School Finance Reform in Vermont: Essays Evaluating Equity, Achievement, and Capitalization under Vermont's Equal Education Opportunity Act

by

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Abstract

SCHOOL FINANCE REFORM IN VERMONT: ESSAYS EVALUATING EQUITY, ACHIEVEMENT, AND CAPITALIZATION UNDER VERMONT'S EQUAL EDUCATION OPPORTUNITY ACT

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This dissertation consists of three papers exploring the impacts of Vermont's Equal Education Opportunity Act, Act 60, on equity, student achievement, and property values.

The first paper evaluates the equity implications of Vermont's Act 60. Using data on per-pupil spending at the town level, it appears that Act 60 did equalize resources across pupils. Further, the relationship between per-pupil spending and a town's income as well as the relationship between per-pupil spending and property wealth were reduced. However, the magnitude of the reduction is diminished when private donations, induced by the high tax price many towns faced under Act 60, are included.

The second paper explores the impacts of spending changes under Act 60 on student achievement. Under Act 60, per-pupil resources changed from year-to-year for many Vermont towns. This paper asks whether these changes in resources were associated with changes in student performance as measured by pass rates on standardized

tests. Data on spending and test score pass rates are available annually from 1999 through 2004. Using these data, fixed effects and instrumental variables estimation techniques are employed. Changes in town spending under Vermont's Act 60 appear to have had a positive impact on 4th grade math pass rates. However, these spending changes did not significantly impact 4th grade writing or reading or 2nd grade reading pass rates. There is suggestive, but inconclusive, evidence that additional resources were more effective at increasing test score pass rates in initially low spending schools. There is not, however, any evidence that money was more effective in schools that were initially low achieving.

The final paper examines how aggregate property values in the state of Vermont responded to changes in spending, tax rates, and student achievement under Act 60. In contrast to much of the literature, this study finds no evidence that property values in Vermont respond to changes in test score pass rates. Aggregate property values, however, do respond negatively to changes in tax rates. The relationship between aggregate property values and school spending proves most interesting. Spending only appears to be positively related to aggregate property values in towns that choose to rely on private donations to supplement school spending provided under the state's school finance system. This could be taken as evidence that additional school spending is only valued in districts that are constrained from spending at the desired level under Act 60 and not by all communities in the state.

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Chapter 1

Introduction: Experiences with School Finance Reform

Improving America's public schools continues to be an issue of tremendous importance to policymakers both at the federal and state level. Over the past few decades, there has been intense debate regarding whether school finance reform at the state level should focus on equalizing resources or providing some adequate level of resources. Equalizing resources across students sits well with the notion that education is a fundamental right, and should be provided as such to all. However, achieving resource equity explicitly limits those with a greater desire for education spending from fulfilling that want. Equalizing resources such that the spending desires of all are met is prohibitively expensive. In this sense, adequacy may be seen as a compromise. Determining what constitutes an adequate education presents its own challenges related to measuring costs and quantifying success.

Debates have also continued over which level of government should be responsible for financing public education. Funding education at the local level and allowing for

a large amount of local control tends to engender greater support for enhanced education spending (even via higher taxes) in property wealthy communities. However, reliance on local level funding tends to lead to inequities given that the tax base is unequally distributed across school districts. Over the past 30 years, almost every state has seen an increase in the proportion of education revenues coming from state sources (Corcoran and Evans, 2008). While moving toward greater levels of state funding may enhance equity, there are concerns regarding loss of local control and subsequently support as well as concerns surrounding the stability of the tax base with state funding.[1]

Calls for equity and adequacy in school funding are grounded in the belief that funding has real consequences for students' educational opportunity and subsequent life success. Hence, an evaluation of school finance reform should not only evaluate whether resources have been effectively equalized or adjusted to an adequate level, but also whether educational quality has been equalized. Specifically, evaluations of success should examine whether students in the districts least well off prior to reform have seen their relative standing increase post reform. A widely available and relatively consistent measure of school quality is student standardized test scores. The notion that more resources improves school quality is intuitively appealing. However, the evidence suggesting that additional resources do in fact improve educational outcomes is less than convincing.

Another important aspect of school finance reforms are their potential for unintended consequences. While school finance reforms intend to redistribute school resources and improve the quality of student outcomes, often times reforms have

[1]State revenues are more likely to be generate by sales taxes or lotteries, where the tax base is more volatile than a property tax base.

unintended redistributional consequences. Specifically, to the extend that property values react to changes in taxes, spending, and (perhaps) test scores across districts, school finance reform can lead to windfall gains and losses in property wealth. Alternatively, school finance reforms that lead to increases in property value can be viewed as those that increase school quality, even if test scores remain relatively flat.

This dissertation explores Vermont's experiences under Vermont's school finance reform, the Equal Education Opportunity Act, Act 60. In order to provide some context for Vermont's experience, school finance reforms undertaken in other states are briefly reviewed. While many states have undertaken reform over the past 30 years, these three were chosen as for two reasons. First, each of these reforms has been widely reviewed in the literature. Second, each reform has elements that make it a good reference point to which reforms in Vermont can be compared.

1.1 School Finance Reform in Other States

1.1.1 California

The California State Supreme Court initiated the modern era of school finance reform with its ruling in the *Serrano v. Priest* case in 1971. The *Serrano* case challenged the constitutionality of California's method of public school finance on the grounds that education was a fundamental right, and school district wealth should not determine a students access to education (Brunner and Sonstelie (2006) provide an comprehensive overview of school finance reform in California). Essentially, the court found that correlation between school spending and school district property wealth was in violation of the state's constitution.

Following the *Serrano* decision the California legislature attempted to equalize

spending by placing revenue limits on high spending towns (although these limits could be exceeded with an override) and by providing more state-aid to poorer districts. A subsequent court ruling found these remedies insufficient. The court in *Serrano II* was more specific in their demands of the legislature, requiring that schools spending vary by no more than $100 per-pupil across districts (Fischel, 1989). As the legislature was exploring school financing schemes capable of generating the equity demanded by the court, voters in California passed Proposition 13.

Proposition 13 was a constitutional amendment that effectively limited property taxes across the state. Proposition 13, passed in 1978, rolled back all assessed property values to the 1975, limited increases in value to two percent annually unless a property was sold, and reduced tax rates to one percent of assessed market value. Fischel (1989) argues that the adoption of Proposition 13 was voters' response to the *Serrano* decisions. Prior to *Serrano*, revenues raised via property taxes in local communities were spent on local public schools. Following the *Serrano* decisions, district expenditures were limited. Additional property tax revenues raised in wealthy communities could not be spent to finance local public schools, so the response was to limit property taxes.

Following school finance reform in California, Downes (1992) found that per-pupil expenditures converged. More importantly, the relationship between spending and property values and weakened, which Downes takes as evidence that a "good faith effort" was made to comply with the ruling given in *Serrano II*. While reform in California did lead to equalization of funding, of concern to policymakers is the fact that funding levels fell post-reform (Silva and Sonstelie, 1995). Spending across schools was equalized in California, however, the total level of spending fell following reform.

Downes (1992) also analyzes the impact of California's school finance reform and

the accompanying spending equalization on student achievement. Downes concludes that while spending equalized, student performance did not appear to follow trend. When the justification for equalizing expenditures is to equalize the quality of education, success of school finance reform should be evaluated in terms of the quality of education. If higher quality education is associated with higher test scores, then test scores should have equalized when spending equalized in California.

School finance reforms are intended to equalize resources for students, and ultimately improve educational opportunities for those previously undeserved. As is often the case with public policies, there are unintended (perhaps unanticipated) consequences. In the case of school finance reform, property owners are often impacted as changes in the tax and school quality package are capitalized into property values. In one of the first studies to examine the impact of changing tax rates following school finance reform on property values, Rosen (1982) found that decreases in property taxes were associated with increased property values. For communities that experienced tax cuts without any accompanying decrease in school spending (poor communities), *Serrano* and Proposition 13 provided a windfall of wealth.

1.1.2 Kentucky

In 1989 the Kentucky Supreme Court found that the state's educational system was unconstitutional. Not only did the court in *Rose v. Council for Better Education, Inc.* find that Kentucky's system of school finance violated the state's Constitution, but the governance of school systems and the curriculum were also found to be in violation of Kentucky's residents' constitutional rights. The goal of reform in Kentucky was to reform the whole system of education, and engage in reform that extended beyond the generation of finance equity. In 1990, Kentucky adopted the Kentucky Education

Reform Act (KERA). Flanagan and Murray (2004) provide an overview of Kentucky's experience with school finance reform.

Pre-reform, Kentucky's local schools were financed primarily via local property tax revenues. Prior to KERA, the state used a power equalizing scheme in an attempt to increase the revenue raising capacity of low wealth districts. This system, however, proved ineffective in Kentucky as low wealth districts were not willing to tax themselves at rates high enough to take advantage of the equalization measure, districts systematically under-assessed property values, and the legislature often left the system underfunded (Flanagan and Murray, 2004). Further exacerbating inequity in Kentucky was the fact that equalization took place at the classroom level, rather than the pupil level. Hence, poorer districts with larger class sizes received less revenue on a per-pupil basis (Murray, Evans, and Schwab, 1998).

Under KERA, Kentucky adopted a new system of education finance. The Support Education Excellence in Kentucky (SEEK) programs intent was to equalize spending across districts. SEEK provided each district a minimum level of per-pupil funding. This level was adjusted based on district demographics, allocating additional funds to districts with a larger proportion of costly to educate students. Local districts were required to levy a tax rate of 0.3% of assessed value per-pupil to contribute toward their own education expenses. Districts were allowed to exceed this minimum level of funding by up to 49.5% above the base. For spending up to 15% above the base, local tax increases were matched by state funds such that local tax increases were guaranteed to raise revenue equivalent to what would be raised in a town with assessed value of 150% of the statewide average. Effectively, this was a matching program for low property wealth towns. Once districts were spending 115% of the base guarantee, there was no matching. No district was allowed to spend more than

149.5% of the base guarantee (Clark, 2003). SEEK also included a large increase in the amount of state funding, enforced local effort provisions, and required that property be assessed at full market value (Flanagan and Murray, 2004).

Clark (2003) and Flanagan and Murray (2004) both provide empirical evidence regarding the impact of KERA's SEEK component on spending equity in Kentucky. Flanagan and Murray find that inequality did decline following the passage of KERA. Overall, the decline in inequality appears to have come from additional state aid that allowed previously low spending districts to increase spending relative to high spending districts. Under KERA, even districts that were at the 95th percentile of the revenue distribution before KERA was adopted experienced an increase in real spending. Clark also illustrates that spending went from being positively related to median income prior to KERA to being negatively related to median income post-KERA.

The evidence of KERA's impact on student achievement is mixed. Studies evaluating the impact of KERA on educational achievement face a special challenge in that KERA represented comprehensive school reform, not just school finance reform. While spending was increasing for a number of previously low spending schools, KERA's curriculum and governance reforms were being adopted simultaneously. Flanagan and Murray use an instrumental variables approach to estimate the impact of additional spending on student achievement under KERA and do not find any significant effect. Clark's evaluation of KERA's effects on student achievement is more comprehensive. Using a difference-in-difference strategy, Clark also fails to find evidence of an across the board increase in achievement in previously poor schools.[2]

[2]Clark does find evidence of improved achievement among black students under KERA. She argues, however, that this is not likely due to KERA's finance reforms, as black students tend to be located in relatively wealthy urban areas. Instead, the achievement improvements may be

1.1.3 Michigan

Unlike school finance reform in California and Kentucky, Michigan's reform was not the result of litigation. Instead, property tax relief was the driving force behind the adoption of Proposal A. Proposal A, adopted in in 1994, increased state sales taxes as a measure to finance schools. In 1993, the legislature had moved to eliminate property taxes as a revenue source for local public schools. Proposal A was the state's response. Cullen and Loeb (2004) review the events leading up to the adoption of Proposal A as well as the policy's structure and impacts.

Under Proposal A, revenues from the state's sales tax, a cigarette tax, a property transfer tax, as well as a proportion of state income taxes are deposited into a School Aid Fund. Revenues from this fund are used to provide state education aid to districts. Districts themselves still levied property taxes. The average property tax rate on homestead property under Proposal A was less than 18% of the previous level, and was levied as a statewide tax instead of being locally determined. The state determined the minimum (foundation) amount it wanted each district to spend per-pupil, and provided aid to any district that was unable to raise this minimum amount via property taxes. Michigan also capped revenues in districts that were high spending prior to Proposal A (Cullen and Loeb, 2004).

Cullen and Loeb (2004) and Roy (2004) both provide evidence on the equalizing effects of Michigan's Proposal A. Overall, resources equalized across districts. Spending on average also increased, while spending increases were relatively larger for previously low spending districts. Prior to reform in Michigan, there was a positive and significant relationship between between a school district's income and spending. After reform, the relationship disappeared (Roy, 2004).

attributable to on of KERA's non-finance elements, specifically curriculum reform.

There is mixed evidence regarding whether additional spending under Proposal A led to increased student achievement in the districts receiving extra funds. On the one hand, Papke (2005, 2008) concludes that additional funds did lead to increases in student achievement. However, Papke only examines 4th grade math pass rates on the state administered standardized test. Roy (2004) is able to replicate Papke's finding that additional funds did lead to improvements in student achievement on the state administered test. Additional resources were not found, however, to improve student performance on national tests such as the ACT and NAEP.

Epple and Ferreyra (2008) use a general equilibrium framework to investigate the impact of Proposal A on housing prices in the Detroit metropolitan area. Epple and Ferreyra find evidence which suggests that changes in property tax rates were fully capitalized into house values. Furthermore, property values appear to have responded positively to additional revenues.

1.2 General Evidence on the Impacts of School Finance Reforms

While single state studies are useful for quantifying the impacts of specific reforms, the variation in school finance programs across states has also been useful for deriving general predictions regarding the impacts of school finance reform.[3] In general, the evidence suggests that school finance reforms do equalize resources (Murray, Evans, and Schwab, 1998). There is concern that equalization may lead to lower levels of overall resources, or a leveling-down effect, as was observed in California. While

[3]Fischel (2006) and Corcoran and Evans (2008) both provide reviews of the literature on the general impacts of school finance reforms

Murray, Evans, and Schwab did not find evidence that suggested leveling-down was likely to occur as the result of court-ordered reform, Downes and Shah (2006) and Manwaring and Sheffrin (1997) find evidence which suggests that leveling-down as the result of school finance reform is not improbable. Rather, the impact of school finance reform on total resources depends on additional state level characteristics. Hoxby (2001) argues that it is the tax price of education following reform that determines whether a state levels-up or levels-down. States with a high tax price of education, such as California, are more likely to see a reduction in the overall level of resources.

Cross state variation can also be used to evaluate how student achievement responds to school finance reforms. One of the problems faced by researchers attempting to compare achievement across states is that measures of achievement generally differ from state to state. College admissions tests, such as the SAT, are one standard measure of achievement. Husted and Kenny (2000) found that states with school finance equalization test to have less within-state variation in SAT scores. However, Husted and Kenny also found that states with school finance equalizations also tended to have lower average SAT scores, leading to the conclusion that equalization may have had a "dumbing down" effect. Card and Payne (2002) found more encouraging results. In their study, states with court-ordered school finance equalizations tended to see improvements in the scores of the state's poor students. Dropout rates are another measure of student achievement that is relatively standardized across states. Hoxby (2001) examines the dropout rates across states and finds that states with stronger equalization do not exhibit lower dropout rates.

Other research has explored the implications of tax limits on student performance. Figlio (1997) and Downes and Figlio (1998) both found that explicit tax limits have a negative impact on student performance. This suggests that resources do matter, as

restricting resources reduced performance. In the same study, Downes and Figlio also found that finance reforms that are not in response to court rulings tend to increase the mean level of student performance.

Hoxby (2001) and Dee (2000) both investigate the impacts of school finance reforms on property values using nationwide datasets. Dee proposes that if changes in spending increase property values in receiving districts, then school quality has improved. Dee found that housing prices did increase dramatically for the poorest school districts in states with court-mandates school finance reform, which he takes as evidence that school finance reform did improve the quality of schools in poor districts. Hoxby (2001) argues that since school finance reforms are capitalized, policymakers not only need to think about how school finance reforms will change tax rates, but also how reforms will change the tax base.

This dissertation evaluates school finance reform in Vermont using a series of three independent papers. The first paper, Chapter 2, explores to what extent school finance reform in Vermont enhanced equity and reduced the relationship between district property wealth and spending as well as the relationship between income and spending. Overall, it appears that resources in Vermont did equalized under the legislation. However, the use of private contributions to fund local education served to undo a portion of the state induced equalization. The second paper, Chapter 3, explores whether it is likely that changes in spending in Vermont led to changes in test pass rates. Under some specifications, there is evidence that additional funding may have positively impacted test score pass rates. However, there is no evidence that additional resources increased test score pass rates in other subjects. The final chapter, Chapter 4, explores how property values in Vermont reacted to changes

in tax rates, spending, and test scores, during Vermont's experiment with school finance reform. Generally, property values in Vermont do not respond positively to additional educational resources. However, towns that were spending constrained under Act 60 do appear to value additional school resources more than towns that were not constrained.

Given that each paper is capable of standing on its own, there is some redundancy is sections describing Vermont's school finance reform and other background information. The benefit of having independent papers is that each paper's contribution to a separate literature can be identified without needing to seek out other sections for background information and context.

Chapter 2

The Impact of School Finance Reform on Equity in Vermont

School finance reform has been, and is likely to continue to be, a contentiously debated issue. Typically, states undertake school finance reform in order to address inequities, perceived or actual, under the current system. In some cases, the reform is mandated as the result of legal action. In other cases, reform is legislatively driven. In either case, school finance reforms seek to find an efficient and fair means of providing a quality public education to all students.

When Vermont undertook school finance reform in 1997, legislators were working under orders from the Vermont Supreme Court to devise a system of school finance that provided equal access to educational funds. Specifically, the court had recently ruled that a school finance system that allowed for correlation between district property wealth and per-pupil spending was unfair. Lawmakers in Vermont were simultaneously trying to devise a method for property tax relief, and an attempt at such relief came in conjunction with school finance reform.

This chapter reviews Vermont's experience with school finance reform, focusing on measuring equity under the reform. The primary focus is on equity in spending. Some typical equity indices are presented, such as the Gini coefficient, the coefficient of variation, and the McLoone Index. There appears to be moderate increases in equity - but the enhanced equity does not appear to be coming from improvement for those districts in the lower half of the distribution.

The relationship between per-pupil spending and property values is also evaluated. School finance reform in Vermont was successful in reducing the correlation between spending and district property wealth. However, there were incentives for towns to avoid participating in the state's system of school finance, and wealthy towns did undertake measures to undermine the state enforced equalization.

2.1 Equalizing Educational Expenditures

Traditionally, public schools in the United States have been funded with local property tax revenues. This method of financing, however, has an inherent equity problem. High property wealth school districts are able to set relatively low tax rates to generate sufficient levels of school funding while low property wealth areas must set high property tax rates to generate comparable levels of funding. Low property wealth areas are more likely to have citizens with a lower ability to pay property taxes and high proportions of students in poverty. Hence, raising sufficient levels of funding in low property wealth districts proves problematic. Given that raising revenues for schools is easier in high property wealth districts, when schools are financed with local property taxes school funding is highly correlated with property wealth. Historically, a variety of methods have been used in an attempt to equalize the resources available

to students in school districts with varying levels of per-pupil property wealth. States may to support educational spending through inter-governmental grants exploiting the equalization opportunities afforded under a system of fiscal federalism.

State intervention in local public financing of education began in the early 1900s when a number of states began giving flat per-pupil grants to local districts (Reschovsky, 1994). Since then, states have continued to wrestle with the issue of how to achieve "equal educational opportunity," a phrase that appears frequently in legal rulings regarding local public school finance. Over time, per-pupil flat grants evolved into what is now known as foundation aid.

2.1.1 Foundation Aid

A foundation aid formula, either by itself or in conjunction with another state aid formula, is currently used by forty-one states (Huang, 2004).[1] The basic foundation formula uses per-pupil grants to ensure a minimum spending level per-pupil. The foundation formula determines the size of the block grant that will be received by district i. Generally, the formula reduces the grant for wealthier districts and includes a cost adjustment for districts with higher needs. A generic foundation formula for a foundation aid grant may take the form

$$G_i = F(1 + C_i) - t^* \cdot V_i$$

where G_i is the per-pupil grant to district i, F is the basic grant per-pupil (the foundation level), C_i is the cost index for district i, t^* is the property tax rate (or tax rate required to receive aid), and V_i is the per-pupil property tax base in district i.

[1]The states that do not use foundation aid formula are Delaware, Indiana, Missouri, Wisconsin, Hawaii, North Carolina, Pennsylvania, Rhode Island, and Washington.

The grant increases with the cost index and decreases with respect to the per-pupil property tax base. Foundation aid becomes zero once a districts ability to raise tax revenues is sufficient to cover its cost.[2] Algebraically,

$$G_i = 0 \text{ if } F(1 + C_i) \leq t^* \cdot V_i.$$

Foundation aid can be seen a filling the gap between what districts are able to raise on their own and what has been determined to be the necessary level of spending to provide the public service in question. In this sense, foundation aid is useful for guaranteeing that all districts meet some minimum level of spending. However, foundation aid is unlikely to be successful at equalizing spending across districts. In order for foundation aid reduce and/or eliminate spending disparities across districts, a couple of conditions must hold.

First, districts must be prevented from spending either more than or less than the foundation level (Downes and Pogue, 1994). If districts with a greater preference for schooling are allowed to spend more, inequality will persist. Districts can choose a tax rate that is greater than t^* in order to achieve high spending levels. To the extent that higher per-pupil property wealth districts choose to raise additional revenues, school spending will continue to be correlated with district property wealth under foundation aid programs (Picus, Goertz, and Odden, 2008).

Second, if districts are able to shift some of their own-source revenue from education and use the revenue to finance other fiscal expenditures, spending on education may increase by less than the amount of the grant. In this sense, foundation aid

[2]Recapture, or negative foundation aid, is one of the unique features of Act 60 and is covered in detail below. Most states that rely on foundation aid to not include recapture for districts that raise more revenue locally than is needed to meet the foundation level.

may serve to "crowd out" existing local spending on education. If foundation aid is provided to school districts as a lump-sum grant, school districts view the foundation aid as additional income. How much the foundation aid actually increases spending on schools depends on the income elasticity of demand for educational expenditures. The demand for education expenditures is relatively income inelastic, with estimated elasticities generally falling in the 0.4 to 0.65 range (Fisher and Papke, 2000). Therefore, foundation aid is predicted to increase local spending by less than the grant's full amount.[3] Duncombe and Yinger (1998) estimate that an additional $1 of unrestricted aid would lead to a $0.33 increase in spending. More recent work has found that local school spending on educational instruction decreases in the years following increases in Title I funding (Gordon, 2004). To the extent that income elasticities vary across districts, foundation aid is not guaranteed to increase spending in low spending districts. While the foundation aid does guarantee a minimum level of spending, in the absence of regulations requiring maintenance of local spending efforts or local tax rates, local contributions to the financing of local education are expected to fall.

2.1.2 Guaranteed Tax Base / District Power Equalization

Guaranteed tax base (GTB) grants, also known as district power equalizing (DPE) grants, are intended to equalize the property tax base across pupils, rather than provide some minimum level of resources. With a GTB grant, any district levying the same tax rate as another district will have the same revenue raising capacity. Spending will differ across communities with different tax rates, but the ability of low property wealth districts to raise revenue will be enhanced. A general formula for

[3]There is evidence that intergovernmental grants tends to increase spending more than would be predicted via an income effect. This phenomena, dubbed the "flypaper effect", has been reviewed by Hines and Thaler (1995).

GTB grants is

$$G_i = F(V^* - V_i)t_i$$

where G_i is the per-pupil grant to district i, F is the foundation grant, V^* is the guaranteed tax base per-pupil, V_i is the tax base per-pupil, and t_i is the property tax rate in district i. Under a guaranteed tax base program, the state sets V^* and allows districts to choose their own tax rates. Such a program does not ensure equity in spending across districts, but gives districts the same opportunity to raise funds should a higher tax rate be chosen. Hence, GTB systems have been said to achieve taxpayer equity, as similar tax rates lead to similar levels of spending (Reschovsky, 1994).

In a pure GTB formula, $F = 0$. The district will receive aid if their tax base, V_i, is less than the guaranteed tax base, V^*. Under a pure GTB formula, districts with high wealth would be required to return property tax revenues in excess of what would have been raised under the guaranteed base given their chosen tax rate to the state. In practice, however, most states that use GTB systems do not require districts to return funds raised by taxing local property that exceeds V^*. As is discussed below, Vermont is an exception.[4]

While foundation aid is analogous to a block grant for education spending, a GTB system is more like a matching grant. For low property wealth towns, the GTB system reduces the price of education spending. A reduced price of education spending is likely to induce the town to spend more, and the lower price also has income effects further boosting education spending. However, the elasticity of demand for education

[4]Texas's system of school finance also has some elements of recapture, where districts with per-pupil property value in excess of $300,000 (in 2001 - 02) were required to share their excess revenues with less fortunate districts. The property wealthy districts were given some discretion as to how these shared funds were spent (Imazeki and Reschovsky, 2004).

spending has been estimated to be relatively inelastic (-0.11 to -0.57).[5] When the price of education falls, districts are predicted to spend more, but are also predicted to use some of the funds received as a grant under the GTB to fund spending in other areas or provide tax relief (Fisher and Papke, 2000).

Guaranteed tax base systems are attractive in that they appeal to notions of fiscal neutrality. In the 1970s, school districts with low property wealth tended to have high tax rates and relatively low per-pupil spending. As a number of states adopted GTB programs low property wealth towns saw taxes fall and spending increase. However, differences in per-pupil spending did not vanish. Instead, under a GTB system, high property wealth towns tended to have higher property tax rates, and therefore higher spending (Picus, Goertz, and Odden, 2008).

The fact that higher wealth districts tend to choose higher property tax rates to generate higher levels of per-pupil spending is evidence that GTB systems are not fully successful in generating wealth neutrality. Wealth neutrality is achieved when per-pupil spending is uncorrelated with school district wealth. If property wealthy districts have a greater preference for educational spending, then a GTB system will not successfully achieve wealth neutrality (Duncombe and Yinger, 1998). Again, wealth neutrality does not necessitate that all districts have the same level of spending per-pupil, but rather requires that spending not be correlated with per-pupil property wealth. Hence, it is apparent that neither foundation aid nor GTB schemes are expected to be fully successful in generating wealth neutrality.

[5]Brasington (2002) estimated the elasticity of demand for education spending to be between -0.11 and -0.17. Bergstrom, Rubinfeld, and Shapiro (1982) estimated the elasticity of demand to be between -0.39 and -0.57.

2.2 Vermont's Experience

Prior to 1997 Vermont school finance system was one based on foundation aid. The state applied the following foundation formula which provided state aid to low property wealth towns:

$$G_i = (F \cdot N_i) - (t_f \cdot V_i \cdot 0.01).$$

In this equation, G_i is the state grant being given to district i, N_i is the number of weighted pupils in district i, F is the foundation amount or the level that the state wants to ensure that each district has, t_f is the minimum tax rate that must be maintained by towns in order to be eligible for foundation aid, and V_i is the fair market value of property in district i. Under this formula the state's share in educational expenditures fluctuated between 20% to 37% (Downes, 2004). The state share of educational expenditures in Vermont was below the national average, which has remained above 47% since the 1970s (Corcoran and Evans, 2008).

As is typically the case for states using foundation aid, property wealthy districts in Vermont were able to spend substantially more, while taxing at lower rates, on their local public schools than property poor districts. For example, two towns that were each spending $6,200 per-pupil were found to have tax rates of $1.08 and $1.53 per $100 assessed market value. Another two towns each spending $6,600 per-pupil had tax rates of $0.83 and $1.79 per $100 in assessed value.[6]

The fact that for two towns spending the same on local public schools, one town was able to raise that revenue by levying a property tax rate half that of the other town was one of the motivating factors behind the *Brigham v. State* case. In this case, heard before the Vermont Supreme Court, the plaintiffs alleged that a system of

[6]These figures come from the Vermont Department of Education's 2001 report "The Equal Educational Opportunity Act: Measuring Equity" available from the Vermont Department of Education.

school funding that made it more difficult for property poor towns to raise revenues for schools violated the state's constitution (Teachout, 1997). Specifically, the charge was that the system violated the state constitution's equal protection clause by failing to provide equal educational opportunity to all the states students. The court agreed, ultimately ruling that a system of education funding should not be a function of district wealth. The court did not explicitly call for equality in school funds, but rather made the point that in order to equal opportunity to exist towns that levy similar tax rates should be able to raise similar funds for local public educational expenditures. This ruling was followed closely by the passage of Vermont's Equal Educational Opportunity Act, Act 60. The act was signed into law on June 26, 1997 and phased in over the next four years.

Act 60 incorporated elements of both foundation aid and GTB school finance schemes. During the 2000-01 school year, the foundation level was about $5,200. This was set to increase over time. In order to receive the foundation grant, all towns were required to levy a tax rate of $1.10 per $100 of assessed value. Each town in Vermont received state aid according to

$$G_i = F(V^* - V_i)t^*.$$

Unlike most other states that use a GTB system Vermont did not set $G_i = 0$ when $V_i > V^*$. Instead, when the the per-pupil property wealth exceeded V^*, the state recaptured this money and sent in to the states sharing pool.

Any town that wanted to raise additional revenues beyond the $5,200 foundation grant, F, could do so by increasing their tax rate. The additional tax revenues, however, did not remain in the town. Instead, these revenues also went into the states

sharing pool.[7] Once all these revenues were collected in the sharing pool, revenues were redistributed on a tax effort basis. Each town that was taxing at the same rate would have the same level of spending. The result of the sharing pool was that high property wealth towns experienced what amounts to a negative matching rate for local education spending. The tax price of education increased dramatically in high property wealth towns, and for 125 of Vermont's 253 towns assessed in Schmidt and Scott (2006), the tax price of education exceed $1, meaning that the town needed to raise more than $1 of local revenues in order to increase education spending by $1. For property poor towns, the tax price of education fell, as education expenditures were subsidized by revenues from the state's sharing pool. Property poor towns could increase tax rates and generate more than $1 in revenues for school spending for each $1 of tax revenue raised.

The adoption of Act 60 resulted in dramatic changes in the levels of school spending and property tax rates across Vermont towns.[8] Figure (2.1) illustrates the changes in spending between FY 1996, the year before Act 60 was passed, and FY 2003, the year before the legislation was superseded by Act 68. The five categories for percentage change in real spending were determined as quantiles. Hence, there are an equal number of towns in each group. While most towns saw real spending increase over the time period, 20% of Vermont towns has lower levels of spending in 2003, post Act 60, than were had in 1996. In the figure, the darker towns experienced larger decreases in spending.

While the primary goal of Act 60 was to eliminate the correlation between property wealth and school spending, a secondary goal was to provide property tax relief. Once

[7] See Chapter 3 for an algebraic exposition of how the state's sharing pool was structured.
[8] Vermont towns and school districts are coterminous.

Act 60 was adopted, low property wealth towns were able to reduce their property tax rates without experiencing a reduction in school spending. As shown in Figure (2.2), 20% of towns in Vermont saw tax rates fall by more than 10% under Act 60. On the other hand, property wealthy towns who wanted to maintain high levels of spending on education would have to do so by raising taxes.[9] While most towns did experience moderate increases in property tax rates over the period, 20% of Vermont towns saw property tax rates increase by more than 30%. In Figure (2.2) the darker towns indicate greater tax increases. Looking at Figure (2.1) and Figure (2.2) together, towns that appear dark on both maps are the towns that experienced large tax increases while seeing spending fall under Act 60.

2.3 Measuring Equity

Equity concepts are often separated into those of horizontal and vertical equity. Horizontal equity evaluates how similar students are treated relative to one another (Downes and Stiefel, 2008). Comparisons of spending across districts evaluate horizontal equity. Vertical equity evaluates how students who are different, suffer an educational disadvantage such as coming from poverty or speaking English as a second language, are treated. Since it is known that some students are more costly to educate, vertical equity evaluates whether there are sufficient additional funds to compensate for the known differences in cost to educate. The measure of spending used here, local spending per equalized pupil, adjusts spending to reflect known differences

[9]For towns with tax prices of education well over $1 (there were a handful of towns with a tax price for education in excess of $5 under Act 60) it became practically impossible to raise tax rates enough to restore previous spending levels.

in anticipated costs to educate.[10]

Table (2.1) presents the summary statistics for local spending per equalized pupil for each year from 1996 through 2004 in real terms. The second panel of Table (2.1) presents commonly reported equity statistics. Local spending per equalized pupil is reported at the town level and is the district budget funded by the general state support grant, local education tax revenues, and any aid from the sharing pool.[11] Local spending per equalized pupil is the measure of spending most commonly reported by the Vermont Department of Education is their literature on school spending equity.

Berne and Stiefel (1984) established a framework for evaluating equity in a state's school finance structure. The equity statistics presented in Table (2.1) evaluate the horizontal equity of school financing in Vermont. In real terms, mean (average) spending increases in every year except 1999 and 2000, while Act 60 was being phased-in.[12] From 1999 to 2000, while average spending was falling median spending increased, suggesting that the fall in average spending was due to reduced spending by some previously very high spending towns. Maximum spending under Act 60 was lower than maximum spending prior to the legislation's adoption. Minimum spending also tended to increase under the Act 60. As the maximum decreased and the minimum increased, the range of spending fell along with the standard deviation in spending.

While Table (2.1) presents simple means, medians, ranges, and standard deviations, using these summary statistics alone does not provide a very complete analysis.

[10]The weights for per-pupil spending adjustments are taken as given. It is beyond the scope of this work to evaluate whether these weights actually reflect the differences in cost to educate across Vermont pupils.

[11]Local spending per equalized pupil does not include federal aid, money coming from a previous surplus, or privately donated dollars. Later in the analysis private donations are incorporated.

[12]Years in the table are fiscal years ending in the summer of the listed year. Spending in each fiscal year corresponds to the previous school year. For example, year 2000 corresponds to spending during the 1999-2000 school year.

The range could be driven by outliers in the data.[13] To address the fact that outliers could be driving the range a restricted range is presented. The restricted range compares an observation at the 95th percentile to one at the 5th percentile. The restricted range is generally less than half of the range itself, and decreased under Act 60.

A problem with the restricted range, like the range, is that only it only looks at points in the distribution. Ranges cannot account for clustering at different points in the distribution, a phenomenon that certainly affects the equity faced in the entire system. Additionally, the restricted range still increases when there is inflation even if the relative difference in real resources has not changed (Odden and Picus, 2008). Since only real values are reported here, inflation is not an issue.

The bottom panel is Table (2.1) presents four commonly used equity indices that are insensitive to inflation. The federal range ratio is the restricted range divided by the observation at the 5th percentile. A lower federal range ratio indicates greater equality. While the federal range ratio did decrease as Act 60 was implemented, it quickly returned to pre-Act 60 levels. The federal range ratio also suffers from the problem that it only looks at two points within the distribution, and does not account for potential clustering at various points throughout the distribution.

The coefficient of variation is the standard deviation divided by the mean. The coefficient of variation measures dispersion around the mean. A coefficient of variation of zero indicates that resources are uniformly distributed across all units of observation. A coefficient of variation of 0.2 indicates that two-thirds of all observations have spending within 20% of the mean, and 95% of all observations have spending

[13]Vermont has a number of small towns with very few pupils. These children attend schools in other towns and spending per-pupil measured at the town level is not reflective of the quality of education the child receives.

within 40% of the mean. A pointed out by Murray, Evans, and Schwab (1998), one problem with the coefficient of variation as a measure of equaity is that it is sensative to extreme values in the distribution. The coefficient of variation does not reflect enhanced equity when money is transferred from rich districts to poor districts rather than middle income districts to poor districts.

The coefficient of variation is lower under Act 60 than it was prior to the adoption of this legislation. However, even at its low of 0.14 it is still higher than the commonly used benchmark level of 0.10 (Odden and Picus, 2008). The magnitude of change is similar to change experienced in Michigan under Proposal A, where Michigan saw the coefficient of variation fall from 0.19 before the proposal's adoption to 0.15 after the adoption of the proposal (Roy, 2004). Neither Vermont nor Michigan was able to reduce the coefficient of variation by as much as Kentucky under the Kentucky Education Reform Act, where the coefficient of variation fell from 0.16 in 1990 to 0.11 in 2000.[14]

The Gini coefficient is a measure of inequality frequently used to analyze income inequality. A Gini coefficient is constructed using a Lorenz curve. The Lorenz curve is derived by first ranking school districts according to per-pupil expenditures. The curve then plots the percentage of districts included against the cumulative percentage of expenditures. If resources were distributed equally, a plot of the percentage of districts included against the cumulative percentage of expenditures would be a 45-degree line. The area between the Lorenz curve and the 45-degree line is the Gini coefficient. A Gini coefficient closer to 1 is less equal while a Gini coefficient of 0 indicates perfect equity (Odden and Picus, 2008). Most Gini coefficients in school finance are in the 0.10 - 0.20 range, with 0.05 regarded as a good target.

[14]The coefficient of variation in Kentucky was computed by referring to Table 2 in Clark (2003).

The Gini coefficient was approximately 0.10 prior to the adoption of Act 60 in Vermont, and fluctuated around 0.08 after Act 60 was fully phased-in, but did creep back up by the end of the Act 60 period. Michigan's Gini coefficients before and after Proposal A were very similar to those in Vermont (Roy, 2004). Again, Gini coefficients in Kentucky were lower pre and post reform (0.08 and 0.06, respectively) (Clark, 2003).

The coefficient of variation and the Gini coefficient tend to show similar patterns of variation and therefore do not need to be reported together (Downes and Stiefel, 2008).[15] The fourth measure of variation presented, the McCloone index, does not always exhibit patterns similar to the other equity measures presented. The McLoone index measures the distribution for districts with spending per-pupil below the median level. The McLoone index is highly popular since much of the concern surrounding equity in school finance focuses on the bottom half of the distribution. The McLoone index is calculated as the ratio of the sum of spending at all districts below the median to the median times the number of districts below the median. The McLoone index reports how much schools below the median are spending relative to what they would be spending if they were at the median. Generally, the McLoone index is in the 0.7 to 0.9 range, with a good McLoone index being about 0.95 (Odden and Picus, 2008). Unlike the other measures of equity under Act 60, the McCloone index did not improve (a higher McCloone index indicates that there is more equity for schools with below median spending). If anything, it appears to have decreased slightly. Act 60 did not substantially increase spending in the bottom half of the distribution relative to the median level.

[15]Both measures have been reported here as there is comparable data based on other states with recent school finance reforms.

Another issue when evaluating the equity of school finance reform is taxpayer equity. The school finance literature defines taxpayer equity as a situation where a set tax rate raises the same amount of revenue per-pupil, regardless of location (Figlio, 2004). Given this definition of taxpayer equity, Act 60 was highly successful. However, Act 60 was not viewed as equitable by many taxpayers in Vermont. Specifically, those in property wealthy towns did not view the sharing pool as fair - believing that resources raised locally should be spent to finance local public schools.

While these equity statistics are useful for evaluating school finance reform, and provide a good benchmark for comparing reforms undertaken in Vermont to those undertaken in other states, it is important to remember that the court in Vermont did not require equity in per-pupil spending. Instead, the court requested that per-pupil spending not be a function of district property wealth. The next section evaluates how well Act 60 reduced the relationship between per-pupil spending and district property wealth.

2.3.1 School Spending's Relationship to Income and Property Wealth

In order to evaluate whether the reforms in Vermont were successful in reducing the relationship between school spending and property wealth, methods similar to those employed by Downes (1992), Clark (2003), Roy (2004), and Downes (2004) are utilized. In order to evaluate whether Vermont's reform successfully reduced the relationship between per-pupil spending and property wealth the relationship between the two is plotted and along with a simple regression line. Specifically, the following

regression is run for each of four years: 1996, 1998, 2000, and 2002.

$$\text{Spend}_i = \alpha + \beta \text{Property Values}_i + \epsilon_i. \tag{2.1}$$

Figure (2.3) presents the scatterplot illustrations as well as the results of the regressions. In Figure (2.3), the measure of spending is local spending per equalized pupil and the measure of property values is the median value of housing in the year 2000.[16] The diameters of the circles in the figure are proportional to average daily membership by town.[17] The regression line is estimated using weighted ordinary least squares, where average daily membership is the weight. The t-statistics associated with the coefficient estimates are given in parenthesis.

Figure (2.3) illustrates that the correlation between local spending per equalized pupil and median property values clearly fell between 1996 (the year before Act 60's adoption) and 2002 (the year after Act 60 was fully phased in). The magnitude and significance of the coefficient on the property value term, β fell. By 2002, there appeared to have been almost no correlation between spending and property values. As such, it would appear that Act 60 was successful in achieving this objective.

One problem with relying on Figure (2.3) to evaluate whether Act 60 did in fact eliminate the relationship between spending and property wealth stems from the fact that Act 60 created incentives for towns to give large amounts in private donations (rather than raise tax rates). Giving money via private donations allowed towns

[16]The median value of housing is respondents estimates regarding how much their property is worth. This measure excludes mobile homes, property on ten or more acres of land, multi-family properties, and properties with a business. This data is available from the U.S. Census Bureau. Results are similar when the analysis using the median value of all owner occupied housing, rather than specified housing.

[17]Average daily membership is the number of students who receive an education at the public expense in each town. The data is available from the Vermont Department of Education.

to avoid the sharing pool. Hence, many towns with a high tax price for education started using privately donated dollars to finance their local public schools.[18] Since private donations are not counted in local spending per equalized pupil Figure (2.3) may overstate the degree to which the relationship between spending and property values was reduced.[19]

Figure (2.4) shows the evolution of the relationship between spending when private donations are included and median property values. Note that the top two graphs in Figure (2.4) are identical to those in Figure (2.3). There was not a major incentive for private donations prior to Act 60 and such donations were not a major part of public school financing. While the correlation between spending and property values still fell when private donations are included, the magnitude of the reduction in the coefficient on spending (β) and in the coefficient's significance is reduced. When private expenditures were not included, there were a number of high property wealth towns that appeared to be spending only the foundation aid provided by levying the required foundation rate (just above $5,000). Figure (2.4) reveals that these towns were setting a low tax rate and had low levels of local spending per equalized pupil, but were using private donations to increase spending without having to share revenues with property poor towns.

Instead of looking at the relationship between spending and property values, Clark (2003) and Roy (2004) estimate Equation (2.1) using income as the independent variable. Doing so measures what is referred to as the income gradient. Figure (2.5) presents the estimates of the income gradients when spending is measured without private donations. Income is measured as the median household income in 1999.[20]

[18]Downes and Steinman (2008) provide an overview of the private donations induced by Act 60.
[19]Data on private donations were provided by the Vermont Department of Education upon request.
[20]Median household income by town for 1999 is available from the 2000 U.S. Census.

Again, there is a marked reduction in the correlation between spending and income after Act 60 was adopted. However, under Act 60 the relationship between income and spending appears to be slightly stronger than the relationship between income and wealth. Act 60 was explicitly designed to break the relationship between spending and property wealth. The legislation would only be expected to break the relationship between spending and income to the degree that property wealth and income are correlated. In each year in of the sample period, the correlation between the median value of specified housing and the median household income was approximately 0.69.[21] The results here are similar to those found by Roy (2004), where the income gradient is significantly reduced under reform, but is not eliminated.[22] Clark (2003) found that the income gradient actually became negative after school finance reform in Kentucky.

Figure (2.6) looks at the income gradient when spending includes private donations. Here again, the diagrams for 1996 and 1998 are unchanged as private donations were not a large component of school funding prior to Act 60. When private donations are included, the reduction in the income gradient effectively disappears. Some of the seemingly low spending high income towns were actually not low spending as private donations were being used to compensate for revenues forgone under the state financing scheme.

Figure (2.7) looks at the evolution of the relationship between spending and property tax rates as Act 60 was implemented. Prior to Act 60, there was no positive relationship between school tax rates and local spending per equalized pupil.[23] By

[21]Fischel (2001) argues that equating property poor districts with low income people creates poor policies since wealthy districts containing poor students may lose resources for schools. His discussion implies that the correlation between income and property wealth is much less than what has been found here.

[22]Roy (2004) found that the income gradient is reduced in the weighted estimates and eliminated in the unweighted regression.

[23]School tax rates, rather than the total property tax rate town residents faced, is used here. The

1999, as Act 60 was phasing in, a positive relationship between school tax rates and local spending per equalized pupil began to emerge. By 2000, this relationship was quite strong.[24] The bottom two graphs in Figure (2.7) show the relationship between spending and the school tax rate when private donations are included under Act 60. There are a number of towns that are able to set relatively low tax rates and generate high levels of spending using private donations rather than by setting high tax rates and participating in the state's sharing pool.

2.3.2 Regression Results

As a more formal evaluation of the evolution of the relationship between spending and property wealth and spending and income Tables (2.3) - (2.8) provide the results of estimating Equation (2.2).

$$\begin{aligned}\text{Spend}_{it} = \alpha_0 + \alpha_1 \text{Y98} + \alpha_2 \text{Y00} + \alpha_3 \text{Y02} + \beta_0 \text{Property Values}_{it} \\ + \beta_1(\text{Y98} \cdot \text{Property Values}_i) + \beta_2(\text{Y00} \cdot \text{Property Values}_i) \\ + \beta_3(\text{Y02} \cdot \text{Property Values}_i) + \gamma_0 X_{it} + \epsilon_{it}. \qquad (2.2)\end{aligned}$$

Spending is defined as local spending per equalized pupil in Tables (2.3), (2.5), and (2.7) and local spending per equalized pupil plus private donations in Tables (2.4), (2.6), and (2.8). Tables (2.3) and (2.4) use the median value of housing as the measure of property wealth. Tables (2.5) and (2.6) use the equalized education grand per-

tax rate is an effective tax rate.

[24]Given the structure of Act 60, one would expect the relationship between spending and school tax rates to be perfectly linear. The scatter is due to the fact that the measure of spending being utilized is local spending per equalized pupil, so that towns with a large proportion of students in poverty, with high numbers of English as a second language students, and with a disproportionate amount of secondary school students receive more money per-pupil.

pupil list and Tables (2.7) and (2.8) use aggregate property values per-pupil, which will be defined and discussed below. $Y98$, $Y00$, and $Y02$ are year dummies, where the omitted category is 1996, the year before Act 60 was adopted.

The vector X_{it} contains a set of control variables. The resident ownership ratio is included to explore whether towns with a larger proportion of vacation property spend more or less per-pupil.[25] The resident ownership ratio is interacted with the year dummies to explore how the relationship changes over time. Dummy variables are included for towns that tuition high school students and for those towns that are members of a union district.[26] School size and population density are also included as control variables.[27]

Table (2.2) contains the summary statistics for the variables used in the regression analysis (other than local spending per equalized pupil, which is in Table (2.1)). A number of the variables are constant as they were only available at one point in time. Equalized education grand lists, aggregate property values, resident ownership ratios, and average daily membership data was available annually.

The first column of Tables (2.3) - (2.8) estimates Equation (2.2) without the control variables, X_{it}. The second column of Tables (2.3) - (2.8) estimates Equation (2.2), using median household income rather than property values, and does not include the control variables in X_{it}. The third column of (2.3) - (2.8) includes both

[25]The resident ownership ratio is derived by dividing the assessed value of all owner occupied property by the total assessed value of property for each town.

[26]Towns that do not have their own high schools and are not members of a union school district tuition high school students to a school of the students choosing. This introduces an element of choice into Vermont's school system. The concept of tuitioned high school students is discussed in more detail in Chapter 4. Union school districts are where a number of towns share a single high school. While most towns do operate their own elementary schools, these schools are small and there are advantages to having larger schools at the secondary level. Overall, 121 out of 251 towns in the state have union high schools, while 66 tuition their high school students.

[27]Data on population density was obtained from the Center for Rural Studies at the University of Vermont: http://crs.uvm.edu/.

the measure of property wealth, income, and the control variables. Columns four, five, and six are the same as the first three, except Equation (2.2) is estimated using weighted least squares, where average daily membership is the weight.

Table (2.3) again shows that there was a strong and positive relationship between property wealth and spending as well as between income and spending prior to reform. Each $1000 increase in the median value of housing was associated with a $24 increase in per-pupil spending. Each $1000 increase in income was associated with a $59 increase in per-pupil spending. The relationship between the median value of housing and per-pupil spending was eliminated following reform, and the magnitude of the income gradient was reduced. The third column of Table (2.3) shows demonstrates that the magnitude of the relationship between spending and income as well as between spending and property wealth is reduced (but remains positive and significant pre-reform) when both measures, and other controls, are included.

The coefficients on the resident ownership ratio and the interaction of this variable with the year dummies reveals an interesting trend. Prior to Act 60, resident ownership ratios were negatively related to per-pupil spending. This observation is quite robust and holds across all specifications using different measures of property wealth. Towns with low resident ownership ratios tend to be towns with a high proportion of vacation properties, which also tend to be property rich towns and have high property wealth per-pupil. Since it was relatively easy for these towns to raise revenue (low tax rates would generate large amounts of revenue) prior to Act 60, the towns with a large number of non-town or non-state residents spent more on education per-pupil.[28] Once Act 60 was implemented, these towns were forced to increase taxes and reduce spending. Fischel (2001) argues that the presence of nonresidential

[28]Many of these towns in Vermont are the home to major ski resorts.

property makes spending on schools appear cheaper than it actually is, so residents respond by spending too much. When Act 60 increased the tax price of education in the high property wealth districts, which were also the districts with low resident ownership ratios, spending decreased in towns with high proportions of non-resident property. Hence, under Act 60 the relationship between resident ownership ratios and per-pupil spending became positive.[29]

In Table (2.4), local spending per equalized pupil plus private donations is the dependent variable. While the relationship between spending and property wealth as well as the relationship between spending and median household income is reduced under Act 60, the magnitude of the reduction lessens when private donations are included. Otherwise, the general patterns in signs and significance of coefficients remain the same.

Tables (2.5) and (2.6) used the equalized education grand list as a measure of property wealth. The equalized education grand list is determined each year via Vermont's Equalization Study.[30] The purpose of the study is to remove assessment bias, and identify the fair market values of all taxable property. The equalized education grand list is the fair market value of properties in a town and represent the towns property wealth. Since the adoption of Act 60, the equalized education grand list has been used to determine state aid and sharing. There are a couple of benefits to

[29]Downes (2004) outlines how the provisions of Act 60, specifically the circuit-breaker feature and power equalization scheme, shift the property tax burden to nonresidential property owners. Towns that saw the greatest increases in tax rates tended to be those with lower resident ownership ratios. Since taxes were capped at 2% of income on the first $75,000 for residents, low income residents of property wealthy towns were protected from Act 60's tax increases. While taxes went up in these towns, much of the additional revenue was used to subsidize spending in property poor towns, or towns with higher resident ownership ratios.

[30]The Vermont Department of Taxes reports data on the equalized education grand list as part of the annual Property Valuation & Review annual report. Detailed information on the equalization procedure is also available from the Vermont Department of Taxes.

using the equalized education grand list as opposed to the median value of housing to examine changes in equity. First, the equalized education grand list is the actual measure of a town's ability to generate revenue. Second, the equalized education grand list is measured annually and is allowed to change over time (the median value of housing data was only available at one point in time, 2000). Since data on the equalized education grand list is not available prior to 1997, there are only three years of data used in the regression analysis.

The relationship between local spending per equalized pupil and the equalized education grand list went from being positive and significant to negative and significant as Act 60 went into effect (see Table (2.5)).[31] When private donations are included (Table (2.6)), the reduction in the relationship between spending and property values in less pronounced.

Since data on the equalized education grand list is not available in 1996, Equation (2.2) is estimated using aggregate property values.[32] Aggregate property values are not adjusted for potential assessment bias. Table (2.7) shows that the relationship between total property wealth and local spending per equalized pupil is eliminated under Act 60. However, once again when private donations are included the relationship between property wealth and spending is not fully eliminated.

[31]Downes (2004) regresses local spending on equalized pupil on the equalized assessed value per-pupil under Act 60 and finds that there is a negative relationship. He also finds that the relationship between median family income and spending remains positive.

[32]Aggregate property values were obtained from the Vermont Department of Taxes Property Valuation & Review annual reports.

2.4 Conclusion

Vermont's Act 60 represents one of the most drastic attempts at school finance equalization to have been undertaken in the United States. While the explicit goal of the reform was to eliminate the relationship between district property wealth and school spending, some equalization in school spending was also an implicit goal. The equity statistics presented here suggest that Act 60 did equalize spending across towns. However, this equity does not appear to be coming from the lower half of the distribution. Rather, the equity seems to be driven by high wealth towns reducing their spending in response to Vermont's property tax revenue sharing reform.

When drafting Act 60, the legislature had been instructed to devise a system of school finance that would give students equal access to educational funds, and not make school spending a function of the property wealth of the town in which the student resided. On this count, the reform was relatively successful. Using various measures of district property wealth, this chapter demonstrated that under Act 60, local spending per equalized pupil was no longer positively related to property wealth. The positive relationship between spending and income was reduced, but not fully eliminated.

The reform is said to have only been relatively successful in reducing the relationship between spending and property wealth due to the private donations that towns began to use to fund local public schools following the adoption of Act 60. Residents of a number of property wealthy towns were opposed to sharing locally raised property tax revenues with other towns. To avoid being forced to share these revenues, residents raised private contributions to fund local public schools, as private contributions were not subject to sharing. Private contributes served to partially undo the

Act 60's reduction in the relationship between property wealth and spending.

Table 2.1: Equity Statistics

Equality in Local Spending Per Equalized Pupil, 1996 - 2004 (2000 Dollars)

	1996	1997	1998	1999	2000	2001	2002	2003	2004
Mean (2000 Dollars)	6,620.33	6,661.52	6,798.07	6,551.70	6,359.50	6,618.39	7,023.27	7,189.99	7,271.34
Median (2000 Dollars)	6,381.66	6,421.26	6,476.56	6,294.45	6,326.09	6,556.94	6,971.35	7,138.12	7,325.69
Maximum (2000 Dollars)	12,245.00	12,850.53	18,201.11	12,843.66	9,131.14	9,104.99	12,219.79	15,746.10	10,010.56
Minimum (2000 Dollars)	3,193.78	2,072.63	4,505.62	4,719.65	1,789.69	4,139.13	3,436.54	4,034.16	4,026.49
Range (2000 Dollars)	9,051.22	10,777.90	13,695.49	8,124.02	7,341.45	4,965.86	8,783.25	11,711.94	5,984.07
Standard Deviation	1138.06	1200.88	1410.74	1115.99	905.13	959.57	1109.44	1234.25	1148.11
Restricted Range	3347.247	3610.747	3872.169	3502.054	2889.77	3201.882	3621.525	4045.419	4058.482

Equity Indices for Local Spending per Equalized Pupil

	1996	1997	1998	1999	2000	2001	2002	2003	2004
Federal Range Ratio	0.6331	0.6939	0.7242	0.6761	0.5738	0.6388	0.6982	0.7855	0.7931
Coefficient of Variation	0.1719	0.1803	0.2075	0.1703	0.1423	0.1450	0.1580	0.1717	0.1579
Gini Coefficient	0.0917	0.0958	0.0997	0.0889	0.0782	0.0825	0.0869	0.0908	0.0886
McLoone Index	0.9060	0.8994	0.9063	0.9117	0.8946	0.8893	0.8843	0.8814	0.8694

Local spending per equalized pupil was measured at the town level. Calculations include 250 Vermont towns where data was available in all years. Dollar values were deflated into 2000 dollars using the Northeast Consumer Price Index (CPI - NE).

Table 2.2: Summary Statistics

Summary Statistics

	1996		1998		2000		2002	
	Mean	St. Dev	Mean	St. Dev	Mean	St. Dev	Mean	St. Dev
Private Spending per-pupil, 2000$					114.71	455.83	177.17	611.89
Median Value of Housing, 2000	105056.90	30474.59	105056.90	30474.59	105056.90	30474.59	105056.90	30474.59
Equalized Education Grand List per-pupil (100s), 2000$			5016.68	12565.12	4637.54	6288.14	5059.40	7656.51
Aggregate Property Values per-pupil (100s), 2000$	4305.40	8089.97	3885.50	5945.29	4410.23	6002.42	4696.84	7825.65
Median Household Income, 1999	40397.25	8326.20	40397.25	8326.20	40397.25	8326.20	40397.25	8326.20
Resident Ownership Ratio	58.67	17.53	59.79	17.41	60.61	16.98	61.52	16.59
Tuition High School Students	0.2629	0.4411	0.2629	0.4411	0.2629	0.4411	0.2629	0.4411
Union High School Students	0.4821	0.5007	0.4821	0.5007	0.4821	0.5007	0.4821	0.5007
Average Daily Membership	471.76	569.74	483.92	583.29	415.62	499.75	404.96	491.70
Population Density, 2000	139.10	497.00	139.10	497.00	139.10	497.00	139.10	497.00

Table 2.3: Spending: Median Value of Housing & Income Gradient

Dependent Variable: Local Spending per Equalized Pupil

	(1)	(2)	(3)	(4)	(5)	(6)
Median Value of Housing, 2000	0.0238		0.0185	0.0240		0.0212
	11.33		*6.72*	*13.52*		*8.59*
Median Value of Housing * 1998	0.0015		0.0000	-0.0024		-0.0041
	0.52		*0.00*	*-0.96*		*-1.21*
Median Value of Housing * 2000	-0.0182		-0.0165	-0.0162		-0.0191
	-6.12		*-4.23*	*-6.33*		*-5.55*
Median Value of Housing * 2002	-0.0217		-0.0208	-0.0196		-0.0240
	-7.32		*-5.30*	*-7.63*		*-6.78*
Median Household Income, 1999		0.0590	0.0276		0.0581	0.0118
		7.21	*2.63*		*10.10*	*1.48*
Median Household Income * 1998		-0.0039	0.0038		-0.0072	0.0055
		-0.34	*0.25*		*-0.90*	*0.51*
Median Household Income * 2000		-0.0211	-0.0021		-0.0256	0.0128
		-1.82	*-0.14*		*-3.09*	*1.18*
Median Household Income * 2002		-0.0264	0.0022		-0.0294	0.0192
		-2.28	*0.14*		*-3.53*	*1.70*
Resident Ownership Ratio			-25.69			-29.54
			-7.01			*-8.43*
Resident Ownership Ratio * 1998			-11.99			-4.04
			-2.32			*-0.82*
Resident Ownership Ratio * 2000			39.39			23.86
			7.59			*5.08*
Resident Ownership Ratio * 2002			40.97			31.74
			7.71			*6.16*
Tuition High School Students			-317.85			-197.98
			-3.89			*-2.58*
Union High School			72.71			177.87
			1.01			*3.24*
Average Daily Membership			-0.20			-0.13
			-2.94			*-3.90*
Population Density, 2000			-0.06			-0.07
			-0.85			*-2.13*
1998	-5.64	322.45	757.02	345.34	389.04	581.60
	-0.02	*0.67*	*1.72*	*1.21*	*1.14*	*1.47*
2000	1655.41	601.09	-777.84	1803.06	1075.66	22.08
	5.10	*1.26*	*-1.76*	*6.10*	*3.03*	*0.05*
2002	2660.50	1445.50	20.56	2751.06	1790.61	340.10
	8.20	*3.03*	*0.05*	*9.26*	*5.02*	*0.82*
Constant	4127.93	4234.29	5218.25	3816.07	4032.34	5657.46
	17.99	*12.54*	*16.77*	*18.81*	*16.53*	*19.83*
# Obs.	991.00	983.00	982.00	991.00	983.00	982.00
R^2	0.25	0.16	0.42	0.31	0.24	0.48

Columns (1), (2), and (3) present the unweighted estimates. Columns (4), (5), and (6) present the weighted estimates with average daily membership being the weight. The coefficient estimates are in plain text while the t-statistics are italicized.

Table 2.4: Spending plus Private Donations: Median Value of Housing & Income Gradient

Dependent Variable: Local Spending plus Private Donations

	(1)	(2)	(3)	(4)	(5)	(6)
Median Value of Housing, 2000	0.0238		0.0188	0.0240		0.0228
	11.82		*6.87*	*14.23*		*9.99*
Median Value of Housing * 1998	0.0028		0.0027	-0.0024		-0.0040
	0.98		*0.69*	*-1.00*		*-1.29*
Median Value of Housing * 2000	-0.0109		-0.0096	-0.0109		-0.0109
	-3.84		*-2.50*	*-4.47*		*-3.42*
Median Value of Housing * 2002	-0.0104		-0.0086	-0.0117		-0.0143
	-3.64		*-2.21*	*-4.77*		*-4.35*
Median Household Income, 1999		0.0590	0.0268		0.0581	0.0062
		7.34	*2.58*		*10.39*	*0.84*
Median Household Income * 1998		0.0003	-0.0008		-0.0072	0.0056
		0.02	*-0.06*		*-0.91*	*0.55*
Median Household Income * 2000		-0.0092	-0.0016		-0.0200	0.0011
		-0.81	*-0.11*		*-2.48*	*0.11*
Median Household Income * 2002		-0.0111	-0.0049		-0.0185	0.0113
		-0.98	*-0.33*		*-2.28*	*1.08*
Resident Ownership Ratio			-25.09			-28.08
			-6.91			*-8.64*
Resident Ownership Ratio * 1998			-7.48			-4.26
			-1.47			*-0.93*
Resident Ownership Ratio * 2000			29.35			16.98
			5.71			*3.90*
Resident Ownership Ratio * 2002			26.94			19.30
			5.12			*4.04*
Tuition High School Students			-296.36			-164.20
			-3.67			*-2.30*
Union High School			68.34			182.24
			0.96			*3.58*
Average Daily Membership			-0.23			-0.18
			-3.43			*-5.67*
Population Density, 2000			-0.08			-0.10
			-1.10			*-3.04*
1998	-157.96	127.37	365.81	341.92	385.80	588.05
	-0.51	*0.27*	*0.85*	*1.26*	*1.16*	*1.60*
2000	1009.82	237.53	-794.37	1279.13	919.73	125.71
	3.24	*0.51*	*-1.82*	*4.56*	*2.67*	*0.34*
2002	1645.79	1006.78	57.62	1980.16	1458.92	522.91
	5.29	*2.15*	*0.13*	*7.01*	*4.20*	*1.35*
Constant	4127.93	4234.29	5201.95	3816.07	4032.34	5667.10
	18.76	*12.77*	*16.87*	*19.80*	*17.00*	*21.42*
# Obs.	992.00	984.00	983.00	992.00	984.00	983.00
R^2	0.32	0.20	0.44	0.38	0.29	0.55

Columns (1), (2), and (3) present the unweighted estimates. Columns (4), (5), and (6) present the weighted estimates with average daily membership being the weight. The coefficient estimates are in plain text while the t-statistics are italicized.

Table 2.5: Spending: Education Grand List & Income Gradient

Dependent Variable: Local Spending per Equalized Pupil

	(1)	(2)	(3)	(4)	(5)	(6)
Equalized Education Grand List	0.0385		0.0184	0.0988		0.0365
	7.06		*3.65*	*7.33*		*2.95*
Equalized Education Grand List * 2000	-0.0695		-0.0452	-0.1067		-0.0968
	-5.71		*-3.41*	*-4.32*		*-3.97*
Equalized Education Grand List * 2002	-0.0708		-0.0423	-0.1367		-0.1036
	-6.75		*-3.83*	*-6.06*		*-4.50*
Median Household Income, 1999		0.0551	0.0804		0.0508	0.0582
		6.63	*10.53*		*9.12*	*11.07*
Median Household Income * 2000		-0.0171	-0.0437		-0.0184	-0.0234
		-1.46	*-4.02*		*-2.26*	*-3.10*
Median Houshold Income * 2002		-0.0225	-0.0513		-0.0222	-0.0263
		-1.92	*-4.71*		*-2.72*	*-3.39*
Resident Ownership Ratio			-42.88			-37.92
			-10.99			*-9.95*
Resident Ownership Ratio * 2000			48.35			25.32
			7.92			*4.66*
Resident Ownership Ratio * 2002			51.86			31.68
			8.47			*5.23*
Tuition High School Students			-443.07			-221.49
			-4.44			*-2.36*
Union High School			-3.41			164.35
			-0.04			*2.45*
Average Daily Membership			-0.15			-0.10
			-1.74			*-2.55*
Population Density, 2000			-0.06			-0.05
			-0.70			*-1.24*
2000	-71.36	278.64	-1334.04	270.44	686.62	-442.35
	-0.63	*0.58*	*-2.75*	*2.32*	*1.98*	*-1.01*
2002	580.61	1123.04	-624.05	954.69	1401.57	-133.69
	5.27	*2.32*	*-1.29*	*8.33*	*4.01*	*-0.29*
Constant	6587.27	4556.74	6205.22	6247.28	4421.37	6571.42
	89.37	*13.28*	*18.52*	*90.49*	*18.63*	*21.88*
# Obs.	752.00	737.00	737.00	752.00	737.00	737.00
R^2	*0.13*	*0.14*	*0.37*	*0.13*	*0.21*	*0.39*

Columns (1), (2), and (3) present the unweighted estimates. Columns (4), (5), and (6) present the weighted estimates with average daily membership being the weight. The coefficient estimates are in plain text while the t-statistics are italicized.

Table 2.6: Spending plus Private Donations: Education Grand List & Income Gradient

Dependent Variable: Local Spending plus Private Donations

	(1)	(2)	(3)	(4)	(5)	(6)
Equalized Education Grand List	0.0375		0.0190	0.0986		0.0362
	6.76		*3.71*	*7.52*		*3.09*
Equalized Education Grand List * 2000	-0.0298		-0.0185	-0.0235		-0.0145
	-2.40		*-1.37*	*-0.98*		*-0.63*
Equalized Education Grand List * 2002	-0.0256		-0.0151	-0.0400		-0.0201
	-2.40		*-1.34*	*-1.82*		*-0.92*
Median Household Income, 1999		0.0593	0.0832		0.0509	0.0580
		7.33	*10.71*		*9.49*	*11.64*
Median Household Income * 2000		-0.0095	-0.0324		-0.0129	-0.0220
		-0.83	*-2.93*		*-1.65*	*-3.08*
Median Houshold Income * 2002		-0.0114	-0.0331		-0.0113	-0.0194
		-0.99	*-2.99*		*-1.44*	*-2.64*
Resident Ownership Ratio			-38.92			-37.49
			-9.86			*-10.39*
Resident Ownership Ratio * 2000			37.87			25.02
			6.11			*4.87*
Resident Ownership Ratio * 2002			35.56			27.22
			5.72			*4.74*
Tuition High School Students			-354.80			-174.83
			-3.50			*-1.97*
Union High School			9.89			155.16
			0.11			*2.44*
Average Daily Membership			-0.13			-0.12
			-1.54			*-3.21*
Population Density, 2000			-0.04			-0.05
			-0.52			*-1.38*
2000	-113.36	110.16	-1133.17	52.68	533.93	-699.06
	-0.99	*0.23*	*-2.30*	*0.46*	*1.60*	*-1.68*
2002	557.21	879.41	-289.67	707.03	1073.12	-330.58
	4.97	*1.86*	*-0.59*	*6.34*	*3.19*	*-0.77*
Constant	6564.10	4361.66	5779.93	6247.49	4418.14	6576.12
	87.56	*13.07*	*17.13*	*93.07*	*19.35*	*23.14*
# Obs.	753.00	738.00	738.00	753.00	738.00	738.00
R^2	*0.12*	*0.20*	*0.36*	*0.18*	*0.27*	*0.46*

Columns (1), (2), and (3) present the unweighted estimates. Columns (4), (5), and (6) present the weighted estimates with average daily membership being the weight. The coefficient estimates are in plain text while the t-statistics are italicized.

Table 2.7: Spending: Total Property Value & Income Gradient

Dependent Variable: Local Spending per Equalized Pupil

	(1)	(2)	(3)	(4)	(5)	(6)
Aggregate Property Values	0.0470		0.0063	0.2068		0.1024
	5.90		*0.77*	*10.13*		*4.90*
Aggregate Property Values * 1998	0.0996		0.0873	0.0567		0.0480
	7.40		*6.02*	*1.89*		*1.51*
Aggregate Property Values * 2000	-0.0775		-0.0290	-0.2163		-0.1638
	-5.79		*-1.96*	*-7.47*		*-5.51*
Aggregate Property Values * 2002	-0.0770		-0.0253	-0.2450		-0.1601
	-6.72		*-2.08*	*-8.95*		*-5.63*
Median Household Income, 1999		0.0590	0.0776		0.0581	0.0572
		7.21	*10.38*		*10.10*	*10.72*
Median Household Income * 1998		-0.0039	-0.0135		-0.0072	-0.0081
		-0.34	*-1.27*		*-0.90*	*-1.08*
Median Household Income * 2000		-0.0211	-0.0426		-0.0256	-0.0223
		-1.82	*-4.04*		*-3.09*	*-2.99*
Median Household Income * 2002		-0.0264	-0.0508		-0.0294	-0.0271
		-2.28	*-4.84*		*-3.53*	*-3.56*
Resident Ownership Ratio			-33.05			-26.84
			-8.17			*-6.54*
Resident Ownership Ratio * 1998			5.27			1.46
			0.90			*0.25*
Resident Ownership Ratio * 2000			39.95			14.03
			6.61			*2.51*
Resident Ownership Ratio * 2002			43.78			22.82
			7.33			*3.73*
Tuition High School Students			-271.02			-172.72
			-3.26			*-2.21*
Union High School			83.22			167.68
			1.14			*3.00*
Average Daily Membership			-0.12			-0.07
			-1.79			*-1.97*
Population Density, 2000			-0.03			-0.04
			-0.38			*-1.04*
1998	-205.45	322.45	94.65	-43.91	389.04	212.76
	-1.94	*0.67*	*0.21*	*-0.38*	*1.14*	*0.49*
2000	88.31	601.09	-761.66	673.23	1075.66	540.16
	0.82	*1.26*	*-1.64*	*5.58*	*3.03*	*1.21*
2002	727.48	1445.50	-54.51	1340.07	1790.61	716.50
	6.95	*3.03*	*-0.12*	*11.39*	*5.02*	*1.55*
Constant	6417.87	4234.29	5487.95	5848.60	4032.34	5539.52
	88.01	*12.54*	*17.11*	*72.18*	*16.53*	*17.75*
# Obs.	1003.00	983.00	982.00	1003.00	983.00	982.00
R^2	0.23	0.16	0.41	0.25	0.24	0.46

Columns (1), (2), and (3) present the unweighted estimates. Columns (4), (5), and (6) present the weighted estimates with average daily membership being the weight. The coefficient estimates are in plain text while the t-statistics are italicized. Aggregate property values are measured in hundreds of dollars.

Table 2.8: Spending plus Private Donations: Total Property Value & Income Gradient

Dependent Variable: Local Spending plus Private Donations

	(1)	(2)	(3)	(4)	(5)	(6)
Aggregate Property Values	0.0470		0.0067	0.2068		0.1026
	5.80		*0.80*	*10.36*		*5.13*
Aggregate Property Values * 1998	0.0957		0.0892	0.0561		0.0472
	6.99		*6.09*	*1.91*		*1.55*
Aggregate Property Values * 2000	-0.0357		0.0009	-0.1298		-0.0772
	-2.62		*0.06*	*-4.58*		*-2.71*
Aggregate Property Values * 2002	-0.0369		-0.0025	-0.1503		-0.0834
	-3.16		*-0.20*	*-5.61*		*-3.06*
Median Household Income, 1999		0.0590	0.0774		0.0581	0.0569
		7.34	*10.24*		*10.39*	*11.15*
Median Household Income * 1998		0.0003	-0.0108		-0.0072	-0.0079
		0.02	*-1.01*		*-0.91*	*-1.10*
Median Household Income * 2000		-0.0092	-0.0286		-0.0200	-0.0203
		-0.81	*-2.68*		*-2.48*	*-2.84*
Median Household Income * 2002		-0.0111	-0.0280		-0.0185	-0.0179
		-0.98	*-2.63*		*-2.28*	*-2.45*
Resident Ownership Ratio			-32.81			-26.40
			-8.02			*-6.72*
Resident Ownership Ratio * 1998			9.18			1.34
			1.55			*0.24*
Resident Ownership Ratio * 2000			33.49			14.15
			5.48			*2.64*
Resident Ownership Ratio * 2002			29.57			16.73
			4.89			*2.86*
Tuition High School Students			-206.31			-138.37
			-2.46			*-1.85*
Union High School			92.95			163.27
			1.26			*3.05*
Average Daily Membership			-0.11			-0.08
			-1.63			*-2.55*
Population Density, 2000			-0.02			-0.03
			-0.21			*-1.05*
1998	-219.94	127.37	-284.63	-42.80	385.80	214.78
	-2.04	*0.27*	*-0.62*	*-0.38*	*1.16*	*0.51*
2000	18.78	237.53	-951.17	457.94	919.73	231.36
	0.17	*0.51*	*-2.02*	*3.88*	*2.67*	*0.54*
2002	716.29	1006.78	-32.58	1135.33	1458.92	584.37
	6.73	*2.15*	*-0.07*	*9.86*	*4.20*	*1.32*
Constant	6417.87	4234.29	5451.12	5848.60	4032.34	5535.29
	86.50	*12.77*	*16.81*	*73.75*	*17.00*	*18.53*
# Obs.	1004	984	983	1004	984	983
R^2	0.21	0.20	0.41	0.28	0.29	0.51

Columns (1), (2), and (3) present the unweighted estimates. Columns (4), (5), and (6) present the weighted estimates with average daily membership being the weight. The coefficient estimates are in plain text while the t-statistics are italicized. Aggregate property values are measured in hundreds of dollars.

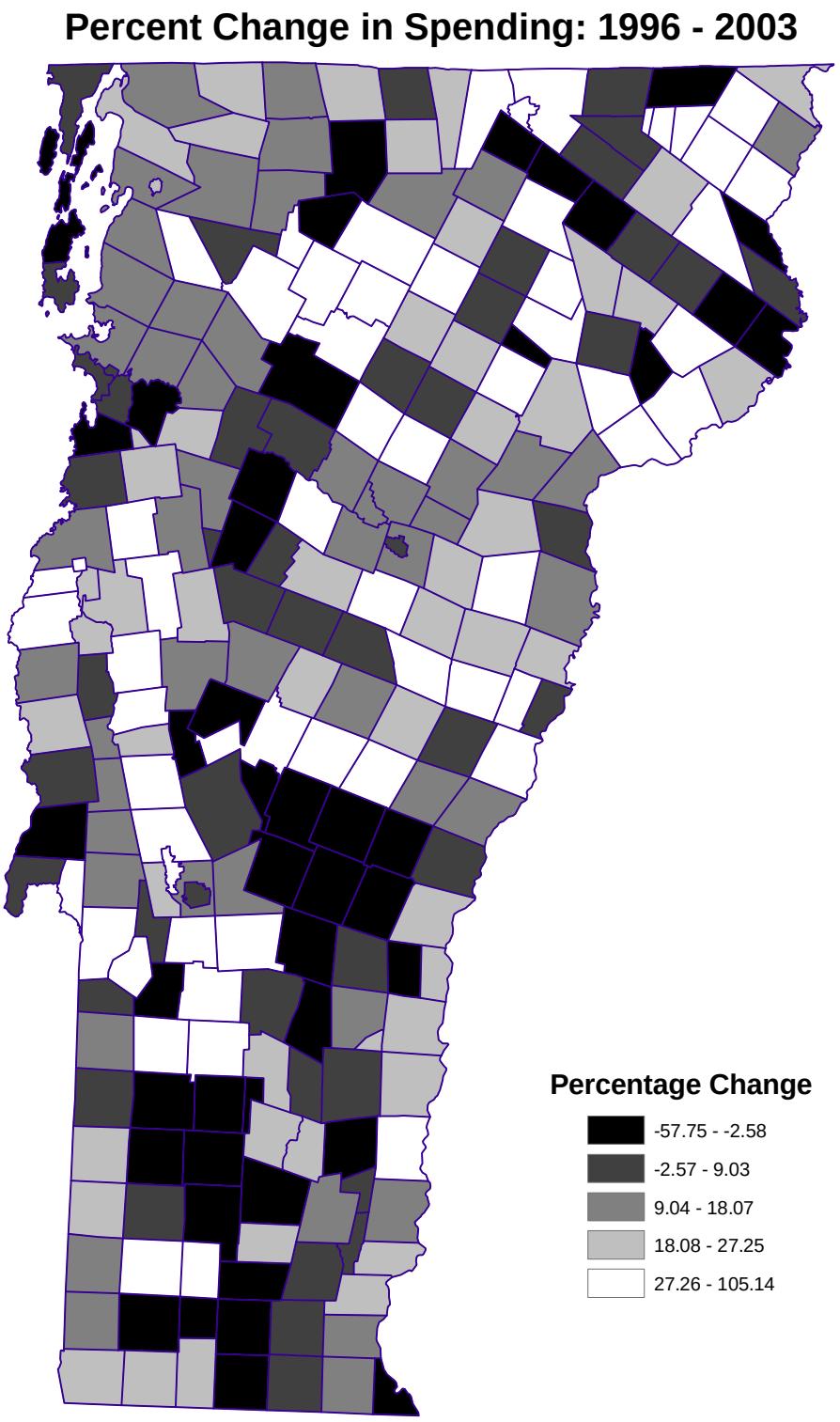

Figure 2.1: Percentage Change in Real Local Spending per Equalized Pupil: 1996 - 2003

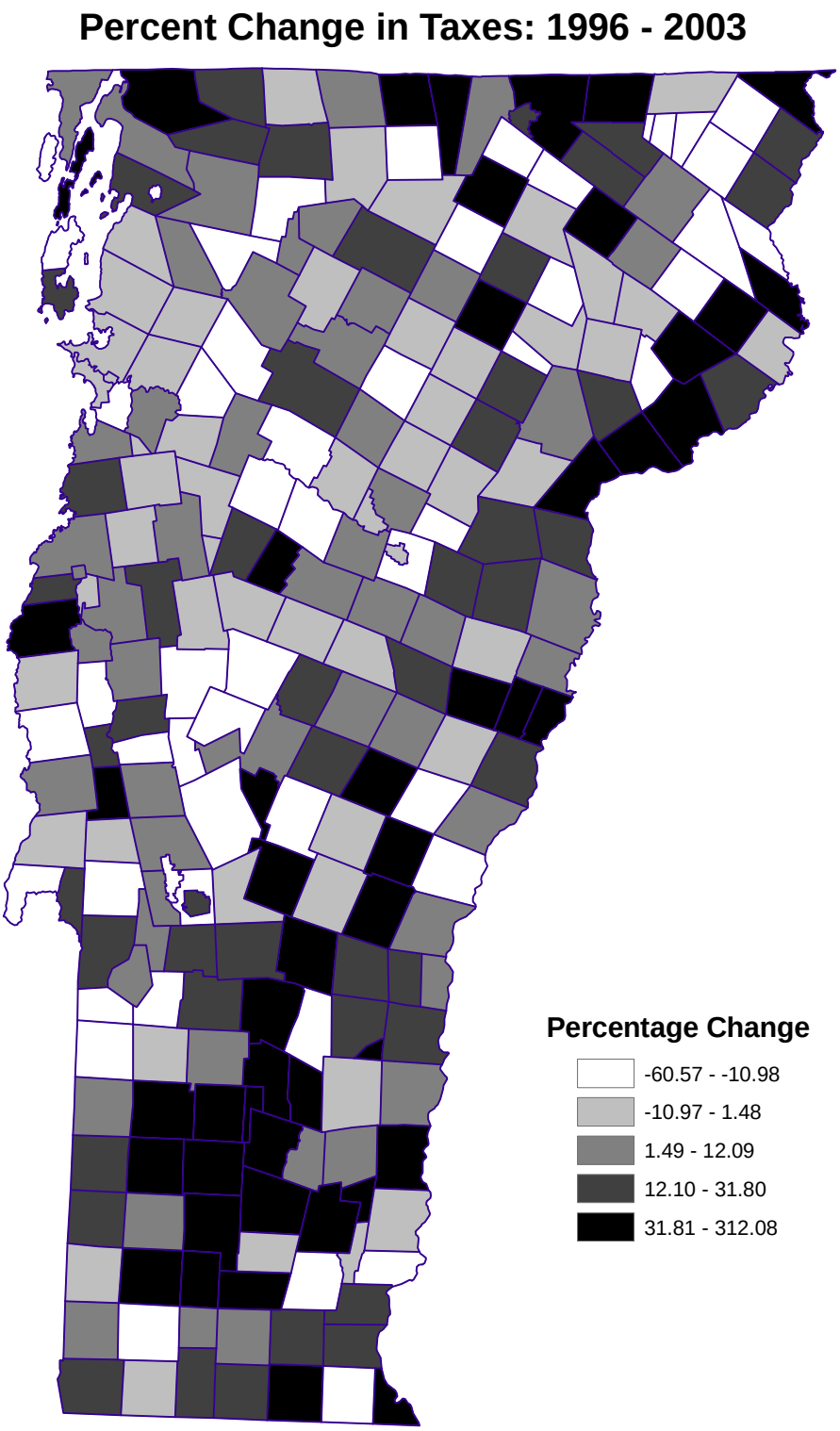

Figure 2.2: Percentage Change in Property Tax Rates: 1996 - 2003

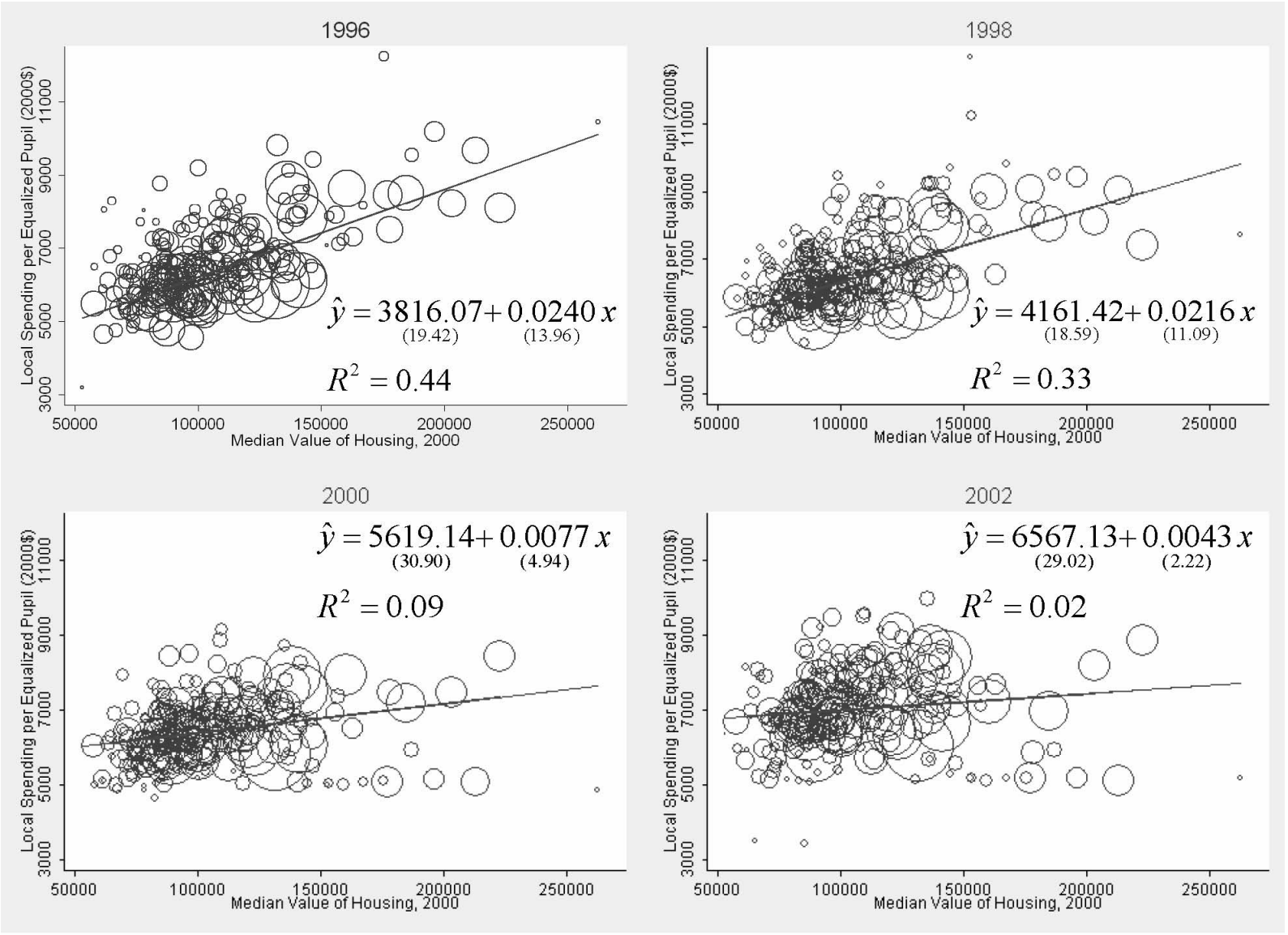

Figure 2.3: Median Value of Housing - Spending Gradient

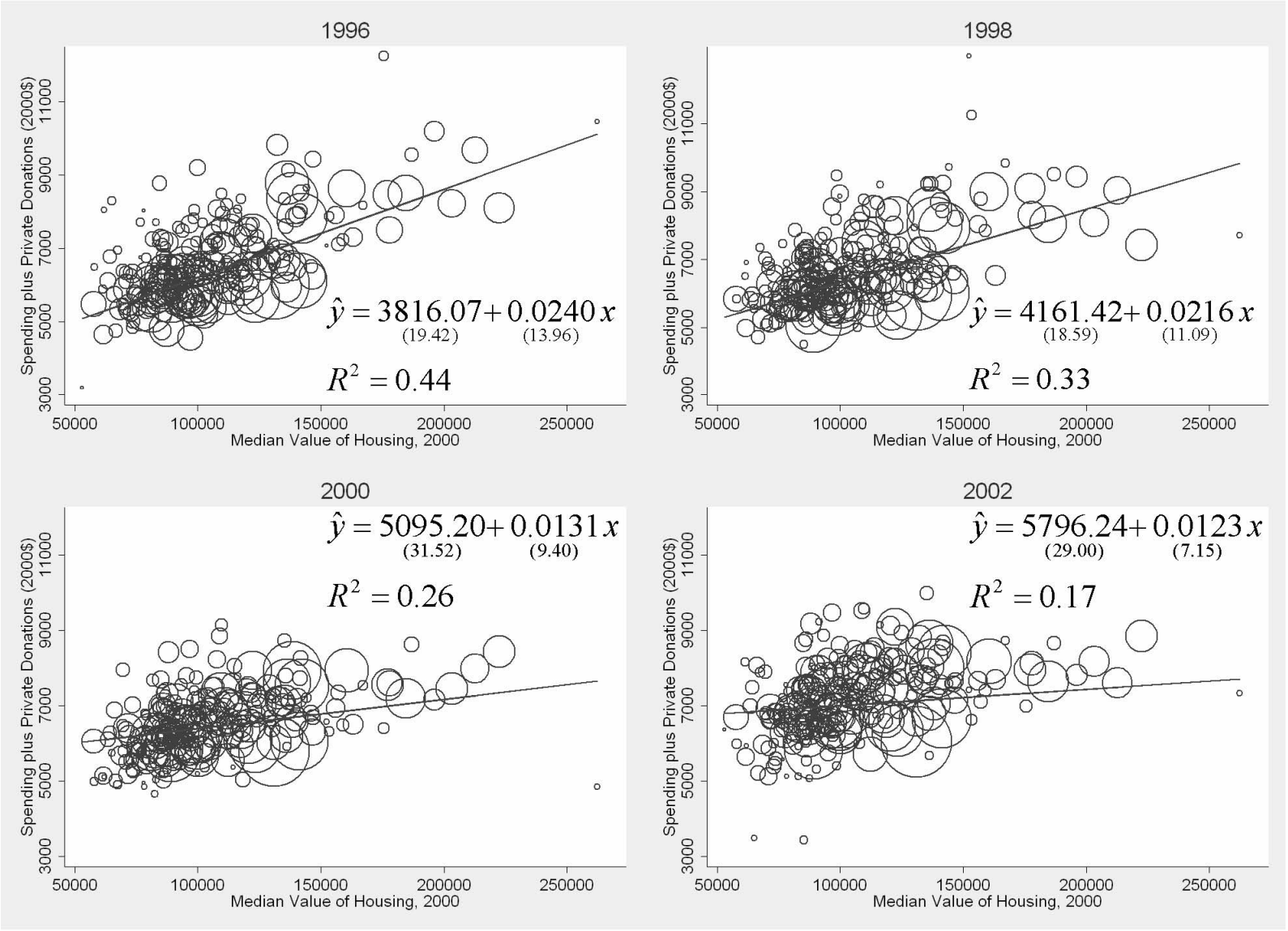

Figure 2.4: Median Value of Housing - Spending plus Private Donations Gradient

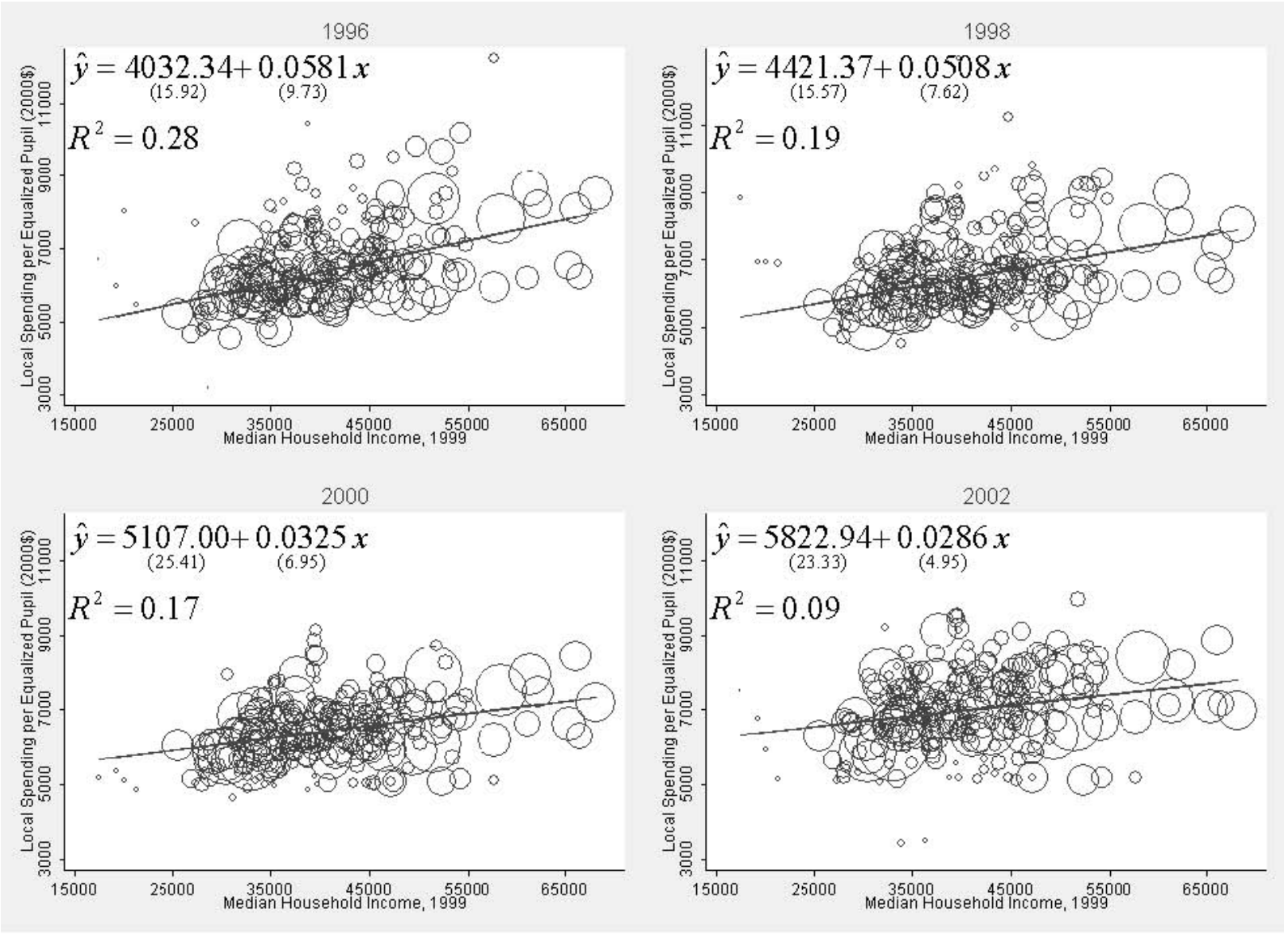

Figure 2.5: Median Household Income - Spending Gradient

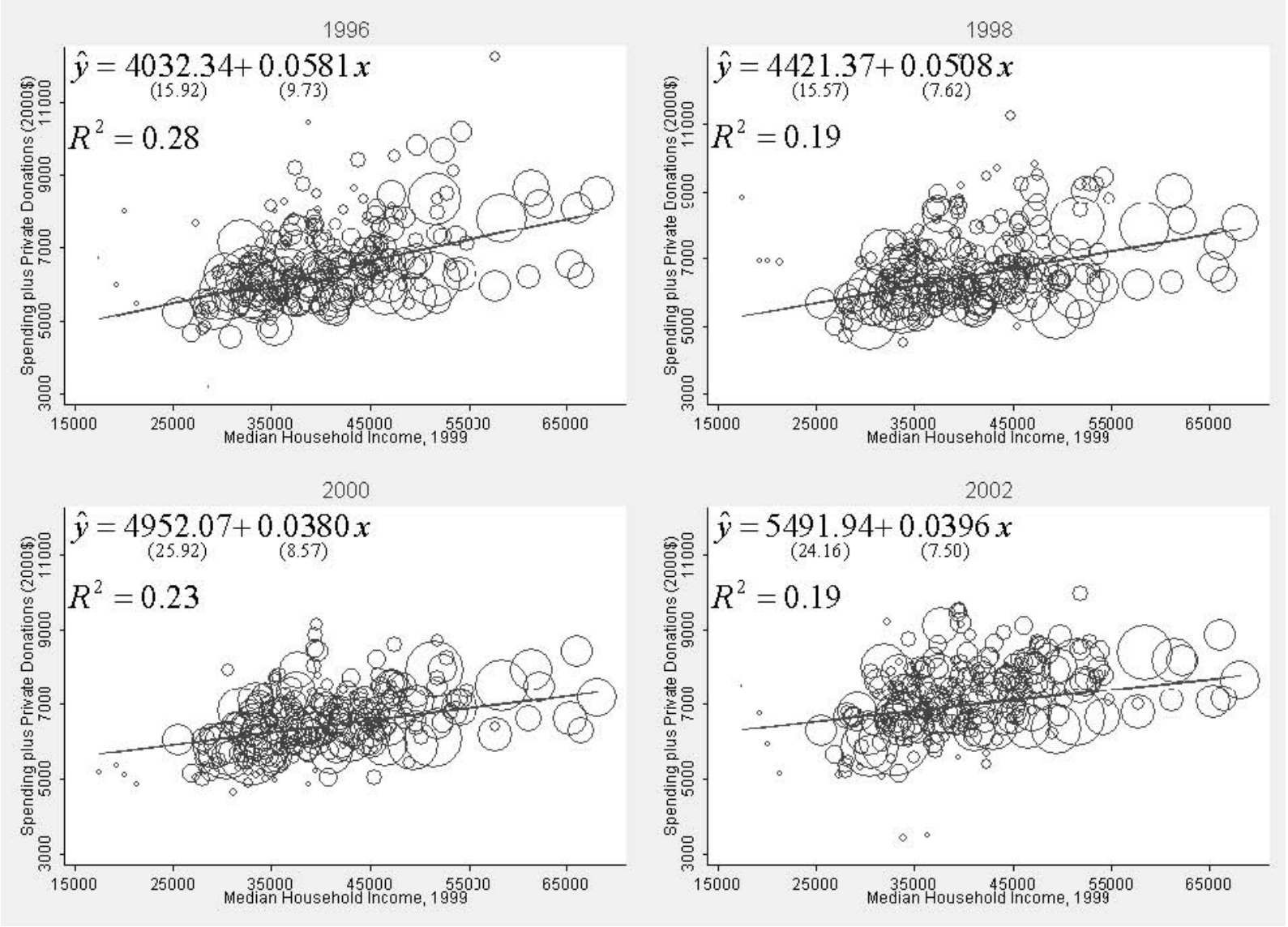

Figure 2.6: Median Household Income - Spending plus Private Donations Gradient

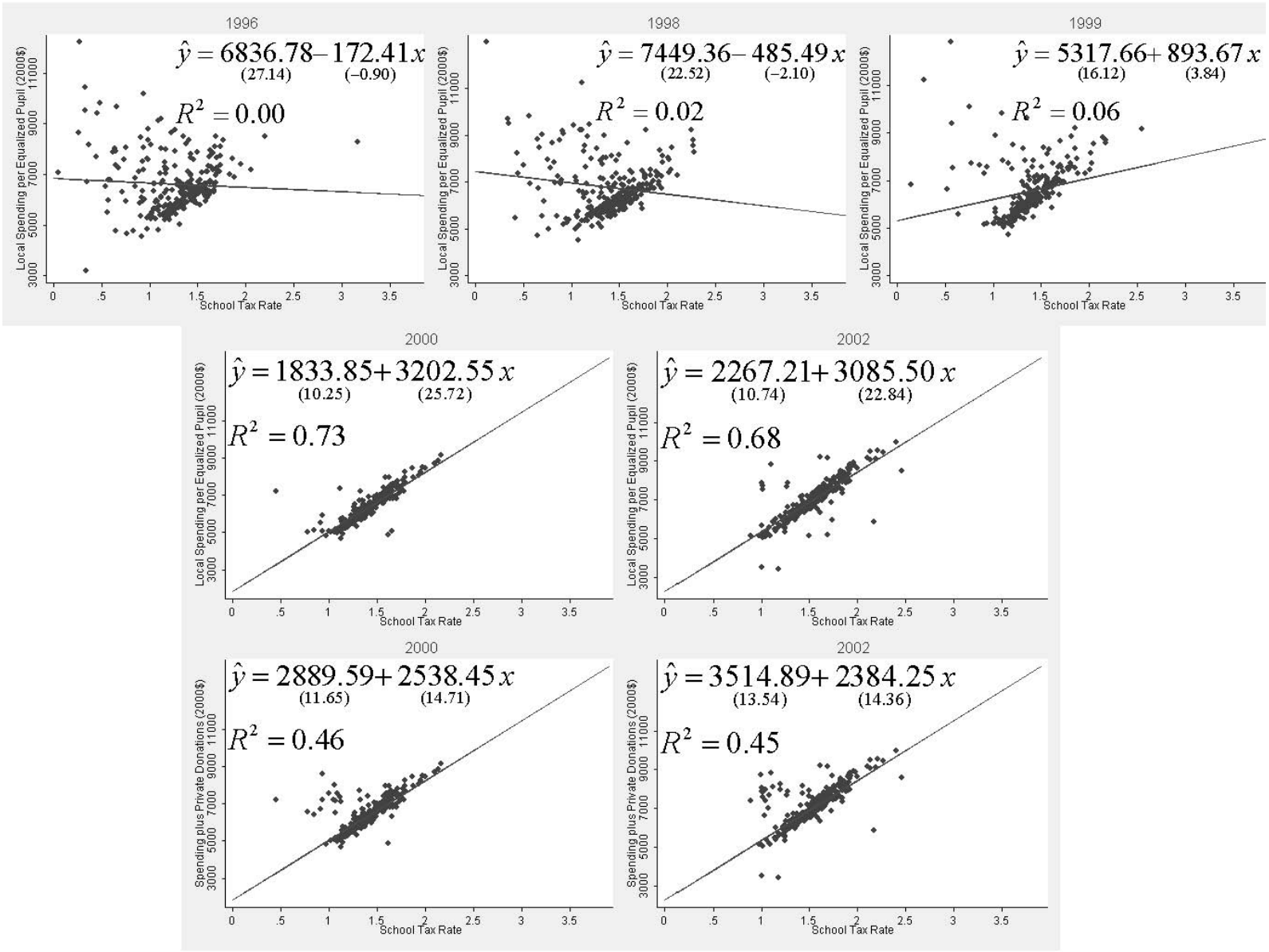

Figure 2.7: School Tax Rate and Spending

Chapter 3

The Effects of Financial Resources on Test Pass Rates: Evidence from Vermont's Equal Education Opportunity[1]

To date, lawsuits challenging states' public school financing methods have been brought in 45 out of the 50 states.[2] In many cases, these lawsuits led to reforms that changed how local public schools were financed. The first state to legislatively respond to school finance litigation was California in 1971. Proposition 13, enacted in response to the *Serrano v. Priest* case, limited property tax rates and assessed

[1]I would like to thank the City University of New York Graduate Center for generous support on this project via a sponsored dissertation fellowship. I am also grateful for advice and comments from Timothy Goodspeed, Clive Belfield, Lawrence Kenny, Anil Nathan, Andre Neveu, Steven Schmidt, and seminar participants at the City University of New York Graduate Center applied economics seminar and Union College Economics Seminar. This paper was presented at the 2009 Eastern Economic Association annual meetings as well as the 2009 American Education Finance Association conference.

[2]http://www.schoolfunding.info/litigation/litigation.php3 [accessed: August 18, 2008]

property value while increased state funding let school resources be distributed more equitably. Since California's reform, dozens of US states have undertaken school finance reforms. Earlier reforms tended to focus on enhancing the equity of resources.[3] More recent reforms have tended to focus on providing the resources necessary such that public schools are able to provide children with an adequate education.[4] In either case, the consequence of reform is a redistribution of resources available for public school students.

The obvious goal of increasing resources available to schools is to enhance the quality of education that those schools are able to provide. One highly available measure of public school quality (especially in the era of No Child Left Behind) is school standardized test scores. This begs the question, do additional resources improve school quality as measured by standardized test scores? Unfortunately, empirical and circumstantial complexities have prevented researchers from being able to answer with a simple yes or no.

The changes in spending that resulted from Vermont's school finance reform, the Equal Education Opportunity Act (Act 60), provide an excellent opportunity for exploring the relationships between spending and student outcomes. First, since Vermont's reforms took place relatively recently (1997), annual data is available on spending and test score pass rates. Vermont reported test score pass rates beginning in 1999 to comply with No Child Left Behind. Unfortunately, comparable test score data from before and after the reform is not available to facilitate a difference-in-difference analysis of the natural experiment. However, with annual spending and test pass rate data, panel data methods can be utilized instead of cross-sectional

[3]California, Texas, and New Jersey have all faced litigation challenging the constitutionality of school finance on equity grounds.

[4]Kentucky's ruling in *Rose v. Council for Better Education* was based on adequacy.

specifications. With panel data, I can control for school specific effects on student performance and include fixed year effects. Furthermore, the structure of Act 60 provides a natural instrument to address the possible endogeneity of spending. The structure of the legislation itself provides and instrumental variable such that spending can be identified in the structural equation. Additionally, since test score pass rates are available for six years, findings on the relationship between money and student achievement are likely to represent long-run trends. Finally, test score pass rates in Vermont are available for four different tests in three different subjects at two grade levels. Hence, the evaluation is not limited to performance in one subject area for one grade level.

This study represents the first comprehensive evaluation of the impact of additional resources on student achievement under Vermont's school finance reform. The finding that money does matter for math achievement is consistent with evidence from reforms in Massachusetts (Guryan, 2003) and Michigan (Papke, 2005, 2008). However, the finding that money does not matter for other subjects suggests that researchers should be cautious when interpreting the results of analysis examining the impact of financial resources on student outcomes when only math scores are examined.

The remainder of the paper proceeds as follows. The next section reviews the literature. Section 2 provides background information on Vermont's Act 60. Section 3 outlines the empirical methodology. Section 4 describes the data. Section 5 presents the results. Finally, section 6 concludes.

3.1 Literature Review

A number of recent studies have found that increased resources can significantly impact student achievement. These studies can broadly be categorized into those that study the impact of resources on achievement across states and those that quantify the impact of resources within states.[5] Interstate studies provide some benchmarks for policymakers in drafting school finance reforms. However, data limitations often prevent researchers from assessing the impact of school finance reforms on elementary school students' performance. Downes and Figlio (1998) use 12th grade math test scores and Card and Payne (2002) rely on SAT scores. Hoxby (2001) uses the dropout rate to measure achievement since it is the only consistent measure available across all states relevant to all students. Similarly, Dee (2005) assessed the graduation rate. Not all interstate studies found a positive impact of spending on achievement. Husted and Kenny (2000) concluded that school finance equalizations decreased average SAT scores while having little, if any, impact on the variation in SAT scores.

Of the studies assessing the impact of revenues on student achievement within a single state Sander (1999), Guryan (2003), and Papke (2005, 2008) conclude that increased spending was associated with increased student achievement in Illinois, Massachusetts, and Michigan, respectively. Roy's (2004) results for Michigan were mixed. Roy (2004) found that additional dollars did lead to higher scores on the state administered test but did not lead to improvements on national tests. Downes (1992) and Clark (2003) did not find any evidence that additional funds improved student performance under reforms in California and Kentucky, respectively.

[5]Recent examples of interstate studies that find that money matters include Figlio (1997), Wenglinsky (1997), Downes and Figlio (1998), Wenglinsky (1998), Hoxby (2001), Card and Payne (2002), and Dee (2005). Sander (1999), Guryan (2003), Roy (2004), and Papke (2005) are single state studies where money is found to impact student achievement.

Policymakers want to draft policies that channel additional funds to where they will be used most effectively. Papke (2005) found that additional funds lead to greater improvements when targeted at schools that were initially low performing.[6] Roy (2004) found that money mattered most for the districts that were initially the lowest spending. Guryan (2003) notes that his results may suggest atypical returns to additional education spending since additional funds under Massachusetts Education Reform Act (MERA) were targeted at under-funded schools.

Two additional interesting findings have emerged from the literature. The first is that money tends to be more effective for younger students (Wenglinsky, 1998; Roy, 2004). The second is that resources may have a larger impact on math test scores (Downes and Figlio, 1998; Downes, Dye, and McGuire, 1998). Sander (1999) notes that math scores "are usually considered more school-related." Some recent studies have focused exclusively on the impacts of additional funds on math test scores (Sander, 1999; Papke, 2005).

It is widely believed that resources are endogenous in determining student achievement. Furthermore, there are a number of unmeasurable factors that impact student achievement, rendering any attempt to specify the relationship between spending and student achievement subject to omitted variable bias. Omitted variables and endogeneity will both lead to bias coefficient estimates in cross-sectional or value-added studies (Ludwig and Bassi, 1999). The literature has addressed the possibility of omitted variables and endogeneity using primarily two different methods. Once approach to address the possibility of omitted variables is to use difference-in-difference, or quasi-experimentation, analysis. The difference-in-difference estimator requires

[6]In a follow-up study, Papke (2008) finds that her results hold when spending is assessed at the district level, as opposed to the school level.

that there be suitable treatment and control groups. Clark (2003) uses a difference-in-difference specification, where Tennessee serves as a control group, in her study examining the impact of Kentucky's school finance reform on student achievement. Often times, however, a suitable control group is not available. When panel data is available, fixed effects can be used to help reduce possible omitted variable bias. Guryan (2003) and Papke (2005, 2008) have utilized the latter approach.

Instrumental variables techniques are generally employed to address omitted variable bias as well as the likely endogeneity of spending in the process determining student achievement. National studies have been able to use state property tax limitations (Figlio, 1997) or school finance reforms (Card and Payne, 2002) as instruments. Sander (1999) uses the percent of the population within a district that is school aged to instrument for spending. The correlation between spending and the percentage of school aged children in a district is presumably low as the partial R^2 from the first stage regression is 0.04. Others single state studies have utilized stronger instruments. Clark (2003), Guryan (2003), Roy (2004), and Papke (2005, 2008) all use the structure of the state's financing system, specifically the level of state or foundation aid, to identify spending in the structural relationship between spending and student achievement.

Despite data improvements allowing panel data and instrumental variables techniques to be utilized in estimating the impact of money on educational achievement, the literature has not reached a consensus regarding the magnitude of the marginal impact, if any, that additional spending has on student achievement.

3.2 School Finance Reform in Vermont

In 1997, Vermont passed the Equal Education Opportunity Act, or Act 60. This legislation was in response to a Vermont state Supreme Court ruling in *Brigham v. State (1997)*. The court ruled that the state's pre-1997 method of financing public schools violated the equal protection clause of the state constitution by not providing Vermont students with equal access to educational funds.[7]

Prior to 1997, Vermont's schools were financed primarily through local property taxes. Towns would choose a property tax rate and revenues from that tax would be used to fund schools attended by residents of that town.[8] Towns with high property wealth were able to levy low tax rates and achieve relatively high per-pupil revenues.[9] Since a lower tax rate was capable of raising more revenue per-pupil in property wealthy towns as opposed to property poor towns, school spending was positively related to property wealth. Breaking this link was one of the two objectives of Act 60.[10]

Under Act 60, Vermont established a statewide property tax of $1.10 for each $100 is assessed property value.[11] Towns were allowed to phase in this tax rate change through FY2001 (tax year 2000).[12] Beginning in 2001, each town that levied

[7]Downes (2004) presented evidence on the equalizing impact of Act 60 with respect to spending. His study also suggests that Act 60 equalized math test scores using data through the final year of Act 60 phase-in, 2001.

[8]The state also provided school districts with foundation grants.

[9]Vermont's high property wealth towns are often referred to as "gold towns." Many of these towns are the home to the state's major ski resorts.

[10]Prior to the passage of Act 60, many Vermonters had been growing dissatisfied with increasing property tax liabilities. State politicians elected in 1996 were committed to property tax reform (Mathis, 2001). Providing property tax relief was the second objective of Act 60.

[11]Tax adjustments were made for households earning less than $75,000 such that the school tax did not exceed 2% of household income.

[12]From this point on, all revenue and expenditure data refer to fiscal years. Each fiscal year corresponds to the previous tax year and school year. For example, fiscal year 2001 corresponds to tax year 2000 and academic year 2000-01.

local property taxes of $1.10 per $100 in assessed property value received $5,194 per-pupil in foundation aid from the state. Any town that wanted to spend more than the foundation level could levy additional school property taxes.

These additional revenues were sent to the state's "sharing pool." The sharing pool was the sum of all local tax revenues raised from local tax levies exceeding the $1.10 minimum. The sharing pool can be represented as

$$SP = \sum \tau_i' \cdot \frac{V_i}{C_i}, \tag{3.1}$$

where SP is the total resources held in the sharing pool, τ_i is the local tax levy in excess of $1.10, V_i is the assessed value of property and C_i is the Common Level of Appraisal (CLA).[13]

In return for levying additional taxes towns were rewarded with additional spending on local education. Additional spending on education for the town was directly proportional to their local excess tax levy, such that

$$AR_i = \delta \tau_i', \tag{3.2}$$

where AR_i are the additional funds the state gives to town i and δ is the guaranteed tax base or equalized yield. Since the system is designed to be self financing,

$$\sum AR_i = SP. \tag{3.3}$$

[13]The state assigns each town a CLA in order to equalize assessed property values. Towns with relatively low assessments relative to other towns receive a low CLA, and vice versa. A low CLA increases the basis for taxation in the town, where $\frac{V_i}{C_i}$ represents the basis for taxation.

To solve for δ, substitute (3.1) and (3.2) into (3.3) and solve for δ:

$$\delta = \frac{\sum \tau_i' \cdot \frac{V_i}{C_i}}{\sum \tau_i'}. \tag{3.4}$$

Under this system, all towns set their tax rates and made their sharing pool contributions before knowing the amount of extra revenue their taxing efforts would yield. The goal was that all towns levying the same tax rate would spend the same amount per-pupil. This measure did allow for some element of local control. For all towns, raising tax rates would result in additional spending. However, for high property wealth towns there was a strong disincentive to raise tax rates.[14] Before Act 60, every dollar of locally raised school property taxes went to local public schools. After Act 60, locally raised school property taxes went to the sharing pool. These resources were then returned to the town as described above. For many towns with high per-pupil property wealth, the tax price of education spending increased dramatically (Schmidt and Scott, 2006).

The change in incentives led to large changes in per-pupil spending in a number of towns.[15] Stowe, for example, saw real local spending per equalized pupil fall over $5000 between 1997 and 2001.[16] A number of other towns experienced dramatic increases in per-pupil spending, as raising revenue for schools became much less burdensome. In fact, many towns saw per-pupil spending increase while school property

[14]At any point, Vermont towns could spend more by increasing tax rates. Hence, they were not as restricted as districts in California under Proposition 13.

[15]In the sample used for the regression analysis, the mean change in real local spending per equalized pupil between 1997 and 2001 was $290.65 with a standard deviation of $1017.71.

[16]Forty-two schools included in the regression analysis were located in towns where real local spending per equalized pupil fell between 1997 and 2001. Twenty-seven schools included in the regression analysis were located in towns where real local spending per equalized pupil increased by over $1000 over the same period.

taxes went down.[17]

A number of towns responded to Act 60 by forming local education foundations, non-profit organizations explicitly designed to raise money for local public schools (Downes and Steinman, 2008). If a town was able to solicit donations for a private foundation, revenues raised would not have to become part of the state's sharing pool. Any revenues raised via private donations could be spent in their entirety in local public schools. Therefore, towns with high tax price for education were incentivized to set a low tax rate in order to avoid subsidizing education spending in low property wealth districts, and raise local revenues via private foundations. In 2001, the first year Act 60 was fully implemented, 24 towns raised revenues using private donations to local education foundations an effort to avoid the sharing pool. Stowe, via private contributions to local education foundations, raised $3,410.54 per equalized pupil in 2001 (2004 dollars). However, 98 out of Vermont's 240 public elementary schools were located in towns that experiences a decline in local spending per equalized pupil in real terms during the Act 60 phase-in period between 1999 and 2001. Between 2000 and 2004 schools in towns that used local education foundations to avoid the sharing pool raised an average of $1,839 (in 2004 dollars) with a standard deviation of $1095.

During Act 60 phase-in and while the policy was in place there was a lot of variation in per-pupil spending. The between standard deviation for real local spending per equalized pupil was $968 while the within standard deviation was $680. After the phase-in of Act 60 was complete, spending levels failed to stabilize. The average change in real spending between 2001 and 2004 was only $315 but the standard deviation of the change in spending exceeded $1000. Twenty-five schools were located in

[17]There are 55 schools included in the regression analysis located in towns where property tax rates fell and spending increased.

towns where spending fell by more than $1000 between 2001 and 2003. When private donations are included between 2000 and 2004, the between standard deviation of spending is about $1000 while the within standard deviation was $555.

The variation in spending could have been coming from a number of sources. Since towns did not know exactly how much revenue additional tax levies would yield, maintaining constant levels of real expenditures may have been difficult. Another potential source of variation in the local spending per equalized pupil comes from the existence of private foundations. Foundation donations were relatively unpredictable, and lowering tax rates with the expectation of being able to utilize private donations did not yield reliable revenues. While determining the source of variation is beyond the scope of this paper, what is important is that there was variation in spending.

3.3 Empirical Method

An education production function is used to estimate the impact of additional funds on student achievement. The education production function is an attempt to estimate the contribution of inputs into the educational process on an overall measure of output. One frequently used and widely available measure of output is the percentage of students passing a standardized test within a school, the test pass rate. Spending and other school-level demographic variables are inputs the production function.

Explicitly, the education production function takes the form

$$P_{it} = X_{it}\beta + \rho R_{it} + T_t\lambda + \varepsilon_{it} \tag{3.5}$$

where P_{it} is the percent of students achieving the standard or achieving the standard

with honors on the respective test, X_{it} is school characteristics such as the percentage of students eligible for free/reduced price lunch and the number of students in the school, R_{it} is a measure of spending per-pupil, and ε_{it} is the random error. The production function also includes fixed year effects, T_t, capturing factors changing from year to year that are constant across schools, such as the difficulty of the test.

State tests are particularly susceptible to artificial score increases. This is especially true when tests are first introduced. In the years following the first administration of a test teachers learn what material is likely to appear on the test and prepare students accordingly. Hence, increased test score pass rates following the first administration of a standardized test may represent teachers' learning of what will likely appear on the test and not general learning by the students.[18] Furthermore, since state tests are given annually and schools' results published, there is pressure for schools to improve pass rates, even if higher scores may not represent true learning.[19] Clark (2003) and Roy's (2004) findings confirm that performance gains on state standardized tests are not always mirrored by performance increases on nationwide tests.[20]

Since the data is a panel, and since real spending has varied over time, school fixed effects, μ_i, are used to control school attributes that remain the same over time.

[18]National tests such as the NAEP are given every other year to a random sample of schools. Even if a school participates in one year, the school may not participate again when the next NAEP is given.

[19]With the accountability measured under No Child Left Behind high-stakes testing in particular may create incentives for teachers to cheat on tests. Jacob and Levitt (2003) provide evidence that incentive systems may induce teacher cheating. While Vermont's tests are not categorized as high stakes the potential for inflation in state standardized test scores over time should be addressed.

[20]I was unable to obtain permission to access Vermont student's SAT scores and therefore have not explored how Vermont student's test scores responded to nation wide standardized tests under Act 60.

In order to isolate these fixed effects the error term is decomposed as follows:

$$P_{it} = X_{it}\beta + \rho R_{it} + T_t\lambda + \mu_i + e_{it}. \tag{3.6}$$

Essentially, μ_i is a dummy for each individual school and controls for school heterogeneity. Attributes that remain relatively constant over time within schools, such as the neighborhood of the school, historical attitude toward community involvement in the school, and regional differences in education cost are captured in this term. Capturing school specific effects reduces the scope for omitted variable bias to the degree that these unobserved factors are correlated with the regressor. Few studies, Guryan (2003) and Papke (2005) being the notable exceptions, are able to employ fixed effects analysis. Since real spending does change under Vermont's Act 60 fixed effects can be utilized.

Fixed effects, however, cannot be relied on to eliminate the bias in the coefficient on spending. Per-pupil spending may still be correlated with the unobserved determinants of achievement, e_{it}, that do change over time. Parental motivation, household environment, and changes in attitudes toward education may not be adequately controlled for by the independent variables. If spending is not orthogonal to the unobserved determinant of test scores OLS will not yield an unbiased coefficient estimate.

Furthermore, spending and test performance may be jointly determined. If schools with low test scores respond by spending more, then causality between spending and test scores potentially runs in both directions.[21] If this is the case, spending is endogenous due to simultaneity. The potential for omitted variable bias and simultaneity

[21] Ferguson and Ladd (1996) present evidence of such compensatory spending in Alabama.

suggest that the potential endogeneity of spending should be explored when estimating (3.6). Fortunately, the endogeneity of spending in the regression equation can be addressed using Instrumental Variables (IV) techniques. In order for a variable to be an effective instrument, the variable must be highly correlated with the endogenous regressor and uncorrelated with the error term. Under Vermont's Act 60, local spending per equalized pupil is highly correlated with school property tax rates.[22] In 1997, before Act 60 implementation began, the correlation coefficient between local spending per equalized pupil and school property tax rates was 0.09. In 2001, the first year that Act 60 was fully implemented, the correlation between local spending per equalized pupil and school property tax rates was 0.85. School finance reform in Vermont was explicitly designed to generate this correlation.[23]

Tax rates, however, are not a determinant of schools' test pass rates. Intuitively, since Act 60 dramatically increased the tax price of education in high property wealth towns (towns where residents spent relatively more on education pre-reform), high property wealth towns will not necessarily raise property tax rates to fund education. Even if higher property wealth towns prefer to spend more on education, these towns may not choose to raise taxes since the majority of revenue raised would go into the states sharing pool rather than be spent in the local public school. Hence, tax rates are directly related to local spending per equalized pupil but are not likely related to unobserved determinants of student test performance. The identification strategy is

[22]Vermont property taxes between 1999 and 2004 had two components: the school tax and the local/municipal property tax levy. Each is reported separately.

[23]Theoretically, the correlation between tax rates and spending per-pupil in the town should be exactly one. In our analysis we use local spending per equalized pupil. The correlation between local spending per equalized pupil and tax rates is less than one since local spending equalized pupil provides more funds to communities with a higher proportion of costly to educate students. The adjustment is made based on the proportion of students in poverty, students with limited English proficiency, and secondary students.

to treat town level school tax rates as exogenous and use these tax rates to identify the impact of spending in a town on a school's test score pass rates.

When private contributions are included, the argument for exogeneity of the instrument is not as strong. Towns that prefer more education spending are able to spend more, even with a low tax rate, by using private foundations. Hence, the correlation between tax rates and spending is lower, 0.68 in 2001, when private donations are included. Furthermore, spending when private donations are included is driven more by preferences for education spending rather than tax rates. Tax rates are also less likely to be orthogonal to the error term when private spending is included. For this reason, the instrumental variables results for total spending (local spending plus private spending) should be viewed with caution.

3.4 Data Sources

The dependent variable in the analysis is a school's average pass rate for 4th grade students on math, reading, and writing tests. Schools' average pass rates on 2nd grade reading tests are also available.[24] Test results are not reported for private schools, limiting the sample to public elementary schools. Furthermore, if the number of test takers is low, the average pass rate may not be publicly available. Consequently a number of schools are not in the sample for the full 6 year period. The result is an unbalanced panel. Since the objective is to analyze the impact of spending on student achievement, data on schools' pass rates are merged with school demographic data.

[24]Fourth grade test pass rates are students achieving the standard or achieving the standard with honors on Vermont's New Standards Reference Exam (NSRE). Second grade pass rates are the percentage of students achieving the standard or achieving the standard with honors on the Developmental Reading Assessment (DRA) exam. All pass rate data is publicly available from the Vermont Department of Education.

Then, school level data is matched to data on spending per-pupil at the town level.

School demographic information comes from the Common Core of Data (CCD). The CCD School Universe Survey contains information on the number of students enrolled in the school, the number of students eligible for free or reduced price lunch, and the number of minority students.[25] The CCD data are gathered annually and the variables can be included in a fixed effects analysis.[26]

Each school is matched to a town since an explicit measure of spending per student at the school level is not available. Spending measures at the town and level are used to measure per-pupil spending. In Vermont, the vast majority of towns only operate one school or are part of a joint or union district.[27] In Vermont, there are 240 schools located within one of 215 towns.[28] For towns that were part of a joint school district, where more than one town's students attend a given school, spending was calculated as a weighted average of the spending in all towns attending the school.[29]

Data is available on local spending per equalized pupil throughout the sample period.[30] Local spending per equalized pupil includes the foundation grant received

[25]Since there are few ethnic minority students in Vermont, and since there is little variation in the percentage of ethnic minorities enrolled in a given school over time, I do not present results including this variable.

[26]Analysis including the pupil-teacher ratio produced results similar to those presented here, suggesting that additional resources have an impact on achievement even after controlling for the pupil-teacher ratio. Examining how additional funds are being spent, although an important avenue of research, is beyond the scope of this paper.

[27]Spending at the town level is used to pay for elementary as well as high school students. The data does not specify how much spending goes toward funding elementary education and how much is allocated for secondary education. In towns that have multiple elementary schools it is unknown how much spending takes place at each specific school.

[28]In Vermont, towns and school districts generally are coterminous. Spending in gores, unorganized towns, and towns that did not have an elementary school and were not part of a joint or union elementary school were not included in the measures of school spending.

[29]The weights were the number of equalized pupils in each town.

[30]Equalized pupils are weighted pupils. Students in poverty and those with limited English proficiency receive a larger weight. Secondary students are also given greater weight than elementary students. Consequently, spending measures reported per equalized pupil adjust for anticipated cost differentials.

by each town and any additional funds coming from the sharing pool for towns taxing above the minimum required rate. Local spending per equalized pupil does not include federal grants or parental contributions. Local spending per equalized pupil tends to overstate the effects of equalization in that parental contributions are omitted (Downes and Steinman, 2008). The Vermont Department of Education maintained a spreadsheet of private contributions reported in each town between 2000 and 2004.[31] Private contributions are categorized as being those that resulted from Act 60 and those that did not.[32] Total spending in the regression analyses below is local spending per equalized pupil plus any private contributions, whether they were Act 60 induced or otherwise.

The instrument in the dataset is the school tax rate. As was done for the town spending above, to find the school tax rate associated with each school, for schools with students coming from multiple towns, the tax rate is a weighted average of the attending towns' tax rates.

Table (3.1) contains summary statistics. To tabulate the summary statistics for spending, a school is included in each year even if test score pass rate data is only available in one year. Hence, spending measures are for spending in the same 236 schools throughout the sample period. Test score pass rates are not available for all schools for all years since the state does not report test score pass rates if there was a small number of test takers. For 2003 and 2004 test score pass data are only available for schools where more than ten students took the test, hence the smaller number of test score pass rate data points in those years. School enrollment is the number

[31] I would like to thank Brad James at the Vermont Department of Education for providing access to this spreadsheet.

[32] Some towns received private contributions for local public school prior to Act 60's implementation. Generally, these contributions were very small.

of students that attend the school. Generally, enrollment data were complete in the CCD. Data on the percent of children eligible for free/reduced price lunch was more likely to be missing. In order to maximize the number of observations available for our analysis imputed values for free/reduced price lunch variable and school enrollment are used.[33]

As indicated by the summary statistics, average real expenditures per-pupil increased in each year that Act 60 was in effect.[34] For 4th graders, math and writing pass rates increased substantially between 1999 and 2004. However, 4th grade reading pass rates were relatively constant over the sample period. Second grade reading pass rates also increased between 1999 and 2004. The empirical analysis investigates whether it is likely that increased spending caused higher test scores.

Since local spending per equalized pupil is available for all years test score pass rates were available under Act 60, the effect of local spending per equalized pupil on the test score pass rate is assessed using an unbalanced panel spanning 1999 through 2004. The impact of local spending per equalized pupil on test score pass rates can be interpreted as the impact of spending as intended by the reforms. When private contributions are included in the analysis, the unbalanced panel spans 2000 to 2004.

3.5 Results

Column (1) in Tables (3.2) - (3.5) presents the results of using pooled OLS to estimate equation (3.5). The dependent variable is the test score pass rate. Local spending per equalized pupil is included in logarithmic form to incorporate the diminishing

[33]Missing data was imputed using forward and lagged values of the missing variable. In all cases, there was at least one year when each variable was available. Using imputed values for enrollment and free/reduced price lunch does not change the conclusions drawn from the regression analysis.

[34]All spending variables are reported in 2004 dollars. Real values were calculated using the CPI-U.

effect of spending. The demographic control variables are the percent of students eligible for free/reduced price lunch and school enrollment. The percent of students eligible for free/reduced price lunch is a measure of poverty status within the school. The square of the percent of students eligible for free/reduced price lunch is included to account for potential non-linearities. The log of school enrollment is included in quadratic form to allow for potential diminishing or increasing effects. Fixed year effects, T_t, are included but school fixed effects are not. When the relationship between spending and pass rates is estimated using pooled OLS, the coefficient on local spending per equalized pupil is insignificant. Contemporaneous spending is used in all specifications. Using lagged spending or average spending over the past two or three years did not change the results substantially.[35]

In the pooled OLS specification, the percentage of students eligible for free or reduced price lunch has a negative and significant impact on average test score pass rates for each test. For 4th grade math, a 10 point increase in the percent of students eligible for free or reduced price lunch decreases the average pass rate by 5.7 percentage points. The fully robust t-statistic is -4.37. This finding is consistent across tests, although the magnitude of the coefficient and t-statistic is smaller in the 2nd grade reading specification. The quadratic term indicates that there are small diminishing effects associated with the percentage of students eligible for free or reduced price lunch.

The benefit of having panel data is the ability to control for the effects of unchanging unobservable variables over time. If there are attributes of a school that lead to higher performance that differ across schools but do not change from year

[35]Standardized tests for 2nd and 4th grade students in Vermont are administered in the spring. Hence, local spending per equalized pupil represents school resources available in the academic year preceding the test.

to year, this unobserved school effect can be accounted for in a fixed effects framework. If, however, these effects vary between schools but are uncorrelated with the other regressors then a random effects framework would be most appropriate. Take the regression equation where the error term has been decomposed (equation (3.6) above):

$$P_{it} = X_{it}\beta + \rho R_{it} + T_t\lambda + \mu_i + e_{it}.$$

Again, μ_i is the individual school effect. If μ_i is uncorrelated with X_{it} and R_{it} then random effects will be the consistent and efficient regression technique. However, if μ_i is correlated with elements in X_{it} or R_{it} the random effects estimator will be inconsistent. Given that there are few elements in X_{it} it is highly likely that there is correlation between the unobserved component of test score pass rates and the explanatory variables. The Huasman test is used to evaluate whether μ_i is correlated with elements in X_{it} or R_{it}. As expected, the Hausman test suggests that unobserved school effects are correlated with the other explanatory variables. Hence, fixed effects is the preferred estimation technique.

Column (2) in Tables (3.2) - (3.5) presents the results adding school level fixed effects in estimating the relationship between local spending per equalized pupil and test score pass rates. When fixed effects are used the coefficient on spending is positive for each test pass rate but only marginally significant for 2nd grade reading (p-value = 0.06). F-tests in each case confirm the joint significance of the school level fixed effects. As expected, the t-statistics of the percentage of student eligible for free or reduced price lunch are smaller under the fixed effects specification. Variation in poverty is greater between schools than within schools. Nonetheless, the percentage of student eligible for free/reduced price lunch is still significant at the 10% level for

4th grade writing and 2nd grade reading pass rates in the fixed effects specifications. When fixed effects are included, school size has a positive and significant impact on 4th grade math pass rates and marginally significant (p-$value$=0.09) positive impact on 4th grade reading pass rates. School size does not appear to have an impact on 4th grade writing or 2nd grade reading pass rates.

Column (3) in Tables (3.2) - (3.5) presents the results of using first difference estimation. If the error terms are serially correlated the first difference estimates will be preferred to the fixed effects estimates. If the error term is not serially correlated the fixed effects estimates are more efficient than the first difference estimates. The drawback to using the first difference estimator is the loss of one year of data. The first difference estimates of the impact of spending are positive for the four pass rates but never statistically significant.

Even after fixed effects, if spending remains correlated with the time-varying component of the error term, the coefficient on spending will be biased. In order to address possible endogeneity I use an instrumental variables approach. As was stated above, the instrument is the school tax rate in each town. Following Papke (2005), I also interact the tax rate with dummy variables for the years 2001, 2002, 2003, and 2004. Hence, there are five instruments for local spending per equalized pupil. F-tests for the joint significance, presented in Tables (3.2) - (3.5), consistently have a p-value of 0.00. Shea's partial R^2 also indicates that the instruments explain variation in local spending per equalized pupil.

Column (4) in Tables (3.2) - (3.5) contains the 2SLS estimates of the impact of local spending per equalized pupil on test pass rates. For 4th grade math and 4th grade reading, the coefficient on spending is positive but insignificant. For 4th grade writing and 2nd grade reading the coefficient on spending is negative and significant

at the 10% level for 2nd grade reading. This model, however, does not include the school level fixed effects which are significant.

The fixed-effects instrumental variables specification results are presented in column (5) of Tables (3.2) - (3.5). Table (3.2) presents the results for 4th grade math pass rates. The point estimate for the impact of spending on 4th grade math pass rates is 22.78, which suggests that a 10% increase in spending leads to a 2.28 percentage point increase in the pass rate. This result, however, is only significant at the 10% level. The Hausman specification test has a p-value of 0.09, which is weak evidence that spending is endogenous.

With the inclusion of the year tax rate interactions as instrumental variables in the fixed effects specification, there are four overidentifying restrictions. This allows me to test whether the tax rate fails the exogeneity assumption. Specifically, a Hansen J test explores whether the instruments are uncorrelated with the error process. The null hypothesis is that the instruments are exogenous. In Column (5) of (3.2), the Hansen J statistic has a p-value of 0.93, failing to reject the null hypothesis of instrument exogeneity. This test, however, has low power. A difference-in-Sargan statistics, or a C-statistic, is used to test whether the school tax rate is exogenous. The null hypothesis is that the specified instrument is exogenous. In Column (5) of (3.2) the C-statistic has a p-value of 0.44, failing to reject the null hypothesis that the school tax rate is an appropriate instrument. The most compelling evidence that my instrument is exogenous is that regressions where the school tax rate is the only instrument produce results that are very similar to those presented in Column (5) of Tables (3.2) - (3.5). Hence, the school tax rate appears to be exogenous in the process generating pass rates.

As was stated above, if spending is treated as endogenous, the fixed effects specifi-

cation estimated using instrumental variables suggests that local spending per equalized pupil has a positive and marginally significant (p-value of 0.08) impact on 4th grade math pass rates. This, however, is not the case for other tests. Looking at Column (5) in Tables (3.3) - (3.5) the coefficient on spending is insignificant. Further, for 4th grade reading and writing tests, the Hausman specification test does not suggest that spending is endogenous.

Column (6) in Tables (3.2) - (3.5) presents the results of first difference specifications estimated with instrumental variables. In Table (3.2), the fixed effects and first difference specifications where spending is treated as being endogenous produce very similar results. The Hausman specification test in Column (6) of Table (3.2) has a p-value of 0.04 suggesting that spending is very likely endogenous. The point estimate on spending suggests that a 10% increase in spending would increase the pass rate by 3.04 percentage points, but this estimate is only significant at the 10% level. Nonetheless, the similarity in the estimates produced using fixed effects to those using a first differenced specification are encouraging, and suggest that the result is robust.

Table (3.2) suggests that spending may have an impact on test score pass rates for math. The results in Tables (3.3) - (3.5), however, do not find that spending has a positive impact on reading or writing test score pass rates. Furthermore, for 4th grade reading and writing there is no evidence that spending is endogenous. For 2nd grade reading, the Hausman specification test has a p-value of 0.07, suggesting that spending may be endogenous. The coefficient on spending in the fixed effects instrumental variables specification, however, is negative and insignificant. There is no evidence that spending is endogenous in the first difference specification for 2nd

grade reading.[36]

Table (3.6) presents the results of additional spending on 4th grade math pass rates in Vermont to Papke (2005) results quantifying the impact of additional spending on 4th grade math pass rates in Michigan. Papke predicts that a 10% increase in spending leads to a 3.73 percentage point increase in the pass rate. Papke (2008) finds similar results when spending and achievement are measured at the district level. I predict that a 10% increase in spending leads to a 2.28 percentage point increase in the pass rate. An increase of 2.28 percentage points is about 0.14 standard deviations. Guryan (2003) finds that a $1000 increase in spending led in Massachusetts led to a 0.5 standard deviation increase in mean test scores across subjects. Guryan (2003) estimates suggest that a 10% increase in spending increases test scores by about 0.20 standard deviations.

3.5.1 Spending Including Private Donations

Under Act 60, a number of towns attempted to avoid the sharing pool and collected private donations to fund local public schools. Data on these private donations is available from 2000 - 2004. Including local spending per equalized pupil and private donations does not alter the general conclusions. Tables (3.7) - (3.10) present the results using data from 2000 - 2004 when private donations data are available. Table (3.7) looks at 4th grade math pass rates. The results of a Hausman specification test in Columns (5) and (6) of Table (3.7) clearly indicate that spending when private donations are included should be treated as endogenous in determining 4th grade

[36]The fact that spending only appears endogenous in the 4th grade math regressions may indicate that the spending is in fact endogenous for math scores and the difference in the OLS and IV fixed effects regressions is not resulting from measurement error in spending. If math scores are viewed by the school and community as a better indicator of student learning, more resources may be devoted when it is expected that math scores would be lower.

math test score pass rates. The magnitude of the coefficient is stronger, suggesting that a 10% increase in spending including private donations led to a 4.38 - 6.08 percentage point, or roughly 0.30 standard deviation, increase in math pass rates. Again, however, this result is only significant at the 10% level. In Tables (3.8) - (3.10), Hausman specification tests indicate that spending with private donations does not appear endogenous when estimating 4th grade writing or reading or 2nd grade reading pass rates. Furthermore, none of the fixed effects or first difference specifications where spending is treated as exogenous (Columns (2) and (3) in Tables (3.8) - (3.10)) indicate that spending has a significant impact on reading or writing pass rates.

3.5.2 Initially High Spending versus Initially Low Spending

Table (3.11) presents the results from splitting the sample into two groups based on 1997 (pre-Act 60) spending levels for 4th grade math pass rates. Schools that were initially high (low) spending were those that were spending above (below) the median in 1997. These results explore the hypothesis that under-funded schools may experience higher returns to additional funds. Since spending was found to be endogenous in the process determining 4th grade math pass rates above fixed effects and first difference results are presented for both the case when spending is treated as being endogenous and when spending is treated as exogenous. The top portion of Table (3.11) presents the results using local spending per equalized pupil as the measure of resources. The bottom portion of Table (3.11) presents the results using local spending per equalized pupil as well as private donations to measure resources.

Looking at the top portion of Table (3.11), the coefficient on local spending per equalized pupil appears positive but statistically insignificant at the 10% level in all

but the first difference instrumental variable specification. There does not appear to be any evidence that schools that were low spending pre-reform responded differently to additional funds post-reform. From the bottom panel of Table (3.11), the Hausman specification test confirms that spending should be treated as being endogenous for schools in initially high spending towns. The coefficient estimates on local spending per equalized pupil are similar to those obtained in the full sample but are statistically insignificant at the 10% level in the smaller sample. For initially low spending schools, spending plus private donations appears to have a significant impact on 4th grade math pass rates in the fixed effects, fixed effects with instrumental variables, and first difference with instrumental variables specification. The t-statistic in the first difference specification is smaller than in the fixed effects specification. However, since the point estimates are relatively close and first difference specifications are less efficient it seems reasonable to conclude that when private donations are included, additional spending does have a positive and significant impact on initially low spending schools. This is not to say, however, that the results of additional spending in initially low spending schools is different from additional spending in initially high spending schools. Although the coefficient on spending with private donations is only significant in the initially low spending sample, the magnitude of the coefficients in both samples is very similar.

Table (3.12) contains the results of fixed effects and first difference estimates for reading and writing tests where spending is treated as exogenous.[37] In the top panel of Table (3.12), where writing and reading test pass rates are regressed on local spending per equalized pupil and other controls, spending is never statistically

[37]Hausman specification tests suggest that OLS is appropriate in these cases. This was expected as endogeneity of spending was not detected in the full sample specifications above.

significantly different from zero. In the bottom panel of Table (3.12), where test writing and reading test pass rates are regressed on local spending per equalized pupil plus private donations and other controls, a pattern begins to emerge between initially low spending and initially high spending groups. For all writing and reading test pass rates, the coefficient on spending plus private donations is positive and larger for the initially low spending group. For 4th grade writing, the coefficient on spending plus private donations for initially high spending schools is negative and significant while the coefficient on spending for the initially low spending schools is positive but not significantly different from zero at the 10% level. Given the small sample sizes, this pattern is suggestive but certainly not conclusive.

3.5.3 Initially High Achieving versus Initially Low Achieving

For schools that are initially high achieving, additional dollars may be less likely to lead to improvements in the pass rate on tests of basic skills. Table (3.13) presents the results from splitting the sample into two groups. The initially high achieving schools are those that had a median pass rate above 45.67% or where pass rates were not available in 1999. Out of the 105 schools that were classified as being low achieving based on their math pass rate in 1999, 69 of these schools were also categorized as being low spending. Hence, low achieving schools are more likely to be low spending schools, but the correlation is far from perfect. Looking at the upper portion of Table (3.13), the Hausman specification test in Columns (3) and (4) suggests that spending is likely endogenous for initially high achieving schools. For initially high achieving schools, the fixed effects instrumental variables specification suggests that a 10% increase in spending leads to a 3.4 percentage point increase in the math pass rate. This result is only significant at the 10% level. The magnitude of the first

difference instrumental variables estimate is slightly larger but the accompanying larger standard errors render that result insignificant at conventional levels. When looking at initially low achieving schools, there is no evidence that additional spending leads increasing 4th grade math pass rates.

Similar conclusions can be drawn looking at the lower panel in Table (3.13) where spending and private donations are included. The magnitude of the coefficient on spending with private donations is larger than the one on spending alone but it is statistically insignificant in the fixed effects with instrumental variables and the first difference with instrumental variables specifications. Spending including private donations remains insignificant for schools that were initially low achieving with respect to 4th grade math pass rates.

These results differ dramatically from those in Papke (2005) and Papke (2008). Papke (2005)'s fixed effects with instrumental variables coefficient on spending was nearly four times larger for schools that were initially low performing versus those that were initially high performing. According to Papke (2005), her results "lend support to policies that increase spending at low-performing schools." It appears that increased spending at low performing schools may be more effective in increasing 4th grade math pass rates than increased spending at high performing schools in Michigan, but this does not appear to be the case in Vermont.

Table (3.14) again splits the sample into groups that were initially high achieving versus those that were initially low achieving based on writing and reading test scores. Out of the 105 schools that were initially low achieving in 4th grade math, 74 were also low achieving in 4th grade writing and 79 low achieving in 4th grade reading. Sixty-four of the 105 schools that were low achieving in 4th grade math were low achieving on all 4th grade tests. Table (3.14) does not provide any evidence that

money is more effective at improving reading or writing test scores for schools that were initially low achieving as opposed to those that were initially high achieving. In fact, spending and spending plus private donations are statistically insignificant in all of the specifications in Table (3.14).

3.6 Conclusion

Policy changes in Vermont provide a unique opportunity for studying the effects of education spending on student achievement as measured by the pass rate on standardized tests. This study is the first to explore the impacts of funding changes under Vermont's Act 60 on student achievement. The availability of annual data allows for the use of fixed effects which I supplement with a first difference analysis. The design of Act 60 allowed the tax rate to be used as an instrumental variable to identify spending in the structural equation.

The evidence suggests that spending may have had an impact on 4th grade math test pass rates. A 10% increase in spending increased 4th grade math pass rates by 2 - 6 percentage points (depending on what type of spending is analyzed). The effect, however, is only significant at the 10% level. The finding that spending may have significantly increased performance on 4th grade math tests is consistent with other findings in the literature. However, I do not find evidence that spending led to changes in writing or reading test pass rates.

While others have found that spending in under-funded or poorly performing schools results in greater returns (greater test score gains) this research does not suggest this was the case in Vermont. There is moderate evidence that school that were initially low spending were more responsive to additional funds, but the results

are not conclusive.

Overall, these results underscore the importance of looking at multiple measures of performance or to take care when interpreting the impact of spending in studies that focus only on mathematics test scores. Data limitations prevented precision in this research. States should continue to make efforts to track classroom level resources which would help researchers specifically quantify the impact of additional resources on student achievement.

Table 3.1: Summary Statistics

	1999	2000	2001	2002	2003	2004
Local Spending per Equalized Pupil						
Mean	$7,124.73	$7,073.31	$7,320.22	$7,767.05	$7,975.16	$8,246.21
Standard Deviation	$1,096.58	$920.57	$994.66	$1,119.13	$1,160.83	$1,162.32
N	236	236	236	236	236	236
Local Spending + Private Contributions						
Mean		$7,177.62	$7,453.81	$7,910.40	$8,131.81	$8,394.65
Standard Deviation		$920.23	$966.13	$1,083.41	$1,101.54	$1,102.74
N		235	235	235	235	235
School Property Tax Rate						
Mean	1.42	1.44	1.52	1.56	1.61	1.59
Standard Deviation	0.25	0.22	0.25	0.27	0.29	0.26
N	236	236	236	236	236	236
4th Grade Math Pass Rate						
Mean	47.22	47.63	47.31	50.64	52.37	57.69
Standard Deviation	15.18	16.05	16.14	17.23	15.76	16.86
N	215	215	217	216	183	181
4th Grade Writing Pass Rate						
Mean	57.96	53.72	55.76	58.06	60.97	67.05
Standard Deviation	15.27	16.52	15.79	16.09	15.40	15.49
N	215	215	218	215	183	181
4th Grade Reading Pass Rate						
Mean	77.18	73.63	73.76	73.86	75.28	75.60
Standard Deviation	12.70	14.47	13.56	12.64	11.98	13.62
N	215	215	218	215	183	181
2nd Grade Reading Pass Rate						
Mean	71.83	75.49	77.52	80.03	82.28	81.87
Standard Deviation	14.97	14.51	14.30	12.40	11.42	10.76
N	223	220	221	217	185	184
% Eligible for Free / Reduced Price Lunch						
Mean	25.96	30.68	30.29	30.84	31.81	34.41
Standard Deviation	16.99	17.34	16.80	16.91	17.72	17.08
N	234	234	234	234	234	234
School Enrollment						
Mean	250.55	249.92	246.97	242.52	234.19	229.34
Standard Deviation	190.86	197.05	203.00	202.31	192.84	190.38
N	236	236	236	236	236	236

School spending, tax rate, and demographic data are summarized in each year for each school where at least one year's pass rates are available. In 2003 and 2004, test pass rate data is only available for schools where at least 10 students took the test. Spending is reported in 2004 dollars.

Table 3.2: 4th Grade Math

Dependent Variable: 4th Grade Math Pass Rate

	(1) OLS	(2) FE	(3) FD	(4) IV	(5) FE IV	(6) FD IV
log(spending)	2.54	5.92	7.42	6.89	22.78	30.35
	0.50	*0.96*	*0.81*	*0.94*	*1.76*	*1.68*
% free/reduced lunch	-0.57	-0.18	-0.04	-0.55	-0.21	-0.06
	-4.37	*-0.95*	*-0.20*	*-4.29*	*-1.08*	*-0.27*
% free/reduced lunch^2	0.00	0.00	0.00	0.00	0.00	0.00
	1.87	*0.55*	*0.04*	*1.84*	*0.61*	*0.07*
log(school size)	0.84	75.93	43.14	-0.24	68.26	33.78
	0.08	*2.02*	*0.77*	*-0.02*	*1.81*	*0.61*
log(school size)^2	-0.21	-7.16	-3.93	-0.11	-6.44	-3.09
	-0.22	*-2.18*	*-0.82*	*-0.11*	*-1.95*	*-0.65*
Obs.	1225	1225	982	1225	1221	982
R^2	0.18	0.11	0.02	0.18	0.10	0.01
Groups		225			221	
Shea's R^2				0.51	0.21	0.24
First Stage F				32.14	10.40	21.16
p value				*0.00*	*0.00*	*0.00*
Hausman Test				1.74	2.94	4.06
p value				*0.19*	*0.09*	*0.04*
Hansen J Statistic				0.49	0.86	2.05
p value				*0.97*	*0.93*	*0.73*
C-Statistic				0.00	0.60	1.08
p value				*0.98*	*0.44*	*0.30*

Coefficients are in plain font and t-statistics are italicized. Reported t-statistics are calculated using standard errors clustered at the town level and robust to heteroskedasticity and within-school serial correlation. Constants and time dummies are included but not reported. For the fixed-effects specifications R^2 values are net of school fixed effects.

Table 3.3: 4th Grade Writing

Dependent Variable: 4th Grade Writing

	(1) OLS	(2) FE	(3) FD	(4) IV	(5) FE IV	(6) FD IV
log(spending)	-2.71	0.46	0.76	-9.45	0.24	-3.57
	-0.60	*0.08*	*0.11*	*-1.59*	*0.02*	*-0.21*
% free/reduced lunch	-0.60	-0.29	-0.13	-0.63	-0.29	-0.13
	-5.47	*-1.90*	*-0.55*	*-5.69*	*-1.90*	*-0.55*
% free/reduced lunch^2	0.00	0.00	0.00	0.00	0.00	0.00
	2.65	*1.61*	*0.50*	*2.73*	*1.61*	*0.50*
log(school size)	0.43	38.59	45.08	2.12	38.69	46.93
	0.04	*1.09*	*1.08*	*0.20*	*1.10*	*1.11*
log(school size)^2	-0.17	-4.00	-3.97	-0.34	-4.01	-4.13
	-0.18	*-1.34*	*-1.13*	*-0.34*	*-1.35*	*-1.16*
Obs.	1225	1225	982	1225	1221	982
R^2	0.18	0.12	0.04	0.17	0.12	0.04
Groups		225			221	
Shea's R^2				0.51	0.21	0.24
First Stage F				32.02	10.25	21.03
p value				*0.00*	*0.00*	*0.00*
Hausman Test				4.33	0.00	0.12
p value				*0.04*	*0.98*	*0.73*
Hansen J Statistic				1.82	2.59	2.90
p value				*0.77*	*0.63*	*0.58*
C-Statistic				0.63	0.38	0.40
p value				*0.43*	*0.54*	*0.53*

Coefficients are in plain font and t-statistics are italicized. Reported t-statistics are calculated using standard errors clustered at the town level and robust to heteroskedasticity and within-school serial correlation. Constants and time dummies are included but not reported. For the fixed-effects specifications R^2 values are net of school fixed effects.

Table 3.4: 4th Grade Reading

Dependent Variable: 4th Grade Reading

	(1) OLS	(2) FE	(3) FD	(4) IV	(5) FE IV	(6) FD IV
log(spending)	4.42	4.62	0.43	0.68	2.38	-0.88
	1.34	*1.16*	*0.07*	*0.13*	*0.23*	*-0.07*
% free/reduced lunch	-0.48	-0.22	-0.12	-0.49	-0.21	-0.11
	-5.65	*-1.56*	*-0.67*	*-5.82*	*-1.52*	*-0.66*
% free/reduced lunch^2	0.00	0.00	0.00	0.00	0.00	0.00
	1.92	*1.18*	*0.60*	*1.97*	*1.16*	*0.59*
log(school size)	1.20	45.19	64.71	2.14	46.22	65.27
	0.14	*1.68*	*1.60*	*0.25*	*1.72*	*1.61*
log(school size)^2	-0.25	-4.06	-5.61	-0.34	-4.16	-5.66
	-0.33	*-1.80*	*-1.64*	*-0.45*	*-1.85*	*-1.65*
Obs.	1225	1225	982	1225	1221	982
R^2	0.20	0.03	0.02	0.20	0.03	0.02
Groups		225			221	
Shea's R^2				0.51	0.21	0.24
First Stage F				32.02	10.25	21.03
p value				*0.00*	*0.00*	*0.00*
Hausman Test				2.09	0.07	0.02
p value				*0.15*	*0.79*	*0.90*
Hansen J Statistic				0.55	1.88	0.99
p value				*0.97*	*0.76*	*0.91*
C-Statistic				0.16	0.00	0.14
p value				*0.69*	*1.00*	*0.71*

Coefficients are in plain font and t-statistics are italicized. Reported t-statistics are calculated using standard errors clustered at the town level and robust to heteroskedasticity and within-school serial correlation. Constants and time dummies are included but not reported. For the fixed-effects specifications R^2 values are net of school fixed effects.

Table 3.5: 2nd Grade Reading

Dependent Variable: 2nd Grade Reading

	(1) OLS	(2) FE	(3) FD	(4) IV	(5) FE IV	(6) FD IV
log(spending)	-3.05	9.30	5.14	-10.21	-7.01	13.60
	-0.78	*1.92*	*0.87*	*-1.68*	*-0.61*	*0.91*
% free/reduced lunch	-0.28	-0.47	-0.26	-0.30	-0.45	-0.27
	-2.85	*-2.70*	*-1.52*	*-3.02*	*-2.63*	*-1.55*
% free/reduced lunch^2	0.00	0.00	0.00	0.00	0.00	0.00
	0.75	*2.51*	*1.51*	*0.82*	*2.53*	*1.50*
log(school size)	2.44	-25.23	-56.34	4.26	-19.35	-59.68
	0.28	*-0.83*	*-1.29*	*0.49*	*-0.61*	*-1.33*
log(school size)^2	-0.17	1.63	4.22	-0.35	1.10	4.50
	-0.22	*0.65*	*1.19*	*-0.44*	*0.42*	*1.24*
Obs.	1242	1242	988	1242	1238	988
R^2	0.14	0.14	0.01	0.14	0.13	0.01
Groups		228			224	
Shea's R^2				0.50	0.21	0.25
First Stage F				26.04	7.29	19.59
p value				*0.00*	*0.00*	*0.00*
Hausman Test				6.48	3.20	0.68
p value				*0.01*	*0.07*	*0.41*
Hansen J Statistic				2.26	1.60	3.17
p value				*0.69*	*0.81*	*0.53*
C-Statistic				0.21	0.01	0.01
p value				*0.65*	*0.92*	*0.94*

Coefficients are in plain font and t-statistics are italicized. Reported t-statistics are calculated using standard errors clustered at the town level and robust to heteroskedasticity and within-school serial correlation. Constants and time dummies are included but not reported. For the fixed-effects specifications R^2 values are net of school fixed effects.

Table 3.6: Results Compared to Papke (2005)

Dependent Variable: 4th Grade Math Pass Rate								
	Results from Table 2				Results from Papke (2005)			
	OLS	FE	IV	FE IV	OLS	FE	IV	FE IV
log(spending)	2.54	5.92	6.89	22.78	8.44	7.18	22.16	37.31
	0.50	*0.96*	*0.94*	*1.76*	*4.91*	*2.45*	*4.04*	*2.39*
% free/reduced lunch	-0.57	-0.18	-0.55	-0.21	-0.58	-0.15	-0.60	-0.09
	-4.37	*-0.95*	*-4.29*	*-1.08*	*-13.81*	*-1.39*	*-11.23*	*-0.70*
% free/reduced lunch^2	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	1.87	*0.55*	*1.84*	*0.61*	*3.33*	*3.17*	*1.20*	*0.55*
log(school size)	0.84	75.93	-0.24	68.26	-4.99	-14.05	-16.74	15.50
	0.08	*2.02*	*-0.02*	*1.81*	*-0.62*	*-0.69*	*-1.53*	*0.33*
log(school size)^2	-0.21	-7.16	-0.11	-6.44	0.29	1.00	1.37	-0.83
	-0.22	*-2.18*	*-0.11*	*-1.95*	*0.71*	*1.75*	*0.97*	*3.81*
log(per-pupil exp. 1994)							-13.51	
							4.87	
y96*(log per-pupil exp. 1994)							10.76	13.82
							2.91	*4.02*
y97*(log per-pupil exp. 1994)							11.69	14.57
							3.02	*4.66*
y98*(log per-pupil exp. 1994)							6.33	9.46
							3.51	*5.24*
Obs.	1225	1225	1225	1221	7242	7242	4853	4853
R^2	0.18	0.18	0.11	0.10	0.40	0.36	0.36	0.22
School Level Fixed Effects		yes		yes		yes		yes

Coefficients are in plain font and t-statistics are italicized. Reported t-statistics are robust to heteroskedasticity and within-school serial correlation. Constants and time dummies are included but not reported. For the fixed-effects specifications R^2 values are net of school fixed effects. Papke's (2005) instrumental variable was the log of the foundation grant and the log of the foundation grant interacted with year dummies.

Table 3.7: 4th Grade Math with Private Contributions

Dependent Variable: 4th Grade Math Pass Rate

	(1) OLS	(2) FE	(3) FD	(4) IV	(5) FE IV	(6) FD IV
log(spending+private)	2.45	6.88	2.28	10.93	43.81	60.84
	0.36	*0.65*	*0.17*	*1.05*	*1.87*	*1.81*
% free/reduced lunch	-0.52	-0.01	0.01	-0.48	-0.01	0.01
	-3.60	*-0.05*	*0.04*	*-3.35*	*-0.04*	*0.04*
% free/reduced lunch^2	0.00	0.00	0.00	0.00	0.00	0.00
	1.40	*-0.30*	*-0.17*	*1.32*	*-0.36*	*-0.24*
log(school size)	3.44	64.98	46.09	1.10	70.24	39.59
	0.31	*1.37*	*0.69*	*0.10*	*1.43*	*0.62*
log(school size)^2	-0.42	-6.43	-4.26	-0.19	-6.77	-3.69
	-0.42	*-1.57*	*-0.75*	*-0.19*	*-1.59*	*-0.68*
Obs.	1005	1005	769	1005	1001	769
R^2	0.17	0.12	0.02	0.16	0.10	-0.01
Groups		222			218	
Shea's R^2				0.40	0.25	0.23
First Stage F				14.60	9.03	15.46
p value				*0.00*	*0.00*	*0.00*
Hausman Test				2.53	4.64	7.20
p value				*0.11*	*0.03*	*0.01*
Hansen J Statistic				0.77	0.38	3.95
p value				*0.94*	*0.98*	*0.41*
C-Statistic				0.33	0.34	2.95
p value				*0.57*	*0.56*	*0.09*

Coefficients are in plain font and t-statistics are italicized. Reported t-statistics are calculated using standard errors clustered at the town level and robust to heteroskedasticity and within-school serial correlation. Constants and time dummies are included but not reported. For the fixed-effects specifications R^2 values are net of school fixed effects.

Table 3.8: 4th Grade Writing with Private Contributions

Dependent Variable:4th Grade Writing

	(1) OLS	(2) FE	(3) FD	(4) IV	(5) FE IV	(6) FD IV
log(spending+private)	-0.01	-5.98	-3.30	-10.43	-1.70	6.63
	0.00	*-0.55*	*-0.26*	*-1.26*	*-0.08*	*0.22*
% free/reduced lunch	-0.56	-0.08	0.05	-0.61	-0.08	0.05
	-4.99	*-0.37*	*0.17*	*-5.36*	*-0.37*	*0.17*
% free/reduced lunch^2	0.00	0.00	0.00	0.00	0.00	0.00
	2.44	*-0.16*	*-0.32*	*2.58*	*-0.17*	*-0.33*
log(school size)	-1.79	7.20	51.85	1.10	7.80	50.74
	-0.17	*0.17*	*1.06*	*0.10*	*0.19*	*1.04*
log(school size)^2	0.04	-1.49	-4.48	-0.25	-1.53	-4.38
	0.04	*-0.43*	*-1.09*	*-0.25*	*-0.44*	*-1.07*
Obs.	1005	1005	769	1005	1001	769
R^2	0.18	0.14	0.01	0.17	0.14	0.01
Groups		222			218	
Shea's R^2				0.40	0.25	0.23
First Stage F				14.63	8.98	15.29
p value				*0.00*	*0.00*	*0.00*
Hausman Test				4.06	0.06	0.18
p value				*0.04*	*0.81*	*0.67*
Hansen J Statistic				1.20	2.64	3.55
p value				*0.88*	*0.62*	*0.47*
C-Statistic				0.02	0.12	0.77
p value				*0.90*	*0.73*	*0.38*

Coefficients are in plain font and t-statistics are italicized. Reported t-statistics are calculated using standard errors clustered at the town level and robust to heteroskedasticity and within-school serial correlation. Constants and time dummies are included but not reported. For the fixed-effects specifications R^2 values are net of school fixed effects.

Table 3.9: 4th Grade Reading with Private Contributions

Dependent Variable: 4th Grade Reading

	(1) OLS	(2) FE	(3) FD	(4) IV	(5) FE IV	(6) FD IV
log(spending+private)	8.84	6.41	9.18	2.05	1.33	0.80
	2.03	*0.61*	*0.71*	*0.30*	*0.07*	*0.03*
% free/reduced lunch	-0.41	-0.13	-0.05	-0.45	-0.13	-0.05
	-4.92	*-0.73*	*-0.25*	*-5.16*	*-0.73*	*-0.25*
% free/reduced lunch^2	0.00	0.00	0.00	0.00	0.00	0.00
	1.39	*0.58*	*0.58*	*1.51*	*0.59*	*0.61*
log(school size)	-1.97	23.93	45.16	-0.09	23.22	46.10
	-0.23	*0.83*	*0.96*	*-0.01*	*0.80*	*0.98*
log(school size)^2	0.04	-2.49	-3.94	-0.15	-2.44	-4.02
	0.05	*-1.03*	*-1.01*	*-0.19*	*-1.01*	*-1.03*
Obs.	1005	1005	769	1005	1001	769
R^2	0.20	0.01	0.00	0.19	0.01	0.00
Groups		222			218	
Shea's R^2				0.40	0.25	0.23
First Stage F				14.63	8.98	15.29
p value				*0.00*	*0.00*	*0.00*
Hausman Test				2.71	0.12	0.19
p value				*0.10*	*0.73*	*0.67*
Hansen J Statistic				0.31	1.87	1.25
p value				*0.99*	*0.76*	*0.87*
C-Statistic				0.01	0.00	0.52
p value				*0.93*	*1.00*	*0.47*

Coefficients are in plain font and t-statistics are italicized. Reported t-statistics are calculated using standard errors clustered at the town level and robust to heteroskedasticity and within-school serial correlation. Constants and time dummies are included but not reported. For the fixed-effects specifications R^2 values are net of school fixed effects.

Table 3.10: 2nd Grade Reading with Private Contributions

Dependent Variable: 2nd Grade Reading

	(1) OLS	(2) FE	(3) FD	(4) IV	(5) FE IV	(6) FD IV
log(spending+private)	-11.12	3.45	2.70	-15.81	-10.30	10.98
	-2.29	*0.33*	*0.18*	*-1.86*	*-0.49*	*0.42*
% free/reduced lunch	-0.26	-0.32	-0.33	-0.28	-0.31	-0.33
	-2.62	*-1.63*	*-1.67*	*-2.66*	*-1.61*	*-1.66*
% free/reduced lunch^2	0.00	0.00	0.00	0.00	0.00	0.00
	0.46	*1.35*	*1.17*	*0.52*	*1.37*	*1.12*
log(school size)	0.22	-18.83	-37.73	1.62	-21.13	-38.93
	0.03	*-0.60*	*-0.85*	*0.19*	*-0.69*	*-0.87*
log(school size)^2	-0.03	0.98	2.75	-0.17	1.17	2.85
	-0.05	*0.38*	*0.76*	*-0.22*	*0.46*	*0.78*
Obs.	1015	1015	770	1015	1011	770
R^2	0.11	0.08	0.01	0.11	0.08	0.01
Groups		224			220	
Shea's R^2				0.38	0.22	0.22
First Stage F				12.74	6.14	13.38
p value				*0.00*	*0.00*	*0.00*
Hausman Test				1.15	0.73	0.19
p value				*0.28*	*0.39*	*0.67*
Hansen J Statistic				3.53	4.13	3.17
p value				*0.47*	*0.39*	*0.53*
C-Statistic				1.42	0.62	0.28
p value				*0.23*	*0.43*	*0.60*

Coefficients are in plain font and t-statistics are italicized. Reported t-statistics are calculated using standard errors clustered at the town level and robust to heteroskedasticity and within-school serial correlation. Constants and time dummies are included but not reported. For the fixed-effects specifications R^2 values are net of school fixed effects.

Table 3.11: 4th Grade Math: Initially High versus Initially Low Spending

Dependent Variable: 4th Grade Math Pass Rate

	High Spending				Low Spending			
	(1) FE	(2) FD	(3) FE IV	(4) FD IV	(5) FE	(6) FD	(7) FE IV	(8) FD IV
log(spending)	2.53	5.96	14.65	5.96	14.36	9.64	24.55	37.45
	0.35	*0.54*	*1.04*	*0.54*	*1.13*	*0.64*	*1.46*	*1.69*
% free/reduced lunch	-0.13	0.09	-0.16	0.09	-0.33	-0.05	-0.35	-0.07
	-0.47	*0.24*	*-0.58*	*0.24*	*-1.32*	*-0.21*	*-1.35*	*-0.26*
% free/reduced lunch^2	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	0.54	*-0.61*	*0.60*	*-0.61*	*0.58*	*0.21*	*0.62*	*0.25*
log(school size)	64.36	50.86	57.74	50.86	105.76	4.74	107.54	1.22
	1.69	*0.78*	*1.52*	*0.78*	*1.45*	*0.06*	*1.48*	*0.02*
log(school size)^2	-5.68	-4.15	-5.06	-4.15	-10.62	-1.11	-10.74	-0.80
	-1.82	*-0.76*	*-1.63*	*-0.76*	*-1.68*	*-0.16*	*-1.71*	*-0.12*
Obs.	596	594	470	470	629	512	627	512
R^2	0.1	0.05	0.09	0.04	0.16	0.02	0.16	0.01
Groups	112		110		113		111	
Shea's R^2			0.25	0.24			0.36	0.33
First Stage F			11.44	15.50			4.46	11.31
p value			*0.00*	*0.00*			*0.00*	*0.00*
Hausman Test			1.26	1.93			0.46	1.93
p value			*0.26*	*0.17*			*0.50*	*0.16*
Hansen J Statistic			2.27	3.77			5.98	6.02
p value			*0.69*	*0.44*			*0.20*	*0.20*
C-Statistic			1.35	3.25			2.51	2.01
p value			*0.25*	*0.07*			*0.11*	*0.16*

	High Spending				Low Spending			
	(1) FE	(2) FD	(3) FE IV	(4) FD IV	(5) FE	(6) FD	(7) FE IV	(8) FD IV
log(spending + private)	-14.15	-17.69	41.39	54.00	33.87	28.19	46.65	61.33
	-0.91	*-0.90*	*1.08*	*1.05*	*2.45*	*1.61*	*2.53*	*2.51*
% free/reduced lunch	0.45	0.87	0.48	0.94	-0.38	-0.35	-0.38	-0.38
	1.24	*2.14*	*1.26*	*2.16*	*-1.33*	*-1.30*	*-1.35*	*-1.37*
% free/reduced lunch^2	-0.01	-0.01	-0.01	-0.02	0.00	0.00	0.00	0.00
	-1.35	*-2.92*	*-1.32*	*-2.89*	*0.70*	*1.27*	*0.70*	*1.30*
log(school size)	60.36	37.61	60.96	26.84	72.16	40.99	77.86	40.59
	1.24	*0.52*	*1.20*	*0.38*	*0.91*	*0.41*	*1.01*	*0.43*
log(school size)^2	-5.42	-2.94	-5.21	-1.98	-8.11	-4.58	-8.60	-4.56
	-1.36	*-0.49*	*-1.24*	*-0.34*	*-1.19*	*-0.53*	*-1.30*	*-0.56*
Obs.	485	364	483	364	520	405	518	405
R^2	0.11	0.08	0.07	0.03	0.17	0.03	0.17	0.02
Groups	110		108		112		110	
Shea's R^2			0.22	0.20			0.34	0.33
First Stage F			7.34	9.18			2.61	9.23
p value			*0.00*	*0.00*			*0.03*	*0.00*
Hausman Test			4.19	4.08			0.43	2.00
p value			*0.04*	*0.04*			*0.51*	*0.16*
Hansen J Statistic			1.04	4.06			5.14	4.57
p value			*0.90*	*0.40*			*0.27*	*0.33*
C-Statistic			0.34	4.01			1.86	0.44
p value			*0.56*	*0.05*			*0.17*	*0.51*

Coefficients are in plain font and t-statistics are italicized. Reported t-statistics are calculated using standard errors clustered at the town level and robust to heteroskedasticity and within-school serial correlation. Constants and time dummies are included but not reported. For the fixed-effects specifications R^2 values are net of school fixed effects.

Table 3.12: Reading and Writing: Initially High versus Initially Low Spending

	4th Grade Writing				4th Grade Reading				2nd Grade Reading			
	High Spending		Low Spending		High Spending		Low Spending		High Spending		Low Spending	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	FE	FD	FE	FD	FE	FD	FE	FD	FE	FD	FE	FD
log(spending)	3.29	2.61	-2.68	11.23	3.67	2.09	10.88	8.27	8.34	1.70	-0.44	-1.67
	0.50	*0.31*	*-0.19*	*0.64*	*0.87*	*0.30*	*0.82*	*0.56*	*1.65*	*0.29*	*-0.03*	*-0.07*
% free/reduced lunch	-0.30	-0.17	-0.31	-0.02	-0.12	0.07	-0.28	-0.29	-0.34	0.01	-0.43	-0.23
	-1.34	*-0.41*	*-1.28*	*-0.07*	*-0.72*	*0.29*	*-1.21*	*-1.13*	*-1.21*	*0.02*	*-1.88*	*-0.89*
% free/reduced lunch^2	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	0.83	*0.14*	*1.32*	*0.43*	*0.11*	*-0.38*	*1.14*	*1.30*	*0.53*	*-0.80*	*2.06*	*1.36*
log(school size)	32.95	37.00	52.58	79.98	55.74	89.32	11.78	1.07	-10.28	-63.83	-61.78	-60.61
	0.84	*0.63*	*0.72*	*0.96*	*1.62*	*1.76*	*0.24*	*0.01*	*-0.25*	*-1.13*	*-1.57*	*-0.97*
log(school size)^2	-3.30	-4.24	-5.46	-5.62	-4.89	-7.70	-1.18	0.05	0.27	4.85	4.70	4.57
	-1.02	*-0.81*	*-0.88*	*-0.79*	*-1.67*	*-1.77*	*-0.29*	*0.01*	*0.08*	*1.03*	*1.47*	*0.90*
Obs.	597	471	628	511	597	471	628	511	602	474	640	514
R^2	0.11	0.03	0.15	0.07	0.03	0.02	0.05	0.05	0.10	0.02	0.20	0.03
Groups	112		113		112		113		113		115	

	4th Grade Writing				4th Grade Reading				2nd Grade Reading			
	High Spending		Low Spending		High Spending		Low Spending		High Spending		Low Spending	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	FE	FD	FE	FD	FE	FD	FE	FD	FE	FD	FE	FD
log(spending + private)	-29.06	-28.55	23.16	26.97	-6.09	0.21	22.53	21.69	1.48	-8.15	4.00	17.80
	-2.12	*-1.81*	*1.36*	*1.31*	*-0.49*	*0.01*	*1.37*	*1.19*	*0.12*	*-0.48*	*0.22*	*0.68*
% free/reduced lunch	-0.03	0.47	0.09	0.13	0.01	0.31	-0.23	-0.33	0.02	0.12	-0.48	-0.53
	-0.07	*0.97*	*0.34*	*0.47*	*0.03*	*1.00*	*-0.79*	*-1.11*	*0.06*	*0.30*	*-2.37*	*-2.47*
% free/reduced lunch^2	0.00	-0.01	0.00	0.00	0.00	0.00	0.00	0.00	0.00	-0.01	0.00	0.00
	-0.92	*-1.97*	*-0.44*	*-0.10*	*-0.24*	*-0.95*	*0.71*	*1.39*	*-0.40*	*-0.83*	*2.56*	*2.63*
log(school size)	6.54	39.25	-5.78	62.97	16.47	56.82	29.75	-3.07	14.08	-36.21	-68.12	-46.06
	0.14	*0.65*	*-0.08*	*0.68*	*0.48*	*1.00*	*0.53*	*-0.04*	*0.33*	*-0.66*	*-1.50*	*-0.69*
log(school size)^2	-1.21	-3.93	-0.55	-4.53	-1.70	-4.73	-3.12	0.06	-1.99	2.66	5.03	3.33
	-0.30	*-0.77*	*-0.09*	*-0.59*	*-0.59*	*-1.01*	*-0.68*	*0.01*	*-0.54*	*0.58*	*1.37*	*0.61*
Obs.	486	365	519	404	486	365	519	404	488	368	527	402
R^2	0.13	0.04	0.19	0.02	0.02	0.02	0.04	0.02	0.08	0.02	0.10	0.01
Groups	110		112		110		112		110		114	

Coefficients are in plain font and t-statistics are italicized. Reported t-statistics are calculated using standard errors clustered at the town level and robust to heteroskedasticity and within-school serial correlation. Constants and time dummies are included but not reported. For the fixed-effects specifications R^2 values are net of school fixed effects.

Table 3.13: 4th Grade Math: Initially High versus Initially Low Achieving

Dependent Variable: 4th Grade Math Pass Rate

	Initially High Achieving				Initially Low Achieving			
	(1) FE	(2) FD	(3) FE IV	(4) FD IV	(5) FE	(6) FD	(7) FE IV	(8) FD IV
log(spending)	4.83	14.28	33.95	43.24	-0.02	-7.32	10.79	14.44
	0.53	*1.17*	*1.75*	*1.58*	*0.00*	*-0.66*	*0.75*	*0.72*
% free/reduced lunch	0.12	0.15	0.07	0.10	-0.30	0.05	-0.32	0.05
	0.47	*0.46*	*0.27*	*0.28*	*-1.16*	*0.18*	*-1.23*	*0.17*
% free/reduced lunch^2	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	-0.47	*-0.14*	*-0.39*	*-0.04*	*0.48*	*-0.84*	*0.54*	*-0.81*
log(school size)	81.77	73.84	70.24	66.23	38.36	-12.60	35.94	-23.20
	1.59	*1.03*	*1.34*	*0.94*	*0.93*	*-0.16*	*0.88*	*-0.30*
log(school size)^2	-7.17	-6.02	-6.00	-5.27	-3.91	0.57	-3.75	1.41
	-1.65	*-1.01*	*-1.36*	*-0.91*	*-1.06*	*0.08*	*-1.02*	*0.21*
Obs.	629	499	626	499	596	483	595	483
R^2	0.09	0.07	0.05	0.05	0.24	0.02	0.23	0.01
Groups	120		117		105		104	
Shea's R^2			0.21	0.22			0.26	0.31
First Stage F			6.07	9.23			5.83	20.56
p value			*0.00*	*0.00*			*0.00*	*0.00*
Hausman Test			4.33	3.05			0.79	2.30
p value			*0.04*	*0.08*			*0.37*	*0.13*
Hansen J Statistic			0.74	3.62			1.13	2.02
p value			*0.74*	*0.46*			*0.89*	*0.73*
C-Statistic			0.86	2.91			0.00	1.28
p value			*0.36*	*0.09*			*0.95*	*0.26*

	Initially High Achieving				Initially Low Achieving			
	(1) FE	(2) FD	(3) FE IV	(4) FD IV	(5) FE	(6) FD	(7) FE IV	(8) FD IV
log(spending + private)	-1.55	-3.36	64.18	70.07	11.80	8.74	29.35	38.77
	-0.10	*-0.17*	*1.47*	*1.19*	*0.81*	*0.46*	*1.39*	*1.28*
% free/reduced lunch	0.24	0.34	0.22	0.33	-0.22	0.00	-0.20	0.02
	0.77	*0.83*	*0.67*	*0.74*	*-0.65*	*-0.01*	*-0.58*	*0.05*
% free/reduced lunch^2	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	-0.60	*-0.34*	*-0.57*	*-0.35*	*0.09*	*-0.74*	*-0.01*	*-0.82*
log(school size)	92.69	81.27	102.13	84.54	19.14	-15.30	26.29	-20.99
	1.47	*0.96*	*1.62*	*1.06*	*0.30*	*-0.16*	*0.40*	*-0.22*
log(school size)^2	-8.42	-6.72	-8.83	-6.82	-2.77	0.52	-3.49	0.88
	-1.56	*-0.96*	*-1.63*	*-1.04*	*-0.50*	*0.06*	*-0.62*	*0.10*
Obs.	514	388	511	388	491	381	490	381
R^2	0.09	0.05	0.04	0.01	0.18	0.02	0.17	0.02
Groups	118		115		104		103	
Shea's R^2			0.18	0.16			0.39	0.37
First Stage F			4.48	8.11			5.51	12.08
p value			*0.00*	*0.00*			*0.00*	*0.00*
Hausman Test			4.19	3.26			1.08	2.02
p value			*0.04*	*0.07*			*0.30*	*0.16*
Hansen J Statistic			1.34	4.16			1.35	1.29
p value			*0.85*	*0.38*			*0.85*	*0.86*
C-Statistic			0.46	3.87			0.04	0.25
p value			*0.50*	*0.05*			*0.85*	*0.61*

Coefficients are in plain font and t-statistics are italicized. Reported t-statistics are calculated using standard errors clustered at the town level and robust to heteroskedasticity and within-school serial correlation. Constants and time dummies are included but not reported. For the fixed-effects specifications R^2 values are net of school fixed effects.

Table 3.14: Reading and Writing: Initially High versus Initially Low Achieving

	4th Grade Writing				4th Grade Reading				2nd Grade Reading			
	High Achieving		Low Achieving		High Achieving		Low Achieving		High Achieving		Low Achieving	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	FE	FD	FE	FD	FE	FD	FE	FD	FE	FD	FE	FD
log(spending)	1.08	0.88	-5.41	-6.30	-0.93	-3.14	6.29	2.10	2.83	-3.83	9.59	12.34
	0.16	*0.09*	*-0.74*	*-0.61*	*-0.21*	*-0.47*	*1.03*	*0.21*	*0.56*	*-0.64*	*1.38*	*1.15*
% free/reduced lunch	-0.07	-0.03	-0.31	0.03	-0.15	-0.05	0.06	0.01	-0.45	-0.45	-0.21	0.09
	-0.41	*-0.09*	*-1.06*	*0.08*	*-1.00*	*-0.16*	*0.27*	*0.05*	*-2.25*	*-2.13*	*-0.96*	*0.36*
% free/reduced lunch^2	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	0.19	*0.44*	*0.47*	*-0.62*	*-0.03*	*-0.08*	*-0.39*	*-0.06*	*1.66*	*2.10*	*1.00*	*-0.14*
log(school size)	32.11	90.01	51.76	-5.78	24.73	60.03	57.61	52.93	-40.38	-84.83	53.05	22.31
	0.82	*1.68*	*0.88*	*-0.07*	*0.76*	*1.20*	*1.48*	*0.74*	*-1.24*	*-1.65*	*0.95*	*0.25*
log(school size)^2	-2.83	-7.49	-5.87	-0.23	-2.14	-4.91	-5.21	-4.99	3.06	6.58	-5.77	-3.04
	-0.88	*-1.70*	*-1.14*	*-0.03*	*-0.80*	*-1.18*	*-1.58*	*-0.82*	*1.16*	*1.60*	*-1.13*	*-0.38*
Obs.	644	512	581	470	627	495	598	487	637	506	605	482
R^2	0.14	0.11	0.23	0.02	0.12	0.07	0.03	0.00	0.06	0.04	0.31	0.06
Groups	122		103		120		105		117		111	

	4th Grade Writing				4th Grade Reading				2nd Grade Reading			
	High Achieving		Low Achieving		High Achieving		Low Achieving		High Achieving		Low Achieving	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	FE	FD	FE	FD	FE	FD	FE	FD	FE	FD	FE	FD
log(spending + private)	4.05	5.63	-11.82	-11.97	8.41	9.18	4.13	9.66	-6.76	-2.63	8.22	2.69
	0.26	*0.32*	*-0.74*	*-0.66*	*0.62*	*0.50*	*0.26*	*0.53*	*-0.51*	*-0.17*	*0.54*	*0.11*
% free/reduced lunch	0.08	0.26	-0.20	0.06	-0.16	0.12	-0.03	-0.15	-0.29	-0.37	-0.29	-0.32
	0.30	*0.69*	*-0.45*	*0.12*	*-0.78*	*0.42*	*-0.11*	*-0.48*	*-1.03*	*-1.39*	*-1.06*	*-1.00*
% free/reduced lunch^2	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	-0.46	*-0.47*	*-0.12*	*-0.66*	*0.22*	*-0.36*	*0.21*	*0.84*	*0.70*	*1.25*	*0.81*	*0.59*
log(school size)	17.20	82.37	20.66	13.73	2.09	19.10	56.16	91.98	-30.19	-72.78	52.89	41.05
	0.37	*1.36*	*0.26*	*0.12*	*0.05*	*0.32*	*1.46*	*1.15*	*-0.80*	*-1.46*	*0.86*	*0.45*
log(school size)^2	-1.67	-6.72	-3.75	-1.92	-0.34	-1.61	-5.48	-8.10	2.12	5.63	-6.48	-4.80
	-0.45	*-1.37*	*-0.54*	*-0.20*	*-0.11*	*-0.32*	*-1.69*	*-1.20*	*0.71*	*1.41*	*-1.15*	*-0.58*
Obs.	527	400	478	369	512	386	493	383	521	393	494	377
R^2	0.11	0.02	0.20	0.03	0.01	0.01	0.02	0.01	0.05	0.02	0.14	0.02
Groups	120		102		117		114		116		108	

Coefficients are in plain font and t-statistics are italicized. Reported t-statistics are calculated using standard errors clustered at the town level and robust to heteroskedasticity and within-school serial correlation. Constants and time dummies are included but not reported. For the fixed-effects specifications R^2 values are net of school fixed effects.

Chapter 4

The Response of Property Values to Vermont's Act 60[1]

Reliance on local property taxes to finance local public education has led to great inequities in local expenditures per-pupil. States across the nation have been undertaking various school finance restructurings in an attempt to bring about greater equity. Often these reforms are driven by litigation. Since 1970, 39 state supreme courts have ruled on the legality of school finance systems and 17 have found that the system in place does violate the state constitution (Corcoran and Evans, 2008). Vermont is one of these 17 states.

As a result of Vermont's litigation school finance reforms were undertaken in both 1997 and 2004 (Act 60 in 1997 and Act 68 in 2004). In this paper, we study the market's response to changes in local school spending, tax rates, and student

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achievement under Vermont's school finance system in place from 1997 through 2004. Education finance reforms in Vermont set out to meet two objectives. First, the state wanted to equalize access to educational funding. The reforms were effective in reducing the relationship between property wealth and education spending. There is also evidence that spending equity increased as a result of the 1997 reform. Prior to 1997, there were large differences in property tax rates across the state. Act 60 also set out to achieve property tax equity. Here the legislation was more successful. This paper does not evaluate Act 60 for its relative success. Instead, we exploit the changes in property tax rates and school spending under Act 60 to explore how aggregate property values respond to changes in tax rates, local education spending, and student achievement.

The paper proceeds as follows. Section 1 reviews the literature. Section 2 provides a brief overview of Vermont's Act 60. Section 3 outlines the methodology. Section 4 describes the data. Section 5 presents our results. Section 6 concludes.

4.1 Literature Review

In 1956 Charles Tiebout demonstrated that public goods could in fact be provided optimally via market mechanisms. Different communities provide alternative tax, expenditure, and service levels. Individuals sort themselves based on preferences. If more spending is valued to less, spending will increase the value of property in higher spending communities. Communities that have higher tax rates for a given service level should have lower property values. Outputs from the public service (like test scores with schooling) should increase the value of property to the degree that they are valued by consumers. Under the Tiebout hypothesis, tax, expenditure, and

service levels are capitalized into local property values.

Oates (1969) was the first to empirically test the Tiebout Hypothesis as it related to public schooling. Oates found that property values were higher where spending was higher. A number of researchers have extended upon Oates' early results (Black (1999); Brasington (1999); Bradbury, Mayer, and Case (2001); Brunner, Murdoch, and Thayer (2002); Brasington and Haurin (2006)). All of these studies found that spending and test scores were positively capitalized into house values. Others have found that expenditures are not capitalized into house values once achievement measures are included (Hayes and Taylor (1996); Downes and Zabel (2002); Crone (2006)). This could be explained as reflecting the fact that school inputs are not consistently translated into measurable outputs. If homeowners do not believe that more money leads to better schools than property values will not increase when expenditure levels increase.

Other studies have examined the impact of test scores on property values without considering expenditure levels. Test scores are generally found to be positively capitalized into housing values (Jud and Watts (1981); Weimer and Wolkoff (2001); Kane, Reigg, and Staiger (2006)). However, some recent studies have found mixed evidence regarding test score capitalization. Clapp, Nanda, and Ross (2008) found evidence that test scores were not being viewed as an important signal of school quality in Connecticut. However, the authors concluded that test scores did seem to play a larger role in property valuation in more recent years. This shift is attributed to increasing awareness surrounding test scores following No Child Left Behind. Dills (2004) also found mixed evidence of test score capitalization. Improved scores on high stake tests in Texas were not capitalized, while changes in SAT and ACT scores were reflected in property values. If improved test scores on state administered tests

are not perceived as quality improvements then capitalization is not expected.

Loubert (2005) utilized school finance reform in Texas to explore the relationship between property values, spending, and test scores. She found that per-pupil spending had a positive impact on property values prior to school finance equalization in Texas. After reform, however, spending appeared to have a negative impact on the price of a home once achievement and the percentage of the population that was white was controlled for. Overall, school finance equalization in Texas led to a convergence in house values as the premium associated with being in a preferred school district diminished.

Hoxby and Kuziemko (2004) also examined the impacts of Texas's school finance reform on property values in Texas. In Texas, it appears that school finance reform did lead to an increase in property values in towns with low per-pupil property value pre-reform. However, while there was convergence in property values between the high wealth and low wealth districts, the net effect of school finance reform in Texas was to reduce overall property value in the state. With a smaller property tax base it becomes more difficult to raise revenues via property taxes to finance public schools.

When examining capitalization in a state experiencing transition into or transition under school finance reform it is instructive to also look at how property values respond to changes in the tax rate that correspond to the reform. While there is a larger literature on the capitalization of property taxes into property values (see Yinger, Bloom, Boerch-Supan, and Ladd (1988) and Fischel (2001) for surveys), many studies exploring capitalization do not explicitly include the tax rate in their analysis. When addressing capitalization in a state undergoing reform, property taxes are changing such that capitalization of these taxes can be assessed. Clapp, Nanda, and Ross (2008) find that about 30% of property tax obligations are capitalized into

property values. Less than full capitalization can be evidence that property tax rates are not expected to persist.

There are two ways test scores can be capitalized into housing values. Higher test scores tend to be found in districts with higher property values. Changes in test scores may also be capitalized into house values. The literature has explored whether it is the level of test scores or changes in test scores that are valued by the housing market. Hayes and Taylor (1996) found that changes in test scores by local schools improved property values. On the other hand, Brasington (1999) found that changes in schooling variables were not consistently capitalized into property values. In Brasington and Haurin (2006) it is shown that while the level of test scores may positively influence property values, changes in test scores are not capitalized. Dills (2004) found that changes in pass rates in Texas did not increase aggregate property values.

Capitalization studies testing the Tiebout Hypothesis have focused on property values in urban and suburban districts. Capitalization as measured via existing home prices will be stronger when the supply of housing is relatively inelastic (Hilber and Mayer (2001); Brasington (2001)). In areas with relatively low population density, where the supply of housing units is relatively elastic, increased demand for housing may not result solely in increasing prices of existing homes. When the demand for housing in communities where housing is elastically supplied, aggregate property values will increase via new development. Hence, capitalization can occur via both increases in prices of existing homes and construction of new homes. Dills (2004) took this into consideration and used the total value of housing in a school district as her dependent variable. Previous studies of capitalization conducted in primarily non-urban settings where only the value of existing homes is measured may understate

the market's overall response to changes in taxes, spending, and schooling variables.

Cross-sectional studies relating schooling expenditures, test scores, and tax rates are likely to suffer from omitted variable bias. Two approaches have emerged in the literature to address the possibility that neighborhood unobservables artificially inflate coefficient estimates. The first is to use boundary discontinuities (Black (1999); Kane, Reigg, and Staiger (2006); Davidoff and Leigh (2008)). These studies identify school district boundaries that divide otherwise homogeneous neighborhoods. House prices on either side of these boundaries can then be compared to isolate the impact on house values arising from different schooling characteristics. Black (1999) found cross-sectional estimates of the effect of test scores on housing prices that fail to control for neighborhood unobservables is twice that of a specification that does control for the unobservables. Kane, Reigg, and Staiger (2006) found an even greater difference in the coefficients between the cross-sectional and boundary discontinuity specification.

Boundary discontinuity methods provide a good estimate of the difference in magnitude of estimates that fail to control for neighborhood unobservables. However, boundary discontinuities may not be appropriate in states where towns and school districts are coterminous. Neighborhoods are not likely to be continuous across boundaries. When school districts and towns are coterminous fixed effects have been used to control for neighborhood unobservables (Clapp, Nanda, and Ross, 2008).[2] Arguably, fixed effects is the more appropriate method when towns and school districts are coterminous as boundary discontinuity methods rely on neighborhoods that change continuously.

[2]Clapp, Nanda, and Ross (2008) use town level fixed effects as well as census tract fixed effects. The coefficient estimates on test scores and property tax rates are insensitive to the alternative specifications.

4.2 Changes in Tax Rates and School Spending Under Act 60

Vermont under Act 60 provides a unique circumstance for studying the impact of capitalization. Ideally, to evaluate the impact of school finance reform on property values, one should be able to account for test scores, spending, and tax rates both before and after reform has been implemented. Unfortunately, a consistent measure of school performance at the school level was not available for this study. However, the substantial variation in tax rates, spending, and test score pass rates that occurred under Act 60 allows for an exploration of the relationship between these variables and aggregate property values in towns across the state.

Vermont adopted Act 60 in 1997 in response to a ruling in the Vermont Supreme Court case *Brigham vs. State*. In ruling with the plaintiff, the court found that Vermont's system of school finance violated the equal protection clause of the State's constitution by not providing all students equal access to education funds in Vermont. The purpose of Act 60 was twofold. First, the Act sought to make school spending a direct function of a towns tax effort.[3] Second, the legislation set out to reduce tax rates for many Vermonters who felt that they were having to levy relatively high taxes to achieve relatively low spending levels. Property wealthy towns, referred to as "gold towns" by the local media, were forced to raise taxes substantially as Vermont adopted a statewide minimum tax rate. Each town was required to pay the minimum statewide tax rate in order to qualify for foundation aid as part of the statewide school

[3]Theoretically, this should raise multicollinearity concerns in the analysis below. However, by using the total tax rate, rather than the school tax rate, and local spending per equalized pupil these concerns are largely mitigated. The correlation between the total tax rate and local spending per equalized pupil in any given year never exceeds 0.57. Using local spending per equalized pupil including private donations further reduces this correlation.

finance system.

Even after Act 60 was fully phased-in there continued to be substantial variation in tax rates and school spending within towns. There are a couple of reasons why this variation was likely to have occurred. First, schools were not certain the amount of revenue an additional unit of taxation would yield. All tax revenues were collected by the state. Any tax revenues collected from school tax rates set above the minimum required rate were sent into the sharing pool. Revenues from the sharing pool were then redistributed on a tax effort basis such that each town taxing at the same rate was able to spend the same amount per-pupil. Consequently, high property wealth towns that were able to raise more revenue per-pupil for a fixed tax rate subsidized low property wealth towns, with low revenue raising capacity. However, since towns moved first by setting their tax rate. Once tax rates were set by towns, this determined the size of the sharing pool, and how much additional revenue each unit of additional tax levy would yield. Given the uncertainty regarding how much additional spending additional tax levies would raise may have led towns to adjust their tax rates in the early years of Act 60.

A second factor which may explain why tax rates and spending varied under Act 60 is the use of private contributions by towns to avoid Act 60's sharing pool. A number of Vermont's high property wealth towns chose to set school tax rates at or very close to the state mandated minimum. As a result, these towns were restricted to spend at the minimum amount of per-pupil spending. However, these towns were able to generate additional revenues via private contributions.[4] As property wealthy towns began to rely on private contributions to supplement spending received under

[4]Downes and Steinman (2008) describe the use of private contributions as a means of funding education in Vermont.

Act 60 per-pupil spending remained relatively variable.

Table (4.1) provides information on the overall, between, and within variation for the primary variables of interest. While there is more within variation between 1996 and 2004, the period that captures pre- and post-Act 60 levels, there is still substantial within variation during the Act 60 period, between 2000 and 2004. This variation is used to explore how aggregate property values respond to changes in tax rates, school spending, and student achievement.

4.3 Theoretical & Empirical Framework

The literature relies on hedonic price models to evaluate how much various attributes of property are worth. Conceptually, a piece of property is a collection of attributes, or characteristics, of that property. Hedonic models decompose the total value of a piece of property in an attempt to isolate the value consumers place on individual attributes associated with a specific property. Rosen (1974) formalized the hedonic pricing method. In a simplified version of Rosen's hedonic model, housing can be viewed as a vector of housing characteristics ($Z = (z_1, z_2, ..., z_n)$). Consumers have a utility function $U(x, z_1, z_2, ..., z_n)$ where x represents all other consumption goods. The utility function is assumed strictly concave and twice differentiable. Consumers maximize this utility function subject to their budget constraint $y = x+p(z_1, z_2, ..., z_n)$ where y is income and $p(z_1, z_2, ..., z_n)$ is expenditures on property. Setting the price of x equal to 1, maximization of the utility function subject to the budget constraint yields the usual first order conditions, $\frac{U_{z_i}(x,Z)}{U_x(x,Z)} = p_{z_i}(Z)$, where U_{z_i} is the partial derivative of the utility function with respect to z_i and p_{z_i} is the the partial derivative of expenditures on property with respect to characteristic z_i. This states that the

marginal rate of substitution between property characteristic z_i is equal to the price of z_i. Under this framework, a consumer is willing to pay p_{z_i} for a marginal change in z_i.

In order to estimate the value property owners place on various attributes of property, one can estimate $p(Z)$ and then partially differentiate $p(Z)$ with respect to the attribute of interest. Empirically, one can estimate house prices as a function of a house's attributes

$$ln(HousePrice)_{idt} = \alpha_t + X'_{it}\gamma + Z'_{dt}\beta + \mu_d + \varepsilon_{idt}. \tag{4.1}$$

In this specification, X_{it} are attributes of the house i, such as the number of bedrooms, number of bathrooms, square footage, age, etc. at time t. The vector Z_{dt} contains characteristics of the neighborhood d, which includes schooling variables. It is typical to include year fixed effects, α_t, to allow for real appreciation in house prices over time. The term μ_d, a town fixed effects term, captures characteristics unique to the town (or district) that do not vary over time, such as travel time to major metropolitan areas. To the extent that demographic and economic conditions remain stable over time, they are also captured by the town fixed effects.

In rural areas, capitalization can take place via two different channels. The first is through price appreciation of existing homes. Most previous works have examined this type of capitalization.[5] In areas with developable land, capitalization can also

[5]Capitalization studies using sales data include Hayes and Taylor (1996), Bogart and Cromwell (1997), Black (1999), Brasington (1999), Weimer and Wolkoff (2001), Brunner, Murdoch, and Thayer (2002), Loubert (2005), Brasington and Haurin (2006), Crone (2006), Kane, Reigg, and Staiger (2006), Clapp, Nanda, and Ross (2008), and Davidoff and Leigh (2008). Bradbury, Mayer, and Case (2001) and Figlio and Lucas (2004) were able to build a sample of repeated sales and control for a property's physical characteristics. Downes and Zabel (2002) used data from the American Housing Survey containing owner's evaluation of house price.

occur through the construction of new housing units. Following Dills (2004) capitalization is assessed as it occurs through both channels.[6] In a rural state like Vermont capitalization via changes in the price of existing homes is expected to be relatively small.[7] Simply looking at the change in prices of existing homes may fail to capture the market's overall response to tax and expenditure policy changes.[8]

To assess the effects of Act 60 on property values in the state a long-difference specification is utilized. Difference specifications help reduce omitted variable bias by controlling for neighborhood unobservables over time (Bradbury, Mayer, and Case, 2001; Downes and Zabel, 2002). The long difference specification also helps address the issue of noise in the test score variable, or the measure of school quality. Test scores tend to be vary noisy and may change from year to year for reasons that are not related to school quality. In Vermont, where the number of students taking the test in each town in each year is very small, test scores are likely to fluctuate quite a bit from year to year. Following Dills (2004), it is postulated that the change in test scores over a number of years is more likely to represent persistent changes in school quality then to be the result of random variation. Using aggregated property values and a long-difference specification results in the following relationship utilized in the empirical evaluation:

$$ln \sum_i (HousePrice)_{id,2004} - ln \sum_i (HousePrice)_{id,2000} = \\ (\alpha_{2004} - \alpha_{2000}) + (Z'_{d,2004} - Z'_{d,2000})\beta + (\varepsilon_{d,2004} - \varepsilon_{d,2000}). \quad (4.2)$$

[6]Reback (2005) also uses the total value of residential property in a district to identify the capitalization effects of increased school choice.

[7]Hilber and Mayer (2001) demonstrate that capitalization is greater in towns where there is less developable land.

[8]By examining changes in the total value of property the analysis avoids the selection bias problems inherent in works that rely on sale price data.

In addition to using the long-difference specification, the panel nature of the data allows for a fixed effects analysis.[9] Like the difference regression, fixed effects methods control for unobservables that vary across towns but do not change across time. The fixed effects analysis involves estimating equation (4.1). When towns and districts are coterminous, as is the case in Vermont, fixed effects are more appropriate than boundary discontinuity methods, as neighborhoods are unlikely to change continuously at town (school district) borders. To explore whether the fixed effects have successfully controlled for omitted variables the results of the fixed effects regression are compared to the results from a pooled cross section specification. If the difference in coefficients is similar to the magnitude of difference found in the boundary discontinuity literature it is likely that the fixed effects are successfully capturing neighborhood unobservables.[10]

4.4 Data

The fact that taxes, school spending, and test scores were changing as Act 60 was phased-in between 1997 and 2000 and under Act 60 from 2001 through 2004 make Vermont an ideal place to study the impacts of spending and schooling variables on property values.[11] Under a school funding system that relies purely on local tax revenues for local public school spending, taxes and spending would be collinear. As Act 60 was phased in and while Act 60 was in place, changes in total property taxes

[9] The long difference specification is a fixed effects specification where only two time periods are observed. Here, a fixed effects specification refers to the case where annual data is used.

[10] This empirical method does not explicitly address the potential endogeneity inherent in the relationship between property values, spending, and test scores. This issue is addressed below.

[11] Act 60 was fully implemented for the 2000-2001 school year, which corresponds to the 2001 fiscal year.

and school spending (especially once private donations are included) in Vermont were not highly correlated. The rural nature of the state lends itself to a study of aggregate property values, as opposed to use of actual sale data. Additionally, since school districts and towns are coterminous in Vermont there are no problems in identifying which school district various pieces of property belong.[12]

Aggregate property values are reported in the Vermont Department of Taxes's Property Valuation and Review (PVR) Annual Report.[13] The reports aggregate taxable land value in each town for four different categories: corporations, state residents, non-state residents, and town residents. In the analysis below, the aggregate value of non-corporate property values is the dependent variable. While only town residents take advantage of the local public service that is the public school system, the value of all properties would be expected to reflect access to the school system.

The analysis uses effective property tax rates, where the effective property tax rate is $\frac{CurrentTaxRate*CurrentGrandList}{EqualizedGrandList}$. The equalized grand list for each town adjusts for assessment bias using the ratio of assessed value to fair market value. Effective tax rates adjust for any systematic under assessment.[14]

Data on spending per equalized pupil at the town level is available from the Vermont Department of Education.[15] Local spending per equalized pupil is education that is paid for by the state foundation grant and local taxation that is not redis-

[12]There are 12 towns in Vermont that have multiple schools. For these towns, schooling variables are averaged at the town level.

[13]Annual reports from 2002 forward are available online through the Vermont Department of Taxes. Earlier reports were provided upon request.

[14]Brad James at the Vermont Department of Education made the effective tax rate data available.

[15]Equalized pupils are determined as $EP = \frac{(1*Elem)+(1.25*Second)+(0.2*ESL)}{Pupils} * (1.25 * Poverty)$ where $Elem$ is the number of elementary students, $Second$ is the number of secondary students, ESL is the number of students for whom English is their second language, $Pupils$ is the total number of pupils in the town, and $Poverty$ is the ratio of students in poverty. Spending per equalized pupil attempts to adjust for known differences in cost to educate.

tributed via the sharing pool. Local spending per equalized pupil does not include school spending resulting from endowments, parental fund raising, or federal sources. To provide a more accurate picture of resources available to local public schools, funds coming from private contributions are added to local spending per equalized pupil.[16] Private contributions can be categorized as either those resulting from Act 60 or not. A town's private contributions are presumed not to be the result of Act 60 if the town had negligible private donations prior to Act 60's adoption. Failing to account for these private funds overstates the equalization impacts of Act 60.[17]

Data on student achievement comes from the Vermont Department of Education. Vermont uses the New Standards Reference Exam to evaluate achievement in math, reading, and writing. The analysis focuses on 4th grade performance in these topics.[18] Performance data on the Developmental Reading Assessment, which measured 2nd grade reading achievement, is also included. In the analysis, the previous years pass rates are used. Tests are administered in the spring. Since the aggregate property values reflect property values used to calculate tax liability in the following year, current years test scores would not be known. The analysis uses the pass rate, which is the percentage of students achieving the standard or achieving the standard with honors, for each school. To address the fact that test scores are likely a noisy measure of school quality, pass rates are averaged across disciplines. An overall pass rate is the average pass rate in 4th grade reading, writing, and math. Pass rates are also

[16]Brad James at the Vermont Department of Education provided access to data on these private funds for the years 2000 through 2004. Since there was little incentive for private donations prior to the adoption of Act 60 one would expect the quantity of donated funds to be small before this time.

[17]Downes and Steinman (2008) show that non-traditional revenue sources increased after Act 60 was phased-in and diminished the potential equalizing effects of the legislation.

[18]The New Standards Reference Exam is also administered to students in 8th and 10th grade. The analysis, however, focuses on primary schools where there tends to be one school per town. Many Vermont towns participate in Union School Districts and do not operate their own high schools.

smoothed by using a two year average of pass rates. Due to the fact that pass rates are only available from 1999 onward, smoothed pass rates are calculated using the next years pass rates. The 1999 smoothed pass rate is the average of the 1999 and 2000 pass rates. For the long-difference specification, the first measure of the change in test pass rates in the simple difference between the pass rates in 1999 and 2003. The second measure used is the smoothed change in the pass rate, where the smoothed change is the difference between the average of the 1999 and 2000 pass rates and the average of the 2003 and 2004 pass rates.

Generally, pass rates are only reported when at least 10 students have taken the test, although there are some exceptions for years prior to 2003. For towns that have joint school districts at the elementary level, towns are matched to the school test data where their children attend school. There is no way to match students in towns that tuition elementary school students, so these towns are not included in the regression analysis including pass rates.

Average annual wages by town are computed annually by the Vermont Department of Labor. The average annual wage in each town in the total annual wages divided by the town's average employment. The Vermont Department of Labor also maintains unemployment statistics at the town level. The U.S. Census Bureau maintains data on the number of children in each school district between the ages of 5 and 17 and the number of these children that are in poverty. This data is used to derive the percentage of children in poverty in each town. School demographic data, such as the percentage of black and percentage of Hispanic students, as well as the pupil-teacher ratios, are taken from the Common Core of Data (CCD).

Since school districts and towns are coterminous in Vermont, and Vermont towns tend to be small, a number of towns do not educate their own high school students.

Fifteen Vermont towns do not operate a school at all. Many towns (66) tuition their high school students to neighboring towns. Towns that tuition high school students allow residents to select a public high school and then pay the maximum allowable tuition to that high school. For private schools, the town pays what it would have spent to send that child to a Vermont public high school. This effectively allows for school choice at the high school level.[19] Another 121 towns in Vermont are part of a Union High School, such that students from many towns attend one high school.

Table (4.2) contains the summary statistics for the long-differences on variables between 2000 and 2004. Column 1 contains the summary statistics for all available data. Column 2 presents the data for towns without missing information. The number of observations in the second column is equal to the number of observations in the long-difference regression specification. In the sample used in the analysis, real aggregate property value increased by 14.62% between 2000 and 2004. Aggregate property values, along with other nominal measures, are deflated to 2000 dollars using the Northeast Consumer Price Index. In the sample taxes decreased, on average, by 0.26 percentage points. This reflects the fact that Act 60 did result in tax decreases for a large number of taxpayers. The summary statistics show that total property taxes declined slightly by more than property taxes used to fund schools. Real local spending per equalized pupil increased by more than 13% between 2000 and 2004. Real local spending per equalized pupil plus private donations increased by more than 14%. Average real wages increased by 6% during the period. Child poverty fell by nearly a full percentage point. Pupil-teacher ratios also declined, reflecting an increase in the number of teachers per-pupil.[20] The change in the percent of black and Hispanic

[19]Since 1999 tuitioned students have not been allowed to use the state provided tuition funds at religious schools.

[20]Much of the decline in pupil-teacher ratios in Vermont can be attributed to de-

children over the five year period is relatively small, less than half a percentage point each.

From Table (4.2) it can be seen that test score pass rates generally increased between 1999 and 2003 (4th grade reading appears to be the exception). The standard deviations of the smoothed pass rate differences are smaller than the standard deviations of the non-smoothed differences, as using smoothed pass rates tends to diminish the impacts of irregular test performance.

The final variable is the indicator variable for whether or not a town tuitions high school students. Thirty-nine of the towns in the sample used in the difference estimation tuition their high school students. For these 39 towns, elementary students attend a school within the town or attend a school as part of a joint school or union elementary school district. The proportion of towns that tuition students in the sample is less than the proportion that tuition high school students statewide, reflecting the fact that smaller towns tend to not have data on test pass rates as many do not operate schools at all. Smaller towns are also more likely to tuition their high school students.

The third column in Table (4.2) contains the change in variables between 2000 and 2004 for the 16 towns without missing data that funded schools with Act 60 induced private donations in 2004. While aggregate property value growth in these towns was similar to the rest of the state, trends in taxes and spending were quite different. Between 2000 and 2004, in contrast to the statewide trends average property taxes in these towns increased. Real spending per equalized pupil grew at a slower rate than for the state as a whole. As expected, the change in local spending when private

creasing enrollments rather than increased teacher hiring. The Vermont Housing Finance Agency outlines the issue of declining enrollments in a report available at http://www.vhfa.org/documents/housing_education.pdf, accessed on March 23, 2009.

donations were included exceeded the statewide trend.[21]

Table (4.3) contains the summary statistics, by year, on the variables used in the fixed effects analysis. Real aggregate property values increase between 2000 and 2004. Property taxes remain relatively stable, until decreasing in 2004. In 2004, Act 60 was superseded by Act 68. Under Act 68, residential property was taxed at a lower rate than non-residential property. The tax rate used in the analysis for 2004 is the total tax rate imposed on residential property values.[22] Local spending per equalized pupil and local spending per equalized pupil plus private donations both increase throughout the sample period. Since spending is measured in fiscal years, spending in 2004 represents spending under the last year Act 60 was in place, before any changes in spending resulting from the adoption of Act 68 went into effect. The average pass rate for 4th grade students also increases throughout the sample period.[23]

4.5 Results

Table (4.4) contains the results of estimating equation (4.2). As is typical in the literature, a modified log-level specification is used. Variables that represent proportions, or percentages, are left as levels. Variables that are measured in dollar terms are measured in logs. For the log-log variables, the coefficients represent elasticities. The right hand side variables in our specification include tax rates, per-pupil spending,

[21] In 2000, Act 60 was already implemented. Therefore, changes under Act 60 are not as dramatic as the changes that took place during the Act 60 phase-in period. The difference in real local spending per equalized pupil statewide between 1999 and 2004 was a positive 10%. However, the real change in local spending per equalized pupil for the 22 towns that used private donations was nearly negative 30%. Once private donations are included, the average change in spending amongst the 22 towns utilizing private donations was less than 3%.

[22] Admittedly, future work on capitalization should take into account the tax rate differentials on homestead versus non-homestead tax rates.

[23] While only the average 4th grade pass rate is presented here, the results using pass rates on individual tests remain the same.

average annual wages, unemployment levels, child poverty levels, the percentage of children that are black or Hispanic, and the smoothed pass rate for the average 4th grade pass rates. In this specification, there is reason to believe that test score pass rates may be endogenous.[24] Communities with higher wealth (and correspondingly higher property values) are also likely to have students with better standardized test performance. The concern is that in communities where aggregate property values are rising, the new residents also devote more resources to education. Consequently, there is an upward bias on the coefficient on the relationship between test scores and aggregate property values. This should be taken into account when interpreting coefficients.

The first column of Table (4.4) shows that total tax rates are negatively capitalized into property values.[25] A one percentage point increase in property taxes is associated with a decrease in aggregate property values that exceeds 10%.[26] The coefficient on spending is negative, but statistically insignificant. As expected, the coefficient on the change in unemployment is negative, but only has a p-value of 0.12. Directly interpreted, this coefficient would suggest that a town with a one percentage point increase in unemployment experienced a 1.9% decline in aggregate property values. However, this estimate is very imprecise. The coefficient on the percentage of children in poverty is estimated to be negaive and significant at the 5% level. A one percentage point increase in the percentage of children in poverty is associated with a 2.8% decline in aggregate property values. The coefficient on the percentage of students who are

[24]Using the previous years test score pass rates reduces the likelihood that test score pass rates are endogenous.

[25]If school property taxes are used as the right hand side variable, instead of total property taxes, the coefficient remains negative but is not longer statistically significant at conventional levels.

[26]In the fixed effects analysis below taxes are measured as the present value of property taxes such that the estimated coefficient represents the degree of capitalization.

black has a positive and marginally significant relationship with aggregate property values. While this result differs from most of the findings in the literature, it is likely driven by the fact that Vermont is racially homogeneous and there are very few African American students overall. The coefficient on the percentage of students who are Hispanic is negative but statistically insignificant.

The coefficient on the change in aggregate pass rates between 1999 and 2003 is negative and insignificant. The model was estimated using pass rates on 4th grade math, reading, and writing as well as 2nd grade reading. In no case was the coefficient on the change in test pass rates significant.[27] As stated above, this estimate represents the upward bound on the relationship between aggregate property values and test scores. Therefore, it does not appear that pass rates on standardized tests in Vermont are capitalized into property values. This result is consistent with the findings of Dills (2004). Dills (2004) concludes that parents in Texas do not view pass rates on Texas's standardized tests as evidence of increased school quality.[28] The same appears to be true in Vermont. While test pass rates generally increased under Act 60, these increases were not viewed as evidence of increasing school quality by homeowners in Vermont.

The second column of Table (4.4) includes a dummy variable indicating whether

[27]An earlier version of this paper reported that the change in 2nd grade reading scores was positively related to changes in aggregate property values, and that the relationship was statistically significant. The earlier version of the paper regressed the changes in property values on contemporaneous, rather than lagged, changes in test scores. As stated above, contemporaneous test pass rates are not known to property owners and would not be expected to be capitalized. Hence, the prior finding of a significant relationship between test score pass rates and aggregate property values is likely a result of endogeneity rather than a causal relationship. As towns aggregate property values increase, they are likely attracting residents and students who are likely to perform better on standardized tests.

[28]Dills (2004) does find that SAT and ACT pass rates are positively capitalized. This suggests that homeowners in Texas do value school quality, but that the state administered tests in Texas are not a good indicator of quality.

a town tuitions their high school students. As noted above, towns that tuition high school students essentially have school choice at the high school level.[29] It is interesting that this coefficient is positive and significant, given that it might be expected that tuitioning high school students would have already been capitalized into housing values at the start of the sample period. Nonetheless, the coefficient on the tuition dummy suggests that towns that tuition high school students experienced aggregate growth in property values 6% more than towns that did not tuition high school students. This relationship, however, is only signifiant at the 10% level is this specification and insignificant in other specifications. Hence, the results should only be taken as suggestive, not definative.

The third column of Table (4.4) regresses the change in aggregate property values on spending including private donations as well as the other right hand side variables. Once private donations are included, the coefficient on spending becomes positive, but remains statistically insignificant. The fourth column in Table (4.4) explores the possibility that property values in communities that choose to rely on private donations, presumably in an effort to avoid participating in Act 60's sharing pool, may respond differently to changes in spending and tax rates. To explore this possibility, a dummy variable for communities that rely on private donations is included.[30] This dummy variable is also interacted with the spending and tax rate variables. The coefficient on the private donations dummy is negative and insignificant. The interaction terms, however, produces interesting results. When the dummy for private donations is in-

[29]Reback (2005) finds that the adoption of a school choice program in Minnesota is capitalized as one would predict. Properties are found to increase in value when additional options for schooling become available, and residents take advantage of being able to attend schools in districts other than their own. Properties located in preferred school districts decrease in value as a result of school choice, as the value of being a resident of a preferred school district diminishes.

[30]This dummy variable takes a value of one if there were Act 60 induced private donations in 2004.

teracted with the tax term, the coefficient on the change in the tax rate becomes even more negative and significant. A one percentage point increase in taxes is now associated with a 16% decrease in aggregate property values. However, this impact is diminished for those towns with private donations. In fact, the results seem to show that communities using Act 60 induced private donations did not negatively capitalize changes in tax rates. The coefficient on the interaction of spending including private donations with the dummy for private donations is positive and has a p-value of 0.13. An F-test rejects that the coefficient on local spending per-pupil with private donations and the coefficient on local spending per-pupil with private donations interacted with the donation dummy are equal at the 5% level.

This result suggests that while spending is not capitalized into property values across all communities, it may be capitalized in some communities. Specifically, the 16 communities with private donations may experience an appreciation in aggregate property values as a result of additional school spending. Since both aggregate property values and spending are measured in logs, the coefficient is interpreted as an elasticity. A 1% increase in spending including private donations is assocated with a 0.43% increase in aggregate property values. In those studies that found school spending mattered even after controlling for student achieveemnt, there was variation in how responsive property values were to changes in spending. Studies that find positive capitalization of spending tend to find similar elasticities. For example, Brasington and Haurin (2006) find that the elasticity of house prices with respect to school expenditures is 0.49 in Louisiana.

The result here seems consistent with the story told by Bradbury, Mayer, and Case (2001) in their evaluation of property tax limits in Massachusetts. Bradbury, Mayer, and Case find that school spending has a greater impact on property values in towns

that are most constrained by Proposition 2 1/2. Communities effectively constrained by Proposition 2 1/2 had an elasticity of house prices with respect to spending of 0.57.[31] If one views towns in Vermont that use private contributions as towns that feel constrained under Act 60 the results have similar implications. Bradbury, Mayer, and Case conclude that property tax limits in Massachusetts prevented communities from spending the desired amount on education, as evidenced by higher property values in communities that were able to spend more on their schools. The general finding that spending is negatively capitalized is consistent with Loubert (2005), where she finds that spending has a negative and significant impact on property values, once achievement and the percent of the students that are white are controlled for, after school finance equalization took place in Texas.

Table (4.5) presents the results of estimating a modified version of equation (4.1) first as a pooled cross section and second using two-way fixed effects. The equation that is estimated has been modified so that the dependent variable is the natural logarithm of aggregate property values in town (district) d at time t,

$$ln(\text{Aggregate Property Values})_{dt} = \alpha_t + X'_{dt}\gamma + Z'_t\beta + \mu_d + \varepsilon_{dt}. \tag{4.3}$$

Since towns and school districts are coterminous, school district level variables and school level variables have been matched to towns.

All columns include year dummy variables and a constant which are not reported. The first column in Table (4.5) contains the results from regressing aggregate property values on taxes, spending, schooling, and economic variables. As predicted, higher average annual wages are associated with higher aggregate property values, higher child

[31] Spending was generally capitalized in Massachusetts, although the elasticity of house prices with respect to spending was much lower for the state as a whole at 0.23.

poverty rates are associated with lower aggregate property values, and higher rates of unemployment are also associated with lower aggregate property values. Higher pupil-teacher ratios, higher percentages of black and Hispanic students, and higher test pass rates are all associated with higher property values. The coefficient on spending without private donations is negative and significant. The coefficient on taxes is insignificant. In the second column of Table (4.5), the spending once private donations are included is positive. The third column in Table (4.5) interacts spending and taxes with the Act 60 induced private donation dummy, to see how capitalization differs across these two types of towns.

In Table (4.5), the variable included in the regressions for taxes is an approximation of the present value of property taxes, given that property taxes are expected to remain constant over time.[32] The interpretation of the coefficients on the property tax term is as a percentage of the change in future property tax burdens that are capitalized into property values.

In the pooled OLS specification, many of the coefficients are significant. These results, however, are likely to be misleading to the extent that the variation in the right hand side variables occurs between towns, rather than within towns, during the sample period. Hence, the positive coefficient on test pass rates may be reflecting the fact that towns with higher aggregate property values may also be towns that tend to have students who perform better on standardized tests. The pooled OLS specifi-

[32]Specifically, the variable included in the regression analysis uses the approximation for the present value of property taxes per dollar of property value outlined in Yinger, Bloom, Boerch-Supan, and Ladd (1988) and employed by Clapp, Nanda, and Ross (2008). In this paper, PV of Property Taxes = $log[r + rate]$, where r is the discount rate and $rate$ is the property tax rate, or property tax dollars owed per \$100 of assessed value. The analysis uses a discount rate of 0.03. If the discount rate is assumed to be higher, there is a greater degree of property tax capitalization. Hence, a low discount rate represents a conservative estimate of the degree of tax capitalization.

cation does not effectively control for town characteristics which are likely correlated with the right hand side variables. Omitted variable bias appears to be inflating the coefficients on average annual wages, the pupil-teacher ratios, percentage of students who are black, percentage of students who are Hispanic, and the pass rate.

Columns (4)-(6) of Table (4.5) contain the results of estimating equation (4.3) using time and town level fixed effects. The first thing to note is that the coefficients on average annual wages, child poverty, the pupil-teacher ratio, percent black, and percent Hispanic are no longer significant. These variables tend to change little within towns over time. The coefficient on the unemployment rate is significant at the 10% or 5% level, depending on the specification. In column (4), a one percentage point increase in the rate of unemployment is associated with a 1% decrease in aggregate property values. In the OLS specifications, the coefficient on the pupil-teacher ratio was positive, which is counterintuitive. However, once the town-level fixed effects are included, the coefficient on the pupil-teacher ratio is negative, yet insignificant. In the fixed effects specification, there is no evidence that changes in the 4th grade pass rate impact aggregate property values.

The fixed effects specifications clearly suggest that property taxes are negatively capitalized into property values. However, changes in the property tax rate are not fully capitalized. Depending on the specification, the present value of future tax liabilities is capitalized at a rate between 24% to 38%. This less than full capitalization is in aggreement with the findings in the literature.

Column (4) in Table (4.5) shows that the coefficient on local spending per equalized pupil is very similar to the one obtained in the long difference specification, however it is now significant at the 5% level. Interpretted directly, a 1% increase in local spending per equalized pupil is associated with a 0.08% decrease in aggregate

property values. Column (5) measures spending when private donations are included. While the coefficient is negative, it is no longer statistically significant. When spending and taxes are interacted with the dummy indicating Act 60 induced donations, the results are very similar to those obtained in the long difference specification. Property taxes are negatively capitalized but the coefficeint on the donation term interacted with property taxes is positive. The coefficient on spending including private donations is negative and insignificant, yet the coefficient on spending interacted with the private donations dummy is positive and significant at the 5% level. Under this specification, towns that were choosing to utilize private donations to fund local public schools saw aggregate property values increase by 0.33% for each 1% increase in spending. The donation dummy itself is negative and highly significant, indicating that a town adopting private contributions experienced decreases in aggregate property value.

Table (4.6) uses the time period from 1996 through 2003 to look at the impact of changes in taxes and spending on aggregate property values. The first two columns use the long difference specification and the second two columns are the results from a fixed effects specification. Overall, these results confirm that changes in tax rates are negatively capitalized in Vermont. In the long difference specifications, changes in spending does not appear to have a significant impact on aggregate property values. However, under the fixed effects specification, the negative relationship between spending and property values remains. Hence, it does not appear that the negative coefficient on spending is an artifact of the relationship between spending and property values under Act 60. When Vermont adopted Act 60, it appears that changes in spending were not capitalized into property values as one would predict. Instead, increases in spending are associated with falling aggregate property values while re-

ductions in spending are associated with higher property values. This would suggest that unlike the case in Texas, property values in Vermont did not converge following the adoption of Act 60. Further exploration of this issue is in order to determine the specific characteristics of Vermont's school finance reform, or characteristics unique to the state of Vermont, make it such that additional spending is not valued by homeowners.

4.6 Conclusion

This study exploits the variation in spending, tax rates, and student achievement under Vermont's Act 60 to explore the impact of changes in these variables on aggregate property values. Aggregate property values are used to address the concern that capitalization is less than complete when the supply of housing is relatively elastic, as is the case in a rural state like Vermont. Assessing changes in aggregate property values captures the increase in property values that occurs via the traditional capitalization channels. Changes in aggregate property values also captures increased demand for a district's tax, expenditure, service package which may occur via new construction of housing units.

Overall, this study finds no evidence that changes in test pass rates on state administered tests are associated with increases in aggregate property values. This evidence is consistent with recent evidence from Texas (Dills, 2004). While test scores did improve during the sample period, these improvements were not associated with increases in aggregate property values. This could be taken as evidence that higher test scores were not being viewed as evidence of improvements in school quality.

What this study does find is that tax rates, and in some specifications school

spending, are negatively capitalized into property values. Increases in tax rates are expected to reduce property values, and this is found to be the case in Vermont. Changes in tax rates without corresponding changes in service levels redistributes wealth amongst property owners.

The coefficient on local spending per equalized pupil is consistently negative and in the fixed effects specification the coefficient is significant. A one percent increase in local spending per equalized pupil is associated with a 0.08% decline in aggregate property values. When private donations are included, and spending is interacted with a dummy variable indicating whether a town had Act 60 induced private donations an interesting pattern emerges. For towns that relied on private donations to finance local public schools, additional spending was positively associated with increases in aggregate property values. For these towns, a one percent increase in spending including private donations was associated with a 0.33% increase in aggregate property values in the fixed effects model and a 0.43% increase in aggregate property values in the long difference model. This results appears to be consisent with the finding that property values are more responsive to spending in communities that are constrained. In Vermont, towns that chose to utilize private contributions to finance local public schools were contrained by Act 60 from spending the desired amount on education.

The finding that increased spending may be negatively related to aggregate property values in Vermont certainly warrants further investigation. Since spending was increasing in low property wealth towns this would suggest that property values diverged, rather than converged following school finance reform in Vermont. An understanding of how property values react to school finance reform is necessary to understand the impact that reform will have on the property tax base in the state. It would be useful for policymakers to understand in what types of districts property

values are likely to respond to changes caused by school finance reform. This can help policymakers design reforms that limit erosion of the tax base and minimize unintended redistribution of wealth.

Table 4.1: Variation in Variables of Interest

1996 - 2004	Mean	Overall	Between	Within
Log (Aggregate Property Values)	17.9676	1.4751	1.4559	0.2518
Total Property Tax Rate	1.9447	0.4571	0.3107	0.3362
Total School Property Tax Rate	1.4315	0.3594	0.2360	0.2718
Log (Local Spending per Equalized Pupil)	8.8087	0.1697	0.1180	0.1241
Log (Local Spending per Equalized Pupil plus Private Donations)	8.8223	0.1668	0.1287	0.1086
2000 - 2004				
Log (Aggregate Property Values)	18.0218	1.3528	1.3478	0.1375
Total Property Tax Rate	1.9915	0.4093	0.3469	0.2187
Total School Property Tax Rate	1.4771	0.3028	0.2417	0.1831
Log (Local Spending per Equalized Pupil)	8.8246	0.1675	0.1422	0.0901
Log (Local Spending per Equalized Pupil plus Private Donations)	8.8491	0.1591	0.1328	0.0890
4th Grade Pass Rate (1999 - 2003)	60.0745	12.8177	10.0312	8.1888
4th Grade Pass Rate Smoothed (1999 - 2003)	60.4763	11.7438	10.1644	6.0513

Pass rates from 1999 - 2003 are included here since previous years pass rates are used in the regression analysis.

Table 4.2: Summary Statistics: Differences

Summary Statistics: Changes between 2000 and 2004

	(1) All Available Data					(2) No Missing Data					(3) Towns with Private Contributions				
	Obs.	Mean	St. Dev	Min	Max	Obs.	Mean	St. Dev	Min	Max	Obs.	Mean	St. Dev	Min	Max
Property Values, Taxes, Spending															
Change in Log Aggregate Property Value	262	0.160	0.246	-1.43	1.84	180	0.146	0.226	-1.43	0.68	16	0.150	0.215	-0.39	0.59
Change in Property Tax Rate	261	-0.245	0.340	-1.21	1.02	180	-0.264	0.273	-1.21	0.98	16	0.082	0.355	-0.62	0.98
Change in School Tax Rates	261	-0.220	0.281	-1.35	0.99	180	-0.225	0.221	-0.96	0.56	16	0.113	0.257	-0.54	0.42
Change in Log Local Spending per Equalized Pupil	253	0.132	0.123	-0.29	1.07	180	0.135	0.088	-0.20	0.38	16	0.057	0.124	-0.20	0.26
Change in Log Local Spending With Private Contributions	253	0.141	0.129	-0.20	1.07	180	0.146	0.102	-0.18	0.74	16	0.182	0.230	-0.14	0.74
Town Level Economic / Demographic Variables															
Change in Log Average Annual Wages	232	0.056	0.123	-0.34	0.57	180	0.060	0.110	-0.34	0.51	16	0.001	0.124	-0.34	0.17
Change in % Unemployment	247	1.221	2.180	-1.80	22.40	180	0.975	1.048	-1.10	8.40	16	1.344	1.971	0.00	8.40
Change in % of Children in Poverty	255	-1.018	3.031	-35.00	10.00	180	-0.917	1.746	-6.83	2.79	16	0.107	1.274	-2.59	2.24
Change in Pupil-Teacher Ratio	231	-1.162	2.278	-8.00	14.20	180	-1.109	1.842	-6.90	3.10	16	-1.644	2.306	-5.20	2.70
Change in % Black Students	231	0.083	1.341	-10.10	5.33	180	0.195	1.129	-4.22	5.33	16	0.099	1.486	-1.95	4.00
Change in % Hispanic Students	231	0.382	1.321	-1.81	16.67	180	0.367	0.814	-1.81	3.99	16	0.051	0.351	-0.32	1.05
Test Score Pass Rates															
Change in 4th Grade Math Pass Rate	181	6.229	16.877	-46.76	68.48	171	6.350	16.855	-46.76	68.48	16	1.521	17.367	-46.76	19.28
Change in 4th Grade Math Pass Rate Smoothed	192	8.581	13.783	-31.12	46.28	180	8.135	13.637	-31.12	46.28	16	5.913	12.611	-20.69	19.56
Change in 4th Grade Reading Pass Rate	180	-1.803	12.851	-45.73	39.86	169	-1.305	12.988	-45.73	39.86	16	-5.834	10.984	-20.78	12.94
Change in 4th Grade Reading Pass Rate Smoothed	193	0.194	11.773	-35.00	36.50	180	0.341	11.581	-35.00	36.50	16	-1.288	7.144	-14.38	14.00
Change in 4th Grade Writing Pass Rate	180	2.695	16.915	-36.33	63.36	169	2.647	16.702	-36.33	63.36	16	0.524	17.074	-35.82	27.78
Change in 4th Grade Writing Pass Rate Smoothed	193	7.649	14.483	-39.75	55.98	180	7.786	14.424	-39.75	55.98	16	5.520	11.764	-16.41	29.89
Change in 2nd Grade Reading Pass Rate	181	11.152	18.784	-37.33	100.00	161	11.440	17.640	-37.33	100.00	15	-1.219	14.044	-37.33	16.00
Change in 2nd Grade Reading Pass Rate Smoothed	196	8.986	14.500	-30.36	80.00	173	9.292	13.480	-22.00	80.00	16	2.530	10.059	-16.67	12.41
Change in 4th Grade Average Pass Rate	179	2.626	12.770	-36.21	41.24	169	2.780	12.856	-36.21	41.24	16	-1.018	11.782	-22.91	12.93
Change in 4th Grade Average Pass Rate Smoothed	192	5.354	10.641	-28.35	40.93	180	5.357	10.817	-28.35	40.93	16	3.456	7.880	-7.68	15.30
Tuition Dummy															
Dummy Variable for Tuition High School Students	253	0.261	0.440	0.00	1.00	180	0.217	0.413	0.00	1.00	16	0.375	0.500	0.00	1.00

Aggregate property values, spending, and wages are deflated to $2000 using the Northeast Consumer Price Index. Property tax rates are taxes owed per $100 of assessed value. Smoothed values for test score pass rates are the average of the current year and previous years pass rates.

Table 4.3: Summary Statistics: Panel

Summary Statistics: Panel Data from 2000 - 2004															
	2000			2001			2002			2003			2004		
Full Sample	Obs.	Mean	St. Dev.	Obs.	Mean	St. Dev.	Obs.	Mean	St. Dev.	Obs.	Mean	St. Dev.	Obs.	Mean	St. Dev.
Log (Aggregate Property Values)	262	17.971	1.352	262	17.983	1.316	262	17.998	1.361	262	18.028	1.371	262	18.130	1.367
Total Property Tax Rate	261	2.015	0.406	262	2.045	0.419	262	2.084	0.414	262	2.044	0.400	262	1.770	0.323
Log(Local Spending per Equalized Pupil)	253	8.747	0.156	253	8.787	0.148	254	8.844	0.162	254	8.866	0.168	254	8.879	0.166
Log (Local Spending per Equalized Pupil plus Private Donations)	253	8.765	0.154	253	8.811	0.138	254	8.871	0.151	254	8.893	0.152	254	8.906	0.155
Log (Average Annual Wages)	235	10.007	0.247	234	10.031	0.235	234	10.050	0.237	234	10.054	0.257	234	10.058	0.245
Unemployment Rate	247	3.014	1.751	247	3.902	3.971	247	4.722	5.143	247	4.864	3.686	248	4.492	5.226
% of Children in Poverty	255	9.886	6.435	259	10.076	7.341	259	10.720	7.591	259	10.060	5.340	259	8.776	4.645
Pupil-Teacher Ratio	231	12.683	2.252	233	12.049	2.190	233	11.636	2.416	233	11.507	2.265	236	11.496	2.671
% Black Students	231	0.865	1.229	233	0.931	1.474	233	1.000	1.384	233	0.992	1.216	236	0.966	1.123
% Hispanic Students	231	0.311	0.550	233	0.383	0.609	233	0.482	0.698	233	0.580	1.135	236	0.689	1.361
4th Grade Pass Rate Smoothed	226	59.315	11.469	227	58.419	11.447	227	59.783	11.461	219	61.200	11.515	195	64.211	12.169
Donation Dummy	262	0.065	0.247	262	0.092	0.289	262	0.088	0.284	262	0.076	0.266	262	0.084	0.278
Sample once Towns with all Years Missing Eliminated	Obs.	Mean	St. Dev.	Obs.	Mean	St. Dev.	Obs.	Mean	St. Dev.	Obs.	Mean	St. Dev.	Obs.	Mean	St. Dev.
Log (Aggregate Property Values)	226	18.276	0.869	226	18.278	0.866	226	18.306	0.860	226	18.336	0.866	226	18.443	0.851
Total Property Tax Rate	226	2.065	0.346	226	2.089	0.380	226	2.137	0.368	226	2.112	0.357	226	1.817	0.274
Log(Local Spending per Equalized Pupil)	224	8.764	0.126	224	8.798	0.137	224	8.852	0.147	224	8.881	0.143	224	8.901	0.145
Log (Local Spending per Equalized Pupil plus Private Donations)	224	8.782	0.123	224	8.822	0.125	224	8.877	0.135	224	8.907	0.124	224	8.929	0.130
Log (Average Annual Wages)	216	10.020	0.234	217	10.041	0.226	217	10.059	0.228	217	10.068	0.244	216	10.074	0.229
Unemployment Rate	219	2.857	1.167	219	3.478	1.516	219	4.143	2.137	219	4.585	2.566	219	3.903	2.090
% of Children in Poverty	224	9.885	5.432	226	10.061	5.635	226	10.712	5.968	226	10.167	5.147	226	8.861	4.465
Pupil-Teacher Ratio	221	12.735	2.071	223	12.081	1.984	223	11.672	2.330	223	11.566	2.247	226	11.565	2.637
% Black Students	221	0.904	1.242	223	0.902	1.114	223	1.007	1.311	223	1.004	1.156	226	0.996	1.124
% Hispanic Students	221	0.325	0.558	223	0.390	0.607	223	0.494	0.698	223	0.539	0.822	226	0.636	0.878
4th Grade Pass Rate Smoothed	223	59.321	11.476	226	58.423	11.472	226	59.825	11.468	218	61.250	11.517	195	64.211	12.169
Donation Dummy	226	0.066	0.249	226	0.097	0.297	226	0.088	0.285	226	0.075	0.264	226	0.088	0.285

Aggregate property values, spending, and wages are deflated to $2000 using the Northeast Consumer Price Index. Property tax rates are taxes owed per $100 of assessed value. Smoothed values for test score pass rates are the average of the current year and previous years pass rates.

Table 4.4: Long Difference Results

Dependent Variable: Change in Log (Aggregate Property Values) between 2000 and 2004

	(1)	(2)	(3)	(4)
Change in Property Tax Rate	-0.1059	-0.1193	-0.1210	-0.1608
	-1.77	*-2.02*	*-2.05*	*-2.16*
Change in Log Local Spending per Equalized Pupil	-0.0751	-0.0775		
	-0.43	*-0.45*		
Change in Log Local Spending With Private Contributions			0.1109	0.0054
			0.87	*0.02*
Act 60 Induced Private Contribution Dummy				-0.0090
				-0.10
Change in Property Tax Rate * Donation Dummy				0.2229
				1.61
Change in Log Spending with Private Contributions * Donation Dummy				0.4261
				1.54
Change in Log Average Annual Wages	-0.2942	-0.2614	-0.2807	-0.2142
	-1.36	*-1.21*	*-1.35*	*-0.98*
Change in % Unemployment	-0.0194	-0.0156	-0.0197	-0.0240
	-1.57	*-1.23*	*-1.50*	*-1.82*
Change in % of Children in Poverty	-0.0276	-0.0275	-0.0271	-0.0299
	-2.10	*-2.09*	*-2.07*	*-2.19*
Change in Pupil-Teacher Ratio	-0.0031	-0.0026	-0.0012	0.0003
	-0.41	*-0.36*	*-0.15*	*0.04*
Change in % Black Students	0.0250	0.0263	0.0254	0.0269
	1.64	*1.72*	*1.67*	*1.83*
Change in % Hispanic Students	-0.0043	-0.0051	-0.0035	-0.0020
	-0.22	*-0.27*	*-0.19*	*-0.10*
Change in 4th Grade Average Pass Rate Smoothed	-0.0006	-0.0002	-0.0004	-0.0005
	-0.49	*-0.18*	*-0.32*	*-0.38*
Dummy Variable for Tuition High School Students		0.0614	0.0583	0.0500
		1.76	*1.64*	*1.40*
# Obs.	180	180	180	180
R^2	0.11	0.12	0.12	0.14

Coefficients are in plain font and t-statistics are italicized. Reported t-statistics are calculated using standard errors clustered at the town level and are robust to heteroskedasticity and within-town serial correlation. Constants and time dummies are included but not reported.

Table 4.5: Panel Specifications

Dependent Variable: Log(Aggregate Non-Corporate Property Values)						
	Pooled OLS			Fixed Effects		
	(1)	(2)	(3)	(4)	(5)	(6)
Present Value of Change in Tax Liabilities	0.0827	-0.7598	1.1607	-0.2469	-0.2415	-0.3474
	0.21	*-2.02*	*2.98*	*-2.45*	*-2.37*	*-2.71*
Log(Local Spending per Equalized Pupil)	*-0.75*			*-0.08*		
	-3.49			-2.19		
Log (Local Spending per Equalized Pupil plus Private Donations)		0.5257	-0.4149		-0.0498	-0.0687
		2.46	*-1.86*		*-0.94*	*-0.89*
Donation Dummy			-3.7433			-3.3244
			-0.46			*-2.89*
PV Change in Tax Liability * Donation Dummy			-1.3151			0.2524
			-1.15			*1.29*
Log (Spending plus Donations) * Donation Dummy			0.7604			0.3277
			0.84			*2.21*
Log (Average Annual Wages)	1.6015	1.5437	1.5926	-0.0299	-0.0346	-0.0048
	13.81	*12.85*	*12.42*	*-0.65*	*-0.75*	*-0.09*
Unemployment Rate	-0.0102	-0.0062	-0.0211	-0.0067	-0.0073	-0.0127
	-1.05	*-0.68*	*-2.07*	*-2.32*	*-2.45*	*-2.75*
% of Children in Poverty	-0.0184	-0.0107	-0.0129	-0.0113	-0.0107	-0.0136
	-4.51	*-2.57*	*-2.93*	*-1.64*	*-1.56*	*-1.73*
Pupil-Teacher Ratio	0.0325	0.0411	0.0555	-0.0038	-0.0039	-0.0036
	3.50	*4.31*	*5.58*	*-1.33*	*-1.34*	*-1.07*
% Black Students	0.0575	0.0585	0.0459	0.0003	-0.0004	0.0021
	2.49	*2.46*	*2.00*	*0.05*	*-0.08*	*0.36*
% Hispanic Students	0.1744	0.1892	0.1391	-0.0024	-0.0025	-0.0049
	6.10	*6.42*	*4.96*	*-0.25*	*-0.26*	*-0.53*
4th Grade Pass Rate Smoothed	0.0125	0.0108	0.0078	0.0000	0.0000	0.0001
	7.07	*6.17*	*4.29*	*0.00*	*-0.01*	*0.08*
# Obs.	1244	1244	1039	1244	1244	1039
R^2	0.36	0.35	0.44	0.23	0.23	0.25
F-Test for significance of fixed effects				217.79	219.39	143.39

Coefficients are in plain font and t-statistics are italicized. Reported t-statistics are calculated using standard errors clustered at the town level and are robust to heteroskedasticity and within-town serial correlation. Constants and time dummies are included but not reported. For the fixed effects specifications, the R^2 reported is net of fixed effects (the within R^2).

Table 4.6: Specifications Spanning 1996 - 2003

	Difference between 1996 and 2003		Fixed Effects	
	(1)	(2)	(3)	(4)
Change in Property Tax Rate	-0.2259	-0.2300		
	-4.76	*-4.83*		
Change in Log Local Spending per Equalized Pupil	0.0877			
	0.39			
Change in Log Local Spending With Private Contributions		0.2087		
		0.85		
Change in Log Average Annual Wages	-0.0679	-0.0598		
	-0.45	*-0.41*		
Change in % Unemployment	-0.0518	-0.0501		
	-3.91	*-3.84*		
Present Value of Change in Tax Liabilities			-0.2888	-0.2828
			-3.15	*-3.05*
Log(Local Spending per Equalized Pupil)			-0.1630	
			-3.57	
Log (Local Spending per Equalized Pupil plus Private Donations)				-0.1838
				-3.62
Log (Average Annual Wages)			0.0053	0.0004
			0.12	*0.01*
Unemployment Rate			-0.0030	-0.0034
			-1.22	*-1.38*
# Obs.	233	233	2110	2110
R^2	0.24	0.25	0.20	0.19
F-Test for Fixed Effects			320.34	309.07

Coefficients are in plain font and t-statistics are italicized. Reported t-statistics are calculated using standard errors clustered at the town level and are robust to heteroskedasticity and within-town serial correlation. Constants and time dummies are included but not reported. For the fixed effects specifications, the R^2 reported is net of fixed effects (the within R^2).

Chapter 5

Conclusion

This dissertation explores equity, student achievement, and property values under Vermont's Act 60. While a number of previous works have explored the impacts of school finance reforms in a number of states, very little academic research has been explored the consequences of school finance reforms in Vermont. Understanding the implications of Vermont's reforms and the state's unique circumstances and experiences will help policymakers in the future to draft efficient and effective school finance policies.

Generally, states that undertake school finance reform do experience an equalization in resources. Spending per pupil did equalize in Vermont following Act 60, and the magnitude of equalization was similar to what other states experienced. Of concern is the fact that school finance equalizations may lead to a reduction in the overall level of resources available. In Vermont, where the tax price of education increased dramatically for many towns following the policy's adoption, concerns that leveling-down may occur were well founded. However, over time Vermont does not appear to have suffered from reduced resources.

While reform does appear to have equalized resources as measured by local spending per equalized pupil, the strong incentives that Act 60 created to avoid participating in the state financing system led to a large increase in private donations in property wealthy towns. These donations worked to undo the legislations equalizing power, diminishing the equity achievements of the reform.

Whether low income districts that received funds under Act 60 were likely to have seen improvements in test scores is another issue investigated. Since test score pass rate data are only available after Act 60 had already begun to phase-in, this study is not able to fully exploit the natural experiment. However, there was a substantial amount of variation in spending as Act 60 was phased-in and while the policy was in place. This variation is exploited to identify the potential impacts of additional dollars on student achievement in Vermont.

Overall, the evidence suggests that there may have been an impact in spending on 4th grade math pass rates. However, this conclusion is sensitive to model specification. There is no evidence that addition funds had any impact on reading or writing test pass rates. Further, there is no evidence to suggest that initially low achieving schools were more likely to experience increases in test pass rates following Act 60. Low spending schools, on the other hand, may have been more likely to be able to turn additional resources into results as measured by test pass rates.

Finally, the impact of tax rates, spending, and achievement on property values is explored. Contrary to what was found in nationwide studies of capitalization, extra spending in Vermont does not appear to be associated with increases in aggregate property values. Homeowners in Vermont do not appear to value additional spending under Act 60. Property values do respond positively to extra spending in towns that relied on private donations, suggesting that these towns may have been prevented

from spending the desired amount on schools under Vermont's school finance reform.

In sum, this work suggests that the employment of a combination formula for school aid using foundation aid and a guaranteed tax base system can enhance the equity of a state's school finance system. However, the equalizing effects of school finance reform will be tempered when school districts that desire higher levels of spending, but are disincentivized from spending more under the state's program, are able to generate alternative revenues for education. Further, this type of reform may not be successful at generating greater equity for the bottom half of the distribution. Instead, it appears that equity in Vermont was achieved by forcing previously high spending towns to spend less.

In addressing the questions as to whether additional resources result in enhanced performance on standardized tests, the evidence is mixed. Overall, there is not strong evidence that additional funding led to improvements in a broad measure of student performance. However, the evidence that suggests that additional funding may have been more effective in increasing math pass rates and in previously low spending schools suggests that more research should be done on the effectiveness of targeted funds. In Vermont, additional funds entered budgets without stipulations regarding where such funds were to be used.

Finally, as property values did not respond positively to additional spending under Act 60, it would appear that the legislation was not viewed as increasing the quality of public schools in receiving districts. Spending itself does not appear to have raised test scores, nor do property values respond to changes in test scores. When test scores and tax rates are held constant, additional spending on local public schools does not increase a town's aggregate property value relative to other towns. Hence, in Vermont, it does not appear that spending was being viewed as synonymous with

quality from the perspective of property owners.

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