Fiscal Effects of the Voter Initiative in the First Half of the Twentieth Century

John G. Matsusaka
Marshall School of Business
University of Southern California

This paper compares the fiscal policy of initiative and non-initiative states in the first half of the 20th century. States with initiatives are found to have had higher combined state and local expenditure, but lower state and higher local expenditure, after controlling for income and other demographics. This, together with existing evidence from later in the century, suggests that the voter initiative does not have a consistent effect on the overall size of state and local government. However, it does systematically lead to more decentralized expenditure.

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I. Introduction

The core idea of the economic approach to government is that policy is the equilibrium outcome of competition between pressure groups.[t] While it is fairly clear that this competition causes policy to respond to the interests of voters, the response may be sluggish and incomplete.[2] A relatively new empirical literature has documented that the way preferences are translated into policy depends on decision making institutions. These institutions set the rules for competition between political groups. They include among other things how legislatures are organized, whether executives have veto power, whether legislatures are constrained in their abilities to tax, spend, and borrow, and whether representative terms are limited? While the literature demonstrates that the null hypothesis "institutions don't matter" can be rejected, we are still in the early stages of quantifying effects and few general principles have emerged. Because most studies have focused on the postwar United States, it is difficult to know whether the empirical relations being unearthed are specific to the time and place of the data.

The voter initiative is a case in point. In an earlier paper (Matsusaka, 1995), I studied the fiscal behavior of state and local government over the 1960 to 1990 period. I found that initiative states by which I mean states where citizens are allowed to propose and pass laws directly without recourse to their elected representatives spent less, decentralized spending from state to local governments, and utilized less redistributional financing than non-initiative states. Based on these data alone, however, we cannot tell whether initiatives systematically cut and decentralize government spending or whether this was a particular feature of the 30-year period I studied. The question is relevant both for policy makers who are searching for a way to reduce the size of government and decentralize decision making, and for scholars who are interested in understanding why representative governments sometimes fail to satisfy constituent desires.

In this paper, I try to shed some light on these issues by studying the fiscal effects of the voter initiative in the *first half* of the 20th century. Because the state initiative was first adopted in 1898, the evidence here and in my earlier paper together provide an overview of the entire American experience with this form of direct democracy, at least with regard to fiscal policy. A broad purpose of the study is to show by example

how we can gain insight into the workings of decision-making institutions by looking at other historical periods

Comparison of the evidence from the two time periods also sheds light on a puzzle posed by Peltzman (1992): Why have elected representatives in the postwar period increased government spending faster than the voters wanted?[4] One explanation is that overspending was an historical coincidence representative preferences happened to diverge from voter preferences, or representatives were slow to learn that voter preferences had soured on spending. The other explanation is that it is the *nature* of government to spend too much because of problems with fiscal commons, or bargaining power of bureaucracies. The two explanations can be distinguished by their implication about spending behavior in the first half of the century. The "overspending by-nature" view predicts overspending in the first half of the century as well, while the "historical coincidence" view does not. One way to gauge whether representatives were overspending in the first half of the century is to compare the spending levels of initiative and non-initiative states. If initiative states spent less than non-initiative states, then the overspending-by-nature view becomes more plausible; otherwise, the historical coincidence view gains appeal.

The final purpose of the paper is to provide some grist to policymakers who see direct democracy as a cure or cause of various ailments in the body politic. Direct democracy is enjoying a resurgence of popularity. In the 1990s, the number of state initiatives will reach a record high. Initiatives are increasingly influencing the political agenda at the state level and sometimes the national level, pushing issues such as immigration, affirmative action, assisted suicide, and medical marijuana to the front burner. In 1993, Mississippi joined the ranks of states that allow the initiative, bringing the total to 24, and New Jersey, Rhode Island, and Texas have recently discussed adoption.[5] In the midst of all this direct decision making, the debate over its merits is strangely anachronistic, sometimes involving little beyond reference to *The Federalist Papers* and a discussion of California's tax-cutting Proposition 13. To some extent, the problem is due to the dearth of statistical evidence about the effects of the initiative. It seems like an opportune time to begin assembling some of this information

The main findings can be summarized as follows

> 1. Combined expenditure (and revenue) of state and local governments was *higher* in initiative than non-initiative states in the first half of the century, in contrast to the pattern for later in the century.

> 2 State and local expenditure was more decentralized in initiative states than non- initiative states. More specifically, in initiative states, relatively less spending originated from the state government and relatively more from local governments. This pattern mirrors the second half of the century.

The conclusion from a full century of evidence is that the initiative does not appear to have a systematic effect on the total size of government. However, it does seem to lead regularly to devolution of spending authority from state to local governments.

The plan of the paper is the following. The next section briefly elaborates on the evidence that recent governments have been overspending, and discusses how the data in the paper can be used to distinguish between candidate explanations. Section III discusses the empirical strategy and data. Evidence on the fiscal effects is reported in Sections IV (size of government), V (centralization), and VI (categories of spending). In Section VII, I try to tie together the evidence here with the evidence from the second half of the century. Section VIII concludes.

II. Two Explanations for Government "Overspending" and a Test

As noted in the introduction, a goal of the paper is to provide some evidence on two hypotheses about government spending. The puzzle at its heart is the apparent tendency of elected officials, at least in the last few decades, to spend more public money than their constituents want them to spend. The paper by Peltzman (1992) provides the most compelling evidence of this. Peltzman studied election returns between *1950* and 1988 for presidential, gubernatorial, and U.S. senate races. After controlling for macroeconomic conditions, incumbency, and so on, he found that the faster government spending grew while an official was in office, the fewer votes he (or his party's nominee) received in the next election.[6] In fact, the point estimates implied that voters viewed the marginal dollar of spending as essentially worthless. My study of voter initiatives (Matsusaka, *1995),* which covered roughly the same time period (1960-1990), points in the same direction. If direct decision making allows voters to choose the spending levels they want (or at least get closer to what they want), then the finding that initiative states spent less than non-initiative states suggests that representatives were overspending. Of course, the conclusion I draw from these findings that representatives have been spending more than the electorate wants is not the only reasonable conclusion, but it strikes me as sufficiently plausible to begin investigating why this might be happening.[7]

There are many candidate explanations, but for the purposes of this paper they can be organized into two groups.[8] Group I explanations maintain that overspending in the recent past was an historical coincidence resulting from a transitory divergence between constituent and representative preferences, while group II explanations posit that government by its very nature tends to overspend. A list of some of the more prominent members of each group follows, with references to more detailed descriptions.

Group I (Historical accident) One explanation is that legislators in the recent past were fiscally more liberal than their constituents, and were willing to pay the price at the polls to indulge themselves. The voting market is fairly efficient so we expect that representatives will tend to have similar preferences as their constituents on average. However, the voting market is not perfect, so there may be short term deviations between legislator and voter preferences.[9] Gerrymandering can also cause legislative policies to deviate from the preferred policies of the electorate.10 A different explanation is that representatives try to satisfy their constituents, but sometimes make mistakes due to the difficulty of inferring voter preferences."

III. Empirical Strategy and Data

The empirical strategy essentially is to compare the fiscal policies of states that do and do not provide for the initiative.[4] Figure 1 shows the states that permitted the initiative in the first half of the century, and indicates the year of adoption. The initiative came to the United States during the Progressive Movement around the turn of the century. The first state to adopt was South Dakota in 1898, and Los Angeles was the first city in 1900. The following decade saw a burst of adoption activity, then the movement slowed, and the situation stabilized after Massachusetts's adoption in *1918.15* By the middle of the century, there were 19 initiative states and 29 non-initiative states. It is this cross- sectional and temporal variation that the regressions will exploit.

I want to measure whether availability of the initiative had a material effect on a state's fiscal policy. The workhorse is a regression of the form

$$G_{st} = \mathbf{A}\, X_{st} + \mathbf{B}\, I_{st} + e_{st}.$$

where G_{st} is the fiscal variable of interest (for example, expenditure) for state *s* in year t, X_{st} is a vector of control variables (for example, state income) that are intended to capture non-institutional determinants of fiscal policy, I_{st} is a vector of institutional variables representing availability of the initiative, and e_{st} is an error. The estimated parameters are A and B. If the initiative is unimportant, then $B=0$.[16]

The main obstacle to execution of a historical study like this is the paucity of state and local fiscal data prior to 1950. Aggregate numbers are not too hard to find, but data on the tax and spending policies of individual states and localities is hard to come by. Fortunately, a unique data set was recently assembled from the original documents of the U.S. Census by Richard Sylla, John Legler, and John Wallis.[17] There are some inconsistencies in the information collected by the Census Bureau, but the primary sources are good enough to allow construction of comparable numbers for each state and its local governments for 1902, 1913, 1932, and 1942. With information available for 48 states each year (Alaska and Hawaii are missing), the basic sample has 192 observations.

Summary statistics for fiscal variables appear in Table 1. All of the numbers are expressed in per capita terms, and stated in 1942 dollars (using the consumer price index). To give some context to these numbers, Figure 2 plots real state and local spending per capita between 1902 and 1942.[18] Although there are significant gaps in the data even at this aggregate level, a gradual upward movement in both series can be seen.

Table 2 provides summary statistics for the main control variables: income, population, population density, population growth rate, urban population, and federal aid. These variables can be thought of as arguments in the demand and supply functions for public spending. For example, in a median voter framework, they would parameterize the demand of the median voter. Financial numbers are expressed in per capita terms and converted to 1942 dollars using the CPI. The controls are fairly standard for a study like this, and as will be seen, do an excellent job accounting for variations in policy. Most of the numbers come from the Census. The Appendix gives more details on data sources and construction of the variables.

IV. Size of Government: State and Local Government Combined

A. Basic Results

I begin by studying the overall size of government. Did voters use the initiative to cut back the size of government in the first half of the Twentieth Century, as they did in the second half? Because initiatives can (and do) affect fiscal policies at both the state and local level, the natural starting point is to examine combined state and local spending.

Table 3 presents the basic results and illustrates the format I use throughout the paper. Each column is a regression. The dependent variable in column (1) is real combined state and local expenditure per capita. Beneath the coefficient estimates is the (White) standard error. In addition to the indicated variables, all regressions here and throughout include four year-specific dummies whose coefficients are not reported.

The variable of interest in regression (1) is the dummy variable equal to 1 if a state allowed the initiative.[19] The coefficient on the dummy is positive and statistically significant at better than the *5* percent level. The point estimate indicates that an initiative state spent *$4.50* per capita more that a non-initiative state after controlling for income. other demographics, and federal aid. From Table 1, we see that the mean per capita

spending level during the period ranged from *$25.25* in 1902 to $87.04 in 1932. The initiative then was associated with *5* percent to 18 percent higher spending on average. While the initiative obviously was not the main determinant of spending, its quantitative effects were not trivial. The overall explanatory power of the regression as measured by R2 is 0.911, a high number that is attributable primarily to the income variable and year dummies.

In column (2), the dependent variable is combined revenue instead of expenditure. We expect to see results similar to those in column (1) because revenue needs to equal expenditure over time. The coefficient on the initiative dummy is significantly positive in the revenue regression, and its magnitude is greater than in column (1). The point estimate of $6.01 per capita implies that the initiative increased state revenue by 7 percent to 26 percent of the year averages.

The initiative process differs by state in terms of how proposals are qualified for the ballot, what is needed to pass a measure, and what types of issues can be considered. An important difference for the purposes of this paper is the number of signatures required to qualify a measure for the ballot. In all states, a measure appears on the ballot only after its sponsor has collected a certain number of signatures from his fellow citizens. The signature requirement is typically expressed as a fraction of votes cast in the state's previous gubernatorial election, and in the sample period ranged from a low of 2 percent in North Dakota to a high of 10 percent in Arizona, Idaho, Maine, and Nevada.[20]

The regressions in column (3) and (4) introduce a variable equal to the signature requirement for an initiative state, and zero for non-initiative states. The variable is effectively an interaction term between the initiative dummy and the signature requirement. This specification makes allowance for the fact that the initiative is more available" in states with low signature requirements than states with high signature requirements. In light of columns (1) and (2), we expect to find a negative coefficient on the signature requirement variable. As can be seen, the coefficient is negative in both the expenditure and revenue equations, and statistically different from zero at better than the *5* level. The coefficient on the initiative dummy remains positive and statistically significant.

The significance of these coefficients individually is less important than their combined effects. To determine the full effect of the initiative in columns (3) and (4), it is necessary to test whether linear combinations of the dummy and signature requirement variables are different from zero. This is done in the usual way by adding the coefficient on the initiative dummy to the signature-weighted coefficient on the signature requirement variable, and performing an F-test for the hypothesis that the linear combination is zero.

Table 4 gives the results of the calculations. The estimates in column (3') are derived from regression (3) of Table 3, and column (4') is derived from regression (4) of Table 3 The main entry in each column gives the full effect of the initiative relative to a non- initiative state. The standard error is in parentheses below the coefficient. It can be seen that initiative states had higher spending levels than non-initiative states for all signature levels that appear in the sample. The effects are statistically significant for signature requirements up to 7 percent for expenditure, and up to 8 percent for revenue. For the modal state, with a signature requirement of *5* percent, the initiative was associated with $7.02 more expenditure per capita (8 percent to 28 percent relative to annual mean expenditure), and $9.06 more revenue per capita (11 percent to 39 percent relative to annual mean revenue).[21]

B. Robustness: Are the Results Spurious?

Correlation does not imply causality. How confident can we be that the initiative *caused* initiative states to have higher spending levels? The results would be spurious if the higher spending in initiative states was

actually caused by an omitted variable, and that variable happened to be correlated with the initiative variables. This section reports the results of several tests that are designed to evaluate the robustness of the results.

Table *5* presents the findings. As before, each column reports a regression. The dependent variable is either combined state and local expenditure or combined revenue, as indicated at the top of the column. Beneath the regression, I indicate the full effect of the initiative by signature requirement, as in Table 4. To conserve space, I do not report the coefficients on the standard controls. The first two regressions are the same as those in columns (3) and (4) of Table 3 except for the addition of two dummy variables, one for southern states and one for western states.[22] The southern dummy is a common control in regressions like this, and the western dummy is suggested by Figure 1, which shows that the initiative is primarily a western phenomenon. The theoretical basis for including these dummies is not entirely clear, but they provide a brute force way to check whether the initiative effects are actually regional effects in disguise. The southern dummy is significantly negative and the western dummy is positive but not statistically different from zero. The important point is that inclusion of the dummy variables has little effect on the initiative coefficients, which decline modestly but remain statistically significant at conventional levels. The full effects by signature requirement reported at the bottom of the columns indicate a statistically significant effect of the initiative for signature percentages up to 6 percent in the expenditure equation and 7 percent for the revenue equation.

Another possibility is that initiative states had an underlying demand (ideology) for government spending that is not captured by the control variables. One way to check for this is to take into account the voting behavior of a state's U.S. senators. If voters in a state had a high demand for spending then their senators are likely to have had relatively liberal voting records. The regressions in columns (3) and (4) attempt to control for an unobserved ideology by adding the mean NOMINATE score for each state's U.S. senators to the regressions in columns (1) and (2).23 The NOMINATE scores for each senator were calculated by Poole and Rosenthal (1991). The scores give the spatial location of each senator's roll call votes on a unit hypercube. The estimation procedure of Poole and Rosenthal allows for multidimensional issue spaces, but the data suggest that a single dimension is adequate for most issues. I take the first dimension score for each senator, and calculate the average value for each state. A score of +1 can be thought of as the most "conservative," and —1 is the most "liberal."

The NOMINATE variable has the sign predicted by theory (states with conservative senators spent less than states with liberal senators) but the coefficients are not significant at conventional levels. More to the point, inclusion of the NOMINATE variable has no material effect on the estimated initiative effects. The coefficients and full effects by signature requirement remain significant and their magnitudes change very little.[24]

Another concern is the possibility that the results are driven by an outlier, the state of Nevada. As Table 1 indicates, Nevada had the highest per capita revenue in 1932, the highest expenditure in 1942, and the highest per capita state expenditure in every year but 1932. Its per capita expenditure and revenue ranked in the top 3 in every year but 1902. The state's per capita numbers may be less accurate than other states because Nevada had a very small and rapidly changing population during sample period. Since Nevada is an initiative state, we want to be sure that the effects are not coming from this state alone. The regressions in columns *(5)* and (6) simply delete Nevada from the sample. As can be seen, this does not have a material effect on the results of interest.

The final possibility I try to evaluate is that initiative states were high spending states to begin with. This is related to the endogeneity issue: perhaps it was their high spending nature that led these states to adopt the initiative. To check this, I re-estimated the regressions in columns *(5)* and (6) with state fixed effects (that

is, dummy variables for each state). This essentially de-means the data state-by-state, so that the coefficients on the initiative variables are estimated using only observations from states that adopted during the sample period, that is, excluding South Dakota, Utah, and Oregon. Another obvious drawback to this approach is that it consumes 47 degrees of freedom, roughly a quarter of the total. Nevertheless, it is useful to know whether the results can survive this relatively steep hurdle.

The estimates are reported in columns (7) and (8). The magnitudes of the coefficients are quite a bit smaller than in previous regressions, indicating that initiative states had more government spending before they adopted the initiative. We still observe positive effects in both equations, but the coefficients fail to achieve significance. However, the full effects are still significantly different from zero in the revenue equation for signature requirements between 4 percent and 8 percent, even with 47 fewer degrees of freedom. For the modal state with a *5* percent signature requirement, the initiative increases revenue by nearly $5, which is 6 percent to 21 percent of the mean. Thus, we can reject the hypothesis that initiative states raised more revenue only because they were high-tax states before they adopted the initiative.

The basic result therefore survives these robustness checks. None of which establish conclusively that the observed correlations are causal. But at least there are grounds to reject the idea of spurious correlation based on regional effects, unobserved preferences for spending, and unobserved (time-stationary) state-specific effects such as a demand for spending that led the state to adopt the initiative.

V. Centralization: State Versus Local Expenditure

I next turn to the issue of centralization of expenditure. Government centralization, by most measures, has increased during the 20th century.[25] Recently, there has been renewed interest in understanding the benefits and costs of centralization, stirred in part by a belief that government may have become too centralized.[26] Although some of the tradeoffs are understood for example, local decision making can lead to policies closer to citizen preferences through the use of better information and Tiebout competition, while complicating cooperation and limiting redistribution how they stack up is unclear. One thing we know little about is the extent to which the rise in centralization has been in response to (or contrary to) voter demands. For the 1960-1990 period, I found that initiative states had significantly less centralization of expenditure than non-initiative states, which suggests that from the electorate's viewpoint too many expenditure decisions were made in the state capital and too few by local governments. In this section I investigate whether initiative states were more or less centralized in the first half of the century than non-initiative states. As with the overall size of government, this will suggest whether centralization generally tends to be excessive, or whether the situation in the later 20th century was an historical coincidence.

Table 6 presents the results of estimating the expenditure regressions separately for state and local governments. Each column reports a regression in which the dependent variable is either expenditure per capita by local governments or state expenditure per capita, as indicated at the top of the column.

The first two regressions include the initiative dummy but not the signature requirement. For local governments, the effect of the initiative was positive and significant at the *5* percent level. For state government, the initiative coefficient was negative but not statistically significant. Although initiative states had more total spending, the point estimates imply that they also had a lower ratio of state to local expenditure. The magnitude of the local government effect is large: *$5.16* per capita ranges from 9 percent to *25* percent of the annual average local government expenditure.

In columns (3) and (4), the signature requirement variable is introduced. The estimated coefficients go in the expected direction. In the local government equation, both initiative coefficients are now significant at better than the 1 percent level. In the state government equation, the initiative dummy coefficient is now significant and the signature requirement variable comes close. In columns *(5)* and (6), I introduce three controls that were used earlier to check for robustness, the two region dummies and the mean NOMINATE score. The results appear to be robust to their inclusion.[27]

Table 7 reports the full effect of the initiative by signature requirement for the regressions in columns (3) to (6). The estimates in column (3') are derived from the regression in column (3) of Table 6, and so on for columns (4'), *(5'),* and (6'). The story is essentially the same whether or not the robustness variables are included. The initiative had a significant positive effect on local spending for signature requirements up to 9 percent, and the effect can be rejected from zero at conventional significance levels for signature requirements up to 7 percent. The initiative effect was weaker for state spending: the initiative was associated with a reduction in spending for signature requirements up to 8 percent, and the effect is significant for signature requirements up to 4 percent. The point estimates are large. If we focus on the last two equations with the robustness controls, the modal initiative state (with a *5* percent signature requirement) spent *$6.75* more at the local level than a non-initiative state, and $1.81 less at the state level. As a percentage of mean annual spending levels, this implies between 11 percent and 33 percent higher local spending, and between 4 percent and 37 percent lower state spending.

The finding then is that the initiative increased local spending and reduced state spending (although the latter effect is much weaker). Put another way, the initiative had the effect of decentralizing expenditure. One interpretation of the main result of Section IV — higher overall spending by initiative states — is that voters generally had a higher demand for government spending than their representatives were delivering. However, if this interpretation is correct, we would probably expect to see higher local *and* state spending in initiative states, which we do not. Instead, what we seem to be observing is a demand for more local spending coupled with a view that state spending is excessive. A few more thoughts on interpretation appear in Section VII.

VI. Accounting for the Differences

A. Spending by Function

So far, we have found two fiscal effects of the initiative in the first half of the century higher combined state and local spending, and decentralization of expenditure from state to local governments. In order to understand better the nature of these effects, this section takes a closer look at the composition of government spending. I divide expenditure into seven functional categories, and estimate the effect of the initiative on spending in each category. The key questions are: what *type* of spending was higher in initiative states, and what *type* of spending was decentralized?

The components of each of the seven categories of spending are reported in Table 8. Summary statistics are presented in Table 9. Some unavoidable deficiencies in this classification scheme originate from the way the Census Bureau collected the information. In particular, capital outlays appeared as a separate category; they were not apportioned to the functional categories (schools to the education category, road construction to the highways category, etc.) Since roughly 80 percent of capital outlays went to highways, I assigned outlays to the highways category. There is also a large residual category that grows over time. It includes interest payments, expenditure of public enterprises, and contributions to trust funds, among other things. In addition, the time series are not strictly comparable over time. While these data problems are vexing, there

is no obvious reason why they should introduce anything other than white noise error. Still, as will be seen, the data are rather noisy, and this imprecision will make it difficult to draw strong conclusions.

I estimated a regression for each category for combined state and local expenditure, local expenditure, and state expenditure, 21 regressions in all. I included all the standard control variables as well as the region dummies and the mean NOMINATE score. For each regression, I then calculated the full effect of the initiative by signature requirement Table 10 reports those estimates.

The first question is what accounts for the increase in combined state and local expenditure? The estimates do not tell a clean story. If we focus on the modal *(5* percent signature requirement) state, the answer is welfare spending and "other" spending. The coefficient on welfare spending is significantly different from zero at better than the 1 percent level. (It is also significantly different from zero for signature requirements up to 9 percent.) If we focus on states with an 8 percent signature requirement (the second most popular requirement), the initiative also appears to have driven up public safety spending.

The results are also mixed for state and local spending individually, but they give a somewhat cleaner accounting for how decentralization was achieved. For most signature requirements, the initiative appears to have resulted in higher local spending on education, highways, welfare and "other" spending. There was also a smaller positive effect on public safety and general government expenditure. The education and public safety effects are statistically different from zero for at least 4 signature percentages. For state government, the initiative appears to have reduced highways and education spending, and to a lesser extent, spending on general government. The main message seems to be that voters wanted more local and less state involvement in education and highways decisions

B. Why Did the Initiative Matter: Initiatives in California, North Dakota, and Oregon

So far, the paper has measured differences between initiative and non-initiative states, and attributed those differences to availability of the initiative. In this section, I examine the actual initiatives in three states to try to understand *why* availability of the initiative matters. Theory suggests that the initiative can influence policy in three ways: (1) citizens can propose and approve policies directly, (2) the threat of an initiative can cause the legislature to approve policies different from those it would pass in the absence of an initiative, and (3) election returns from initiative contests can convey information to representatives about citizen preferences that they later incorporate into policy

Table 11 presents a list of all initiatives with non-trivial fiscal impact in the states of California, North Dakota, and Oregon in the first half of the century. I chose these states because they are the leading users of the initiative in the century. In addition to a brief description of the aims of the initiative, indicate whether each passed or failed.

The qualitative nature of the information in Table 11 does not lend itself to sharp conclusions. Clearly, it is not the case that the initiative was always used to increase spending. However, the list of measures that passed is fairly consistent with the statistical evidence. For example, 7 of the 9 successful measures in California increased spending or made it easier to borrow. Four of those measures increased education spending, two of them increased public aid (one to veterans, and the other to the aged and blind), and one made it easier to finance highway construction,

Of the 11 successful initiatives in North Dakota, two increased taxes, two made it easier to borrow, three increased spending (for old age pensions, financially distressed schools, and highways), and two dedicated state sales tax revenue for schools and welfare. Only one measure did not increase spending, an initiative in

1922 that eliminated minimum salaries for teachers. This measure could be viewed as a decentralizing policy if it gave local decision makers more of a say in setting compensation.

Fourteen initiatives passed in Oregon. Of these, two increased taxes, one made it easier to borrow, and five increased spending (three for education, one for old age pensions, and one for rural credits). On the other hand, two measures cut taxes by increasing exemptions, and two measures set borrowing limits (one on the state, and the other on counties).

The overall impression is that the fiscal effects detected in the statistical analysis more spending by initiative states, especially on education, welfare, and highways were in many cased explicitly forced by initiatives. This suggests that the initiative device affects policy directly, not just through the threat it provides. Because we can see the initiative directly at work increasing government spending in the historical record, Table 11 also lends some corroboration to the interpretation of the evidence offered above that the initiative causes the fiscal difference between states.

The list of initiatives that failed is much longer than the list that passed. It can be seen that several attempts to limit taxes and spending were defeated. Not counting the numerous single tax proposals, California voters rejected limits on the growth of school taxes in 1918, and rejected a measure that would have eliminated the income tax in 1936 and 1939. North Dakota rejected property tax limits in 1924, school tax limits in 1934, and repeal of the income tax in 1944. Oregon voters refused to reduce motor vehicle license fees in 1926, and rejected limits on taxes and debt in 1932, limits on property and school taxes in 1934 and 1936. A fair number of education, welfare, and highways measures also went down to defeat. The electorate's taste for such spending was apparently discriminate. In general, voters registered their opinions on a wide variety of spending, taxing, and borrowing measures, approving some and rejecting many. It seems plausible, although we can only speculate at this point, that in these states the initiative may have had an effect by sending a fairly clear signal to legislators about citizen preferences. That is, even the initiatives that failed may have influenced policy by signaling voter disapproval of their policies to legislators.

VII. Discussion

A central finding of the paper is that combined state and local expenditure and revenue were significantly higher in initiative states than non-initiative states prior to 1950. This seems to imply that the initiative is not inherently a device that reduces the size of government. Certainly, it can have this effect as we saw in the 1960-1990 period, but whether or not it does seems to depend on time-varying features of the political and economic environment. A more plausible view may be that the initiative moves policy closer to voter preferences in situations where representative and voter preferences diverge. According to this view, representatives in the first half of the century were slow to respond to increased voter demands for spending, particularly local spending on education and highways, while in the second half of the century, elected officials were slow in responding to voter demands for government downsizing. This story is somewhat plausible at an anecdotal level, but a systematic investigation is in order.

The other main result of the paper is that initiative states spent less at the state level and more at the local level in the first half of the century, that is, their spending decisions were more decentralized. This is the same pattern that appeared in my earlier study using data for the 1960-1990 period. Since the two studies cover essentially the entire history of the initiative in the United States, decentralization appears to be a general characteristic of the American experience with the initiative. Although the initiative does not seem to be inherently an anti-spending device, the evidence suggests that it is an anti- centralization device. Why this is so remains a question for future investigation. It seems plausible that agency problems would be more severe in distant, monopolistic state capitals than in cities and counties. But this would imply a larger

reduction in state expenditure relative to local expenditure, not an increase in local expenditure. One conjecture is that agency problems that sometimes lead to overspending also cause state governments to aggregate decision making authority. In other words, we may be observing not a failure of local governments to provide enough spending, but an excessive transfer of spending authority from the local governments to the state. Of course, this begs the question why vote-maximizing state politicians don't try to make something out of this.

What about the broad theoretical issue raised in Section II? The evidence points in an unanticipated direction. Apparently, voters do not consistently believe that the overall size of government is too large. They thought state and local spending was excessive during 1960-1990, but believed it was insufficient in the early part of the century. However, it seems that voters view *state* government as systematically too large, both in absolute terms and relative to local government. Thus, the evidence provides mixed support for those theories in which it is the nature of government to overspend. The view that all governments spend too much due to (say) common pooi problems or bureaucratic monopolies is not easy to reconcile with the evidence why do common pool problems (say) appear in state but not local government? On the other hand, the fact that state governments do seem to be too centralized throughout the century suggests that there is a systematic malady in government to be explained.

VIII. Conclusion

This paper documents fiscal effects of the state-level voter initiative in the U.S. in the first half of the 20th century. It extends my earlier paper that studies the last half of the century (Matsusaka, 1995). Because the state initiative was first adopted in 1898, the two papers together provide an overview of the entire American experience with this form of direct democracy, at least with regards to fiscal policy.

Two empirical patterns appear to be robust. First, initiative states spent more than non initiative states in the first half of the century. Second, initiative states decentralized expenditure (from state to local governments) more than non-initiative states state spending was lower and local spending was higher in initiative states. For both results, there is some reason to believe that the initiative caused the fiscal differences.

Several conclusions emerge. First, the initiative does not appear to be inherently an anti- spending device. Although the initiative led to cuts in the size of government after 1960, the fact that it led to increases in spending prior to *1950* suggest that its effect is conditional. I conjecture that the initiative's main function in this respect is to bring fiscal policy more in line with the electorate's preferences. When representatives are more conservative fiscally than the voters, the initiative leads to higher spending, and when representatives are more liberal, the initiative leads to reduced spending. The bottom line in this regard is that those who are looking for way to systematically make government smaller than what legislators approve should not depend on the initiative.

On the other hand, although the initiative does not systematically shrink the government sector, it does seem to systematically reduce the centralization of government expenditure. For the entire century, the evidence uniformly shows that initiative states have a higher ratio of local to state spending. Thus, proponents of decentralization may find the initiative to be an effective tool.

These results raise some interesting theoretical issues. Peltzman (1992) found that voters in the last half the century were more fiscally conservative than their representatives. The reason, he suggested was either that in the last half of century a historically coincidental divergence between voter and representative preferences occurred, or because governments inherently overspend, as predicted by various theories such as the fiscal commons or monopoly bureaucracy theories. The finding that initiative states spent more in the

first half of the century and less in the second half than non-initiative states tends to favor the historical coincidence view. If government systematically exceeded voter demands, we would probably see the initiative used to cut spending in the first half of the century as well

Although the comparison of the evidence from different time periods tends to undercut the view of government as systematically too large, it does suggest a different problem: government may be systematically *too centralized.* Some attention has been paid to this issue in the theoretical literature, but I am not aware of an obvious explanation for this behavior. The fact that voters reserve their hostility for state government presents a puzzle that the present data cannot resolve.

Footnotes

1This is true of the pressure group models of Stigler (1971), Peltzman (1976), and Becker (1983), as well as the median voter model of Downs (1957).

2 For evidence on state policies, see Erikson, Wright, and McIver (1993).

3 The literature is too voluminous to do more than scratch the surface. For recent evidence concerning fiscal policy, see Poterba (1994, 1995), and Del Rossi and Inman (forthcoming) on budgeting rules, Cram and Muris (1995) on legislative organization, Gilligan and Maisusaka (1995) on electoral systems, Rueben (1996) on tax and expenditure limitations, and Poterba (1996) and Bohn and Ininan (1996) oil balanced budget rules

4 I review the evidence that suggests overspending below. Throughout the paper, I use the term "overspending" to mean that spending levels exceed the electorate's preferences. The term is not intended in a normative sense or in relation to economic efficiency.

5 Direct democracy is rare outside the United States, but interest is growing. The initiative was adopted, albeit in an emasculated form, in the Canadian provinces of Saskatchewan in 1991 and British Columbia in 1995. Italian referendums in 1991 and 1993 were instrumental in breaking up and restructuring the old party system. For a more systematic survey, see Butler and Ranney (1994). A recent feature in *The* Economist (December 21, 1996) argued that the "next big change in human affairs will probably not be a matter of economics, or electronics, or military science," but the eclipse of representative government by direct democracy institutions. (December 21, 1996) argued that the "next big change in human affairs will probably not be a matter of economics, or electronics, or military science," but the eclipse of representative government by direct democracy institutions.

6 In a predecessor to Peltzman's study, Niskanen *(1975)* examined presidential elections from 1896 to 1972 and also found (using somewhat different methodology) that voters penalized the president for spending growth.

7 My interpretation depends on the assumption that policies in initiative states are closer to the voters' preferences than policies in non-initiative states. Prevailing theory supports this conclusion, and it enjoys modest empirical support, but one can imagine situations where it might not be true. The pioneering theoretical paper is Romer and Rosenthal (1979), but perhaps the earliest model is in Denzau, Mackay, and Weaver (1981). Gerber (1996, 1997) argues that state parental abortion notification and death penalty policies are closer to voter preferences in initiative states using opinion poll data. A simple statement of the basic theoretical argument, and identification of situations where initiatives can lead to policies that make the voter worse off are in Matsusaka and McCarty (1998).

8 Peltzman (1980) and Higgs (1987) contain surveys and offer novel hypotheses.

9 Both information imperfections and collective action problems could lead to incomplete monitoring and discipline of political agents. For evidence that voting markets process information efficiently (which is not the same as perfectly) and discipline shirking representatives, see Peltzman (1990) and Loft and Bronars (1993).

10 For example, Gilligan and Matsusaka (forthcoming) shows how gerrymandering can systematically distort policy from the median voter outcome even in a median voter world.

11 Matsusaka (1992) and Matsusaka and McCarty (1998) explore this issue.

14 The initiative is distinct from other direct democracy devices in that it allows citizens to *propose* laws. Another device, often called a "referendum" allows voters to nullify a measure that the legislature has previously approved. A "referred/legislative" measure allows the electorate to vote on a measure proposed by the legislature.

15 Mississippi adopted the initiative in 1916, but it was declared unconstitutional by the state supreme court in 1922. Since 1918, five states have adopted the initiative: Alaska in 1959 when it entered the Union, Wyoming in 1968, Illinois in 1970, Florida in 1978, and Mississippi (again) in 1993. See Magleby (1994).

16 Matsusaka (1995) gives a theoretical underpinning for this approach.

17 These data are available through the ICPSR. I thank John Wallis for providing me with the fiscal data as well as demographic data, and for answering my repeated queries about the numbers.

18 The underlying numbers were cobbled together from a variety sources. In addition to the Sylla-Legler Wallis numbers, I used *Statistical Abstract of the United States, 1924-1926, 1928, 1929, 193 1-1933, 1940 1942, 1924-1926, 1928, 1929, 193 1-1933, 1940, 1942, 1924-1926, 1928, 1929, 193 1-1933, 1940, 1942,* and *Historical Statistics of the United* States. Documentation is sketchy, and different sources sometimes give different numbers, so the series are only roughly comparable across time.

19 In order to qualify as an initiative state, I required that the initiative was adopted one year before the year in question. For example, if a state adopted the initiative in 1913, it was not counted as an initiative state for that year. I did this because most states approve their budgets for fiscal year t midway through calendar year t-1.

20 Some details: North Dakota's signature requirement was a flat 10,000 (since 1978 it is 2 percent of resident). I convened it to 2 percent following Magleby (1984). Some states have different signature requirements for statutory measures and those that amend the constitution. In these cases, I used the lower of the two. I made no adjustment for states in which the signature requirement is a percentage of something other than votes in the previous gubernatorial election. The coding of each state is reported in the Appendix.

21 It is interesting that throughout this paper the initiative ceases to have a measurable effect when the signature requirement reaches approximately 10 percent. This is the same pattern I found using data from 1960-1990.

22 The southern dummy was set equal to 1 for the 11 states of the Confederacy. The western dummy was set equal to 1 for Arizona, California, Colorado, Idaho, Nevada, Montana, New Mexico, Oregon, Utah, Washington, and Wyoming, which follows the Census Bureau classification.

23 The number of observations falls by 3 because the territories of Arizona, New Mexico, and Oklahoma did not have U.S. senators in 1902, and therefore it is not possible to calculate a mean NOMINATE score for them.

24 also directly tested for underlying ideological preference differences between initiative and non- initiative states. For the four sample years, the mean NOMINATE score for senators of initiative states was *-0.045* and the mean for non-initiative states was 0.014. The difference is not statistically significant. I also compared information from Gallup polls taken over 1937-1939 using data that Robert Erikson kindly provided me (see Erikson, Wright, and Mclver (1993) for details.) The difference between the percentage of poll respondents who said they were conservative and the percentage who said they were liberal averaged 3.8 percent in initiative states and 0.4 percent in non-initiatives states. The difference is not statistically significant.

25 For example, Wallis (1995) documents an increasing centralization of state and local government spending throughout the century.

26 See Ferejobn and Weingast (1997).

27 also estimated the regressions with state-specific fixed effects. When the signature requirement variable is included, the initiative effects are positive for local spending and significantly different from zero for 7, 8, and 9 percent signature requirements. The effects are negative for state spending, but not statistically significant. When the signature requirement variable is excluded, the coefficient on the initiative dummy is *3.75* for local spending (significant at the 10 percent level) and —0.87 for state spending (not significant).

Appendix

Data Construction and Sources

I*nitiative Variables*; Signature requirements were assigned following Magleby (1984): 2% (North Dakota), *5%* (Colorado, Massachusetts, Montana, Missouri, South Dakota, Utah), 6% (Ohio, Oregon), 7% (Nebraska), 8% (Arkansas, California, Michigan, Oklahoma, Washington), 10% (Arizona, Idaho, Maine, Nevada). Mississippi was coded as a non-initiative state for the years of the sample.

Fiscal Data The data come from the Censuses of 1902, 1913, 1932, and 1942, as described in the text. In what follows, parentheses indicate terms that follow directly from the ICPSR documentation by Sylla, Legler, and Wallis (SLW) that accompanies the data. "ISO" corresponds to their classification codes. Federal aid was subtracted from revenue.

1902 ; Combined state and local, state-only, and local-only numbers are those classified as TGG, SSS, and LTT in SLW.

1913; State-only numbers correspond to SSS in SLW. Local numbers were calculated by summing "Counties" (CCC) and "Incorporated Places over 2,500" (L 11) and multiplying by the 1902 ratio of LTT/(LTT-L03). This corrects for the fact that the 1913 Census did not include local governments with populations less than 2,500. Combined state and local numbers were calculated by adding the state and local numbers.

1932 Combined state and local and state-only numbers correspond to TGG and 555, respectively in SLW. Local numbers were calculated as the difference between TGG and SSS.

1942 For combined numbers I used TGG. For state and local numbers, I used SSS and LTT. I subtracted "Provision for Debt Repayment" (ISO 4100) from expenditure to make the numbers comparable to the other years.

Price Level I converted nominal values to real values using the Consumer Price Index as reported in *Historical Statistics of the United States.*

Revenue from Federal Government The numbers came from SLW, and were constructed in the same way as the fiscal data. For 1902, 1913, and 1932, the numbers are "Subventions and Grants" (ISO 2300). For 1942, I used ISO 2350, revenue "From Federal Government."

Income Real income per capita for each state in years ending in "0" came from the Census and were provided to me by John Waffis from the Census. I interpolated geometrically to find per capita income in other years.

Population Demographics Population and urban population for each state in years ending in "0" were taken from the Census and provided to me by John Wallis. I interpolated geometrically to find population and urban population in other years.

Mean NOMINATE Score I took the average first dimension score for all the state's U.S. senators for the congress that met in the same calendar year as the fiscal year of the data.

Categories of Expenditure The 1913 numbers were rescaled as described above under Fiscal Data except for two cases. For Rhode Island, the local number for "General Government" is not rescaled because the scale factor would be infinite. For Nevada, the local education numbers are not rescaled because the scale factor would be 48. For 1932, I calculated combined state and local expenditure directly instead of using the reported numbers because the two are different. The spending categories are Education (ISO 31), Welfare (ISO 32), Highways (ISO 33 and ISO 42), Public Safety (ISO 34), Conservation and Health (ISO 35), and General Government (ISO 36). "Other" is total expenditure minus the sum of the listed individual categories. I included capital outlays (ISO 42) in the Highways category.

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