Educational Resources and Student Achievement:

Evidence from the Save Harmless Provision in New York State

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Abstract: A long-standing debate in the economics of education literature is whether increasing educational resources moves the needle on student achievement. Education finance reformers advocate delivering extra resources to disadvantaged school districts to close academic achievement gaps, but their efforts are subject to criticism from skeptics who believe that extra resources do not actually improve performance. This study leverages variation in per-pupil expenditures from a specific provision of the state aid formula in New York State that allows districts to maintain prior levels of total state aid even as their student enrollment declines. We uncover achievement gains of approximately .047 standard deviations in math and .042 standard deviations in English corresponding to $1,000 in additional per-pupil spending. This study strengthens the case that school resources matter, and that sustained financial investments can help districts maintain and improve quality of public education. **JEL Codes** I22, C36, H75.

1 Introduction

Disparities between disadvantaged students and their wealthy counterparts are a regular empirical finding in the education literature. Racial minorities have on average lower scores on standardized achievement tests and lower graduation rates (Fryer and Levitt 2006; Hanushek and Rivkin 2006; Heckman and LaFontaine 2010). Research suggests that much of this gap reflects underlying socioeconomic differences (Clotfelter, Ladd and Vigdor 2009; Fryer and Levitt 2004). Today, individuals in the lowest income decile have four years less educational attainment than individuals in the highest income decile, at a time when education has become even more essential to financial stability. Between 1997 and 2007, wages grew by 25% for college graduates, while they stagnated for high school graduates and declined by 13% for high school dropouts (Duncan and Murnane 2011). If education and income are causally linked, achievement gaps will lead to widening socioeconomic disparities and income inequality.

While consensus exists on the presence and consequences of academic achievement gaps, solutions remain more controversial. Many reform efforts have focused on delivering extra funding to low-performing schools. The first series of U.S. education finance reforms focused on equalizing educational expenditures between districts, and the second series then attempted to deliver supplementary resources to low-performing districts to account for high need student populations. But while states nationwide have been largely successful in these instrumental goals, disparities in performance persist (Lafortune, Rothstein and Schanzenbach 2016; Yinger 2004). Hanushek (1994) documents 3.5% real annual increases in per-pupil expenditures (PPE) between 1970 and 1990, and Bifulco (2005) documents that, since 1987, PPE in the average black student’s district have outpaced those in the average white student’s district by approximately $400. Despite these massive investments, there have been few obvious improvements in disadvantaged public school districts, and a complicated empirical history has left researchers unconfident in claiming a definitive causal link between educational expenditures and student performance.

Dating back to early attempts in the 1960's (Coleman, 1966) scholars struggled to link educational resources to a definitive positive impact on student achievement. In the late 1990's, two teams conducted a famous pair of metanalyses on the topic, with Hanushek (1997) claiming no relationship and Greenwald, Hedges and D. (1996) claiming a positive relationship. As this history pre-dated the widespread adoption of quasi-experimental methods in economics research, it is now clear that the early body of research failed to identify the nature of this relationship. Jackson, Johnson and Persico (2016) suggest that direct estimates of the effect of educational resources on student performance are likely to be biased downwards, since educational policymakers often invest additional resources to low performing schools. This phenomenon creates a simultaneity issue that cannot be directly controlled for and which must be addressed through experimental or quasi-experimental methods.

More recent research, focusing mostly on the impacts of state level school finance reforms (SFRs) has found evidence that is suggestive of a positive relationship between educational resources and student achievement which develops over time (Lafortune et. al., 2016; Card & Payne, 2002; Guryan, 2001). An influential study on this subject by Jackson et al (2016), measures the effect of per pupil educational expenditures on long term student outcomes and finds a positive effect that is stronger for disadvantaged students. Papke (2005) estimates the effect of per-pupil expenditures on immediate academic achievement estimate, using exogenous variation in state educational aid resulting from Michigan's school finance reform to identify positive impacts on student outcomes. She finds that a 10% increase in current year per-pupil expenditures is associated with a 2 percentage point increase in the pass rate on year end examinations. More recently, Lafortune et al. (2016) explore the impacts of statewide school finance reforms on immediate academic outcomes in a national sample and estimate treatment effects of .01 to .024 standard deviations per $1000 of per-pupil expenditures, one of the most clear and intuitive estimates currently available.

While scholars are now beginning to accept a positive relationship between spending and student achievement, it is important to explore how this relationship holds up in different contexts. The literature is mostly comprised of SFR studies, which examine the effects of delivering large exogenous increases of funding to low-performing school districts. However, there are many other circumstances in school finance systems that can lead to variation in educational spending. For instance, a recent working paper by Jackson, Wigger and Xiong (2018) explores the effect of budgetary cuts on student achievement, an effect that may plausibly differ from the effects of a budgetary increase. Once again, they find evidence of a positive relationship between spending and achievement.

This paper contributes to the literature by employing quasi-experimental methods to investigate variation in educational spending resulting from another unique budgetary circumstance. During the 2007-08 school year, NYS reformed its education finance system and implemented a need-based foundation aid formula that included a number of idiosyncratic rules and policies. One of these provisions was the continuation of a policy called “Save Harmless,” which stipulated that districts could not lose money if their estimated need declined. The largest impact of this provision was that districts did not lose funding when their enrollment decreased, leading districts with declining enrollment to have systematically higher per-pupil expenditures. While this policy was in place, New York experienced the highest levels of population loss in the country, causing significant changes in enrollment and rapidly compounding increases in resources available per pupil. The confluence of these factors offers an opportunity to explore a new budgetary dynamic that has not yet been examined: What happens when schools maintain constant levels of funding, but distribute them to student populations that are rapidly fluctuating in size?

By controlling for demographic changes associated with these enrollment losses, we can isolate plausibly exogenous variation in school resources associated with enrollment change. Leveraging this variation through instrumental variable estimation, we find positive effects of expenditures on elementary and middle school test scores. Our estimates are comparable in magnitude to those of Lafortune et al (2016). We probe the validity of these inferences, specifically the validity of our exclusion restriction assumptions, through a series of robustness checks assessing key threats to validity and emerge confident that we have identified a true spending effect. We conclude that the Save Harmless policy treatment provides reasonably exogenous variation in school resources, conditional on district and year fixed effects, district enrollment and district level demographic composition.

This study is the first to our knowledge to assess the impact of a Save Harmless provision on student achievement. In addition, it is the first to estimate the impacts of educational resources in New York State following their 2007 school finance reform, and the most recent estimate of the impact of current year educational resources on immediate academic outcomes. Since the New York reform has been noteworthy to scholars for both the magnitude of its investments, and the political controversies surrounding it, identification of the effects of these investments can inform arguments about optimal levels of spending and possible adjustments to state aid formulas.

2.1 Background on School Finance Reforms

Traditionally districts serving poor students have weaker property tax bases and therefore less revenue available per pupil. These funding gaps between socioeconomically disadvantaged and wealthy public school districts have acted as a common target for educational reforms. Such efforts led to state-level school finance reforms (SFR's) beginning in that 1970's, which sought to equalize spending across districts, and adequacy-based SFR's beginning in the 1980's and 1990's which delivered extra resources to low-performing districts. Hanushek (1994) charts the trajectory of the early equity-based finance reforms, documenting 3.5% annual increases in expenditures between 1970 and 1990. Lafortune, Rothstein and Schanzenbach (2016) analyze later adequacy-based reforms and document a 40% increase in spending between 1990 and 2012, which was concentrated in low-performing districts.

An extensive literature now examines the impacts of SFR's not only on funding levels but also on student achievement. Card and Payne (2002) analyze a national sample of pre-1992 data and suggest that SFR's led to reduction in achievement gaps between rich and poor students. Guryan (2001) found mixed evidence that SFR's improved test scores in Massachusetts. SFR's in California, Kansas, Kentucky, and Michigan were also examined, with mixed results (Downes, 1992; Deke, 2003; Clark, 2003; Roy, 2011). The heterogeneity of results across different contexts led most to consider the effect of SFR's an open question until recently. Two national quasi-experimental analyses have recently changed this perception, leading to a broader consensus that SFR's improve student performance. Lafortune, Rothstein and Schanzenbach (2016) analyze post-1990 reforms and found effects on student achievement that develop incrementally over time. Jackson, Johnson, and Persico (2016), summarized in more detail below, looked broadly at historical school finance reforms and long-term student outcomes using an event study design. However, now that these precedents have been established, new research is needed to demonstrate that these findings hold up in different institutional contexts.

A parallel line of inquiry attempts to parameterize the relationship between per-pupil educational spending and student achievement. An extensive associational literature has attempted to document this relationship; however, it has been subject to persistent controversy as researchers disagree over whether the evidence conclusively demonstrates that this connection exists. In the late 1990's, several independent research teams analyzed the existing literature, and arrived at different and contradictory conclusions about the relationship between school spending and school performance (Hanushek 1997; Greenwald, Hedges and Laine, 1996; and Verstegen and King 1998). High quality experimental and quasi-experimental research has shown that reducing class size increases student achievement (Angrist and Lavy 1999; Krueger 1999), but some have shown null effects (Hoxby 2000). Others have warned that the benefits of class size reductions when implemented at scale may be tempered by general equilibrium effects on teacher quality (Jepsen and Rivkin 2002).

Many of the earliest studies on school resources employed education production function designs with endogenous operationalization of school resources. Because education policy-makers commonly deliver extra resources to low-performing schools or cohorts of students with higher need, direct estimates of this relationship, even with district fixed effects, will likely be biased downwards. More sophisticated contemporary research has used quasi-experimental methods to estimate effects of educational expenditures on student outcomes. Using state aid reforms as an instrument for educational expenditures, Jackson, Johnson and Persico (2016) identify causal relationships between per-pupil expenditures (PPE) and completed schooling, wages and reduced adult poverty in a national sample. Furthermore, they find that PPE have a larger effect on performance in socioeconomically disadvantaged student populations. Michigan's school finance reform has proved very useful for estimating these effects, with Papke (2005; 2008), Chaudhary (2009) and Roy (2011) using state aid grants as an instrument for resources and Hyman (2017) leveraging this variation to examine long term outcomes. Guryan, (2001) leverages a school finance reform in Massachusetts to offer 2SLS estimates of the effect of per pupil dollars finding positive effects on 4^{th} grade test scores. Outside of the US context, other studies such as Leuven et al. (2007) and Haegeland, Raaum, and Salvanes (2012) have examined these effects in an international context, with mixed results.

This study will contribute to the literature by exploring the relationship between educational spending and student achievement in a previously unexamined institutional context. Specifically, by leveraging variation in funding from the confluence of NYS population trends and the "Save Harmless" policy in the NYS state aid formula, we will explore what happens to educational outcomes when spending is held roughly constant, but distributed to a rapidly contracting student population. Based on previous evidence of a positive relationship between spending and achievement, we expect that districts who experience large population losses, but are able to maintain their revenue base, will experience increases in student outcomes. This natural experiment differs from previous quasi-experimental approaches, which studied the impact of large scale investments resulting from school finance reforms. Unlike those studies, which estimated the effect of increasing resources, this study explores the effect of holding resources constant and distributing them to a smaller group of students. This research will contribute to ongoing policy debates surrounding educational finance reform in New York State, and can generalize to nationwide debates over how education costs and revenues matter in the context of student enrollment change.

2.2 New York State Aid Reform and the "Save Harmless" Provision

Over the past quarter century, New York State has been a hotbed of school finance reform. In the mid 1990's, despite average property tax rates that were among the highest in the nation, less-privileged schools demonstrated persistently substandard performance. According to reformers, state aid programs that had focused on equalizing spending between districts failed to consider substantial cost differences between districts. An extended debate over optimal solutions led to several proposals for new state aid funding strategies. Leading this effort, Duncombe and Yinger (2000) argued for a performance-based formula that would account for student characteristics such as poverty and limited English proficiency, as well as for regional cost disparities. Such a formula would attempt to go beyond equalizing spending and invest more resources in disadvantaged school districts to equalize performance.

Educational reform in NYS was not only an academic exercise but was the target of intense political advocacy. In 1993, an advocacy group known as the Campaign for Fiscal Equity launched a protracted legal campaign to deliver financial remediation to underperforming schools. Their advocacy led to years of litigation, culminating in a landmark ruling in the case of Campaign for Fiscal Equity vs. the State of New York (2003). The court declared that the state of New York had violated the constitutional right of students to a sound basic education and directed the state to implement educational finance reforms to close performance gaps between school districts. This resulted in the Education Budget and Reform Act of 2007 (EBRA) which introduced a performance-based foundation aid formula that adjusted for pupil needs, including enrollment, poverty and limited English proficiency, along with regional cost differences (Abbott, Hodgens and Wenzel 2013).

One noteworthy provision of the new foundation aid formula was that it maintained an archaic provision of NYS education finance called the "Save Harmless" provision. Under this clause, school districts that experienced declining enrollment would not experience cuts to their state aid allotment. Save Harmless was implemented in 1976, to prevent schools with declining enrollment from shutting down (Levine 1976). The policy was immediately met with criticism, as many worried that it privileged certain school districts at the expense of others. As early as the 1980's, this provision was a political football being debated in relation to equity issues. For example, a 1983 New York Times article described debate over whether wealthy school districts should receive Save Harmless guarantees (Chira 1983). When the EBRA was implemented at the start of the 2007-08 academic year it maintained the Save Harmless provision, guaranteeing that districts with declining enrollment or pupil need would not only receive equal funding to the year prior, but would also receive a 3% adjustment for inflation. This provision drew the ire of some, who claimed that the provision delivered millions of dollars in aid to students who didn't exist, when those resources could have been distributed to high-need districts in pursuit of equity gains (Cunningham 2014).

The Save Harmless provision offers the potential for a natural experiment, and we harness the variation in state educational aid to school districts arising from the policy. Under this provision, districts that experience declining enrollment receive artificially inflated levels of per-pupil educational aid. Employing a conditional exogeneity argument, we demonstrate that, conditional on district and year fixed effects, current student enrollment and demographic controls, within-district year-to-year enrollment change and the corresponding impact of the Save Harmless provision generate plausibly exogenous variation in educational resources. We defend this argument through a rigorous set of robustness checks evaluating the key threats to validity.

2.3 Save Harmless and NYS Demographic Trends

The execution of the Save Harmless policy is especially salient in NYS, due to demographic trends occurring during the period of our study. At the turn of the new millennium, NYS was experiencing steady growth, but analysts noted declining population in the "Rust Belt" cities of Upstate NY and upstate counties in general (Wing 2003). Over the next ten years growth declined, with NYS's percentage growth rate ranking 46th in the nation and growth concentrated in the downstate region with 17 upstate counties losing population. (NYS Department of Labor 2011). Between 2010 and 2015, the last five years of our study, this trend multiplied, with 41 out of 50 upstate counties losing population. While downstate growth was slow at a paltry .33%, upstate NY lost 65,638 people for a growth rate of -1.04%, producing a statewide growth rate of -.12% (Empire Center 2016). In 2016, Forbes ranked NYS number 1 for losing the most net migrants nationwide, with 126,000 people leaving the state (Kotkin 2016).

The declining statewide population carried over to declining enrollment in NYS school districts. Over the period in our sample, the mean percentage change in enrollment from the 2006-07 academic year to the current year was -5.61% with 81% of observations in our sample showing declining enrollment. The mean percentage change in observations with declining enrollment was -8.17%. Figure I first shows graphically how the rate of enrollment change between 2007 and 2015 varies across school districts in the state. Out of 652 districts in our analysis sample, only 42 experienced zero years of declining enrollment during the period of our study. For the purpose of our analysis we therefore remove these districts from our sample. The second map in Figure I indicates how, despite consistent enrollment declines in the upstate region, total inflation-adjusted expenditures still increased over time. These two patterns in juxtaposition, create the growth in per-pupil expenditures we investigate in this study.

The graphics in Figure II further demonstrate that enrollment declines were a persistent trend in NYS school districts and led to marked increases in per-pupil expenditures (PPE). All graphics absorb district fixed effects. The plot of enrollment over time shows that average district enrollment declined year after year in districts across NYS, starting at a peak of 2,600 in the 2007-08 academic year and falling to 2,400 in 2014-15. The plot of the relationship between PPE and percent enrollment decrease demonstrates the impact of the Save Harmless policy. As enrollment declines, district PPE increases markedly, with the graphic showing a steep trend. This illustrates the logic behind our natural experiment; districts that experienced declining enrollment had clear increases in PPE, and enrollment declines seem consistent across district types, which under certain conditions could create plausibly exogenous variation in PPE.

The biggest potential threat to this natural experiment is that district demographic composition changed as enrollment declined in a way which would influence student performance. This could occur if students from certain demographic groups were more likely to move out of a district. We explore these relationships and find that while two out of five demographic characteristics (percentage of students with Limited English Proficiency and percentage of students with disabilities) have minimum association with our measure of enrollment change, three demographic variables (percentage free lunch eligibility, percentage black students and % Hispanic students) are significantly correlated (See Appendix Table 3). Our instrument has a moderate positive relationship with percentage free lunch eligibility that is significant at the .10 significance level and a highly significant positive relationship with percentage black students that is very small in magnitude. This indicates that the proportion of poor and black students in the district increases as enrollment declines. These findings are not too troubling, because they are either statistically weak or very small, and if we expect enrollment decline to have a positive effect on achievement, its positive correlation with poor and black students should work against our hypothesis, biasing results downward. More concerning is the finding that our instrument has a significant negative correlation with percentage Hispanic enrollment, which is relatively large in magnitude. This indicates that Hispanic student movement into and out of districts may be related to overall within-district enrollment change patterns, a condition that could bias our results upward. While controlling for these student demographic variables eliminates any direct threat to the validity of our estimates, the relationships with these demographic controls suggest potential lingering concerns about selection on unobservables. In robustness checks we probe the effect of various demographic controls on our estimates.

3. Data

The analysis in this paper is based on publicly available data from the New York State Education Department (NYSED). Financial variables are drawn from the Fiscal Analysis and Research Unit's (FARU) Fiscal Profile Reporting System (FPRS). Test scores, demographics and other control variables are drawn from NYSED School Report Card data, which contain both school- and district-level data. Merging these data sources together results in a complete set of 678 district matches accounting for all districts in New York State included in the FARU data. Since some of the districts only serve elementary or high school students, but not both, we further restrict our sample to only K-12 school districts, resulting in a sample of 652 districts. The sample excludes the New York City Public School District, because it differs meaningfully from the rest of the state on size, population, and organizational and financial structure. (The data on NYC schools was furthermore incompatible for matching to the fiscal database.) Data spans an eight-year period from the 2007-08 to the 2014-15 academic years.

The key explanatory variable of interest in this study is *Save Harmless*, a continuous measure of the inverse percentage change in enrollment in each district from the 2006-07 academic year. This serves as a proxy variable for the effect of the Save Harmless provision since districts experiencing larger enrollment declines would receive larger benefits in terms of available per-pupil foundation aid. It is calculated by first subtracting enrollment in the current period from base year enrollment, and dividing by enrollment in the base year. If there is a decline in enrollment from the base period, this variable will have a positive value. To aid interpretation we multiply this ratio by 100. Figure III demonstrates that enrollment declines compounded year over year in the period of our study. Thus, measuring enrollment decline from a base year ensures that we capture the cumulative nature of the enrollment trends, giving us greater ability to detect spending effects.

Because this variable is dependent on student enrollment, and enrollment can be measured in different ways, selection of an appropriate measure is essential to successful identification in our models. We choose a measure from the FARU data set called Duplicated Combined Adjusted Average Daily Membership (DCAADM). This measure includes not only students enrolled in district classes, but all students for which the district has financial obligations, including charter school students, students in alternative special education placements, incarcerated students, and students for which the district pays tuition to another district, such as in urban-suburban programs. It is important to include all students for which the district has a financial obligation when working with per-pupil financial measures, because failing to include these could misspecify the fiscal impact of declining enrollment, biasing resulting estimates. NYS considers DCAADM the best enrollment measure to use when working with financial variables, and calculates its official per pupil financial statistics using this measure

The dependent and endogenous variables in this study include both institutional and student outcomes. The institutional variables include a measure of expenditures per pupil (total expenditures divided by DCAADM), and a measure of the student-teacher ratio which is BEDS enrollment (a measure of enrollment in district classes) divided by total teachers. We also test state aid revenues per pupil (total state aid divided by DCAADM), local revenues per pupil (total local revenue divided by DCAADM), total instructional expenditures, total debt service expenditures and total administrative cost revenues, along with the following expenditure ratios: instructional (instructional expenditures divided by total expenditures), debt (debt payments divided by total expenditures) and administrative (administrative expenditures divided by total expenditures).The student outcomes are average mathematics and English standardized tests scores for grades three through eight. This is an averaged measure of district performance on end of year standardized testing administered to all students in grades three through eight. The test score scale changed in the 2012-13 academic year, and so we standardize these measures by year to have mean of zero and standard deviation of one.

Finally, our analysis considers district-level student demographic characteristics. These include percent free lunch eligibility, percent black students, percent Hispanic students, percent limited English proficiency (LEP), and percent of students with disabilities. Four of these variables were directly reported from New York State. Percent students with disabilities is calculated by dividing total students with disabilities in grades three through eight by total students in grades three through eight. Analysis in our models employs data from the 2007-08 academic year, in which the EBRA was implemented, to the 2014-15 academic year. All financial variables are adjusted for inflation, reported in year 2016 dollars and divided by 1,000. Additional minor variable construction details are provided in an appendix. We provide summary statistics of all dependent and independent variables in Table I.

4 Methods

Our analysis has two primary objectives. First, we use a two-way fixed effects model to estimate the direct effects of year-to-year enrollment declines on per-pupil expenditures (PPE). We also use these first stage models to explore the impacts of Save Harmless treatment on fiscal resources and allocations. Second, we use an instrumental variables strategy to estimate the impact of PPE on student performance, assigning the Save Harmless policy as an instrument for expenditures. These primary methods, along with a comprehensive set of robustness tests, are described in this section.

4.1 Estimating the Effect of Enrollment Declines on Institutional Resources

To evaluate the direct impact of the Save Harmless policy on institutional outcomes we estimate a two-way fixed effects model according to the following equation:

$$y_{dt} = \gamma_0 Save\ Harmless_{dt} + \gamma_1 Enrollment_{dt} + \gamma_2 X_{dy} + \theta_d + \tau_t + \varepsilon_{dt} \quad \text{(Equation 1)}$$

Where y is an outcome of interest for district d in year t, γ_0 is the coefficient on our primary explanatory variable $Save\ Harmless$ (representing the degree of enrollment decline) and γ_1 is the effect of enrollment (DCAADM), controlling for which allows us to isolate the impact of the Save Harmless conditional on the level of student enrollment. γ_2 represents coefficients on X, a vector of district level time-varying characteristics, and ε is a stochastic error term for district d in year t. We include θ_d, a vector of district fixed effects which can control for time-invariant characteristics of the district, including any underlying factors that may cause the district to have larger or smaller average student enrollment declines. We also include a vector of year fixed effects τ_t which accounts for all observed and unobserved characteristics of each year in our sample, including secular time trends in population change. All models are estimated with Huber-White robust standard errors clustered by district, to address heteroscedasticity and autocorrelation within districts.

4.2a Estimating the Effect of Per-Pupil Expenditures on Student Performance

A more generalizable question than the impact of the Save Harmless policy on institutional outcomes, is the direct effect of per-pupil educational expenditures (PPE) on student achievement, which constitutes a perennial debate in the educational literature. A naïve approach to estimating this relationship would be to estimate an ordinary least squares model of the following form:

$$y_{dt} = \alpha_0 + \alpha_1 PPE_{dt} + \alpha_2 X_{dt} + \varepsilon_{dt} \qquad \text{(Equation 2)}$$

Where y is an outcome of interest for district d in year t, α_0 is the y-intercept, α_1 is the association of PPE with educational performance, α_2 is a vector of coefficients on a vector of district level demographic and institutional characteristics (X), and ε is a stochastic error term for district d in year t . However, estimates of this form are likely to contain bias, since the level of school resources in a district is correlated with student characteristics. Even after incorporating district and year fixed effects, estimates are likely to be downward biased because US educational authorities typically target increased resources to districts with current performance deficits or higher need (Jackson et al., 2016). This endogeneity violates the necessary assumptions to identify an unbiased estimate of the coefficient α_1 through linear regression.

If *Save Harmless* has a strong independent impact on PPE and student-teacher ratios, as we have predicted, it is possible to use our models from section 4.1 as a first stage in a two-stage least squares (2SLS) model to estimate the direct impact of PPE on student performance. This requires us to either demonstrate that enrollment change is unlikely to affect school districts in any way other than through its direct impact on educational resources, or to fully control for these alternate mechanisms. If we can isolate the exogenous impact of *Save Harmless,* we can use the two-way fixed effect models to predict values of district PPE, and then use predicted values in a second model to identify the effect of educational expenditures on student outcomes.

We describe our 2SLS approach as an extension of the method described in Section 4.1. First, we estimate the first stage model according to Equation 1 listed above, with per-pupil expenditures and the student-teacher ratio for district d in year t as the two outcomes of interest. If we accept the conditional exogeneity argument – which we defend in the following section – then these predictions of expenditures per pupil are un-confounded by unobservable impacts of *Save Harmless.* We can use them to identify the impact of PPE and student-teacher ratio on performance in a second stage equation of the following form:

$$Student\ Outcome_{dt} = \delta_1 \widehat{Resources}_{dt} + \delta_2 Enrollment_{dt} + \delta_3 X_{dt} + \theta_d + \tau_t + \varepsilon_{dt}$$

(Equation 3)

Here, $Student\ Outcome$ is one of our four measures of student performance in district d in year t, δ_1 is the effect of a one thousand dollar increase in PPE or one student decrease in the student teacher ratio from $\widehat{Resources}$, our predicted values of school resources. All other terms are identical to the first stage model. If our assumptions hold, then δ_1 will reflect the true impact of educational resources on student performance in this context. All models are estimated with Huber-White robust standard errors clustered by district, to address heteroscedasticity and autocorrelation within districts.

4.2b Save Harmless Treatment as an Instrument

We show in the results section that *Save Harmless* is a strong predictor of PPE, but to serve as a plausible instrument, our variable must meet three additional assumptions. These are conditional independence, the exclusion restriction, and monotonicity. We interrogate these assumptions with a series of falsification tests. We only require the instrument to be exogenous conditional on the controls in our model, including: current student enrollment levels, district and year fixed effects, and the vector of time-varying student and institutional characteristics.

Conditional Independence

The first assumption that our instrument must meet is that of conditional independence. For this assumption to be upheld, our instrument must not be correlated with the error term in our outcome equation. The largest threat to this assumption is that the composition of test-taking students may change as enrollment declines, although it is unclear whether such a change would represent positive or negative selection out of school districts.

We conduct a test of the conditional independence assumption which regresses demographic characteristics of students on our measure of Save Harmless treatment, in a model similar to the first stage equation with district and year fixed effects (Appendix Table 3). Save Harmless effects on several measures of student characteristics, including percent free lunch eligibility, percent students with disabilities and percent limited English proficiency, are insignificant at the .05 level. However, percent black students and Hispanic students are significantly related. A one percentage point decline in enrollment is associated with a .02 percentage point increase in the proportion of black students and a .11 percentage point decrease in the proportion of Hispanic students. We control directly for these variables along with the other demographic variables in our models, so this finding poses no direct threat to validity, but could potentially signal the existence of some unobservable selection mechanism beyond what we are able to address with our vector of demographic controls.

Exclusion Restriction

In order for our instrumental variables assumptions to hold, our instrument must not affect academic performance through any mechanism other than educational resources. If there is another variable correlated with both the instrument and the outcome, it has the potential to confound our estimates. The finding that two of the five demographic covariates is correlated with our instrument does warrant some attention[1]. While it is possible that there is student sorting on unobservables, we believe that our controls for five major types of student demographics significantly mitigates these effects. We explore this threat further in our robustness check section.

One further potential threat to the validity of our instrument is that effects could be driven by institutional impacts associated with enrollment change that are not caused by educational resources. One likely way this could happen were if our results were being driven by

[1] In Appendix Table 6, we test the relationship between our demographic controls and our measures of student achievement by regressing academic outcomes on the full vector of controls in our first stage model, excluding our instrument. % free lunch eligibility has a significant effect of -.005 standard deviations (sd) on both Math and English test scores ($p < .01$). % Black has a statistically insignificant impact on Math scores, and a statistically significant -.01 sd impact on English scores ($p < .10$). % Hispanic has a significant -.03 sd impact on math scores ($p < .01$) and a -.01 effect on English scores ($p < .05$). We compare this to reduced form estimates including our instrument. A 1% enrollment decrease produces a .01sd increase in math scores and a .009 increase in English, in models that control for minority composition. Since a 1% decrease in enrollment is associated with a .05 % increase in free lunch eligibility, a .02% increase in black students and a .11 % decrease in Hispanic students, uncontrolled estimates would be biased upwards by approximately .0005, and -.003 sd in Math and .0005, .0002 and -.001 in English for a -.0025 sd cumulative effect in Math and a -.0003 sd cumulative effect in English. These effects are significant in comparison to Math estimates, but negligible in comparison to our English estimates. We therefore proceed with caution in interpreting our math results, which seem particularly sensitive to demographic confounding, while we more confidently interpret our English results which are more robust.

observations in which districts experienced enrollment *increase*. In these observations, districts receive an influx of new students. A significant literature documents a negative impact on student achievement resulting from students changing schools. For instance, Rockoff and Lockwood find that movement from elementary to middle school is associated with a .15 standard deviation decline in test scores (.0015 per 1% increase), and Brummet (2014) documents that displacement from mass school closures in Michigan caused negative impacts of .002 standard deviations per 1 percentage point increase in displaced students within a school. Schwartz, Stiefel and Cordes (2017) find that being a new student decreases achievement in Math and English by .5 standard deviations, which would imply an effect of -.005 sd per 1% increase in new students. To ensure that our results are not being driven by this confounder we conduct two robustness checks at the conclusion of the paper.

Monotonicity

Finally, for our instrumental variables assumptions to hold, the enrollment change measure must meet the assumption of monotonicity. This means that percentage change in enrollment ("Save Harmless") should affect PPE in the same direction in all districts. A simplified version of this test might be to demonstrate that enrollment decline has a positive relationship with PPE in subgroups of district types. We partition our sample based on four need-to-resource categories specified by NYS to classify districts by wealth and population concentration. These are high need urban, high need rural, medium need and low need. If the effect is positive or statistically indistinguishable from zero in all models, then the assumption is upheld. We run our first stage regression in the four subsamples of districts. The results are included in Appendix Table 7. The results show a positive relationship of enrollment decline with PPE in all models. These findings illustrate that the impact of Save Harmless does not vary substantively across different district types.

5. Results

The empirical strategy in this study proceeds in two steps. First, we employ a two-way fixed effects model to identify the impacts of Save Harmless treatment on institutional outcomes in NY school districts. In the second step, we employ a two-stage least squares (2SLS) approach to identify the direct impacts of PPE and student-teacher ratio on student outcomes.

Our first set of results pertains to the two-way fixed effects models estimating the impact of the *Save Harmless* enrollment decline measure on educational inputs (per-pupil expenditures, state aid per-pupil, local revenue per-pupil and average class size as measured by the student-to-teacher ratio). The results of these models are provided in Table II. All models contain district and year fixed effects. Column 1 shows large and statistically significant effects of enrollment declines on PPE. This makes intuitive sense; if Save Harmless districts receive equal funding as prior years in which they had fewer students, dividing their total expenditures by a smaller enrollment figure will lead to larger measures of PPE. The average effect on treated districts is an increase of approximately $200 dollars per pupil per 1% decline in enrollment. This finding carries over to state aid. Column 2 shows that a 1% decline in enrollment predicts a $90 increase in state aid per-pupil.

Because the increase in total per-pupil expenditures from enrollment decline outpaces that of state aid per pupil, it is likely that local revenues also experience growth per pupil from a decrease in the denominator of the number of students. We observe this in column 3, which shows a statistically significant increase in local revenue per pupil of approximately $100. This is also somewhat expected. Even if many residents move out of a given school district, they will either maintain possession of their property or sell their property to a new owner, leaving the property tax base unchanged. Unless local school budget decisions are extremely responsive to enrollment declines and significantly reduce property tax rates, or property values drop rapidly in pace with enrollment loss, local revenues will likely remain relatively constant while enrollment declines. In column 4 we observe a statistically significant decline in student teacher ratios of approximately .06 students per teacher corresponding to a 1% enrollment change. These results also confirm logical predictions; if Save Harmless districts lose students, but funding is sustained to avoid layoffs, we should see a decline in average student-teacher ratios.

Next, we examine the effect of Save Harmless on institutional spending decisions (Table III). We consider impacts on the three major spending categories: instructional spending, debt payments and administrative expenses. The results show small shifts in spending composition, with effect sizes less than .1% change in relative allocation per 1% enrollment decline. Instructional spending decreases as a share of expenditures, while debt payments and administrative expenditures increase. These changes likely reflect a minor shift towards higher relative reliance on fixed costs and lower relative reliance on variable costs as the number of students decreases. Comparing raw measures of expenditures show how minor these changes are. There is null impact on total instructional spending and debt payments. Administrative spending increases by $30,000, compared to $8,130,000 per district in annual expenditures. We conclude that the increase in per-pupil expenditures from Save Harmless does not lead to observable additional investments or reallocation of resources. This makes our natural experiment unique, as it creates a situation where most institutional arrangements remain constant, while only student enrollment decreases. Thus, we are able to explore the effects of arbitrary concentration of uniform educational resources, rather than being concerned that new money is used differently than under normal circumstances. To strengthen this argument, we control for these expenditure ratios in all our models, to remove variation in spending composition and isolate the singular effect of holding resources constant while decreasing the number of students. This makes the implications of our results unique in comparison to the broader school finance literature.

We now proceed to direct estimation of the effect of PPE on student outcomes. The first stage models show that the Save Harmless indicator is a strong instrument for PPE, with an F-statistic of approximately 425. A one percent decline in student enrollment translates to a $200 increase in per-pupil expenditures, as reported already in Table II. Below we describe the second stage results of our 2SLS models which are reported in Table 4.[2] We estimate the effects of PPE on average math and English tests scores in grades three through eight. The results are dramatic (see Table V). Both results are highly significant, with p-values of .000. The effect sizes are large at .0468 standard deviations, in math and .0420 standard deviations in English, a gain of approximately one seventh of a grade level in Math and one ninth of a grade level in English (Bloom, Hill, Rebeck Black and Lipsey 2008).

While our models exploit within-district variation and therefore measure the effect of a $1000 spending increase on change in test scores, context matters to the interpretation of these results. They are not simply equivalent to a helicopter drop of money, due to the cumulative nature of the enrollment trends in NYS as operationalized by our instrument and their effect of district level spending. As we see in the Figure III graph of the Save Harmless measure plotted over time, enrollment decline trends in a linear pattern. Enrollment decrease compounds year after year at a rate of approximately 1.42% per year, which equates to an additional $294 per

[2] R-squared values for these models are suppressed. According to Wooldridge (2015), R-squared figures in 2SLS are often negative, and interpretations of R-squared do not lend valuable information in the context of two-stage least squares instrumental variables estimation.

year. Thus, a district exposed to $294 increased PPE in period *t* was likely exposed to $294 extra PPE in period t-1, and in period t-2, and so on. Thus, it is important to calculate the total investment, when converting this result to a per-dollar expected impact.

Our back-of-the-envelope calculations indicate that $1 as represented in our coefficient for current year PPE change is typically associated with $3 in cumulative spending over several prior years. Sample calculations supporting this ratio are shown in Table 5. This table documents the average enrollment decline from the prior year in each district, along with the average cumulative enrollment decline from the base year, the average extra PPE delivered each year, and the average cumulative PPE delivered up through that year. We find that the average district (in the average year) was exposed to $1,323 extra dollars in the current year, and $4,410 over all prior periods, from cumulative enrollment declines. To the extent that previous period expenditures still affect current student achievement conditional on current expenditures, our point estimates of 0.04 to 0.05 standard deviations test score growth corresponding to a $1,000 current investment may in fact reflect a larger investment over multiple years.[3]

To compare to a commonly-used approach in the education finance literature, we estimate two-way fixed effects models of the direct effect of PPE on our dependent variables, according to equation 3. Both results are statistically significant at the .01 level, with effect sizes of .0177 sd in Math and .0174 sd in English. The results of these analyses are presented in Table A.8. These results are similar to our 2SLS estimates when adjusted for cumulative expenditures, but diverge from former research much of which has failed to identify significant positive effects

[3] Roughly, dividing the current year investment by the cumulative investment in Table 5 produces a proportion of .3. If we assume that prior period investments have equally large effects as current period investments, our effect sizes would then be closer to .0140 standard deviations (sd) in Math and .0126 sd in English per $1000 PPE. These effect sizes would place our estimates squarely within the range of the benchmark of between .01 and .024 sd per $1000 PPE established by LaFortune et al (2016).

in two-way fixed effects models. It is possible that since growth in PPE during this period was so extreme due to enrollment change, and this variation was driven by our Save Harmless mechanism rather than traditional need-based school finance rules, the estimates are free of the downward bias and simultaneity issues that plague these estimates in other contexts.

Robustness Checks

To test the robustness of our findings we first estimate models with alternate control variable strategies. Since the main threat to our model is that estimates are being driven by within-district shifts in student demographic composition, we are particularly interested in how sensitive our estimates are to the introduction of the five demographic controls. If Save Harmless is essentially exogenous, results should be robust to different model specifications, with similar point estimates between models. The results of these analyses are included in Tables A.9. In the models of the effect of PPE on outcomes, effect sizes are relatively consistent in English, with effect sizes changing by only approximately .005 standard deviations from the highest to lowest estimate (or approximately 10% of the largest estimate). The math results show a somewhat problematic result in this test, with effect sizes shrinking by .019 standard deviations (or approximately 30% of the largest estimate). This is consistent with our finding that the most influential percent Hispanic confounder is more significantly related to math scores, with a larger effect size. While our final estimates account for this confounder, the sensitivity of the result to these controls leads us to be cautious about the possibility of confounding by unobservables for our math results.

To further probe the sensitivity of our results we estimate models with district-specific linear time trends. This modification to the model estimates a different trend slope for each district in the sample, controlling for factors that trend linearly within a district over the period of our sample. Since enrollment declined monotonically in our sample, correlated demographic shifts would also trend monotonically, making this a powerful adjustment for potential confounders[4].

The tradeoff is that this correction fundamentally changes the identifying assumption of our model. Since our instrument essentially operationalizes a linear enrollment trend, detrending the data strips out all cumulative variation in our sample. According to Wooldridge (2003), two-way fixed effects models with individual specific trends are analogous to estimating a two-way fixed effects model on first differenced data. Identification now comes from variation in the magnitude of year-to-year enrollment declines rather than the accumulation of enrollment declines over time. Instead of detecting positive effects from districts who experienced large consistent declines year after year, we are now identifying off of districts who experienced extreme variation in their year-to-year enrollment change.

The models with linear trends are presented in Appendix Table 10. Effects in math, while in the same direction, are about one half the size of our main estimates, and not precisely estimated. Results in English, while similar in magnitude to the math results, are significant at the .05 level. These results confirm the findings of the previous section, that our results in math are more sensitive than those in English but adds greater confidence that enrollment change drives an effect in English that is robust to multiple alternative specifications. We emerge

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[4] In Table A.5 we test this correction on correlations between our instrument and demographic characteristics. We see that the instrument now achieves balance on the problematic % Hispanic composition variable, along with % free lunch eligibility, % limited English proficiency and % students with disabilities. The tradeoff is that there is now a significant negative relationship between % Black composition and the instrument. However, the effect is much smaller than the effect on % Hispanic in the main models, at less than 1/5 the size, and coupled with the controls for all monotonically changing unobservable district characteristics, this model is far less unconfounded.

confident that educational resources do have a significant and meaningful effect, but that the effect might operate differently in English and Math.

One concern with our results is that large population losses in a local area might lead to shifts in the local macroeconomy, which could have resulting impacts on student achievement. To test this hypothesis, we estimate models that control for local unemployment rate.[5] The results of these analyses are included in Appendix Table 11. Unemployment rate shows insignificant correlation with our instrument and produces minimal effect on the estimated coefficients in our main model.[6]

The final endogeneity concern that we address is the possibility that results are driven by the negative impact of new students on test scores, as discussed in section 4.2b. Since districts with enrollment growth are likely to have more new students than districts with enrollment declines, we restrict our sample to only observations in which districts experienced an enrollment decrease. This ensures that no aspect of the result is being driven by comparisons to districts with enrollment increase since 2007. The results of these tests are included in Appendix Table 12. The results in this subsample are fundamentally equivalent in effect size and significance to those in the main models. This demonstrates that the effects detected are entirely driven by districts maintaining higher funding levels through the Save Harmless provision.

While this alleviates some concern about new students, it does not eliminate it. Districts experiencing enrollment decline likely still have a population of new students each year, and the

[5] We gathered data on local unemployment rates from the NYS Department of Labor, which was available monthly for the period in our sample for all geographic areas with greater than 25,000 in population. We collapsed by mean to the academic year, and then matched to our data at the city level, and at the county level for districts whose home city had less than 25,000 residents. Approximately 10% of observations yielded a city match, and the remaining 90% were matched at the county level.

[6] One concern is that we draw a positive coefficient on the relationship between unemployment and test scores – an unexpected direction. Nonetheless, with its minimal association with our instrument it is unlikely to change estimates, even if it had the expected negative effect on achievement.

size of this population is perhaps correlated with the magnitude of enrollment decrease. Thus, our model could be picking up the influence of higher populations of new students in districts with smaller enrollment decline. To ensure this was not the case we acquired supplemental data from NYSED on the percentage of students in grades 3-8 who were not enrolled in the district in the year prior, although this data was only available since the 2012-13 school year. Nonetheless, the performance of this variable in this restricted period can inform our understanding of the possibility of confounding by new students.

Some exploratory analyses show that districts with enrollment decline do in fact have significant populations of new students, and the size of this population is negatively correlated with the magnitude of enrollment decrease. In Appendix table 13 we explore the relationship between this variable and our instrument, along with our measures of student achievement. The variable is, as expected, highly correlated with our instrument. A 1% enrollment decline is correlated with a .2% decline in the percentage of new students. However, this variable does not have a significant effect on student achievement, with a point estimate of -.005 standard deviations per 1% increase in new students.

In appendix table 14 we estimate our 2SLS models on the restricted three-year sample. This yields estimates that are significantly larger than in our main model, however this is because districts in later years have greater cumulative exposure to enrollment declines and increased revenue. When we introduce controls for percent new students, the results do not yield meaningfully. In particular, coefficients only change by .0029 sd (4%) in Math and .001 sd (1.4%) in English. Thus, we emerge confident that the effects of new students are not significantly biasing results in the main sample.

6. Discussion

The findings in this study show clear and compelling evidence that educational resources improve student learning, providing a new contribution to an old debate over the relationship between educational resources and student performance. While previous studies have showed positive impacts of SFR's, this study explores the effects of variation in educational spending in a previously unexplored context, in which resources are held constant while enrollment undergoes massive shifts. Thus, we identify the impact of the concentration of existing resources, rather than the delivery of new ones. While more research is needed to resolve this debate across different contexts and with different empirical strategies, our results, coupled with other findings from quasi-experimental studies, should suggest that allowing districts to maintain high levels of funding even during periods of enrollment loss benefits student achievement.

Efforts to deliver additional resources to low-performing schools have been a long-standing endeavor among educational reformers. After succeeding in equalizing raw measures of expenditures per pupil, advocates advanced the further argument that under-performing school districts actually needed more money than wealthy districts to achieve the same level of performance (Bifulco 2005; Duncombe and Yinger 2000; Lafortune, Rothstein and Schanzenbach 2016; Yinger 2004). While such performance-based educational finance reforms have flourished in recent years, the field still lacked credible evidence that increases in spending could meaningfully change educational performance and help close these gaps. With inconclusive findings in the economics of education literature, critics of school spending have been able to argue that such reforms are futile and misguided.

The findings of this study provide additional evidence that undermines these critiques. They show that even small increases in PPE, on the order of $1,000 per pupil, or less than five percent of current average education spending in New York, can increase educational performance by at least 0.015 standard deviations and possible by as much as .045 standard deviations. These effects are similar in magnitude to recent estimates provided by Lafortune et. al (2016).[7] As achievement gaps between privileged and disadvantaged students remain a persistent source of inequality in society (Clotfelter, Ladd and Vigdor 2009; Fryer and Levitt 2004; Fryer and Levitt 2006; Hanushek 1997; Reardon 2011), this study provides a persuasive case that greater investment in education could help close these gaps. Coupled with other credible results from quasi-experimental analyses, (e.g. Jackson, Johnson and Persico 2016; Papke 2005; Guryan, 2001) this study builds onto a growing body of evidence that educational resources contribute to improved student outcomes. Nonetheless, the results suggest that the effects of spending are not large enough to completely solve the problem of academic achievement gaps. Closing these gaps, which currently measure approximately 1 standard deviation, would require doubling per pupil expenditures, which would likely be practically and politically infeasible even if only implemented in targeted districts. Nonetheless, these findings caution against spending cuts, and support targeted expenditure increases as a policy instrument to improve academic performance.

These results also have policy implications for the effectiveness or desirability of Save Harmless policies. Save Harmless policies are controversial, because critics say they divert resources from real students with real needs to support students that don’t exist (Chira 1983; Cunningham 2014; Levine 1976). However, our findings show that while these policies may contribute to equity losses in theory, in practice they can have broad and equally-distributed impacts on student achievement. In the context of our study, nearly every district in NYS

[7] Lafortune et al. (2016) use ten years of spending as their independent variable, and so we compare our one-year effect size to their estimates divided by ten. Other recent high quality studies are more difficult to compare our effect sizes to given that they either did not focus on test score outcomes or provided test score outcomes in terms of proficiency rates.

benefitted from additional financial aid under the Save Harmless policy, and experienced notable improvements in student achievement. In a state like New York, which is threatened by serious demographic changes which could undermine the stability of school districts state wide, Save Harmless policies helped schools weather the disruptions. These results may not generalize to other states, where there is more pronounced variation in areas with and without enrollment decline, which could lead to unevenly distributed benefits. But in states where the policy's impacts are evenly distributed, Save Harmless policies can mitigate adverse demographic trends through resource stability.

This study is subject to some limitations. The instrumental variables approach employed in this study requires strong assumptions about the impact of unobservables on our estimates, and the centrality of per-pupil expenditures to the impacts of the Save Harmless policy. We may not be able to perfectly defend the validity of these assumptions given the limitations of our data, and the unique conditions of the natural experiment upon which our analysis is based. We probe the sensitivity of our estimates to violations of these assumptions and find that they have limited impact on our estimates, though our estimates of effects on English scores may be more robust than those on math scores.

While we cannot entirely rule out confounding by time-varying unobservables, we have demonstrated a few conclusive findings. Enrollment declines during the period of our study were associated with substantial concentration of resources in New York school districts, and this resource concentration was associated with highly significant increases in student achievement. When we quantify these achievements in terms of their relationship with per-pupil-educational expenditures, we estimate effect sizes consistent with the hypothesis that these gains were produced entirely by educational spending, based on credible benchmarks from the literature. As in all cases of empirical analysis, the results of this study cannot provide the definitive statement on relationship between educational resources and student performance across all educational contexts. But they do construct a narrative of how, in this case, state aid maintenance over time resulted in smaller class sizes and marked educational improvement.

We offer more current estimates of the causal impact of PPE than the studies we have cited, using data as recent as the 2014-15 academic year. We also offer the first estimates to our knowledge of the impact of PPE in New York State following their 2007 educational finance reform, which allows our research to inform discussions of school finance reforms at both the state and national level. The fact that we find positive effects of increased spending even in New York State, which boasts the highest per-pupil spending in the country, suggests that resources are important even above some adequacy threshold. Finally, this study provides the first examination of the effects of Save Harmless policies on student outcomes, which makes a salient contribution to the literature on school finance systems. In recent years, 22 percent of total state and local spending have gone to funding elementary and secondary education (US Census, 2014). Continuous and rigorous analysis of the benefits that accrue to students can inform how decision-makers allocate resources to and within the education sector during times of fiscal scarcity.

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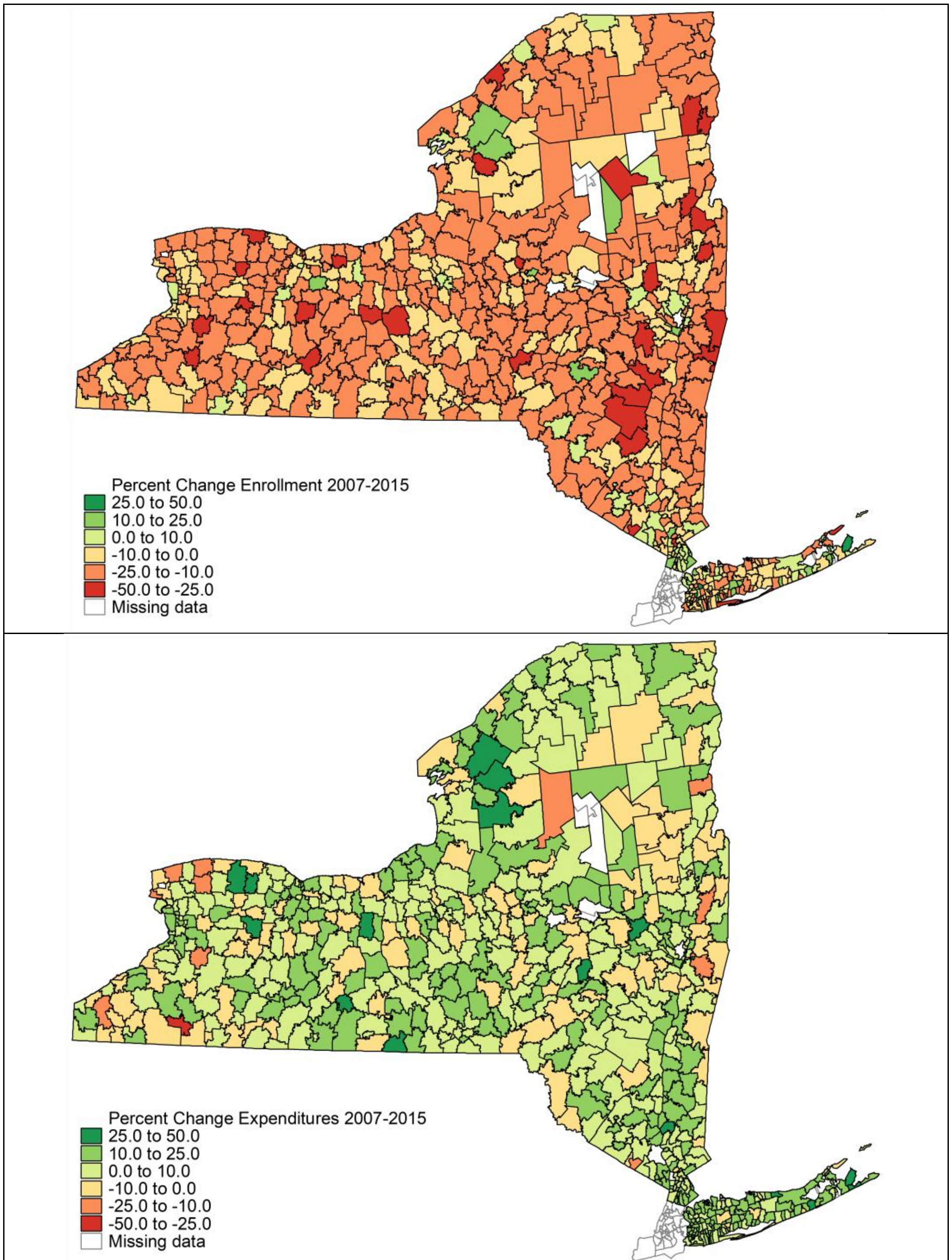
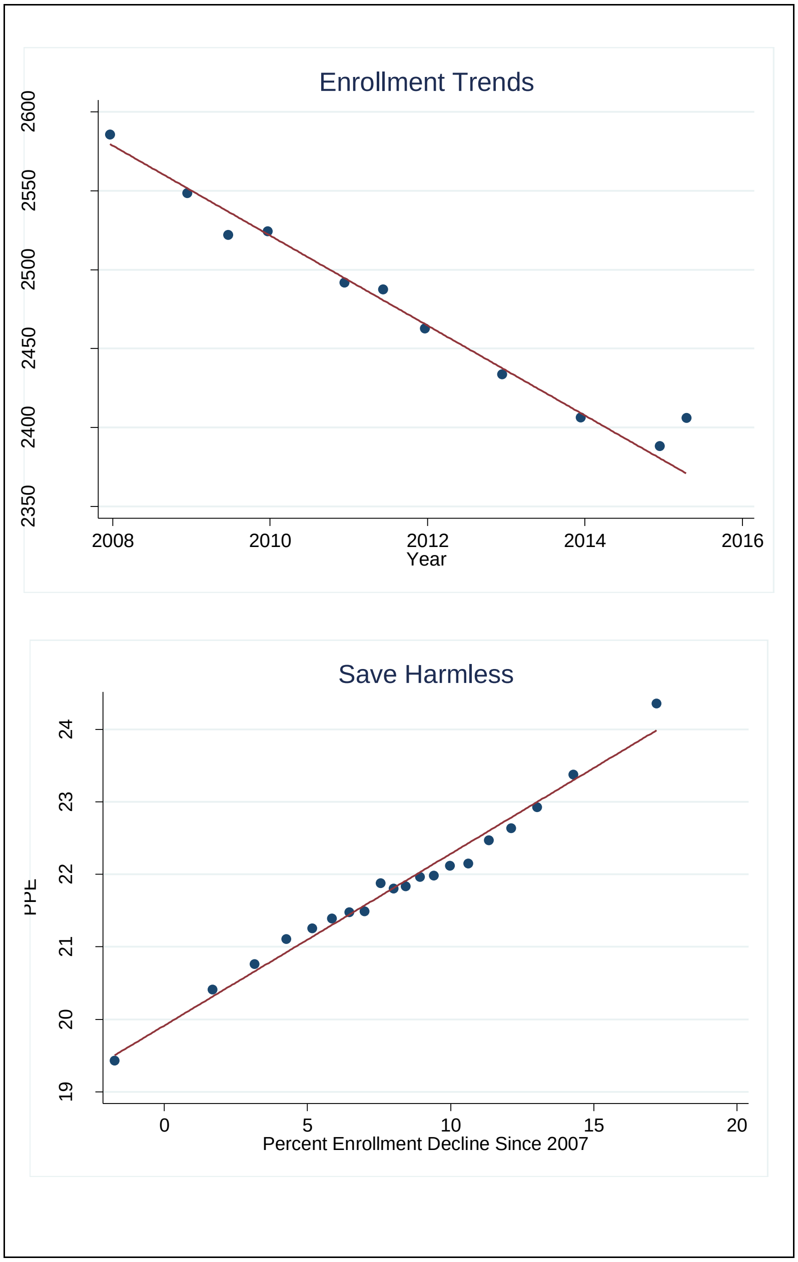
Tables and Figures

Figure I: Patterns in Changes in Student Enrollment and Total Expenditures Across NYS



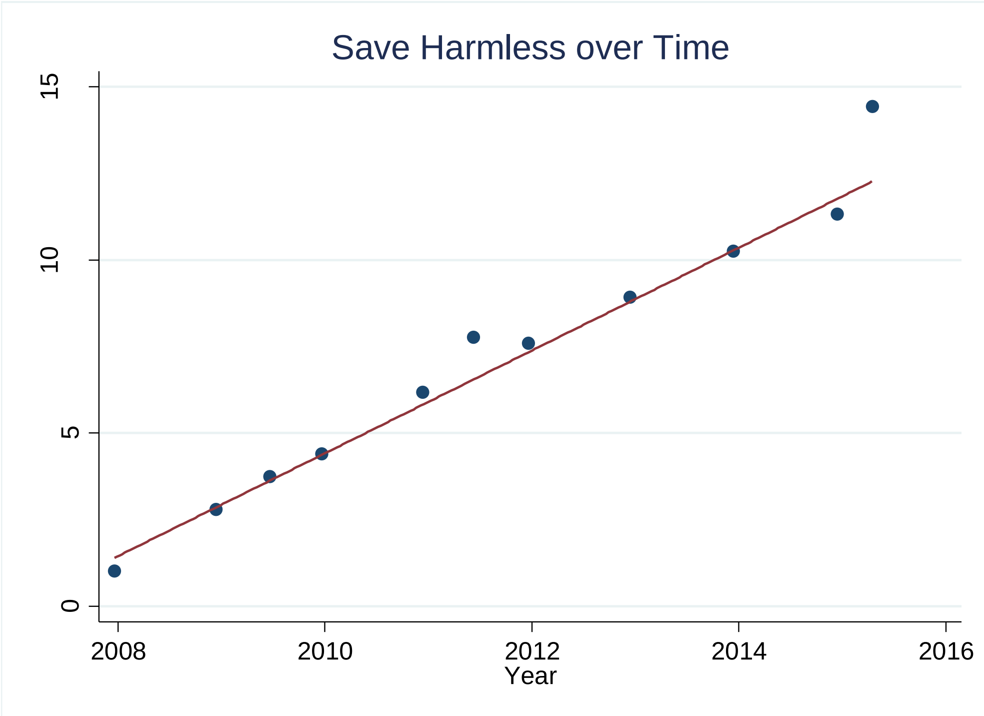
Note. Expenditures are inflation adjusted to $2016 dollars.

Figure II: Enrollment Decline and the Relationship between Enrollment Change and PPE



Note. These figures are binned scatter plots absorbing district fixed effects with linear trend lines added. The first graph plots average district student enrollment across New York between 2008 and 2016; the second graph plots per-pupil expenditures (PPE) by the percent enrollment decline since the 2006-2007 school year.

Figure III: Enrollment Decline Under Save Harmless by Year



Note. This figure is a binned scatter plot absorbing district fixed effects with linear trend line added. This figure plots enrollment decrease since 2007 against time.

Table I: Descriptive Statistics					
Variable	Obs	Mean	Std. Dev.	Min	Max
Math Scores	4,821	0.00	1.00	-3.71	3.38
English Scores	4,821	0.00	1.00	-3.60	3.51
Save Harmless	4,821	6.63	6.97	-39.34	36.28
PPE	4,821	21.82	4.78	12.80	70.73
Student-Teacher Ratio	4,821	11.38	1.90	4.21	58.00
State Aid PP	4,821	8.87	4.04	0.98	20.93
Local Revenue PP	4,821	10.97	6.90	1.95	49.58
Instructional Expenditures	4,821	28968.99	37601.05	1676.11	467439.40
% Instructional	4,821	0.53	0.05	0.21	0.69
Debt Expenditures	4,821	3636.85	6070.29	0.00	188577.70
Debt Ratio	4,821	0.08	0.04	0.00	0.67
Administrative Expenditures	4,821	7201.04	9215.37	463.56	124614.30
Administrative Ratio	4,821	0.14	0.02	0.05	0.29
Enrollment	4,821	2477.45	3183.05	117.00	43898.00
% Free Lunch Eligibility	4,821	25.67	15.35	0.00	95.00
% Black	4,821	4.87	9.23	0.00	80.00
% Hispanic	4,821	6.29	9.66	0.00	71.00
% LEP	4,821	1.59	3.30	0.00	33.00
% Students with Disabilities	4,821	15.56	4.64	3.52	52.67
% New Students	1,777	7.32	2.66	1.42	20.20
% Unemployment	4,821	7.19	1.57	3.34	11.72

Table II: First Stage Models				
	(1)	(2)	(3)	(4)
VARIABLES	PPE	State Aid PP	Local Revenue PP	Teacher Student Ratio
Save Harmless	0.2058**	0.0951**	0.1008**	-0.0559**
	(0.0100)	(0.0054)	(0.0129)	(0.0068)
Enrollment	-26.6090**	1.9208	-7.4544+	4.1870**
	(4.4410)	(1.7353)	(4.1959)	(1.3204)
Administrative Ratio	11.6565+	9.0253**	12.0445	0.7273
	(5.9797)	(1.4022)	(7.4308)	(0.8461)
Debt Ratio	-25.4448**	0.0350	-5.9857**	-2.6594**
	(1.7971)	(1.0521)	(1.6614)	(0.7139)
Instructional	0.0002	0.0006**	-0.0005+	-0.0003*
	(0.0003)	(0.0001)	(0.0003)	(0.0001)
% Free Lunch	-0.0147**	-0.0015	-0.0099*	0.0043+
	(0.0048)	(0.0023)	(0.0042)	(0.0026)
% Black	0.0201	-0.0012	0.0267	-0.0622**
	(0.0250)	(0.0123)	(0.0255)	(0.0172)
% Hispanic	0.0864**	0.0188*	0.0658**	-0.0037
	(0.0161)	(0.0079)	(0.0190)	(0.0102)
% LEP	-0.0480	-0.0588**	-0.0281	0.0155
	(0.0449)	(0.0194)	(0.0432)	(0.0292)
% Students with Disabilities	0.0122+	-0.0007	0.0027	-0.0084**
	(0.0063)	(0.0030)	(0.0066)	(0.0030)
Constant	36.0151**	5.7365**	14.4091**	13.2644**
	(1.4162)	(0.8462)	(1.5743)	(0.5483)
Observations	4,821	4,821	4,821	4,821
R-squared	0.6984	0.5294	0.4226	0.0542
F-Statistic (Save Harmless)	425.69	313.85	61.05	66.9
Year FE	x	x	x	x
District FE	x	x	x	x
Number of District	610	610	610	610

Robust standard errors in parentheses
** p<0.01, * p<0.05, + p<0.1

Table III: Effects on Educational Expenditure Allocation

VARIABLES	(1) Instructional Expenditure	(2) Instructional Ratio	(3) Debt Expenditures	(4) Debt Ratio	(5) Administrative Expenditures	(6) Administrative Ratio
Save Harmless	-1.0580	-0.0008**	-3.4917	0.0005**	31.9403**	0.0002**
	(11.9988)	(0.0001)	(8.4676)	(0.0002)	(6.4381)	(0.0001)
Observations	4,821	4,821	4,821	4,821	4,821	4,821
R-squared	0.2494	0.4149	0.0336	0.0523	0.2011	0.2324
Covariates	x	x	x	x	x	x
Year FE	x	x	x	x	x	x
District FE	x	x	x	x	x	x
Number of Districts	610	610	610	610	610	610

Robust standard errors in parentheses
** p<0.01, * p<0.05, + p<0.1

Table IV: 2SLS Estimates of the Effect of PPE on Test Scores		
	(1)	(2)
VARIABLES	Math	English
PPE	0.0468**	0.0420**
	(0.0131)	(0.0106)
Enrollment	0.6624	1.2679+
	(0.8828)	(0.7489)
Administrative Ratio	-0.4397	-0.2570
	(0.4377)	(0.4143)
Debt Ratio	1.2809*	1.4109**
	(0.5373)	(0.4465)
Instructional Ratio	0.0002*	0.0000
	(0.0001)	(0.0001)
% Free Lunch Eligibility	-0.0045**	-0.0042**
	(0.0014)	(0.0012)
% Black	-0.0099	-0.0175*
	(0.0095)	(0.0083)
% Hispanic	-0.0280**	-0.0098*
	(0.0061)	(0.0047)
% LEP	-0.0228	-0.0063
	(0.0153)	(0.0119)
% Students with Disabilities	-0.0021	-0.0030+
	(0.0019)	(0.0017)
Constant	-1.8656**	-1.5656**
	(0.6806)	(0.5322)
Observations	4,821	4,821
Year FE	x	x
District FE	x	x
Number of District	610	649
Robust standard errors in parentheses		
** p<0.01, * p<0.05, + p<0.1		

Table 5: Current Year vs. Cumulative Investments from Enrollment Decline			
Current Year Decline	Save Harmless	Current Year Extra PPE	Cumulative Extra PPE
1.43%	1.43%	$294	$294
1.43%	2.86%	$588	$882
1.43%	4.29%	$882	$1764
1.43%	5.72%	$1176	$2940
1.43%	7.15%	$1470	$4410
1.43%	8.58%	$1764	$6174
1.43%	10.01%	$2058	$8232
1.43%	11.44%	$2352	$10584
	Mean:	$1323	$4410

Appendix

Data Construction Supplementary Information

Percent free lunch eligibility included one value greater than 100, we replace this value with the mean of the prior year and the following year observations.

Approximately 200 observations for percent students with disabilities were systematically suppressed in years 2012-13 and 2013-14. To preserve this variables, we used Stata 14.2 SE's impute command, which is a regression based imputation that uses observed information to fill in missing values. The imputation for the students with disabilities variable was calculated using the following variables: percent black, percent Hispanic, percent free lunch eligibility, percent limited English proficiency, enrollment, per-pupil expenditures and per-pupil special education expenditures. A comparison of the original vs. the imputed variables is provided in Appendix Table 1.

The test score dependent variables were missing between 50 and 100 observations each without complete overlap between missing observations. For this reason, we restrict our analysis sample to only those observations for which both variables are not missing. This leads to a total of approximately 100 missing observations for each variable, out of approximately 5,200 possible observations.

Appendix Table 1: Comparison of Original vs. Imputed Variable

Variable	Obs	Mean	Std. Dev.	Min	Max
% Students with Disabilities	4,618	15.54	4.73	3.52	52.67
% Students with Disabilities (Imputed)	4,821	15.56	4.64	3.52	52.67

Appendix Table 2: Conditional Independence

	(1)	(2)	(3)	(4)	(5)
VARIABLES	%Free Lunch	% Black	% Hispanic	% LEP	%SWD
Save Harmless	-0.0001	0.0707**	-0.1183**	-0.0132*	-0.0222
	(0.0291)	(0.0164)	(0.0170)	(0.0064)	(0.0231)
Observations	4,821	4,821	4,821	4,821	4,821
R-squared	0.4577	0.1159	0.3444	0.0463	0.0556
Year FE	x	x	x	x	x
District FE	x	x	x	x	x
Number of District	610	610	610	610	610

Robust t-statistics in parentheses
** p<0.01, * p<0.05, + p<0.1

Appendix Table 3: Conditional Independence (With Controls)					
	(1)	(2)	(3)	(4)	(5)
VARIABLES	%Free Lunch	% Black	% Hispanic	% LEP	%SWD
Save Harmless	0.0537+	0.0247**	-0.1126**	0.0023	-0.0290
	(0.0319)	(0.0086)	(0.0138)	(0.0050)	(0.0265)
Observations	4,821	4,821	4,821	4,821	4,821
R-squared	0.4674	0.2107	0.4332	0.1087	0.0604
Covariates	x	x	x	x	x
Year FE	x	x	x	x	x
District FE	x	x	x	x	x
Number of District	610	610	610	610	610
Robust standard errors in parentheses					
** p<0.01, * p<0.05, + p<0.1					

Appendix Table 4: Conditional Independence (With Controls & District-Specific Linear Trends)					
	(1)	(2)	(3)	(4)	(5)
VARIABLES	%Free Lunch	% Black	% Hispanic	% LEP	%SWD
Save Harmless	-0.0016	-0.0203**	0.0084	-0.0001	0.0278
	(0.0556)	(0.0059)	(0.0119)	(0.0032)	(0.0376)
Observations	4,821	4,821	4,821	4,821	4,821
R-squared	0.6017	0.7629	0.7956	0.5809	0.3184
Covariates	x	x	x	x	x
Year FE	x	x	x	x	x
District FE	x	x	x	x	x
District -Specific Linear Trends	x	x	x	x	x
Number of District	610	610	610	610	610
Robust standard errors in parentheses					
** p<0.01, * p<0.05, + p<0.1					

Appendix Table 5: Conditional Independence (Predicted PPE)					
	(1)	(2)	(3)	(4)	(5)
VARIABLES	%Free Lunch	% Black	% Hisp	% LEP	%SWD
PPE	0.2618+	0.1192**	-0.5908**	0.0110	-0.1410
	(0.1581)	(0.0411)	(0.0840)	(0.0242)	(0.1295)
Observations	4,821	4,821	4,821	4,821	4,821
Covariates	x	x	x	x	x
Year FE	x	x	x	x	x
District FE	x	x	x	x	x

Number of District	610	610	610	610	610

Robust t-statistics in parentheses
** p<0.01, * p<0.05, + p<0.1

Appendix Table 6: Reduced Form Models

VARIABLES	(1) Math	(2) Math	(3) English	(4) English
Save Harmless	0.0096**		0.0086**	
	(0.0027)		(0.0022)	
Administrative Ratio	-0.5831	-0.2698	0.1506	0.4317
	(0.7983)	(0.8061)	(0.6751)	(0.6838)
Debt Ratio	0.1060	0.1265	0.2325	0.2510
	(0.3758)	(0.3756)	(0.3517)	(0.3526)
Instructional Ratio	0.0898	-0.0759	0.3424	0.1937
	(0.3873)	(0.3907)	(0.3520)	(0.3550)
Enrollment	0.0002*	0.0001	0.0000	-0.0001+
	(0.0001)	(0.0001)	(0.0001)	(0.0000)
% Free Lunch Eligibility	-0.0052**	-0.0050**	-0.0048**	-0.0046**
	(0.0014)	(0.0014)	(0.0012)	(0.0012)
% Black	-0.0089	-0.0063	-0.0167*	-0.0144+
	(0.0094)	(0.0096)	(0.0082)	(0.0082)
% Hispanic	-0.0239**	-0.0282**	-0.0062	-0.0101*
	(0.0061)	(0.0061)	(0.0047)	(0.0046)
% LEP	-0.0250+	-0.0243	-0.0084	-0.0077
	(0.0146)	(0.0148)	(0.0117)	(0.0117)
% Students with Disabilities	-0.0015	-0.0017	-0.0025	-0.0027
	(0.0018)	(0.0018)	(0.0017)	(0.0017)
Constant	-0.1798	0.2291	-0.0533	0.3135
	(0.3551)	(0.3394)	(0.2919)	(0.2881)
Observations	4,821	4,821	4,821	4,821
R-squared	0.0411	0.0332	0.0203	0.0126
Year FE	x	x		
District FE	x	x		
Number of District	610	610	610	610

Robust t-statistics in parentheses
** p<0.01, * p<0.05, + p<0.1

Appendix Table 7: Monotonicity (PPE)

	(1)	(2)	(3)	(4)

VARIABLES	PPE (Urban High Need)	PPE (Rural High Need)	PPE (Medium Need)	PPE (Low Need)
Save Harmless	0.160**	0.224**	0.200**	0.320**
	(0.0213)	(0.0155)	(0.0131)	(0.0339)
Observations	333	1,305	2,470	713
R-squared	0.471	0.716	0.720	0.742
Covariates	x	x	x	x
Year FE	x	x	x	x
District FE	x	x	x	x
Number of District	42	165	312	91

Robust standard errors in parentheses
** p<0.01, * p<0.05, + p<0.1

Appendix Table 8: Two-Way Fixed Effects Estimates of the Effects of PPE on Student Achievement

VARIABLES	(1) Math	(2) Math
PPE	0.0177**	0.0174**
	(0.0058)	(0.0050)
Observations	4,821	4,821
R-squared	0.0374	0.0175
Covariates	x	x
Year FE	x	x
District FE	x	x
Number of District	610	610

Robust t-statistics in parentheses
** p<0.01, * p<0.05, + p<0.1

Appendix Table 9: Alternate Control Variable Strategies (PPE)

VARIABLES	(1) Math	(2) Math	(3) Math	(4) English	(5) English	(6) English
PPE	0.0587**	0.0656**	0.0468**	0.0404**	0.0454**	0.0420**
	(0.0125)	(0.0139)	(0.0131)	(0.0099)	(0.0109)	(0.0106)
Enrollment	0.0002*	0.0002**	0.0002*	0.0000	0.0001	0.0000
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Administrative Ratio		1.0838	0.6624		1.2758+	1.2679+
		(0.9301)	(0.8828)		(0.7588)	(0.7489)
Debt Ratio		-0.5931	-0.4397		-0.2695	-0.2570
		(0.5153)	(0.4377)		(0.4306)	(0.4143)
Instructional Ratio		1.7360**	1.2809*		1.4893**	1.4109**

		(0.5565)	(0.5373)		(0.4565)	(0.4465)
% Free Lunch Eligibility			-0.0045**			-0.0042**
			(0.0014)			(0.0012)
% Black			-0.0099			-0.0175*
			(0.0095)			(0.0083)
% Hispanic			-0.0280**			-0.0098*
			(0.0061)			(0.0047)
% LEP			-0.0228			-0.0063
			(0.0153)			(0.0119)
% Students with Disabilities			-0.0021			-0.0030+
			(0.0019)			(0.0017)
Constant	-1.7524**	-3.0074**	-1.8656**	-0.9343**	-2.0499**	-1.5656**
	(0.3802)	(0.6966)	(0.6806)	(0.2871)	(0.5474)	(0.5322)
Observations	4,821	4,821	4,821	4,821	4,821	4,821
Year FE	x	x	x	x	x	x
District FE	x	x	x	x	x	x
Number of District	610	610	610	610	610	610

Robust standard errors in parentheses
** p<0.01, * p<0.05, + p<0.1

Appendix Table 10: District-Specific Linear Time Trends

VARIABLES	(1) Math	(2) English
PPE	0.0194	0.0224*
	(0.0132)	(0.0109)
Observations	4,821	4,821
Covariates	x	x
Year FE	x	x
District FE	x	x
Linear Trends	x	x
Number of District	610	610

Robust standard errors in parentheses
** p<0.01, * p<0.05, + p<0.1

Appendix Table 11: Unemployment Control

VARIABLES	(1) % Unemployed	(2) Math	(3) English
Save Harmless	0.0021		
	(0.0015)		

PPE		0.0467**	0.0419**
		(0.0131)	(0.0107)
% Unemployed		0.0505*	0.0367+
		(0.0221)	(0.0208)
Observations	4,821	4,821	4,821
Covariates	x	x	x
Year FE	x	x	x
District FE	x	x	x
Number of District	610	610	610
Robust standard errors in parentheses			
** p<0.01, * p<0.05, + p<0.1			

Appendix Table 12: Restricted Sample (Decline Only)				
	(1)	(2)	(3)	(4)
VARIABLES	Math	English	Math	English
PPE	0.0468**	0.0420**	0.0430**	0.0435**
	(0.0131)	(0.0106)	(0.0151)	(0.0127)
Observations	4,821	4,821	4,143	4,143
Covariates	x	x	x	x
Year FE	x	x	x	x
District FE	x	x	x	x
Enrollment Decline			x	x
Number of District	610	610	610	610
Robust standard errors in parentheses				
** p<0.01, * p<0.05, + p<0.1				

Appendix Table 13: % New Student Descriptives			
	(1)	(2)	(3)
VARIABLES	% New Students	Math	English
Save Harmless	-0.1909**		
	(0.0363)		
% New Students		-0.0071	-0.0042
		(0.0054)	(0.0049)
Observations	1,777	1,777	1,777
R-Squared	0.0678	0.0409	0.0112

Covariates	x	x	x
Year FE	x	x	x
District FE	x	x	x
Number of District	609	609	609

Robust standard errors in parentheses
** p<0.01, * p<0.05, + p<0.1

Appendix Table 14: New Student Controls

VARIABLES	(1) Math	(2) English	(3) Math	(4) English
PPE	0.0708**	0.0613**	0.0679**	0.0603**
	(0.0171)	(0.0162)	(0.0178)	(0.0160)
% New Students			-0.0039	-0.0014
			(0.0059)	(0.0051)
Observations	1,777	1,777	1,777	1,777
Covariates	x	x	x	x
Year FE	x	x	x	x
District FE	x	x	x	x
Number of District	609	609	609	609

Robust standard errors in parentheses
** p<0.01, * p<0.05, + p<0.1