

# Fiscal Effects of the Voter Initiative: Evidence from the Last 30 Years

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In 23 American states, citizens can initiate and approve laws by popular vote; in the other 27 states, laws can be proposed only by elected representatives. This paper compares the fiscal behavior of state and local governments over the last 30 years under these two institutional arrangements. The main finding is that spending is significantly lower, on the order of 4 percent, in states with voter initiatives than in pure representative states. It is also found that local spending is higher and state spending is lower in initiative states. On the revenue side, initiative states rely less on broad-based taxes and more on charges tied to services. Taken together, the evidence indicates that the initiative leads to a reduction in the overall size of the government sector and suggests that it causes a decline in the level of redistributive activity.

## I. Introduction

The economic theory of government pioneered by Stigler (1971), Peltzman (1976), and Becker (1983) views policy as the outcome of competition between pressure groups. Groups are assumed to compete within the context of a given set of “rules,” or institutions. By examining environments in which institutions are held constant, researchers have been able to focus on noninstitutional explanations for political outcomes. Among the most successful of these studies

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are those that have shown a strong linkage between legislative outcomes and constituent characteristics (e.g., Peltzman 1984; Gilligan, Marshall, and Weingast 1989).

An interesting question that emerges from this line of research is whether the *ceteris paribus* assumption about institutions is important and, if so, how important. In actuality, institutions vary substantially across political units (countries, states, and localities) and across time. Countries differ in whether they are democratic or not and, if democratic, whether they have parliamentary or presidential systems, whether they are centralized or organized as a federal system, and so on. States display differences in a wide assortment of institutions. Notable examples include term limits, executive vetoes, balanced-budget requirements, and tax and spending limits. Variation in the institutional context of political competition suggests that it may be fruitful to look for institutional explanations for policy outcomes as well.

This paper is an attempt to complement the economic theory of government by providing evidence on the effects of an institution that is widely used in American state and local governments, the voter initiative. The federal organization of the United States provides a hospitable environment to study this process. At present, citizens in 23 American states can propose and pass state laws directly—without recourse to their elected representatives—by means of initiatives. In the other 27 states and at the federal level, laws can originate only from the elected legislature. In this paper, I compare the state and local governmental finances of initiative states and what I call “pure representative” states over the last 30 years, 1960–90. Examination of budget behavior allows not only a statistical evaluation of whether the initiative matters but also a quantitative evaluation of how much it matters. There are other policies that could be compared between states. For example, what sort of social and regulatory laws do they have? However, fiscal outcomes are a natural place to begin looking for effects of institutional differences.

There are theoretical reasons to expect that the initiative matters for government outcomes. First, there is an emerging literature on legislative organization, often identified with Weingast, Shepsle, and Johnsen (1981) and Weingast and Marshall (1988). According to this body of research, legislatures are designed to increase the gains from trade between representatives by reducing transaction costs associated with approval of government projects. The gains-from-trade hypothesis argues that because of vote trading, projects that are highly valued by some representatives may be approved even if they do not command majority support. As a result of this logrolling,

legislatures generally do not implement median voter outcomes. However, vote trading is not possible with direct voting, so initiatives are likely to yield policies closer to median voter outcomes. If the gains-from-trade hypothesis is correct, then fiscal outcomes in initiative states should be different from those in pure representative states.

Another theoretical reason that the initiative might matter is found in the agenda control literature associated with Romer and Rosenthal (1979). This work shows that if a person has monopoly power to initiate legislation, then he can drive the policy outcome away from the median voter's ideal point. This is possible because the agenda setter can force the median voter to approve an undesirable policy by threatening reversion to an even less desirable status quo policy. In a legislature, the agenda process tends to be restricted in a number of ways. Typically, committees have an exclusive right to originate legislation in their area of policy expertise. On the other hand, under the voter initiative the agenda process is completely open, at least in principle.<sup>1</sup> Any citizen may propose any law he likes, and as long as a uniform set of conditions are satisfied, his proposal is put before the electorate. By removing agenda control from the legislature, the initiative alters the balance of power between competing pressure groups, which is likely to result in different policy outcomes.

A third reason to expect that the initiative might matter stems from the fact that representatives and constituents have incomplete information about each other. The limited information that constituents have about their representatives gives rise to the possibility of legislator "shirking": representatives may be able to implement policies contrary to constituent interests without fear of being punished at the polls (see, e.g., Kalt and Zupan 1984, 1990). A less appreciated fact is that representatives also suffer from limited information, in particular, information about constituent preferences. This can cause even well-intentioned legislators inadvertently to implement policies that deviate from those the median voter would like (Matsusaka 1992). In the presence of these information imperfections, the initiative can lead to policy outcomes different from those the legislature would choose.

In addition to these theoretical issues, evidence concerning the fiscal consequences of the initiative has a bearing on policy. Opinion polls consistently show strong support for the idea of direct lawmaking, at both the state and national levels. Accordingly, during the

<sup>1</sup> Monopoly control of the agenda process is less apparent for state initiatives than the school budget referendums studied in Romer and Rosenthal (1978).

last decade, several states considered amending their constitutions to provide for voter initiatives, and the idea of national initiatives was discussed. While the merits of the initiative are debated on a variety of grounds, at the heart of many discussions is the question of what effect it has on policy and, in particular, whether citizen initiatives lead to more or less government. For example, some see the initiative as a panacea for what they consider the runaway growth of government. Others view it as an invitation for a myopic electorate to lavish spending on itself. None of these arguments at present is based on more than anecdotal evidence, typically drawn from a few prominent cases such as California's Proposition 13. Things are made even murkier by the fact that the efficacy of these highly visible tax and spending limitation measures is in dispute (see Dougan 1988). Comparative evidence on government behavior can help to focus the debate between proponents and critics of direct lawmaking.

The paper's main finding is that states with the initiative have significantly lower spending per capita. For a typical initiative state with relatively easy ballot access, expenditure is about 4 percent less than in a similar state with a pure representative form of government. The finding that the initiative inhibits government spending appears to corroborate Peltzman's (1992) finding that elected representatives spend more than the median voter wishes.

A second finding is that availability of the initiative leads to a particularly large reduction in the state component of state and local spending and an increase in the local component. State-level expenditure in a typical initiative state is about 12 percent lower and local spending is about 10 percent higher than in a comparable pure representative state. Thus, in addition to reducing overall spending, the initiative causes a shift away from state and toward local spending.

A third finding concerns the revenue side of the budget: initiative states rely less on taxes and more on charges for services than pure representative states. For a typical state, taxes are roughly 8 percent lower and charges are 7 percent higher. It is inherently less redistributive to raise revenue with charges than taxes because charges require the people who consume government services to bear more of the associated costs. The shift in spending toward local government also limits potential redistribution. Taken together, these findings seem to suggest that one of the broad consequences of the initiative is to reduce the amount of government redistribution.

The paper proceeds as follows. Section II provides background information on voter initiatives in the United States. Section III develops an empirical framework for the analysis. Sections IV and V are devoted to documenting the effects of the voter initiative on fiscal outcomes. Section VI considers the possibility that estimated initiative

effects are proxies for unobserved state ideologies. Section VII offers an interpretation of the evidence and discusses the implications. Section VIII presents a conclusion.

## II. Overview of the Initiative

Direct democracy is a public decision-making process in which enactment of laws depends on a direct vote of citizens, not their elected representatives. In one type of direct democracy, laws are proposed by representatives, whereas in the other they originate with the citizens themselves. Both types have been present in the United States from the beginning, such as when the Massachusetts legislature held a referendum on the state's new constitution in 1778 or when New England communities held town meetings. The citizen-initiated type of direct democracy did not appear at the state level until South Dakota adopted the initiative device in 1898 at the onset of the Progressive movement. Over the next three decades, 18 states followed, amending their constitutions to allow citizens to initiate and pass laws directly. Since then, the only continental states to adopt direct law-making at the state level were Wyoming in 1968, Illinois in 1970, and Florida in 1978, although Minnesota and Rhode Island came close in the 1980s. Counting Alaska, which entered the Union with the initiative as part of its constitution, there are now 23 "initiative" states, states that allow citizens to initiate and enact laws without recourse to their elected representatives. Table 1 lists these states and indicates the year they first made the initiative available. The list includes both populous industrialized states and sparse rural states. All regions of the country are represented, but the initiative is especially prevalent west of the Mississippi River.

The initiative has a standard form: a citizen is allowed to propose a new law, and if he can collect a certain number of signatures from fellow citizens, the proposed law is placed on a statewide ballot. The voters can vote either for it or against it. If a majority vote in favor of it, the proposal becomes law.

The details of how the initiative is administered vary across states. The most important difference is the number of signatures required to place a measure on the ballot. The signature requirement is usually expressed as a fraction of the number of votes cast in the preceding gubernatorial election. For example, in order to qualify for the ballot in California, a statutory petition needs signatures equal to 5 percent of the number of votes cast in the most recent gubernatorial election. As the signature requirement rises, it becomes increasingly difficult to use the initiative. Thus, from a conceptual point of view, it is reasonable to say that among the initiative states, some of them have

TABLE 1  
VOTER INITIATIVE IN THE UNITED STATES

State	Year Initiative Was Adopted	Signatures Required to Qualify for Ballot (%)
Alaska	1959	10
Arizona	1910	10
Arkansas	1909	8
California	1911	5
Colorado	1910	5
Florida	1978	8
Idaho	1912	10
Illinois	1970	8
Maine	1908	10
Massachusetts	1918	5
Michigan	1908	8
Missouri	1908	5
Montana	1906	5
Nebraska	1912	7
Nevada	1904	10
North Dakota	1914	2
Ohio	1912	6
Oklahoma	1907	8
Oregon	1902	6
South Dakota	1898	5
Utah	1900	10
Washington	1912	8
Wyoming	1968	15

NOTE.—This table is adapted from Magleby (1984).

“more of the initiative” than others. This turns out to be important below.<sup>2</sup> Table 1 lists the signature requirement to qualify a measure for the ballot in each state, taken from Magleby (1984).<sup>3</sup> About half the states also include a geographic requirement that limits the number of signatures that can be collected from any given region.

<sup>2</sup> The signature requirement (as a percentage) is used throughout the paper as a proxy for ease of use of the initiative. However, it may not be equally easy to collect the signatures of 5 percent of California voters and 5 percent of Montana voters. For one thing, there are more California voters, which means the absolute number of required signatures is greater. On the other hand, Montana voters are more dispersed geographically. To get an idea of how good a proxy the signature requirement is, I regressed the number of initiatives that qualified for the ballot in a state over 1950–80 ( $N$ ) on the inverse of the signature percentage ( $S$ ) for the 19 states that allowed voter initiatives during the sample period. The data were taken from Magleby (1984). The estimated regression is

$$N = 7.09 + 115.63 \times S, \quad R^2 = .308.$$

The  $t$ -statistic on the signature variable is 2.749.

<sup>3</sup> I follow Magleby in classifying North Dakota as a 2 percent state. However, prior to 1979, the signature requirement in North Dakota was expressed as an absolute number of signatures: 10,000 for a statute and 20,000 for a constitutional amendment.

Another difference between states concerns whether citizens are allowed to use initiatives to amend the state constitution or only pass laws. For the purposes of this study, the distinction is ignored because governmental finances, the focus of the study, rarely enjoy constitutional protection. Thus almost all aspects of a state's budget can be reached by statutory initiatives. If this study had concerned social legislation, then constitutional protections would be more important, and it would be desirable to distinguish between statutory and constitutional amendment initiatives. The signature requirements in table 1 refer to whichever is lower, the percentage for statutes or the one for constitutional amendments.

A weaker form of direct democracy is the *referendum*. The referendum allows citizens to approve or nullify laws originating in the state legislature. A referendum can reach the ballot either by citizen petition, as with initiatives, or by legislative referral. However, new legislation cannot be proposed by way of a referendum. For the purposes of this study, a state that provides for either type of referendum but not the initiative is classified as a noninitiative or pure representative state.

### III. Empirical Methods

The empirical methods are framed by the following model. Suppose that in each period  $t$ , state  $i$  chooses how much to spend on project  $j$ ,  $G_{it}^j$ , how much revenue to raise from source  $k$ ,  $R_{it}^k$ , and net borrowing,  $B_{it}$ , to satisfy an income constraint:

$$\sum_j G_{it}^j = \sum_k R_{it}^k + B_{it} + F_{it}. \quad (1)$$

Here  $F_{it}$  represents federal aid to state  $i$  in period  $t$ . The decision maker maximizes an objective function,  $U$ , written as

$$U = U(G_{it}^1, \dots, G_{it}^J, R_{it}^1, \dots, R_{it}^K, B_{it}, \mathbf{X}_{it}, \Theta_{it}), \quad (2)$$

where  $J$  is the number of expenditure categories,  $K$  is the number of revenue sources,  $\mathbf{X}_{it}$  is a vector representing exogenous decision factors, and  $\Theta_{it}$  captures the institutional context of the decision-making process, specifically whether and to what extent state  $i$  allows citizen initiatives in period  $t$ . For example, in the median voter model,  $U$  is the utility function of the median voter and  $\mathbf{X}_{it}$  are "preference" parameters. Alternatively, the decision maker is a vote-maximizing legislator and  $\mathbf{X}_{it}$  are the characteristics of his constituents. The formulation in (2) indicates that, in principle, the decision maker can be concerned with any category of spending and any revenue source, as well as net borrowing.

In each period  $t$ , the decision maker in state  $i$  chooses  $G_{it}^j$  for all  $j$  and  $R_{it}^k$  for all  $k$  to maximize  $U$  subject to the income constraint (1). This optimization process yields reduced-form functions for  $G_{it}^j$  and  $R_{it}^k$ , denoted  $G^{j*}(\Theta_{it}, \mathbf{X}_{it}, F_{it})$  and  $R^{k*}(\Theta_{it}, \mathbf{X}_{it}, F_{it})$ , respectively. Net borrowing is completely determined once spending and taxes are chosen. The expenditure functions,  $G^{j*}$ , are approximated by a linear function:

$$G_{it}^j = \alpha + \beta \Theta_{it} + \gamma \mathbf{X}_{it} + \delta F_{it} + u_{it}. \quad (3)$$

The revenue equations are defined analogously. In equation (3),  $\alpha$ ,  $\beta$ ,  $\gamma$ , and  $\delta$  are unknown parameters and  $u_{it}$  is an error term. The paper's approach essentially is to estimate equation (3) using ordinary least squares. If the initiative does not matter, then  $\beta = 0$ .

The basic regressions are estimated on a sample of 49 states ( $i = 1, \dots, 49$ ). Following other researchers, I omit Alaska, an outlier, from all calculations. The regressions span 30 years at 5-year intervals beginning in 1960 ( $t = 1960, 1965, \dots, 1990$ ). The vector  $\Theta_{it}$  contains a dummy variable indicating whether or not a state provides for voter initiatives, as well as a variable equal to the signature requirement for those states that do.

The vector  $\mathbf{X}_{it}$  contains six variables: (i) income, (ii) population density, (iii) metropolitan population, (iv) population growth rate, (v) value of mineral production, and (vi) a measure of the "conservativeness" of the state's U.S. senators. This is a deliberately short list but has considerable explanatory power. The four demographic variables are commonly used in the empirical literature on government budgets, are theoretically linked to the benefit and cost of government spending, and are statistically significant in most regressions in the paper. Income is perhaps the most well-known predictor of government size. The main theoretical reason to include it in the regressions is that the demand for a number of government services is believed to be positively related to income. Population density is theoretically linked to spending in two ways. First, a dense population may increase the marginal benefit of spending if it creates unique public good problems. Second, dense populations may present opportunities for economies of scale in production of government services. The third variable, the percentage of the state's population living in a metropolitan area, is included to control for potential differences between urban and rural areas in benefits of spending and costs of raising revenue. The fourth demographic variable is the growth rate of the population over the preceding four years. Population growth is expected to lead to a short-run demand for certain services that are typically provided by the government, particularly capital goods such as highways, streets, sewers, and schools.



Mineral production is included in the  $\mathbf{X}_i$  vector because the presence of large mineral deposits gives a state the option to shift its tax burden toward severance taxes. The political cost of such taxes is likely to be lower if mineral resources are owned by nonresidents. In addition, severance taxes may be less distortionary than income or sales taxes. Unlike the four demographic variables, mineral production is seldom used as a control in spending regressions.<sup>4</sup>

It turns out that these variables do a good job explaining the data. Still, it is unlikely that they completely capture constituent interests or the set of factors that determine the demand for spending. To try to capture residual preferences over fiscal outcomes, the last control is a variable that represents the ideology of the state's U.S. senators. The particular measure I use is based on the D-NOMINATE estimates of Poole and Rosenthal (1991); D-NOMINATE is an estimation procedure that provides a spatial location for each U.S. senator based on the senator's roll-call votes. Poole and Rosenthal report that roll-call voting behavior can be characterized for the most part by a one-dimensional model. Accordingly, I take the estimated first-dimension position of each senator and average it across a state's senators. With some caveats, this spatial position can be thought of as indicating where a senator lies on the liberal-conservative spectrum. Theoretically, the positions range from  $-1$  for the most liberal to  $+1$  for the most conservative.<sup>5</sup> This appears to be the most appropriate ideology variable for panel data. Unlike other commonly used measures such as Americans for Democratic Action ratings of senators or state votes for the Democratic candidate in the presidential election, the D-NOMINATE estimates are comparable across time as well as in the cross section. Still, this ideology variable differs in kind from the other controls; it may be partially endogenous, and its meaning is open to more than one interpretation. Some caution would seem to be in order, then, when introducing it into the  $\mathbf{X}_i$  vector.

Regression (3) suggests a number of econometric issues that I try to address. First, because panel data are used, a fixed time effect is included. This takes the form of a separate intercept for each year. It would not be informative to include fixed state effects for individual states because only three states changed their initiative status during the sample period. A dummy variable for the 11 southern states

<sup>4</sup> Abrams and Dougan (1986) argued that it should be used more often. The paper's findings do not change if this variable is excluded.

<sup>5</sup> Senators were matched to fiscal years as follows. The Eighty-sixth Congress was used for 1960, the Eighty-eighth for 1965, the Ninety-first for 1970, the Ninety-third for 1975, the Ninety-sixth for 1980, the Ninety-eighth for 1985, and the One-hundredth (the most recent available at the time of writing) for 1990.

that formed the Confederacy is included, however, to adjust for a particular well-known fixed effect.<sup>6</sup> It is hoped that these two fixed effects and the controls are sufficient to allow unbiased estimates of  $\beta$ . However, it is likely that the error term,  $u_{it}$ , remains heteroskedastic with respect to both time and states. To correct for this, White standard errors are used for all hypothesis tests. Because each regression includes the same set of explanatory variables, there is no gain to estimating them as a system of equations.

In regression (3), federal revenue transfers are taken to be exogenous with respect to state fiscal decisions. This is not a bad assumption for block grants and general revenue sharing, which annually constituted between 10 and 25 percent of federal aid during the period under consideration. It is less appropriate for categorical grants that are delivered in the form of matching funds (aid for highway construction, e.g.). However, the majority of categorical aid is awarded on the basis of formulas set by federal officials, by Congress in the case of formula grants, and by administrators in the case of project grants. Given that these formulas reflect political bargaining at the federal level (see Hale and Palley 1981), a state's ability to increase its federal aid by altering its fiscal behavior may be small. In any event, if federal aid is endogenous, its inclusion does not bias estimates of  $\beta$ .

A final issue before I turn to the regressions is the implied exogeneity of  $\Theta_{it}$ . One reason not to be overly concerned with the possibility that  $\Theta_{it}$  is endogenous is the fact that most states adopted their direct democracy laws more than 40 years before the start of the sample period. I have also estimated the regressions that follow after excluding states that adopted the initiative during the sample period (not reported), and the thrust of the results is the same.

The majority of the data were collected from *Governmental Finances*, a publication of the U.S. Bureau of the Census. Metropolitan population data were taken from *State and Metropolitan Area Data Book*, also published by the Bureau of the Census. Mineral production data came from *Minerals Yearbook*, a publication of the Bureau of Mines, for 1960–75. Mineral data for 1980–90 were constructed from information in *Minerals Yearbook* and *Statistical Abstract of the United States*. Initiative data were taken from Magleby (1984). A more detailed description of the data is given in the Appendix. Summary statistics on the controls and federal aid are reported in table 2. Here and throughout, all dollar values are expressed (i) in 1990 dollars using the consumer price index and (ii) in per capita terms.

<sup>6</sup> The estimated initiative effects are essentially the same if the southern states are simply deleted from the regressions instead.

**TABLE 2**  
**SUMMARY STATISTICS FOR CONTROL VARIABLES**

Year	Mean	Standard Deviation	Minimum		Maximum	
Income per Capita						
1960	9,132	1,872	5,179	Miss.	13,306	Del.
1970	11,469	1,774	7,956	Miss.	15,314	Conn.
1980	12,952	1,605	9,442	Miss.	16,093	Conn.
1990	16,539	2,781	11,944	Miss.	24,319	Conn.
Population Density per Square Mile						
1960	130	193	3	Nev.	817	N.J.
1970	148	221	3	Wyo.	960	N.J.
1980	157	223	5	Wyo.	986	N.J.
1990	169	235	5	Wyo.	1,035	N.J.
Metropolitan Population (% of Total)						
1960	60.40	24.25	12.76	S.Dak.	99.48	N.J.
1970	63.78	22.89	15.71	Idaho	100.00	N.J.
1980	63.80	22.34	18.33	Idaho	100.00	N.J.
1990	64.77	21.76	20.46	Idaho	100.00	N.J.
Growth Rate of Population over Previous Four Years (%)						
1960	6.93	6.80	-6.35	W.Va.	32.63	Fla.
1970	3.69	4.35	-5.08	N.Dak.	14.40	Wash.
1980	7.21	6.70	-2.91	N.Y.	30.98	Nev.
1990	2.27	5.55	-10.45	Wyo.	24.82	Nev.
Mineral Production per Capita						
1960	652	1,039	10	Del.	5,835	Wyo.
1970	767	1,337	10	Del.	7,159	Wyo.
1980	1,325	2,746	5	Del.	17,286	Wyo.
1990	942	2,009	11	Del.	12,432	Wyo.
D-NOMINATE Average for U.S. Senators						
1960	-.064	.277	-.544	Mich.	.609	Nebr.
1970	-.065	.249	-.526	Minn.	.596	Ariz.
1980	-.063	.246	-.514	Mich.	.508	Utah
1990	-.075	.286	-.598	Md.	.602	Idaho
Revenue from Federal Government per Capita						
1960	212	93	84	N.J.	662	Wyo.
1970	393	127	221	Ind.	855	Wyo.
1980	605	123	351	Ind.	928	Wyo.
1990	568	140	377	Fla.	1,174	Wyo.

NOTE.—Each row reports summary statistics for 49 states; Alaska is excluded from all calculations. Unless indicated otherwise, numbers are in 1990 dollars.

#### IV. Evidence on Expenditure

This section reports evidence on the central question: What effect, if any, does the initiative have on the level of government spending? As a backdrop to the investigation, table 3 presents summary statistics for various spending categories at 5-year intervals. These statistics provide a condensed history of state and local expenditure.

The first measure is direct general expenditure by state and local governments. This definition of spending includes education, welfare, hospitals, highways, airports, ports, transit subsidies, police and fire protection, prisons, parks, housing programs, sewers, government administration, and interest on general debt, in short, almost everything. The only excluded expenditures are utilities, liquor stores, employee retirement or other insurance trusts, and transfers to the federal government. State-local and local-state intergovernmental revenue transfers net out. Roughly 85 percent of all state and local spending is classified as direct general expenditure.

A steady upward trend in expenditure per capita can be seen, with a slowing from 1975 to 1985. Real spending over the full 30-year period increased by a factor of 2.5. In addition to a substantial variation in expenditure over time, a healthy cross-sectional variation is evident. The standard deviations hover at about 20 percent of the mean, and the maximum spending level is typically more than twice the minimum.

The table also reports separately the history of capital and current (noncapital) direct general expenditure. The current expenditure series echoes the pattern for total spending, but the capital outlay series is fairly stable. Relative to its mean, capital spending exhibits much more cross-sectional variation. The share of expenditure devoted to capital projects fell from roughly 30 percent in 1960 to 15 percent in 1990. The majority of capital spending was used to construct highways and school buildings.

Finally, the table presents summary statistics separately for state and local spending. The state spending series is state (only) general expenditure. Note that general expenditure is reported, not direct general expenditure as in the preceding series. The difference between the two definitions is that general expenditure includes state revenue transfers to local governments. Spending for utilities, liquor funds, and trusts is still excluded. The other series is local direct general expenditure less revenue from the state. The separate state and local series sum to the combined direct general expenditure series. Both state and local spending rose during the sample period, with a slowing from 1975 to 1985. A slight shift away from local spending can also be seen.

**TABLE 3**  
**STATE AND LOCAL EXPENDITURE PER CAPITA, 1960-90**

Year	Mean	Standard Deviation	Minimum	Maximum		
<b>State and Local Direct General Expenditure (Total)</b>						
1960	1,273	262	808	S.C.	2,006	Wyo.
1965	1,614	349	987	S.C.	2,637	Nev.
1970	2,129	417	1,539	S.C.	3,506	Hawaii
1975	2,517	450	1,767	Ark.	3,914	N.Y.
1980	2,531	400	1,904	Ark.	3,704	Wyo.
1985	2,772	536	2,059	Ark.	5,061	Wyo.
1990	3,233	561	2,276	Ark.	5,021	N.Y.
<b>State and Local Direct General Expenditure (Current)</b>						
1960	890	175	568	S.C.	1,303	Nev.
1965	1,180	228	768	S.C.	1,663	Calif.
1970	1,648	324	1,183	Ark.	2,564	N.Y.
1975	2,034	384	1,365	Ark.	3,304	N.Y.
1980	2,119	346	1,572	Ark.	3,163	N.Y.
1985	2,329	451	1,541	Utah	3,747	Wyo.
1990	2,745	481	2,048	Ark.	4,309	N.Y.
<b>State and Local Direct General Expenditure (Capital)</b>						
1960	382	110	212	N.J.	775	Wyo.
1965	434	152	219	S.C.	1,030	Nev.
1970	481	133	335	S.C.	1,130	Hawaii
1975	483	138	260	R.I.	984	Hawaii
1980	412	135	244	Mass.	994	Wyo.
1985	443	214	248	Maine	1,314	Wyo.
1990	489	154	229	Ark.	933	Ariz.
<b>State General Expenditure</b>						
1960	739	206	374	N.J.	1,453	Wyo.
1965	955	258	506	N.J.	1,689	Wyo.
1970	1,321	360	856	Ohio	2,916	Hawaii
1975	1,599	319	1,132	Mo.	3,030	Hawaii
1980	1,641	311	1,140	Fla.	2,523	Hawaii
1985	1,677	372	1,155	N.H.	3,081	Wyo.
1990	2,086	438	1,391	Tex.	3,270	N.Y.
<b>Local Direct General Expenditure Minus State Aid</b>						
1960	533	160	227	S.C.	925	N.Y.
1965	659	205	275	S.C.	1,260	Nev.
1970	808	221	436	W.Va.	1,412	N.Y.
1975	918	282	468	Ark.	1,832	N.Y.
1980	890	235	517	Ky.	1,593	N.Y.
1985	1,094	285	580	Ky.	1,980	Wyo.
1990	1,147	317	605	Ark.	2,281	N.Y.

NOTE.—Each row reports summary statistics for 49 states; Alaska is excluded from all calculations. All numbers are in 1990 dollars.

Table 4 reports estimates of equation (3) using the first series, state and local direct general expenditure, as the dependent variable. Each column in the table is a regression. Coefficient estimates for the fixed year effects are not presented. It is natural to begin by considering consolidated state and local expenditure because initiatives can be and have been used to alter the behavior of local as well as state governments. For example, California's Proposition 13 restricted local property taxes.

Before I focus on the initiative variables and the specifics of columns 1–5, several broad observations can be made about the regressions and the controls. First, the regressions account for 93 percent of the variation in expenditure, almost all of it, suggesting that the fundamental explanatory variables have been captured. Second, income per capita is highly significant in all regressions: each dollar of income raised spending by about 13 cents. Income was by far the most important determinant of spending. This can be seen from a simple regression of expenditure on income and the year dummies alone (not reported). The  $R^2$  of such a regression using the full sample is .779. Thus income and the time dummies alone can explain over three-quarters of the variation in spending over the sample period. This pattern, which recurs throughout the paper, helps to put the results that follow in perspective. It shows that the initiative is not the primary determinant of fiscal policy.

Population density and metropolitan population were negatively and positively associated with spending, respectively. The coefficients on these controls are statistically significant at the 1 percent level in all regressions. Each person per square mile reduced expenditure at the margin by about 30 cents, suggesting that it may have been less expensive to provide government services to people in dense areas. An additional 1 percent of the population residing in a metropolitan area, on the other hand, led to roughly \$3.00 more expenditure per capita, suggesting that cities made demands on government services beyond those of rural areas. The correlation between population density and the percentage of the population that is metropolitan is less than might be expected: in the cross section, one series can explain about 30 percent of the variation of the other. Population growth and mineral production were positively related to spending, but the coefficients generally do not achieve statistical significance.

Federal aid, not surprisingly, was linked to greater state and local spending. Expenditure rose by more than \$2.00 for each dollar of federal revenue. Federal revenue appears to have been a complement to rather than a substitute for state and local revenue. This is the oft-noted "flypaper effect" (Rubinfeld 1987). As discussed above, it may be prudent not to take this coefficient at face value.

TABLE 4  
STATE AND LOCAL DIRECT GENERAL EXPENDITURE REGRESSIONS

VARIABLE	REGRESSION				
	(1)	(2)	(3)	(4)	(5)
Dummy = 1 if initiative state	-62.28*** (26.46)	-60.00** (26.39)	-134.86** (64.30)	-126.10* (67.71)	-127.96* (68.64)
Signature requirement	...	...	10.47 (8.19)	7.00 (8.60)	5.13 (8.65)
Income per capita	.126*** (.010)	.126*** (.010)	.128*** (.011)	.130*** (.011)	.133*** (.011)
Population density	-.303*** (.060)	-.292*** (.060)	-.299*** (.060)	-.324*** (.058)	-.287*** (.064)
Metropolitan population (% of total)	3.00*** (.72)	3.08*** (.73)	3.04*** (.72)	2.87*** (.70)	2.42*** (.74)
Growth rate of population over previous four years	3.94* (2.10)	3.54 (2.19)	2.69 (2.23)	3.01 (2.47)	1.18 (2.27)
Mineral production per capita	.016 (.010)	.015 (.010)	.011 (.010)	.004 (.011)	.002 (.011)
D-NOMINATE average for U.S. senators	...	40.76 (46.15)	31.08 (45.86)	22.93 (43.64)	15.24 (44.66)
Revenue from federal government per capita	2.27*** (.17)	2.29*** (.17)	2.29*** (.17)	2.40*** (.16)	2.29*** (.18)
Dummy = 1 if southern state	-110.90*** (32.82)	-111.69*** (32.82)	-107.31*** (33.08)	-103.21*** (33.99)	-82.24*** (33.08)
Dummy = 1 if western state	...	...	...	...	77.58** (35.74)
$R^2$	.932	.932	.933	.935	.935
$\bar{R}^2$	.929	.929	.929	.931	.932
Observations	343	343	343	336	336

NOTE.—Each column is a regression. The dependent variable is combined state and local direct general expenditure. Variables with dollars as units are expressed in 1990 dollars. Alaska is excluded from all regressions. Wyoming is excluded from regressions 4 and 5. The data are pooled from 1960 to 1990. In parentheses beneath each coefficient is the (White) standard error. All regressions were run with seven year-specific fixed effects; those coefficients are not reported.

\* Significant at the 10 percent level.  
 \*\* Significant at the 5 percent level.  
 \*\*\* Significant at the 1 percent level.

Columns 1 and 2 report regressions of state and local direct general expenditure on the controls and a dummy variable equal to one if state  $i$  provided for the initiative in year  $t$  according to table 1.<sup>7</sup> As discussed above, some caution is called for with respect to the D-NOMINATE variable, so regression 1 is estimated without it and regression 2 with it. In both regressions, the coefficient on the initiative dummy variable indicates that initiative states spent \$60 per capita less than pure representative states. Both coefficient estimates are statistically significant. The D-NOMINATE control is statistically insignificant and does not materially alter the other coefficients. This is the general pattern with respect to the D-NOMINATE variable throughout the paper, but I retain it in all subsequent regressions for the interested reader.

The regression in column 3 is the same as in column 2 except that a variable is introduced equal to the signature requirement for an initiative state and zero otherwise, as per table 1.<sup>8</sup> The signature requirement variable is effectively an interaction term between the initiative dummy and the signature requirement. This regression allows for different degrees of the initiative. As discussed above, it is plausible to suppose that a state with a 5 percent signature requirement has "more of the initiative" than a state with a 10 percent signature requirement. If, as columns 1 and 2 indicate, the initiative reduced expenditure, then the coefficient on the initiative dummy should be negative and the coefficient on the signature requirement should be positive.

This is the pattern observed. The coefficient on the dummy variable remains negative and statistically significant. The coefficient on the signature requirement variable is positive but does not achieve statistical significance at conventional levels ( $p = .201$ ). The significance of these coefficients individually is less important than their combined effect. To determine whether the voter initiative matters in column 3, it is necessary to test whether linear combinations of the coefficients are different from zero. Table 5 provides this information under the heading 3'. The main entry in column 3' reports the full effect of the initiative given a signature requirement (relative to pure representative government). These values are calculated in the usual way by adding the dummy coefficient to the signature coefficient weighted by the particular signature requirement. In parentheses beneath each value is the estimated standard error.

<sup>7</sup> There is some question whether Illinois should be counted as an initiative state because state courts have made the initiative inutile in practice by restricting it to amendments of a single technical clause of the state's constitution. In any case, the substance of the findings is invariant to the classification of Illinois.

<sup>8</sup> There is one elaboration: California's signature requirement was 8 percent until 1966.



TABLE 5  
EFFECT OF THE INITIATIVE ON STATE AND LOCAL DIRECT GENERAL  
EXPENDITURE BY SIGNATURE REQUIREMENT

Signature Requirement	(3')	(4')	(5')
2%	-113.92** (49.82)	-112.10** (52.04)	-117.70** (52.88)
3	-103.44** (43.09)	-105.10** (44.64)	-112.57** (45.43)
4	-92.97** (36.95)	-98.10*** (37.74)	-107.45*** (38.47)
5	-82.50*** (31.75)	-91.10*** (31.70)	-102.32*** (32.34)
6	-72.02*** (28.02)	-84.10*** (27.07)	-97.19*** (27.60)
7	-61.55** (26.38)	-77.10*** (24.68)	-92.07*** (25.04)
8	-51.08* (27.22)	-70.10*** (25.17)	-86.94*** (25.33)
9	-40.60 (30.33)	-63.10** (28.39)	-81.81*** (28.39)
10	-30.13 (35.11)	-56.10* (33.56)	-76.68** (33.47)
15	22.24 (69.35)	-21.09 (70.35)	-51.05 (70.25)

NOTE.—The main entry indicates the effect of the initiative on per capita combined state and local direct general expenditure given a signature requirement relative to a state that has no initiative. The number at the top of each column (without the prime) indicates the regression in table 4 that is used to calculate the numbers. Standard errors are in parentheses beneath each estimate.

\* Significance level for the hypothesis that the marginal effect is zero is 10 percent.

\*\* Significance level for the hypothesis that the marginal effect is zero is 5 percent.

\*\*\* Significance level for the hypothesis that the marginal effect is zero is 1 percent.

The main thing to note under 3' is that the estimated initiative effect is significantly negative for signature requirements below 9 percent. The most common signature requirement is 5 percent. According to the point estimate, in such a state expenditure was \$82.50 per capita lower than in an otherwise identical pure representative state. This value is significantly different from zero at better than the 1 percent level. To put this estimate in perspective, note that spending averaged a little less than \$2,300 per person during the sample period. With this as a benchmark, the initiative reduced expenditure by approximately 4 percent. When a two-standard-deviation rule was applied, the "true" initiative effect in such a state ranged from -\$19 to -\$146 or, in percentage terms, -1 percent to -6 percent.<sup>9</sup>

<sup>9</sup> I also estimated this and subsequent regressions using splines for various regions of the signature requirement variable. These regressions gave broadly similar results, suggesting that the linear parameterization reported does not obscure any important variations.

All initiative states have a signature requirement between 2 and 10 percent except for Wyoming at 15 percent. This implies that estimated effects outside the 2–10 percent range should be taken with a grain of salt. In addition, the slope of the signature requirement line is particularly sensitive to the data for Wyoming. Scanning over the summary statistics in tables 2 and 3 gives some reason to be concerned about this because Wyoming is the extreme observation for a disproportionate number of series. In at least one sample year, the state was the least dense, had the slowest-growing population, enjoyed the most mineral production, received the most revenue from the federal government, and spent the most by any of the six expenditure measures. Complicating matters is the fact that Wyoming adopted the initiative during the sample period. Another obdurate fact is that although the initiative was available in the state from 1968 to 1990, it was never used. All of this suggests that the regressions might achieve a better fit, particularly in the estimate of the signature requirement coefficient, if the Wyoming observations are removed.

This is done in the regression reported in column 4 of table 4 and summarized under the heading 4' in table 5. The sense of the estimates is the same in columns 3 and 4, but the initiative effects are slightly more pronounced in the latter. According to 4', a state with a 5 percent signature requirement spent \$91.10 less than a comparable pure representative state. The two-standard-deviation bounds on this estimate are  $-\$28$  to  $-\$154$ , or  $-1$  percent to  $-7$  percent. In addition, the initiative coefficients are statistically significant for all signature requirement levels in the sample. Given the concerns with the Wyoming observations, they are excluded from the remaining regressions reported in the paper. Exclusion of these observations does not affect the sign of the initiative coefficients, but slightly improves the fit of the regressions.

The final regression in table 4, reported in column 5, is the same as that reported in column 4 except for the addition of a dummy variable for western states.<sup>10</sup> There is not a strong theoretical reason to include this variable, nor is there much of an empirical precedent in the literature, but it serves as a check on the possibility that the initiative effects are actually western effects. This is an issue of some concern given the clustering of initiative states in the west. Column 5 shows that inclusion of the western dummy does not materially change the coefficients, nor does it alter the main conclusions. Indeed, as can be seen in column 5' of table 5, the estimated initiative

<sup>10</sup> Following Census Bureau classifications, the western states are Alaska, Arizona, California, Colorado, Hawaii, Idaho, Montana, Nevada, New Mexico, Oregon, Utah, Washington, and Wyoming.

effects become larger and their statistical significance increases. Thus it is unlikely that the initiative variables are proxying for a western effect.<sup>11</sup> In Section VI, I return to the related question whether they are proxying for state ideology.

Tables 4 and 5 document that overall expenditure was lower in initiative states than in otherwise similar pure representative states. The next two tables look for initiative effects in the composition of spending. The regressions are reported in table 6, and the full initiative effects by signature requirement are reported in table 7. Two breakdowns are investigated: current spending versus capital spending, and state spending versus local spending.

The first set of regressions, for current and capital expenditure, serve primarily as a check on the results in tables 4 and 5. There are two reasons to expect the initiative to have different effects on current and capital spending. First, and perhaps most important, many capital projects are large enough to require debt financing. As of 1990, 22 states required direct voter approval of debt issues (13 of them were initiative states), which effectively made them initiative states with respect to capital outlay. In addition, 24 states had constitutional limits on the amount of debt that could be issued (Kiewiet and Szakaly 1994). Second, capital spending tends to be lumpy. This creates bubbles in the data that do not accurately represent the true flow of services provided by the expenditure. Capital and, by extension, total direct general expenditure, then, are likely to be afflicted by "measurement error" (Peltzman 1992). For both these reasons, noncapital direct general expenditure is likely to give more precise estimates of the effect of the initiative. The first reason and, to some extent, the second also suggest that there should be little observable effect of the initiative on capital outlay. In addition, it is possible that the initiative has a different impact on the decision-making process for current spending than capital spending, as might be the case, for example, if legislatures tended to deliver more of one type of spending than constituents wanted.

In fact, a pronounced initiative effect can be seen for current spending (cols. 1, 2, and 2'), but not for capital spending (cols. 3, 4, and 4'). The initiative dummy is negative and statistically significant in column 1, and the estimated effects by signature requirement in column 2' are significantly negative in all cases. For a state with a 5 percent signature requirement, the point estimate indicates that current expenditure was \$81.12 per capita less than a comparable state with a pure representative form of government. This implies about

<sup>11</sup> All regressions reported in the paper were also estimated with a western dummy included; there were no substantive changes in the initiative coefficients.

TABLE 6  
STATE AND LOCAL EXPENDITURE REGRESSIONS

VARIABLE	STATE AND LOCAL DIRECT GENERAL EXPENDITURE (Current)			STATE AND LOCAL DIRECT GENERAL EXPENDITURE (Capital)			STATE GENERAL EXPENDITURE		LOCAL DIRECT GENERAL EXPENDITURE MINUS STATE AID	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)		
Dummy = 1 if initiative state	-70.05*** (21.06)	-110.55** (55.76)	-7.88 (13.12)	-15.55 (35.25)	-152.68*** (26.32)	-240.68*** (62.50)	74.76*** (22.92)	114.58** (56.20)		
Signature requirement	...	5.89 (7.11)	...	1.11 (4.72)	...	12.79* (7.49)	...	-5.79 (6.59)		
Income per capita	.105*** (.010)	.107*** (.010)	.023*** (.007)	.024*** (.007)	.046*** (.010)	.050*** (.010)	.082*** (.011)	.081*** (.011)		
Population density	-.162*** (.54)	-.162*** (.54)	-.162*** (.030)	-.162*** (.030)	-.107 (.070)	-.108 (.069)	-.216*** (.064)	-.216*** (.064)		
Metropolitan population (% of total)	2.92*** (.73)	2.87*** (.72)	.01 (.55)	.00 (.54)	.78 (.78)	.67 (.76)	2.15*** (.77)	2.20*** (.75)		
Growth rate of population over previous four years	-4.34** (2.19)	-4.93** (2.34)	8.06*** (1.02)	7.95*** (.98)	4.62* (2.73)	3.35 (2.80)	-.91 (2.16)	-.33 (2.36)		
Mineral production per capita	-.003 (.011)	-.003 (.011)	.007* (.004)	.007* (.004)	.013 (.010)	.014 (.010)	-.009 (.008)	-.010 (.008)		
D-NOMINATE average for U.S. senators	-17.37 (42.51)	-19.23 (42.20)	42.51 (28.17)	42.16 (27.99)	-48.90 (41.37)	-52.93 (40.95)	74.04* (42.59)	75.86* (42.04)		
Revenue from federal government per capita	1.96*** (.13)	1.97*** (.13)	.43*** (.09)	.43*** (.09)	2.09*** (.16)	2.11*** (.16)	.30* (.17)	.29* (.17)		
Dummy = 1 if southern state	-80.32*** (29.44)	-75.93*** (30.01)	-28.11 (19.67)	-27.28 (18.43)	-112.66*** (28.65)	-103.13*** (29.04)	4.23 (28.09)	-.08 (28.71)		
R <sup>2</sup>	.940	.940	.460	.460	.870	.871	.712	.713		
R <sup>2</sup>	.937	.937	.435	.433	.864	.865	.699	.699		

NOTE.—Each column is a regression. The dependent variable is given at the top of each column. Variables with dollars as units are expressed in 1990 dollars. Alaska and Wyoming are excluded from all calculations. The data are pooled from 1960 to 1990. Each regression has 336 observations. In parentheses beneath each coefficient is the (White) standard error. All regressions were run with seven year-specific fixed effects; those coefficients are not reported.

\* Significant at the 10 percent level.  
 \*\* Significant at the 5 percent level.  
 \*\*\* Significant at the 1 percent level.

TABLE 7  
EFFECT OF THE INITIATIVE ON DIRECT GENERAL EXPENDITURE CATEGORIES BY SIGNATURE REQUIREMENT

Signature Requirement	State and Local Direct General Expenditure (Current) (2')	State and Local Direct General Expenditure (Capital) (4')	State General Expenditure (6')	Local Direct General Expenditure Minus State Aid (8')
2%	-98.78** (42.91)	-13.32 (26.72)	-215.11*** (49.25)	103.00** (44.37)
3	-92.89** (36.87)	-12.21 (22.72)	-202.32*** (43.05)	97.22** (38.79)
4	-87.01*** (31.28)	-11.09 (19.06)	-189.53*** (37.33)	91.43*** (33.57)
5	-81.12*** (26.42)	-9.98 (15.97)	-176.75*** (32.34)	85.65*** (28.92)
6	-75.24*** (22.78)	-8.86 (13.83)	-163.96*** (28.47)	79.86*** (25.15)
7	-69.35*** (20.99)	-7.75 (13.12)	-151.17*** (26.22)	74.07*** (22.71)
8	-63.46*** (21.53)	-6.63 (14.06)	-138.39*** (26.01)	68.29*** (22.05)
9	-57.58** (24.23)	-5.52 (16.36)	-125.60*** (27.89)	62.50*** (23.31)
10	-51.69* (28.50)	-4.40 (19.55)	-112.81*** (31.48)	56.72** (26.22)

NOTE.—The main entry indicates the effect of the initiative on the expenditure category indicated at the head of the column given a signature requirement relative to a state that has no initiative. The number at the top of each column (without the prime) indicates the regression in table 6 that is used to calculate the numbers. Standard errors are in parentheses beneath each estimate.

- \* Significance level for the hypothesis that the marginal effect is zero is 10 percent.
- \*\* Significance level for the hypothesis that the marginal effect is zero is 5 percent.
- \*\*\* Significance level for the hypothesis that the marginal effect is zero is 1 percent.

a 4 percent reduction in current spending from a sample average level of about \$1,850.

A comparison of column 4' in table 5 and column 2' in table 7 reveals that the magnitude of the initiative effects is roughly the same for total spending and current spending, but the estimates are somewhat more precise when capital spending is removed. In addition, the  $R^2$  for current expenditure is slightly higher and the  $R^2$  for capital spending is substantially lower than the  $R^2$  for total direct general expenditure. Thus there is some support for the measurement error interpretation.

The initiative coefficients in the capital expenditure regressions are small and do not approach statistical significance, although the point estimates indicate that the initiative had a mild depressing effect on capital spending. The story is the same for the effects by signature requirement in column 4'. This is consistent with the idea that direct

bond approval and borrowing limits equalized behavior when it came to debt. The overall pattern gives some support to the assumption that we are observing initiative effects in tables 4 and 5 rather than some other effect that is proxied by the initiative. However, the results are also consistent with the interpretation that citizens used the initiative to cut current spending but not capital spending, or that legislatures spent the right amount on capital but more than the electorate wanted on noncapital projects. This issue is pursued in Section VII.

It is interesting to compare the coefficients on the control variables in columns 1 and 2 with those in columns 3 and 4. Plausibly, population growth led to an increase in capital spending and a smaller decrease in noncapital spending, indicating that growing populations led states to partially substitute expenditure from current to capital projects. Less obviously, mineral production seems to have spurred capital expenditure but had no effect on noncapital expenditure. Density reduced expenditure in both categories by equal amounts, but metropolitan populations put pressure only on current expenditure. Federal aid stimulated spending on both current and capital projects, but had a particularly large effect on the latter.

The final set of expenditure regressions investigate how the initiative affected the division of spending between state and local governments. Columns 5 and 6 report regressions in which the dependent variable is state (only) general expenditure. Recall that state general expenditure is equal to state direct general expenditure plus state transfers to local governments. Intergovernmental transfers are a huge part of state budgets: in 1990, states transferred \$172 billion in aggregate to local governments compared to a direct expenditure of \$397 billion. The reason general expenditure is used rather than direct general expenditure is that general expenditure gives a more accurate picture of the resources under control of the state government. This is not a material decision: it turns out that the results are similar using direct general expenditure. The dependent variable in columns 7 and 8 is local direct general expenditure minus revenue transferred from the state.

The regressions reveal that the initiative had a sizable negative effect on general expenditure at the state level and a positive effect on expenditure by local governments. The dummy coefficient in column 5 indicates that state governments in initiative states spent, on average, \$152.68 per capita less than their pure representative counterparts. The coefficient is significant at better than the 1 percent level. Column 6' in table 7 gives the corresponding effects for column 6. A state with a 5 percent signature requirement spent \$176.75 per person less than an otherwise identical pure representative state.

Compared to an average state general expenditure of about \$1,430 per capita, this implies that the initiative cut back state spending by roughly 12 percent. The initiative effect is significantly negative for all signature requirement levels in the table.

In contrast, the coefficient on the dummy variable in regression 7 indicates that local expenditure less state aid in initiative states was \$74.76 per capita higher. This estimate and the estimates by signature requirement under 8' in table 7 are all statistically distinguishable from zero. A state with a 5 percent signature requirement spent \$85.65 per person more at the local level than a pure representative state, a 10 percent increase from the average expenditure level of \$865.

The state-level cutbacks in 6' exceed in magnitude the combined state and local cutbacks in 2' and 4'. This finding is striking. Given that the initiative led to lower combined state and local spending, it might have been expected that both state and local expenditure were reduced. To the contrary, initiative states *increased* local expenditure at the same time they cut state expenditure. It is sometimes "explained" that initiatives drive up local expenditure by forcing cuts at the state level, but given that the initiative can and does influence spending at both levels, this begs the question why voters prefer such an outcome. Section VII discusses this issue in greater depth.

The coefficients on the controls reveal several differences between state and local spending. Population density and metropolitan populations primarily affected local expenditure, the former negatively and the latter positively. The additional spending that accompanied a growing population tended to come from the state. State expenditure rose \$2.00 for every dollar of federal aid, compared to only 30 cents per dollar for local expenditure.

To summarize, this section documents three effects that the availability of the citizen initiative appears to have had on government expenditure. First, the initiative seems to have caused a reduction in combined state and local government expenditure. Second, the initiative was associated with large reductions in current expenditure and small or no reductions in capital expenditure. Third, the initiative led states to shift spending away from state and toward local government. In addition, the magnitude of all estimated initiative effects was largest for states with relatively easy ballot access.

## V. Evidence on Finance

The preceding section investigated the relation between the initiative and expenditure. This section investigates how the initiative affected the way in which expenditure was financed. Given a level of spending,

TABLE 8

STATE AND LOCAL REVENUE AND NET DEBT PER CAPITA, 1960-90

Year	Mean	Standard Deviation	Minimum	Maximum
General Revenue				
1960	1,036	218	649 Ky.	1,524 Nev.
1965	1,296	251	833 Ark.	1,825 N.Y.
1970	1,703	331	1,137 Ark.	2,587 N.Y.
1975	1,957	366	1,310 Ark.	3,068 N.Y.
1980	2,026	396	1,431 Ark.	3,373 Wyo.
1985	3,002	667	2,017 Ark.	5,977 Wyo.
1990	3,400	618	2,177 Ark.	5,235 N.Y.
Tax Revenue				
1960	846	180	519 Ala.	1,270 N.Y.
1965	1,034	208	662 Ark.	1,544 N.Y.
1970	1,335	292	848 Ark.	2,197 N.Y.
1975	1,491	309	984 Ark.	2,490 N.Y.
1980	1,471	306	1,025 Miss.	2,371 N.Y.
1985	1,678	388	1,115 Miss.	3,133 Wyo.
1990	1,881	389	1,264 Miss.	3,267 N.Y.
Charges and Miscellaneous Revenue				
1960	190	62	96 R.I.	375 Wyo.
1965	262	80	138 R.I.	512 N.Dak.
1970	368	102	219 Maine	699 Wyo.
1975	465	114	293 Maine	768 N.Dak.
1980	555	168	322 Maine	1,154 Wyo.
1985	1,325	379	817 Maine	2,844 Wyo.
1990	1,519	356	903 Ark.	2,628 Wyo.
Change in Face Value of Outstanding Debt from Previous Year				
1960	117	73	15 R.I.	282 Miss.
1965	165	186	-157 Tenn.	1,134 Wash.
1970	152	123	-84 Wyo.	473 Vt.
1975	178	146	-75 Mont.	669 Oreg.
1980	330	260	-17 N.Y.	1,284 Wyo.
1985	393	742	-192 Va.	5,307 N.Dak.
1990	194	189	-181 Wyo.	549 Tenn.

NOTE.—Each row reports summary statistics for 49 states for the indicated year, except for net debt in 1960, which is calculated using 48 states. All numbers are in 1990 dollars. Alaska is excluded from all calculations, and Hawaii is excluded from net debt calculations for 1960.

the revenue to pay for it must come from the federal government or from own sources in the form of taxes or charges, or must be borrowed. Table 8 presents a condensed history of state and local finance. The first series is general revenue from own sources. This includes all government revenue except revenue from liquor stores, utilities, and income trust funds. In 1990, general revenue constituted



over 80 percent of revenue from own sources. In the second and third series, general revenue is broken down into two components, (i) tax revenue and (ii) charges and miscellaneous revenue. Taxes are defined to be compulsory contributions exacted by a government for public purposes. Most tax revenue came from property, sales, and income taxes. Charge revenue was received as payment for specific services, including fees and assessments. The two largest revenue sources classified as charges were tolls from roads and tuition payments for education. Miscellaneous revenue includes interest earnings and special assessments.

It can be seen that general revenue tracked direct general expenditure during the sample period. This means only that net borrowing tended to be zero on average. Net borrowing must be zero in the long run; perhaps 30 years is the long run. The general revenue series appears to be a little more variable relative to its mean than the direct general expenditure series, especially after 1980. A dramatic change over time in the revenue mix is evident, a shift toward charges and away from taxes. Over the entire 30-year period, taxes grew by 122 percent and charges and miscellaneous revenue grew by 699 percent. Put differently, in 1960, roughly 80 cents out of every general revenue dollar came from taxes. By 1990, only 55 cents out of every general revenue dollar came from taxes. The biggest change in the revenue mix occurred between 1975 and 1985, during which time taxes grew by 13 percent and charges grew by 185 percent. This period included the passage of California's Proposition 13 in 1978 and the high-water mark of the tax revolt movement that it engendered.

The last series is the change in the face value of outstanding state and local debt from the previous year, called "net debt" for short. For example, net debt in 1990 is calculated by subtracting total debt in 1990 from total debt in 1989. Net debt is approximately equal to net borrowing for a given year. The difference is that net debt does not include changes in cash holdings. Most states have balanced-budget requirements, but generally they apply to the operating budget and do not preclude issuance of debt. Not surprisingly, there is not much of a trend in the net debt series. The thing that stands out about net debt is its variability, both over time and in the cross section. Apparently, states tended to use debt as a buffer in order to smooth expenditure and revenue.

Table 9 presents the revenue regressions. This table is the analogue to tables 4 and 6. Each column is a regression. The dependent variable is indicated at the top of each column. Table 10 reports the initiative effect by signature requirement as in tables 5 and 7. The preceding section shows that expenditure was lower in initiative

TABLE 9  
STATE AND LOCAL REVENUE AND NET DEBT REGRESSIONS

VARIABLE	GENERAL REVENUE			TAX REVENUE		CHARGES AND MISCELLANEOUS			NET DEBT	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)		
Dummy = 1 if initiative state	-59.95* (31.57)	-58.02 (83.93)	-63.74*** (18.41)	-217.55*** (47.11)	8.79 (21.21)	159.53*** (57.01)	31.46 (40.12)	277.31 (277.49)		
Signature requirement	...	.45 (10.75)	...	22.35*** (5.65)	...	-21.90*** (7.60)	...	-35.75 (35.33)		
Income per capita	.116*** (.014)	.116*** (.015)	.102*** (.008)	.108*** (.008)	.014 (.010)	.008 (.010)	.049* (.025)	.039*** (.016)		
Population density	-.476*** (.072)	-.476*** (.072)	-.161*** (.050)	-.163*** (.051)	-.314*** (.046)	-.314*** (.047)	.026 (.068)	.029 (.069)		
Metropolitan population (% of total)	5.34*** (.99)	5.34*** (.98)	2.57*** (.59)	2.38*** (.57)	2.77*** (.69)	2.95*** (.69)	-1.62 (1.77)	-1.32 (1.47)		
Growth rate of population over previous four years	-.36 (3.13)	-.41 (3.28)	-2.19 (1.93)	-4.42** (2.04)	1.83 (1.81)	4.02** (1.87)	5.70** (1.92)	9.27*** (3.48)		
Mineral production per capita	.010 (.011)	.010 (.011)	-.007 (.007)	-.005 (.007)	.016** (.008)	.015* (.008)	.029 (.028)	.027 (.025)		
D-NOMINATE average for U.S. senators	24.11 (52.10)	23.97 (52.12)	-44.07 (33.85)	-51.12 (32.83)	68.18* (34.81)	75.08** (34.93)	24.47 (34.92)	35.61 (36.73)		
Revenue from federal government per capita	1.57*** (.20)	1.57*** (.21)	1.06*** (.13)	1.11*** (.13)	.51*** (.11)	.46*** (.11)	.45** (.20)	.37*** (.15)		
Dummy = 1 if southern state	-141.82*** (40.72)	-141.48*** (42.68)	-100.15*** (22.76)	-83.49*** (23.04)	-41.66 (28.33)	-57.99** (29.10)	58.88 (54.21)	32.10 (34.54)		
$R^2$	.926	.926	.895	.899	.900	.903	.147	.166		
$\bar{R}^2$	.922	.922	.890	.894	.895	.898	.106	.124		

NOTE.—Each column is a regression. The dependent variable is given at the top of each column. Variables with dollars as units are expressed in 1990 dollars. Alaska and Wyoming are excluded from all calculations and Hawaii in 1960 is excluded from the net debt regressions. The data are pooled from 1960 to 1990. The regressions in cols. 1-6 have 336 observations; the regressions in cols. 7 and 8 have 335 regressions. In parentheses beneath each coefficient is the (White) standard error. All regressions were run with seven year-specific fixed effects; those coefficients are not reported.

\* Significant at the 10 percent level.

\*\* Significant at the 5 percent level.

\*\*\* Significant at the 1 percent level.

TABLE 10

EFFECT OF THE INITIATIVE ON REVENUE AND NET DEBT BY SIGNATURE REQUIREMENT

Signature Requirement	General Revenue (2')	Tax Revenue (4')	Charges and Miscellaneous (6')	Net Debt (8')
2%	-57.13 (66.04)	-172.85*** (36.86)	115.73*** (43.24)	205.81 (207.10)
3	-56.68 (55.33)	-150.05*** (32.01)	93.82** (36.77)	170.06 (171.99)
4	-56.24 (46.87)	-128.15*** (27.47)	71.91** (30.83)	134.31 (136.98)
5	-55.79 (39.53)	-105.80*** (23.41)	50.01* (25.76)	98.56 (102.20)
6	-55.34 (34.06)	-83.45*** (20.14)	28.11 (22.19)	62.81 (67.98)
7	-54.89* (31.44)	-61.10*** (18.09)	6.20 (20.90)	27.06 (35.96)
8	-54.45* (32.38)	-38.74** (17.68)	-15.70 (22.28)	-8.70 (21.48)
9	-54.00 (36.60)	-16.39 (19.03)	-37.61 (25.92)	-44.45 (46.10)
10	-53.55 (43.15)	5.96 (21.81)	-59.51* (31.02)	-80.20 (79.28)

NOTE.—The main entry indicates the effect of the initiative on per capita revenue or net debt given a signature requirement relative to a state that has no initiative. The financial category is indicated at the head of each column. The number at the top of each column (without the prime) indicates the regression in table 9 that is used to calculate the numbers. Standard errors are in parentheses beneath each estimate.

\* Significance level for the hypothesis that the marginal effect is zero is 10 percent.

\*\* Significance level for the hypothesis that the marginal effect is zero is 5 percent.

\*\*\* Significance level for the hypothesis that the marginal effect is zero is 1 percent.

states. The adding-up condition (1) implies that general revenue must have been lower as well, as long as net borrowing was zero on average and federal aid is held constant. Thus general revenue regressions do not offer the potential for much new information, except perhaps a crude sense of the appropriateness of the zero net borrowing condition. Even so, for completeness, columns 1 and 2 report regressions in which the dependent variable is general revenue. The initiative coefficient in column 1 is negative and statistically different from zero at the 10 percent level. The magnitude of the coefficient is similar to that of the initiative coefficient in the total expenditure regressions (cols. 1 and 2 of table 4). The effects by signature requirement under 2' in table 10 are negative for all signature levels and marginally significant. Again these estimates are comparable to those for total expenditure (table 5), but the standard errors are quite a bit larger. In all, the point estimates of the initiative coefficients in the general revenue regressions are not inconsistent with adding up and zero net borrowing, but the size of the standard errors precludes statistical

confidence. This suggests that (i) the underlying revenue series is more variable than the expenditure series, or (ii) 30 years is too short a period for the zero net borrowing condition to apply.

The main question of interest is whether and to what extent the initiative affects the way expenditure is financed. The remaining columns of tables 9 and 10 report separate estimates for the three revenue sources. The dependent variable is tax revenue in columns 3, 4, and 4', charges and miscellaneous revenue in columns 5, 6, and 6', and net debt in columns 7, 8, and 8'.

It is evident that the initiative had a pronounced effect on the finance mix. The tax regressions show that initiative states raised less revenue from taxes than pure representative states. All three initiative coefficients in columns 3 and 4 are significant at better than the 1 percent level. The coefficient on the dummy variable in column 3 indicates that initiative states raised \$63.74 per capita less in taxes than pure representative states, other things equal. Column 4' shows that the initiative effect was negative for signature requirements below 10 percent and statistically significant for signature requirements under 9 percent. In a 5 percent signature requirement state, citizens paid \$105.80 per capita less in taxes than citizens in an otherwise identical pure representative state. This implies approximately an 8 percent reduction from the average tax level of \$1,390.

The flip side of the tax effect is seen in the charge regressions: citizens in initiative states paid more in charges. The initiative dummy is positive and insignificant in column 5, and both initiative coefficients are statistically significant in column 6. Column 6' indicates a positive initiative effect for signature requirements below 8 percent, statistically significant for those below 6 percent. According to the point estimates, in a 5 percent state, citizens paid \$50.01 per capita more in charges than citizens in pure representative states. Column 6' also suggests that fees were significantly lower in initiative states with a 10 percent signature requirement, a somewhat anomalous estimate.

Not surprisingly, the evidence on net debt is inconclusive. The point estimates suggest that initiative states borrowed more, but the standard errors are huge.

To summarize, this section identifies one notable effect that availability of the voter initiative had on the finance mix. Initiative states raised more revenue from charges and less from taxes than pure representative states. This is somewhat surprising; given that initiative states spent less, reductions in both taxes and charges might have been expected. One "explanation" is that the initiative was used to cut taxes, forcing legislators to raise more revenue from fees, but again this begs the question why voters used the initiative to push down taxes and not charges. Section VII offers an interpretation.

## VI. Are Initiative States Different?

At this point, it is natural to wonder: Are the regressions detecting a true initiative effect, or are the initiative variables proxying for an unobserved variable that itself pushes down spending? Perhaps the main reason to suspect such a variable might exist is that the states with the initiative do not appear to be a random sample of all states: they are clustered in the west. For example, 17 of 24 states west of the Mississippi River provide for the initiative compared to only 6 of 26 east of the Mississippi. Obviously, states did not adopt the initiative at random. The questions arise: (i) Was there a hostility toward government spending, perhaps an ideological predisposition, that led the initiative states to adopt the device; and (ii) if so, did this hostility persist to the sample period as an unmeasured factor driving down expenditure?

The answer to these questions is almost certainly, no. First, most states adopted the initiative as a result of the Progressive movement. Histories of the Progressive movement and the Populist movement that foreshadowed it suggest that the movements were motivated by a desire to reduce a perceived corruption, or capture, of government by corporations, railroads, and the “money interests.”<sup>12</sup> Accordingly, the Progressive movement’s reform agenda also included things such as the secret ballot, open primaries, and civil service. It is difficult to find mention in the historical literature of a desire to cut back the size of government. Moreover, the other issues with which the Progressives and Populists were associated do not suggest a desire to shrink government, particularly their proposals for government ownership of railroads and utilities and a progressive income tax. To the best of my knowledge, there is not yet a compelling explanation why the Progressives succeeded in some states and not in others, but the conjecture in Price (1975, p. 248) has at least a little plausibility: “It may well be that the initiative was able to catch on in at least some western states because the political institutions and channels for doing things were not as firmly rooted in tradition as they were in the eastern, southern, and midwestern states. After all, the Progressive Movement swept the western states only a few decades after most had attained statehood.”

Second, even if people were more fiscally conservative in initiative states when they adopted the device, this creates a potential problem for the present study only if they remained more conservative into the 1960–90 sample period. Erikson, Wright, and McIver (1993) pro-

<sup>12</sup> The historical literature on these movements is voluminous and disputatious. Hofstadter (1955) is a classic reference; a more recent study is McMath (1993). Cronin (1989) focuses specifically on adoption of direct legislation devices.

TABLE 11  
COMPARISONS OF STATE IDEOLOGIES

Ideology Measure	Initiative State	Pure Representative State	<i>t</i> -Statistic
Gallup polls, 1937–39	–3.39 (2.52) <i>N</i> = 23	–.08 (2.13) <i>N</i> = 24	1.00
Gallup polls, 1947–64	–4.14 (2.23) <i>N</i> = 23	–3.75 (2.61) <i>N</i> = 24	.11
CBS News/ <i>New York Times</i> polls, 1976–88	–14.27 (1.74) <i>N</i> = 24	–14.31 (1.38) <i>N</i> = 24	–.02
D-NOMINATE average for U.S. senators, 1960–90	–.069 (.024) <i>N</i> = 145	–.047 (.018) <i>N</i> = 198	.75

NOTE.—The main entry in each cell is the mean ideology. In parentheses beneath the mean is the standard error; *N* is the number of observations. The *t*-statistic is for the hypothesis that the mean for initiative states is less than the mean for pure representative states. The polling data have a theoretical range of –100 to +100, where higher numbers are associated with liberal self-identification. The D-NOMINATE numbers have a theoretical range of –1 to +1, where higher numbers are associated with more conservative positions on roll-call votes. Alaska is omitted from all calculations, Nevada is omitted from the polling data, and Hawaii is omitted from the Gallup poll data.

vided evidence that state ideologies do not persist over such long periods of time. They used opinion poll data to measure how conservative or liberal each state was in two periods, 1937–39 and 1976–88. The raw correlation in ideologies between the two periods is .03 (see their table 9.1).

Third, there is little evidence that initiative states were more conservative than pure representative states in the first place. Each row of table 11 compares the mean ideology of initiative states with the mean ideology of pure representative states. The first three rows use data supplied to me by Robert Erikson from the data used in his aforementioned book.<sup>13</sup> The first two rows report mean ideologies based on Gallup polls. These polls essentially asked respondents whether they were liberal or conservative. The responses were aggregated for each state and normalized to yield a number with a theoretical range of –100 to +100, where higher numbers are associated with more liberal respondents. Initiative states were more conservative than pure representative states according to these measures in 1937–39 and 1947–64, but the differences were small and statistically insignificant.

<sup>13</sup> I brush over a number of qualifications and details concerning these numbers that are discussed in Erikson et al. (1993).

The next two rows are more relevant in that they compare state ideologies within the 1960–90 sample period. The third row reports measures computed from CBS News/*New York Times* polls taken over the 1976–88 period. Like the Gallup numbers, they yield an ideology score for each state with a theoretical range of  $-100$  to  $+100$ , where high numbers indicate liberal respondents. According to this measure, on average, initiative states were more liberal but by a trivial and statistically insignificant amount. Finally, the last row compares the mean D-NOMINATE score for senators from the two kinds of states over 1960–90. Recall that D-NOMINATE ranges from  $-1$  to  $+1$ , where high numbers are associated with conservative roll-call voting by the state's senators. Consistent with the CBS News/*New York Times* numbers, senators from initiative states were more liberal than senators from pure representative states, but the difference is trivial and far from statistical significance. In short, table 11 provides no compelling evidence that initiative states were more ideologically conservative than pure representative states.

To summarize, this section makes three observations. First, historical accounts give no reason to suspect that direct legislation was adopted with the intention of limiting the size of government. Second, even if the residents of initiative states were more fiscally conservative than residents of pure representative states when they adopted the initiative, there is evidence showing that state ideologies did not persist from before World War II. Third, measures of state ideology provide no evidence that residents of initiative states were more conservative than their pure representative counterparts before World War II, immediately after, or during the 1960–90 sample period that this paper studies. Taken together with the fact that the initiative effects survive inclusion of the D-NOMINATE control in the regressions, there is a good case against the idea that the effects are proxying for unobserved fiscal conservatism in the electorate.

## VII. Discussion

This section offers several observations on the findings. First, it appears that availability of the voter initiative does have an effect on fiscal outcomes: it reduces total expenditure, shifts spending toward local governments, decreases the share of revenue raised from taxes, and increases the share raised from charges. Thus there is evidence that this institution affects the outcome of political competition. Moreover, the size of the effect is quantified: in a state with a 5 percent signature requirement, combined state and local direct general expenditure is about 4 percent lower, state general expenditure

is about 12 percent lower, and tax revenue is about 8 percent lower than in similar pure representative states. These do not seem like trivial effects. However, it is clear that they are secondary effects. The initiative makes a difference at the margins, but the prime mover is income.

Other research has been inconclusive about the effects of tax, spending, and debt limitation provisions (e.g., Abrams and Dougan 1986; Dougan 1988; Kiewiet and Szakaly 1994). Two observations can help reconcile these findings with the evidence of an initiative effect. First, the threat of an initiative may be enough to motivate representatives to cut spending. An actual statutory provision may be unnecessary. Second, tax and expenditure limitations in pure representative states may not be credible. If a legislature can muster enough votes to pass such regulations, why can't it just cut spending and taxes directly? Together, these two points suggest that initiative states might be driven to cut spending regardless of provisions requiring such action whereas pure representative states are unlikely to be bound by such provisions. More generally, this suggests that the effects of the initiative as an institution may extend beyond the particular pieces of legislation it engenders by providing a continual threat to the legislature.

Under the assumption that fiscal outcomes in initiative states are close to median voter outcomes, the results in this paper can be interpreted as evidence that legislatures do not implement median voter outcomes.<sup>14</sup> In this light it appears that over the last 30 years the median voter wanted significantly less government expenditure than his representatives delivered. The evidence at this point does not allow one to say with confidence whether this is due to vote trading in legislatures (Weingast et al. 1981; Weingast and Marshall 1988), agenda control differences between institutions (Romer and Rosenthal 1979), or perhaps, simply, "shirking" or "mistakes" by individual members (Kalt and Zupan 1984, 1990; Matsusaka 1992). However, the gains-from-trade theory is unique in its prediction that legislatures systematically spend more than the electorate would like. The logrolling theory as developed by Weingast et al. (1981) posits that spending programs provide benefits to a particular subset of the population, such as when a park or a street is constructed in a neighbor-

<sup>14</sup> While plausible, the assumption that policy outcomes are closer to median voter outcomes in initiative states than in pure representative states can be disputed. However, the only direct evidence on this question of which I am aware supports the conjecture. Gerber (1994) used opinion poll data to determine the preferred policy of each state's median voter on parental consent laws for teenage abortions and found that the actual laws were closer to the population median in initiative states.



hood. The cost of such a program is spread over the entire population because governments rely on broad-based taxes, such as income and sales taxes. Consequently, representatives view the tax base as a "common pool" from which to finance particularistic projects for their constituents, leading to a familiar problem of overexploitation. This logrolling theory also predicts a positive relation between expenditure and the number of seats, or districts, in a state's legislature, a prediction that found support in Gilligan and Matsusaka (in press).

The results also seem to corroborate Peltzman's (1992) findings on the relation between government expenditure and gubernatorial elections. Studying approximately the same time period (1950–88) as in this paper, Peltzman found a negative relation between growth in state spending and votes received by the incumbent governor when he (or his party's nominee) stood for re-election. This gives the image, in Peltzman's words, of "voters as fiscal conservatives," at least compared to their representatives. While the results presented above tend to confirm this characterization, the different effects observed for state compared to local expenditure suggest that voters were not hostile to spending per se. They were hostile to the way spending took place. Contrary to the fiscal conservative image, it appears that voters wanted more local spending than their representatives delivered.

What does it mean that initiative states have more local expenditure and less state expenditure than pure representative states? Voters might prefer local disbursement of funds because local officials have better information how to productively use the funds. However, there is another explanation that is broadly consistent with this and other findings in the paper: voters dislike redistribution, at least at the margin.

There is not direct evidence for this interpretation, but a thread of hostility toward redistribution seems to be running through the results. It can be seen in the expenditure evidence by noting that there is a more limited potential for redistribution when funds are disbursed locally than when they are disbursed at the state level. It can also be seen in the revenue mix. When revenue is generated by charges, those who use services pay for them. When revenue comes from taxes, there is less of a link between those who benefit from government spending and those who pay for it. For example, the cost of a college education can be paid by the student who receives the education in the form of a tuition charge, or it can be paid by someone else in the form of taxes. The more a state relies on charges relative to taxes, the less it can redistribute. By shifting financing away from taxes and toward charges, the initiative leads to a revenue mix that is inherently less redistributive. This interpretation of the evidence is also consistent with Peltzman's finding that welfare expen-

diture was particularly costly to incumbent governors on election day.<sup>15</sup>

### VIII. Conclusion

The broad objective of this paper is to use the voter initiative to evaluate whether the institutional context of political competition makes a difference for political outcomes, specifically, for fiscal outcomes. The evidence is that although demographic factors are by far the most important determinants of fiscal behavior, availability of the initiative does matter as well. After one controls for income, population density, metropolitan population, population growth, mineral production, ideology of U.S. senators, and federal aid, initiative states have lower combined state and local direct general expenditure, spend more locally and less at the state level, and rely less on taxes and more on charges to generate revenue than pure representative states. The easier it is to use the initiative, as measured by signature requirements to qualify a petition for the ballot, the larger the difference in fiscal outcomes between initiative and pure representative states.

In addition to showing that availability of the initiative leads to different fiscal outcomes, the paper quantifies the magnitudes of the effects. For a state with a 5 percent signature requirement (the modal state), state and local direct general expenditure per capita is about 4 percent lower, state-level general expenditure is about 12 percent lower, local expenditure is about 10 percent higher, taxes are about 8 percent lower, and charges are about 7 percent higher. The estimates suggest that the initiative becomes ineffective when signature requirements are as high as 10 percent.

The evidence taken together also seems to suggest a unifying theme for the effects of the voter initiative: less redistribution. The three pieces of evidence on this are that (i) the initiative leads to less government overall as measured by expenditure, (ii) the initiative shifts expenditure to local governments and away from state governments, and (iii) initiative states raise more of their revenue by directly charging the people who consume government services. This conclusion should be considered tentative pending additional evidence. If correct, one might expect to see initiative effects in specific categories of spending, for example, pure transfer programs such as welfare. However, I estimated a number of exploratory regressions on catego-

<sup>15</sup> It is also consistent with the evidence that initiative states cut back current spending but left capital spending the same, because welfare and other transfer programs are counted as current expenditure. However, the caveats regarding the capital expenditure estimates should be kept in mind.

ries of expenditure, including welfare, education, and highways, and was unable to find significant initiative effects. The problem seemed to be caused by the high variability of narrow expenditure categories. Further investigation along these lines would seem to be in order. If the conclusion holds up, there remains the question why the initiative leads to less redistribution (or why legislatures redistribute more). The emerging literature on congressional organization and logrolling suggests some answers, but more work on this question would also seem to be called for.

## Appendix

State and local fiscal data were taken from the following issues of *Governmental Finances*: 1960, 1964–65, 1969–70, 1974–75, 1979–80, 1984–85, and 1989–90. This source also provided income and population data. State-only fiscal data were taken from the issues of *Governmental Finances* listed above; *Compendium of State Government Finances*, 1960 and 1965; and *State Government Finances*, 1975 and 1985. All these serials are publications of the Bureau of the Census. Metropolitan population data were collected from *State and Metropolitan Area Data Book*, also a publication of the Bureau of the Census. The 1960 numbers were taken from the 1986 book; the 1970, 1980, and 1990 numbers were taken from the 1991 book; and the 1965, 1975, and 1985 numbers were interpolated geometrically. Mineral data for 1960–75 were taken from the following issues of *Minerals Yearbook*: 1961, 1965, 1970, and 1975. *Minerals Yearbook* is a publication of the Department of the Interior, Bureau of Mines. Mineral data for 1980 were taken from *Statistical Abstract of the United States*, 1990, a publication of the Bureau of the Census. Mineral data for 45 states for 1985 were taken from *Statistical Abstract of the United States*, 1990. Mineral data for five states for 1985 and all states for 1990 were constructed by adding together data on nonfuel mineral production, petroleum production, and natural gas production and imputing a value for other minerals (especially coal), all taken from *Statistical Abstract of the United States*, 1993.

*Direct general expenditure.*—Direct general expenditure consists of payments to employees, suppliers, contractors, beneficiaries, and other final recipients of government payments. This includes all government expenditure other than utility, liquor store, employee retirement or other trust fund, and intergovernmental expenditure.

*General expenditure.*—General expenditure includes all government expenditure other than utility, liquor store, and employee retirement or other insurance trust expenditure. General expenditure is equal to direct general expenditure plus intergovernmental expenditure.

*Capital outlay.*—Capital outlay is direct expenditure for construction of buildings, roads, and other improvements; for purchase of equipment, land, and existing structures; and for payments on capital leases. This includes amounts for additions, replacements, and major alterations to fixed works and structures, but does not include expenditure for repairs to such work.

*General revenue.*—General revenue is all government revenue except utility, liquor store, and insurance trust revenue. General revenue from own sources excludes revenues received from other governments.

*Taxes.*—Taxes are compulsory contributions exacted by a government for public purposes, except employee and employer assessments for retirement and social insurance purposes (which are classified as insurance trust revenue). This includes property tax, sales tax, and income tax revenues. It excludes charges for services and revenues from utilities and liquor stores.

*Charges.*—Charges are revenue received from the public for performance of specific services benefiting the person charged and from sales of commodities and services, except liquor store sales. This includes fees, assessments, and other reimbursements for services, rents, and sales derived from commodities or services furnished incident to the performance of particular functions, gross income of commercial activities, and the like.

*Debt.*—Debt is all long-term credit obligations of the government and its agencies, whether backed by the government's full faith and credit or non-guaranteed, and all interest-bearing short-term credit obligations. This includes judgments, mortgages, and revenue bonds, as well as general obligation bonds, notes, and interest-bearing warrants. It excludes non-interest-bearing short-term obligations, interfund obligations, amounts owed in a trust or agency capacity, advances and contingent loans from other governments, and rights of individuals to benefits from government-administered employee retirement funds.

*Metropolitan population.*—Total resident population in metropolitan statistical areas and consolidated metropolitan statistical areas as defined by the Office of Management and Budget.

*Mineral production.*—Mineral production is the marketed value of all fuel and nonfuel minerals produced, including petroleum, natural gas, and coal.

*Revenue from federal government.*—Revenue from federal government is revenue received by a state or local government directly from the federal government in the form of shared revenues and grants-in-aid, as reimbursements for performance of general government functions and specific services.

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