

# The Effects of Financial Resources on Test Pass Rates: Evidence from Vermont's Equal Education Opportunity Act

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## Abstract:

This paper explores the impact of changes in financial resources on student performance under Vermont's Equal Education Opportunity Act (Act 60). Act 60 caused dramatic changes in per-pupil resources 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. The changes in town spending caused by Vermont's Act 60 appear to have had a positive and significant impact on 4<sup>th</sup> grade math pass rates and 2<sup>nd</sup> grade reading pass rates. However, these spending changes did not significantly impact 4<sup>th</sup> grade reading or writing pass rates. When resources are measured at the school level, however, there does not appear to be a relationship between spending and test score pass rates.

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To date, lawsuits challenging states' public school financing methods have been brought in 45 out of the 50 states.<sup>1</sup> 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 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.<sup>2</sup> 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.<sup>3</sup> 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. With

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<sup>1</sup> <http://www.schoolfunding.info/litigation/litigation.php3> [accessed: August 18, 2008]

<sup>2</sup> California, Texas, and New Jersey have all faced litigation challenging the constitutionality of school finance on equity grounds.

<sup>3</sup> Kentucky's ruling in *Rose v. Council for Better Education* was based on adequacy.

annual spending and test pass rate data, panel data methods can be utilized instead of cross-sectional or difference-in-difference specifications. With panel data, we can control for school specific effects on student performance and include fixed year effects. Second, the structure of Act 60 provides a natural instrument to address the possible endogeneity of spending. The instrumental variables model is overidentified allowing for testing of the instruments' exogeneity. Third, 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.

Results from the research provide evidence that town spending increased 4<sup>th</sup> grade math and 2<sup>nd</sup> grade reading test pass rates. Spending did not appear to significantly increase 4<sup>th</sup> grade reading or 4<sup>th</sup> grade writing test pass rates. When the sample is split to evaluate initially low performers separately from initially high performers, there is no evidence that there is a higher return to funding for either low-spending or low-performing schools. Also, when spending is examined at the school level, there does not appear to be a relationship between spending and test pass rates.

The remainder of the paper proceeds as follows. Section 2 reviews the literature. Section 3 provides background information on Vermont's Act 60. Section 4 outlines the empirical methodology. Section 5 describes the data. Section 6 presents the results. Finally, section 7 concludes.

## *II. Literature Review*

A number of studies have examined the relationship between school resources and student achievement by exploiting the variation in expenditure programs across states. The results of such inter-state studies provide good background for predictions about the impacts of single-state school finance reforms. Using SAT scores, Card and Payne (2002) found that spending equalization between richer and poorer districts is associated with a modest equalization in test scores from students with different family backgrounds. Card and Payne do not, however, find evidence that increased expenditures increase SAT scores generally. Husted and Kenny (2000) argued that equalization of school spending results in schools that are less efficient, where efficiency is measured in a school's ability to produce measurable student achievement. Equalization leads to a reduction in mean SAT scores without necessarily reducing the variance in scores.<sup>4</sup> Figlio (1997) and Downes and Figlio (1998) examined the impact of tax and/or expenditure limits on student performance. Figlio (1997) found that tax limitations reduced 8<sup>th</sup> grade student performance across disciplines. Downes and Figlio (1998) found that generally, tax limits reduced 12<sup>th</sup> grade math performance. However, if the reforms were not in response to court mandates, then student achievement in mathematics increased. Students in initially low spending districts were found to benefit most from such reforms. Hoxby (2001) provided evidence that equalization may be more likely to improve achievement in schools that otherwise would have been low spending.

With the exception of Figlio (1997) all of the aforementioned studies use achievement measures from high school seniors. Wenglinsky (1998) found that there was no relationship

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<sup>4</sup> A reduction in the variance of test scores would indicate equalization in student achievement. If average test scores fall and the variance in test scores fall then the expected trade-off between equity and efficiency holds true. Husted and Kenny (2000) provide evidence that this trade-off is weak in the realm of school expenditures and output, if it exists at all.

between certain types of spending and achievement for 12<sup>th</sup> grade students. In earlier work, Wenglinsky (1997) found that spending on instruction and central office administration is associated with improvements in achievement for 8<sup>th</sup> graders and even more so for 4<sup>th</sup> graders. His results suggest that targeted funds are more effective in improving standardized test scores for younger students as opposed to older ones.

Inter-state studies provide relatively broad generalizations which may be useful to policymakers in the early stages of planning reform. In order to precisely quantify the effects of finance reforms single-state studies may provide better insight. Single-state studies have proven useful for a number of reasons. First, single-state studies may be able to focus on achievement in younger students as the availability of consistent national data on achievement is limited.<sup>5</sup> Second, single-state studies are often able to exploit policy changes that lead to variation in per-pupil expenditure as a result of school finance equalizations. Third, single-state studies do not have to find a method to control for often immeasurable differences in states' educational climates. Finally, single-state studies do not have to address aggregation problems that often plague inter-state studies.<sup>6</sup> In spite of the benefits that single-state studies offer the results typically do not yield a clear consensus on the relationship between school resources and student achievement.

California was one of the first states to undertake dramatic school finance reform and ultimately limit property taxes via Proposition 13.<sup>7</sup> Downes (1992) demonstrated that while per-

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<sup>5</sup> Most national studies use achievement measures for 12<sup>th</sup> grade students such as SAT/ACT scores and the dropout rate since these measures are consistent on a national scale. While the National Assessment of Educational Progress (NAEP) is administered nationally few national studies have utilized this dataset (Wenglinsky (1997, 1998) being the exceptions). Figlio (1997) used the National Education Longitudinal Study (NELS) survey of 8<sup>th</sup> graders administered in 1988 and 1990.

<sup>6</sup> Hanushek, Rivkin, and Taylor (1996) demonstrate that using aggregated data exacerbates bias in regressions on the school resources coefficients. This bias seems to be especially pronounced when data is aggregated to the state-level for intra-state studies.

<sup>7</sup> See Fischel (1989) for a discussion of how *Serrano v. Priest* led to Proposition 13.

pupil expenditures did equalize during the late 1970s and into the 1980s, the performance among 6<sup>th</sup> grade students on state tests did not. Sonstelie et al. (2000) did not definitively state that California's school finance equalization was the cause of declines in SAT and NAEP performance during the 1990s, but did note that the "timing of the drop in test scores is suggestive."

Kansas and Kentucky both implemented school finance equalizations in the early 1990s. An examination of trends in pass rates on statewide standardized tests in Kansas did not indicate that equalization in spending equalized test outcomes (Duncombe and Johnson 2004). However, Deke (2000) found that increases in spending in Kansas did increase the probability of a student pursuing post-secondary education. In an evaluation of Kentucky's school finance reform, Clark (2003), using ACT and NAEP scores, did not find evidence that increased funds to poorer districts reduced the achievement gap.<sup>8</sup>

In the mid-1990s Michigan reduced property taxes and adopted a state sales tax as the primary means of funding schools.<sup>9</sup> The result was that spending increased sharply for low-spending districts while holding constant for high spending districts (Cullen and Loeb, 2004). Papke (2005) found that the increases in spending significantly increased 4<sup>th</sup> grade student mathematics performance on state exams, particularly for those schools with initially low performance levels. However, Roy (2004) found that these results did not transcend tests. Roy was able to replicate Papke's finding that more funds did lead to improvements in performance on state tests. However, performance did not appear to improve on the ACT or NAEP exams

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<sup>8</sup> After the Kentucky Education Reform Act (KERA) the only group to experience significant achievement gains was black students. Clark (2003) concludes that the improvement in black student's test scores is not necessarily attributable to increased resources. Black students tend to be from relatively wealthier urban areas, who did not receive additional funds as a result of KERA.

<sup>9</sup> One unique feature of Michigan's school finance reform was that it was legislatively driven rather than the result of complying with a court ruling.

when more funding became available. Roy's (2004) results highlight the importance of relying on multiple measures of achievement.<sup>10</sup>

Another drawback of the Papke (2005) study is that it only looked at pass rates in mathematics.<sup>11</sup> Other studies have found that changes in financing have a greater impact on math test scores than reading scores (Downes and Figlio 1998, Roy 2004). Hence, studies focusing solely on mathematics performance are more likely to generate significant results.

Massachusetts also passed legislation to reform school finance in the 1990s. The Massachusetts Education Reform Act (MERA) centralized funds and channeled resources towards previously low spending districts. Guryan (2003) found that increased expenditures following MERA did improve student outcomes on state tests for both 4<sup>th</sup> and 8<sup>th</sup> grade students. However, Guryan's study only has three years of post-MERA data. Hence, it is not known if the performance gains persisted.

### ***III. Background***

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.

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

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<sup>10</sup> State test results are often widely published and made available to parents and communities. This may put more pressure on schools to perform well on these exams and may result in "teaching to the test". Independent tests, such as the NAEP, SAT, and ACT, are less likely to result in test outcome manipulation and may give a more accurate picture of student learning.

<sup>11</sup> The reading test changed during the sample period such that comparable reading pass rates were not available.

schools attended by residents of that town.<sup>12</sup> Towns with high property wealth were able to levy low tax rates and achieve relatively high per-pupil revenues.<sup>13</sup> 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.<sup>14</sup>

Under Act 60, Vermont established a statewide property tax of \$1.10 for each \$100 of assessed property value.<sup>15</sup> Towns were allowed to phase in this tax rate change through FY2001 (tax year 2000). As of FY2001, each town that levied local property taxes of \$1.10 per \$100 of 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

$$SP = \sum \tau'_i \cdot \frac{V_i}{C_i}, \quad (1)$$

where  $SP$  is the total resources held in the sharing pool,  $\tau'_i$  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).<sup>16</sup>

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<sup>12</sup> The state also provided school districts with foundation grants.

<sup>13</sup> 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.

<sup>14</sup> 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.

<sup>15</sup> Tax adjustments were made for households earning less than \$75,000 such that the school tax did not exceed 2% of household income.

<sup>16</sup> 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.

Additional spending on education for the town is directly proportional to their local excess tax levy, such that

$$AR_i = \delta \tau_i', \quad (2)$$

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

$$\sum AR_i = SP. \quad (3)$$

Setting the sum of additional education revenues equal to the sharing pool one can solve for  $\delta$ , the equalized yield:

$$\delta = \frac{\sum \tau_i' \cdot \frac{V_i}{C_i}}{\sum \tau_i'}. \quad (4)$$

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.<sup>17</sup> 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).

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<sup>17</sup> 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.

The change in incentives led to large changes in per-pupil spending in a number of towns.<sup>18</sup> Stowe, for example, saw real local spending per equalized pupil fall over \$5000 between FY1997 and FY2001.<sup>19</sup> 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 taxes went down.<sup>20</sup> Local spending per equalized pupil varied dramatically during the Act 60 phase-in period.

After the phase-in of Act 60 was complete, spending levels failed to stabilize. Most districts experienced increases in real local spending per equalized pupil during the post phase-in period. However, there were a handful of districts that experienced declines.<sup>21</sup> The variation in spending could have been coming from a number of sources. Since schools 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. Towns may have chosen to contribute privately to their schools and lower their tax burden, thus reducing reported local spending per equalized pupil.<sup>22</sup> If the towns did not contribute privately throughout the entire sample period, this would cause variation in local spending per equalized pupil.<sup>23</sup> While

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<sup>18</sup> In the sample used for the regression analysis, the mean change in real local spending per equalized pupil between FY1997 and FY2001 was \$290.65 with a standard deviation of \$1017.71. In this section, the restricted sample refers to the sample where 4<sup>th</sup> grade math scores were available. Fourth grade reading, 4<sup>th</sup> grade writing, and 2<sup>nd</sup> grade reading samples largely contain the same schools so separate breakdowns are not provided.

<sup>19</sup> Forty two towns in our restricted sample saw real local spending per equalized pupil fall between FY1997 and FY2001. Twenty seven towns in our restricted sample saw real local spending per equalized pupil increase by over \$1000 in the same period.

<sup>20</sup> There are 55 towns included in the regression analysis where property tax rates fell and spending increased.

<sup>21</sup> Five districts in our restricted sample saw real local spending per equalized pupil fall between FY2001 and FY2004. For the restricted sample, the average increase in real spending per equalized pupil during this period was \$889.07 with a standard deviation of \$577.78.

<sup>22</sup> Local education foundations worked throughout the state to raise funds locally and circumvent the state's sharing pool. Local spending per equalized pupil does not include privately donated dollars.

<sup>23</sup> It can be argued that private contributions undo some of the observed changes in spending. While it is highly unlikely that private contributions undo all the changes in spending, to the extent that legislated changes are undone our results can be viewed as the impact of state legislated spending on education achievement.

determining the source of variation is beyond the scope of this paper, what is important is that there was variation in spending.

#### ***IV. Empirical Method***

Most empirical investigations of the relationship between schooling expenditures and student achievement employ either a difference-in-difference (DID) approach or use an education production function. To use the DID method the researcher must be able to identify the treatment and control groups. In this study, we investigate the impacts of legislatively driven spending changes in Vermont on the achievement of Vermont students. Since data is only available on Vermont students, and all students in the state of Vermont are subject to treatment, a suitable control group is not available.<sup>24</sup>

Estimation of an education production function is more appropriate given the nature of our data and circumstances of the reform. 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. Since the unit of analysis is the school, inputs should be measured at the school level. Inputs in the education production function include school demographics as well as the closest available approximations of spending at specific schools.<sup>25</sup>

Explicitly, our education production function takes the form

$$P_{it} = \gamma_t \lambda + X_{it} \beta + R_{it} \rho + \varepsilon_{it} \quad (5)$$

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<sup>24</sup> One could separate the sample into schools whose spending continued to follow trend after Act 60 implementation and label students in these schools as the control group. This label, however, would be artificial and would not represent a true DID analysis.

<sup>25</sup> Spending is measured either at the town level or at the district level. The measure of spending used here is discussed in the data section.

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, the number of students taking the test, and the pupil-teacher ratio at the school,  $R_{it}$  is the measure of per-pupil school resources, or spending per-pupil, and  $\varepsilon_{it}$  is the random error. The production function also includes fixed year effects,  $\gamma_t$ , 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.<sup>26</sup> 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.<sup>27</sup> Roy's (2004) finding confirms that performance gains on state standardized tests are not always mirrored by performance increases on nationwide tests.<sup>28</sup>

Since the data is a panel, and since real spending has varied over time, school fixed effects,  $\mu_i$ , are used to control school attributes that remain the same over time. In order to isolate these fixed effects the error term is decomposed as follows:

$$P_{it} = \gamma_t \lambda + X_{it} \beta + R_{it} \rho + \mu_i + e_{it} . \quad (6)$$

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<sup>26</sup> 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.

<sup>27</sup> 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..

<sup>28</sup> We were 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.

Essentially,  $\mu_i$  is a dummy variable 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, Papke (2005) being the notable exception, are able to employ fixed effects analysis. In order to prevent the fixed effects from capturing school spending real spending must change over time. Papke (2005) was able to use fixed effects analysis to study the effects of spending on student pass rates in Michigan following the adoption of Michigan's Proposal A. Since real spending does change under Vermont's Act 60 we are able to utilize fixed effects.

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.<sup>29</sup> If this is the case, spending is endogenous due to simultaneity. The potential for omitted variable bias and simultaneity suggest that the potential endogeneity of spending should be explored when estimating Equation (6).

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<sup>29</sup> Ferguson and Ladd (1996) present evidence of such compensatory spending in Alabama.

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.<sup>30</sup> In FY1997, before Act 60 implementation began, the correlation coefficient between local spending per equalized pupil and school property tax rates was 0.09. In FY2001, 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.<sup>31</sup>

Tax rates, however, are not a determinant of schools' test pass rates. Since our empirical model is overidentified, overidentification tests can be used to confirm that the instruments are uncorrelated with the error term. 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) will not necessarily raise property tax rates to fund education. Even if higher property wealth towns did have a higher preference for educational expenditures, they 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. The direct correlation between school tax rates and test pass rates was low throughout our sample period.<sup>32</sup> Hence, tax rates are directly

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<sup>30</sup> Vermont property taxes between 1999 and 2004 had two components: the school tax and the local/municipal property tax levy. Each is reported separately.

<sup>31</sup> 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.

<sup>32</sup> The highest correlation between school property tax rates and pass rates on any test in any year was in FY2000 when the correlation coefficient between 4<sup>th</sup> grade math pass rates and local school property tax rates was 0.17.

related to per-pupil spending but are not likely related to unobserved determinants of student test performance.<sup>33</sup>

## ***V. Data Sources***

The dependent variable in the analysis is a school's average pass rate for 4<sup>th</sup> grade students on math, reading, and writing tests. We also have schools' average pass rates on 2<sup>nd</sup> grade reading tests.<sup>34</sup> 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. Since the objective is to analyze the impact of spending on student achievement, data on schools' pass rates are merged with school demographic data. Then, school level data is matched to data on spending per-pupil at the town level and the district 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 pupil-teacher ratio, the number of students eligible for free or reduced price lunch, and the number of minority students.<sup>35</sup> The CCD data are gathered annually and the variables can be included in a fixed effects analysis.

Each school is matched to a town and also a district since an explicit measure of spending per student at the school level is not available. Spending measures at the town and district levels

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<sup>33</sup> By construction, Act 60 makes it such that towns that tax at a higher rate spend more per-pupil on education, so that each town with the same tax rate spends the same on education per-pupil. This does not mean that \$1 of locally generated property tax revenues necessarily results in an additional dollar of local spending. The tax price for education varies widely across towns.

<sup>34</sup> 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.

<sup>35</sup> 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, we do not present results including this variable.

are used to approximate per-pupil spending at the school level. In Vermont, the vast majority of towns only operate one school or are part of a joint or union district. The unrestricted sample contains 240 schools, which belong to one of 213 school districts, located within one of 215 towns.<sup>36</sup> Education spending reported at the town level our first approximation of school level spending.<sup>37</sup> 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.<sup>38</sup>

Each town reports two types of spending: local spending per equalized pupil and budgeted spending per equalized pupil.<sup>39</sup> Local spending per equalized pupil includes the foundation grant received 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 2007). In spite of its drawbacks, local education spending per equalized pupil is the spending measure used by the Vermont Department of Education in reports on school resource equity.

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<sup>36</sup> 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.

<sup>37</sup> 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.

<sup>38</sup> The weights were the number of equalized pupils in each town.

<sup>39</sup> 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.

Towns also report budgeted spending per equalized pupil, which includes the funds omitted from the local spending measure.<sup>40</sup> The problem with looking at budgeted expenditures is that the pupil counts used here do not include tuitioned students.<sup>41</sup> Budgeted spending per equalized pupil is based on town level pupil counts. So if a town receives a large number of tuitioned students, budgeted spending per equalized pupil will be artificially high. The number of tuitioned students varies widely across towns. Schools receive students tuitioned from towns that do not operate their own schools. There are 17 Vermont towns that tuition all their students. The data does not track where tuitioned students go to appropriately adjust pupil counts in the budgeted expenditures calculations. Furthermore, there are 66 towns that tuition only high school students. Typically, residents of these towns choose which high school they would like to attend, and the town pays tuition to the chosen school. Here again, we cannot track where tuitioned secondary students are going to adjust per-pupil budgeted expenditures calculations. The consequence of these inaccurate pupil counts is that budgeted spending tends to overstate spending inequality. Below results using local spending per equalized pupil are presented and discussed.<sup>42</sup>

Using town spending data is one way to approximate resources flowing into local public schools. Another way to approximate school level resources is with district level data. District level spending was obtained from the CCD School District Finance Survey. Data available at the district level includes total current expenditures per-pupil and total instructional expenditures per-pupil. Instructional expenditures includes teachers salaries and benefits as well as expenditures on classroom supplies and materials. Current expenditures includes instructional

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<sup>40</sup> Budgeted spending is determined by the town annually.

<sup>41</sup> Many small towns do not operate their own elementary school. Even more do not operate a high school. These towns pay tuition to other local public schools to educate their children.

<sup>42</sup> The regression results using budgeted spending per equalized pupil presented in Appendix 5 are similar to results using local spending per equalized pupil.

expenditures, administrative expenditures, operational expenses, as well as expenditures on support services such as food, medical, and transportation. Capital expenditures are not included.<sup>43</sup> For the present analysis, one distinct disadvantage in using instructional spending and current spending is the loss of our instrument. While local spending and budgeted spending per equalized pupil are both highly correlated with schools tax rates, instructional spending and current spending are not. In FY2001, the correlation coefficient for school property tax rates and instructional spending was 0.10 and the correlation coefficient for school property tax rates and current spending was 0.09.<sup>44</sup>

The instrument in the dataset is the school tax rate.<sup>45</sup> The school tax rate is used to instrument for local spending per equalized pupil and budgeted spending per equalized pupil. 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.

Appendix 1 contains a key for variable abbreviations. The summary statistics for the full sample are presented in Appendix 2. The spending measures in our full sample include spending for all school districts, whether or not test score pass data is available. For 2003 and 2004 test score pass data are available for schools where more than ten students took the test. For 1999 – 2002 mean pass rates are reported for the school even when the number of test takers was less than ten. Generally, pupil teacher ratios and schools size (membership) information were

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<sup>43</sup> For the seven districts that consolidated to become three districts during our time period we aggregated the data from individual districts prior to consolidation to create a consistent measure of spending for the 1997 – 2004 period.

<sup>44</sup> This suggests that Act 60 was successful in making local spending per equalized pupil at the town level directly proportional to the tax rate but did not successfully do the same for current and instructional expenditures. Full exploration of the equalizing effects of Act 60 on different measures of student resources is beyond the scope of this paper, but will be explored in detail elsewhere.

<sup>45</sup> Throughout the sample period, Vermont distinguished between the school property tax rate and the local or municipal property tax rate.

complete in the CCD. The percent of free or reduced price lunch data 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, membership (school size), and pupil teacher ratios are calculated.<sup>46</sup>

In order to create a balanced panel, only schools where the mean pass rate was reported in all six years are used. Consequently, schools with very few test takers are dropped from the sample. With very few test takers, average pass rates are less likely to be a reliable indicator of the school's quality. Summary statistics for this sample are presented in Appendix 3. As the summary statistics show, restricting our sample in this way tends to increase the membership and number of test takers variables. This shows that larger schools are more likely to be in the sample for all six years. Restricting the sample in this way, however, does not change our dependent variable, average pass rate, or spending, our variable of interest.<sup>47</sup> For 4<sup>th</sup> grade math, reading, and writing there are 166 schools where the pass rate is available for each year. For 2<sup>nd</sup> grade reading, there are 167 schools where the pass rate is available for each year.<sup>48</sup>

As indicated by the summary statistics, real expenditures per pupil increased over the six year period.<sup>49</sup> For fourth graders, math and writing scores increased (but not reading). Second grade reading scores also increased over the time period. The empirical analysis investigates whether it is likely that increased in spending caused higher test scores.

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<sup>46</sup> 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.

<sup>47</sup> Unbalanced panel regressions yield quantitatively similar results.

<sup>48</sup> Using imputed values increased the sample size from 111 to 166 for the math, reading, and writing and from 110 to 167 for the DRA. Regression results using non-imputed data are quantitatively similar.

<sup>49</sup> All spending variables are reported in 2004 dollars. Real values were calculated using the CPI-U.

## ***VI. Results***

For comparison, first a pooled OLS model with year dummy variables is analyzed. In the pooled OLS specification, the pass rate is regressed on local spending per equalized pupil, the number of test takers, the pupil/teacher ratio in the school, the percent of students in the school who are eligible for free or reduced price lunch, and school size. For the pupil/teacher ratio, the percent of students eligible for free or reduced price lunch, and school size quadratic terms are included to capture non-linearities.<sup>50</sup> Spending and school size are included on the right hand side in logarithmic form to account for their diminishing effect on pass rates. The results from regressing the pass rate of 4<sup>th</sup> grade math, reading, writing and 2<sup>nd</sup> grade reading on local education spending is presented in the first column of tables (1) – (4). The coefficient on local spending per equalized pupil is positive in all cases but only significant for 4<sup>th</sup> grade math pass rates. The pupil-teacher ratio has negative and significant effect; in all cases the p-value is 0.05 or less. The percentage of students eligible for free or reduced price lunch also has a negative and highly significant effect; the p-value is close to zero in all cases. The number of test takers and the school size coefficients are insignificant.<sup>51</sup>

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 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 (6) above):

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<sup>50</sup> There was no evidence of diminishing effects for the number of test takers.

<sup>51</sup> The coefficient on budgeted expenditures are similar in sign and significance but tend to be larger in magnitude.

$$P_{it} = \gamma_t \lambda + X_{it} \beta + R_{it} \rho + \mu_i + e_{it} \quad (6)$$

Again,  $\mu_i$  is the individual school effect. If  $\mu_i$  is uncorrelated with  $X_{it}$  and  $R_{it}$  than random effects will be the consistent and efficient regression technique. However, if  $\mu_i$  is correlated with elements in  $X_{it}$  or  $R_{it}$  the random effects estimator will be inconsistent. The Hausman test is used to evaluate whether  $\mu_i$  is correlated with elements of  $X_{it}$  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 3 in Tables 1 – 4 present the results of the fixed effects analysis. An F-test of the joint significance of the fixed effects confirms that these individual effects are significant. After controlling for the effect of each individual school, the estimated effect of spending on pass rates tends to be larger. However, the spending coefficient is only significant for 2<sup>nd</sup> grade reading pass rates. The magnitude and significance for the coefficient on school level variables, such as the pupil teacher ratio, the percent of students eligible for free or reduced price lunch tends, tend to diminish when school level fixed effects are introduced. Papke (2005) finds a similar pattern and attributes the reduction in coefficients and significance to the fact that school level variables tend to vary much more across schools than within schools over short time frames.<sup>52</sup> The pupil-teacher ratio is no longer significant for any of the pass rates. The percentage of students eligible for free or reduced price lunch is still estimated to have a negative impact on all pass rates but is only significant for 4<sup>th</sup> grade writing and 2<sup>nd</sup> grade reading. After the introduction of fixed effects, the coefficient on the number of test takers is negative and significant at the 10% level for all the 4<sup>th</sup> grade pass rates. Within schools, each additional pupil that takes the test tends to

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<sup>52</sup> For the number of test takers, pupil-teacher ratios, the percentage of students eligible for free or reduced price lunch, and school size the “between” standard deviations are much larger than the “within” standard deviations.

bring down the mean pass rate by 0.1 percentage points. With pooled OLS, the cross-sectional variation in school size masked this trend.

Next, the possibility that spending is endogenous is explored. Spending decisions may depend on test score pass rates. Spending could be higher if students are suspected to be low performers, biasing the spending coefficient down (Papke, 2005). Alternatively, spending could be higher in communities that have a greater preference toward education, and are willing to put more time and effort into educating their children. These communities are likely to have higher pass rates, biasing the coefficient on spending upward. For the regressions presented, the instrument for local spending per equalized pupil is the school property tax rate. Act 60 required that spending be directly related to the property tax rate set by a town but not directly related to district property wealth. Hence, there is a strong correlation between local spending per equalized pupil in a town and the town's school property tax rate. There is, however, no reason to suspect that high tax towns (or low tax towns) are likely to have higher pass rates. Under Act 60, high property wealth towns were not incentivized to raise local property taxes for schools. If wealthy towns did raise taxes, much of the revenue raised locally went into the sharing pool and was ultimately used to finance schooling in other districts. But high property wealth does not necessarily mean the residents of the town own the property. High valued property could be owned by corporations that put pressure on the town to keep property taxes low to create area jobs. The property could also be owned by out of state residents who may not take an interest in the quality of local schools. This may incentivize local residents to increase property taxes since the burden is being shifted to non-residents. In any case, there is not a clear cut relationship nor pattern of incentives between property tax rates and school pass rates.

Column (2) in tables 1 – 4 present two-stage least squares (2SLS) estimates where local spending per equalized pupil is treated as endogenous using the instrument is the school property tax rate in the town. The model is overidentified since the contemporaneous and lagged property tax rates are included. Overidentification allows the validity of the instruments to be more closely examined. In order for an instrumental variable to be valid it must be highly correlated with the regressor suspected to be endogenous while the instrument itself remains exogenous. First stage regressions confirm that the instrument is highly correlated with the suspected endogenous regressor. Shea's partial  $R^2$  is 0.48 for the 2<sup>nd</sup> grade reading test and 0.54 for all 4<sup>th</sup> grade tests. The first stage F-statistics all exceed 457, indicating that school property taxes and the lag of school property taxes are highly significant in the first stage regression. Since the model is overidentified, one can test the overidentifying restrictions. Essentially, an overidentification examines whether the additional instruments are uncorrelated with the error term (Wooldridge, 2002). The overidentification test's null hypothesis is that the instruments are exogenous. Rejecting this null usually indicates that the instruments are not exogenous, but rejection can also be evidence of model misspecification (Cameron and Trivedi, 2005). Tables (1) – (4) report Hansen's J statistic as the heteroskedasticity robust overidentification test statistic. For 4<sup>th</sup> grade math, 4<sup>th</sup> grade reading, and 2<sup>nd</sup> grade reading the overidentification test statistic fails to reject the null hypothesis of instrument exogeneity. In the 4<sup>th</sup> grade writing specification, the overidentification test statistic does reject the null hypothesis of instrument exogeneity. The fixed effects, however, are significant in the 4<sup>th</sup> grade writing specification. Before selecting a preferred specification fixed effects results are compared to fixed effects with instrumental variables specifications.

The instruments address the potential endogeneity of spending when fixed effects are also included. Fixed effects with instrumental variables results are presented in column (4) of tables 1 – 4. Shea's partial  $R^2$  is 0.27 for all 4<sup>th</sup> grade specifications and 0.22 for 2<sup>nd</sup> grade reading scores. First stage F-statistics are all greater than 115 and highly significant, confirming that school property tax rates are highly correlated with local spending per equalized pupil. Furthermore, in all four cases the overidentification test statistic is insignificant, failing to reject a null hypothesis of instrument exogeneity. A Davidson-MacKinnon test is used to determine if spending is endogenous in the fixed effects specifications.<sup>53</sup> Here, the null hypothesis is that fixed effects estimated using OLS is consistent. For 4<sup>th</sup> grade reading, 4<sup>th</sup> grade writing, and 2<sup>nd</sup> grade reading there does not appear to be any evidence that local spending per equalized pupil is endogenous. Hence, the preferred specification for assessing the impacts of spending on test score pass rates for 4<sup>th</sup> grade reading and writing and 2<sup>nd</sup> grade reading is fixed effects estimated with OLS. When school level fixed effects are included, there is no evidence that local spending per equalized pupil is endogenous. For 4<sup>th</sup> grade math pass rates, there is weak evidence that local spending per equalized pupil is endogenous (the p-value on the Davidson-MacKinnon test is 0.09). Papke (2005) also determined it was appropriate to treat spending as endogenous when estimating 4<sup>th</sup> grade math scores.<sup>54</sup>

For 4<sup>th</sup> grade reading, 4<sup>th</sup> grade writing, and 2<sup>nd</sup> grade reading the fixed effects estimates obtained using OLS are the most reliable. Here, spending is only significant for 2<sup>nd</sup> grade reading. The point estimate suggest that a 10% increase in local spending per equalized pupil

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<sup>53</sup> The Davidson-MacKinnon test is similar to the Durbin-Wu-Hausman test. The Davidson-MacKinnon test compares the OLS coefficients of a fixed effects regression to those found using instrumental variable techniques. If the difference in the coefficients is systematic then instrumental variables techniques should be used. If not, then the OLS estimates are consistent.

<sup>54</sup> The fact that spending only appears endogenous in the 4<sup>th</sup> 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.

increases the pass rate by 1.24 percentage points. For 4<sup>th</sup> grade math scores, if spending is in fact endogenous, local spending per equalized pupil does have a significant impact on pass rates. When spending is treated as endogenous, a 10% increase in local spending per equalized pupil leads to a 2.57 percentage point increase in the 4<sup>th</sup> grade math test pass rate.

Our results are most directly comparable to Papke (2005) who used test pass rates on 4<sup>th</sup> grade math scores in Michigan.<sup>55</sup> When spending was treated as endogenous Papke's (2005) coefficient estimates on spending increased five-fold for the fixed effects specification. Papke (2005) found that a 10% increase in spending increases the pass rate by 3.73 percentage points. The impact of spending on 4<sup>th</sup> grade math pass rates found here are highly consistent with those of Papke (2005). However, spending does appear to impact pass rates for 4<sup>th</sup> grade reading or writing, contradicting Papke (2005). In the preferred specification for 4<sup>th</sup> grade reading and writing (the fixed effects model), there is no evidence that spending caused improvements in test score pass rates. For 2<sup>nd</sup> grade reading, there is evidence that spending did lead to higher pass rates, but there is not any evidence to suggest that spending should be treated as endogenous.<sup>56</sup>

#### *Effects for Initially Low Spending or Low Achieving Schools*

Another concern is that schools that are initially low spending or initially low performing may respond differently to additional funds than those that are initially high spending or high performing. If a school is initially high spending, extra dollars may be less likely to lead to improvements in the pass rate on tests of basic skills. If a school is initially high performing, it

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<sup>55</sup> A side by side comparison of Papke (2005) and the 4<sup>th</sup> grade math results is in Appendix 4.

<sup>56</sup> The impact of current spending and past spending was also explored. Including lagged expenditures or measuring spending as the average of current year spending and previous year spending does not change the conclusions presented here. These regression results are available upon request.

may be more difficult for schools to improve performance even when provided with extra financial resources.

Tables 5 and 6 present the results from splitting the sample into schools that were above median spending or below median spending for the 1996-1997 school year.<sup>57</sup> Columns (1) – (4) of table 5 present the results for 4<sup>th</sup> grade math scores. Columns (1) and (2) present the fixed effects and fixed effects with instrumental variables for schools that were initially high spending. Columns (3) and (4) present the fixed effects and fixed effects with instrumental variables for schools that were initially low spending. The Davidson-MacKinnon statistic suggests that math scores for 4<sup>th</sup> graders are only endogenous for those districts that were initially high spending. Further, the coefficient on local spending per equalized pupil is positive and significant only for those schools that were initially high spending. Many schools that were high spending prior to Act 60 reduced local spending per equalized pupil as a result of the legislation. Between FY1997 and FY2001 42 out of the 166 schools used in the regression analysis experienced a decrease in local spending per equalized pupil. Among those schools that experienced a decrease in spending as a result of Act 60, average pass rate growth over the period was 1.7% slower than pass rate growth for schools that did not decrease real local spending per equalized pupil during the phase-in of Act 60. To some degree, falling pass rates in schools with spending reductions may be driving the positive coefficient on local spending per equalized pupil for initially high spending schools. In 2000, 4<sup>th</sup> grade math pass rates fell in 25 out of the 42 schools that reduced spending following Act 60. In 2001, 4<sup>th</sup> grade math pass rates fell in 20 of the 42 schools that reduced spending following Act 60.

Column (3) in table 5 present the preferred specification for 4<sup>th</sup> grade math pass rates in initially low spending schools. Even though the coefficient on local spending per equalized pupil

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<sup>57</sup> This is before Act 60 phase-in began.

is insignificant, there is some evidence that money may matter for districts that were initially low spending. The coefficient on the pupil-teacher ratio is negative and significant for schools that were initially low spending. The positive impact of the pupil-teacher ratio on 4<sup>th</sup> grade math pass rates could be mean that increased spending on classroom teachers may improve performance in initially low spending schools.<sup>58</sup>

For 4<sup>th</sup> grade writing, 4<sup>th</sup> grade reading, and 2<sup>nd</sup> grade reading there is no evidence that spending is endogenous in any of the specifications. The fixed effects results provide the best estimate of the impact of local spending per equalized pupil on reading and writing test score pass rates.

The results from reading and writing pass rates can also address the question as to whether schools that were high spending before Act 60, and tended to be those that were most financially constrained by the reform, showed that spending did significantly impact student performance. Increases in test scores amongst schools that were high spending before the reform could mean that marginal dollars are more effective in those schools. Column (5) in table 6 shows the results for 2<sup>nd</sup> grade reading pass rates for initially high spending schools from the preferred specification, fixed effects. The pupil-teacher ratio has a negative and significant impact on pass rates only for those districts that were initially high spending. Initially high spending districts saw less of a reduction in pupil-teacher ratios than did the initially low spending group as Act 60 took hold and was in place between 1999 and 2003. If pupil-teacher ratios were falling more slowly for initially high spending schools, it could indicate that these schools were more financially constrained. For the initially high spending groups, marginal resources could bring larger gains in achievement as measured by the pass rate. This story is

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<sup>58</sup> Removing the pupil-teacher ratio from the regression does not make the coefficient on local spending per equalized pupil significant.

consistent with the statistical evidence presented for 2<sup>nd</sup> grade reading scores, but does not appear to be consistent with 4<sup>th</sup> grade reading and writing test score pass rates.

One may be more inclined to believe that schools that were initially low spending would benefit more from additional resources than those that were initially higher spending. Low property values prior to Act 60 denied students living in low property wealth districts access to educational funds. When Act 60 equalized the tax base spending in many of these low property wealth districts increased. The overarching question here is “did spending increases lead to greater achievement as measured by test score pass rates?” Generally, the evidence does not support this assertion. As shown in column (5) of table 6, there is moderate evidence that spending in schools that were initially low spending may improve 4<sup>th</sup> grade reading pass rates (the coefficient on the spending coefficient has a p-value of 0.12). There is not any evidence that 4<sup>th</sup> grade math and writing pass rates nor 2<sup>nd</sup> grade reading pass rates increased as a result of additional spending in initially low spending schools. Furthermore, for all pass rates, Chow tests fail to reject the null hypothesis that the models should be estimated as pooled samples with initially above high spenders combined with initially low spenders.<sup>59</sup>

The sample can also be split to compare the effects of spending on schools with initially low pass rates to schools with initially high pass rates.<sup>60</sup> These results appear in tables 7 and 8. The Davidson-MacKinnon test does not suggest that spending is endogenous for any of the test score pass rate specifications. Looking at the fixed effects results, the magnitude of the coefficient local spending per equalized pupil is very similar for 4<sup>th</sup> grade math and 2<sup>nd</sup> grade reading; additional dollars do not appear to influence schools that were initially low performing

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<sup>59</sup> The F-statistic for the Chow tests is compared to the critical value for F(181, 634). For the fixed effects estimates on the math test the F-statistic is 0.15. The math fixed effects instrumental variable and other fixed effects specifications have an even lower F-statistic.

<sup>60</sup> The median pass rate in 1999 is used to split the sample.

differently than those that were initially high performing.<sup>61</sup> Columns (1) and (3) in table 8 show that the effects of spending on the 4<sup>th</sup> grade reading pass rate is positive and significant for initially low achieving schools while the effect of spending for schools with initially higher pass rates is negative but insignificant. However, Chow tests again indicate that the models should be estimated as pooled samples.<sup>62</sup>

These results differ from those found in Papke (2005). Papke was able to conclude that spending more effectively increased performance on 4<sup>th</sup> grade math tests in initially low performing schools. Here, there is little or no difference in the estimated impact of spending on 4<sup>th</sup> grade math pass rates between the initially low performing and high performing schools. Also there is no evidence that low performing schools see greater increases in pass rates in other grade levels or subjects from additional spending. One possible explanation is that initially low performing schools in Michigan were deprived of financial resources critical to basic skills learning, while initially low performing schools in Vermont were not.

### *Other Measures of Spending*

Local spending per equalized pupil measures spending per student in the town where the school is located. Local spending is a good indicator of the town's willingness to spend on education. However, local spending per equalized pupil does not distinguish between elementary and secondary school students, does not include parental contributions or federal aid, and generally does not isolate resources going directly to the classroom. Furthermore, local education

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<sup>61</sup> The insignificance of the spending coefficient for 2<sup>nd</sup> grade reading pass rate for school with initially low pass rates (column (7) in table 8) may be the result of the low sample size.

<sup>62</sup> The F-statistic for F(181, 634) is 0.20 for the reading test. None of the other fixed effects specifications have an F-statistic that exceeds 0.42.

spending per equalized pupil may understate actual spending in schools receiving private donations, overstating spending equalization between schools.

Other common measures of school resources are used to further explore the relationship between spending and test pass rates - current and instructional expenditures per-pupil at the district level. In Vermont, districts and towns are coterminous. A number of districts only have one school serving elementary students. In an attempt to look at classroom resources more closely, current spending and instructional spending are used in regressions for schools where there was only one elementary school in the district.<sup>63</sup> For 2<sup>nd</sup> grade, 134 of the 167 schools are the sole elementary school in the district. For 4<sup>th</sup> grade, 138 of the 166 schools are the only elementary schools in the district. Tables 9 and 10 present the fixed effects regressions for current expenditures per-pupil and instructional expenditures per-pupil, respectively.<sup>64</sup> In no case, are any of the coefficients on spending significant.<sup>65</sup> Looking only at town level resources may lead one to conclude that more spending may lead to higher test score pass rates for 4<sup>th</sup> grade math and 2<sup>nd</sup> grade reading tests. However, increasing resources that are most closely related to classroom activities, does not appear to increase test pass rates.

## ***VII. Conclusions***

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. The availability of annual data allows for the use of fixed effects. Further, the design of

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<sup>63</sup> There may be one school in the district if the school serves elementary and secondary students, if the students attend a separate high school within the district, or if the students attend a union high school.

<sup>64</sup> Current and instructional expenditures are measured per-pupil, not per equalized pupil, and therefore do not adjust for anticipated cost differentials.

<sup>65</sup> Results using all 167 schools for 2<sup>nd</sup> grade students and 166 schools for 4<sup>th</sup> grade students also produce insignificant coefficients on current and instructional spending.

Vermont's Act 60 provides a natural instrument for addressing the possible endogeneity of spending.

In assessing the effects of spending on test score pass rates in Vermont under Act 60, evidence shows that town level spending may have led to improvements in 4<sup>th</sup> grade math pass rates and 2<sup>nd</sup> grade reading pass rates. For 4<sup>th</sup> grade math pass rates, point estimates suggest that a 10% increase in local spending per equalized pupil would increase pass rates by about 2.57 percentage points. For 2<sup>nd</sup> grade reading, the point estimate suggests a 10% increase in local spending per equalized pupil leads to a 1.24 percentage point increase in the pass rate. For 4<sup>th</sup> grade reading and writing, there is no evidence that increased spending leads to higher test pass rates.

While others have found that spending on under-funded or poorly performing schools may result in greater returns (greater test score gains) this research does not suggest this was generally the case in Vermont. There is moderate evidence that spending in initially low spending schools or initially low performing schools for 4<sup>th</sup> grade reading may provide greater returns. However, the result does not hold for other types of test scores.

Finally, to address concerns that local spending per equalized pupil may not fully capture spending at the school level, we turn to instructional spending and current spending from the CCD. Neither of these measures of spending appears to have a positive and significant impact on test score pass rates.

Overall, these results underscore the importance of looking at multiple measures of performance and spending, especially as the data generally available is far from precise. States should continue to make efforts to track classroom level resources. This should help researchers quantify the impact of additional resources on measurable indices of student achievement.

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Table 1

Dependent Variable: 4th Grade Math Pass Rate				
	Pooled OLS	IV	Fixed Effects	FE-IV
llsepr	7.92 <i>0.04</i>	8.55 <i>0.11</i>	9.86 <i>0.18</i>	25.74 <i>0.02</i>
n_tested	0.02 <i>0.23</i>	0.02 <i>0.24</i>	-0.11 <i>0.01</i>	-0.11 <i>0.01</i>
i_puptch	-3.76 <i>0.05</i>	-3.78 <i>0.05</i>	0.14 <i>0.95</i>	0.31 <i>0.90</i>
i_puptch <sup>2</sup>	0.13 <i>0.07</i>	0.13 <i>0.07</i>	-0.02 <i>0.79</i>	-0.03 <i>0.77</i>
i_pfrl	-0.50 <i>0.00</i>	-0.50 <i>0.00</i>	-0.14 <i>0.51</i>	-0.18 <i>0.38</i>
i_pfrl <sup>2</sup>	0.00 <i>0.01</i>	0.00 <i>0.01</i>	0.00 <i>0.86</i>	0.00 <i>0.75</i>
li_member	12.16 <i>0.27</i>	12.13 <i>0.27</i>	35.42 <i>0.36</i>	31.09 <i>0.48</i>
li_member <sup>2</sup>	-1.18 <i>0.22</i>	-1.17 <i>0.22</i>	-3.34 <i>0.29</i>	-2.95 <i>0.41</i>
Obs.	996	996	996	996
R <sup>2</sup>	0.22	0.22	0.16	0.16
School Level Fixed Effects			yes	yes
F-Test: u <sub>i</sub> =0			5.15	5.12
p value			<i>0.00</i>	<i>0.00</i>
Shea's R <sup>2</sup>		0.54		0.27
First Stage F		564.98		150.30
p value		<i>0.00</i>		<i>0.00</i>
Hansen J Statistic		6.20		5.25
p value		<i>0.10</i>		<i>0.15</i>
Durban Wu Hausman Statistic		0.03		
p value		<i>0.86</i>		
Davidson MacKinnon Statistic				2.80
p value				<i>0.09</i>

<sup>1</sup>Coefficients are in plain font and p-values are italicized

<sup>2</sup>Reported standard errors are robust to heteroskedasticity and within-school serial correlation

<sup>3</sup>R<sup>2</sup> reported are net of fixed effects

<sup>4</sup>Constant and time dummies are included but not reported

<sup>5</sup>Fixed effects are for 166 schools

Table 2

Dependent Variable: 4th Grade Writing Pass Rate				
	Pooled OLS	IV	Fixed Effects	FE-IV
llsepr	0.09 <i>0.98</i>	-11.04 <i>0.04</i>	2.53 <i>0.69</i>	-3.12 <i>0.81</i>
n_tested	0.03 <i>0.05</i>	0.04 <i>0.03</i>	-0.11 <i>0.02</i>	-0.11 <i>0.01</i>
i_puptch	-3.39 <i>0.02</i>	-3.17 <i>0.03</i>	-0.94 <i>0.55</i>	-1.01 <i>0.55</i>
i_puptch <sup>2</sup>	0.12 <i>0.02</i>	0.11 <i>0.03</i>	0.04 <i>0.46</i>	0.04 <i>0.48</i>
i_pfri	-0.59 <i>0.00</i>	-0.64 <i>0.00</i>	-0.45 <i>0.02</i>	-0.43 <i>0.04</i>
i_pfri <sup>2</sup>	0.00 <i>0.00</i>	0.00 <i>0.00</i>	0.00 <i>0.05</i>	0.00 <i>0.07</i>
li_member	7.26 <i>0.51</i>	7.84 <i>0.47</i>	-19.69 <i>0.69</i>	-18.04 <i>0.69</i>
li_member <sup>2</sup>	-0.86 <i>0.36</i>	-0.96 <i>0.31</i>	1.09 <i>0.78</i>	0.94 <i>0.80</i>
Obs.	996	996	996	996
R <sup>2</sup>	0.22	0.21	0.18	0.17
School Level Fixed Effects			yes	yes
F-Test: u <sub>i</sub> =0			3.80	3.80
p value			<i>0.00</i>	<i>0.00</i>
Shea's R <sup>2</sup>		0.54		0.27
First Stage F		564.83		150.43
p value		<i>0.00</i>		<i>0.00</i>
Hansen J Statistic		3.34		1.39
p value		<i>0.07</i>		<i>0.24</i>
Durban Wu Hausman Statistic		9.38		
p value		<i>0.00</i>		
Davidson MacKinnon Statistic				0.30
p value				<i>0.58</i>

<sup>1</sup>Coefficients are in plain font and p-values are italicized

<sup>2</sup>Reported standard errors are robust to heteroskedasticity and within-school serial correlation

<sup>3</sup>R<sup>2</sup> reported are net of fixed effects

<sup>4</sup>Constant and time dummies are included but not reported

<sup>5</sup>Fixed effects are for 166 schools

Table 3

Dependent Variable: 4th Grade Reading Pass Rate				
	Pooled OLS	IV	Fixed Effects	FE-IV
llsepr	4.35 <i>0.18</i>	1.34 <i>0.75</i>	5.11 <i>0.28</i>	2.82 <i>0.76</i>
n_tested	0.01 <i>0.59</i>	0.01 <i>0.52</i>	-0.09 <i>0.01</i>	-0.09 <i>0.01</i>
i_puptch	-3.22 <i>0.01</i>	-3.16 <i>0.01</i>	-1.13 <i>0.37</i>	-1.15 <i>0.40</i>
i_puptch <sup>2</sup>	0.12 <i>0.00</i>	0.12 <i>0.01</i>	0.05 <i>0.20</i>	0.05 <i>0.24</i>
i_pfri	-0.47 <i>0.00</i>	-0.48 <i>0.00</i>	-0.28 <i>0.11</i>	-0.27 <i>0.12</i>
i_pfri <sup>2</sup>	0.00 <i>0.01</i>	0.00 <i>0.01</i>	0.00 <i>0.21</i>	0.00 <i>0.23</i>
li_member	2.59 <i>0.78</i>	2.75 <i>0.76</i>	10.16 <i>0.75</i>	10.83 <i>0.76</i>
li_member <sup>2</sup>	-0.38 <i>0.64</i>	-0.40 <i>0.61</i>	-0.91 <i>0.73</i>	-0.97 <i>0.74</i>
Obs.	996	996	996	996
R <sup>2</sup>	0.24	0.24	0.04	0.04
School Level Fixed Effects			yes	yes
F-Test: u <sub>i</sub> =0			3.32	3.32
p value			<i>0.00</i>	<i>0.00</i>
Shea's R <sup>2</sup>		0.54		0.27
First Stage F		564.83		150.43
p value		<i>0.00</i>		<i>0.00</i>
Hansen J Statistic		0.34		0.01
p value		<i>0.56</i>		<i>0.95</i>
Durban Wu Hausman Statistic		1.16		
p value		<i>0.28</i>		
Davidson MacKinnon Statistic				0.08
p value				<i>0.78</i>

<sup>1</sup>Coefficients are in plain font and p-values are italicized

<sup>2</sup>Reported standard errors are robust to heteroskedasticity and within-school serial correlation

<sup>3</sup>R<sup>2</sup> reported are net of fixed effects

<sup>4</sup>Constant and time dummies are included but not reported

<sup>5</sup>Fixed effects are for 166 schools

Table 4

Dependent Variable: 2nd Grade Reading Pass Rate				
	Pooled OLS	IV	Fixed Effects	FE-IV
llsepr	3.59 <i>0.27</i>	1.21 <i>0.80</i>	12.44 <i>0.01</i>	8.89 <i>0.39</i>
n_tested	-0.03 <i>0.10</i>	-0.03 <i>0.11</i>	-0.05 <i>0.24</i>	-0.05 <i>0.22</i>
i_puptch	-2.21 <i>0.05</i>	-2.11 <i>0.07</i>	-2.01 <i>0.16</i>	-2.02 <i>0.16</i>
i_puptch <sup>2</sup>	0.08 <i>0.06</i>	0.07 <i>0.09</i>	0.08 <i>0.16</i>	0.08 <i>0.14</i>
i_pfrl	-0.27 <i>0.00</i>	-0.28 <i>0.00</i>	-0.35 <i>0.02</i>	-0.35 <i>0.01</i>
i_pfrl <sup>2</sup>	0.00 <i>0.40</i>	0.00 <i>0.36</i>	0.00 <i>0.02</i>	0.00 <i>0.01</i>
li_member	7.28 <i>0.42</i>	7.32 <i>0.42</i>	-5.48 <i>0.89</i>	-4.43 <i>0.91</i>
li_member <sup>2</sup>	-0.43 <i>0.58</i>	-0.45 <i>0.57</i>	0.08 <i>0.98</i>	-0.01 <i>1.00</i>
Obs.	1002	1002	1002	1002
R <sup>2</sup>	0.20	0.20	0.18	0.18
School Level Fixed Effects			yes	yes
F-Test: u <sub>i</sub> =0			3.10	3.08
p value			0.00	0.00
Shea's R <sup>2</sup>		0.48		0.22
First Stage F		457.80		115.29
p value		0.00		0.00
Hansen J Statistic		0.25		0.70
p value		0.62		0.40
Durban Wu Hausman Statistic		0.59		
p value		0.44		
Davidson MacKinnon Statistic				0.15
p value				0.70

<sup>1</sup>Coefficients are in plain font and p-values are italicized

<sup>2</sup>Reported standard errors are robust to heteroskedasticity and within-school serial correlation

<sup>3</sup>R<sup>2</sup> reported are net of fixed effects

<sup>4</sup>Constant and time dummies are included but not reported

<sup>5</sup>Fixed effects are for 167 schools

Table 5

	4th Grade Math				4th Grade Writing			
	Above Median Spending		Below Median Spending		Above Median Spending		Below Median Spending	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Fixed Effects	FE-IV	Fixed Effects	FE-IV	Fixed Effects	FE-IV	Fixed Effects	FE-IV
llsepr	2.73	33.58	19.40	12.28	2.25	4.97	3.07	-0.68
	<i>0.72</i>	<i>0.01</i>	<i>0.16</i>	<i>0.55</i>	<i>0.76</i>	<i>0.74</i>	<i>0.85</i>	<i>0.98</i>
n_tested	-0.17	-0.14	-0.08	-0.08	-0.12	-0.11	-0.11	-0.11
	<i>0.03</i>	<i>0.05</i>	<i>0.16</i>	<i>0.13</i>	<i>0.14</i>	<i>0.15</i>	<i>0.05</i>	<i>0.04</i>
i_puptch	2.73	3.03	-5.50	-5.55	0.80	0.82	-4.61	-4.65
	<i>0.18</i>	<i>0.25</i>	<i>0.03</i>	<i>0.04</i>	<i>0.63</i>	<i>0.64</i>	<i>0.15</i>	<i>0.19</i>
i_puptch <sup>2</sup>	-0.14	-0.14	0.20	0.20	-0.03	-0.03	0.17	0.17
	<i>0.09</i>	<i>0.19</i>	<i>0.03</i>	<i>0.03</i>	<i>0.66</i>	<i>0.67</i>	<i>0.13</i>	<i>0.16</i>
i_pfri	-0.35	-0.49	0.10	0.11	-0.48	-0.49	-0.46	-0.45
	<i>0.21</i>	<i>0.12</i>	<i>0.76</i>	<i>0.72</i>	<i>0.16</i>	<i>0.17</i>	<i>0.08</i>	<i>0.12</i>
i_pfri <sup>2</sup>	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	<i>0.27</i>	<i>0.22</i>	<i>0.31</i>	<i>0.29</i>	<i>0.19</i>	<i>0.21</i>	<i>0.14</i>	<i>0.19</i>
li_member	61.98	43.77	-16.66	-22.74	-9.01	-10.57	-39.80	-42.54
	<i>0.16</i>	<i>0.38</i>	<i>0.87</i>	<i>0.80</i>	<i>0.87</i>	<i>0.84</i>	<i>0.72</i>	<i>0.68</i>
li_member <sup>2</sup>	-4.94	-3.41	0.45	0.93	0.55	0.68	2.37	2.59
	<i>0.15</i>	<i>0.39</i>	<i>0.96</i>	<i>0.90</i>	<i>0.90</i>	<i>0.87</i>	<i>0.80</i>	<i>0.76</i>
Obs.	498	498	498	498	498	498	498	498
R <sup>2</sup>	0.15	0.10	0.24	0.24	0.15	0.15	0.22	0.22
School Level Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes
F-Test: u <sub>i</sub> =0	5.43	5.22	4.81	4.78	3.93	3.92	3.17	3.16
p value	<i>0.00</i>	<i>0.00</i>	<i>0.00</i>	<i>0.00</i>	<i>0.00</i>	<i>0.00</i>	<i>0.00</i>	<i>0.00</i>
Shea's R <sup>2</sup>		0.28		0.32		0.28		0.32
First Stage F		77.69		20.99		77.58		19.62
p value		<i>0.00</i>		<i>0.00</i>		<i>0.00</i>		<i>0.00</i>
Hansen J Statistic		0.48		0.06		4.35		1.87
p value		<i>0.49</i>		<i>0.81</i>		<i>0.04</i>		<i>0.17</i>
Davidson MacKinnon Statistic		8.26		0.16		0.05		0.04
p value		<i>0.00</i>		<i>0.69</i>		<i>0.82</i>		<i>0.85</i>

<sup>1</sup>Coefficients are in plain font and p-values are italicized

<sup>2</sup>Reported standard errors are robust to heteroskedasticity and within-school serial correlation

<sup>3</sup>R<sup>2</sup> reported are net of fixed effects

<sup>4</sup>Constant and time dummies are included but not reported

<sup>5</sup>Fixed effects are for 83 schools

Table 6

	4th Grade Reading				2nd Grade Reading			
	Above Median Spending		Below Median Spending		Above Median Spending		Below Median Spending	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Fixed Effects	FE-IV	Fixed Effects	FE-IV	Fixed Effects	FE-IV	Fixed Effects	FE-IV
llsepr	-0.67	-4.34	22.93	30.05	11.91	3.22	17.23	10.36
	<i>0.87</i>	<i>0.69</i>	<i>0.12</i>	<i>0.09</i>	<i>0.02</i>	<i>0.78</i>	<i>0.23</i>	<i>0.64</i>
n_tested	-0.17	-0.18	-0.04	-0.04	-0.09	-0.09	-0.03	-0.03
	<i>0.01</i>	<i>0.01</i>	<i>0.37</i>	<i>0.38</i>	<i>0.20</i>	<i>0.23</i>	<i>0.64</i>	<i>0.62</i>
i_puptch	-0.32	-0.36	-2.95	-2.89	-4.06	-3.89	0.04	0.03
	<i>0.83</i>	<i>0.81</i>	<i>0.24</i>	<i>0.31</i>	<i>0.03</i>	<i>0.04</i>	<i>0.99</i>	<i>0.99</i>
i_puptch <sup>2</sup>	0.02	0.02	0.12	0.12	0.17	0.16	0.00	0.00
	<i>0.76</i>	<i>0.77</i>	<i>0.17</i>	<i>0.24</i>	<i>0.03</i>	<i>0.04</i>	<i>1.00</i>	<i>0.98</i>
i_pfri	-0.16	-0.14	-0.38	-0.39	-0.18	-0.17	-0.48	-0.48
	<i>0.56</i>	<i>0.60</i>	<i>0.15</i>	<i>0.13</i>	<i>0.37</i>	<i>0.40</i>	<i>0.05</i>	<i>0.03</i>
i_pfri <sup>2</sup>	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.01
	<i>0.71</i>	<i>0.75</i>	<i>0.23</i>	<i>0.22</i>	<i>0.57</i>	<i>0.55</i>	<i>0.04</i>	<i>0.02</i>
li_member	18.37	20.48	22.35	27.57	6.51	13.82	-65.73	-71.14
	<i>0.62</i>	<i>0.62</i>	<i>0.80</i>	<i>0.73</i>	<i>0.89</i>	<i>0.77</i>	<i>0.30</i>	<i>0.25</i>
li_member <sup>2</sup>	-1.27	-1.44	-1.98	-2.39	-1.00	-1.65	5.02	5.45
	<i>0.66</i>	<i>0.66</i>	<i>0.79</i>	<i>0.73</i>	<i>0.80</i>	<i>0.67</i>	<i>0.33</i>	<i>0.27</i>
Obs.	498	498	498	498	504	504	498	498
R <sup>2</sup>	0.05	0.04	0.08	0.08	0.16	0.15	0.21	0.21
School Level Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes
F-Test: u <sub>i</sub> =0	3.37	3.37	2.96	2.91	2.73	2.7	3.21	3.17
p value	<i>0.00</i>	<i>0.00</i>	<i>0.00</i>	<i>0.00</i>	<i>0.00</i>	<i>0.00</i>	<i>0.00</i>	<i>0.00</i>
Shea's R <sup>2</sup>		0.28		0.32		0.25		0.23
First Stage F		77.58		95.48		69.23		59.52
p value		<i>0.00</i>		<i>0.00</i>		<i>0.00</i>		<i>0.00</i>
Hansen J Statistic		0.54		0.17		0.04		2.22
p value		<i>0.46</i>		<i>0.68</i>		<i>0.83</i>		<i>0.14</i>
Davidson MacKinnon Statistic		0.16		0.21		0.73		0.12
p value		<i>0.69</i>		<i>0.65</i>		<i>0.39</i>		<i>0.73</i>

<sup>1</sup>Coefficients are in plain font and p-values are italicized

<sup>2</sup>Reported standard errors are robust to heteroskedasticity and within-school serial correlation

<sup>3</sup>R<sup>2</sup> reported are net of fixed effects

<sup>4</sup>Constant and time dummies are included but not reported

<sup>5</sup>Fixed effects are for 83 schools in columns 1, 2, 3, 4, 7, and 8 and 84 schools in columns 5 and 6

Table 7

	4th Grade Math				4th Grade Writing			
	Above Median Pass Rate		Below Median Pass Rate		Above Median Pass Rate		Below Median Pass Rate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Fixed Effects	FE-IV	Fixed Effects	FE-IV	Fixed Effects	FE-IV	Fixed Effects	FE-IV
llsepr	5.17 <i>0.64</i>	26.40 <i>0.11</i>	7.20 <i>0.38</i>	23.81 <i>0.12</i>	5.45 <i>0.41</i>	-14.91 <i>0.39</i>	-1.81 <i>0.85</i>	9.56 <i>0.62</i>
n_tested	-0.17 <i>0.02</i>	-0.15 <i>0.03</i>	-0.03 <i>0.58</i>	-0.03 <i>0.59</i>	-0.01 <i>0.90</i>	-0.02 <i>0.79</i>	-0.14 <i>0.02</i>	-0.13 <i>0.02</i>
i_puptch	-0.59 <i>0.75</i>	-0.17 <i>0.93</i>	0.10 <i>0.98</i>	0.05 <i>0.99</i>	2.14 <i>0.23</i>	1.54 <i>0.42</i>	-4.60 <i>0.09</i>	-4.71 <i>0.11</i>
i_puptch <sup>2</sup>	0.00 <i>0.97</i>	-0.01 <i>0.94</i>	-0.02 <i>0.90</i>	-0.02 <i>0.90</i>	-0.09 <i>0.10</i>	-0.07 <i>0.24</i>	0.20 <i>0.04</i>	0.20 <i>0.04</i>
i_pfri	0.23 <i>0.41</i>	0.12 <i>0.69</i>	-0.30 <i>0.28</i>	-0.33 <i>0.22</i>	-0.29 <i>0.20</i>	-0.19 <i>0.48</i>	-0.41 <i>0.21</i>	-0.41 <i>0.22</i>
i_pfri <sup>2</sup>	0.00 <i>0.29</i>	0.00 <i>0.47</i>	0.00 <i>0.64</i>	0.00 <i>0.59</i>	0.00 <i>0.25</i>	0.00 <i>0.47</i>	0.00 <i>0.51</i>	0.00 <i>0.54</i>
li_member	15.34 <i>0.79</i>	6.89 <i>0.90</i>	46.32 <i>0.39</i>	52.01 <i>0.46</i>	7.97 <i>0.89</i>	16.28 <i>0.79</i>	-29.21 <i>0.70</i>	-25.31 <i>0.73</i>
li_member <sup>2</sup>	-1.28 <i>0.78</i>	-0.49 <i>0.91</i>	-4.46 <i>0.33</i>	-5.07 <i>0.41</i>	-0.91 <i>0.84</i>	-1.71 <i>0.73</i>	1.32 <i>0.84</i>	0.86 <i>0.90</i>
Obs.	492	492	504	504	504	504	492	492
R <sup>2</sup>	0.11	0.10	0.28	0.27	0.17	0.15	0.30	0.29
School Level Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes
F-Test: u <sub>j</sub> =0	4.27	4.22	3.24	3.22	3.18	3.13	2.66	2.66
p value	<i>0.00</i>	<i>0.00</i>	<i>0.00</i>	<i>0.00</i>	<i>0.00</i>	<i>0.00</i>	<i>0.00</i>	<i>0.00</i>
Shea's R <sup>2</sup>		0.25		0.29		0.31		0.22
First Stage F		64.91		82.69		91.36		55.30
p value		<i>0.00</i>		<i>0.00</i>		<i>0.00</i>		<i>0.00</i>
Hansen J Statistic		0.44		0.01		1.93		0.11
p value		<i>0.51</i>		<i>0.95</i>		<i>0.16</i>		<i>0.74</i>
Davidson MacKinnon Statistic		2.19		1.71		2.54		0.46
p value		<i>0.14</i>		<i>0.19</i>		<i>0.11</i>		<i>0.50</i>

<sup>1</sup>Coefficients are in plain font and p-values are italicized

<sup>2</sup>Reported standard errors are robust to heteroskedasticity and within-school serial correlation

<sup>3</sup>R<sup>2</sup> reported are net of fixed effects

<sup>4</sup>Constant and time dummies are included but not reported

<sup>5</sup>Fixed effects are for 82 schools in columns 1, 2, 7, and 8 and 84 schools in columns 3, 4, 5, and 6

Table 8

	4th Grade Reading				2nd Grade Reading			
	Above Median Pass Rate		Below Median Pass Rate		Above Median Pass Rate		Below Median Pass Rate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Fixed Effects	FE-IV	Fixed Effects	FE-IV	Fixed Effects	FE-IV	Fixed Effects	FE-IV
llsepr	-5.38	-3.20	14.26	6.79	9.11	5.87	10.84	12.55
	<i>0.30</i>	<i>0.78</i>	<i>0.05</i>	<i>0.66</i>	<i>0.03</i>	<i>0.55</i>	<i>0.17</i>	<i>0.48</i>
n_tested	-0.05	-0.05	-0.11	-0.11	-0.08	-0.08	-0.01	-0.01
	<i>0.41</i>	<i>0.45</i>	<i>0.02</i>	<i>0.01</i>	<i>0.07</i>	<i>0.06</i>	<i>0.92</i>	<i>0.92</i>
i_puptch	-0.04	0.02	-2.27	-2.21	-2.86	-2.90	0.56	0.54
	<i>0.98</i>	<i>0.99</i>	<i>0.34</i>	<i>0.39</i>	<i>0.02</i>	<i>0.05</i>	<i>0.82</i>	<i>0.82</i>
i_puptch <sup>2</sup>	0.00	0.00	0.12	0.12	0.09	0.10	-0.01	-0.01
	<i>0.97</i>	<i>1.00</i>	<i>0.17</i>	<i>0.21</i>	<i>0.05</i>	<i>0.08</i>	<i>0.93</i>	<i>0.94</i>
i_pfrl	-0.10	-0.11	-0.12	-0.11	-0.31	-0.30	-0.16	-0.16
	<i>0.62</i>	<i>0.65</i>	<i>0.65</i>	<i>0.66</i>	<i>0.01</i>	<i>0.02</i>	<i>0.47</i>	<i>0.51</i>
i_pfrl <sup>2</sup>	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	<i>1.00</i>	<i>0.99</i>	<i>0.77</i>	<i>0.79</i>	<i>0.01</i>	<i>0.02</i>	<i>0.36</i>	<i>0.36</i>
li_member	30.07	29.52	-23.95	-23.46	-41.22	-41.75	100.70	99.84
	<i>0.36</i>	<i>0.50</i>	<i>0.67</i>	<i>0.67</i>	<i>0.27</i>	<i>0.25</i>	<i>0.07</i>	<i>0.11</i>
li_member <sup>2</sup>	-2.45	-2.40	1.41	1.41	3.15	3.19	-10.10	-10.03
	<i>0.35</i>	<i>0.49</i>	<i>0.77</i>	<i>0.76</i>	<i>0.29</i>	<i>0.28</i>	<i>0.04</i>	<i>0.07</i>
Obs.	504	504	492	492	534	534	468	468
R <sup>2</sup>	0.11	0.11	0.07	0.07	0.10	0.10	0.33	0.33
School Level Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes
F-Test: u <sub>i</sub> =0	2.65	2.65	2.41	2.4	2.79	2.77	2.34	2.33
p value	<i>0.00</i>	<i>0.00</i>	<i>0.00</i>	<i>0.00</i>	<i>0.00</i>	<i>0.00</i>	<i>0.00</i>	<i>0.00</i>
Shea's R <sup>2</sup>		0.31		0.22		0.24		0.21
First Stage F		90.75		56.69		67.23		48.47
p value		<i>0.00</i>		<i>0.00</i>		<i>0.00</i>		<i>0.00</i>
Hansen J Statistic		0.84		2.29		0.14		1.07
p value		<i>0.36</i>		<i>0.13</i>		<i>0.71</i>		<i>0.30</i>
Davidson MacKinnon Statistic		0.05		0.31		0.13		0.01
p value		<i>0.83</i>		<i>0.58</i>		<i>0.72</i>		<i>0.92</i>

<sup>1</sup>Coefficients are in plain font and p-values are italicized

<sup>2</sup>Reported standard errors are robust to heteroskedasticity and within-school serial correlation

<sup>3</sup>R<sup>2</sup> reported are net of fixed effects

<sup>4</sup>Constant and time dummies are included but not reported

<sup>5</sup>Fixed effects are for 84 schools in columns 1 and 2, 82 schools in columns 3 and 4, 89 schools in columns 5 and 6, and 78 schools in columns 7 and 8

Table 9

Fixed effects estimation for districts with a single school  
 Column headings are the dependent variable, test score pass rates

	4th math	4th write	4th read	2nd read
ltelsppr	6.53	3.97	5.41	-1.09
	<i>0.30</i>	<i>0.55</i>	<i>0.34</i>	<i>0.88</i>
n_tested	-0.16	-0.13	-0.08	-0.02
	<i>0.01</i>	<i>0.04</i>	<i>0.11</i>	<i>0.79</i>
i_puptch	0.35	-1.97	-1.13	-2.09
	<i>0.88</i>	<i>0.26</i>	<i>0.45</i>	<i>0.29</i>
i_puptch <sup>2</sup>	-0.04	0.08	0.05	0.09
	<i>0.63</i>	<i>0.18</i>	<i>0.33</i>	<i>0.26</i>
i_pfri	-0.21	-0.45	-0.25	-0.44
	<i>0.38</i>	<i>0.04</i>	<i>0.17</i>	<i>0.02</i>
i_pfri <sup>2</sup>	0.00	0.00	0.00	0.01
	<i>0.56</i>	<i>0.09</i>	<i>0.33</i>	<i>0.02</i>
li_member	56.24	-21.83	20.48	-15.04
	<i>0.19</i>	<i>0.71</i>	<i>0.60</i>	<i>0.79</i>
li_member <sup>2</sup>	-4.73	1.52	-1.60	0.59
	<i>0.16</i>	<i>0.75</i>	<i>0.61</i>	<i>0.90</i>
Obs.	828	828	828	804
R <sup>2</sup>	0.16	0.16	0.03	0.17
School Level Fixed Effects	yes	yes	yes	yes

<sup>1</sup>Coefficients are in plain font and p-values are italicized

<sup>2</sup>Reported standard errors are robust to heteroskedasticity and within-school serial correlation

<sup>3</sup>R<sup>2</sup> reported are net of fixed effects

<sup>4</sup>Constant and time dummies are included but not reported

<sup>5</sup>Fixed effects are for 138 schools in columns 1, 2, and 3 and 134 schools in column 4

Table 10

Fixed effects estimation for districts with a single school  
 Column headings are the dependent variable, test score pass rates

	4th math	4th write	4th read	2nd read
ltinsppr	3.91 <i>0.41</i>	3.48 <i>0.52</i>	2.39 <i>0.57</i>	-3.81 <i>0.32</i>
n_tested	-0.16 <i>0.01</i>	-0.13 <i>0.04</i>	-0.08 <i>0.11</i>	-0.02 <i>0.77</i>
i_puptch	0.31 <i>0.89</i>	-1.96 <i>0.27</i>	-1.18 <i>0.44</i>	-2.30 <i>0.24</i>
i_puptch <sup>2</sup>	-0.04 <i>0.63</i>	0.08 <i>0.18</i>	0.05 <i>0.32</i>	0.09 <i>0.23</i>
i_pfri	-0.21 <i>0.38</i>	-0.45 <i>0.04</i>	-0.25 <i>0.17</i>	-0.44 <i>0.02</i>
i_pfri <sup>2</sup>	0.00 <i>0.56</i>	0.00 <i>0.09</i>	0.00 <i>0.33</i>	0.01 <i>0.02</i>
li_member	52.02 <i>0.21</i>	-22.27 <i>0.70</i>	15.50 <i>0.69</i>	-19.32 <i>0.72</i>
li_member <sup>2</sup>	-4.46 <i>0.18</i>	1.53 <i>0.74</i>	-1.26 <i>0.68</i>	0.91 <i>0.84</i>
Obs.	828	828	828	804
R <sup>2</sup>	0.16	0.16	0.03	0.17
School Level Fixed Effects	yes	yes	yes	yes

<sup>1</sup>Coefficients are in plain font and p-values are italicized

<sup>2</sup>Reported standard errors are robust to heteroskedasticity and within-school serial correlation

<sup>3</sup>R<sup>2</sup> reported are net of fixed effects

<sup>4</sup>Constant and time dummies are included but not reported

<sup>5</sup>Fixed effects are for 138 schools in columns 1, 2, and 3 and 134 schools in column 4

## Appendix 1

<u>Variable</u>	<u>Description</u>
pass_math	4th grade math pass rate
pass_read	4th grade reading pass rate
pass_write	4th grade writing pass rate
pass_dra	2nd grade reading pass rate
llsepr	log (real local spending per equalized pupil)
lbpepr	log (real budgeted spending per equalized pupil)
ltinsppr	log (real total instructional spending per pupil)
ltelsppr	log (real total current spending per pupil)
n_math	number of test takers, 4th grade math
n_read	number of test takers, 4th grade reading
n_write	number of test takers, 4th grade writing
n_dra	number of test takers, 2nd grade reading
n_tested	number of test takers
puptch	pupil-teacher ratio
pfri	percent of students eligible for free or reduced price lunch
member	number of students in the school
i_puptch	imputed pupil-teacher ratio (when missing)
i_pfri	imputed percent of students eligible for free or reduced price lunch (when missing)
i_member	imputed number of students in the school (when missing)
li_member	log (imputed number of students in the school (when missing))
taxs	school property tax rate

## Appendix 2

Variable	1999			2000			2001			2002			2003			2004		
	N	Mean	SD	N	Mean	SD	N	Mean	SD	N	Mean	SD	N	Mean	SD	N	Mean	SD
Pass Rates																		
pass_math	215	47.22	15.18	215	47.63	16.05	217	47.31	16.14	216	50.64	17.23	183	52.37	15.76	181	57.69	16.86
pass_read	215	77.18	12.70	215	73.63	14.47	218	73.76	13.56	215	73.86	12.64	183	75.28	11.98	181	75.60	13.62
pass_write	215	57.96	15.27	215	53.72	16.52	218	55.76	15.79	215	58.06	16.09	183	60.97	15.40	181	67.05	15.49
pass_dra	223	71.83	14.97	220	75.49	14.51	221	77.52	14.30	217	80.03	12.40	185	82.28	11.42	184	81.87	10.76
Log of Spending																		
llsepr	240	8.86	0.15	240	8.85	0.13	240	8.89	0.14	240	8.94	0.15	240	8.97	0.16	240	9.00	0.15
lbpepr	240	9.07	0.39	240	9.08	0.39	240	9.13	0.38	240	9.19	0.38	240	9.22	0.38	240	9.26	0.38
ltinsppr	240	8.56	0.24	240	8.63	0.25	240	8.68	0.26	240	8.74	0.27	240	8.80	0.27	240	8.85	0.27
ltelsppr	240	8.99	0.21	240	9.05	0.21	240	9.11	0.21	240	9.17	0.22	240	9.23	0.22	240	9.28	0.23
Number of Test Takers																		
n_math	215	36.37	31.26	215	34.90	31.95	217	33.35	29.59	216	32.52	29.00	183	35.22	28.82	181	35.35	28.08
n_read	215	35.04	30.48	215	34.62	31.66	218	32.99	29.47	215	32.48	28.96	183	34.93	28.62	181	34.99	28.06
n_write	215	35.04	30.48	215	34.62	31.66	218	32.99	29.47	215	32.48	28.96	183	34.93	28.62	181	34.99	28.06
n_dra	223	33.64	26.66	220	32.57	25.94	221	32.18	25.87	217	31.95	25.26	185	34.51	24.81	184	33.55	23.82
School Demographics																		
puptch	237	13.60	2.61	237	12.88	2.39	238	12.28	2.33	238	11.90	2.51	238	11.76	2.37	240	11.68	2.68
pfrl	163	27.05	16.03	213	31.01	15.82	221	31.59	16.33	233	30.50	16.13	235	31.86	17.68	221	35.57	16.65
member	237	248.23	192.31	237	247.76	198.31	238	240.36	197.24	238	238.65	202.99	238	231.02	193.76	240	225.83	190.70
Imputed Demographics																		
i_puptch	240	13.59	2.599	240	12.87	2.374	240	12.29	2.328	240	11.89	2.497	240	11.75	2.359	240	11.68	2.683
i_pfrl	237	25.99	16.9	237	30.65	17.23	237	30.27	16.7	237	30.79	16.81	237	31.84	17.61	237	34.42	16.98
i_member	240	246.6	191.6	240	246.1	197.7	240	243.1	203.5	240	238.7	202.7	240	230.6	193.2	240	225.8	190.7

### Appendix 3

Variable	1999		2000		2001		2002		2003		2004	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
pass_math	45.96	14.02	46.68	14.90	47.22	15.25	50.19	16.29	52.53	15.79	57.85	16.29
llsepr	8.85	0.13	8.87	0.12	8.90	0.12	8.97	0.13	8.99	0.14	9.02	0.13
lbpepr	9.07	0.46	9.09	0.46	9.14	0.45	9.20	0.45	9.23	0.45	9.27	0.45
ltinsppr	8.54	0.23	8.62	0.25	8.67	0.26	8.74	0.26	8.78	0.26	8.83	0.27
ltelsppr	8.96	0.20	9.03	0.21	9.09	0.22	9.16	0.22	9.20	0.22	9.25	0.22
n_math	42.75	32.18	41.34	33.03	40.00	30.85	38.10	29.98	36.81	29.06	36.67	28.44
i_puptch	13.90	2.26	13.17	2.10	12.57	2.12	12.24	2.19	12.14	2.13	12.06	2.12
i_pfrl	25.95	17.57	30.22	17.31	30.27	17.00	30.57	17.33	31.97	18.24	33.84	17.49
i_member	303.97	189.81	299.53	187.49	296.56	196.36	290.85	197.29	285.62	195.12	280.90	192.10
taxs	1.41	0.23	1.45	0.21	1.54	0.24	1.58	0.25	1.62	0.28	1.59	0.25

n=166 in each year

Variable	1999		2000		2001		2002		2003		2004	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
pass_read	76.63	11.73	73.34	13.51	73.59	12.90	73.98	11.77	75.42	11.69	76.16	12.96
pass_write	57.23	14.82	52.91	16.00	55.47	15.36	58.27	15.83	61.09	15.20	67.70	14.74
llsepr	8.85	0.13	8.87	0.12	8.91	0.12	8.97	0.13	8.99	0.14	9.02	0.13
lbpepr	9.07	0.46	9.09	0.46	9.14	0.45	9.20	0.45	9.23	0.45	9.27	0.45
ltinsppr	8.54	0.23	8.62	0.25	8.68	0.26	8.73	0.26	8.78	0.26	8.83	0.27
ltelsppr	8.96	0.20	9.03	0.21	9.09	0.22	9.15	0.22	9.20	0.22	9.25	0.22
n_read	41.11	31.52	41.00	32.74	39.67	30.80	37.87	29.92	36.54	28.82	36.28	28.45
n_write	41.11	31.52	41.00	32.74	39.67	30.80	37.87	29.92	36.54	28.82	36.28	28.45
i_puptch	13.86	2.27	13.15	2.12	12.55	2.13	12.20	2.19	12.10	2.12	12.03	2.11
i_pfrl	25.91	17.55	30.15	17.26	30.22	16.96	30.49	17.28	31.84	18.17	33.68	17.47
i_member	303.47	190.23	299.12	187.83	296.25	196.61	290.53	197.53	285.27	195.39	280.52	192.39
taxs	1.41	0.23	1.45	0.21	1.55	0.24	1.58	0.25	1.62	0.28	1.60	0.25

n=166 in each year

Variable	1999		2000		2001		2002		2003		2004	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
pass_dra	71.86	12.94	75.63	12.09	78.16	11.79	80.31	11.33	81.92	11.16	82.40	10.31
llsepr	8.86	0.14	8.87	0.11	8.91	0.12	8.97	0.13	8.99	0.13	9.02	0.14
lbpepr	9.08	0.46	9.09	0.45	9.15	0.45	9.20	0.45	9.23	0.45	9.27	0.45
ltinsppr	8.56	0.22	8.63	0.24	8.69	0.25	8.75	0.26	8.79	0.26	8.84	0.27
ltelsppr	8.97	0.20	9.05	0.21	9.11	0.22	9.16	0.22	9.21	0.22	9.26	0.22
n_dra	40.79	27.13	39.25	26.35	38.98	26.29	38.01	25.75	36.76	25.08	35.54	24.10
i_puptch	13.82	2.14	13.20	2.08	12.59	2.13	12.31	2.17	12.19	2.17	12.08	2.19
i_pfrl	24.75	16.93	28.80	16.77	28.75	16.68	29.10	16.72	30.89	17.99	32.93	17.56
i_member	300.24	191.42	299.86	198.71	297.13	207.04	292.24	207.70	281.55	196.78	276.65	194.25
taxs	1.42	0.22	1.46	0.21	1.55	0.24	1.58	0.25	1.63	0.28	1.61	0.26

n=167 in each year

## Appendix 4

Dependent Variable: 4th Grade Math Pass Rate

	Pooled OLS	IV	Fixed Effects	FE-IV		Pooled OLS	IV	Fixed Effects	FE-IV
llsepr	7.92	8.55	9.86	25.74	log(average per pupil exp.)	8.44	22.16	7.18	37.31
	<i>3.82</i>	<i>5.41</i>	<i>7.24</i>	<i>11.28</i>		<i>1.72</i>	<i>5.48</i>	<i>2.93</i>	<i>15.64</i>
n_tested	0.02	0.02	-0.11	-0.11					
	<i>0.02</i>	<i>0.02</i>	<i>0.05</i>	<i>0.04</i>					
i_puptch	-3.76	-3.78	0.14	0.31					
	<i>1.92</i>	<i>1.93</i>	<i>2.18</i>	<i>2.42</i>					
i_puptch <sup>2</sup>	0.13	0.13	-0.02	-0.03					
	<i>0.07</i>	<i>0.07</i>	<i>0.08</i>	<i>0.09</i>					
i_pfrl	-0.50	-0.50	-0.14	-0.18	lunch	-0.58	-0.60	-0.15	-0.09
	<i>0.08</i>	<i>0.08</i>	<i>0.21</i>	<i>0.20</i>		<i>0.04</i>	<i>0.05</i>	<i>0.11</i>	<i>0.13</i>
i_pfrl <sup>2</sup>	0.00	0.00	0.00	0.00	lunch <sup>2</sup>	0.00	0.00	0.00	0.00
	<i>0.00</i>	<i>0.00</i>	<i>0.00</i>	<i>0.00</i>		<i>0.00</i>	<i>0.00</i>	<i>0.00</i>	<i>0.00</i>
li_member	12.16	12.13	35.42	31.09	log(enroll)	-4.99	-16.74	-14.05	15.50
	<i>11.03</i>	<i>11.03</i>	<i>38.98</i>	<i>44.09</i>		<i>8.05</i>	<i>10.97</i>	<i>20.33</i>	<i>47.09</i>
li_member <sup>2</sup>	-1.18	-1.17	-3.34	-2.95	log(enroll) <sup>2</sup>	0.29	1.37	1.00	-0.83
	<i>0.96</i>	<i>0.97</i>	<i>3.15</i>	<i>3.58</i>		<i>0.71</i>	<i>0.97</i>	<i>1.75</i>	<i>3.81</i>
					log(per pupil exp. 1994)		-13.51		
							<i>4.87</i>		
					y96*(log per pupil exp. 1994)		10.76		13.82
							<i>2.91</i>		<i>4.02</i>
					y97*(log per pupil exp. 1994)		11.69		14.57
							<i>3.02</i>		<i>4.66</i>
					y98*(log per pupil exp. 1994)		6.33		9.46
							<i>3.51</i>		<i>5.24</i>
Obs.	996	996	996	996	Obs.	7242	4853	7242	4853
R <sup>2</sup>	0.22	0.22	0.16	0.16	R <sup>2</sup>	0.40	0.36	0.36	0.22
School Level Fixed Effects			yes	yes	School Level Fixed Effects			yes	yes

<sup>1</sup>Coefficient estimates are in plain font and standard errors are italicized

<sup>2</sup>Reported standard errors are robust to heteroskedasticity and within-school serial correlation

<sup>3</sup>For Papke (2005) *lunch* is the percent of students eligible for free or reduced price lunch and *enroll* is school enrollment

## Appendix 5

Dependent Variable: 4th Grade Math Pass Rate

	Pooled OLS	IV	Fixed Effects	FE-IV
lbpepr	-0.47 <i>0.53</i>	20.74 <i>0.18</i>	21.01 <i>0.02</i>	63.13 <i>0.06</i>
n_tested	0.03 <i>0.16</i>	0.03 <i>0.18</i>	-0.12 <i>0.01</i>	-0.12 <i>0.01</i>
i_puptch	-3.68 <i>0.05</i>	0.04 <i>0.99</i>	0.51 <i>0.82</i>	1.46 <i>0.56</i>
i_puptch <sup>2</sup>	0.12 <i>0.08</i>	0.01 <i>0.90</i>	-0.04 <i>0.66</i>	-0.07 <i>0.44</i>
i_pfrl	-0.53 <i>0.00</i>	-0.55 <i>0.00</i>	-0.13 <i>0.51</i>	-0.19 <i>0.35</i>
i_pfrl <sup>2</sup>	0.00 <i>0.01</i>	0.00 <i>0.01</i>	0.00 <i>0.89</i>	0.00 <i>0.79</i>
li_member	12.97 <i>0.25</i>	-2.65 <i>0.88</i>	39.92 <i>0.31</i>	43.53 <i>0.32</i>
li_member <sup>2</sup>	-1.29 <i>0.19</i>	0.05 <i>0.97</i>	-3.68 <i>0.25</i>	-3.88 <i>0.28</i>
Obs.	996	996	996	996
R <sup>2</sup>	0.22	-0.12	0.17	0.14
School Level Fixed Effects			yes	yes
F-Test: u <sub>i</sub> =0			5.24	5.06
p value			0.00	0.00
Shea's R <sup>2</sup>		0.01		0.07
First Stage F		2.61		29.83
p value		<i>0.07</i>		<i>0.00</i>
Hansen J Statistic		4.77		2.19
p value		<i>0.03</i>		<i>0.14</i>
Durban Wu Hausman Statistic		2.33		
p value		<i>0.13</i>		
Davidson MacKinnon Statistic				1.93
p value				<i>0.16</i>

<sup>1</sup>Coefficients are in plain font and p-values are italicized

<sup>2</sup>Reported standard errors are robust to heteroskedasticity and within-school serial correlation

<sup>3</sup>R<sup>2</sup> reported are net of fixed effects

<sup>4</sup>Constant and time dummies are included but not reported

<sup>5</sup>Fixed effects are for 166 schools

## Appendix 5 Cont.

Dependent Variable: 4th Grade Writing Pass Rate

	Pooled OLS	IV	Fixed Effects	FE-IV
lbpepr	-1.78 <i>0.00</i>	-30.42 <i>0.06</i>	5.99 <i>0.44</i>	-19.40 <i>0.62</i>
n_tested	0.03 <i>0.05</i>	0.03 <i>0.09</i>	-0.11 <i>0.02</i>	-0.11 <i>0.01</i>
i_puptch	-3.70 <i>0.01</i>	-8.70 <i>0.03</i>	-0.84 <i>0.59</i>	-1.41 <i>0.44</i>
i_puptch <sup>2</sup>	0.13 <i>0.01</i>	0.28 <i>0.03</i>	0.03 <i>0.51</i>	0.06 <i>0.38</i>
i_pfri	-0.59 <i>0.00</i>	-0.56 <i>0.00</i>	-0.45 <i>0.02</i>	-0.42 <i>0.05</i>
i_pfri <sup>2</sup>	0.00 <i>0.00</i>	0.00 <i>0.05</i>	0.00 <i>0.05</i>	0.00 <i>0.08</i>
li_member	8.59 <i>0.43</i>	29.84 <i>0.12</i>	-18.54 <i>0.70</i>	-20.30 <i>0.66</i>
li_member <sup>2</sup>	-0.97 <i>0.30</i>	-2.80 <i>0.09</i>	1.00 <i>0.80</i>	1.09 <i>0.77</i>
Obs.	996.00	996.00	996.00	996.00
R <sup>2</sup>	0.22	-0.4	0.18	0.17
School Level Fixed Effects			yes	yes
F-Test: u <sub>i</sub> =0			3.78	3.74
p value			<i>0.00</i>	<i>0.00</i>
Shea's R <sup>2</sup>		0.01		0.07
First Stage F		2.57		28.68
p value		<i>0.08</i>		<i>0.00</i>
Hansen J Statistic		1.92		1.21
p value		<i>0.17</i>		<i>0.27</i>
Durban Wu Hausman Statistic		4.18		
p value		<i>0.04</i>		
Davidson MacKinnon Statistic				0.57
p value				<i>0.45</i>

<sup>1</sup>Coefficients are in plain font and p-values are italicized

<sup>2</sup>Reported standard errors are robust to heteroskedasticity and within-school serial correlation

<sup>3</sup>R<sup>2</sup> reported are net of fixed effects

<sup>4</sup>Constant and time dummies are included but not reported

<sup>5</sup>Fixed effects are for 166 schools

## Appendix 5 Cont.

Dependent Variable: 4th Grade Reading Pass Rate

	Pooled OLS	IV	Fixed Effects	FE-IV
lbpepr	-1.27 <i>0.02</i>	3.21 <i>0.78</i>	13.66 <i>0.10</i>	8.29 <i>0.76</i>
n_tested	0.01 <i>0.48</i>	0.01 <i>0.48</i>	-0.09 <i>0.01</i>	-0.09 <i>0.01</i>
i_puptch	-3.36 <i>0.01</i>	-2.57 <i>0.27</i>	-0.87 <i>0.50</i>	-1.00 <i>0.51</i>
i_puptch <sup>2</sup>	0.12 <i>0.00</i>	0.10 <i>0.17</i>	0.04 <i>0.31</i>	0.05 <i>0.36</i>
i_pfrl	-0.48 <i>0.00</i>	-0.49 <i>0.00</i>	-0.28 <i>0.11</i>	-0.28 <i>0.12</i>
i_pfrl <sup>2</sup>	0.00 <i>0.01</i>	0.00 <i>0.01</i>	0.00 <i>0.21</i>	0.00 <i>0.22</i>
li_member	3.76 <i>0.68</i>	0.44 <i>0.97</i>	12.60 <i>0.69</i>	12.22 <i>0.73</i>
li_member <sup>2</sup>	-0.50 <i>0.54</i>	-0.21 <i>0.85</i>	-1.09 <i>0.68</i>	-1.08 <i>0.71</i>
Obs.	996	996	996	996
R <sup>2</sup>	0.24	0.22	0.04	0.04
School Level Fixed Effects			yes	yes
F-Test: u <sub>i</sub> =0			3.35	3.32
p value			<i>0.00</i>	<i>0.00</i>
Shea's R <sup>2</sup>		0.01		0.07
First Stage F		2.57		28.68
p value		<i>0.08</i>		<i>0.00</i>
Hansen J Statistic		0.36		0.00
p value		<i>0.55</i>		<i>0.98</i>
Durban Wu Hausman Statistic		0.17		
p value		<i>0.68</i>		
Davidson MacKinnon Statistic				0.04
p value				<i>0.84</i>

<sup>1</sup>Coefficients are in plain font and p-values are italicized

<sup>2</sup>Reported standard errors are robust to heteroskedasticity and within-school serial correlation

<sup>3</sup>R<sup>2</sup> reported are net of fixed effects

<sup>4</sup>Constant and time dummies are included but not reported

<sup>5</sup>Fixed effects are for 166 schools

## Appendix 5 Cont.

Dependent Variable: 2nd Grade Reading Pass Rate

	Pooled OLS	IV	Fixed Effects	FE-IV
lbpepr	0.01 <i>0.99</i>	4.20 <i>0.78</i>	21.42 <i>0.01</i>	18.15 <i>0.53</i>
n_tested	-0.03 <i>0.11</i>	-0.03 <i>0.20</i>	-0.05 <i>0.29</i>	-0.05 <i>0.26</i>
i_puptch	-2.06 <i>0.06</i>	-1.41 <i>0.57</i>	-1.69 <i>0.24</i>	-1.74 <i>0.24</i>
i_puptch <sup>2</sup>	0.07 <i>0.09</i>	0.05 <i>0.50</i>	0.06 <i>0.25</i>	0.07 <i>0.23</i>
i_pfrl	-0.29 <i>0.00</i>	-0.29 <i>0.00</i>	-0.34 <i>0.02</i>	-0.34 <i>0.01</i>
i_pfrl <sup>2</sup>	0.00 <i>0.34</i>	0.00 <i>0.36</i>	0.00 <i>0.02</i>	0.00 <i>0.01</i>
li_member	7.34 <i>0.42</i>	4.84 <i>0.70</i>	0.91 <i>0.98</i>	0.50 <i>0.99</i>
li_member <sup>2</sup>	-0.46 <i>0.56</i>	-0.25 <i>0.81</i>	-0.42 <i>0.89</i>	-0.40 <i>0.89</i>
Obs.	1002	1002	1002	1002
R <sup>2</sup>	0.20	0.18	0.18	0.18
School Level Fixed Effects			yes	yes
F-Test: u <sub>i</sub> =0			3.14	3.08
p value			<i>0.00</i>	<i>0.00</i>
Shea's R <sup>2</sup>		0.00		0.06
First Stage F		1.59		25.50
p value		<i>0.20</i>		<i>0.00</i>
Hansen J Statistic		0.23		1.02
p value		<i>0.63</i>		<i>0.31</i>
Durban Wu Hausman Statistic		0.09		
p value		<i>0.76</i>		
Davidson MacKinnon Statistic				0.01
p value				<i>0.91</i>

<sup>1</sup>Coefficients are in plain font and p-values are italicized

<sup>2</sup>Reported standard errors are robust to heteroskedasticity and within-school serial correlation

<sup>3</sup>R<sup>2</sup> reported are net of fixed effects

<sup>4</sup>Constant and time dummies are included but not reported

<sup>5</sup>Fixed effects are for 167 schools