Local Response to Fiscal Incentives in Heterogeneous Communities

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Abstract

I examine the impact of a property tax relief program in New York State that lowered the marginal cost of school expenditure to homeowners. I find that a typical school district, which received 20% of its revenue through the program in the school year 2001-2002, raised expenditure by 4.1% and local property taxes by 6.8% in response to the program. I then examine how the preferences of various groups of local taxpayers affect educational spending by identifying systematic variation across districts in the response to fiscal incentives. These results support the hypothesis that homeowners are more influential on local expenditure decisions than renters, owners of second homes, or owners of non-residential property.

Keywords: property taxation; fiscal federalism; school expenditure

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1. Introduction

While research on the impact of fiscal incentives on public expenditure is longstanding (e.g., Bergstrom and Goodman, 1973; Feldstein, 1975), few studies examine exogenous variation in fiscal incentives, and fewer measure the relative influence of groups within heterogeneous communities.[1] In this paper, I examine how school expenditure was affected by a property tax relief program for homeowners in New York State. I then use variation in the response to fiscal incentives to learn about how different groups influence spending on schools.

The New York School Tax Relief Program lowered the marginal cost of local school spending for homeowners by paying a percentage of their school district property taxes. The phase-in of program benefits, as well as variation across districts in homeownership rates, home values, and the fraction of non-residential property, created exogenous variation in fiscal incentives that had statistically and economically significant effects on school expenditure. A typical school district, which received 20% of its revenue through the program in the school year 2001-2002, raised expenditure by 4.1% and local property taxes by 6.8%.

Additionally, the distribution of tax relief was an important determinant of the degree of local response. Conditional on the fraction of district property taxes paid by the state, districts with more renters, vacant residential property, and non-residential property (and thus more generous benefits for homeowners) raised expenditures by a significantly greater amount. This supports the notion that local homeowners exert greater influence on local expenditure decisions than owners of second homes, renters, or owners of non-residential property.

Finally, I explore the relative impact of tax relief for elderly and non-elderly homeowners

[1] A number of studies examine how spending relates to variation in the composition of local residents (e.g., Cutler et al., 1993; Poterba, 1996; Harris et al., 2001; Reback, 2008; and Hilber and Mayer, 2009), or examine the relationship between desired spending and household characteristics using micro-data (e.g., Rubinfeld, 1977; Bergstrom et al., 1982; Gramlich and Rubinfeld, 1982; Lankford, 1985).

using the fact that the program provided them with different benefits. I find no evidence that providing tax relief for elderly homeowners had a greater impact on spending than tax relief for non-elderly homeowners. However, the lack of variation in the proportion of households who are elderly limits the conclusions that can be drawn from this part of my analysis.

Sections 2 and 3 contain details on the tax relief program and the data used in my analysis. I present a conceptual framework in Section 4 and my main empirical estimates in Section 5. Sections 6 and 7 contain additional empirical analyses, and Section 8 concludes.

2. Local School Finance and Tax Relief in New York State

School districts in New York State are fiscally independent from other local government, and the state does not redistribute local property tax revenue across districts. The state provides districts with lump-sum grants in a progressive manner based on district property value and income, but these grants are funded with statewide income and sales taxes and lottery revenues.

School district budgets in New York must be approved in annual referenda.[2] By law, voters are mailed information such as the proposed budget, the percentage change from the current budget, the CPI, and their estimated property taxes if the proposed budget is approved. Turnout for budget referenda is typically low, but these votes can be highly contested; during the period from 1991 to 1997, 23% of budget referenda did not pass.[3]

Once a budget is approved, the local tax burden is divided among property owners in proportion to property value. This is described by Equation 1, where t_{ij} is the tax burden for a representative household i with property in district j, v_{ij} is the value of the household's local

[2] New York's five largest cities (Buffalo, New York City, Rochester, Syracuse, and Yonkers) operate on a different funding system and are treated differently by NYSTAR. I therefore exclude them from my analysis.
[3] The median turnout rate for school budget referenda was roughly 14% in 2003 (the first year for which data are available). The median county turnout for statewide elections in 1999—a year without presidential, gubernatorial, or congressional campaigns—was about 37%. Further analysis of referenda can be found in Ehrenberg et al. (2004).

property, V_j is the total value of district property, B_j is the school budget, and Aid_j is outside aid.

$$(1)\ t_{ij} = \frac{v_{ij}}{V_j}\left(B_j - Aid_j\right)$$

The marginal cost of school expenditure to a household is often referred to as the household's "tax-price" of public spending. Differentiating Equation 1 with respect to the school budget (B_j), we see that a household's tax-price is equal to the fraction of local taxable property it owns.

New York's School Tax Relief Program (NYSTAR), created in 1997, exempts part of the taxable value of owner-occupied homes from taxation by school districts, and reimburses the district for any property taxes forgone. The effect of NYSTAR on a homeowner's property taxes is shown by Equation 2, where x_{ij} is the amount of the exemption. If the school budget and outside aid are held constant, the homeowner's property taxes fall. Also, given the amount of the exemption, the *percentage* tax reduction will greater for owners of less valuable homes.

$$(2)\ t_{ij} = \frac{v_{ij} - x_{ij}}{V_j}\left(B_j - Aid_j\right)$$

We can also see that NYSTAR will reduce a homeowner's tax-price of school expenditure; essentially, the state shoulders part of the homeowner's tax burden. A lower tax-price provides an incentive to increase school spending, which I discuss in greater detail in Section 3. [4]

There are two types of exemptions: Enhanced NYSTAR for homeowners aged 65 or older with income below $60,000, and Basic NYSTAR for all other homeowners.[5] To receive benefits, homeowners submit a one-page application to the local tax assessor. Due to income

[4] Note that NYSTAR is different than a typical "homestead exemption," used in many states for property tax relief. Unlike NYSTAR, homestead exemptions are usually for special groups (e.g., elderly or disabled persons), given at the option of the local community, and not funded by the state (see Mikhailov, 1998; Sexton, 2003). Without funding, exemptions simply shift local tax burden from those receiving them to those who do not.

[5] Income for purposes of Enhanced NYSTAR eligibility in 1998-1999 excluded supplemental security income, welfare, returns of capital, gifts, inheritances, and a few items like veterans disability compensation and foster care payments to grandparents. In 1999-2000, it was redefined as federal adjusted gross income less IRA distributions.

restrictions, recipients of Enhanced NYSTAR must reapply annually, and are reminded to do so by the local assessor. By law, school districts must notify property owners about the program, and, when mailing information about budget referenda, districts must explicitly state how the NYSTAR exemption will impact a homeowner's property taxes if the budget is approved.

The program was phased in over a period of four years. The Enhanced NYSTAR exemption of $50,000 became available in the school year 1998-1999. The Basic NYSTAR exemption became available the following year at a level of $10,000, and rose in $10,000 increments until reaching $30,000 in the school year 2001-2002. In eight counties with high average home values (Dutchess, Nassau, Orange, Putnam, Rockland, Suffolk, Sullivan, and Westchester), the dollar amounts of NYSTAR exemptions were raised by the ratio of the county's three-year median home sale value to the state median. For example, in Westchester County—with the highest average home values in the state—Basic and Enhanced NYSTAR exemptions in 2001-2002 were $70,386 and $117,310, respectively.

Using data from the 2000 Census on owner-occupied home values (and setting aside the eight counties with larger exemption levels), I find that 15% of districts had a median value worth $60,000 or less, so at least half of the homeowners in those districts were eligible for at least a 50% reduction in tax-price in 2001-2002; 92% of districts had a median value of $120,000 or less (at least a 25% reduction). In the remaining eight counties, NYSTAR exemptions were still substantial, e.g., 25% of districts in Westchester had median home values below $280,000, implying at least a 25% tax-price reduction for half of their homeowners.

The creation and phase-in of NYSTAR was an economically significant change in New York's system of public school finance. Payments to districts for NYSTAR exemptions rose from about $500 million in 1998-1999 (about 3% of school district revenue) to about $2 billion

(9% of district revenue) in 2001-2002. The overall share of school district revenue coming from the state rose from 40% in 1997-1998 to 51% in 2001-2002.

While the goal of NYSTAR was to reduce property taxes, the fiscal incentives it created were well perceived by some economists, who wrote that NYSTAR "will have some unintended consequences…it will result in higher school tax rates, and hence in higher taxes on commercial and industrial property, including rental housing" (Duncombe and Yinger, 1998). In an effort to prevent the crowding out of tax reductions, Governor George Pataki's original proposal for the program contained a provision to limit property tax growth to the lesser of 4% or the rate of inflation. However, though Pataki pushed the tax cap as "a critical part of my STAR plan" (New York State Governors Office, 1997), it was excluded in the final bill passed by the legislature.

3. Data and Descriptive Statistics

My analysis is based on demographic and financial data on a balanced panel of 619 school districts in New York State from the school years 1995-1996 through 2001-2002. Excluded from my analysis are New York's five largest cities (whose school districts are not fiscally independent from city governments), a small number of districts that serve only elementary students, only high school students, or only disabled students, and districts that merged during this time period. District level data on the number and type of NYSTAR exemptions, payments to districts made through the NYSTAR program, and aggregate property value and taxes by property class (e.g., residential, commercial, etc.) come from the New York State Office of Real Property Services. District finance and enrollment data come from the New York State Education Department and the Office of the Comptroller, and the Office of Taxation and Finance provided me with annual data on the aggregate adjusted gross income and the number of income tax returns filed in each school district. Information on district level

demographics, including homeownership and the value of owner-occupied homes, comes from the 1990 and 2000 Census School District Demographics files. In some instances, I use the 1990 and 2000 Integrated Public Use Microdata Series (IPUMS) data for New York State as a whole.

The means and standard deviations for a number of key variables are shown in Table 1. In the school year 1997-1998, average per pupil spending was $10,530 (the national average was around $7,200), and about 90% was operational expenditure. Average state aid for operations was $3,417 per pupil and average federal aid per pupil was $374. The fraction of local property taxes paid through NYSTAR grew from 6% in 1998-1999 to 20% in 2001-2002. Other variables used in analysis below are listed in Table 1 and described in Section 4. To provide a sense of heterogeneity within districts, Table 1 includes statistics on the dispersion of household income and dispersion in adults' years of education (25 years and older), taken from the 2000 Census.

The costs and benefits of operational expenses are realized concurrently, and operational state aid is lump-sum. In contrast, capital projects involve local bond issues that are subject to separate referenda, and state aid for capital expenses is based on a complex system of matching grants.[6] I therefore focus on operational expenses in my analysis, but use "expenditure" or "spending" to mean "operational expenditure" throughout the paper. Financial data are adjusted to 1999-2000 dollars using the Northeast urban consumer price index, averaged from the July prior to each school year to the following June.[7]

4. Conceptual Framework

Studies of how fiscal incentives effect local government are largely based on the idea that

[6] Complete details on state aid for schools are given in annual reports ("State Formula Aids and Entitlements for Schools in New York State") published by the State Aid Unit of the New York State Education Department.
[7] Direct measures of the cost of public education are not available, and the CPI may not fully capture changes in the cost of education (see Rothstein and Hawley Miles, 1995). However, the flexible time controls used in my regression analysis ensure that any broad changes in education costs do not drive my results.

decisions are based on the preferences of the median voter (Black, 1948, Inman, 1979).[8] The median voter's identity is unknown, and researchers assume a relation between preferences and characteristics, e.g., the median voter is the voter with median income (Bergstrom and Goodman, 1973). However, a central assumption in this framework is that the identity of the median voter does not change in response to fiscal policy. This is unlikely to hold in the case of NYSTAR, because its effects will vary among households within a school district, depending on homeownership, age, income, and property value.[9] My conceptual framework treats the community as a single decision maker, rather than focus on a median voter.[10] I later adjust the framework to compare the impacts of fiscal incentives for elderly and non-elderly homeowners.

Figure 1 illustrates the trade-off faced by a community between spending on public schools and aggregate consumption of other goods. A community must spend lump-sum aid from the state (*Aid*) on public schools, but has the option of using all of its remaining income (*Y*) for consumption (depicted by point A). If the community wishes to increase spending on public schools it can collect property taxes and reduce consumption (depicted by point B).

Aggregate local tax collection is shown in Equation 3, following earlier notation.

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[8] It is well known that a budget maximizing agenda setter can achieve higher spending than preferred by the median voter (Romer and Rosenthal, 1979 and Romer et al., 1992). Since school boards propose budgets, this result is potentially relevant to NYSTAR. However, in a dynamic framework where the reversion amount is the previous budget (as it is in New York), even in an agenda setting model a school budget can only rise when the median voter's preferred spending level rises. Also, the agenda setters—school board members—are elected officials, and may be voted out if they repeatedly propose budgets larger than desired by the median voter.

[9] To see this, suppose a district contains homeowners A, B, and C, with assessed property values of $25,000, $50,000, and $100,000, respectively, and assume that their preferences for school spending are ordered in the same way as their property values, making B the median voter. NYSTAR, when fully phased in, exempted $30,000 of value from taxation, and would give a 100% reduction in tax price to homeowner A. Clearly this could change the ordering of preferences for school spending in the district; homeowner A is likely to prefer very large increases in the school budget, relative to B or C, since the direct cost to A of additional spending (i.e., A's tax-price) is zero.

[10] Eom et al. (2005) analyze the impacts of NYSTAR using a median voter framework, and maintain the assumption that the median voter does not change. Because they focus on the impact of spending on student outcomes, their framework addresses the demand for educational services, determined by a production function with marginal cost and efficiency terms. As they discuss, our methodologies are not inconsistent, but are geared towards the particular questions on which we focus. While demand for education services may be more appealing on some grounds, expenditure is the actual choice variable and is commonly used in both theoretical and empirical work on this topic.

$$(3)\ T_j = \sum_{k \in j} t_{kj} = \frac{\sum_{k \in j} \left(v_{kj} - x_{kj} \right)}{V_j} \left(B_j - Aid_j \right)$$

In the absences of exemptions (x_{kj}), the community must raise a dollar in taxes to raise the school budget by one dollar. In other words, the "community tax-price" (dT_j/dB_j) equals one. If local homeowners receive exemptions through NYSTAR, the community tax-price will fall below one. Specifically, it will equal one minus the fraction of local taxes being paid by the state. In Figure 1, a lower community tax-price (P_1) shifts the community budget constraint outward, increasing the amount spent on public schools for any given (positive) level of property taxation. This allows the community to increase school spending *and* reduce taxes (point C).

What is most interesting in Figure 1 is that income and lump-sum aid are crucial for determining the impact of a change in the community tax-price. To clarify this point, notice that if the community's budget constraint is extended to the vertical axis, it crosses at the point $\tilde{Y}_1$. A community with income of $\tilde{Y}_1$ and no lump-sum aid might choose the same aggregate spending and consumption (point B). However, an equivalent change in tax-price for a community with income $\tilde{Y}_1$ would shift the budget constraint to a much greater degree and allow much higher levels of both consumption and school spending (point D). In other words, matching grants can have smaller effects in communities that receive lump-sum aid. This interaction has received little attention in the fiscal federalism literature. Seminal papers on matching and lump-sum grants (Bradford and Oates, 1971, Oates, 1972) consider the two grant structures in isolation and do not discuss the implications of combining both types.

One can account for the interaction of lump-sum and matching grants, by using the point at which the budget constraint crosses the y-axis—the sum of aggregate income and the product of lump-sum aid and the tax-price. Borrowing from the labor supply literature, I refer to this

point as "virtual income" (Burtless and Hausman, 1978). Figure 1 shows that a reduction in tax-price for a community receiving outside aid leads to a fall in virtual income (from $\tilde{Y}_1$ to $\tilde{Y}_2$), and that the total effect of a lower tax-price can be separated into a price effect (point B to point D) and virtual income effect (point D to point C). This notion is stated formally in Equation 4.

$$(4)\ \Delta E = \frac{\partial E}{\partial P}\Delta P + \frac{\partial E}{\partial Y}\Delta \tilde{Y} \qquad \frac{\partial E}{\partial P} < 0\ ,\ \frac{\partial E}{\partial Y} > 0$$

Because price and virtual income effects are of opposite signs, estimates of price effects that fail to control for virtual income will be biased towards zero. Importantly, the issue of virtual income will come into play in any public finance system that involves both lump-sum grants and changes in tax-prices. For example, many states' school finance systems lower tax-prices in poor school districts in an attempt to equalize spending, and these poor districts also receive lump-sum grants from the federal government under its Title I program.

5. Empirical Specification and Main Regression Estimates

Following previous research (e.g., Feldstein, 1975; Gramlich and Rubinfeld, 1982), I begin by specifying the demand for school expenditure as a log linear equation (Equation 5).

$$(5)\ ln E_{jt} = \alpha_j + \pi ln \tilde{Y}_{jt} + \delta ln P_{jt} + \beta Z_{jt} + \varepsilon_{jt}\ \ where\ \tilde{Y}_{jt} = Y_{jt} + Aid_{jt} * P_{jt}$$

Expenditure (E_{jt}) for district j in year t is a function of a district fixed effect (α_j), virtual income ($\tilde{Y}_{jt}$), community tax-price (P_{jt}), time varying district characteristics (Z_{jt}) and other factors (ε_{jt}).

In my analysis, I measure community tax-price (P_{jt}) using one minus the fraction of local property taxes paid by the state. Setting the community tax-price to one in the absence of NYSTAR is done for simplicity. Even if other factors affect a district's marginal cost of spending (e.g., property tax deductions), changes in the *log* of community tax-price due to NYSTAR will be measured correctly, and the inclusion of district fixed effects will account for

preexisting and persistent differences in marginal costs. So as long as factors such as tax deductibility do not vary significantly over time within districts, they will not bias my results.

A well known empirical regularity in public economics is the "flypaper effect": a dollar of lump-sum aid tends to increase public spending by more than a dollar of income (Hines and Thaler, 1995). In order to incorporate this into my specification, I split virtual income into components that separately capture the impacts of income and lump-sum aid (Equation 6).

$$(6)\quad ln\left(\tilde{Y}_{jt}\right) = ln\left(Y_{jt}\right) + ln\left(1 + \frac{P_{jt} * Aid_{jt}}{Y_{jt}}\right)$$

My data on income come from tax filings, and include the number of income tax returns filed in each district and each year. Changes in tax returns will likely capture variation in a district's adult population and employment, while changes in average taxable income will reflect growth in earnings conditional on employment. Since no annual data on school district population exist (only student enrollment), I separately estimate coefficients for the natural log of households paying taxes (H_{jt}) and the log of mean taxable income ($\overline{Y}_{jt}$); see Equation 7.

$$(7)\quad ln\left(E_{jt}\right) = \alpha_j + \pi_1 ln\left(H_{jt}\right) + \pi_2 ln\left(\overline{Y}_{jt}\right) + \pi_3\left(1 + \frac{P_{jt} * Aid_{jt}}{Y_{jt}}\right) + \delta ln\left(P_{jt}\right) + \beta Z_{jt} + \varepsilon_{jt}$$

The coefficients π_1, π_2, and δ can be interpreted as elasticities of spending with respect to the number of income taxpayers, mean gross income, and tax-price only when Aid_{jt} is zero. For simplicity, I refer to estimates of π_1, π_2, and δ as elasticities, but this is only technically correct for a (hypothetical) district receiving no lump-sum aid. Additionally, π_3 is not the elasticity of spending with respect to lump-sum aid, because the effect of a proportional change in lump-sum aid is mediated by community tax-price and the ratio of aid to income.

Many factors that affect the demand for school expenditure are hard to measure but

relatively stable over time (e.g., general preferences for education spending, strength of teachers' unions), and will be captured by district fixed effects. One important time-varying characteristic I observe is student enrollment. I control for both the log of student enrollment and the change in log enrollment from the previous year. This allows temporary changes in enrollment to have different impacts on spending than persistent trends, and allows spending to adjust to persistent enrollment changes over a two-year period. Because much of the variation in the incidence of NYSTAR occurred over time, I also include a linear year trend for each county and an additional trend for city school districts, and examine other time controls in robustness checks.[11]

Results from a least squares regression of Equation 7 are shown in Column 1 of Table 2. Standard errors are clustered by school district and all regressions are unweighted, though the results are not qualitatively different if regressions are weighted by enrollment or by the number of households in the 2000 census.[12] The estimated elasticity of expenditure with respect to community tax-price ($\hat{\delta}$) is -0.220 with a standard error of 0.021. The estimated elasticities of expenditure with respect to the number of taxpayers ($\hat{\pi}_1$) and mean income ($\hat{\pi}_2$) are, respectively, 0.190 and 0.012, though the latter is statistically insignificant. The coefficient estimate on the component of virtual income containing state aid ($\hat{\pi}_3$) is 0.880. Wald tests of the

[11] Two issues which are not incorporated in my empirical framework are worth mentioning. First, funding for the NYSTAR program (through state taxes or budget cuts) could affect school expenditure. Second, the capitalization of a decrease in property taxes could increase desired expenditure on education (and other goods). While both effects are possible, I view them as secondary to the direct impact of the program via fiscal incentives, and several factors suggest their omission is not significant. Rather than decrease expenditures or increase taxes, the New York State government increased expenditures during this period and held income tax rates unchanged after a small reduction in 1995. Thus, any effect on local expenditure due to the cost of funding NYSTAR would have had to come through expected future changes in policy. Also, the fiscal incentives of NYSTAR give rise to a free-rider problem; the impact of any individual district's budget on the total cost of the program is negligible. Last, but not least, although capitalization may affect spending via an income effect, capitalization also crowd outs the benefits of NYSTAR by making owner occupied properties more valuable and thus making homeowners responsible for a greater share of local taxes. Thus, to the extent that capitalization occurs, it will bias my estimates towards zero.

[12] Note that the R-squared in this regression is quite high due to the inclusion of district fixed effects. If I demean the dependent variable at the district level prior to running the regression, the estimates are unchanged but the R-squared falls to 0.70. Thus, the specification explains about 70 percent of the within-district variance in expenditure.

pair-wise equality of the three coefficients are all rejected. The estimated elasticity of spending with respect to enrollment is positive (0.205), while the elasticity with respect to enrollment changes is negative (-0.125), implying that temporary shifts in enrollment have smaller impacts than permanent shifts and that spending adjusts gradually in response to enrollment shifts.

The community tax-price elasticity estimate implies that a (hypothetical) district receiving no lump-sum state aid would be expected to raise expenditure by 2.3% as a result of NYSTAR paying 10% of its property tax revenue. In order to measure the total effect of a change in tax-price for a typical district, one must account for the interaction of community tax-price and lump-sum aid. For a district with the state average ratio of lump-sum aid to aggregate income (0.05), a fall in community tax-price from 1 to 0.9 is expected to increase expenditure by 1.9%.[13] The additional revenue for increased expenditure must be generated by property taxes. Assuming that local taxes funded 60% of expenditure (the state average in the year before NYSTAR began), an increase of 1.9% in expenditure would necessitate a 3.2% increase in property taxes. In contrast, for a district with a ratio of lump-sum aid to income at the 95th percentile (0.12) that only relies on local property taxes for 20% of its expenditure, the same decrease in community tax-price raises expenditure by 1.4% and property taxes by only 1.8%.

These coefficients are consistent with the flypaper effect. In a district with the average ratio of aid to expenditure (0.40), a 1% increase in grant aid spent entirely on schools raises expenditure by 0.4%. Assuming a community tax-price of one and the average ratio of aid to income (0.05), my estimates imply spending increases by only 0.04% (0.88*ln(1.0505/1.05)), or roughly 10 cents for each additional grant dollar. In contrast, the propensity to spend out of

[13] The community tax price falls from 1 to 0.9. The increase in expenditure is: -0.22*(ln(0.9)-ln(1))+0.88*(ln(1+0.05*0.9)-ln(1.05)) = 0.019. The price effect on expenditure is -0.22*(ln(0.9)-ln(1)) = 0.023.

income is estimated to be *at most* 2.4 cents on the dollar.[14]

To document the importance of virtual income, I estimate a specification that does not adjust for the interaction between lump-sum aid and community tax-price (Table 2, Column 2).[15] The estimated community tax-price coefficient shrinks towards zero, from -0.220 to -0.171, as expected. Bias due to the omission of virtual income may be limited here, since NYSTAR is more beneficial to districts that have low property values and are more dependent on state aid.

My analysis suggests that NYSTAR had important impacts on school expenditure and taxation. A typical school district, which received 20% of its revenue through NYSTAR in the school year 2001-2002, raised operational expenditure by 4.1% and local property taxes by 6.8% in response to fiscal incentives. This implies substantial crowd-out of tax relief for households owning relatively expensive homes and a considerable increase in taxes for owners of non-residential property, second homes, or rental property.

5.1 Robustness Checks: Endogenous Take-up and Alternate Time Controls

Homeowners must apply for NYSTAR exemptions, and one might be concerned that take-up is correlated with unobservable characteristics that affected spending growth. I therefore estimate Equation 6 via two-stage least squares. To construct instrumental variables, I multiply the number of households that are eligible for exemptions by the statutory value of their exemptions, and divide by the total property value in the school district.[16] This approximates the

[14] With a ratio of aggregate income to expenditure of 8 (0.4/0.05), a 1% increase in income spent fully on schools raises expenditure 8%. Taking the estimate of π_1, a 1% increase in the number of taxpayers would increase expenditure by 0.19%, implying 0.024 (.19/8) cents of each dollar of community income spent on education.

[15] Specifically, I replace $ln\left(1+\frac{P_{jt}*Aid_{jt}}{Y_{jt}}\right)$ with $ln\left(1+\frac{Aid_{jt}}{Y_{jt}}\right)$.

[16] The number of non-elderly homeowners in each district (all of whom became eligible for the Basic exemption in 1999-2000) is given in the census. To find the number of elderly homeowners who are eligible for the Enhanced exemption, I need the number of elderly homeowners with income below $60,000. This is not provided in the 2000 census school district tabulations, and I estimate it by combining district level tabulations of household income and age with the state-wide fraction of elderly homeowners in specific income ranges that own their homes, taken from the IPUMS data. A more detailed explanation is included in the Online Appendix.

fraction of local property taxes that the state would have been paid if *all* eligible homeowners signed up. I use these approximations (along with the aid and income measures) to construct instruments for $ln(P_{jt})$ and $ln\left(1+\frac{P_{jt}*Aid_{jt}}{Y_{jt}}\right)$. These instruments are very strong, with t-statistics above 100. The estimated elasticity of spending with respect to community tax-price in the second stage regression (-0.230) is nearly identical to OLS (-0.220), with a standard error of 0.023.

Another concern is that time-varying unobservable factors correlated with the incidence of NYSTAR may not be captured by county and city specific linear trends. I therefore estimate a number of specifications that include alternate sets of time controls. First, I run specifications that control for average per-pupil instructional spending of states that, like New York, do not redistribute local revenue across districts as part of their school finance equalization (Delaware, Mississippi, Nevada, North Dakota, and North Carolina). Second, I control for spending of neighboring states (Connecticut, New Jersey and Pennsylvania). These data come from the National Public Education Financial Survey, collected by the National Center for Educational Statistics, and information on school finance systems comes from American Education Finance Association (1999) and Hoxby (2001). Estimated coefficients on tax-price from these estimates are -0.194 (school finance) and -0.262 (neighbors), and in both cases are highly significant.

Next, I run a specification that includes interacts a linear time trend with the values of the instrumental variable for P_{jt} in the school year 2001-2002, after the program was fully implemented. I also run a specification that includes county-year fixed effects and a separate set of year effects for city school districts. The coefficients on tax-price from these estimates -.214 (interacted trend) and -0.144 (county-year effects), and again in both cases are highly significant. While the country-year effects estimate is somewhat more conservative, I prefer the estimates based on variation over time in incentives from NYSTAR. This variation is truly exogenous,

whereas variation within years in the impact of NYSTAR is due to differences in districts' property values, tax-base composition, and demographics.

6. Did the Impact of NYSTAR Vary with the Distribution of Tax Relief?

Tax relief from NYSTAR was not distributed equally among local voters and property owners. In this section, I extend my main results by using variation in the incidence of tax relief within communities to examine how different groups of taxpayers influence local expenditure. My analysis is based on the notion that fiscal incentives lead to greater increases in expenditure if they are targeted to groups of taxpayers that are relatively influential.

There are good reasons to believe that homeowners are a relatively influential group of local voters and property owners. Homeowners' incentives to take an interest in the quality of local public services are well documented (see Fischel, 2001; Dehring et al., 2008). Moreover, homeowners have the right to vote in local elections, unlike owners of second homes or other types of property (who are likely to live elsewhere), and are more likely to vote in local elections than renters (DiPasquale and Glaeser, 1999).[17]

Nevertheless, local voters may perceive significant costs to taxing properties they do not own. A number of studies document a negative relation between tax rates and firm location (Wasylenko, 1980; Fox, 1981; Charney, 1983; McGuire, 1985) and property tax rates and economic growth (Dye et al., 2001). Increases in property taxes could lead the tax-base to shrink and homeowners' tax burdens to rise in the long-run. Increases in property taxation might also lead to higher prices for local consumers or lower wages and employment for local workers.

In addition to the differences in preferences of homeowners and other groups, there may

be variation in preferences for local spending among homeowners within the same community. NYSTAR exemptions can be seen as progressive, because tax-prices fall by a greater amount for homeowners with less expensive homes. If those with less valuable homes tend to prefer lower spending levels than their wealthier neighbors, the progressivity of NYSTAR may mitigate differences in spending preferences. However, if preferred expenditure and home value were negatively correlated, NYSTAR will tend to exacerbate these differences in preferences. Though the empirical evidence is mixed (see, for example, Gramlich and Rubinfeld, 1982), home value and spending preferences are typically assumed to be positively correlated in theoretical models of local public finance (e.g., Epple et al., 1984; Calabrese et al., 2008). However, whether progressive fiscal incentives lead to larger changes in district spending is an empirical question.

To gain insight into the influence of various groups of local voters and property owners on local spending, I measure whether responses to NYSTAR varied systematically with the distribution of tax relief. My empirical specification is based on a simple accounting relationship. The fraction of total property taxes paid by the state through NYSTAR is equal to the product of three factors: the fraction of all property value that is occupied residential property (*resocc%*), the fraction of occupied residential property value that is eligible for an exemption (*eligible%*), and the fraction of eligible property value exempt through the NYSTAR program (*exempt%*). This accounting relationship is shown by Equation 8; recall that the community tax-price (P_{jt}) equals one minus the fraction of local property taxes paid through NYSTAR.

$$(8)\quad P_{jt} = 1 - resocc\%_{jt} * eligible\%_{jt} * exempt\%_{jt}$$

Holding P_{jt} constant, variation across districts in the residential share (*resocc%*) or the

[17] To the best of my knowledge, no public data exist on geography of property ownership. However, using data on non-residential property sales in New York State from 1996-2003 (excluding the 5 largest cities), I find that 42% of buyers live in a different city than the purchased property. The school districts in my sample are much smaller than cities, so the fraction of non-residential property owners that vote locally is likely to be quite small.

fraction of residential property that is eligible for an exemption (*eligible%*) must be balanced by variation in the fraction of eligible housing exempt from taxation (*exempt%*). For example, conditional on P_{jt}, homeowners in districts with higher share of non-residential property or a higher share of renters will have a higher share of *their* property exempt from taxation. If homeowners have greater influence on the margin of local public decisions than other groups, then districts where a given amount of tax relief is concentrated among a small group of homeowners may see larger increases in local expenditure.

The distribution of tax relief within districts also depended on variation in the generosity of benefits *among* homeowners. This is driven by variation in the value of owner-occupied homes; less expensive homes receive greater tax-price reductions. However, whether the progressive nature of tax relief leads to greater increases in spending is theoretically ambiguous.

To investigate these issues, I interact community tax-price with measures of tax-relief distribution. This is shown by Equation 9, where W_j is a matrix of interaction terms. District fixed effects, time and enrollment controls are omitted for simplicity. W_j contains three terms: the fraction of property that is not occupied residential (*1-resocc%*), the fraction of occupied residential property *ineligible* for an exemption (*1-eligible%*), and the variation owner-occupied home value within the district (σ^{value}).[18] Since all owner-occupied homes were eligible for exemptions after 1999, variation in eligibility for exemptions is mostly due to the fraction of

[18] To measure the fraction of district property that is not occupied residential, I use the occupancy rate from the 2000 census and the average non-residential property value share from 1991-1995 (which helps avoid concerns regarding reverse causality). The fraction of occupied residential property that is ineligible for an exemption is based on the same measure of eligible households used to construct the instrumental variables discussed in Section 5. To measure variation in the value of owner-occupied homes within district, I use self-reported home value from the 2000 census and take the ratio of the inter-quartile range to the median. I find similar results using the coefficient of variation, which can be approximated using tabulations of homes across value categories. Non-residential property value share and variation in owner-occupied home value do not vary over time, so their main effects will be captured by the district fixed effects. I include the fraction of eligible households as a control, but it is not statistically significant and its omission does not materially my results.

households who rent.

$$(9)\ ln(E_{jt}) = \pi\left[ln(H_{jt}), ln(\bar{Y}_{jt}), ln\left(1+\frac{P_{jt}*Aid_{jt}}{Y_{jt}}\right)\right] + \delta W_j\, ln(P_{jt}) + \varepsilon_{jt}$$

By specifying the terms in this way, the main effect of community tax-price can be interpreted as the elasticity of spending in a (hypothetical) district where all property consists of identically valued homes, all of which receive an exemption. In this "uniform" district, NYSTAR is equivalent to a matching grant—tax-prices change uniformly for all taxpayers—and the community tax-price coefficient can be considered an estimate of an individual household's price elasticity of demand for educational expenditure.

Using Equation 9, I the estimated community tax-price elasticity for a hypothetical "uniform" district is low (-0.033), though imprecisely estimated (standard error 0.061) due to the fact that the mean values of the interacted variables are far from zero (Column 1 of Table 3). If one takes this coefficient as a structural estimate of a single household's tax-price elasticity of demand, it is notably smaller than estimates from previous cross-sectional studies (see Inman, 1979; Gramlich, 1977). Estimated interactions of community tax-price with measures of the distribution of tax relief are all negatively signed and statistically significant (with a p-value of 0.107 on the interaction of tax-price with the fraction of occupied residential property ineligible for an exemption). Together, the results support the hypothesis that concentration of tax relief among homeowners and the progressive nature of tax benefits led to greater local response.

A potentially important issue in the estimation of Equation 9 is the aggregation of all unoccupied residential and non-residential property. Local residents may perceive higher costs to taxing properties that differ in their mobility, the value of their productive assets, the success of their owners in influencing the opinions of local decision makers, etc. For example, there is evidence that local residents perceive greater costs to taxing industrial property than commercial

or agricultural property (Ladd, 1974, Abeyratne and Johnson, 1998). All else equal, districts that contain unoccupied or non-residential property for which these perceived costs of taxation are higher should react significantly less to the fiscal incentives provided by NYSTAR.

To test this hypothesis, I allow for separate interactions between community tax-price and five separate categories of property that is not occupied residential.[19] The first is vacant residential property, which in practice varies mostly due to the presence of seasonal or recreational property linked to natural amenities like beaches and mountains. The second is "immobile" non-residential properties, classified in my data as Vacant Land, Wild, Forested, Conservation Lands and Public Parks, and Recreation and Entertainment.[20] The third is "semi-mobile" non-residential property, classified in my data as Commercial, Agricultural, or Community Services (i.e., educational, correctional, or health facilities). The fourth is industrial property, which may be particularly mobile in response to taxation, and the fifth category consists of public utilities. While utilities may resemble industrial properties in some ways, they may be considerably less mobile and frequently negotiate agreements with local governments whereby they pay a set amount of taxes over a number of years. Unfortunately, I lack data on these agreements, and one must be cautious in interpreting the estimated interaction of tax-price with the (measured) share of property taxes paid by public utilities.

If voters perceive roughly equal costs to additional taxation on all types on property that is not owner-occupied residential, then estimates of these additional interaction terms should be statistically indistinguishable from one another. This prediction is strongly rejected by the data (Column 2 of Table 3), supporting the notion that local residents perceive different costs to

[19] As above, for each category I use its average fraction of local property value in the district from 1991-1995.
[20] Recreation and Entertainment contains both property whose value appears commercial (e.g., movie theaters) and property whose value is closely tied to land (e.g., golf courses). The character of my results is not different if this class is included in the "semi-mobile" group, along with commercial property.

raising taxes on different kinds of property. The signs and magnitudes of these estimates further suggest that perceived costs are positively related to property mobility. A district with average characteristics is estimated to have a community tax-price elasticity of -0.27 (standard error 0.02). If the share of property value that is occupied residential falls by 10% and is replaced with vacant residential property, the estimated tax-price elasticity changes to -0.36. Likewise, 10% increases in immobile or semi-mobile property are both estimated to magnify community tax-price elasticity to -0.31.[21] In contrast, replacing occupied residential property with industrial or public utility property is expected to *shrink* tax-price elasticity to 0.26, though this change is not statistically significant. Replacing linear year trends with year effects (Table 3 column 3) produces very similar results.

In Section 5, I established that the fiscal incentives created by NYSTAR had a large overall impact on school expenditures and taxation. The results presented above show that the response to NYSTAR depended on the distribution of tax relief, and suggest that homeowners, as a group, are more influential in local decisions than renters (who were ineligible for benefits), owners of vacant residential (vacation) property, or non-residential property. They also indicate that voters consider the cost of raising taxes on other properties whose value is mobile.

7. The Impact of Tax Relief for Elderly and Non-Elderly Homeowners

Increases in the share of elderly residents within a geographic area are negatively correlated with changes in school spending (Cutler et al., 1993; Poterba, 1996, and Harris et al., 2001), and studies indicate that households without school-aged children prefer significantly lower spending on public schools (see Rubinfeld 1977; Bergstrom et al., 1982; Gramlich and

[21] The impact of changing the share of immobile property is not statistically significant. However, this may be due to the fact that immobile property share is highly correlated (about 0.7) with vacant residential share. As mentioned above, these properties are most prevalent in beach and mountain vacation areas.

Rubinfeld, 1982; Lankford, 1985). Given these facts, the rise in the elderly population share from roughly 13% today to over 20% in 2050 may have a significant impact on public support for expenditure on elementary and secondary education.

However, there is evidence that elderly households may be induced to support education. Elderly households will sell their homes to younger families, and have an incentive to maintain the quality of local schools (Hilber and Mayer, 2009). Also, most states in the U.S. have programs to lower property taxes for elderly households, and Reback (2008) finds empirical evidence that these programs increase elderly support for local public school expenditure.

Whether giving fiscal incentives to elderly households has a larger impact on expenditure than giving incentives to other households is theoretically ambiguous. One reason to think that the elderly homeowners are an influential group is that they are likely to vote in local elections, even more so than non-elderly homeowners (DiPasquale and Glaeser, 1999). On the other hand, the preferences of the elderly may be relatively inframarginal. In other words, if elderly preferences, on average, are far from the median, then lowering the marginal cost of schooling for the elderly may not affect their support for increases in expenditure.

The variation in the timing of Enhanced and Basic NYSTAR exemptions creates an opportunity to examine the relative influence of elderly and non-elderly homeowners on the margin of local expenditure decisions. In particular, this variation allows for identification of the impact of tax relief for each group. The relative magnitudes of these impacts can then be compared, taking into account the relative size of each group as a fraction of local households.

I define the "group tax-price" as the aggregate amount of money a group must pay when educational spending rises by one dollar. In New York, this is just the amount of local *taxable* property owned by the group divided by total local property value. I construct measures of the

amount of taxable property owned by elderly and non-elderly homeowners, using census data and data on local property value; the Online Appendix provides a detailed explanation. Group tax-prices are lower on average for elderly homeowners (0.16) than non-elderly homeowners (0.49), due to the fact that the elderly always constitute a smaller set of local households than the non-elderly. Thus, for each additional dollar of spending, the elderly *as a group* pay less than the non-elderly. Since, I estimate the impact of a proportional reduction in group tax-price, scaling by the size of the group does not affect my results.

Equation 10 shows my regression specification with group tax-price terms for elderly (P_{jt}^{e}) and non-elderly (P_{jt}^{n}) homeowners. District fixed effects, time controls, and enrollment variables are omitted for simplicity. The three income measures are still specified at the community level because time-varying group-specific income data are unavailable and because the variation in the third income component comes mostly from lump-sum aid, making group-specific terms highly collinear.

$$(10)\quad \begin{aligned} ln(E_{jt}) = {} & \pi_1 ln(H_{jt}) + \pi_2 ln(\overline{Y}_{jt}) + \pi_3 ln\left(1 + \tfrac{(P_{jt})*Aid_{jt}}{Y_{jt}}\right) \\ & + (\delta^e + \lambda^e(I_j^e - \bar{I}^e) ln(P_{jt}^e) + (\delta^n + \lambda^n(I_j^n - \bar{I}^n)) ln(P_{jt}^n) + \varepsilon_{jt} \end{aligned}$$

Group tax-prices are included as separate covariates, and are also interacted with the fraction of district households in each group (I_j^e and I_j^n) relative to the state means (I^e and I^n). These interactions are expected to be negative (i.e., giving tax relief to a larger fraction of local taxpayers has a greater impact on spending), and allow me to compare the impact of tax relief for elderly and non-elderly homeowners, conditional on the groups being of equal size.[22]

The group tax-price coefficients are both negative and statistically significant, but the interactions of group tax-price with group size are not statistically different from zero (Column 1

of Table 4). The latter finding hinders my ability to compare group tax-price elasticities under the presumption of equal group size. This problem is illustrated in Figure 2. Estimates of group tax-price elasticity for non-elderly households are roughly the same whether non-elderly homeowners constitute 25% or 75% of local households, and the estimated tax-price elasticity for elderly homeowners is actually *smaller* when they constitute a larger fraction of local households. Given a strong prior belief that increases in group size should tend to increase group influence, it is hard to believe that the group size interactions in Equation 10 are well identified.[23]

One possible solution to this problem is to assume that the impact of changing the tax-price of a particular group is *proportional* to group size and, drop the main effects of group tax-price from the regression specification. Then one only needs to compare the coefficient estimates on the interactions of group tax-price and group size to compare the relative impacts of tax relief for elderly and non-elderly homeowners. Estimated interaction terms for elderly and non-elderly homeowners (-0.126 and -0.183, respectively) are both statistically significant, and a Wald test of the equality of the elderly and non-elderly coefficients is rejected at the 5% level (Column 2 of Table 4). If I include year fixed effects instead of trends (Column 3 Table 4), the coefficient for non-elderly homeowners remains slightly larger (-0.099) but is no longer statistically different from the effect for elderly households (-0.077). These results provide some suggestive evidence that non-elderly homeowners have equal or greater influence on the margin of public expenditure decisions as elderly homeowners.

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[22] Note that the interactions with group size are conditional on group tax-price, not community tax-price, so there is no underlying tradeoff between benefit generosity and the number of households receiving benefits.

[23] A primary reason why the relation between group size and tax-price effects may be poorly identified is that variation in the size of each group across districts is quite small relative to the difference in size between the groups. The mean fraction of households that are elderly homeowners is 0.195 with a standard deviation of 0.045 and the mean fraction of households that are non-elderly homeowners is 0.562 with a standard deviation of 0.100.

8. Conclusions

Using plausibly exogenous variation from a large tax-relief program in New York State, I find that tax-price reductions for homeowners led to a significant increase in school district expenditures and property taxes. I also find evidence that targeting fiscal incentives to homeowners (as opposed to renters or owners of second homes or non-residential property) led to larger impacts on school expenditure, and that local residents perceive greater costs to raising taxes on local property whose value is mobile, such as commercial and industrial property.

These findings have important implications for fiscal policy. Local governments and local taxpayers react to changes in fiscal incentives and constraints, and policymakers should take these behavioral responses into account when designing policies. For example, income tax deductibility of property taxes provides marginal incentives to raise local property tax levies, particularly in communities with high income levels. In addition, rather than providing uniform incentives for all local taxpayers, state policies directed at local public finance (e.g., lump-sum and matching grants, as well as school finance equalization systems) may do well to consider the relative influence of particular groups.

The program I study was aimed at reducing school district property taxes, but it may have had other effects. For example, it reduced the cost of homeownership, and may have induced some households to switch from renting to owning. Reducing school district property taxes may also have effects on other local public goods (fire protection, police, libraries, parks, etc.). These topics, while beyond the scope of my paper, are worthy of future research.

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Table 1: Summary Statistics for Analysis Sample (619 School Districts)

	Mean	Standard Deviation
Expenditure per Pupil 1997-1998	$10,530	$2,846
Operational Exp. Per Pupil 1997-1998	$9,385	$2,710
Operational State Aid per Pupil 1997-1998	$3,417	$1,470
Federal Aid per Pupil 1997-1998	$374	$247
% Property Taxes Paid by NYSTAR 1998-1999	5.7%	2.8%
% Property Taxes Paid by NYSTAR 1999-2000	10.8%	4.4%
% Property Taxes Paid by NYSTAR 2000-2001	15.5%	6.1%
% Property Taxes Paid by NYSTAR 2001-2002	20.1%	8.0%
% Households Elderly Homeowners, Census 2000	19.5%	4.5%
% Households Non-Elderly Homeowners, Census 2000	56.2%	10.1%
% Vacant Housing Units, Census 2000	13.8%	14.6%
Grant Aid/Aggregate Income 1997-1998	4.9%	3.6%
Avg. Residential Share of Property Taxes 1991-1995	64.2%	13.0%
Avg. "Immobile" Share of Property Taxes 1991-1995	7.4%	9.1%
Avg. "Semi-Mobile" Share of Property Taxes 1991-1995	17.9%	8.6%
Avg. "Industrial" Share of Property Taxes 1991-1995	3.1%	5.2%
Avg. "Public Service" Share of Property Taxes 1991-1995	9.9%	10.1%
Variation in Owner-Occupied Home Values, Census 2000	0.533	0.147
80/20 Ratio of Household Income, Census 2000	3.817	0.777
80/20 Ratio of Individual Years of Education, Census 2000	1.269	0.097

Note: Statistics for expenditure and aid are in nominal dollars. "Immobile" property consists of the classes "Vacant Land," "Wild, Forested, Conservation Lands and Public Parks," and "Recreation and Entertainment"; "Semi-Mobile" property consists of the classes "Commercial," "Agricultural," and "Community Services." A full description of property classification in New York is given in table A.2. Variation in owner-occupied home values is specified as the difference in value between the 75th percentile and the 25th percentile divided by the median. The 80/20 ratio in years of education is measured for adults 25 years and older.

Table 2: Impact of NYSTAR on Educational Expenditure

	(1)	(2)
Log Community Tax-Price	**-0.220**	**-0.171**
	(0.021)**	**(0.019)****
Log Mean Income	0.012	0.020
	(0.012)	(0.012)
Log Tax Returns	0.190	0.199
	(0.034)**	(0.034)**
Log (1+(1-P)*Aid/Y)[a]	0.880	
	(0.216)**	
Log (1+Aid/Y)		1.292
		(0.182)**
Log Enrollment	0.205	0.207
	(0.038)**	(0.036)**
Change in Log Enrollment	-0.125	-0.123
	(0.032)**	(0.031)**
Observations	4333	4333
R-squared	0.9989	0.9989

Note: The dependent variable in all regressions is the natural log of operational expenditure and the unit of observation is at the school district - year level. All regressions include school district fixed effect, county trends, and a trend for city school districts. Standard errors (in parentheses) are clustered by school district. * significant at 5%; ** significant at 1%. [a]The term P refers to the community tax price, Aid is lump-sum aid received by the school district, and Y is aggregate school district income.

Table 3: Local Response and the Distribution of Tax Relief

	(1)	(2)	(3)
Log Community Tax Price	-0.033	-0.032	-0.011
	(0.061)	(0.061)	(0.070)
Log Community Tax-Price Interacted with:			
Property Share Not Occupied-Residential	-0.163		
	(0.094)+		
Fraction Occupied Residential Ineligible for Exemption	-0.126	-0.153	-0.163
	(0.078)	(0.081)+	(0.110)
Variation in Owner-Occupied Home Values[a]	-0.218	-0.112	-0.050
	(0.090)*	(0.098)	(0.107)
Vacant Residential Property Share		-0.857	-0.886
		(0.261)**	(0.294)**
"Immobile" Property Share		-0.357	-0.320
		(0.261)	(0.289)
"Semi-mobile" Property Share		-0.340	-0.338
		(0.154)*	(0.169)*
Industrial Property Share		0.154	0.172
		(0.200)	(0.218)
Public Utility Property Share		0.068	0.027
		(0.131)	(0.149)
Log Mean Income	0.012	0.012	0.041
	(0.012)	(0.012)	(0.019)*
Log Tax Returns	0.185	0.166	0.197
	(0.034)**	(0.035)**	(0.039)**
Log (1+(1-P)*Aid/Y)[b]	0.941	0.955	0.995
	(0.221)**	(0.220)**	(0.325)**
Log Enrollment	0.218	0.225	0.228
	(0.038)**	(0.039)**	(0.042)**
Change in Log Enrollment	-0.136	-0.136	-0.141
	(0.031)**	(0.031)**	(0.033)**
County Trends, City Trend	Yes	Yes	No
County*Year Effects, City*Year Effects	No	No	Yes
Observations	4333	4333	4333
R-squared	0.9988	0.9988	0.9988

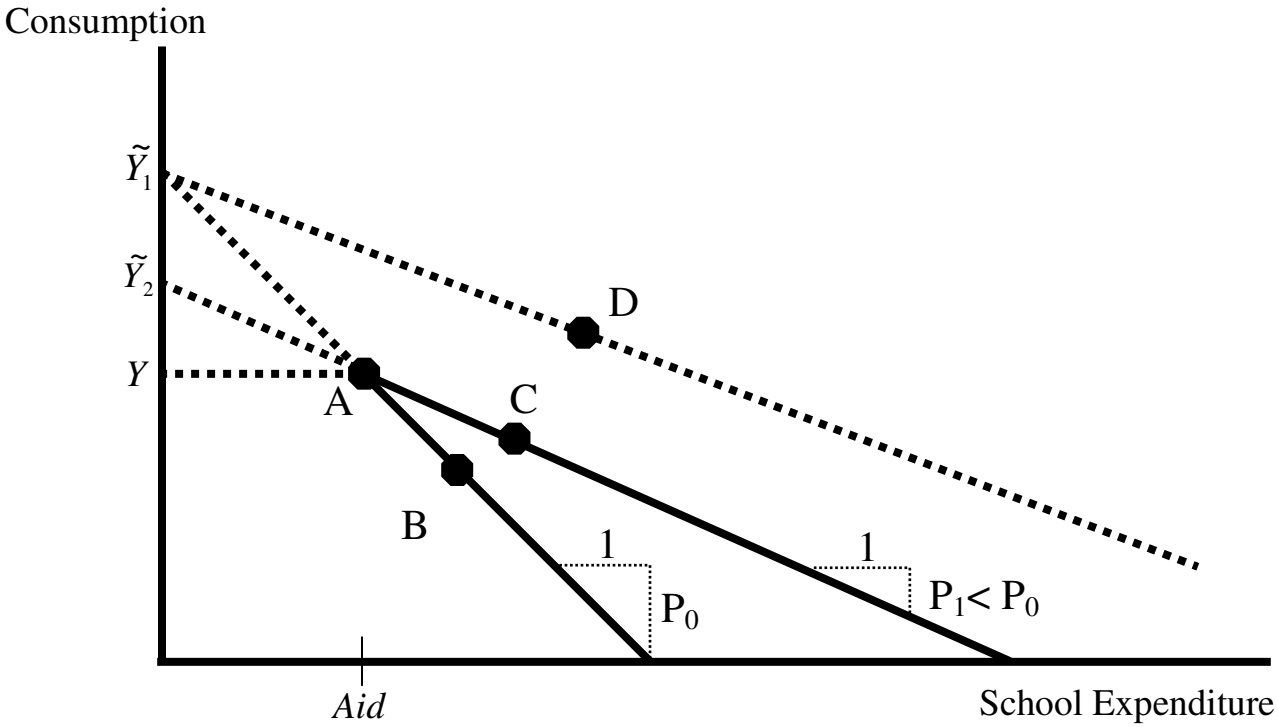
Note: The dependent variable in all regressions is the natural log of operational expenses and the unit of observation is at the school district - year level. All regressions include school district fixed effects. Standard errors (in parentheses) are clustered by school district. + significant at 10%; * significant at 5%; ** significant at 1%. [a]Variation in owner-occupied home values is specificied as the difference between the 75th and 25th percentile home values divided by the median home value. [b]The term P refers to the community tax price, Aid is lump-sum aid received by the school district, and Y is aggregate school district income.

Table 4: Relative Effects of Tax Relief for Elderly and Non-Elderly Homeowners

	(1)	(2)	(3)
Log of Group Tax-Price for			
Elderly Homeowners	-0.030		
	(0.004)**		
Non-Elderly Homeowners	-0.113		
	(0.012)**		
Interaction of Log Group Tax-Price with Group Size:			
Elderly Homeowners	0.063	-0.126	-0.077
	(0.073)	(0.018)**	(0.041)+
Non-Elderly Homeowners	-0.024	-0.183	-0.099
	(0.066)	(0.021)**	(0.055)+
Log Mean Income	0.026	0.018	0.029
	(0.014)	(0.014)	(0.018)
Log Tax Returns	0.167	0.169	0.181
	(0.035)**	(0.036)**	(0.040)**
Log (1+(1-P)*Aid/Y)[a]	0.902	0.836	0.684
	(0.247)**	(0.249)**	(0.331)*
Log Enrollment	0.231	0.230	0.226
	(0.039)**	(0.039)**	(0.042)**
Change in Log Enrollment	-0.149	-0.143	-0.148
	(0.032)**	(0.032)**	(0.034)**
County Trends, City Trend	Yes	Yes	No
County*Year Effects, City*Year Effects	No	No	Yes
Observations	4333	4333	4333
R-squared	0.9988	0.9988	0.9988

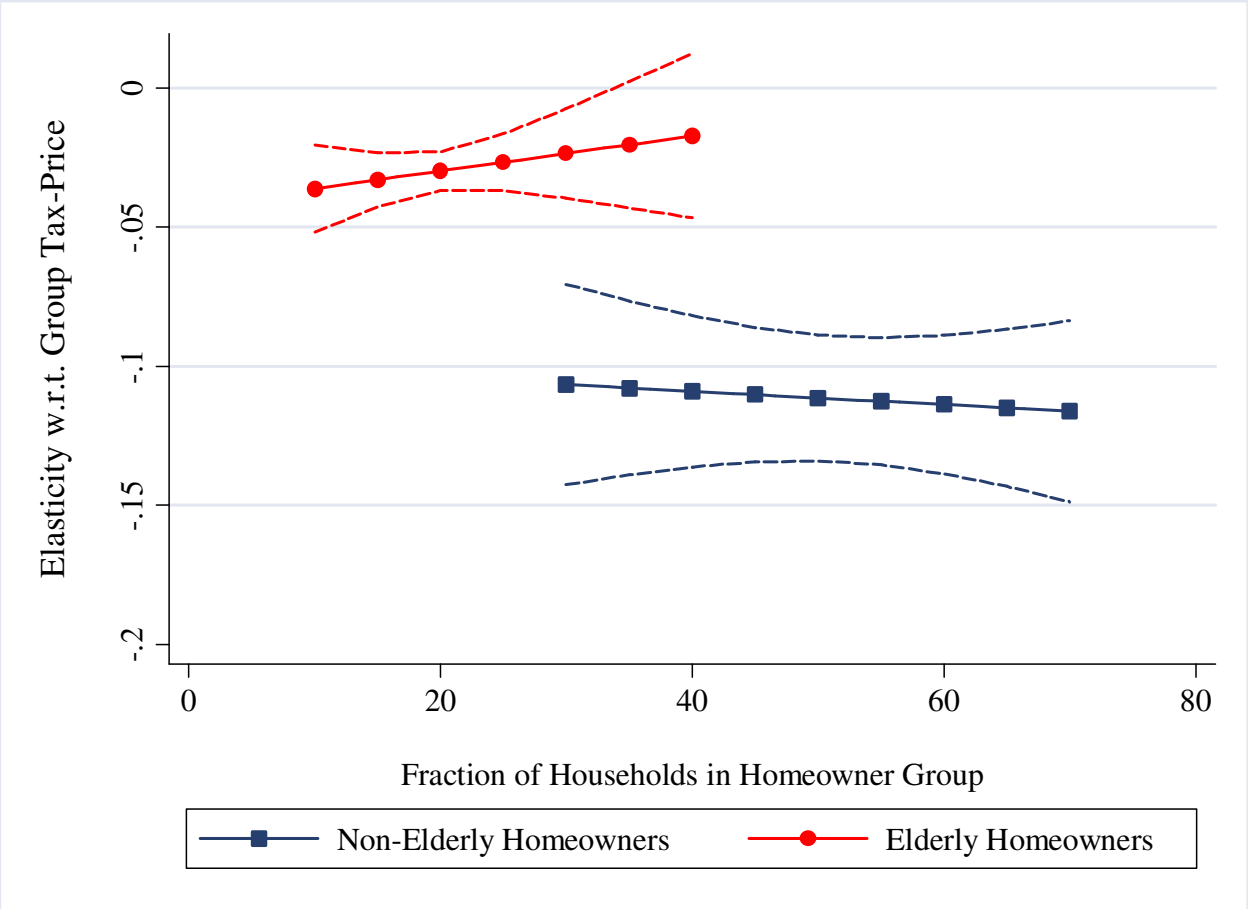
Note: The dependent variable in all regressions is the natural log of operational expenditure and the unit of observation is at the school district - year level. All regressions include school district fixed effects. Standard errors (in parentheses) are clustered by school district. + significant at 10%; * significant at 5%; ** significant at 1%. [a]The term P refers to the community tax price, Aid is lump-sum aid received by the school district, and Y is aggregate school district income.

Figure 1: The Community Budget Constraint, Tax-Price, and Virtual Income



Notes: Y is community income and *Aid* is a lump-sum grant received from outside sources. The point A represents a choice to levy no property taxes. If the community chooses to levy taxes, that will reduce consumption, and increase educational expenditure. Such a choice is represented by point B. P_0 is the original community tax-price (i.e., the loss in consumption associated with a unit increase in school expenditure) and $\tilde{Y}_1$ is the original virtual income level. A fall in community tax-price from P_0 to P_1 shifts the budget constraint and decreases virtual income to $\tilde{Y}_2$. This may push the community to choose point C, which can be interpreted as a pure price effect (from B to D) and a virtual income effect (from D to C).

Figure 2: Estimated Group Tax-Price Elasticities of Expenditure



Note: The solid line shows estimated elasticities at various levels of population for each group; Dotted lines represent 95% confidence intervals.