FISCAL EFFECTS OF THE VOTER INITIATIVE IN THE FIRST HALF OF THE TWENTIETH CENTURY*

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Abstract

This paper compares the fiscal policy of initiative and noninitiative states in the first half of the twentieth century. States with initiatives had higher combined state and local expenditure after controlling for income and other demographics but a lower ratio of state to local expenditure. 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.

I. Introduction

The core idea of the economic approach to government is that policy is the equilibrium outcome of competition between pressure groups.[1] 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 legislatures are constrained in their abilities to tax, spend, and borrow; and what system is used

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[1] This is true of the pressure group models of George J. Stigler, The Theory of Economic Regulation, 2 Bell J. Econ. Mgmt. Sci. 3 (1971); Sam Peltzman, Toward a More General Theory of Regulation, 19 J. Law & Econ. 211 (1976); and Gary S. Becker, A Theory of Competition among Pressure Groups for Political Influence, 98 Q. J. Econ. 371 (1983), as well as the median voter model of Anthony Downs, An Economic Theory of Democracy (1957).

[2] For evidence on state policies, see Robert S. Erikson, Gerald C. Wright, & John P. McIver, Statehouse Democracy: Public Opinion and Policy in the American States (1993).

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to elect representatives.[3] While the literature demonstrates that the null hypothesis ''institutions do not 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, I studied the fiscal behavior of state and local government from 1960 to 1990.[4] 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 noninitiative states. On the basis of 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 twentieth 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

[3] The literature is too voluminous to do more than scratch the surface. For recent evidence concerning fiscal policy, see James M. Poterba, State Responses to Fiscal Crises: The Effects of Budgetary Institutions and Politics, 102 J. Pol. Econ. 799 (1994); James M. Poterba, Capital Budgets, Borrowing Rules, and State Capital Spending, 56 J. Pub. Econ. 165 (1995); W. Mark Crain & Lisa K. Oakley, The Politics of Infrastructure, 38 J. Law. & Econ. 1 (1995); and Alison F. DelRossi & Robert P. Inman, Changing the Price of Pork: The Impact of Local Cost Sharing on Legislators' Demands for Distributive Public Goods, 71 J. Pub. Econ. 247 (1999), on budgeting rules; W. Mark Crain & Timothy J. Muris, Legislative Organization of Fiscal Policy, 38 J. Law & Econ. 311 (1995), on legislative organization; James M. Poterba, Budget Institutions and Fiscal Policy in the United States, 86 Am. Econ. Rev. 395 (1996); and Henning Bohn & Robert P. Inman, Balanced Budget Rules and Public Deficits: Evidence from the States, 45 Carnegie-Rochester Conf. Ser. Pub. Pol'y 13 (1996), on balanced budget rules; and Thomas W. Gilligan & John G. Matsusaka, Deviations from Constituent Interests, 33 Econ. Inquiry 383 (1995), Thomas W. Gilligan & John G. Matsusaka, Fiscal Policy, Legislature Size, and Political Parties: Evidence from State and Local Governments in the First Half of the 20th Century, (Working paper, Univ. Southern California, Marshall Sch. Bus. 1999); and John R. Lott, Jr., & Lawrence W. Kenny, Did Women's Suffrage Change the Size and Scope of Government? 107 J. Pol. Econ. 1163 (1999), on electoral systems.

[4] John G. Matsusaka, Fiscal Effects of the Voter Initiative: Evidence from the Last 30 Years, 103 J. Pol. Econ. 587 (1995).

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

Comparison of evidence from the two time periods also sheds light on a puzzle posed by Sam Peltzman: Why have elected representatives in the postwar period increased government spending faster than voters wanted?[5] One explanation is that overspending was a historical coincidence—representative preferences happened to diverge from voter preferences, or representatives were slow to learn that voter preferences had soured on spending.[6] The other explanation is that it is the nature of government to spend too much—because of problems with fiscal commons or monopoly power of bureaucracies.[7] 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 noninitiative states. If initiative states spent less than noninitiative states, then the overspending-by-nature view becomes more plausible; otherwise, the historical coincidence view gains appeal.[8]

[5] The most convincing evidence of government ''overspending''—in the sense of expenditure exceeding the electorate's preferences—is in Sam Peltzman, Voters as Fiscal Conservatives, 107 Q. J. Econ. 327 (1992). Peltzman studied election returns for presidential, gubernatorial, and U.S. Senate races between 1950 and 1988 and found that the faster spending grew while an official was in office, the fewer votes he (or his party's nominee) received in the next election. William A. Niskanen, Bureaucrats and Politicians, 18 J. Law & Econ. 617 (1975), reported similar results. My finding in Matsusaka, *supra* note 4, that initiative states spent less than noninitiative states over roughly the same period points in the same direction if we assume that policy outcomes in initiative states are more likely to reflect voter preferences.

[6] Transitory deviations between legislator and voter preferences can be caused by gerrymandering (see Thomas W. Gilligan & John G. Matsusaka, Structural Constraints on Partisan Bias under the Efficient Gerrymander, 100 Pub. Choice 65 (1999)) or collective action problems that make it difficult to monitor and discipline representatives. Even well-meaning representatives might have difficulty inferring what voters want or be slow to learn when voter preferences change (see John G. Matsusaka, Economics of Direct Legislation, 107 Q. J. Econ. 541 (1992); and John G. Matsusaka & Nolan M. McCarty (Working paper, Univ. Southern California, Marshall Sch. Bus. 1999)).

[7] For the fiscal commons argument, see James M. Buchanan & Gordon Tullock, The Calculus of Consent (1962); Barry R. Weingast, Kenneth A. Shepsle, & Christopher Johnsen, The Political Economy of Benefits and Costs: A Neoclassical Approach to Distributive Politics, 89 J. Pol. Econ. 642 (1981); and Robert P. Inman & Michael A. Fitts, Political Institutions and Fiscal Policy: Evidence from the U.S. Historical Record, 6 J. L. Econ. & Org. 79 (1990). William A. Niskanen, Bureaucracy and Representative Government (1971), contains the best-known development of the bureaucratic monopoly theory.

[8] This interpretation depends on the assumption that policies in initiative states are closer to the electorate's preferences than policies in noninitiative states. Prevailing theory supports this conclusion (for example, see Arthur T. Denzau, Robert J. Mackay, & Carolyn Weaver,

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The final purpose of the paper is to provide some grist to policy makers 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 reached a record high. Initiatives increasingly influence the political agenda at the state level and sometimes the national level, recently 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.[9] 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 noninitiative 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 noninitiative states. That is, 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.

3. The conclusion from a 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 consistently to devolution of spending authority from state to local governments.

The paper proceeds according to the following plan. The next section dis-

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On the Initiative-Referendum Option and the Control of Monopoly Government, in Tax and Expenditure Limitations (Helen F. Ladd & T. Nicolaus Tideman eds. 1981); and Elisabeth R. Gerber, Legislative Response to the Threat of Popular Initiatives, 40 Am. J. Pol. Sci. 99 (1996)), but there are counterexamples (see Matsusaka & McCarty, *supra* note 6). The most direct evidence is in Elisabeth R. Gerber, The Populist Paradox: Interest Group Influence and the Promise of Direct Legislation (1999), which (arguably) shows that state abortion and death penalty policies are closer to the median voter's preference in initiative states.

[9] Direct democracy is less common outside the United States (with Switzerland a notable exception), 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 Referendums around the World: The Growing Use of Direct Democracy (David Butler & Austin Ranney eds. 1994). A recent special feature in The Economist, December 21, 1996, at 1, 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.

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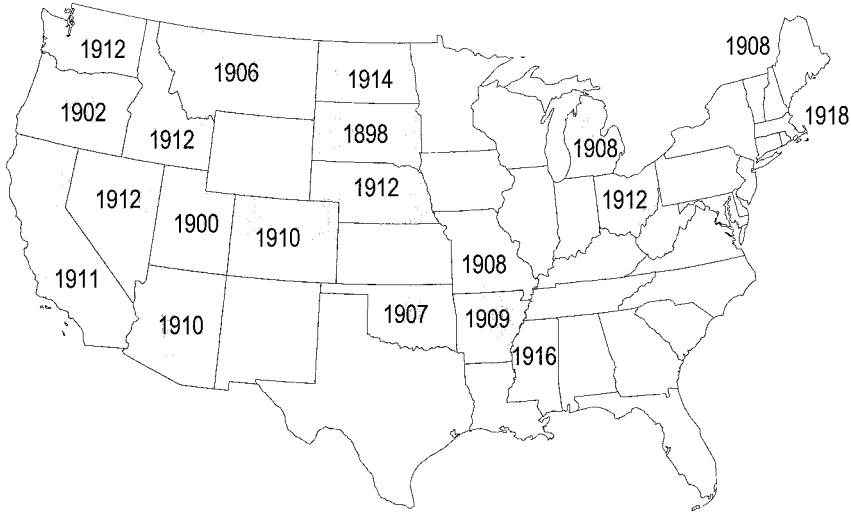

FIGURE 1.—Initiative states and year of adoption, 1898–1949

cusses the empirical strategy and data. Evidence on the fiscal effects is reported in Sections III (size of government) and IV (centralization). Section V takes a closer look at actual initiatives in California, North Dakota, and Oregon, the three states that use initiatives the most. Section VI concludes.

II. 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.[10] 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 became dormant after Massachusetts's adoption in 1918.[11] By the middle of the century, there were 19 initiative states and 29

[10] 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.

[11] 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 David B. Magleby, Direct Legislation in the American States, in Butler & Ranney eds., *supra* note 9.

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noninitiative 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

$$G_{st} = AX_{st} + BI_{st} + e_{st},$$

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 noninstitutional 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 $B = 0$.[12]

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 difficult to find, but data on the tax and spending policies of individual states and localities are 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.[13] There are some inconsistencies in the information collected by the Census Bureau, but the primary sources are good enough to allow the 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 were not yet admitted), the basic sample has 192 observations.

Summary statistics for fiscal variables appear in Table 1. All the numbers are expressed in per capita terms and stated in 1942 dollars (using the consumer price index [CPI]).[14] To give some context to these numbers, Figure 2 plots real state and local spending per capita between 1902 and 1942.[15] Although there are significant gaps in the data even at this aggregate level, a gradual upward movement in both series can be seen.

[12] Matsusaka, *supra* note 4, gives a theoretical underpinning for this approach.

[13] These data are available through the Inter-university Consortium for Political and Social Research (ICPSR). I thank John Wallis for providing me with the fiscal data and for answering my numerous queries about them.

[14] The numbers can be roughly converted to 1999 dollars by multiplying them by 10.

[15] The underlying numbers were cobbled together from a variety of sources. In addition to the Sylla-Legler-Wallis numbers, I used U.S. Dept. Commerce, Bureau of the Census, Statistical Abstract of the United States (1924–26, 1928, 1929, 1931–33, 1940–42), and U.S. Dept. Commerce, Bureau of the Census Historical Statistics of the United States (1989). Documentation is sketchy, and different sources sometimes give different numbers, so the series are only roughly comparable across time.

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TABLE 1

STATE AND LOCAL EXPENDITURE AND REVENUE PER CAPITA

Year	Mean	S.D.	Minimum	Maximum
Expenditure per capita, state and local:				
1902	25.25	12.83	7.49 (N.C.)	63.57 (Mass.)
1913	42.41	21.44	11.02 (S.C.)	108.68 (Cal.)
1932	87.04	30.09	39.56 (Ga.)	157.65 (N.J.)
1942	83.62	25.29	37.48 (Ark.)	149.44 (Nev.)
Revenue per capita, state and local:				
1902	23.35	11.40	6.27 (Miss.)	54.84 (Mass.)
1913	36.45	17.60	10.54 (S.C.)	80.26 (Cal.)
1932	72.41	24.64	27.07 (Ark.)	120.38 (Nev.)
1942	84.05	24.60	40.98 (Ark.)	137.98 (Cal.)
Expenditure per capita, local:				
1902	20.35	11.01	5.30 (S.C.)	50.45 (Mass.)
1913	35.26	18.96	8.61 (S.C.)	95.49 (Cal.)
1932	59.05	24.58	18.66 (Ark.)	122.55 (N.J.)
1942	41.61	16.27	13.69 (Ark.)	84.67 (N.Y.)
Expenditure per capita, state:				
1902	4.90	2.58	1.43 (Okla.)	13.83 (Nev.)
1912	7.16	3.59	2.40 (N.C.)	20.22 (Nev.)
1932	27.99	11.38	13.24 (Miss.)	68.17 (Del.)
1942	42.01	11.76	23.68 (Ga.)	81.40 (Nev.)

NOTE.—Each row reports summary statistics for 48 states. Oklahoma, New Mexico, and Arizona are included although they were territories before entering the Union in November 1907, January 1912, and February 1912, respectively. All numbers are in 1942 dollars.

Table 2 provides summary statistics for the main control variables: income, population, population growth rate, rural population, male population, immigrant population, population over the age of 65, and federal aid.[16] 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. Appendix A gives more details on data sources and construction of the variables.

[16] Federal aid in all regressions is total transfers to state and local governments from the federal government.

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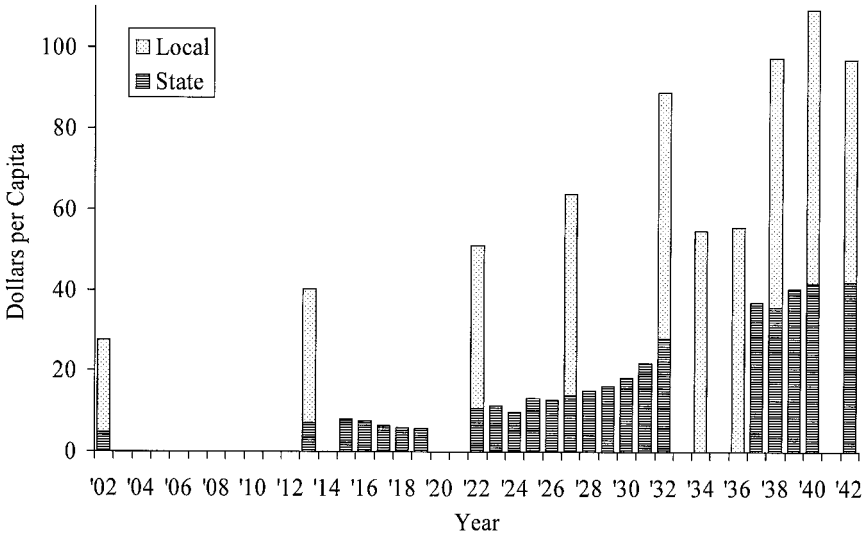

FIGURE 2.—Real state and local expenditure per capita, 1902–42

III. SIZE OF GOVERNMENT: STATE AND LOCAL GOVERNMENT COMBINED

A. Basic Results: A First Cut

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 used throughout the paper. Each column is a regression. The dependent variable in column 1 is combined state and local expenditure per capita in 1942 dollars. Beneath the coefficient estimates are the (White) standard errors. 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 one if a state allowed the initiative.[17] The coefficient on the dummy is posi-

[17] The initiative might not affect fiscal policy until it has been in place for a number of years if it takes time for proponents to utilize it or it is optimal to adjust gradually to a new fiscal equilibrium. In order to count as an initiative state, I required that the initiative was adopted 1 year before the year in question. For example, if a state adopted the initiative in 1913, it was not counted as an initiative state until 1914. The key coefficients are similar if alternative lag lengths are used instead, such as the 6-year lag in an earlier version of the paper.

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TABLE 2

SUMMARY STATISTICS FOR CONTROL VARIABLES

Year	Mean	S.D.	Minimum	Maximum
Income per capita:				
1902	419.32	173.88	155.57 (N.C.)	809.10 (Mont.)
1913	506.86	174.44	223.48 (Miss.)	857.84 (Cal.)
1932	546.97	202.58	206.32 (Miss.)	1,006.88 (N.Y.)
1942	663.70	223.22	275.20 (Miss.)	1,201.57 (Del.)
Population (in millions):				
1902	1.64	1.60	.05 (Nev.)	7.61 (N.Y.)
1913	1.99	1.92	.08 (Nev.)	9.48 (N.Y.)
1932	2.58	2.56	.09 (Nev.)	12.76 (N.Y.)
1942	2.80	2.76	.12 (Nev.)	13.74 (N.Y.)
Rural population (fraction of total population):				
1902	.67	.21	.11 (R.I.)	.92 (N.D.)
1913	.61	.21	.09 (R.I.)	.88 (N.D.)
1932	.54	.20	.08 (R.I.)	.83 (N.D.)
1942	.52	.18	.09 (R.I.)	.79 (Miss.)
Growth rate of population over previous 5 years (fraction):				
1902	.13	.11	.01 (Neb.)	.68 (Okla.)
1913	.11	.08	.01 (Vt.)	.31 (Wash.)
1932	.06	.05	−.01 (Mont.)	.25 (Cal.)
1942	.04	.04	−.03 (S.D.)	.15 (Fla.)
Immigrant population (fraction of total population):				
1902	.14	.10	.002 (N.C.)	.34 (N.D.)
1913	.14	.10	.003 (N.C.)	.32 (R.I.)
1932	.09	.07	.003 (N.C.)	.25 (N.Y.)
1942	.07	.06	.002 (N.C.)	.21 (N.Y.)
Male population (fraction of total population):				
1902	.53	.03	.49 (Mass.)	.62 (Wyo.)
1913	.52	.03	.49 (Mass.)	.63 (Nev.)
1932	.51	.02	.49 (Mass.)	.58 (Nev.)
1942	.51	.01	.49 (Mass.)	.55 (Nev.)
Population over 65 (fraction of total population):				
1902	.04	.01	.02 (Okla.)	.08 (Vt.)
1913	.04	.01	.02 (Wyo.)	.08 (Vt.)
1932	.06	.01	.03 (S.C.)	.09 (N.H.)
1942	.07	.01	.04 (N.M.)	.10 (N.H.)
Revenue from federal government per capita:				
1902	1.55	1.26	.13 (Mass.)	6.77 (Cal.)
1913	3.03	2.85	.21 (S.C.)	12.19 (Vt.)
1932	10.48	7.69	2.72 (Mass.)	38.27 (Nev.)
1942	8.81	6.22	4.01 (Ala.)	39.34 (Nev.)

NOTE.—Each row reports summary statistics for 48 states. Oklahoma, New Mexico, and Arizona are included although they were territories before entering the Union in November 1907, January 1912, and February 1912, respectively. Income and revenue from federal government are in 1942 dollars.

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TABLE 3

COMBINED STATE AND LOCAL EXPENDITURE AND REVENUE REGRESSIONS

Variable	Expenditure (1)	Revenue (2)	Expenditure (3)	Revenue (4)
Dummy = 1 if initiative state	5.21**	5.62**	9.22*	11.89*
	(1.71)	(1.75)	(4.41)	(4.91)
Signature requirement (%)	...	...	−.57	−.89
			(.55)	(.62)
Income per capita	.05**	.05**	.05**	.05**
	(.01)	(.01)	(.01)	(.01)
Population	1.12*	1.16*	1.11*	1.14**
	(.47)	(.43)	(.47)	(.44)
Rural population (fraction of total)	−14.34	−12.30	−14.66	−12.80
	(9.47)	(8.91)	(9.54)	(8.95)
Growth rate of population over previous 5 years	26.64*	19.84*	28.56*	22.84*
	(10.44)	(8.49)	(11.47)	(9.40)
Immigrant population (fraction of total)	36.77**	36.79**	35.44**	34.71**
	(11.78)	(11.98)	(11.92)	(11.84)
Male population (fraction of total)	8.04	54.48	6.43	51.96
	(62.72)	(59.32)	(62.99)	(59.75)
Population 65 or older (fraction of total)	172.51**	236.36**	173.49**	237.88**
	(64.42)	(62.46)	(64.90)	(62.95)
Revenue from federal government per capita	1.84**	.96**	1.88**	1.02**
	(.16)	(.13)	(.16)	(.13)
R^2	.926	.922	.927	.923
Adjusted R^2	.921	.917	.921	.917

NOTE.—Each column reports a regression. The dependent variable is combined state and local expenditure in columns 1 and 3 and combined revenue in columns 2 and 4. Variables with dollars as units are expressed in 1942 dollars. Each regression uses observations from 48 states/territories. The data pool the years 1902, 1913, 1932, and 1942, giving 192 observations. All regressions include four year-specific dummies (coefficients not reported). In parentheses beneath each coefficient is the (White) standard error.

* Significant at the 5% level.

** Significant at the 1% level.

tive and statistically significant at better than the 1 percent level. The point estimate indicates that an initiative state spent $5.21 per capita more that a noninitiative state after controlling for income, other demographics, and federal aid. From Table 1, we can infer that the mean per capita spending level during the period was $59.58. The initiative then was associated with 9 percent higher spending on average. While the initiative obviously was not the main determinant of spending, its quantitative effect was not trivial.

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The overall explanatory power of the regression as measured by R^2 is .926, 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 must 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 $5.62 per capita implies that the initiative increased state revenue by 10 percent relative to its sample average of $54.07.

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 collects a certain number of signatures from his fellow citizens. The signature requirement is typically expressed as a percentage of the votes cast in the state's previous gubernatorial election; in the sample period this percentage ranged from a low of 2 percent in North Dakota to a high of 10 percent in Arizona, Idaho, Maine, and Nevada.[18]

The regressions in columns 3 and 4 introduce a variable that is equal to the signature requirement for initiative states and zero for noninitiative 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 in 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, although it does not achieve statistical significance at conventional levels. The coefficient on the initiative dummy remains positive and statistically significant.

The significance of these coefficients individually is less important than their full effect, which takes into account both the availability of the initiative and the signature requirement. The full effect is the sum of the coefficient on the initiative dummy and the signature-weighted coefficient on the signature requirement variable. The statistical significance of the full effect

[18] Here are some details: North Dakota's signature requirement was a flat 10,000 (since 1978 it is 2 percent of residents). I converted it to 2 percent following Magleby, *supra* note 11. 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 Appendix A.

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TABLE 4

FULL EFFECT OF THE INITIATIVE ON STATE AND LOCAL EXPENDITURE AND REVENUE BY SIGNATURE REQUIREMENT

Signature Requirement (%)	Expenditure (3′)	Revenue (4′)
2	8.09*	10.12**
	(3.42)	(3.78)
3	7.52*	9.23**
	(2.95)	(3.24)
4	6.96**	8.34**
	(2.53)	(2.73)
5	6.39**	7.46**
	(2.15)	(2.29)
6	5.82**	6.57**
	(1.86)	(1.94)
7	5.25**	5.68**
	(1.71)	(1.75)
8	4.69**	4.80**
	(1.73)	(1.77)
9	4.12*	3.91*
	(1.92)	(1.99)
10	3.55	3.02
	(2.22)	(2.35)

NOTE.—The main entry indicates the full effect of the initiative on expenditure or revenue (as indicated at the top of the column) given a signature requirement, relative to a state that did not have the initiative. Standard errors are in parentheses beneath each estimate. The numbers in column 3′ are derived from regression (3) of Table 3. The numbers in column 4′ are derived from regression (4) of Table 3.

* Significant at the 5% level.

** Significant at the 1% level.

can be tested using the *F*-statistic for the hypothesis that the linear combination is zero.

Table 4 gives the results of these calculations. The estimates in column 3′ are derived from regression (3) of Table 3, and those in column 4′ are derived from regression (4) of Table 3. The main entry in each column gives the full effect of the initiative relative to a noninitiative state. The standard error is in parentheses below the coefficient. It can be seen that initiative states had higher spending and revenue than noninitiative states for all signature levels that appear in the sample. The effects are statistically significant for signature requirements up to 9 percent for expenditure and revenue. For the modal state, with a signature requirement of 5 percent, the initiative was associated with $6.39 more expenditure per capita (11 percent

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relative to mean expenditure) and $7.46 more revenue per capita (14 percent relative to mean revenue).[19]

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 that 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 by introducing additional control variables.

Table 5 presents the findings. As before, each column reports a regression. The dependent variable is combined state and local expenditure in panel A and combined revenue in panel B. At the bottom of each column, 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 in each panel reestimate the regressions in Table 3 after adding two region dummy variables, one for southern states and one for western states.[20] 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.[21] The southern dummy is negative and statistically insignificant, and the western dummy is significantly positive. The important point is that inclusion of the region dummies has little effect on the initiative effects, which decline but remain statistically significant at conventional levels, whether measured unconditionally or conditional on the signature requirement. The full effects by signature requirement reported at the bottom of columns A2 and B2 indicate a statistically significant effect of the initiative for signature percentages up to 8 percent in both the expenditure and revenue equations.

Another possibility is that voters in initiative states had an underlying

[19] 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 for 1960–90.

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

[21] Since the initiative states are clustered in the West, there is also the danger that the western dummy will absorb part of the initiative effect, biasing down the relevant coefficients.

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TABLE 5

OTHER SPECIFICATIONS OF COMBINED STATE AND LOCAL EXPENDITURE AND REVENUE REGRESSIONS

A. DEPENDENT VARIABLE: EXPENDITURE

Variable	(A1)	(A2)	(A3)	(A4)	(A5)	(A6)
Dummy = 1 if initiative state	4.22*	9.32*	3.46*	8.31+	4.90+	−5.49
	(1.74)	(4.44)	(1.72)	(4.50)	(2.51)	(6.02)
Signature requirement (%)	...	−.73	...	−.69	...	1.54+
		(.55)		(.56)		(.82)
Dummy = 1 for southern state	−.69	−.43	−2.89	−2.66	...	...
	(2.32)	(2.32)	(2.64)	(2.63)		
Dummy = 1 for western state	4.78+	5.27+	4.31	4.72+	...	...
	(2.47)	(2.52)	(2.70)	(2.72)		
Mean NOMINATE for U.S. senators	...	...	−4.93*	−4.69*	−2.03	−2.16
			(2.20)	(2.21)	(2.40)	(2.41)
State fixed effects?	No	No	No	No	Yes	Yes
R^2	.927	.928	.928	.929	.964	.965
Adjusted R^2	.922	.922	.922	.922	.948	.948
Observations	192	192	189	189	189	189
		Full Effect		Full Effect		Full Effect
Signature requirement (%):						
2		7.86*		6.93*		−2.42
3		7.13*		6.23*		−.88
4		6.40*		5.54*		.65
5		5.67**		4.85*		2.19
6		4.94**		4.16*		3.73
7		4.21*		3.46*		5.26*
8		3.48*		2.77		6.80*
9		2.75		2.08		8.33**
10		2.02		1.39		9.87**

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B. Dependent Variable: Revenue

Variable	(B1)	(B2)	(B3)	(B4)	(B5)	(B6)
Dummy = 1 if initiative state	4.45*	11.90*	3.65*	10.48*	2.92	−7.56
	(1.83)	(4.92)	(1.85)	(5.04)	(2.03)	(5.42)
Signature requirement (%)	...	−1.07+	...	−.98	...	1.55*
		(.61)		(.62)		(.76)
Dummy = 1 for southern state	−1.28	−.92	−3.53	−3.21	...	...
	(2.16)	(2.12)	(2.46)	(2.41)		
Dummy = 1 for western state	5.31*	6.03*	5.04+	5.62*	...	...
	(2.47)	(2.50)	(2.67)	(2.69)		
Mean NOMINATE for U.S. senators	...	...	−5.81**	−5.47**	−3.27	−3.39+
			(2.01)	(2.01)	(1.93)	(1.94)
State fixed effects?	No	No	No	No	Yes	Yes
R^2	.924	.925	.926	.927	.971	.971
Adjusted R^2	.918	.919	.919	.920	.957	.957
Observations	192	192	189	189	189	189
		Full Effect		Full Effect		Full Effect
Signature requirement (%):						
2		9.77*		8.53*		−4.46
3		8.70**		7.55*		−2.91
4		7.63**		6.58*		−1.36
5		6.57**		5.60*		.19
6		5.50**		4.63*		1.74
7		4.44*		3.65+		3.29
8		3.37+		2.68		4.84*
9		2.30		1.70		6.39*
10		1.24		.72		7.94*

Note.—Each column reports estimates from a regression. The dependent variable is state and local expenditure in panel A and revenue in panel B. Each column first reports select coefficients and (White) standard errors in parentheses. The bottom of each column contains the full effect of the initiative conditional on signature requirement, relative to a state that does not have the initiative. Each regression uses data from 48 states/territories and 4 years, 1902, 1913, 1932, and 1942. All regressions include the control variables from Table 2 and four year-specific dummies.

+ Significant at the 10% level.
* Significant at the 5% level.
** Significant at the 1% level.

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 A3, A4, B3, and B4 attempt to control for unobserved ideology by adding the mean NOMINATE score for each state's U.S. senators to the regressions in columns 1 and 2.[22] The NOMINATE scores for each senator were calculated by Keith Poole and Howard Rosenthal.[23] The scores give the spatial location of each senator's roll call votes in 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 took the first-dimension score for each senator and calculated the average value for each state. A score of +1 can be thought of as the most conservative and −1 as the most liberal.

The NOMINATE variable has the sign predicted by theory (states with conservative senators spent less than states with liberal senators), and the coefficients are different from zero at conventional levels of significance. More to the point, inclusion of the NOMINATE variable does not eliminate the initiative effects. The coefficients and full effects by signature requirement remain significant, and their magnitudes still suggest nontrivial economic effects.[24]

Another concern is whether 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 three 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 the sample period. Since Nevada is an initiative state, we want to be sure that the effects are not coming from

[22] The number of observations falls by three because the territories of Arizona, New Mexico, and Oklahoma did not have U.S. senators in 1902.

[23] Keith T. Poole & Howard Rosenthal, Patterns of Congressional Voting, 35 Am. J. Pol. Sci. 228 (1991).

[24] I also directly tested for underlying ideological/preference differences between initiative and noninitiative states. For the four sample years, the mean NOMINATE score for senators of initiative states was −0.045 and the mean for noninitiative states was 0.014. The difference is not statistically significant. I also compared information from Gallup polls taken over 1937–39 using data that Robert Erikson kindly provided me (see Erikson, Wright, & McIver, *supra* note 2, 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 noninitiatives states. The difference is not statistically significant.

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this state alone.[25] To investigate this, I reestimated (but do not report to conserve space) the preceding regressions after deleting Nevada from the sample. The estimates change little, and if anything the initiative effects become larger.

The regressions to this point address the most concrete concerns but do not rule out the possibility that initiative states are fundamentally different from noninitiative states in some unmeasured way. The final two regressions in each panel control directly for state-specific determinants of expenditure and revenue by adding state fixed effects (SFEs) to the regressions. The limitation of this approach is that the initiative variable comes close to being a fixed effect itself (it is literally a fixed effect for three states), so the SFEs are likely to strip out a good part of the initiative effect. The challenge this presents to the data (essentially by biasing down the coefficients) provides a tough test for the initiative effect.

As can be seen, the SFEs tend to cut down the initiative effect, but not entirely. The coefficient on the initiative dummy in column A5, which does not include a signature requirement interaction, is actually a little larger than in the other regressions and is different from zero at more than the 10 percent level. The fall in statistical significance is not surprising given the loss of 48 degrees of freedom when the SFEs are included. In column A6, the initiative coefficients reverse sign, but the full effects by signature requirement implied by the coefficients tell more or less the same story as before: the initiative tends to be associated with higher spending. If we focus on signature requirements in the 5–8 percent range, where most of the data lie, the full effects are positive in all cases, comparable in magnitude to the other regressions, and statistically different from zero for signature requirements 7 percent and above.[26]

The SFEs take a bigger toll in the revenue equations. In column B5, the initiative dummy remains positive but does not achieve statistical significance at conventional levels. The sign reversal appears again in column B6, but the story for the full effects does not change. We see positive effects of the initiative for signature requirements above 4 percent, where most of the data lie, and the effects are significantly different from zero for signature requirements of 8 percent and above. Thus, even if we make the most ex-

[25] John Joseph Wallis, The Political Economy of New Deal Spending Revisited, Again: With and Without Nevada, 35 Explorations in Econ. Hist. 140 (1998), documents that inclusion or exclusion of Nevada dramatically affects the findings of political economy studies of New Deal spending.

[26] The sign reversal may occur because neither initiative variable displays much time-series variation. Once the SFEs are included, each equation comes close to having three SFEs, the actual state fixed effect and the two initiative variables, creating possible collinearity problems.

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treme assumption—that the SFEs strip out only noninitiative effects—there is still evidence of a positive relation between initiatives and the size of government.

The basic result therefore appears to survive these robustness checks,[27] none of which establishes 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 state-specific factors.

IV. Centralization: State versus Local Expenditure

I next turn to the issue of centralization of expenditure. Government centralization, by most measures, has increased during the twentieth century.[28] 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.[29] Although some of the trade-offs are understood—for example, local decision making can result in 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 1960–90 period, I found that initiative states had significantly less centralization of expenditure than noninitiative 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.[30] This section investigates whether initiative states were more or less centralized in the first half of the century than noninitiative 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 twentieth century was a historical coincidence.

Table 6 presents the results of estimating the expenditure regressions separately for state and local governments. Each column reports a regression

[27] Another approach is to estimate the regressions using only the initiative states. I did this and found positive initiative effects this way as well.

[28] For example, John Joseph Wallis, Form and Function in the Public Sector: State and Local Government in the United States, 1902–1982 (Working paper, Univ. Maryland, October 1995), and John Joseph Wallis, American Government Finance in the Long Run: 1790 to 1990, 14 J. Econ. Persp. 61 (2000), document an increasing centralization of state and local government spending throughout the century.

[29] See Robert P. Inman & Daniel L. Rubinfeld, Rethinking Federalism, 11 J. Econ. Persp. 43 (1997); and John A. Ferejohn & Barry R. Weingast, The New Federalism: Can the States Be Trusted? (1997).

[30] Matsusaka, *supra* note 4.

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TABLE 6

SEPARATE LOCAL EXPENDITURE AND STATE EXPENDITURE REGRESSIONS

Variable	Local (1)	State (2)	Local (3)	State (4)	Local (5)	State (6)
Dummy = 1 if initiative state	4.91*	.32	13.57**	−4.18	13.00**	−4.55+
	(1.71)	(.95)	(3.72)	(2.61)	(3.87)	(2.70)
Signature requirement (%)	...	...	−1.22**	.64+	−1.34**	.63+
			(.46)	(.36)	(.47)	(.37)
Includes dummy for southern and western states, and mean NOMINATE for U.S. senators?	No	No	No	No	Yes	Yes
R^2	.850	.921	.853	.922	.857	.926
Adjusted R^2	.840	.915	.842	.917	.844	.890
Observations	192	192	192	192	189	189
			Full Effect	Full Effect	Full Effect	Full Effect
Signature requirement (%):						
2			11.12**	−2.91	10.32**	−3.29
3			9.90**	−2.27	8.98**	−2.67
4			8.68**	−1.64	7.64**	−2.04
5			7.45**	−1.00	6.30**	−1.41
6			6.23**	−.37	4.96**	−.79
7			5.00**	.27	3.62*	−.16
8			3.78**	.90	2.29	.47
9			2.56	1.54	.95	1.09
10			1.33	2.17	−.39	1.72

NOTE.—Each column reports estimates from a regression. The dependent variable is either local expenditure or state expenditure, as indicated at the top of the column. Each column first reports select coefficients and (White) standard errors in parentheses. The bottom of each column contains the full effect of the initiative conditional on signature requirement, relative to a state that does not have the initiative. Each regression uses data from 48 states/territories and 4 years, 1902, 1913, 1932, and 1942, and includes four year-specific dummies and all the control variables from Table 2.

+ Significant at the 10% level.

* Significant at the 5% level.

** Significant at the 1% level.

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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. Only the coefficients on the initiative variables are reported to conserve space.

The first two regressions include the initiative dummy but not the signature requirement. For local governments, the effect of the initiative is positive and significant at the 1 percent level. For state government, the initiative coefficient is positive 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: $4.91 per capita is 13 percent of the average local government expenditure in the sample ($35.07).

In columns 3 and 4, the signature requirement variable is introduced. The estimated coefficients in the local government equation go in the expected direction, but the state coefficients suggest a negative effect for low signature requirements. In the local government equation, both initiative coefficients are significant at better than the 1 percent level. In the state government equation, only the signature requirement coefficient is significant. The bottom sections of columns 3 and 4 report the full effect of the initiative by signature requirement. The initiative had a positive effect on local spending for all signature requirements, and the effect can be rejected from zero at conventional significance levels for signature requirements up to 8 percent. The initiative effect was muddled for state spending: the point estimates suggest a negative effect for low signature requirements and a positive effect for high signature requirements, but none of the effects can be distinguished from noise.

To test for robustness, I also ran a set of regressions with additional control variables, none of which changed the basic message. Columns 5 and 6 report the results when region dummies and the NOMINATE variable are added, a representative case. The estimated effects decline, but the initiative was still associated with significantly higher local spending and lower but not statistically significant state spending. When I ran the regressions with SFEs (not reported), the coefficient on the initiative dummy ($5.41) was significantly positive in the dummy-only local expenditure regression, and the full effect was significantly positive for signature requirements in the 6–10 percent range when the signature requirement variable was added. State spending was negative but insignificant.

The finding then is that the initiative increased local spending and perhaps slightly reduced state spending. The ultimate consequence of the initiative was to decentralize expenditure. One interpretation of the main result of Section III—higher overall spending by initiative states—is that voters generally had a higher demand for government spending than their repre-

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sentatives were delivering. However, if this interpretation is correct, we might 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 (again, possibly) a view that state spending is excessive.

V. A Closer Look at Actual Initiatives in California, North Dakota, and Oregon

So far, the paper has measured differences between initiative and noninitiative states and attributed those differences to availability of the initiative. In this section, I examine the actual initiatives in three states. 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 different policies than 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. The first and third channels would presumably leave traces in the historical record of initiatives. The purpose of this section is to see if the history of actual initiatives tells the same story as the statistical evidence.

I began by assembling a list of all initiatives with nontrivial fiscal impact in the states of California, North Dakota, and Oregon in the first half of the century. I chose these states because they were the leading users of the initiative. Each initiative was then classified into one of three groups: (1) those that primarily increased borrowing, expenditure, or taxes; (2) those that reduced or limited borrowing, spending, or taxes; and (3) those that changed the distribution of spending or taxes.[31] Table 7 reports the numbers. The actual initiatives are listed in Appendix B.

The initiative was not always used to increase spending, but this was the case more often than not. Seven of the nine successful measures in California increased spending, taxes, or borrowing (four for education, one for veterans, one for the aged and blind, and one for highways). Of the 11 successful initiatives in North Dakota, three increased spending (for old-age pensions, financially distressed schools, and highways), two increased taxes, and two made it easier to borrow. Of the 14 initiatives passed in Oregon, three increased spending (one for state schools, one for old-age pensions, and one for rural credits), three increased taxes, and one made it easier to borrow. For the three states overall, 21 initiatives were approved that

[31] The last category includes single tax proposals that shifted all taxes to land and measures earmarking state revenues for particular spending programs such as schools.

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TABLE 7

NUMBER OF INITIATIVES IN CALIFORNIA, NORTH DAKOTA, AND OREGON THAT PROPOSED TO INCREASE AND DECREASE SPENDING, 1900–49

	Number That Increased Borrowing, Expenditure, Taxes	Number That Decreased Borrowing, Expenditure, Taxes	Number That Earmarked Expenditure
Initiatives that passed:			
California	7	2	0
North Dakota	7	2	2
Oregon	7	7	0
Total	21	11	2
Initiatives that failed:			
California	15	5	5
North Dakota	5	4	1
Oregon	17	9	5
Total	35	26	11

increased spending, taxes, or borrowing compared to 11 initiatives that reduced spending, taxes, or borrowing.

The general impression is that the fiscal effects detected in the statistical analysis—more spending by initiative states—were in many cases explicitly forced by initiatives. This suggests that the initiative device affected policy directly, not just through the threat it provided. Because we can see the initiative directly at work increasing government spending in the historical record, Table 7 also lends some corroboration to the interpretation of the evidence offered above that the initiative caused the fiscal differences between states.[32]

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. 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 declined to reduce motor vehicle license fees in 1926, rejected limits on taxes and debt in 1932, and rejected limits on property and school taxes in 1934 and 1936. Overall, however, most of the defeated measures would have increased ex-

[32] Initiatives that clearly decentralized are harder to find. The most obvious cases were three education measures in California that required the state to raise more money for public schools and transfer it to local governments. A 1922 initiative in North Dakota that eliminated minimum salaries for teachers may have had a decentralizing effect if it gave local decision makers more of a say in setting compensation.

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penditure or taxes. Such a pattern is not too surprising—we expect to see pro-spending interest groups make more proposals when the electorate has an appetite for more spending. The fact that so many measures went down to defeat indicates that the electorate's taste for spending was discriminating.

VI. Conclusion

This paper documents some fiscal effects of the state-level voter initiative in the United States in the first half of the twentieth century. It extends my earlier paper that studies the last half of the century.[33] 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 regard to fiscal policy.

Two empirical patterns appear to be robust. First, initiative states spent more than noninitiative states in the first half of the century. Second, initiative states decentralized expenditure (from state to local governments) more than noninitiative 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 antispending device. I conjecture that the initiative's main effect is to bring fiscal policy more in line with the electorate's preferences. When representatives are more fiscally conservative than the voters, the initiative leads to higher spending, and when representatives are more liberal, the initiative leads to lower spending. According to this view, in the first half of the century representatives were slow to respond to increased voter demands for spending, while in the second half of the century elected officials were slow to respond to voter demands for government downsizing. The bottom line is that those who are looking for a way to make government smaller should not rely on the initiative.

On the other hand, although the initiative does not always 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.

Finally, these results provide some insight into the causes of government overspending in the second half of the century. The finding that initiative states spent more in the first half of the century than noninitiative states

[33] Matsusaka, *supra* note 4.

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suggests that overspending is a transitory historical event, not an inevitable feature of government. If governments systematically exceed voter demands, we should probably see the initiative used to cut spending in the first half of the century as well. This evidence would seem to be problematic, then, for theories that imply systematic overspending, such as the fiscal commons and bureaucratic monopoly theories. Although the comparison of the evidence from different time periods tends to undercut the view of government as systematically too large, it suggests 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. The fact that voters reserve their hostility for state (and not local) government presents a puzzle that would seem to merit further inquiry.

APPENDIX A

Data Construction and Sources

Initiative Variables. Signature requirements were assigned as follows: 2 percent (North Dakota), 5 percent (Colorado, Massachusetts, Montana, Missouri, South Dakota, Utah), 6 percent (Ohio, Oregon), 7 percent (Nebraska), 8 percent (Arkansas, California, Michigan, Oklahoma, Washington), 10 percent (Arizona, Idaho, Maine, Nevada).[34] Mississippi was coded as a noninitiative 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'' (L11) 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 SSS, 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.

[34] These numbers are taken from David B. Magleby, Direct Legislation: Voting on Ballot Propositions in the United States (1984).

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Price Level. I converted nominal values to 1942 dollars using the CPI 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. Personal income per capita for each state in 1900 and 1920 came from the census; values for 1930 and 1940 came from *Survey of Current Business*. Number for 1910 were interpolated so that the 20-year change was allocated in the same share as nominal gross national product changed, 22 percent. I then interpolated geometrically to find per capita income in the sample years and converted to 1942 dollars using the CPI. Lawrence Kenny and John Wallis kindly provided the data.

Population Demographics. Population, urban population, male population, and population over the age of 65 for each state in years ending in ''0'' were taken from the census and provided to me by John Wallis. Lawrence Kenny provided immigrant (foreign-born) population data, taken from the census. Values for sample years were calculated by geometric interpolation.

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.

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APPENDIX B

TABLE B1

CALIFORNIA, NORTH DAKOTA, AND OREGON INITIATIVES WITH FISCAL IMPACT, 1900–1949

A. California

Year	Approved	Failed
1912		Allowed property taxes to be set locally
1914	Abolished poll taxes	Authorized $1.25 million in state bonds for building construction in Los Angeles
	Authorized $1.8 million in bonds to build University of California campus at Berkeley	
1916		Prohibited all taxes except on land
1918	Prohibited all taxes except on land	Restricted the growth of county and school taxes
1920	Increased interest rate limit on highway bonds to 6% from 4.5%, shifted the repayment from counties to state	Increased salary of judges
	Set minimum levels of state and county funding for public education	
	Created a property tax with revenues dedicated to the state university	
	Prohibited all taxes except on land	
1922	Provided aid for veterans to buy homes	Required publicly owned utilities to pay property taxes
	Authorized $50 million in water and power bonds	
	Prohibited all taxes except on land	
1924	Authorized bonds for water and power	Prohibited local taxes of highway transportation companies, substituted a lower state tax
1926	Appropriated $60 million to construct state highways	Increased gas tax, dedicated revenue to state highway fund
	Authorized $500 million in water and power bonds	
1932		Established state income and sales taxes, dedicated revenue to school equalization fund
1936	Repealed the 1935 state income tax	
	Required gas tax revenue to be used for highways	
1938	Eliminated sales taxes	
	Provided weekly payments to people age 50 and above	
1939	Provided weekly payments to people age 50 and above	
1942	Abolished personal income taxes unless approved by referendum or legislative supermajority	

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1944	Increased state funding for public schools by two-thirds	Provided weekly payments to retirees over the age of 60
1946	Increased state funding for public schools and set minimum teacher salaries	
1948	Increased aid for the aged and blind	Provided annual subsidies of $25 million for public housing
1949	Eliminated minimum payments for the aged and blind	

B. North Dakota

Year	Approved	Failed
1918	Authorized legislature to impose an acreage tax for crop hail insurance Increased state's borrowing authority to $10 million from $200,000	
1922	Repealed salary minimums for teachers Increased state's borrowing authority to $20 million from $10 million	
1924		Reduced and limited property taxes
1926	Increased state gasoline tax from 1 cent to 2 cents per gallon	
1928		Authorized $28 million in bonds to idemnify depositors who lost money in bank closures
1930		Increased the state gasoline tax from 2 cents to 4 cents per gallon
1932	Reduced the taxable assessed value of property from 75% to 50%	
1934		Set a maximum levy for school taxes
1938	Provided an old-age pension of $40 per month	
1939		Diverted highway funds to old-age assistance fund for 2 years
1940	Created an income tax Earmarked sales tax revenue for schools and welfare I Earmarked sales tax revenue for schools and welfare II Increased funding for financially distressed schools Permitted abatement of taxes based on excessive valuation	Created a graduated land tax
1942		Created a graduated land tax
1944	Authorized $12 million in highway bonds	Repealed the state income tax

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TABLE B1 (*Continued*)

C. OREGON

Year	Approved	Failed
1906	Imposed profit tax on sleeping car, refrigerator car, and oil companies	Abolished tolls on two roads and provided for state ownership
	Imposed profit tax on express, telegraph, and telephone companies	
1908		Adopted single tax
1910	Established state school at Monmouth	Established a state normal school at Weston
	Abolished poll tax and gave counties a single tax option	
	Permitted counties to vote bonds for road improvements	
	Established property tax for state university	
1912	Limited the amount of indebtedness for construction of state roads	Established three measures that empowered counties to issue road bonds
	Limited the amount of indebtedness for construction of county roads	
	Provided tax exemption for household effects	
	Provided for an income tax	
	Provided tax exemption for certain intangible property	
	Revised inheritance tax laws, making them progressive	
	Adopted single tax	

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1914	Established graduated personal tax Established equal assessment and taxation and a $300 exemption	Provided a $1,500 tax exemption
1916	Established rural credits	Established a full rental value land tax and a homemakers' loan fund
1920		Adopted single tax
1921		Adopted single tax
1922	Created income tax	Established tax for 1925 exposition
1924	Repealed income tax	
1926	Established income tax	Established income tax with property tax offset
1928	Reduced motor vehicle license fees Established income tax	Established 5-cent gasoline tax
1932	Established personal income tax	Controlled level of taxes and debt
1934		Limited property taxes
1936	Limited and reduced property taxes	Limited taxes for large school districts
1938		Provided annunity for retirees, levied taxes
1942	Distributed surplus state funds to school districts, reduced school taxes	
1944	Provided monthly annunities from income tax	Increased state taxes for public schools
1946	Created a basic school support fund paid for by an annual tax levy	Created state old-age and disability pension fund
1948	Established old-age pension Increased income tax exemptions	

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