IESP

Does Title I Increase Spending and Improve Performance?

Evidence from New York City

Working Paper #09-09
August 2009

Meryle G. Weinstein, Ph.D.
Research Scientist and Assistant Director,
Institute for Education and Social Policy

Leanna Stiefel
Professor of Economics and of Education Policy,
Wagner and Steinhardt Schools

Amy Ellen Schwartz
Professor of Economics, Education and Public Policy
Director, Institute for Education and Social Policy
Wagner and Steinhardt Schools

Luis Chalico,
Ph.D. Candidate, Wagner School

NYU Steinhardt
Steinhardt School of Culture, Education, and Human Development

NYU Wagner
Robert F. Wagner Graduate School of Public Service

Direct correspondence to Leanna Stiefel, Professor, Institute for Education and Social Policy, 665 Broadway, Suite 805, New York, NY 10003. Email: leanna.stiefel@nyu.edu. Any opinions expressed are those of the authors.

Editorial Board

Abstract

Since its inception as part of the 1965 *Elementary and Secondary Education Act* (ESEA), Title I has provided the largest amount of federal funding aimed at improving the academic achievement of poor children. In this paper, we examine the impact of Title I on school spending and school performance, using New York City public school data. Based on a regression discontinuity design (RD) with panel data, and including separate analyses for elementary/middle and high schools, we estimate local average treatment effects of Title I.

Title I provides additional funding for schools serving high concentrations of poor children, but there are few curricular or programmatic constraints. Unfortunately it is possible that Title I funds supplant state or local funds, resulting in muted net impacts on spending and student outcomes. At the same time, the success of Title I in improving test scores offers important insight into the effectiveness of school-based compensatory funding in general, such as weighted student funding within districts or state compensatory aid across districts.

Overall, the results indicate that Title I changes the mix of spending, enabling high schools to significantly increase the amount of money they spend on direct services to students and to improve their pupil-teacher ratios (while reducing experienced teachers). Elementary and middle schools do not increase spending as much, which is consistent with our finding that state compensatory education funds may be supplanting some Title I funding in schools. Since schools just below the Title I cutoff are similar to those just above the cutoff, this finding may be an equitable, albeit unintended result.

Finally, additional Title I spending does not improve the achievement of students and may even reduce school-wide average test scores in elementary and middle schools. These effects for both spending and scores seem to increase with the length of time schools are Title I eligible and to be stronger for ones that are always Title I eligible compared to those that go in and out of eligibility.

1. Introduction

Since its inception as part of the 1965 *Elementary and Secondary Education Act* (ESEA), Title I has provided the largest amount of federal funding aimed at improving the academic achievement of poor children. As the fourth largest of all federal programs for low-income children (behind Medicaid, food programs, and Temporary Assistance to Needy Families), Title I allocated $13.9 billion in FY 2008, reaching over half the public schools in the country and almost 17 million students.[1,2] The recent federal economic stimulus bill (the American Recovery and Reinvestment Act of 2009 or ARRA) allocates an additional $13 billion to Title I, over two years. Given the amount of money available, should we expect more spending in schools eligible for Title I funding and better academic performance as a result? The answer is not obvious. State or local funds may decline in schools receiving federal Title I funding, resulting in little or no increase in total spending, and it is notoriously difficult to improve student performance of poor children.

Although numerous previous studies estimate the effectiveness of Title I funding in improving student achievement, many suffer from biases due to unobserved differences between Title I and ineligible schools. Further, these studies evaluated the "old" Title I, in which Title I distributed funds based on a mixture of student performance and poverty, rather than solely based on poverty, as is done today. Finally, few studies have looked at the effects on high schools and their students.

This paper improves on these earlier studies in several ways. First, we examine the impact of Title I eligibility on school spending overall and on state compensatory education

[1] http://www.ed.gov/programs/titleiparta/index.html
[2] The numbers of participants has increased faster than the amount of funding or numbers of districts or schools covered because school wide programs have replaced targeted assistance programs. In school wide programs, all students are counted as Title I whereas in targeted assistance programs, only those receiving assistance are counted.

funds separately, to gain insight into possible supplanting. Both spending and outcome analyses use a regression discontinuity (RD) design with panel data. (Alternate specifications use borough and school fixed effects.) Separate analyses for New York City (NYC) elementary/middle and high schools provide different estimates by level. This design provides credible local average treatment estimates of the effects of Title I on both spending and outcomes and controls for additional unobservable characteristics with fixed effects. To our knowledge, only one other paper combines an RD design with panel data and fixed effects (Cellini, Ferreira and Rothstein, 2009).

Since Title I is the largest federal compensatory aid program to education, its effectiveness largely determines the success of federal spending in improving learning outcomes for poor children. The 2001 *No Child Left Behind* (NCLB) legislation relies on Title I funding to provide the federal resources for struggling schools. Further, the 2009 economic stimulus package channels a large portion of new education funds through Title I, making it especially important to understand the effectiveness of Title I in targeting funds and improving outcomes at this time.

Additionally, the implications of our findings reach far beyond the federal program itself. Most states also use compensatory aid programs to target additional resources to schools and districts with low achieving and/or high poverty children. Like Title I, few detailed programmatic or curricular specifications are attached.[3] Thus, analyses of Title I offer insights that may be relevant to these programs as well. Further, most school budgets leave little

[3] Title I is a funding mechanism rather than a specific program or policy aimed at educating poor students. In its early years of implementation, school districts assigned staff and services rather than unrestricted dollars to schools (Chambers et al., 1993; Goertz, 1988). The *Improving America's Schools Act* (*IASA*) of 1994 gave principals greater discretion over the use of their funds in return for greater accountability; NCLB further increased the accountability measures and added sanctions for schools failing to meet these measures. The only current requirement for how Title I funds are spent, in addition that they be spent for services to poor children, is that schools must set aside ten percent of their Title I dollars for professional development and one percent for parental involvement programs.

flexibility for principals in how money is spent. Funds from Title I and other compensatory spending programs, however, are often unrestricted as to type of inputs, providing principals with precious discretional dollars. If schools (and principals) know best how to educate their students and how to use funds most effectively, these resources should be quite productive and the dearth of evidence showing such productivity in previous studies is disappointing

Finally, understanding the effect of Title I funding on student performance will shed light on the possible impacts of new district finance proposals such as "Weighted Student Funding (WSF)".[4] As with Title I, WSF allocates money to schools based on the characteristics of their students, rather than allocating teacher positions, as historically is the case for intra-district funding. In NYC, WSF aimed to give principals substantial control over budgets, with few constraints on how money was used. Therefore, credible evidence on the effect of Title I on student performance will prove useful to federal and state policymakers and provide evidence relevant to changes in how schools are funded.

The findings presented in this paper are relevant to to other contexts, as its public schools educate children that are in many ways similar to urban school children elsewhere. Indeed, while some areas of NYC are quite unique (e.g., Times Square, Wall Street), there are large areas that are representative of urban settings around the country (e.g., Queens, Staten Island). More generally, a study of NYC schools and students provides valuable insight into the issues and constraints faced by large urban schools in other states. NYC is not as different from most other large, urban districts as is often thought. Recent research has shown striking similarities in intra-district resource distribution (Rubenstein et al., 2007), efficiency and racial test score gaps for example.

[4] Other large cities have adopted this system (Edmonton, Canada, Seattle, Cincinnati, and Houston) and the Fordham Foundation gathered a broad group of educators in support of such a school finance system in 2006 (Fordham Foundation, 2006).

The rest of this paper is organized as follows. First, we discuss key features of the Title I program, including how Title I funding is allocated, both nationally and locally, highlighting features critical to modeling the effects of the program. We then review previous literature on Title I funding and its impact on school spending and student achievement. Next, we describe our methods, followed by data, empirical results, discussion and further exploration of results, and, finally, conclusions.

2. Key Features of Title I

The Allocation of Title I Funds in General

The allocation of Title I funds from the federal government to local schools follows a complicated process. First, each year Congress sets the total amount of funds available for distribution by the U.S. Department of Education (USDOE) (Figure 1). USDOE then uses several grant formulas based on the number of poor children to determine each local education agency's (LEA) share. Basic grants provide all LEAs with an allocation per poor student, enabling almost all LEAs to be eligible for at least some funding. Other Title I formulas provide additional funds based on increasing numbers and percentages of poor school-age children, thereby giving large and high-poverty LEAs increasingly more money than small and low-poverty ones. The total LEA allocation flows through each state's Department of Education, which then distributes the monies to the LEAs. Finally, states can keep one percent of the total allocation for program administration to provide additional resources to schools in greatest need of improvement.

LEAs allocate funds to their schools as follows. First, LEAs set a school "Title I eligibility cutoff" for poverty, equal to the average child poverty rate in the district, below which

schools will receive no Title I funds. Then the LEA sets the Title I per-pupil amount of funds, which is the total district allocation divided by the number of poor students in schools above the Title I cutoff, including students from both public and non-public schools. Importantly, in eligible schools all poor students are eligible to receive services. Conversely, no students in ineligible schools are eligible to receive services, even if they are poor.[5]

According to Title I regulations, all schools with at least 75% poverty must receive funds and schools with less than 35% poverty cannot receive funds.[6] To reiterate, not all schools receive Title I money, even if there are poor students at the school.[7]

The Allocation of Title I Funds in New York City

Since 2000, NYC has spent over $12 billion dollars annually on public schools, with Title I funding providing approximately five percent of the total (see Table 1). The amount of Title I revenue NYC received increased between 1996-97 and 2003-04, with an especially large increase (from $474 million to $638 million) between 2001-02 and 2002-03. The new federal economic stimulus bill (ARRA) will send an additional $350 million to New York City in both FY2010 and FY 2011 (NYCDOE, 2009), making this is a good time to assess the impact of Title I on spending and performance.

---

[5] If an eligible school has a high enough concentration of poor students to be a "school wide program," and applies for and receives school-wide status, then all students, whether they are poor or not, are eligible to receive services. Since 1978, Title I schools have been able to adopt comprehensive schoolwide programs. In 1988, schools with a poverty rate of at least 75% where able to use Title I funds for these programs while currently schools with 40% or more poverty are able to have schoolwide programs. Nationally, the number of schoolwide programs has tripled between 1994-95 and 2004-05 (National Assessment of Title I). In New York City, the number of Title I schools approved for school-wide programs has also increased over time. Among elementary and middle schools, 40.5% in 1997 to 66.1% in 2003, among high schools 10.6% to 57.1%.

[6] www.ed.gov/policy/elsec/leg/esea02/pg1.html

[7] Some schools that do not have Title I status receive some funds for homeless students and those in foster care, regardless of the poverty rate at their school. These are not usually large because incidence of children with these characteristics is not large and most already attend schools that are Title I eligible. On average, Title I schools received approximately $722,120 in 2003, with the top 10% of Title I schools receiving over $1.1 million. Non-Title I schools, on average, received $7,870.

An important feature of NYC, relevant to the modeling of Title I impacts, is that the city allocates Title I funds to five counties (called boroughs in NYC[8]), all of which are within its borders.[9] This means that each borough receives a share of the city's Title I money in proportion to the poverty count in its own borough (as of the previous year). For most of the period of this study (through 2003), the school Title I poverty cutoffs were the same across all boroughs, resulting in a per-pupil Title I amount that *differed* by borough. (That is, the uniform cutoff makes the eligible children a higher proportion of poor children in some boroughs compared to others.)[10] For example, while the 2002-03 citywide per-pupil Title I amount was $948.17, it varied from a low of $763 per child in the Bronx to a high of $2,194 per child in Staten Island (NYCDOE, 2002). In terms of modeling impacts of Title I, this feature means that controls for boroughs or schools may be needed.

NYC, like other school districts, also receives compensatory education funds other than Title I. New York State provides Pupils with Compensatory Needs (PCEN) funds, which are intended for programs for poorly performing students, particularly those students who are English Language Learners (ELL), regardless of poverty status. While the total amount available to NYC is less than half that of Title I, PCEN may still have an impact on the amount of total compensatory funds available for schools (see Table 1 for comparisons between Title I and

[8] Manhattan, Brooklyn, Bronx, Staten Island and Queens.
[9] Prior to 1999, all Title I funds were allocated to counties rather than individual LEAs. The allocation process was changed so that high-poverty LEAs in low-poverty counties received a more equitable amount of funding (Natriello & Dill, 1999). NYC, however, differs from other LEAs in that it consists of five counties embedded within one school district instead of a number of LEAs embedded within one county. Even though it is no longer required to do so, NYC continues to allocate funds based on counties
[10] Even after 2003, when the cutoffs by boroughs differ, the per pupil amounts also differ since the cutoffs were not set to make them equal.

PCEN funding amounts) and thus it is necessary to look at this other compensatory aid in order to understand patterns of total resources available to improve outcomes.[11]

3. Previous Literature on the Effects of Title I on Spending and Outcomes

In this section we first review the literature on the impact of Title I funding on school resources and then on student achievement.

Impact on School Resources

From the early authorizations, ESEA legislated that state and local funds must be comparable between Title I funded and non-Title I funded schools and that Title I funds must supplement not supplant state and local funds.[12] Enforcement is limited by the difficulty of identifying the amount a school would have received in the absence of Title I funding. Interestingly, although economic theory suggests that local school districts would fully substitute intergovernmental funding for their own, there is substantial evidence of a "flypaper" effect, whereby funds from a higher level of government "stick" where they are allocated (that is overall spending increases). (See Hines and Thaler, 1995; Fisher and Papke, 2000; and Knight, 2002 for a review of theory and empirical results on the flypaper effect.) Thus, understanding whether (and how much) intergovernmental aid increases spending is an empirical question.

[11] Anecdotal evidence suggests that PCEN funds in New York City have been used to "compensate schools with insufficient numbers of low-income students to be eligible for Title I funds with additional PCEN funding." See correspondence between Noreen Connell, Executive Director of Educational Priorities Panel and Beverly Donohue, Chief Financial Officer, New York City Board of Education, April 9, 1999, www.edpriorities.org/Pubs/Opinion/Letters99_PCEN.4.9_Print.html, accessed October 12, 2006.

[12] http://www.ed.gov/legislation/ESEA/sec1120a.html. SEC. 1120A. FISCAL REQUIREMENTS. (A) Except as provided in subparagraph (B), a State or local educational agency shall use funds received under this part only to supplement the amount of funds that would, in the absence of such Federal funds, be made available from non-Federal sources for the education of pupils participating in programs assisted under this part, and not to supplant such funds.

Several researchers have employed quasi-experimental designs to disentangle the causal impact of intergovernmental aid on districts and schools. Gordon (2004), using a national sample of school districts, took advantage of a natural experiment arising from a switch in the basis for eligibility for Title I funding from Census poverty data from 1980 to 1990. Updated Census numbers provided an exogenous change in federal revenue for a large number of districts in the United States. Gordon found that in the short run spending increased with almost no offsetting reductions in state or local funds. Over the longer term, however, spending remained flat due mostly to local offsetting responses. That said, Gordon did not examine the different impacts on schools within large districts. Her results imply, however, that in the short run within districts, Title I schools may receive additional funds with no reductions in funds for non-Title I schools, but in the long run when district spending is flat, net school spending should not increase.

Two studies focus on the relationship between Title I and total *school* spending -- that is at the school rather than the district level -- using a regression discontinuity (RD) design. Van der Klaauw (2008) used cross-sectional data from three years of NYC elementary and middle schools and found no effect on school-level spending. Van der Klaauw, however, was limited to three non-continuous years of data between 1993-94 and 2000-01 and did not use panel data methods. Further, during one of those years (1993-94), non-Title I schools received money for poorly performing Title I eligible students, therefore interfering with the regression discontinuity design, which in its purest form, is based on a clear cutoff for eligibility that denies non-Title I eligible schools any money.

Matsudaira *et. al* (2006) also used an RD design to study the impact of Title I funding in a large northeastern city. They find that while federally funded revenues increased in Title I

compared to non-Title I schools, total direct expenditures did not differ. Their data were limited to two years (2001 and 2002) and did not use longitudinal methods to address potential unobserved differences between schools. In addition neither Van der Klaauw nor Matsudaira included high schools.

Impact on Student Academic Performance

Evidence on the effectiveness of Title I funding in improving student academic performance has come primarily from two large congressionally mandated studies, one meta-analysis of seventeen studies, and the two regression discontinuity studies citied previously. The first congressionally mandated study of Title I, "Sustaining Effects," included five separate analyses, one of which traced students longitudinally for three years between 1976 and 1979 (Carter, 1984). Overall students in Title I schools were found to achieve more than similar students in non-Title I schools -- more in math than in reading and more in elementary than middle grades. Further, very low achieving students in Title I schools achieved less and did not advance enough to reach parity with "regular" students (Carter, pp 6-7).

The second Congressionally mandated study, "Prospects," was conducted over four years from 1991 to 1994 (Borman, D'Agostino, Wong, and Hedges, 1998). This analysis of achievement growth of Title I students compared to non-participants found mixed evidence on achievement. When more growth in achievement for Title I students was found, consistent with the results from Sustaining Effects, it was for students who were initially in need of less remediation.

Borman and D'Agostino's (1996) meta-analysis of seventeen evaluation studies commissioned or performed by the federal government (including Sustaining Effects and

Prospects) found a "modest" impact for students in Title I schools, with an increasingly positive impact as programs were in existence longer. That said, none of these studies claimed to have found a causal effect of Title I. Although they constructed and reported on comparison groups, the authors were clear that unobserved differences between the comparison students and the students in Title I schools could not be ruled out. As discussed by Borman and D'Agostino (1996), the impact of Title I varies by program implementation, time of year the test is taken, grade level, and other unobservable differences. For example, since districts set different cutoffs for Title I school eligibility, national studies compare students attending schools with varying levels of poor students. Finally, the two previously discussed regression discontinuity studies also found no school-level performance effect in either reading or math (Van der Klaauw, 2008; Matsudaira *et. al*, 2006).

Thus, while the effects of Title I funding on student academic performance have been studied intensively, shortcomings in methods or data have limited their ability to demonstrate causal effects for schools and for more recently reauthorized versions of Title I, specifically NCLB.

This study seeks to improve on these previous studies by using seven years of school-level data and longitudinal quasi-experimental methods to estimate impacts. Thus we are better able to estimate credible causal effects. Additionally, in contrast to the studies reviewed, we examine the impact of Title I spending on high schools. Data on the impact of Title I at the high school level is especially important in light of the next reauthorization of NCLB, which will likely include accountability measures for high schools.

4. Methods

A key challenge in estimating the effects of Title I on school spending or outcomes is that Title I eligible schools differ from non-Title I schools in numerous ways. Some of these differences are known to determine spending and outcomes (for example, following Rubenstein *et al.,* 2006, size of school and percentages of special needs and poor students) and can be included as covariates in models. Nonetheless, there are likely to remain significant unobserved differences that could also determine spending and outcomes.

We use an RD design to obtain unbiased estimates. Our RD model exploits an important feature of the Title I program – the use by school districts of a poverty cut-off rate to determine whether schools are eligible for Title I funding. Specifically, school *s* is eligible for Title I funding in year *t* if its poverty rate (p_{st}), which is measured as the previous year's school rate, is greater than or equal to the district's poverty cut-off rate for that year, p_t. If $p_{st} < p_t$, the school is not eligible. This discontinuity in the funding eligibility creates a quasi-experiment whereby small differences in the school poverty rate affect eligibility but are unlikely to reflect significant other observed or unobserved differences in the schools. Because a school's poverty rate for Title I is measured by its previous year's actual rate, gaming to move that poverty rate is unlikely.

There are several ways to implement an RD design. We use a parametric procedure that employs data on all schools and years (see Hahn, Todd, and van der Klaauw, 2001, for presentation of alternative methods). School (or borough) fixed effects control further for unobserved but time invariant, school characteristics. This use of school effects also allows us to estimate impacts on schools that change Title I eligible, becoming newly (in) eligible over time.

We estimate the following equation:

$$Y_{st} = \beta_0 + \beta_1 TitleI_{st} + g(p_{st}) + \beta_2 X_{st} + \beta_3 Z_{st} + \theta_s + \Phi_t + u_{st} \quad (1)$$

where s indexes schools and t indexes time. Y_{st} is the outcome of interest, for example spending on direct services or math achievement, $TitleI_{st}$ is a dummy variable that takes a value of 1 if school s is Title I eligible in year t and 0 if it is not. Thus it identifies the discontinuity in Title I funding. $g(p_{st})$ is a polynomial function of the percent poverty (p_{st}), where the percent is centered on the Title I poverty eligibility cutoff for the year, and the polynomial is allowed to differ on either side of the cutoff. This polynomial absorbs variation from schools far from the eligibility cutoff. (See Imbens and Lemieux, 2008; Cellini *et* al, 2009, for explanations and use of this techniques).

In this model, the coefficient on $TitleI_{st}$ (β_1) captures the difference in outcomes at the discontinuity and thus provides a direct estimate of the local average effect of Title I. X_{st} is a set of school-level student characteristics such as the percentage of students who are eligible for English language learning programs, Z_{st} is a set of school characteristics, including size, θ_s is a school or borough fixed effect, Φ_t is a set of year fixed effects, and u_{st} is a random error term.[13] In the elementary/middle school models of academic outcomes, which are measured for grades 3 through 8, grade effects are also included.

In summary, this study is designed to provide unbiased estimates of the local average treatment effects of Title I eligibility. It adds to the literature not only in the results but also by applying the method to longitudinal data, which permit the addition of school fixed effects to better control for unobserved difference. Further, one interpretation is that the school fixed effects allow comparison between schools that change status while the borough effects illustrate effects for schools that mostly remain either Title I or not.

Is the RD Appropriate?

There are a number of conditions that must be met before a causal effect of Title I status on outcomes can be inferred from the estimates that are produced by the RD design. First, there should be a truly discontinuous jump in the Title I status at the poverty cutoff. Second, the other determinants of the outcome (X's and Z's) should be the same in a close band below and above the cutoff, indicating that there are no other close discontinuities. Third, the outcome should be a continuous function of the poverty at the Title I cutoff. Finally, the schools should not be able to manipulate their Title I status, which means that the density of percent poverty above and below the cutoff should be similar. These conditions are addressed below.

Figure 2 displays one year of elementary and middle schools receipt of Title I funds graphed against the eligibility cutoff for funds. The cutoff produces a "sharp" discontinuity, with no misclassifications. This holds for all years, save one in which three schools below the cutoff were classified as eligible (less than .4% of the schools) and for all years of our high school sample.

Table 2 presents descriptives for schools within five percentage points of poverty of the Title I eligibility cutoffs in selected years for elementary and middle schools (panel A) and for high schools (panel B). Statistically significant (at 5% or better) between Title I and non-Title I schools are bolded. There are few differences and, when they occur, only in a few years, indicating similarity in observed features of schools close to the cutoff. As expected the percent poverty does differ, since the eligibility is based on a sharp cutoff in this variable.

Figure 3 shows the relationship between the percent poverty (and Title I cutoff) and direct expenditures per pupil for one year, illustrating the continuous nature of this outcome at the cutoff.

Finally, as already pointed out, a school's eligibility is based on its previous year's poverty level and the district poverty cutoff for eligibility is based on this year's average poverty, which makes it unlikely that schools will be able to manipulate their status.

5. Data

We use school-level data from 1996-97 through 2003-04 obtained from three NYCDOE. sources: the *Annual School Reports* (*ASRs*), the *School-Based Expenditure Reports (SBERs)*, and the *BOR1 Allocation Memorandum*. *ASRs* are comprehensive reports containing school-level student and teacher characteristics, test results, and four-year graduation and dropout rates. *SBERs* provide school-level spending and are disaggregated in a variety of ways.[14] BOR1 provides data on Title I status of the school (i.e. whether NYC classifies the school as a Title I school) and the percent poverty on which eligibility is based. Notice that status and eligibility need not match since some schools are grandfathered. A total of 940 elementary and middle schools (through grade 8) are in the database, with 776 of them having the crucial BOR1 data on Title I status for all years in the analysis. The high school panel has complete data for 167 schools, the majority of regular high schools operating for more than four years as of 2001-02.[15]

For elementary and middle schools, we use data for 1996-97 through 2002-03, during which the only change in criteria for Title I eligibility in New York was in the citywide

[14] These are organized by functional areas (for example, direct versus classroom instruction), program (for example distinguishing special education versus general education programs), and location (for example, by borough).
[15] For two reasons we exclude schools designated solely for full-time special education students (i.e., the school for deaf children). First, many of these students were not required to take citywide or state standardized tests during much of the study period. Second, many of these schools do not receive Title I funds since the delivery of specialized student services are prescribed in each student's Individualized Education Program and are paid for out of other funding sources. We do, however, include special education students attending public schools with general education students.

eligibility cutoff.[16] In particular, eligibility cutoffs in NYC's five boroughs were the same. For high schools, our analysis uses only three years of data, 2001-02 through 2003-04, because high schools were often grandfathered into Title I status even after their poverty levels should have made them ineligible before these years. Thus the discontinuity was not at all sharp. In 2001-02, however, grandfathering stopped. Although a very small number of high schools were misclassified as Title I (in)eligible, the number was too small to be substantively meaningful and they were removed following sensitivity analyses.[17] We include 2003-04, even though Title I cutoffs differed by borough beginning in that year, in order to have enough years for estimation, and we modify the estimation equation appropriately as explained in the results section.

Table 3 shows the poverty eligibility cutoff and the distribution of Title I and non-Title I schools by year. Between 1997 and 2003, the eligibility cutoff ranged from a low of 62% in 2002-03 to a high of 68.36% in 2000-01 and, in any given year, over 70% of the elementary and middle schools (EMS) in New York City were eligible for Title I funding. The lowest percentage of Title I eligible schools included in our EMS sample is 71.8% in 2001-02, when the eligibility cutoff was the highest, and the highest percentage is 76.8% in 2002-03, when the eligibility cutoff was 62%. Compared to the EMS sample, fewer high schools are Title I eligible. In 2003-04, 54.6% of high schools were Title I eligible, but this varies considerably by borough. Staten Island is not included in the analyses since only three of its schools were ever funded in the period.

[16] We do not include 2003-04 because cutoffs differed by borough and we do not need additional years to estimate the effect of Title I eligibility well.

[17] One of the misclassified schools was in the data twice; a few of the ineligibles that were classified in the BOR1 as "Title I" were spending very low amounts of Title I funds (less that $80 per pupil, which is way below the average of around $500 per pupil) and thus most likely "mistakes." Models that included these misclassified schools yielded the qualitatively similar results.

Among the EMS sample, 12.4% of schools changed Title I status at any time during the seven-year study period, while 19.3% of high schools did so, thus making estimation with school fixed effects possible (Table 4). Of these changes, 53 (14) EMS (high schools) changed more than once.

6. Results

In this section, we first present results for how Title I affects expenditures. We measure expenditures as the per-pupil spending on direct services (money spent on classroom instruction, support services, and school administration) and revenues in two categories: per-pupil Title I funds and per-pupil PCEN funds (the state compensatory education program). Thus, we aim to determine whether direct expenditures increase with Title I and then to explore whether state compensatory funds substitute for Title I funds in schools below the cutoff.[18] Next, we present performance analyses. Performance for elementary and middle schools is measured by test scores in grade 3 through 8. These scores are reported as standardized grade by year "z" scores, where to obtain grade/year scores, individual student scores for each grade are subtracted from each grade/year mean score and divided by the system wide standard deviation. Thus, by construction, system wide grade z scores in each year have a mean of zero and a standard deviation of one.[19] Performance in high school is measured by the percent of students in a high school cohort that begins together in 9th grade and that alternatively graduated in four years or dropped out. These data are based on NYC DOE reports that track entering 9th graders over four years.

[18] We also estimated the same models with total expenditures and found qualitatively similar results. Direct expenditures are most relevant because they are used in the schools themselves rather than for system wide functions.

[19] The student-level data, aggregated to the school-grade level, come from a dataset developed by two of the authors for use in previous studies. See Schwartz, A.E., L. Stiefel, and D.Y. Kim (2004).

Complete regressions are shown for the first set of spending and outcome results for elementary and middle schools, followed by coefficients on the Title I dummy alone for high schools. Complete results for all models are available from authors.

Per-Pupil Spending and Revenues

Table 5 presents results examining the impact of Title I eligibility on per-pupil expenditures and revenues in elementary and middle schools based on a cubic polynomial in percent poverty.[20] Coefficients on all variables except middle school and year effects are shown. The year effects indicate a continual increase in funding over the years, *ceteris paribus* on other variables. Finally, model 1 (columns 1, 3, 5) includes borough fixed effects while model 2 (column 2, 4, 6) includes school fixed effects.

To begin, most of the coefficients on school characteristics are consistent with theory and previous work. Larger school enrollment is related to lower spending per pupil, while larger percentages of English language learners and of special education students result in higher spending per pupil or have no effect. Increases in recent immigrants generally have a negative effect or no effect on spending.

Turning to the effect of Title I on direct expenditures, model 2, which best controls for unobservables, shows that there is an increase in spending of approximately $284 per pupil. This is not a large effect especially since NYC receives about $500 per pupil in Title I funds (see Table 1). In the alternate specification (model 1), which uses borough effects, we see no statistically significant increase in expenditures as a result of Title I. Why don't Title I schools have higher direct expenditures?

[20] Results of all degrees of polynomials are qualitatively similar (and available from authors), although the level of significance of the effect at the discontinuity differs slightly between specifications. That is, in the quadratic and linear (but not quartic) specifications, the effect of Title I eligibility on direct expenditures, with borough effects, becomes statistically significant. The quartic and cubic specifications control better for the effect of poverty around the cutoff and thus we prefer them.

Perhaps the answer lies in the impact of Title I on Title I revenues and state PCEN compensatory funds. On the one hand, with respect to Title I revenues themselves, as seen in models 1 and 2 (columns 3 and 4), the difference between Title I and non-Title I schools is, on average, between $478 and $508 per pupil. This amount is around the average per pupil Title I funding shown in Table 1 over the period. Thus, Title I revenues appear to increase in Title I schools. On the other hand, as shown in models 1 and 2 for PCEN funds (columns 5 and 6), Title I *decreases* PCEN funding in Title I schools compared to ineligible ones by an average between $119 and $127. This suggests that part of the reason that direct expenditures do not increase more is that they are supplanted by PCEN funds.

Results for high schools are markedly different. As shown in Table 6, high schools that are eligible for Title I funding spend approximately $600 to $650 more on direct expenditure than do ineligible schools. As with EMS, they receive more in Title I funds (between $464 and $488 on average), however they do not receive lower amounts of PCEN funding,[21] suggesting the interesting finding that there is not supplanting at the high school level.

Summary of Findings on Spending and Revenues

The above analyses give us consistent estimates for elementary and middle schools that show that Title I status contributes to increased per-pupil direct expenditures for those schools that change Title I eligibility over time. We also find that while Title I funds reach EMS schools targeted by the legislation, more PCEN funding goes to schools that are not eligible to receive

[21] In the high school panel, in one set of estimations, the degree of the polynomial makes a difference to results – for polynomials of cubic form, the direct expenditures do not increase in either with no effects or with school effects regressions, at a statistically significant level of 10% or less.

Title I funds, implying supplanting. While the amount of supplanting is not large enough to offset the total amounts available through Title I, it is not trivial either.

High schools are different, however. Direct expenditures and Title I funds increase in eligible schools, but PCEN funds do not change. Thus, there is no evidence of supplanting of state funds for federal funds and Title I increases direct expenditures considerably more in high schools than in lower level schools.

Math and Reading Performance in Elementary and Middle Schools

Turning to the estimates of the impact of Title I status on student performance, we first analyze school-level math and reading test scores for elementary and middle schools, averaged across all grades that a school serves. We include controls similar to the ones in the spending equations plus additional variables to reflect more student characteristics, consistent with the education production function literature. Table 7 presents these results.

Again note that coefficients on control variables are consistent with most literature: *ceteris paribus*, scores increase as the percentage of students that are Asian and female increases, while scores decrease as the percentage of black, Hispanic, English language learners, and special education students increases.

As to the effect of Title I, although none of the coefficients on the Title I eligibility dummy is significant at the 5% level, a number are negative and significant at the 10% level, indicating that there may be a slight negative local average treatment effect in the range of -.03 to -.04 standard deviations. While these findings are disturbing, there may be several reasons why we see these negative impacts.

First, we found that Title I eligible elementary and middle schools do not receive a large increase in funds, compared to non-Title I eligible schools. Therefore, the funds that these schools do receive may not be enough to provide the services required to increase the test scores.

Second, we do not know how Title I schools are serving eligible students. Some schools close to the cutoff may provide services only to eligible students (pull-out programs) while others may provide services to all students in the school, regardless of eligibility. Lastly, some schools may change Title I status over time and may receive different amounts of funds based on the length of time they are Title I. We address this below.

High School Outcomes

Table 8 presents results for high school outcomes. The effects of Title I funding are uniformly zero. At best, one could say that the funding and increase in direct expenditures that follow Title I do not harm students, but they do not help them either.

7. Further Exploration of Results – The Timing of Impacts

The spending results for elementary and middle schools hint strongly at some reduction of state and local funds (PCEN funds in particular) in schools that are Title I eligible, but with an effect on direct expenditures *not* felt in schools that are newly Title I eligible. That is, in the fixed effects regressions, Title I results in higher direct expenditures, perhaps indicating that schools that change status (the newly eligible schools) receive more at first, but that over time they see reductions. To explore this further, we created new dummy variables that define the patterns of eligibility over the years in the panel and modified expenditure and test score regressions. In Table 9, FirstTI indicates the first year that a school becomes Title I eligible;

TwoTI indicates that a school is in its second or higher years of Title I eligibility; FirstnotTI indicates the first year a school exits from Title I eligibility; TwonotTI (omitted) indicates the second or future years that a school is ineligible. Here we use only six years of data to be able to determine cleanly the first year of (in) eligibility. Further we use only a linear variable in percent poverty to preserve degrees of freedom. In effect, these newly created variables split the Title I categories into two parts – the newly eligible (ineligible) and the longer eligible (ineligible). As shown in Table 9, direct expenditures are higher in the second and future years than in the first, although still significantly lower than the additional Title I funding of around $500. Title I funding also is higher in the years after the first and declines a great deal the first year that a school becomes ineligible. Finally, PCEN funding declines with Title I eligibility and more after the first year. Thus, these regressions indicate a larger impact in years beyond the first rather than a diminished effect as time goes on.

The Title I elementary and middle schools and to a greater extent the high schools spend more on direct services than their non-eligible counterparts. What do these dollars buy? To explore this question, we regressed three real resource variables on the Title I dummy. The three resource variables are the pupil-teacher ratio, the percent of teachers with masters' degrees and the percent of teachers with more than five years of experience teaching. The pupil- teacher ratio captures the teacher resources available in the school, although this ratio is generally smaller than class size because teachers may tutor students or provide other help outside the classroom. The percent of teachers with masters' degrees measures additional education although it may not correlate well with increased student performance. There is, however, some research that shows that teachers with little experience do worse at increasing student performance than ones with more (Rockoff, 2004; Rivkin, *et al,* 2005; Clotfelter, *et al, 2007*) and thus the variable indicating

more than five years of experience is meaningful. Table 10 displays the coefficients for the Title I eligibility dummy for elementary and middle schools (panel A) and for high schools (panel B). There is little evidence of any effect on these resources for elementary and middle schools, although the percent masters' shows some minor negative effects for Title I schools at around the 10% significance level. For high schools, however, pupil-teacher ratios are lower by a little over one student at just above the 5% significance level, but also teachers with over five years of experience are considerably lower in Title I schools. These results makes some sense given that the Title I high schools are estimated to spend around $600 per pupil more than the ineligible ones, an amount considerably higher than the elementary and middle schools, for which they obtain more teacher resources, but ones with less experience.

In light of somewhat higher spending, but not too much effect on the measured real resources, we explore further the (slight) negative results for test scores in elementary and middle schools. To do this, we re-estimate the score equations with the new Title I variables created to indicate first and future years of (in) eligibility. Table 11 shows these results. Here we see some indication (in the borough effects regressions only) that the negative effects show up after the first year and again in the first year that schools are not Title I eligible. In yet another specification shown in columns five and six, with the Title I eligibility dummy and dummies for schools that are always or never (with sometimes omitted) Title I, we see that the ones that go in and out of Title I do somewhat better (that is, always and never do worse).

These results indicate that the pattern of eligibility over time has an effect, but it is not consistent with the idea that the latter years see a lessening of funds or of the negative scores, as we hypothesized might be the case from the RD results. In addition, while Title I eligible high

schools show some improvement in their pupil-teacher ratios, they also have fewer experienced teachers.

8. Conclusions and Discussion

This paper reports the results of a study of the impact of Title I on school spending and student achievement in New York City. One of the primary differences between this study and those done previously is that we use of panel data over a seven-year period that has consistent program implementation instead of single years of data and years in which program requirements change. We believe that the ability to use longitudinal data and school fixed effects improves knowledge about expenditures based on Title I status. In addition, we are able to analyze results for high schools, also over time.

Overall, the results indicate that Title I brings increased federal education funds to schools and that these funds enable high schools close to the Title I eligibility cutoff to significantly increase the amount of money spent on direct services to students and to improve their pupil-teacher ratios (while having fewer experienced teachers). Elementary and middle schools do not increase funds as much, which is consistent with our finding that New York City may be using PCEN funds to supplant some of the Title I funding for elementary and middle schools that do not have poverty rates higher than the eligibility cutoff. Since schools just below the Title I cutoff are similar to those just above the cutoff, this finding may be an equitable, albeit unintended, result.

Additional amounts of funds that Title I schools receive, compared to non-Title I schools, seen not to narrow the achievement gaps between poor student and their more advantaged peers and may even reduce school wide average test scores somewhat in elementary and middle

schools. These effects for both spending and scores seem to increase the longer schools are Title I and to be stronger for ones that are always Title I versus ones that go in and out of eligibility.

These findings are important and they also lead to several hypotheses that need to be tested in order to understand why they occur. The results in the RD estimations indicate the local average treatment effect, or what is happening right around the cutoff, but we do not know how the schools near the cutoffs are serving the Title I students. There are informal reports that a common use of Title I funds is to hire extra personnel to tutor students who are lagging. But this tutoring could remove the students from their regular classroom causing them to miss material or be stigmatized or both. Or, alternatively, since we are measuring school-wide scores, perhaps the served Title I students in schools right above the cutoff are doing better than their unserved classmates, but the school average is dominated by the unserved students. To explore such hypotheses, student level, longitudinal analyses would be required.

Another hypothesis is that for schools near the cutoff, the administrative burden of accounting for and using the extra Title I funding is large relative to the benefits. Strings may be attached, such as making sure only Title I eligible students are served, that make the money ineffective. In addition, in elementary and middle schools it is clear that PCEN funds go down so that the net amount of additional funding is not great.

Perhaps when schools have higher percents of poverty (further from the cutoff) they receive more funding or have more freedom (more likely to be school wide programs?), and it is the ones near the cutoff that suffer most.

Finally, the performance effects over time need to be further explored. For example, the evaluation literature almost always cautions against looking for effects in short time periods. Some say it may take between five and 10 years to turn around a school (Darling-Hammond,

1988; Haynes, 1998; Hess, 1995; Levin, 1991).Thus the specific path and adjustment process over a number of years may be important and differ from that found looking at one year and all other years out as we did in Table 11. In addition, on the spending side, the federal government may restrict how Title I funds are used, but these restrictions may contradict the plan that a district has for funding its schools. For example, a district may wish to distribute funds according to student characteristics without a sharp cutoff (making the reduction in PCEN funds seem reasonable) or may even have a delivery plan that differs from the Title I restrictions that require the school to serve only eligible students with the new funds.

What we have found is that around the Title I cutoff, there is significant substitution of funds in schools that miss being Title I eligible, thus blunting the net amount received by the eligible schools. In addition, at best, just above the cutoff there is no effect on performance (high schools) and possibly even a negative effect for elementary and middle schools. These results are consistent with other studies of districts that find reduction of Title I funds over the years and are not inconsistent with all the Title I performance evaluations that rarely find any effect on student performance. Still there are a number of hypotheses about why these results occur that could be explored with longitudinal student data but are for future work.

References

Borman, G. D., J. V. D'Agostino, K. K. Wong, et al (1998). "The Longitudinal Achievement of Chapter 1 Students: Preliminary Evidence from the Prospects Study." *Journal of Education for Students Placed At Risk*, 3(4) 363-399.

Borman, G., & J. V. D'Agostino (1996). Title I and Student Achievement: A meta-analysis of federal evaluation results. *Educational Evaluation and Policy Analysis, 18*(4), 309-326.

Brown, C. A. (2005). *Federal Title I Funding Distribution: Are the poorest children receiving their share of the funds?* Paper presented at the American Educational Finance Association, Louisville, KY.

Carter, L. (1984). "The Sustaining Effects Study of Compensatory and Elementary Education," Educational Researcher 13(7), 1984, 4-13.

Cellini, Stephanie R., Fernando Ferreira and Jesse Rothstein (forthcoming). "The Value of School Facility Investments: Evidence from a Dynamic Regression Discontinuity Design," *Quarterly Journal of Economics.*

Chambers, J. C., T. Parrish, & M.E. Goertz (1993). *Chapter 1 resources--Are they truly supplemental?* Paper presented at the annual meeting of the American Education Finance Association.

Clotfelter, C. T., H. F. Ladd & J. L. Vigdor (2007). "How and Why Do Teacher Credentials Matter for Student Achievement?" *NBER Working Paper No. W12828*

Darling-Hammond, L. (1988). Policy and professionalism. In A. Lieberman (Ed.), *Building a professional culture in schools* (pp. 57–77). New York: Teachers College Press.

Fisher, R. C. & L. E. Papke (2000). "Local Government Responses to Education Grants," *National Tax Journal*, 53 (1):153-168.

Goertz, M. E. (1988). *School districts' allocation of Chapter 1 resources.* ETS RR-88-16. Princeton, NJ: Educational Testing Service.

Gordon, N. (2004). "Do Federal Funds Boost School Spending? Evidence from Title I," Journal of Public Economics 88(9-10):1771-1792.

Hahn, J., P.Todd, and W. Van der Klaauw (2001). "Identification and estimation of treatment effects with a regression-discontinuity Design," *Econometrica,* 69(1):201-209.

Haynes, N. M. (1998). Lessons learned. *Journal of Education for Students Placed at Risk, 3*(1), 87–99.

Hess, Jr., A. G. (1995). *Restructuring urban schools: A Chicago perspective.* New York: Teachers College Press.

Hines, J. R. and R. H. Thaler (1995). "Anomalies: The Flypaper Effect," *Journal of Economic Perspectives*, 9(4):217-226.

Imbens, G. and T. Lemieux (2008). " Regression discontinuity designs: A guide to practice," *Journal of Econometrics.*

Knight, B. (2002). "Endogenous Federal Grants and Crowd-out of State Government Spending: Theory and Evidence from the Federal Highway Aid Program," *The American Economics Review* 92(1):71-92.

Levin, H. M. (1991). *Learning from Accelerated Schools.* Philadelphia: Pew Charitable Trusts.

Matsudaira, J. D., A. Hosek, and E. Walsh (2006). "An Integrated Assessment of the Effects of Title I on School Behavior, Resources, and Student Achievement." Presented at the American Education Finance Association Conference, March 24-26, Denver, Colorado.

Natriello, G. and E. L. McDill (1999). "Title I: From Funding Mechanism to Educational Program," in The Harvard Civil Rights Project, *Hard Work for Good Schools: Facts Not Fads in Title I Reform*, Cambridge, MA: Harvard University.

National Assessment of Title I Interim Report: Executive Summary (2006). Washington DC: U.S. Department of Education, Institute of Education Sciences.

New York City Department of Education, 2009 School Allocation Memorandum 5/20/2009.

New York City Department of Education. *BOR Allocation Memo #1 FY 2001 Initial Allocations Tax Levy and Reimbursable*. New York City Department of Education. Available: http://www.nycenet.edu/offices/d_chanc_oper/budget/DBOR/AllocationMemo/FY02-03/NumberMemo/03BORno1/bor1.html [2004, December].

Nichols, Austin (2007). "Causal Inference with Observational Data," http://pped.org/stata/ciwod.pdf

Porter, J. R., (2003). "Estimation in the Regression Discontinuity Model," Harvard University, manuscript.

Rivkin, S. G., E. A. Hanushek, & J. F. Kain (2005). "Teachers, Schools, and Academic Achievement," *Econometrica*, 73(2): 417-458.

Rockoff, J. (2004). "The Impact of Individual Teachers on Student Achievement: Evidence from Panel Data," *The American Economic Review,* 94(2):247-252.

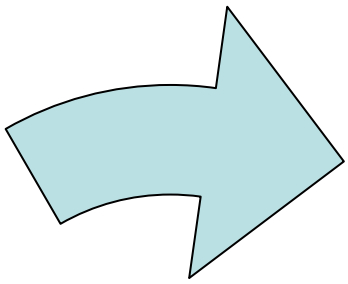
Rubenstein, R., L. Stiefel, A. E. Schwartz et al (2004). "Distinguishing Good Schools from Bad in Principle and Practice." In *Developments in School Finance 2003*, NCES, pp. 53-70.

Rubenstein, R., A. E. Schwartz, L. Stiefel and H.B. H. Amor (2007). "From Districts to Schools: the Distribution of Resources across Schools in Big City School Districts," *Economics of Education Review*, 26: 532-545.

Schwartz A. E., L. Stiefel, & D. Y. Kim, (2004). "The Impact of School Reform on Student Performance: Evidence from the New York Network for School Renewal Project." *Journal of Human Resources*. 39(2): 500-522

Van der Klaauw, W. (2008). "Breaking the link between poverty and low student achievement: An evaluation of Title I," *Journal of Econometrics*. 142(2): 271-309

Figure 1: How Title I Funds are Allocated

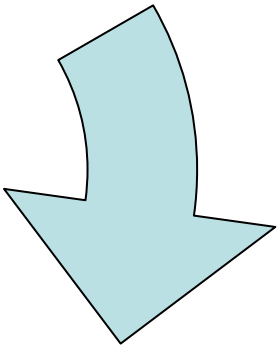


Step 1

U.S. Department of Education
Funds are allocated to local education agencies (LEAs) based on the numbers of poor children, whose count is obtained from the Census Bureau.

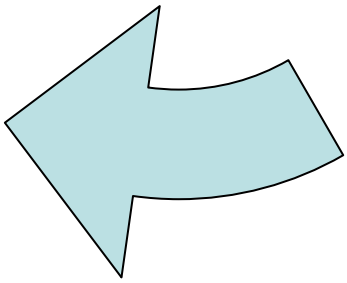
Step 2

States
Individual LEA allocations are totaled and then allocated to each state Department of Education. A state can keep up to 1% of the total state allocation for administration of the Title I programs plus for schools in need of improvement



Step 3

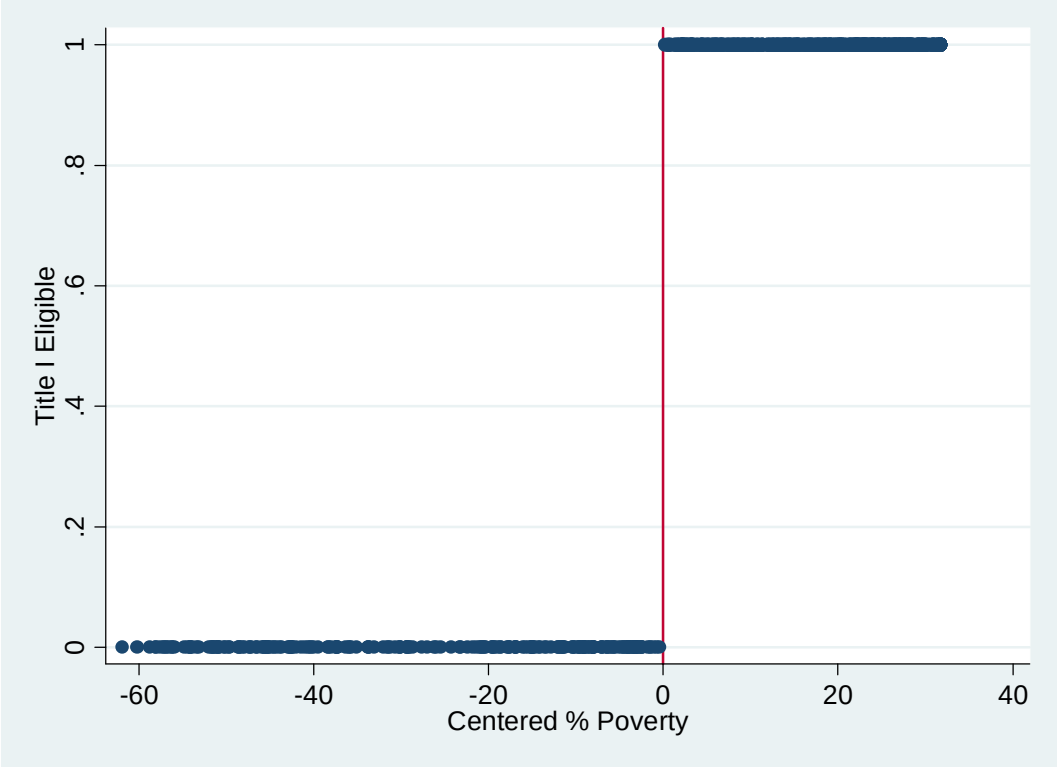
Local Education Agencies
The allocation to an LEA is based on the same or more recent counts of child poverty used in Step 1, minus any state administrative costs.



Step 4

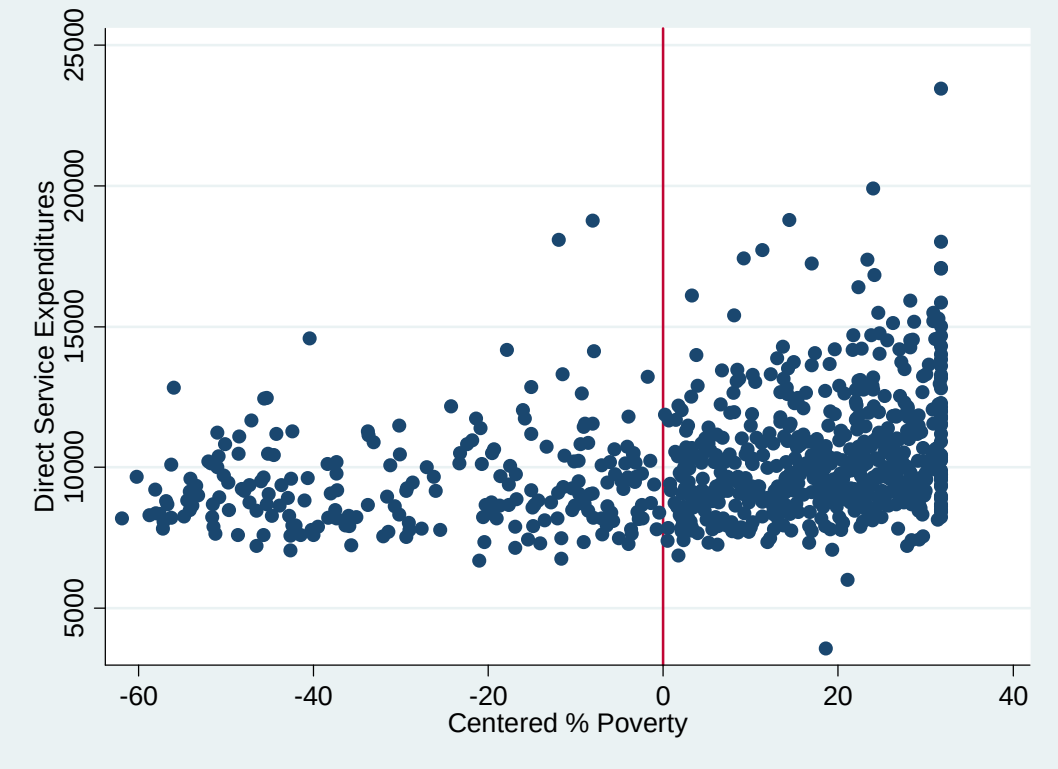
Schools
LEAs then allocate Title I funds to individual schools. All schools with at least 75% poverty must receive money, but districts have the discretion of setting the cutoff for Title I status as low as 35%.

Figure 2: Probability of Receiving Title I funds versus New York City Poverty Cutoff for Title I (centered on zero), Elementary and Middle Schools, 2001-02



Notes:

Figure 3: Direct Expenditures per Pupil and Title I Poverty Cutoff (centered on zero), Elementary and Middle Schools, 2001-02



Notes and draw line though points

Table 1: Total Spending and Compensatory Education Revenues in New York City, 1996-97 through 2003-04

	Total Spending		Title I Funds			PCEN Funds		
	Real $ (in billions)	Per-pupil $	Real $ (in millions)	% Total Spent	Per-pupil ($)	Real $ (in millions)	% Total Spent	Per-pupil ($)
1996-97	12.01	11,232	543.45	4.5	506	369.34	3.1	343
1997-98	12.52	11,790	561.76	4.7	557	305.83	2.4	280
1998-99	13.01	11,951	501.17	4.1	492	291.91	2.3	274
1999-00	13.32	12,105	474.66	3.6	431	295.41	2.2	269
2000-01	13.67	12,365	467.21	3.4	422	286.80	2.1	260
2001-02	13.39	12,186	474.06	3.5	432	286.39	2.1	261
2002-03	13.86	12,691	637.94	4.6	584	292.18	2.1	267
2003-04	14.00	12,884	696.46	5.0	640	275.67	2.0	253

Sources: School-Based Expenditure Reports, System wide Summary, 1996-97 through 2003-04
(i) Monetary values expressed in 2004 Dollars

Table 2: Mean of School Characteristics, by Title I Eligibility: New York City Elementary, Middle and High Schools, Selected Years, within 5 % Band of Percent Poverty on Either Side of Eligibility Cutoff

Elementary and Middle Schools

	1997-98		1999-00		2001-02	
	TI	Not TI	TI	Not TI	TI	Not TI
% Asian	17.77	22.09	17.39	22.78	19.49	18.36
% Hispanic	**29.74**	**20.69**	28.04	26.97	28.60	26.01
% Black	35.18	26.41	30.98	24.77	32.68	30.46
% White	**17.29**	**30.82**	23.58	25.49	19.22	25.17
% Female	49.17	48.38	48.86	49.31	49.07	48.23
% ELL	15.06	15.51	14.06	15.53	13.09	11.16
% Recent Immigrant	11.27	11.59	10.02	11.56	10.12	9.18
% Full-Time Special Education	4.42	6.07	4.29	3.74	3.67	4.08
% Free Lunch	**71.43**	**63.91**	**70.70**	**66.30**	**70.95**	**62.99**
Total Registration	**1097.18**	**894.42**	983.54	868.4	961.12	944.7
N Schools	39	24	57	20	65	27

Bolded numbers statistically different

High Schools

	2001-02		2003-04	
	TI	Not TI	TI	Not TI
% Asian	5.13	1.05	7.58	9.30
% Hispanic	45.92	54.00	43.80	33.94
% Black	42.93	42.80	41.90	42.18
% White	5.98	2.15	6.70	14.55
% Female	57.26	49.20	45.12	46.83
% ELL	**8.09**	**23.10**	9.54	6.81
% Recent Immigrant	3.68	7.45	6.80	9.29
% Full-Time Special Education	3.80	7.74	5.52	3.91
% Free Lunch	**70.76**	**67.25**	**63.84**	**52.81**
Total Registration	1103.33	1320	1581	1300.16
N Schools	9	2	5	8

Bolded numbers statistically different

Note: Definition of variables is as follows:

% Asian: Percentage of Asian students in a school

% Hispanic: Percentage of Hispanic students in a school

% black: Percentage of black students in a school

% white: Percentage of white students in a school

% Female: Percentage of female students in a school

% ELL: Percentage of limited English proficiency students in a school

% Recent Immigrant: Percentage of recent immigrant students in a school

% Full-time Special Education: Percentage of full time special education students in a school

% Free Lunch: percentage of free lunch eligible students in a school

Total Registration: Total enrolment in a school

Table 3: Distribution of Title I School Status by Year by Level

	Poverty Cutoff for Title I Eligibility	Schools Eligible for Title I Funds		Change Ineligible to Eligible	Schools Ineligible for Title I Funds		Change Eligible to Ineligible	Total Schools
	%	N	%		N	%		N
Elementary/Middle Schools								
1996-97	65.99	571	73.58	--	205	26.42	--	776
1997-98	66.66	571	73.58	9	205	26.42	9	776
1998-99	66.30	579	74.61	15	197	25.39	7	776
1999-00	66.99	578	74.48	9	198	25.52	10	776
2000-01	68.36	572	73.71	10	204	26.29	16	776
2001-02	68.24	557	71.78	10	219	28.22	25	776
2002-03	62.00	593	76.42	43	183	23.58	7	776
High Schools								
2001-02	68.24	87	52.10	--	80	47.90	--	167
2002-03	62.00	97	58.08	13	70	45.40	3	167
2003-04								
Manhattan	60.00	42	75.00	5	14	25.00	0	56
Bronx	60.00	26	92.86	0	2	7.14	0	28
Brooklyn	60.00	29	56.86	3	22	43.14	0	51
Queens	57.86	8	25.00	0	24	75.00	0	32
Total	--	105	62.87	8	62	37.13	0	167

Note: i) Change columns are defined with respect to the previous year

Table 4: Change in Title I Eligibility over Time by School Level

	Elementary/Middle Schools 1996-97 through 2002-03		High Schools 2001-02 through 2003-04	
	N	%	N	%
Always Title I Eligible	521	67.1	84	50.3
Never Title I Eligible	159	20.5	60	35.9
Change in Title I Status	96	12.4	23	13.8
(More than once)	53		14	
Total	776	100.0	167	100.0

Notes:

i. Number of schools that changed at least once (i.e. some change more than once—see next row)

ii. Staten Island high schools not included because too few for estimations.

Table 5: Per Pupil Expenditures and Funds, RD estimates, Cubic Polynomial Models, Elementary and Middle Schools, 1997-2003

	Direct Expenditure		Title I Funds		PCEN Funds	
	(1)	(2)	(3)	(4)	(5)	(6)
Title I	185.237	284.300***	507.642***	477.592***	-119.405***	-126.873***
	(158.439)	(91.745)	(21.338)	(23.492)	(17.907)	(17.489)
%Poverty	-0.092	-37.951***	-2.133	-4.552*	3.316*	0.871
	(16.660)	(13.009)	(2.065)	(2.558)	(1.950)	(2.192)
%PovertySq	-0.425	-1.682***	-0.141*	-0.070	-0.008	-0.003
	(0.635)	(0.565)	(0.076)	(0.103)	(0.072)	(0.088)
%Poverty Cub	-0.006	-0.019***	-0.002**	-0.001	-0.000	-0.000
	(0.007)	(0.007)	(0.001)	(0.001)	(0.001)	(0.001)
TitleI*%Poverty	31.134	45.432**	12.061***	7.188	-5.443*	-6.524**
	(28.807)	(21.294)	(3.998)	(4.443)	(3.003)	(3.122)
TitleI*%PovertySq	-1.730	0.485	0.128	0.167	0.155	0.329**
	(1.605)	(1.240)	(0.230)	(0.253)	(0.160)	(0.158)
TitleI*%PovertyCub	0.051*	0.040*	0.001	-0.000	-0.002	-0.006**
	(0.027)	(0.022)	(0.004)	(0.004)	(0.003)	(0.003)
Total Registration	-1.907***	-4.377***	-0.118***	-0.360***	-0.060***	-0.126***
	(0.733)	(0.220)	(0.010)	(0.035)	(0.006)	(0.017)
%ELL	18.768***	-0.232	0.493	2.704**	6.369***	2.797***
	(2.568)	(7.673)	(0.416)	(1.324)	(0.258)	(0.691)
%Recent Immigrant	-18.930***	-3.151	-1.330	-1.067	0.319	-1.501
	(4.848)	(9.475)	(0.829)	(2.116)	(0.545)	(1.524)
%Special Ed	205.671***	48.491***	0.220	-1.247	-0.130	-2.282**
	(4.882)	(12.795)	(0.923)	(1.696)	(0.412)	(0.905)
Constant	12,281.01***	13,841.03***	174.256***	592.462***	381.235***	461.292***
	(429.647)	(451.945)	(68.860)	(97.140)	(64.027)	(79.307)
Borough Effects	Yes	No	Yes	No	Yes	No
School Effects	No	Yes	No	Yes	No	Yes
N Schools	776	776	776	776	776	776
Observations	5432	5432	5432	5432	5431	5431
R-squared	0.67	0.90	0.75	0.85	0.32	0.65

(i) Robust standard errors in parentheses
(ii) * significant at 10%* * significant at 5%; ** significant at 1%
(iii) Dollars Adjusted by 2003 Consumer Price Index
(iv) Year dummies and middle school dummy included but not shown
(v) See definition of variables in note to Table 4

Table 6: Per Pupil Expenditures and Funds, RD estimates, Cubic Polynomial Models, High Schools, 2002-2004

	Direct Expenditure		Title I Funds		PCEN Funds	
	(1)	(2)	(3)	(4)	(5)	(6)
Title I	589.14***	391.70**	510.03***	461.28***	-4.22	-14.61
	(138.22)	(139.60)	(37.01)	(70.52)	(36.04)	(26.46)
School Effects	No	Yes	No	Yes	No	Yes
N Schools	167	167	167	167	167	167
Observations	473	473	473	473	473	473
R-squared	0.61	0.87	0.70	0.89	0.35	0.82

(i) Robust standard errors in parentheses
(ii)* significant at 10%; ** significant at 5%; *** significant at 1%
(iii) Controls include quartic in percent poverty differing above and below Title I cutoff, total registration, %ELL, %Recent Immigrant, %Special Ed, and year effects.
(iv) Only cubic polynomial shows different significance levels than quartic, quadratic or linear.

Table 7: Academic Outcomes, RD Estimates, Cubic Polynomial, Elementary and Middle Schools 1997-2003

	Math		Reading	
	(1)	(2)	(3)	(4)
Title I	-0.035*	-0.011	-0.037*	-0.031*
	(0.020)	(0.017)	(0.019)	(0.016)
%Poverty	-0.005**	-0.002	-0.002	-0.000
	(0.002)	(0.003)	(1.979)	(0.002)
%PovertySq	-0.000	-0.000	0.000*	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
%Poverty Cub	-1.37	-4.85	8.86***	-4.72
	(9.74)	(1.27)	(9.31)	(1.23)
TitleI*%Poverty	-0.004	0.005	-0.012***	0.000
	(0.004)	(0.004)	(0.004)	(0.003)
TitleI*%PovertySq	0.000	0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
TitleI*%PovertyCub	1.56	5.76*	-3.65	3.26
	(3.31)	(3.07)	(3.12)	(2.89)
Total Registration	0.000***	-4.29	0.000	-7.35
	(7.48)	(0.000)	(7.27)	(0.000)
%ELL	-0.008***	-0.007***	-0.007***	-0.004***
	(0.000)	(0.001)	(0.000)	(0.001)
%Recent Immigrant	-0.005***	-0.000	0.007***	-0.002
	(0.000)	(0.001)	(0.001)	(0.001)
%Special Ed	-0.001***	0.000	-0.001**	0.001
	(0.001)	(0.001)	(0.000)	(0.001)
% black	-0.008***	-0.010***	-0.006***	-0.006***
	(0.000)	(0.001)	(0.000)	(0.001)
% Hispanic	-0.006***	-0.010***	-0.004***	-0.008***
	(0.000)	(0.001)	(0.000)	(0.001)
% Asian	0.004***	-0.001	0.001***	0.002***
	(0.000)	(0.001)	(0.000)	(0.001)
% female	0.018***	0.003***	0.019***	0.004***
	(0.001)	(0.001)	(0.001)	(0.001)
Constant	-0.237***	0.639***	-0.434***	0.270**
	(0.094)	(0.109)	(0.089)	(0.115)
Borough Effects	Yes	No	Yes	No
School Effects	No	Yes	No	Yes
N Schools	776	776	776	776
Observations	5432	5432	5432	5432
R-squared	0.66	0.83	0.63	0.81

(i) Robust standard errors in parentheses
(ii) * significant at 5%; ** significant at 1%
(iii)Year, middle school, and grade dummies are included but not shown.

Table 8: Academic Outcomes After Four Years, RD Estimates, Cubic Polynomials, High Schools, 2002-04, Coefficient on Title I Eligible Indicator

	Graduation Rate		Dropout Rate	
	(1)	(2)	(3)	(4)
Title I	8.61	3.59	-5.02	1.73
	(6.44)	(3.82)	(2.97)	(4.81)
School Effects	No	Yes	No	Yes
N Schools	166	166	166	166
Observations	455	455	455	455
R-squared	0.31	0.92	0.32	0.90

Notes:

(i) Robust standard errors in parentheses

(ii) * significant at 10%; ** significant at 5%; *** significant at 1%

(iii) Controls include cubic in percent poverty differing above and below Title I cutoff, total registration, %ELL, %Recent Immigrant, %Special Ed, and year effects

(iv) See text for definition of variables.

Table 9: Per Pupil Expenditures and Funds, Elementary and Middle Schools, Exploration of Time Path of Title I eligibility

	Direct Expenditure		Title I Funds		PCEN Funds	
	(1)	(2)	(3)	(4)	(5)	(6)
%Poverty	7.346***	-1.707	0.880***	-5.865***	3.305***	0.650
	(2.064)	(3.985)	(0.249)	(0.708)	(0.215)	(0.610)
TitleI*%Poverty	-0.151	-3.865	8.011***	10.963***	-2.482***	-1.075
	(3.616)	(5.436)	(0.488)	(1.015)	(0.343)	(0.718)
FirstTI	172.643	165.190*	388.839***	464.611***	-109.725***	-112.485***
	(160.748)	(89.755)	(20.569)	(24.150)	(16.635)	(17.911)
Two TI	264.232**	236.166**	537.160***	584.968***	-129.884***	-159.929***
	(89.372)	(87.195)	(11.187)	(22.243)	(9.376)	(19.768)
FirstnotTI	135.657	-5.152	60.474**	92.861***	23.703	-1.343
	(189.000)	(84.713)	(23.551)	(25.098)	(22.230)	(20.614)
School Effects	No	Yes	No	Yes	No	Yes
N Schools	776	776	776	776	776	776
Observations	4656	4656	4656	4656	4656	4656
R-squared	0.64	0.90	0.75	0.83	0.32	0.66

Notes:
(i) Robust standard errors in parentheses
(ii)* significant at 10%; ** significant at 5%; *** significant at 1%
(iii) Controls include total registration, %ELL, %Recent Immigrant, %Special Ed, and year effects.

Table 10: Resources, RD Estimates, Cubic Polynomial Models

Panel A: Elementary and Middle Schools, 1997-03

	Pupil Teacher Ratio		Percent Teachers Masters Degree		Percent Teachers Greater than 5 Years Experience	
	(1)	(2)	(3)	(4)	(5)	(6)
Title I	0.004	-0.567	-1.772*	-1.292	-1.220	-0.591
	(0.157)	(0.722)	(0.966)	(0.809)	(1.301)	(1.022)
Borough Effects	Yes	No	Yes	No	Yes	No
School Effects	No	Yes	No	Yes	No	Yes
N Schools	634	634	776	776	776	776
Observations	4388	4388	5426	5426	5431	5431
R-squared	0.04	0.19	0.50	0.79	0.32	0.65

Notes:
(i) Robust standard errors in parentheses
(ii)* significant at 10%; ** significant at 5%; *** significant at 1%

Panel B: High Schools, 2002-04

	Pupil Teacher Ratio		Percent Teachers Masters Degree		Percent Teachers Greater than 5 Years Experience	
	(1)	(2)	(3)	(4)	(5)	(6)
Title I	-1.11	-1.09*	0.67	-4.80	-19.16**	-9.92**
	(0.77)	(0.61)	(4.31)	(3.30)	(7.05)	(4.50)
School Effects	No	Yes	No	Yes	No	Yes
N Schools	164	164	164	164	164	164
Observations	452	452	434	434	434	452
R-squared	0.49	0.92	0.25	0.86	0.28	0.91

Notes:
(i) Robust standard errors in parentheses
(ii)* significant at 10%; ** significant at 5%; *** significant at 1%
(iii) Controls include cubic in percent poverty differing above and below Title I cutoff, total registration, %ELL, %Recent Immigrant, %Special Ed, year effects and forums middle school dummy.

Table 11: Academic Outcomes, Elementary and Middle Schools, 1998-2003, Exploration of Time Path of Title I eligibility

	Math		Reading		Math	Reading
	(1)	(2)	(3)	(4)	(5)	(6)
%Poverty	-0.006***	-0.002**	-0.007***	-0.001	-0.006***	-0.008***
	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)
TitleI*%Poverty	0.000	0.002***	-0.001	0.001	0.001*	0.000
	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)
FirstTI	0.014	-0.001	0.005	-0.017		
	(0.020)	(0.017)	(0.023)	(0.022)		
Two TI	-0.041***	-0.019	-0.041***	-0.020		
	(0.012)	(0.016)	(0.011)	(0.018)		
FirstnotTI	-0.055***	-0.010	-0.040**	0.004		
	(0.020)	(0.016)	(0.021)	(0.018)		
TitleI					-0.0146	-0.001
					(0.0141)	(0.014)
Always					-0.031***	-0.049***
					(0.010)	(0.010)
Never					-0.022*	-0.013
					(0.013)	(0.013)
School Effects	No	Yes	No	Yes	No	No
N Schools	764	764	764	764		
Observations	15,396	15,393	15,227	15,227		17,779
R-squared	0.66	0.84	0.63	0.81		0.63

Notes:
(i) Robust standard errors in parentheses
(ii)* significant at 10%; ** significant at 5%; *** significant at 1%
(iii) Controls include total registration, %ELL, %Recent Immigrant, %Special Ed, %black, %Hispanic, %Asian, %female, grade effects, middle school dummy, and year effects.

NEW YORK UNIVERSITY

INSTITUTE FOR
Education and Social Policy

The Institute for Education and Social Policy is a joint research center of NYU's Wagner and Steinhardt Schools. Founded in 1995, IESP brings the talents of a diverse group of NYU faculty, graduate students and research scientists to bear on questions of education and social policy. We are one of the nation's leading academic research centers addressing urban education issues and a substantial amount of our work focuses on New York City public schools and students. More information is available on our website at **http://steinhardt.nyu.edu/iesp**.

665 Broadway, Suite 805, New York, NY 10003 p 212.998.5880 f 212.995.4564