which must be dealt with for general fixed effects models, in this specification, the problem can be overcome. In particular, since the model I am estimating is *linear*, it is possible to estimate the model consistently. To see this, assume that I am performing the asymptotics on the unit dimension (although the same holds if I perform the asymptotics on the time dimension). Subtracting each observation from the previous observation, I can rewrite (1)

$$y_{2it} - y_{2i(t-1)} = \beta_1(x_{1it} - x_{1i(t-1)}) + \beta_2(x_{2it} - x_{2i(t-1)}) + \lambda_t - \lambda_{(t-1)} + v_{it} - v_{i(t-1)}$$

which I can consistently estimate as $N \rightarrow \infty$. For a more detailed discussion, see Chamberlain, pp. 226-227.

³⁷ These states are Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Texas and Virginia.

³⁸ Baltagi, p. 10.

³⁹ The complete results are available upon request.

Republican dummy in this period would not be robust if the spending variable was decomposed into a finer-grained analysis of spending *allocations*. Another potential explanation is that the analysis ignores another aspect of variation that is potentially significant. In Alt and Lowry's analysis, they find that under divided government, a Republican governor dummy gets a positive albeit insignificant coefficient in their expenditure equation. Perhaps a control which indicated the composition of the branches would filter out such an effect.⁴⁰

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Alt and Lowry, p. 819.

Table A1. Partisan Spending: OLS Estimates

<i>Dependent Variable:</i>		
<i>y_{2it} : Log(Per Capita Spending)</i>	<i>1866-1912</i>	<i>1913-1970</i>
<i>x_{2it} : Republican Party Strength</i>	0.168** (3.46)	-0.073** (-3.01)
<i>x_{3it} : Republican Governor</i>	0.012 (0.37)	0.021* (1.63)
<i>R-squared</i>	0.73	0.97
<i>n</i>	943	1862

Note: t-statistics in parentheses

* $p < 0.1$ (two-tailed)

** $p < 0.01$ (two-tailed)

Appendix 3. Heteroscedasticity and Autocorrelation Consistent Standard Errors

As noted, there is significant potential for autocorrelation in the error structure of the data. This is a possibility since there are possibly some temporally-dependent omitted variables in the model, and more generally, because of the panel structure of the data.

The effect of autocorrelation in linear and nonlinear models is well-understood. While the point estimates of the coefficients β are consistent, the standard errors are not. Therefore, following the suggestions of White and Newey and West, I recalculate the standard errors in the following way.⁴¹ Let $f(y_{it}|\theta)$ be the joint density of observation it given the parameters θ , and L denote the likelihood

function, so that $\log L(\theta) = \sum_{i,t} \log f(y_{it}|\theta)$. Let $s_{it} = \frac{\partial \log L_{it}(\theta)}{\partial \theta}$ and $\mathbf{H} = \frac{\partial s}{\partial \theta}$. Finally, let $\mathbf{\Omega}$ be the actual covariance matrix of the error terms. Then a consistent estimator of the variance-covariance \mathbf{V} matrix of the maximum likelihood estimator $\hat{\theta}$ is given by:

$$\hat{\mathbf{V}} = \mathbf{H}^{-1} \mathbf{\Omega} \mathbf{H}^{-1}$$

As White argues, the requirement is for a consistent estimator of $\mathbf{\Omega}$. Following Newey and West's suggestion, a consistent estimator is:

$$\hat{\mathbf{\Omega}} = \mathbf{S}_0 + \sum_{j=1}^L \mathbf{S}_j$$

where

$$\mathbf{S}_0 = \sum_{i,t} s_{it} s_{it}' \quad (\text{A4-1})$$

$$\mathbf{S}_j = \sum_{i=1}^N \sum_{t=1}^{T_i} w_j (s_{it} s_{i(t-j)}' + s_{i(t-j)} s_{it}') \quad (\text{A4-2})$$

where

$$w_j = 1 - \frac{j}{L+1}$$

Note that these estimators have been adapted for a panel structure in which I assume that there is no covariance *across* states. In other words, I assume $C(s_{it}, s_{jt}) = 0$ for $j \neq i$.

Using these results, I can derive the specific estimator for the variance-covariance matrix of the probit model I utilize in Section 3. The log-likelihood function for the probit model is

$$\ln L = \sum_{i,t} \{y_{it} \ln \Phi_{it} + (1 - y_{it}) \ln(1 - \Phi_{it})\}$$

⁴¹ Greene, pp. 360-364, 391, 422-423; White, pp. 817-824; Newey and West, pp. 703-705.

where $\Phi_{it} = \Phi(\beta' \mathbf{x}_{it})$ and Φ indicates the cumulative distribution function for a standard normal random variable. Taking the first derivative with respect to β , I have

$$s_{it} = (y_{lit} - \Phi_{it}) \frac{\phi_{it}}{\Phi_{it}(1 - \Phi_{it})} \mathbf{x}_{it} = \lambda_{it} \mathbf{x}_{it} \quad (\text{A4-3})$$

where ϕ_{it} is a standard normal probability density function evaluated at $\beta' \mathbf{x}_{it}$, and

$$\lambda_{it} = (y_{lit} - \Phi_{it}) \frac{\phi_{it}}{\Phi_{it}(1 - \Phi_{it})}.$$

Substituting (A4-3) into (A4-1) and (A4-2) I get

$$\hat{\Omega} = \sum_{i,t} \lambda_{it}^2 \mathbf{x}_{it} \mathbf{x}_{it}' + \sum_{j=1}^L \sum_{i=1}^N \sum_{t=j}^{T_i} \{w_j \lambda_{it} \lambda_{i(t-j)} (\mathbf{x}_{it} \mathbf{x}_{i(t-j)}' + \mathbf{x}_{i(t-j)} \mathbf{x}_{it}')\}$$

Further, since \mathbf{H}^{-1} is simply the estimated covariance matrix from the misspecified model, a consistent estimator for variance-covariance matrix of $\hat{\beta}$ is

$$V(\hat{\beta}) = \mathbf{H}^{-1} \hat{\Omega} \mathbf{H}^{-1}$$

Finally, as Greene notes, I must choose L based on an assumption about the data generating process. Here, I assume that the error structure follows a first order autoregressive process (AR(1)) which occurs over ten years. Since the temporal unit of analysis is a legislative election cycle, or two years, I use $L = 5$.

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