For Whom the TEL Tolls:

Can State Tax and Expenditure Limits Effectively Reduce Spending?

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Can voters stop state governments from spending at high rates through the enactment of tax and expenditure limits (TELs), or do these laws become dead letters? We draw upon the principal-agent literature to theorize that TELs – one of the most frequent uses of the initiative process across the country – may be circumvented by the sorts of elected officials who would inspire their passage.

In order to investigate our claim, we conduct an event study. First, we test for the effectiveness of TELs across states using a differences-in-differences model. Second, we dissect our treatment variable using different legal provisions of the limits to test whether there is a uniform effect across different types of TELs. Finally, we compare state fiscal patterns before and after adoption on a state-by-state basis. Using this simple approach and other methods, we show that TELs are largely ineffective, and that state officials can circumvent them by raising money through fees or borrowing. Our finding is consistent with recent studies showing that policies passed through direct democracy can often be thwarted by the politicians charged with implementing them.

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Tax and expenditure limits (TELs) belong to a general class of political phenomena that attempt a tough trick: locking in the preferences of a set of political principals by constraining the future actions of potentially unknown and hostile agents. Either voters are trying to limit state lawmakers, or legislators in one era are attempting to slow the growth of government under future lawmakers. Regardless, the proponents of these limits face the common delegation problem, made especially challenging by the fact that they are trying to constrain the behavior of agents long into the future, when they may be unable to monitor these agents.

We see this challenge as similar to the dilemma faced by legislators attempting to control the executive branch (McKelvey and Ordeshook 1984; Shepsle and Weingast 1984; McCubbins et. al 1987, 1989; Kiewiet and McCubbins 1991; Lupia and McCubbins, 1998; Epstein and O'Halloran 1999; Huber and Shipan 2002), legislators on the floor delegating power to committees (Fenno 1973; Krehbiel 1991; Rohde 1991; Aldrich and Rohde 1998, 2000; Cox and McCubbins 1993, 2005), members of Congress trying to discipline the budgetary decisions of future Congresses (Schick 1995, 2005), and voters giving over power to elected officials (Gerber et al 2001, 2004).[1] Tax and expenditure limits fall into this troublesome category because lawmakers charged with implementing a limit may be hostile to the goals of its backers. Why else the need for a limit in the first place? It may also be the case that after the enactment of a TEL the principals themselves may change, either by population migration or changes in preferences over time. This means that principals themselves may push the lawmakers to find ways around the proscribed limitation. Either way, it is difficult to tie the sovereigns hands, whether we believe that it is the legislature pr the people that is sovereign (North and Weingast 1989) We suspect that the lawmakers subject to a spending or revenue limit may respond to demands by their constituents who want to see government grow at a faster rate than the limit proscribes. Lawmakers may have the ability to circumvent limits in ways that are buried deep in the details of thousand-page budget documents. Because it is difficult to monitor state fiscal actions, the initiative proponents who sponsored the TELs may be unable to follow their implementation.

Our main conjecture is that agency problems prevent tax and spending limits from having their intended effect of reducing the size of state government.[2] This contention stands in contrast to the empirical findings of many previous works, such as Misiolek and Elder (1988), Elder (1992), Shadbegian (1998), Bails and Tieslau (2000), and New (2001). However, we suspect that much of the previous scholarship that finds TELs to be effective may be based on the flawed assumption that TELs uniformly affect fiscal outcomes, and that previous results were driven by a few disparate cases. We design our empirical strategy to respond to this challenge.

We take advantage of the fact that most TELs are a recent phenomenon to conduct comparisons of spending before versus after adoption in each TEL state. We employ an event study following the models used in the literature on finance and test the effect of TELs in multiple stages. First, we examine the effect of TELs in a pooled model, where we assume a uniform effect of the treatment across states. It is with this model where we are most likely to find an effect. Second, we break down the different types of TELs and test for the effectiveness of specific legal provisions, thereby relaxing the assumption that TELs have a uniform effect. In our last model, we examine each state one at a time, emphasizing that for TELs to have a general effect there must be evidence at the individual unit of analysis. Our consistent finding is that TELs are almost never effective. Finally, we explore why TELs do not result in smaller government, looking at shifts to charges and fees that occur following the enactment of a TEL.

In contrast to much of the previous literature, our results do not prove sanguine for TEL backers, and support our conjecture that they rarely overcome their agency problem.

I. Literature Review

Previous studies explore the effects of TELs by making cross-state comparisons, often supplemented by multiple observations of each state's fiscal activities over time (Abrams and Dougan 1986; Misiolek and Elder 1988; Elder 1992; Shadbegian 1996; Mullins and Joyce 1996; Shadbegian 1998; Bails and Tieslau 2000; New 2001; Mullins 2004). They typically regress some measure of a state's fiscal behavior upon a dichotomous variable indicating the presence in each state of their institution of interest (a treatment) as well as a set of covariates or "control factors" that are meant to make the states in the cross-state comparison actually comparable. They interpret the coefficient on their treatment variable as the estimated effect of the TEL.

For example, Elder (1992) regresses taxes collected on dummy variables indicating expenditure limits and revenue limits, along with a vector of control variables for the years 1950-1985. He finds differential effects based on the type of limit enacted, noting that "States with Revenue limitation laws have experienced no change in tax growth, whereas there is strong evidence of a reduction of tax growth in states with expenditure limitations" (Elder 1992, p. 58). Bails and Tieslau (2000) use a similar panel research design for the years 1969-1994. The coefficient of their singular TEL dummy variable is significant and negative, indicating to them "that real per capita state and local spending in states that have a tax or spending limit in place will be more than $41 lower than in those states that do not have such limitations in place" (Bails and Tieslau 2000, p. 270). Finally, New (2001) uses a panel, time series regression from 1972-1996 to regress annual change in state and local expenditures on TEL dummy variables indicating TELs passed by citizen initiative or by the legislature. He states that "The model predicts that, if other factors are held constant, per capita state and local expenditures will decrease by $16.29 every year after a state has passed a TEL by citizen initiative. Conversely, the model predicts that TELs enacted by state legislatures will actually cause per capita expenditures to increase by $14.00" (New 2001, p. 8). His interpretation, however, does not follow traditional confidence levels of statistical significance.

Other authors have come to contradictory empirical conclusions using a cross-sectional research design. Abrams and Dougan (1986) conduct a cross-sectional regression for the year 1980. They find that constitutional limits, the subject of their analysis, are statistically insignificant in all of their models. However, they note that "it is quite possible that the endogenous nature of such limits may obscure their true effects" and that a "single equation cross-section regressions may fail to estimate the full impact of those limits". They therefore do not "reject with the appropriate degree of confidence that the hypothesis that Constitutional Limits [their treatment variable] reduces spending." Mullins and Joyce (1996) regress state fiscal limitations on four dimensions of fiscal structure (government size, revenue source reliance, state revenue shares, state expenditure shares) in a panel study for the years 1970-1990. They conclude that "state limitations appear to have a very limited effect" (Mullins and Joyce 1996, p. 95).

There have been several other studies of the effectiveness of TELs that use improved research designs. Cox and Lowery (1990), updated in King-Meadows and Lowery (1996), for example, conduct regression analysis individually on three TEL states (Michigan, South Carolina, and Tennessee) and compare the results to three comparable states without TELs (Ohio, North Carolina, and Kentucky, respectively). They find no evidence that any TEL worked in the 1990 analysis, and only marginal evidence in the 1996 analysis as Tennessee then showed a decrease in spending. This is a strong research design, complete with both a pretest and a comparable group, but is limited in its scope since only three TEL states were considered.

A few other authors follow the research design we turn to first, an interrupted time series. Howard (1989) looks at changes in the ratio of state taxes collected to personal income before and after implementation of a TEL. She finds no significant alterations in this ratio both when pooled between all TEL states and within single states in comparison to non-TEL states. Similarly, Bails (1990) takes the average percent change of revenue and expenditures for TEL states and non-TEL states. Using a difference of means test, he does not find a significant difference between the two. Stansel (1994) compares state fiscal activities before and after TEL passage, finding that some measures seem to work while others fail. Finally, Joyce and Mullins (1991) chart deviations of TEL states from the national average in several revenue and expenditure categories. They argue that "states with local TELs have short term overall declines, but state taxes increased in the long term for those states." Though the empirics of these efforts are relatively straightforward, they succeed in resolving some of the challenges posed by endogeneity because they compare states before and after adoption, relative to a comparable group. This time series approach holds constant the unmodeled differences between states that might make some more likely than others to adopt TELs.

By contrast, studies that rely primarily on cross-sectional variation across disparate states may produce biased estimates of TEL effects due to the potential endogeneity of TEL adoption. It is quite possible that the forces that lead to TEL passage also affect a state's fiscal choices. If so, the endogeneity previously noted by Rueben (1997, p. 8) and by Shadbegian (1998, pp. 125-

26) represents a significant challenge to the inferences made in these cross-sectional studies. One possible source of this endogeneity is that states that implement spending or revenue caps may also have a different sort of electorate—that is, one more inclined to keep the size of government small (Joyce and Mullins 1991). Such differences are not surprising given other evidence that spending is systematically different in states with an initiative or referendum process (Matsusaka 1995, 2000, 2004).

For the study of TELs, concerns about endogeneity can take two possible forms. First, since TELs are not randomly assigned across states, the types of states that adopt them may be very different than the types of states that do not.[3] A cross-sectional investigation of their effects might suffer from this sort of bias, but our differences-in-differences approach controls for the constant characteristics of states that can determine TEL adoption.

Second, since TELs are not randomly assigned across time within adopting states, there may be a shift toward a lower-spending "mood" in a state that occurs simultaneous with adoption. Our approach does not solve this problem. But note that because we do not find any apparent TEL effect, this sort of endogeneity raises little cause for concern. If a shift to a low-spending mood came at the same time as TEL adoption, one might find an apparent TEL effect and wonder whether it was instead caused by the shift in mood. But we do not find that spending is lower after TEL adoptions. Neither a shift in mood nor a shift in laws brings a clear change in state spending, so we have no worries about untangling the effects of mood and law.

Beyond concerns about endogeneity, we contend that many studies, including panel studies, incorrectly assume that TELs have a uniform effect, regardless of their specific provisions or method of passage.[4] As we later demonstrate, not all TELs are created equal. By unpooling the data and separately analyzing each TEL, we can conduct a more fine-grained analysis that relaxes the assumption that TELs of all types have the same effects.

Regardless of the approach and techniques used, the literature is mixed on whether TELs do or do not effectively limit revenues and expenditures. We believe that this topic is important enough to merit renewed attention with increased consideration of research design. This topic is important because the effectiveness of tax and expenditure limits – often passed via direct democracy – fits in with a larger controversy in the literature on the initiative process. Currently, there is a significant debate between works which find that the initiative process is an effective check on the legislature and helps move policy toward the median voter's preferences (Bowler, Donovan, and Tolbert 1998; Gerber 1996, 1998, 1999; Lupia and Matsusaka 2004; Matsusaka & McCarty 2001), and research contending that initiative victories are ephemeral and that the devil is in the implementation (Gerber et al. 2001, Bali 2003, Gerber et al. 2004; Garrett and McCubbins 2008). Since many TELs have been initiated or endorsed by voters, our analysis here can contribute to this larger debate.

II. Data and Methods

The research design we employ in this study is similar to that most commonly used in the financial literature (see MacKinlay 1997 for a survey). As suggested by MacKinlay, the gold standard for event studies is to test the effect of the event on each case individually. The silver standard is combining these individual results and checking to see if the combined effects are real or due to chance alone. For instance, if the event truly has no effect, then on average half the firms should have positive abnormal returns and half negative. Finally, the bronze standard is pooling all cases together and testing for a common effect. This last approach is the easiest hurdle to surmount and the one that we start with for a first look at the effectiveness of TELs.

A fundamental analysis of the effectiveness of a TEL begins with a full understanding of its actual language and structure. No two TELs are exactly the same, as they can vary across numerous legal and political dimensions. Mullins and Wallin (2004, p.9) note that state government TELs take the form of revenue limits, expenditure limits, a combination of both, or restrictions on "growth in general fund expenditures or appropriations," and that they can be "tied to growth in population, income, prices, the economy, or wages." In addition, Poterba and Rubin (1999) argue that they can be binding or not. Following Poterba and Rubin, our definition of TELs is limited to those that constrain the whole of taxing and spending, not single segments such as property tax limits.[5] Table 1 reports the presence and timing of TEL enactments, as well as two key provisions of the laws. In our second model testing TEL effectiveness, we use such provisions to categorize TELs into different types, testing each of these types of TEL to see if any of them affect spending.

[Insert Table 1 Here]

The dependent variables in our analysis measure state and local fiscal behavior. Our dependent variable follows the type of TEL. To measure a spending limit's overall effectiveness, we look at whether it changes total state and local spending levels. To measure a revenue limit's overall effectiveness, we look at whether it changes total state and local revenue levels. For each, we look at per capita levels, reported in appropriate editions of the U.S. Census Bureau's annual *State and Local Government Finance* publication.[6] We collected this time series from fiscal year 1969 to 2000, and deflated all estimates by the implicit price deflator.[7] We prefer to use state and local fiscal behavior rather than state-only fiscal behavior for two reasons. First, almost half of all TELs adopted in the U.S. include provisions that limit both levels of government. Second, if the result of a TEL is simply to push fiscal burdens down to lower levels in order to substitute local spending or revenue for states dollars, this consequence is not in line with the spirit of tax and expenditure limits, even if it technically follows the letter of the law. We expect this sort of substitution to be typical if the TEL makes it possible. As an additional check on our results, however, we also collect state-only spending.[8] Our measure of state and local spending is direct general expenditures, whereas our measure of state-only spending is total expenditures. The type of state and local revenue in our series is general revenue from own sources.

We collected our covariates, all of which we expect will lead to an increase in spending or revenues, as follows:

a. *Per-capita income* is measured as total personal income divided by total population and deflated by the consumer price index. We collected this time series from fiscal years 1971 to 2000 using estimates from the Bureau of Economic Analysis.
b. The *elderly population rate* is calculated as the number of persons 65 years or older divided by the state's total population. We collected this time series from fiscal years 1971 to 2000 using estimates from the Census Bureau.[9]
c. The *school-age population rate* is calculated as the number of persons ages five to seventeen divided by the state's total population. We collected this time series from fiscal years 1971 to 2000 using estimates from the Census Bureau.
d. Political variables on the party controlling each house of the *state legislature* and *governor's office* were collected for the years 1971 to 2000 using Council of State Governments' *Book of the States.* Each variable takes on the value of one if the Democratic Party controls the chamber/office and zero otherwise.

III. A Panel Study; Differences-in-Differences

In this section, we present the results of differences-in-differences tests (DD estimation) of the effect of TELs. Here we are assuming that the effect of TELs is uniform across states, regardless of the differences in their letter of their laws. As noted, this specification of the model is the first step in our three-part event study. This should be the easiest hurdle to surmount since it is possible that results will be driven by a few disparate cases.

A differences-in-differences analysis is the most widely used econometric technique for observational studies of policy impacts (Wooldridge 2002). In a DD estimation, one compares the difference in outcomes before and after the policy intervention for groups that were given the policy treatment to the same difference for unaffected groups. In the language of research design, this is a two-group, nonequivalent group design with both pre-test and post-test observations. As before, we use this estimator to test the often-made political prediction that TELs would slow the growth of spending in states that passed them, while spending in unconstrained states would continue to skyrocket. DD estimation allows us to compare the spending patterns of states with TELs against those in states that never adopted limits both before and after adoption, conducting both a pre-test and a post-test for treatment and comparison groups. If states with similar spending levels before the tax revolt diverge after one of them passes a TEL, then the implication is that this change resulted from the institution itself.

Our differences-in-differences specification is as follows:

$$\Delta(FB_{it}) = \alpha + \beta_1 (TEL_{it}) + \gamma_1\Delta(SAP_{it}) + \gamma_2(SAP_{it}) + \gamma_3\Delta(EP_{it}) + \gamma_4(EP_{it}) + \gamma_5\Delta(PCI_{it}) + \gamma_6(PCI_{it}) + \gamma_7(HD_{it}) + \gamma_8(SD_{it}) + \gamma_9(GD_{it}) + S_i + D_t + \varepsilon_{it}$$

Variables are indexed for state *i* and year *t*. Where FB is state fiscal behavior (state and local revenue for revenue limits, state and local spending for spending limits), TEL is the presence of a tax and expenditure limit, SAP is the school age population rate, EP is the elderly population rate, PCI is per-capita income, HD is a dummy variable coded as one if the state's lower house has democratic party majority, SD is a dummy variable coded as one is a state's upper house has a democratic party majority, GD is a dummy variable coded as one if a state's governor is a member of the democratic party, S stands for a vector of state fixed effects, and D a vector of times dummies. The latter two terms are crucial to differences-in-differences estimation, as they serve the purpose of holding constant differences in the differences between states and any changes due to common trends.

In all specifications of our models, we first difference the data, as this, we believe is the variable subject to political choice. It also helps to correct for autocorrelation. This latter point is important to our analysis because, as noted by Bertrand et al. (2003), serial correlation often inflates standard errors in DD estimation leading to over-rejection of the null hypothesis. Indeed, from a series of simulations, those authors found effects "significant at the 5 percent level for up to 45 percent of the placebo interventions." (Bertrand et al 2003, 1). First differencing the data has the secondary benefit of relaxing the common trend assumption, implicit in all differences–in-difference estimation (See Moffit 1991 for a review). The common trend assumption is that, after controlling for changes in relevant factors such as per-capita income and population increases, control and treatment cases follow identical trends that are fully captured by time dummies. By detrending the data using first-differences, we reduce the likelihood that uncommon trends create spurious correlations.

[Insert Tables 2 & 3 Here]

Table 2 presents the results for both state and local and state only spending. Table 3 presents the results for state and local revenue. We estimate our models using panel correlated standard errors. In each of these models, the coefficient on the TEL term is negative but falls short of statistical significance, even though we analyze 1420 cases.[10] More specifically, in the models of state and local fiscal behavior, the coefficient on the effect of TELs falls just above the 15% level of significance for the effect of spending limits on state and local spending, and barely below this level for the effect of revenue limits on state and local revenue.[11] The effect of TELs on state only spending does not approach significance in any model. While this model provides sufficient statistical proof that TELs, by themselves, do not have a significant effect, it is worthwhile to discern the conditions under with TELs may have an effect and what that effect might be.

IV. A Panel Study; Differentiated Treatment Effects

While the previous results disconfirmed a uniform and statistically significant effect of TELs, the coefficients of our variables indicating the presence of a revenue or spending limit were both negative and nearing the 15% confidence level. It may well be that certain kinds of TELs, under certain conditions, are indeed effective. We would not be able to find this in the pooled model presented above. As such, the next step in our analysis is to unpack the singular treatment variable used to indicate the presence or absence of a TEL. This weakens the assumption in the differences-in-difference model that all TELs have a uniform effect. We break down this variable in a manner following New (2001) and Stansel (1994) by looking at the legal provisions of TELs. Specifically, we look at whether or not a TEL is constitutional versus statutory, whether it limits state and local fiscal outcomes or simply state fiscal outcomes and whether it limits spending to increases in inflation versus any other less stringent benchmark, such as per-capita income.

Looking at these three provisions alone, we identify seven different types of spending limits and four different types of revenue limits that have been enacted in the states. Type One is a statutory TEL that only limits state spending and is not tied to inflation. Type Two is a constitutional TEL that only limits state spending and is not tied to inflation. Type Three is a constitutional TEL that limits both state and local spending but is not tied to inflation. Type Four is a statutory TEL that covers both state and local spending but is not tied to inflation. Type Five is a statutory TEL that is tied to inflation but only limits state spending. Type Six is a statutory TEL that is both tied to inflation and limits both state and local spending. Finally, Type Seven is the most stringent type of TEL by the letter of the law, one that is constitutional, limits both state and local spending, and is tied to inflation. Table 4 indicates how limits in different states fall into these seven groupings.[12]

[Insert Table 4 Here]

The analysis presented in this section parallels our differences-in-differences analysis, except in the specification our treatment variable: We use state and year fixed effects, a first-differenced dependent variable, and panel-correlated standard errors. The reference category in each of these regressions is the state-years where no TEL is in place.

[Insert Tables 5 Here]

We test for the effect of our seven treatment variables on both state and local spending, state and local revenue, and state only spending. The results of this exercise appear in Tables 5 and 6. Looking first at the seven groupings of spending limits, it is evident that only one of the seven types of TELs had a negative and significant effect on changes in spending per-capita. In addition, two of the groups have positive coefficients and one is significant. The group with a significant effect is Type Two, representing a constitutional TEL that limits only state spending and is not tied to increases in inflation. This result is surprising because this is among the types of TELs that is generally considered to have weak provisions. The states within this grouping are Texas, Connecticut, Rhode Island, South Carolina, and Oklahoma. We will return to this finding when we analyze the states individually.[13]

[Insert Table 6 Here]

We turn now to the results for the four groupings of revenue limits. These results show that all four types of revenue limits have insignificant effects on revenue per-capita. While all the coefficients' signs were negative, no coefficient approached traditional levels of significance. These results do not provide much evidence for the effectiveness of TELs, but we nonetheless want to go one step further and to analyze the effectiveness of TELs on a state-by-state basis.

V. Individual State-by-State Time-Series Analysis

In this section we estimate ordinary least-squares, time-series regressions for each state individually. This research design takes into account two important possibilities not accounted for in previous research: 1) Spending and revenue respond to different factors in different states and 2) TELs vary and are unlikely to have uniform effects on fiscal activities. As before, we use spending per-capita as the dependent variable for states that passed spending limits and revenue per-capita for states that passed revenue limits. We again first difference our dependent variable.

Using the same acronyms as employed above, our state-by-state specification for an individual state at time *t* is as follows. Instead of time dummies, we rely on a linear time trend. This variable allows us to treat time in our individual time series regressions in a manner that is similar to the manner in which we treat time in our panel study, where we used fixed year effects.

$$\Delta(FB_t) = \alpha + t_1 (TEL_t) + \gamma_1\Delta(SAP_t) + \gamma_2(SAP_t) + \gamma_3\Delta(EP_t) + \gamma_4(EP_t) + \gamma_5\Delta(PCI_t) + \gamma_6(PCI_t) + \gamma_7(HD_t) + \gamma_8(SD_t) + \gamma_9(GD_t) + \gamma_{10}TREND_t\ \varepsilon_i$$

If a TEL puts a state on a course toward smaller government than it would otherwise have followed, the coefficient on the dummy variable signifying TEL implementation will be negative and significant. Yet in nearly every case, the TEL effect fell short of significance, while other factors appeared to exert important influences on spending. For example, in California, the first differences of per-capita income and school age population rate both significantly increased spending. While none of the other variables in the model were significant, the model still explains almost half of all variations in state spending. Most importantly, and consistent with previous studies of California (Kiewiet and Szakaly 1996; Kousser, McCubbins, and Rozga 2007), the variable indicating TEL implementation is not significant. This pattern is typical for the regressions that we estimated in each TEL state.

[Insert Table 7 Here]

Table 7 reports the estimated coefficients and standard errors from each of these regressions for our treatment variable, the implementation of a TEL. We conduct this analysis on both state and local spending as well as state-only spending. We omit the estimated effects of the covariates in this discussion, but make the full results of all regressions available at http://mccubbins.ucsd.edu. Of the twenty states that passed spending limits in our sample, only one significantly reduced state and local spending at the 5% level following the implementation of a TEL, Colorado.[14] The coefficient for the TEL variable in Colorado's analysis, -3.07, suggests that the implementation of this state's TEL, *ceteris peribus*, led to a $348 decrease in changes in state and local expenditures per-capita once its effect is converted into 2005 dollars. This is a sizable amount, especially relative to the effects typically found in the literature that pools all TELs together (thus watering down the singularly strong Colorado effect). We will return to a discussion of Colorado in the conclusion.

While this is an important demonstration that a single TEL was effective, the overwhelming message of the table is that only one of twenty spending limits appears successful at reducing state and local spending at the 5% confidence level. This number of significant effects is not more than would be expected due to chance alone, although we do not believe the experience of Colorado to be chance. The verdict is even worse for state-only level spending, since no TEL significantly reduced spending in that arena. Further, the fact that there is an almost equal mix of positive and negative coefficients suggests that these results are not simply an artifact of low-powered estimates. [15]

[Insert Table 8 about here]

Table 8 also demonstrates that of the seven states that passed revenue limits in our sample, not a single one appears to have significantly reduced revenue following the implementation of a TEL. In fact, the coefficients of the TEL variable indicate increased revenue collection more often than not. While several panel studies and cross-sectional studies have found TEL effects, neither our differences-in-differences nor our state-by-state designs reveal much evidence that TELs, in and of themselves, have affected state fiscal policy at all. If it were true that TELs had an effect, it should have manifested itself in these tests. We now turn to a "hunting" expedition, in which we will impose even stronger assumptions on the data in order to be as generous as possible to the hypothesis that TELs exert an effect.

VI. Matching Structural Breaks to TEL Implementation

It is possible that TEL effects do not occur immediately in the first year in which the measure is implemented. Here, we allow the timing of a potential TEL effect to vary around that year. If a TEL systematically altered state fiscal activities, either suddenly or gradually, its implementation would coincide with a structural break in the economic time series.

The case of Idaho exemplifies why structural break analysis is important. Figure 1 graphically presents Idaho's state and local spending per-capita relative to the spending of all states that did not adopt a TEL. As is clear in the figure, Idaho indeed experienced a structural break that decreased spending, but this was several years prior to TEL implementation.

[Insert Figure 1 about here]

To test for this structural breaks, we use the Clemente, Montañés, Reyes (1998) unit root tests with one structural break. To account for the possibility of another event that significantly affected fiscal activity more than a TEL, such as the passage of a property tax limitation act or the enactment of the income tax, we also implement the Clemente, Montañés, Reyes (1998) unit root tests with two structural breaks. Fundamentally, in searching for a unit root, we are searching for a change in trend, or a change in the generating function for a time-series. Using first differences would make finding such a break problematic. Thus for this test, we use the undifferenced fiscal behavior for each state in each year as our dependent variable.

Each test for structural breaks is implemented using both the Additive-Outlier (AO) model and the Innovative-Outlier (IO) model proposed by Perron (1989). The IO model is useful to demonstrate a gradual shift in the mean occurring slowly over time. The AO model is useful to demonstrate a break that occurs abruptly, like a crash. For full specifications of each model see Perron and Vogelsang (1992) and Clemente, Montañés, Reyes (1998). Each model utilizes an endogenous selection procedure wherein the break date is selected when the t-statistic for testing unit roots is minimized.

Our findings show that the implementation of a TEL is rarely associated with a structural break in state fiscal activity. Only two out of twenty states with spending limits were identified as having structural breaks coinciding with the implementation of a TEL: Missouri and, again, Colorado. As shown in Figure 2, an abrupt structural break is evident in Colorado in the same years its TEL was implemented. This figure also shows that a gradual break in spending was identified in Missouri in the same year as its TEL implementation. Not surprisingly given the regression results, no structural breaks in state and local revenue per-capita coincide with the implementation of a revenue TEL.

[Insert Figure 2 about here]

Of the twenty states that passed spending limits, five had structural breaks in the exact fiscal year, or fiscal year prior to, passage of a TEL (but thus in each case at least a year prior to its implementation). Louisiana, Montana, New Jersey, Utah, and Washington all experienced such "early" structural breaks. This finding may speak to the anticipation of constituent demands for lower spending by state legislators, and thus suggests that TELs are endogenous to the state's spending "mood." However, it also shows endogeneity was not problematic in the other states. Nor does this finding lead to the conclusion that the TELs by themselves were effective. These results do suggest that perhaps TELs together with a certain constellation of political forces are needed for fiscal reform. More importantly, they suggest a reason why many cross-sectional studies have identified apparent decreases in spending linked to TELs: The enactment of fiscal limits may represent verification that a tax revolt has already occurred in a state, even though they are not powerful weapons in that revolutionary struggle.

If the passage of these limits – either by voters or by legislators themselves – is the symbol rather than the cause of a shift toward decreased expenditures, then their presence will be correlated with lower spending. Our time series analysis within each state, though, makes it clear that this correlation is indeed an artifact. We are not the first to point out this endogeneity and its potential consequences (See Reuben 1997, Shadbegian 1998). These arguments make it clear that endogeneity would act in favor of TELs being effective at decreasing state spending, and suggest that their real effects may only be discerned with a research design that takes endogeneity seriously. We have attempted to do so, and find that only in rare cases do TELs, in and of themselves, have bite.[16]

VII. Explaining Why TELs Fail to Reduce Spending

As we noted in the beginning of this paper, TELs are subject to a principal-agent dilemma. TELs attempt to lock in the preferences of a set of political principals, voters or legislators at one time, by constraining the future actions of potentially hostile agents, legislators at a later time. As shown in our empirical section, only in rare circumstances do these constraints work. We highlight a few ways in which state elected officials avoid the bite of TELs, often without repercussions due to inadequate monitoring.

We frame this explanation, following Kousser, McCubbins, and Rozga (2007), with an example from one of the famous cases of a high spending state, California. California's spending limit, Proposition 4, passed with 74.3% of the vote in a November, 1979 special election. The so-called Gann limit sets a cap on the expenditure of revenues from taxation, which increases over time based on a population plus inflation index. The key detail that hindered the Gann limit's impact on state spending, however, was that it limited only expenditures of revenues from taxation. The exclusion of non-tax revenues in the calculation of the Gann limit left many avenues open for the state to raise money that it could spend freely.

For instance, in fiscal year 1979 and years following, California witnessed a sharp increase in the portion of its revenues that it raised from charges and fees, which were categorized as non-tax revenues. In fiscal year 1969, charges and fees made up 18.1% of the state's revenue (compared to 22.4% in the average non-TEL state); by 1979, the state's figure increased to 23.3% (compared to 25.8% in the comparison group). By 1994, California's proportion, 31.6%, exceeded the comparable group average of 30.9%. Although this rise in taxes and fees is most likely also a byproduct of Proposition 13, the property tax limit and supermajority requirement passed in early 1978, it also worked as a method of adapting to the state's spending limit.

We test this hypothesis using regression analysis. We run ordinary least squares regressions individually in each state with the proportion of charges and fees as our dependent variable. Our estimation is as follows, with the dependent variable being the amount of charges and fees (CF) collected by state and local governments as a proportion of all general revenues. For an individual state at time *t* we estimate:

$$(CF_i) = \alpha + \beta_1 (TEL_t) + \gamma_2(SAP_t) + \gamma_3(EP_t) + \gamma_4(PCI_t) + \gamma_5(HD_t) + \gamma_6(SD_t) + \gamma_7(GD_t) + \gamma_8(TREND_t) + \varepsilon_i$$

[Insert Table 9 about here]

The results of these regressions, reported in Table 9, show that 20 of 23 states had in increase in charges and fees following their TELs. We combine both spending and revenue limits into a single table. Five of those states increased their proportion of charges and fees following the enactment of a tax and expenditure limit significantly at the 5% level. Two states, Utah and North Carolina, significantly reduced reliance on charges and fees. These results suggest that government responses to TELs warrant further analysis.

VIII. Where a TEL Worked: Colorado

Our evidence from Table 7 suggests that Colorado's TEL was the only limit that successfully reduced state and local expenditures. This section explores how and why Colorado's TEL was able to successfully reduce state expenditures. Colorado's Taxpayer's Bill of Rights (TABOR) initiative, passed in 1992,[17] has many of the characteristics that prior research would suggest are necessary to ensure its success. This constitutional amendment limits all taxes and revenues at the state and local level, and requires voter approval for any tax increases or to change TABOR itself. These provisions means that TABOR is quite strict in the letter of its law; most TELs limit only revenue *or* expenditures and half of all TELs limit only state fiscal activity, not local government as well. The caps on expenditures and revenues are indexed partially to inflation, which generally grows at a slower rate than previously popular TEL indexes such as personal income (National Conference of State Legislatures, 2005). In sum, the amendment's drafters appear to have learned from the mistakes of past TELs.

Political conditions in Colorado also generally bode well for the initiative's effectiveness. Colorado had a Republican-controlled legislature throughout the period of our study, but TABOR came into effect just when gubernatorial party control was beginning to shift. The state was moving from the era of moderate Democratic governors Dick Lamm (1974-1986) and Roy Romer (1986-1998) into conservative Republican Bill Owens' tenure. Presumably, a unified Republican government would be unlikely to attempt to circumvent the spirit of a TEL. Even though it was externally imposed by a citizen initiative, TABOR set limits that subsequent state lawmakers, especially after 1998, would want to follow.

The acid test of TABOR effectiveness, however, came in the years following the end of our empirical analysis when the spirit of TABOR began to be undermined. The Colorado legislature passed a higher education voucher plan that awarded money to students who then took the money to a state university or one of a few private universities participating in the plan. Prior to the higher education voucher plan, state money given directly to the universities was subject to TABOR revenue limits. However, with this change, money flowed to students and then to public universities, thus escaping the cap imposed by TABOR. This allowed state universities to raise tuition as they saw fit (Frates 2005). The design of this voucher plan is a classic example of how political agents attempt to work around the limits imposed by their principals, and suggests that even if TABOR appears successful on its face we have to be aware of how the state has increased tuition and/or fees in other areas that used to be funded by the state.

Finally, the epilogue to the TABOR story illustrates how spending limits can be easily undone when it is easy to amend the constitution. By 2005, anticipated cuts under TABOR were so severe that Colorado's Republican Governor Bill Owens, a fiscal conservative, backed a drive to call a five year "Timeout for TABOR" (Halper, 2005). Because Colorado has the initiative process, Owens was able to work with legislative leaders and the state's political establishment to place this temporary suspension of the state's spending cap on the November 1st, 2005 ballot as Proposition C. In an election that featured only half as much turnout as the contest in which TABOR first passed, Prop. C won with 52% of the vote while its companion Prop. D – which would have earmarked how state officials could spend their new money – failed (Brown, 2005). At least for the next five years, Colorado lawmakers will have great freedom to spend what they can raise, thanks to their ability to amend the state's constitution.

Conclusion

Based upon the logic of principal-agent relationships, we doubted that those who enact tax and spending limits would be able to constrain the future actions of lawmakers possessed of different goals and direct control of state purse strings. With only one exception, the data confirmed these doubts. The answer to our question of "Can state tax and expenditure limits effectively reduce spending and revenues?" is a clear "No." Records of spending and revenue patterns show that TELs have in almost every instance failed to constrain the size of government in American states.

Building on the existing literature, we took advantage of the relatively recent adoption of TELs in the states to estimate differences-in-differences models of their policy impact.. Instead of exclusively looking for a common TEL effect across states, we built towards a model that looked for an effect within each state individually. This method allows for varied effects of different TELs and enables us to hold constant the myriad factors which determine state spending or revenue patterns. We have argued that cross-sectional analyses fall short in this regard because they are unable to successfully hold constant all the relative differences that effect state spending and are subject to selection biases. Using this method, our full multivariate analyses show that in only one state, Colorado, was there a significant indication that a TEL succeeded in reducing state and local spending per-capita. There was some level of support for the hypothesis that Missouri's TEL also constrained state fiscal policy. Additionally, no state with a revenue limit successfully reduced state and local revenues per-capita. As these results do not greatly surpass what we would expect to find by chance alone, we conclude that TELs in and of themselves are not effective mechanisms for budgetary control.

We believe that our findings have significant implications for a wider literature. As noted, many TELs are passed through the initiative process. The inability of voters (principals) to constrain their legislators and governors (agents) in this instance casts doubt upon the overall effectiveness of the initiative process. As noted in Gerber et al. 2001, "initiatives do not implement or enforce themselves" (Gerber et al. 2001 p. 109). We agree with this assertion and argue that officials in the vast majority of states have been able to circumvent the TELs that were intended to limit them.

Table 1. States Enacting Tax and Expenditure Limits, 1969-2001

	Spending Limit (year passed)	*Revenue Limit (year passed)*	*Constitutional or Statutory?*	*Tied to Income or Inflation?*
Alaska	1982		constitutional	inflation
Arizona	1978		constitutional	income
California	1979		constitutional	income
Colorado	1992	1992	constitutional	inflation
Connecticut	1991		statutory	greater of two
Florida		1994	constitutional	income
Hawaii	1978		constitutional	income
Idaho	1980		statutory	income
Louisiana	1993	1991	constitutional	income
Massachusetts		1996	statutory	state wages
Michigan		1978	constitutional	income
Missouri	1980	1996	constitutional	state revenues
Montana	1981		statutory	income
Nevada	1979, 1984		statutory	inflation
New Jersey	1990		statutory	income
North Carolina	1991		statutory	income
Oklahoma	1985		constitutional	inflation
Oregon	1979, 2001		statutory	income
		2000	constitutional	state revenues
Rhode Island	1992		constitutional	state revenues
South Carolina	1980		constitutional	greater of two
Tennessee	1978		constitutional	income
Texas	1978		constitutional	income
Utah	1989		statutory	both
Washington	1993	1979	statutory	inflation

Notes: Data on the presence and timing of TELs is taken from Poterba and Rueben (1999), and details of their constitutional status and indexing mechanism from National Conference of State Legislatures (2005). Colorado passed a statutory spending limit in 1991 and then a constitutional limit in 1992. Nevada's 1979 limit was nonbinding, according to Poterba and Rueben (1999), and Oregon's recent passages of both tax and expenditure limits are taken from National Conference of State Legislatures (2005). Because of Alaska's unique fiscal conditions – state revenues fluctuate dramatically with price changes in taxed natural resources – we exclude this state from our analyses below.

Table 2. Differences-in-Differences Estimate of the Effect of Spending Limits

Dependent Variable: Changes in Spending Per-Capita		
	State and Local	**State Only**
Spending Limit	-0.186	-0.0768
	(0.13)	(0.11)
Elderly Population Rate	0.813	-6.890
	(9.96)	'(9.87)
School Age Population Rate	-11.82	-8.531
	(7.69)	(6.69)
Per-Capita Income	0.00943	0.00448
	(0.0088)	(0.0085)
Changes in Elderly Population Rate	-117.9**	-53.69
	(49.9)	(39.4)
Changes in School Age Population Rate	-22.91	-23.48
	(26.0)	(22.9)
Changes in Per-Capita Income	0.0667***	0.0247
	(0.019)	(0.017)
Democratic Governor	0.0302	0.0585
	(0.070)	(0.061)
Democratic Lower House	-0.0546	-0.0657
	(0.13)	(0.13)
Democratic Upper House	0.179	0.207*
	(0.12)	(0.12)
State Fixed Effects	Included	Included
Year Fixed Effects	Included	Included
Constant	1.978	2.657
	(3.12)	(3.09)
Observations	1420	1420
Number of States	49	49
R-squared	0.33	0.29
Panel Corrected Standard errors in parentheses *** $p<0.01$, ** $p<0.05$, * $p<0.1$		

Table 3. Differences-in-Differences Estimate of the Effect of Revenue Limits

Dependent Variable: Changes in State and Local Revenue Per-Capita	
Revenue limit	-0.244
	(0.17)
Elderly Population Rate	-3.963
	(9.29)
School Age Population Rate	-15.84**
	(7.89)
Per-Capita Income	0.00595
	(0.0100)
Changes in Elderly Population Rate	12.79
	(68.7)
Changes in School Age Population Rate	-42.63
	(30.8)
Changes in Per-Capita Income	0.0494**
	(0.021)
Democratic Governor	0.127
	(0.080)
Democratic Lower House	-0.133
	(0.14)
Democratic Upper House	0.240*
	(0.14)
State Fixed Effects	Included
Year Fixed Effects	Included
Constant	3.003
	(3.05)
Observations	1420
Number of States	49
R-squared	0.26
Panel Corrected Standard errors in parentheses *** p<0.01, ** p<0.05	

Table 4. Types of TELs, by legal provisions

Type	Constitutional	State and Local	Inflation	Spending Limits	Revenue Limits
1	No	No	No	LA, MT, NC	WA
2	Yes	No	No	TX, CT, RI, SC, OK	FL, MO
3	Yes	Yes	No	AZ, CA, TN, HI, MO	MI
4	No	Yes	No	ID, NJ, OR	MA
5	No	No	Yes	NV	
6	No	Yes	Yes	WA, UT	
7	Yes	Yes	Yes	CO	CO

Table 5. Differences-in-Differences Estimates of the Effects of Spending Limits, by type

Dependent Variable: Changes in Spending Per-Capita	State and Local	State Only
Type 1 Spending Limit (NC, S, O)	0.00943	0.144
	(0.23)	(0.19)
Type 2 Spending Limit (C, S, O)	-0.379**	-0.365**
	(0.17)	(0.17)
Type 3 Spending Limit (C, SL, O)	-0.432	0.148
	(0.41)	(0.17)
Type 4 Spending Limit (NC, SL, O)	-0.309	-0.0408
	(0.32)	(0.48)
Type 5 Spending Limit (NC, S, I)	-0.439	-0.805
	(0.73)	(0.47)
Type 6 Spending Limit (NC, SL, I)	-0.0114	0.0579
	(0.46)	(0.29)
Type 7 Spending Limit (C, SL, I)	-0.593	-0.0259
	(0.41)	(0.35)
Elderly Population Rate	0.984	-6.194
	(9.79)	(9.62)
School Age Population Rate	-11.31	-8.720
	(8.21)	(6.96)
Per-Capita Income	0.0111	0.00517
	(0.0088)	(0.0083)
Changes in Elderly Population Rate	-112.8**	-52.81
	(51.3)	(40.6)
Changes in School Age Population Rate	-21.88	-22.28
	(26.9)	(23.0)
Changes in Per-Capita Income	0.0659***	0.0237
	(0.019)	(0.017)
Democratic Governor	0.0247	0.0482
	(0.071)	(0.062)
Democratic Lower House	-0.0508	-0.0573
	(0.13)	(0.13)
Democratic Upper House	0.172	0.195*
	(0.12)	(0.12)
State Fixed Effects	Included	Included
Year Fixed Effects	Included	Included
Constant	1.651	2.527
	(3.19)	(3.04)
Observations	1420	1420
Number of States	49	49
R-squared	0.33	0.29
Panel Corrected Standard errors in parentheses *** p<0.01, ** p<0.05		

Note: C: Constitutional, NC: Non-Constitutional, SL: State and Local, S: State Only, I:Pegged to Inflation, O: Pegged to Other mechanism

Table 6. Differences-in-Differences Estimates of the Effects of Revenue Limits, by type

Dependent Variable: Changes in State and Local Revenue Per-Capita	
Type 1 Revenue Limit (NC, S, O)	0.00594 (0.42)
Type 2 Revenue Limit (C, S, O)	-0.140 (0.31)
Type 3 Revenue Limit (C, SL, O)	-0.427 (0.41)
Type 4 Revenue Limit (NC, SL, O)	-0.447 (0.39)
Type 7 Revenue Limit (C, SL, I)	-0.0343 (0.37)
Elderly Population Rate	-3.353 (9.37)
School Age Population Rate	-16.23** (7.89)
Per-Capita Income	0.00639 (0.010)
Changes in Elderly Population Rate	15.61 (68.9)
Changes in School Age Population Rate	-42.57 (30.9)
Changes in Per-Capita Income	0.0489** (0.021)
Democratic Governor	0.122 (0.081)
Democratic Lower House	-0.133 (0.14)
Democratic Upper House	0.242 (0.14)
State Fixed Effects	Included
Year Fixed Effects	Included
Constant	2.942 (3.06)
Observations	1420
Number of States	49
R-squared	0.26
Panel Corrected Standard errors in parentheses *** p<0.01, ** p<0.05	

Table 7. State-by-State Estimates of the Effects of Spending Limits

	Changes in State and Local Per-Capita Spending		Changes in State Only Per-Capita Spending	
	Coefficient (Standard Error)	R-Squared	Coefficient (Standard Error)	R-Squared
Arizona	-1.45(1.31)	0.45	-0.39(0.99)	0.53
California	-1.21(1.54)	0.45	0.19(1.24)	0.46
Colorado	3.07(1.04)**	0.56	-1.34(.22)	0.51
Connecticut	0.10(1.41)	0.76	0.09(2.00)	0.51
Hawaii	3.87(2.93)	0.43	4.24(2.65)	0.39
Idaho	1.3(0.92)	0.78	-0.10(0.96)	0.39
Louisiana	1.46(1.58)	0.34	-1.19 (1.32)	0.26
Missouri	-0.95(1.51)	0.34	-0.16 (1.04)	0.35
Montana	0.89(1.88)	0.30	-0.33 (1.16)	0.27
Nevada	-1.57(3.65)	0.30	1.29 (2.47)	0.15
New Jersey	0.53(3.01)	0.32	-1.90(5.76)	0.19
North Carolina	-1.07(0.72)	0.69	0.35 (0.70)	0.40
Oklahoma	0.09(1.70)	0.11	-1.83(1.42)	0.54
Oregon	-1.61(3.22)	0.27	0.08(2.58)	0.30
Rhode Island	-1.64(1.85)	0.23	-3.76(0.25)	0.22
South Carolina	0.69(1.44)	0.41	0.16(0.96)	0.42
Tennessee	-2.30(1.59)	0.43	-0.81 (1.37)	0.19
Texas	0.31(1.07)	0.34	0.98 (0.81)	0.37
Utah	-2.88(1.69)	0.56	-0.40 (1.49)	0.27
Washington	0.05(1.83)	0.48	-0.30 (1.49)	0.42

*Notes: Each line presents coefficients and standard errors for a TEL indicator variable, taken from a separate time-series regression estimated in each state and controlling for a linear annual trend, per capita income, population % aged 5 to 17, population % aged 65 and over, changes in per capita income, changes in population % aged 5 to 17, changes in population % aged 65 and over, and dummy variables for democratic control of the governor's office, lower legislative house, and upper legislative house. A * indicates that the estimated coefficient is significant at the 5% confidence level. Full regression results available at http://mccubbins.ucsd.edu.*

Table 8. State-by-State Estimates of the Effects of Revenue Limits

	Changes in State and Local Per-Capita Revenue	
	Coefficient (Standard Error)	**R-Squared**
Colorado	0.25 (1.66)	0.40
Florida	-0.66 (1.44)	0.58
Louisiana	-0.23 (1.63)	0.22
Massachusetts	-3.12 (2.02)	0.48
Michigan	-2.21 (1.87)	0.45
Missouri	1.47 (1.58)	0.44
Washington	1.84 (2.06)	0.61

*Notes: Each line presents coefficients and standard errors for a TEL indicator variable, taken from a separate time-series regression estimated in each state and controlling for a linear annual trend, per capita income, population % aged 5 to 17, population % aged 65 and over, changes in per capita income, changes in population % aged 5 to 17, changes in population % aged 65 and over, and dummy variables for democratic control of the governor's office, lower legislative house, and upper legislative house. A * indicates that the estimated coefficient is significant at the 5% confidence level. Full regression results available at http://mccubbins.ucsd.edu.*

Table 9. State-by-State Estimates of the Effects of TELs on Charges and Fees

	DV: Charges Fees as a proportion of General Revenue		
	Coefficient (se)		**R-Squared**
Arizona	0.030	(0.021)	0.88
California	0.016	(0.015)	0.96
Colorado	-0.017	(0.011)	0.95
Connecticut	-0.012	(0.011)	0.92
Florida	-0.0116	(0.012)	0.96
Hawaii	0.038	(0.021)	0.77
Idaho	0.01	(0.014)	0.94
Louisiana	-0.009	(0.017)	0.93
Massachusetts	0.013	(0.008)	0.98
Michigan	0.028**	(0.011)	0.96
Missouri	0.012	(0.009)	0.95
Montana	0.075**	(0.037)	0.94
Nevada	0.052**	(0.01)	0.92
New Jersey	0.01	(0.009)	0.96
North Carolina	-0.02**	(0.008)	0.96
Oklahoma	0.041**	(0.010)	0.79
Oregon	0.009	(0.021)	0.95
Rhode Island	-0.028	(0.015)	0.94
South Carolina	0.023**	(0.012)	0.96
Tennessee	0.007	(0.016)	0.91
Texas	0.007	(0.01)	0.95
Utah	-0.065**	(0.022)	0.72
Washington	-0.002	(0.008)	0.87

*Notes: Each line presents coefficients and standard errors for a TEL indicator variable, taken from a separate time-series regression estimated in each state and controlling for a linear annual trend, per capita income, population % aged 5 to 17, population % aged 65 and over, changes in per capita income, changes in population % aged 5 to 17, changes in population % aged 65 and over, and dummy variables for democratic control of the governor's office, lower legislative house, and upper legislative house. A ** indicates that the estimated coefficient is significant at the 5% confidence level. Full regression results available at http://mccubbins.ucsd.edu.*

Figure 1. State and Local Spending Per-Capita in Idaho

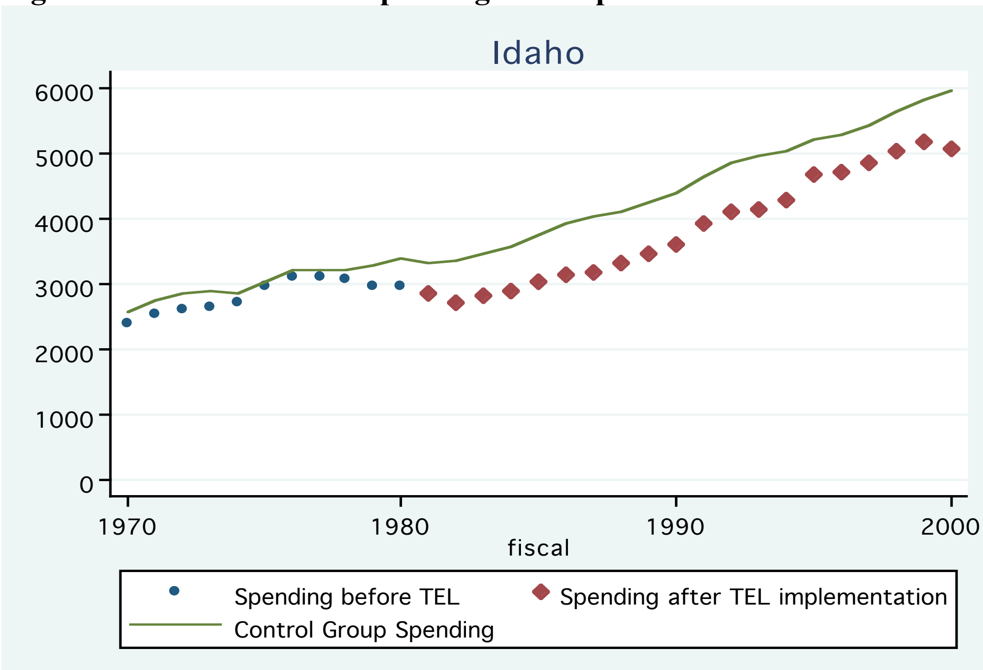
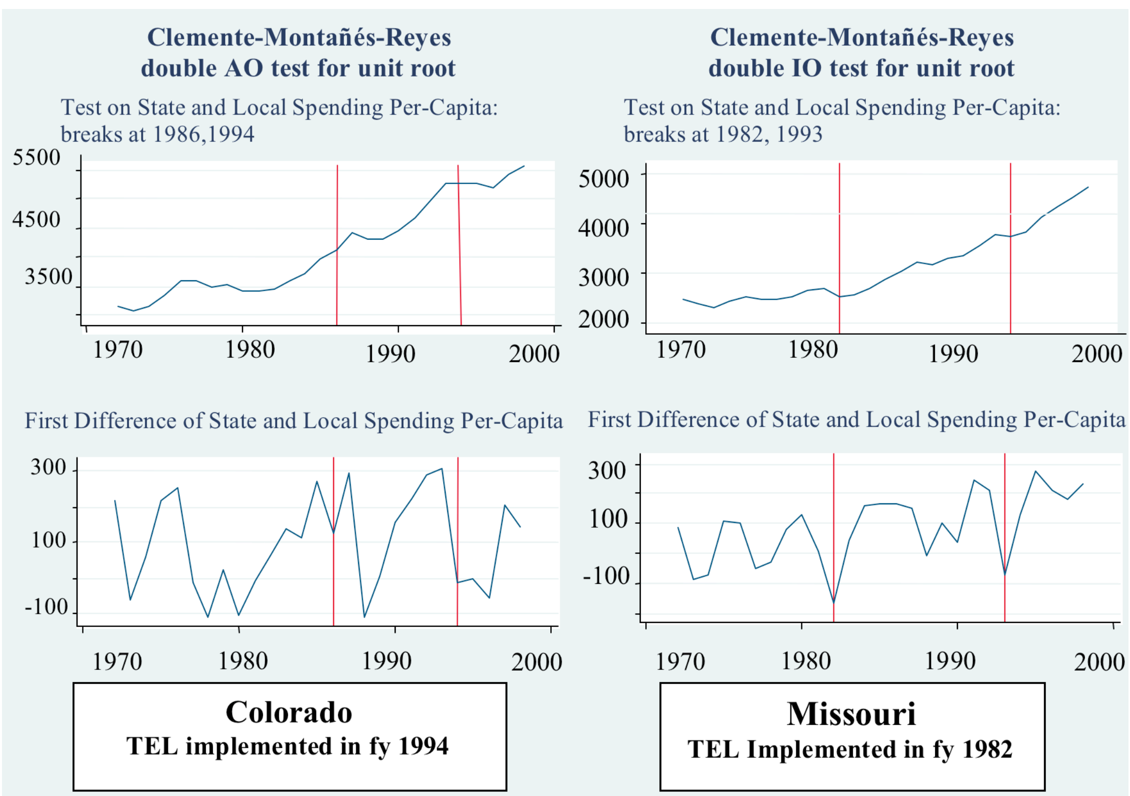


Figure 2. Structural Breaks in Colorado and Missouri



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[1] On agency in general, see for example Alchian and Demsetz (1972), Ross (1973), Holstrom (1979), Grossman and Hart (1983), and Bernheim and Whinston (1986). The problem could also be that there are multiple principals and that the principals change over time, a social choice problem. In either event, it is a hard problem.

[2] Smith (1998) demonstrates that this was the intent of most TEL proponents. One of the authors of California's 1979 spending limit, Craig Stubblebine, reports that while some proponents of TELs aimed to shrink the size of state government overall, others (such as himself) merely wanted to limit tax revenues so that residents would pay more fees only for the services they choose to use. This is an example of multiple principals with conflicting preferences, a condition that can make principal-agent problems even more difficult to overcome. (Telephone interview conducted by Thad Kousser on March 25, 2005).

[3] We find evidence of this as well. We compare states considered "overspenders" at the time their TEL was passed to all "overspending" states that never passed TELs. Overspending is operationalized as per-capita spending more than the average of our comparable group of the 24 states that never passed TELs. The "overspending" states that passed TELs consistently spend less than those that did not pass TELs, even before any TELs were passed. This is evidence that TEL states are different than non-TEL states in systematic ways, even before the TELs were passed. Further explanation of this analysis and a useful graph are available at http://mccubbins.ucsd.edu.

[4] For important exceptions to this approach, see Stansel (1994) and New (2001), which feature specifications that allow different types of TELs to have distinct effects. Stansel (1994) finds that TELs are more likely to be effectively when they apply to all types of spending or revenues (rather than taxes only), and when they require voter approval to override. New (2001) shows that TELs linked to personal income growth are less effective than those tied to inflation, and finds suggestive evidence that limits that are passed by legislatures themselves are less effective than those imposed by initiatives.

[5] In this paper, we do not make conjectures about the effectiveness of TELs that only apply to specific types of taxes, such as property to sales tax limits. It may very well be that specific TELs are more effective than general TELs, perhaps due to ease of monitoring (Gerber et al. 2001).

[6] The data and coding used for the analysis in this paper are available upon request from the authors or can be downloaded at http://mccubbins.ucsd.edu. We are grateful to the research assistance of Geoffrey Peppler in assembling this dataset. We gathered data for fiscal years 1997-2000 from the Census' website, http://www.census.gov/govs/www/estimate.html, accessed in March, 2005. The data from fiscal years 1969-1996

was recorded from hard copies of the census publication. Unfortunately, the Census does not report state-by-state detailed spending and revenue figures from fiscal year 2001, ending our time series.

[7] The implicit price deflator is commonly used for government financial data. This data is available at http://research.stlouisfed.org/fred2/data/GDPDEF.txt

[8] Analysis is not run on state-only revenue due to lack of data availability.

[9] Population sources from the Census Bureau: Intercensal Estimates of the Resident Population of States, 1970 to 1980. http://www.census.gov/popest/archives/pre-1980/e7080sta.txt; Resident Population for Selected Age Groups: 1980 to 1989, http://www.census.gov/popest/archives/1980s/estage80.txt; Population Estimates for the U.S., Regions, and States by Selected Age Groups and Sex: Annual Time series, July 1, 1990 to July 1, 1999 (includes revised April 1, 1990 population counts) http://www.census.gov/popest/archives/1990s/ST-99-09.txt; Single Year of Age and Sex Population Estimates: April 1, 2000 to July 1, 2004 http://www.census.gov/popest/states/files/SC-EST2004-AGESEX_RES.csv

[10] We also run this model without levels on the left-hand side of the equation. Theoretically, this suggests that legislators do not take account of current levels of expenditure when making year adjustments. In this model, the TEL variable falls just short of significance at the 5% level. Results are available at http://mccubbins.ucsd.edu.

[11] We also test for an immediate effect of TELs by using a first-differenced version of our treatment variable. This specification of the variable is never significant. These results are available at http://mccubbins.ucsd.edu.

[12] Poterba and Reuben 1998 identify Louisiana as passing a 1991 binding revenue limit. Unfortunately, we were not able to readily locate the legal provisions for this TEL so it is excluded from this analysis.

[13] TELs fare worse in an analysis of an immediate effect on spending, not reported here. No type of TEL was significant at the 5% level in the analysis on either state only nor state and local spending.

[14] Regressions were also run controlling for the effects of federal aid. We do not include aid in our primary model because it is an endogenous variable to state spending due to the practice of federal matching. The inclusion of aid in our model has the following effects: The Louisiana spending limit positively and significantly affects state and local spending. The Rhode Island spending limit negatively and significantly affects state and local spending. The Massachusetts revenue limit negatively and significantly reduces state and local revenue.

[15] We also estimated these models with a first-differenced version of the treatment variable. This variable, coded as 1 in the first year of TEL implementation and 0 in all other years, would capture any abrupt changes in spending or revenue that occur immediately when the TEL was implemented. This variable had a significant and negative coefficient in only one case, South Carolina.

[16] As a simple test of the endogeneity hypothesis, we test for direction of causality as derived by Granger (1969) and popularized by Sims (1972). The Granger Causality test involves using a series of F-tests to test whether lagged information on a variable Y provides any statistically significant information about a variable X in the presence of lagged X. In this case, our Y variable is total state and local spending per-capita and our X variable is the dummy variable indicating implementation of a TEL. In only one state, California, do we find a significant relationship between both variables, meaning Granger-causality runs in both directions. Spending affects the presence of the TEL in only 2 states, Washington and Rhode Island. Finally, there is modest indication that a TEL Granger-causes spending in Arizona, Louisiana, and Tennessee.

[17] The Colorado General Assembly did pass a spending limit in 1991 (National Conference of State Legislatures, 2005), but since TABOR came in the next year, it is not possible to measure the impact of this TEL.