Local Response to Fiscal Incentives in Heterogeneous Communities

Jonah E. Rockoff*
Columbia Business School and NBER

January, 2010

Abstract

I examine how a property tax relief program in New York State affected local educational spending. This program, which lowered the marginal cost of school expenditure to homeowners, had statistically and economically significant effects on local government behavior. 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. 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.

* E-mail: jonah.rockoff@columbia.edu. I would like to thank Patrick Bayer, David Cutler, Caroline Hoxby, Marty Feldstein, Larry Katz, Chris Mayer, Randall Reback, and seminar participants at Harvard University, Columbia Business School, Dartmouth College, the Federal Reserve Bank of New York, Federal Reserve Board of Governors, University of Virginia, Rochester University, Wharton, and the NBER Summer Institute for extremely helpful advice and comments on this work. Patrice Cohen, Jack Conroy, Steve Fountain, Bill Heidelmark, Bruce Jesiolowski, and others in the New York State government generously provided much of the data for this project. This work was supported by the Taubman Center for State and Local Government at the Kennedy School of Government and the Earle A. Chiles Foundation.

One of the most important functions of local government is to provide public education, but preferences for school expenditure vary among local voters. Families with children benefit from school quality, but other households pay most of the costs. Residential property owners may benefit through capitalization of school quality, but non-residential property is generally taxed at the same rate. I examine how school expenditure is 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 spending on local schools for homeowners by paying a percentage of their school district property taxes. It gave greater reductions to homeowners with less expensive homes, and differential relief to elderly and non-elderly homeowners. 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 the fiscal incentives faced by school districts.

I find these fiscal incentives 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%. I also find that 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 result 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. I also find evidence that local residents perceive significant costs to raising taxes on commercial and industrial property whose value is potentially mobile.

Finally, I use the difference in the timing of benefits for elderly and non-elderly homeowners to estimate the relative impact of tax relief for these groups on expenditure. I am unable to discern any significant difference in the effect on district spending of lowering the marginal cost of schools for these two groups. However, the lack of large variation in the proportion of households who are elderly and the endogeneity of existing variation limits the conclusions that can be drawn from this finding.

Details on the tax relief program and my data are given in Sections 2 and 3. Section 4 presents methods and main results, with extensions in Sections 5 and 6. Section 7 concludes.

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

School districts in New York State are fiscally independent from other local governments and special agencies (e.g., town, county, fire and water districts). Unlike a number of states, New York does not have a school finance equalization scheme that redistributes local revenue in an effort to equalize spending across districts. Lump-sum grants given by the state to school districts are distributed progressively based on district property value and income, but all funds are raised via statewide income and sales taxes and lottery revenues.

School district budgets in New York must be approved in annual referenda. 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; in the five years prior to the start of the tax relief program I study, 23% of budget referenda did not pass.[1]

Once a budget is approved, the local tax burden is divided among local property owners in proportion to value; this apportionment is described by Equation 1, where "*local revenue*"

represents the difference between the total budget and outside aid.[2] Setting a budget level is equivalent to setting a tax rate: dividing both sides of Equation 1 by *property value*$_i$, shows that the tax rate equals local revenue over total property value. The marginal cost of school expenditure to a property owner is the "tax-price" of public spending. Taking the derivative of the right hand side of Equation 1 with respect to the *local revenue* term, one can see that a property owner's tax-price is equal to the fraction of local taxable property he/she owns.

$$(1)\ property\,tax_i = local\,revenue * \frac{property\,value_i}{\sum_i property\,value_i}$$

New York's School Tax Relief Program (NYSTAR), created in 1997, exempts a fixed amount from the taxable value of owner-occupied homes from taxation by school districts. However, the property taxes forgone by the district are reimbursed by the state. The impact of NYSTAR on homeowner's property taxes is shown by Equation 2, where X_i is the value of the home exempt from taxation. Holding local revenue constant, local property taxes fall. Moreover, the reduction in taxes will greater for homes with lower value.

$$(2)\ \ property\,tax_i = local\,revenue * \frac{property\,value_i - X_i}{\sum_i property\,value_i}$$

NYSTAR also reduces the tax-price of school expenditure for local homeowners; the marginal cost of local revenue is lower because the state shoulders part of the burden.[3] Lower tax-prices provide an incentive to increase school spending, which I discuss in greater detail in Section 3.

---

[1] In the school year 2003-2004 (the first year in which data are available) the median turnout rate for these budget referenda was roughly 14%. The median county level voter turnout for general statewide elections in 1999—a year without presidential, gubernatorial, or congressional campaigns—was about 37%. For more on these referenda, see Ehrenberg et al. (2004).

[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] More broadly, NYSTAR can be viewed as a "homestead exemption," a common type of property tax relief used in nearly every state in the U.S. However, unlike NYSTAR, homestead exemptions are often only for particular groups (e.g., elderly or disabled persons), given at the option of the local community, and not state funded (see Mikhailov (1998) and Sexton (2003)). Without state funding, exemptions merely shift tax burden from property owners receiving exemptions to other owners of local property.

There are two types of exemptions: Enhanced NYSTAR, for homeowners aged 65 or older whose incomes were below $60,000, and Basic NYSTAR, for all other homeowners.[4] By law, school districts must notify owners of local residential property about NYSTAR, but homeowners must submit a one-page application to their local tax assessor in order to get an exemption. Reapplication is necessary for Enhanced NYSTAR, due to income restrictions, and tax assessors must mail annual application reminders. The potential issue of endogenous take-up is addressed in Section 4.4. The program also requires that school budget information distributed prior to referenda include information on the effect of NYSTAR on property tax bills.

NYSTAR exemptions were phased in over time, as shown in Figure 1. Enhanced NYSTAR became available in the school year 1998-1999 and exempted $50,000 of home value. Basic NYSTAR exemptions were phased in, starting at $10,000 in the school year 1999-2000 and increasing in $10,000 increments until reaching $30,000 in the school year 2001-2002. In eight counties with the highest home values, 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.[5] For example, in Westchester County—the most expensive in the state—Basic and Enhanced NYSTAR exemptions in 2001-2002 were $70,386 and $117,310, respectively.

As mentioned above, property tax reductions from NYSTAR are greater for smaller value homes within a district. Variation in tax reductions across the state can be gauged from the median owner-occupied home in each school district reported in the 2000 Census. Setting aside the eight counties with higher exemption levels, 15% of school districts had a median owner-occupied home worth $60,000 or less, so by 2001-2002 at least half of all homeowners in those districts were eligible for at least a 50% reduction in tax-price. 72% of districts had a median

[4] Details on the income rules for Enhanced NYSTAR are discussed in the Online Appendix.
[5] These counties are: Dutchess, Nassau, Orange, Putnam, Rockland, Suffolk, Sullivan, and Westchester.

value of $90,000 or less (implying at least a 33% reduction in tax-price), and 92% of districts had a median value of $120,000 or less (at least a 25% reduction). In the eight counties with higher exemption levels, NYSTAR exemptions were still substantial. For instance, 25% of districts in Westchester County 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 State's system of public school finance. Payments to reimburse districts for NYSTAR exemptions rose from about $500 million in 1998-1999 to about $2 billion in 2001-2002. The payments constituted a significant part of districts' revenue, rising from roughly 3% in 1998-1999 to over 9% in 2001-2002, and the overall share of school district revenue coming from the state rose from 40% in 1997-1998 to 51% in 2001-2002.

The NYSTAR program was proposed by then Governor George Pataki as a means to reduce property taxes. Nevertheless, the incentives created by NYSTAR were well perceived by economists (see Duncombe and Yinger (1998)) and were a part of the discussion surrounding the legislation. In an effort to prevent the crowding out of tax reductions via increased expenditure, Pataki's original proposal contained a cap on annual increases in school budgets, but this provision was removed by the state legislature.

3. Data

My analysis is based on demographic and financial data on school districts in New York State from the school years 1995-1996 through 2001-2002. Data by school district on the number and type of NYSTAR exemptions, the dollar amounts of 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. School 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 household demographics, including homeownership and the value of owner-occupied homes, comes from the 1990 and 2000 Census School District Demographics files and the 1990 and 2000 Integrated Public Use Microdata Series (IPUMS) samples for New York State.

I use a balanced panel of 619 school districts, which vary substantially in size, demographic composition, and other characteristics. Excluded from my analysis are the five largest cities (whose school districts are not fiscally independent from city governments), districts that serve only elementary students, only high school students, or only disabled students, and districts that merged during this time period.

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 in my sample was $10,530 (the national average was around $7,200), and about 90% of this amount was operational expenditure. The remaining 10% is capital expenditure (e.g., debt service payments for school construction and purchases of school buses). 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 often 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 exclude capital expenses from my analysis and focus on operations, though I 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. Empirical Analysis

Research on impact of fiscal incentives on local public expenditure is longstanding (e.g., Bergstrom and Goodman (1973), Feldstein (1975)), but few studies examine exogenous variation in fiscal incentives, and fewer measure the relative influence of groups within communities.[8] The paper closest in spirit to my work is Anderson (2006), who uses a tax policy change in Minnesota to measure the relative influence of voting and non-voting property owners on local governmental expenditure. In addition, Eom et al. (2005) examine the same New York State program that I analyze here, but focus on whether spending leads to higher student achievement.

Studies of fiscal incentives are largely based on the idea that local government decisions are based on the preferences of the median voter (Black (1948), Inman (1979)).[9] Since the median voter's identity is unknown, researchers assume a relation between preferences and observable characteristics, e.g., the voter with median preferences is the voter with median

[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.
[8] Two other groups of studies use different approaches to measure how heterogeneous preferences affect local spending. The first examines variation in the composition of local residents (e.g., Cutler et al. (1993), Poterba (1996), and Harris et al. (2001), Hilber and Mayer (2009), and Reback (2008)). The second examines 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)).
[9] I use the median voter framework as a starting point in structuring my analysis, but 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. 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, in this case 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.

income (Bergstrom and Goodman (1973)).

The fiscal incentives created by NYSTAR vary among households within a school district, depending on homeownership, age, income, and property value. Because a central assumption in the median voter framework is that the identity of the median voter does not change in response to fiscal policy, this framework is not appropriate.[10] Nevertheless, policies such as NYSTAR can be useful for learning about the impact of fiscal incentives and the relative influence of different local groups. My empirical framework treats the community as a single decision maker facing a single budget constraint, but it is the budget constraint of the community as a whole, not the budget constraint of a presumed median voter.[11] I later adjust this framework slightly to compare the impacts of fiscal incentives for elderly and non-elderly homeowners.

4.1 Public Choice and the Community Budget Constraint

In my theoretical framework, the community acts as a single decision maker who faces a trade-off between spending on public schools (E) and aggregate consumption of all other goods (C). The community's budget constraint (shown in Figure 2a) is determined by aggregate community income (Y), lump-sum aid (Aid), and the community tax-price (P). A community has the option to spend only lump-sum aid on public education (depicted by point "A"), or to increase spending on schools by collecting property taxes (with one such choice depicted by point "B"). For each additional dollar of spending on public schools, the district reduces

[10] 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.

[11] 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.

consumption of other goods by *P* dollars.

NYSTAR exemptions shift the budget constraint outward by lowering the community tax-price (Figure 2b). As the community tax-price falls, the budget line rotates around point A, increasing the amount spent on public schools for any given (positive) level of property taxes. The district is now able to increase public school spending *and* lower property taxes (point C).

Point A is crucial for determining the impact of change in tax-price. To clarify this point, I extend the original community budget line back to the vertical axis, where it crosses at the point $\widetilde{Y}_1$. A community with aggregate income of $\widetilde{Y}_1$ and no lump-sum aid that chooses point B would have the same aggregate spending on public schools and aggregate consumption as the original district. However, an equivalent change in tax-price for the community with real income $\widetilde{Y}_1$ creates a much larger shift in its budget constraint, allowing it to choose a combination of consumption and school expenditure such as point D.

The interaction of lump-sum aid and matching grants can be accounted for by specifying 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 a phrase from the labor supply literature, I refer to this point as "virtual income" (Burtless and Hausman (1978)). Figure 2b shows that a shift in the budget constraint due to a fall in tax-price leads to a fall in virtual income, i.e., from $\widetilde{Y}_1$ to $\widetilde{Y}_2$. The total effect of lowering tax-prices consists of a price effect (point B to point D) and virtual income effect (point D to point C). This notion is summarized by Equation 3.

$$(3)\ \Delta E = \frac{\partial E}{\partial P}\Delta P + \frac{\partial E}{\partial Y}\Delta \widetilde{Y} \qquad \frac{\partial E}{\partial P} < 0\ ,\ \frac{\partial E}{\partial Y} > 0$$

Price and virtual income effects are of opposite signs, so the change in expenditure is smaller than the price effect for districts receiving lump-sum aid. Estimates of price effects that fail to control for virtual income will be biased towards zero.

The potential importance of this interaction is demonstrated in Figure 3. Take a district that receives neither lump-sum nor matching grants and optimizes by choosing point A. This district would choose point B (a corner solution with no property taxes) if given a very large lump-sum grant, and would choose point C if given a very large decrease in community tax-price. Each grant by itself causes a similarly large increase in educational expenditure. However, the addition of a second grant (lump-sum added to matching, or vice versa) would move the district to D and have only a small impact on expenditure. The key to this result is virtual income, which falls precipitously in a move from B to D (matching added to lump-sum) and rises only slightly in a move from C to D (lump-sum added to matching).[12]

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. Yet the issue of virtual income comes into play in any public finance system that involves both lump-sum grants and changes in tax-prices. Furthermore, many school finance equalization systems—which vary tax-prices across districts—will be affected by this issue because of lump-sum federal grants to schools under Title I.

4.2 Empirical Specification for Main Estimates

[12] There are two additional income effects through which NYSTAR could affect school expenditure but are not included in this framework. First, funding the NYSTAR program with state taxes or a decrease in state expenditures might decrease 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. In addition, both would be extremely difficult to incorporate into an empirical analysis without information on voters' expectations regarding future fiscal policy decisions by the state. Finally, several factors suggest that their omission does not pose a problem. 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 decrease in local expenditure due to the cost of funding NYSTAR must come through expected future changes in policy, not real changes seen during this period. Moreover, 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 lead to increased spending though an income effect, it would work to undo the benefits of NYSTAR by making owner occupied properties relatively more valuable and thus making homeowners responsible for a greater share of local taxes. In other words, to the extent that capitalization occurs, it will bias the effects I measure towards zero.

I follow previous researchers (e.g., Feldstein (1975), Gramlich and Rubinfeld (1982)) and specify the demand for local public expenditure as a log linear equation (Equation 4).

$$(4)\ \ln E_{jt} = \pi \ln \widetilde{Y}_{jt} + \delta \ln P_{jt} + \beta Z_{jt} + \varepsilon_{jt}$$

Public school expenditure (E_{jt}) in district j during year t is a function of virtual income ($\widetilde{Y}_{jt}$), community tax-price (P_{jt}), district specific characteristics (Z_{jt}) and unobservable factors (ε_{jt}). The community tax price reflects what the community must pay in property taxes to spend an additional dollar on education. For my analysis, community tax-price (P_{jt}) is equal to one minus the fraction of local property taxes paid by NYSTAR (1-*nystar%*$_{jt}$).[13]

A well known empirical regularity in public economics is the "flypaper effect," i.e., an additional dollar of lump-sum aid tends to increase public expenditure by a larger amount than an additional dollar of income (Thaler (1995)). I incorporate this into my analysis by splitting virtual income into components that separately capture the impact of aggregate income and the impact of variation in lump-sum aid (Equation 5).[14]

$$(5)\ \ln\left(\widetilde{Y}_{jt}\right) = \ln\left(Y_{jt}\right) + \ln\left(1 + \frac{\left(1 - nystar\%_{jt}\right) * Aid_{jt}}{Y_{jt}}\right)$$

Note that the impacts of grants and aggregate income on expenditure are still closely linked. When aggregate income increases, the first term on the right hand side of Equation 5 rises and the second term falls. When lump-sum aid increases, the degree to which the second term rises is mediated by the ratio of lump-sum aid to income. Compared with a district that relies almost

[13] Setting the community tax-price to one prior to NYSTAR will not account for other factors that may cause the marginal cost of spending to vary across districts, such as property tax deductibility. Nevertheless, changes in the log of community tax price due to NYSTAR will be estimated correctly, and the inclusion of district fixed effects will account for preexisting differences in factors such as greater proportions of households who deduct property taxes and the greater value of these deductions in high income areas. So, to the extent that factors such as frequency and value of deductibility do not vary significantly over time within districts, they should not bias my results.

[14] To see how Equation 5 is derived, note that virtual income ($\widetilde{Y}_{jt}$) is equal to the sum of actual income (Y_{jt}) and the product of state aid (Aid_{jt}) and the district's tax price (1-*nystar%*$_{jt}$). After factoring actual income from both terms, one can separate virtual income into the two natural logs shown in Equation 5.

exclusively on income to fund its schools, a district that relies heavily on grants should increase spending by a greater proportion when lump-sum aid rises by a given proportion.

In addition to data on aggregate income, I possess data on the number of income tax returns filed in each district and each year. Information on the total number of tax returns is likely to capture variation over time in districts' adult population and employment, while the average taxable income will reflect growth in earnings conditional on employment. No annual data on school district population exist (only student enrollment), so this is a potentially important control. I therefore separately estimate coefficients for the natural log of aggregate income into the log of taxpayers and the log of mean taxable income.

Equation 6 incorporates these more flexible specifications of income effects.

$$(6) \quad ln(E_{jt}) = \alpha + \pi_1 ln(taxpayers_{jt}) + \pi_2 ln(\overline{Y}_{jt}) + \pi_3 \left(1 + \frac{(1 - nystar\%_{jt} * Aid_{jt}}{Y_{jt}}\right) + \delta ln(1 - nystar\%_{jt}) + \beta Z_{jt} + \varepsilon_{jt}$$

Note that 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 tax-price and the ratio of aid to income.

Of course, other factors affect the demand for school expenditure. Some are hard to measure but relatively stable over time (e.g., general preferences for education spending, strength of teachers' unions), and can be captured by district fixed effects. One important time-varying characteristic I observe is the number of students enrolled. I control for both the log of student enrollment and the change in log enrollment from the previous year. This allows temporary

fluctuations 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.

Finally, much of the variation in the incidence of NYSTAR occurred over time and will be correlated with any factors that led to secular increases in expenditure. I therefore include a linear year trend for each county and an additional trend for city school districts, and examine other time controls in robustness checks.

4.3 The Average Impact of NYSTAR on Public School Expenditure

Results from a least squares regression of Equation 6 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. 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 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 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 changes 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%.[15] All of the additional revenue for increased expenditure must be generated by additional property taxation. 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 a flypaper effect of state aid. 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%. If we assume the district has community tax price equal to one and the average ratio of aid to income (0.05), I estimate that a 1% increase in grant aid would lead to a 0.04% increase in expenditure ($0.88*\ln(1.0505/1.05) \approx 0.0004$), which implies that 10 cents of each additional grant dollar is spent on schools. In contrast, the propensity to spend out of income is estimated to be *at most* 2.4 cents on the dollar.[16]

To document the effect of virtual income, I estimate a specification that does not adjust for the interaction between lump-sum aid and tax-price (Table 2, Column 2).[17] As expected, the estimated tax-price coefficient shrinks towards zero, from -0.220 to -0.171. Note that there is a high correlation (about 0.6) between *nystar%* and the ratio of lump-sum aid to aggregate income—districts with low property values tend to be more dependent on state aid—which limits the amount of bias in this setting.

[15] The tax price (*1-nystar%*) 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 $-0.22*(\ln(0.9)-\ln(1)) = 0.023$.

[16] 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_I, 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.

A typical school district, which received 20% of its revenue through NYSTAR in the school year 2001-2002, is estimated to have raised operational expenditure by 4.1% and local property taxes by 6.8% in response to fiscal incentives.[18] This suggests very little crowd out of property tax relief for homeowners with inexpensive homes, but a substantial loss in tax relief for owners of expensive homes (e.g., 40% of the benefits for a $200,000 home), and a considerable increase in taxes for owners of non-residential property, second homes, or rental property.

4.4 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, using predicted values of *nystar%* as instruments for community tax-price and the component of virtual income that contains *nystar%*.[19] The estimated impact of NYSTAR in the 2SLS regression (-0.230) is nearly identical to OLS (-0.22), with a standard error of 0.023.

Another potential concern is that time-varying unobservable factors correlated with the incidence of NYSTAR may not be accurately captured by county and city specific linear trends. If educational expenditure was growing for other reasons in a non-linear fashion that correlated with *nystar%*, then my estimate will be biased upward. I therefore estimate a number of specifications that include alternate sets of time controls. These include: (1) a linear time trend and an interaction of the linear trend with predicted values of *nystar%* in the year after the program was fully implemented (2) a control for the average per-pupil instructional spending of

---

[17] Specifically, I replace $\ln\left(1+\frac{(1-nystar\%_{jt})*Aid_{jt}}{Y_{jt}}\right)$ with $\ln\left(1+\frac{Aid_{jt}}{Y_{jt}}\right)$.

[18] Recall, this calculation is based on a ratio of state aid to income of 0.05 and 40% of revenue from state aid.

[19] Predicted values of *nystar%* equal the number of households eligible for exemptions (based on the 2000 census) multiplied by the statutory value of their exemptions, divided by the total property value in the school district. A detailed explanation of how I calculate the number of elderly households eligible for Enhanced NYSTAR benefits is included in the Online Appendix.

states that, like New York, do not redistribute local revenue across districts as part of their school finance equalization (3) a control for the average per-pupil instructional spending in neighboring states and (4) county-year fixed effects and a separate set of year effects for city school districts.[20] The coefficients on tax-price from these estimates range from -0.144 to -0.262, and in all cases are highly significant.[21]

5. Variation in Local Response and the Distribution of Tax Relief

As explained in Section 2, the benefits of NYSTAR were not distributed equally among local voters and property owners. Variation in the incidence of tax relief within communities provides an opportunity to examine how the preferences of different groups of taxpayers influence expenditure on local schools. If particular groups of taxpayers have greater influence on the decision to change local spending, then fiscal incentives targeted to those groups may lead to greater increases in expenditure.

Homeowners' incentives to take an interest in the quality of local public services are well known and have been documented empirically (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 homeowners are also far more

[20] These data come from the National Public Education Financial Survey, collected by the National Center for Educational Statistics. Spending figures are adjusted for inflation using the appropriate regional CPI for all urban consumers. Operational expenditure is not available so I use instructional expenditure as a proxy. Other states with no school finance equalization systems are Delaware, Mississippi, Nevada, North Dakota, and North Carolina (American Education Finance Association (1999), Hoxby (2001)). Neighboring states are Connecticut, New Jersey and Pennsylvania.

[21] The smallest estimate (-0.14) comes in a specification with county-year and city district-year fixed effects. However, it is important to point out that all of the truly exogenous variation in community tax-price occurs within districts over time. The variation in tax price due to NYSTAR that occurs within years is due to differences in districts' property values, tax-base composition, and demographics. Thus, estimates that include controls for county-year fixed effects may well be inferior to those with linear trends.

likely to vote in local elections than renters (DiPasquale and Glaeser (1999)).[22] Yet it is still theoretically unclear that changes in fiscal incentives for homeowners have a larger impact on spending decisions than for other groups. Renters might vote as frequently as homeowners if the benefits of doing so greatly increased, and low voter turnout for school budget referenda means that even a small, highly-motivated group could have an impact on the school budget.

Importantly, local voters might perceive significant costs to taxation of properties they do not own; the increased tax burden on other properties may lead the tax-base to shrink, shifting taxes onto them in the long-run. A number of studies document a negative relation between tax rates and firm location (see Wasylenko (1980), Fox (1981), Charney (1983), and McGuire (1985)) and tax rates and economic growth (e.g., Dye et al. (2001)). Increases in property taxation might also lead to higher prices for local consumers or lower wages and employment for local workers. Furthermore, property owners who do not live locally may still be able to influence opinions of local voters or government officials through means other than voting.

Finally, there is likely to be important variation in preferences for local spending among homeowners within the same community. As discussed in Section 2, NYSTAR exemptions are progressive within districts because tax-prices fall by a greater amount for homeowners with less expensive homes. If preferred school expenditure levels and home value were positively correlated within communities, then the progressivity of NYSTAR will tend to mitigate differences across homeowners by giving stronger incentives to those with lower spending preferences. Similarly, 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

[22] 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

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 ultimately an empirical question.

5.1 Empirical Strategy

My empirical strategy is based on measuring whether responses to NYSTAR depend systematically on the distribution of tax relief among local taxpayers and residents. I begin by noting that the fraction of total property taxes paid by the state through NYSTAR (*nystar%*) is equal to the product of three factors (shown in Equation 7): 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%*).

$$(7)\quad nystar\%_{jt} = resocc\%_{jt} * eligible\%_{jt} * exempt\%_{jt}$$

Holding constant the fraction of taxes paid by the state (*nystar%*), variation across districts in the residential share (*resocc%*) or the fraction of residential property that is eligible for exemptions (*eligible%*) must be balanced by variation in the fraction of eligible housing exempt from taxation (*exempt%*).[23] Homeowners receiving exemptions in districts with high shares of non-residential property, high shares of renters, etc. may have stronger incentives to increase spending (conditional on *nystar%*) since the share of their property exempt from taxation is greater. If homeowners have greater influence on the margin of local public decisions

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.

[23] To illustrate, imagine community A, where all property value is made up of identical owner-occupied residences worth $100,000, and community B, where half the property value is made up of identical owner-occupied residences worth $50,000, and the rest consists of unoccupied residential property or non-residential property. If all owner-occupied residences receive a $30,000 NYSTAR exemption in both communities, the level of *nystar%* in both communities will be equal to 30%. However, *resocc%* will be higher in community A and *exempt%* will be higher in community B.

than these other groups, then districts where tax relief is concentrated among a small group of homeowners may see larger increases in local expenditure, even though a large share of local (renter) households or local property owners receive no tax relief.

The second source of variation in tax relief within each community is the generosity of benefits among homeowners receiving exemptions. Variation in tax relief is driven directly by variation in the value of owner-occupied homes; less expensive homes receive greater tax-price reductions. As mentioned above, whether this type of progressive tax relief would lead to greater increases in expenditure is theoretically ambiguous.

In order to examine how the distribution of tax relief affects local response, I start with the specification shown in Equation 6 and add interactions between community tax-price and measures of distribution. Equation 8 shows this empirical specification, where district fixed effects, enrollment, and time controls are omitted for simplicity.

$$(9)\ \ln(E_{jt}) = \pi\left[\ln(taxpayers_{jt}), \ln(\overline{Y}_{jt}), \ln\left(1+\frac{(1-nystar\%_{jt})*Aid_{jt}}{Y_{jt}}\right)\right] + \delta W_j \ln(1-nystar\%_{jt}) + \varepsilon_{jt}$$

The matrix W_j includes three interaction terms: the fraction of district property that is *not* occupied residential (*1-resocc%*), the fraction of occupied residential property that is *not* eligible for an exemption (*1-eligible%*), and the variation in the value of owner-occupied homes within the district (σ^{value}).[24] By including the interaction terms in this way, the main effect of community tax-price can be interpreted as the elasticity of expenditure in a (hypothetical) district where all property consists of identically valued homes, all of which receive an exemption. In

[24] The fraction of district property that is not occupied residential is measured as 1 minus the product of the average non-residential property value share in each district from 1991-1995 and the occupancy rate from the 2000 census. Using property value shares prior to 1996 helps avoid concerns regarding reverse causality. The fraction of occupied residential property that is not eligible for an exemption is measured as 1 minus the fraction of eligible households (used in the instrumental variables analysis in Section 4.4). I find very similar results approximating eligibility using only the homeownership rate in the 2000 census. The non-residential property value share and the variation in the value of owner-occupied homes 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.

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. Since all owner-occupied homes were eligible for exemptions after 1999, most of the variation in *1-eligible%* is due to the fraction of local households who rent.

There are many ways in which one could measure variation in the value of owner-occupied homes within school districts. I use the difference between the 75th and 25th percentile (i.e., the inter-quartile range) home value divided by the median home value, using self-reported value from the 2000 census. I find similar results using the coefficient of variation, which can be approximated using tabulations of homes across value categories.

5.2 Estimates of Variation in Response to Fiscal Incentives

Estimates of Equation 8 are displayed in Column 1 of Table 3. Estimated interactions of community tax-price with measures of the distribution of tax relief are all negatively signed and (marginally) statistically significant.[25] These results provide significant evidence that concentration of tax relief among homeowners and the progressive nature of tax benefits led to greater local response.

Although the estimated baseline elasticity for a hypothetical "uniform" district is fairly low, response to fiscal incentives was much greater in a typical school district. Baseline community tax-price elasticity (i.e., the main effect) is -0.033 with a standard error of 0.061. Given that the mean values of the interacted variables are quite far from zero, it is not surprising that the coefficient is not tightly estimated. Nevertheless, if one takes this coefficient as an estimate of households' tax-price elasticity of demand, it is notably smaller than previous cross-sectional studies (see Inman (1979) and Gramlich (1977)).

A potentially important issue in the estimation of Equation 8 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. 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 several interactions between community tax-price and the fraction of non-owner occupied residential property in several categories. First, I allow for a separate interaction between tax-price and vacant residential property. Note that, empirically, the variation in vacancy is mainly due to seasonal or recreational property, whose value is likely tied to natural amenities.[26] Next, I separate non-residential property into four groups, based on my priors regarding the mobility of property value, and measure the fraction of local property in each group.[27] The first group, which I refer to as "immobile," is comprised of properties that are clearly tied to the land on which they are situated. The second group, which I refer to as "semi-mobile," is comprised of properties that derive part of their value from the production of goods and services, not solely from land, and therefore may be mobile in response to taxation.[28] The third group consists of industrial properties, which are separated because of evidence from previous empirical studies that the costs to taxing industrial property may be greater than

---

[25] The p-value on the interaction of tax-price with the fraction of residents ineligible for exemptions is 0.107.
[26] A regression of the percentage of vacant units on the percentage of vacant seasonal/recreational units yields a coefficient of 1.02 and R^2 of 0.98. Nine of the ten school districts with the most vacant property are located in Suffolk County (well known for its beaches) or Hamilton County (a well known ski resort area). Thus, much of the variation in vacant residential properties reflects the presence of second homes close to natural amenities.
[27] As above, I use the mean fraction of local property value in each group from 1991-1995.
[28] The "immobile" group contains property classified as "Vacant Land," "Wild, Forested, Conservation Lands and Public Parks," or "Recreation and Entertainment." "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 group containing "Commercial" property. The "semi-mobile" group contains property classified as "Commercial," "Agricultural," or "Community Services" (e.g., educational, correctional, or health facilities).

commercial or agricultural property (Ladd (1974), Abeyratne and Johnson (1998)).

The fourth group of non-residential properties consists of "Public Services" properties, i.e., public utilities. Utilities may be similar to industrial properties in the economic value they impart to the local community, but they may be considerably less mobile. Furthermore, school districts and other local governments frequently negotiate payment in-lieu-of tax (PILOT) agreements with utility owners, whereby a set amount of taxes are paid annually over a number of years. (PILOT agreements protect utility owners from increases in taxes imposed after location decisions have been made, and they protect local residents from fluctuations in tax revenue that would arise from variation in the market value of these large properties.) Unfortunately, I cannot distinguish payments made through PILOT agreements from normal tax collection. Thus, one must be cautious in interpreting the estimated interaction of tax-price with the (measured) share of property taxes paid by public utilities.[29]

If voters perceive roughly equal costs to additional taxation on all types on non-residential property, then one would expect estimates of these additional interaction terms to 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 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 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 increases in absolute value to -0.36.

[29] To the extent that PILOT agreements are present, I will measure the fiscal incentives facing a school district incorrectly. I will underestimate the share of local property taxes paid (on the margin) by owners of residential and other non-residential properties, I will underestimate lump-sum payments from outside sources received by the school district, and I will underestimate the change in community tax-price for the district.

Likewise, 10% increases in immobile or semi-mobile property are both estimated to magnify community tax-price elasticity to -0.31.[30] 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. I also estimate a regression that includes interactions of a year trend with each measure of tax-base composition to address a concern that property composition may be correlated with time-varying unobservables. These interactions are neither individually nor jointly significant and only reduce the precision of my estimates.

The degree of local response to the fiscal incentives created by NYSTAR depended on the distribution of tax relief. My results 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.

6. The Relative Impact of Tax Relief for Elderly versus Non-Elderly Homeowners

Increases in the share of elderly residents within a geographic area are negatively correlated with school spending (Cutler et al. (1993), Poterba (1996), and Harris et al. (2001)). In addition, a number of studies of survey and voting data indicate that households without school-aged children prefer significantly lower spending on public schools (see Rubinfeld (1977), Bergstrom et al. (1982), Gramlich and Rubinfeld (1982), Lankford (1985)). Given that elderly households are unlikely to contain school aged children, the rise in the elderly population share from 13% today to over 20% in 2050 may have a significant impact on public support for expenditure on elementary and secondary education.

[30] 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.

There is, however, evidence suggesting that elderly households may support education. First, the correlation between the share of elderly residents and funding for education at the state and local level was positive in the early part of the twentieth century (Goldin and Katz (1998) and Hoxby (1998)). In addition, elderly households will sell their homes to younger families, and have an incentive to maintain the quality of local schools (Hilber and Mayer (2009)). Finally, almost every state in the U.S. has created programs designed 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 specify the tax-price for a group of households is the amount of money the group as a whole must pay when educational spending rises by one dollar. Since I do not have data on the amount of taxable property owned by elderly and non-elderly homeowners, I construct this variable using 2000 census tabulations and other data sources (see the Online Appendix for 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, on average, roughly 20% of households are elderly homeowners and roughly 55% of households are non-elderly homeowners. Thus, for each additional dollar of spending, elderly homeowners as a group pay less on average than non-elderly homeowners. However, I estimate the impact of a proportional reduction in group tax-prices, so scaling by the size of the group does not affect my results.

Equation 9 shows my regression specification with group tax-price terms for elderly (P^e_{jt}) and non-elderly (P^n_{jt}) homeowners. District fixed effects, time controls, and enrollment variables are omitted for simplicity. Income measures are at the community level because group-specific income data that vary over time within school districts are not available. In addition, virtually all of the variation in the third component of virtual income comes from lump-sum aid, and group-specific terms for this component would be highly collinear.

$$(9)\quad \ln(E_{jt}) = \pi_1 \ln(taxpayers_{jt}) + \pi_2 \ln(\overline{Y}_{jt}) + \pi_3 \ln\left(1 + \frac{(1-nystar\%_{jt})*Aid_{jt}}{Y_{jt}}\right) + (\delta^e + \lambda^e(I^e_j - \bar{I}^e)\ln(P^e_{jt}) + (\delta^n + \lambda^n(I^n_j - \bar{I}^n))\ln(P^n_{jt}) + \varepsilon_{jt}$$

Group tax-prices are interacted with the difference between the fraction of district households in each group (I^e_j and I^n_j) and the state mean. These interactions measure the additional impact of giving tax relief to a larger group of taxpayers, and are expected to be negative, i.e., giving tax relief to a larger fraction of local taxpayers has a greater impact on spending.[31] The estimated relation between tax-price effects and group size can be used to compare tax relief for elderly and non-elderly homeowners, conditional on the two groups being of equal size.

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

[31] Note that this interaction with group size is 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.

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 4. Estimated group tax-price elasticities 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 9 are well identified.[32]

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 group tax-price terms from Equation 9, leaving only interactions between group tax-prices and group size. Comparing the relative impact of tax relief for elderly and non-elderly homeowners is then equivalent to comparing the size of the coefficient estimates on these interactions. Estimates from the restricted specification are shown in Column 2 of Table 4. Estimated tax-price elasticities for both 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. If year fixed effects are substituted for trends (Column 3 Table 4), the tax-price elasticity for non-elderly homeowners remains slightly larger (-0.099) but is no longer

[32] 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. Sorting of elderly and non-elderly homeowners across school districts may be equally problematic, as it can generate correlations between the portion of households in each group and unobservable factors that affect local response. However, sorting based on preferences for spending should reinforce correlations between group presence and local response. For example, if the elderly prefer low expenditures on schools, they may move to districts where other residents prefer low expenditure levels, and districts with more elderly households will also be districts where the elderly are closer to the margin of local decisions. Nevertheless, I cannot rule out that sorting occurs in such a way as to mitigate the relation between the impact of tax relief targeted to particular groups and the relative influence of groups within a community. Given the results in the previous section, one might wonder whether sorting of households based on the composition of property affects these results. Adding interactions between group tax-prices and property composition (as in Section 5) does not noticeably change my findings.

statistically different from the effect for elderly households (-0.077). Together, 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.

7. Conclusion

I find that tax-price reductions for homeowners in New York State led to an increase in local school district expenditures, crowded out a significant portion of the intended tax relief, and raised taxes for other property owners. Using variation in the composition of taxpayers across school districts, I find evidence that targeting of fiscal incentives to homeowners (as opposed to renters or owners of second homes or non-residential property) led to larger impacts on school expenditure. I also find evidence that local residents perceive significant costs to raising taxes on local property whose value is mobile, such as commercial and industrial property.

Reducing school district property taxes for homeowners may have other effects as well. For example, this policy reduced the cost of homeownership, and may have induced some households to switch from renting to owning. Reducing property taxes paid to school districts may also have effects on other local public goods (fire protection, police, libraries, parks, etc.). Both of these topics, while beyond the scope of this paper, are worthy of future research.

My findings have two important implications for state fiscal policy. Local governments and local taxpayers react to changes in fiscal incentives and constraints, and state policymakers can and should take these behavioral responses into account when designing policies. This point has been made by other researchers, but it is worth repeating here. In addition, most current policies—from common lump-sum and matching grants to complicated school finance equalization systems—provide fiscal incentives that are uniform across all local taxpayers. My results suggest state governments should consider the relative influence of particular groups of local taxpayers when constructing policies that seek to change local taxation and expenditure.

References

Abeyratne, Frederick and Thomas G. Johnson, "Use Value Taxation and the Impact of Tax Rate Change on the Value of Agricultural Real Property Tax Base," University of Missouri-Columbia Working Paper, March 1998.

American Education Finance Association Annual Yearbook of the American Education Finance Association 1998-1999 Cambridge, MA: Ballinger Publishing, 1999

Anderson, Nathan, "Voting, Opportunity Costs, and Property Taxes," Unpublished Manuscript, March 2006.

Bergstrom, Theodore C. and Robert P. Goodman, "Private Demands for Public Goods," American Economic Review, June, 1973.

Bergstrom, Theodore C., Daniel L. Rubinfeld and Perry Shapiro, "Micro-Base Estimates of Demand Functions for Local School Expenditures," Econometrica, September, 1982.

Black, Duncan, "On the Rationale of Group Decision-Making," Journal of Political Economy, February, 1948.

Bradford, David F. and Wallace E. Oates, "Towards a Predictive Theory of Intergovernmental Grants," American Economic Review, May 1971.

Burtless, Gary and Hausman, Jerry A. "The Effect of Taxation on Labor Supply: Evaluating the Gary Negative Income Tax Experiment," Journal of Political Economy, December 1978.

Calabrese, Stephen, Dennis Epple, and Richard Romano, "Inefficiencies from Metropolitan Political and Fiscal Decentralization: Failures of Tiebout Competition," Unpublished Manuscript, March 2008.

Charney, Alberta H., "Intra-urban Manufacturing Location Decisions and Local Tax Differentials," Journal of Urban Economics, 1983.

Cutler, David M., Douglas W. Elmendorf, and Richard J. Zeckhauser, "Demographic Characteristics and the Public Bundle," NBER Working Paper 4281, February, 1993.

Dehring, Carolyn A., Craig A. Depken and Michael R. Ward, "A Direct Test of the Homevoter Hypothesis," Journal of Urban Economics 64(1): 155-170, 2008.

DiPasquale, Denise and Edward L. Glaeser, "Incentives and Social Capital: Are Homeowners Better Citizens?" Journal of Urban Economics, 1999.

Duncombe, William D., and John Yinger, "An Analysis of Two Educational Policies in New York State: Performance Standards and Property Tax Relief," in Educational Finance to Support Higher Learning Standards, edited by J. H. Wyckoff (Albany: New York State Board of Regents), 1998.

Dye, Richard F., Therese J. McGuire, and David F. Merriman, "The Impact of Property Taxes and Property Tax Classification on Business Activity in the Chicago Metropolitan Area," Journal of Regional Science, 2001.

Ehrenberg, Ronald G., Randy A. Ehrenberg, Christopher L. Smith and Liang Zhang (2004) "Why Do School District Budget Referenda Fail?" Educational Evaluation and Policy Analysis, 2004.

Eom, Tae Ho, William Duncombe, and John Yinger, "The Unintended Consequences of Property Tax Relief: New York's STAR Program," Syracuse University Center for Policy Research Working Paper, 2005.

Epple, Dennis, Radu Filimon, and Thomas Romer, "Equilibrium among Local Jurisdictions: Toward an Integrated Treatment of Voting and Residential Choice," Journal of Public Economics, August, 1984.

Fischel, William A. "The Homevoter Hypothesis," (Cambridge: Harvard University Press, 2001).

Feldstein, Martin S., "Wealth Neutrality and Local Choice in Public Education," American Economic Review, March, 1975.

Fox, William F., "Fiscal Differentials and Industrial Location: Some Empirical Evidence," Urban Studies, 1981.

Goldin, Claudia and Lawrence F. Katz, "The Origins of State-Level Differences in the Public Provision of Higher Education: 1890:1940," American Economic Review, May, 1998.

Gramlich, Edward M., "Intergovernmental Grants: A Review of the Empirical Literature," in The Political Economy of Fiscal Federalism, Ed.: W. Oates (Lexington, MA: DC Heath and Company, 1977)

Gramlich, Edward M. and Daniel L. Rubinfeld, "Micro Estimates of Public Spending Demand Functions and Tests of the Tiebout and Median Voter Hypotheses," Journal of Political Economy, June, 1982.

Harris, Amy Rehder, William N. Evans and Robert M. Schwab, "Education Spending in an Aging America," Journal of Public Economics, 2001.

Hilber, Christian and Christopher Mayer, "Why do Households without Children Support Local Public Schools? Linking House Price Capitalization to School Spending," Journal of Urban Economics, 2009.

Hoxby, Caroline, "How Much Does School Spending Depend on Family Income? The Historical Origins of the Current School Finance Dilemma," American Economic Review, May, 1998.

Hoxby, Caroline, "All School Finance Equalizations Are Not Created Equal," Quarterly Journal of Economics, November, 2001.

Inman, Robert P., "The Fiscal Performance of Local Governments: An Interpretative Review," in Current Issues in Urban Economics, Ed.: P. Mieszkowski and M. Straszheim (Baltimore: The Johns Hopkins University Press, 1979)

Ladd, Helen F., "Local Education Expenditures and the Composition of Property Tax Base," National Tax Association - Tax Institute of America, Proceedings of Sixty-Seventh Annual Conference, 1974.

Lankford, R. Hamilton, "Preferences of Citizens for Public Expenditures on Elementary and Secondary Education," Journal of Econometrics, 1985.

McGuire, Therese, "Are Local Property Taxes Important in the Intrametropolitan Location Decisions of Firms? An Empirical Analysis of the Minneapolis-St. Paul Metropolitan Area," Journal of Urban Economics, 1985.

Mikhailov, Nikolai "Types of Property Tax and Assessment Limitations and Tax Relief Programs," Lincoln Institute of Land Policy Working Paper, 1998.

Oates, Wallace E., "Fiscal Federalism," (New York: Harcourt Brace Jovanovich, 1972).

Poterba, James, "Demographic Structure and the Political Economy of Public Education," NBER Working Paper 5677, July, 1996.

Reback, Randall, "Preference Aggregation and Tax Price Discrimination in an Imperfect Tiebout World," Unpublished Manuscript, Barnard College 2008.

Romer, Thomas and Howard Rosenthal, "Bureaucrats Versus Voters: On the Political Economy of Resource Allocation by Direct Democracy," Quarterly Journal of Economics, November, 1979.

Romer, Thomas, Howard Rosenthal and Vincent G. Munley, "Economic Incentives and Political Institutions: Spending and Voting in School Budget Referenda," Journal of Public Economics, 1992.

Rothstein, Richard and Karen Hawley Miles, "Where's the Money Gone? Changes in the Level and Composition of Education Spending," Washington DC: Economic Policy Institute, 1995.

Rubinfeld, Daniel L., "Voting in a Local School Election: A Micro Analysis," The Review of Economics and Statistics, February, 1977.

Sexton, Terri A. "Property Tax Systems in the United States: The Tax Base, Exemptions, Incentives, and Relief," Unpublished Manuscript, UC Davis, 2003.

Thaler, Richard, "The Flypaper Effect," Journal of Economic Perspectives, Fall, 1995.

Wasylenko, Michael J., "Evidence of Fiscal Differentials and Intrametropolitan Firm Relocation," Land Economics, August, 1980.

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	0.533	0.147
Dispersion of Household Income (80/20 Ratio)	3.817	0.777
Dispersion of Individual Education Levels (80/20 Ratio)	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. Dispersion in education is measured for adults 25 years and older.

Table 2: The Average Impact of NYSTAR on Educational Expenditure

	OLS	Naïve OLS
Community Tax-Price Elasticity	**-0.220**	**-0.171**
Ln (1- nystar%)	**(0.021)****	**(0.019)****
Ln (Mean Income)	0.012	0.020
	(0.012)	(0.012)
Ln (# Tax Returns)	0.190	0.199
	(0.034)**	(0.034)**
$\text{Ln}\left(1+\frac{(1-\text{nystar\%})*\text{Aid}}{\text{Aggregate Income}}\right)$	0.880	
	(0.216)**	
$\text{Ln}\left(1+\frac{\text{Aid}}{\text{Aggregate Income}}\right)$		1.292
		(0.182)**
Ln (Enrollment)	0.205	0.207
	(0.038)**	(0.036)**
ΔLn (Enrollment)	-0.125	-0.123
	(0.032)**	(0.031)**
District Fixed Effects	Yes	Yes
County Trends, City Trend	Yes	Yes
Observations	4333	4333
R-squared	0.9989	0.9989

Note: The dependent variable in all regressions is the natural log of operational expenditure. Estimates from column 2 are referred to as naïve because they ignore the interaction between tax-prices and lump-sum grants in the determination of educational expenditure. Calculations of effects for a typical district assume a tax-price of 1 and ratio of grant aid to aggregate income of .05. Standard errors (in parentheses) are clustered by school district. * significant at 5%; ** significant at 1%.

Table 3: Local Response and the Distribution of Tax Relief

	(1)	(2)	(3)
Community Tax Price Elasticity	-0.033	-0.032	-0.011
	(0.061)	(0.061)	(0.070)
Community Tax-Price Interacted with:			
Property Share Not Occupied-Residential	-0.163		
	(0.094)+		
Fraction of Residents Ineligible for Exemption	-0.126	-0.153	-0.163
	(0.078)	(0.081)+	(0.110)
Variation in Owner-Occupied Home Values	-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)
Ln (Mean Income)	0.012	0.012	0.041
	(0.012)	(0.012)	(0.019)*
Ln (# Tax Returns)	0.185	0.166	0.197
	(0.034)**	(0.035)**	(0.039)**
$\text{Ln}\left(1+\frac{(1-\text{nystar}\%)*\text{Aid}}{\text{Aggregate Income}}\right)$	0.941	0.955	0.995
	(0.221)**	(0.220)**	(0.325)**
Ln (Enrollment)	0.218	0.225	0.228
	(0.038)**	(0.039)**	(0.042)**
ΔLn (Enrollment)	-0.136	-0.136	-0.141
	(0.031)**	(0.031)**	(0.033)**
District Fixed Effects	Yes	Yes	Yes
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. 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. Standard errors (in parentheses) are clustered by school district. + significant at 10%; * significant at 5%; ** significant at 1%.

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

	(1)	(2)	(3)
Group Tax-Price Elasticity			
Elderly Homeowners	**-0.030**		
	(0.004)**		
Non-Elderly Homeowners	**-0.113**		
	(0.012)**		
Interactions of 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)+**
Ln (Mean Income)	0.026	0.018	0.029
	(0.014)	(0.014)	(0.018)
Ln (# Tax Returns)	0.167	0.169	0.181
	(0.035)**	(0.036)**	(0.040)**
$\text{Ln}\left(1+\frac{(1-\text{nystar}\%)*\text{Aid}}{\text{Aggregate Income}}\right)$	0.902	0.836	0.684
	(0.247)**	(0.249)**	(0.331)*
Ln (Enrollment)	0.231	0.230	0.226
	(0.039)**	(0.039)**	(0.042)**
ΔLn (Enrollment)	-0.149	-0.143	-0.148
	(0.032)**	(0.032)**	(0.034)**
District Fixed Effects	Yes	Yes	Yes
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. Group tax-prices are defined as the amount of money a group as a whole must pay when expenditure rises by one dollar. Standard errors (in parentheses) are clustered by school district. + significant at 10%; * significant at 5%; ** significant at 1%.

Figure 1: Phase-in of NYSTAR Property Tax Exemptions

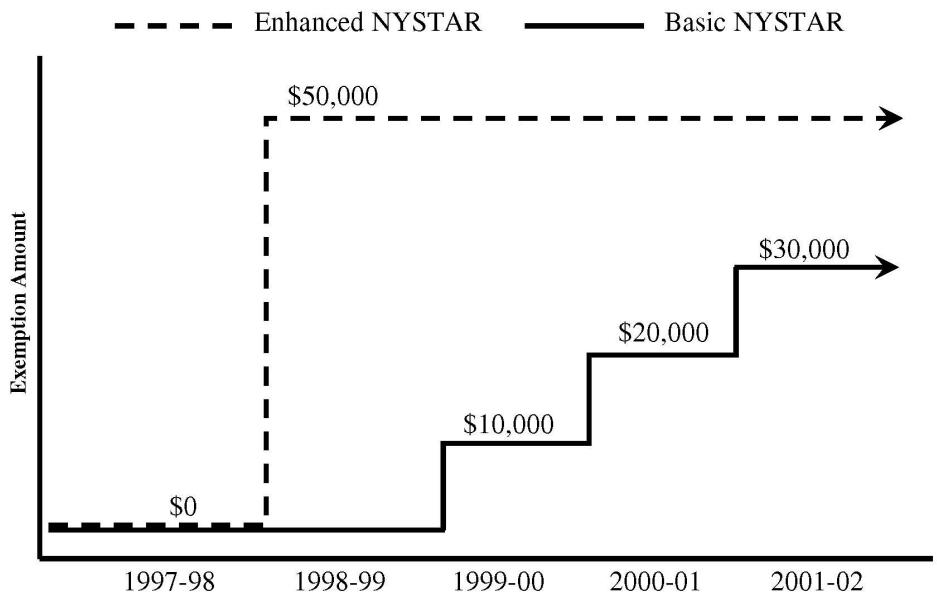
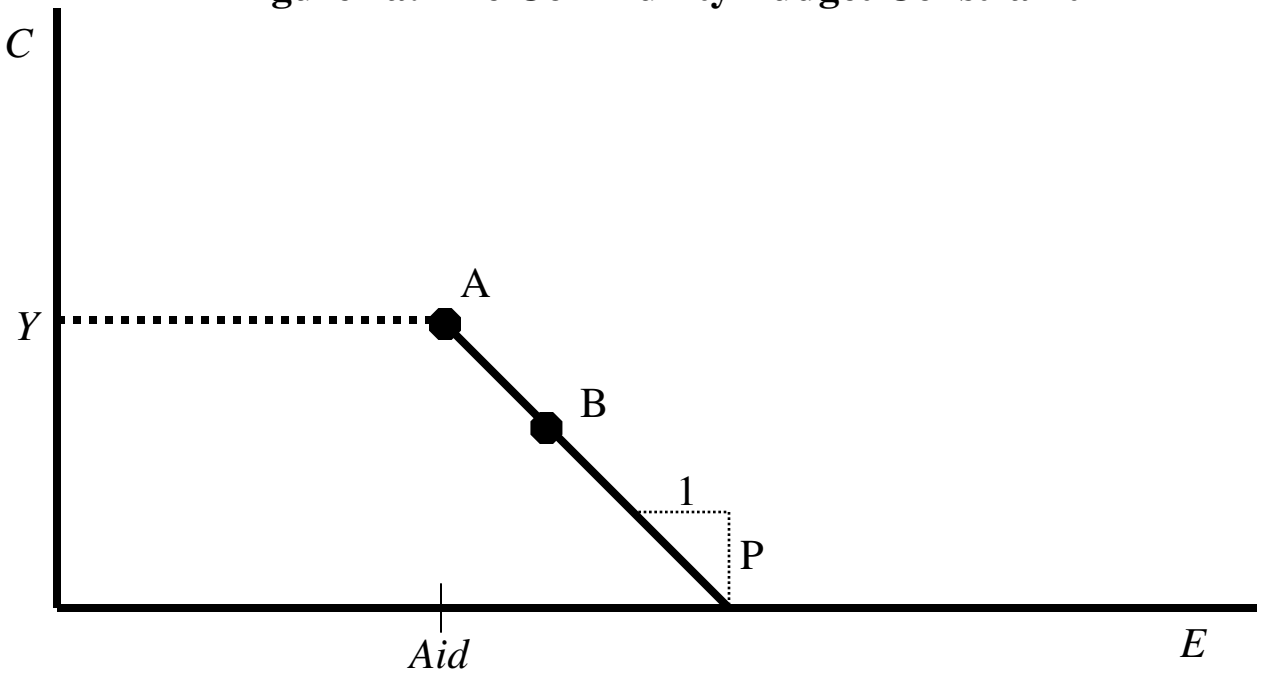
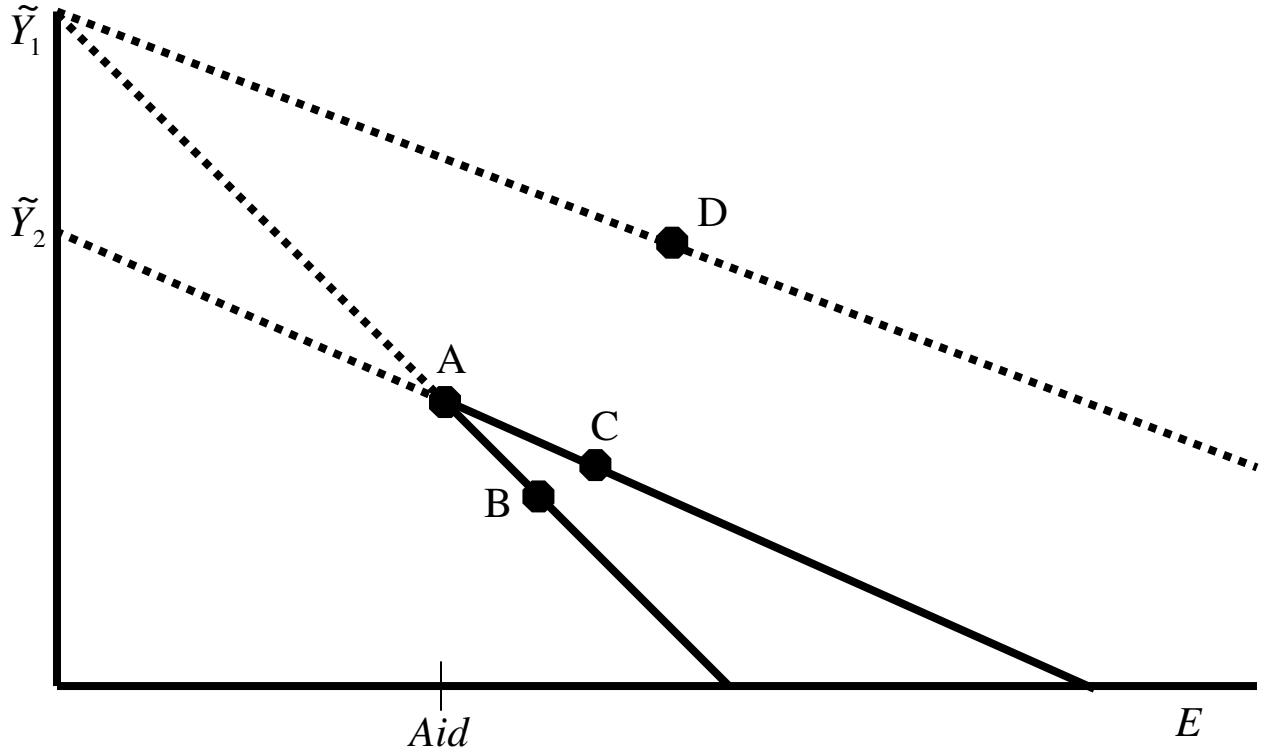


Figure 2a: The Community Budget Constraint



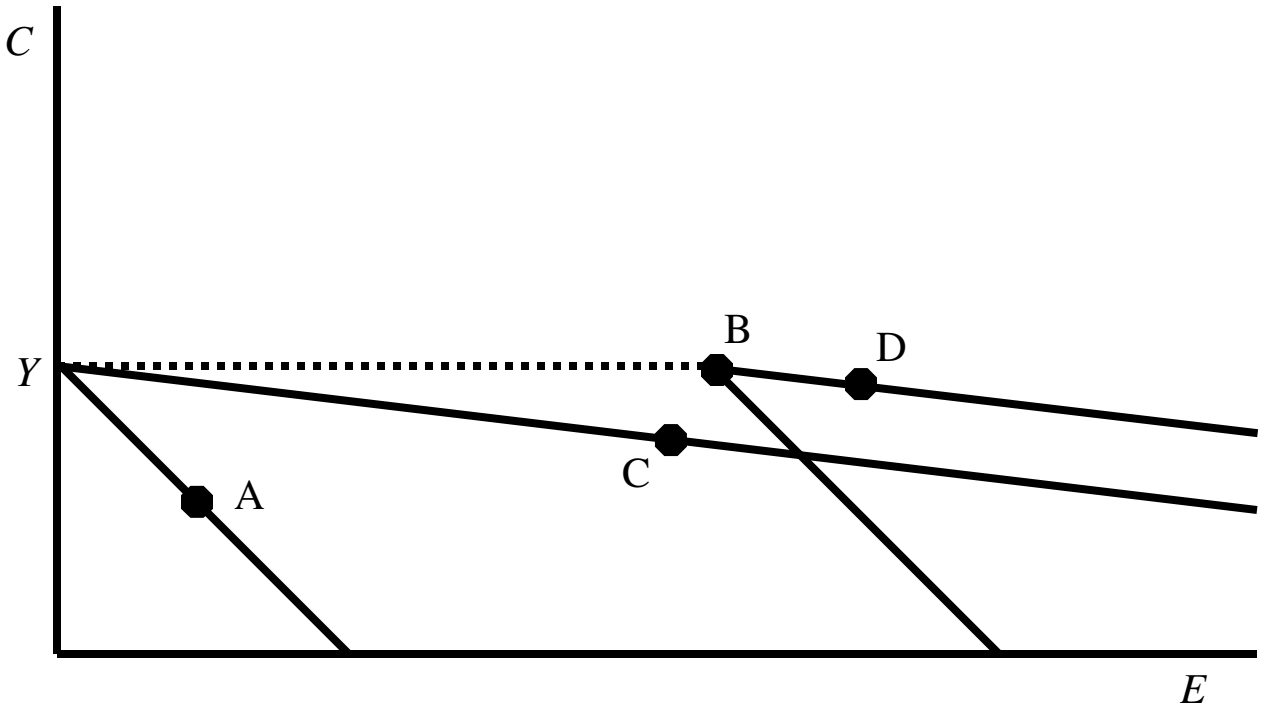
Notes: *Y* is community income and *Aid* is a lump-sum grant received from outside sources. A community choosing A will spend outside aid but levy no property taxes, while a community choosing B will levy taxes, lower consumption (*C*), and increase educational spending (*E*). *P* is the community tax-price.

Figure 2b: Tax-Price Changes and Virtual Income



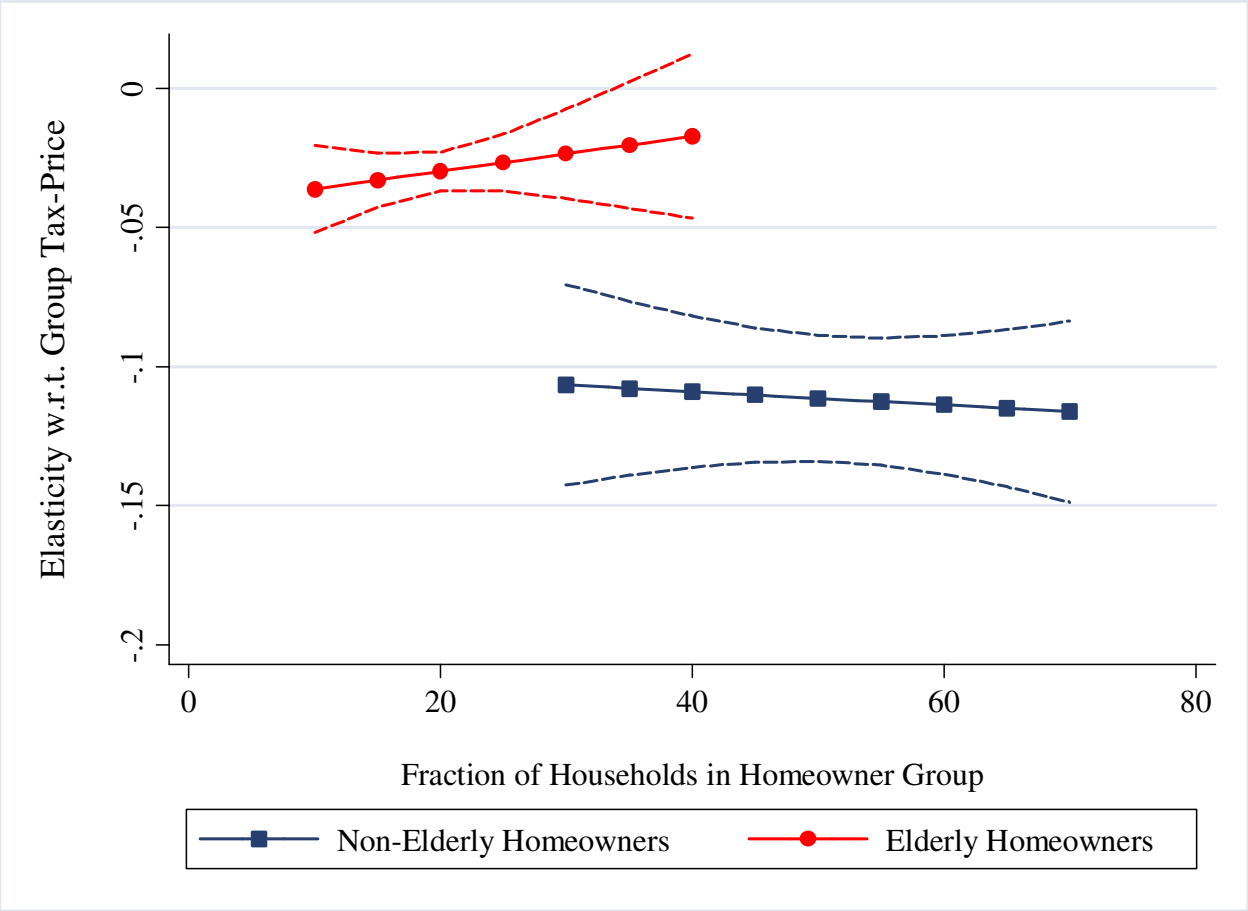
Notes: Virtual income is originally $\tilde{Y}_1$. A fall in community tax-price decreases virtual income to $\tilde{Y}_2$. The pure price effect is the change from B to D and the virtual income effect is the change from D to C.

Figure 3: Interaction of Lump-sum and Matching Grants



Note: This figure illustrates the interaction between lump-sum and matching grants in the determination of district expenditure. A school district receiving no grants selects point A, and the receipt of a generous lump-sum or a generous matching grant moves the district to either point B or C, respectively. The receipt of both grants moves the district to point D.

Figure 4: Elasticity of Expenditure with Respect to Group Tax-Price



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