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## The Effects of Tax or Expenditure Limits on State Governments

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THE EFFECTS OF TAX OR EXPENDITURE LIMITS  
ON STATE GOVERNMENTS

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## I. Introduction

In the late 1970s the term "tax revolt" became a standard part of the vocabulary of politics. The most widely noted skirmish in this uprising was California's Proposition 13, a referendum to limit property tax revenue. At about the same time a number of states, including California, enacted the conceptually distinct and potentially more far-reaching limits on public sector budgeting generally referred to as tax- or expenditure-limits, or TELs. While property-tax limitations are manifestations of voter dissatisfaction with a particular type of tax, and seem to have been rather straightforward reactions to rapid increases in real estate prices (Oakland, 1979), TELs suggest a fairly widespread dissatisfaction with the overall size of the nonfederal public sector.

The sudden emergence of TELs as potential constraints on state governments raises a number of theoretical and empirical questions. Under what conditions might a majority of voters believe that their state's budget is too large? More than half of the TELs that were enacted were approved by the legislature rather than by a referendum or a constitutional convention. Why would a legislature adopt such a measure? Are TELs generally binding, or are they the rough equivalent of the ceiling on the national debt? Are there important differences between TELs that represent constitutional changes and those enacted as ordinary statutes? This paper presents some preliminary answers to these questions, but by no means can they be considered settled.

## II. The Logic of Budgetary Limits

Despite the potentially far-reaching effects of TELs, the literature of the period is rather vague about the logic underlying them. There appears

at least superficially to be one inherent contradiction in proposals for constraints on the budgetary process. If the budgets enacted by democratically elected representatives do not represent a majority-rule equilibrium, what reason is there to believe that a democratically chosen TEL will represent such an equilibrium? Alternatively, if the vote on a TEL does represent the preference of a majority, why does an unrestricted budget fail to do so?

A. Monopoly Government

Brennan and Buchanan (1979) resolve this contradiction by means of a model of monopoly government created by popular consent. Subject to the constraints agreed upon by the general population in designing a constitution, Brennan's and Buchanan's "Leviathan" government maximizes the difference between tax revenue and government-service provision. They postulate that this difference is proportional to the level of tax revenue, so that the government's problem reduces to maximizing gross revenues. They conclude (p. 14) that a TEL is a solution to the representative citizen's problem of how to "obtain the benefits of public goods supply without exposing himself to gross exploitation by government -- exploitation in the form of disastrously excessive tax burdens (and correspondingly excessive levels of public goods)."

The Leviathan model, then, resolves the apparent contradiction of a TEL by emphasizing the consensual nature of constitutions and the monopoly position of government once it is in operation. Such an approach seems to be unhelpful in explaining the passage of statutory TELs, or in the failure of the citizens of two-thirds of the states to recognize and exploit their mutual gains from restraining their governments. Brennan and Buchanan also

neglect the restraining effects of two different competitive forces in the public sector -- competition among governments (Tiebout, 1956) and competition for operating monopolies (Demsetz, 1968; see also Stigler, 1972).

If government budgets are determined in a competitive political setting, however, why are TELs ever enacted? Such instances seem to provide prima facie evidence that democratically elected legislatures do not costlessly perceive and maximize the utility of the median voter.

#### B. Budget Limits as Signals

The simplest explanation of TELs is that they serve as signals during periods of change in the equilibrium size of the budget. Thus if demographic or economic variables in a state warrant a long-term decline in the rate of growth of total state spending, a citizen-initiated TEL could serve as a signal to the legislature if the changes in those variables are first perceived or anticipated by the electorate. Similarly, a statutory TEL could serve as a signal to voters that such changes have been recognized and will be acted upon in the future. In either case, fundamental changes in the budget can be obtained without correspondingly sweeping changes in the legislature. This is clearly desirable to incumbent legislators, so the existence of statutory TELs is not as mysterious as it may seem at first glance. Voters may also prefer to retain incumbent representatives (Bernhardt and Ingberman, 1985), so they would prefer a TEL to a wholesale turnover of the legislature. Budget limits that act only as signals, then, serve to reduce the costs of changes in political equilibrium, but they do not deflect public-sector spending from the long-run path it would have followed in the absence of those limits.

### C. Agency Costs

A third possible explanation for the passage of a TEL arises from the nature of budgetary equilibrium when voters (or perhaps legislators) have imperfect control over the behavior of their agents in the budget process. Niskanen's (1971) model of budget-maximizing bureaucrats is based on the scarcity of information available to legislators about the operations of the bureaus to which they have delegated day-to-day decision-making authority. Legislators or citizens, aware of the budgetary implications of this information asymmetry, might easily find that a limit on total appropriations is preferable to setting an arbitrary limit on the budget of each bureau. Under a restriction on total spending, each bureau competes with other bureaus for additional funds, so that the incentives to produce information about the demand for the services provided are not as attenuated as in the case of a fixed amount of spending per bureau.

Similar logic applies to the agenda-setter model of Romer and Rosenthal (1980), although less straightforwardly. In their model, the principal is not a legislator trying faithfully to represent the median voter, but rather the median voter himself, with legislators acting as wayward, budget-maximizing agents. Whereas Niskanen's model implies that a legislature might impose a binding TEL on itself, Romer and Rosenthal's does not. Moreover, unless a voter referendum on a proposed TEL is initiated and specified by citizens, it is likely to be either so harsh as to be rejected by the median voter or so accommodating as to be completely ineffective. Romer and Rosenthal (1982) present evidence in support of their model that is based on the results of school expenditure referenda.

A somewhat subtler aspect of agency costs is suggested by Barro (1973). In that model legislators, viewed as imperfectly controlled agents of the

voters, do not prefer a higher level of spending per se. Instead, they find it optimal to cast some of their votes on appropriations and other issues in accordance with the interests of well-organized interest groups rather than the interests of the median voter. Because of lobbying during the legislative session, the budget that is adopted is different from the budget that would be approved by a popular referendum (assuming that a unique majority-rule equilibrium budget exists). As long as the costs involved in voting directly on all budgetary issues exceed the losses that the legislative budgeting process imposes on the members of the coalition that would prevail under direct voting on the budget, the legislatively-determined budget is a second-best optimum for the voters. Clearly, however, a method for controlling this shirking by legislators that entailed low enough costs would be readily adopted by the voters. Under certain conditions, a limit on budget size may be one such method, as will now be shown.

Consider the following model of budget determination by a vote-maximizing legislature. Let there be two groups, the members of which value private consumption and one governmentally provided good. The members of Group 1 are organized sufficiently to make campaign contributions to incumbent legislators, who in turn spend their contributions to increase their votes from the members of Group 2. The level of contributions made by Group 1 ( $C_1$ ) in turn depends on the amount of  $G_1$ , its members' preferred public good, that its members receive, and on their private consumption, which is negatively related to their tax liability ( $T_1$ ). The model therefore consists of vote functions for Groups 1 and 2,

$$(1a) \quad V_1 = V_1(G_1, T_1) \quad \text{and}$$

$$(1b) \quad V_2 = V_2(G_2, T_2, C_1)$$

where  $V_1$  and  $V_2$  are the votes cast for the incumbents by the members of the two groups; a contribution function for Group 1,

$$(2) \quad C_1 = C_1(G_1, T_1) ,$$

two tax functions,

$$(3a) \quad T_1 = T_1(G_1 + G_2) \quad \text{and}$$

$$(3b) \quad T_2 = T_2(G_1 + G_2) ,$$

where the units of  $G_1$  and  $G_2$  are defined so that the (constant) marginal cost of each is equal to unity, and a budgetary balance constraint,

$$(4) \quad G_1 + G_2 \leq T_1 + T_2 .$$

A budget consists of an allocation  $(G_1, G_2)$  and tax functions  $T_1(\cdot)$  and  $T_2(\cdot)$ . The legislature is assumed to choose the budget

$(T_1^*(G_1^*, G_2^*), T_2^*(G_1^*, G_2^*))$  that maximizes  $V_1 + V_2$  subject to equations (1a), (1b), (2) and (4). For simplicity, assume that the tax functions  $T_1^*(\cdot)$  and  $T_2^*(\cdot)$  are predetermined, so that in a given legislative session budgetary choices are concerned solely with selecting the optimal expenditure vector  $(G_1^*, G_2^*)$ . Assuming that Group 1 lobbies for higher  $G_1$  and lower  $T_1$ , but not for lower  $G_2$  directly, the first-order conditions imply

$$(5) \quad \frac{\partial V_1}{\partial G_1} + \frac{\partial V_1}{\partial T_1} \frac{\partial T_1}{\partial G_1} + \frac{\partial V_2}{\partial T_2} \frac{\partial T_2}{\partial G_1} + \frac{\partial V_2}{\partial C_1} \left[ \frac{\partial C_1}{\partial G_1} + \frac{\partial C_1}{\partial T_1} \frac{\partial T_1}{\partial G_1} \right] -$$

$$\frac{\partial V_2}{\partial G_2} + \frac{\partial V_2}{\partial T_2} \frac{\partial T_2}{\partial G_2} + \frac{\partial V_1}{\partial T_1} \frac{\partial T_1}{\partial G_2} + \frac{\partial V_2}{\partial C_1} \frac{\partial C_1}{\partial T_1} \frac{\partial T_1}{\partial G_2} .$$

For purposes of comparison, suppose that Groups 1 and 2 have identical voting functions and marginal tax rates, so that if there were no role for contributions to play each group would receive equal government budget



shares. Given Group 1's assumed superiority at lobbying, the legislature's marginal rate of substitution between  $G_1$  and  $G_2$  takes the form

$$(6) \quad MRS_{1,2} = \frac{a + \frac{\partial V_1}{\partial G_1} + \frac{\partial V_2}{\partial C_1} \frac{\partial C_1}{\partial G_1}}{a + \frac{\partial V_2}{\partial G_2}},$$

so that Group 1's equilibrium budget share is larger than would be the case in the absence of agency costs.

A budget limitation represents a possible way for the legislature to commit itself to a smaller budget than would be the outcome of the process modeled above. The question to be answered is whether it could ever be in the interest of the members of Group 2 to vote for a TEL. To answer the question, let us modify the budgetary model to represent the case of a binding limit on total spending. In this circumstance, the corresponding equations are:

$$(1a') \quad V_1 = V_1(G_1)$$

$$(1b') \quad V_2 = V_2(G_2, C_1)$$

$$(2') \quad C_1 = C_1(G_1)$$

$$(3') \quad T_1 = T_2 = \frac{1}{2}\bar{B}$$

$$(4') \quad G_1 + G_2 \leq \bar{B}$$

and the legislature's marginal rate of substitution is

$$(6') \quad MRS'_{1,2} = \frac{\frac{\partial V_1}{\partial G_1} + \frac{\partial V_2}{\partial C_1} \frac{\partial C_1}{\partial G_1}}{\frac{\partial V_2}{\partial G_2}}.$$

Since the additional terms in equation (6) are equal to

$$a = \frac{\partial V_1}{\partial T_1} \frac{\partial T_1}{\partial G_1} + \frac{\partial V_2}{\partial T_2} \frac{\partial T_2}{\partial G_1} + \frac{\partial V_2}{\partial C_1} \frac{\partial C_1}{\partial T_1} \frac{\partial T_1}{\partial G_1} < 0$$

, it follows that the legislature's isovote loci under a TEL are flatter at any point in  $(G_1, G_2)$  space than are the corresponding loci under an unrestricted budgetary process. Figure 1 illustrates equilibrium budget allocations in the absence of any restrictions (point A) and under an arbitrarily specified budget limit (point B). The relevant isovote loci for each case are  $V_{NT} V'_{NT}$  and  $V_T V'_T$  respectively, while the corresponding indifference curves of a member of Group 2 are  $U_{NT} U'_{NT}$  and  $U_T U'_T$ . It is apparent from the diagram that a TEL can benefit citizens such as the members of Group 2, but can it be expected to command a majority of votes? The answer depends on both the shape of the isovote loci when legislators can commit perfectly to a given course of action,  $MRS_{1,2} = -(\partial V_1 / \partial G_1) / (\partial V_2 / \partial G_2)$ . Figure 2 illustrates two possible isovote loci. Under one of them the TEL depicted would be approved (AA'); under the other (BB') it would not. It is clear from the diagram that the existence of a potentially successful TEL is more likely the greater is the excess of the unconstrained budget over the precommitment budget, the greater the differences between the constrained (equation 6') and the unconstrained (equation 6) isovote curves, and the less convex the isovote loci under precommitment.

To summarize, the implications of the three basic models of budget limits that have been considered are quite different. The Leviathan model predicts widespread adoption of TELs as citizen initiatives, and that those TELs will prove to be effective constraints on state governments. The model of TELs as signals suggests that they are likely to be enacted in states that are in the early stages of a period of substantial fiscal adjustment,

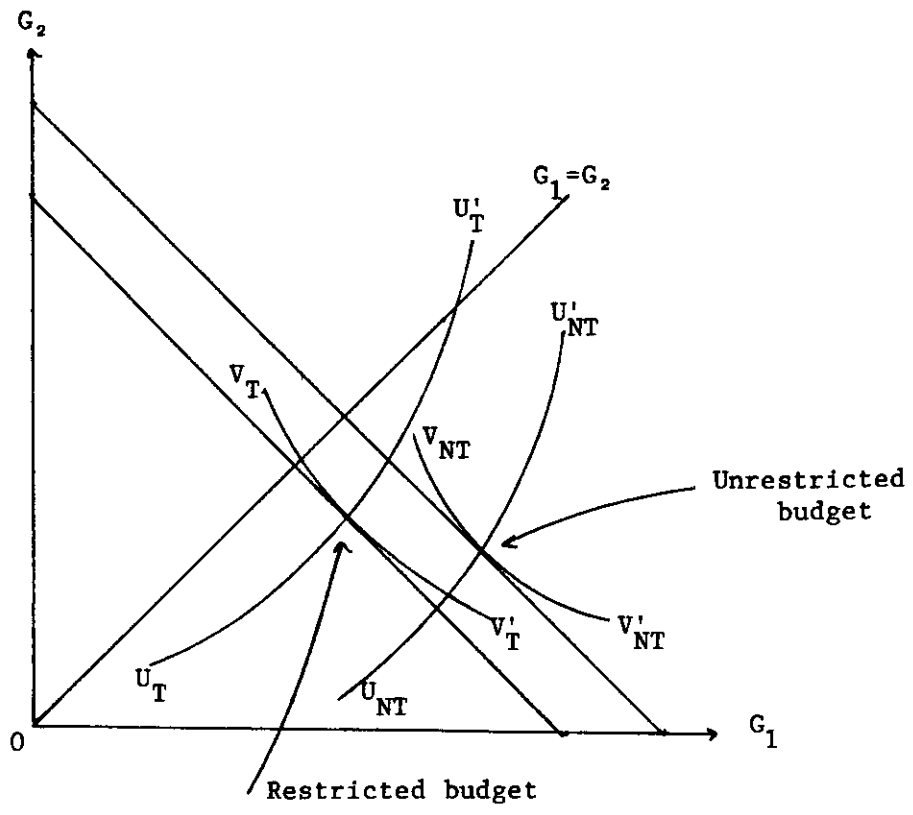


Figure 1

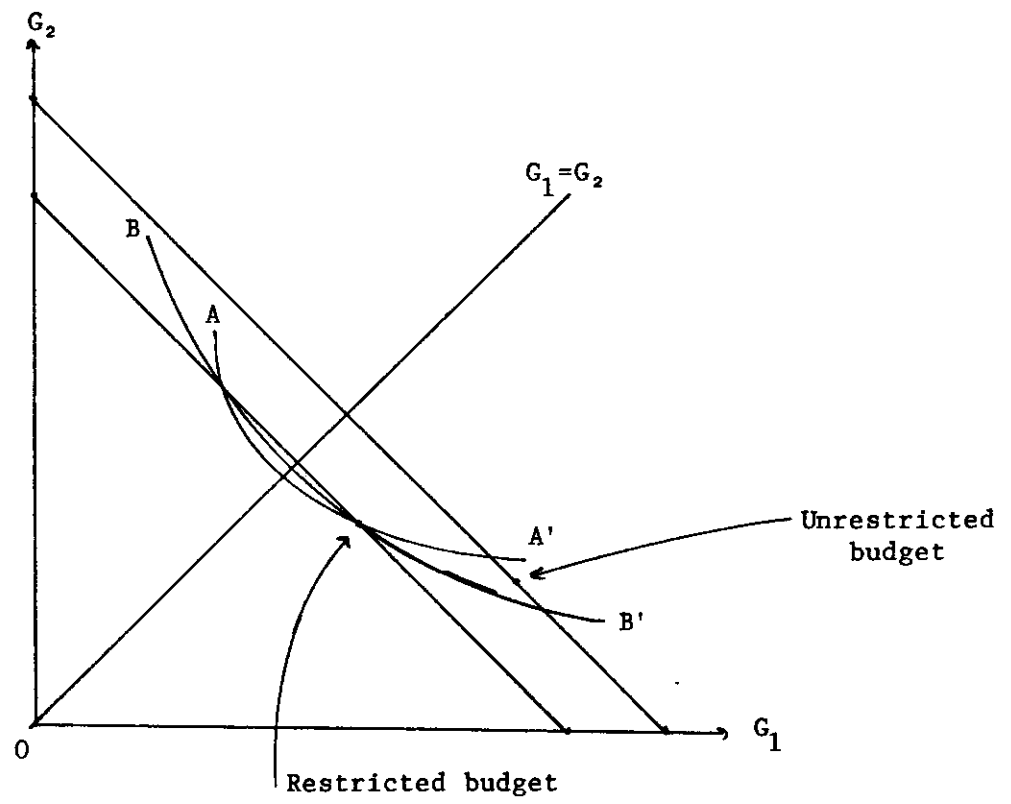


Figure 2

and that their effect will not be to alter the functional relationship between state spending and economic or demographic variables. The predictions of the various agency-cost models depend on the precise nature of the principal-agent relationship. The Niskanen model suggests widespread adoption of TELs by legislatures, and that those TELs will cause a clear reduction in spending. The agenda-setter model implies that although a variety of types of TELs may be approved, only citizen-initiated ones will be effective. Finally, the interest-group model predicts that, although only some or a few states may enact TELs, they may be enacted either by citizen initiatives or legislative action, and those that are enacted will be effective in reducing public spending.

### III. The Effects of Tax or Expenditure Limits

#### A. An Overview of Budget Limits

Compared to such fiscal reform measures as balanced-budget requirements, supermajority provisions for appropriations, or a line-item veto, expenditure limits are remarkably simple and direct. Rather than altering the process by which appropriations are made, TELs specify broad budgetary outcomes. The most common form of a TEL fixes spending or revenue as a fraction of state personal income; twelve states' limits take this form. Colorado's limit fixes the rate of growth of state spending at seven percent annually, while New Jersey's (which was allowed to expire in 1983) fixed the growth rate to that of per-capita state income. In California and Alaska, state expenditures may not grow at more than the sum of the rates of population growth and inflation.

Table 1 briefly characterizes the TELs enacted by the states analyzed in this paper. Several patterns are evident from that table. The year 1978 was a year of unusual citizen involvement in the design of what could be called their states' fiscal constitutions. It was the peak year for passage of TELs, and every limit adopted that year represented an amendment to the state's constitution. It was also the only year in which every limit that was enacted was approved by a referendum or a convention. There was also a distinct western aspect to tax or expenditure limitations in that year: only five of the 16 states in Table 1 were located east of the Mississippi.

Five states that have enacted budgetary limits are excluded from the sample analyzed in this paper. The legislatures of Nevada (in 1979) and Rhode Island (in 1977) approved limits on the governor's proposed general fund expenditures, but these are explicitly nonbinding, advisory measures. In 1979 the Utah legislature passed a limit on state appropriations that would have been one of the most stringent TELs passed anywhere, but it was never formally implemented as a budgetary procedure. Massachusetts (1986) and New Mexico (1987) adopted TELs too recently for any effects to be discernible.

There are several possible effects of a TEL on governmental budgets. The most likely effect is on the variable being directly controlled, the total size of the state government's budget. A limit that specifies a maximum total budget below what would otherwise prevail will necessarily exert an apparent effect. This effect can be mitigated in practice if government officials move some items off the budget in the case of an expenditures limit, or by replacing taxes with fees in the case of a tax limit. In California and Oregon, however, the limit itself is to be adjusted to offset such actions. Of course, the simplest reason why a TEL

might exert no effect on state budgets is that it is set at a nonbinding level.

The allocation of responsibility for public service provision to different levels of government may also be affected by a limit on a state's budget. For example, if state aid to education were reduced, some or all of that reduction could be offset by increased property taxes or other direct sources of revenue to local governments. However, the TELs in nine of the states listed in Table 1 contain provisions that attempt to offset or prevent the transfer of responsibility for programs to lower levels of government as an attempt to circumvent the limit. Clearly, however, a local government that seeks to offset a reduction in state-provided services will be able to do so.

The third possible effect of a TEL is to alter the shares of the state budget allocated to different programs, as illustrated in Figure 1 earlier. While the anticipation of such an effect may play a role in determining public support for a budget limit, it is not an aspect of TELs that is analyzed empirically in this paper.

#### B. Previous Empirical Findings

Owing perhaps to the relatively brief history of TELs in the United States, little prior investigation of their effects has been carried out. Bails (1982) estimated the probable effects of various types of TELs by calculating what their effects would have been had they been in force in earlier years. He found that if per-capita state expenditures had been subject to a limit holding them to their 1974 fractions of per-capita state income, only 19 of the 50 states would have been compelled to reduce their budgets in 1979. It also turns out that the states listed in Table 1 are

not disproportionately represented among those 19 states (seven of the 16 states in Table 1 would have exceeded such a TEL, compared to 12 of the remaining 34 states). However, it should also be noted that of the five states for which Bails found a statistically significant excess of actual 1979 state spending over his hypothetical limit, three were states that did adopt TELs. In any event, Bails did not attempt to compare the actual levels of spending in states with TELs either to the levels of spending in states without TELs or to the level of spending that would have prevailed without TELs in states that did adopt them.

As part of a study of the efficacy of several types of constitutional restrictions on governmental budgets, Abrams and Dougan (1986) estimated the effect on state expenditures of TELs that were enacted by amendments to the states' constitutions. They found no significant effect on total state spending and a marginal reduction in combined state and local spending, but noted that this could have been due to the endogeneity of the limits combined with the use of solely cross-sectional observations. For example, if TELs are enacted only in those states where public spending is a relatively large fraction of state income, then single-equation cross-section estimation may fail to estimate the full impact of those limits. The results reported in the next section represent an attempt to obtain more precise estimates of the effects of TELs than those that have been available until now.

### C. Time Series Evidence of the Effects of Budget Limits

To discern whether adoption of a TEL actually leads to a reduction in spending in a given state, I analyze the variation in the levels of

government spending over the period from 1960 to 1984. For each of the 16 states that enacted a TEL, I estimate the level of spending by the state, and by state and local governments combined, as a function of economic and demographic variables with some allowance for serial correlation in those variables. The test for a significant reduction in spending due to a TEL in each state is a one-tailed test of the significance of the estimated coefficient of a dummy variable equal to one in years in which a TEL is in effect and zero otherwise.

The expenditure function estimated for each state is a standard government-spending equation, for which the independent variables can be derived from either a median-voter model (Borcherding and Deacon, 1972; Bergstrom and Goodman, 1973) or an interest-group model of public budgeting (Abrams and Dougan, 1986). For each state, four basic variants of the spending equation were estimated, consisting of two alternative functional forms and two alternate dependent variables:

$$(7) \quad E_t = \alpha_0 + \alpha_1 P_t + \alpha_2 Y_t + \alpha_3 A_t + \alpha_4 S_t + \alpha_5 D_t + \alpha_6 T_t + \alpha_7 T_t^2 + u_t$$

and

$$(8) \quad E_t = \beta_0 + \beta_1 P_t + \beta_2 Y_t + \beta_3 A_t + \beta_4 S_t + \beta_5 D_t + \gamma(L)E_{t-1} + u_t,$$

where  $E_t$  represents either real state government expenditures per capita or combined real state and local expenditures per capita in year  $t$ . The other variables are population ( $P$ ), real state personal income per capita ( $Y$ ), real per-capita federal aid to the state government or to state and local governments combined ( $A$ ), real per-capita severance tax revenue ( $S$ ), a dummy variable equal to one in years in which a TEL was in effect and zero otherwise ( $D$ ), an index of time ( $T$ ), a polynomial in the lag operator ( $\gamma(L)$ ), and a mean-zero, normally distributed and serially independent random disturbance ( $u$ ).<sup>1</sup> The severance-tax variable is included because the



exportability of the burden of such taxes may induce a higher level of spending in states endowed with a greater capacity for levying such taxes. A measure of severance tax capacity is not available as a complete time series from 1960 to 1984, however, so actual severance tax revenues are used as a proxy for the severance tax base.

Each equation was estimated for a linear specification using weighted least squares to induce homoskedasticity in the residuals. The weight employed was the variable Y, state personal income per capita. The choice between specification (7) and (8) for each state was made on the basis of the standard error of the regression and the serial independence of the residuals. For specification (8), the Durbin-Watson test was used to test for serially correlated residuals. Inclusion of a lagged dependent variable causes the Durbin-Watson statistic to be biased toward rejection of the null hypothesis that the residuals are serially correlated. However, when several other independent variables are included, a D-W statistic above the critical value for rejecting the null hypothesis that the residuals are correlated is usually a reliable basis for failing to reject the null hypothesis that the residuals are not serially correlated (Johnston, 1972).

Tables 2a, 2b, 3a, and 3b report the estimated values of the parameters  $\alpha_1$ ,  $\alpha_2$ ,  $\alpha_3$  and  $\alpha_5$  for specification (7) or  $\beta_1$ ,  $\beta_2$ ,  $\beta_3$  and  $\beta_5$  for (8) as noted in each table. The estimates obtained from the lagged dependent variable specification are not the estimates of the  $\beta_1$  parameters, but rather of  $\beta_1[\gamma(L)]^{-1}$ , which estimates the long-run effect of each independent variable on  $E_t$ . The tables treat separately state expenditures and combined state and local expenditures, as well as states with constitutional or statutory tax or expenditure limits.

On the whole these equations appear to provide fairly good estimates of government expenditures in each state. In those states with insignificant reported parameter estimates, the  $R^2$ 's are still high due to the explanatory power of the unreported time trends (all the regressions explain 98 or 99 percent of the variation in  $E_t$ , so those numbers are not reported in the tables). Since the revenue and expenditure variables are all in per capita terms, the parameters of the population variable indicate the presence of scale economies (if negative) or congestion effects (if positive) in the provision of government services. The interpretation of the coefficients on all the variables depends on the particular functional form estimated. This is especially clear for the coefficients on per capita income. In those states for which a time trend specification is reported, the coefficient on income represents the effect of high-frequency movements in that variable. It is for this reason that the estimates of  $\alpha_2$  are usually negative, since expenditures on such items as unemployment insurance and welfare benefits are negatively related to the business cycle. In contrast, the estimates of  $\beta_2\gamma[(L)]^{-1}$  are positive, for they represent the long-run marginal effect of income on government expenditure. The coefficients on per-capita federal aid are generally as expected in sign and magnitude. Federal grants impart a greater stimulus to government spending than does personal income for two reasons. First, these grants exert a price effect when they are matching grants, as many are. Second, categorical grants have been shown to exert a "flypaper effect," meaning that even in the absence of any price effect state and local governments' marginal propensities to spend out of such grants are greater than their marginal propensities to spend out of community income (see Courant, Gramlich, and Rubinfeld, 1979; Inman, 1979; and Fisher, 1982). The results reported in the tables, therefore, appear to

generate reasonable estimates of what government spending levels would be in the absence of TELs. I now consider the implications of the estimates of the TEL coefficients reported in these tables.

A principal implication of the Brennan and Buchanan analysis of TELs is that they can be expected to reduce public spending because they are constitutional reforms imposed on legislatures. They state that "tax limitation proposals did not emerge out of normal political pressures. Interparty competition and parliamentary procedures apparently failed to secure policies that the electorate desired. ... Tax limitation is a constitutional affair" (1979, p. 13; emphasis in original). A brief comparison of the a and b versions of Tables 2 and 3 poses a substantial challenge to the Leviathan model. Of the eight states with constitutional TELs, evidence of a significantly negative effect of those limits is found in two of them. By contrast, the TEL dummies have significantly negative coefficients (at the .05 significance level) in five of the eight states with statutory limits. This pattern is consistent with the predictions of Niskanen's model and the interest-group model presented in Section II, in which there is no necessary distinction between constitutional and statutory TELs. Voters who find it costly to monitor each step in the complicated process of budgetary authorization will find it relatively cheap merely to monitor any votes by legislators to repeal or amend a TEL. Thus, the actions of self-interested legislators may be effectively circumscribed by either type of TEL.

The null hypothesis that the differences in the numbers of significant TEL coefficients between states with constitutional or statutory limits are due to chance cannot be rejected at conventional significance levels. Since the number of estimates is too small for the chi-square test, the

appropriate test is Fisher's exact test (Brownlee, 1965). The critical value for rejecting the null hypothesis in this case is in excess of .15.

Treating the two sets of states as one sample, then, I test whether the total number of significantly negative TEL coefficients is greater than what would be expected by chance when the test of significance is a one-tailed test at the .05 critical value. On the basis of Fisher's exact test, the calculated marginal significance level of the seven significant coefficients out of the 16 estimated for both state and combined state and local spending is about .04, indicating rejection of the null hypothesis that so many significant estimates were obtained by chance. The somewhat more straightforward test of joint significance of the TEL coefficients, estimating a single coefficient for the pooled sample of all 16 states, cannot be performed because of the inappropriateness of pooling these observations.

The estimates presented in Tables 2 and 3 suggest that as a general rule TELs can be expected to reduce government spending. These results are incomplete, however, because they do not allow for the possibility that the TEL variables are proxies for a general, nationwide change in some unobserved variable or variables that occurred at about the same time that most of the TELs were adopted. To analyze this possibility, I estimated equations (7) and (8) using weighted least squares for each of the 34 states that had not adopted a budget limit during the sample period. For this sample, the variable  $D_t$  was set equal to one for the year 1978 and every subsequent year. This year was chosen because it was the peak year for the passage of TELs. Table 4 reports the t-statistics for the estimated coefficients of  $D_t$  for each definition of government spending. The criteria for the choice of functional form were the same as for the sample of TEL

states, and the entries in Table 4 are the values obtained from the equation selected responding to those criteria.

Among the regressions of state-only expenditure on the independent variables, six yield significantly negative coefficients on the post-1977 dummy variable. Of the regressions specifying combined state and local spending as the dependent variable, eight estimate a significantly negative departure from the previous pattern of expenditure in the period during and after the tax revolt. However, only two of the states with significantly lower state and local spending also fall below their predicted levels of state-only expenditure during the same time period. In other words, in four of the six states in which state spending was lower than would have been expected on the basis of past experience, that decline in spending was fully offset by an increase in spending by local governments. In six of the eight states in which combined state and local spending fell below its predicted level, this decline was due solely to reductions in the level of local spending relative to the independent variables (except  $D_t$ ) included in the regressions. These patterns are in marked contrast to the results for the TEL states. Of the six states exhibiting significant effects of their TELs on state spending, five also manifested similar effects on state and local expenditure. In addition, only one of the six states exhibiting apparent negative effects of their TELs on combined state and local spending did not exhibit a similar effect on state-only expenditures.

These patterns suggest three hypotheses to be tested. The first is that there is no significant difference between the proportions of TEL and non-TEL states in which state-only spending fell relative to its predicted value. This hypothesis cannot be rejected at the .10 significance level.<sup>2</sup> The second test is that there is no significant difference between the

proportions of TEL and non-TEL states in which combined state and local spending fell relative to its conditioned expectation. This hypothesis cannot be rejected at the .20 significance level.<sup>3</sup> On the basis of these comparisons of states that adopted TELs and those that did not, there does not seem to be a significant difference between the budgetary choices made at the state or local levels during the sample period.

This inference is not supported by the third hypothesis tested, however. The null in this case is that there is no significant difference between the proportion of TEL and non-TEL states in which both state-level and state and local expenditures fell below their predicted levels after passage of a TEL or, alternatively, after 1977. The proportion of TEL states in which there was a significant decline in spending at both the state level and the combined state and local level relative to its expected magnitude after passage of the limit is 5/16, whereas in non-TEL states the comparable proportion after 1977 is 2/34. The difference between these two frequencies is significant at the .025 level.

The implication of these results is that the fiscal performance of state governments subject to tax or expenditure limits has been significantly different from what it would have been in the absence of such limits. Per capita public expenditure in those states has been lower than what would have been expected on the basis of the previous performance of the states with TELs. It also appears to be lower than what would be expected on the basis of expenditure levels in states without budgetary limits. This latter inference is based on the significantly larger number of states in which both state-only and combined state and local expenditure fell relative to their expected levels. The importance of this test derives from the fact that it is the only indication of a decline in state spending that is not

offset by increased local spending. In four of the six non-TEL states in which state spending was lower than expected after 1977, the apparent austerity at the state level was offset by higher local spending. This occurred in only one of the six TEL states where state spending fell. The case of the six non-TEL states in which only local spending fell is more difficult to interpret. It seems to suggest, together with the case of the non-TEL states where only state spending fell, that the monitoring costs facing voters at the local government level are substantially lower than they are at the state level. This is consistent with the view that the narrower the range of governmental functions, and so the less elaborate the appropriations process, the easier it is for voters to monitor and control their representatives' behavior. Thus, TELs do not appear to be necessary to achieve cuts in local government spending, but they do seem to enable reductions in state government spending greater than what could be realized without them.

#### D. Cross-Section Evidence on the Adoption of Budget Limits

If it is true that tax or expenditure limits at least occasionally lead to reductions in public sector budgets, and if this effect is the response intended by those who approve such limits, then it ought to be possible to explain the approval of TELs on the basis of data available at the time that those TELs were adopted. To test this proposition, I estimated the following cross-section equation by means of both probit and ordinary least squares techniques:

$$(9) \quad D_i = \gamma_0 + \gamma_1 E'_i + \gamma_2 \dot{E}'_i + \gamma_3 A'_i + \gamma_4 \dot{A}'_i + \gamma_5 B_i + \gamma_6 \dot{B}_i + \gamma_7 I_i + e_i ,$$

where  $i$  indexes the 50 states,  $D_i = 1$  if state  $i$  ever adopted a TEL and zero otherwise,  $E'_i$  represents combined state and local expenditure as a fraction

of personal income in state  $i$ ,  $A'$  represents federal aid to state  $i$  and its local governments as a fraction of state personal income,  $B_i$  denotes state and local debt as a fraction of personal income in state  $i$ ,  $I_i$  is a measure of the political ideology of the U.S. senators<sup>4</sup> from state  $i$ , and a dot over a variable denotes the percentage change in the value of that variable over the previous five years. For each state with a TEL, the year of observation is the year prior to adoption of the limit. For states without TELs, the year of observation is 1979.

The parameter estimates obtained from both estimation techniques are reported in Table 5. The coefficient on state and local expenditures as a fraction of state income is strongly significantly positive in the probit specification and weakly so in the OLS specification. Although the  $R^2$ s are low, the probit model predicts 38 of the 50 states correctly, which is significantly better than random assignment of states at a critical value less than .01 ( $\chi^2 = 7.25$ ).

The probit results are interesting for more than just these summary statistics, however. Table 6a reports the results for each of the 16 states with budget limits. Seven of the states with TELs were predicted by the probit model to have a TEL. Of these, five were found to have reduced state and local spending by means of their TELs. Of the nine TEL states not predicted to have such limits, seven or eight (depending on the categorization of the estimated TEL coefficient for Louisiana) were found to have had ineffective TELs. The distributions of significant and insignificant TEL coefficients across the two columns of Table 6a are significantly different at either the .07 or the .007 critical value, depending on the classification of Louisiana. The pattern of results for the individual states is rather clear: of the states that adopted TELs,



those limits were likelier to be effective in states within that subsample that had unusually high levels of state and local spending relative to personal income.

It is possible that this finding merely reflects mean reversion in the sample as a whole. That is, it may be the case that spending is always likelier to fall relative to income if it is unusually high. To test this possibility, I constructed Table 6b, which is the analogue of Table 6a for the 34 states without TELs. Of these states, not one was both predicted to adopt a TEL and found to have significantly reduced combined state and local spending below its expected level after 1977. The difference between the distributions of the entries in the two rows is not statistically significant. Thus, those states that had apparently effective TELs were disproportionately states in which public expenditures were comparatively large fractions of personal income. No such pattern is present among the non-TEL states in which state and local spending fell unexpectedly in the years immediately following the tax revolt.

#### IV. Conclusion

A limit on taxes or government expenditure is at first glance a paradoxical policy. If special-interest lobbies are powerful enough to skew budget allocations toward their preferred items as well as to increase the budget's overall size, why are they not powerful enough to block passage of a TEL, or at least to ensure that any TEL that is passed is nonbinding? On the other hand, if unorganized voters have the ability to enact as seemingly drastic a measure as a TEL, how did their elected representatives come to approve budgets widely viewed as too large?

Three broad classes of hypotheses have been proposed as possible explanations of TELs: government as Leviathan, TELs as signals to legislators, and TELs as instruments of control over agents.

The predictions of the Leviathan hypothesis are not generally consistent with the evidence presented here. If governments consistently act as Brennan and Buchanan believe, why have the voters in only about one-third of the states managed to get TELs adopted? Furthermore, why is it that those TELs that take the form of constitutional amendments are not more effective at constraining Leviathan than TELs that are merely statutory and therefore easily subject to repeal by the legislature?

The hypothesis that a TEL is merely a signal from the general public to a legislature that is unaware of a fall in the demand for public services finds some support in the results of this study. Under this hypothesis, if all relevant variables were included in the estimated spending equations, TELs would not be correlated with shifts in those functions; they would merely be correlated with large shifts in the values of some of the independent variables. Indeed, because of the time likely to elapse between the beginning of a movement for passage of a TEL to its actual enactment, it is probable that the signalled changes in the relevant variables would already have occurred before a TEL had been adopted. The lack of a significantly negative effect of TELs on public sector budgets in over half the states in which they have been adopted is consistent with this interpretation for those states.

The hypothesis that TELs represent an innovation in the technology by which voters can reduce their costs of monitoring their representatives is also supported by the evidence presented here. Rational voters can know that their representatives will appropriate too large a budget if

unconstrained, because of the high cost of monitoring all the relevant legislative activity. However, a representative's single vote on a TEL proposal -- either to enact a new one or to amend or repeal an existing one -- is comparatively easy to monitor and therefore increases the principal's control over his agent. This function is served by either a constitutional or a statutory TEL, so that there is no necessary superiority of the former over the latter. Also, if TELs must be simple -- as they are in practice -- in order to be effective as cheap monitoring devices, then voters in states where special interests do not excessively influence appropriations may prefer an unconstrained budgetary process to a constrained one. Thus, the agency-costs approach implies that TELs need not be adopted universally, but that they should prove effective when they are adopted. In a significant number of states, this is indeed the case.

Furthermore, the passage of an effective TEL appears to be positively related to the ratio of combined state and local spending to state personal income. This finding is consistent with both the interest-group model of budgeting and the signalling model of TELs. In those states where budgets are higher than desired by a majority of voters, TELs provide a way for that majority to control its representatives. In states where reductions in the equilibrium budget are anticipated, TELs may serve as signals that increase the likelihood that incumbent legislators will continue to adjust the budget to economic and demographic developments. In these states, tax or expenditure limits do not have discernible effects independent of the observed values of the relevant economic variables.

Footnotes

1. State government expenditure, federal aid to stte governments, and severance-tax revenue are from State Government Finances. Consolidated state and local expenditure and federal aid are from Governmental Finances. State population is obtained by comparison of per capita and total expenditures.
2. Using Fisher's exact method, the calculated probability of obtaining six or fewer significantly negative coefficients from the 34 non-TEL states when their true distribution is the same as that of the TEL states is slightly greater than 0.12.
3. Again using Fisher's exact method, the calculated probability of obtaining eight or fewer significantly negative coefficients from the non-TEL states when their true distribution is the same as that of the TEL states is in excess of 0.24.
4. Debt figures are from State Government Finances and Government Finances. The measure of ideology used was the average of the scores received by the two senators from the Americans for Democratic Action in the appropriate year. The source for this variable is Congressional Quarterly Weekly Almanac.

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Table 1

Summary Characteristics of State Tax or Expenditure Limits

<u>Year</u>	<u>State</u>	<u>Limit Applies to</u>	<u>Type</u>	<u>Method of Approval</u>
1976	New Jersey	Expenditures	Statutory	Legislative Vote
1977	Colorado	Expenditures	Statutory	Legislative Vote
1978	Arizona	Expenditures	Constitutional	Legislative Referendum
	Hawaii	Expenditures	Constitutional	Constitutional Convention
	Michigan	Revenues	Constitutional	Citizen Initiative
	Tennessee	Expenditures	Constitutional	Constitutional Convention
	Texas	Expenditures	Constitutional	Legislative Referendum
1979	California	Expenditures	Constitutional	Citizen Initiative
	Louisiana	Revenues	Statutory	Legislative Vote
	Oregon	Expenditures	Statutory	Legislative Vote
	Washington	Revenues	Statutory	Citizen Initiative
1980	Idaho	Expenditures	Statutory	Legislative Vote
	Missouri	Revenues	Constitutional	Citizen Initiative
	So. Carolina	Expenditures	Statutory	Legislative Referendum
1981	Montana	Expenditures	Statutory	Legislative Vote
1982	Alaska	Expenditures	Constitutional	Legislative Referendum

Source: ~~Advisory Commission on Intergovernmental Relations (1985).~~



Table 2a

State Expenditure Equations for States with Constitutional TELs

<u>State</u>	<u>Independent Variables</u>				<u>SER<sup>a</sup></u>	<u>D-W</u>
	<u>Popu- lation</u>	<u>Personal Income</u>	<u>Federal Aid</u>	<u>TEL</u>		
Alaska <sup>b</sup>	1208 (1.24)	-0.05 (1.21)	1.00 (3.14)	120 (2.65)	.04	1.84
Arizona <sup>b</sup>	33 (0.29)	0.004 (0.17)	1.28 (3.62)	-90 (2.71)	.03	2.52
California <sup>b</sup>	236 (0.53)	0.018 (0.34)	0.38 (0.65)	2151 (1.66)	.04	1.59
Hawaii <sup>c</sup>	-1384 (1.89)	0.28 (2.89)	1.11 (1.93)	-150 (4.42)	.04	2.03
Michigan <sup>b</sup>	1398 (4.47)	0.004 (0.19)	1.37 (4.66)	3 (0.03)	.02	2.08
Missouri <sup>b</sup>	476 (0.80)	0.01 (0.53)	-0.01 (0.00)	12 (0.16)	.04	1.88
Tennessee <sup>b</sup>	-191 (1.23)	0.01 (0.66)	1.22 (5.15)	7 (0.20)	.01	1.96
Texas <sup>b</sup>	3 (0.02)	0.002 (0.07)	0.90 (2.52)	-26 (0.17)	.02	1.87

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(Absolute values of t-statistics in parentheses)

- a. Standard error of the regression as a fraction of the mean of the dependent variable, based on unweighted observations.
- b. Quadratic time trend included, but parameter estimates not reported.
- c. Dependent variable lagged one year also included, but parameter estimates not reported.

Table 2b

State Expenditure Equations for States with Statutory TELs

<u>State</u>	<u>Independent Variables</u>				<u>SER<sup>a</sup></u>	<u>D-W</u>
	<u>Popu- lation</u>	<u>Personal Income</u>	<u>Federal Aid</u>	<u>TEL</u>		
Colorado <sup>b</sup>	78	-0.06	0.81	-47	.03	2.43
	(0.45)	(1.96)	(3.35)	(1.75)		
Idaho <sup>b</sup>	120	-0.04	1.71	-49	.03	1.86
	(1.00)	(1.36)	(6.36)	(2.73)		
Louisiana <sup>b</sup>	649	0.002	0.22	-154	.03	1.94
	(3.07)	(0.01)	(0.57)	(2.02)		
Montana <sup>b</sup>	444	-0.11	1.03	-27	.02	1.91
	(2.16)	(6.18)	(10.41)	(3.05)		
New Jersey <sup>b</sup>	-180	-0.02	1.77	-225	.04	1.42
	(0.51)	(0.62)	(5.61)	(1.64)		
Oregon <sup>c</sup>	523	0.03	0.62	16	.03	1.91
	(2.10)	(1.36)	(3.04)	(0.34)		
South Carolina <sup>d</sup>	262	0.06	1.19	-99	.05	2.41
	(0.62)	(1.68)	(2.52)	(0.96)		
Washington <sup>b</sup>	722	-0.04	1.17	68	.05	2.33
	(2.99)	(1.34)	(2.93)	(0.63)		

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(Absolute values of t-statistics in parentheses)

- a. Standard error of the regression as a fraction of the mean of the dependent variable, based on unweighted observations.
- b. Quadratic time trend included, but parameter estimates not reported.
- c. No time trend or lagged dependent variables included.
- d. Three lagged dependent variables included, but not reported.

Table 3a

Combined State and Local Expenditure Equations for States with Constitutional TELs

<u>State</u>	<u>Independent Variables</u>			<u>TEL</u>	<u>SER<sup>a</sup></u>	<u>D-W</u>
	<u>Popu- lation</u>	<u>Personal Income</u>	<u>Federal Aid</u>			
Alaska <sup>b</sup>	1460 (1.03)	-0.14 (2.21)	1.23 (2.20)	152 (2.18)	.05	2.37
Arizona <sup>b</sup>	-133 (0.85)	0.05 (1.66)	1.23 (4.35)	-150 (3.53)	.02	1.70
California <sup>b</sup>	101 (0.23)	-0.015 (0.32)	0.53 (1.80)	2179 (1.85)	.02	1.58
Hawaii <sup>c</sup>	-1565 (0.95)	0.40 (1.82)	0.04 (0.28)	-345 (4.51)	.04	2.09
Michigan <sup>b</sup>	2187 (5.70)	-0.04 (1.84)	1.18 (7.47)	-141 (1.05)	.02	2.61
Missouri <sup>b</sup>	722 (0.69)	0.05 (0.99)	0.08 (0.26)	129 (0.99)	.04	1.47
Tennessee <sup>b</sup>	189 (0.54)	0.04 (0.64)	0.65 (1.79)	-18 (0.21)	.03	1.96
Texas <sup>b</sup>	368 (1.31)	-0.01 (0.32)	0.51 (1.24)	0 (0.00)	.02	2.01

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(Absolute values of t-statistics in parentheses)

- a. Standard error of the regression as a fraction of the mean of the dependent variable, based on unweighted observations.
- b. Quadratic time trend included, but parameter estimates not reported.
- c. Dependent variable lagged one year also included, but parameter estimates not reported.

Table 3b

Combined State and Local Expenditure Equations for States with Statutory TELs

<u>State</u>	<u>Independent Variables</u>				<u>SER<sup>a</sup></u>	<u>D-W</u>
	<u>Popu- lation</u>	<u>Personal Income</u>	<u>Federal Aid</u>	<u>TEL</u>		
Colorado <sup>b</sup>	118 (0.37)	-0.14 (2.76)	1.21 (4.41)	-92 (1.81)	.03	2.10
Idaho <sup>b</sup>	253 (1.41)	-0.06 (1.57)	1.51 (5.13)	-63 (2.29)	.03	1.95
Louisiana <sup>b</sup>	886 (2.30)	0.02 (0.27)	-0.025 (0.07)	-184 (1.67)	.03	1.91
Montana <sup>b</sup>	1134 (3.24)	-0.19 (5.87)	1.35 (7.85)	-32 (2.01)	.02	1.79
New Jersey <sup>b</sup>	-928 (2.17)	0.08 (2.43)	2.10 (6.92)	-558 (3.54)	.03	1.85
Oregon <sup>b</sup>	-270 (0.38)	0.01 (0.14)	1.44 (4.87)	31 (0.38)	.02	2.04
South Carolina <sup>c</sup>	-125 (0.20)	0.12 (2.53)	1.15 (1.96)	-25 (0.17)	.03	2.35
Washington <sup>b</sup>	753 (1.95)	-0.05 (1.00)	1.50 (3.31)	80 (0.51)	.04	2.15

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(Absolute values of t-statistics in parentheses)

- a. Standard error of the regression as a fraction of the mean of the dependent variable, based on unweighted observations.
- b. Quadratic time trend included, but parameter estimates not reported.
- c. Dependent variable lagged one year also included, but parameter estimates not reported.

Table 4

T-Statistics of Estimated Coefficients of Post-1977 Dummy for Non-TEL States

<u>State</u>	<u>Dependent Variable</u>		<u>State</u>	<u>Dependent Variable</u>	
	<u>State Exp.</u>	<u>State/ Local Exp.</u>		<u>State Exp.</u>	<u>State/ Local Exp.</u>
Alabama	0.50	-0.23	Nevada	-0.57	-0.24
Arkansas	-0.47	1.30	New Hampshire	-5.06*	-3.47*
Connecticut	0.19	0.55	New Mexico	0.22	-0.21
Delaware	-3.75*	-0.89	New York	-7.28*	-1.08
Florida	0.71	-2.13*	North Carolina	0.64	0.34
Georgia	1.09	-0.04	North Dakota	-0.81	0.24
Illinois	-1.64	-2.22*	Ohio	-1.80*	-1.35
Indiana	-0.84	-1.82*	Oklahoma	-0.58	-0.83
Iowa	0.83	0.36	Pennsylvania	-2.58*	-1.89*
Kansas	0.06	1.82	Rhode Island	-0.19	-0.65
Kentucky	3.11	2.02	South Dakota	1.53	0.75
Maine	-1.87*	-1.71	Utah	1.04	0.33
Maryland	-0.84	-1.55	Vermont	-1.16	-1.73*
Massachusetts	0.04	-1.55	Virginia	-1.66	-2.38*
Minnesota	-1.49	-2.98*	West Virginia	2.45	2.05
Mississippi	-0.72	0.61	Wisconsin	0.00	-0.57
Nebraska	-0.52	-0.13	Wyoming	0.48	0.56

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\*Significantly negative at a critical value of .05.

Table 5

Estimated Parameters of Equations Predicting Adoption of a TEL

<u>Variables</u>	<u>Probit</u>	<u>OLS</u>
Expenditure/Income	24.9 (2.16)	4.3 (1.64)
Percent Change, Expenditure/Income	-17.2 (1.08)	-3.2 (0.64)
Federal Aid/Income	-19.0 (0.68)	-6.5 (0.75)
Percent Change, Federal Aid/Income	12.4 (1.51)	2.9 (1.17)
Debt/Income	-8.8 (1.58)	-0.03 (0.16)
Percent Change, Debt/Income	0.2 (0.10)	-0.12 (0.18)
Ideology	0.006 (0.58)	0.001 (0.41)
Constant	-3.97 (2.46)	-0.24 (0.64)
Log Likelihood	-26.5	-29.4
R <sup>2</sup>	.18	.13

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(Absolute value of t-statistics in parentheses)

Table 6a

Contingency Table for States with TELs

<u>Results from Equation (7) or (8)</u> <u>for State and Local Spending</u>	<u>Results from Equation (9)</u>	
	<u>Predicted</u> <u>to Have TEL</u>	<u>Not Predicted</u> <u>to Have TEL</u>
<b>Significantly Negative</b>		
TEL Coefficient	5	1 (2)*
<b>Not Significantly Negative</b>		
TEL Coefficient	2	8 (7)*

\*Number in parentheses is cell entry if Louisiana's coefficient  
(t = -1.67) is treated as significant.

Table 6b

Contingency Table for States without TELs

<u>Results from Equation (7) or (8)</u> <u>for State and Local Spending</u>	<u>Results from Equation (9)</u>	
	<u>Predicted</u> <u>to Have TEL</u>	<u>Not Predicted</u> <u>to Have TEL</u>
<b>Significantly Negative</b>		
TEL Coefficient	0	6
<b>Not Significantly Negative</b>		
TEL Coefficient	3	25