THE EFFECT OF TAX AND EXPENDITURE LIMITS ON
STATE AND LOCAL GOVERNMENTS

by

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ABSTRACT

This dissertation assesses the impact of tax and expenditure limits on the ability of state and local governments to raise and spend revenues. Currently twenty-four states have laws limiting government appropriations or revenues.

The first chapter broadly examines the effects of state level limitation laws on both state and aggregate local spending. Studying the effect of tax and expenditure limits is complicated by the potential endogeneity of these limits. Voters might be more likely to pass fiscal limitations in states with higher levels or growth rates of government spending. Using historical information on the availability of direct legislation and recall procedures as instrumental variables for the passage of tax and expenditure limits, I find that state general expenditure as a percentage of personal income is two percent smaller in states with binding limits. This effect is partially offset by higher local spending in these states.

Chapters two and three examine the labor market effects of tax and expenditure limits. Chapter two, which was coauthored with James Poterba, describes the effect of fiscal institutions on the relative wages of state and local government employees, and their private sector counterparts, during the 1979-1992 period. Empirical analysis of data from the Current Population Survey suggests that states with limitations on local property taxes, and to a lesser extent states with state-wide tax and expenditure caps, had slower relative public sector wage growth during the 1980s. These results are robust to our attempt to control for the endogeneity of fiscal institutions by using various features of the state legislative environment as instrumental variables for the fiscal institutions.

Chapter three, which was co-authored with David Figlio of the University of Oregon, examines the effects of tax limitation laws on the labor supply decisions of potential teachers. In this chapter, I examine how the passage of tax limitation laws at the school district level has affected the relative quality of new teachers. Using data from the National Longitudinal Survey of 1972 and the High School and Beyond datasets, we find that the average quality of education majors has declined by 10 percent in states which imposed tax limitation laws relative to states without such laws. In addition, tax limits also reduce the probability that a newly hired teacher has attended a selective or highly selective college.

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Chapter 3: How Tax Limits Affect Teacher Quality
Joint with David Figlio, University of Oregon

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Introduction

Explaining persistent growth of government during the twentieth century is one of the principal questions in public finance and political economy. A central concern is the effect of various fiscal institutions on government growth. The "Taxpayer Revolt" in the late 1970s and 1980s led to the enactment of many laws limiting state and local tax and expenditure growth. Passage of these limits coincided with a slowdown in the overall rate of state and local government growth. Nearly two decades after this revolt began, total state and local spending is 11.0 percent of GDP (1994) compared with 11.9 percent in 1976. We are also faced with a new round of tax limitation laws being proposed and passed at the same time that state and local governments are being asked to take a larger role in providing services such as welfare and health services. Examining the effects from the tax limits that were passed in the late 1970s will help us understand the possible pitfalls faced by state and local government officials as these new limits are implemented.

In this dissertation, I examine the role these earlier tax limitation laws have played in curtailing the growth rate in state and local expenditures, as well as affecting the level of government providing services. I find that tax limit laws do constrain government growth in some states. I further examine the characteristics of these laws and the effect of these laws on the public sector labor force.

Chapter one explores the effect of constitutional and legislative tax and expenditure limits on state and local government spending over the last two decades. Currently twenty-four states have laws limiting government appropriations or revenues. In spite of this aggregate evidence of a decrease in state and local spending, prior work has found mixed evidence on whether fiscal institutions matter. This could be due to the potential endogeneity of the proposal or passage of these limits. The passage of limits could reflect changes in voter preferences about the size of government, and these changes could lead to changes in state revenue or expenditure levels, but limits may not play a causal role. The alternate finding could also be true; if limits were more likely to be passed in states with higher growth rates in government spending, then cross-sectional comparisons of states with and without limits could lead to a positive correlation between limits and state government growth. Using historical information on the availability of direct legislation and recall procedures as instrumental variables for tax and expenditure limits, I find that state general expenditure as a percentage of personal income is two percent smaller in states with binding limits. This effect is partially offset by higher local spending in these states. In addition, the type of limit seems to matter; limits on both state and local governments produce greater reductions in spending than state limits alone and revenue limits appear more constraining than expenditure limits.

I then more specifically examine how these cuts in government spending affect the public sector labor force. In a joint paper with James Poterba, I describe the effect of fiscal institutions on the pattern of relative wages between state and local government employees and their private sector counterparts during the 1979-1992 period. Empirical analysis of data from the Current Population Survey suggests that states with limitations on local property taxes, and to a lesser extent states with state-wide tax and expenditure caps, display slower relative public sector wage growth during the 1980s. These results are robust to our attempt to control for the endogeneity of fiscal institutions by using various features of the state legislative environment as instrumental variables for the fiscal institutions. We also find that states with more favorable public sector bargaining environments seem to have been more likely to pass tax and expenditure limits.

The third chapter in this dissertation, which was co-authored with David Figlio, examines the passage of tax limitation laws at the school district level and the effects these limitations have on the supply of new teachers. Previous authors, have found that the passage of tax limitation laws leads to both reduced spending and lower student achievement. However the economics of education literature has found little direct effect of spending on student outcomes. We posit that tax limitation laws might affect student outcomes by discouraging more qualified individuals from becoming teachers in tax limit states. Using data from the National Longitudinal Survey 1972 and High School and Beyond datasets we find that the average quality of education majors has declined 10 percent in states which imposed tax limitation laws, relative to states without such laws. In addition, tax limits also reduce the probability that a newly hired teacher has attended a selective or highly selective college.

Chapter 1

Tax Limitations and Government Growth: The Effect of State Tax and Expenditure Limits on State and Local Government

Explaining persistent growth of government during the twentieth century is one of the principal questions in public finance and political economy. A central concern is the effect of various fiscal institutions on government growth. The "Taxpayer Revolt" in the late 1970s and 1980s led to the enactment of many laws limiting state and local tax and expenditure growth. Passage of these limits coincided with a slowdown in the overall rate of state and local government growth. Nearly two decades after this revolt began, total state and local spending is 11.0 percent of GDP (1994) compared with 11.9 percent in 1976.

In spite of this aggregate evidence of a decrease in state and local spending, prior work has found mixed evidence on whether fiscal institutions matter. The passage of limits could reflect changes in voter preferences about the size of government, and these changes could lead to changes in state revenue or expenditure levels, but limits may not play a causal role. The alternate finding could also be true; if limits were more likely to be passed in states with higher growth rates in government spending, then cross-sectional comparisons of states with and without limits could lead to a positive correlation between limits and state government growth.

Currently twenty-four states have constitutional or statutory state tax and/or expenditure limits (TELs). Beginning in the late 1970s, states passed limits on the growth rate of state spending or revenues, usually limiting this growth rate to the growth rate of personal income. Concurrently, many referenda were proposed limiting the use of local property taxes. Proposition 13 in California and Proposition 2 1/2 in Massachusetts are the most famous taxpayer initiatives for property tax reform. Many more state limits than local limits were passed; of 11 local property tax limits proposed during the 1975-1985 period, three were passed, while of 30 state limits proposed, 26 were passed[1].

Right after the passage of these limits their effectiveness was questioned. In 1982 the National Tax Journal published a symposium on TELs. The contributors explored how binding these limits were in individual states, and most found little response in state revenues or expenditures . These studies necessarily focused on a short period after TELs were enacted.

More recent empirical studies have also found weak, if any, evidence that TELs affect revenues or expenditures. Abrams and Dougan (1986) explore the effect of different constitutional restrictions on state and local spending levels in 1980. Borrowing limits and line item-veto laws do not seem to matter. State TELs seem to have a negative and marginally significant effect on state and local spending in 1980, but a weak positive effect on state level spending. Abrams and Dougan do find that states with reelection restrictions have higher levels of spending.

Dougan (1988), estimating individual time series regressions for states with TELs,

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[1]The difference between states which currently have limits and states which passed limits in the 1975-1985 period is caused by a limit expiring in New Jersey and by passage of multiple limits in Missouri and North Carolina.

finds mixed evidence on the effects of TELs. In some states, limits seem to have been effective, while in others, there were positive changes in expenditure levels after TELs were passed. Dougan finds evidence to support the hypothesis that TELs act as signals between voters and the legislature, and he reports weak evidence that some limits are effective controls. Bails (1990) compares average growth rates across states with and without limits using linear trend models and concludes that limits are not effective. Finally, NASBO (1988) examines revenue and expenditure levels as a percent of personal income and finds little difference between these variables in states with and without limits as well as little change in these percentages across the two groups from 1980 to 1987.[2]

The foregoing evidence looks at the effect of limits in the period after passage. There is some evidence of short-run effects in states responses to the fiscal shocks of the early 1990s. Poterba (1994) finds that in response to unexpected deficits states with TELs raised taxes less often than states without TELs. This evidence does not resolve the question of whether these effects persist, and whether they affect long run spending levels. Poterba and Rueben (1995) find evidence that the presence of both state and local limits slow the growth in relative wages for state and local workers.

There are many factors that could explain a limited impact of TELs. For example, legislatures may circumvent limits, by transferring program responsibility to sub-state

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[2] More evidence exists on the effectiveness of local limits, with many researchers studying the effects of various local limits. Using panel data, Preston and Ichniowski (1992) find that state limits on local property taxes decrease the growth rate in both property tax revenues and overall municipal revenues. Figlio (1995) finds local limits significantly affect school input measures. He also finds that limits are endogenously determined and that instrumenting for the presence of limits significantly increases the size of the measured effects.

governments or by financing spending with new debt. The endogeneity of the limits may also explain their small apparent effects, since states with higher spending may be more likely to adopt TELs. Previous studies have also failed to distinguish between different types of TELs, and have not taken account of the purely advisory nature of certain limits

This paper explores the effect of constitutional and legislative limits on the size of state and local government over the last two decades. By estimating spending and revenue equations after correcting for simultaneity in the adoption decision and by looking at how spending patterns evolve after passage of limits, I present new evidence that state-level limitations do reduce the level of state government expenditures. I then analyze whether spending reductions are offset by additional local spending.

This paper is divided into five sections. Section one describes TELs in more detail and focuses on the differences in laws passed by different states. Section two briefly examines different theories of government and the predicted effects of limits and presents the empirical strategy of this paper. Section three describes factors that affect the passage of TELs. I present empirical results which show that states with historical direct legislation processes that encourage voter participation in the legislative process were more likely to enact limits in the 1976-1986 period.

Section four describes the empirical results and the data used for this study. While ordinary least squares estimation suggests that states with TELs do not have lower long run levels of spending; once the endogenous nature of these limits is recognized, the results indicate that states with limits seem to spend less, although this decrease is partially offset by additional local spending. In addition, the type of limit seems to matter. Limits on both state and local governments produce a greater reduction in overall spending than limits only on state governments. Finally, section five concludes and describes further directions for this research.

1. The History and Structure of Tax and Expenditure Limits

TELs were initially introduced in the late 1970s. Many theories try to explain the sudden tax revolt of this period. These include beliefs that government spending was higher than voters preferred, a growing disillusionment with government in general after Watergate, and public outrage over increasing state and local government surpluses while disposable personal income was falling. In addition, Fischel (1994) argues that when school finance equalization lawsuits led to equal funding of public schools, the severing of the relationship between local taxes and services led to Proposition 13 in California and in general to the tax revolt of the late 1970s. The introduction of state tax limits coincided with a general public discontent with the size of government in general and with the property tax in particular. TELs often arose as compromise measures passed by state legislatures in response to proposed referenda on property tax limits.

Public demand for property tax limits was motivated by several factors. Between 1970 and 1980, property values increased dramatically. As these increases were reflected in assessments, property taxes also rose. Rising property valuations and tax levels were especially noticeable in California, where rising house values and mandatory property reassessment led to large increases in property tax bills even though local (and state) governments, were operating with large surpluses. Figure 1, panel 1 illustrates the level of

California state and local revenues and expenditures as a percent of personal income. In 1978, not only were revenue levels at a historically high level, they were also almost twenty percent higher than expenditures. While several TELs were passed earlier than the limit in California, the passage of Proposition 13 is generally acknowledged as the beginning of the tax revolt movement in the United States.

Several examples of the history of limit passage illustrate the heterogeneity in state TELs and in how they were enacted. California's Proposition 13, passed in 1978, limited the maximum property tax to one percent of "fair cash value of property," which equals cash value of property as stated in the 1975-1976 tax bill or appraisal value at time of construction or change of ownership.[3] Changes in appraised property value were limited to the minimum of the change in the consumer price index or two percent. In addition, the state was not allowed to introduce any new taxes on real property and any increase in state taxes had to be approved by two-thirds of each house. Finally, passage of any other new local taxes required the approval of two-thirds of all registered voters.

Prior to passage, politicians, academics and newspapers predicted fiscal disaster if Proposition 13 was passed. In fact, the state used its $6.8 billion surplus to make up the 30 percent cut in property tax revenues caused by passage of Proposition 13. This state bailout was made permanent by a state commission. This led to a change in the composition of state and local finance in California and a substantial loss of local autonomy. Proposition 13 was quickly followed by Proposition 4, which limited state and local expenditure growth to the

[3]O'Sullivan, Sexton and Sheffrin (1994) examine the legacy of Proposition 13, especially measuring the effects of the re-valuation provision on home ownership and mobility decisions.

lesser of the change in per capita personal income or the inflation rate plus the change in population. In this case, voters wanted to curb the increasing state spending which occurred after Proposition 13 was passed and local revenues were constrained. Dworak (1980) examines the events leading up to passage of propositions 13 and 4 and gives a history of earlier, unsuccessful limitation attempts in California.

The New Jersey legislature's 1976 passage of an expenditure limit is an early TEL. This constrained future expenditure to the same percent of personal income as in the prior year. The legislature passed this law after spending two acrimonious sessions trying to resolve court mandated property tax revisions and school financing restrictions. After closing the New Jersey public schools in 1976 when a budget could not be agreed upon, an income tax was grudgingly introduced. The new income tax statute contained an expenditure limit to signal that the income tax would not be further used to increase the size of the state government. The statute also limited the growth in local spending to 5 percent per year. The limit was extended once and then allowed to expire in 1983. The New Jersey income tax is still in effect. Figure 1, panel 2 shows the aggregate revenue and expenditure levels for New Jersey over this period.

In contrast, some TELs were enacted by state legislatures to avoid passage of more binding limits - usually on property taxes. In November 1978, voters in Nevada approved Proposition 6, a measure similar to Proposition 13, by a 3:1 margin. However, citizen initiatives must be approved in consecutive biennial elections. Before the second vote in 1980, the state legislature produced a tax reduction package that reduced both local property taxes and eliminated state sales taxes on food. It also included limits on the growth rate of proposed state and local expenditure. This tax relief package was passed conditional on the defeat of Proposition 6. Proposition 6 was defeated, and while spending and taxes temporarily decreased, they quickly rose again given the non-binding nature of the limit in place. Total state and local spending and revenues are shown in the third panel of Figure 1. In 1994, a more binding limit was enacted limiting growth in state and local expenditure to the inflation rate.[4] Finally, the fourth panel of Figure 1 shows state and local revenues and expenditure in New York, a state which never proposed or passed a spending limit despite high levels of state and local taxes.

Table 1 presents information on the characteristics of state TELs and Figure 2 illustrates the pattern of passage of limits over time and differences in the restrictiveness of limits.[5] Twelve of the twenty-four state limits currently in force restrict the growth in state expenditures or revenues to the growth rate in personal income averaged over some prior period. In five states the limits restrict the size of general fund appropriations to a certain percent of state personal income, while in four states growth is restricted to some function of inflation and population growth. Finally, three states restrict the absolute expenditure growth rate.[6] TELs typically apply to only a subset of spending. Spending on capital

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[4]There has recently been renewed activity in the passage of tax limits. For a review of other recent enactments see Moore & Stansel (1994). In addition, both Figlio(1996) and Dye and McGuire (1996) have found real short term effects of these new limits.

[5]More detailed information on each state is presented in Appendix A.

[6]Since virtually all states have balanced budget rules, revenue limits and expenditure limits should be effectively equivalent, differing only by the amount of existing surpluses or a state's borrowing for current expenditures. Different effects on spending and revenues can shed light on how binding state balanced budget rules are in practice.

projects, and from federal funds, is excluded. In addition, a number of states exclude additional spending which occurs as a result of federal or court mandates. Thirteen states can override the limits with a supermajority vote. Five states require a simple majority and the declaration by the governor of a state of emergency. Half of these limits are constitutional and half are statutory.

The majority of TELs were initiated or proposed by legislatures, although several prominent ones resulted from citizen initiatives. A majority of the legislative proposals encompassed original citizen initiatives. This could reflect legislatures responding to voter demand and passing limits in a speedier, less costly fashion. An alternate, more cynical interpretation is that legislatures preempted voter initiatives to control the structure of the limitation.

During the 1970s, many states also passed legislation which imposed limits on city or county property taxes. States have historically prohibited intra-state governments from having local income and sales taxes, and in cases where such taxes are allowed, the rates are set by the state government. Such limits increase the effect of limits on state spending. Thirty-four states currently limit local property taxes in some way. Most of these limits apply to assessment increases or set maximum rates. Only California, Idaho and Massachusetts have laws like Proposition 13. An additional two states (New Jersey and Michigan) have binding limits on overall local spending or revenue as well.

Of the 25 limits passed, three limits were written as purely advisory laws, suggesting growth rates or only affecting budgeted revenues or expenditures. An additional four states have laws which are advisory in practice. These states, including Nevada until 1994, have limits which can be over-ridden or amended by a simple legislative majority. In these states increasing spending or taxes beyond the limited amount is no more difficult in practice than passing any appropriations bill.

2. Conceptual Framework and Empirical Methods

In section 2.1, I discuss different models of government and what these models say about the effectiveness of TELs. I then examine the potential effects of limits and how these differ with the type of limit. I am interested in how states respond to limits and whether different types of limits have different effects. In section 2.2, I discuss the empirical modelling strategy I employ and how testing the effectiveness of these limits is complicated by their endogeneity.[7]

2.1 Models of State Government

Studying the effect of TELs on the level of spending at first glance seems superfluous. In a world of perfect information, there would be no role for limits, since expenditure levels would be optimally set. If legislators cannot perfectly predict voter wishes vis-a-vis the appropriate level of spending, then TELs can act as signals. Laws which explicitly limit the growth rate of spending should lead to less spending since limits should act as a strong indicator of voters' preferences. However, if government spending reflects not just the wishes of voters but also other goals of legislators, as in a Leviathan or

[7]In future work, I intend to examine more closely the political economy of the adoption process and what proposing and passage of limits and other fiscal institutions reveals about the appropriateness of median voter, Tiebout and Leviathan models of government behavior.

budget maximizing bureaucratic model of government, then the level of spending might be higher than optimal from the voters' perspective. Legislators could try to maintain this level of spending by circumventing the limits. This could lead to effects voters never intended, such as a proliferation of special districts or a transfer of program responsibility between the different levels of government.[8,9]

Abrams and Dougan (1986), building on the "Leviathan" models of Brennan and Buchanan (1979), develop a theoretical framework for considering the effect of changes in government institutions. Dougan (1988) applies this theory to TELs and gives three explanations for the passage of TELs.

Traditional Leviathan theory posits that government acts as a monopoly created by voters. Once in power, government agents maximize the size of government subject to the constraints of being re-elected and (possibly) constitutional constraints. TELs act as a further constraint on governments from growing too large. If this were true, TELs would only be enacted as voter initiatives, all states who could would adopt TELs, and TELs would lead to slower growth in the size of government.

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[8]I will discuss the relationships between TELs and revenues and expenditures. TELs can affect both revenues and spending given the almost linear relationship between them. In general, when I am focusing on government actions and legislators I will talk about expenditures since legislatures typically have certain activities or functions they want the state to provide. In contrast, when I talk about limits from the voter's perspective I will talk about revenues, since voters in general want to minimize taxes.

[9]In a standard median voter model the level of expenditure is determined by the preferences of the median voter. Half of the voters would prefer less spending but the other half would prefer more spending. Constitutional restrictions and referenda leading to tax and expenditure limits would not receive the super-majority needed to pass if this model were correct.

Dougan (1988) proposes two other reasons for passage of limits, which allow for differences between states and explain both ineffective limits and legislatively proposed limits. One theory states that limitations act as a signal of changing voter preferences. Given that passing referenda is a lengthy process, changes in revenues or spending could occur before the TEL takes effect. Similarly a TEL proposed by the legislature could be a signal from the legislature that the change in preferences was in fact recognized and that levels of spending will reflect the change in preferences.

The second theory postulates that if voter tastes change, or if the size of government is getting far beyond voter optimum levels, then voters might adopt a TEL as a signal or warning to elected officials. States with faster growth rates would then be more likely to pass these limits, which could then lead to slowed growth after the limits were in place. TELs could be a less costly way of changing levels of spending than having to replace the entire legislature. Under this model, changes in the size of government would occur with or without passage of the limit if voter long-term preferences are satisfied. TELs simply act as a way to change the equilibrium size of government. If this were the reason for TELs being passed, government growth would be lower because of changes in voter preferences; thus a correlation between TELs and slower growth in the size of government is not necessarily evidence that TELs do act as an effective restraint.

2.2 Empirical Method

It is difficult to argue that the passage of a tax or expenditure limit is an exogenous shock, uncorrelated with the determinants of state taxes and expenditures. Such policy changes are especially prone to the criticism raised by Besley and Case (1994) in their critique of the "Natural Experiment" in public policy. Besley and Case question the randomness in assignment across governments of policies that these same governments set. If states with higher levels of spending are more likely to pass TELs, and spending patterns persist over time, then ensuing data analysis could reveal a spurious positive relationship between TELs and spending. This finding could emerge even if TELs actually have a negative effect. Alternately, if voter tastes for spending change so that voters prefer less government spending, and this shift motivates voters to propose or pass TELs, then a negative coefficient on TELs could indicate that they act as a proxy for the change in voter tastes and the actual limit could have little effect as a constraint. Using an instrumental variable approach, I hope to separate the constraining effect of these limits from the changes in underlying voter preferences which could have led to their passage. I will compare the results of simple OLS regressions from regression results that instrument for the enactment of a TEL.

To solve the endogeneity problem, I need to find variables that are correlated with the probability of a state passing a limit but which do not directly affect the level of revenues raised by a state. If revenues are a function of state attributes, voter attributes, and the presence of TELs (equation 1), and if the probability of a state having a TEL is a function of current and prior revenue levels, other state attributes and voter attributes including tastes for the size of government and taste for participation in the legislative process, then we have a simultaneous equation model. It is impossible to tell whether TELs are effectively constraining state and local government revenues without estimating both equations.

Formally, I assume that

$$Rev_{jt} = X_{jt} * \beta_1 + Z_{jt} * \beta_2 + TEL_{jt} * \beta_3 + Year_t * \beta_4 + \epsilon_{jt} \tag{1}$$

where X_{jt} is a vector of state attributes including gross state product, federal grants, the state unemployment rate, state population and growth in population as well as some state demographic variables, and Z_{jt} is a vector of observable and unobservable voter attributes including a proxy for the liberalness of voters, the average ADA score of each state's representatives by year and an unobservable taste for government services. I also model the probability of having a TEL as

$$TEL_{jt} = Prob(TEL_{jt}=1) = X_{jt} * \alpha_1 + Z * \alpha_2 + Year * \alpha_3 + Inst * \alpha_4 + \mu_{jt}. \tag{2}$$

Inst is a vector of variables which do not directly affect Rev_{jt}, but help predict whether a state has a limit based on the other theories of limit passage. Using single equation OLS regression techniques, it is impossible to separate out the direct effects of TELs on revenue and expenditure levels if there are unobservable voter attributes Z_k that affect both REV_{jt} and TEL_{jt}. These unobserved voter attributes will become unmeasured components of ϵ and μ and bias the estimated coefficient on TEL_{jt}. Since I am primarily interested in identifying the effects of TELs on revenues and expenditures, I focus on finding instruments that are correlated with the probability of a state having a TEL, but are not correlated with the random component of state revenue or expenditure levels. Therefore, my empirical strategy will be to find instruments which help explain enactment of TELs and yet are not directly related to voter tastes for government revenues or expenditures.

In addition to estimating equation (1) which assumes ϵ_{jt} and TEL_{jt} are uncorrelated, I also estimate (1) substituting TEL'_{jt}, the predicted probability of state j having a limit in year k, for TEL_{jt}. Comparing the results of these two specifications will provide evidence on the endogeneity of the passage of limits. Under the null hypothesis of random state assignment of limits, the OLS estimates of equation (1) should be both consistent and efficient, while the IV estimate will be consistent. However, if passage of TELs is not random across states, then the OLS estimates can be biased and inconsistent while the IV estimates are consistent. One way of implementing a Hausman (1978) specification test in this case is to include the estimated residuals from equation (2) in the IV regression of equation (1). I estimate

$$Rev_{jt} = X_{jt}*\beta_1 + Z_{jt}^{obs}*\beta_2 + TEL'_{jt}*\beta_3 + Year_t*\beta_4 + \hat{\mu}_{jt}*\beta_5 + \epsilon_{jt}. \qquad \textbf{(3)}$$

where $\hat{\mu}_{jt}$ is the estimated residual from equation (2). The null of random assignment is rejected if $\beta_3 \neq \beta_5$ in equation (3).

I also estimate OLS regressions including state fixed effects. The estimate of *TEL* in these regressions is dependent only on within-state, across-time effects. This can better control for unobserved fixed state effects. Unfortunately, due to the time invariance of the instruments I use, state fixed effects cannot be included in the IV estimates.

3. The Determinants of Tax and Expenditure Limit Passage

The effectiveness of TELs can be either under- or over-stated due to the endogenous nature of passage. States with higher levels of spending might be more likely to pass limits if voter tastes change, or states with voters who prefer less government services might be more likely to pass limits. This makes testing for the endogeneity of limits critical to understanding any role these institutions can have on the size of government. As outlined in section 2.2, I need to use information which is related to enactment of state limits but not directly related to government revenues or spending. If the probability of a state enacting a limit is related both to voter tastes for government spending and to voter tastes for participation in government, then variables which affect voter participation can be used to estimate which states have TELs.

In section 3.1, I give detailed information on the instruments used - these include direct legislation rules and information on voter ability to recall elected officials. Section 3.2 then presents linear probability models of TEL enactment (equation 2). These regressions are not a complete description of why states pass or have limits; they are used to predict the probability of passing a TEL using information which is believed not to affect state revenue and expenditures directly.

3.1 Description of Instruments

The primary instrument I use for TELs is an indicator variable which equals 1 if a state has a constitutional procedure for citizens proposing and passing referenda or initiatives.[10] Typically, direct legislation provisions have always been part of the state's constitution or were passed in the 1900s. I have information on whether states allow

[10]Information on direct legislation is taken from Magleby (1984). Matsusaka (1995) includes a good discussion of the different aspects of direct legislation provisions and the political climate in which they were passed.

statutory initiatives, constitutional initiatives or referenda; because the same state typically has more than one procedure I consider only whether a state has any direct legislation process.[11] Figure 3 presents a cross-state comparison of direct legislation laws and highlights the high correlation between citizen initiative and referenda rules. I use information on direct legislative processes from Magleby (1982). Due to the recent enactment of their initiative laws, I do not count Florida, Illinois and Wyoming as having direct referendum laws. These states first allowed initiatives to be proposed in 1978, 1970 and 1968 respectively. The classification of these three states as non direct legislation states is done to avoid another potential source of endogeneity; a change in direct legislation rules can reflect voter discontent with the size of government. In some specifications I also include variables on the ability of voters to recall legislators. These laws were also passed during of the Populist movement (1890-1920).[12]

Existing empirical evidence on the link between direct legislation processes and spending and revenue levels is inconclusive. Zax (1989) and Farnham (1990) find small, marginally significant, and positive effects of direct legislation processes on government

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[11]The states with historical provisions for direct legislation are Alaska, Arizona, Arkansas, California, Colorado, Idaho, Kentucky, Maine, Maryland, Massachusetts, Michigan, Missouri, Montana, Nebraska, Nevada, New Mexico, North Dakota, Ohio, Oklahoma, Oregon, South Dakota, Utah, and Washington.

[12]Another set of potential instruments is information on school finance lawsuits. Fischel (1989) argues for a link between school finance lawsuits and at least local property tax limits. It is not clear, however, that the overall level of revenues and expenditures are not also directly affected by these lawsuits. I have used information on the presence or success of a lawsuit in alternate regression specifications as instruments and covariates. The inclusion of these variables does not change the estimated effect of state TELs in the state finance equations.

expenditures in cross-sectional regressions. However, Matsusaka (1995) finds that states with direct legislation provisions have lower levels of both state and state and local expenditures. Matsusaka looks at state-level data over the 1960-1990 time period while the earlier work focussed on single cross-section results.

To explore the difference in these two sets of findings, I estimate reduced form revenue and spending equations for the period between 1961 and 1992

$$Rev_{jt} = X_{jt}*\gamma_1 + Direct_{jt}*\gamma_2 + Year_t*\gamma_3 + Region_k*\gamma_4 + \psi_{jt} \quad (4)$$

where X_{jt} denotes state specific variables including per capita income, population and federal grants received, $Year_t$ is a set of annual indicator variables and $Region_k$ is a set of nine regional indicator variables. $Direct_{jt}$ is an indicator variable which equals one if voters in state j have the ability to pass legislation directly. Table 2 presents the results of estimating equation 4 from 1961-1990. Columns 1 and 4 present regression results for the entire sample period. State and local per capita expenditure is \$66 (34)[13] less in states with direct legislation laws than in states without direct legislation laws, similarly state and local revenue is \$70 (34) less in states with direct legislation laws. These numbers are not statistically different from those found in Matsusaka (1995) despite differences in covariates and regression specifications.

I then test for a structural change in the model and estimate separate reduced form

[13]Standard errors are presented in parentheses for reported estimated coefficients. Standard errors have been adjusted to account for within state-group correlation and between group heteroskedasticity using White (1980) techniques.

equations before and after the tax revolt of the late 1970s. I find that states with direct referendum laws spend approximately \$104 (41) less per capita after 1977 than states without direct referenda laws as compared to spending \$28 (32) less before 1978. Similarly state and local revenues were \$128 (46) less in states with direct legislation rules after 1977 as compared with \$16 (32) less in states with direct legislation rules before 1978. In the above results, states with direct legislation are defined as those states with citizen initiative or referenda laws in place before 1960.[14] The null hypothesis of no structural break in these equations is also rejected at standard confidence levels. This lends support to the hypothesis that the way direct legislation provisions affect state revenues or expenditures is through passage of TELs.

3.2 Determinants of TEL Adoption

Table 3 presents estimates for a linear probability model of a state having a TEL in place. Columns 1 and 2 include only the instruments and year effects. My primary specification will only use the presence of direct referenda laws, while the alternate specification will also include information on the ability of voters to recall elected officials.[15] The ability of voters to pass legislation directly increases the probability of passing a TEL by 16 percent (6). Column 2 includes an indicator variable for voters' ability to recall

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[14]The results are even more pronounced if states with newly passed laws are included as direct referenda states after passage of the new rules. Similarly the difference in results is stronger if the period around the tax revolt (1975-1980) is omitted from both samples.

[15]Including a second instrument allows me to test the appropriateness of excluding the instruments from the revenue and expenditure equations directly. I will also later be interested in disentangling the effects of different types of limits and will need more than one instrument.

elected officials. The coefficient on the presence of direct legislation laws decreases to 12 percent (5). States with recall laws are 13 percent (7) more likely to have passed a limit.[16] Columns 3 and 4 present linear probability models including the covariates from equation 3. The coefficients on the direct legislation and recall variables do not significantly change.

The covariates have marginal explanatory power in predicting passage of limits and the estimated effects go in the expected direction. States with higher gross state product are less likely to adopt a limit, while faster growing states and states with a higher population are more likely to pass limits. A higher unemployment rate also seems to increase the probability of having a state limit in place. These variables reflect how well a state is doing comparatively over time and also the fact that limits are more likely to be in place during periods of economic distress. In addition, the presence of more children in the population, which could act as a proxy for higher state and local spending, increases the probability of having a limit. In contrast, the percent of population over 65 does not seem to affect the probability of having a TEL.

States with higher federal grants are less likely to have a limit in place. This reflects their lower cost of providing a given level of government services.[17] Finally, while I include

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[16]Including information on school finance suits does not statistically effect the predictive ability of the direct legislation variable. The coefficient on recall laws decreases to 9.5 percent (6), while the inclusion of a school finance lawsuit increases the probability of passage of a TEL by 15.4 percent (7.2).

[17]Depending on the nature of the Federal Aid received, either the marginal or average cost of government services can be affected. If federal aid is given in block grants, then the price of the marginal government purchase is unchanged. However, if federal grants require matching state funds, then the marginal price of the given service has decreased and we could expect increased government purchases.

average ADA scores for house members this does not seem to affect the probability of passing a TEL. I also include yearly indicator variables, which are statistically significant. Unsurprisingly, states are more likely to have TELs in later years.[18]

4. Empirical Findings

I estimate linear expenditure and revenue models for state and local governments for the 48 continental states.[19] I would like to test the response of both state finances covered by TELs and the response of non-restricted funds. Examining expenditures which are explicitly covered by limits will give the clearest evidence on the direct effect of limits. Changes in these expenditures or revenues could reflect a decrease in total government spending or a shift in expenditure to another (non-covered) category of government. I would therefore like data on both state general fund spending and revenues as well as overall state and local finances before and after limits were enacted.[20] General fund data will give evidence of a direct effect of limits, while data on broader measures of state and local finances can be used

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[18] Regressions based only on across state differences produce very similar results given the time invariant nature of the instruments. Regression results include standard errors which have been adjusted to allow for within state correlation and between state heteroskedasticity.

[19] In the results presented I omit Hawaii and Alaska from the estimation. Including or excluding Hawaii does not affect the results. Including Alaska, which has extremely high severance taxes, does seem to matter. State and local revenues as a percent of personal income for Alaska in 1990 are 55 percent, much higher than the US average of 19 percent. However, most of the revenues are from taxes on natural resources and the burden of these taxes do not fall on Alaskan residents. In contrast, 1990 state and local revenues from property, sales and income taxes was only 5.7 percent of personal income.

[20] I define $state_j$ as having a limit in year j, if a limit was passed (and not repealed) prior to the beginning of fiscal year j. States with advisory limits or limits which can be overridden by a simple legislative majority are not counted as "limit" states.

to test for offsetting effects. To measure these two effects I use data from two sources, The National Association of State Budget Officers (NASBO) and The Department of Commerce, Bureau of the Census (Census). Section 4.1 briefly describes these datasets and discusses the relative strengths and weaknesses of each.

I estimate two OLS regression models, including and excluding state fixed effects, and an IV model where the presence or absence of a binding limit is replaced with the predicted probability of state *j* having a TEL in year *t* from equation 2. Since most limits constrain state expenditures to a percent of personal income, the dependent variable is expenditure or revenue as a percentage of state personal income. The coefficients on the covariates are interpreted as the effect of changes in the covariate relative to the value of state personal income. Estimating aggregate government expenditure and revenue equations using total or per capita spending gives similar results to those reported below.

Section 4.2 presents regression results on the responsiveness of state general fund expenditures and revenues to limits using the NASBO data. This data best measures categories of state spending which are explicitly covered by TELs. Section 4.3 then uses the Census data to explore the effect of limits on a more broadly defined set of both state and local finance data for a longer period of time than that available in the NASBO dataset. Finally, Section 4.4 explores differences between various types of limits.

4.1 Government Finance Data

State expenditure and revenue data are from two sources, Fiscal Survey of the States, a collection of annual surveys of state budget officers published by NASBO, and Government Finances and State Government Finances annual statistics published by the

Department of Commerce, Bureau of the Census. I will refer to these data sets as the NASBO data and the Census data respectively.

The NASBO data includes information on general fund spending and revenues. These figures measure what administrators in each state consider the state's general fund and so best measure the category of spending covered by state limits. The disadvantage of this data is that it is only available for a limited time period (1982-1990), which is after passage of most of the TELs. In addition, the amount of total state spending which is included in this survey can differ greatly from state to state. This data will best capture any direct effects of TELs but cannot be used to measure offsetting effects which might occur.

The Bureau of the Census collects annual information on both state and aggregate within state local spending and revenues. I use data from 1970-1991, covering the period before limits were adopted as well as that following the tax revolt. This data has the advantage of covering a longer time period and it also permits me to examine both state and local spending and revenues, as well as different categories of spending.

The disadvantage of the Census data is the breadth of the definition of general fund finances. Although the definition of general fund revenues and expenditures in the Census Data is more uniform across states than in the NASBO data, the only government finances excluded from the general fund category are those of state-owned utilities and liquor stores and insurance trust revenue. While these categories of spending and revenues are never directly covered by TELs, the reported general fund figures do include other categories of spending and revenues which are also not covered directly by TELs. These categories include intergovernmental aid and revenues and expenditures from special funds. However, the Census data can be used to check if the enactment of TELs had an effect on aggregate state spending or led to a re-categorization of spending or revenue sources.

The Census data can also be used to check for effects of TELs on the size of the total state and local government. By examining local spending and revenues and aggregate state and local finances, I can test for a shift in the level of service provision. One caveat must be made about the local data. While this is the primary source of annual local government finance data available, it is not based on a comprehensive accounting of local governments. It is based on total sampling of local entities above a certain size and a proportional sampling of others based on financial activity.[21] This leads to an additional source of variance, due to sampling error in the local spending and revenue equations and the total state and local finance equations.

To illustrate the difference between the two datasets, note that the NASBO data estimates of average state general fund spending is 6 percent (standard deviation 1.4) of personal income in 1986, while the Census data estimate is 8.25 percent (2.1). The difference between the revenue figures is more striking; the NASBO estimate is 5.6 percent (1.4) compared to overall state general revenues of 12.1 percent (3.2) as reported in the Government Finances data. About half of this difference (3.3 percent) is due to state transfers to local governments. The other major differences in these data are from spending on capital projects and special fund finances.

[21]The government areas which are sampled with certainty are county governments with population greater than 50,000; all municipal and township governments with more than 25,000 residents and all school district governments with enrollment greater than 5,000.

4.2 The Effect of TELs on State Revenues and Expenditures Using NASBO Data

Table 4 presents results using the NASBO data. These regressions use data from 1982-1990 and examine state general fund data only. Column 1 estimates a standard reduced-form demand equation for state expenditures with an indicator variable for a potentially binding limit on state expenditures or revenues.[22] The resulting estimates show a small, albeit insignificant effect of having a limit.[23] The hypothesis that the OLS estimated coefficient equals zero cannot be rejected at standard confidence intervals. Column 2 presents OLS estimates including state fixed effects. Once again the hypothesis of no effect of state limits cannot be rejected.

The third column of Table 4 presents the IV estimates of the effect of TELs on state expenditures using information on direct legislation laws to address the endogeneity of TELs. The IV results suggest a substantial <u>negative</u> effect of state TELs on state general fund expenditures. The estimated effect is large, with limits estimated to reduce state spending by 2.1 percent (1.0) of personal income, or nearly 30 percent of state general fund spending. This finding underscores the importance of recognizing the endogenous nature of the imposition of state fiscal institutions. I explicitly test the importance of instrumenting by including $\hat{\mu}$, the residual from the TEL linear probability model in the specification. The

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[22] Bergstrom and Goodman(1973) and Borcherding and Deacon(1972) are examples of other work using this standard methodology.

[23] All regression results are presented with White (1980) standard errors. These have been adjusted to allow for within state group correlation and between group heteroskedasticity. These adjusted standard errors are two to five times larger than those reported using standard regression techniques.

coefficient on $\hat{\rho}$ is -.001 (.004) which is statistically different from -.021, the estimated coefficient on TEL'_{jt}. We can therefore reject the null hypothesis of random assignment of TELs across states at a 90 percent significance level.

The fourth column of Table 4 presents instrumental variable estimates using both information on direct legislation rules and information on voters' ability to recall elected officials. The estimated effect decreases slightly to 2.0 percent (0.9).[24] Including a second instrument also allows us to explicitly test whether using these variables as instruments is appropriate. Performing a test of the over-identifying restriction leads us to reject the null hypothesis that the instruments belong directly in the spending equation.[25]

Columns 5-8 present similar results for general fund revenues. The results are almost identical to those found for general fund expenditures. The general fund, as defined by state budget officers, excludes state transfers and is required to be in balance at the end of the year in twenty-four states[26]. In OLS regressions, the state TELs are found to have a small negative effect on general fund revenues. This result is reversed when state fixed

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[24] Comparing this IV specification to the OLS results in column 1, we can again reject the hypothesis of random assignment of limits across states at a 95 percent significance level.

[25] To test the direct explanatory power of the instruments in explaining the level of state spending, I run a regression of the IV residuals on the instruments. Examining the NR^2, from this regression, we can reject the null hypothesis of the instruments directly affecting the level of state spending. For this equation $NR^2 = .432$.

[26] Every state except Vermont has some sort of balanced budget rule. Forty-four states require that the governor submit a balanced budget, thirty-seven require the legislature pass a balanced budget while twenty-four require that the end of year actual expenditure and revenue values balance. Poterba (1995) discusses these balanced budget rules and summarizes recent work on the effectiveness of the different limits.

effects are included in the specification. The IV results (Table 4, Columns 7 and 8) suggest a substantial negative effect of state TELs on state government revenues. The presence of a binding limit decreases general fund revenues by 2.5 percent (1). Again, we can reject the null hypothesis of random assignment of limits across states at standard significance levels. The remainder of the covariates in this equation are marginally significant. Given that general funds are defined differently across states, most of the variation within this panel is across states and this difference in definitions decreases the explanatory power of the covariates. An increase in gross state product as a percent of personal income is correlated with a greater than proportional increase in state spending. Higher ADA scores reflect more liberal voters and are also correlated with higher spending and revenue. Finally, a higher unemployment rate leads to an increase in state general fund spending. The signs on the other covariates are not statistically different from zero. Although these variables are often included in other studies, and are sometimes statistically significant, the normalization of the dependent variable by state personal income may help explain the divergence in findings of significant effects of covariates in other studies and this study.

4.3 State TELs and Government Expenditures and Revenues: Census of Governments Data

Table 5 presents estimates of the effect of TELs on state and local general fund expenditures; as noted above the definition of state general expenditures is broader than that used by NASBO and includes more categories of spending. This will allow us to test for increases in non-covered expenditures by comparing the two sets of estimates. Regressions also include the period before and after passage of these limits (1970-1991) for the 48 continental states. Column 1, again, presents the results from a standard reduced form spending equation. The resulting estimate of the effect of the limit is negligible. Column 2 presents OLS estimates including state fixed effects, and we again cannot reject the hypothesis of no effect of state limits.

The third column of Table 5 presents the instrumental variable estimates of the effect of TELs on state expenditures using information on direct legislation laws to address the endogeneity of TELs. In contrast to the OLS results, and consistent with the NASBO data results, the IV results suggest a substantial negative effect of state TELs on state government expenditures. The estimated effect is large, with limits estimated to reduce state spending by 1.8 percent (0.6) of personal income. This is a slightly smaller effect than that found using the NASBO data, but is not statistically different from that result[27]. Thus, there is little evidence of state budget officials responding to binding TELs by transferring services and responsibility to off-budget activity. I test and reject the hypothesis of random assignment of limits across states. The signs on the other covariates are as expected and do not change direction when switching from OLS to IV estimation. If gross state product increases as a percent of personal income, there is no corresponding increase in state spending. Higher state population leads to a decrease in state spending as a percentage of personal income. The percent of the population which is of school age is negatively related to the amount of

[27]Limiting the sample period of the regression to the same years covered by the NASBO data produces a smaller, but not statistically different, decrease in the coefficient estimates in state spending and revenue regressions. Therefore, there is some evidence that TELs decrease total state spending more in the period directly following passage and then become less constraining as state governments move some budget items into non-covered areas. This linkage warrants further investigation.

state spending. If the school age population increased from fifteen to twenty percent of the population this would lead to a decrease in state spending as a percent of personal income of four-tenths of one percent. This decrease would be more than offset by a larger estimated effect on local spending.

An increase in federal funds leads to a more than one-for-one increase in state expenditure. This is probably due to matching features in most federal aid programs. This is significant evidence of a "fly-paper" effect that has been found by other researchers. Hines and Thaler (1995) summarize recent studies of this effect. Higher average ADA scores reflect more liberal voters, and are correlated with higher state spending. Finally, a higher unemployment rate has little effect on state spending.

The last six columns of Table 5 explore the effects of state limits on local government spending. These regressions are estimated less precisely than the state expenditure equations, with most of the variation occurring between states rather than over time. The results in Columns 4-6 show that the OLS estimate of the effect of state limits on local government spending is small and statistically not different from zero. The IV results, which are shown in Column 6, suggest a small positive effect of limits on local spending, with an estimated .5 percent increase in local spending. These results suggest that there is some shifting of spending from state to localities when states enact TELs.[28]

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[28]Using alternative instrument sets does not change the predicted effect of limits on state expenditures. However, the choice of instrument set does affect the magnitude, but not the sign, of the predicted effect of local governments. It is clear that some offset occurs although the magnitude of this effect is not precisely known. Therefore, the effect of potentially binding state TELs on aggregate state and local spending is unclear.

The results in the last three columns show the effect of TELs on total state and local spending. Not surprisingly, the OLS estimates are small and statistically indistinguishable from zero. The IV estimate of the coefficient is negative but the hypothesis that this coefficient is zero cannot be rejected at standard confidence intervals. The pattern of results in Table 5 suggest that TELs reduce state level spending, but that approximately one third of this reduction is offset by higher local spending. This suggests the importance of examining total state and local spending even when analyzing state level institutions.

Table 6 presents regression results for state and local revenues, in contrast to expenditures in Table 5. Again, there seems to be little effect of limits in the OLS regressions presented in Column 1. Including state effects does not change this result. When the endogeneity of limits is accounted for there again seems to be a negative (-1.9 percent) effect of limits on state general revenues.

This is very similar to the estimates of the effects of binding TELs on revenues found in Table 4. This is some evidence that state governments are not reallocating revenues to special funds and away from general funds in response to tax limits.

Columns 4-6 present similar results on the effect of state limits on local revenues. Column 4 presents OLS estimates and again there is little evidence of an effect of state TELs on local revenues. The IV estimate is presented in Column 6. There is evidence of a 1.5 percent (.9) increase in local general fund revenues.

Finally, in the last three columns of Table 6, I examine the effect of limits on total state and local revenues. Again, the OLS estimate of the aggregate effect of limits is small and not significantly different from zero. The IV results (presented in Column 9) indicate that state limits on total state and local spending are associated with a small decrease in spending. However, the aggregate effect of TELs on total state and local revenue is again not significantly different from zero.

4.4 Differences Between Types of Limits

Table 7 examines the effect of different types of TELs on state and local expenditures and revenues. If there are differences between types of limits, this would lend weight to the hypothesis that certain limits act as more than signals between voters and elected officials, or that certain types of limits are more binding than others.

Given the limited number of instrumental variables available, I cannot explore differences in all the characteristics of TELs at once. Instead I look at pair-wise comparisons of different traits. Since certain limit characteristics are highly correlated, attributing the entire effect to the studied trait would be misleading. For example revenue limits are more likely to be passed as citizen initiatives, so a stronger effect of revenue limits could be reflecting an effect of citizen initiatives. The regression specifications include the same set as covariates as those reported in Tables 5 and 6. The coefficients on the covariates remain relatively constant over the different specifications, and are not reported in Table 7. Since in almost all cases the OLS results are not statistically different from zero and the hypothesis of random assignment of types of limits across states is rejected, I only present results from the IV specification.

The first set of estimates in Table 7 compares the effect on expenditures of limits only on state governments versus limits on both state and local governments. The coefficient on states with limits on both state and local governments measures the additional effect of limiting both state and local governments. Column 1 presents spending results; states with binding limits spend 3.5 percent less than states with no limits. However, the hypothesis that this effect is not statistically different from zero cannot be rejected. This effect is smaller, but still negative, if the limit also extends to localities as well.

Column 3 explores the effect of various limits on local spending. The regression equations explain less of the variation in local spending than in state spending. Column 3 presents the IV results for local general fund spending. In states with state only limits, local spending increases by 4.7 percent, a larger increase than the decrease in state level spending. However, in states with both state and local limits, local spending is decreased by 3.6 percent. Column 5 shows the effect of limits on aggregate state and local spending. Column 5 reveals a positive, not significantly different from zero, effect of state only limits. In contrast, states with both state and local limits have a lower level of aggregate spending.

Columns 2, 4 and 6 present similar results for total general revenues. The IV results reveal that while there seems to be little effect of limits only on state governments, when limits are extended to local governments, state general fund revenues are 3.6 percent smaller than they otherwise would have been. This decrease in state total revenue is offset by a small increase in local revenues, leading to an overall 2.1 percent decrease in state and local revenues in states with limits on both state and local governments.

The next set of rows in Table 7 explore the different effects of revenue and expenditure limits on state and local governments. Column 1 presents evidence on the effect of revenue and expenditure limits on state spending. The results in column 1 suggest that expenditure limits reduce state spending by 2.7 percent while revenue limits decrease state spending by only 1.2 percent. However, the null hypothesis of no difference in these two coefficients cannot be rejected at standard confidence levels. The effect is different for local spending; local spending decreases by 1.7 percent if a state revenue limit is in place, although this estimate is not significantly different from zero, while a state expenditure limit leads to a 6.7 percent increase in local spending. Since revenue limits usually include state tax revenues (including revenue which will be transferred to local governments), while expenditure limits do not usually cover state financed local spending, this result is consistent with a theory of states trying to circumvent limits by re-allocating the level of government at which services are provided. Finally, the IV estimates in column 5 suggest that the net effect of revenue limits on state and local spending is to decrease the size of government by 2.9 percent while the corresponding effect of expenditure limits is to increase spending by 4.1 percent. The effect of revenue and expenditure limits on state and local revenues is similar. Revenue limits seem to decrease both state and total state and local revenues by 2.4 percent while expenditure limits actually seem to increase revenue levels by 5.1 percent, mostly at the local level. Thus revenue limits seem to have a much greater effect.

The third aspect of limits examined is whether the limit in place is a statutory or constitutional limit. While the hypothesis of equal effects of both statutory and constitutional limits on state revenues and expenditures cannot be rejected, the two types of limits have notably different effects on the size of total state and local government. Statutory limits lead to greater local spending while constitutional limits do not. This may reflect the more narrow focus of statutory limits on state general fund spending, unlike constitutional limits which typically include provisions which restrict the shifting of responsibility of state programs to local governments.

5. Conclusions

This paper explores the effect of constitutional and legislative TELs on state and local government spending over the last two decades. Currently twenty-four states have laws limiting government appropriations or revenues. Prior work on this question has produced little evidence that these limits matter. However, studying the effect of TELs is complicated by the potential endogeneity of these limits. The presence of these limits may be correlated with voter preferences. Using historical information on the availability of direct legislation procedures as instrumental variables for TELs, I find that state general expenditure as a percent of personal income is two percent smaller in states with limits. This effect is partially offset by higher local spending in these states. In addition, the type of limit seems to matter. Limits on both state and local governments produce greater reductions in spending than state limits alone. Revenue limits appear more constraining than expenditure limits, and constitutional limits appear to have a greater effect than statutory limits.

In further work I plan to explore differences in the effects of limits on different types of revenues and expenditures. The role of capital vs operating budgets clearly warrants attention. In addition, it should be possible to study how different components of the operating budget are affected by different types of limits. In one example of this type of analysis, Poterba and Rueben(1995) find that relative wages for workers in state and local government grew more slowly in states with TELs in the 1979-1991 time period than in states without these limits. I hope to further explore the effects of these limits on public sector workers by examining both total wage bills and the number of workers in the state and local sector.

There is also further work to be done exploring how the local offset to the decrease in state spending occurs. Local spending can increase in two ways; existing local governments could spend more or there could be an increased number of local governments. The effects of increased unfunded mandates are very different than the effects of a state creating and funding more special districts to avoid TELs. There is also the issue of whether this shift in government expenditure reflects an actual transfer of responsibility or just a transfer of funding. Exploring the connections between state and local governments and the constraining nature of these limits well become more important as the states' role in providing government services increases in the next few years.

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Table 1: Information on States with TELs

	Expenditure		Revenue	
	Potentially Binding	Non-Binding	Potentially Binding	Non-Binding
Citizen Proposed	3	0	4	0
Legislature Proposed	7	8	0	1
Statutory	4	6	2	1
Constitutional	7	2	2	0
Local Limit Included In TEL	3	1	2	0
Need to Adjust Limit If Switch Spending Responsibility	6	2	3	0

Notes: Missouri has both a revenue and an expenditure limit. South Carolina passed a statutory limit in 1980, which was made part of the constitution in 1984. Non-binding limits are those which are purely advisory or only require a legislative majority to override or amend.

Table 2: State and Local Government Reduced Form Regressions, 1961-1977 and 1978-1992

	Expenditure	Expenditure 1961-1977	Expenditure 1978-1992	Revenue	Revenue 1961-1977	Revenue 1978-1992
Direct Legislation	-65.84 (33.75)	-28.14 (32.43)	-104.37 (40.77)	-70.11 (33.79)	-15.65 (32.33)	-127.82 (45.83)
Per Capita Income	0.11 (0.01)	0.11 (0.02)	0.12 (0.01)	0.12 (0.01)	0.11 (0.01)	0.13 (0.01)
Population	-0.21 (5.20)	1.26 (6.59)	-1.81 (4.32)	3.10 (4.16)	6.62 (5.40)	-0.01 (3.41)
Federal Grants Received	2.24 (0.08)	2.03 (0.22)	2.55 (0.21)	2.60 (0.19)	2.04 (0.20)	3.19 (0.24)
No. Obs.	1536	816	720	1536	816	720
Adj. R-Squared	.9281	.9011	.9046	.9294	.9296	.9020

Regression results exclude Alaska and Hawaii, and measure per capita state and local revenues and spending in 1982 dollars. Annual indicator variabes and nine regional variables are also included. Data is from the Department of Commerce, Bureau of the Census. Standard errors are reported in parentheses and are adjusted to allow for correlation within-state groups and between group heteroscedasticity.

Table 3: Linear Probability Models of States Having Binding Tax or Expenditure Limits

	Instruments Only		Including Regression Covariates	
Direct Legislation	.161 (.057)	.121 (.050)	.145 (.051)	.111 (.050)
Recall		.133 (.067)		.125 (.063)
GSP			-.231 (.179)	-.276 (.197)
Population			.008 (.006)	.008 (.005)
Change in Population			.422 (.670)	.185 (.577)
% Population 5-17			2.206 (2.689)	2.122 (2.388)
% Population Over 65			-.267 (1.642)	.043 (1.612)
Federal Grants			-1.968 (2.486)	-1.443 (2.374)
ADA House Score			.033 (.092)	-.015 (.089)
Unemployment Rate			1.497 (1.164)	1.028 (1.075)
Adj. R-Squared	.1363	.1671	.1673	.1975

Regressions exclude Alaska and Hawaii and include year indicator variables. Standard errors are reported in parentheses and are adjusted to allow for within state-group correlation and between group heteroscedasticity. Non-indicator variables are normalized by state personal income.

Table 4: The Effect of Binding State Limits on State Spending and Revenues from General Fund, Using NASBO Data

	State General Fund Spending				State General Fund Revenues			
	OLS	Fixed Effects	IV	Alternate IV	OLS	Fixed Effects	IV	Alternate IV
Binding State Limit	-.003	.005	-.021	-.020	-.004	.005	-.025	-.022
	(.004)	(.005)	(.010)	(.009)	(.004)	(.005)	(.010)	(.009)
Gross State Product	.023	.035	.012	.013	.028	.015	.016	.017
	(.017)	(.013)	(.017)	(.017)	(.014)	(.007)	(.015)	(.016)
Population	.111	.143	.187	.269	.064	-.076	-.042	-.030
	(.213)	(.182)	(.210)	(.198)	(.216)	(.146)	(.208)	(.196)
Change in Population	-.007	-.072	.024	.021	-.014	-.020	.021	.017
	(.042)	(.023)	(.037)	(.039)	(.041)	(.020)	(.034)	(.036)
% Pop School Age	-.016	-.252	.089	.080	.038	-.047	.157	.144
	(.193)	(.121)	(.200)	(.185)	(.174)	(.093)	(.175)	(.158)
% Pop Over 65	-.098	-.517	-.106	-.105	-.083	-.337	-.092	-.091
	(.114)	(.190)	(.098)	(.097)	(.116)	(.154)	(.096)	(.096)
Federal Grants	.107	.280	.113	.112	.060	.330	.067	.066
	(.265)	(.101)	(.276)	(.282)	(.231)	(.101)	(.243)	(.247)
House ADA Score	.034	.006	.037	.037	.033	.003	.036	.036
	(.010)	(.003)	(.009)	(.010)	(.009)	(.003)	(.009)	(.009)
Unemployment Rate	.061	.056	.134	.128	.077	.040	.160	.151
	(.086)	(.035)	(.075)	(.070)	(.083)	(.027)	(.075)	(.070)
Adj R-Squared	.2234	.9129	393.80	7.82	.2410	.9263	379.60	8.29

Notes for Table 4: Regression equations include annual indicator variables for 1982-1990. There are 9 annual observations for the 48 continental states. Standard errors are reported in parentheses and have been adjusted to allow for within-state correlation and heteorskedaticity between groups. Variables are measured as a percent of state personal income. Fixed effects specifications include individual state-fixed effects. Data is from National Association of State Budget Officers "Fiscal Survey of the States". For IV estimates F-statistics (18, 413) are presented instead of R-squared measures. The IV specification uses direct legislation information to instrument for the presence of a TEL. The alternate IV specification also includes information on voter's ability to recall elected officials.

Table 5: The Effect of Binding State Limits on State and Local Spending, Bureau of the Census Data

	State Spending			Local Spending			State and Local Spending		
	OLS	Fixed Effects	IV	OLS	Fixed Effects	IV	OLS	Fixed Effects	IV
Binding State Limit	.001	.000	-.018	.001	-.002	.005	.002	-.001	-.012
	(.003)	(.002)	(.006)	(.005)	(.002)	(.010)	(.005)	(.003)	(.009)
Gross State Product	-.025	-.003	-.050	.368	-.015	.373	.343	-.018	.324
	(.066)	(.060)	(.060)	(.090)	(.111)	(.090)	(.059)	(.132)	(.064)
Population	-.138	-.019	-.122	.126	.064	.122	-.011	.045	.000
	(.023)	(.071)	(.025)	(.060)	(.119)	(.060)	(.053)	(.088)	(.055)
Change in Population	-.024	.006	-.009	.039	-.076	.035	.014	-.071	.027
	(.018)	(.028)	(.028)	(.050)	(.035)	(.056)	(.045)	(.053)	(.049)
% Pop School Age	-.135	-.106	-.089	.477	.110	.467	.342	.004	.378
	(.100)	(.106)	(.042)	(.171)	(.111)	(.215)	(.123)	(.151)	(.155)
% Pop Over 65	-.126	.145	-.132	.241	-.472	.242	.114	-.326	.109
	(.085)	(.228)	(.031)	(.114)	(.241)	(.123)	(.099)	(.372)	(.041)
Federal Grants	1.429	.926	1.378	.504	.242	.516	1.933	1.168	1.893
	(.131)	(.188)	(.045)	(.224)	(.282)	(.254)	(.192)	(.427)	(.202)
House ADA Score	.011	.002	.014	-.004	-.009	-.005	.007	-.007	.009
	(.004)	(.004)	(.004)	(.010)	(.006)	(.011)	(.008)	(.009)	(.010)
Unemployment Rate	.020	.043	.046	.145	.186	.139	.165	.229	.186
	(.047)	(.050)	(.064)	(.075)	(.039)	(.087)	(.061)	(.062)	(.067)
Adj R-Squared	.7517	.9175	84.69	.3391	.8680	18.68	.6901	.8721	73.03

Table 6: The Effect of Binding State Limits on State and Local Revenues, Bureau of the Census Data

	State Revenues			Local Revenues			State and Local Revenues		
	OLS	Fixed Effects	IV	OLS	Fixed Effects	IV	OLS	Fixed Effects	IV
Binding State Limit	.000 (.004)	.001 (.002)	-.019 (.008)	-.000 (.004)	-.004 (.003)	.015 (.009)	.000 (.005)	-.002 (.004)	-.004 (.009)
Gross State Product	-.250 (.057)	-.168 (.119)	.225 (.068)	.330 (.089)	-.092 (.102)	.349 (.090)	.580 (.083)	.260 (.211)	.574 (.087)
Population	.066 (.030)	-.185 (.107)	-.050 (.025)	.065 (.042)	.039 (.144)	.052 (.060)	-.001 (.043)	-.146 (.094)	.002 (.055)
Change in Population	-.014 (.032)	-.060 (.045)	.003 (.028)	.003 (.032)	-.044 (.018)	-.010 (.030)	-.010 (.048)	-.104 (.051)	-.007 (.015)
% Pop School Age	.172 (.131)	.022 (.112)	.222 (.053)	.311 (.103)	.188 (.083)	.273 (.128)	.483 (.139)	.210 (.167)	.494 (.058)
% Pop Over 65	-.016 (.105)	-.147 (.301)	-.022 (.038)	.233 (.099)	-.264 (.196)	.238 (.100)	.217 (.106)	-.411 (.449)	.216 (.042)
Federal Grants	1.989 (.135)	1.314 (.385)	1.935 (.160)	.250 (.179)	.160 (.224)	.292 (.204)	2.239 (.185)	1.475 (.595)	2.227 (.202)
House ADA Score	.002 (.006)	-.009 (.007)	.005 (.003)	-.003 (.007)	-.007 (.004)	-.005 (.008)	-.000 (.010)	-.015 (.010)	-.000 (.010)
Unemployment Rate	.109 (.076)	.012 (.046)	.137 (.064)	.009 (.069)	.171 (.038)	-.013 (.087)	.118 (.074)	.183 (.070)	.124 (.033)
Adj R-Squared	.7460	.9229	88.52	.2864	.8310	13.56	.7457	.8772	102.00

Notes for Table 5 and 6: Regression equations include annual indicator variables for 1970-1990. There are 1006 state-year observations for the 48 continental states. Variables are measured as a percent of state personal income. Fixed effects specifications include individual state-fixed effects. Data is from the Bureau of the Census "Government Finances" and "State Government Finances". For IV estimates F-statistics (30,975) are presented instead of R-Squared measures. Standard errors are given in parentheses and are adjusted to allow for within state correlation and between state heteroskedasticity. IV regressions use direct legislation rules as an instrument for the presence of TELs.

Table 7: The Effects of Different Types of Limits on State and Local Government

	State General Fund		Local General Fund		State and Local General Fund	
	Spending	Revenues	Spending	Revenues	Spending	Revenues
State Limits	-.035	.008	.047	-.009	.010	-.001
	(.028)	(.010)	(.014)	(.011)	(.012)	(.010)
TEL extends to Localities	.025	-.044	-.083	.021	-.044	-.021
	(.014)	(.022)	(.029)	(.022)	(.024)	(.015)
F-Statistics	59.91	76.72	8.37	4.90	.6119	92.22
Binding Revenue Limit	-.012	-.020	-.017	-.006	-.029	-.024
	(.008)	(.008)	(.014)	(.011)	(.011)	(.013)
Binding Expenditure Limit	-.027	-.001	.067	.048	.041	.051
	(.007)	(.007)	(.012)	(.010)	(.010)	(.011)
F-Statistics	67.37	98.16	7.06	4.53	46.61	41.10
Constitutional Limit	-.013	-.017	-.057	-.052	-.070	-.021
	(.011)	(.011)	(.037)	(.030)	(.033)	(.289)
Statutory Limit	-.035	-.004	.185	.139	.150	.048
	(.016)	(.016)	(.054)	(.045)	(.049)	(.660)
F-Statistics	.6320	.7348	2.24	18.89	11.34	58.40

Note: Regressions are run with the same covariates found in Tables 5 and 6 Instrumental variable results are presented, where the different types of limits are instrumented for using direct legislation rules and information on voters ability to recall elected officials. Variables are measured as a percent of state personal income. The effect of the types of limits have been estimated in grouped pairs. Data is from the Department of Commerce, Bureau of the Census "Government Finances" and "State Government Finances". F-statistics (30,975) are presented.

Figure 1: State and Local Finances as a Percent of Personal Income

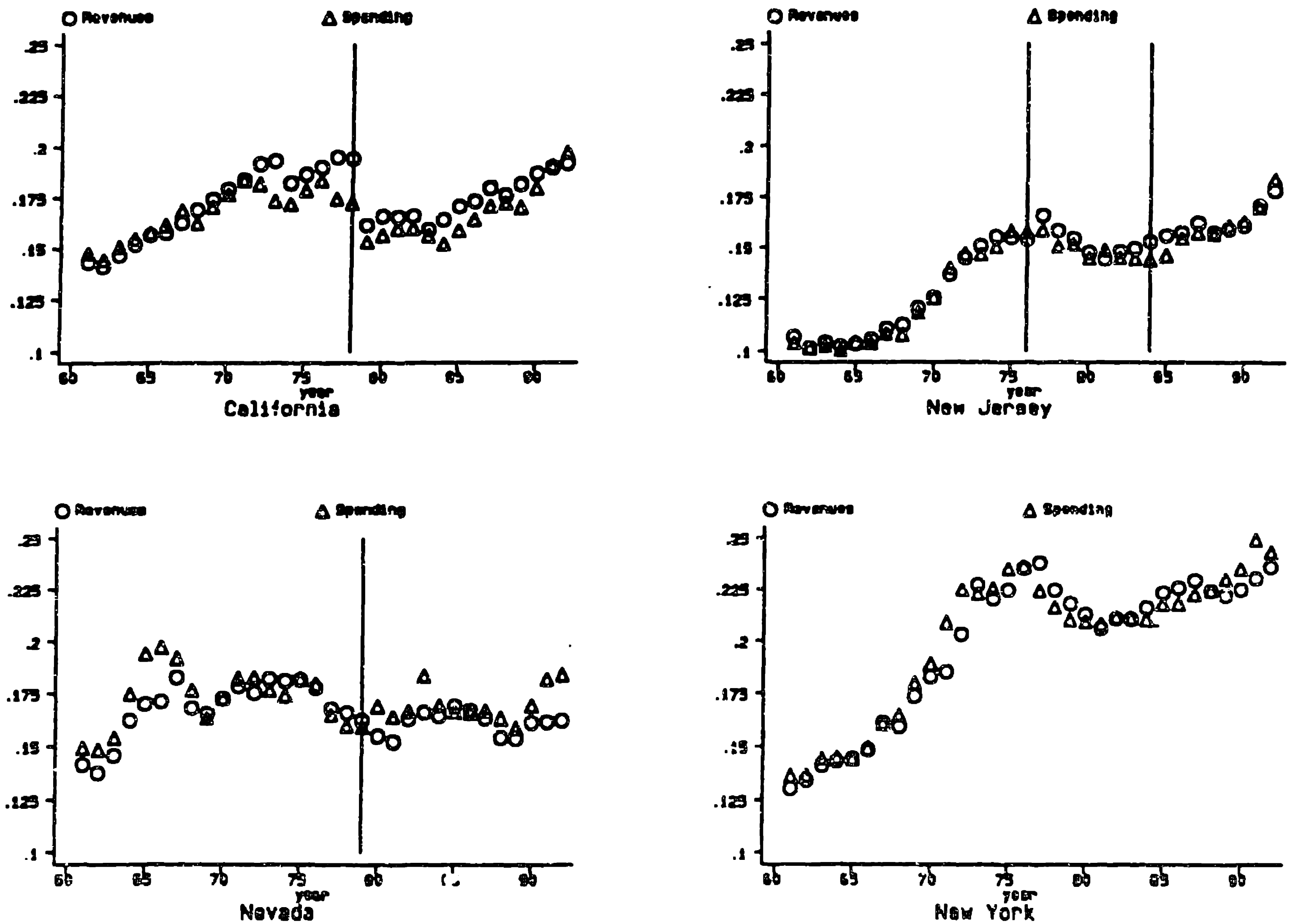
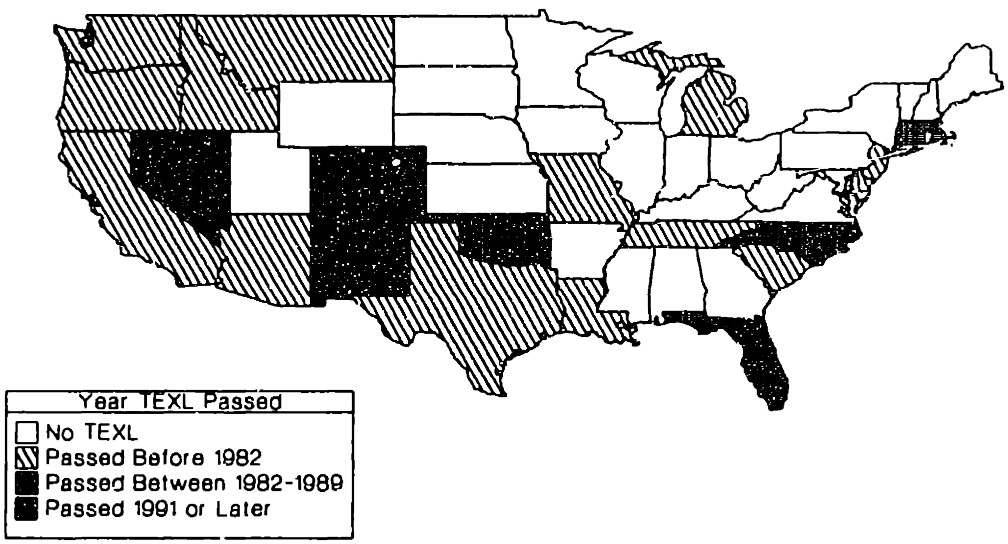


Figure 2:

States with Tax and/or Expenditure Limits



States with Tax and/or Expenditure Limits

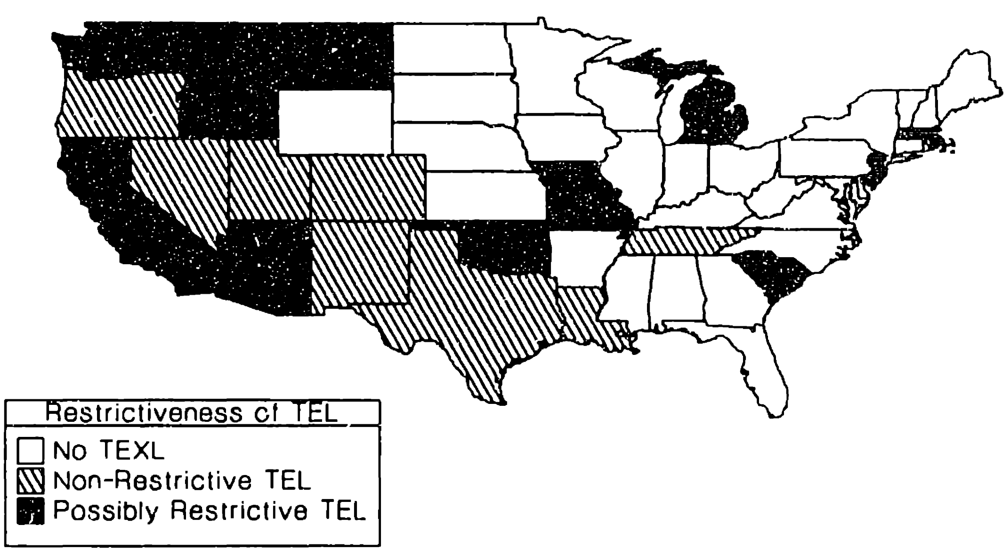


Figure 3:

States With Direct Referendum

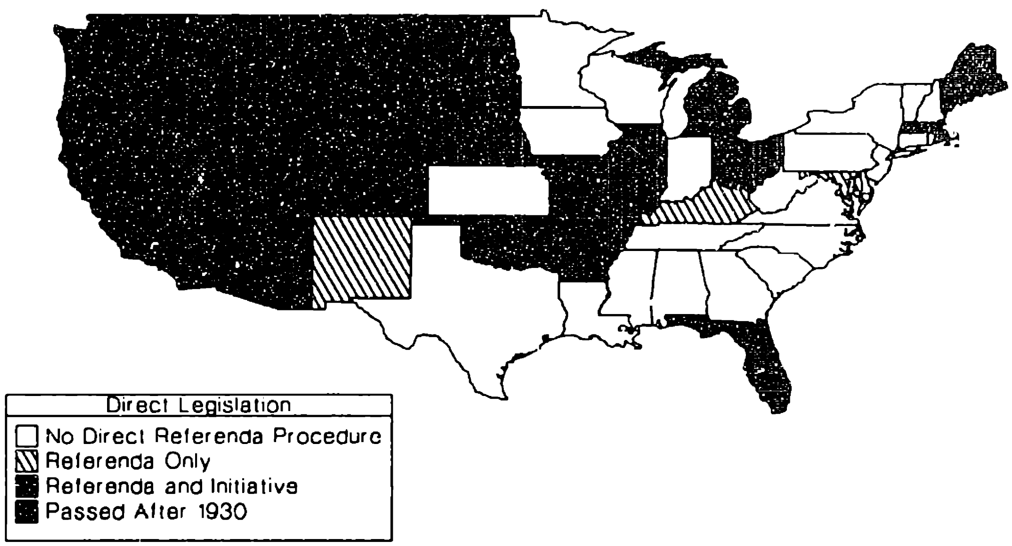
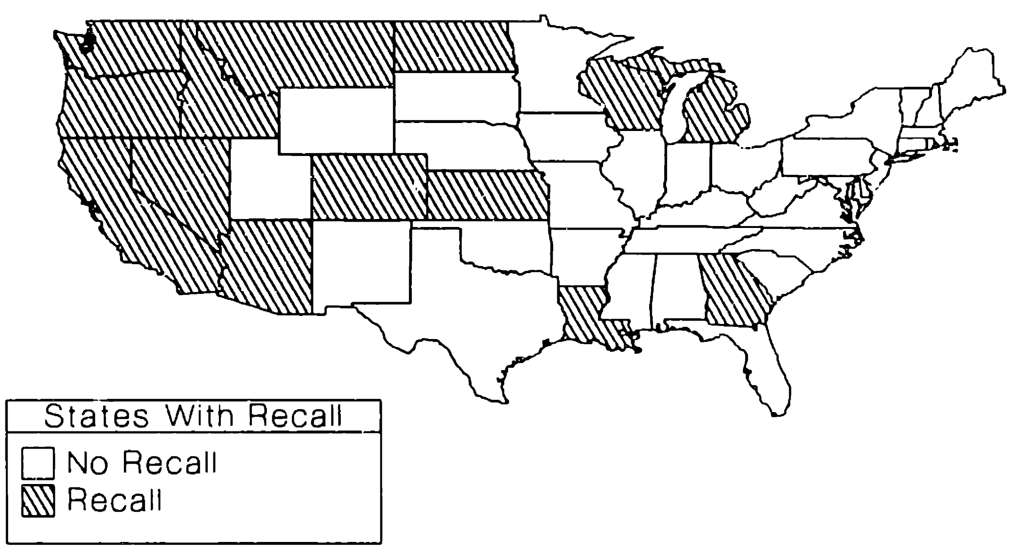


Figure 4:

States with Recall Laws



Appendix A: List of Adoption Dates of Tax and Expenditure Limits

State	Year Binding	Type of Limit	Need Supermajority or Referendum for Waiver
AK	1982 Binding	ΔExp < ΔCPI +Δ Pop Const, Legis Ref	3/4 of legislators and voter referenda
AZ	1978 Binding	Approp<7% of State Personal Income(PI), Const, Legis Ref	2/3 of each house
CA	1979 Binding	ΔExp < ΔCPI +Δ Pop Cons, Cit Init, Local Limit	Can exceed but need to pay back following years
CO	1977	ΔExp<7% of State PI, Stat, Legis	Binding rule passed in 1991
CT	1991	ΔExp< Max of 5% or 5-year average of PI or CPI growth rate	3/5 of each house and governor declares emergency
FL	1994	Growth rate of PI	2/3 of each house
HI	1978 Binding	ΔApp<Δ PI over past 3 years Const,	2/3 of each house
ID	1980 Binding	Exp<5.33% of State PI Stat, Local Limit	2/3 of each house
LA	1979	ΔTax Rev<ΔPI, Stat, Legis	In 1991 need 2/3 each house
MA	1986 Binding	ΔRev<Δ wage & salaries past 3 years Stat, Initiative, Local Limit	

Appendix A: List of Adoption Dates of Tax and Expenditure Limits (Cont.)

State	Year	Type of Limit on Growth	Need Supermajority or Ref for Waiver
MI	1978 Binding	ΔRev<ΔPI last year or past 3 yrs Const, Cit Init, Local Limit	2/3 of each house and governor declares emergency
MO	1980 Binding	ΔRev & ΔExp<ΔPI last year or past 3 yrs, Const, Cit Init	2/3 of each house and governor declares emergency
MT	1981 Binding	ΔExp<Δ wage & salaries past 3 yrs Stat, Legis	2/3 of each house and governor declares emergency
NV	1979	1+ΔGov Proposed Exp<(1+Δpop)(1+ΔInf) Stat, Legis	1994 Binding Law Passed
NJ	1976 Binding	ΔExp<ΔPI, Stat, Legis, Local Limit	expired in 1983
NM	1987	ΔGov Proposed Exp <Δ wages & salaries, Stat, Legis	
NC	1991	Less Than 7% personal Income	
OK	1985 Binding	ΔExp<12%, Const, Board of Equalization	
OR	1979	ΔExp<Δ PI over prior 2 years Stat, Legis Vote	
RI	1977	ΔGovernor's Prop Exp<6% Stat, Legis	Not Binding
SC	1980 Binding	ΔExp < Δ PI or, Exp< 9.5% PI Stat 80, Const 84, Legis	2/3 majority of legislature, every 5 years legislature can review limit.
TN	1978	ΔExp<Δ in PI, Const, Const Conv Ref	

Appendix A: List of Adoption Dates of Tax and Expenditure Limits (Cont.)

State	Year	Type of Limit on Growth	Need Supermajority or Ref for Waiver
TX	1978	ΔExp $<\Delta$ in PI, Const, Legis Ref	
UT	1979	ΔExp $<.85\ \Delta$PI, Stat, Legis Not Implemented	2/3 majority of legislature
WA	1979 Binding	ΔRev $<\Delta$ Avg PI over prior 3 years Stat, Cit Init	2/3 majority of legislature

Source: Information taken from "Significant Features of Fiscal Federalism" Volume 1: 1992 ACIR;"State Tax and Expenditure Limits: There is No Story", National Association of State Budget Officers 1988 and augmented with information from newspapers and other primary sources.

Chapter 2
Fiscal Institutions and Public Sector Labor Markets
Joint with Jim Poterba, MIT

Total compensation for state and local government workers in the United States rose ten percent faster than that for civilian workers between 1982 and 1993. These statistics have sparked a public policy debate on the role of public sector pay increases in contributing to the fiscal problems of state and local governments during this period, and more generally on compensation policy in the public sector.[29] Much of this debate has proceeded without regard to a voluminous literature in labor economics, surveyed by Ehrenberg and Schwarz (1986) and recently extended by Katz and Krueger (1991), on the pay premium associated with working in the public rather than the private sector. Ehrenberg and Smith (1994) summarize these studies as suggesting a public sector wage premium for women, and a small wage penalty for men. Katz and Krueger (1991) find that poorly-educated workers enjoyed a growing public sector wage premium during the 1980s, while better-educated workers faced a shrinking public sector premium.

While there has been substantial discussion of aggregate trends in relative pay in the public and private sectors, differences across jurisdictions in this pay relationship have received less attention. This paper contributes to this gap, focusing in particular on the effect of fiscal institutions on the public sector-private sector wage differential. The paper is

[29]Examples of recent policy discussions focusing on this issue include Cox and Brunelli (1992), who attribute fiscal stress to rising public sector pay, and Belman and Heyward (1992), who argue that wages in the public sector are insignificantly different from those in the private sector.

divided into five sections. Section one describes the variation across jurisdictions in the public sector wage premium. Section two summarizes variation in the fiscal environment, particularly the nature of tax and expenditure limits that affect state and local spending, across states. The third section presents evidence on the correlation between property tax limits, and state tax or expenditure limits, and the growth rate of public sector wages during the 1979-1991 period. We find that states with such limits exhibit slower growth in public sector wages, relative to private sector wages, than states without such limits. Section four discusses the potential endogeneity of fiscal institutions, and reports instrumental variable results that rely on aspects of the state legislative environment to instrument for fiscal institutions. A brief conclusion suggests directions for further work.

1. Spatial Heterogeneity in the Public Sector Wage Premium

There is substantial heterogeneity across states in the magnitude of the wage differential between observationally-comparable public sector and private sector employees. Three previous studies have explored the geographic differences in the public sector wage premium. Borjas (1986) uses data from the 1980 Census to estimate state-specific wage premia for several large states. He then attempts to explain these findings in a model of public demand for government services. Katz and Krueger (1991) plot public sector versus private sector wage levels across states, implicitly showing heterogeneity in the relative public sector wage, although they do not explore the source of these differences. Gyourko and Tracy (1991), whose analysis is most similar to that reported below, examine data on workers from a cross-section of SMSAs drawn from the 1980 Census of Population, and find that the level of public sector wages is lower in SMSAs that are affected by tax or expenditure limits. They also consider variables that affect the bargaining environment between public sector workers and local governments.

We estimate interstate differences in the public sector wage premium in a standard Current Population Survey-based wage equation.[30] Our wage equation relates the logarithm of an individual's hourly wage, $\ln(w_{it})$, to a set of individual characteristics (X_{it}) that can affect marginal productivity, an indicator variable ($SLGOV_{it}$) for working in the public sector, and state effects interacted with this indicator variable. The equation also includes state effects to control for interstate differences in wage patterns that affect both the private and public sectors.

The set of individual characteristics in our wage equation includes education, experience (age - education - 6), marital status, race, residence in an SMSA, as well as an indicator variable for part-time employment. We allow education to affect wages through a set of four categorical variables (EDUC) for number of years of schooling, corresponding to less than twelve years, twelve years, which typically corresponds to completing high school, thirteen to fifteen years, sixteen years, which typically corresponds to completing college, and more than sixteen years. The wage equation includes quartic powers of experience. We also include a set of control variables for ten broad occupational classifications, such as

[30]Moore and Newman (1991) summarize the previous literature that has used individual-level wage equations to evaluate the public sector wage premium. They also note that since wage equations estimated on individual data typically lack information on precise job characteristics, there may be omitted factors, such as the riskiness of some types of public sector jobs, that contribute to wage differentials.

managerial and technical, sales, or crafts, in some equations.[31] The resulting equation is:

$$\ln w_{it} = X_{it}\beta_t + \sum_{j=1}^{48} (\alpha_{jt} + SLGOV_{it}*\delta_{jt})*STATE_{ijt} + \epsilon_{it} \tag{1}$$

$STATE_{ijt}$ is an indicator variable for worker i, in year t, residing in state j. We focus on interstate differences in the conditional means of the public and private wage distributions. We have also estimated state effects using quantile regression methods, and the differences across states in the resulting fixed effects coefficients are very similar to those from the least squares equations.

We estimate (1) using data from the merged outgoing rotation groups in the CPS for the years 1979-1992.[32] We exclude self-employed individuals from our analysis, because it is difficult to measure their wage rates. We also exclude federal government employees, because they are neither private sector nor state and local government employees.[33] We estimate equation (1) separately for men and women.

State effects interacted with the public sector indicator variable substantially improve

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[31]The set of variables included in this wage equation is similar to that in Katz and Krueger (1991, 1992), although our approach is somewhat different. They estimate separate wage equations for workers in the public and private sectors, and then predict average wages in each sector for hypothetical workers with fixed characteristics. We estimate a single wage equation each year for all men, and all women, and impose the same coefficient vector β_t for the private and public sectors.

[32]Changes in the CPS in 1992 make it impossible to estimate the same wage equation on pre- and post-1992 data. We discuss this issue in more detail in the appendix.

[33]If we include federal employees, and allow a separate average wage premium for these workers, our results on the relative wages of state-local government and private sector employees are not affected. The average wage premium for federal workers, relative to private sector workers, is positive.

the explanatory power of the estimated wage equations. When we estimate a wage equation pooling the outgoing rotation groups in the 1990 and 1991 CPS data files, for example, adding state effects in this interactive way raises adjusted R^2 from .398 to .427 for men, and from .345 to .389 for women.[34] Moreover, the estimated coefficients reveal substantial differences in relative wages in different states. Figure 1 plots the distribution of estimated state-specific state and local wage premia for 1990/1991. The dark shaded bars show the estimated public sector wage premia for men, while the lighter shaded bars describe the distribution for women. The figure shows that for men, the public sector premium exceeds .10 in only one state, while it falls below -.15 in two states. Most states are concentrated between premia of -.10 and 0. For women, the distribution of state premia is right-shifted relative to that for men. In six states, the estimated premium is above .10, and only two states have negative estimated premia.[35]

State-specific public sector wage premia also display substantial persistence over time. Figure 2a shows the scatter-plot of the state-specific wage premium for men estimated from the 1979/80 CPS data, and the corresponding estimate of the state-specific premium from 1990/91. Figure 2b presents an analogous scatter-plot of the estimated wage premia for women. Both figures show strong positive correlation between the estimated premia at the beginning of the 1980s and the beginning of the 1990s. For men, the correlation of the

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[34]We pool consecutive years of the CPS data to increase our sample size for individual states.

[35]The estimates of state-specific wage premia are from equations that exclude occupation indicator variables. Including these variables affected the level, but not the pattern, of the state-specific wage premia.

forty-eight state effects is .63, while for women, the correlation is .62. Even though these correlations are positive, there is evidence of regression toward the national mean wage premium.

Figures 2a and 2b identify individual state observations by their two-letter abbreviations, so they also provide information on which states have high, and low, public sector wage premia. For men, California shows the highest premium in 1990/91, followed by New York, Florida, Arizona, and Connecticut. The states with the lowest wage premium for men are West Virginia, Utah, Kansas, Wyoming, and New Hampshire. There is only a weak correlation (ρ = .35) between the estimated premium for men and women.

Table 1 presents information on the estimated state and local government wage premia for the twelve largest states in 1990. These are the states for which the CPS data provide the most precisely estimated $\delta_{j,1979/80}$ and $\delta_{j,1990/91}$. The point estimates show substantial and statistically significant changes in relative wage premia for several states. In California, for example, the estimated premium for men increases 14.8% during the twelve year period, although there is relatively little change for women. In New York, the estimated premia for both men and women increase during the 1980s. In only one of these twelve states, North Carolina, do men experience a decline in the estimated public sector wage premium during this period. In contrast, the premium for women declines in five of the twelve states.

The absence of any association between the outlying state wage premia in various years is likely to reflect both measurement error in the estimated effects, and genuine shifts in relative wage premia. The standard deviation of the <u>change</u> in the state wage premium between 1979/80 and 1990/91 is .043 for men, and .042 for women. The average standard deviation of the estimated state-specific coefficients in the two years is .028 for men and .023 for women. Measurement error alone would therefore predict a standard deviation of the change of .040 for men, and .033 for women.[36] For larger states with greater samples in the CPS, however, the state effects are estimated much more precisely. The standard deviations of the New York and California effects for 1990/91, for example, are .012 and .013 respectively.

Before considering explanations of state variation in the public sector wage premium, we should note one difficulty with our focus on wages rather than total compensation in the two sectors. Public sector workers often receive more generous benefits packages than their private sector counterparts. Our analysis focuses on wages, and therefore does not precisely measure the total compensation received by workers in the two sectors. Time series evidence, however, suggests that relative compensation and relative wages in the two sectors move closely together. Data from the Employment Cost Index (ECI) indicate that in 1993, benefit costs averaged 43.8% of wage costs for public sector workers, and 40.3% for those in the private sector. Between 1981 and 1994, wages and salaries grew 93.1% in the public sector, and 74.0% in the private sector. Over the same period, total compensation costs grew 104.2% in the public sector, and 84.2% in the private sector. Thus, both wage and non-wage compensation increased faster for public sector than private sector workers. The difference in wage growth is similar to the difference in total

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[36]If the two estimates of the state effects are independent, then the variance of their difference is just twice the own variance.

compensation growth, which while not dispositive, suggests that focusing exclusively on the determinants of relative wage levels in the public and private sectors should capture the broad compensation trends in the two sectors.

2. State Fiscal Limits and Labor Organizing Provisions: Institutional Variation

One potential explanation for differences in the relative public and private sector wage across states relates to differences in the institutional environment in which public sector wages are determined. Fiscal institutions are one source of such differences; state labor laws relating to public sector unionization are another. A number of previous studies have investigated the link between collective bargaining institutions and wages, but, with the notable exception of Gyourko and Tracy (1991), there has been relatively little previous work on fiscal institutions and public sector wages.

There is a small but growing literature on the effects of fiscal institutions on the level of state and local government spending. Examples include Abrams and Dougan (1986), Ichniowski and Preston (1991), and Rueben (1995). Ehrenberg (1979) presents a theoretical discussion of how tax and expenditure limits could affect relative wages in the public and private sectors. He argues that such laws could reduce the demand for public sector labor, he observes that they may also reduce the supply of labor to this sector, as potential employees conclude that public sector jobs are prospectively less attractive. The net effect of these laws is therefore ambiguous a priori.

We focus on two types of fiscal institutions: overall limits on state or state and local taxes or expenditures, and limits on local property tax collections. Rueben (1995) presents more detail on the nature of these limits, and explores how these limits affect total state and local government spending. Our analysis relies on her classification of both types of limits. Further information on the variation in fiscal institutions across states may be found in the Advisory Council on Intergovernmental Relations (1987) and the National Association of State Budget Officers (1991) summaries of these rules.

We define a tax and expenditure limit variable as the fraction of the 1979-1991 period during which the state exhibited such a fiscal limit. We classify states as having limits if the ACIR (1987) indicates that they have a limit that actually constrains spending decisions. States with purely "advisory" limits are excluded from this category. Most of the states with such limits enacted them in the 1979-1981 period. Figure 3a shows the pattern of such limitation laws during our sample period. Seventeen states are classified as having limits at some point in the sample period by this definition; thirteen states have such limits for the entire 1979-1991 period. In our analysis below, we use an indicator variable equal to unity for states with tax limits in force for the whole 1979-1991 period, zero for states with no tax limits, and we construct values between 0 and 1 for those states that adopted tax limits during this period.

With respect to property tax limits, we consider two sets of criteria for states into the limit category. The first, and broader, definition, sets an indicator variable for property tax limits equal to unity if the state has any type of limit on property tax collections. Many such limits date from the 1930s, and are unlikely to bind during our sample period. The second definition sets an indicator variable equal to unity only for those states that enacted property tax limits after 1970. These limits were part of the "taxpayer revolt" of the late 1970s, and are much more likely to have placed binding limits on local property tax collections. By the first definition, thirty-eight continental states have property tax limits. The second and more limited definition yields twenty-four states with property tax limits. Figure 3b shows the pattern of states with property tax limits.

In addition to fiscal institutions, we also consider labor market institutions that affect the bargaining power of public sector employees. We use two variables to measure these institutions. The first is an indicator variable for the presence of public sector right-to-work laws, and the second is Valletta and Freeman's (1988) "summary bargaining environment statistic." This variable, which is based on rules in place in 1984, combines information on many aspects of state public sector labor legislation. Higher values of this variable correspond to more pro-labor legislation.[37]

3. Empirical Results on Institutional Variation and Wage Patterns

Our analysis of how various institutions affects wages in the public sector concentrates on the change in relative wages between 1979 and 1991, rather than the level of wages in either year. We focus on changes because the level is likely to be affected by current as well as historical factors that may be difficult to measure. For example, the wage differential may be different in a state that recently adopted right-to-work laws for the public

[37]Freeman and Valetta (1988) and Zax and Ichniowski (1990) find that pro-labor legislation increases the probability that public sector workers will be represented by a union. Ichniowski and Zax (1991) study the effects of right-to-work laws on union representation. Ehrenberg and Schwarz (1986) survey the voluminous literature on the relationship between union representation and public sector wages, which generally suggests that such representation raises wages.

sector than in a comparable state that has had similar legislation for several decades.

We assume that the state and local government wage premium in state j in 1991 can be written as a function of the corresponding state premium in 1979, and a variety of institutional factors. We therefore estimate equation (1) with the following modification to the specification of the coefficients on the state and local sector dummy:

$$\delta_{j,1991} = \delta_{j,1979} + \sum_{k=1}^{N} \theta_k * INST_{kj} \ . \tag{2}$$

Table 2 presents the results of estimating equation (2). We report only the coefficients on how the variables such as fiscal limits and bargaining environment affect the change in the state-specific public sector wage premium between 1979 and 1991. We present separate estimates of wage equations for men and women. This allows us to estimate separate state effects for the level of wages, and the level of the state and local wage premium, for men and women in different states. We report several different specifications for both men and women.

The specification in the first column of Table 2 includes only the fiscal limitation variables in explaining the cross-state patterns of public sector wage premia. For both men and women, the results suggest that fiscal limits reduce the relative growth of public sector wages. A state with such a tax or expenditure limit in effect for the full twelve years of our sample period is predicted to experience a 4% decline in relative public sector vs. private sector wages for men, and a 1.3% (statistically indistinguishable from zero) decline for women. The effects of property tax limits are even more pronounced, with a -3.9% effect on men's wages and -4.5% for women. Both of these effects are statistically significant at standard confidence levels. The larger estimated effect of property tax limits on women's wages may reflect the greater presence of women in teaching, which is locally-financed.

The specification in the second column of Table 2 distinguishes recently enacted property tax limits from all limits; the results do not suggest substantial differences between these groups of states. The third specification includes the average unemployment rate in each state during our sample period, and finds that higher unemployment rates lower the relative wage of public sector workers. This could reflect a composition effect, with lower paid private sector workers losing jobs in economic downturns while comparable public sector workers are not laid off, or it could reflect economic pressures on the public sector bargaining process.

The specifications in the last two rows of Table 2 include the variables for collective bargaining institutions as well as the indicators for fiscal institutions. These labor market institutions have statistically significant effects on the relative growth of public and private sector wages. States with more generous legislative environments for public sector collective bargaining experience more rapid wage growth, while relative public sector wages grew more slowly in states with right to work laws. As for the case of tax limitation laws, the effects are substantively large: a right to work law is estimated to reduce public sector relative wage growth by as much as 7.5% for men, and 6.4% for women, during an eleven-year period.

Our analysis has focused on the <u>change</u> in the public sector wage premium between 1979 and 1991, but we can also ask whether the <u>level</u> of the wage premium is affected by either fiscal or bargaining institutions. Table 3 presents results relating the level of the wage premium in 1979/80 to the variables we considered in Table 2. We find a positive association between the level of the wage premium and state-level tax or expenditure limits for both men and women, but no relationship between the wage premium and the presence of property tax limits. We partly interpret this as evidence on the determinants of passage of these limits: voters in states with higher public sector wages may have found it more attractive to enact spending limits to reign in public sector labor costs.

The results in Table 3 do not suggest any relationship between right to work laws and the level of public sector wages in 1979, although the substantial negative effect on the 1979-1991 change implies that cross-sectional regressions estimated for the 1991 sample <u>would</u> show significant negative level effects. Our indicator variable for the generosity of the public sector bargaining environment suggests that public sector wages are higher in 1979 in states with a more favorable environment, and that they grew more rapidly during the ensuing decade.

A central question of interpretation with both the level and change results is whether fiscal and labor market institutions are proxying for differences across states in the attitude of voters toward the public sector in general and public sector workers in particular. The results from the 1979 level regressions do not support such explanations for the effects of fiscal limits: states with such limits have higher, not lower, public sector wages in 1979, while the "heterogeneous public sector tolerance" model would suggest that lower wages would be expected in light of the results on 1979-1991 changes. Only one set of results, those for the collective bargaining index variable, support the hypothesis that states with more generous views toward public sector workers enact such legislation and therefore grant higher wage increases during the 1979-1991 period.

4. Addressing the Endogeneity of Fiscal Institutions

Studying the levels as well as the changes of public sector relative wages can provide a check on some endogeneity explanations for the passage of state and local tax limits. An alternative strategy for exploring this issue, however, involves searching for instrumental variables that may be correlated with the passage of tax and expenditure limits, but which will not directly influence relative public sector wages. This is the strategy that Rueben (1995) employs in her analysis of how state tax and expenditure limits affect the growth rate of state spending.

We consider two institutional variables that can affect the likelihood of enacting a tax or expenditure limit: whether the state constitution permits state-wide referenda to enact legislation (so-called "direct legislation" states), and whether voters can recall elected officials. It is sometimes argued that grass-roots 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 should affect the changes 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 tax or expenditure limits, conditional on a level of voter support for such measures.

The results in Table 4 are instrumental variables estimates of linear regression models like those in Table 2, in which characteristics of the state fiscal environment are enacted with state indicator variables to explain the change from 1979/80 to 1990/91 in the relative public sector wage premium. We now estimate these models using 2SLS, with two instrumental variables (direct referenda and recall) and two endogenous variables, the state tax and expenditure limit variable and the property tax limit variable.

The coefficient estimates in Table 4 follow the same broad pattern as those in Table 3, but the absolute value of the coefficients on the tax and expenditure limit variables is typically greater than that for the variables in Table 2. For property tax limits, for example, the 2SLS estimate of the effect on wages for men is -6.3%, and for women the estimated effect is -14.2%. This is a substantial change relative to -4.2% and -5.6% in the ordinary least squares estimates. The impact of state tax and expenditure limits is also increased when the model is estimated by instrumental variables.

These findings support the earlier results, and provide at least some evidence against the possibility that the ordinary least squares results are simply the result of endogenous fiscal institutions.

5. Conclusions

This paper has presented suggestive evidence on the link between tax and expenditure limits, and the public sector bargaining environment, and the evolution of public sector wages. The results imply that the rate of growth of public sector wages, by comparison to private sector wages, is slower in states with more restrictive fiscal environments, and in states with less favorable environments for public sector unions. These results raise an obvious question about the extrapolation of these results over long time periods. If the wages in one sector grow more slowly than wages in another sector for an extended period, then the slow-growth sector will contract and eventually vanish. Our results may therefore be describing the adjustment to changes in the fiscal environment during the 1970s and 1980s, rather than permanent effects of fiscal variables.

We have not considered the potential selection biases that plague studies of inter-sectoral wage differences, whether between the public and private sectors or the union and non-union sectors. This is because we have not found variables that are likely to affect the probability of public sector employment, but not public sector wages, and that could consequently be used to identify selection models. Further evidence on the potential importance of such effects, and in particular on whether such effects could be more important in some states than others, is an important topic for future investigation.

A second issue that warrants investigation is the effect of fiscal and labor market institutions on public sector employment. While fluctuations in the relative wage paid to public and private sector workers can affect total spending by state and local governments, changes in employment could have even larger effects. There are many dimensions on which employment can be affected by fiscal and labor market institutions: downsizing existing departments, increased reliance on volunteers, privatization of government functions, and elimination of services are all examples of adjustments that could affect the size of the public sector workforce. Poterba and Rueben (1995) present some preliminary evidence suggesting that property tax limits and labor market institutions can interact in important ways to affect the growth of public sector employment, but this topic deserves further analysis.

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Table 1: Estimates of State and Local Employee Wage Premia for the Twelve Largest States, 1979/80 and 1990/91

State	1990 Population Millions	Men 1979/80	Men 1990/91	Women 1979/80	Women 1990/91
California	29,760	-.052	.069	.097	.077
		(.012)	(.013)	(.012)	(.014)
New York	17,990	-.051	.048	.022	.073
		(.012)	(.012)	(.013)	(.013)
Texas	16,987	-.123	-.090	-.024	-.066
		(.017)	(.019)	(.015)	(.016)
Florida	12,938	-.041	.046	.060	.095
		(.021)	(.019)	(.018)	(.016)
Pennsylvania	11,882	-.127	-.049	.041	.065
		(.017)	(.020)	(.017)	(.020)
Illinois	11,431	-.094	-.011	.075	.034
		(.018)	(.019)	(.018)	(.018)
Ohio	10,847	-.172	-.063	.008	.077
		(.017)	(.018)	(.017)	(.018)
Michigan	9,295	-.112	-.037	.074	.133
		(.018)	(.018)	(.019)	(.019)
New Jersey	7,730	-.076	.006	.068	.053
		(.019)	(.018)	(.020)	(.017)
North Carolina	6,629	-.045	-.048	.012	.038
		(.023)	(.018)	(.017)	(.016)
Georgia	6,478	-.149	-.052	.001	-.054
		(.024)	(.036)	(.022)	(.030)
Massachusetts	6,016	-.127	-.044	.045	.007
		(.019)	(.019)	(.020)	(.019)

Notes: Results are from regressions run on data from the Outgoing Rotation Groups of the CPS for full-time employees. Regressions also controlled for educational attainment, experience, marital status, race, SMSA status and state of residence. Standard errors are reported in parentheses.

Table 2: Fiscal & Labor Market Institutions and Relative Changes in Public Sector Wages, 1979/80 - 1990/91

Variable	Men				
State Tax & Expenditure Limits	-.026 (.022)				
Binding State Limits		-.006 (.022)	-.000 (.020)	-.020 (.019)	-.015 (.017)
Recent Local Property Tax Limits	-.042 (.019)	-.036 (.019)	-.030 (.019)	-.042 (.016)	-.037 (.015)
Average Unemployment Rate, 1980-89			-.014 (.006)		-.017 (.005)
Collective Bargaining Index				.059 (.030)	.034 (.030)
Right to Work Law				-.052 (.016)	-.072 (.018)
	Women				
State Tax & Expenditure Limits	.007 (.018)				
Binding State		.007 (.017)	.011 (.016)	-.007 (.016)	-.003 (.015)
Recent Local Property Tax Limits	-.056 (.016)	-.038 (.007)	-.051 (.017)	-.063 (.013)	-.059 (.013)
Average Unemployment Rate, 1980-89			-.012 (.006)		-.015 (.005)
Collective Bargaining Index				.049 (.033)	.027 (.032)
Right to Work Law				-.047 (.019)	-.064 (.019)

Notes: Results are from regressions run on data from the Outgoing Rotation Groups of the CPS for full-time workers. Regressions also controlled for educational attainment, experience, marital status, race, SMSA status and state of residence. In addition, these demographic characteristics (excluding state of residence) were allowed to differ in the latter period. A set of state specific indicator variables are also included for state and local employment. Reported coefficients are for interactions of variables with an indicator variable for employment in the state and local sector in 1990/91. State tax and expenditure limits were only allowed to affect state workers and local property tax limits were allowed to affect local workers. Standard errors are reported in parentheses and are corrected for within-state correlation. The adjusted R-squared are .400 and .348 for men and women respectively.

Table 3: Fiscal & Labor Market Institutions Effects on the Level of Public Sector Relative Wages, 1979/80

Variable	Men				
State Tax & Expenditure Limits	.056 (.023)				
Binding State Limits		.042 (.026)	.050 (.025)	.039 (.025)	.046 (.024)
Recent Local Property Tax Limits	-.017 (.022)	-.023 (.022)	-.017 (.022)	-.022 (.020)	-.018 (.020)
Average Unemployment Rate, 1980-89			-.010 (.005)		-.010 (.005)
Collective Bargaining Index				.046 (.027)	.031 (.025)
Right to Work Law				.001 (.019)	-.009 (.020)
	Women				
State Tax & Expenditure Limits	.056 (.017)				
Binding State		.056 (.018)	.054 (.019)	.054 (.017)	.051 (.018)
Recent Local Property Tax Limits	-.011 (.015)	-.015 (.015)	-.017 (.015)	-.015 (.014)	-.016 (.014)
Average Unemployment Rate, 1980-89			.003 (.003)		.004 (.003)
Collective Bargaining Index				.025 (.017)	.031 (.018)
Right to Work Law				-.002 (.014)	.002 (.014)

Notes: Results are from regressions run on data from the Outgoing Rotation Groups of the CPS for full-time workers. Regressions also controlled for educational attainment, experience, marital status, race, SMSA status and state of residence. In addition, these demographic characteristics (excluding state of residence) were allowed to differ in the latter period. A set of state specific indicator variables are also included for state and local employment. Reported coefficients are for interactions of variables with an indicator variable for employment in the state and local sector in 1990/91. State tax and expenditure limits were only allowed to affect state workers and local property tax limits were allowed to affect local workers. Standard errors are reported in parentheses and are corrected for within-state correlation. The adjusted R-squared are .400 and .348 for men and women respectively.

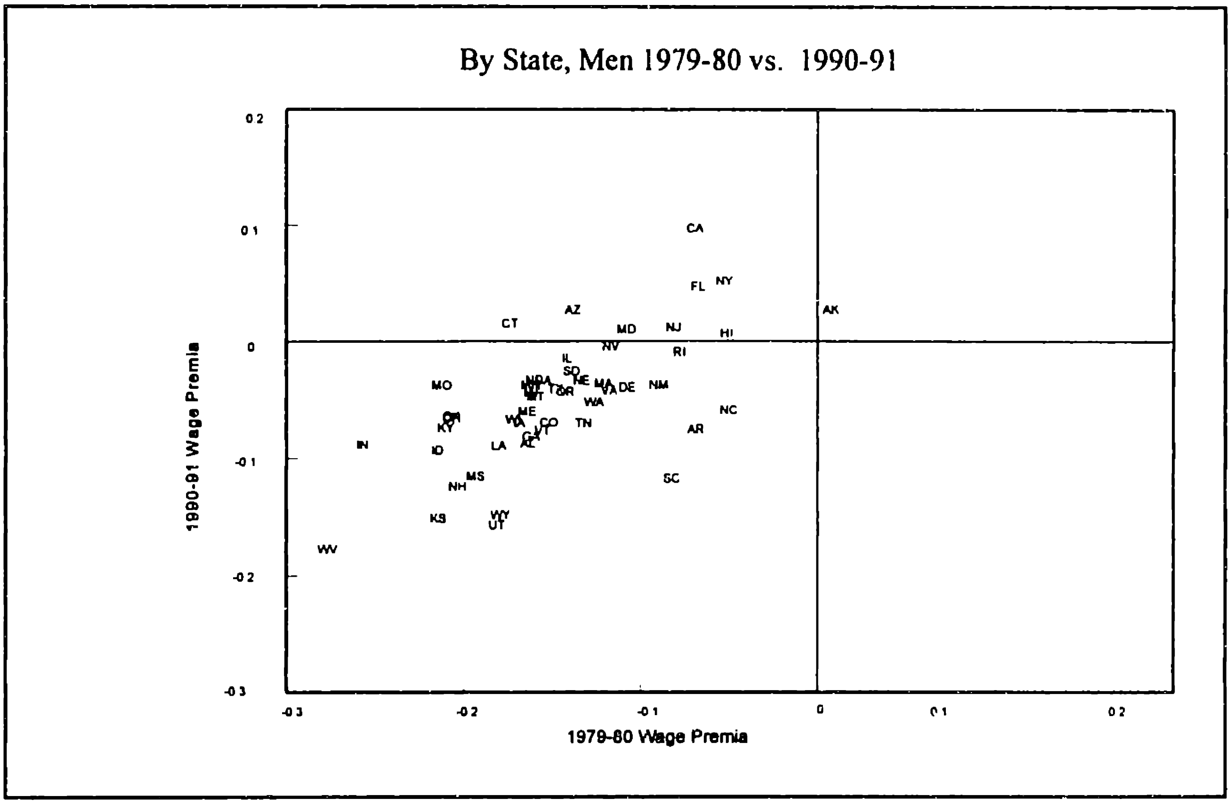
Table 4: Fiscal & Labor Market Institutions and Relative Changes in Public Sector Wages Instrumenting for Tax Limit Passage, 1979/80 - 1990/91

Variable					
	Men				
State Tax & Expenditure Limits	-.043 (.026)				
Binding State Limits		-.038 (.031)	-.019 (.032)	-.069 (.032)	-.048 (.033)
Recent Local Property Tax Limits	-.063 (.021)	-.057 (.021)	-.043 (.022)	-.083 (.023)	-.067 (.023)
Average Unemployment Rate, 1980-89			-.014 (.003)		-.016 (.005)
Collective Bargaining Index				.055 (.016)	.032 (.017)
Right to Work Law				-.063 (.016)	-.079 (.013)
	Women				
State Tax & Expenditure Limits	-.073 (.024)				
Binding State		-.073 (.029)	-.061 (.016)	-.125 (.032)	-.109 (.032)
Recent Local Property Tax Limits	-.142 (.020)	-.135 (.020)	-.127 (.017)	-.174 (.022)	-.163 (.023)
Average Unemployment Rate, 1980-89			-.008 (.003)		-.011 (.003)
Collective Bargaining Index				.034 (.014)	.018 (.014)
Right to Work Law				-.080 (.011)	-.090 (.012)

Notes: Results are from regressions run on data from the Outgoing Rotation Groups of the CPS for full-time workers. Regressions also controlled for educational attainment, experience, marital status, race, SMSA status and state of residence. In addition, these demographic characteristics (excluding state of residence) were allowed to differ in the latter period. A set of state specific indicator variables are also included for state and local employment. Reported coefficients are for interactions of variables with an indicator variable for employment in the state and local sector in 1990/91. State tax and expenditure limits were only allowed to affect state workers and local property tax limits were allowed to affect local workers. Standard errors are reported in parentheses.

Figure 1: Distribution of State and Local Worker Wage Premia, Across States 1991

Figure 2: Wage Premia for State and Local Sector Workers



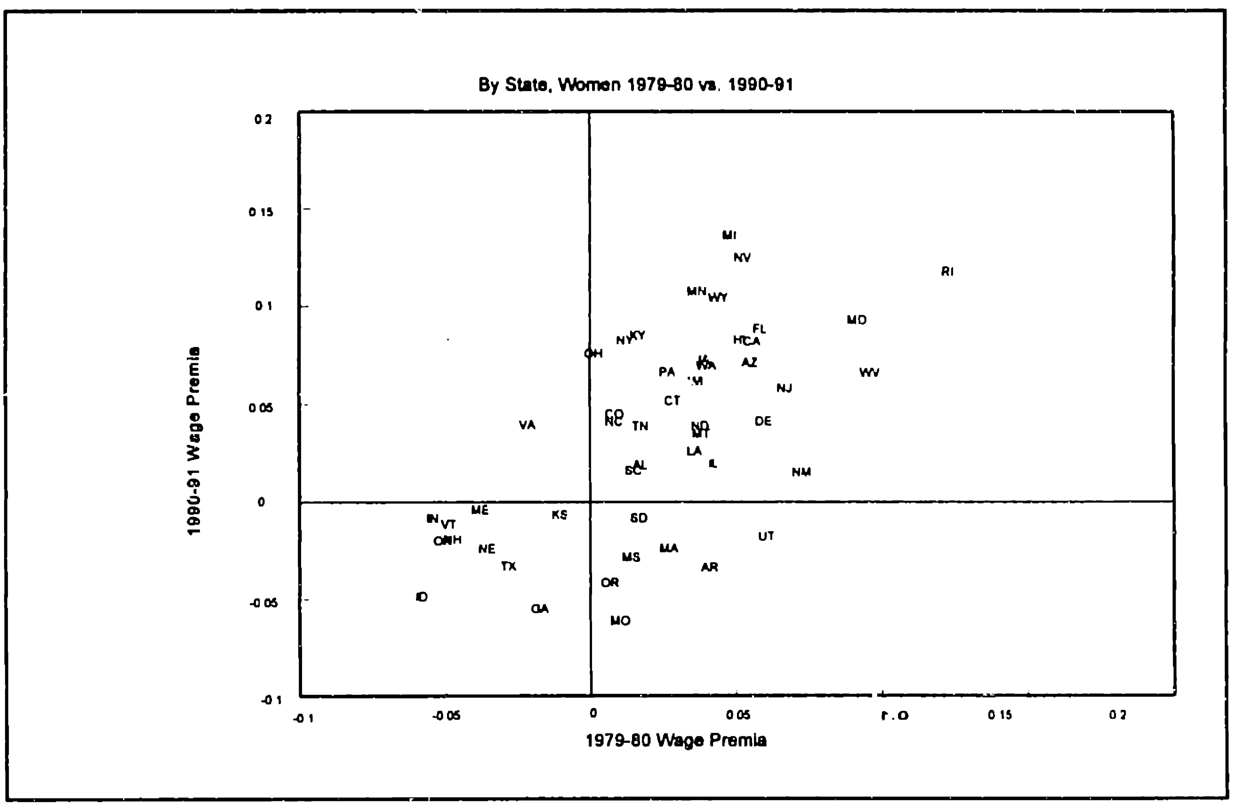
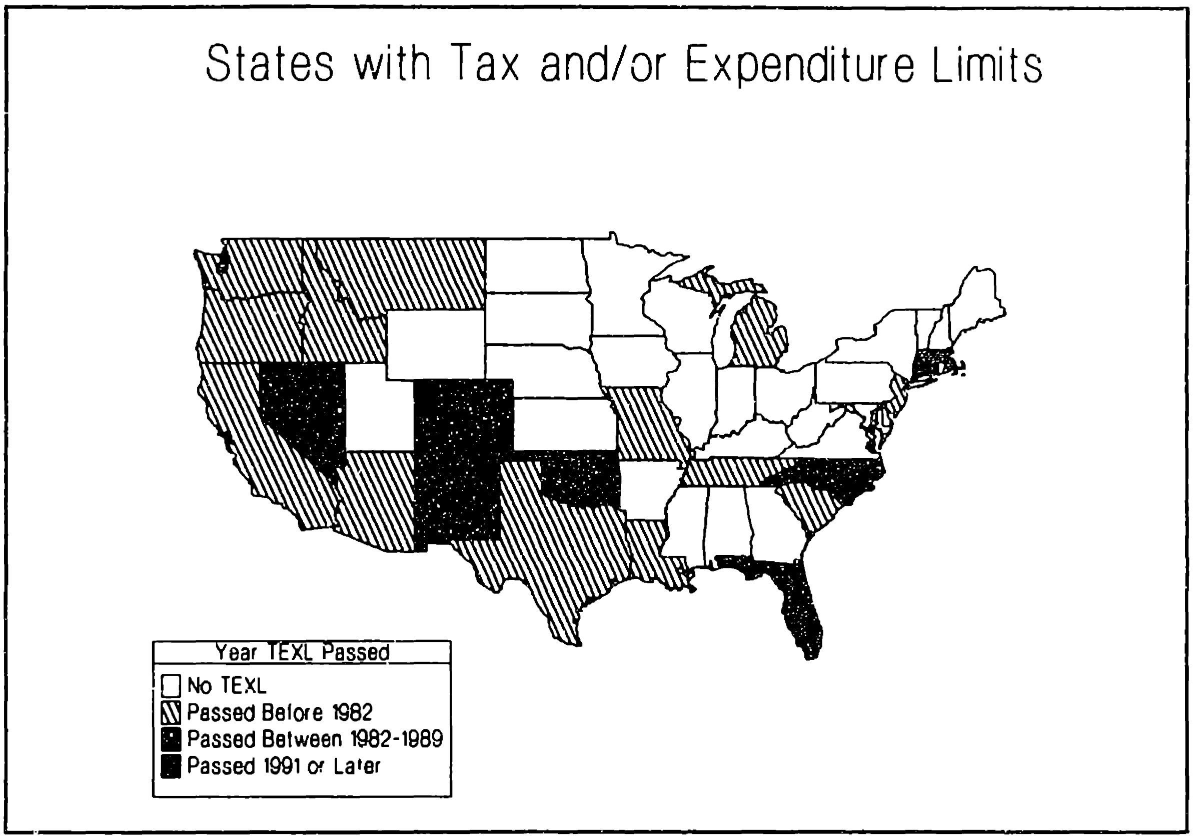
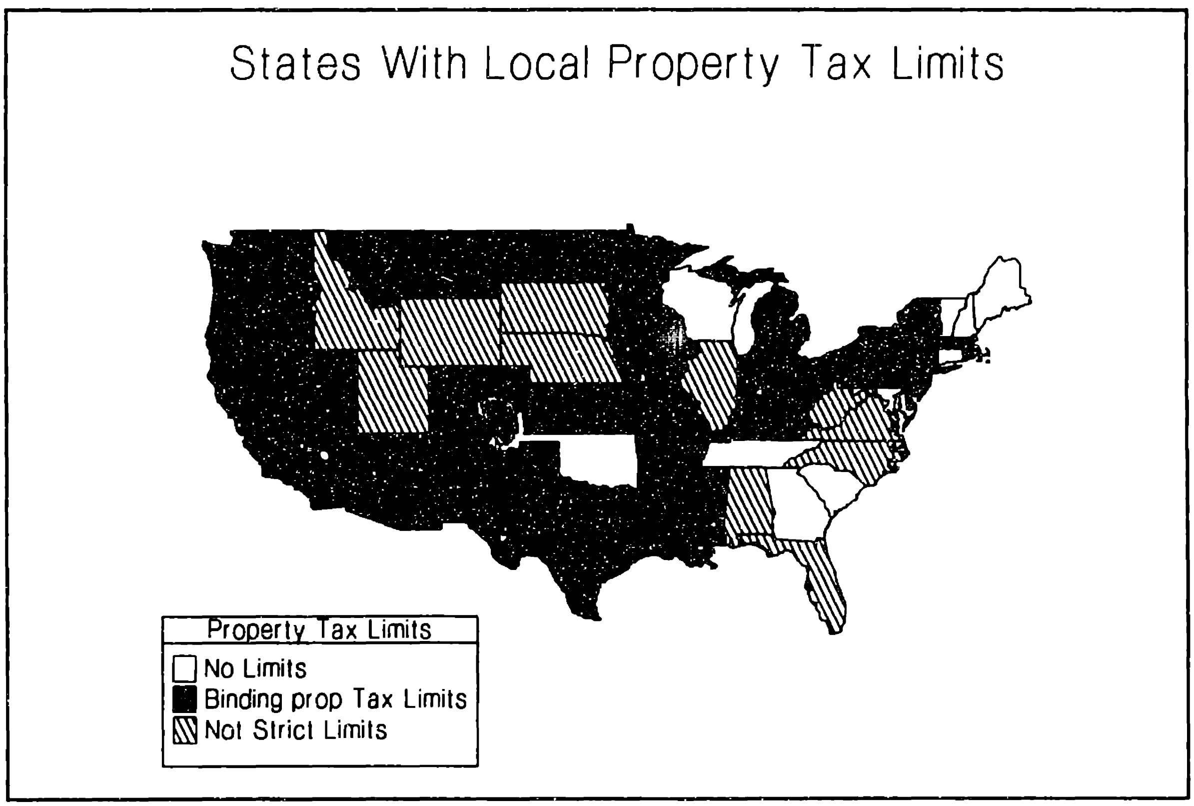


Figure 3


States with Tax and/or Expenditure Limits
Year TEXL Passed
No TEXL
Passed Before 1982
Passed Between 1982-1989
Passed 1991 or Later


States With Local Property Tax Limits
Property Tax Limits
No Limits
Binding prop Tax Limits
Not Strict Limits

Chapter 3
How Tax Limits Affect Teacher Quality
joint with David Figlio, University of Oregon

In the last few years there has been a renewed interest on the part of voters for property tax relief or tax limitation laws in general. The number of tax limitation laws on the ballot across the country has increased to levels not seen since the "tax revolt" of the late 1970s and early 1980s. In 1996, for instance, there were 9 initiatives on the ballots of 8 states, of which 7 passed.[37] While this new era of tax revolt is occurring it is important to reflect on what, if any, effects these new restrictions have on the functioning of state and local government and more specifically on what the effect of these limits might have on locally provided goods, such as public education.

To date, several recent papers have investigated the effects of these recent limits. However, the results from these papers have been mixed so far. Dye and McGuire (forthcoming) find that after the Illinois legislature passed a property revenue cap on localities in the suburban Chicago area in 1991, the level of growth in property taxes fell significantly. However, they also find that while overall school district revenues fell, the cuts in spending occurred in non-instructional areas, suggesting that the incidence of the

[37]California, Oklahoma and Oregon passed or strengthened existing property tax laws, with Oklahoma passing a limit on the growth rate of assessments and a limit on property tax rates. Idaho and Nebraska also had property tax limit initiatives on the ballot in 1996, but these ballot measures were defeated. The remaining three ballot measures required a 2/3 majority of voters to pass new state taxes or fees (Florida), or required a 2/3 majority of legislators or a majority of voters to pass new state taxes (Nevada and South Dakota).

Illinois tax limit has not been borne by instruction.[38] In a study on the effect of Oregon's Measure 5, a property tax rate limitation measure passed in 1990, Figlio (1997a) finds that the tax cap significantly decreased teacher-student ratios.[39] Unlike Dye and McGuire, Figlio finds that the incidence of Oregon's tax limit has been borne more by instruction than by administration. This difference in results across states could reflect differences in the stringency of the laws, differences in state fiscal and economic environments or perhaps differences in short run dynamics. Thus it may be more informative to evaluate more long run evidence.

During the late 1970s and early 1980s, numerous states around the country imposed limits on the level or growth rate of local public school revenues or expenditures. Investigating the long-term effects of this "local property tax revolt" should provide insight into the likely long-run effects of the current wave of tax limits. In recent years, several authors have attempted to gauge whether these policies have affected per pupil spending and other educational quality measures. Various authors, including Figlio (1997b), have found that tax revolt-era limits have led to substantially lower levels of measured school services, such as higher student-teacher ratios and lower per pupil spending. Figlio (1997b) and Downes and Figlio (1997) also document a large and significant link between tax limits and student test performance.

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[38]In a follow-up study, Downes, Dye and McGuire (1997) find that this "collar county" tax limit had little effect on student achievement, presenting some short term evidence of a lack of effect in at least one public sector output.

[39]While not reported in Figlio's paper, Measure 5 has also resulted in reduced per pupil school expenditures, relative to what would have been expected in the absence of the tax cap.

Despite this evidence that limits lead to decreased student performance *and* lower measured school inputs, the mechanism through which tax limitation rules and the resulting changes in school spending is translated into student outcomes is not well understood. Hanushek (1996) and Betts (1995) summarize vast literatures which suggest that measured school inputs have little or no relationship to student achievement or subsequent labor market outcomes. Given these findings, so well-accepted in the economics literature that they have virtually become "stylized facts," it may be surprising that the presence of tax limits could actually substantially affect student achievement.

We suggest that tax limits could affect student achievement not directly through reduced spending, but rather indirectly through the quality of teachers in affected schools. Unlike the lack of a relationship between spending (or class sizes) and student outcomes, there is considerable evidence that specific teacher attributes, such as the selectivity of the teacher's undergraduate institution (used to proxy for underlying teacher academic ability) and the teacher's choice of major, strongly affect student achievement (see, e.g., Ehrenberg and Brewer, 1994). If tax limits systematically affect teacher quality attributes, this would be a much more compelling explanation for why tax limits appear to reduce student achievement.

We are interested in gauging whether tax limits have deterred higher-quality potential teachers from entering the teaching force in the states that have passed local limits. We compare teacher attributes in different states over time before and after these limitations were passed. Tax limits could deter college students from becoming education majors if they view a tax limit as a threat to finding a job, future pay increases or working conditions. College students may therefore choose a different, non-teaching career route than they would have ordinarily chosen in the absence of the tax limit. If individuals with stronger abilities are deterred at a higher rate than those with lower ability, then the long run effect of passing a tax limit law would be a decrease in the quality of the teaching force within a state.[40]

Studying the effect of tax limits on the quality of teachers is made more complicated by the fact that these limits exert a direct constraint on the demand for teachers by school districts and an indirect effect on the supply of individuals who choose to become teachers. It may be the case that the qualifications of hired teachers increases due to the lessened demand for incoming teachers as long as school districts can measure applicant ability and there is no change in applicant pool after the passage of a tax limit. While we are interested in the quality of teaching overall within school districts we would also like to disentangle the effect on the demand for teachers from possible supply effects of becoming a teacher. We do this by focussing on the decision of new entrants into the labor market at two points in the decision process. We examine the ability level of people who major in education and then examine the qualifications of new teachers.

This paper is divided into four sections. In section one we describe more formally the decision process behind becoming an education major and the relationship between the passage of tax limitation laws and ability level. Section two presents empirical evidence on

[40]Similarly, if tax limits lead to a decrease in the relative wage of public school teachers as compared to teacher wages in other states this quality drain could be accelerated. While we do not investigate the migration patterns of existing teachers in this version, we hope to do so in subsequent versions of this paper.

the change in average ability level of education majors and non-majors before and after the tax revolt occurred in states that did and did not pass limits. Section three then examines how the attributes of new teachers has changed after the tax revolt, focussing on the probability of new teachers to be education majors and on the selectivity of the colleges attended by these new teachers. Section four concludes and describes further directions for this research.

1. Conceptual Framework

We assume that there is a direct demand effect of tax limits on the budget of school districts. If the price for teachers is set by contract, that is $w_{new} = w_{contract}$, then a binding tax limitation law leads to a decrease in the number of available positions. If there is *no* change in who decides to qualify to become a teacher and school districts can ascertain ability, then this would lead to an increase in the average ability of teachers in tax limit states.[41]

However, there is no reason to believe that individuals will not react to the information about working conditions which the passage of a tax limit gives them about both the probability of finding a teaching job and the attractiveness of teaching jobs. Indeed, individuals could even possibly overreact and believe that the passage of a tax limit has a *greater effect* on job openings and wages than it actually does. Evidence has borne out that tax limitation laws have had a negative effect on both the relative wages of teachers in tax

[41]If school districts can adjust wages and can measure ability, then a budget cut caused by a tax limit can directly decrease ability if school districts respond to budget cuts by decreasing wages and looking for a lower qualified workforce.

limit states (Poterba and Rueben, 1997; Figlio, 1997b), and the number of teaching positions available (Figlio, 1997b). We wish to more fully examine this supply effect.

Formally, we are interested in the effects of tax limits on the average ability of potential new teachers. We will abstract from the decision to become a teacher and assume that an individual must be an education major within a given state in order to qualify to become a teacher[42]. We model the decision of individual j to become an education major as follows. Individuals have ability a which ranges from $\underline{a}$ to $\underline{\underline{a}}$, with $\underline{a} \leq a_j \leq \underline{\underline{a}}$, and have taste for becoming a teacher b which ranges from $\underline{b}$ to $\underline{\underline{b}}$, with $\underline{b} \leq b_j \leq \underline{\underline{b}}$. Individual j becomes an education major if

$$\Omega = E|V(ed\text{-}major)| \ - \ E|V(non\text{-}ed\ \ major)| \ >0$$

the expected utility from becoming an education major is greater than the expected utility of majoring in some other field. We will also assume that E| V(ed-major) | = U(being a teacher) $*P_k$(teacher) , where U(.) represents the expected utility of becoming a teacher and P_k(teacher) is the (individual-specific, even though subscripts are dropped) probability of teaching jobs being available in state k and that V(.) is increasing in U and P.

We also define U(being a teacher) = $f(s_k(t_k),e_k(t_k),b_j)$, where t_k is an indicator for whether state k adopts a local tax limitation law, s_k reflects the salary of being a teacher in

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[42]This assumes potential teachers cannot attend college out of state. To gauge how restrictive this assumption is we measured migration patterns for new college graduates (those under 30) in the 1980 five percent sample of the U.S. Census. We find 74 percent of college graduates under the age of 30 are in the same state in 1980 as they were in 1975. This percentage is even greater for teachers under the age of 30, who were in the same state in both years 82 percent of the time.

state k and is a function of t_k and the state economy, e_k reflects the teaching environment in state k which is also a function of t_k, and includes factors like class size and teaching responsibilities, and again b_j is the taste for teaching of individual j. We assume that f(.) is increasing in salary, teaching environment, and the individual's taste for teaching, but that both salary and teaching environment decrease when a tax limit is imposed.[43]

Finally, we assume that the probability of a job being available is a function of individual ability (a_j) and q_k , the number of open teaching positions in state k, which is itself a function of t_k and state demographics, like number of school age children and that this probability is increasing in teacher ability but decreasing in t_k. Finally, we take a simplified approach to representing the returns to becoming a non-education major. The expected utility of majoring in some other subject in college is given as $E|V(\text{non-major})| = Z(a_j,b_j) - c(a_j)$, where Z is the return to being a non-education major and c is the cost of being a non-education major. We assume that Z is an increasing function of ability, but is not affected by the imposition of a tax limit in the state and that the cost of being a non-education major is decreasing in ability.

We would like to calculate what the effect of a change in tax limit status will have on the average ability of education majors in state k. Reiterating our expression from above,

[43]Figlio (1997b) and Poterba and Rueben (1995) find that relative teacher salaries decrease when states impose tax limits. Figlio's (1997b) findings of increased student-teacher ratios also provide suggestive evidence that teaching environment decreased with tax limits as well.

we suggest that an individual j in state k selects into education if:

$$\Omega = E|V(ed\text{-}major)| \; - \; E|V(non\text{-}edmajor)| \; > 0$$

$$\equiv \; U(s(t),e(t),b)p(a,q(t)) \; - \; [z(a,b) \; - \; c(a)] > 0$$

where the person who is just indifferent between becoming an education major or a non-education major has $\Omega(a)=0$. Applying the implicit function theorem, it can be shown that $da/dt<0$ if

$$U \cdot P'(a) < (Z'(a) - c'(a))\,.$$

While we would expect this to be the case, especially given that Ballou (1996) provides evidence suggesting that $P'(a)$ may even be zero (or negative!) in public schools, the fact that we can not sign this effect with certainty makes this an empirical question. In the next section we will estimate the empirical magnitude of this effect.

2. Tax Limits and the Ability Levels of Education Majors

We are interested in gauging whether the passage of tax and expenditure limits has systematically affected the quality of individuals entering the teaching force. To do so, we must gather data on the relative quality of new teachers across states, before and after the passage of tax and expenditure limits. The approach we take is as follows: we use two nationally representative longitudinal data sets, the National Longitudinal Survey of the High School Class of 1972 (NLS-72) and the High School and Beyond (HSB) survey (sophomore cohort) to get, in essence, two cohorts of potential teachers, one that graduated from high school and selected college majors well before the tax revolt of the late 1970s, and the other that graduated high school after the tax revolt had passed. In each of these data sets, all high school seniors took a series of academic examinations in mathematics and verbal ability. Since we know the discipline into which each college-attending sample member selected, we can determine whether there are systematic differences in academic ability between those who select into education and those who do not in each cohort. We can then investigate whether the passage of tax limits has systematically affected this difference, and in which direction.

To address this question, we must have a measure of academic ability that can be compared across time. Fortunately, both NLS-72 and HSB tested every student in each respective cohort in mathematics and verbal ability. The two rounds of tests are measured in different units, but can be adjusted so that they are directly comparable. We perform this adjustment as follows: We assume that any given score on the ACT test is time-invariant (that is, a "23" on the ACT in 1972 means the same thing as a "23" on the ACT in 1982). Since a sizeable subset of each cohort took the ACT exam, we can use the ACT as a benchmark to determine a score matching between the NLS-72 and HSB. For the students in each cohort who took the ACT, we regress the student's ACT score on the relevant test's composite (verbal plus math) ability score.[44] We use these regression results to construct an "ACT-equivalent" score for every student in both cohorts. Using this algorithm, we convert each NLS-72 student's ability test score into HSB units.

[44]Details on these regressions are available upon request.

Almost without exception, teachers in the United States have at least a bachelor's degree. When we compare ability levels of people selecting into education to others, we must choose a comparison group with a comparable level of education. Therefore, we limit our sample to individuals who have graduated from college by the last round of each sample (1986 for the NLS-72 cohort; 1992 for the HSB cohort). We have also experimented with restricting this "completion by" date for the NLS-72 cohort to 1978, for fear that we might have a few individuals in the NLS-72 cohort who attended college and chose their majors *after* the tax revolt had begun. It turns out that this sampling modification does not change our estimated parameters of interest in any substantive way. In all, we have 1,450 observations with complete data in the NLS-72 cohort and 2,933 observations with complete data in the HSB cohort, for a total of 4,383 observations. Of these, 477 individuals, or eleven percent, selected into an education major[45].

Since we are interested in estimating the effect of the passage of tax and expenditure limits on a college student's decision of whether to major in education, we must first address the question: *whose* tax limit is relevant? Does the student respond to a tax limit in his or her home state, for instance, or a tax limit in the state in which he or she is attending college? Evidence from the NLS-72 suggests that the second option is probably more likely: in the cases in which a student attends a college out of state, college students are more likely

[45]In most specifications we also control for individual characteristics like gender and race, and our NLS-72 sample is decreased by 25 observations when these controls are added. The omission of these observations from our raw sample does not affect our results or the proportion of education majors in our sample.

to remain in the state where they attended college than return to the state in which they graduated from high school. Therefore, the results that we report in this paper count as the relevant tax limit any policy changes that take place in the *state where the individual attends college*. As a sensitivity test, we have also estimated our models in which the relevant policy changes are those that take place in the state where the student attended high school;[46] it turns out that this modification barely changes the parameter estimates of interest.

We initially present some basic descriptive analysis. Table 1 compares college graduates with education majors to college graduates majoring in other fields in both cohorts of students. The first row of the table reports mean HSB-equivalent ability test scores, by cohort and major. Each cell also presents the standard deviation and the number of observations within that category for illustrative purposes. We observe that in both cohorts, education majors average considerably lower ability test scores than do non-education majors. In the NLS-72 (students who graduated from high school in 1972), education majors averaged 12.4 percent lower ability test scores than did non-education-major college graduates. This gap had shrunk to a still sizeable and statistically significant 9.7 percent by the HSB cohort (students who graduated from high school in 1982).

We then compare education majors to non-education majors in states that passed tax limits. The results of these comparisons are reported in the next two rows of Table 1. In the

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[46]While the NLS-72 identifies the school district attended by students, the restricted-access version of the HSB does not even identify the *state* where the student went to high school. However, the identities of colleges attended are reported in HSB. Therefore, we say that the high school's state (in the HSB cohort) is the state where the largest number of students from that school eventually attended college.

NLS-72, our pre-tax revolt cohort, we find that education majors in states that would eventually pass tax limits averaged 10.9 percent lower ability levels than did non-education majors, while education majors in states that never passed tax limits averaged 13.4 percent lower ability levels than did non-education majors.[47] By the HSB, after tax revolt-era limits were passed, these relative levels had been reversed. Education majors in tax limit states averaged 13.2 percent lower achievement levels than did non-education majors (that is, the gap widened even as nonmajor achievement fell slightly in limit states), while the gap between education majors and non-education majors in no-limit states fell to 7.5 percent. Therefore, the qualitative evidence suggests that while states that never passed tax limits saw a 46 percent decrease in the gap between education majors and non-education majors over the decade encompassing the tax revolt, this gap in tax limit states actually *increased* by 17 percent. Moreover, the average absolute ability level of education majors fell over this decade as compared to a modest increase in absolute ability level of education majors in non tax revolt states.

The above comparisons do not control for any changes in the individual attributes of individuals across the two samples. Surely there are other variables, such as gender and race, that affect a potential teacher's decision to enter the education profession and our

[47]We categorize states as tax limit states only if they imposed a tax limit on school districts that either explicitly limited revenues or expenditures, or limited both the tax rate and assessment growth. We further restrict our sample of tax limit states to those which imposed a limit during the 1976-1983 time period. The states that passed these "potentially binding" types of tax limits on school districts during the tax revolt are Arkansas, California, Idaho, Kentucky, Louisiana, Massachusetts, Michigan, Mississippi, Missouri, New Jersey, New Mexico, Ohio, Texas and Washington. In table 2 we examine how changes to this categorization effect our results. Information on tax limitation laws is primarily from Mullins and Cox(1994).

quality measure, standardized test scores. If for some reason our sample systematically and disproportionately consisted of particular groups of individuals in certain states, it is possible that we may be attributing to changes in public policy differences in average ability stemming from variations in demographic characteristics. Our concern in misattributing causation to the passage of tax limits to an artifact of our data is increased due to the limited number of education majors we have for certain states.

Therefore, in the bottom panel of Table 1 we repeat the above analysis, but this time control for differences in individual race and gender.[48] Here, we report the mean residuals of regressions of teacher ability on race and gender, across major and cohort. We find that the results are qualitatively similar to those reported above. Again we find opposite effects in tax-limit and no limit states. While the gap in ability decreased by half in states that did not pass a tax and expenditure limit over this time period, the gap *increased* by 18 percent in the states that passed tax limits during the relevant period. Therefore, after controlling for individual attributes we still find that tax limits are associated with decreases in teacher ability, relative to the changes in teacher ability that occurred in no-limit states.

These results suggest that the differences between tax limit states and those without limits may be systematically different before and after the tax revolt. At the bottom of Table 1 we present some preliminary evidence to this effect. While the pre-limit difference in education major/non-education major difference in tax limit and non-tax limit states (i.e. the difference in difference) is small and positive (although not statistically significant), after the

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[48] We also examined including a measure of socio-economic status, which increased the effect on education major quality of the imposition of tax limit laws.

tax revolt the ability gap between education majors and non-majors was significantly larger in tax limit states than in no-limit states. While the ability gap between education majors and non-education majors shrunk over the sample period in no-limit states, it increased in tax limit states.

Why might the gap in average ability between education majors and non-education majors have shrunk in no-limit states during the 1970s and early 1980s? One possible reason is that teacher salaries, relative to those of other college-educated adults, may have risen during this period in no-limit states. The decrease in the ability gap becomes even larger when one controls for gender. This is expected, because in the early 1970s a larger fraction of high-ability women entered the teaching profession than did in the early 1980s. Failure to take into account this "natural" change in the demographic structure of who selects into teaching would lead to an understatement of the degree to which the gap in ability has decreased over time.

Differences-in-differences-in-differences results

We are now ready to more formally discuss the apparent effects of tax limits on the gap in average ability between education majors and non-education majors. To do so, we adopt a differences-in-differences-in-differences estimation strategy, similar to that used by Gruber and Madrian (1994). Essentially, we wish to determine whether the change over time in the ability gap between education majors and non-education majors differs systematically depending on whether the student's state passed a tax limit during the intermediate period.

The first cell in Table 2 presents the estimated parameter δ in the equation

$$\text{test score} = \theta X + \alpha \text{ year } (Y) + \beta \text{ limit } (L) + \gamma \text{ edmajor } (E)$$
$$+ \zeta Y{*}L + \eta Y{*}E + \lambda E{*}L + \delta E{*}L{*}Y + \epsilon ,$$

where individual and time subscripts are assumed but omitted and Y={0 if NLS-72 (pre tax revolt) and 1 if HSB (post revolt)} and X is a vector of individual specific characteristics (race, ethnicity, gender and family socio-economic status). We can interpret δ as the difference between tax limit states and no-limit states in the change from the NLS-72 cohort to the HSB cohort in the ability gap between education majors and non-education majors, holding constant observable characteristics. A negative value of δ would indicate that, all else equal, the ability gap between education majors and non-education majors has widened since the tax revolt in tax limit states, relative to no-limit states.[49] We correct all standard errors for within-state error correlation and heteroskedasticity.

We observe a negative and statistically significant relationship between tax limits and the relative ability level of education majors relative to non-education majors. This eight point change in the relative ability level of education majors translates into a 10 percent decrease in ability level, when evaluating ability at the mean value.

In addition to repeating the analysis from Table 1, in Table 2 we present results including state fixed effects and allowing changing economic and demographic factors within a state to have an effect on average student ability. If there is some systematic difference in the relative test scores of education and non-education majors within a state,

[49]Later in the paper we explicitly control for time-invariant and time-varying state-specific effects.

we do not want to attribute any of its effect to the introduction of tax limits. Similarly, variables that might vary across time and across states that might change the demand for teachers or the attractiveness of other job opportunities should also be controlled for.

We therefore include state per-capita income and percent of state population that is between five and seventeen for each state and year in the regressions.[50] Including these covariates slightly increases the size of the difference in test scores of education and non-education majors between tax limit and no tax limit states and also increases the precision of our estimate. Controlling for state covariates, the over time change in average ability in education and non-education majors is again negative and significant at the five percent level. We then examine how sensitive our results are to how we categorized the different states. The third through fifth panels of Table 2 report the differential in quality results by type of limit and by passage date of limit. During the tax-revolt period 14 states passed limits, and of these, five limits required states to rollback property tax rates if revenues rose by more than a certain amount after reassessments. These limits were passed to curtail the growth in revenues caused solely by large increases in property values, but did not limit revenue growth in non-assessment years.[51] Including a separate variable for these states

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[50]We also tested our specification by including a number of other state specific variables including population, total state income, percent of the population over 65 and under 5 and a measure of the state's liberalness based on the state's U.S. Senators' scores on selected votes as determined by Americans for Democratic Action. The results did not change significantly when these variables were included and they had no predictive power once state fixed effects were added. If state fixed effects are excluded and these other covariates are included, again the results do not change significantly.

[51]The states with only revenue limits in new assessment years are Kentucky, Louisiana, Michigan, Missouri and Texas.

increases the magnitude of the effect of tax limits on relative ed-major quality slightly to 10 points. As predicted, the estimated relative decline in education major quality in assessment rollback states is lower than in tax limit states-an estimated reduction of six points. While this forty percent decline in effect seems economically large, we cannot reject the null hypothesis that the two types of laws had the same effect on education major quality.

Panels 4 and 5 of Table 2 expand the number of states considered tax limit states by including an additional seven states which passed potentially binding limits in the 1969-1974 period. Unlike the limits passed in the late 70's, most of these earlier limits (5) were expenditure limits which typically gave the state more control over the growth rate in per pupil spending in the school district.[52] Comparing this larger group of states does not (quantitatively or statistically) significantly change our earlier findings. We also cannot reject the hypothesis that expenditure and revenue limits have the same effect on relative school quality.

Our results may also be confounded by the fact that many states enacted school finance reforms over the same period. As a first pass, assume that all school finance reforms are created equal. Now we again estimate the effects of tax limits on teacher ability, but this time we control for the implementation of *any type* of school finance reform identified by

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[52] The states which passed limits in this earlier period were Arizona, Colorado, Indiana, Iowa, Kansas, Minnesota, and Utah. States with only expenditure limits are Arizona, Colorado, Kansas, Minnesota, New Jersey and Iowa. California and Massachusetts have both an expenditure limit and a revenue limit.

Downes and Shah (1995).[53] Therefore, the second column of Table 2 presents the estimated parameter δ in the equation

$$\text{test score} = \theta X + \alpha Y + \beta L + \gamma E + \zeta Y^*L + \eta Y^*E + \lambda E^*L + \delta E^*L^*Y + \mu \text{ reform state (R)} + \nu Y^*R + \xi E^*R + \pi E^*R^*Y + \epsilon ,$$

where all notation is as before, except that now we include a separate set of school finance reform variables in the model. (Where appropriate, state-specific effects and covariates are part of X) We find qualitatively similar though slightly larger results.

Next, we take to heart Hoxby's (1996) concern that not all school finance reforms are created equal. Here, we replace the school finance reform variables described above with two sets of reform variables and the resultant parameters $\{\mu, \nu, \xi, \pi\}$, one set each for the states that implemented pro-spending and anti-spending school finance reforms, as defined by Hoxby (1996).[54] We report our estimate of δ in this specification in the third column of Table 2.

We observe that when we differentiate between pro-spending and anti-spending

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[53]On this basis, states that passed school finance reforms during the relevant time period were Arkansas, California, Connecticut, Kansas, Kentucky, New Jersey, Utah, Washington, West Virginia, Wyoming and Arizona, Florida, Georgia, Idaho, Illinois, Iowa, Maine, Maryland, Massachusetts, Minnesota, Missouri, New Hampshire, New Mexico, Ohio, Oklahoma, Rhode Island, South Carolina, South Dakota, Tennessee, Vermont, Virginia and Wisconsin The first set of states passed court mandated reforms while the second set of states passed state legislated school finance reforms.

[54]Hoxby (1996) identifies as "pro-spending" the school finance reforms in Connecticut, New Jersey, New York, Pennsylvania, and Rhode Island. Vermont could also be considered "pro-spending," as it repealed an "anti-spending" reform during this period (it turns out not to matter in this analysis how Vermont is categorized). States identified as having "anti-spending" reforms are California, Delaware, Maryland, Minnesota, New Mexico, Utah, Virginia and Wyoming.

school finance reforms, the estimated effect of tax limits is still larger and more statistically significant than was reported in the second column of the table. This result suggests that, holding constant whether a state also implemented a pro-spending or anti-spending school finance reform, the difference between limit and no-limit states in the education major-nonmajor ability gap increased after passage of the tax limits. As before, this result holds for all of our categorizations of tax limit states.

This portion of our analysis, therefore, leads to several distinct conclusions. First, we present significant evidence that tax revolt-era tax and expenditure limits have led to an increase in the ability gap between education majors and non-education majors in the affected states. This effect is not only statistically significant, but is substantial in magnitude as well. In addition, we find this result is invariant to increasing the tax-revolt period to include expenditure limits passed in the early 1970s. Finally, we find that controlling for whether the state also passed school finance reforms is important in gauging the degree to which the tax limits affected teacher quality. It is interesting to note that the difference-in-difference-in-difference coefficients on the school finance reform variables are generally not statistically significant, nor are they typically sizeable in magnitude. Therefore, while we find substantial evidence that tax limits led to diminished teacher quality, we generally cannot differentiate from zero the parallel effect of school finance reforms.

3. Changes in attributes of new teachers

The results presented in the preceding discussion suggest that tax limits have had substantial negative effects on the average relative ability level of education majors. But this is only an imperfect approximation of the effects of tax limits on *teacher* quality. While the vast majority of them do, not all education majors eventually become teachers. In addition, not all public school teachers were education majors in college. Therefore, it is always possible that the decrease in the overall ability level of education majors as an apparent consequence of tax limits is not an accurate representation of what actually happened to the average ability level of teachers in general.

Ideally, we would be able to repeat the analyses presented in Tables 1 and 2 using employment as a public school teacher, rather than college education major, to split the sample. Then we could directly test the hypothesis that tax limits not only affected average education major ability, but also affected average public school teacher ability. Unfortunately, we do not have the data to conduct such a test. While the NLS-72 and HSB each identify an individual's occupation, so it is possible to tell whether the individual is (or was) a school teacher, it is impossible in either survey to identify the *sector* of the teacher's employment. If the average ability level of private school teachers, and if the proportion of new teachers entering the public and private sectors, are time-invariant in a state, this would not necessarily be a problem. But there is little reason to believe that this would be the case. Instead, if tax limits are associated, as the evidence suggests, with fewer public school teachers per student or increased private school enrollments, one would suspect that the proportion of new teachers entering the private sector should have systematically increased in tax limit states. So even if the average ability level of private school teachers in a state is time-invariant, we cannot use the available data to directly estimate the effect of tax limits on public school teacher ability levels.

We can, however, use a different data set to indirectly measure the relationship between tax limits and teacher qualifications. Specifically, we use the 1987-88 round of the Schools and Staffing Survey (SASS), conducted by the U.S. Department of Education, to make inferences about the changes in teacher attributes over time. While we do not have ability test measures for teachers in the SASS, we do have information of specific teacher credentials, such as the college major and the identity of the college attended. Since numerous college guidebooks categorize colleges in terms of their selectivity, we are consequently able to identify the rough relative selectivity of the colleges that the teachers attended. College selectivity has been shown (e.g., by Loury and Garman, 1996) to be strongly related to ability test scores, and has also been shown (e.g., by Ehrenberg and Brewer, 1994) to be strongly independently correlated with student academic achievement.

Using the SASS, we compare the attributes of public school teachers who entered the teaching force in 1984 through 1987 (the four most recent years in the sample, and all following the tax revolt) to those who entered the teaching force from 1974 through 1977 (preceding the tax revolt, and around the same age group as the NLS-72 sample). A drawback of using the SASS is that we rely on observations of people who *remained in teaching through the tax revolt* to make inferences about who selected into teaching prior to the tax revolt. While higher ability teachers tend to have shorter spells of teaching and higher levels of mobility (Eberts and Stone, 1984; Murnane and Olsen, 1989; and Rickman and Parker, 1990), this identification strategy is still acceptable as long as the relationship between teacher ability and mobility, say, is not systematically related to the passage of tax limits, as any differences will be absorbed into a cohort-specific fixed effect. We have no way of telling for certain whether such a relationship exists. If, however, tax limits disproportionately induce high-ability teachers to leave teaching, the remaining teachers from this pre-revolt cohort will on average be weaker than they would have been in the absence of a tax limit. If this is the case, we understate (in magnitude) the estimated effect of tax limits on teacher ability using data from after the tax revolt.

The first row of Table 3 presents the estimated effect of tax limit imposition on the probability that a teacher graduated from a selective college. We define a selective college as one that Lovejoy's guide to colleges identifies as "competitive," "highly competitive," or "most competitive." Specifically, we present the estimated change in probability of having the given credential given a change in tax-limit status. We estimate a maximum likelihood probit model in which the dependent variable is the probability of graduating from a selective college, and we control for cohort effects in tax limit and no limit states, the percentage of the state population that is school-aged during the appropriate cohort, and state per-capita income during the relevant period.

We estimate that tax limits are associated with about five percentage points' lower probability that a newly-hired teacher will have graduated from a selective institution, all else equal. Given that the pre-revolt probability of graduation from a selective institution (measured from the cohort who began teaching from 1974 to 1977) was 67 percent, this implies that tax limits are associated with new teachers who are about 7.5 percent less likely to have graduated from selective institutions than would have occurred in the absence of the tax limit. The results are roughly the same magnitude whether or not we control for the existence of school finance reforms, either generically or using Hoxby's (1996) pro-spending or anti-spending characterizations. Therefore, these results are consistent with those presented in Table 2. These results provide suggestive evidence that tax limits are associated with lower levels of teacher ability, all else equal.

Since two-thirds of teachers in our sample graduated from selective institutions, we also experimented with other thresholds of college selectivity. Therefore, in the second row of Table 3 we present the results of differences-in-differences regressions in which we estimate the effects of tax limits on the probability that the teacher will have graduated from an elite college, one ranked "highly competitive" or better by Lovejoy's guide. We find that in the specification excluding school finance reforms, tax limits are associated with 2.5 percentage points' lower probability that a new teacher will have graduated from a highly selective college. Since ten percent of our pre-tax revolt cohort graduated from a highly selective institution, this implies that tax limits have been associated with a 25 percent lower probability that a new teacher attended an elite undergraduate institution. Therefore, while tax limits apparently reduce the probability that public schools within tax limit states will hire teachers from *any* selective institution, public schools are *especially* less likely to hire teachers from highly selective colleges on a proportionate basis.

Might tax limits also affect the distribution of education majors versus non-education majors amongst newly-hired teachers? To address this question, we estimate the probability that a teacher will have an education major, using the same methods as before. We find, as is shown in the third row of Table 3, no evidence that tax limits are related to the probability of hiring an education major. However, *conditioning on hiring an education major*, tax limits are significantly associated with lower ability levels amongst the teachers that public schools hire. Specifically, among education majors, tax limits are associated with 4.8 percentage points (7.4 percent) lower probability of hiring education majors who graduated from selective institutions in general, and 2.4 percentage points (30 percent) lower probability of hiring education majors who graduated from highly selective colleges. Therefore, while public schools subject to tax limits are no more or less likely to hire education majors as a result of the tax limitation policy, they *are* more likely to hire education majors who graduated from less selective institutions.

4. Conclusions

Prior authors who have investigated the effects of fiscal constraints on school services have found that tax limits are associated with higher student-teacher ratios and lower teacher salaries. But the education literature strongly suggests that there is little relationship between these measured school inputs and student achievement. Therefore, it is difficult to conceive of how the relationships between tax limits and student achievement found by Figlio (1997b) and Downes and Figlio (1997) are due to changes in measured school inputs such as the student-teacher ratio.

We find that tax limits apparently systematically deter high-ability individuals from entering public school teaching in affected states. Tax limits are associated with significantly lower average education major ability levels (relative to nonmajor college graduates), and are also apparently associated with lower ability levels of public school teachers as well. Since specific teacher attributes such as teacher ability and college selectivity apparently have large effects on student performance, we argue that the link between tax limits and student achievement comes through diminished teacher quality.

Our results have strong policy implications. Teachers clearly respond to fiscal constraints, either directly (as tax limits may be bad advertising for jobs at public schools) or indirectly (as tax limits lead to fewer, lower-paying teaching jobs). Since teacher quality seems to matter in determining student performance but class sizes, for instance, do not, it seems that schools faced with increased fiscal constraints may wish to attempt to maintain teacher compensation at the expense of class size. However, such an approach may not be a silver bullet. If teachers--particularly high-ability ones--respond more to environmental factors such as class size than to financial factors such as salary, then a policy of maintaining teacher compensation while substantially increasing class size might exacerbate the situation. This trade-off becomes even more problematic if it is difficult for school districts to measure a job applicant's ability level.

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Table 1: Descriptive Statistics of Composite (Verbal+Math) Test Scores in NLS-72 and HSB for Education Majors and Non-Majors

	NLS-72			HS&B		
Sample	Ed major	Not ed major	Difference (std error)	Ed major	Not ed major	Difference (std error)
Reported Composite Test Scores:						
Full sample	76.32 [21.84] 266	87.17 [21.55] 1184	-10.85 (1.47)	76.05 [20.01] 211	84.26 [21.24] 2722	-8.21 (1.51)
Tax limit on schools	77.43 [20.66] 104	86.92 [21.75] 453	-9.49 (2.34)	73.44 [21.68] 83	84.61 [20.92] 1013	-11.17 (2.39)
No tax limit on schools	75.61 [22.60] 162	87.32 [21.44] 731	-11.71 (1.88)	77.74 [18.74] 128	84.05 [21.43] 1709	-6.31 (1.95)
Residuals After Partialing Out Gender and Race:						
Tax limit on schools	-6.84 [19.57] 102	1.75 [20.40] 445	-8.58 (2.22)	-9.26 [21.60] 83	1.16 [20.15] 1013	-10.42 (2.31)
No tax limit on schools	-9.94 [21.78] 159	1.87 [20.22] 719	-11.81 (1.80)	-5.62 [17.83] 128	0.31 [20.62] 1709	-5.94 (1.87)
Differences in Ed/Non-Ed Test-Scores By Tax Limit Status			3.23 (2.87)			-4.48 (2.98)
Difference in difference in difference						-7.71 (4.00)

Notes: Test scores are expressed in High School and Beyond equivalent test scores. The first row of each cell presents the mean value. Standard deviations are presented in the second row of each cell in brackets. The number of observations is given in the third row of each cell. The differences listed are the difference in means for education majors and non-education majors, with standard errors listed below.