

The Relationship Between Education Finance Reform and Tax and Expenditure Limitations

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Abstract. Since the success of *Serrano v. Priest* in California, at least 43 states have experienced legal challenges to their education financing formulas and in 19 states court rulings have forced changes in these formulas. A result of this litigation is a transfer of resources from districts with higher property tax values to those with lower property tax values. Over roughly the same time span, more than 30 states have imposed new restrictions limiting tax collections, tax expenditures or both (TEs) at the local level. There is evidence that the effect of education finance reform depends upon whether a TEL is in place. Thus to understand fully the impact of such litigation, one must consider whether reform makes the passage of a TEL more likely. In this paper we use data for all states in which referenda are possible over the 1978 – 1990 period to investigate whether TEL referendums are more likely to pass in states where courts have ordered education finance reform. We find that the probability of TEL success in a statewide election is significantly higher if the state has experienced education finance reform.

1. Introduction

In 1971, a group of school children and their parents filed a lawsuit challenging the legality of California's method of financing public education. At issue was the consequence of California's predominant reliance on local property taxes to fund school districts. Deep disparities in taxable property across districts were reflected in disparities in per pupil expenditures across these districts. In *Serrano v. Priest* (*Serrano*), as the case became known, the

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plaintiffs alleged that with access to educational resources dependant upon where a student resided, the state was not providing equal protection to all students. They argued that this was in violation of “equal protection” clauses found in both the U.S. Constitution and California’s own state constitution. The Supreme Court of California agreed and ordered a reform of education finance in California.¹

The U.S. Supreme Court, in a separate case (*San Antonio Independent School District v. Rodriguez*), subsequently ruled that disparate education financing within a state does not violate the U.S. Constitution. However, since California’s funding strategy violated its own constitution, it was forced to remedy the situation. The legislature’s first proposed reform was insufficient to satisfy the Supreme Court of California. Consequently, the legislature put forth a proposal, which would have guaranteed all districts a certain level of funding so long as they taxed property at a particular rate. While districts could tax above this rate, the ability of a district to have funding at this level was independent of the property wealth of a district and thus was accepted by the court as meeting the standard of equal protection.²

The extent to which this proposed financing could succeed in providing “equal protection” was never demonstrated. Soon after its passage by the legislature and acceptance by the court, Californians imposed severe restrictions on the ability of districts to raise tax revenue via property taxes by passing Proposition 13.³ Proposition 13, a very restrictive property tax rate limit that applies to all levels of local government including school districts, was approved by the electorate of California in 1978.⁴ As a result of the combination of the court ruling and Proposition 13, school districts became increasingly reliant on state funding.

Serrano and Proposition 13 serve as examples of two important trends in education finance reform. Since the success of *Serrano* at least 43 states have seen legal challenges to their education financing formulas through 1998, and 19 of these were successful (See Minorini and Sugarman, 1999, and Long, 1999). Over roughly the same time span, more than 30 states have imposed new restrictions limiting tax collections, expenditures or both (TELS). Silva and Sonstelie (1995) and Fischel (1989) have argued that in the case of California, education finance reform and the imposition of Proposition 13 are related. Fischel suggests that *Serrano* disrupted a political equilibrium that existed prior to the ruling. In Fischel’s argument, Californians practiced a method of fiscal zoning in which the property tax was an efficient price

¹ See Minorini and Sugarman (1999), Murray Evans and Schwab (1998), McCarthy (1994), Fishel (1989), and Downes (1992) for a more detailed discussion of this important legal action. Our discussion draws most heavily on Minorini and Sugarman.

² The California Supreme Court upheld a lower court order requiring that wealth related inequalities in per-student expenditures not exceed \$100 (Minorini and Sugarman, 1999).

³ See Fischel (1989) for a discussion of Proposition 13.

⁴ Oakland (1979) estimates that Proposition 13 reduced property tax revenues by 57 percent.

mechanism for local public goods. Household's sorted themselves in a way consistent with their demand for public goods. The result was that households with a high demand for education tended to reside in school districts with high spending and vice versa. *Serrano* disrupted this equilibrium by ordering the redistribution of revenue from high-spending districts to low-spending districts. Fischel argues that voters imposed Proposition 13 in order to limit the redistribution efforts; education finance reform *caused* Proposition 13.

The purpose of this paper is to examine more generally and empirically whether education finance reform played a role in the wave of new tax and expenditure limitations across the country. To our knowledge none of the published work evaluating the imposition of TELs considers the role of education finance reform. Alm and Skidmore (1999) presents the only national study evaluating the adoption of TELs. Importantly, if these two reform movements are related, researchers seeking to understand the effects of reform or TELs must use appropriate estimation techniques.

In the following section, we provide a brief introduction to education finance reform litigation. In section 3, we review the literature on the imposition of TELs. In section 4, we address the question of whether court ordered finance reform increases the likelihood that a referendum on TELs will pass. In our conclusion, we summarize our findings and discuss useful future work.

2. Education Finance Reform

In 1973, the U.S. Supreme Court heard arguments in *San Antonio Independent School District v. Rodriguez (Rodriguez)*. As in *Serrano*, the plaintiff argued that deep disparities in education expenditures per student across districts were inconsistent with the equal protection clause in the 14th amendment to the U.S. Constitution. However, no specific reference is made to education in the Constitution and the court ruled that education is thus not a fundamental Constitutional right. As such, the court refused to apply the most demanding standards for equal protection (McCarthy, 1994). Under the standards applied, Texas' goal of local control over education was sufficient to justify the existing education policy. Texas had a minimum foundation aid program, which the court felt provided adequate education. Thus the equal protection clause was not violated (McCarthy, 1994 and Minorini and Sugarman, 1999).

As a consequence of this decision, the judicial battle for education finance reform has been fought almost exclusively in state courts. Most state constitutions mandate that the state provide for public education. This has opened up two broad strategies for challenging states' education funding formulas. (Murray, Evans and Schwab, 1998 and McCarthy, 1994). One ap-

proach has been to apply the strategy in *Rodriguez* and *Serrano* and argue that equal protection requirements have not been met. Since education is mentioned expressly in state constitutions, a higher standard is required to justify deviations from equal protection. Applying these higher standards has been sufficient to reveal unconstitutional practices and force reform in several states (Minorini and Sugarman, 1999).

A second approach is to argue that the specific language of the state constitution has been violated and thus remedies are required. For example, The West Virginia and New Jersey Constitutions require provision of a “thorough and efficient” system of schools (Minorini and Sugarman, 1999, p. 50 and p. 52). The state of Washington requires “ample provision of education” (McCarthy, 1994). In each of these states, such language has been interpreted by the courts to require states to significantly alter education finance. New Jersey is a particularly interesting case. The court battle, through several different cases, has lasted more than 20 years. Recent litigation, *Abbott v. Burke*, has resulted not only in a requirement of equalization in expenditures but also additional expenditures in poorer districts in response to the larger cost of meeting the needs of disadvantaged students (Minorini and Sugarman, 1999).

Such court ordered finance reforms have undoubtedly played a role in bringing about the observed decreased reliance on local property taxes and increased reliance on state funding of education. Murray, Evans and Schwab (1998) provide the most convincing evidence to date that education finance reform has been successful in decreasing inequality in per pupil expenditures and limiting the dependence of per pupil expenditures on local property wealth. They report (page 799) that inequality in education expenditures “has fallen at the same time that household income inequality grew significantly suggesting that the link between wealth and school resources weakened.”

The effect of reform on total education spending is less certain. Murray, Evans and Schwab find that court ordered finance reform increases spending in the poorest districts by 11 percent while leaving spending in the wealthier districts unchanged. An implication is that total education expenditures increase in the wake of reform. However, Blankenau and Skidmore (2001) find that reform has had no effect on total expenditures. To provide an explanation for this finding, Blankenau and Skidmore model reform as increasing the progressivity of state education expenditures. In response, poorer districts may decrease or increase less rapidly “own source” tax collection as they substitute state for local revenue to some extent. Left unconstrained, wealthier districts may respond to lost state funds by increasing own source revenue. However, a TEL is modeled as a freeze on local tax rates. Thus own source spending falls or grows more slowly in poorer districts, but cannot rise in wealthier districts and own source spending falls in aggregate. Blankenau and Skidmore show that increased state funding just offsets re-

ductions in own source funding so that the net effect on total expenditures is negligible.

In a political economy context, this finding is of particular importance if court ordered finance reform increases the likelihood of a TEL being implemented. Since much education finance reform resulted from court order, it is reasonable to consider their existence as determined outside the political process. However, most TELs come into existence as the result of direct political action, often following a referendum (Alm and Skidmore, 1999). Since court ordered finance reform changes the nature of education spending, it is not unlikely that they would change the choice of education *finance*. Anecdotal evidence is provided in the literature. As previously mentioned, Fischel (1989) argues that in California, *Serrano* caused Proposition 13. Murray, Evans and Schwab point out that in New Jersey the reform ordered in the *Abbott* case eventually led to lower property taxes and lower state aid as wealthier districts, teachers unions and antitax organizations responded angrily to higher state taxes and increased progressivity in state expenditures. If such responses are typical (or more likely) following reform, the expected impact of reform is decreased total education expenditures.

Indeed, a preliminary examination suggests that education finance reform has been followed by the imposition of a TEL. During the 1978 to 1990 period, 63 TEL initiatives were placed on statewide ballots across the nation. Of the 63, about 40 percent were successful. However, 25 of the 63 initiatives occurred in states following education finance reform and of those 12 were successful. Conversely, in the absence of reform there were 38 TELs on the ballot, of which 13 were successful. Thus, in the presence of education finance reform the success rate was 48 percent, whereas in the absence of reform the success rate was just 34 percent. We explore the linkage between education finance reform and TELs more rigorously in section 4.

3. Tax and Expenditure Limitations

There is a considerable body of literature that examines why voters have supported the tax and expenditure limitation referenda. A majority of these studies have used survey data on attitudes toward government and the imposition of TELs within a particular state at a specific point in time (Levy, 1975; Mariotti, 1978; Courant, Gramlich, and Rubinfeld, 1980; Gramlich, Rubinfeld and Swift, 1981; Ladd and Wilson, 1982, 1983; Gramlich and Rubinfeld, 1982; Stein, Hamm, and Freeman, 1983). Alm and Skidmore (1999) present the only analysis that examines the actual conditions across the states over time that influence support for or opposition to TELs. They note, as others have, that the imposition of the new wave of TELs across the states is not an exogenous event. Rather, conditions within the states may

have changed and these underlying factors may have led to the imposition of TELs.

Alm and Skidmore suggest that preferences and other demand side factors, as well as supply side factors, change over time and that these changes may lead to disequilibrium in the political market. Stigler (1972) noted that the political marketplace is subject to changing forces that alter the political equilibrium. An adjustment then becomes necessary, but imperfections in the political market may prevent outcomes that reflect the preferences of the median voter. These imperfections may take the form of the Niskanen (1971) model in which there is a budget-maximizing bureaucrat. There is also the model of government as leviathan, as put forth by Brennan and Buchanan (1980). TELs, in this framework, are interpreted as an attempt to reduce the government's ability to control the agenda and thus create political competition. Alm and Skidmore show in their empirical analysis that changing fiscal and economic factors play a role in the success of TELs in statewide elections.

As previously discussed, Fischel (1989) argues that in the case of California it was the court ordered education finance reform which created a disequilibrium and led to the success of Proposition 13 in 1978. However, the hypothesis that education finance reform has led to the imposition of TELs in states across the nation remains untested. The analysis below demonstrates that indeed the presence of education finance reform increases the likelihood of TEL passage. This finding has important implications for those evaluating the effects of either.

4. Interrelatedness of Education Finance Reform and TELs

Our hypothesis is that education finance reform increases the probability that a TEL will be successful in a statewide election. To test this hypothesis, data is collected on all TEL initiatives that appeared on a statewide ballot for each year over the period 1978 – 1990. As shown in Table 1, a total of 58 measures appeared on a statewide ballot, and 25 passed. Appendix Table A presents detailed information on the specific characteristics of each measure. Passage of these TELs is a function of economic, fiscal, and political characteristics of the state at a particular point in time. The factor that is of direct relevance for this study is whether the presence of education finance reform plays a role in the likelihood of TEL passage. This and other factors are discussed in greater detail later.

Table 1. State Experiences With Tax and Expenditure Limitations

Year	Total TELs	Successes	Failures
1978	17	12	5
1979	2	2	0
1980	13	4	9
1981	1	1	0
1982	4	2	2
1983	0	0	0
1984	5	0	5
1985	0	0	0
1986	7	3	4
1987	1	0	1
1988	5	1	4
1989	0	0	0
1990	3	0	3
Total	58	25	33

One approach to estimating the impact of these independent variables on the probability of TEL passage is to employ a simple probit procedure assuming that the probability of TEL passage depends on the characteristics of the state at a particular point in time. However, use of the simple probit procedure may generate inconsistent estimates because the sample is nonrandom. In order for TEL passage to occur, two sequential conditions must be met: the issue must be placed on the ballot (the “ballot issue”), and then the measure must receive support from a majority of the voters (the “passage issue”). As stated by Alm and Skidmore (1999), a set of threshold conditions must exist in a given state year before a TEL is placed on the ballot, and then another set of conditions must be met in order for the measure to pass. The sample is therefore truncated from below.

To correct for this potential sample selection bias we first estimate a model of the ballot issue using a standard probit analysis. Subsequently we estimate the passage equation using a probit analysis with the inverse Mill’s ratio generated from the ballot equation included as an additional independent variable. Maddala (1983) demonstrates that this approach yields consistent estimates.⁵

To demonstrate the need for this refinement, consider first the ballot issue. Let B_{it} be a discrete random variable equal to one when a TEL is on the ballot in state i during year t and zero otherwise. The presence of a TEL ini-

⁵ However, this approach is inefficient. Alm and Skidmore (1999) estimated a similar two-equation system simultaneously using maximum likelihood procedures. The maximum likelihood procedure yields consistent and efficient estimates.

tiative on the ballot is assumed to be determined for each state i in each year t by a set of observable variables, Z_{it} , and a random component u_{it} . More precisely, the value of B_{it} is such that:

$$B_{it} = \begin{cases} 1 & \text{if } Z_{it}\mathbf{a} + u_{it} \geq 0 \\ 0 & \text{if } Z_{it}\mathbf{a} + u_{it} < 0 \end{cases}$$

where α is a vector of coefficients. The random component, u_{it} is assumed to have a mean of zero.

Next, consider the passage issue. The presence of a TEL on the ballots requires $Z_{it}\alpha + u_{it} \geq 0$.

Its passage is assumed to be determined by a set of observable variables, X_{it} , and a random component e_{it} . X_{it} may contain any subset of Z_{it} and may be contained in Z_{it} . Let P_{it} be a discrete random variable equal to one when a TEL referendum passes in state i during year t and zero if it fails to pass. The value of P_{it} is determined as follows:

$$P_{it} = \begin{cases} 1 & \text{if } B_{it}=1 \text{ and } X_{it}\mathbf{b} + e_{it} \geq 0 \\ 0 & \text{if } B_{it}=1 \text{ and } X_{it}\mathbf{b} + e_{it} < 0 \end{cases}$$

where β is a vector of coefficients. It is assumed that the expected value of e_{it} given X_{it} is zero in the population ($E(e/X_{it})=0$). However, we observe only observations such that $Z_{it}\alpha + u_{it} \geq 0$. While $E(u/Z_{it})=0$,

$E(u/Z_{it}\mathbf{a} + u_{it} \geq 0)$ may be non-zero. Furthermore, we assume that u and e have a bivariate normal distribution with means of zero, variances of \mathbf{s}_u^2 and \mathbf{s}_e^2 , and a covariance of \mathbf{s}_{ue} . In this

case $E(e/\{X_{it}, Z_{it}, B_{it}=1\}) = \mathbf{s}_{ue}E(u/Z_{it}, B_{it}=1)$. Thus when the error terms are correlated and $E(u/Z_{it}, B_{it}=1)$ is nonzero, our estimate is not consistent. As suggested by Maddala (1983), we add the inverse Mill's ratio in the passage equation to correct for any selection bias.

Our principle concern in this paper is in evaluating whether education finance reform has played a role in the outcome of TEL referenda. To test this, we include in X and Z a dummy variable taking a value one in those state years in which a reform is in effect. In separate models we employ two measures of education finance reform. In the first the dummy variable is equal to one if a state has experienced a successful education finance court ruling and zero otherwise. In the second, the dummy variable is 1 if the state experiences education finance reform (court ordered or any type of reform as

defined by Manwaring and Shefrin (1997)) and zero otherwise.⁶ We present in Appendix Table B a summary of recent education finance reform activity for all states.

Our controls match those in Alm and Skidmore (1999). They suggest that voter support for TELs is the result of changes in public good demand and supply characteristics and imperfections in the political system that create a disequilibrium. We include as controls a number of variables that measure changes in the demand-side and supply-side characteristics that may determine support for (or against) a TEL measure. We present definitions and descriptive statistics in Table 2.⁷ Demand-side variables included in the empirical analysis are the percentage change in state real income over the previous five years and several proxies for the tax price of public services and state demographic characteristics. The first tax price proxy is percentage change federal transfers to state and local governments over a five-year period. Reductions in federal transfers increase the tax price of state and local public services and thus increase the probability of TEL success. We also include the percentage change over a five-year period in the ratio of itemized federal income tax returns to total returns. A reduction in this ratio increases the cost to state taxpayers of providing state and local public services, and thus increases the probability of TEL passage. We also include changes over a five-year period in the proportion of the population that is over the age of 65 and that is of school age to capture changing demographic characteristics that may lead to support for TELs.⁸

On the supply side, we include the percentage change over a five-year period in total state tax revenue, property tax revenues, the share of local revenues relative to total state and local revenues, and welfare spending. These variables measure changes in the composition of state revenue and spending that may influence TEL referenda outcomes. We include several other variables that control for the general political environment. For example, we include a dummy variable that is equal to one if the state already has a TEL and zero otherwise. The final control is a dummy variable that is equal to one if the Republican Party has control of the Governor's office, the House, and the Senate.

⁶ Reform has also been imposed via legislative action, but this type of reform is considered to be weaker.

⁷ These data are collected from the United States Department of Commerce publications (*State Government Finances* and *Government Finances the Book of the States*), The Council of State Governments, Lexington Kentucky, and various issues of *Significant Features of Fiscal Federalism* (Advisory Commission on Intergovernmental Relations).

⁸ Alm and Skidmore (1999) also examine the effects of other variables like welfare spending, political control, and the specific characteristics of TELs, but these variables were found not to play a role in TEL passage.

Table 2. Variable Definitions, Means, and Standard Deviations

Variable	Mean	Standard Deviation
Percent change in tax price over a three- year period	0.0274	0.2159
Percent change in real state income over a five-year period	0.1461	0.1200
Percent change in real intergovernmental transfers from federal government of five-year period	0.0543	0.1767
Percent change in population over a five year period	0.0570	0.0743
Percent change in real property taxes over a five year period	0.0715	0.2208
Percent change in real total tax revenues over a five year period	0.1411	0.1666
Percent change in the ratio of local revenues to total state and local revenues over a five year period	-0.0051	0.0778
Percent change in real welfare expenditures over a five year period	0.2063	0.2325
Percent change in population over the age of 65 over a five year period	0.0527	0.0418
Percent change in population between the ages of 5 and 17 over a five year period	-0.0630	0.0608
Dummy variable equal to 1 if the state government is controlled by Republicans and 0 otherwise	0.0810	0.2732
Dummy variable equal to 1 if the state government is controlled by Democrats and 0 otherwise	0.3494	0.4774
Dummy variable equal to 1 if a TEL has already been imposed and 0 otherwise.	0.5949	0.4915
Dummy variable equal to 1 if successful education finance litigation has occurred and zero otherwise	0.1603	0.3675
Dummy variable equal to 1 if education finance reform has occurred and zero otherwise	0.5566	0.4976

It should also be noted that Alm and Skidmore examined a wide range of other factors that could play a role in the success of a TEL referenda. These factors include growth in sales and income taxes, growth in welfare and health expenditures, number of public sector employees, and a dummy variable that indicates democratic control of state government. However, these factors proved not to be statistically significant determinants of TEL success. They also measure growth of all variables over one-year and three-year periods rather than a five- year period. Their results are largely unaffected under these alternative specifications, and therefore we do not estimate these specifications.

Table 3 presents four sets of maximum likelihood estimates. Model 1 omits education finance reform as an explanatory variable, whereas Models 2 and 3 include dummy variables for whether or not the state has experienced successful litigation or education finance reform, respectively. For comparison, Model 4 presents an estimation that omits the inverse Mill's ratio.

Table 3. Estimates of The Ballot and Passage Equations (Absolute value of asymptotic t-statistics in parentheses).

Variable	Model 1	Model 2	Model 3	Model 4
<u>Ballot Equation</u>				
Constant	-1.166** (6.976)	-1.374** (5.958)	-1.091** (6.109)	
Personal Income	0.132 (0.115)	0.127 (0.111)	0.140 (0.122)	
Federal Transfers	2.041** (3.876)	2.034** (3.854)	2.027** (3.848)	
Deductibility	0.323 (0.702)	0.320 (0.694)	0.250 (0.5345)	
Population	-0.024 (0.016)	-0.027 (-0.017)	-0.019 (0.012)	
TEL Already Imposed	-0.083 (0.479)	-0.085 (0.491)	-0.060 (0.344)	
Total Tax Revenues	-0.520 (0.516)	-0.512 (0.508)	-0.444 (0.438)	
Property Tax Revenues	-0.166 (0.287)	0.265 (0.285)	0.194 (0.333)	
Local Revenues/Total Revenues	-1.409 (1.153)	-10414 (1.157)	-1.345 (1.103)	
Republican Control	0.284 (0.989)	0.276 (0.952)	0.296 (1.030)	
Successful Litigation		-0.035 (0.169)		
Education Finance Reform			-0.192 (1.157)	
<u>Passage Equation</u>				
Constant	-11.26 (1.506)	19.66* (1.929)	8.734 (1.301)	0.460 (0.882)
Personal Income	17.99** (2.840)	25.24** (2.870)	18.33 (2.931)	19.28** (2.676)
Tax Price	-4.902* (1.908)	-6.889** (2.113)	-3.931* (1.672)	-2.754 (1.392)
Population	-8.873 (1.301)	-15.59* (1.786)	-8.990 (1.299)	-9.444 (1.235)
TEL Already Imposed	-1.280** (2.005)	-1.490** (2.030)	-1.637** (2.466)	-2.073** (3.039)
Total Tax Revenues	-14.61** (2.596)	-21.14** (2.786)	-17.06** (2.846)	-19.41** (2.891)
Property Tax Revenues	3.584 (1.639)	6.346** (2.097)	4.443* (1.906)	4.995** (2.060)
Local Revenues/Total Revenues	18.45** (2.134)	28.05* (2.436)	15.58* (1.889)	8.368* (1.779)
Successful Litigation		2.054** (2.315)		1.130* (1.729)
Education Finance Reform			1.438* (1.904)	
Inverse Mill's Ratio	-6.403 (1.436)	-11.68* (1.907)	-5.179 (1.249)	
Log-Likelihood	-23.25	-20.245	-22.38	-22.71
Percent Correct Predictions	74.4%	84.2%	80.7%	75.4%

* Significant at the .10 level for a two-tailed test. **Significant at the .05 level for a two-tailed test.

Consider first the differences between Model 1, and Models 2 and 3. Note that the log likelihood becomes smaller and that the percent correct

predictions increases substantially when we include either dummy variable in the analysis. The improvement is most pronounced with the successful litigation dummy variable, increasing the percent correct predictions of the model from 75.4 percent to 84.2 percent. The marginal probability of TEL success increases by 0.41 when successful litigation has occurred. This finding confirms our hypothesis: education finance reform is positively correlated with the Tax Revolt of the late 1970s and 1980s. Further, this nationwide analysis suggests that this phenomenon is not restricted to California, but appears to be present where education finance reform has taken place.

The other variables in the models are generally in line with expectations. An increase in the tax price of local government services increases the probability of TEL success. Increases in income increase the likelihood of TEL success, whereas increases in population, federal transfers, and the presence of an existing TEL reduce this likelihood. Somewhat surprisingly, increases in total state tax revenue are associated with a lower probability of TEL success, but increases in property taxes increase this probability. It therefore appears that voters are less concerned about increasing taxes in general, but are particularly sensitive to property tax increases. Similarly, an increasing share of local spending out of total state spending increases the likelihood of TEL success.

Note that the inverse Mill's ratio is negative and significant, indicating that the unobserved factors in the ballot equation are inversely related to the unobserved factors in the passage equation. That is, the unobserved factors that determine whether or not a TEL issue is placed on the ballot is inversely related to the unobserved factors that determine success in the passage equation. Importantly, a significant coefficient on the inverse Mill's ratio indicates that a standard probit estimation would yield biased estimates. Model 4 presents a simple probit absent the inverse Mill's ratio to highlight the differences in the coefficient estimates.

5. Conclusions

Given that the nature of referendum processes is complex, we acknowledge that our analysis does not capture all of the relevant factors that determine the outcomes of these processes. We note that outcomes are likely to depend on purely local events, and that our nationwide study is limited in its ability to capture these factors. However, in the original Alm and Skidmore (1999) study, changes in demand and supply side factors that determine TEL outcomes without considering education finance reform yield predictive power better than 75 percent. In this paper, we show that including education finance reform as an explanatory variable increases the predictive ability to better than 84 percent. That is, we improve the model fit by correctly predicting about six more outcomes in the passage equation. Also, our analysis shows that the presence of education finance reform increases the

marginal probability of success in the passage equation by 0.41.⁹ These results provide strong empirical support for the hypothesis that education finance reform has played an important role the tax revolt.

These findings suggest that studies evaluating the effects of TELs may suffer from omitted variable bias unless the presence of education finance reform is accounted for in the analysis. This study also confirms the concerns that numerous researchers have expressed in treating TELs as an exogenous determinant of government budgetary outcomes.¹⁰ Appropriate procedures must be used to evaluate endogenously determined fiscal institutions such as the recent tax and expenditure limitation movement.

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⁹ Calculating the change in the unconditional probability of TEL success in response to a change in a variable requires several steps. First, the initial level of the unconditional probability (UP_{it}) is calculated by substituting values for β , and the independent variables in the estimated Passage equation, including the coefficient on the inverse Mill's ratio. Mean values of the independent variables are used to generate the initial UP_{it} . Second, the new level of the unconditional probability that emerges with a change in the relevant independent variable. For example, if one is interested in the impact of education finance reform, then the new UP_{it} is found by substituting the new value of education finance reform. In this case, we calculated the initial unconditional probability by setting the education finance reform dummy variable equal to 0. The new estimate for the unconditional probability is calculated by setting the education finance reform dummy variable equal to 1. The difference between the initial probability and its new level is a measure of the marginal probability.

¹⁰ See for example, Poterba, 1995a, 1995b; Rueben, 1996; Shadbegian 1999, Blankenau and Skidmore, 2002.

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Appendix

Appendix Table A. TEL Outcomes

Year	State	Type of Limit	Outcome
1976	Colorado	Require electoral approval of all tax increases	Failed
1976	Michigan	Limit state taxes and spending to 8.3% of state personal income (bond payments are exempt).	Failed
1976	Montana	Limit state spending to \$375 million for each biennium until 1983.	Failed
1976	North Dakota	Limit sales tax rates to 3% from 4%.	Passed
1978	Alabama	Lower property tax assessment by 5%; and limit property tax rates.	Passed
1978	Arizona	Limit state appropriations to 7% of state personal income.	Passed
1978	Colorado	Limit state spending growth to cost of living and population increases.	Failed
1978	California	Limit property tax rates to 1%; limit growth in annual assessment and roll back assessments to 1976 levels.	Passed
1978	Hawaii	Limit state spending growth to growth in state economy.	Passed
1978	Idaho	Limit property taxes to 1% of market value.	Passed
1978	Illinois	Impose ceiling on state taxes and spending (non-binding).	Passed
1978	Michigan	Limit property tax assessment to 25% of full cash value.	Failed
1978	Michigan	Limit state revenue growth to state personal income growth.	Passed
1978	Michigan	Cut property taxes in half; and impose 5.6% ceiling on income tax rate.	Failed
1978	Missouri	Require state legislature to reduce property taxes.	Passed
1978	Nebraska	Limit growth in local government spending to 5% per year.	Failed
1978	North Dakota	Decrease personal income tax; and increase corporate income tax.	Passed
1978	Nevada	Limit property tax rates to 1% (with resubmission to voters in 1980).	Passed
1978	Oregon	Limit property tax rates to 1.5% of full cash value.	Failed
1978	South Dakota	Require voter approval or 2/3 legislative approval for tax increases.	Passed
1978	Texas	Limit state revenue growth to growth of state's economy (except when legislature adopts emergency action).	Passed
1979	California	Limit state and local appropriations growth to cost of living and population increases (debt and mandates do not apply).	Passed
1979	Washington	Limit state revenues to last year's ratio of revenue to average state income over 3 years.	Passed
1980	Arizona	Limit sum of all state and local property tax to 1% of full cash value.	Failed
1980	Louisiana	Reduce property taxes.	Passed
1980	Massachusetts	Limit property taxes to 2.5% of full cash value.	Passed
1980	Massachusetts	Limit state tax increases to state income growth.	Failed
1980	Michigan	Reduce property taxes by 50%; and increase income taxes.	Failed
1980	Michigan	Limit property assessment rates to 25% of true cash value.	Failed
1980	Nevada	Limit property tax rates to 1.5% of 1975 levels of full cash value (or later sale when changing ownership).	Failed
1980	Ohio	Reduce property taxes.	Passed
1980	Oregon	Limit property taxes to 1% of full cash value.	Failed
1980	Oregon	Limit oil and gas tax rates to 6% of market value.	Passed
1980	South Dakota	Limit property tax rates to 1% of market value.	Failed
1980	Utah	Eliminate sales tax on food.	Failed
1980	Utah	Limit property tax rates to 1% of 1977 levels of fair market value.	Failed
1981	Washington	Repeal inheritance and gift tax.	Passed
1982	Colorado	Reduce valuation of residential realty.	Passed
1982	North Dakota	Repeal property tax.	Failed
1982	Oregon	Limit realty tax rates to 1.5% of 1979 levels of market value; and limit growth of value increases to 2% per year.	Failed
1982	West Virginia	Limit assessment ratio to 60%.	Failed
1984	Arkansas	Limit assessment ratio to 20%.	Passed
1984	California	Revise Proposition 13 of 1978.	Failed
1984	Michigan	Roll back taxes to 1981 levels; and require voter approval for any new or increased tax or fee.	Failed
1984	Nevada	Limit property taxes; and require voter approval or 2/3 legislative approval for any increase in taxes/ fees.	Failed
1984	Oregon	Limit property tax rates to 1.5% of assessed value; and limit assessment increases to 2% per year.	Failed
1986	California	Require voter approval to raise local property tax.	Passed
1986	Colorado	Require voter approval for any state or local tax increase (licenses, fees, fines, and permits are exempt).	Failed
1986	Massachusetts	Limit state revenues to growth in state employee wages; require that state revenues must decrease if local revenues increase, and vice versa; and phase out 1978 surtax on income.	Failed
1986	Montana	Abolish property taxes; and replace lost revenues with a sales tax.	Failed
1986	Oregon	Impose property tax ceiling at 1986 level.	Passed
1986	Oregon	Limit property tax rates to 2% of assessed value in 1987 and 1.5% after.	Failed

Appendix table A (continued)

Year	State	Type of Limit	Outcome
1986	Utah	Limit state property tax rates to 2.4 mills.	Passed
1987	Washington	Limit sum of state and local property tax rates to 1% (school construction is exempt).	Failed
1988	Arkansas	Require voter approval or 3/5 legislative approval to pass tax measures.	Failed
1988	Colorado	Limit income taxes to 90% or 1987 levels unless voters approve increase.	Failed
1988	Nevada	Prohibit wage or income tax on individuals.	Passed
1988	South Dakota	Limit property tax rates to 2.5% on nonagricultural land and 1% on agricultural land using 1984 values.	Failed
1988	Utah	Roll back personal income, sales, motor fuel, and tobacco taxes to 1986 levels (effective December 1989).	Failed
1990	California	Increase voting requirements for new or increased taxes.	Failed
1990	Colorado	Limit income and property taxes; and impose government spending limits.	Failed
1990	Massachusetts	Reduce income tax rates from 5.95% to 4.25%.	Failed
1990	Oregon	Limit property tax rates to: 1% for school purposes 1.5% for nonschool purposes (1991) 1.5% for nonschool purposes (1992) 1.25% for nonschool purposes (1993) 1% for nonschool purposes (1994) 0.75% for nonschool purposes (1995) 0.5% for nonschool purposes (1996).	Passed
1990	South Dakota	Limit growth rate of property taxes to 2% per year.	Failed

Source: Various issues of the *State Tax Review*, 1978-1990.

Appendix – Table B. Dates of litigation, Reform, and types of Education Finance System by State.

State	Successful Litigation	Litigation	Strong Reform	Weak Reform	Education Finance System
Alabama	1993				Foundation
Alaska					Foundation
Arizona		1973		1980	Foundation
Arkansas	1983	1983			Foundation
California	1976	1976	1978	1977	State
Colorado		1982		1973	Foundation
Connecticut	1977	1977	1978		Foundation
Delaware		1981			Foundation
Florida				1973	Foundation
Georgia		1981		1986	Two Tier/Comb.
Hawaii					State
Idaho		1975		1978	Foundation
Illinois				1973	Two Tier/Comb.
Indiana					Foundation
Iowa				1972	Foundation
Kansas			1976	1973	Power Equal.
Kentucky	1989	1989	1989		Two Tier/Comb.
Louisiana				1988	Foundation
Maine				1978	Foundation
Maryland		1983		1987	Foundation
Massachusetts	1993			1985	Two Tier/Comb.
Michigan		1973			Power Equal.
Minnesota				1973	Foundation
Mississippi					Foundation
Missouri				1977	Two Tier/Comb.
Montana	1989	1989	1989		Foundation
Nebraska					Two Tier/Comb.
Nevada					Foundation
New Hampshire	1994			1985	Two Tier/Comb.
New Jersey	1973	1973	1976		Power Equal.
New Mexico		1974			Foundation
New York		1982			Power Equal.
North Carolina					Foundation
North Dakota					Foundation
Ohio		1979		1975	Foundation
Oklahoma		1987		1987	Two Tier/Comb.
Oregon		1976			Foundation
Pennsylvania					Two Tier/Comb.
Rhode Island					Power Equal.
South Carolina				1985	Foundation
South Dakota				1977	Power Equal.
Tennessee	1993	1993		1986	Two Tier/Comb.
Texas	1989	1973	1989	1977	Two Tier/Comb.
Utah			1975	1984	Foundation
Vermont					Foundation
Virginia				1987	Foundation
Washington	1978	1978	1978	1975	State
West Virginia	1979	1979	1979		Foundation
Wisconsin	1976	1976		1973	Power Equal.
Wyoming	1980	1980	1983		Foundation

Sources: Manwaring and Sheffrin (1977) and McCarthy (1994).