Are Property Tax Limitations More Binding over Time?

Abstract *- In 1991, a property tax limitation measure was imposed in five Illinois counties. Dye and McGuire (1997) studied its short–term impact. With the limit now in effect for over a decade and extended to many more counties, we assess its long–term impact. Because jurisdictions brought under the limitation since 1997 have done so after a county–option referendum, our estimation strategy treats the measure as endogenous. We find that the restraining effect of the limit on the growth of property taxes is stronger in the long run than the short run, and that the growth of school expenditures is slowed by the measure.*

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INTRODUCTION

In July 1991, Illinois enacted a limit on the growth rate of property tax revenues for some, but not all, local jurisdictions in the Chicago metropolitan area. The "tax cap," as it is known in Illinois, initially affected municipalities, school districts and other types of local governments in the five metropolitan "collar" counties, but not those in the central metropolitan county of Cook.[1] Dye and McGuire (1997) exploit this "natural experiment" to finesse some of the econometric difficulties in earlier studies of property tax limitations and find that the limitation measure effectively constrained the growth rate of property taxes in the first three years of the limit.[2] Dye and McGuire (1997) could only speculate about the longer–term effects of the limit. In this paper, we explore whether property tax limitations become more binding over time.

Dye and McGuire (1997) venture that the long–run effects of the limit could be stronger or weaker than the short–run effects. A limit that applies to the growth rate of property taxes could become more binding over time because the easy means of accommodating the cap may become exhausted after the first few years. In addition, the impact of such a limit may compound over time because the revenue base to

[1] Also exempt from the tax limitation were "home–rule" municipalities in the collar counties. Home–rule status comes with a population of 25,000 or a local referendum, and carries fewer restrictions on setting property tax rates or on raising other type of revenues.

[2] See Merriman (1987) and Rueben (2000) for studies of the effect on tax revenues and spending, and see Figlio (1997) and Downes, Dye and McGuire (1998) for studies of the effect of limitations on educational outcomes.

which the growth limit applies is lower in each future year than it might otherwise have been. On the other hand, with time, local officials might devise means for circumventing the state–imposed limit. There is some evidence, for example, that local officials in Arizona circumvented that state's property tax limit in part by shifting to forms of property taxation not subject to the limit (see Fisher and Gade (1991)). Also, the extraordinarily large number of local jurisdictions in Illinois is often attributed to Illinois' much older property tax *rate* limit—creating a new local government was one means of circumventing that limit.

In this paper, we estimate the effects of the Illinois property tax limitation measure after many more years of experience and after many more jurisdictions have been brought under its purview. The collar–county jurisdictions have now been subject to the tax cap for over ten years. In the spring of 1995, following a non–binding referendum favoring the cap in Cook County, the Illinois legislature extended the tax cap to Cook County. In the spring of 1996, enabling legislation was passed allowing voters in the remaining 96 counties in Illinois to approve by countywide referenda imposition of the limit on their non–home–rule local governments. As of September 2003, local jurisdictions in 39 counties in Illinois were subject to the tax cap. Thus, the treatment and control groups have changed over time, and in all but the six–county metropolitan area, the tax cap is endogenously chosen by the county voters. This local–option policy *selection* complicates the econometrics of estimating the impact of the cap. Addressing the problem of endogenous policy change is an important concern of the present paper.

We estimate the effect of the tax cap using a statewide sample of municipalities and a statewide sample of school districts in Illinois.[3] We ask two related questions: (1) is the tax cap effective; and (2) is the tax cap more binding over time? Our answer to the first question is yes; the tax cap appears to slow the growth of municipal and school property taxes and the growth of school expenditures. For property taxes, our answer to the second question is yes; the cap appears to have a larger impact for municipalities and school districts subject to the cap throughout the nine–year period of analysis relative to those with fewer years of experience under the cap. For school expenditures, there does not seem to be a difference between the short–run and long–run effects of the cap.

DATA AND SPECIFICATION

The property tax extension limitation law, as it is formally named, or tax cap, as it is more commonly known, limits the growth in total property tax collections of a local government to a price inflation factor. There are exceptions for newly developed property in its first year on the rolls, for debt service for obligations incurred before the imposition of the cap, and for override by special referendum. The tax cap is imposed on all local governments in designated counties,[4] with the exception of home–rule jurisdictions.[5]

All municipalities and school districts in Illinois for which data are available over the sample period are included.[6]

[3] School districts in Illinois are independent of municipalities. They overlap and are seldom coterminous.

[4] Special rules are in place to designate whether local governments that cross the border between capped and uncapped counties are subject to the cap.

[5] All of the local governments in the state of Illinois with home–rule status are municipalities, with the sole exception that the county government in Cook County has home–rule powers.

[6] This is nearly the entire population of municipalities and school districts. Excluded are the small number of cases with missing data, values of zero for property tax collections, or growth rates of greater than 100 percent or less than negative 50 percent, (which probably result from the assignment of taxes or expenditures to the wrong year).

Even though other types of government, such as townships and library districts, are also subject to the cap, we focus on municipalities and school districts because together they are responsible for raising over three–quarters of total property taxes (Illinois Department of Revenue, 2002).

Table 1 indicates the number of counties and the associated number of "treatment" municipalities and school districts made subject to the cap in different years, as well as the number of "control" jurisdictions with home–rule status or located in never–capped counties. In 1991, all 144 school districts and 112 non–home–rule municipalities in the five heavily populated suburban collar counties were made subject to the cap, with 113 collar–county municipalities exempt because of their home–rule powers. In 1995, the cap was extended to another 144 school districts and 54 non–home–run municipalities in the central metropolitan county of Cook, with the city of Chicago and 15 other home–rule municipalities in Cook County exempt. Eighteen counties had successful referenda to impose the cap on their school districts and non–home–rule municipalities in 1997, the first year of the local option, and four or fewer additional counties voted to impose the cap in each of the subsequent years. The capped–by–referendum and never–capped counties are scattered around the state; there is little in the way of obvious characteristics to distinguish them.

Outside the six Chicago–area counties, the imposition of the cap follows a countywide vote of residents in jurisdictions that would be subject to the cap. Even though the vote is countywide rather than jurisdiction–by–jurisdiction, it may not be appropriate to treat the tax cap as exogenous in these jurisdictions for at least two reasons. First, there may be an omitted factor or set of factors that is a co–determinant of both property tax growth rates and whether voters elect to impose the cap. For example, there may be a taste for or against local government that we do not measure. Second, it may be that past growth of property taxes influences voters and is correlated with future growth of property taxes.

To control for omitted co–determinants of property tax growth rates and tax cap status, we employ fixed effects estimation. The implicit assumption is that the unob-

TABLE 1
NUMBER OF ILLINOIS SCHOOL DISTRICTS AND MUNICIPALITIES BY TAX CAP AND HOME–RULE STATUS

Effective Year of Tax Cap	Number of Counties with Jurisdictions Newly Subject to the Cap	Capped School Districts	Capped Municipalities	Home–rule Municipalities in Counties with the Cap
1991	5 (Collar)	144	112	113
1995	1 (Cook)	144	54	16
1997	18	80	210	9
1998	4	19	36	4
1999	4	51	46	3
2000	2	18	23	2
2001	3	4	21	2
2003	2	5	6	4
Total	39	465	508	153
	Counties	School Districts	Non–home–rule Municipalities	Home–rule Municipalities
Never Capped	63	431	627	127

Notes: Counts of the number of counties are included for reference purposes. Counts of home–rule municipalities and never–capped jurisdictions are included to indicate the numbers in the control group. Since our sample period ends in 1999, jurisdictions subject to the cap in later years are treated as never capped. Also, home–rule status can change from year to year, so the sample counts may differ slightly from what is shown here.

served heterogeneity does not vary over time, which seems reasonable given our relatively short panel and the nature of the suspected unobserved local preferences. We consider the fixed effects estimator as our base specification. To test the robustness of the model and to address other potential sources of endogeneity of the tax cap variable, we estimate the equation using both an instrumental variables model and a matching estimator technique.

Each regression includes a full set of yearly dummy variables to capture statewide changes in economic conditions and relevant policies over time. We also include a dummy variable for the "window" year—the year immediately after a county has voted to impose tax caps, but before the caps are in effect—which arguably provides an incentive to increase property taxes. With a fixed effects specification, the possible additional control variables are limited to those that vary over time. We include a variable for the residential share of total property value (equalized assessed value or EAV) to capture the possibility that heavily residential jurisdictions might be more, or less, willing to increase property taxes than jurisdictions with a greater share of commercial and industrial property. For the municipal tax growth regressions only, we include a dummy variable for home–rule status to capture the notion that home–rule status municipalities have access to a broader array of revenue sources and, thus, may not put as much pressure on the property tax. Finally, for the school tax and expenditure growth regressions, we include a measure of the growth in the number of pupils (the change in the log of "average daily attendance") to capture the fact that state aid and other school budget elements are strongly linked to pupil counts.

RESULTS

In Tables 2 and 3, we report the results of a fixed effects estimation of the impact of the tax cap on four separate dependent variables of interest: municipal property tax growth rates (Table 2), school district property tax growth rates (Table 2), school district operating expenditure growth rates (Table 3), and school district instructional expenditure growth rates (Table 3).[7] Our interest in the latter two variables stems from Dye and McGuire's (1997) finding that the tax cap had a significant short–run effect on operating expenditures, which includes instructional, administrative and support services, but no short–run effect on instructional expenditures. The authors interpreted these finding as being consistent with a bureaucratic model of local government behavior in which any excess spending by bureaucrats would likely appear in the categories of administrative and support services. In addition, McGuire (1999) argues that *any* effect of a limitation that provides for overrides can be interpreted as evidence for the bureaucratic model. If, instead, the median voter model were operative, a binding cap would be overridden by the voters and the limit would have no effect. Because political processes are not perfect, it may take several years for the voters to succeed in overriding the limit. Thus, to shed light on whether local government behavior is more consistent with the bureaucratic model or the median voter model, as well as to examine the ability of local governments to absorb the cumulative effect of limits on growth rates, it is necessary to analyze long–run results.

For each dependent variable, the tables present the results of estimating two regressions that differ only in their treat-

[7] The property tax data are from the Illinois Department of Revenue by "assessment" year, the calendar year prior to the collection of the taxes, and cover the years 1988 to 1999. The school expenditure data are from the Illinois State Board of Education by academic year and cover AY 1987–88 to 2000–01.

TABLE 2
EFFECT OF ILLINOIS TAX CAP ON GROWTH IN LOCAL GOVERNMENT TAXES, BY TYPE OF GOVERNMENT

	Municipalities		School Districts	
	(1)	(2)	(3)	(4)
Constant	0.0758**	0.0698**	0.0954**	0.0964**
	(3.93)	(3.61)	(7.34)	(7.44)
Residential Share of EAV	–0.0005*	–0.0004	–0.0007**	–0.0007**
	(1.82)	(1.52)	(2.62)	(2.71)
Home–rule	–0.0207*	–0.0148		
	(1.76)	(1.25)		
Growth in No. of Pupils			0.1227**	0.1266**
			(7.08)	(7.31)
Year 1989	–0.0042	–0.0042	–0.0141**	–0.0141
	(1.01)	(1.01)	(3.89)	(3.90)
Year 1990	–0.0024	–0.0024	0.0031	0.0030
	(0.57)	(0.58)	(0.84)	(0.83)
Year 1991	0.0011	0.0006	0.0001	–0.0008
	(0.27)	(0.15)	(0.04)	(0.23)
Year 1992	–0.0015	–0.0020	0.0085**	0.0075**
	(0.36)	(0.49)	(2.27)	(2.02)
Year 1993	0.0026	0.0020	0.0227**	0.0217**
	(0.62)	(0.48)	(6.01)	(5.77)
Year 1994	0.0076*	0.0090**	0.0257**	0.0284**
	(1.81)	(2.13)	(6.79)	(7.47)
Year 1995	0.0116**	0.0127**	0.0328**	0.0345**
	(2.73)	(2.99)	(8.39)	(8.84)
Year 1996	0.0124**	0.0134**	0.0329**	0.0347**
	(2.79)	(3.01)	(8.11)	(8.56)
Year 1997	0.0139**	0.0141**	0.0320**	0.0333**
	(3.13)	(3.19)	(7.70)	(8.02)
Year 1998	0.0138**	0.0150**	0.0222**	0.0270**
	(3.08)	(3.35)	(5.24)	(6.30)
Year 1999	0.0082*	0.0092**	0.0291**	0.0336**
	(1.81)	(2.04)	(6.81)	(7.76)
Window Year	0.0208**	0.0211**	0.0170**	0.0161**
	(3.08)	(3.13)	(2.69)	(2.55)
Capped Years 1–9	–0.0334**		–0.0717**	
	(8.63)		(24.70)	
Capped Years 1–3		–0.0288**		–0.0659**
		(7.16)		(21.68)
Capped Years 4–9		–0.0519**		–0.0875**
		(8.77)		(22.67)
Number of Observations	13,068	13,068	10,226	10,226
F–Statistic (equation)	1.36**	1.37**	1.65**	1.70**
F–Stat (Cap 1–3 ≠ Cap 4–9)		17.04**		38.45**

Notes: All specifications include fixed effects.
Absolute values of t–statistics in parentheses with * for ten percent and ** for five percent significance.

TABLE 3
EFFECT OF TAX CAP ON GROWTH IN ILLINOIS SCHOOL DISTRICT EXPENDITURES, BY TYPE OF SPENDING

	Operating Expenditures		Instructional Expenditures	
	(1)	(2)	(3)	(4)
Constant	0.0298**	0.0296**	0.0286**	0.0286**
	(4.00)	(3.97)	(3.60)	(3.60)
Residential Share of EAV	0.0003**	0.0003**	0.0005**	0.0005**
	(2.30)	(2.33)	(3.22)	(3.21)
Growth in No. of Pupils	0.2212**	0.2205**	0.2620**	0.2621**
	(20.11)	(20.03)	(22.34)	(22.33)
School Year 1988–89	0.0054**	0.0054**	–0.0013	–0.0013
	(2.16)	(2.16)	(0.50)	(0.50)
School Year 1989–90	0.0152**	0.0152**	0.0112**	0.0112**
	(6.01)	(6.01)	(4.18)	(4.18)
School Year 1990–91	0.0216**	0.0216**	0.0180**	0.0180**
	(8.50)	(8.50)	(6.67)	(6.66)
School Year 1991–92	0.0076**	0.0076**	0.0072**	0.0072**
	(3.00)	(3.00)	(2.65)	(2.65)
School Year 1992–93	–0.0047*	–0.0045*	–0.0077**	–0.0078**
	(1.81)	(1.74)	(2.80)	(2.81)
School Year 1993–94	0.0043*	0.0045*	0.0027	0.0026
	(1.67)	(1.74)	(0.97)	(0.95)
School Year 1994–95	0.0054**	0.0056**	–0.0058**	–0.0059**
	(2.06)	(2.13)	(2.08)	(2.10)
School Year 1995–96	0.0087**	0.0083**	0.0004	0.0005
	(3.31)	(3.13)	(0.15)	(0.18)
School Year 1996–97	0.0184**	0.0182**	0.0094**	0.0095**
	(6.83)	(6.73)	(3.28)	(3.29)
School Year 1997–98	0.0215**	0.0212**	0.0136**	0.0137**
	(7.78)	(7.65)	(4.63)	(4.63)
School Year 1998–99	0.0265**	0.0263**	0.0230**	0.0230**
	(9.38)	(9.31)	(7.64)	(7.64)
School Year 1999–2000	0.0188**	0.0181**	0.0081**	0.0083**
	(6.57)	(6.21)	(2.67)	(2.68)
School Year 2000–01	0.0268**	0.0262**	0.0030	0.0032
	(9.20)	(8.86)	(0.98)	(1.02)
Window Year	–0.0053	–0.0047	–0.0011	–0.0013
	(1.33)	(1.18)	(0.27)	(0.30)
Capped Years 1–9	–0.0149**		–0.0123**	
	(8.00)		(6.23)	
Capped Years 1–3		–0.0161**		–0.0121**
		(7.99)		(5.65)
Capped Years 4–9		–0.0124**		–0.0130**
		(4.96)		(4.84)
Number of Observations	11,802	11,802	11,886	11,886
F–Statistic (equation)	1.11**	1.09**	1.05	1.04
F–Stat (Cap 1–3 ≠ Cap 4–9)		2.36		0.09

Notes: All specifications include fixed effects.
Absolute values of t–statistics in parentheses with * for ten percent and ** for five percent significance.

ment of the tax cap variable. In the first specification, the tax cap variable, called *Capped Years 1–9*, takes a value of one for jurisdictions in any year in which they are subject to the cap, and zero otherwise. So, for example, this variable has a value of zero for a school district in one of the collar counties in the years 1988 through 1990 (recall the cap was not imposed on non–home–rule jurisdictions in the collar counties until 1991), and a value of one for the same district from 1991 on. In the second specification, there are two tax cap variables: *Capped Years 1–3* takes a value of one for jurisdictions that have been capped for one to three years, while *Capped Years 4–9* takes a value of one for jurisdictions that have been capped for four to nine years. So, for year 1999, the same collar county school district, which has been capped for nine years, is coded as a zero for the *Capped Years 1–3* variable and one for the *Capped Years 4–9* variable.

The first specification does not distinguish how long the cap has been in place. The coefficient on the tax cap variable in Table 2, specification 1 indicates that property taxes grew 3.34 percentage points slower in municipalities that were subject to the cap compared to municipalities that were never hit with the cap. Table 2, column 3 tells a similar, if more dramatic, tale for school districts. When we turn to Table 3 to examine components of school spending, our results differ from those reported in Dye and McGuire (1997) in that we find that the cap slowed the growth of both operating and instructional spending. These results are not inconsistent. In the short run, school districts may have greater ability to protect instructional spending, but with time the cap may affect instructional spending as well as spending on administration and support staff.

The second specification allows us to address the second question we pose above: is the tax cap more binding over time? We find that the growth rate of property taxes is slowed to a greater extent for municipalities and school districts subject to the cap for four to nine years relative to those subject to the cap for zero to three years. In Table 2, column 2, for example, the tax cap slowed municipal property tax growth by 2.88 percent in the first three years, our short run, compared to 5.19 percent after four or more years in place, our long run. (An F–test shown in the bottom row of the table indicates that the difference in the short–run and long–run coefficients is significant.) This may reflect the cumulative nature of a cap on the annual growth rate of revenues.

The results for the two tax cap duration variables in the second specification for school spending reveal a different story. In Table 3, column 2, school operating spending declined 1.61 percent in years one to three, but only 1.24 percent in years four to nine; however, according to the F–test, these are not significantly different. In Table 3, column 4, instructional spending grew slower for school districts hit by the cap relative to those never subject to the cap by the same amount (about one and one–quarter percentage points), regardless of how long the cap had been in effect.

ADDITIONAL CONTROL FOR ENDOGENEITY

As noted above, one of our ambitions is to treat carefully the problem of endogenous policy choice. Recall that many of our observations are for jurisdictions that were subjected to the cap after a countywide referendum. Our basic strategy is to address this concern by estimating our equations with a fixed effects specification (Wooldridge, 2002, chapter 10), using time–invariant jurisdictional dummy variables to control for omitted variables that may influence both revenue growth rates and tax cap status. In this section, we test the robustness of the results and control for other possible sources of endogeneity by employing two different approaches.

To simplify the presentation, we re–estimate only the first specification for each of the four dependent variables.

Our first alternative specification follows Boarnet and Bogart (1996) by using a probit equation to generate a predicted value for tax cap status. The two–stage estimation procedure is a straightforward generalization of our earlier results, using an instrumental variable in place of tax cap status to control for any remaining endogeneity problems. Because the tax cap was exogenously imposed by the state legislature in non–home–rule jurisdictions in the collar counties in 1991 and in Cook County in 1995, for these jurisdictions we use actual tax cap status as the measure of the tax cap variable. For all other jurisdictions (all jurisdictions in the downstate counties), the estimated probit probabilities form the instrument for the tax cap variable.

The first–stage probit regressions are presented in Table 4. As explanatory variables in the probit equation for municipalities, we have dummy variables for the years in which the downstate counties were allowed to hold referenda on the tax cap, the residential share of EAV, the population in 1990, the per capita income in 1989, and

TABLE 4
PROBIT MODELS OF TAX CAP STATUS

	Municipal Taxes	School Taxes	School Expenditures
Constant	–2.163**	–2.025**	–1.985**
	(7.66)	(13.77)	(13.59)
Residential Share of EAV	0.016**	0.015**	0.014**
	(6.25)	(7.17)	(6.96)
Year 1998	0.110	0.148	
	(1.59)	(1.59)	
Year 1999	0.251**	0.495**	
	(3.71)	(5.56)	
School Year 1999–2000			0.143
			(1.54)
School Year 2000–01			0.489**
			(5.50)
Population 1990 (1000s)	0.015**		
	(2.54)		
Per Capita Income 1989 (1000s)	0.014		
	(0.87)		
Poverty Rate 1989	–0.108		
	(0.25)		
Average Daily Attendance (1000s)		0.026	0.027
		(1.34)	(1.37)
Change in Natural Log of Average Daily Attendance		0.212	0.847
		(0.27)	(1.12)
Low–Income Pupils (% of Avg. Daily Attendance)		0.450**	0.406*
		(1.99)	(1.80)
Number of Observations	2,481	1,699	1,701
Cases Correct	1,857	1,374	1,379
Average Likelihood	0.577	0.633	0.634

Notes: Absolute z–values in parentheses with * for ten percent and ** for five percent significance.

the 1989 poverty rate; identification comes from the latter three variables and from the nonlinearity of the probit model. For the school district equations, the explanatory variables are the referendum–year dummy variables, the residential share of EAV, the change in the log of the average daily attendance (number of pupils), the average daily attendance, and a time–invariant measure of "low income pupils;" identification comes from nonlinearity and the latter two variables. The fit of the equations is good, with significant t–statistics on at least three of the explanatory variables, and three–quarters of the cases correctly predicted in each equation. Noteworthy results include the finding that, in all three equations, jurisdictions with a larger share of residential property in the tax base are more likely to be capped. For municipal taxes, the significant coefficient on the population variable suggests that large cities are more likely to be capped. For school districts, the low–income pupil variable is significant.

The second–stage results are presented in the second row of Table 5. For comparison purposes, the first row reproduces the results for the tax cap variable (*Capped Years 1–9*) from the first specification (column 1 or 3) from Tables 2 and 3. With the exception of the result for municipal tax, in which we find a stronger effect once we instrument for the tax cap, the results are nearly identical to our earlier results, suggesting that the straightforward fixed effects estimator controls adequately for tax cap endogeneity.[8] We again find that the tax cap significantly reduces the growth rates of taxes and expenditures.

Our final method of controlling for endogeneity is based on the matching estimator. Wooldridge (2002, p. 620) presents an excellent overview. The nonparametric procedure matches each treatment municipality or school district with a weighted average of similar observations from the control group. The estimated probit probabilities form the basis for

TABLE 5
TAX CAP EFFECT ESTIMATES FROM ALTERNATIVE SPECIFICATIONS
TO CONTROL FOR ENDOGENOUS CAP STATUS

	Municipal Taxes	School Taxes	School Operating Expenditures	School Instructional Expenditures
Fixed effects specification (reproduces specifications 1 and 3 from Tables 2 and 3)				
Capped Years 1–9	–0.0334**	–0.0717**	–0.0149**	–0.0123**
	(8.63)	(24.70)	(8.00)	(6.23)
Instrumental variables specification with Probit estimation of tax cap variable in first stage (see Table 4)				
Tax Cap Instrument	–0.0520**	–0.0860**	–0.0177**	–0.0144**
	(9.30)	(25.36)	(8.03)	(6.14)
Matching estimator specification— Capped treatment group versus never capped control group				
Collar Counties	–0.0297**	–0.0787**	–0.0209**	–0.0203**
v. Never Capped	(2.85)	(9.81)	(5.24)	(4.55)
Cook County	–0.0321**	–0.0523**	–0.0053*	–0.0055
v. Never Capped	(3.57)	(9.68)	(1.74)	(1.62)
Downstate cap in 1997	–0.0197**	–0.0212**	–0.0024	–0.0058
v. Never Capped	(4.21)	(3.41)	(0.50)	(1.14)

Notes: Absolute values of t–statistics in parentheses with * for ten percent and ** for five percent significance.

[8] The first–stage probit estimation procedure is taken into account in constructing the standard error estimates using the results of Murphy and Topel (1985).

the matching.[9] The technique does not allow for the treatment switching on at different times, so we make three separate comparisons all with the same control group of never–capped jurisdictions in downstate counties. The three treatment groups are jurisdictions in the collar counties capped in 1991, jurisdictions in Cook County capped in 1995, and jurisdictions in downstate counties capped in 1997. The results are presented in the last three rows of Table 5. The similarity of the matching estimator to the results in the rest of the table provides additional support for the conclusion that the fixed effects estimator controls sufficiently for endogeneity.

Using three methods of controlling for endogeneity, we find robust results indicating that tax caps reduce property tax growth rates, with an effect that is more pronounced over time. Results indicate that school district spending is also reduced by imposition of the tax cap, but there is no discernable difference between the long–run and short–run effects for either operating or instructional expenditures.

CONCLUSION

Employing an unusual sample of jurisdictions all within the same state in which some jurisdictions are subject to a property tax limitation and others are not, we find strong evidence that the property tax cap in Illinois slowed the growth rate of property taxes for municipalities and schools. The restraining impact of the cap on property taxes was stronger in the long run than in the short run. School expenditures also grew more slowly in districts subject to the cap relative to those not hit by the cap, but the effect of the cap was, if anything, weaker in the long run relative to the short run. We conjecture that the difference in the long–run effect of the cap on taxes as compared to the long–run effect on spending may be due to an influx of state aid for school districts in the latter part of the period, which would alleviate pressure to keep spending growth in line with property tax growth. When we examine trends in state aid, we do not find evidence consistent with this hypothesis. State aid to school districts does not appear to increase more in the long run relative to the short run. In contrast to the findings in Dye and McGuire (1997), we find little difference in the effect of the cap on operating and instructional spending, especially in the long run. One possible explanation is that, once the cap has been in effect for many years, the ability of school districts to protect instructional spending from the effects of the cap is limited.

The finding that the cap continues to bind in the long run is consistent with voters preferring to place constraints on their elected officials, perhaps because of perceived (or real) bureaucratic waste or indifference. If, instead, the voters felt that state–imposed constraints were taking them away from their preferred public spending outcome, they could vote to override the limits, which the voters in Illinois seldom do. The notion that voters' preferences are reflected in the decisions of local officials is difficult to reconcile with our findings.

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[9] A more complete explanation of this application of the matching estimator technique is available from the authors.

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