Several recent studies have investigated the relationship between direct democracy and public policy outcomes, with mixed findings. These inconsistencies may stem, in part, from researchers’ failure to recognize that direct democracy institutions are distributed nonrandomly across the American states. That is, certain factors may lead a state to adopt the initiative process and influence other policy choices. We revisit the question of how the initiative influences state fiscal policy using panel data from 1960–2000 and a full-information maximum likelihood estimator that explicitly accounts for the endogeneity of the initiative. Our findings suggest that failure to endogenize the initiative in empirical analyses leads to substantially biased estimates of its effects. In particular, we find that once factors that predict whether a state has adopted the initiative are controlled, the initiative has a positive effect on state revenue generation and spending.

Does direct democracy affect the substance of public policy? In particular, does direct democracy influence the level of public spending? These questions have been the subject of much research in the American states in recent years (Camobreco 1998; Farnham 1990; Gerber 1996a, 1996b; Gilligan and Matsusaka 2001; Lascher, Hagen, and Rochlin 1996; Matsusaka 1995, 2000; Matsusaka and McCarty 2001; Zax 1989). Some believe that the exercise of direct democracy, particularly the ballot initiative, by the state electorate is among the most profound forces shaping American public life in the last 100 years (Scheiber 1997). Certainly the popularity of the initiative hit an all-time high in recent decades, with 634 such measures appearing on state ballots between 1980 and 1998.1

However, the evidence on the net policy impact of the initiative is rather
mixed (Smith 2001; Bertelli and Richardson 2004). For example, Matsusaka (1995) found that the initiative reduced state and local (combined) direct general expenditures and revenues per capita in 1960-90, while Zax (1989) found that the institution increased per capita government spending in 1980. Camobreco (1998) found the initiative had no effect on state and local combined per capita expenditures and tax effort in 1988 and 1990.2

Thus, when, how, and if the initiative affects state policymaking systematically continues to be debated (Gerber 1996b, 1999; Camobreco 1998; Lascher, Hagen, and Rochlin 1996; Matsusaka and McCarty 2001; Marino and Matsusaka 2001; Smith 2001; Ellis 2002; Bowler and Donovan 2004). This debate is based not only on statistical modeling and data differences, but also on theoretical differences. As Matsusaka (2003b, 4) put it, “How [the initiative and referendum] change policies is not entirely clear theoretically.”

We believe that one source of the discrepancies among these studies lies in the failure to model explicitly the endogeneity of the initiative institution itself. Because states choose whether to allow the ballot initiative, factors that influence the adoption of this institution may also impact state policy outputs.3 While several studies (Camobreco 1998; Matsusaka 1995; Bowler and Donovan 2002; Lascher, Hagen, and Rochlin 2001) have noted that jurisdictions with the initiative may differ fundamentally from non-initiative jurisdictions, to date, no empirical study has incorporated this selection mechanism into a model of the policy effects of the ballot initiative. We do so in this study.

Specifically, we test the relationship between the initiative and state fiscal policy by modeling the possibility that initiative adoption is endogenous to the American states. In contrast to the fixed-effects, panel regression models employed in past research (Matsusaka 1995), we use a full-information maximum likelihood estimator that jointly models whether a state has the initiative and the independent influence of this institutional arrangement on state revenue and spending for 1960–2000. We also explore and develop theoretical arguments to explain how and why the initiative influences state policy outcomes, and also to distinguish why some states adopted the initiative while others did not.

We demonstrate that states with the initiative do not necessarily spend less than states without the initiative, as some have argued (Matsusaka 1995, 2004; Matsusaka and McCarty 2001), and also that ignoring states’ voluntary adoption of direct democracy when analyzing fiscal outputs generates biased and unreliable estimates of initiative effects. In contradiction to other studies, especially Matsusaka (1995), we find that the initiative actually increased state expenditures, revenues, and taxes in 1960–2000. Rather than reducing the
size of the public sector, the initiative appears to have fostered an expanded fiscal role for state government, a pattern quite similar to that for the 1902–42 period (Matsusaka 2000).

DIRECT DEMOCRACY AND POLICY: THE BASIC ARGUMENTS AND FINDINGS

Scholars have explored how direct democracy institutions (the referendum, initiative, and recall) may shape policy outcomes by altering legislators’ and voters’ incentives and behaviors. Because direct democracy provides additional, and perhaps more meaningful, opportunities for citizens to register their positions on policymaking than candidate elections, issues of representation are at the core of these arguments. The basic premise is that the initiative increases voter sovereignty, providing a clearer expression of the median voter’s position on specific issues and exerting pressure on legislators to follow the will of their constituents rather than their own preferences or those of interest groups and other elites (Zimmerman 1999).

Thus, the initiative goes beyond simply improving the flow of information between voters and their elected representatives and acts as a preemptive check on the representatives’ behavior. In addition to allowing voters to establish or eliminate programs, allocate funds, impose or remove regulations, and prohibit legislatures from taking specific actions, the initiative also allows voters and groups to signal their policy preferences to legislators (Gerber 1999). The mere threat of voter oversight of legislative action may be sufficient to alter legislative behavior. Indeed, in some cases, the initiative appears to encourage preemptive legislative adoption of proposals that might be popular among voters (Zax 1999, 268; Gerber 1999b). In summary, the initiative may have both direct (by changing the specific policy on the ballot) and indirect (by altering legislative behavior) effects on public policy.

The typical approach to testing the policy effect of the initiative has assessed whether policy outcomes are closer to the median voter’s preferences under different institutional arrangements. Some scholars have investigated this link directly (Lascher, Hagen, and Rochlin 1996; Gerber 1996a; Matsusaka 2001), while others have employed a more indirect strategy. For example, rather than measuring voter preferences, Matsusaka (1995) draws on legislative organization theory to develop hypotheses about how state policy would differ across traditional representative institutions and direct democracy arrangements. He tests these hypotheses by incorporating a dummy variable for state initiative status in various regression models. Based on insights from studies of legislative vote trading and logrolling in the United States Congress
MATSUSAKA (1995, 588–9) argues that state legislatures generally do not implement median voter outcomes since vote trading tends to increase government spending beyond what the median voter prefers. Consequently, policy outcomes in initiative states should not only differ from those in non-initiative states but, more specifically, spending should be lower in these states.

A potential problem with this logic is that state legislatures are not institutional replicas of Congress. Instead, the structure and processes of many state assemblies vary considerably, not only from those of Congress, but also from those of other state assemblies (Hamm and Squire 2005). For example, effective vote-trading depends on institutional mechanisms capable of promoting credible commitments. Because of the uncertainty of the future status of today’s bargains, legislators must devise institutions to guarantee their long-term durability and ensure the flow of benefits beyond the present legislative session (Weingast and Marshall 1988). The structure of the congressional committee system provides such guarantees by encouraging vote-trading. But in state legislatures, the structure and importance of committees vary quite markedly across both time and space (Hamm and Squire 2005). Thus, the gains-from-trade thesis derived from congressional scholarship cannot easily be applied to statehouses.

Matsusaka (1995) also relies on studies of congressional agenda control to explain why fiscal policies are likely to differ across initiative and non-initiative states. With monopoly control of the legislative agenda, the sequence of votes on proposals can be manipulated to drive policy outcomes away from the median voter’s preference and toward the preferred outcome of the agenda setter (Cohen 1979; McKelvey 1979; Romer and Rosenthal 1978; Schofield 1978). Given the committee system and other formal legislative rules and procedures, agenda setting is likely to be much more open to manipulation in pure representative government than when the initiative affords citizens the opportunity to propose their own laws. The initiative removes agenda control from the legislature, alters the balance of power between competing pressure groups, and, ultimately, produces different policy outcomes (Matsusaka 1995, 589; Cronin 1989; Gerber 1999; Donovan and Bowler 1998; Rosenthal 1998).

Although this view predicts that differences in agenda control will cause policy to differ between pure representative and direct democracy arrangements, the nature of these predicted policy differences is not clear. For instance, in their formal model of monopoly games, Denzau, Mackay, and Weaver (1981) derive results that predict reduced government expenditures with the initiative. But this conclusion is driven not by the process of speci-
fying alternatives to government proposals through ballot initiatives but, rather, by Denzau, Mackay, and Weaver’s additional assumption about the level of expenditure in the absence of the initiative (Zax 1989, 269). Following Shepsle (1978), Denzau, Mackay, and Weaver assume that legislatures are dominated by high expenditure demanders. If so, the presence of the voter initiative would check these high demanders, thereby driving down expenditures in initiative states.

Apart from the potential problem of applying theories of congressional organization to state legislative bodies, the argument that the initiative constrains legislative high demanders is neither universally accepted nor consistent with existing empirical evidence. Specifically, between 1900 and 1940, state ballot initiatives were more likely to require increases than decreases in expenditures and the size of government (Matsusaka 2000; Zimmerman 1999). These pro-spending initiatives might reflect the preferences of the majority (Matsusaka 2003a; Lupia and Matsusaka 2004, 472) or the dominance of special interests (Ellis 2002; Smith 1999, 2001; Smith and Lubinski 2002; but see also Gerber 1999). If most ballot measures were adopted and implemented as passed, it might be plausible that the initiative would serve as a check on agenda control and government spending. However, the empirical record suggests otherwise.

For instance, whereas tax limitation initiatives (such as California’s Proposition 13 in 1978) have received considerable media attention, such initiatives are relatively rare and typically do not pass (Mikesell 1986; Ellis 2002). Zax (1989, 269) found that only one of the 12 Proposition 13–style initiatives appearing on state ballots in 1979–84 was approved. Similarly, the Congressional Research Service (Oakley and Neale 1995) found that of the 495 proposals appearing on state ballots in 1976–92, those relating to borrowing, spending, and taxation far outnumbered any other type, but only 41 percent of these were eventually endorsed by the electorate. Even assuming that the successful ballot initiatives were uniformly fiscally conservative, how likely is it that they altered the status quo? Gerber, Lupia, and McCubbins (2004, 46) answer emphatically:

The likelihood of full compliance for many initiatives is very low. Indeed, our effort produces an ironic result: the kinds of policy changes that are most likely to prevail as initiatives (as opposed to prevailing in a standing legislature) are less likely to be implemented and enforced, all else constant. (emphasis in the original)

Theoretically, then, the initiative may be far less effective in enforcing fiscal conservatism than Matsusaka (1995, 2003a) suggests. The raw data provide
additional evidence. During 1960–2000, mean per capita expenditures, own-source revenues, and tax collections for initiative states were not always below the corresponding averages for non-initiative states (Figure 1). Indeed, in 1960–85, initiative states’ average expenditures and revenues consistently exceeded the average for non-initiative states. Initiative states’ average tax collections were, in turn, higher in 1960–70, marginally lower in 1975, marginally higher in 1980, and then consistently lower only in 1980–2000. Whatever conclusion one may draw from these data, it is not one of consistently lower spending and revenue collections in initiative states.

Thus, the historical record and contemporary political theory both suggest that the relationships between the adoption, use, and consequences of the statewide initiative have yet to be fully explained. Does the initiative have an independent impact on state spending and revenue once factors influencing its adoption have been modeled explicitly? It is to this question that we now turn.

SPECIFYING THE RELATIONSHIP BETWEEN THE INITIATIVE AND STATE POLICY OUTCOMES

To model properly the relationship between the voter initiative and policy outputs, we must account for the extent to which initiative states differ from non-initiative states. This is a classic question of endogeneity: Does a common set of factors influence both a state’s decision to allow for the ballot initiative and its policy outputs? For example, if states desiring less government spending are more likely to adopt the initiative, then these states will likely have lower government spending but not as a result of the initiative. Failure to control for this correlation between institutional choice and policy output will bias any estimate of the true fiscal effect of the initiative (Greene 2003; Maddala 1983). Both the historical record and extant research provide ample grounds to suspect such endogeneity.

The first evidence of endogeneity in this relationship is the distribution and timing of state initiative adoptions. Seventeen of the 24 initiative states are located in the West, and 19 of these 24 states established the initiative between 1898 and 1918. The list of states with the initiative, along with their accompanying signature requirements for qualifying measures for the ballot, is reported in Table 1. We classify a state as having an initiative if its voters have access to either a statutory or a constitutional initiative, and we use the lowest signature requirement in the state in our measure (Matsusaka 1995).

Although a purely random process may have produced this pattern of initiative adoption, historical treatments suggest otherwise. During the late
Figure 1. Mean State Government Per Capita Expenditures and Revenues, 1960–2000

Mean Direct General Expenditures (per capita)

Mean Own-Source General Revenue (per capita)

Mean Tax Revenue (per capita)
19th century, Populist direct democracy proponents, who resided primarily in western states, were convinced that special interests had captured state legislatures, rendering them unable or unwilling to pass laws favored by the majority of citizens (Zimmerman 1989). The first proponents of the initiative tended to include the most leftist factions (including socialists), but in rural states the Farmers’ Alliance was typically at the forefront, along with other labor federations comprised mostly of miners. As the direct democracy movement gained momentum, women’s suffragists and middle-class Progressive reformers lent support, giving the direct democracy movement the credibility and support it needed to secure the adoption of the initiative and other institutions in a number of states in the early 20th century. The
adoption of the initiative was most common in states where the Progressive movement was a strong force (Cronin 1989, 53).

Thus, states that established the initiative during the Progressive Era shared a common political history and culture at that time. But what remains unclear is whether these states continue to share a political history and culture that shapes their contemporary political development and policy outputs. Matsusaka (1995, 615) explored, and ultimately dismissed, this possibility, but other scholars have been more cautious. For example, Camobreco (1998, 823) admits that “Matsusaka’s (1995) findings regarding the fiscal policies of initiative and non-initiative states may reflect history, as western states are more likely to have adopted the initiative and may also be more likely to prefer local-level taxing and spending than state-level taxing and spending.” Furthermore, Bowler and Donovan (2002, 383) argue that “western states that make more frequent use of the initiative tend to have greater in-migration, which could also vary with a more efficacious population. Each factor could cause an observed relationship between initiative use and efficacy to be spurious.” And finally, according to Lascher, Hagen, and Rochlin (1996, 769–70):

The possibility of the confounding effects of the endogeneity of the initiative process must also be considered. Perhaps the initiative process was established in particular states precisely to counter unusual tendencies toward unresponsiveness. In that case the absence of an apparent interaction between ideology and responsiveness might actually reflect unmeasured, underlying responsiveness problems in the initiative states.

Thus, both the historical record and the suspicions of various scholars suggest the endogeneity of the initiative in explaining state policy. It is likely that some of the factors that led states to adopt the initiative also influence the types of policies these states pursue.11 Although some scholars have acknowledged this possibility (Bowler and Donovan 2002; Camobreco 1998; Lascher, Hagen, and Rochlin 1996; Matsusaka 1995), no study has explicitly incorporated the endogenous choice of the initiative into a model of state policy choice. Rather, scholars commonly try to circumvent this endogeneity problem with arguments that emphasize the temporal ordering of events. For instance, some have claimed that since the initiative was established in most states so long ago, the decision to adopt it is, in practice, exogenous to policy decisions made in the sample period for most research (Bowler and Donovan 2002; Lascher, Hagen, and Rochlin 1996; Matsusaka 1995).

But the issue of the temporal ordering of events is not at the heart of the endogeneity concern. Rather, the question is whether potentially unobserved
factors that led certain states to adopt the initiative also account for some of
the cross-state variation in public policy that researchers wish to explain. If
some unobserved common factors are associated with both initiative status
and policy outputs, estimates of the impact of initiative status on policy
outputs will be biased unless we can account for these common factors in
our model. Given non-random assignment of subjects to a particular condi-
tion (such as, direct democracy), modeling the impact of this condition by
means of a dummy variable in a single-equation model leads to a correlation
between this indicator and the error term, yielding biased and inconsistent
estimates of the effects of the condition on the dependent variable (Madd-
dala 1983, 257–64; Achen 1986). As our empirical analyses will demonstrate,
endogeneity concerns for the impact of the initiative on state government
revenue and spending are well founded.

STATES, THE INITIATIVE, AND FISCAL POLICY:
DATA AND METHODS

Because our primary focus is on the fiscal effects of the initiative, we model
our empirical inquiry on what is likely the most often cited work on the
political economy of state-level direct democracy: Matsusaka’s (1995) study
of the initiative’s fiscal effects in 1960–90. While Matsusaka explores the
initiative’s effect on several state and local fiscal measures, we restrict our
attention to total state and local general expenditures per capita, total
state and local general revenue per capita, and total state and local tax collec-
tion per capita.12 We use the United States Census Bureau’s State and Local
Government Finances reports to construct a panel design, with measure-
ments taken at five-year intervals from 1960 to 2000. In Table 2, we present
the means and standard deviations of the three fiscal series as reported by
Matsusaka (1995) and the corresponding statistics for the series we use in
our analyses (from State and Local Government Finances reports).

There is close correspondence between our data and Matsusaka’s data
for the overlapping years (1960–90), except for 1985–90 own-source general
revenue. The source of this discrepancy may lie in our use of the most recent
Census revisions of earlier estimates. However, as we demonstrate, our results
are consistent across all three fiscal series and, hence, cannot be attributed
to coding errors or data discrepancies.

In constructing a model of state fiscal policy, we build on Matsusaka
(1995) and others (Berry 2001; Shadbegian 1999; Preston and Ichniowski
1991; Poterba 1995a, 1995b; Crain and Crain 1998) by including a set of inde-
pendent variables to control for the demand for, and costs of, public goods
provision, as well as the state political environment (see the Appendix for more details). These factors must be accounted for in our model to achieve an unbiased estimate of the impact of the initiative on fiscal policy. However, motivated by our fundamental concern that common unobserved factors influence both a state’s fiscal policy and its decision whether to adopt the initiative, we pursue a modeling strategy that departs from the conventional single-equation approach. Specifically, we employ the following full-information maximum likelihood model, specified in the usual latent variable formulation:\(^{13}\)

Table 2. State and Local Expenditure and Revenue per Capita, 1960–90

<table>
<thead>
<tr>
<th>Year</th>
<th>Direct General Expenditure (per capita)</th>
<th>Own-Source General Revenue (per capita)</th>
<th>Tax Revenue (per capita)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1960</td>
<td>1,273</td>
<td>1,274</td>
<td>218</td>
</tr>
<tr>
<td>1965</td>
<td>1,614</td>
<td>1,618</td>
<td>349</td>
</tr>
<tr>
<td>1970</td>
<td>2,129</td>
<td>2,123</td>
<td>417</td>
</tr>
<tr>
<td>1975</td>
<td>2,517</td>
<td>2,487</td>
<td>450</td>
</tr>
<tr>
<td>1980</td>
<td>2,531</td>
<td>2,529</td>
<td>400</td>
</tr>
<tr>
<td>1985</td>
<td>—</td>
<td>—</td>
<td>—</td>
</tr>
<tr>
<td>1990</td>
<td>—</td>
<td>—</td>
<td>—</td>
</tr>
<tr>
<td>1995</td>
<td>1,274</td>
<td>1,274</td>
<td>361</td>
</tr>
<tr>
<td>1990</td>
<td>2,487</td>
<td>2,487</td>
<td>452</td>
</tr>
<tr>
<td>1995</td>
<td>3,218</td>
<td>3,218</td>
<td>555</td>
</tr>
<tr>
<td>2000</td>
<td>—</td>
<td>—</td>
<td>—</td>
</tr>
</tbody>
</table>

Note: Alaska is excluded from all calculations. All numbers are in 1990 dollars.
In this specification, \( y_{it} \) corresponds to state fiscal policy variables (direct general expenditure, own-source general revenue, and tax revenue), \( z_{it} \) is the dummy variable indicating whether a state \( i \) has the initiative in year \( t \), \( s_{it} \) corresponds to the signature requirements accompanying the state’s initiative, and \( x_{it} \) is the covariate vector of factors hypothesized to influence these fiscal outcomes (per capita personal income, population density, metropolitan population, population growth, non-fuel mineral resources, federal aid, government ideology, south, and a dummy variable for \( t-1 \) panel years). Three of these variables are commonly argued to tap demand for public goods and services: per capita personal income, the relative size of the metropolitan population, and the population growth rate (Matsusaka 1995). While high-income states likely face greater demand for goods and services, population growth is also expected to raise demand for public goods and services. The proportion of a state’s population living in a metropolitan area controls for urban-rural differences in demand for public services and the cost of providing these services.

We also control for the value of a state’s non-fuel resources and its population density. A state’s population density is likely to influence fiscal policy by affecting the marginal benefit of spending and the opportunities for scale economies in the production and delivery of public goods and services. Substantial mineral resources offer fiscally stressed states the opportunity to shift the tax burden toward severance taxes (Matsusaka 1995).

While scholars have found correlations between state fiscal policy and budget institutions, it is not clear whether these correlations reflect the impact of fiscal discipline, institutions, or “voter tastes for fiscal restraint” (Poterba 1995a, 399). Matsusaka (1995), Poterba (1995a), and Shadbegian (1999) wrestle with this problem by controlling for state voter preferences via a
measure of state political ideology, D-NOMINATE scores.\textsuperscript{14} We use Berry, Ringquist, Fording, and Hanson’s (1998) more comprehensive and dynamic measure of state government political ideology.\textsuperscript{15} Like most measures of ideological preference, Berry et al. use the liberal-conservative spectrum with lower values indicating more liberal state governments. We expect liberal state governments to exhibit a greater propensity both to tax and spend (Shadbegian 1999, 229; Poterba 1995a).

Finally, we control for federal intergovernmental revenue transfers to states, regional differences, and signature requirements accompanying initiative use. We include a dummy variable for states in the South to control for the well-documented differences between these and other states (Matsusaka 1995).\textsuperscript{16}

In equation 2, $w_{it}$ is a vector of covariates hypothesized to influence whether a state chooses to establish the initiative. Specifically, within $w_{it}$ we nest a subset of the $x_{it}$ variables: per capita income, metropolitan population, and population growth. The relative size of the metropolitan population controls for urban-rural differences in preferences for the initiative (Hersch and McDougall 1997, 332). Population growth controls for potential increases in the demand for government services that, in turn, are expected to increase demand for procedural constraints on state fiscal behavior. Finally, states with greater taxable income are argued to favor the initiative because it is seen as a “means of reducing the size of government and thereby conserving private wealth” (Hersch and McDougall 1997, 334–5).

Of principal theoretical interest in the initiative model are the three variables unique to this equation: the ideological dissonance between a state’s government and its voters, the size of the state legislature, and state legislative professionalism. The descriptive literature on the diffusion of direct democracy suggests that states came to rely on the initiative to counter poor legislative representation (Cronin 1989), which we operationalize with measures of ideological dissonance and legislature size. We expect that where a state’s citizenry is markedly more conservative or liberal than its government the likelihood of initiative availability increases and that legislative size is inversely related to initiative status since smaller legislatures likely have weaker representation.\textsuperscript{17}

Given the Progressive Era reformers’ desire to wrestle legislative control from statehouses dominated by special interests (Smith and Lubinski 2002; Hofstatter 1955; Hayes 1964), and given that direct democracy weakens legislative control over both policy and electoral rules (as evidenced by the popularity of ballot initiatives for term limits and campaign finance reform), we expect the initiative to be less frequent in states with more professionalized
legislatures (Hersch and McDougall 1997). We test this hypothesis using King’s (2000, 329–30) index of state legislative professionalism.

As a result, we hypothesize that legislative size and professionalism and ideological dissonance between citizens and government influence the likelihood of initiative adoption but not fiscal policy. On the other hand, we hypothesize that per capita income, the relative size of the metropolitan population, and population growth influence both a state’s propensity to adopt the initiative and its fiscal policy.

Of fundamental importance in our analysis is the ability to test for the endogeneity of the initiative in this process. Full-information maximum likelihood estimation of our model allows for this by way of the parameter $\rho$, a measure of the strength and direction of the correlation between the error terms of the two equations. A statistically significant correlation will support our hypothesis of endogeneity; that is, that common factors influence state adoption of the initiative and fiscal policy. If the correlation is non-zero, the fiscal effect of the initiative estimated from a single-equation model, as done by Matsusaka (1995), will generally be biased, although both the direction and the size of this bias are difficult to predict (Maddala 1983).

**FINDINGS**

Given the significance of Matsusaka’s (1995) work on the fiscal impact of direct democracy and our desire to establish a baseline from which to compare our own results, we begin by presenting our replication of Matsusaka’s three single-equation models of state fiscal outputs. These are fixed-effects ordinary least squares regression models (with robust standard errors) of state direct general expenditure per capita, own-source general revenue per capita, and tax revenue per capita, 1960–90. To facilitate direct comparison of our estimates with Matsusaka’s (1995), we report his estimates alongside ours in Table 3.

Focusing on the estimated impact of the initiative, our results are most similar to Matsusaka’s (1995) for the tax revenue and own-source general revenue models. For the own-source general revenue model, the initiative coefficients are quite comparable and neither he nor we find a statistically significant effect. However, in the tax revenue model, although the coefficient on our initiative variable is statistically significant, it is much smaller than that of Matsusaka (−129.49 compared to −217.55). We find the same pattern in the case of direct general expenditure, although here the difference in statistical significance is greater. The most likely explanation for
Table 3. Impacts on Per Capita State and Local Expenditure and Revenue, 1960–90

<table>
<thead>
<tr>
<th>Variables</th>
<th>Direct General Expenditures</th>
<th>Own-Source General Revenue</th>
<th>Tax Revenue</th>
</tr>
</thead>
<tbody>
<tr>
<td>Initiative</td>
<td>127.96* (68.64)</td>
<td>–57.65 (60.26)</td>
<td>–58.02 (83.93)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>–217.55*** (47.11)</td>
</tr>
<tr>
<td>Signature requirement</td>
<td>5.13 (8.65)</td>
<td>–3.06 (7.32)</td>
<td>0.45 (10.75)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>22.35*** (5.65)</td>
</tr>
<tr>
<td>Per capita personal income</td>
<td>0.13*** (0.01)</td>
<td>.012*** (0.01)</td>
<td>0.11*** (0.01)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>0.10*** (0.01)</td>
</tr>
<tr>
<td>Population density</td>
<td>–0.28*** (0.06)</td>
<td>–0.25*** (0.07)</td>
<td>–0.47*** (0.06)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>–0.16*** (0.05)</td>
</tr>
<tr>
<td>Metropolitan population</td>
<td>2.42*** (0.74)</td>
<td>1.84*** (0.83)</td>
<td>5.34*** (0.98)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>2.38*** (0.57)</td>
</tr>
<tr>
<td>Population growth</td>
<td>1.18 (2.27)</td>
<td>0.73 (2.16)</td>
<td>–0.41 (3.28)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>–4.42** (2.04)</td>
</tr>
<tr>
<td>Non-fuel mineral resources</td>
<td>0.00 (0.06)</td>
<td>0.02* (0.07)</td>
<td>0.01 (0.06)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>–0.00 (0.05)</td>
</tr>
<tr>
<td>D-NOMINATE average</td>
<td>15.24 (44.66)</td>
<td>39.22 (46.48)</td>
<td>23.97 (52.12)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>51.12 (32.83)</td>
</tr>
<tr>
<td>Federal aid</td>
<td>2.29*** (0.18)</td>
<td>2.22*** (0.18)</td>
<td>1.57*** (0.21)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>1.11*** (0.13)</td>
</tr>
<tr>
<td>Southern state</td>
<td>–83.24*** (33.08)</td>
<td>–111.39*** (31.99)</td>
<td>–141.48*** (42.68)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>–116.99*** (23.04)</td>
</tr>
<tr>
<td>Western state</td>
<td>77.58** (35.74)</td>
<td>–6.86 (40.53)</td>
<td>–59.66 (96.99)</td>
</tr>
<tr>
<td>Constant</td>
<td>–350.65*** (96.99)</td>
<td>–361.51*** (91.21)</td>
<td>–281.90*** (68.06)</td>
</tr>
</tbody>
</table>

R²: 0.935 0.934 0.926 0.921 0.899 0.907
N: 336 336 336 336 336 336

*p < 0.10; **p < 0.05; ***p < 0.01

Note: These models are estimated using ordinary least squares regression. All models include seven year–specific fixed effects whose coefficients are not reported. Robust standard errors are in parentheses. Shaded columns represent regression coefficients and ancillary statistics reported in Model 5, Table 4, and Models 2 and 4, Table 9, of Matussaka 1995.
the differences across our estimates and Matsusaka’s is the coding of the D-NOMINATE variable. ¹⁹

Of central concern to our study is the fact that the single-equation models in Table 3 do not incorporate the self-selection of states into the initiative and non-initiative camps. Our two-equation model (Table 4) allows us to test and correct for this endogeneity. In particular, the negative, statistically significant estimates of ρ in each initiative/fiscal policy model in Table 4 suggest that valid estimation of initiative effects is impossible within the single-equation regression framework premised on the exogeneity of the initiative. Thus, the fiscal effect of the initiative estimated from a single-equation model will be understated.

As the results in Table 4 demonstrate, properly accounting for endogeneity in the process yields estimates of the fiscal effect of the initiative that differ sharply from those reported in Table 3.²⁰ We find that after taking into account the factors that predict whether a state allows for the initiative, the residual effect of the initiative on state fiscal behavior is positive and statistically significant, which is the opposite of the effect Matsusaka (1995) found in his single-equation models. For example, the estimates in Table 4 suggest that average per capita expenditures are $164 greater in initiative than non-initiative states and that initiative states generate $159 more in own-source per capita revenues and $160 more in per capita taxes, ceteris paribus. In addition, controlling for initiative endogeneity changes the estimated impacts of other covariates. Comparing the coefficients for the control variables in our models (Table 4) with those in Matsusaka’s models (Table 3), we discern smaller impacts of federal aid and the metropolitan population proportion. And while Matsusaka finds no statistically significant impact of mineral resources on direct general expenditures, we find a distinct, positive effect in two of our three models.

While our primary goal in this study is not to explain why some states choose to have the initiative, our initiative selection equations suggest that a state’s legislative landscape and ideological dissonance play a role in determining its initiative status. Larger state legislatures appear to reduce the probability of a state having the initiative, presumably because the threat of poor representation is reduced, ceteris paribus. Likewise, we find that the initiative is more commonly found in states with ideological dissonance between the citizenry and state government officials.
### Table 4. Simultaneous Estimation of Influences on State Fiscal Policy and Initiative Status, 1960–2000

<table>
<thead>
<tr>
<th>Variables</th>
<th>Expenditure</th>
<th>Revenue</th>
<th>Tax</th>
</tr>
</thead>
<tbody>
<tr>
<td>Initiative</td>
<td>163.78 ***</td>
<td>159.19 ***</td>
<td>159.82 ***</td>
</tr>
<tr>
<td>Signature requirement</td>
<td>−5.35</td>
<td>−7.31 **</td>
<td>−3.83</td>
</tr>
<tr>
<td>Per capita personal income</td>
<td>0.11***</td>
<td>0.12***</td>
<td>0.10***</td>
</tr>
<tr>
<td>Population density</td>
<td>−0.23***</td>
<td>−0.24***</td>
<td>−0.01</td>
</tr>
<tr>
<td>Metropolitan population</td>
<td>2.93***</td>
<td>2.15***</td>
<td>1.48 **</td>
</tr>
<tr>
<td>Population growth</td>
<td>−3.14</td>
<td>−8.81 ***</td>
<td>−7.44 ***</td>
</tr>
<tr>
<td>Non-fuel mineral resources</td>
<td>0.10**</td>
<td>0.20***</td>
<td>0.07 **</td>
</tr>
<tr>
<td>Federal aid</td>
<td>1.94***</td>
<td>1.09***</td>
<td>0.60 ***</td>
</tr>
<tr>
<td>Government ideology</td>
<td>0.68</td>
<td>0.40</td>
<td>0.90 **</td>
</tr>
</tbody>
</table>
### Table 4, continued.

<table>
<thead>
<tr>
<th>Variables</th>
<th>Expenditure (Initiative)</th>
<th>Revenue (Initiative)</th>
<th>Tax (Initiative)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Size of state legislature</td>
<td>—</td>
<td>—</td>
<td>—</td>
</tr>
<tr>
<td></td>
<td>-0.01***</td>
<td>-0.01***</td>
<td>-0.00***</td>
</tr>
<tr>
<td>(Initiative)</td>
<td>(0.00)</td>
<td>(0.00)</td>
<td>(0.00)</td>
</tr>
<tr>
<td>Legislative professionalism</td>
<td>—</td>
<td>—</td>
<td>—</td>
</tr>
<tr>
<td></td>
<td>4.50***</td>
<td>4.52***</td>
<td>5.23***</td>
</tr>
<tr>
<td>(Initiative)</td>
<td>(0.69)</td>
<td>(0.65)</td>
<td>(0.63)</td>
</tr>
<tr>
<td>Ideological dissonance</td>
<td>—</td>
<td>—</td>
<td>—</td>
</tr>
<tr>
<td></td>
<td>0.01***</td>
<td>0.01***</td>
<td>0.01***</td>
</tr>
<tr>
<td>(Initiative)</td>
<td>(0.00)</td>
<td>(0.00)</td>
<td>(0.00)</td>
</tr>
<tr>
<td>South</td>
<td>-94.13***</td>
<td>-57.86</td>
<td>-91.39***</td>
</tr>
<tr>
<td></td>
<td>(28.69)</td>
<td>(32.99)</td>
<td>(19.27)</td>
</tr>
<tr>
<td>Constant</td>
<td>-250.10</td>
<td>-448.40**</td>
<td>557.82***</td>
</tr>
<tr>
<td></td>
<td>(202.74)</td>
<td>(231.40)</td>
<td>(176.21)</td>
</tr>
<tr>
<td></td>
<td>-0.64***</td>
<td>-0.52***</td>
<td>-0.74***</td>
</tr>
<tr>
<td></td>
<td>(0.09)</td>
<td>(0.13)</td>
<td>(0.09)</td>
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<tr>
<td>Log likelihood</td>
<td>-3,248.3</td>
<td>-3274.4</td>
<td>-3100.7</td>
</tr>
<tr>
<td>(\chi^2(18))</td>
<td>(7,251.4)</td>
<td>(5,574.8)</td>
<td>(3,411.2)</td>
</tr>
<tr>
<td>Wald (\chi^2(1)) test statistic</td>
<td>22.35***</td>
<td>9.73***</td>
<td>21.41***</td>
</tr>
</tbody>
</table>

**p < 0.05; ***p < 0.01

Note: N = 441 in each model. Full-information maximum likelihood estimates are reported, with robust standard errors in parentheses. Coefficients for eight temporal dummy variables included in each equation not shown. Alaska is excluded from all calculations. All monetary variables are measured in 1990 dollars.
CONCLUSIONS

We find that once the endogeneity of the initiative is properly accounted for, the initiative actually leads to an increase in government spending and taxation in the states, not a decrease as other scholars have contended (Matsusaka 1995). Why did we find the results we did, and how can we explain why our results differ from Matsusaka’s? Since direct democracy is argued to make policy more responsive to public opinion, we believe that part of the answer lies in examining this relationship more closely.

Looking at public opinion over time, the evidence does not support the claim of an increasingly fiscally conservative populace. Instead, beginning in the early 1980s, government policy was becoming more fiscally conservative while the electorate was becoming more supportive of government spending (Mayer 1993; Stimson 1999; Lupia and Matsusaka 2004; Camobreco 1998). Stimson’s (1999) measure of the electorate’s “policy mood” reveals a similar pattern, with a marked increase in liberal preferences for a more active government from 1958 to 1961, followed by a general shift toward a conservative, less-active government mood from 1962 to 1980, and then a sharp increase in activist government liberalism thereafter. Therefore, if the initiative makes policy more responsive to public opinion, more often than not, public opinion called for more, not less, government spending from 1960–2000. The positive coefficient we estimate for the initiative in our models reflects such an effect.

This study illustrates the importance of model specification and estimation in the face of potential endogeneity. Our findings demonstrate the confusion that can result from ignoring the choice inherent in a state’s decision of whether to adopt the ballot initiative. If some of the very factors that lead states to rely on the initiative also influence state fiscal policy, ignoring this fact will yield spurious results. Almost a decade ago, Poterba (1995b, 185) urged students of political economy to ask why, if state fiscal institutions have important effects on policy outcomes, “some states do not have these institutions” (see also Poterba and Rueben 1995; Poterba 1995a). In this study, we have heeded this advice and explicitly modeled the endogeneity of the initiative. To be sure, while much remains to be learned about the impacts of institutions of direct democracy, future research should proceed by using careful theory and method in equal measure.
APPENDIX: VARIABLE MEASUREMENT

All fiscal data are measured in per capita 1990 dollars.

**Dependent Variables**

*Direct general expenditure*: All state and local government expenditure, excluding utility, liquor store, employee retirement or other trust fund, and intergovernmental expenditure.

*Own-source general revenue*: All state and local government revenue except utility, liquor store, insurance trust, and intergovernmental revenue.

*Tax revenue*: All state and local property, sales, and income tax revenue; taxes on insurance trust revenues, service charges, utilities, and liquor are excluded.

Source for the previous three variables: United States Bureau of the Census, *Annual Surveys of State and Local Government Finances*, various years.

*Initiative*: Coded 1 for states where citizens can propose a law by placing a proposition on the ballot; 0 otherwise.

**Independent Variables**

*Signature requirement*: The percentage of signatures required to place a measure on the ballot (most recent requirements) where the percentage is based on votes cast for governor in the most recent gubernatorial election (AZ, AR, CA, ME, MA, MI, MO, MS, MT, OH, OK, OR, SD, UT, WA), votes cast in the last election (AK, WY), votes cast in the last presidential election (FL), votes cast for the Secretary of State (CO), or registered voters (ID, NE, NV).

Source: The Initiative and Referendum Institute (http://www.iandrinstitute.org/).

*Metropolitan population*: The percentage of a state’s population living in a metropolitan statistical area, as defined by the Census Bureau.


*Population growth*: The percentage change in a state’s population over the preceding five years.


*Population density*: The number of persons per square mile living in a state.


*Per capita personal income*: The sum of wage and salary disbursements, other labor income, proprietors’ income with inventory valuation and capital consumption adjustments, rental income of persons with capital consumption adjustment, personal dividend income, personal interest income, and transfer payments to persons, less personal contributions for social insurance, of the residents of a state divided by the state’s population as of July 1.

**Non-fuel mineral resources:** The estimated value of all mineral resources in a state, excluding petroleum, natural gas, and coal. We use the published valuation of state non-fuel mineral resources for 1980–2000 and estimate the share of non-fuel to total mineral resources (using published estimates of both variables for 1980) for 1960–75. Source: United States Bureau of the Census, *Statistical Abstract of the United States*, various years.

**Federal aid:** All revenue transfers to a state and its local governments from the federal government through shared revenues and grants-in-aid. Source: United States Bureau of the Census, *Annual Survey of State and Local Government Finances*, various years.

**Government ideology:** Berry et al.’s (1998) annual index based on the ideology scores of the governor and the two major parties in each legislative chamber, computed on the basis of an algorithm that includes ADA ratings of United States senators, outcomes of congressional elections, the partisan division of the state legislature, and the governor’s party to measure the political ideology of the state government, measured on a 0–100 scale.

**Ideological dissonance:** The difference between Berry et al.’s (1998) measure of “a state’s citizen ideology”—the weighted average ideological score for the state’s congressional delegation—and its score on the government ideology measure above.

**Size of the state legislature:** The total number of seats in both legislative chambers. Source: *The Book of the States*, Hofferbert (1991) and the United States Bureau of the Census, *Statistical Abstract of the United States*, various years.

**Legislative professionalism:** A biennial average of the salary and expenses (in 1990 dollars) of state legislators, the number of days in the legislative session, and the expenditures for services and operations (minus legislator compensation) per legislator (in 1990 dollars). This index is available for 1963–64, 1973–74, 1983–84, and 1993–94. Because this score is fairly static across each decade, we use the 1963–64 value for 1960 and 1965, the 1973–74 value for 1970 and 1975, and so forth. Source: King 2000.

**South:** Coded 1 if the Bureau of the Census designates the state as located in the South; 0 otherwise.
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
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<tr>
<td>Per capita personal income&lt;sup&gt;a&lt;/sup&gt;</td>
<td>9,368</td>
<td>11,080</td>
<td>12,961</td>
<td>14,312</td>
<td>15,405</td>
<td>16,833</td>
<td>18,501</td>
<td>21,314</td>
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<td>(1,808)</td>
<td>(1,918)</td>
<td>(2,073)</td>
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<td>(2,102)</td>
<td>(2,336)</td>
<td>(2,897)</td>
<td>(2,964)</td>
<td>(3,391)</td>
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<td>154</td>
<td>158</td>
<td>163</td>
<td>169</td>
<td>175</td>
<td>185</td>
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<td>(193)</td>
<td>(209)</td>
<td>(222)</td>
<td>(223)</td>
<td>(223)</td>
<td>(228)</td>
<td>(236)</td>
<td>(239)</td>
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<td>(251)</td>
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<td>Metropolitan population</td>
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<td>61.16</td>
<td>63.34</td>
<td>63.36</td>
<td>63.52</td>
<td>64.10</td>
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<td>(22.31)</td>
<td>(21.65)</td>
<td>(21.08)</td>
<td></td>
<td>(20.43)</td>
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<td>Population growth</td>
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<td>5.05</td>
<td>7.83</td>
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<td>4.47</td>
<td>4.00</td>
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<td>(6.60)</td>
<td>(5.62)</td>
<td>(4.78)</td>
<td>(6.29)</td>
<td>(5.48)</td>
<td></td>
<td>(4.86)</td>
</tr>
<tr>
<td>Non-fuel mineral resources&lt;sup&gt;a&lt;/sup&gt;</td>
<td>199.31</td>
<td>204.68</td>
<td>222.93</td>
<td>242.82</td>
<td>286.69</td>
<td>173.81</td>
<td>241.95</td>
<td>236.75</td>
<td>164.65</td>
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<td>(308.69)</td>
<td>(302.96)</td>
<td>(389.99)</td>
<td>(448.95)</td>
<td>(435.19)</td>
<td>(430.67)</td>
<td>(400.73)</td>
<td>(265.08)</td>
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<td></td>
</tr>
<tr>
<td>Federal aid&lt;sup&gt;a&lt;/sup&gt;</td>
<td>213</td>
<td>295</td>
<td>391</td>
<td>555</td>
<td>605</td>
<td>551</td>
<td>566</td>
<td>840</td>
<td>816</td>
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<tr>
<td>(94)</td>
<td>(144)</td>
<td>(126)</td>
<td>(115)</td>
<td>(123)</td>
<td>(133)</td>
<td>(141)</td>
<td>(211)</td>
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<td>(189)</td>
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<td>D-NOMINATE average</td>
<td>−0.070</td>
<td>−0.071</td>
<td>−0.065</td>
<td>−0.065</td>
<td>−0.063</td>
<td>0.009</td>
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<td>47.08</td>
<td>35.14</td>
<td>55.95</td>
<td>45.91</td>
<td>51.98</td>
<td>52.11</td>
<td>42.34</td>
<td>45.76</td>
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<td>(27.58)</td>
<td>(27.02)</td>
<td>(21.79)</td>
<td>(23.96)</td>
<td>(18.95)</td>
<td>(20.92)</td>
<td>(22.50)</td>
<td>(29.42)</td>
<td>(27.05)</td>
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<tr>
<td>Ideological dissonance</td>
<td>−5.66</td>
<td>−5.51</td>
<td>8.89</td>
<td>−7.50</td>
<td>−3.50</td>
<td>−6.74</td>
<td>−3.93</td>
<td>3.29</td>
<td>5.13</td>
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<td>(17.37)</td>
<td>(18.33)</td>
<td>(16.90)</td>
<td>(12.57)</td>
<td>(10.05)</td>
<td>(14.03)</td>
<td>(15.19)</td>
<td>(26.20)</td>
<td>(22.85)</td>
<td></td>
</tr>
<tr>
<td>Legislative professionalism&lt;sup&gt;b&lt;/sup&gt;</td>
<td>0.16</td>
<td>0.16</td>
<td>0.24</td>
<td>0.24</td>
<td>0.24</td>
<td>0.25</td>
<td>0.25</td>
<td>0.25</td>
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<tr>
<td>(0.08)</td>
<td>(0.08)</td>
<td>(0.13)</td>
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<td>(0.14)</td>
<td>(0.14)</td>
<td>(0.14)</td>
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<td>(0.14)</td>
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<tr>
<td>Size of the state legislature</td>
<td>159.26</td>
<td>159.32</td>
<td>155.20</td>
<td>155.20</td>
<td>155.20</td>
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<td>(69.70)</td>
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<td>(59.12)</td>
<td>(59.12)</td>
<td>(59.12)</td>
<td></td>
<td>(59.12)</td>
</tr>
</tbody>
</table>

<sup>A1</sup> Note: Alaska is excluded from all calculations
<sup>a</sup> In 1990 dollars per capita. See the Appendix for details on measurement and source of all these variables.
<sup>b</sup> Legislative professionalism scores are only available for four time points (1963–64, 1973–74, 1983–84, and 1993–94). We used the score closest to a given year in our analyses.
ENDNOTES

The authors’ names are in alphabetical order indicating equal co-authorship.

1. See the Initiative and Referendum Institute Web site: www.iandrinstitute.org/usage/byyear.htm
2. Camobreco (1998) did find that the interaction of an initiative and public opinion liberalism in a state attenuates the otherwise positive effects of both these variables on per capita taxation.
3. See Knight (2000) and Besley and Case (1994) for other inquiries into policy endogeneity.
5. Typically, this has involved assessing the fit between voter ideal policy preferences and actual policy outcomes.
6. These data are drawn from the United States Bureau of the Census’ Annual Survey of State and Local Government Finances (various years).
7. We recognize the classical econometric distinction between sample selection bias and endogeneity. Sample selection bias refers to problems arising from a dependent variable that is only observed for a restricted, non-random sample of cases. Endogeneity refers to the situation where an independent variable in a model is correlated with unobserved variables relegated to the error term. See Maddala (1983) and Amemiya (1985) for details of these estimation issues.
9. This figure increases to 21 of these 24 states if one considers that one initiative state (Mississippi) first adopted the initiative in 1914, lost it due to a legal technicality in 1922, and re-adopted it in 1992, and that in another state (Illinois), although voters overwhelmingly approved a ballot proposition calling for the initiative in 1910, the state legislature did not adopt it until 1970 (http://www.iandrinstitute.org/Mississippi.htm, http://www.iandrinstitute.org/Illinois.htm).
10. Populists also favored government ownership of railroads, the elimination of monopolies, a graduated income tax, free coinage of silver, a vastly expanded supply of money, and other efforts that were expected to improve the credit and social conditions of hard-pressed rural families (Cronin 1989, 43).
11. While some of these common factors may be observable, it is impossible to rule out all unobserved, shared factors. Fortunately, the models we estimate allow us to test for unobserved commonalities.
12. Our central focus is on total fiscal effects in a state rather than on revenue and spending shifts between states and their local governments as a consequence of the initiative. The latter is an intriguing question in its own right, and one we reserve for future research. We are indebted to an anonymous reviewer for bringing this point to our attention.
13. Other specifications of endogeneity models are possible. For example, we could use fixed-effects or first-differencing methods if all states had enacted the initiative by 1960 (Wooldridge 2002, 638). However, a few states established the initiative between 1960 and 2000. We use the latent variable specification because we do not observe a state’s preference for the initiative directly but only whether it has the initiative.
14. In our replication of Matsusaka’s (1995) basic model, we follow his use of the D-NOMINATE measure of state ideology. See endnote 19 for more details.

15. While other measures of state political ideology exist (Wright, Erikson, and McIver 1993; Medoff 1997), we use the Berry et al. (1998) measures of citizen and government political ideologies because they are constructed with the most exhaustive array of factors. Berry et al.’s measures incorporate data from congressional and state elections, congressional roll call voting, and the partisan distribution of state legislatures and governors. In addition, both of Berry et al.’s measures are dynamic and publicly available for the decades of our study.

16. Matsusaka (1995) also included a dummy variable for western states in his state and local general expenditure regressions. The extremely high incidence of the initiative in the West leads us to exclude this variable from our models due to multicollinearity.

17. Weingast, Shepsle, and Johnsen (1981) posit the “law of 1/n” in governmental fiscal behavior, where fiscal inefficiencies increase with the number of single-member districts, which is the size of the legislative body (Gilligan and Matsusaka 1995). Ought, then, legislative size be included in our fiscal policy equation? Two factors kept us from doing so. First, since replicating and testing Matsusaka’s (1995) models is key to our project and because he does not include any measure of legislative size in his analyses, we follow suit. Second, recent evidence suggests that the configuration of districts, not the number of districts, influences fiscal policy (Crain 1999).

18. We are indebted to an anonymous referee for raising the issue of legislative professionalism’s potential relationship with the initiative. Of course, there is debate as to whether professional legislatures are more or less responsive to their constituents (Maestas 2000), and alternative arguments could be made about how professionalism and initiative status might be related. Note that state legislative size and professionalism are not correlated with one another.

19. Comparing Matsusaka’s (1995, 596, Table 2) D-NOMINATE averages against our statistics (Table A1 in the Appendix) shows a cumulative difference in the means of 0.07. Some discrepancy is to be expected given that our D-NOMINATE series, which uses congressional roll call voting to measure a state’s political ideology, differs from Matsusaka’s. In particular, we employ a different matching scheme when assigning congresses to the specific panel years. Whereas Matsusaka (1995, 595) matches the 100th Congress (1987–88) to 1990, we use the 99th Congress because it is the last congress for which Poole and Rosenthal’s 1991 D-NOMINATE series is available. This matching scheme highlights a measurement concern. Although the 1960, 1970, and 1980 D-NOMINATE averages are drawn from active congresses, Matsusaka’s use of the 88th Congress for 1965, the 93rd for 1975, and the 98th for 1985 ignores the fact that each of these congresses ended in the year prior to the one that it is supposed to represent (the 88th Congress ended in October 1964, the 93rd in December 1974, and the 98th in October 1984). Note that we also estimated the models in Table 3 replacing D-NOMINATE with DW-NOMINATE. DW-NOMINATE is a dynamic version of D-NOMINATE and its scores for one congress are directly comparable to scores from other congresses. Most importantly, this series spans the entire period of our study. Replacing D-NOMINATE with DW-NOMINATE yields results that are closer to Matsusaka’s for some models but more divergent for others. Specifically, the initiative coefficient is -100.78 for direct general expenditure, -87.09 for own-source general revenue, and -105.62 for tax revenue, and it is statistically significant in all three cases.
20. Replacing Berry et al.’s (1998) government ideology variable with the D-NOMINATE scores only strengthens this conclusion. These results are available from the authors.

REFERENCES


