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Supplanting or Supplementing? The Stickiness of Title I Revenues in Post-Adequacy Era

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Supplanting or Supplementing?
The Stickiness of Title I Revenues in Post-Adequacy Era

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Abstract

This paper examines how school districts respond to federal Title I funding in the post-adequacy era. I find that fiscal adjustment occurs through capital investment rather than operating budgets. Using a regression discontinuity design centered on the Title I Concentration Grant eligibility threshold with district-level data from 2008–2017, I show that districts at the eligibility margin have significantly lower capital spending, approximately 6–7 percent below the sample mean. These effects are mainly driven by reduced construction expenditure and lower long-term debt issuance. By contrast, operating revenues show no evidence of systematic crowd-out. These patterns are consistent with institutional constraints introduced during the adequacy era, including maintenance-of-effort requirements and weighted student funding formulas, which limit fiscal substitution in operating budgets. The results suggest that while post-adequacy institutions may have constrained traditional crowd-out, fiscal adjustment has shifted to capital investment, where regulatory oversight is weaker. Because school facility investments disproportionately benefit disadvantaged communities, these findings raise important equity concerns for federal education policy. (*Keywords:* Fiscal Federalism, Intergovernmental Grants, Education Finance, Title I)

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1 Introduction

Educational funding plays a critical role in providing equal opportunities, yet the distribution of resources has been historically unequal (Card & Payne, 2002; Jackson et al., 2016; Lafortune et al., 2018; Reeves, 2013). The cornerstone of federal K-12 aid is Title I of the 1965 Elementary and Secondary Education Act (ESEA), the largest federal program supporting elementary and secondary education. As of fiscal year 2023, Title I alone accounted for $19 billion of the U.S. Department of Education's $79.6 billion budget. The program's fundamental goal is to supplement the resources available to disadvantaged students, ensuring additional services beyond what state and local funds provide. Indeed, Shores et al. (2021) show that well-targeted Title I funds can significantly close spending gaps between low- and non-low-income students.

However, the effectiveness of Title I hinges on a foundational question in public finance: do intergovernmental grants generate new spending, or do they simply supplant funds that state and local governments would otherwise spend? Theory offers competing predictions. The "flypaper effects" suggest that grant money sticks where it hits – implying that a dollar of federal aid should boost public spending by something close to a dollar (Hines Jr & Thaler, 1995; Inman, 2008; Oates, 1999). However, if local governments treat federal dollars as a substitute for their own funds, this "crowd-out" effect will make the increase in spending far less (Baicker & Gordon, 2004; Chakrabarti & Setren, 2011; Fisher & Papke, 2000; Nguyen-Hoang & Yinger, 2020; Steinberg et al., 2016). This debate directly challenges Title I's core statutory mandate, as the program explicitly requires that federal funds 'supplement, not supplant' existing state and local expenditures. Yet this debate has focused almost entirely on operating revenues and expenditures. School districts also make substantial capital investment, such as buildings, infrastructure and long-term debt, that fall outside the regulatory framework governing operating budgets. Whether federal grants affect these less-constrained margins remains an open question.

Empirical research on Title I has largely supported the crowd-out view, though it is

important to recognize the specific institutional contexts in which these findings were established. Gordon (2004) exploited variation in Title I funding generated by decennial Census updates—under which poverty estimates changed only once every ten years—and showed that districts offset nearly all of their Title I grants within a few years of each update. The predictability and infrequency of these funding shifts arguably gave districts enough time to adjust their own fiscal behavior strategically. Cascio et al. (2013) examined the initial introduction of Title I in the South, a setting in which federal funds represented an entirely new revenue source, and found that each federal dollar led to only a 50-cent increase in total spending.

At the school level, Van der Klaauw (2008) and Matsudaira et al. (2012) used regression discontinuity designs within large urban districts to examine how Title I school designation affected individual schools' finances and student outcomes. Both studies found that Title I eligibility did not translate into significant increases in total per-pupil expenditure, partly because districts reallocated other compensatory education funds away from newly designated Title I schools. Notably, Van der Klaauw (2008) found that adverse effects on student performance diminished over time – from measurable negative impacts in 1993 and 1997 to no significant effects by 2001 – suggesting that the impacts of Title I may not be static.

While these studies represent foundational contributions, they share important limitations for understanding the contemporary Title I landscape. The district-level evidence relies on large, discrete, and often predictable shifts in Title I funding – either through program introduction or decennial Census cycles – that no longer characterize how districts experience Title I variation. The school-level evidence, meanwhile, captures within-district resource reallocation rather than the intergovernmental fiscal responses central to the flypaper and crowd-out debate. Neither body of work examines how the post-adequacy institutional environment may have altered the fundamental fiscal dynamics between federal transfers and local government behavior.

The contemporary landscape in which Title I operates has been reshaped by a wave of

court-ordered "adequacy" reforms that began in the late 1990s. This "adequacy era" shifted the focus of school finance from merely equal funding to guaranteeing that all students receive resources adequate to meet state-mandated educational standards. In response, states implemented new funding formulas that typically guaranteed districts a base funding level and directed more resources to low-income students (Candelaria & Shores, 2019; Darling-Hammond, 2019; Lafortune et al., 2018). Critically, these reforms were not merely about increasing funding level; they established a new institutional architecture designed to ensure resources were spent effectively. This often included strengthening state fiscal oversight through required local effort provisions and expenditure monitoring protocols (Verstegen & Jordan, 2009). In effect, the combination of guaranteed foundation funding, substantial state aid, and mandated local effort created a structure in which extra resources could not be easily directed away. As a result, any additional dollar was more likely to result in higher spending on education (Card & Payne, 2002; Duncombe et al., 2003; Hoxby, 2001). This new state-level context, which made local budgets "stickier," potentially created a structure where federal resources might also be more likely to "stick" where they were intended. Moreover, the widespread adoption of weighted student funding formulas – which allocate additional state resources based on the share of disadvantaged students – created a mechanism through which federal eligibility thresholds could mechanically trigger additional state transfers, potentially reversing the pattern of fiscal substitution documented in earlier work (Candelaria et al., 2024; Fischer et al., 2021). These reforms were further reinforced at the federal level by the No Child Left Behind Act of 2001, which introduced stricter accountability requirements for Title I, tying funds to specific school improvement strategies and measurable outcomes (Ohnemus, 2002).

Another critical institutional shift directly relevant to the identification of Title I's fiscal effects is the 1999 transition from decennial Census data to annual Small Area Income and Poverty Estimates (SAIPE) for determining district-level eligibility (Skinner & Riddle, 2020). Under the previous system, eligibility thresholds changed only once every ten years,

allowing districts to anticipate funding cycles and engage in the strategic fiscal substitution documented by Gordon (2004). The shift to SAIPE introduced annual updates with greater temporal volatility and less predictability, fundamentally altering the strategic environment in which districts operate. These convergent institutional changes raise an important question: Has the institutional context of the post-adequacy era changed the relationship between federal Title I grants and local fiscal behavior?

The post-adequacy institutional framework generates distinct predictions for different components of district budgets. Operating expenditures, which are subject to state maintenance-of-effort requirements, foundation funding guarantees, and heightened accountability under NCLB, face multiple institutional constraints that limit districts' ability to substitute federal aid for their own resources. Capital expenditures, by contrast, are typically financed through local bond elections and fall largely outside the scope of these regulatory frameworks. If adequacy-era reforms have successfully constrained fiscal substitution in operating budgets, districts' fiscal effort may differ across budget categories, with capital investment — where institutional oversight is weaker and voter attention is lower — serving as a potentially flexible margin.

This paper contributes to this debate by revisiting the impact of Title I on school funding in the post-adequacy era. I use district-level data from 2008 through 2017, a period when these new institutional frameworks were firmly in place. To isolate the causal effects of Title I, I employ a regression discontinuity design centered on one of the program's sharpest eligibility thresholds. Title I operates through multiple grant streams, each governed by distinct eligibility rules based on district poverty concentrations. I focus on the Concentration Grant threshold, under which districts become eligible for additional funding when their share of children in poverty reaches 15 percent of the school-age population. This threshold provides a clean source of quasi-experimental variation: districts just above and below the cutoff are similar in observable characteristics but differ discretely in their Title I allocation.

The primary finding of this paper concerns capital expenditure. Districts at the Title

I eligibility margin have significantly lower capital spending, by 6-7 percent relative to the sample mean. Decomposing this result reveals that the difference is concentrated in construction expenditure and accompanied by lower long-term debt issuance, suggesting that eligible districts undertake fewer bond-financed infrastructure projects. This finding is robust across multiple specifications, including both parametric models with fixed effects and nonparametric regression discontinuity estimators with data-driven bandwidth selection. This pattern carries important implications for educational equity: recent research shows that capital investments disproportionately raise achievement in disadvantaged communities (Rauscher, 2020), while providing limited marginal benefit to wealthier districts that already invest heavily in facilities (Brunner, 2007; Taylor, 2015). Lower capital spending among eligible districts may therefore have long-term consequences for educational infrastructure in disadvantaged communities.

While capital spending is significantly lower among eligible districts, operating budgets tell a different story. In contrast to the near-complete crowd-out documented by Gordon (2004) and the partial substitution found by Cascio et al. (2013), I find no evidence of systematic crowd-out in operating revenues. Local revenue is statistically indistinguishable from zero at the eligibility threshold, and state revenue, if anything, is higher among eligible districts, consistent with formula-driven crowd-in under weighted student funding systems. The divergence between operating and capital pattern is consistent with the institutional architecture of the post-adequacy era. Operating budgets face multiple regulatory constraints that limit fiscal substitution, while capital investment remains a less regulated margin.

In sum, this paper documents a critical evolution in the fiscal landscape surrounding Title I. In the post-adequacy era, districts at the Title I eligibility margin do not exhibit lower operating revenues, but have significantly lower capital expenditure — a pattern consistent with the institutional constraints of this period. This finding highlights the need for policymakers to consider the full portfolio of district budgets when designing intergovernmental grants and suggests that programs like Title I may benefit from safeguards that

extend beyond operating budget protections to encompass capital investment.

2 Background: Title I in the Post-Adequacy Institutional Environment

2.1 Title I Allocation Structure and the Concentration Grant Threshold

Title I of the Elementary and Secondary Education Act (ESEA) represents the federal government's primary redistributive mechanism in K-12 education, providing over $19 billion annually through a multi-grant formula system. The program operates through four distinct funding streams: Basic Grants, Concentration Grants, Targeted Grants, and Education Finance Incentive Grants (EFIG), each governed by eligibility thresholds based on the number and share of eligible children in a district.[1]

The allocation process follows a two-tiered structure. The Department of Education first calculates district-level allocations using national formulas that incorporate district poverty counts and state per-pupil expenditure. State Education Agencies then distribute these funds according to their own institutional rules and policy priorities. This layered structure means that identical federal allocations can produce different local fiscal impacts depending on a state's institutional environment (Bradford & Oates, 1971), a feature that becomes particularly relevant in the context of post-adequacy reforms discussed below.

This study focuses on the Concentration Grant threshold, which requires that the number of formula-eligible children exceed 6,500, or constitute at least 15 percent of the district's total 5-to-17 population. Districts that do not meet the Concentration Grant eligibility criteria receive no funding from this grant stream; those that do receive additional per-child allocations that meaningfully increase their total Title I award. While the full eligibility rule

[1] See Table 1 for a detailed summary of eligibility thresholds and student weighting rules for each grant type.

includes an alternative count-based criterion, the primary margin of variation relevant to this study is the 15 percent proportion threshold, as discussed in detail in Section 4. The 15 percent cutoff is well suited to a regression discontinuity design because, unlike the Basic Grant threshold at 2 percent or the Targeted/EFIG threshold at 5 percent, which virtually all districts exceed, the Concentration Grant cutoff falls near the middle of the poverty distribution, providing substantial numbers of districts on both sides.

This analysis further benefits from the use of official Title I allocation files, which contain the precise formula counts used in actual funding calculations. These counts incorporate not only poverty estimates from SAIPE but also children who are neglected, delinquent, or in foster care, components that are typically unavailable from other district-level data sources. By using these pre-calculated administrative counts, this study eliminates the measurement error that would arise from attempting to reconstruct eligibility from publicly available data.

2.2 Adequacy Reforms and the Changing State Fiscal Environment

The institutional context in which districts respond to Title I has been fundamentally reshaped by a wave of court-ordered adequacy reforms beginning in the late 1990s. These reforms shifted the focus of state school finance systems from equalizing per-pupil spending across districts to ensuring that all districts receive funding levels adequate to meet state educational standards (Candelaria & Shores, 2019; Lafortune et al., 2018). In practice, this entailed the adoption of foundation funding formulas that guaranteed districts a base level of state support, often accompanied by required local effort provisions and enhanced expenditure monitoring (Verstegen & Jordan, 2009).

A particularly consequential feature of this era is the widespread adoption of weighted student funding formulas (WSFFs). These formulas allocate a base amount per student and apply multipliers for disadvantaged populations, including low-income, English learner, and special education students (Candelaria et al., 2024; Fischer et al., 2021). By 2021, thirty-

three states had implemented WSFFs as their primary distribution mechanism (Fischer et al., 2021). The structure of these formulas parallels the logic of Title I: both direct additional resources toward districts with higher concentrations of disadvantaged students. As a result, when a district crosses a Title I eligibility threshold and its share of formula-eligible children increases, the same demographic shift can mechanically trigger additional state transfers through the WSFF. This creates the possibility of formula-driven state "crowd-in," a pattern that stands in sharp contrast to the absence of systematic state response documented by Gordon (2004) in the pre-adequacy era.

The combination of guaranteed foundation funding, substantial state aid, and mandated local effort provisions created a fiscal architecture in which districts' operating budgets became considerably more constrained. Unlike the earlier period when districts could offset federal grants by reducing their own revenue effort, the post-adequacy framework made such adjustments more difficult in the operating budget. However, this does not imply that all avenues for fiscal adjustment were closed. Capital expenditures, which are typically funded through bond elections and fall outside the scope of most maintenance-of-effort requirements, remained a potentially flexible margin through which districts could respond to changes in federal funding.

2.3 The SAIPE Transition and the Changing Strategic Environment

The strategic environment in which districts respond to Title I was further altered by the 1999 transition from decennial Census data to annual SAIPE estimates for determining district-level poverty (Skinner & Riddle, 2020). Under the decennial system, poverty measures changed only once every ten years. This stability allowed districts to anticipate funding changes well in advance. Gordon (2004) provides direct evidence of this dynamic, showing that districts engaged in fiscal substitution within three years of Census updates. The predictability and infrequency of these updates created conditions conducive to strategic

long-term adjustment of local revenue in response to federal transfers.

The shift to SAIPE fundamentally changed this calculus. Annual updates introduced greater temporal volatility in eligibility determinations, making it substantially more difficult for districts to predict their Title I funding trajectory and plan offsetting fiscal adjustments accordingly. This institutional change suggests that even absent the adequacy-era constraints on operating budgets, the scope for strategic fiscal substitution may have narrowed simply because the informational basis for such behavior was disrupted.

Taken together, the convergence of adequacy-driven funding reforms, strengthened accountability under NCLB, and the modernization of eligibility determination through SAIPE suggests that the fiscal dynamics between federal Title I transfers and local government behavior may differ substantially from those documented in the earlier literature. The analysis that follows provides the first district-level evidence on this question using data from a period when these institutional changes were firmly in place.

3 Data

My analysis combines several data sources at the school district level. The primary dataset for this study comes from the Department of Education's Title I Part A Allocation data, which contains detailed information on Title I. I complement this with district characteristics and financial data from the National Center for Education Statistics (NCES) Common Core of Data (CCD). I describe the datasets and variables in more detail.

3.1 Title I Allocation Information

The Department of Education in the United States provides allocation of Title I and detailed information to state education agencies when the allocation for the next academic year is determined. I use fiscal year revenue files for Title I allocation, a district-level dataset which include final fiscal allocation of Title I to local education agencies by grant type,

their hold-harmless base for each grant, and total Title I amount. The files also include formula counts used to determine Title I allocation, containing total formula count, 5-17 population, proportion of formula children compared to 5-17 population, eligible children for each grant, and weighted counts for Targeted grants and EFIG. From this dataset, I construct a binary eligibility indicator for the Concentration Grant based on the proportion of formula children relative to the district's 5-to-17 population, following the official calculation process. Districts with this proportion at or above 15 percent are classified as eligible. The study spans the 2008-2009 through 2017-2018 academic years, comprising 136,547 observations with approximately 13,000 observations per year.

More significantly, this dataset facilitates the first comprehensive analysis of Title I's fiscal effects using official district-level allocation data. While Gordon (2004) examined fiscal substitution in Title I's decennial census era, no subsequent study has analyzed fiscal responses after the change of poverty estimates to the modern SAIPE-based allocation system. Gordon and Reber (2023) is the only other study that utilized this dataset to examine Title I allocation mechanisms, rather than fiscal behavioral responses. The dataset uniquely enables a national-scale regression discontinuity design for several reasons. First, it provides the precise formula counts used in actual Title I allocation calculations, eliminating measurement error that would otherwise compromise identification. Second, the formula counts incorporate not only 5-17 children under poverty from SAIPE data, but also children who are neglected, delinquent, or in foster care—components that are typically unavailable at the district level from other sources. These additional components present significant data collection challenges: they come from different institutions with varying collection timelines, are often published only at aggregated state levels, and require de-duplication procedures to address potential double-counting across different administrative datasets. By using these pre-calculated, official formula counts, this study overcomes these limitations and supports clean identification at the Concentration Grant threshold used in this study.

3.2 District Finances, Enrollment, and Demographic Information

I connect the Title I Part A Allocation data with multiple public datasets from the National Center of Education Statistics (NCES) Common Core of Data (CCD). The CCD provides two essential sources of information: the Local Education Agency Finance Survey (F-33) and the Local Education Agency Universe Survey, commonly referred to as the nonfiscal data.

The F-33 survey offers comprehensive financial information for school districts across the country. From this survey, I extract revenue data by source—federal, state, and local. For expenditure categories, I focus specifically on four key measures: current elementary and secondary education expenditure (representing total current spending), capital outlays (covering construction, land acquisition, and equipment), current services expenditure (including student support services, instructional staff support, and administration), and current instructional expenditure. To ensure comparability across year, I adjust all financial variables to constant 2017 school year dollars using the `cpiget` Stata command by Shores and Candelaria (2020), which is specifically calibrated for education finance research. This adjustment ensures that financial comparisons across different academic years reflect real changes rather than nominal inflation effects. I then normalize all financial variables on a per-pupil basis to facilitate meaningful comparison across districts of varying sizes.

The CCD's annual nonfiscal survey complements the financial data with detailed demographic and enrollment information. I use this survey to obtain total student enrollment (fall membership) and student demographic composition, including the percentage of students by race and ethnicity (White, Black, Hispanic, and other), as well as the proportion of English Language Learners (ELL) and special education students.

3.3 Analytic Sample

After merging with CCD data, my initial sample contains 134,437 observations, representing a reduction of 2,110 observations (1.5% of the original sample) from excluding non-typical

public school districts organized under specialized or alternative administrative structures. I further exclude districts with missing values for total enrollment and key finance variables, resulting in a full analytic sample of 128,717 observations, with approximately 12,800 observations per year.[2]

For the main regression discontinuity analysis, I restrict the sample to districts within 3 percentage points of the 15 percent Concentration Grant eligibility threshold. This bandwidth provides 31,534 district-year observations, with 15,504 above and 16,030 below the cutoff. Table 2 presents summary statistics for both the full sample and this analytic sample. Districts near the Concentration Grant threshold are broadly representative of the full sample in terms of revenue composition, expenditure patterns, and demographic characteristics. The primary differences reflect the mechanical features of the research design: districts in the analytic sample have somewhat fewer formula-eligible children and a lower proportion of Title I children, consistent with the sample being centered around the 15 percent eligibility cutoff rather than spanning the full poverty distribution. The choice of a 3 percentage point bandwidth and sensitivity to alternative bandwidths are discussed in Sections 4 and 6.

[Table 2 about here]

4 Empirical Strategy

4.1 Research Design: The Concentration Grant Threshold

To identify the causal effect of Title I funding on district fiscal behavior, I exploit a sharp eligibility threshold in the Concentration Grant formula. Districts become eligible for Concentration Grant funding when the proportion of formula-eligible children reaches 15 percent

[2]For New York City's public school district, which appears as five separate county-based observations in Title I allocation files (New York, Bronx, Kings, Queens, and Richmond Counties) but as a single district in CCD, I manually aggregated these separate allocations into one consolidated NYC district observation to ensure proper merging with the financial data.

of their total 5-to-17 population.[3] Districts below this threshold receive no Concentration Grant allocation; those above it receive additional per-child funding that meaningfully increases their total Title I award. This sharp discontinuity in funding at the 15 percent cutoff provides the basis for a regression discontinuity design, in which I compare districts just above and below the threshold under the assumption that they are similar in all relevant respects except for their Concentration Grant eligibility.

I focus on this particular threshold for several reasons. First, unlike the Basic Grant threshold at 2 percent and the Targeted/EFIG threshold at 5 percent, which virtually all districts exceed, the Concentration Grant cutoff provides sufficient observations on both sides of the threshold for a well-powered RD design: approximately 57 percent of districts meet the proportion criterion, with the remaining 43 percent falling below. Second, the 15 percent cutoff offers stronger comparability between districts on either side of the threshold. At the Basic Grant and Targeted/EFIG margins, districts just below 2 and 5 percent poverty respectively are among the least disadvantaged in the country and differ fundamentally from those just above, raising concerns about the validity of the local comparison. At the Concentration Grant margin, districts on both sides have moderate poverty concentrations and already participate in all other Title I grant streams, making them more plausible counterfactuals for one another. Third, the count-based alternative criterion (6,500 children) affects too few districts to support a credible RD design. The distribution of formula-eligible children is extremely right-skewed, with over 75 percent of districts having fewer than 550 formula children, leaving the 6,500 cutoff with insufficient observations for meaningful analysis.

It is important to be explicit about what this design identifies and what it does not. The regression discontinuity estimates capture the local average treatment effect of Con-

[3]The full Concentration Grant eligibility rule also includes an alternative count-based criterion of 6,500 formula children. I exclude 210 district-year observations in which the proportion of formula children falls below 15 percent but the count exceeds 6,500, as these districts receive Concentration Grant funding through the count-based criterion and would contaminate the control group. This exclusion has no meaningful effect on the estimates. Fewer than 2 percent of remaining districts qualify through the count criterion, and nearly all of them already meet the proportion criterion as well.

centration Grant eligibility for districts near the 15 percent cutoff. These are districts with moderate poverty concentrations, and the estimated effects may not generalize to districts at the extremes of the poverty distribution. Moreover, this is a cross-sectional comparison: within each year, I compare districts that fall just above and below the eligibility threshold as determined by that year's SAIPE-based poverty estimates. This differs from the identification strategy employed by Gordon (2004), who exploited within-district changes in Title I funding triggered by decennial Census updates over a ten-year horizon. The distinction is substantively important. Under the decennial system, districts experienced large, infrequent, and predictable funding shifts that allowed for strategic long-term fiscal adjustment. Under the contemporary SAIPE-based system, eligibility is reassessed annually with considerably less predictability, making cross-sectional variation at the eligibility threshold a natural and appropriate source of identification. The estimates should therefore be interpreted as the fiscal response to eligibility status in a given year, rather than the cumulative effect of sustained funding changes over multiple years.

4.2 Estimation

The standard RD framework identifies the treatment effect as the discontinuity in the conditional expectation of the outcome at the eligibility threshold:

$$\delta = \lim_{\epsilon \to 0} E[Y_d \mid R_d = c + \epsilon] - \lim_{\epsilon \to 0} E[Y_d \mid R_d = c - \epsilon] \tag{1}$$

where Y_d is the outcome for district d, R_d is the running variable (the proportion of formula-eligible children in the district's 5-to-17 population), and $c = 0.15$ is the Concentration Grant eligibility cutoff.

My main specification implements this through an ordinary least squares model of the form:

$$Y_{dt} = \beta_0 + \beta_1 \text{Treat}_{dt} + \beta_2 R_{dt} + \beta_3 (\text{Treat}_{dt} \times R_{dt}) + X_{dt} + \sigma_s + \pi_t + \varepsilon_{dt} \tag{2}$$

where Y_{dt} is the outcome for district d in year t, and Treat_{dt} is an indicator equal to one if district d's proportion of formula children equals or exceeds 15 percent in year t. R_{dt} is the running variable centered at the cutoff, so that $R_{dt} = 0$ at the threshold. The interaction $\text{Treat}_{dt} \times R_{dt}$ allows for different slopes on either side of the cutoff. X_{dt} includes district-level covariates: the racial and ethnic composition of the student body (proportions of White, Black, Hispanic, and other race students), and the proportions of English Language Learners and special education students. σ_s and π_t denote state and year fixed effects, respectively. The parameter of interest is β_1, which captures the discontinuity in the outcome at the eligibility threshold.

I include state fixed effects because they absorb persistent cross-state differences in education finance systems, funding levels, and institutional environments that would otherwise contribute to residual variation. Given that Title I interacts with heterogeneous state funding architectures, controlling for these differences substantially improves precision. Year fixed effects account for common temporal shocks such as changes in federal appropriations or macroeconomic conditions. I restrict the sample to districts within a 3 percentage point bandwidth of the cutoff (i.e., districts with formula-eligible shares between 12 and 18 percent). This yields 31,429 district-year observations. I verify that the results are not sensitive to this choice by re-estimating the model with bandwidths of 1 and 5 percentage points in Section 6.

To assess the robustness of the parametric results, I complement the main specification with nonparametric estimates using the bias-corrected local polynomial approach of Calonico et al. (2014). Agreement between the parametric and nonparametric estimates provides additional confidence in the findings, as the two approaches rely on different assumptions about the functional form of the relationship between the running variable and the outcome. Throughout the analysis, standard errors are clustered at the state level to account for within-state correlation in district fiscal behavior.

4.3 Validating the Design

The credibility of the regression discontinuity design rests on two key identifying assumptions: the absence of systematic manipulation of the running variable at the eligibility threshold, and the comparability of districts just above and below the cutoff. I examine these assumptions through density tests and covariate balance tests, respectively.

[Figure 1 about here]

To test for manipulation, I apply the density test proposed by Cattaneo et al. (2020) to the distribution of the running variable around the 15 percent cutoff. Figure 1 displays the distribution of districts around the threshold. The test yields a t-statistic of 1.46 ($p = 0.14$), providing no evidence of systematic sorting around the eligibility cutoff. This is consistent with the institutional features of the Concentration Grant formula: the running variable is constructed from official formula counts that incorporate SAIPE poverty estimates and counts of neglected, delinquent, and foster care children, components over which individual districts have limited control. Moreover, because SAIPE estimates are updated annually with considerable volatility, districts cannot reliably predict whether they will fall just above or below the threshold in a given year. A potential concern is that observations near the cutoff may disproportionately influence the estimates. To address this, I conduct donut hole tests in Section 6 that exclude observations within 0.1, 0.3, and 0.5 percentage points of the cutoff. The results are robust to these exclusions.[4]

[Table 3 about here]

Next, I turn to a covariate balance test to verify the quasi-random assignment assumption, which requires that districts just above and below each cutoff be similar in observable characteristics. Table 3 presents results from the covariate balance test within the main

[4]Appendix Figure A1 provides state-level histograms of the running variable. The absence of visible clustering around the threshold across states further supports the validity of the design.

analytic bandwidth. The results show that the demographic composition of districts is statistically indistinguishable on either side of the cutoff: the proportions of Black, Hispanic, White, and other race students, as well as the shares of English Language Learners and special education students, show no significant differences between treated and control districts. Total enrollment and the number of formula-eligible children are significantly higher among districts above the cutoff, which reflects the mechanical relationship between these variables and the running variable rather than a violation of the identifying assumption. Because all outcome variables are measured on a per-pupil basis, these enrollment differences do not mechanically affect the estimates.[5]

5 Results

5.1 First Stage Results: Change in Title I Allocation

The identification strategy leverages discontinuities in the Title I funding formula to estimate the causal effects of federal aid on local fiscal behavior.Table 4 presents first-stage estimates of the effect of Concentration Grant eligibility on Title I revenue. I report results in two units: per eligible child, which reflects the program's statutory allocation mechanism, and per pupil, which corresponds to the unit of measurement used for all second-stage outcomes.

Crossing the 15 percent eligibility threshold generates a substantial increase in Title I funding per eligible child. The TWFE estimate indicates an increase of $84.6 (SE: 8.2, p¡$0.001), while the nonparametric estimate yields a similar magnitude of $90.4 (SE: 9.3, p ¡0.001). On a per-pupil basis, the effects are naturally smaller because Title I funds allocated to eligible children are spread across the entire district enrollment. The TWFE estimate is $7.9 per pupil (SE: 3.8, p 0.001). This attenuation is mechanical: in districts near the 15

[5]Appendix Table A2 confirms that the main results are virtually unchanged across specifications with no covariates, the baseline set of demographic covariates, and an expanded set that additionally controls for enrollment, further indicating that the observed imbalances do not drive the findings.

percent cutoff, formula-eligible children constitute roughly one-sixth of total enrollment, so the per-eligible-child effect is diluted accordingly when expressed on a per-pupil basis.

[Table 4 about here]

[Figure 2 about here]

Figure 2 provides visual confirmation of the first stage. The figure displays a clear upward discontinuity in Title I revenue at the 15 percent threshold, with smooth trends on either side of the cutoff. The precision and consistency of the first-stage estimates across specifications, combined with the absence of manipulation at the threshold (Figure 1) and the comparability of districts on either side of the cutoff (Table 3), validate the use of the Concentration Grant threshold to examine how districts respond fiscally to changes in Title I funding. The magnitude and statistical significance of these first-stage effects support interpreting the reduced-form estimates as the causal effects of Title I on district financial behavior.

Because Title I is allocated on the basis of eligible children, per eligible child is the natural unit for measuring the first stage. However, when examining how districts adjust their overall budgets in response to federal aid, per-pupil measures are more appropriate, as revenue and expenditure decisions are made at the district level and affect all students. I therefore report all second-stage outcomes on a per-pupil basis.

5.2 Effects on District Revenue

Panel A of Table 5 presents reduced-form estimates of the effect of Concentration Grant eligibility on district revenues. I find no evidence of systematic crowd-out in operating revenues. This pattern differs from the near-complete fiscal substitution documented by Gordon (2004) and the partial crowd-out found by Cascio et al. (2013).

[Table 5 about here]

Local revenue shows no statistically significant response to Title I eligibility. The TWFE point estimate of $42.9 per pupil (SE: 93.8) is small relative to the sample mean of $7,053.8 and is statistically indistinguishable from zero. The 95 percent confidence interval [-141, 227] is wide, and does not allow me to rule out crowd-out at the magnitudes documented in the earlier literature. However, the point estimate is positive, and the bulk of the confidence interval lies above zero, suggesting that a pattern of near-complete crowd-out is unlikely to characterize the post-adequacy fiscal environment. This pattern is consistent with institutional constraints established by adequacy-era reforms, including maintenance-of-effort provisions and foundation funding guarantees, though the imprecision of the estimate warrants caution in drawing strong conclusions about operating revenues alone.

State revenue responds positively, though the statistical significance varies across specifications. The TWFE estimate of $97.7 per pupil (SE: 68.7) is positive but not statistically significant [95% CI: -27, 233], while the nonparametric estimate of $105.7 per pupil (SE: 61.4) is statistically significant at the 10 percent level (Table 6). The confidence interval includes zero but is centered well above it, providing suggestive evidence of a positive state fiscal response. The direction of this effect is consistent with formula-driven crowd-in under weighted student funding systems: because many states now allocate resources through formulas that apply multipliers for disadvantaged students, the same demographic characteristics that trigger Title I eligibility can mechanically generate additional state transfers. While not definitive, this pattern differs from the absence of systematic state response documented in the pre-adequacy literature (Gordon, 2004).

Federal revenue increases by $14.9 per pupil in the TWFE specification (SE: 12.2), consistent in direction with the per-pupil first-stage effect of $7.9, though the estimate is not statistically significant. Total revenue increases by $182.1 per pupil in the TWFE specification and $142.3 in the nonparametric specification, though neither estimate is statistically significant. The positive direction is consistent with the combined pattern of no local crowd-out and positive state response.

[Figure 3 about here]

Figure 3 provides visual confirmation of these patterns, showing no discontinuity in local revenue at the eligibility threshold and a positive shift in state revenue.

5.3 Effects on District Expenditure

Panel B of Table 5 presents the expenditure results. The central finding is the significant and robust decline in capital expenditure among eligible districts. The TWFE estimate indicates a difference of \$85.2 per pupil (SE: 31.9, $p < 0.01$), while the nonparametric estimate in Table 6 yields -\$70.7 per pupil (SE: 24.7, $p < 0.01$). Relative to the sample mean of \$1,150.3, these estimates represent a decline of approximately 6 to 7 percent. This finding is consistent across both the parametric and nonparametric approaches and remains robust to alternative bandwidths and donut hole specifications, as documented in Section 6.

In contrast, operating expenditure categories show uniformly positive but statistically insignificant estimates. Elementary and secondary expenditure is higher by \$148.9 per pupil (SE: 95.0), instructional expenditure by \$85.2 (SE: 54.6), and current services by \$68.9 (SE: 42.8). The nonparametric estimates in Table 6 yield similar magnitudes, with both instructional expenditure (\$78.5, SE: 44.4) and current services (\$57.3, SE: 34.7) statistically significant at the 10 percent level. These positive point estimates are consistent with the revenue results: if total resources flowing to districts are not reduced, spending on core educational services should remain stable or higher.

The divergence between operating and capital expenditure patterns is consistent with the institutional structure of the post-adequacy era. Operating budgets are subject to state maintenance-of-effort requirements, foundation funding guarantees, and heightened accountability, all of which constrain districts' flexibility over current educational spending. Capital expenditures, which are typically financed through local bond elections and fall outside the scope of most maintenance-of-effort provisions, represent a less regulated budget category. The results suggest that the cross-sectional difference between eligible and ineligible districts

is concentrated in capital spending rather than operating budgets, a pattern consistent with the institutional constraints of the adequacy era.

This finding carries important implications for educational equity. Recent evidence demonstrates that capital investments disproportionately raise achievement in disadvantaged communities (Rauscher, 2020), while providing more limited marginal benefits in wealthier districts that already invest heavily in facilities (Brunner, 2007; Taylor, 2015). To the extent that lower capital spending among marginally eligible districts reflects a broader pattern, this difference may have implications for long-term educational infrastructure in disadvantaged communities. However, because the estimates are local to districts near the 15 percent eligibility threshold, caution is warranted in generalizing this pattern.

[Figure 4 about here]

Figure 4 illustrates this divergence visually: operating expenditure categories show no discontinuity at the threshold, while capital outlay exhibits a clear downward shift among eligible districts.

5.4 Unpacking the Capital Expenditure Result

To better understand the nature of the capital expenditure finding, I decompose total capital outlays into subcategories and examine related capital financing variables. Table 7 presents the results.

Panel A reports estimates for four capital outlay subcategories. The negative effect on total capital expenditure is driven almost entirely by construction, which shows a reduction of \$55.8 per pupil (SE: 26.2, $p < 0.05$). This accounts for roughly two-thirds of the total capital expenditure difference of \$85.2. Land and existing structures show a smaller negative estimate of \$3.7 per pupil (SE: 1.9, $p < 0.10$), while instructional and other equipment show no meaningful differences. The concentration of the effect in construction rather than equipment is notable: construction projects are typically large, long-term investments

financed through bond issuance, whereas equipment purchases are more commonly funded from operating budgets or short-term allocations.

Panel B turns to capital financing. Districts above the eligibility threshold issue significantly less long-term debt, with an estimate of -$81.6 per pupil (SE: 35.0, $p < 0.05$) against a sample mean of $900.8. This magnitude closely parallels the total capital expenditure difference, suggesting that lower capital spending among eligible districts operates primarily through reduced debt financing rather than reallocation of existing revenues. State revenue designated for capital outlay and debt services is also lower among eligible districts by $6.6 per pupil (SE: 3.0, $p < 0.05$), relative to a mean of $92.1. One possibility is that this reflects reduced state matching or debt service support that accompanies lower bond activity, though other explanations cannot be ruled out.[6]

Taken together, these patterns paint a consistent picture. The capital expenditure difference between eligible and ineligible districts is concentrated in construction, accompanied by lower long-term debt issuance and reduced state capital support. While the cross-sectional design does not allow me to establish the temporal sequence of these relationships, the alignment across spending categories and financing channels is consistent with a pattern in which Title I eligibility is associated with lower levels of new infrastructure investment.

6 Robustness Check

The main findings are robust to a range of alternative specifications and sample restrictions. Appendix Table A1 presents results from two sets of sensitivity analyses. Panel A

[6]The nonparametric estimate for long-term debt issuance is -$66.3 but statistically insignificant. This likely reflects the distributional properties of the variable: over half of district-year observations report zero debt issuance, producing a highly skewed distribution (skewness: 3.03) that reduces the precision of local polynomial estimators. The parametric specification, which absorbs state and year variation through fixed effects, achieves greater precision in this setting. Interest on debt shows a small and statistically insignificant difference of -$7.2 per pupil in the main specification (SE: 6.4), though the nonparametric estimate is larger and statistically significant at -$12.6 (SE: 6.4, $p < 0.05$). Because interest payments reflect the stock of accumulated debt rather than current-year borrowing, the weaker result in the parametric specification is not surprising in a cross-sectional framework. Appendix Table A4 reports the full set of nonparametric estimates.

reports estimates using alternative bandwidths of 1 and 5 percentage points. The capital expenditure reduction remains negative across all bandwidths, with the narrowest bandwidth (1pp) yielding a larger point estimate of -$102.7 per pupil ($p<0.05$), consistent with the estimate becoming more local to the cutoff. The wider bandwidth (5pp) produces a smaller estimate of -$40.4 that is no longer statistically significant, reflecting attenuation as more distant districts are included in the comparison. The revenue results are also stable: local revenue remains statistically indistinguishable from zero and state revenue remains positive across all bandwidths.

Panel B reports donut hole specifications estimated using the nonparametric approach, which assigns the greatest weight to observations nearest the cutoff and is therefore most sensitive to their exclusion. Excluding observations within 1 percentage point of the cutoff yields a capital expenditure estimate of -$146.6 (SE: 97.5), larger in magnitude than the main estimate though less precisely estimated due to the substantial reduction in sample size. At narrower exclusions of 0.5, 0.3, and 0.1 percentage points, the estimates are -$53.3, -$48.7, and -$41.9 ($p<0.10$), respectively. The sign remains consistently negative across all specifications, and the attenuation in precision at wider donut holes reflects the loss of observations rather than a reversal of the finding. Notably, several revenue and operating expenditure outcomes that are imprecisely estimated in the main specification become statistically significant in the donut hole specifications. For instance, total revenue and local revenue show significant positive effects in the 1pp donut, while operating expenditure categories including instruction and services show significant increases across multiple donut specifications. These patterns further reinforce the conclusion that Title I eligibility does not crowd out operating revenues or expenditures, and suggest that the main estimates may be conservative.

Appendix Table A2 examines the sensitivity of the main results to the inclusion of covariates. Capital expenditure estimates are virtually identical across specifications with no covariates (-$86.8, $p<0.01$), demographic controls only (-$86.0, $p<0.05$), and the full base-

line that additionally includes enrollment (-$85.2, p < 0.05). The revenue estimates exhibit similar stability. This confirms that the covariate imbalance in enrollment documented in Table 3 does not drive the results.

Appendix Table A3 presents fuzzy RD estimates that scale the reduced-form effects by the first-stage change in Title I revenue. Using per-eligible-child Title I revenue as the endogenous variable, the estimates imply that each additional dollar of Title I funding per eligible child reduces per-pupil capital expenditure by approximately $0.87 (SE: 0.35, p<0.05), while per-pupil state revenue increases by $1.41 (SE: 0.79, p <¡0.10).These magnitudes are economically plausible: a less-than-dollar-for-dollar capital expenditure response to a one-dollar increase in per-eligible-child funding is consistent with the fiscal adjustment being partial rather than complete. By contrast, the per-pupil specification yields substantially inflated estimates, with capital expenditure declining by $12.03 (SE: 6.22, p <0.10) per dollar of per-pupil Title I revenue. This exaggeration arises because the per-pupil first stage is mechanically attenuated, producing an artificially small denominator in the fuzzy RD ratio. The contrast between the two panels provides additional justification for the measurement approach adopted throughout the analysis: using per-eligible-child units for the first stage, where the allocation mechanism operates, and per-pupil units for the second stage, where district-wide fiscal decisions are observed.

A potential concern with the cross-sectional design is that capital investment decisions typically span multiple years, raising the question of whether a single year's eligibility status can meaningfully relate to capital expenditure. To address this, I examine whether lagged Concentration Grant eligibility predicts current capital expenditure. Table A5 reports the results. Column (1) estimates a lagged RD specification using year $t-1$ eligibility and the corresponding running variable. Districts that were eligible in the prior year have significantly lower capital expenditure in the current year ($-$100.6, $p < 0.05$), providing direct evidence that the relationship between eligibility and capital spending extends across years. Column (2) adds current eligibility status as an additional control. The lagged RD estimate remains

significant (−$96.4, $p < 0.05$), and current eligibility is independently associated with lower capital spending (−$68.6, $p < 0.01$). The persistence of the lagged effect after conditioning on current eligibility status suggests that the capital expenditure finding is not merely a contemporaneous cross-sectional association but reflects a temporal relationship consistent with the multi-year nature of capital planning.

7 Discussion and Conclusion

This study examines how Title I funding affects district fiscal behavior in the post-adequacy era. The primary finding of this study concerns capital expenditure. Districts at the Concentration Grant eligibility margin have significantly lower capital spending — by approximately $85 per pupil in the main specification and $71 in the nonparametric specification, representing a 6 to 7 percent difference relative to the sample mean. This finding is robust across alternative bandwidths, donut hole specifications, and covariate choices. Decomposing this result reveals that the difference is concentrated in construction expenditure and is accompanied by lower long-term debt issuance and reduced state capital support, suggesting that eligible districts undertake fewer bond-financed infrastructure projects. Moreover, lagged eligibility significantly predicts current capital expenditure even after conditioning on current eligibility status, providing evidence that the relationship extends beyond contemporaneous cross-sectional association.

The operating budget, however, shows no such pattern. While earlier research found near-complete crowd-out of operating revenues (Gordon, 2004) and only partial pass-through of federal dollars (Cascio et al., 2013), my findings suggest that these patterns may not characterize the post-adequacy fiscal environment. Using a regression discontinuity design centered on the Concentration Grant eligibility threshold with district-level data from 2008 through 2017, I find no evidence of systematic crowd-out in operating revenues. Local revenue is statistically indistinguishable from zero at the eligibility margin, though the imprecision of

the estimate does not allow me to rule out moderate levels of crowd-out. State revenue, if anything, is higher among eligible districts, consistent with formula-driven crowd-in under weighted student funding systems. These patterns are consistent with the institutional constraints of the adequacy era – including maintenance-of-effort provisions, foundation funding guarantees, and strengthened accountability – limiting the scope for the direct fiscal substitution documented in the earlier literature.

The near-complete crowd-out documented by Gordon (2004) reflected a specific institutional context: decennial Census updates that allowed districts to anticipate funding changes over long horizons. The patterns documented here, under the contemporary SAIPE-based system where eligibility is reassessed annually, suggest that the fiscal landscape has changed. The divergence between operating and capital expenditure patterns is consistent with the institutional architecture of the post-adequacy era. Operating budgets are subject to regulatory constraints that limit districts' flexibility over current spending, while capital expenditures — typically financed through bond elections and outside the scope of most maintenance-of-effort provisions — represent a less constrained budget category. The cross-sectional differences documented here suggest that the relevant fiscal margin lies outside the operating budget, in a domain where institutional oversight is weaker. Because this study employs a cross-sectional design while the earlier literature estimated dynamic responses to funding changes over time, direct comparison of magnitudes requires caution. Nonetheless, these patterns suggest that the fiscal landscape surrounding Title I has meaningfully evolved since the earlier literature.

Recent evidence demonstrates that capital investments disproportionately benefit disadvantaged communities (Rauscher, 2020), while providing more limited marginal returns in districts that already invest heavily in facilities (Brunner, 2007; Taylor, 2015). Lower capital spending among eligible districts may therefore carry long-term consequences for educational infrastructure in the communities Title I is designed to serve. A broader question for policymakers is whether the current federal framework, which focuses oversight on operating

expenditures, adequately addresses the full portfolio of district fiscal decisions. The findings of this study suggest that programs like Title I may benefit from safeguards that extend beyond operating budget protections to encompass capital investment as well.

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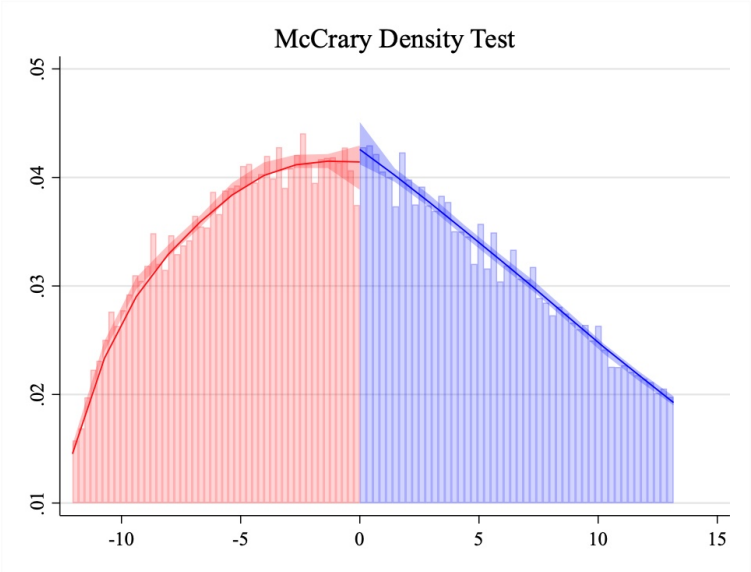
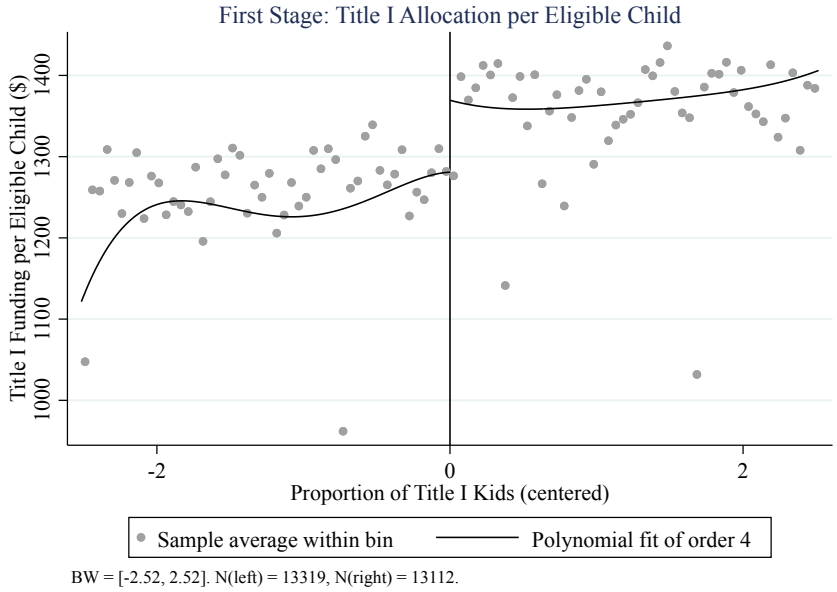
Figure 1: Density of observations



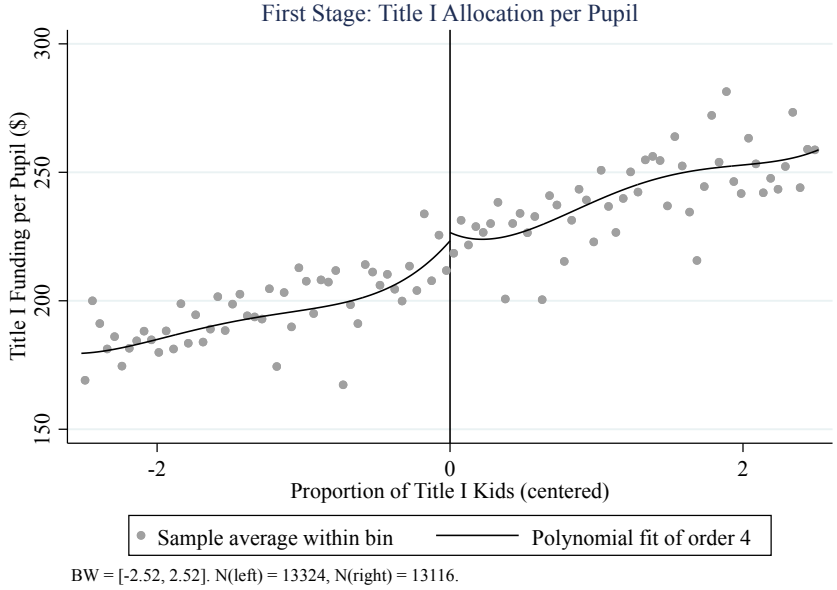
Note: This figure displays the distribution of the running variable around the 15 percent Concentration Grant eligibility threshold, centered at zero. Red bars and curve represent observations below the cutoff; blue bars and curve represent observations above. The curves are local polynomial density estimates from `rddensity` (Cattaneo et al., 2020) using a polynomial order of 2 and a bias-correction order of 3. The density test yields a t-statistic of 1.46 ($p = 0.14$), providing no evidence of systematic manipulation at the threshold.

Figure 2: First-Stage Discontinuity at the Title I Concentration Grant Threshold

(a) Title I Funding per Eligible Child



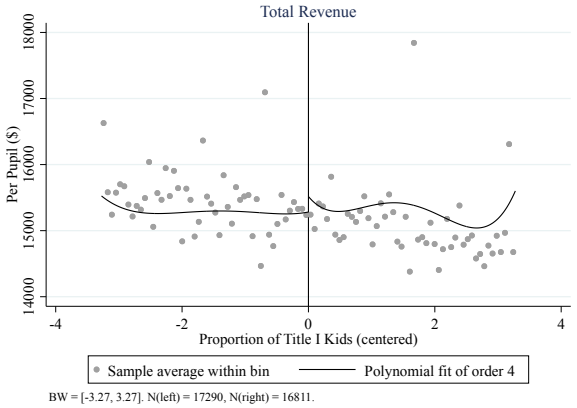
(b) Title I Funding per Pupil



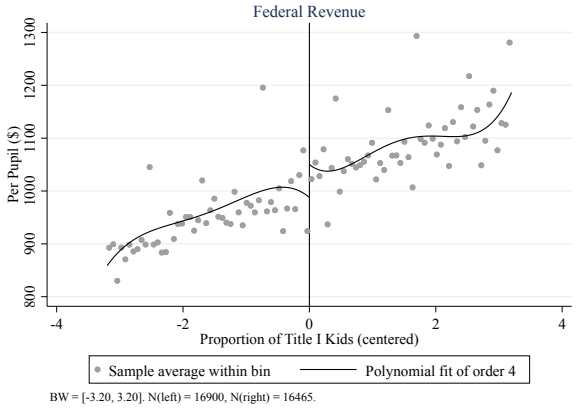
Notes: The figure plots binned averages of Title I funding against the running variable, defined as the proportion of eligible children centered at the 15 percent Concentration Grant eligibility threshold. Each point represents the average outcome within evenly spaced bins of the running variable. The solid lines show polynomial fits estimated separately on each side of the cutoff using the `rdplot` procedure (Calonico et al., 2014). Panel (a) reports Title I funding per eligible child and Panel (b) reports Title I funding per pupil.

Figure 3: Main RD Evidence: Title I Concentration Grant Eligibility and School Revenue

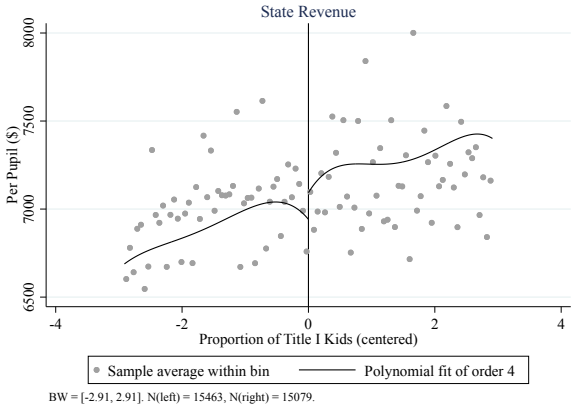
(a) Total Revenue



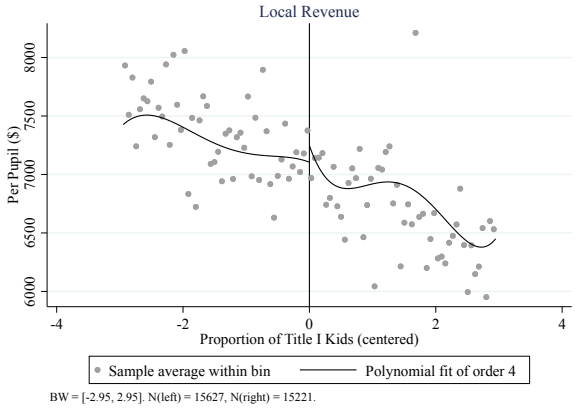
(b) Federal Revenue



(c) State Revenue



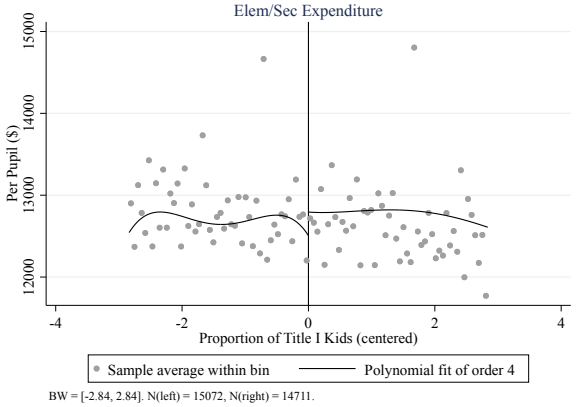
(d) Local Revenue



Notes: The figure plots binned averages of district revenue against the running variable, defined as the proportion of Title I eligible children centered at the 15 percent Concentration Grant eligibility threshold. Each point represents the average outcome within bins of the running variable. The solid curves show fourth-order polynomial fits estimated separately on either side of the cutoff using `rdplot`. Bandwidths and bin selection follow the default procedure of `rdplot` (Calonico et al., 2014). Panel (a) shows total district revenue per pupil, Panel (b) federal revenue per pupil, Panel (c) state revenue per pupil, and Panel (d) local revenue per pupil. All revenue measures are expressed in constant 2017 school-year dollars. The vertical line indicates the eligibility cutoff.

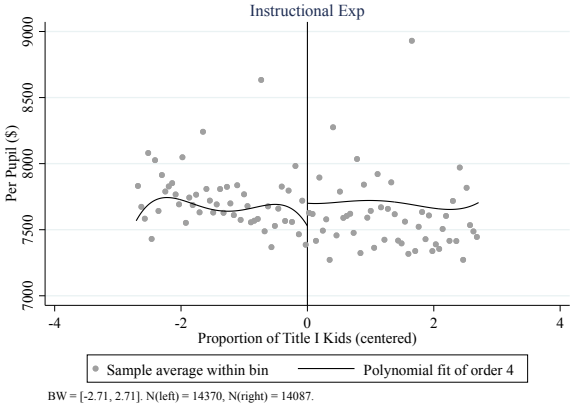
Figure 4: Main RD Evidence: Title I Concentration Grant Eligibility and School Expenditure

(a) Current Elementary/Secondary Expenditure



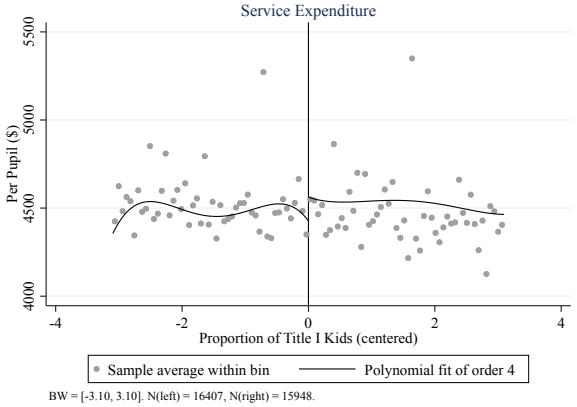
BW = [-2.84, 2.84]. N(left) = 15072, N(right) = 14711.

(b) Current Instructional Expenditure



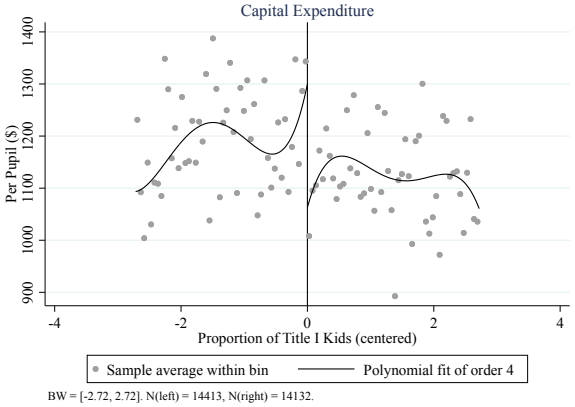
BW = [-2.71, 2.71]. N(left) = 14370, N(right) = 14087.

(c) Current Service Expenditure



BW = [-3.10, 3.10]. N(left) = 16407, N(right) = 15948.

(d) Capital Outlay



BW = [-2.72, 2.72]. N(left) = 14413, N(right) = 14132.

Notes: The figure plots binned averages of district expenditure against the running variable, defined as the proportion of Title I eligible children centered at the 15 percent Concentration Grant eligibility threshold. Each point represents the average outcome within bins of the running variable. The solid curves show fourth-order polynomial fits estimated separately on either side of the cutoff using `rdplot`. Bandwidths and bin selection follow the default procedure of `rdplot` (Calonico et al., 2014). Panel (a) shows current elementary and secondary expenditure per pupil, Panel (b) current instructional expenditure per pupil, Panel (c) current service expenditure per pupil, and Panel (d) capital outlay per pupil. All expenditure measures are expressed in constant 2017 school-year dollars. The vertical line indicates the eligibility cutoff.

Table 1: Title I Grant Eligibility and Weighting Formula

Eligibility Requirements for Title I Grants			
Grants	**Requirements**		
Basic	The district must have more than 10 formula children "AND" the number of those children must exceed 2% of the total 5-to-17 population of the districts.		
Concentrated	The district must have at least 6,500 formula children "OR" the number of those children must be or exceed 15% of the total 5-to-17 population of the districts.		
Targeted and EFIG	The district must have more than 10 formula children "AND" the number of those children must exceed 5% of the total 5-to-17 population of the districts.		
Weighting Formula for Targeted Grants and EFIG			
Weight	**Formula Count**	**Weight**	**Proportion**
1	Up to 691	1	Up to 15.58%
1.5	692 – 2,262	1.75	15.58% - 22.11%
2	2263 – 7,851	2.5	22.11% to 30.16%
2.5	7,852 – 35,514	3.25	30.16% to 38.24%
3	Over 35,515	4	Over 38.24%

Table 2: District Fiscal and Nonfiscal Characteristics

	Full Sample (1)	**3% Bandwidth** (2)
Per Eligible Child Title I	1297.6	1302.3
	(501.9)	(493.9)
Per Pupil Title I	271.6	217.6
	(214.7)	(112.5)
Panel A: District Revenue and Expenditures		
Total Revenue	15511.2	15268.9
	(6214.9)	(5850.2)
Federal Revenue	1168.2	1021.8
	(815.9)	(634.7)
State Revenue	6959.6	7084.7
	(3648.8)	(3640.4)
Local Revenue	7203.2	7051.9
	(5491.9)	(4858.8)
Current Elementary/Secondary Expenditure	13014.2	12731.9
	(4990.9)	(4644.6)
Current Instructional Expenditure	7775.1	7671.5
	(3021.9)	(2802.0)
Current Service Expenditure	4663.9	4506.6
	(2054.1)	(1908.7)
Capital Outlays	1107.5	1152.2
	(1608.2)	(1630.4)
Panel B: District Demographics		
Fall Enrollment	3536.7	3121.5
	(12608.2)	(9650.6)
Number of Title I Formula Children	740.4	510.1
	(3685.4)	(1604.4)
Proportion of Title I Formula Children (/5-17 population)	0.181	0.150
	(0.101)	(0.0172)
Proportion of White Students	0.728	0.789
	(0.270)	(0.217)
Proportion of Black Students	0.0688	0.0409
	(0.152)	(0.0908)
Proportion of Hispanic Students	0.129	0.110
	(0.198)	(0.156)
Proportion of Other Race Students	0.0740	0.0596
	(0.129)	(0.0892)
Proportion of ELL Students	0.0483	0.0409
	(0.0889)	(0.0707)
Proportion of Special Education Students	0.140	0.141
	(0.0531)	(0.0496)
Observations	128,717	31,534

Notes: Descriptive statistics are based on districts receiving Title I revenue from the 2008–09 through 2017–18 academic years. Districts that receive Title I but are not included in the Common Core of Data are excluded. All dollar amounts are adjusted to constant 2017 school-year dollars. Financial variables are winsorized by capping values that exceed 1.5 times the 95th percentile at that threshold. Column (1) presents the full sample; Column (2) restricts the sample to districts within a 3 percentage point bandwidth of the Concentration Grant eligibility threshold (15 percent). Means are unweighted. For the full sample, enrollment-weighted proportions of White, Black, and Hispanic students are 52.3%, 15.3%, and 23.8%, respectively, which align more closely with national averages. Mean values are reported with standard deviations in parentheses.

Table 3: Covariate Balance Test

	Proportion of Students						**Number of Students**	
	(1) Black	(2) Hispanic	(3) White	(4) Other Race	(5) ELL	(6) Special Ed	(7) Total Enrollment	(8) Title I Kids
RD Estimates	0.001	-0.003	0.001	0.001	-0.001	-0.000	586.694**	94.119**
	(0.003)	(0.002)	(0.003)	(0.001)	(0.001)	(0.001)	(249.915)	(41.219)
Observations	31,429	31,429	31,429	31,429	31,429	31,429	31,429	31,429
Mean	0.0404	0.110	0.791	0.0593	0.0406	0.141	2826.6	465.4

Notes: Estimates use the indicated student covariate as the dependent variable, controlling for state and year fixed effects. The sample consists of districts within 3 percentage points of the 15 percent Concentration Grant eligibility threshold. The regression discontinuity design does not include additional district covariates, producing more conservative estimates. Standard errors are clustered at the state level and reported in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4: First Stage: Effects of Title I Formula Cutoffs on Title I Revenues

	Per Eligible Title I		**Per Pupil Title I**	
	(1) FE Estimates	(2) Non-parametric	(3) FE Estimates	(4) Non-parametric
RD estimates	84.614***	90.397***	7.905**	7.160**
	(8.206)	(9.251)	(3.777)	(3.110)
Observations	31,429	[13,319, 13,112]	31,429	[13,324, 13,116]
Mean	1301.9		218.1	
Bandwidth		[2.515, 2.515]		[2.517, 2.517]

Notes: This table reports estimates of the effect of Concentration Grant eligibility on Title I revenue. Columns (1) and (3) report OLS estimates with state and year fixed effects using districts within 3 percentage points of the 15 percent Concentration Grant eligibility threshold. Columns (2) and (4) report nonparametric regression discontinuity estimates using the bias-corrected local polynomial approach of Calonico et al. (2014) with data-driven bandwidth selection. For nonparametric estimates, the observation counts are reported as [left of cutoff, right of cutoff] and bandwidths as [left bandwidth, right bandwidth] in percentage points. Columns (1) and (2) measure Title I revenue per eligible child; Columns (3) and (4) measure Title I revenue per pupil. All dollar amounts are adjusted to constant 2017 school-year dollars. All specifications control for district demographics (racial composition, special education, and English learner proportions) and student enrollment. Standard errors are clustered at the state level and reported in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5: Effects on District Finance (Per Pupil)

	(1)	(2)	(3)	(4)
Panel A: Revenues				
	Total Revenue	Federal Revenue	State Revenue	Local Revenue
RD Estimates	182.145	14.878	97.727	42.858
	(133.579)	(12.152)	(68.749)	(93.814)
N	31,429	31,429	31,429	31,429
Mean	15,278.7	1,021.7	7,093.0	7,053.8
Panel B: Expenditures				
	Elem/Sec Exp	Instruction	Services	Capital Outlays
RD Estimates	148.928	85.165	68.927	-85.163**
	(94.979)	(54.630)	(42.826)	(31.874)
N	31,429	31,429	31,429	31,429
Mean	12,739.3	7,675.8	4,509.4	1,150.3

Notes: This table reports reduced-form estimates of the effect of Concentration Grant eligibility on district revenues (Panel A) and expenditures (Panel B). All models are OLS with state and year fixed effects, using districts within 3 percentage points of the 15 percent Concentration Grant eligibility threshold. All specifications control for district demographics (racial composition, special education, and English learner proportions) and total enrollment. All dollar amounts are adjusted to constant 2017 school-year dollars and measured on a per-pupil basis. Standard errors are clustered at the state level and reported in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 6: Robustness Check: *rdrobust*

	(1)	(2)	(3)	(4)
Panel A: Revenues				
	Total Revenue	Federal Revenue	State Revenue	Local Revenue
Formula Cutoff	142.305	16.207	105.702*	-0.749
	(101.889)	(10.174)	(61.365)	(94.442)
N	[17,290, 16,811]	[16,900, 16,465]	[15,463, 15,079]	[15,627, 15,221]
Bandwidth	[3.275, 3.275]	[3.201, 3.201]	[2.912, 2.912]	[2.947, 2.947]
Panel B: Expenditures				
	Elem/Sec Exp	Instruction	Services	Capital Outlays
Formula Cutoff	125.646	78.523*	57.280*	-70.742***
	(78.474)	(44.426)	(34.718)	(24.709)
N	[15,072, 14,711]	[14,370, 14,087]	[16,407, 15,948]	[14,413, 14,132]
Bandwidth	[2.838, 2.838]	[2.709, 2.709]	[3.096, 3.096]	[2.719, 2.719]

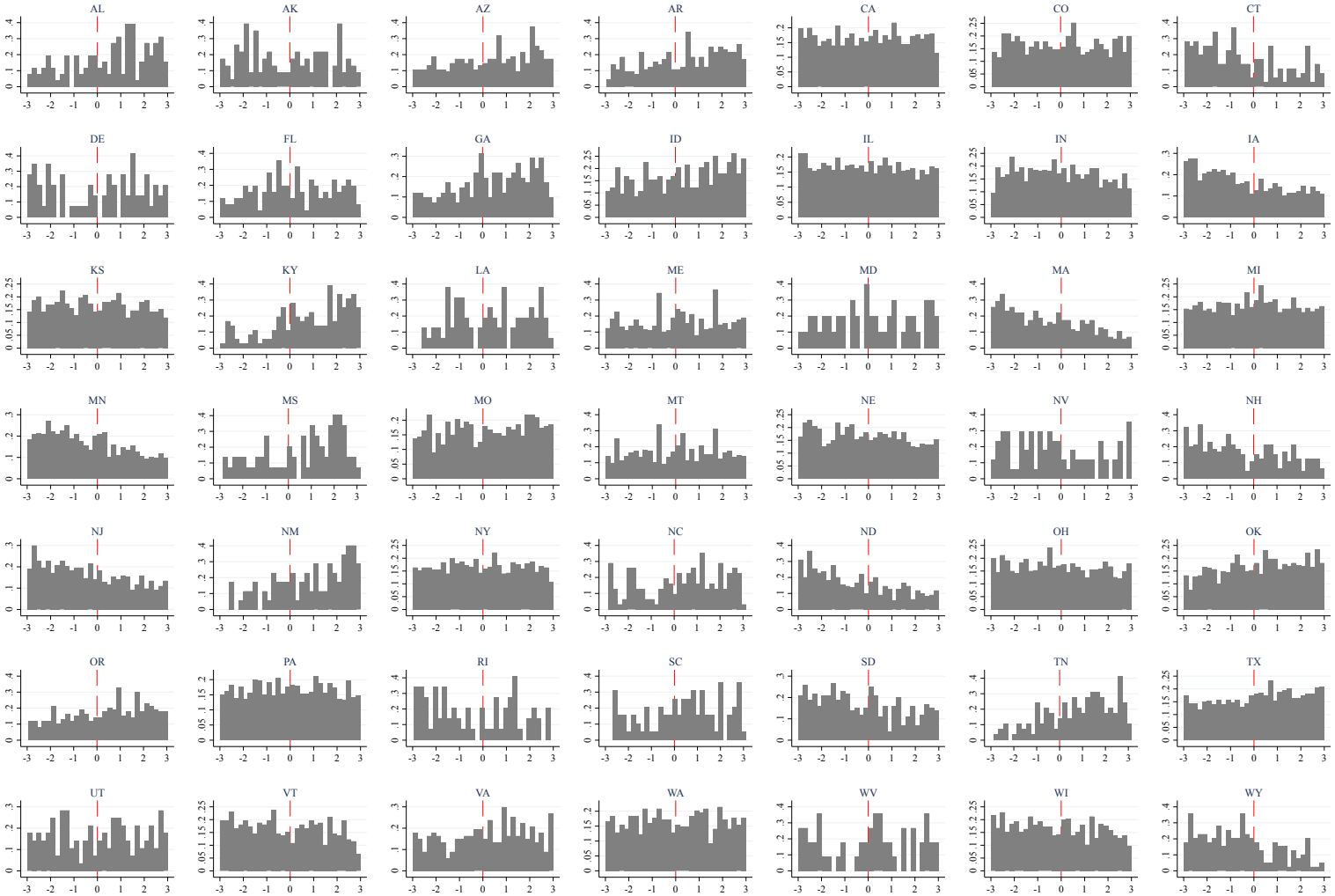
Notes: This table reports nonparametric regression discontinuity estimates of the effect of Concentration Grant eligibility on district revenues (Panel A) and expenditures (Panel B), using the bias-corrected local polynomial approach of Calonico et al. (2014) with data-driven bandwidth selection. All specifications control for state and year indicators, district demographics (racial composition, special education, and English learner proportions), and total enrollment. Observation counts are reported as [left of cutoff, right of cutoff] and bandwidths as [left bandwidth, right bandwidth] in percentage points. All dollar amounts are adjusted to constant 2017 school-year dollars and measured on a per-pupil basis. Standard errors are clustered at the state level and reported in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 7: Capital Subcategories and Financing

	(1)	(2)	(3)	(4)
Panel A: Capital Outlay Subcategories				
	Construction	Land & Existing Structures	Instructional Equipment	Other Equipment
RD Estimates	-55.818**	-3.704*	0.202	-3.815
	(26.171)	(1.917)	(1.974)	(5.151)
N	31,429	31,429	31,429	31,429
Mean	754.9	30.06	65.01	203.9
Panel B: Capital Financing				
	Long-term Debt Issued	St. Revenue for Capital & Debt	Interest on Debt	
RD Estimates	-81.630**	-6.601**	-7.151	
	(34.955)	(2.984)	(6.422)	
N	31,429	31,429	31,429	
Mean	900.8	92.08	292.5	

Notes: This table decomposes the capital expenditure result into subcategories (Panel A) and examines related capital financing variables (Panel B). Panel A reports estimates for four capital outlay subcategories: construction, land and existing structures, instructional equipment, and other equipment. Panel B reports estimates for long-term debt issued during the fiscal year, state revenue designated for capital outlay and debt services, and interest on debt. All models are OLS with state and year fixed effects, using districts within 3 percentage points of the 15 percent Concentration Grant eligibility threshold. All specifications control for district demographics (racial composition, special education, and English learner proportions) and total enrollment. All dollar amounts are adjusted to constant 2017 school-year dollars and measured on a per-pupil basis. Standard errors are clustered at the state level and reported in parentheses. ($^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.)

Figure A1: Density of the Running Variable by State



Note: Histograms of the proportion of eligible children (centered at the 15 percent Concentration Grant threshold) for each state. The vertical red line marks the cutoff.

Notes: These panels show histograms of the running variable—the proportion of Title I eligible children—centered at the 15 percent Concentration Grant eligibility threshold for each state. The vertical red line marks the cutoff at zero. The figure provides a visual check for potential sorting or manipulation around the eligibility threshold.

Table A1: Additional Sensitivity Analysis

Panel A. Alternative Bandwidths

	Main (TWFE 3pp)		BW 1pp		BW 5pp	
Total Revenue	182.145	(133.579)	127.325	(212.128)	101.435	(94.403)
Federal Revenue	14.878	(12.152)	31.619	(20.212)	4.637	(10.687)
State Revenue	97.727	(68.749)	116.807	(98.786)	67.698	(57.208)
Local Revenue	42.858	(93.814)	-5.822	(177.860)	26.242	(70.429)
Elem/Sec Exp	148.928	(94.979)	111.500	(144.967)	103.231	(68.736)
Instruction	85.165	(54.630)	62.370	(74.682)	53.416	(40.062)
Services	68.927	(42.826)	52.923	(70.006)	53.777*	(31.941)
Capital Outlays	-85.163**	(31.874)	-102.722**	(40.467)	-40.432	(24.431)

Panel B. Donut Hole Regression Discontinuity

	Donut 1pp		Donut 0.5pp		Donut 0.3pp		Donut 0.1pp	
Total Revenue	262.065**	(119.002)	129.340	(110.093)	184.389	(114.172)	150.496	(100.480)
Federal Revenue	33.358	(27.481)	-11.729	(15.564)	18.089*	(9.460)	11.653	(8.151)
State Revenue	-7.910	(138.121)	74.116	(80.731)	144.364**	(62.033)	79.670	(62.437)
Local Revenue	442.543**	(196.475)	77.603	(87.272)	32.076	(93.022)	57.010	(75.869)
Elem/Sec Exp	595.434**	(302.383)	123.196	(122.481)	181.917**	(92.127)	101.221	(75.293)
Instruction	388.787**	(175.527)	95.617	(83.496)	124.875**	(53.805)	55.683	(42.107)
Services	239.943**	(110.688)	45.227	(47.683)	79.919**	(38.368)	48.689	(39.172)
Capital Outlays	-146.647	(97.484)	-53.315	(51.124)	-48.677	(33.381)	-41.915*	(24.190)

Notes: This table reports additional sensitivity analyses for the main revenue and expenditure outcomes. Panel A presents OLS estimates with state and year fixed effects using alternative bandwidths of 1 and 5 percentage points around the 15 percent Concentration Grant eligibility threshold, alongside the main 3 percentage point estimates for comparison. Panel B presents donut hole specifications using the nonparametric regression discontinuity approach of Calonico et al. (2014) with data-driven bandwidth selection, which exclude observations within 1, 0.5, 0.3, and 0.1 percentage points of the cutoff. Panel A specifications control for state and year fixed effects, district demographics (racial composition, special education, and English learner proportions), and total enrollment. Panel B specifications control for state and year indicators, district demographics, and total enrollment. All dollar amounts are adjusted to constant 2017 school-year dollars and measured on a per-pupil basis. Standard errors are clustered at the state level and reported in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A2: Covariate Sensitivity Analysis

	(1) No covariates	(2) Demographics Only	(3) Full Controls (Baseline)
Panel A: First Stage			
Title I per Eligible	86.408***	86.488***	84.614***
	(8.171)	(8.111)	(8.206)
Title I per Pupil	7.472*	7.762**	7.905**
	(4.008)	(3.766)	(3.777)
Panel B: Revenue			
Federal Revenue	11.639	11.377	14.878
	(13.662)	(12.014)	(12.152)
State Revenue	85.899	84.569	97.727
	(73.447)	(69.953)	(68.749)
Local Revenue	3.988	10.572	42.858
	(95.458)	(93.968)	(93.814)
Panel C: Expenditure			
Elem/Sec Exp	108.971	109.010	148.928
	(99.295)	(94.221)	(94.979)
Instruction	63.969	65.484	85.165
	(56.556)	(54.788)	(54.630)
Services	52.580	51.328	68.927
	(44.729)	(42.303)	(42.826)
Capital Outlays	-86.823***	-85.977***	-85.163**
	(31.401)	(31.614)	(31.874)
State, Year FE	X	X	X
Demographics		X	X
Enrollment			X

Notes: This table examines the sensitivity of the main results to the inclusion of covariates. Column (1) includes only state and year fixed effects with no district-level covariates. Column (2) adds district demographic controls (racial composition, special education, and English learner proportions). Column (3) is the baseline specification, which additionally controls for total enrollment. Panel A reports first-stage estimates for Title I revenue per eligible child and per pupil. Panel B reports revenue outcomes and Panel C reports expenditure outcomes, all measured on a per-pupil basis. All models are OLS with state and year fixed effects, using districts within 3 percentage points of the 15 percent Concentration Grant eligibility threshold. All dollar amounts are adjusted to constant 2017 school-year dollars. Standard errors are clustered at the state level and reported in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A3: Fuzzy Regression Discontinuity Results

Panel A: Per eligible Title I first stage			
	Capital Outlays	State Revenue	Local Revenue
RD Estimates	-0.866**	1.411*	-0.150
	(0.350)	(0.788)	(1.159)
First stage	86.94***	88.54***	84.56***
	(7.522)	(10.98)	(10.50)
N	[25103, 23668]	[12837, 12750]	[15317, 14933]
Bandwidth			
Panel B: Per pupil Title I first stage			
	Capital Outlays	State Revenue	Local Revenue
RD Estimates	-12.03*	15.39*	2.564
	(6.216)	(9.095)	(11.50)
First stage	6.996**	7.258**	7.013**
	(3.065)	(3.188)	(2.841)
N	[19086, 18427]	[16532, 16067]	[21083, 20184]
Bandwidth	[3.604, 3.604]	[3.124, 3.124]	[3.980, 3.980]

Notes: This table reports fuzzy regression discontinuity estimates using the approach of Calonico et al. (2014) with data-driven bandwidth selection. Panel A uses per-eligible-child Title I revenue as the endogenous variable; Panel B uses per-pupil Title I revenue. Conventional point estimates are reported with robust bias-corrected inference. The first-stage estimates and corresponding standard errors are also reported for each specification. Observation counts are reported as [left of cutoff, right of cutoff] and bandwidths as [left bandwidth, right bandwidth] in percentage points. All specifications control for state and year indicators, district demographics (racial composition, special education, and English learner proportions), and total enrollment. All dollar amounts are adjusted to constant 2017 school-year dollars. Standard errors are clustered at the state level and reported in parentheses.

Table A4: Capital Subcategories and Financing (rdrobust)

Panel A: Capital Outlay Subcategories				
	Construction	Land & Existing Structures	Instructional Equipment	Other Equipment
RD Estimates	-48.040**	-5.719***	0.428	-1.754
	(23.407)	(1.788)	(1.804)	(4.989)
N	[14328, 14046]	[11937, 11928]	[11359, 11403]	[16521, 16054]
Bandwidth	[2.702, 2.702]	[2.270, 2.270]	[2.162, 2.162]	[3.122, 3.122]
Panel B: Capital Financing				
	Long-term Debt Issued	St. Revenue for Capital & Debt	Interest on Debt	
RD Estimates	-66.253	-5.989**	-12.636**	
	(41.965)	(2.673)	(6.424)	
N	[16706, 16212]	[17444, 16958]	[15376, 14987]	
Bandwidth	[3.162, 3.162]	[3.306, 3.306]	[2.895, 2.895]	

Notes: This table reports nonparametric regression discontinuity estimates corresponding to the main results in Table 7, using the bias-corrected local polynomial approach of Calonico et al. (2014) with data-driven bandwidth selection. Conventional point estimates are reported with robust bias-corrected inference. Panel A reports estimates for four capital outlay subcategories; Panel B reports estimates for capital financing variables including long-term debt issued during the fiscal year, state revenue designated for capital outlay and debt services, and interest on debt. All specifications control for state and year indicators, district demographics (racial composition, special education, and English learner proportions), and total enrollment. Observation counts are reported as [left of cutoff, right of cutoff] and bandwidths as [left bandwidth, right bandwidth] in percentage points. All dollar amounts are adjusted to constant 2017 school-year dollars and measured on a per-pupil basis. Standard errors are clustered at the state level and reported in parentheses.

Table A5: Lagged Eligibility and Capital Expenditure

	(1)	(2)
	Current Capital Expenditures	
Current		-68.604***
		(21.154)
L1 RD estimates	-100.574**	-96.369**
	(46.176)	(46.177)
N	28074	28074

Notes: This table examines whether lagged Concentration Grant eligibility predicts current capital expenditure. The sample is restricted to districts within 3 percentage points of the 15 percent cutoff based on the year $t-1$ running variable. Column (1) estimates a regression discontinuity specification using year $t-1$ eligibility and the corresponding running variable. Column (2) adds an indicator for current-year eligibility as an additional control; this indicator does not have its own RD structure and should be interpreted as a conditional association rather than a separate RD estimate. All models include state and year fixed effects and the same set of district-level covariates as the main specification. All dollar amounts are adjusted to constant 2017 school-year dollars and measured on a per-pupil basis. Standard errors clustered at the state level are reported in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.