

TAX LIMIT REPEAL AND SCHOOL SPENDING

Phuong Nguyen-Hoang

This is the first study to evaluate the effects on school spending of repeal of constitutional property tax levy limits. While several studies report that the imposition of tax limits constrains local government and school district spending, this study fails to reject the null hypotheses that tax limit repeal in New York State induced no significant immediate or gradual impact on the average spending of either all districts subject to tax limits or districts that were spending at their limits. The latter finding suggests that these “at limit” districts were not constrained by tax limits, either because the desired spending levels were exactly “at limit,” or because the limits were no longer binding after the districts utilized other non-property tax revenues such as state aid, especially from the supplemental Hurd aid program.

Keywords: tax and expenditure limitations, tax limit repeal, school spending, New York

JEL Codes: H71, H72, I22

I. INTRODUCTION

The 1978 enactment of Proposition 13 in California marked the beginning of a spate of tax revolts in the United States. With the exception of Connecticut, New Hampshire, and Vermont, as of 2010 all states had enacted some form of constitutional or statutory state-imposed tax and expenditure limitation (TEL) on their local governments (Mullins, 2010).¹ Property tax levy limits, which constrain the total amount of revenue that can be generated from property taxes independent of tax rates, are the second most popular TEL measure, exceeded only by specific property tax rate limits.² Property tax levy limits (hereinafter referred to as tax limits) are imposed simultaneously on counties, municipi-

¹ This study focuses exclusively on TELs that states impose on local government budgets. These state-imposed local TELs should be distinguished from state-level TELs that apply to state government budgets (Kioko, 2011), and from TELs that local governments impose on their own budgets (Brooks and Phillips, 2010).

² Property tax levy limits and specific property tax rate limits are just two types of TELs. Other types include general revenue limits, expenditure limits, assessment increase limits, and full disclosure in taxation.

palties, and school districts in 17 states, and on either counties or municipalities in 9 states (Mullins, 2010). Given the prevalence of TELs and tax limits, the effect of their *imposition* on local government spending has received substantial research attention. However, no empirical study has examined how tax limit *repeal* affects local government or school district spending. This study is intended to fill this gap in the literature.

A principal purpose of tax limits is to restrict or reduce the growth of revenue, and thus of public expenditures. Studies have found that relative to state-level TELs, state-imposed local TELs are more effective in achieving these goals (Rose, 2010). A closely related question involves how school district spending responds to tax limit repeal. This study examines two null hypotheses regarding a repeal of TELs in New York State: (1) tax limit repeal had no discernible effect on the average spending of all school districts that were subject to tax limits; and (2) even the districts that were spending at their tax limits (hereafter referred to as “at limit” districts) showed no significant spending response to tax limit repeal. These hypotheses are tested on the repeal of tax limits imposed on small city school districts (SCSDs) in New York State (NYS). While many NYS districts have never had tax limits, 65 SCSDs’ property tax levies became subject to constitutional limits beginning in 1951. Voters subsequently revoked these limits in a statewide election in 1985.

Selection bias is a major methodological challenge in estimating the effects of a non-experimental social intervention. This bias results from estimation error due to systematic differences in observed and unobserved characteristics between treatment and comparison units. Difference-in-differences (DID) estimation combined with propensity score matching proves to be effective in addressing selection bias for causal inference in non-experimental studies (Heckman, Ichimura, and Todd, 1997). More specifically, matching and DID regression address selection bias on observable variables and unobservable variables, respectively.³ Heckman, Ichimura, and Todd (1997) showed that bias due to selection on observables is larger and thus more important than that due to selection on unobservables. This study examines the repeal of tax limits on SCSDs, adopting a DID estimation on data preprocessed with propensity score matching procedures.

Preprocessing data seeks to match SCSDs to comparison districts by propensity scores, thereby ensuring the comparability of the two district groups. A district’s propensity score represents the probability of observing the district in the treatment group. It is derived from a logistic regression of the dichotomous treatment/comparison variable on pre-repeal covariates. Those districts whose propensity scores are too high or too low to find matches are eliminated. This allows causal inferences to be drawn from a data region that covers both SCSDs and comparison districts.

³ Matching can also help address extrapolation bias and make parametric regressions on a matched dataset independent of modeling assumptions (i.e., model independence) (King and Zeng, 2007). Extrapolation bias refers to misleading inferences made from comparing the treatment and control groups in a region where data are only available for either the treatment or the comparison groups.

To preview the results, this study fails to reject both null hypotheses. The failure to reject the first null hypothesis can be readily explained by the fact that the estimated effects of tax limit repeal on school spending are attenuated toward zero by the presence in the sample of districts that were spending far below their tax limits. Failure to reject the second null hypothesis may seem counter-intuitive. A brief example, however, demonstrates how this empirical finding might be explained. Suppose that two school districts, *Constrained* and *Unconstrained*, are exhausting their tax limits. The maximum levels of expenditures per pupil that *Constrained* and *Unconstrained* are allowed to spend under their tax limits are lower than or equal to (respectively) the levels desired by the majority of local residents. Tax limit repeal induces *Constrained* to increase its spending per pupil to the desired level. In contrast, *Unconstrained* does not react to tax limit repeal because it has been able to spend at its desired level despite the tax limits. This example suggests that exhausting tax limits does not necessarily indicate constraint or imply a positive response to tax limit repeal. Only those districts constrained from reaching their preferred levels of spending by tax limits will react positively to repeal.

The rest of this paper is structured as follows. Section II provides background material on SCSDs and the constitutional property tax levy limits imposed on them. Section III discusses districts' potential responses to tax limit repeal, while Section IV reviews the existing literature on the effects of TELs on local government spending. Section V presents empirical strategies to estimate the impacts of tax limit repeal. Regression results are discussed in Section VI, and conclusions are drawn in Section VII.

II. SCSDs AND CONSTITUTIONAL TAX LIMITS

Almost all of the cities in NYS were incorporated or chartered during the nineteenth century.⁴ The incorporation of cities in the state does not depend on population size, but rather reflects the wishes of local residents (State of New York Department of State, 2009).⁵ As defined in the NYS Constitution, small city school districts are coterminous, wholly or partly, with cities that have a population of less than 125,000⁶ according to the latest federal census.⁷ These districts had no taxing power and received funding from property tax revenue collected by their city councils (together with state education aid) until 1951. That year, SCSDs were granted fiscal independence, or the power to tax

⁴ The City of Rye, chartered in 1942, is the most recently chartered city in NYS.

⁵ In fact, many towns and villages are as large as, or even larger than Buffalo, NYS's largest city other than New York City. For example, Hempstead has a 2005 estimated population of 751,276, which is more than twice that of Buffalo (New York State Department of State, 2009).

⁶ Although population size has nothing to do with the incorporation of cities, it is used for classification purposes. The population-based classifications of cities in NYS changed a few times before 1951.

⁷ The Albany City School District is the only one that became subject to the tax limits as a result of a change in its population size. The population of this district has declined below the threshold of 125,000 since the 1970 Census.

real estate within their jurisdiction. However, between 1951 and 1986, property taxes were subject to constitutional limits due to a 1949 amendment to the NYS Constitution.

During this 35-year period, annual tax levies for operating purposes in these districts were restricted to a maximum of 2 percent of the five year average full (100 percent) value of taxable property.⁸ The tax limits applied to 65 districts, including all 57 SCSDs and eight non-city districts that were coterminous, partly or wholly, with a city limit. The tax limits were repealed in November 1985 with a voter-approved amendment to the NYS Constitution. This constitutional amendment can be considered exogenous in that, according to the 1980 Census, the population of all SCSDs combined accounted only for about 11 percent of the total NYS population.⁹ The repeal became effective at the beginning of academic year 1986–1987.

The tax limit imposed on a SCSD prior to 1986 was generally based on three factors: the district's average five year full valuation, the specified percentage limits, and the exclusions (i.e., expenditures not subject to limits). In terms of the first factor, districts have no control over full valuation because property assessment is conducted by cities, towns, and villages in NYS. As regards the second factor, in 1951, percentage limits were set for SCSDs at 1.25 percent, 1.5 percent, 1.75 percent, or 2 percent, depending on the property tax levy in effect for fiscal year 1947–1948 (Table 1). SCSDs whose percentage limits were lower than the 2 percent ceiling could raise their limits gradually by 0.25 percent (but not more than once annually) if the proposed increase was approved by a 60 percent or greater majority in a general or special election (New York State Board of Equalization and Assessment, 1983). This 60 percent voting margin requirement represented a major hurdle for SCSDs. In fact, few even attempted to raise their percentage limits (Curley, 1986). Moreover, when a rate increase was put forward, voters tended not to approve the initiative. The most recent successful referendum to increase a percentage limit occurred in New Rochelle in 1968 (Knowlton, 1985).

As to the third factor, exclusions, not all expenditures financed by tax levies were subject to tax limits. Taxes raised for capital expenditures were excluded from limits. Capital items included debt service (interest and principal payments) on long-term bonds (i.e., due more than one year from issue),¹⁰ and items with “periods of probable usefulness,” such as equipment and improvements to property.

⁸ Full valuation is derived by adjusting assessed value with state-determined equalization ratios. The use of full valuation is intended to address artificial across-district variations in taxing power as a result of variations in local assessments (Galie, 1996). Also, the five-year averages are meant to minimize annual fluctuations in real estate value.

⁹ Residents in non-city districts showed their support for this constitutional amendment largely out of concern that state aid distributed to their school districts were reduced by the growth of special state aid for SCSDs (i.e., Hurd Aid, which will be discussed in greater detail in the succeeding section) (Ebdon, 1997).

¹⁰ The total bonded indebtedness of SCSDs and non-city school districts was, however, subject to constitutional debt limits of five and ten percent, respectively, of their five-year average full valuation of taxable real estate. Also, unlike long-term bonds, debt service appropriations for short-term notes (i.e., due within a year), namely tax anticipation notes (TANs), revenue anticipation notes (RANs), and budget notes, are *not* excluded from SCSDs' tax limits because they are intended to fund current operating expenses. More specifically, TANs and RANs are issued to bridge temporary cash flow gaps between daily operating spending and revenues (property taxes and state aid) that are received on a quarterly or yearly basis; budget notes are issued to cover unforeseen emergencies. The extent to which SCSDs utilized these short-term financing instruments is, however, unknown.

Table 1
65 SCSDs by Percentage Limits During 1980–1986

16 districts with a 1.25% limit			
Auburn	Canandaigua	Elmira	Fulton
Hudson	Lackawanna	<i>Lisbon</i>	Middletown
Niagara Falls	<i>Oriskany</i>	Oswego	Plattsburgh
Rensselaer	Tonawanda	Utica	<i>Westmoreland</i>
17 districts with a 1.5% limit			
Albany	Beacon	Binghamton	Cohoes
Corning	Cortland	Geneva	Hornell
<i>Lansingburgh</i>	Lockport	Oneonta	Port Jervis
Poughkeepsie	Schenectady	<i>Stockbridge</i>	Watertown
Watervliet			
22 districts with a 1.75% limit			
Dunkirk	Glen Cove	Glens Falls	<i>Glens Falls Common</i>
Gloversville	<i>Hendrick Hudson</i>	Ithaca	Jamestown
Johnstown	Kingston	Little Falls	Mechanicville
Newburgh	North Tonawanda	Ogdensburgh	Oneida
Rome	Salamanca	Saratoga Springs	Sherrill
Troy	White Plains		
10 districts with a 2% limit			
Amsterdam	Batavia	Long Beach	Mt. Vernon
New Rochelle	Norwich	Olean	Peekskill
Rye	<i>Rye Neck</i>		

Note: The 8 non-city districts in italics are subject to tax limits and are thus referred to as SCSDs.
Source: New York State Board of Equalization and Assessment (1983).

Table 2 provides an example of how tax levies subject to tax limit and the percentage of the limit exhausted are determined for a district with the maximum (2 percent) percentage limit. In this table, the percent of tax limit exhausted is computed as the ratio of the district's total tax levy (minus exclusions) to the tax levy limit. Table 3 reveals substantial variation among SCSDs regarding the extent to which they exhausted their limits between 1980 and 1986. Column 8 of Table 3 shows that during this period, more than 40 percent of the districts, on average, exhausted less than 80 percent of their tax limits; such districts were not likely to be constrained by the limits. Of the remaining districts, 10 exhausted between 95 percent and 98 percent of their tax limits, while nine districts on average used at least 98 percent of their limits.¹¹ Given that the percentage

¹¹ The percent of tax limit exhausted does not depend on a district's percentage limit. For instance, while Fulton and Long Beach districts are both considered "at limit" districts in Table 3, their percentage limit levels are 1.25 percent and 2 percent, respectively. Also, all else being equal, an "at limit" SCSD can still have a yearly increase in its revenue — and thus spending — as long as its total property value rises annually.

Table 2
Sample Tax Limit Calculation (\$Million)

(a) 5 year total full valuation	10.500
(b) 5 year average full valuation	2,100
(c) Constitutional property tax levy limit (2% of (b))	42
(d) Total tax levy	43
(e) Less total exclusions (e.g., levies for debt service)	-1.5
(f) Tax levy subject to tax limit	41.5
(g) Percent of tax limit exhausted (= (f) / (c))	98.8

of tax limit exhausted is truncated at 100 percent for seven year averages, these nine SCSDs can be considered “at limit.” (In Table 3, the remaining districts are labeled as either “near limit” or “far from limit” for ease of reference.) As will be seen in the following section, however, it is difficult to make an accurate prediction, *a priori*, as to how these “at limit” districts will respond to tax limit repeal.

III. POTENTIAL RESPONSES TO THE REPEAL OF TAX LIMITS

School district spending in the United States is typically financed by property taxes (or own-source revenue) and non-property tax revenues, such as user fees and state aid. A district is fiscally constrained when it is unable to reach its desired level of spending using *all* available funding sources. The following discussion assumes that unless state-imposed tax limits stipulate otherwise, a district’s actual spending reflects the desired level of spending. In a public-choice framework,¹² a district’s level of spending is the one desired by the majority of its local residents (i.e., as chosen by the median voter).¹³ Using the Bergstrom and Goodman (1973) definition, the median voter is the citizen with a median income who is assumed to own a house of median value.

Given this assumption, suppose that we have four SCSDs (*A*, *B*, *C* and *D*) with the same desired level of spending, E^* . However, constitutional property tax levy limits are set such that the maximum spending levels these four districts can reach are E^A ($>E^*$),

¹² The median voter framework is just one of the formal public choice voting models. As reviewed in Gill and Gainous (2002), none of the voting models perfectly describes reality. However, the arguments regarding the district’s potential reactions to tax limit repeal remain valid under a different public choice model.

¹³ A district’s actual spending can be higher than the level preferred by the median voter as a result of imperfections in political processes (e.g., infrequent elections leading to lags in the adjustment of actual expenditures to desired expenditures) (Ladd, 1978). However, Munley (1984) provided empirical findings that there was no statistically significant difference between the actual level of spending per pupil in NYS school districts and the level preferred by the median voters in those districts.

Table 3
 Distribution of SCSDs by Percent of Tax Limit Exhausted between 1980 and 1986

Percent of Tax Limit Exhausted (C)	Fiscal Year							Seven Year Averages (8)	Groups (9)
	1980 (1)	1981 (2)	1982 (3)	1983 (4)	1984 (5)	1985 (6)	1986 (7)		
C ≥ 98	9	9	17	20	21	20	18	9	At limit
95 ≤ C < 98	6	8	6	3	2	1	2	10	Near limit
90 ≤ C < 95	8	6	3	3	4	6	5	4	-Far from limit
80 ≤ C < 90	9	12	15	9	10	10	9	14	
40 ≤ C < 80	28	25	19	26	22	23	26	24	
C < 40	4	4	4	3	5	4	4	3	
Total	64	64	64	64	64	64	64	64	

Notes: This table does not include the Lisbon Central School District because it did not become subject to tax limits until 1983.

Sources: New York State Board of Equalization and Assessment (1983) for 1980–1983, New York State Education Department (1985) for 1984–1985, and author’s calculations for 1986. We followed the calculation illustrated in Table 2 to derive the 1986 values. Because data on exact total exclusions from tax limits were not available, we used the average amount of excluded expenditures in 1983 and 1985. The results were checked against the total number of districts in each grouping reported in the NYS Office of the State Comptroller’s Annual Report of Constitutional Tax Limit Data for School Districts (cited in Ebdon, 1997). Also, the estimation results reported in Tables 6 and 7 are robust to groupings based on their six year average tax limit exhaustion (1980–1985) or on their limit exhaustion in 1985 with a 14 year data sample (i.e., with a missing year of 1986).

$E^B (=E^*)$, $E^C (<E^*)$, and $E^D (<E^*)$, respectively (districts C and D will be differentiated below).¹⁴ While districts A and B can still obtain the desired spending level of E^* under tax limits, the limits constrain districts C and D to spend at their suboptimal levels of E^C and E^D , respectively. These two fiscally constrained districts may engage in various strategies to circumvent tax limits. First, if legally allowed, they may attempt to change budgeting practices. This strategy involves classifying operating budget expenses as capital expenditures by assigning them “periods of probable usefulness,” because, as indicated earlier, taxes raised for such expenses are not subject to tax limits. Two court rulings in 1974 and 1978 struck down this practice of exclusion as unconstitutional.¹⁵ SCSDs were, therefore, highly unlikely to continue this strategy after 1978.¹⁶

Second, districts C and D might improve their efficiency in the production of education to circumvent tax limits.¹⁷ Greater efficiency would allow them to provide a pre-limit level of educational services (e.g., student performance) with lower spending. While school district (in)efficiency cannot be directly observed (Duncombe and Yinger, 2007), fiscally constrained districts may try to become more efficient by cutting back on non-essential or administrative spending while preserving essential or instructional spending.¹⁸ This scenario does not seem to be the case for “at limit” SCSDs in column

¹⁴ One might consider why voters support TELs imposed on school districts in state elections if school spending reflects the local median voter’s preference for school services. TELs may be passed first out of concern that district officials are budget maximizers (Niskanen, 1971) and agenda-setters. As argued in Romer and Rosenthal (1979) and empirically supported in Holcombe and Kenny (2008), district officials may take advantage of limited choice in up-or-down budget referendums to obtain a higher spending outcome. Although this spending outcome reflects the preference of the median voter, it may still be higher than the median voter’s most preferred level. District officials may also time election dates strategically to change the identity of the median voter for a pro-spending outcome (Meredith, 2009). Second, voters who support TELs may prefer lower taxes and more efficiency in government with the same level of services not only for their own jurisdiction (Ladd and Wilson, 1982, 1983) but also for other jurisdictions in the state (Vigdor, 2004).

¹⁵ State legislation passed in 1969 allowed SCSDs to treat spending on social security and pensions as excludable “capital” items. Other legislation was adopted in 1973 to allow SCSDs to treat health and dental payments similarly. The NYS Court of Appeals declared these exclusions unconstitutional in *Hurd v. City of Buffalo* (1974), and *Bethlehem Steel Corporation v. Board of Education, City School District of Lackawanna* (1978).

¹⁶ The city school district of New Rochelle provides an extreme example of the impact of the 1974 court decision. For this district, the decision meant that \$4 million (of its total \$25 million school budget) for excludable social security and pension payments had to be financed within its tax limit of two percent. It resulted in a cut of so many teaching positions that Superintendent Robert Spillane exclaimed, “It’s a disaster!” (Greenhouse, 1974, p. 30).

¹⁷ This strategy is relevant only for inefficient districts. A district is inefficient if it deviates from the best available technology, which usually means the best teaching methods and management practices. Examples of bad management practices include paying teachers more than the prevailing market wage, or spending on expensive junkets for district officials or staff (Duncombe and Yinger, 2011).

¹⁸ This allocation of school inputs in times of fiscal constraint, however, does not always necessarily imply greater school district efficiency (i.e., the ability to produce unchanged student outcomes for lower levels of spending) (Downes and Figlio, 2008).

8 of Table 3. The pre-repeal annual trend of administrative spending¹⁹ as a share of total operating expenditures for these “at limit” SCSDs was similar to that for SCSDs in the “far from limit” group or for non-city school districts.²⁰ Specifically, the average administrative share of total operating spending for these three district groups went up to 5.8 percent, 5.8 percent, and 6.5 percent in 1986 from 5 percent, 5.2 percent, and 6.1 percent in 1985, respectively.

Third, districts *C* and *D* could utilize non-property tax revenues, such as non-property taxes, user charges, and federal or state aid. Local governments were found to utilize more user charges, federal aid, and state aid in response to the imposition of TELs (Shadbegian, 1999; Skidmore, 1999). Counties in NYS may choose to share their retail sales tax with other within-county jurisdictions. By 1985, however, only seven (out of 62) counties shared a portion of their sales tax revenues with school districts within their jurisdictions. Moreover, only two SCSDs in Erie County (Lackawanna and Tonawanda) received sales tax revenues transferred from the county. SCSDs may also choose to impose a consumer utility tax of up to 3 percent (i.e., a form of user charge); however, only thirteen SCSDs chose to do so (Curley, 1986). All in all, non-property taxes represented a very small portion (1.1 percent) of SCSDs’ total revenue over the sample period.

Relative to non-property taxes, federal and especially state aid played a larger role in financing school districts in NYS. As with non-city districts, SCSDs, either “at limit” or “far from limit” districts, experienced declines in federal aid as a share of total operating expenditures during the pre-repeal period.²¹ SCSDs relied more on state aid, especially supplemental aid called Hurd aid, for additional revenue.²² This special state aid, which is in addition to regular formula-based state education aid available for all districts,²³ is appropriated just for SCSDs. Table 4 shows that during the period 1980–1986, while regular state aid to SCSDs remained stable at between 43 percent and 44 percent of total expenditures, the total amount of Hurd aid increased from \$9.645 million to \$95.6 million, with the number of recipient SCSDs rising from 20 to 49. The ratio of Hurd aid to regular state aid distributed to all SCSDs increased from 2.7 percent in 1980 to 17.2 percent in 1986.

There are two important features of Hurd aid. First, Hurd aid was not discontinued immediately after tax limit repeal but instead declined gradually. A bill signed by Gov-

¹⁹ Administrative spending is expenditures for board of education and executive administration services.

²⁰ The comparison school districts in this section are SCSDs that exhausted less than 90 percent of tax limits and non-city districts retained from the matching process described in Section V.

²¹ This federal aid share for “at limit” SCSDs, “far from limit” SCSDs, and non-city districts declined steadily from 7.6 percent, 6.7 percent, and 5.1 percent in 1980 down to 4.8 percent, 4.2 percent, and 3.5 percent in 1986, respectively.

²² Hurd aid reflects additional state aid made available to SCSDs after a 1974 lawsuit, *Hurd v. City of Buffalo*, in which a taxpayer named Hurd successfully claimed that he had been over-taxed.

²³ Regular state aid was basically distributed using a per-capita formula that took into account the value of a district’s taxable property (Suchecky, 1987).

Table 4
Hurd Aid and Regular State Aid for SCSDs During 1980–1986

Year	Hurd Aid (\$Million)	Number of SCSDs Receiving Hurd Aid	Regular State Aid to SCSDs (\$Million)	Hurd Aid as a Percentage of Regular State Aid	Total Expenditures of SCSDs (\$Million)	Regular State Aid as a Percentage of Total Operating Expenditures
(1)	(2)	(3)	(1)/(3)	(4)	(3)/(4)	
1980	9.6	20	361.9	2.7	823.2	44.0
1981	22.9	22	392.9	5.8	886.4	44.3
1982	40.3	35	414.2	9.7	950.2	43.6
1983	49.2	39	439.8	11.2	1,015.8	43.3
1984	57.2	45	459.7	12.4	1,081.5	42.5
1985	78.6	49	500.3	15.7	1,164.4	43.0
1986	95.6	49	556.8	17.2	1,275.4	43.7

Source: Knowlton (1985) for Hurd aid and author's calculations.

ernor Mario Cuomo in August 1987 guaranteed that total Hurd aid appropriations to all SCSDs would decrease no more than 2 percent each year (Spencer, 1987).²⁴ Second, the distribution of this aid is not formula-based. Knowlton (1985, p. 87) noted from interviews with state and school officials that the aid was more readily available to SCSDs with greater access to “members of the majority party in one or ideally both houses of the legislature,” and that the need for Hurd aid was justified independently of the tax limit issue. In 1985,²⁵ while 18 “at limit” SCSDs in column 7 of Table 3 received some Hurd aid, the aid was also distributed to three SCSDs that used less than 40 percent of their tax limit (i.e., Ogdensburg, Hornell, and Salamanca) (New York State Education Department, 1985).²⁶

Although there was no clear relationship between the amount of Hurd aid per pupil and the degree of constraint, a few “at limit” SCSDs managed to increase Hurd aid over time. For instance, Poughkeepsie and Peekskill, which are two of the nine “at limit” SCSDs in column 8 of Table 3, received greatly increased Hurd aid per pupil over time. While Poughkeepsie’s Hurd aid per pupil in 1985 dollars was \$326, \$896, and \$1,553 in 1980, 1982, and 1985 respectively, the real Hurd aid per pupil distributed to Peekskill increased from \$31 in 1980 to \$794 in 1982, to as high as \$1,915 in 1985. In short, Hurd aid was the highest potential non-property tax revenue source that could help districts *C* and *D* achieve their desired levels of spending under tax limits.²⁷

Now suppose that district *C* is able to obtain additional Hurd aid and/or adopt any other strategy (e.g., improving efficiency) such that this previously constrained district becomes unconstrained under tax limits ($E^C = E^*$). In other words, district *D* is the only one that is still constrained by tax limits from reaching its desired spending level. When the tax limits are repealed, districts *A*, *B* and *C* do not react to tax limit repeal because they are able to finance their educational services at their desired level, E^* , with or without tax limits. More importantly, of the three “at limit” districts (*B*, *C*, and *D*), only district *D*, all other things equal, is predicted to respond to tax limit repeal

²⁴ This does not necessarily mean that all SCSDs get a two-percent reduction in Hurd aid across the board. Also, a SCSD may have a decrease in Hurd aid and an increase in regular state aid. For example, the Hudson City School District’s 1988–1989 budget reflected an increase in regular state aid from \$6.45 million to \$6.83 million and a decrease in Hurd aid to \$1.47 million from \$1.53 million (Sheffer, 1988).

²⁵ The most recent district-level data on Hurd aid is available for 1985.

²⁶ Knowlton (1985) also found no significant differences between Hurd aid recipient and non-recipient SCSDs on several characteristics such as (minority) enrollment, total tax rates, and adjusted gross income.

²⁷ Aside from these three strategies, local residents in districts *C* and *D* who would prefer greater spending could, of course, “vote with their feet” and move to an unconstrained district or state. Such flight will change the demographic composition of the district, and could thus make the constrained level of spending more reflective of the wishes of the populace (e.g., $E^C = E^*$). “At limit” SCSDs’ annual average enrollment, which was highly correlated with population, declined during the pre-repeal period of 1980–1986. The mean annual enrollment of nine “at limit” SCSDs in column 8 of Table 3 fell by between 1 percent and 4.6 percent during this period. However, these enrollment decreases were highly unlikely to be associated primarily with fiscal constraints facing the “at limit” districts. Other SCSDs and non-city districts, on average, also experienced enrollment decreases during this time. More importantly, the mean enrollment changes were not statistically different from those for other SCSDs or for non-city districts.

by raising spending per pupil to the desired level (E^*), which the tax limit kept it from reaching.²⁸

One way to measure the degree of constraint imposed by a TEL on local governments is to determine how close such governments are to their tax limits. For example, Bradbury, Mayer, and Case (2001) consider a community to be fiscally constrained if it is at its levy limit, which is defined as a levy within 0.1 percent of levy limit. The above discussion suggests, however, that exhausting tax limits does not necessarily indicate constraint. While some “at limit” local governments that are unable to achieve a desired level of spending under tax limits will increase spending upon tax limit repeal, others are predicted *not* to respond to the repeal if they were not fiscally constrained by tax limits (i.e., they were able to reach their preferred expenditure levels under the limits). They were fiscally unconstrained either because the limits were set at or near the preferred spending levels, or because they responded to the limits by utilizing additional revenue sources or changing their practices in such a way that made the limits no longer binding.

IV. LITERATURE REVIEW

Although no study has considered the fiscal effects of the repeal of a state-imposed TEL, the impact of TELs on local government expenditures, including school spending, has been a major research topic.²⁹ TEL studies can either reflect a cross-section of states or be state-specific. Earlier state cross-section studies found little or no effect of TELs on government spending (Advisory Commission on Intergovernmental Relations, 1977; Lowery, 1983). Later studies with better-identified models³⁰ found that state-imposed TELs led to a decrease in local government revenue (Preston and Ichniowski, 1991; Shadbegian, 1999), and thus to a decline in spending (Shadbegian, 1998, 2003).

Since this study looks into property tax levy limits in NYS, we will now narrow the focus to state-specific studies that examine the effects on local government spending of state-imposed property tax levy limits and of property tax rate limits combined with assessment restrictions. Joyce and Mullins (1991) classify tax levy limits and assessment-plus-rate limits as “potentially binding” TELs.³¹ Several studies explore the fiscal effects of California’s 1978 Proposition 13, which imposed both rate and assessment limits on local governments, and Massachusetts’s Proposition 2½, which limited annual property tax levy growth and effective property tax rate to 2.5 percent starting

²⁸ This section does not address when “at limit” local governments can be expected to be constrained by tax limits. The important issue of equilibrium expectation is beyond the scope of this paper and merits future research.

²⁹ Studies on state-level TELs, such as Abrams and Dougan (1986), Kousser, McCubbins, and Moule (2008), and Shadbegian (1996), are not reviewed.

³⁰ These studies included population and income variables missing in the earlier studies.

³¹ Limitations on either tax rates or assessments alone may be circumvented by alterations in assessment practices or increases in tax rates, respectively. Limits on both assessment increases and tax rates are equivalent to restricting the property tax levy.

in fiscal year 1982. Galles, Long, and Sexton (1995) found that the annual growth rate of the real per capita expenditures for all California local governments averaged 1.98 percent between 1979 and 1990, which was smaller than 2.85 percent from 1960 to 1978. It was not until 1989 that the average real per capita spending of California cities and counties exceeded the pre-Proposition 13 peak level in 1977 (Galles and Sexton, 1998).³² These two papers, however, attributed this slow spending growth more to the economic recession of the early 1980s than to Proposition 13.

Unlike California's Proposition 13, students of TELs are more in agreement regarding the constraining effects of Proposition 2½ in Massachusetts on local government spending. Hale (1993) found that average real municipal spending in Massachusetts might have increased annually by more than 1.8 percent without Proposition 2½. Bradbury, Mayer, and Case (2001) also showed that between 1990 and 1994, a town whose levy was 5 percent below the levy limit imposed by Proposition 2½ raised spending 2 percentage points more than an "at limit" town (one that was 0.1 percent or less below the limit). However, these studies do not necessarily provide conclusive evidence on whether either of the propositions constrained local governments in California or Massachusetts. More specifically, a town that is spending at limit is unconstrained (i.e., similar to district *B* in the preceding section) if the "at limit" level corresponds to the one that would have been chosen by the majority of residents. It could also be the case that the propositions in California and Massachusetts became no longer binding after local governments, as in the case of district *C* discussed above, managed to offset property tax revenue losses with revenues from other sources such as user fees (e.g., California cities (Reid, 1988)), or state aid (e.g., Massachusetts school districts (Shadbegian and Jones, 2005)).

Other studies examine school districts' response to TELs through their allocation of expenditures by function (i.e., administrative or instructional spending). Dye and McGuire (1997) found that the cap limiting growth of property taxes in jurisdictions in five metropolitan counties surrounding Cook County, Illinois, reduced school districts' total operating expenditures but had no effect on their instructional expenditures. This finding implies that districts subject to the cap maintained instructional spending at the expense of administrative spending, which indicates improved efficiency. Examining the same cap as Dye and McGuire (1997), Dye, McGuire, and McMillen (2005) found that both total operating and instructional expenditures of the capped districts in Illinois declined by about 1.2 percent in the long run. The authors argued that the districts' ability to protect essential or instructional spending from the effects of the cap eroded over time. Figlio (1998) likewise concluded that a 1990 Oregon tax limit that capped property tax rates to a specific percentage of the property's fair market assessed values induced the districts to reduce as much instructional as administrative spending. All else equal, the capped districts in Illinois and Oregon, like district *D* in the earlier example, would have raised their spending had the caps been repealed.

³² The effects of Proposition 13 on school district spending are not reviewed because scholars argue that the court case *Serrano v. Priest*, not Proposition 13, was the major cause of declines in educational expenditures (Fischel, 1989; Silva and Sonstelie, 1995).

Ebdon (1997) is the only previous study to consider the policy intervention highlighted in this study (i.e., the 1986 NYS repeal of tax limits on SCSDs). Ebdon found that property tax levy limits in the state lowered spending in SCSDs by 2 percent during the period 1984–1986. The current study differs from Ebdon in several ways, however. First, Ebdon (1997) was interested primarily in the effect of tax limits on school expenditures, rather than repeal of such limits on spending. Second, the Ebdon study suffered from some data and methodological weaknesses. The current study employs a longer data panel (1980 to 1994 rather than 1984 to 1990). Post-repeal growth trends in spending manifest themselves more clearly with longer panel data. Furthermore, Ebdon (1997) did not employ matching to deal with selection on observables, and did not account for growth trends in spending or the time-invariant unobservables that may bias the DID estimator.³³

V. EMPIRICAL STRATEGY

This study employs panel data over 15 years (1980–1994). The full dataset contains 64 SCSDs³⁴ and 631 comparison districts (627 non-city³⁵ and 4 large city districts).³⁶ (The last column of Table 5 reports the data sources). The repeal of tax limits on these 64 SCSDs in 1986 can be analyzed with difference-in-differences (DID) estimation procedures. The DID identification strategy removes biases from unobserved time-invariant differences between the treatment and comparison groups (when treatment and comparison districts are compared only in the post period), or from trends (when comparisons over time only in the treatment group are made). The DID estimator measures the impact of tax limit repeal by comparing the before-and-after mean differences in total operating education expenditures per pupil between SCSDs and comparison school districts in NYS that never had tax limits (and thus underwent no policy change). For this estimator to be valid, pre-intervention trends in expenditures per pupil must be similar for SCSDs and comparison districts (i.e., the common trend assumption). This assumption may not hold if SCSDs and comparison districts do not have similar distributions of observed attributes. Similar distributions of observed attributes for both district groups are a key non-experimental data feature for valid causal inference (Heckman, Ichimura, and Todd, 1997).

³³ Ebdon (1997) adopted a random effects specification for the full data sample.

³⁴ The Lisbon Central School District, which is in part coterminous with the city of Ogdensburg, did not become subject to constitutional tax limits until fiscal year 1982–1983 as a result of an incorporation of part of Ogdensburg. It was deleted as a SCSD from the dataset to ensure the consistency during the sample years 1980 to 1994.

³⁵ Given that district consolidations are found to have substantial expenditure implications (Duncombe and Yinger, 2007), 30 non-city districts (or about 4.5 percent of the total non-city districts) that were consolidated during the sample period are eliminated from the dataset.

³⁶ The four large city districts are Buffalo, Rochester, Syracuse, and Yonkers. The fifth large city district, New York City, is not included because data during the sample years were not available. All four large city and other non-city districts will be referred to as non-city districts for simplicity.

Table 5
Descriptive Statistics

	All Data				Matched Data				
	Means of		Standardized		Means of		Standardized		
	SCSDs	Non-City Districts	Differences	Mean	SCSDs	Non-City Districts	Mean Differences	Data Sources	
Pre-repeal controls used in the propensity score estimation									
Enrollment	4,325	2,568	0.64		4,325	4,338	-0.04	(1)	
Percentage of limited English proficiency students	1.68	0.82	0.49		1.68	1.9	-0.14	(1)	
State aid per pupil (\$)	2,855	2,740	0.13		2,855	2,834	0.06	(2)	
Federal aid per pupil (\$)	306	222	0.69		305	290	0.15	(2)	
Tax price (\$)	28.6	28	0.07		28.6	28.7	-0.06	(2), (3)	
Median household income (\$)	29,217	36,392	-0.95		29,217	30,892	-0.13	(3)	
Percentage of population that graduated from college	14.7	16.9	-0.29		14.7	13.5	0.15	(3)	
Percentage of senior citizens (age 65 or over)	14.4	11.8	0.95		14.4	14.2	0.07	(3)	
Percentage of youths (age 6-17)	18.3	21.3	-1.14		18.3	18.33	-0.01	(3)	
Percentage of African Americans	5.22	2.61	0.33		5.22	6.13	-0.11	(3)	
Percentage of urban population	45.2	33.1	0.25		45.2	50.8	-0.12	(3)	
Percentage of owner-occupied housing units (<i>Owner</i>)	58.5	76.9	-1.63		58.5	57.7	0.07	(3)	
Enroll × <i>Owner</i>	242,700	191,300	0.35		242,700	248,888	-0.04		
Propensity scores	0.543	0.05	1.59		0.543	0.542	0.005		
Dependent variable that is not specified in the propensity score estimation for the sample period (1980-1994)									
Total operating expenditures per pupil	7,023	7,888			7,023	7,210		(1)	
Number of observations	64	631			64	495			

Notes: While the covariates in italics are taken from 1980 Census, all of the remaining covariates are average values of pre-treatment years (1980-1986). Data on monetary variables (namely expenditures, state and federal aid, tax price, median household income) are inflation-adjusted (using government price indexes published the Bureau of Economic Analysis) and in 1994 dollars. The tax price is measured as the ratio of median housing price (from Census) to total taxable property values per pupil (NYS Office of the State Comptroller). State aid includes receipts from all types of state aid available to school districts (including Hurd aid for SCSDs). The covariate on the percentage of urban population can be balanced because the incorporation of NYS cities with which SCSDs are associated does not depend on population. While all SCSDs find their matches, 136 comparison districts whose propensity scores lie outside the common support region are discarded.

Sources: (1) NYS Department of Education, (<http://www.p12.nysed.gov/irs/>); (2) NYS Office of the State Comptroller, (<http://www.osc.state.ny.us/localgov/index.htm>); (3) U.S. Census. While data from the 1990 and 2000 Censuses at the school district level are available on the websites of the National Center for Education Statistics, (<http://nces.ed.gov/surveys/sdds/dlmain90.asp>), the studies numbered 3517 and 3518 at the Inter-University Consortium for Political and Social Research, (<http://www.icpsr.umich.edu/icpsrweb/landing.jsp>), contain the school district-level data from the 1980 Census.

This study combines DID regression with propensity score matching, which is the recommended practice for the estimation of causal effects for non-experimental studies (Heckman, Ichimura, and Todd, 1997; Rubin, 1979). The goal of matching is to use estimated propensity scores to find comparison districts that look as much as possible like SCSDs. Matching reduces both the variance of the DID estimator (Ho et al., 2007) and selection bias due to observable pre-existing differences between SCSDs and comparison districts, thereby helping ensure the validity of the common trend assumption.³⁷ The DID regression on matched samples accounts for unobserved time-invariant differences, and adjusts for small remaining observable differences.

Matching involves two major steps. The first is estimation of propensity scores. As introduced in Rosenbaum and Rubin (1983), propensity scores indicate the propensity or probability that a district with specific characteristics will be observed in the treatment group. The scores are estimated non-parametrically (i.e., without specifying a functional form) with a logistic regression of a dummy variable (SCSDs = 1, Others = 0) on a constant term, and all pre-treatment covariates.³⁸ These covariates must be balanced in the two district groups to get a consistent estimate of propensity scores. Balancing tests are based on the covariates' standardized mean differences, also known as standardized bias. The standardized bias is defined as the weighted difference in means divided by the standard deviation in the treatment group (Ho et al., 2011). Two groups are considered well matched when all of their covariates have standardized biases of less than 0.25 (Ho et al., 2007). In other words, SCSDs and their matches now have similar distributions of observed attributes.

The second step involves finding matches for SCSDs using a matching algorithm and the estimated propensity scores. This study employs the full (optimal) matching algorithm introduced in Rosenbaum (1991), because full matching has been proven to perform better than other matching algorithms in minimizing variance and bias (Gu and Rosenbaum, 1993; Hansen, 2004). Based on the estimated propensity scores, full matching divides all of the districts in the common support region into a collection of matched sets in which a SCSD may have one or more comparison districts, or vice versa. This matching method then assigns weights to member districts in the matched

³⁷ Matching does not address selection on unobservables explicitly. However, once matched, SCSDs and comparison districts are assumed to have no significant differences in unobservable characteristics. If this assumption does not hold, the DID regression will have already eliminated potential bias due to time-invariant unobservables. Also, as indicated earlier, the constitutional amendment repealing tax limits on SCSDs, which was approved by a majority of *all* voters in NYS, can be considered an exogenous shock to the whole state education system. Therefore, remaining bias after the combination of matching and regression caused by the correlation between time-varying unobservables and treatment is highly likely to be minimal.

³⁸ This step also eliminates both comparison districts with propensity scores beyond SCSDs' minimum score and any SCSD with propensity scores greater than the comparison group's maximum score. The remaining SCSDs and matches have overlapping propensity scores in the so-called region of common support. This elimination process helps address extrapolation bias.

sets. A weight of 1 is assigned to a SCSD, whereas a comparison district in a matched set gets a weight equal to the number of SCSDs in the set divided by the number of comparison districts in that set. For instance, each comparison district in a matched set with one SCSD and three comparison districts would receive a weight of one third. Similarly, a weight of three halves would be assigned to two comparison districts matched to three SCSDs.

Table 5 shows that SCSDs are indeed substantially different from the comparison group. The standardized mean differences of many pre-treatment covariates in the original full dataset are larger than the recommended threshold of 0.25, whereas those in the matched dataset lie within the threshold. Also, while the propensity scores of all SCSDs are in the common support region, 136 (or 21.6 percent) of the comparison districts could not be matched with a SCSD because of their low propensity scores. These districts were discarded.

As a summary of the empirical strategy, we implement full matching with the *MatchIt* package in the R statistical software (Ho et al., 2011). The matching procedures produce the common-support weighted data subset. This data subset is then exported into Stata for the weighted DID regression estimation specified as follows.

Suppose Y_{it} is the total operating expenditures per pupil at time t of school district i . The standard DID estimation for panel data can be represented by a reduced-form expenditure function

$$(1) \quad Y_{it} = a + \pi_i + \tau_t + \gamma X_{it} + \delta T + \theta(T \times D_{it}) + \varphi(R \times T) + \alpha(D_{it} \times R) + \beta(D_{it} \times R \times T) + \varepsilon_{it}$$

where a is a constant and ε is the error term. School district fixed effects, π_i , control for time-invariant district-specific unobservables (including unobserved efficiency³⁹ as in Downes and Pogue (1994)),⁴⁰ whereas τ_t represents a vector of year dummies controlling for annual macroeconomic shocks influencing all school districts. With subscripts dropped, X represents a vector of control variables. The linear trend variable, T , which is coded from 1 to 15 for sample years 1980–1994, accounts for the underlying trend in school spending.⁴¹ A clear spending trend, if uncontrolled, would threaten the internal validity of parameter estimates (Langbein and Felbinger, 2006). The dummy variable

³⁹ While fixed effects control for temporally stable efficiency, scholars may use some of the variables in X (e.g., state aid, tax price) to control for efficiency that varies over time (Duncombe and Yinger, 2011). For ease of interpretation, the efficiency effects of these variables are disregarded.

⁴⁰ These fixed effects also account for potential time-invariant differences in budget approval practices. It was not until 1998 that 57 out of 65 SCSDs had a budget vote, which was the norm in non-city school districts. More specifically, budgets were determined by school board members after a public hearing during the sample years of this study (1980–1994). Nguyen-Hoang (2012) reports that the 1998 introduction of budget referendums in these 57 SCSDs induced them to cut back on school spending. Since budget practices did not change for any district during the sample period, they can be plausibly assumed to be accounted for by fixed effects.

⁴¹ A squared trend variable is not specified because the quadratic term is not statistically significant if included.

D is equal to 1 for SCSDs that received the intervention (i.e., tax limit repeal), and 0 for the remaining comparison districts that did not receive the intervention. The dummy variable R is equal to 1 for the post-repeal period and 0 for the pre-repeal period.⁴²

The specification of (1) is intended to explore the potential effects of tax limit repeal on SCSDs' spending per pupil in terms of changes in both intercept and slope.⁴³ Figure 1 illustrates how the major coefficients in (1) are interpreted in a scenario in which tax limit repeal induces the intercept shift of, and the slope change in, SCSDs' spending per pupil. While the coefficient δ shows the annual trend in spending for comparison school districts in the pre-repeal period, θ provides a test of whether the spending trends of SCSDs and comparison districts are similar during this period. In this example, θ is assumed to be equal to 0, which satisfies the common trend assumption, φ represents the common trend shift in the post-repeal period, and α and β , which are the coefficients of primary interest, indicate the intercept shift and slope (or trend) change, respectively, for SCSDs' total operating expenditures per pupil in the post-repeal period. More specifically, while α indicates an immediate effect of tax limit repeal on school spending, β represents the gradual impact on SCSDs' spending trends of an additional increment in spending per pupil for each passing year after the repeal.

The control variables in X consist of student characteristics, income variables, tax price, and variables representing demographics. Spending per pupil depends on the size of the student body. Studies reviewed in Fox (1981) and Andrews, Duncombe, and Yinger (2002) provide empirical evidence that economies of size in education may help larger districts incur smaller costs per pupil.⁴⁴ The variable in X reflecting the number of disadvantaged students is the percent of limited English proficiency (LEP) students.⁴⁵ Income variables in X are median household income, and total federal and state education aid per pupil. Higher median income is predicted to lead to greater school spending.⁴⁶ Relative to private income, federal and state aid can have a stronger positive impact

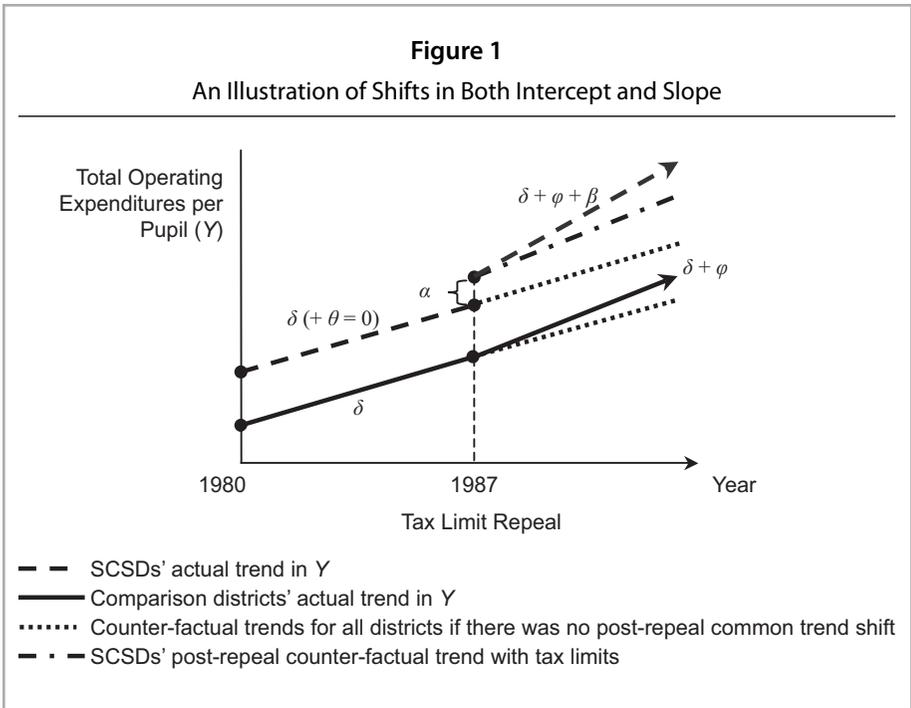
⁴² Unlike the DID specification for a data panel of length 2, (1) does not specify D or R as separate variables. They will, if included, be dropped because of their collinearity with π_i or τ_t , respectively. The exclusion of D does not mean that the two groups of SCSDs and comparison districts are constrained to the same pre-repeal intercept. Rather, district fixed effects, π_i , allow for each district to have their own pre-repeal school spending levels.

⁴³ The model reflected in (1) is similar to Langbein and Felbinger's (2006) interrupted time-series comparison-group model.

⁴⁴ Diseconomies of scale start to set in at an enrollment of about 6,000 (Andrews, Duncombe, and Yinger, 2002). Our data, however, do not allow identification of this threshold of scale diseconomies since squared enrollment is insignificant had the wrong sign if included.

⁴⁵ The vector X does not specify the share of low-income (or free-lunch) students. Data on student poverty levels are only available from 1987, which is the first year of the post-repeal period. The results on the key DID estimators (trend and slope shifts) do not change if the percent of low-income students is included on the assumption that its value in the pre-repeal period is equal to that in 1987.

⁴⁶ It should be noted that this effect is ambiguous, as higher income may lead to switching to private schools and thus lower demand for public schools.



on educational expenditures as a result of the flypaper effect, whereby “money sticks where it hits.”⁴⁷ However, a district may increase its spending by less than \$1 in receipt of one dollar of intergovernmental aid because greater aid may induce the district to provide its residents with some property tax relief (Duncombe and Yinger, 2001). Tax price measured as the ratio of median housing price to total taxable property values per pupil⁴⁸ indicates the median voter’s cost of a \$1 increase in district spending per pupil. Tax price is expected to have a negative relationship with school expenditures.

Finally, four demographic variables are included in X : the share of the population that (1) graduated from college, (2) own and occupy a housing unit, (3) are senior citizens (age 65 and over), and (4) are youths (ages 6–17). People in these population groups may have different preferences for school spending. For instance, people with a college

⁴⁷ Existing studies have provided theoretical explanations for, and empirical evidence of, the flypaper effect — a phenomenon in which intergovernmental grants increase public spending more than an equivalent rise in personal income (Deller and Maher, 2005; Hines and Thaler, 1995; Roemer and Silvestre, 2002).

⁴⁸ Some scholars (e.g., Wang, Duncombe, and Yinger (2011)) call this ratio the tax share. However, other studies (e.g., Romer and Rosenthal (1982), Santerre (1989)) define tax share as median housing price divided by total taxable property (i.e., the tax price is equal to the product of tax share and enrollment).

education may desire greater school spending (Bergstrom, Rubinfeld, and Shapiro, 1982; Hilber and Mayer, 2009). Homeowners are also expected to prefer higher educational expenditures because school quality is capitalized into their property values (Nguyen-Hoang and Yinger, 2011). The effect on school spending of a high percentage of senior citizens (who usually do not live with children) may be neutral (Fletcher and Kenny, 2008), negative (Cattaneo and Wolter, 2009; Harris, Evans, and Schwab, 2001), or positive, depending on the expected capitalization of school spending into house values and intergenerational altruism (Brunner and Balsdon, 2004; Hilber and Mayer, 2009).

The above strategy is used to investigate the first null hypothesis — that tax limit repeal had no significant effect on the average spending of all SCSDs. As discussed previously, a district that completely exhausts its tax limit may have a different response to tax limit repeal depending on whether it is truly under fiscal constraint. Only fiscally constrained, “at limit” districts will react positively to tax limit repeal. To examine the second null hypothesis — that “at limit” SCSDs also do not respond to tax limit repeal — additional DID regressions are estimated using (1) with different codings for the dummy variable, D , for both treatment SCSDs ($D = 1$) and comparison districts ($D = 0$). The treatment districts are “at limit” SCSDs that exhausted at least 98 percent of their tax limits either in the last year prior to the repeal (column 7 of Table 3), or on average in the pre-repeal period (column 8 of Table 3).⁴⁹ These “at limit” SCSDs are then compared with three different groups of comparison districts: (1) SCSDs that exhausted less than 90 percent of their tax limits;⁵⁰ (2) matched non-city school districts only; and (3) matched non-city school districts and these “far from limit” SCSDs.⁵¹ Employing different groupings of “at limit” SCSDs and comparison districts provides a check of the robustness of the estimation results.

Ho et al. (2007) argue that because pre-treatment variables are treated as fixed and exogenous in the propensity score estimation, researchers can compute standard errors in parametric regressions using the same methods applied to raw and matched data.⁵² Robust standard errors, which are valid for an estimation combining regression with matching (Emsley et al., 2008), are used to test hypotheses in all of the DID regressions.⁵³

⁴⁹ The 18 “at limit” SCSDs in column 7 include nine “at limit” and eight (out of 10) “near limit” SCSDs in column 8. The 16 out of the 18 “at limit” SCSDs exhausted at least 99 percent of their tax limits.

⁵⁰ Although these “far from limit” SCSDs underwent a policy intervention, their potential unresponsiveness to tax limit repeal makes it plausible to use them as comparison districts. It is still a failure to reject the second null hypothesis if SCSDs that exhausted less than 80 percent (instead of 90 percent) of their tax limits are used.

⁵¹ Using the matching procedures described earlier, we re-estimate propensity scores to obtain separate weighted matched datasets for the latter two comparison groups. In the interest of space, the balancing results for these matched datasets (similar to the first panel of Table 5) are not reported.

⁵² Joffe et al. (2004) provides an example of this practice.

⁵³ Diagnostic tests in Stata (2009) and Drukker (2003) indicated that the matched dataset for DID regressions has cross-sectional heteroskedasticity and within-panel serial correlation, for which robust standard errors are particularly appropriate. Also, the robust or cluster-robust variance-covariance estimators in Stata, produce identical standard errors.

VI. RESULTS

We do not find robust empirical evidence (reported in Table 6) to reject the first null hypothesis that tax limit repeal had no discernible effects on SCSD's spending. In column 1 of this table, the DID estimators with the full unmatched dataset indicate the highly significant effect of tax limit repeal on school spending. Specifically, while tax limit repeal in 1986 led to an immediate increase in 1987 of about \$832 in educational spending per pupil among SCSDs, there was an annual post 1987 decrease of about \$91 in spending per pupil. These results are potentially biased, however, due to the substantial differences between SCSDs and comparison districts reported in Table 5.

Bias in the DID model without matching is minimized if not eliminated when the DID regression is estimated with matching weights on the matched dataset. Column 2 in Table 6 presents the result of the DID regression combined with matching. The two intercept and slope DID estimators, α and β , are no longer statistically significant. This finding indicates that the tax limit repeal did not produce a significant immediate or gradual impact on SCSDs' per pupil school spending, and implies a failure to reject the first null hypothesis.

The coefficients on most of the control variables in column 2 of Table 6 reflect the expected effects on school spending. Unlike the full dataset estimation, the coefficient on $T \times D$, or θ in (1), is not significant, which indicates that SCSDs and comparison districts have the same pre-repeal spending trends (confirming the validity of the common trend assumption). Enrollment has a significant negative impact on educational expenditures per pupil. A one-dollar rise in state or federal aid per pupil leads to an increase of about 33 cents or 52 cents in per pupil spending, respectively. The sizes of these coefficients fall within the range of 20 cents to 90 cents found in previous studies on intergovernmental education aid (Fisher and Papke, 2000). Put differently, school districts in NYS provided a substantial amount of local property tax relief in response to an increase in state and federal aid. While tax price appears to have no significant effect on school spending, an increase in household income leads to a rise in school spending.⁵⁴

Finally, higher proportions of home ownership and people with college educations are associated with greater school spending. School district spending per pupil goes up by \$47 for a 1 percentage point increase in the share of homeowners. Similarly, a 1 percentage point increase in the number of people with a college education leads to an increase of about \$81 in per pupil spending. Greater proportions of youths or senior people in a district do not appear to have significant effects on school spending.

The lack of a significant immediate or gradual effect of tax limit repeal on school spending evident in column 2 of Table 6 may be due to the attenuation towards zero of those districts in the "far from limit" group of SCSDs in Table 3. Relative to these district groups, one would expect "at limit" SCSDs to be more likely to raise their educational expenditures in response to tax limit repeal. However, the empirical evidence in Table 7 does not support this hypothesis. Columns 1 and 2 of this table shows that

⁵⁴ That the coefficients of state and federal aid are larger than that of median household income indicates that the flypaper effect is at work.

Table 6
 Estimation Results for All SCSDs (1980–1994)
 (Dependent Variable: Total Operating Expenditures Per Pupil)

Variables	With Full Dataset (1)	With Matched Dataset (2)
$D \times R$ (DID estimator for an intercept shift) (α)	832.2*** (236.6)	33.58 (384.5)
$D \times R \times T$ (DID estimator for a slope change) (β)	-91.39*** (26.22)	-23.19 (45.31)
$T(\delta)$	72.95** (34.16)	86.24*** (27.90)
$T \times D(\theta)$	36.96** (15.29)	17.02 (25.42)
$R \times T(\varphi)$	200.8*** (26.87)	162.1*** (37.21)
Student characteristics		
Enrollment	-0.155 (0.115)	-0.172*** (0.055)
Percent of LEP students	71.63*** (14.29)	7.74 (19.22)
Income and tax price variables		
State aid per pupil	0.258*** (0.0883)	0.334*** (0.0691)
Federal aid per pupil	0.251 (0.259)	0.517* (0.305)
Median household income	0.092*** (0.013)	0.0739*** (0.0183)
Tax price	-0.474 (3.309)	-0.521 (4.061)

Table 6 (continued)
 Estimation Results for All SCSDs (1980–1994)
 (Dependent Variable: Total Operating Expenditures Per Pupil)

Demographic variables		
Percentage of owner-occupied housing units	55.55*** (11.74)	46.68*** (13.19)
Percentage of population with college education	96.35* (49.12)	80.60*** (27.83)
Percentage of youths (ages 6–17)	–271.6*** (56.68)	–198.2 (162.85)
Percentage of seniors (\geq age 65)	151.3* (90.07)	21.76 (46.02)
Constant	Yes	Yes
Total number of observations	10,425	8,385
Number of SCSDs	64	64
Number of comparison districts	631	495
Overall R-squared	0.50	0.53

Notes: The regressions are estimated with district and year fixed effects. Robust standard errors are in parentheses. The differences between the Census values (1990 versus 1980, and 2000 versus 1990) are distributed evenly among each year to derive annual values for demographic variables (between 1980 and 1990, and between 1991 and 1994, respectively). Asterisks denote significance at the 1% (***), 5% (**), and 10% (*) levels.

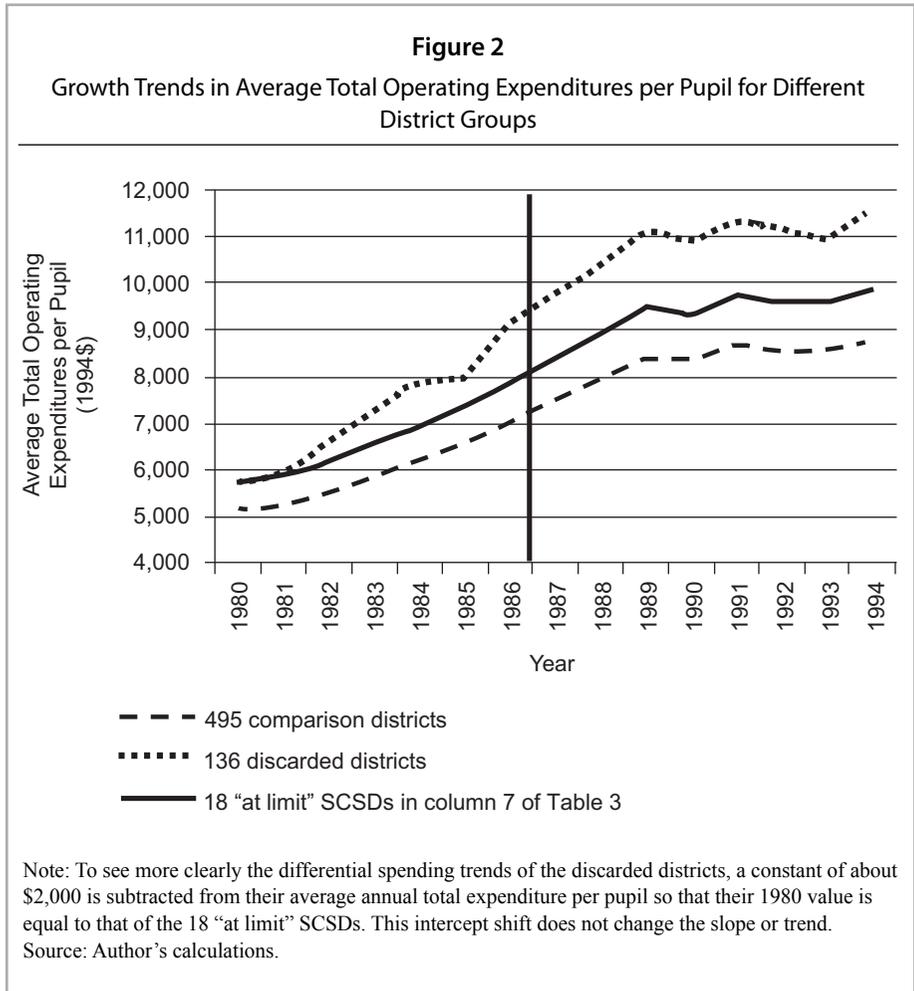
the DID estimators obtained from unmatched comparison districts are significant but, as discussed earlier, potentially biased. The remaining columns provide consistent evidence for the insignificance of the key DID estimators, α and β . These findings suggest that the SCSDs that were at limit in the last year prior to the repeal or on average over the pre-repeal period show no significant immediate or gradual response to tax limit repeal relative to various comparison groups. Put differently, we fail to reject the second null hypothesis.

Table 7
Estimation Results for "At Limit" SCSDs Using Different Comparison Groups (1980–1994)
(Dependent Variable: Total Operating Expenditures per Pupil)

Key variables	"At Limit" SCSDs in the Last Year Prior to Repeat (Column 7 of Table 3)				"At Limit" SCSDs on Average in the Pre-Repeat Period (Column 8 of Table 3)		
	Comparison groups				Comparison groups		
	UNMATCHED All Non-City School Districts Only (NCDs)	UNMATCHED NCDs and SCSDs that Exhausted <90% of Tax Limits	UNMATCHED SCSDs that Exhausted <90% of Tax Limits	MATCHED NCDs	MATCHED NCDs and SCSDs that Exhausted <90% of Tax Limits	UNMATCHED SCSDs that Exhausted <90% of Tax Limits	MATCHED NCDs and SCSDs that Exhausted <90% of Tax Limits
(1)	(2)	(3)	(4)	(5)	(6)	(7)	
$D \times R$ (DID estimator for an intercept shift) (α)	793.9** (336.7)	694.2** (330.6)	123.7 (441.7)	450.2 (525.0)	310.2 (444.7)	333.4 (568.6)	220.4 (444.2)
$D \times R \times T$ (DID estimator for a slope change) (β)	-72.83** (36.49)	-61.89* (36.71)	-19.55 (39.36)	0.0134 (64.09)	-10.98 (48.49)	-17.98 (61.03)	-71.07 (83.04)
$T(\delta)$	65.71* (36.19)	74.78** (33.52)	92*** (34.13)	128.3* (67.25)	76.76** (36.52)	120*** (37.76)	64.25 (50.73)
$T \times D$ (θ)	20.24* (11.64)	20.15 (25.42)	17.04 (32.25)	-22.55 (56.62)	-6.916 (32.47)	46.48 (41.97)	21.98 (55.46)
$R \times T(\varphi)$	207.2*** (28.33)	193.2*** (25.66)	124.8*** (48.65)	117.0* (62.26)	222.9*** (68.38)	105*** (37.24)	214.6*** (56.72)
Total number of observations	9,735	10,320	855	7,485	2,295	750	1,815
Number of SCSDs	18	18	18	18	18	9	9
Number of comparison districts	631	670	39	135	174	41	112
Overall R-squared	0.43	0.43	0.65	0.46	0.40	0.55	0.42

Notes: The regressions are estimated with district and year fixed effects. Robust standard errors are in parentheses. All other unreported control variables are the same as in Table 6. The estimation results for "at limit" SCSDs in column 8 of Table 3 are presented only for two comparison groups. Balanced tests are all done with the same matching procedures described earlier, except that only comparison districts are allowed to be discarded. Although discarding only comparison districts does not address extrapolation bias completely, this procedure facilitates the interpretation of the estimation results. Also, two SCSDs would have been discarded from matched datasets in this table. The failure to reject the second null hypothesis is, however, still robust to discarding these SCSDs. Asterisks denote significance at the 1% (***), 5% (**), and 10% (*) levels.

Figure 2 provides a simple graphical check on the matching and estimation results for column 2 of Table 6.⁵⁵ The clear differential average pre-repeal spending trend of the discarded non-city districts justifies their elimination from the original dataset. Meanwhile, the spending trends are similar for the remaining non-city districts and the SCSDs that were at limit in 1986. More importantly, these “at limit” SCSDs, on average, did not show an abrupt intercept shift or a significant slope change in their post-repeal spending trends.



⁵⁵ Matching results in Table 7 produce similar (unreported) graphs. We also conduct another sensitivity check by re-estimating column 2 of Table 6 and columns 3 to 7 of Table 7 with logarithmic transformations being performed on the dependent variable and all independent variables other than those in percentage terms. The DID estimators are still insignificant in these sensitivity tests.

VII. CONCLUSION

Local governments, including school districts, rely primarily on own-source revenues (mostly property taxes) and intergovernmental transfers (mostly state aid) for funding. They may attempt to maintain their level of spending when either revenue source is cut or constrained by their state. For example, they may increase property taxes to compensate for cuts in intergovernmental grants. On the other hand, local governments whose property taxes are subject to a state-imposed form of TELs may find ways to utilize either nontax or unconstrained revenues or state aid. While substantial research has examined whether local government spending responds symmetrically to increases and cuts in intergovernmental grants,⁵⁶ the existing literature has focused solely on local governments' responses to the imposition of property tax limits. Several studies report that tax limit imposition reduces — or inhibits the growth of — local government and school district spending. This raises the question of how local government or school district spending would react to tax limit repeal. This study represents the first attempt to examine the potential reaction to tax limit repeal of all school districts and especially, “at limit” districts. All else being equal, a school district that exhausts its tax limits completely may either increase or maintain its spending upon tax limit repeal. Only constrained “at limit” districts are expected to respond positively to tax limit repeal.

Taking advantage of the 1986 repeal of constitutional property tax levy limits imposed on small city school districts in New York State, we combine the difference-in-differences estimation specification with propensity score matching techniques to eliminate biases from selection on time-invariant unobservables and observables. The empirical evidence obtained from this combined approach fails to reject the two null hypotheses that tax limit repeal in NYS induced no significant immediate or gradual impact on the spending of (1) the whole group of SCSDs, and (2) those SCSDs that were spending at or very near their limits. This finding suggests that these “at limit” SCSDs were not constrained by tax limits and were able to reach their desired levels of spending under the limits. They were probably able to do so either because the desired spending levels were exactly “at limit”, or the limits became no longer binding after they managed to utilize other non-property tax revenues such as state aid, especially supplemental Hurd aid.

ACKNOWLEDGEMENTS

I would like to thank William Duncombe, John Yinger, Bob Bifulco, and Coady Wing for their helpful comments on earlier versions of this paper, and Teresa Lopes for her valuable help with proofreading. Special thanks go to editors George Zodrow and William Gentry, and two anonymous referees for their helpful comments and suggestions.

⁵⁶ Asymmetry is identified when state and local government spending levels are more responsive to increases in intergovernmental aid than to losses. A form of asymmetry is fiscal replacement in which expenditure reductions are less than grant reductions as a result of increases in property taxes. Fiscal replacement was found in several studies in the United States (Deller and Maher, 2006; Volden, 1999) or elsewhere (Ashworth and Heyndels, 2005; Lago-Penas, 2008; Levaggi and Zanola, 2003).

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