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8 State Fiscal Institutions and the U.S. Municipal Bond Market

James M. Poterba and Kim Rueben

The effects of fiscal institutions on budget deficits, the level and composition of government spending, and the level of government indebtedness are topics of active interest in both economics and political science. Much of the motivation for ongoing research on these issues stems from the fiscal policy experience of developed nations during the last two decades, particularly the rise of substantial peacetime budget deficits. As policymakers have sought methods to reduce deficits and limit the growth of government debt, they have considered a range of possible changes in the institutional structure for fiscal policymaking. Because fiscal policy reforms are relatively rare at the national level, and because such reforms are likely to be correlated with other changes that may affect fiscal policy outcomes, it is difficult to develop empirical evidence on the effects of such institutional changes.

One alternative source of empirical evidence on the effects of budget rules involves comparisons of fiscal policy outcomes across different subnational governments in a federal system. We focus on the states within the United States. While states differ substantially in their incomes, tax bases, and levels of spending as a share of personal income, they operate in a homogeneous legal environment and face many of the same fiscal pressures. They nevertheless exhibit substantial disparities in their budgeting rules and fiscal policy institutions.

Studies of interstate differences in fiscal institutions and fiscal policy have

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produced a growing body of evidence suggesting that fiscal institutions affect the size of state government, the incidence of fiscal deficits, and the level and composition of state borrowing. Rueben (1996) presents evidence of a negative correlation between tax and expenditure limits and state government spending as a fraction of state income. Alt and Lowry (1994), Bohn and Inman (1995), and Poterba (1994) document a negative correlation between state antideficit laws and the average size of state budget deficits. Bunche (1991), Eichengreen (1992), Kiewiet and Szakaly (1996), and von Hagen (1991, 1992) find that states with constitutional restrictions on the legislature's power to issue general-obligation debt issue less debt, and rely more heavily on revenue bonds and other "off-budget" debt, than states without restrictions on debt issue. These studies and others, summarized by Alesina and Perotti (chap. 1 in this volume), Inman (1996), and Poterba (1997), cast doubt on the view that fiscal institutions are simply "veils" that voters see through, with no ultimate effects on fiscal outcomes, and they suggest that changes in fiscal institutions can have real effects on policy choices.

Antideficit rules can affect measured deficits in two ways. First, they may lead to changes in the primary deficit because they constrain the actions, and the incentives, of fiscal policymakers. Second, they may affect the bond market's perception of the borrowing jurisdiction, and thereby affect the required interest rate on outstanding debt. Proponents of antideficit rules argue that such rules should result in lower interest payments, as bond market participants demand a lower risk premium as compensation for potential default.

While numerous studies have examined how fiscal institutions affect primary deficits, there is much less research on how financial markets react to differences in fiscal rules. There is a small literature, including studies by Eichengreen (1992), Goldstein and Woglom (1992), Bayoumi, Goldstein, and Woglom (1995), and Lowry and Alt (1997), on the correlation between fiscal institutions in the U.S. states and the interest rates on the bonds issued by these states. The present paper extends this research by examining a broader range of fiscal institutions, and by paying particular attention to the effect of tax and expenditure limits on borrowing costs. We also study bond market data for the 1973–95 period, a substantially longer sample than earlier studies, and one that includes the state fiscal crisis of the early 1990s.

The paper is divided into four sections. The first describes the conceptual model of bond market equilibrium that underlies our analysis and summarizes previous research on the link between fiscal institutions and borrowing costs. Section 8.2 describes the data on state-specific interest rates and state fiscal institutions that form the basis for this study, and it discusses a variety of issues surrounding specification and estimation. Section 8.3 presents our central findings on the link between fiscal rules and state borrowing costs. We present empirical results from a range of different regression models that explain the level of tax-exempt bond yields. Section 8.4 concludes, suggests several directions for future work, and discusses the key tradeoffs that are involved in selecting fiscal institutions.

8.1 Theoretical Framework and Previous Research

This section summarizes the model of tax-exempt bond yield determination that underlies our empirical analysis. It also presents a brief review of previous studies that have investigated the link between fiscal institutions and interest rates, and places the current research in perspective.

8.1.1 Theoretical Framework

We assume that the market for tax-exempt bonds clears by equating the after-tax return that a "marginal investor" can earn on tax-exempt bonds with the after-tax, risk-adjusted return that is available on a riskless taxable bond. If R_T denotes the taxable yield, and $R_{M,i}$ denotes the tax-exempt yield on bonds issued by state i, then asset market equilibrium requires that $R_{M,i}$ equal the after-tax taxable yield, plus a risk premium:

$$(1) \qquad R_{M,i} = [1 - \tau_{f,i}(B) - \tau_{s,i}(B_i)]R_T + \sigma_i(Z_i, X_i, B_i).$$

In this expression, $\tau_{f,i}$ denotes the marginal federal income tax rate on interest income for an investor in state i, B represents the aggregate stock of tax-exempt debt outstanding, B_i denotes the outstanding debt stock for state i, and $\tau_{s,i}$ denotes the marginal state income tax rate on interest received by residents of state i. By including state taxes in this expression we implicitly assume that the taxable bond is not a Treasury bond. The implicit assumption in this expression is that tax-exempt debt issued by state i is held only by investors who live in state i. There is usually a tax incentive for state residents to hold in-state tax-exempt bonds: many states tax out-of-state "tax exempt" interest, even though such interest is exempt from federal income taxation. While holding in-state debt may not be attractive from a portfolio diversification standpoint, we nevertheless assume that the tax benefits lead to such ownership patterns.

The outstanding stock of debt in state i, B_i, affects equilibrium yields in two ways. First, the size of B_i can affect the state marginal tax rate of the "marginal investor" holding the bonds issued by state i; we assume that B_i is not large enough to affect the marginal *federal* income tax rate at which the tax-exempt bond market clears. This change in the state marginal tax rate can affect the riskless after-tax return that the marginal investor earns on taxable bonds, and therefore the required return on state i's tax-exempt debt. In the simplest clientele models of capital market equilibrium, the first investor to hold tax-exempt bonds is the highest marginal tax rate investor, and an increase in the stock of tax-exempt debt outstanding leads progressively lower marginal tax rate investors to purchase these securities. Formally, $\tau'_{s,i}(B_i) < 0$. The size of this effect depends on the degree of progressivity in the state income tax schedule. If the state has a flat-rate income tax, then $\tau'_{s,i} = 0$. Even in these cases, changes in B_i may still affect required returns through a risk premium effect.

Second, the stock of outstanding debt can affect the risk premium demanded by investors holding state i's bonds. The second term in equation (1), the risk premium on tax-exempt debt issued by state $i(\sigma_i)$, depends on Z_i, a vector of

state budget and tax institutions that affect the expected future supply of tax-exempt debt from state i and the probability of future payment of current interest obligations; X_i, a set of state-specific economic factors, such as the unemployment rate, that affect the probability that the state will be able to repay its obligations; and B_i, the outstanding stock of debt issued by state i. Although state defaults are rare, they have occurred. English (1996) and Ratchford (1941) discuss U.S. state defaults in the nineteenth century in some detail; most of these were the result of aggressive state borrowing to develop unprofitable canal systems in the 1830s and 1840s. The link between the outstanding debt stock and the risk premium is straightforward: for a given state economy, a larger debt burden corresponds to a greater risk of being unable to meet interest obligations. The link between economic conditions and the risk premium is also clear: for a given debt stock, the larger the economic base in the state, ceteris paribus, the lower the chance that the state will default. This is because a larger economy generates a larger tax base.

Fiscal institutions (Z_i) can affect the risk premium on state bonds for several reasons. Rules that make it more difficult for states to raise taxes increase the likelihood of future default on promised interest payments. Antideficit provisions in state constitutions and rules that limit the power of the legislature to issue new debt may affect the future supply of state debt, and therefore alter the chance that the future supply of debt will expand and drive down bond prices.

While the stock of debt outstanding can affect a state's risk premium, the stock of debt outstanding may also *be affected* by the prevailing interest rate on the state's bonds. Metcalf (1993) models the debt issue decisions of states, and finds that more debt is issued when interest rates are lower. Capeci (1994) presents a related empirical study of debt yields and debt issuance decisions by local governments. The interactions between the debt stock and the interest rate complicate our empirical analysis.

Most previous studies of fiscal rules and borrowing costs have included the outstanding debt stock in equations explaining the yields on state general-obligation bonds, but this leads to two problems. First, the stock of debt is endogenous, which means it is difficult to interpret the estimated coefficient on this variable. Second, because some of the variation in the stock of outstanding debt may be due to differences in fiscal institutions across states, controlling for interstate differences in debt outstanding may understate the potential effects of fiscal rules on borrowing costs. We estimate bond yield models with, and without, controls for the outstanding state debt stock as a way to investigate the importance of these effects.

If we were prepared to assume that fiscal institutions did not affect the risk premium on state bonds, then indicator variables for the presence of these institutions would provide instrumental variables that could be used to estimate a structural model of tax-exempt yield determination. The supply of debt from state i would depend on its fiscal institutions, but, without the risk premium effect, these institutions could be excluded from the demand equation for state

i's bonds. We could then estimate two-stage-least-squares models for state borrowing rates as a function of debt stocks and state economic conditions, recognizing the endogeneity of the debt stock. The estimate of the coefficient on the stock of debt in such a debt demand equation, in conjunction with first-stage estimates of how fiscal institutions affect state borrowing, could then be used to estimate the effect of fiscal institutions on state borrowing costs. Because we find statistical evidence against these identification assumptions, however, we pursue a reduced-form strategy in the estimation reported below.

8.1.2 Previous Research

There have been many previous studies of yield determination in the tax-exempt bond market. Most of these studies, which are surveyed in Fortune 1996 and Poterba 1989, compare an index of yields on tax-exempt bonds with the yields on Treasury bonds. The emphasis is therefore on explaining the time series variation in the relative yields on taxable and tax-exempt bonds. Other studies have considered the impact of state-specific factors on state and local borrowing costs, typically using data on the net interest cost (NIC) of specific bond issues. Examples of studies in this vein are Kidwell, Koch, and Stock 1984 and Lovely and Wasylenko 1992. Both of these studies explore the relationship between state income tax codes and state borrowing costs.

The present study is closely related to a number of previous investigations of the relationship between fiscal institutions and the borrowing rates faced by U.S. state governments. All of these studies analyze data from the Chubb Relative Value Study, but they focus on different sample periods. These data are now available for the period 1973–96, and we exploit the full data sample in our analysis.

The first studies of fiscal institutions and general obligation bond yields, by Eichengreen (1992) and Goldstein and Woglom (1992), relate the interest rate on general-obligation debt to an index of the strictness of state antideficit provisions compiled by the Advisory Council on Intergovernmental Relations (ACIR). Eichengreen (1992) examines the relationship between interest rates and (i) an indicator variable for whether the state can carry a deficit from one year to the next, and (ii) the ACIR index. For the 1985–89 period, he finds that both variables are correlated with the interest rates on general-obligation bonds. His estimating equations do not include any controls for state economic conditions. Goldstein and Woglom (1992) study the 1982–90 period, and they also find evidence that the ACIR index of deficit limits matters. They estimate that states with the most restrictive set of fiscal limits face interest rates five basis points lower than states with "average" limits.

The results in both of these studies are difficult to interpret because in addition to control variables for the level of state indebtedness and the observed state deficit, the regression specifications also include a measure of the rating on general obligation debt as reported by Moody's or another rating agency. Yet the state's credit rating, just like its borrowing cost, may depend on its fiscal institutions. If all of the information about future fiscal conditions that

was associated with the presence of a fiscal limit is incorporated in the state's bond rating, and if bond ratings perfectly predict borrowing costs, then it would be possible for the state's borrowing rate to be uncorrelated with fiscal institutions, *conditional* on the state's bond rating, even if changes in fiscal institutions have important effects on borrowing costs. Capeci (1991) discusses the role of credit ratings and economic conditions in determining tax-exempt interest rates. Controlling for the state's bond rating in an interest rate equation can therefore mask the effect of the fiscal variables.

The most sophisticated study of bond yields to date is that by Bayoumi, Goldstein, and Woglom (1995). They analyze interest rate data for the 1981–90 period, and also conclude that fiscal institutions, as measured by the ACIR index, affect state borrowing costs. This study recognizes the potential endogeneity of the level of state debt, and it applies an instrumental variables strategy to estimate the effect of fiscal limits on borrowing costs. The instruments are a set of "year dummies," a set of state-specific demographic variables, and the trend growth rate in state product. Each of these variables could, under plausible modeling assumptions, be correlated with bond yields through channels other than their effect on the endogenous variables, so the ultimate success of this empirical strategy is open to question.

A final paper that explores the Chubb data, by Lowry and Alt (1997), is also concerned with the link between state fiscal institutions and interest costs. The novelty of this paper, however, is the investigation of how fiscal rules interact with economic conditions in determining bond yields. This study estimates statistical models in which the state's borrowing cost depends on the current level of the state deficit, and the interaction of this deficit with the state's fiscal rules. The key finding is that the bond market's reaction to a state deficit depends on whether or not the state has a balanced-budget requirement. States with balanced-budget rules experience smaller increases in their borrowing costs for a given deficit, measured using data from the Census of Governments. Lowry and Alt's (1997) findings, which are based on data for the 1973–90 period, suggest that capital market participants consider the presence of antideficit rules, and their interplay with state economic conditions, in pricing state general-obligation bonds.

The foregoing studies consider a limited range of fiscal institutions in analyzing the determinants of tax-exempt bond yields. Virtually all of the studies consider the ACIR index of state fiscal stringency, which provides a general guide to state antideficit provisions. Yet this index suppresses substantial variation in state fiscal rules. Bohn and Inman (1995), for example, examine the fiscal impact of nine different indicators of state fiscal stringency in their study of state deficit determination. They find that a number of more specialized variables, such as requirements for gubernatorial submission of a balanced budget, legislative passage of such a budget, and a referendum to approve new state debt issues, have distinct effects on budget outcomes. Their study suggests that it is possible to move beyond a single summary statistic for state

fiscal stringency, and the present study therefore explores a broader menu of fiscal institutions than previous bond market studies.

The second important innovation in the current study is the use of the full data sample for the Chubb Relative Value Survey. Of the earlier investigations, only Lowry and Alt (1997) use data for the period before 1980, and none use data from the years since 1990. (This is not an indictment of methodology: several of the studies were completed before the post-1990 data were available.) The pre-1980 and post-1990 data may, however, provide important evidence on fiscal institutions and borrowing costs, since both of these periods were times of extreme fiscal stress for states. The New York City fiscal crisis of the mid-1970s, and the coincidence of an economic downturn and rising state spending needs that led to the state "fiscal crisis" of the early 1990s, are included in our sample period.

A final innovation is our consideration of the possible endogeneity of state fiscal institutions. The potential endogeneity arises from the fact that these institutions are not fixed, but can be changed by voters and legislatures. Besley and Case (1994) have argued that many of the policy differences across states, and within states over time, that are treated as exogenous in empirical research are in fact reflections of underlying voter tastes or economic conditions. It is therefore possible that fiscal institutions are simply a reflection of voter preferences, and as such, that the correlation between these institutions and fiscal policy outcomes just reflects an underlying correlation between voter tastes and fiscal policies. Rueben (1996) finds that the relationship between state fiscal institutions and state spending depends critically upon whether these institutions are treated as exogenous or endogenous. We address the potential endogeneity of fiscal institutions, with limited success, in our empirical work below. To anticipate our findings, we do not find any potential instrumental variables with significant explanatory power for fiscal rules, that is, variables that generate well-fitting "first stages" in a two-stage-least-squares setting. Treating the endogeneity of fiscal rules is therefore an issue that requires further work.

8.2 Data and Estimation Strategy

The estimation strategy we pursue is largely determined by the available data on state general-obligation bond yields. Our dependent variable, R_{it}, is the interest rate on 20-year general-obligation debt issued by state i as reported in the Chubb Insurance Company "Relative Value Survey." This survey, which has been carried out every six months since 1973, asks 20–25 sell-side bond traders at major brokerage houses that deal in tax-exempt bonds to estimate the current yields on general obligation bonds from 40 states. The states excluded from the sample—Arizona, Arkansas, Colorado, Idaho, Indiana, Iowa, Kansas, Nebraska, South Dakota, and Wyoming—are concentrated in the Midwest and Great Plains regions. The participants in the Chubb survey are asked to evaluate "hypothetical" general-obligation bonds that come due in 20 years,

so reported differences in yields should not be attributable to differences in call provisions or other factors, but simply to the perceived riskiness of the state's general obligation debt. Swartz (1989) discusses the responsiveness of these estimated yields to state economic circumstances, and notes that over the time period that the survey data has been collected, there has been some tendency for more rapid incorporation of news into yield spreads. In particular, he claims that while changes in the Chubb values lagged changes in bond ratings in the early part of the sample, they often led rating changes in more recent years.

8.2.1 The Chubb Data and Model Specification

The Chubb survey reports the *relative* yield on a general-obligation bond issued by state i, compared with a similar bond issued by New Jersey. This means that rather than estimating models for the level of the tax-exempt bond yields on bonds issued by state i, we are estimating models for the difference between the yields on the bonds issued by two states, $R_{it} - R_{jt}$, where j denotes New Jersey. To explore the implications of this, we can difference equations like (1) for two states, i and j, and find

$$(2) \quad R_{M,i} - R_{M,j} = [\tau_{s,i}(B_i) - \tau_{s,j}(B_j)]R_T + \sigma_i(Z_i, X_i, B_i) - \sigma_j(Z_j, X_j, B_j).$$

Several terms involving the taxable bond yield, the federal marginal tax rate, and any other systematic factors such as the risk of federal tax reform that affect the yields of all states in the same way, drop out of the expression when we difference the two state yields.

To translate this equation into a form that we can estimate, we linearize equation (2), suppress the M subscript, and add a time subscript for the tax-exempt yields (hence $R_{M,j}$ becomes R_{jt}). This yields

$$(3) \quad \begin{aligned} R_{it} - R_{jt} &= (X_{it} - X_{jt})*\alpha + (Z_{it} - Z_{jt})*\beta + (B_{it} - B_{jt})*\gamma \\ &\quad + (\tau_{s,it} - \tau_{s,jt})*\delta + \theta_t + (\kappa_i - \kappa_j) + (\varepsilon_{it} - \varepsilon_{jt}). \end{aligned}$$

In this expression, R_{it} denotes the nominal interest rate on bonds issued by state i at time t, X_{it} denotes the set of state-specific economic and fiscal conditions that may affect borrowing costs, Z_{it} represents the vector of state budget and tax institutions that may affect the demand for state tax-exempt debt, B_{it} denotes the stock of state debt outstanding, and $\tau_{s,it}$ denotes the top state income tax rate in state i in year t. In some specifications we omit the debt stock variable, for the reasons described above.

The error term in equation (3) consists of three components: a time effect θ_t that captures period-specific shifts in the relative risk premium for New Jersey (state j) relative to all other states; the difference in two state-fixed effects, κ_i and κ_j, which captures the average difference between state-specific factors that affect the borrowing cost for state i and New Jersey (state j); and $\varepsilon_{it} - \varepsilon_{jt}$, which represents the difference in the state-specific error components at time

t. Because most of the variation in fiscal institutions is across states but not across time within states, allowing for state-fixed effects substantially reduces the sample variation in fiscal rules. One important consequence of the data structure is that all of the independent variables need to be measured as deviations from the value for New Jersey.

While the use of survey methods rather than market prices to measure R_{it} raises questions about the reliability of the level of reported tax-exempt yields, our analysis focuses on *differences* in the yields on bonds for various states. Systematic errors in estimating the level of yields will therefore not contaminate the analysis. Previous work using these data, notably Bayoumi, Goldstein, and Woglom (1995), suggests that the yield spread between the highest yield and the lowest yield states responds to economic conditions, and that in recessions, when default risk rises, the range of yields in the Chubb survey increases substantially. By using the expanded data set we can also test how stable this relationship is over time.

The variables that we include in the X_{it} vector are the state unemployment rate, the level of real per capita income in the state, and state general fund revenues as a fraction of per capita income. State revenues are drawn from the U.S. Department of Commerce *State Government Finances* publications; state unemployment rates, population, and per capita income are from the Data Resources @MARKETS data file. We also include variables that proxy for the political climate in the state, on the grounds that such variables may provide information on the future evolution of state deficits. Our principal variable of this type is the Americans for Democratic Action (ADA) score for the state's Senate delegation; this should provide a general indication of the political ideology of the state.

We also include the highest state marginal income tax rate on interest income, as suggested by the equilibrium condition (1), in our regression models. This variable is collected from a review of state income tax forms, augmented with information from the State Tax Module of the NBER TAXSIM program. We lack detailed information on the state tax rates of the investors who own tax-exempt bonds, so we assume that all such investors face the state's highest marginal tax rate.

8.2.2 Measuring State Fiscal Institutions

We consider a range of variables on state fiscal institutions, $\{Z_{it}\}$, that may affect state borrowing costs. Briffault (1996) provides a useful introduction to the budget processes of the U.S. states. The first variable we consider is an index of state constitutional and legislative limits on deficit finance. This is the fiscal institution indicator that was analyzed in many of the studies described above. There is substantial heterogeneity in state balanced-budget rules. Only one state, Vermont, does not have a formal balanced-budget requirement. The balanced-budget requirements in the 49 states with such requirements can be broadly categorized into four groups, depending on the stage in the budget

Table 8.1 **State Fiscal Institutions**

State	Balanced-Budget Stringency	Debt Restriction	Year Passed: Spending Limit	Year Passed: Revenue Limit
Alabama	10	yes		
Alaska	6	yes	1982	
Arizona	10	yes	1978	
Arkansas	9	yes[a]		
California	6	yes[a]	1979	
Colorado	10	no	1992[b]	1992
Connecticut	5	no	1991	
Delaware	10	no		
Florida	10	yes[a]		1994
Georgia	10	yes		
Hawaii	10	yes	1978	
Idaho	10	yes	1980	
Illinois	4	no		
Indiana	10	yes		
Iowa	10	yes		
Kansas	10	yes[a]		
Kentucky	10	yes[a]		
Louisiana	4	no		1991[c]
Maine	9	yes[a]		
Maryland	6	no		
Massachusetts	3	no		1986
Michigan	6	yes[a]		1978
Minnesota	8	yes		
Mississippi	9	yes		
Missouri	10	yes[a]	1980	1980
Montana	10	none	1981	
Nebraska	10	yes		
Nevada	4	yes	1994[d]	
New Hampshire	2	none		
New Jersey	10	yes[a]	1976[e]	
New Mexico	10	yes		
New York	3	yes[a]		
North Carolina	10	none		
North Dakota	8	yes		
Ohio	10	yes		
Oklahoma	10	no	1985	
Oregon	8	yes[a]		
Pennsylvania	6	yes[a]		
Rhode Island	10	yes[a]	1992[f]	
South Carolina	10	yes[a]	1980	
South Dakota	10	yes		
Tennessee	10	no		
Texas	8	yes		
Utah	10	yes		
Vermont	0	yes		
Virginia	8	yes		

Table 8.1 (continued)

State	Balanced-Budget Stringency	Debt Restriction	Year Passed: Spending Limit	Year Passed: Revenue Limit
Washington	8	yes		1979
West Virginia	10	yes		
Wisconsin	6	yes		
Wyoming	8	yes		

Sources: Data on budget stringency rules and debt restrictions are from ACIR 1987 and Rafool 1997. Data on revenue and expenditure limits are from Rueben 1996.

[a]Requires a popular vote to approve debt issue.

[b]Passed a nonbinding spending limit in 1977.

[c]Adopted a nonbinding revenue limit in 1979.

[d]Passed a nonbinding spending limit in 1979.

[e]Spending limit expired in 1983.

[f]Nonbinding limit adopted in 1977.

process at which balance is required. In 44 states, the governor must submit a balanced budget. This is the weakest of the various balanced-budget requirements. In 37 of these states, the legislature must enact a balanced budget. These balanced-budget rules nevertheless allow for actual revenues and expenditures to diverge from balance if realizations differ from expectations. In 6 states, any unexpected deficit must be corrected in the next budget cycle. Finally, in 24 of the 37 states that require the passage of a balanced budget, there is a prohibition on deficit carry-forward into the next budget cycle. This represents the strictest antideficit rule.

Our data on balanced-budget rules are drawn from the Advisory Council on Intergovernmental Relations (hereafter ACIR) (1987) report on institutions that promote fiscal discipline in the states, updated using subsequent issues of the ACIR publication *Significant Features of Fiscal Federalism.* The ACIR index of budget stringency ranges between 0 (lax) and 10 (stringent). Table 8.1 reports this index. We use an indicator variable for whether this index is below 6 in our empirical work below. States with scores below 6 may have requirements that the governor propose or that the legislature pass a balanced budget, but they do not have stricter rules. States that require a balanced budget at the end of the fiscal year score 9 or 10 on the ACIR scale, and states that require a balanced budget over a two-year cycle receive an ACIR score of 8. Only 14 states receive ACIR scores of 6 or below.

We use the discrete indicator variable for the ACIR scores of 0 through 5, rather than the actual value of the ACIR score, because the latter imposes the same fiscal effect of one-unit changes at different levels of the ACIR scale, even though these differentials may be noncomparable. The indicator variable that we use, which was also analyzed in Poterba 1994, captures the key varia-

tion between states with lax and strict budgetary rules, but it downweights the small differences between states near either extreme. Our results are not sensitive to the cutoff that we use to define this indicator variable. In addition, while we focus on whether states have any restrictions on their budgetary outcome, we do explore the separate effects of these variables in some of our analysis below.

While an overwhelming number of states require budgets to be balanced during the current year, states in the Northeast and the upper Midwest are less likely to have stringent antideficit requirements. Many of the states outside those regions with less stringent budget rules, such as California, Nevada, and Louisiana, have more recently passed other fiscal constraints that restrict state revenue or expenditures. There is relatively little change within our sample period in state balanced-budget requirements.

The second fiscal institution that we consider is the ease with which the state can issue long-term general-obligation debt. The second column of table 8.1 lists the states that have some restriction on issuing general obligation debt. Ten states have no restrictions on debt issuance; of the other 40, 38 have constitutional restrictions on debt issue, and 2 have legislative limits. The most common restriction places a dollar limit on the amount of debt outstanding. This limit varies from $50,000 in Rhode Island and Oregon to $3 million in Alabama. In ten states—Arizona, California, Idaho, Kansas, Kentucky, Maine, Missouri, New Jersey, Pennsylvania, and Rhode Island—voters can override the constitutional restrictions on debt levels to issue additional debt. In 3 other states, including New York, voters are required to approve any debt issue. In another 3 states, issuing debt requires a supermajority vote in the state legislature. We define an indicator variable for all states with *any* type of debt restriction and include this indicator variable in our analysis below.

Finally, we consider whether a state has a state tax or expenditure limit (TEL). These laws typically limit the growth rate of general fund expenditures or revenues to the growth rate of personal income, or to some function of that growth rate. Rueben (1996) controls for the endogeneity of TEL passage and finds that states with tax or expenditure limits have lower growth rates in general fund revenues and expenditures. Shadbegian (1996), in a related study, shows that the impact of TELs can depend on the nature of these limits, as well as state economic conditions, such as the growth rate of personal income. Our analysis relies on Rueben's (1996) classification of "binding" state tax and expenditure limits. "Binding" limits are those that cannot be overridden by a simple legislative majority.

Most state limits on tax or expenditure growth were enacted during the "tax revolt" of the late 1970s, although some states have passed such legislation during the 1990s. Twenty-five states have instituted some form of limitation since 1976. The third and fourth columns of table 8.1 show the years in which revenue and expenditure limits were passed in various states. Some states have

both tax and expenditure limits, and some states have enacted more than one tax or expenditure limit during the last three decades. A number of states have adopted nonbinding limits, and then adopted binding limits in later years.

From the standpoint of the tax-exempt bond market, limitations on revenues and limitations on expenditures may have different effects. Limits on the taxing authority of the legislature may increase the risk that future interest payments will not be covered by tax receipts. Limits on expenditures, which in many cases do not apply to interest outlays, are less likely to have adverse effects on the perceived riskiness of state bonds. If anything, expenditure limits may be perceived favorably by municipal bond participants, since such limits constrain the future expenditures that might compete with promised interest payouts. Thus we might expect that states with tax limits would face higher borrowing costs, while those with expenditure limits might face lower borrowing costs.

8.2.3 Estimation Issues

The primary estimation problem that we confront concerns the potential endogeneity of a state's outstanding debt level. We estimate reduced-form models with, and without, the outstanding debt level to evaluate the effect of this variable on the other coefficient estimates. We also instrument for current debt levels using a state's historical constitutional debt restrictions on debt issued, and using information on how difficult it is to change debt restrictions and other fiscal institutions. Unfortunately, these do not appear to be powerful instrumental variables: they do not explain a substantial fraction of the variation in state debt-to-income ratios.

A related concern involves the potential endogeneity of fiscal institutions themselves. There are two empirical strategies for addressing this endogeneity. The first involves controlling for some measure of voter preferences, such as the ADA score of elected officials. This reduces the potential for observed correlations between budget rules and fiscal outcomes to simply reflect a correlation of both of these variables with an omitted third variable, voter tastes for fiscal outcomes. While this approach has been used in a number of empirical studies of the relationship between fiscal rules and tax or expenditure outcomes, it has not been applied in studies of tax-exempt bond yields. The difficulty with this approach is that it is hard to find a set of control variables that completely capture the political tastes of state voters.

A second approach to the endogeneity problem involves modeling the evolution of budget rules and using variables that affect budget rules but not fiscal policy as instrumental variables in a simultaneous equations setting. This approach was developed by Rueben (1996). The difficulty with this approach is finding valid instruments that are correlated with the potentially endogenous fiscal institutions.

Table 8.2 Summary Statistics

Variable	Sample Mean	New Jersey
Interest rate on general-obligation bonds, relative to New Jersey (basis points)	9.98 (24.33)	
Unemployment rate (percentage points)	0.067 (0.021)	0.068 (0.018)
Real per capita income (thousands of 1983 dollars)	13.01 (5.84)	16.82 (7.61)
Revenue/personal income	0.128 (0.061)	0.093 (0.012)
State debt outstanding/personal income	0.086 (0.074)	0.088 (0.059)
State marginal tax rate	0.065 (0.042)	0.035 (0.021)
Lax antideficit rules (1 if ACIR < 6, 0 otherwise)	0.200 (0.400)	0.00 (0.00)
Indicator for restrictions on debt issue	0.550 (0.498)	1.00 (0.00)
Indicator for binding expenditure limit	0.142 (0.350)	0.304 (0.460)
Indicator for binding revenue limit	0.057 (0.231)	0.000 (0.000)
Indicator for legislature must pass balanced budget	0.060 (0.238)	0.00 (0.00)
Indicator for requirement to correct deficit in next budget cycle	0.140 (0.347)	0.00 (0.00)
Indicator for requirement to correct deficit in current two-year cycle	0.140 (0.347)	0.00 (0.00)
Indicator for requirement to correct deficit in current one-year cycle	0.580 (0.493)	1.00 (0.00)

Note: Sample means for the 1973–95 period, with standard deviations shown in parentheses. See text for further discussion.

8.3 Empirical Findings

We present summary statistics from our data set before presenting regression results. Table 8.2 reports sample means for principal variables that we include in our regression equations. The first column shows the sample mean for all states, and the second column shows the mean value for the state of New Jersey. The mean of the actual regression variables is the difference between the two columns.

The average differential between the tax-exempt bond yield for New Jersey and all other states in the sample is just under 10 basis points. Although table 8.2 does not show this, there are substantial interyear differences in the average value of this differential, presumably as a result of changes in New Jersey's fiscal situation relative to that of other states. The maximum annual value of this average spread was 21.7 basis points, in 1984, and the minimum was −7.9 basis points, in 1976. There is also time-related variation in the dispersion of

tax-exempt bond yields. The year with the highest cross-sectional standard deviation of yield spreads was 1975, when this measure was 40.7. This was at a time when New York City's fiscal difficulties were affecting the tax-exempt bond market. The lowest cross-sectional standard deviation was in 1994, 9.2, a time of robust economic growth in most states.

Table 8.2 also presents summary information on other fiscal variables. The average state collects general fund revenue (total revenue less collections for state social insurance trusts) of 12.8 percent of personal income; this ratio is lower, 9.3 percent, for New Jersey. The average value of state debt as a share of state personal income is 8.6 percent, and New Jersey is very similar to the national average on this dimension. The ratio of debt to personal income is typically less than 10 percent, but in some states in some years, the debt burden is substantially higher. Delaware, Rhode Island, and Alaska all have outstanding debt of more than 25 percent of personal income. The average state top marginal income tax rate is between 6 and 7 percent, compared with 3.5 percent in New Jersey.

The bottom half of table 8.2 presents summary statistics on the indicator variables for fiscal institutions, which correspond to the budget rules that were described in table 8.1. The indicator variables for the last four variables measure different degrees of fiscal discipline in correcting budget deficits. The weakest variable is the one for legislative passage of a balanced budget. States that require the next fiscal measure, correction of a deficit within the next year's fiscal cycle, also require legislative passage of a balanced budget each year. The strictest states are those that require deficits to be corrected in the current annual budget cycle. Some states have biennial budget cycles, and the second-to-last variable indicates that the deficit must be corrected within the current biennial cycle.

Table 8.3 presents ordinary least squares (OLS) regression evidence on the association between fiscal rules, state fiscal conditions, and state borrowing rates. Each of the equations include control variables for the state unemployment rate, the level of per capita income in the state, state revenues as a share of personal income, state debt outstanding as a fraction of personal income, the top state marginal tax rate on interest income, and the ADA score for the state's senate delegation. The equation in the first column of table 8.3 shows the effect of using only these control variables to explain the relative yields on tax-exempt bonds. The results indicate that yields rise with the state unemployment rate and the level of debt relative to income, and that yields are lower when state revenue represents a higher fraction of personal income. These results also support the argument, developed in McKinnon 1997 and Bayoumi and Eichengreen 1994, that credit markets exert a disciplinary role on fiscal policy in the U.S. states. The coefficients on the other control variables are statistically indistinguishable from zero; this pattern persists in the other specifications that we estimate. This set of control variables can explain roughly one-third of the variation in the relative yield variable.

Table 8.3 State Fiscal Institutions and State Bond Yields

Variable	OLS (1)	IV (2)	OLS (3)	OLS (4)	OLS (5)
Unemployment rate	567.7	527.4	569.4	539.6	559.6
	(103.6)	(127.6)	(98.4)	(84.2)	(82.0)
Per capita income	1.56	0.56	0.94	0.78	−0.02
	(1.00)	(1.66)	(0.84)	(0.69)	(1.04)
Revenue/income	−72.7	−159.3	−34.7	−33.8	−41.7
	(25.0)	(134.2)	(28.2)	(25.3)	(25.3)
Debt/income	103.3	205.5	75.6	92.1	98.7
	(35.4)	(159.0)	(31.8)	(28.6)	(31.3)
State marginal tax rate	40.7	14.83	47.2	53.3	24.3
	(48.5)	(45.82)	(47.4)	(39.9)	(41.9)
Average ADA score	8.69	8.53	8.54	6.24	8.28
	(6.74)	(6.81)	(5.65)	(5.50)	(5.50)
Lax antideficit rules			13.30	8.42	
			(5.41)	(6.52)	
Limit on issuing debt				−5.38	−6.85
				(4.86)	(4.12)
Binding expenditure limit				−7.08	−5.75
				(3.59)	(3.57)
Binding revenue limit				17.61	14.01
				(6.63)	(6.57)
Legislature pass balanced budget					−10.25
					(8.99)
Correct deficit next cycle					−11.58
					(9.41)
Correct deficit this two-year cycle					−8.08
					(8.17)
Correct deficit this one-year cycle					−17.12
					(8.82)
Adjusted R^2	.35		.39	.44	.46

Note: Data are for 1973–95 for the 40 states covered in the Chubb Relative Value Survey and include 899 observations. Annual indicator variables are included in each regression, and all variables reported are differenced from the New Jersey value. Standard errors, which are in parentheses, control for across-state heterogeneity and within-state correlation. The state debt-to-income ratio is treated as endogenous in the equation reported in column 2, and state fiscal institutions are used as instrumental variables for this estimation.

One noteworthy but statistically insignificant coefficient is that on the state political ideology variable, the ADA score. This variable has a positive coefficient, implying that more liberal states pay more to borrow, but we cannot reject, at standard significance levels, the null hypothesis that this coefficient equals zero. This suggests that the omitted variable problems associated with failure to include a detailed set of variables capturing state political taste may not be critical.

The equation in the second column of table 8.3 includes the same explanatory variables as the equation in the first column, but it treats the state's debt-

to-income ratio as an endogenous variable. We use the set of fiscal institutions, the variables in $\{Z_{it}\}$, as the set of excluded exogenous variables in the estimation. When the debt-to-income ratio is treated as endogenous, its coefficient doubles, but the standard error rises by a factor of five, and we would not reject the null hypothesis that the debt-to-income coefficients in the OLS and instrumental variables (IV) specifications are the same. The other coefficients also change between the two specifications, but given their large standard errors, it is again difficult to draw firm conclusions. We tested, and rejected, the null hypothesis that fiscal institutions only affect bond yields through their effect on the level of outstanding debt. The results therefore suggest that fiscal institutions do not provide a suitable set of instrumental variables for the debt-to-income ratio because they also affect bond yields *directly.*

The equation shown in the third column of table 8.3 returns to the OLS strategy of the first column, but it includes the indicator for lax fiscal rules along with the foregoing control variables. The results suggest that tighter antideficit rules are associated with lower borrowing rates. A state with weak antideficit rules, all else equal, faces a borrowing rate 13 basis points higher than a state with tough antideficit rules. This finding confirms the results in earlier studies using the Chubb data. This effect does not change significantly (it decreases by 1 basis point) if we redefine the lax antideficit rules to include states that can carry over a deficit but must correct it in the next budget cycle, that is, with ACIR scores of 0 through 6 rather than 0 through 5.

The equation in the fourth column of table 8.3 includes the indicator variable for antideficit rules as well as three additional variables: one for the presence of a debt limit, and two variables corresponding to binding expenditure and revenue limits. Adding these variables reduces the statistical significance of the coefficient on the antideficit rule variable, although this coefficient remains positive and greater than its standard error. The debt limit variable has a negative coefficient, consistent with the discussion above, but the coefficient is not statistically significantly different from zero. The expenditure limit variable has a negative and statistically significant effect on yields: states with binding expenditure limits face borrowing costs that average 7 basis points less than states without such limits. The presence of a binding revenue limit has a large and statistically significant positive effect on yields: the presence of such a limit raises a state's borrowing cost by almost 18 basis points. This finding represents an effect of fiscal institutions that has not been documented in previous work, and it suggests that bond market participants view revenue limits as institutions that raise the risk of default.

The equation in the last column of table 8.3 does not include the antideficit indicator variable. Instead, it includes a set of indicator variables for different degrees of stringency in the budget process. The omitted category in this equation is the set of states that have no balanced-budget rules (Vermont) or the relatively weak requirement that the governor submit a balanced budget (New York, Massachusetts, and New Hampshire). The first included category is

states that only require their legislatures to pass a balanced budget. In these states bond yields are lower, by an average of 10 basis points, than in states where the only requirement is that the governor submit a balanced budget. This effect is statistically indistinguishable from zero, however.

The next three variables are indicator variables for progressively more stringent rules that require deficits to be corrected in specified time frames. States that require that deficits be corrected by the end of the next budget cycle also have lower borrowing costs than states with only gubernatorial submission requirements, but again, the effect is statistically indistinguishable from zero. States with the strictest requirements, namely the rule that deficits must be corrected within the *current* fiscal year, have borrowing costs that average 17 basis points below the costs of states in which governors are required to submit balanced budgets. This effect *is* statistically significant at standard confidence levels, and it suggests that much of the power of the "lax antideficit rules" variable is coming from the difference in borrowing costs in states with very strict, and all other, antideficit rules. When the expanded set of fiscal institution indicators is included in the regression specification, the explanatory power of the equation rises. The adjusted R^2 for the equation in the last column of table 8.3 is .46, compared with .44 for the equation in the penultimate column that excludes the detailed indicator variables on antideficit rules.

The equations reported in table 8.4 explore the possibility that the effect of fiscal institutions on borrowing costs is blunted by the inclusion of the debt-to-income ratio in the specifications shown in table 8.3. The equation shown in the first column illustrates the changes in the control variable coefficients when the debt-to-income ratio is deleted. The coefficient on the unemployment rate remains positive and statistically significant, but per capita income, which was insignificantly different from zero in the specifications shown in table 8.3, now becomes statistically significant and positive. Revenue as a percentage of income, which was *negative* in table 8.3, switches signs and becomes positive in table 8.4. The ADA score and the top marginal tax rate are statistically insignificantly different from zero in most of the estimates in table 8.4, as they were in table 8.3.

The sign pattern and the statistical significance of the coefficients on the fiscal institution variables is largely unaffected by exclusion of the debt-to-income ratio. This can be seen by comparing the coefficients in table 8.3 with those in table 8.4. The point estimate on the antideficit rule variable (column 2) rises slightly when the debt-to-income ratio is dropped from the specification. This is consistent with the notion that tight fiscal rules lower the value of the debt-to-income ratio, and that including this ratio in the estimating equation therefore captures some of the effect of these variables.

The equation shown in the third column of table 8.4 models the indicator of lax fiscal rules, the ACIR variable, as endogenous. This corresponds to our earlier discussion of the potential endogeneity of fiscal rules. The difficulty in treating fiscal rules as endogenous is that it is not clear what excluded exoge-

Table 8.4 **State Fiscal Institutions and State Bond Yields Excluding Outstanding State Debt Stock**

Variable	OLS (1)	OLS (2)	IV (3)	OLS (4)	IV (5)	OLS (6)
Unemployment rate	608.4	597.6	605.2	578.3	596.6	609.6
	(104.5)	(94.2)	(102.2)	(83.7)	(123.8)	(80.8)
Per capita income	2.56	1.53	2.26	1.46	0.35	1.02
	(0.95)	(0.81)	(2.20)	(0.68)	(3.98)	(0.96)
Revenue/income	14.7	31.7	19.6	45.2	60.2	43.8
	(23.6)	(24.3)	(40.3)	(20.1)	(78.4)	(23.3)
State marginal tax rate	66.8	66.3	66.7	74.7	68.3	40.4
	(48.9)	(47.1)	(47.4)	(39.0)	(46.9)	(43.0)
Average ADA score	8.86	8.62	8.79	6.39	2.76	8.58
	(7.08)	(5.67)	(6.77)	(5.67)	(10.68)	(5.77)
Lax antideficit rules		15.48	4.50	10.94	7.14	
		(5.07)	(28.42)	(6.43)	(43.92)	
Limit on issuing debt				−6.03	−26.78	−8.03
				(5.28)	(28.74)	(4.88)
Binding expenditure limit				−5.29	4.82	−3.96
				(3.89)	(44.62)	(3.83)
Binding revenue limit				15.87	21.76	12.26
				(6.63)	(52.96)	(6.56)
Legislature pass balanced budget						−12.50
						(8.18)
Correct deficit next cycle						−15.94
						(8.62)
Correct deficit this two-year cycle						−11.13
						(7.85)
Correct deficit this one-year cycle						−19.12
						(8.59)
Adjusted R^2	.32	.37		.41		.43

Note: Data are for 1973–95 for the 40 states covered in the Chubb Relative Value Survey and include 899 observations. Annual indicator variables are included in each regression, and all variables reported are differenced from the New Jersey value. Standard errors, which are shown in parentheses, control for across-state heterogeneity and within-state correlation. The endogenous variable in column 3 is the indicator for lax antideficit rules, and in column 5, all of the variables related to fiscal institutions are treated as endogenous. See text for further discussion of the excluded exogenous variables.

nous variables are available for use as instrumental variables. Such variables need to be correlated with current fiscal rules, but uncorrelated with the error term in equation (3).

The instrumental variables that we consider are related to the current or historical structure of the state political process. They involve constitutional or legal provisions that would make it more or less difficult to adopt fiscal rules, such as tax and expenditure limits or restrictions on state deficits. The five instrumental variables that we use are whether the state constitution permits statewide referenda to enact legislation (so-called direct-legislation states), whether voters can recall elected officials, whether the initial state constitution

included limits on debt issuance, the "signature requirement" (the fraction of the state's voters that must sign a petition in order to place a policy proposal on a statewide ballot for referendum vote), and the year a territory became a state. It is sometimes argued, and the empirical evidence in Matsusaka (1995) suggests, that grassroots campaigns lead to support among voters for tax and expenditure limits, but that such support is much more difficult to generate in elected legislatures. If this is the case, then the direct legislation variable and the "signature requirement" should affect the chances of enacting a tax or spending limit. Similarly, one can argue that recall provisions increase the degree to which elected officials are responsive to voter preferences, and thereby affect the probability that legislatures will enact deficit limits, or tax or expenditure limits, conditional on a level of voter support for such measures. Magleby (1984) provides valuable background on the political consequences of various methods of implementing "direct democracy." The historical debt limit variable is largely determined by when the state was founded, since, as English (1996) explains, states whose constitutions were written after the state debt defaults of the 1830s and 1840s were more likely to place limits on debt. Finally, the year a state constitution was adopted is another way to pick up idiosyncracies in state constitutions that will affect the ease of adopting different fiscal institutions.

The estimates in the third column of table 8.4 are discouraging. While the IV estimates still yield a positive effect of weak antideficit rules on borrowing costs, the standard error of the coefficient estimate (28.42) is so large that a 95 percent confidence interval includes both the OLS estimate and a range of other values. The instrumental variables are "weak" in the sense that we find very imprecise estimates of the coefficient of interest; our instruments do not explain much of the variation in antideficit rules, even though, as Rueben (1996) finds, they do explain a substantial fraction of the variation in tax and expenditure limits.

Returning to OLS results that are similar to those in table 8.3, but that exclude the debt-to-income ratio from the specification, we find that the effect of a binding revenue limit, shown in table 8.4 column 4, is slightly reduced when we exclude the debt-to-income ratio from the specification. Column 4 presents OLS estimates including the variables for lax deficit rules, debt limits, revenue limits, and expenditure limits. The coefficients are broadly similar to those in table 8.3, where the debt-to-income ratio was included in the specification. The fifth column of table 8.4 presents an equation that treats all four of these fiscal institutions as endogenous. Just as with the IV estimates described above, however, the standard errors on all of the estimated coefficients rise substantially. It is not possible to draw any strong inferences from the IV estimates, except that the set of instruments that we have used has low power. Indeed, in first-stage regression equations relating the fiscal institutions to the instrument set, the only endogenous variable that the instruments are jointly statistically significant in explaining is that for a revenue limit.

The last column in table 8.4 shows another OLS equation, in this case estimated with the exhaustive set of fiscal institutional variables. There is no clear pattern of changes in the set of coefficients when compared with those in table 8.3. The estimated effect of the requirement that a deficit be corrected in the current annual budget cycle is larger than in the comparable equation in table 8.3 that included the debt-to-income ratio, but not by a large amount relative to the coefficient standard error. In general, the findings in table 8.4 do not suggest that the inclusion of the debt-to-income ratio has substantially altered the previous findings with regard to the impact of fiscal rules on borrowing rates.

The results from our basic specifications suggest that fiscal institutions affect state borrowing costs, and they provide new information on the types of fiscal rules that have the greatest impact. We now consider whether the impact of these fiscal rules depends on state economic conditions, as measured by the state unemployment rate. We do this by interacting three fiscal rules—the indicator for lax antideficit rules, and the indicators for tax and expenditure limits—with the state unemployment rate. This approach is related to Lowry and Alt's (1997) interaction of the state deficit with fiscal rules.

Table 8.5 presents the results of our unemployment-interaction analysis. The equation in the first column includes only the variable for lax antideficit rules, and this variable interacted with the unemployment rate. The effect of lax budget rules is not significantly affected by state economic conditions. We find similar results with respect to limits on the state legislature's authority to issue debt, as the results in the second column suggest. However, the equation in the second column also interacts the indicator variables for the presence of revenue and expenditure limits with the unemployment rate. While there is weak evidence that the effect of expenditure limits is accentuated in states with higher unemployment rates, there is a statistically significant, and substantively important, interaction effect between revenue limits and the state unemployment rate.

To illustrate this effect, contrast two states, one without a binding revenue limit, and one with such a limit. On average, the state without the revenue limit will face borrowing costs 10 basis points lower than the state with the revenue limit. Now consider a two-percentage-point increase in the unemployment rate in the two states. The estimates in table 8.5 suggest that there will be a 10-basis-point increase in the yield spread between the tax-exempt bonds issued by the two states. For each percentage point that the unemployment rate rises in a state with a binding revenue limit, the state bond yield rises by 5 basis points relative to the yield of a similar state without a revenue limit. This suggests that when state economic conditions deteriorate, revenue limits become a greater concern for bond market participants.

The data sample that we analyze is longer than that in previous studies of fiscal rules and borrowing costs. One advantage of this long sample is that we can examine whether the relationships described above are stable over time.

Table 8.5 State Fiscal Rules, Economic Conditions, and Bond Yields

Explanatory Variable	(1)	(2)	(3)
Unemployment rate	587.79	491.57	497.61
	(104.64)	(185.89)	(88.22)
Per capita income	1.54	1.33	1.32
	(0.81)	(0.66)	(0.66)
Revenue/income	32.06	52.82	−40.20
	(24.20)	(19.84)	(24.00)
State marginal tax rate	65.51	77.75	52.37
	(47.35)	(41.30)	(20.12)
Average ADA score	8.78	4.96	4.78
	(5.74)	(5.63)	(5.55)
Lax antideficit rules	15.56	12.90	12.74
	(5.04)	(6.99)	(6.90)
Lax antideficit rules*unemployment	57.26	68.00	
	(132.47)	(197.89)	
Limit on issuing debt		−4.95	−5.00
		(5.69)	(5.52)
Limit on issuing debt*unemployment		−9.35	
		(163.22)	
Binding expenditure limit		−4.54	−4.62
		(3.92)	(3.91)
Binding expenditure		−125.32	−127.34
limit* unemployment		(98.71)	(98.39)
Binding revenue limit		9.62	9.65
		(6.81)	(6.71)
Binding revenue		521.50	515.63
limit* unemployment		(264.91)	(224.35)
Adjusted R^2	.373	.427	.428

Note: Data are for 1973–95 for the 40 states covered in the Chubb Relative Value Survey and include 899 observations. Annual indicator variables are included in each regression, and all variables are differences from the New Jersey value. Standard errors, which are shown in parentheses, control for across-state heterogeneity and within-state correlation.

Table 8.6 presents four regression equations that address this issue. The equations in columns 1 and 2 correspond to column 2 in table 8.4, but the equation is estimated first for the 1973–89 sample period, and again for 1990–95. While the coefficients on several of the control variables differ across the sample periods, with a lower coefficient on the unemployment rate in the most recent subsample, for example, the coefficient on the indicator variable for lax antideficit rules does not change substantially across samples. An F-test of the hypothesis that all coefficients are the same in two sample periods would nevertheless reject the null of parameter constancy.

The third and fourth columns of table 8.6 present subsample estimates for the expanded equation, including revenue and expenditure limits. The results suggest one interesting pattern: the effect of binding revenue limits on state borrowing costs appears to be larger in the 1973–89 period than in more recent

Table 8.6 State Fiscal Rules and Bond Yields: Are the 1990s Different?

Explanatory Variable	1973–89 (1)	1990–95 (2)	1973–89 (3)	1990–95 (4)
Unemployment rate	654.68	350.56	609.21	355.96
	(103.46)	(79.97)	(94.20)	(89.57)
Per capita income	2.46	−0.50	2.15	−0.27
	(0.94)	(0.56)	(0.79)	(0.48)
Revenue/income	20.23	52.97	37.81	64.08
	(27.45)	(17.75)	(22.55)	(17.34)
State marginal tax rate	68.12	48.86	82.21	59.42
	(49.69)	(40.68)	(42.69)	(35.77)
Average ADA score	12.19	2.38	9.13	1.26
	(7.14)	(3.98)	(7.05)	(4.15)
Lax antideficit rules	15.03	18.98	10.45	16.95
	(5.79)	(3.39)	(7.69)	(3.65)
Limit on issuing debt			−7.40	−1.03
			(6.19)	(2.96)
Binding expenditure limit			−5.90	−4.70
			(5.23)	(2.44)
Binding revenue limit			25.19	4.63
			(9.65)	(2.93)
Adjusted R^2	.385	.511	.432	.543

Note: Data are for 1973–95 for the 40 states covered in the Chubb Relative Value Survey and include 899 observations. Annual indicator variables are included in each regression, and all variables are differences from the New Jersey value. Standard errors, which are shown in parentheses, control for across-state heterogeneity and within-state correlation. Regressions for 1973–89 include 680 observations, while those for 1990–95 have 219 observations. The F-statistic for equal coefficients in columns 1 and 2 is $F(9,858) = 7.42$, and $F(11,852) = 6.26$ for columns 3 and 4, so we reject the null of constant coefficients across the two time periods.

years. When we constrain the coefficients on the variables other than fiscal institutions to be the same for the entire sample, but interact the fiscal institution variables with a post-1990 dummy variable, the same findings emerge. The coefficient on the post-1990 dummy interacted with the indicator for a binding revenue limit is −23.2 (8.14), which suggests a much larger effect of this variable in the earlier part of the sample. The interpretation of this result is unclear. It may be that as more states have adopted revenue limits, bond market participants have become less concerned about the negative effect of these limits on state capacity to service debt. The rise of municipal bond insurance may also be a factor. This is an issue that requires further study.

In addition to the pre- and post-1990 sample divisions, we also explored the sensitivity of our findings to estimation on the "pre–New York City fiscal crisis" sample. We interacted an indicator variable for the pre-1975 period with our standard list of fiscal institutions. The results, which must be viewed with caution in light of the short sample period, suggest that the positive effect of debt restrictions on yields was greater in the years before 1975 than afterward, and that the effect of antideficit rules on borrowing costs was smaller in this

period than subsequently. The effect of revenue and expenditure limits cannot be estimated for the pre-1975 period—there were no such limits. These results provide some support for the notion that bond market participants have become more interested in the role of fiscal institutions in the years since the New York City fiscal problem.

8.4 Conclusion and Future Directions

Our principal finding is that state fiscal institutions affect the required return that lenders demand when states enter the market for tax-exempt bonds. The effects that we uncover are substantively as well as statistically significant. A state with a binding tax limitation statute will face, on average, a borrowing rate between 15 and 20 basis points higher than a state without a tax limitation law. With long-term tax-exempt bond rates averaging something like five percentage points, borrowing cost differentials of this magnitude are not trivial. A state with an expenditure limitation law, in contrast, will face a borrowing rate that is several basis points lower than that of a state without any fiscal limits. Lenders appear to demand higher yields from states with tax limitation laws, presumably because such restrictions may make it difficult to raise taxes to pay interest in the future, while they appear to view spending limitation laws as favorable indicators of the state's future fiscal soundness.

We also confirm, with a longer data sample and somewhat more inclusive empirical model, previous findings that antideficit provisions in the state constitution have an important effect on borrowing costs. Those states with weak antideficit provisions face borrowing costs 10 to 15 basis points higher than similar states with stricter antideficit rules. Restrictions on state authority to issue long-term general-obligation debt are associated with lower borrowing costs, although the point estimates suggest weaker effects for these institutions than for some of the other fiscal rules considered above.

Our focus on the capital market as a way of obtaining evidence on the effects of fiscal institutions could be extended in several directions. One possibility would be to move beyond the use of interest rate differentials to analyze other market-based measures of state default risk. Studying how default insurance rates charged by municipal bond insurers are influenced by fiscal institutions would be one possible extension. This project would require detailed information on default insurance rates for state general obligation bonds, ideally at several different points in time. Our analysis could also be extended to the case of local rather than state governments. Hirsch's (1991) study of the net interest costs on California municipal bonds around the enactment of Proposition 13 provides some evidence that local bond yields are affected by changes in fiscal institutions. Yet another extension would focus on the short-run yield adjustments to economic news, and the effect of fiscal institutions on such adjustments. One could consider how unexpected state deficits that arise within a fiscal year raise borrowing costs for states with weak, and with strong, anti-

deficit policies. Research of this type could complement the evidence in Poterba (1994) on the relationship between antideficit rules and short-run state fiscal adjustment.

Our results raise unanswered questions about why different states choose different fiscal institutions, and what trade-offs are involved in choosing one set of institutions or another. Research is just beginning on the general question of what the optimal fiscal constitution consists of; see Roubini 1995 for a discussion of these issues. With respect to state antideficit rules, there is a small, and as yet inconclusive, literature on how different fiscal rules affect state economic performance. Bayoumi and Eichengreen (1995) and Levinson (1997) find that states with more restrictive fiscal constitutions have higher output volatility, apparently as a result of less fiscal flexibility on the part of state government, while Alesina and Bayoumi (1996) do not find any evidence linking the stringency of state fiscal rules to the variability of state economic activity. With respect to tax and expenditure limits, there is evidence, for example Rueben 1996, that these rules affect the size of government in the state economy. These rules also have effects on state borrowing costs. These studies illustrate the type of research that is needed to identify the net benefits of different fiscal rules. Our findings suggest that voters in states that enact tighter fiscal rules benefit from lower borrowing costs; the unresolved question is what countervailing costs, or additional as-yet-unquantified benefits, these fiscal rules also produce.

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