SHORT-TERM EFFECTS OF A 1990S-ERA PROPERTY TAX LIMIT: PANEL EVIDENCE ON OREGON'S MEASURE 5

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Abstract *- Since the early 1990s, a number of states have imposed limitations on local public school revenues and expenditures. I consider the effects of this trend, which has been likened to the "local property tax revolt" of the 1970s, on the provision of local public education. I use a comprehensive panel of school districts from Oregon and Washington, with annual data from before and after Oregon imposed its limitation in 1990. Controlling for unobserved heterogeneity, I find that Oregon student-teacher ratios have increased significantly as a result of the state's tax limitation. However, I find that the ratio of administrative to educational spending has remained unchanged, or may have even increased, in the wake of the tax limit, suggesting that the incidence of the tax limitation has been borne by instruction at least as much as by administration. I also investigate the distributional effects of this limitation.*

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INTRODUCTION

During the late 1970s and early 1980s, half of the United States imposed limitations on the ways in which public school districts (and other local governments) can collect and spend revenues. This national phenomenon of binding the hands of local governments, sparked in large part by California's Proposition 13, became known as the "local property tax revolt." A major impetus behind these limitations was the widely-held belief that local governments were inefficiently providing services and that limitations would lead to similar public service levels at lower cost. Citrin (1979) and Shapiro, Puryear, and Ross (1979) find, for instance, that the principal reason that people voted for tax revolt–era limitations was to decrease their tax burden without diminishing service levels. Other authors, such as Gramlich, Rubinfeld, and Swift (1981), Ladd and Wilson (1982), and O'Sullivan, Sexton, and Sheffrin (1995), report similar findings. In other words, voters believed that there was no effective trade-off between public and private goods.

In recent years, and especially since 1993, a new local property tax revolt has apparently commenced. A number of states have imposed strict tax revolt–type limitations on school district finances, and several others are considering doing the same. These limitations range from caps in the growth rate of school district expenditures (for instance, in Wisconsin) to Michigan's phasing out the property tax as a school finance vehicle. Other states, such as Illinois, have recently limited revenue and expenditure growth in subsets of the state. Last November, California and Oregon each voted for strengthened tax limits, and tax limitation measures narrowly failed (and may be brought to a vote again) in other states, such as Idaho. In Oregon, the first state to impose a 1990s-era tax limit, school districts have faced real reductions in their property tax revenues (where revenues are deflated by the increased cost of providing public education) without substantial state revenue replacement since the state's Measure 5 was approved by voters in November 1990. This paper gauges the effects of Oregon's Measure 5 on the provision of local public education.[1]

Measure 5 capped property tax rates for all purposes to a specific percentage of assessed value, which in Oregon by law must reflect fair market value. (That is, the *sum* of all individual tax rates on a piece of property is limited by Measure 5.) While Measure 5 called for partial state replacement of lost revenues, this nominal state compensation of local school districts has stayed constant in the years following Measure 5. Given that almost every jurisdiction in Oregon had higher property tax rates than the allowable cap prior to Measure 5, and since local governments had little flexibility in re-assessing property, Measure 5 has effectively bound most localities. For districts with prelimit tax rates above the limit, the gap between actual state compensation and "necessary" state compensation has been increasing with time.

What might be the effect of the 1990s-era tax limits on local public school provision? One possible place to look for guidance is the literature to date analyzing the outcomes of the local property tax revolt of the late 1970s. Most of this research (including, for instance, Reschovsky and Schwartz, 1992; Merriman, 1986; Reid, 1988) involves gauging the effects of a particular state's tax limit on general local government revenues or expenditures. Other authors perform analyses of limitations in a variety of states. For example, Cox and Lowery (1990) compare the effect of state revenue limitations on the size and composition of state government in seven states, including two with no restrictions. They find little evidence that restrictions have affected the size of state government or fiscal centralization. Elder (1992) and Rueben (1995) also use state-level time-series data and find evidence that tax revolt restrictions have controlled the growth of state government. Poterba (1994) shows that states with tax limitations are less likely to respond to positive deficit shocks by raising taxes. Preston and Ichniowski (1992) sample over one thousand municipalities nationwide to determine whether state-imposed revenue limitations have affected local government revenue growth. They find substantial evidence that imposition of limitations on local government property tax assessments and overall own-source tax revenue or expenditure limitations significantly reduces the growth rate of municipal revenues.

None of the aforementioned literature considers the effects of tax limitations on particular service levels provided by local governments. Downes (1992) considers whether the quality of public education in California has converged across communities in the wake of California's "equalizing" Supreme Court ruling, *Serrano v. Priest*, and Proposition 13, the state's revenue and expenditure limitation. Downes finds considerable convergence across California school districts in per pupil spending.

One potential shortcoming of Downes's paper is that it considers only the effect of tax limits on per pupil spending, rather than on particular service levels, such as the student-teacher ratio. If schools will provide the same service levels with less spending, as many proponents of tax limits have intimated, then Downes's results can still be consistent with a case in which the distribution of school service levels remains unchanged. (However, Downes does find little effect on student achievement—which could be considered to be a service level—as a result of Proposition 13 and *Serrano*.) A different reason to be concerned about using Downes's results to generalize to other states or events is that California's case, with arguably the most severe tax revolt–era limitation as well as a substantial court-mandated school finance equalization, is surely a special case.[2]

Figlio (1997) uses a post-tax revolt cross section of schools across 49 states and finds that schools in states with tax revolt–era limitations tend to have significantly higher student-teacher ratios, for instance, than otherwise equal schools in states without tax limitations. On the other hand, Figlio finds that administrative spending and staffing appear to be unchanged by tax limits. Moreover, Figlio demonstrates that, all else equal, by 1988, students who attended schools subject to tax revolt–era limitations performed substantially less well on mathematics, science, social studies, and reading examinations than did their counterparts in schools without limitations. Figlio's paper, however, is not without its drawbacks: although Figlio takes the potential endogeneity of limitations into account, his results are based solely on cross-sectional data collected after the tax revolt occurred.

This paper makes two significant contributions to the existing literature. To my knowledge, this paper is the first to consider the effects of a 1990s-era tax limitation on specific local public school service levels.[3] The distinction between eras is important. The tax revolt of the 1970s was significantly different in nature to the earlier tax revolts, such as the round of tax limits introduced after World War II. Similarly, the 1990s-era tax limits may be different in nature from the tax revolt of the 1970s. For instance, states that have passed 1990s-era tax limits seem to have been less likely to offer significant state replacement of lost funds to local school districts, as was done more prevalently in the 1970s. In addition, 1990s-era tax limits such as Oregon's tend to combine tax rate limits with limits on tax assessments, rather than being general limits on expenditures or revenues, as were more common during the tax revolt. When using 1970s data to make inferences about the current round of tax limits, it would be helpful to have supporting evidence from the 1990s.

The second primary contribution of this paper is that it builds upon the strengths of both Downes and Figlio. Like

Downes, I use data from before and after a property tax limitation, so I can be more comfortable that the effects that I attribute to the tax limit are truly attributable to the limitation. Like Figlio, I evaluate the effects of a tax limit on specific school services, such as the student-teacher ratio, rather than merely on per pupil expenditures. Both of these innovations should help to shed light on the potential effects of the tax limitations currently being imposed or considered across the country. I can also gauge the effects of a tax limit that is closer in nature than Proposition 13 (and without the court-mandated school finance reform) to those recently passed or currently being considered in other states.

This paper examines the effects of a 1990s-era property tax limitation on the provision of local public education. Specifically, I am interested in whether and how schools alter their services in response to limitations. To address this question, I use a comprehensive panel data set, with school finance and service data for every school district in the states of Oregon and Washington, with annual observations from 1987 to 1993. With these data, I can control for unobserved heterogeneity and gauge the degree to which Measure 5 has affected the provision of school services in Oregon. The inclusion of Washington allows for a counterfactual—a state that is economically similar to Oregon but did not impose a new property tax limitation during this time period.

I find that there have been two principal short-term effects of Oregon's Measure 5. First, Oregon schools have unambiguously increased their student-teacher ratios, apparently as a direct consequence of Measure 5. However, the incidence of Measure 5 has apparently been borne by instruction at least as much as by administration, suggesting that, if anything, Oregon schools cut their instructional services more than their administrative overhead in the immediate wake of Measure 5. While my analysis of the distributional effects of Measure 5 suggest that some school districts have been affected much more than others, I find very little evidence suggesting that any school districts had *higher* service levels as a result of Measure 5.

MEASURE 5 AND STUDENT-TEACHER RATIOS IN OREGON

I use two data sources for this analysis: the Common Core of Data (CCD), collected by the U.S. Department of Education, and the 1990 Census of Population's school district–level extract. The CCD is a rich source of administrative and financial data for every school district in the United States and has been published annually from the 1987–8 school year to (at the time of writing) the 1992–3 school year. Because the CCD is comprehensive, I can construct a panel of school districts so that I may control for unobserved heterogeneity. In addition, since the CCD data begin prior to the passage of Measure 5, I can make "before" and "after" comparisons. My unit of observation is the individual school district. All told, I include 305 school districts in Oregon and 296 school districts in Washington.[4]

My principal dependent variable of interest is the student-teacher ratio. Generally speaking, a higher student-teacher ratio can be thought of as a lower school service level, although it does not immediately translate into class size. In the four school years prior to the implementation of Measure 5, the mean (enrollment-weighted) student-teacher ratio in Oregon was 19.2 students per teacher. Prior to Measure 5, Washington's mean student-teacher ratio was 20.4. This difference is statistically significant at any conventional level. In the two years following Measure 5, however, Oregon and Washington changed positions. In the 1991–2 and 1992–3 school years, Oregon's mean student-teacher ratio was 20.9, in contrast to Washington's mean ratio, which remained unchanged at 20.4. So, while before the implementation of Measure 5 Washington's student-teacher ratio was more than six percent higher than Oregon's, in the two years following Measure 5's introduction, Oregon's mean student-teacher ratio was 2.5 percent higher than Washington's.

The preceding discussion provides suggestive evidence that Oregon's tax limit reduced school service levels. But these mean comparisons could potentially be misleading. I therefore provide a parametric analysis of the issue. Using the panel of all Oregon and Washington school districts, I estimate the equation

1

$$s_{it} = \beta L_{it} + \delta X_{it} + \gamma_i + \lambda_t + \varepsilon_{it}$$

where s_{it} represents the student-teacher ratio in school district i in year t; L_{it} is an indicator variable reflecting whether school district i is subject to the Measure 5 tax limit in year t (in practice, $L_{it} = 1$ for all i in Oregon if t = {1991–2, 1992–3}, and 0 otherwise); γ_i is a school district–specific effect (which allows me to control for unobserved heterogeneity); λ_t is a year-specific effect; and the X's reflect time-varying covariates observable to the econometrician. The term ε_{it} is the mean-zero disturbance. I estimate equation 1 with ordinary least squares and correct standard errors for within-state correlation in the disturbance terms (Moulton, 1990).

Several recent authors (e.g., Figlio, 1997; Poterba, 1997; Hoxby, 1994) show that demographic and economic characteristics of the school district affect the provision of measured school quality variables, such as the student-teacher ratio. Many of these variables, such as the age and income distribution of the community, have likely not changed much during the relevant time period and are, in practice, only measurable at one point during the panel. To the extent that these variables have not changed over the sample time period, these variables are subsumed into the district fixed effect γ_i. Other variables, such as the student body population and the percentage of students who can be federally categorized as "special needs" students, do change—sometimes substantially—over the sample period and are observed annually. I therefore include these two variables as controls.

The estimated β (and standard error) from equation 1 is reported in the first column of Table 1. We observe that, after taking into account the time-varying district characteristics that are available in the data, time-invariant district-specific effects and year effects, Measure 5 has been associated with about eight-tenths of a student more per teacher. This result suggests that, on average, Measure 5 has led to about five percent higher student-teacher ratios in the years immediately following its imposition.

Heterogeneity in the Treatment Effect

The preceding evidence suggests that Measure 5 has had substantial average effects on student-teacher ratios in Oregon in the years immediately

TABLE 1
ESTIMATED EFFECTS OF MEASURE 5 ON STUDENT-TEACHER RATIOS IN OREGON—PANEL RESULTS
DEPENDENT VARIABLE: STUDENT-TEACHER RATIO (SIX YEARS OF DATA)

	Model Specification									
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
(1) Subject to Measure 5	0.804** (0.216)	0.633** (0.255)	0.867** (0.316)	−0.251 (0.606)	1.264** (0.550)	0.097 (0.712)	−0.699 (0.570)	−2.086** (0.696)	−1.778** (0.777)	−0.820 (1.252)
(2) Student enrollment in 1989 × (1)/1,000		0.150** (0.053)								0.048 (0.075)
(3) (Student enrollment)^2 in 1989 × (1)/1,000,000		−0.0044** (0.00011)								−0.0026* (0.0015)
(4) Poverty rate in 1990 × (1)			−0.021** (0.009)						−0.015 (0.010)	−0.015 (0.010)
(5) Percent local revenues in 1989 × (1)				1.517* (0.836)		2.387** (0.798)		1.753** (0.810)	1.540* (0.832)	2.076** (0.885)
(6) Per pupil expenditures in 1989 × (1)/1,000					−0.117* (0.070)	−0.157** (0.072)				−0.102 (0.093)
(7) Student-teacher ratio in 1989 × (1)							0.093** (0.037)	0.104** (0.035)	0.099** (0.035)	0.059 (0.054)
Estimated mean effect	0.804	0.788	0.512	0.788	0.510	0.478	0.790	0.614	0.479	0.482
Percentage positive predictions	100.0	99.7	95.8	99.4	93.6	91.3	96.8	91.0	88.7	88.1
R^2	0.754	0.755	0.744	0.754	0.754	0.754	0.752	0.752	0.743	0.744
Number of observations (Number of districts)	2928 (602)	2921 (599)	2864 (584)	2896 (593)	2884 (589)	2884 (589)	2905 (594)	2883 (589)	2846 (580)	2842 (579)

Note: Robust standard errors are in parentheses beneath coefficient estimates. Parameter estimates marked ** are statistically significant at the five percent level; those marked * are statistically significant at the ten percent level. All regressions also include a full set of year and school district fixed effects, as well as annual school district enrollment and the percentage of students governmentally categorized as special needs students. Full sets of regression results are available upon request.

following its imposition. But it is possible that the effects of Measure 5 may vary across districts in systematic, predictable ways. For instance, it is likely that those school districts that, prior to the limit's passage, derived most of their revenues from local sources will have been more severely affected by a tax limit that explicitly lowers local revenues. In the interest of exploring these potential sources of heterogeneity, I estimate a series of models of the form

2

$$s_{it} = \beta L_{it} + \zeta_i L_{it} Z_{ij} + \delta X_{it} + \gamma_i + \lambda_t + \varepsilon_{it}$$

where the Z's are a series of one or more variables representing heterogeneity in prelimit conditions across districts. Specifically, I estimate models in which I interact the Measure 5 dummy variable with various combinations of variables, including prelimit student enrollment (and its square), to capture the effects of Measure 5 on different sized districts; the poverty rate in the district in 1990, to capture heterogeneity across districts with different degrees of wealth; the percent of prelimit revenues from local sources, to proxy for differences in the reliance on local revenues; and prelimit per pupil expenditures and student-teacher ratios, as a proxy for prelimit service levels.[5] In all cases, I continue to control for unobserved district-level heterogeneity, time effects, and the time-varying factors described previously. In addition, in all cases, all coefficients on the tax limit variable and its interactions are jointly significantly different from zero at conventional levels. The results of these regressions are also reported in Table 1.

We observe that, in all specifications, in the vast majority of school districts (88.1 to 100 percent, depending on the model), the estimated effect of Measure 5 is positive. (Note that this is not immediately obvious from observation of the individual coefficients, as often the uninteracted estimated coefficient β is significantly negative or not differentiable from zero.) Moreover, the school districts least likely to have an estimated positive treatment effect are the very smallest districts. Among school districts with 300 or more students, fewer than two percent of the school districts ever have a negative estimated treatment effect of Measure 5. The mean estimated treatment effect ranges from about one-half to eight-tenths of a student per teacher increase, depending on model specification, and is always statistically significant at conventional levels.

More interesting than the mean effects in these alternative models, however, are the estimated effects for school districts at different points in the distribution of the relevant variable. While it would be overly tedious to discuss the results of every model specification, a few illustrative examples may be enlightening. For instance, consider the estimated relationship between the effects of Measure 5 and school district size (specification 2). For virtually all school districts, the estimated effect of the tax limit is positive, but the estimated effect varies considerably by district size. Specifically, the estimated effect increases until student enrollment reaches 17,045 students (only four districts in the state have more), then begins to decrease. Only for Portland is the estimated effect of Measure 5 negative in this specification.[6]

Consider also the estimated relationship between the effects of Measure 5 and the percentage of school district revenues from local sources. The results of specification 4 suggest that school districts that prior to Measure 5 derived 40 percent of their revenues from local sources (one standard below the state mean) experienced less than half of the estimated effect of Measure 5 as those that derived 74 percent of their revenues from local sources (one standard deviation above the mean). Holding constant prelimit spending, as in specification 6, this effect increases in magnitude by 57 percent. Therefore, the evidence suggests that school districts that relied heavily on local funding prior to Measure 5 were the ones most severely affected by the tax limit.

I present Table 2 to offer some concrete examples of the estimated effects of Measure 5 on specific school districts in Oregon. Table 2 presents the range of estimated treatment effects from specifications 3 through 10 from Table 1, for specific school districts in the state. I exclude specification 1 because it is constant for all districts, and I exclude specification 2 because, for districts other than Portland, it typically yields the largest estimated treatment effect of Measure 5. In addition, Table 2 presents district enrollment, prelimit spending per pupil, prelimit student-teacher ratio, and prelimit percentage of revenues from local sources. The districts I present are the 15 largest school districts in the state and the highest and lowest spending urban and nonurban districts in the state.[7] For ease of interpretation, I convert the estimated treatment effects into district-specific percentage change terms.

We observe that, in every case presented, the estimated treatment effects of Measure 5 across specifications are positive, and, almost always, the lower bounds of the estimated treatment effects are statistically significantly different from zero.[8] There exists considerable heterogeneity in the estimated treatment effects within each group presented, suggesting that urban status and prelimit expenditures are probably not the most important factors determining the effect of the tax limit on student-teacher ratios. In general, estimated treatment effects across specifications appear to be positively correlated with the percentage of revenues from local sources.

Sensitivity Check: Evidence from Difference Regressions

Although the fixed-effects models presented above implicitly control for variables that are only measured once over the sample period, it may be useful to estimate a series of models in which the dependent variable is *change* in the student-teacher ratio from before the tax limit (say, 1987) to after the tax limit (say, 1992). I perform a number of variants of this exercise and report the results in Table 3. Here, I explicitly control for a set of demographic and economic variables that I had previously assumed were in the fixed effect. All regressions include year dummies, school district enrollment in 1987–8 and 1991–2 and the percentage of students governmentally categorized as special needs students. Specifications (12) through (17) include a series of demographic variables from the 1990 Census: median family income, percent in poverty, percent of students categorized as "at risk," percent nonwhite, percent of adults who are high school dropouts, percent of adults with bachelor's degrees, and percent of students with poor English skills. Specifications (13) through (17) also include the percent of revenues from local sources in 1989.

The results from these regressions are considerably stronger than those

TABLE 2
ESTIMATED EFFECTS OF MEASURE 5 ON SPECIFIC SCHOOL DISTRICTS IN OREGON
STUDENT-TEACHER RATIO EQUATIONS FROM TABLE 1; SPECIFICATIONS *NOT* BASED SOLELY ON ENROLLMENT

School District	Enrollment (1990)	Per Pupil Expenditures ($1990)	Student-Teacher Ratio (1987)	Range of Estimated Effects (Percent Drop)
I. Largest districts in state				
Portland (large central city)	53,042	6,137	23.1	3.2–4.8%
Salem/Keizer (midsize central city)	27,756	4,787	24.0	2.5–6.0%
Beaverton (suburb of large MSA)	24,874	5,398	20.0	4.8–6.8%
Eugene (midsize central city)	17,904	5,817	19.4	3.7–5.9%
North Clackamas (suburb of large MSA)	12,403	5,471	19.1	4.7–6.1%
Springfield (midsize central city)	10,395	5,155	19.7	2.6–6.4%
Medford (midsize central city)	10,161	4,970	19.2	4.5–6.1%
Bend/Lapine (small town)	9,481	6,602	21.5	3.6–7.1%
Tigard (suburb of large MSA)	8,255	8,360	18.4	4.2–6.8%
Corvallis (large town)	7,421	6,336	17.5	5.3–6.4%
Greater Albany (large town)	7,229	4,950	18.5	3.5–5.4%
Reynolds (small town)	6,975	5,083	20.0	3.9–6.0%
Klamath County (small town)	6,864	5,408	18.4	4.1–6.0%
Roseburg (small town)	6,656	4,122	18.2	3.4–5.7%
Lincoln County (small town)	6,467	5,644	17.7	4.3–5.5%
II. Highest spending urban districts in state (>300 students, excludes Tigard, above)				
Canby UHS	1,198	8,124	18.5	3.2–4.7%
McKenzie	438	7,986	12.1	4.1–7.9%
Crow-Applegate-Lorain	460	7,671	14.7	3.9–5.6%
Carus	320	7,302	17.8	2.0–4.4%
Orient	670	7,200	20.3	1.7–6.1%
Lake Oswego	6,218	7,118	17.7	5.5–7.0%
Reedville	2,176	7,096	19.5	2.2–5.7%
III. Lowest spending urban districts in state (>300 students)				
Aumsville	574	3,558	20.8	1.5–6.2%
Stayton 97J	1,005	3,999	20.2	2.5–6.2%
Welches	519	4,116	19.6	1.8–6.8%
Gaston	671	4,243	19.6	4.4–6.5%
Molalla	1,262	4,356	17.1	1.9–5.3%
Newberg	4,186	4,510	19.9	3.7–5.9%
IV. Highest spending nonurban districts in state (>300 students)				
North Douglas	534	9,099	14.3	1.1–5.1%
Pine-Eagle	358	7,886	10.8	5.1–8.4%
Central Linn	833	7,571	14.6	3.6–5.9%
Columbia County	1,482	7,556	14.5	4.9–6.7%
Klamath Falls UHS	1,920	7,495	20.3	2.1–3.5%
Columbia	1,737	7,445	14.9	3.9–5.8%
Neah-Kah-Nie	847	6,887	14.9	2.9–4.9%
V. Lowest spending nonurban districts in state (>300 students, excludes Roseburg, above)				
Mari-Lynn	301	3,726	21.2	2.2–5.0%
Milton-Freewater	977	4,153	16.8	1.4–5.4%
Crook County	2,730	4,166	18.8	2.9–6.7%
Josephine County	5,883	4,238	18.3	2.9–6.8%
Philomath	1,498	4,324	17.9	3.7–7.1%
Vale	614	4,328	17.2	0.6–6.0%
Brookings-Harbor	1,667	4,340	21.7	2.8–5.5%

TABLE 3

ESTIMATED EFFECTS OF MEASURE 5 ON STUDENT-TEACHER RATIOS IN OREGON—DIFFERENCE RESULTS

DEPENDENT VARIABLE: DIFFERENCE IN STUDENT-TEACHER RATIO FROM 1987 TO 1992

	Model Specification						
Variable	(11)	(12)	(13)	(14)	(15)	(16)	(17)
(1) Subject to Measure 5	2.948** (0.336)						
(2) Subject to Measure 5 (includes demographic controls)		3.082** (0.349)					
(3) Subject to Measure 5 (also includes percent local in 1989)			3.538** (0.604)	3.263** (0.618)	4.584** (0.727)	4.974** (1.131)	0.475 (1.755)
(4) Enrollment in 1987 × (3)/1,000				−3.002** (0.831)		−2.845** (0.830)	−2.638** (0.834)
(5) Enrollment in 1991 × (3)/1,000				2.846** (0.798)		2.700** (0.798)	2.442** (0.825)
(6) Poverty rate in 1990 × (3)					−0.060** (0.021)	−0.055** (0.022)	−0.045** (0.022)
(7) Percent from local sources × (3)						−0.029 (0.025)	−0.040* (0.024)
(8) Student-teacher ratio in 1987 × (3)							0.260** (0.090)
Estimated mean effect	2.948	3.082	3.538	3.453	4.549	4.229	3.802
Percentage positive predictions	100.0	100.0	100.0	99.7	100.0	98.7	98.0
R^2	0.498	0.492	0.491	0.500	0.495	0.503	0.511
Number of observations	588	578	577	577	577	577	577

Note: Robust standard errors are in parentheses beneath coefficient estimates. Parameter estimates marked ** are statistically significant at the five percent level; those marked * are statistically significant at the ten percent level. All regressions also include school district enrollment in 1987–8 and 1991–2 and the percentage of students governmentally categorized as special needs students. Specifications (12) through (17) include a series of demographic variables from the 1990 Census: median family income, percent in poverty, percent of students categorized as at risk, percent nonwhite, percent of adults who are high school dropouts, percent of adults with bachelor's degrees, and percent of students with poor English skills. Specifications (13) through (17) also include the percent of revenues from local sources in 1989. Full sets of regression results are available upon request.

reported in Table 1. When I control for observable demographic and economic characteristics, but do not control for district fixed effects, I find that Measure 5 is estimated to increase student-teacher ratios in Oregon by between 19 and 30 percent, depending on model specification. I therefore conclude that failing to control for district-specific fixed effects likely leads to an overstatement of the estimated effects of Measure 5. This result also suggests that the results of prior studies (e.g., Figlio, 1997) that rely on cross-sectional variation, even with a rich set of control variables, to identify the effects of tax limits should be treated with some caution.

WAS THE INCIDENCE OF MEASURE 5 BORNE BY ADMINISTRATION?

In the preceding section, I find that instructional services have apparently been substantially reduced in the wake of Measure 5. The survey evidence reported above, and informal anecdotal evidence from the state of Oregon surrounding the passage of Measure 5, suggests that voters believed that administration would be cut at least as much as instruction. (Indeed, the pervasive belief in Oregon apparently was that instruction quality would hardly be cut at all and that the incidence of Measure 5 would fall almost exclusively on administration.) To gauge whether this has been occurring, I again estimate equations 1 and 2, this time replacing s_{it} with a_{it}/i_{it}, the ratio of administrative to instructional expenditure.[9] Here, I only have data for the 1989–90, 1990–1, and 1991–2 school years.

I report the results of this analysis in Table 4. While the results are not as strong as those regarding the student-teacher ratios presented above, they are striking in that they suggest that what happened in Oregon following Measure 5 may have even been the *opposite* of what voters for the ballot measure apparently expected. (This result, however, may not be surprising to economists.) The results suggest that the administration-to-instruction ratio in Oregon may have increased on average by as much as four percent (depending on specification) immediately following the imposition of Measure 5. The reader should be careful to note, however, that this effect is imprecisely estimated and that the most likely result, given the parameter estimates, is that Measure 5 had no short-run effect on the ratio of administration to instruction in the state.

Despite the frequent statistical insignificance of the estimated relationship between Measure 5 and the administration-to-instruction ratio, the uniformly positive estimated mean treatment effect of Measure 5 suggests that, at the very least, the incidence of Measure 5 has been borne equally by instruction and administration. Measure 5 may possibly have even led to an *increase* in administrative spending relative to instructional spending in Oregon, all else equal, although one cannot make such a conclusion with much confidence on the basis of these findings. In every specification, the vast majority of school districts were estimated to have increased their administration-to-instruction ratio in the wake of Measure 5, and in many cases, this estimated effect is statistically different from zero. On the other hand, could find no evidence that administration had been cut more than instruction following Measure 5. The results are even stronger for smaller districts—among school districts with 1,000 or fewer students, almost every district, in every specification, is estimated to have

TABLE 4

ESTIMATED EFFECTS OF MEASURE 5 ON ADMINISTRATION-TO-INSTRUCTION RATIO IN OREGON—PANEL RESULTS
DEPENDENT VARIABLE: RATIO OF ADMINISTRATIVE TO INSTRUCTIONAL EXPENDITURES (THREE YEARS OF DATA)

Variable	Model specification					
	(18)	(19)	(20)	(21)	(22)	(23)
(1) Subject to Measure 5	0.0018 (0.0073)	0.0100* (0.0062)	-0.0064 (0.0096)	0.0130 (0.0452)	0.0556* (0.0333)	0.003 (0.043)
(2) Student enrollment in 1989 × (1)/1000		-0.0035** (0.0012)				-0.0040** (0.0013)
(3) Poverty rate in 1990 × (1)			0.0011** (0.0004)			0.0007 (0.0005)
(4) Percent local revenues in 1989 × (1)				-0.0189 (0.0812)	-0.0495 (0.0764)	0.102** (0.040)
(5) Student-teacher ratio in 1989 × (1)					-0.0014 (0.0021)	-0.0032* (0.0018)
Estimated mean effect	0.0018	0.0045	0.0250	0.0019	0.0040	0.0158
Percentage positive predictions	100.0	86.2	100.0	73.0	64.0	73.3
R^2	0.842	0.842	0.821	0.803	0.805	0.832
Number of observations (Number of districts)	1602 (602)	1599 (599)	1568 (584)	1593 (593)	1585 (589)	1561 (580)

Note: Robust standard errors are in parentheses beneath coefficient estimates. Parameter estimates marked ** are statistically significant at the five percent level; those marked * are statistically significant at the ten percent level. All regressions also include a full set of year and school district fixed effects, as well as annual school district enrollment and the percentage of students governmentally categorized as special needs students. Full sets of regression results are available upon request.

increased its administration-to-instruction ratio since Measure 5. Oregon school districts have not cut back administrative expenses in the wake of Measure 5. This result suggests that Measure 5 might have forced smaller districts to increase their administrative staffing, perhaps to hire grant writers or others to handle additional paperwork, while larger districts might have been more able to handle these tasks with existing staffing.

There are at least two possible explanations for the observation that the incidence of Oregon's Measure 5 seems to have been borne at least as much by instruction as by administration. One explanation is that school districts are quasi-monopolists capable of extracting rent. If moving is costly and decision makers value administrative consumption, it is unsurprising that administrators might pass most of the burden of a tax limitation onto instruction.[10] Another explanation of the findings is that perhaps schools in Oregon were "lean" prior to the imposition of Measure 5. Since some level of administration is necessary to run schools and school systems, this base level of administration required could be thought of as a fixed cost. If schools are already operating at this lean level and are faced with additional cuts, they have no choice but to cut instructional services. At present, I have no way of knowing which explanation more closely fits the facts. A third possible explanation is that I am merely picking up an accounting artifact—what gets categorized as instruction and administration may have changed simultaneously with Measure 5's imposition. Upon scrutiny of the detailed accounting records from all 50 states, I could find no evidence to support accounting changes as an explanation for my result.

Concluding Remarks

This paper provides new evidence on the effects of 1990s-era property tax limitations on local public school provision. Using a comprehensive panel of all Oregon and Washington school districts, I estimate the effects of Oregon's 1990 property tax limitation on school services (here, the student-teacher ratio) and the administration-to-instruction spending ratio. The presence of Washington allows for a geographic counterfactual, and the panel of observations prior to and following imposition of Measure 5 allows me to control for unobserved heterogeneity.

I find that Oregon's imposition of a property tax limitation had almost invariably negative effects on local public school provision, if school quality is measured by the student-teacher ratio in the public schools. Ninety-five percent of Oregon school districts with over 300 students raised their student-teacher ratios in the wake of Measure 5, and the estimated direct effect of Measure 5 on the student-teacher ratio is large in magnitude and statistically significant at any conventional level. However, at the same time, Oregon schools apparently did not reduce the ratio of administrative-to-instructional expenditures (and may potentially, on average, have even *increased* this ratio by a statistically significant amount). If the measure of school efficiency put forth by the proponents of tax limitations is that schools provide the same level of educational services with less administrative overhead, it is apparent that the consequence of Measure 5 seems to be different from the desired result.

What will be the long-run results of Measure 5 (or comparable property tax limitations)? Of course, it is impossible to say. The literature concerning whether differences in school quality measures, such as the student-teacher ratio, are associated with differences in student academic achievement has been contentious. While some recent papers (e.g., Sander, 1993; Ferguson and Ladd, 1995) have found a positive relationship between measured school inputs and student achievement, the majority of the existing research summarized by Hanushek (1986, 1991) suggests no systematic relationship between school spending or inputs and student performance. Likewise, studies such as Card and Krueger (1992) that find a strong and significant relationship between school quality measures and future labor market returns have been challenged by Betts (1995) and Heckman, Farrar, and Todd (1996). Despite the lack of consensus in the education and labor market literatures, there is some evidence that students attending schools subject to property tax limits fare worse, all else equal, on standardized tests. Figlio (1997) finds that merely attending a school subject to a tax revolt–era limitation is associated with substantially lower student achievement in mathematics, science, social studies, and reading, holding constant student, family and peer factors. Downes and Figlio (1997) use individual-level data from before and after the tax revolt and find estimated effects of tax limits on math performance that are similar in magnitude. Since my results regarding the effects of Measure 5 on student-teacher ratios are also similar in magnitude to Figlio's cross-sectional findings, perhaps the long-run effect of Measure 5 on student achievement in Oregon will be similar as well.

Certainly, all states are different. Oregon's experience in the wake of Measure 5 may not be generalizable to Idaho, Illinois, Michigan, Wisconsin, and other states that have adopted or are considering adopting limitations on school revenues or expenditures. But I do present suggestive evidence about how school districts respond to property tax limits. If the goal of a property tax limit is to provide the same level of educational services, but with lower administrative overhead, my results suggest that property tax limits are not likely to achieve that goal.

ENDNOTES

I have benefited from conversations with Tom Downes, Kim Rueben, Joe Stone, and Therese McGuire, as well as colleagues at the University of Oregon. Two anonymous referees provided valuable suggestions. All remaining errors are my own.

1 Because property values in Oregon have continued to rise during this period, most taxpayers have still faced nominal increases in their property taxes since Measure 5's passage. These increases in taxes led a number of taxpayers to believe that Measure 5 was ineffective and led to the passage of Measure 47 in November 1996, which capped property taxes at their 1995 nominal level and is independent of property values.

2 Silva and Sonstelie (1995) also find that Proposition 13 and *Serrano* are associated with a substantial decline in school spending.

3 One other current working paper investigates the effects of a 1990s-era tax limit on one measure of school quality—student test performance. Downes, Dye, and McGuire (forthcoming) study the effects of Illinois's recent "collar counties" tax limit on aggregate student achievement. While their study makes a valuable contribution to the literature on the effects of tax limits, the special circumstances surrounding the Illinois tax cap may limit their study's generalizability. Although they find little evidence that the tax limit has affected student achievement, there is little reason to believe that we would observe significant short-run effects on student test performance. In addition, while the Illinois tax limit has the advantage of having a control group in the same state as the treatment group, it is disadvantaged in that the affected school districts are the wealthy suburban areas surrounding Chicago, arguably systematically different in nature from the rest of the state (although obviously some of the richest districts in Illinois are in the "control" group.) Moreover, the control districts may well be affected by the tax cap as well, as unaffected districts in the same Chicago market may provide competition for the tax limit–subject districts (Hoxby, 1994).

[4] I include in my analysis some extremely small rural school districts. It may be that including these districts may introduce noise in the measurement of the dependent variable, as they will be more susceptible to idiosyncratic changes in enrollment. Excluding extremely small districts and focusing on districts with enrollments of, say, 300 or more students, leads to larger estimated effects of Measure 5 than the ones I report herein. The estimated effects are larger still if the sampling criterion is 1,000 or more students.

[5] These variables may potentially proxy for differences in demand for the local public good.

[6] Portland was, in reality, one of only a handful of districts to experience a reduction in student-teacher ratio after Measure 5's imposition.

[7] I adopt the Census definitions of urban and nonurban.

[8] I omit standard errors here only to conserve upon space, but they are, of course, available upon request from the author.

[9] Ideally, I would also estimate models with the dependent variable being the student-to-administrator ratio. Unfortunately, I can only get these data for one year and so cannot perform analyses of the type presented in this paper.

[10] Figlio and O'Sullivan (1997) provide a theoretical test and empirical evidence that local governments respond strategically to fiscal constraints. Their results are supportive of the argument that local governments, such as school districts, may engage in rent-seeking behavior.

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