Fiscal Institutions in U.S. States

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Abstract

This paper examines fiscal institutions in U.S. states, and their fiscal and economic consequences. The main institutions include traditional tax and expenditure limitations, balanced budget requirements, rainy day funds, supermajority rules for tax increases, gubernatorial line-item veto authority, and prohibitions on unfunded mandates. We discuss two important conceptual problems with empirical efforts to measure these institutions' consequences: (i) the endogeneity of the institutions, and (ii) their interactions with each other. In the context of new and recent empirical evidence, we describe econometric techniques to solve each problem separately, and the difficulties inherent in attempts to solve both simultaneously.

Introduction

The fifty U.S. states employ a wide variety of fiscal institutions designed to constrain state tax and expenditure policy. Some, such as balanced budget requirements and supermajority rules for tax increases, have been proposed for the federal government. Others, such as budgetary line-item-veto authority have even been enacted by Congress, but have since been ruled unconstitutional at the federal level. Advocates typically point to the states as examples from which federal policy makers can learn, but to date there is very little systematic evidence on the effectiveness of these state institutions, either individually or in concert. Furthermore, what evidence exists struggles to deal with two difficult empirical problems: the endogenous nature of the fiscal institutions, and their interactions with one another. This paper describes six of the most important state-level fiscal institutions, presents existing and new evidence regarding their consequences, and discusses empirical strategies for handling their complex endogenous and interactive character.

The problem associated with the fact that these rules are often endogenous can be summarized as follows. Suppose we wish to know the effect of traditional tax limitations – laws that limit state tax growth to the growth rate of income, population or inflation – on state fiscal policy. We may speculate that such limits are irrelevant because lawmakers can always circumvent the rules by shifting some of current revenues into the budget of the next fiscal year, or other such financial gimmicks. However, there are two chief difficulties in testing this hypothesis. First, those states whose populations most abhor tax increases may be most likely to pass tax growth limitations. A simple comparison of tax limited states to unlimited states will indeed show that limited states have lower tax increases, but that may be explained by the limited states' underlying aversion to tax increases, rather than the effect of the tax limitation itself. Even within a regression framework, when numerous observable characteristics of states can be controlled for, states' aversion to tax increases will remain a fundamentally important omitted variable that is likely to bias conclusions.

Second, it may be that the presence of tax limitations themselves is a function of past tax growth. If states experiencing tax increases over long periods respond by enacting tax limitations, then a simple comparison of tax limited states to unlimited states will have the opposite problem. It will show that tax limitations do not reduce tax growth and may even be associated with higher tax growth. Here, the effect of tax limits on tax growth has been confounded by the simultaneous effect of tax growth on tax limits. As with the case of omitted variable bias, even in a multiple regression framework that controls for other state characteristics, the simultaneity of taxes and tax limits will bias the analysis.

All is not lost, however, for there are several conventional econometric techniques that can in theory sort out the effect of tax limitations on tax growth, controlling for omitted variables and simultaneity. The most often used approaches include fixed-effects models, which control for omitted constant variables by examining changes in tax limitation status over time within states, and instrumental variables models, which control for simultaneously determined regressors by modeling the process by which tax limitations are enacted as a function of purely exogenous state characteristics. In what follows we describe such approaches, as used to study the principal fiscal institutions in the U.S.

The second serious problem, after endogeneity, confronting analyses of the effect of fiscal institutions on fiscal outcomes is that the various fiscal institutions interact with one another in complex ways. For example, strict state balanced budget requirements may not matter in states with so-called rainy day funds (reserves to be spent in lieu of issuing debt). Similarly, state-level tax limitations may encourage states to mandate local-level expenditures, and may only affect statewide expenditures when combined with prohibitions on such unfunded mandates. The task of identifying the separate and combined effects of fiscal institutions is particularly important because policy makers often consider the institutions in tandem. For example, the U.S. Congress has considered a supermajority rule both with and without a balanced budget amendment. For these reasons, it is important to consider these institutions in combination, rather than one-by-one.

A dilemma arises, however, when one tries to solve the first problem, omitted variable bias or policy endogeneity, and the second problem, multiple interactions among fiscal institutions, at the same time. Both approaches to endogeneity correction, fixed effects and instrumental variables estimation, quickly become intractable when combined with multiple fiscal institutions and their interactions. This is especially true given that there are only fifty U.S. states, and that there is relatively little intertemporal variation in either the institutions themselves or in the exogenous state characteristics typically used to instrument for the endogenous fiscal institutions.

In what follows we describe the piecemeal approach that has been taken to date, and some modest attempts to account for at least some of the interactions among institutions along with the endogeneity of those institutions. In the process, we describe each of the six most important state fiscal institutions, along with some existing and new evidence regarding their consequences.

Table 1 lists the principal fiscal institutions in each of the fifty states. Provisions for balanced budget rules and the line-item veto are typically hundreds of years old and date to the original constitution; for these two institutions, only an indicator for their existence is provided. For the other institutions, the table provides the date of adoption. The table makes clear the critical role of institutions' interactions: some states have none of the listed institutions, some have a few of them, while a few have all of them. In the next section we consider one of the most basic and most common fiscal institutions: traditional tax and expenditure limitations.

Traditional tax and expenditure limitations

Traditional tax and expenditure limitations cap the growth of state taxes or expenditures to the growth in personal income, population or price inflation. Twenty-six states currently have some form of these requirements, many of which were adopted in the 1970s. Table 1 depicts the current list of states with and without tax or expenditure limitations, along with their adoption dates.

Empirical studies have found mixed evidence on the effects of these limitations. Abrams and Dougan (1986) use a 1980 cross section of states to measure the effect of constitutional constraints, including tax and expenditure limitations, on government spending. They find that states with these limitations actually have higher state spending than states without limits, although the result is not statistically significant. They recognize that "the endogenous nature of such limits may obscure their true effects," by which they mean that their cross-section regression picks up the fact that states experiencing large tax or spending increases may respond by enacting tax limits. Without a panel of data, disentangling the effect of limits on spending from the effect of spending on limits is difficult.

Elder (1992) studies tax and expenditure limitations using a 1950-1985 panel of seventeen states with these limitations as of 1985. He finds that these limitations are associated with a statistically significant decline in state tax revenues of about $800 per year. Unfortunately, all of the limitations studied were enacted between 1976 and 1980; by not including the states without these limitations, these results are identified only through variation across states between these years of adoption. As a consequence, one is still left with the concern that tax averse states have enacted these laws, and that the lower spending in tax limited states is due to the underlying tax aversion rather than the tax limitation per se.

Poterba (1996) examines the short-run effects of tax and expenditure limitations by focusing on state responses to fiscal crisis, specifically unexpected deficits. He measures unexpected deficits by the difference between actual and forecasted spending and revenues. He finds that states with these limitations respond more quickly to unexpected deficits through raising taxes.

Rueben (1995) recognizes the potential endogeneity of these limitations and uses the two measures of voter power over the legislature, direct legislation and recall, as instruments for the limitations. Direct legislation allows voters to bypass the legislative process and place constitutional or statutory initiatives, such as tax and expenditure limitations, directly on the ballot. Recall procedures allow voters to remove from office elected officials through referendum. While her ordinary least squares and fixed-effects estimates find little evidence of lower spending, her instrumental variables estimation finds that the tax and expenditure limitations reduce states general expenditures as a percent of personal income by two percentage points. She finds that this reduction is partially offset by higher local spending in these states—evidence that states may substitute mandated local spending for centralized states expenditures. This last results illustrates the interactive nature of these institutions, and Rueben is one of few researchers to have confronted both the endogenous and interactive nature of these fiscal constraints.

While these studies have addressed the problem of institutional endogeneity, with the exception of Rueben (1995), less work has been done on the important interaction effects. For example, the effect of a tax limitation on public expenditures may depend upon the balanced budget rule. Without a strong balanced budget rule, a state forced to reduce taxes may decide to not reduce expenditures and run a deficit. With a strong balanced budget rule, the state will be legally required to match reduced taxes with reduced expenditures. Similarly, one might believe that traditional tax limits and supermajority rules for tax increases are substitutes. Yet Table 1 shows that some state have both, some neither, and some one or the other alone. The next section examines supermajority rules and their effects on spending and revenue growth.

Supermajority voting requirement for tax increases

A supermajority requirement is a constitutional amendment requiring greater than 50 percent legislative vote in both chambers for either a tax increase or a new tax. Twelve states have adopted these requirements as constitutional amendments and sixteen additional state legislatures along with the federal government are considering adoption. The U.S. Congress has voted three times to initiate a two-thirds supermajority constitutional amendment; each time it received a simple majority but not the two-thirds majority required to initiate a constitutional amendment.[1] Supporters have promised to bring the supermajority requirement to a vote every April 15, to coincide with the tax-filing deadline.

Temple (1998) and Knight (1998a) are the only studies on the fiscal effects of supermajority requirements. To control for selection on unobservable characteristics, Temple uses both a fixed-effects and a random growth model from the literature on job training program evaluation. The random growth model includes both fixed effects and a time-invariant, state-specific growth rate in the unobservable variables. She interprets the random growth model as allowing for the possibility that the decision to adopt a limit is a function of an unobservable

[1] To amend the U.S. constitution requires first that either (i) two-thirds of both federal chambers vote to propose an amendment, or (ii) two-thirds of state legislatures vote to convene to propose amendments, and second that three-fourths of the states then ratify the proposed amendment.

state-specific revenue growth rate. Using state-level data from 1970-1994, she concludes that supermajority limitations do not reduce the level of taxation.

Knight focuses on the potential endogeneity of supermajority requirements and draws a different conclusion. First, in an ordinary least squares regression of taxes on supermajority requirements and other state characteristics, Knight finds a small and statistically insignificant coefficient on the indicator for states with supermajority rules. However, he notes that unobserved state attitudes towards taxation will influence both adoption of supermajority requirements and tax policy. Consequently, one cannot distinguish between the effect of such requirements on taxes and the correlation between these requirements and unobserved attitudes towards taxation. He describes two econometric methods that deal with this problem.

Knight's first approach is to include state and year fixed effects to control for the unobserved attitudes towards taxation. When these additional regressors are included, the coefficient on the supermajority indicator is statistically significant, large, and negative. This fixed-effects coefficient can be interpreted as the average change in taxes resulting from adoption of a supermajority rule, holding constant each state's time-invariant unobserved characteristics.

In a second approach to the endogeneity of the supermajority requirements, Knight uses instrumental variables. He notes that the states' supermajority rules are initiated in one of two ways: either by voters through direct legislation or by the legislature. One-half of states with supermajority rules enacted them via voter direct legislation; the other half were enacted by legislatures. In the first case, states' rules regarding voter legislation will be a good instrument for the presence of supermajority rules. Voter direct legislation allows citizens to circumvent the legislative process and place constitutional amendments directly on the ballot, thereby making adoption of constitutional amendments easier. In states where it is easy for voters to pass legislation, supermajority rules will be more prevalent, regardless of the state's inherent tax aversion.

In the second case, states' legislative rules regarding constitutional amendments will be a good instrument for the presence of supermajority rules. Many states (as well as the federal government) require more than a simple majority to initiate a constitutional amendment, making such amendments more difficult to enact. This requirement reduces the likelihood of supermajority requirement adoption because it requires the support of more legislators.[2] Similarly, as of 1995 twelve states required approval of two legislative sessions to initiate an amendment, making the amendment process longer and more difficult. For example, the 1997-1998 Wisconsin Assembly voted in favor of a supermajority requirement for tax increases, but the amendment cannot proceed until the 1999-2000 Assembly also passes it. After the next elections, the legislature may change political leadership and may not approve the amendment.

Knight thus uses three instruments: ease of access to voter direct legislation, supermajority requirements for state constitutional amendments, and the number of legislative sessions required to amend the state constitution. Each should be correlated with the likelihood of a state having a supermajority requirement and uncorrelated with states' underlying tax preferences. He regresses the supermajority requirement for tax increases on these three instruments and other exogenous state characteristics, and then regresses state taxes on the predicted supermajority requirement and exogenous state characteristics in a second stage regression. The resulting coefficient on the predicted supermajority rule for tax increases is large, negative, and statistically significant.

[2] Assuming that the presence of the supermajority rule for constitutional amendments did not change Representative's votes, the U.S. House of Representatives would have passed the supermajority requirement for tax increases in both 1996 and 1997 if the constitution had required a simple majority rather than a two-thirds supermajority to initiate the amendment.

Table 2 summarizes these results as the marginal effects of introducing a two-thirds supermajority requirement. For the OLS estimates, a supermajority requirement is associated with a small, statistically insignificant reduction in the tax rate of 0.24 percentage points, a reduction of 3.4 percent relative to the sample average of 7.12 percent. For the fixed-effects and instrumental variables estimates, a supermajority requirement is associated with large, statistically significant reductions of 8 percent and 47 percent respectively, relative to the sample average. For regressions with the expenditure rate as the dependent variable, summarized in the second row, the marginal effects of a supermajority requirement are larger in absolute terms than those for the tax rate but similar in percentage terms, relative to the sample average of 12.8 percent.

In addition to addressing the endogeneity of the supermajority rules, Knight also studies the interaction between supermajority requirements and state balanced budget rules. States with stringent balanced budget rules should experience greater reductions in expenditures due to supermajority requirements. The natural way to study these interactions would be to include multiplicative interaction terms in the regression analysis. In other words, one would regress state taxes or expenditures on supermajority rules, balanced budget rules, and an indicator for states with both institutions. As described in the introduction, these interactive terms create econometric problems when combined with instrumental variables estimation; specifically, two-stage least squares is no longer appropriate when the regression model is non-linear in the endogenous variable, the supermajority variable in this case. Therefore, Knight estimates the model separately for states with and without strict balanced budget rules; these results are summarized in the bottom three rows of Table 2.

For the fixed-effects model, states with strict balanced budget rules and supermajority rules experience unexpectedly smaller and statistically significant reductions in spending than states with weak balanced budget rules and supermajority requirements; the result is reversed in the instrumental variables model although it is statistically insignificant. Thus, there is little evidence of an interactive effect between balanced budget rule and supermajority rules. However, this division of the already small sample of 50 states into those with and without strict balanced budget rules results in even smaller samples, which may account for the insignificant findings in the third column. Knight's attempt to account for both endogeneity and the interactions with balanced budget requirements illustrates the difficulty confronting this literature: techniques for addressing the two problems simultaneously are seemingly incompatible with small sample sizes. The next section examines balanced budget requirements alone in detail.

State balanced budget requirements

In a sense, balanced budget requirements constitute the ancestor of all U.S. state fiscal institutions. Virtually every state has some requirement that its budget balance, and many of the provisions are hundreds of years old, appearing in states' original constitutions. Despite their prevalence, state balanced budget requirements vary widely in effectiveness.

The Advisory Commission on Intergovernmental Relations (ACIR, 1987) documents five types of balanced budget requirements: (1) the governor has to submit a balanced budget; (2) the legislature has to pass a balanced budget; (3) the state may carry over a deficit but must correct it in the subsequent budget period; (4) the state may not carry over a deficit into the next budget period (often 2 years long); and (5) the state may not carry over a deficit into the next fiscal year. The first two balanced budget requirements dictate the terms of budget *forecasts*, and do not affect what happens at the end of the year if *actual* expenditures exceed *actual* revenues. The third type of balanced budget requirement, which requires that *actual* deficits be corrected in subsequent years' budget forecasts, is also largely ineffective. States with such requirements may continue to carry over deficits from year to year so long as at the beginning of each year revenues are forecast to match expenditures. To comply with balanced budget requirements of this type, some states regularly overestimate future revenues and underestimate future expenditures (Briffault, 1996). The fourth and fifth types of balanced budget requirements require mid-year adjustments to be made when expenditures exceed tax revenues. The fourth type of balanced budget requirements constrains every budget period—two years for states with biennial budget cycles. The fifth constrains every fiscal year, regardless of the length of the state's budget cycle. In practice, only these most stringent balanced budget requirements matter to state fiscal policy.

As suggested above, one might fear that none of these balanced budget requirements is effective because states can make unbalanced budgets appear balanced by using budgeting "gimmicks." In recent years, such gimmicks have caused fiscal crises in New York City and in Michigan, suggesting that credit markets may impose fiscal constraints even in the absence of institutional limitations (Fisher, 1996). Furthermore, despite the prevalence of anecdotes describing deceptive governmental finance, most states' midyear budget gaps are closed using spending decreases or revenue increases, rather than deceptive interfund or intertemporal transfers (GAO, 1993). Finally, whether deceptive budgeting renders fiscal constraints ineffective is ultimately an empirical question that has recently been explored in detail.

Several studies have recently found that strong state balanced budget requirements have large effects on the size of state budget deficits and on states' propensities to run deficits. These papers are summarized in Table 3. The ACIR (1987) index is based on whether or not the state has one of the five types of balanced budget requirements, whether they are statutory or constitutional, and a subjective assessment of their importance by ACIR staff. The index ranges from 0 to 10, but most of the distribution is in the top several values. The average is 8.1, and the standard deviation is 2.6. Twenty-six states have the maximum ACIR index value of 10, and 42 states' indices are 6 or higher. In their study, the ACIR regresses measures of state budget balances on their index values and other explanatory values, and concludes that states with strict rules have lower deficits and lower state expenditures. Though the ACIR study uses a cross-section of 1984 data, its conclusions are supported by the other papers listed in Table 3, all of which use panels of data.[3]

The other studies cited in Table 3 also use the ACIR data on balanced budget requirements, though in differing formats. Alt and Lowry (1994) simultaneously estimate state revenues and expenditures, and find that states subject to strict balanced budget requirements eliminate deficits more quickly. Poterba (1994) finds that states with ACIR indices of 6 or higher enact relatively larger expenditure reductions in response to unexpected short-run deficits. Alesina and Bayoumi (1996) find the value of states' ACIR index to be positively correlated with states' primary budget surpluses. And Bohn and Inman (1996) show that states with the very strictest form of balanced budget requirement, prohibiting them from carrying budget deficits into the next fiscal year, have larger per-capita general fund surpluses and are less likely to run deficits. They explore many of the different types of budget provisions, including the ones at the low end of the ACIR scale, and find that only the strictest "no annual carry-over" requirements significantly affect fiscal policy.

Two recent papers look beyond balanced budget requirements' effect on taxes, to study their effect on the volatility of state economies. In particular, as opponents of balanced budget requirements have long argued, such rules force governments to operate procyclical fiscal policies, thereby exacerbating business cycle peaks and troughs. As the papers described in

[3] The difference between cross-section and panel data is less important for balanced budget requirements because

Table 3 show, to balance their books, fiscally constrained governments have to increase taxes and decrease spending during recession years. Even without a Keynsian multiplier effect, the decrease in government spending at the same time as a decline in private spending will exacerbate economic volatility.

To measure the effect of balanced budget requirements on economic volatility, Alesina and Bayoumi (1996) regress the standard deviation of annual gross state product from 1965 to 1992 on average gross state product over those years, the percentage of state product from mining, an indicator for southern states, and the ACIR index of state balanced budget stringency. They find a small, negative, and statistically insignificant coefficient on the index, and conclude that balanced budget rules have had no intensifying effect on business cycles. However, they do not address either the possibility that unobserved state characteristics are correlated with both business cycle fluctuations and the existence of state balanced budget rules, or the possible interactions between balanced budget rules and other state fiscal institutions.

Levinson (1998) takes a somewhat different approach to measuring the effect of balanced budget rules on state business cycle volatility. It compares business cycles in the 29 states with the strictest balanced budget provisions, to those in the other 21 states. For a measure of business cycle volatility, that paper uses the standard deviation of the deviation of log personal income per capita from its quarterly growth rate over the period 1969-1995. In other words, the log of personal income per capita was regressed on a time trend, and the standard deviation of the residuals from that regression is used as a measure of volatility.

For the 21 states with lenient balanced budget requirements, the average standard deviation is 0.0442. For the 29 strict states the average is 0.0335. The difference is 0.0107, suggesting that lenient states have *more* volatile business cycles on average. While this conflicts

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there have been virtually no changes in states' requirements during the past several decades.

with the intuition that balanced budget requirements increase volatility, it may easily be explained if states have characteristics that are correlated with both mild business cycles and strict balanced budget requirements.

To address this issue, Levinson compares states with strict and lenient balanced budget requirements indirectly, by examining their cyclical fluctuations relative to a second state attribute that is beyond the control of state governments (exogenous) and correlated with the effect of state fiscal policy on business cycle fluctuations. That second attribute is the size of the state, as measured by its population.

Intuition suggests that the fiscal policies of smaller (less populous) states matters less to those small states' economies than do the fiscal policies of larger states to their larger economies. Two pieces of empirical evidence support this contention. First, relatively more workers commute to or from small states to their jobs each day.[4] Second, the fraction of goods and services consumed by each state that comes from outside the state is larger for small states than for large states.[5] These differences in commutation patterns and import propensities suggest that large states have more ability to affect their local economies through fiscal policy. A greater share of the economic incidence of small states' taxes and expenditures will be borne by other states' residents. Any constraint on fiscal policy, such as a balanced budget requirement, will affect the economies of large states more than the economies of small states.

If state government deficits affect large states more than small states, then the volatility difference between lenient and strict states should be larger (more negative or less positive) for large states than for small states. Consider an extreme example. Suppose small states' cyclical

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[4]Calculations from U.S. Census show that in 1990 in the smallest 25 states, 4.5 percent of working residents commuted across state borders, while in the largest 25 states 3.2 percent of workers commuted interstate.

[5]A 1981 study (RSRI) of the 1977 Commodity Flow Survey conducted by the U.S. Department of Transportation calculated this "import propensity" for each of the 48 continental states. These import propensities range from a low of 0.33 for California to a high of 0.65 for North Dakota. The largest 24 states' average was 0.45, while the smallest 24 states' average was 0.53.

fiscal policies have absolutely no volatility consequences, while large states' policies have some. Then the lenient-strict volatility difference for the small states is purely a function of other things, and the lenient-strict difference for the large states will be a function of the other things *plus* the effect of the strict balanced budget requirements. If the other effects are similar for both large and small states, then the difference between the lenient-strict volatility difference for large states and the lenient-strict volatility difference for small states will be a function of the balanced budget requirements alone.

Table 4 presents information from Levinson (1998) supporting this conjecture. The first row of Table 4 shows the difference between the lenient and strict states for the largest 25 states only. That difference is 0.0055. The second row presents the lenient-strict difference for the smallest 25 states, and that difference is 0.0282. For the large states, the difference is less positive than for the small states, suggesting that fiscal policy matters more for large states and reduces business cycle volatility. The difference between these two differences (-0.0228) is statistically significant at 10 percent.

There is an alternative way to examine the data in Table 4. Note that small states have more volatile business cycles than large states. This difference may be due to their economies being less diverse and therefore more sensitive to industry-specific shocks, or it may simply be due to a law-of-large-numbers phenomenon. First examine the lenient states. For the large lenient states the measure of cyclical volatility is 0.0339, and for the small lenient states it is 0.0649. The difference is -0.0311. Next, examining the strict states only, the large-small difference is -0.0083. Thus large states have less volatile business cycles, and for lenient states the large-small difference is even more pronounced. A potential explanation for the difference in these differences (-0.0228) is that the large states have smoother business cycles in part because they are able to have effective countercyclical fiscal policy. When large states have stringent balanced budget requirements, they are less able to deficit spend during recessions, and their business cycles look more like those of small states.

As for the magnitude of these differences, the average size of business cycle fluctuations over all 50 states, as measured by the standard deviation from the trend in log personal income per capita, is 0.0389. The difference-in-differences reported in Table 4 is –0.0228, a large fraction of overall cyclical variation. The implications are therefore significant empirically as well as statistically: in large states, where fiscal policy matters most, the absence of a strict balanced budget requirement is associated with cyclical fluctuations that are less than half as volatile as the national average.

The calculations in Table 4 can be conducted in a regression framework by regressing volatility on a dummy variable for large states, another dummy variable for lenient states, and a third dummy variable for states that are both large and lenient. If no other regressors are included, the coefficient on the large-lenient dummy will be -0.0228, exactly the value of the difference in differences reported in Table 4. When the states' average growth rate 1969-1995, personal income per capita in 1969, the shares of agriculture, mining, services and manufacturing in gross state product, and dummies for four census regions are included as regressors, the coefficient on the interactive term lenient-large dummy remains large, negative and statistically significant (-0.0210).

While the results in Levinson (1998) rely on the assumption that fiscal policy affects large states more than small states, they have important implications. First, they indicate that strict balanced budget requirements exacerbate business cycle fluctuations in precisely the way most economists expect—by forcing state governments to raise taxes and reduce expenditures during recessions. Second, they imply that state governments' fiscal policies affect their local macroeconomic conditions, which in turn has obvious implications for all of the fiscal institution studied in this paper.

Rainy day funds

As a consequence of the fiscal institutions described in this paper – especially tax and expenditure limitations, balanced budget requirements, supermajority rules for tax increases, and prohibitions on unfunded mandates – states have found themselves increasingly constrained in their ability to raise taxes quickly or run fiscal deficits in the event of an unexpected revenue shortfall. Partly in response, starting around 1980 many states began enacting budget stabilization funds, often called "rainy day funds," that allow them to store surpluses to be spent during revenue shortfalls. Prior to 1981, few states had such funds (Gold, 1981, 1984). By 1983, 19 states had rainy day funds in place, and by 1994, 44 states had them (Sobel and Holcombe, 1996). Table 5 depicts the rapid recent spread of these contingency funds.

While a number of researchers have documented the expansion of rainy day funds, as well as the increases in their funding levels, only Sobel and Holcombe (1996) have systematically explored their fiscal consequences. They measure each state's "fiscal stress" during the 1990-91 national recession, where fiscal stress is defined as the amount of discretionary tax increases *plus* the amount of expenditure reductions from their long run trend growth during the recession, divided by the total state budget. They then regress fiscal stress on an indicator for whether the state had a rainy day fund in 1989, a dummy variable for whether explicit formulas or mandates dictate fund contributions during boom years, a dummy variable for whether the fund balances are capped, a variable that measure the size of that cap, as a percent of the state budget, a dummy indicating whether the state's fund can be accessed by regular legislative action, and a measure of the severity of the 1990-1991 recession. The rainy day coefficient is positive and statistically insignificant. However, the coefficient on required contributions is significant and negative, indicating that only those rainy day funds with mandatory contributions matter.

One largely unexplored consequence of these rainy day funds is their interaction with the other fiscal institutions described above. For example, balanced budget requirements should matter less for states with ample rainy day funds. Levinson (1998), described above, does examine the data for the period prior to 1980 and after 1980 separately. This division is motivated by the fact that business cycle swings were larger prior to 1980. It does seem that balanced budget requirements had a larger absolute affect on business cycle volatility before 1980 than after 1980, though that may due to other phenomena such as the 1970s oil shocks, or to changes in national monetary policy.

To test whether rainy day funds affect the efficacy of balanced budget requirements, Levinson redefines strict balanced budget requirements as those that do not allow deficits to be carried into subsequent fiscal years, in states without rainy day funds by 1989 (NASBO, 1989). For these 10 states, the difference between the lenient-strict difference in state volatility in large states and the lenient-strict difference in small states was -0.0156. Recall that for the 21 states with strict balanced budget requirements, ignoring rainy day funds, the analogous difference in differences was -0.0228. The implication is that rainy day funds smooth out cyclical variation or weaken the effect of strict balanced budget requirements.

Though suggestive, Levinson's estimate of the interactive effect of rainy day fund and balanced budget requirements deserve caution. First, the difference between the two measures of balanced budget requirements' effects, with and without rainy day funds, is not statistically significant. This, however, may be due to the fact that only two states (Texas and North Carolina) are both large and without rainy day fund as of 1989. By 1992, even these two states had adopted rainy day funds, rendering the analogous analysis impossible. Second, these rainy day funds differ in scope, flexibility, and levels of funding. Clearly rainy day funds are meaningless if their balances are small. In sum, while the post-1980 decrease in the measured effect of balanced budget requirements may be due to rainy day funds, one cannot rule out the simultaneous decline in aggregate national shocks relative to those of the 1970s. As with the other state fiscal institutions, because of the relatively small sample of states, it is difficult to deal with both the endogeneity of balanced budget requirements and rainy day funds and their potential interactions.

Line-item veto

The line-item veto allows state governors to veto specific spending or taxation provisions of budget legislation, rather than being required to approve or veto the entire legislative package. In 1996, the U.S. Congress approved line-item veto authority for the President. A U.S. District Court ruled the line-item veto unconstitutional in February 1998, and the Supreme Court upheld that ruling in June 1998. Supporters have promised either to reintroduce the line-item veto in a way that does not violate the constitution or to amend the constitution to include explicitly presidential line-item veto power. As of 1995, forty state governors had some form of line-item veto authority.

The theoretical effect of line-item veto authority on spending is ambiguous. Different models of budgetary determination predict different effects of the line-item veto on public expenditures and taxes. Using a median-voter model, Holtz-Eakin (1988) shows that the line-item veto can increase or decrease the public sector size depending on the distribution of voter preferences across districts. The governor is elected by all districts, and thus his preferred spending level represents the spending level preferred by the median voter across all districts.

By contrast, each legislator is elected by a different district, and thus the legislature's preferred spending level is that of the median point in the distribution of median voters across the jurisdictions. In this framework, the governor may prefer a lower or a higher spending level than the legislature.

In the model of Chari, Jones, and Marimon (1997), the executive has different preferences from legislators even with identical jurisdictions. With local public goods financed by universal taxation, legislators internalize only their portion of the tax liability created by the spending in their district. The executive will internalize these fiscal externalities and attempt to reduce expenditures in the districts with politically powerful legislators. Thus, by giving the executive more bargaining power, the line-item veto should lead to both lower and less concentrated total expenditures.

In keeping with the theoretical ambiguity noted by Holtz-Eakin, empirical research on the line-item veto finds only small effects on aggregate spending. Alm and Evers (1991) find that the line-item veto has a small, statistically significant, and negative effect on total state spending in those states where the political parties of the governor and legislature differ. They also find a small effect on the composition of state spending as states with the line-item veto have higher transportation spending.

Dearden and Husted (1993) focus on the ability of the governor to obtain his most preferred spending level. To this end, they use a National Association of State Budget Officers (NASBO) 1983-1989 panel data set that includes both the governor's recommended budget proposal and the final expenditure budget. They find that the line-item veto does result in a spending level closer to that preferred by the governor. The veto is most effective when the governor's opposition party controls the legislature but is short of the votes required to override the line-item veto.

The study of Dearden and Husted rely on the governor's proposal as an exogenous measure of the preferred budget of the governor. Strauch (1998) argues that an executive with line-item veto authority may strategically alter his initial budget proposal relative to an executive without this authority. Using a similar NASBO data set from 1987-1992, he finds evidence for this argument; a governor with line-item veto authority will offer a lower initial spending proposal than governors without this authority. He interprets this finding as evidence that governors use their line-item veto power to curtail legislative spending in the proposal stage of the budget.

Holtz-Eakin (1988) uses a 1965-1983 panel of states and finds that the line-item veto has no effect on long run budgets. In the short run, he finds that governors facing a legislature controlled by the opposition party use the line-item veto to alter the composition of spending. Democratic governors reduce current spending while Republican governors reduce capital spending. Governors of both parties use the line-item veto to shift from non-tax to tax revenues and increase grants to local governments. He concludes that the line-item veto gives the governor short-run political power over the budget but does not alter government spending in the long run.

Holtz-Eakin's use of state fixed effects is the only attempt at correcting for possible endogeneity of this institution. In addition to endogeneity, no studies have addressed the important interaction effects. For example, the line-item veto may provide the governor a key bargaining tool when attempting to comply with a balanced budget amendment. The governor can use this power to lower pork-barrel spending in districts of powerful legislators thus reducing total spending to projected revenue levels.

Mandate funding requirements

One way for states to circumvent the constraints imposed by fiscal institutions is to shift financial responsibility to local governments. A similar dynamic takes place at the federal level, where during the last decade, state and local governments have protested the power of the federal government to mandate public services provision without sufficient funding. In response, President Clinton signed the Unfunded Mandates Reform Act in 1995. In addition to a provision requiring that the Congressional Budget Office estimate the cost of bills with new federal unfunded mandates, the Act allows legislators to raise a point of order against any such bill, although the point of order may be overridden with a simple majority vote. While creating some procedural obstacles to unfunded mandates, this Act does not require the federal government to provide funding for mandates. By contrast, nineteen states have rules that require full funding for state mandates on local governments.

Empirical work has focused on the incidence of unfunded mandates. Baicker (1997) studies the effects of recent federally mandated Medicaid expansions. She concludes that the entire burden of unfunded mandates is borne by decreases in other public welfare spending. This evidence seems to suggest that funding requirements would lead to larger local government spending because they would not need to reduce spending in related categories, public welfare spending in this case, in order to comply with the mandate. However, this line of reasoning ignores the response of the central government to the funding rule; the central government may respond either by funding the mandate through grants or not mandating the spending.

Knight (1998b) uses evidence from the states to study the effects of mandate funding requirements. The paper first presents a theoretical model in which local governments provide a local public good using revenues from a tax on mobile capital. Without mandates, local governments provide inefficiently low amounts of public goods due to the fiscal externality created by the mobile capital. The state legislature, composed of representatives from each local government, thus issues mandates in order to increase provision to the efficient level. The legislature may decide not to fund the mandates for two reasons. First, capital taxes finance the public good with an unfunded mandate; thus, the cost of the public good is partially exported to foreign capital owners. Second, if a majority of mandate compliance costs are concentrated in a minority of the jurisdictions, the median legislator will prefer an unfunded mandate in order to avoid sharing the burden of compliance in high cost jurisdictions.

The model predicts that, in states that would otherwise use unfunded mandates, mandate funding rules will have two effects. First, funding rules increase the "price" of mandates to the median legislator leading to less mandates on local governments, increased competition for capital, and thus lower state and local combined government spending and taxes. Second, funding rules increase grants from state to local governments leading to a more centralized revenue system, measured by the proportion of combined revenues collected by the state government.

Empirically, the regressions in the paper support the first, but not the second effect. As evidence on the first effect regarding government size, funding rules reduce per-capita combined state and local non-grant revenue between $329 and $398, a reduction of 12 to 14 percent relative to the sample average of $2,756. Similarly, funding rules reduce per-capita combined state and local non-grant expenditure between $172 and $371, a reduction of 5 to 11 percent relative to the sample average of $3,332. As evidence on the second effect regarding the centralization of revenue, the paper finds the opposite effect; funding rules are associated with a lower proportion of total state and local revenues attributable to the state government and thus a less centralized tax system. The funding variable is associated with a 1 to 10 percentage point drop in centralization, a reduction of 2 to 17 percent relative to the sample average of 59 percent.

This result could be due to partially funded mandates which are not addressed in the model; the state legislature may respond to a funding rule by removing partially funded mandates leading to a less grants to local governments and thus a less centralized tax system.

To correct for possible institutional endogeneity, the paper employs both state and year fixed effects and instrumental variables similar to those in Knight (1998a), described above in the section on supermajority requirements. In this paper, the results with and without corrections for institutional endogeneity are equal in sign and similar in magnitude.

Conclusion

Using the lessons from studies of six types of fiscal institutions, this paper has summarized the empirical findings on the fiscal and economic effects of these institutions as well as approaches for addressing endogenous institutions and interactive effects. To account for the endogeneity of fiscal institutions, researchers have used two techniques: fixed effects and instrumental variables. Fixed effects use within-state variation over time in fiscal institutions in order to control for unobserved state characteristics that may be important determinants of the adoption of fiscal institutions. Instrumental variables estimation uses variation across states in exogenous determinants of the adoption of fiscal institutions. These variables, such as the difficulty of amending state constitutions, must not directly impact fiscal or economic outcomes. Both of these approaches have yielded substantially different results from comparable studies that use only evidence from cross-sectional variation across states in fiscal institutions in a standard regression framework. This difference in results suggests that these fiscal institutions are indeed endogenous, and that researchers and policy makers should treat with caution evidence obtained from simple cross-section differences between states with and without the institutions.

While a few studies have addressed interactions between fiscal institutions, this area needs future development. The techniques used thus far have are applicable to interactions between only two fiscal institutions. With more than two, the number of interactive terms or subsamples becomes too large given the small number of observations when using evidence from the fifty U.S. states, especially when simultaneously correcting for endogeneity. While the task is difficult, the evidence is crucial as policy makers often consider fiscal institutions in tandem.

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Table 1: list of fiscal institutions

State	Balanced budget rule (ACIR≥8)	Tax and expenditure limits (year)	Supermajority requirements (year)	Line-item veto	Mandate funding rules (year)
Alabama	√				1988
Alaska				√	
Arizona	√	1978	1992	√	
Arkansas	√	1982	1934	√	
California		1979	1979	√	1973
Colorado	√	1977	1992	√	1981
Connecticut		1991		√	
Delaware	√		1980	√	
Florida	√	1994	1971	√	1978
Georgia	√			√	
Hawaii	√	1978		√	1979
Idaho	√	1980		√	
Illinois				√	1981
Indiana	√				
Iowa	√			√	
Kansas	√			√	
Kentucky	√			√	
Louisiana		1979	1966	√	1991
Maine	√			√	1992
Maryland					
Massachusetts		1986		√	1981
Michigan		1978		√	1979
Minnesota				√	
Mississippi	√		1970	√	
Missouri	√	1980		√	1980
Montana	√	1981		√	1974
Nebraska	√			√	
Nevada		1979			1993
New Hampshire					1984
New Jersey	√	1976		√	
New Mexico	√	1987		√	1984
New York				√	
North Carolina	√	1991			
North Dakota				√	
Ohio	√			√	
Oklahoma	√	1985	1992	√	
Oregon		1979	1996	√	
Pennsylvania				√	
Rhode Island	√	1977			1979
South Carolina	√	1980		√	1993
South Dakota	√		1978	√	1978
Tennessee	√	1978		√	
Texas		1978		√	
Utah	√	1979		√	
Vermont					
Virginia				√	
Washington		1979	1993	√	1980
West Virginia	√			√	
Wisconsin				√	
Wyoming				√	
Total	29	25	12	42	19

Sources: Rueben (1995), Knight (1998a,1998b), NASBO Budget Processes in the States (1998), Levinson (1998).

Table 2: Supermajority marginal effects

Dependent variable	Ordinary least squares	Fixed effects	Two-stage least squares
Tax rate	-0.0024 (0.0017)	-0.0057 ** (0.0026)	-0.0322 ** (0.0079)
Expenditure rate, all states	-0.0022 (0.0026)	-0.0095 ** (0.0027)	-0.0564 ** (0.0124)
Expenditure rate, strict balanced budget states	-0.0025 (0.0027)	-0.0069 ** (0.0019)	-0.1392 ** (0.0456)
Expenditure rate, weak balanced budget states	-0.0113 (0.0071)	-0.0332 ** (0.0076)	-0.0870 ** (0.0273)
difference between strict and weak balanced budget states	0.0088 (0.0076)	0.0263 ** (0.0078)	-0.0523 (0.0532)

Source: Knight (1998a)
Notes:
1) The supermajority marginal effect the change in the dependent variable associated with introduction of a 2/3 supermajority requirement.
2) Tax rate is measured as tax revenue divided by private income.
3) Expenditure rate is measured as general expenditures divided by private income.
4) * denotes statistical significant at the 95 percent level

Table 3. Studies of the effect of balanced budget rules on state budget deficits.

Study	Years	Dependent variable	Definition of balanced budget stringency	Results
ACIR (1987)	1984	Budget balance divided by general expenditures, total state spending less federal aid.	Value of ACIR index.	States with strict rules have lower deficits and lower state expenditures.
Alt and Lowry (1994)	1968-87	Revenues and expenditures, simultaneously.	Indicator for states that may not carry over fiscal deficit (ACIR).†	States subject to balanced budget laws eliminate deficits more quickly
Poterba (1994)	1988-92	Difference between forecast and actual budget surplus (deficit).	Indicator for ACIR index ≥ 6.	Unexpected deficits cause larger expenditure reductions in states with strict balance budget rules.
Alesina and Bayoumi (1996)	1988-92	Primary budget surplus (deficit).	Value of ACIR index.	States with more stringent controls have larger primary budget surpluses.
Bohn and Inman (1996)	1970-91	General fund surplus (deficit).	Indicator for states that may not carry over annual deficit.†	States with balanced budget rules have larger per-capita surpluses, and are less likely to run deficits.

†Alt and Lowry, as well as Bohn and Inman, also experimented with *ex ante* provisions, such as that the governor must submit or the legislature must pass balanced budgets, and found no effects on expenditures or revenues.

Table 4. State business cycle volatility and balanced budget requirements: 1969-1995.

	Lenient states' volatility	Strict states' volatility	Difference
Largest 25 states			
Average	0.0339	0.0284	0.0055†
N	14	11	25
Std. Err.	0.0026	0.0019	0.0032
Smallest 25 states			
Average	0.0649	0.0367	0.0282*
N	7	18	25
Std. Err.	0.0129	0.0025	0.0131
Difference (Large-Small)	-0.0311*	-0.0083*	**-0.0228**†
Std. Err.	0.0132	0.0031	0.0135

*Statistically significant at 5 percent.
†Statistically significant at 10 percent.
Source: Levinson (1998)
Volatility is measured as the difference between the actual logarithm of state per capita income and that predicted by the average exponential growth rate 1969-1995.
Strict balanced budget requirements are those that do not allow deficits to be carried over into subsequent fiscal years.

Table 5: Number of states with Rainy Day Funds, Fiscal Years 1982-1994

Fiscal Year	Number of States with Rainy Day Funds
1982	12
1983	19
1984	23
1985	25
1986	28
1987	35
1988	35
1989	38
1990	38
1991	39
1992	43
1993	44
1994	44

Source: Sobel and Holcombe (1996).