CENTRALIZED OR DECENTRALIZED FUNDING?
THE CASE OF TITLE I

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ABSTRACT

The merits of decentralized public goods provision have long been debated by policymakers and academics alike. Although decentralization can improve outcomes and raise productivity, it may hurt the disadvantaged if local elites are able to capture public resources. This paper sheds new light on the decentralization debate in the context of Title I, the largest U.S. federal education funding program. Title I funds can be delivered in two ways: a centralized form called 'targeted assistance' and a decentralized form known as the 'schoolwide program.' I incorporate these two delivery mechanisms into a model featuring the key tension between resource capture by local elites and the benefits of decentralization. The model draws attention to three informative dimensions of comparison: centralized Title I versus no Title I; decentralized Title I versus no Title I; and decentralized Title I versus centralized Title I. I then use regression discontinuity designs to estimate all three comparisons using data from California for the 2008-11 period. Prior research implicitly compared non-Title I schools to schools receiving Title I in a centralized form and found that Title I had negligible effects on student achievement. My results indicate that the negligible impact of Title I is likely caused by the centralized nature of the funds: in its decentralized form, Title I generates a substantial improvement in student achievement, particularly for the socioeconomically disadvantaged. The findings suggest that policymakers should both lower the current decentralization threshold and reconsider the inflexible mandates embedded in the delivery of centralized funds.

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I. Introduction

Whether public services should be provided in a centralized or decentralized form is a matter of policy concern and of keen academic debate. On the one hand, the decentralized provision of public services can increase productivity based on local information[1] (Hoxby, 1999; Bardhan and Mookherjee, 2005) and improve allocative efficiency by creating a better match between government outputs and local preferences (as in Tiebout (1956), Oates (1999) and Besley and Coate (2003)). On the other hand, decentralization may degrade the provision of public services due to public resource capture by local elites (Bardhan and Mookherjee, 2005, 2006).

In the empirical literature, a clear consensus has yet to emerge regarding the arguments for and against decentralization. Attempts to identify the effects of decentralization consist largely of comparisons across country (Davoodi and Zou, 1998; De Mello, 2000), across state (Akai and Sakata, 2002), and across time (Zhang and Zou, 1998; Faguet, 2004). A serious estimation problem lingers in the background, however, stemming from the endogeneity of the decentralization decision, as well-managed institutions are more likely to decentralize than poorly-managed ones. The innovative study by Clark (2009) applies a research design to deal directly with the endogeneity of the decentralization decision, comparing schools whose parents narrowly consented to become autonomous from local authorities to schools whose parents barely rejected autonomy. He finds that the decentralization of school finances in the United Kingdom generated substantial improvements in student achievement, though his paper does not discuss the effects of decentralization on public resource capture, which features prominently here.[2]

In this paper, I explore decentralization in an important new context, examining the effects of decentralization in the case of Title I, the largest U.S. federal education program. Title I of the Elementary and Secondary Education Act was authorized by President Johnson in 1965 and gave additional federal resources to schools with high rates of student poverty. As its stated goal was to improve the outcomes of the disadvantaged, policymakers responded to fears that Title

[1]Increased local information associated with decentralization can improve the productivity of public goods in distinct ways. In Hoxby (1999), local information in the form of property prices reduces the ability of producers to extract rents, while in Bardhan and Mookherjee (2005), local information reduces bureaucratic corruption.

[2]Supporting the need to look at both the merits and costs of decentralization, the empirical literature on the decentralization of school decision-making finds decentralization to generate positive effects for the student body as a whole (Galiani et al., 2002; Woesmann, 2003; Pena and Sole-Olle, 2009; Naper, 2010), but to have adverse consequences for disadvantaged students (Galiani et al., 2002; Reinikka and Svensson, 2004; Galiani et al., 2008).

I might be captured by local elites by centralizing the program to ensure Title I "would target on the disadvantaged, as it was intended to do" (Ginsburg and Cooke, 1976). Much later, in response to President Reagan's decentralization initiatives, Title I funds started to be delivered at the school level in two forms in 1988: a centralized form called 'targeted assistance' and an alternative decentralized provision called 'the schoolwide program.' While the schoolwide program does not affect the level of a school's Title I funding, it can alter the within school distribution of Title I funds – and the effects of this within school decentralization are the focus here.[3]

This paper sheds new light on the circumstances in which decentralization is advantageous. As a first step, I develop a model of education funding that features the key trade-off between the benefits of decentralization and the capture of public resources by the local elite.[4] In the model, the planner wishes to improve the plight of disadvantaged students through the allocation of federal funds. The additional resources improve student achievement; however, under decentralization, a large proportion of federal funds may be captured by advantaged students and their families. In schools with low poverty rates, nearly all public resources are seized by the advantaged, making centralized delivery beneficial. As poverty rates increase, however, the degree of public resource capture becomes progressively less severe. Thus, the planner only allows schools with poverty rates exceeding some threshold to receive funds in the decentralized form, rationalizing the current rule that only schools with poverty rates above forty-percent can opt-in to the schoolwide program. The model also highlights three relevant dimensions along which comparisons can be made: centralized funds compared to no funds, decentralized funds compared to no funds, and decentralized funds compared to centralized funds. I show that if differences in outcomes along all three dimensions are known, two key model parameters are identified, measuring the combined loss of productivity and allocative efficiency under centralization and the severity of public resource capture by local elites, respectively.

Following the compelling study by Van der Klaauw (2008), credible estimates of the effectiveness of Title I have been recovered using regression discontinuity designs (Weinstein et al., 2009;

[3]There is some evidence that districts redirected Title I funds away from their intended use in the neediest schools. For example, the authors in Cascio et al. (2013) suggest that in the 1960s school districts in the South diverted Title I funds intended for disadvantaged black schools to white schools. However, the decentralization tradeoff at the district level is not explicitly explored in this paper.

[4]Hoxby (1995) also examines the equity-efficiency trade-off in public good provision. In her model, decentralization promotes efficiency in the production of the public good at the expense of equity in public good consumption. Her framework focuses on across-school equity, while I concentrate on within-school equity.

Matsudaira et al., 2012). This recent literature finds that Title I provides minimal, if any, improvement in student achievement. Implicit in these designs is a comparison of schools receiving Title I in a centralized form to non-Title I schools,[5] which – as noted – is only one of three possible empirical comparisons. I estimate all three points of comparison, including the relatively unstudied one involving schools with centralized versus decentralized federal funding,[6] which is the main focus of this paper. The three empirical estimates are subsequently used to recover structural parameters from my model.

I use California data for the 2008-11 period[7] to investigate the effects of decentralization on Title I. My analysis identifies the causal effect on student achievement of schools adopting the decentralized funding program relative to being mandated to receive centralized funds. To that end, I use a fuzzy regression-discontinuity (RD) design[8] to control for school selection into the decentralized funding mechanism. Specifically, I exploit a discontinuity in the rule that determines decentralized funding eligibility: schools with poverty rates above a threshold of forty percent are eligible to opt-in to receive their funds in a decentralized form while schools below the threshold must receive their funding in the centralized form. The intuitive idea is that schools near the threshold have similar observable and unobservable characteristics, while their funding mechanism status differs, providing the basis for utilizing a RD approach to identify causal impacts. While the threshold is known, empirical tests show that – in the subset of schools for which manipulation of the poverty rate is difficult – the poverty rate does not appear to be manipulated in order to receive decentralized funding, supporting the comparability of schools around the threshold.

I show that the decentralization of Title I improves student test scores significantly – in math, by about two-thirds of a standard deviation and in English by about one-third of a standard deviation,

[5]This implicit comparison stems from the fact that the schoolwide program was not widely used during the time period examined in all the above studies (1996-2003). In the U.S., schoolwide program use among eligible Title I high schools rose from 10% in 1994-95 to 58% in 2004-2005, with much of this increase occurring after the reauthorization of Title I in 2002 (Stullich, 2007). In my sample, eighty-three percent of eligible Title I middle and high schools opt in to the schoolwide program. Additionally, the short-run effects of the schoolwide program may not be representative of the program's long-term effects due to high initial implementation costs.

[6]The literature on the schoolwide program is largely confined to descriptive surveys and case studies (see Wong and Meyer (1998) for a review). An exception is Robey (2011), who compared outcomes between schools in the schoolwide program versus schools in targeted assistance in the Chicago Metropolitan Area. No significant differences in student test scores were found, but the analysis did not take into account the fact that schoolwide program schools differ systematically from targeted assistance schools.

[7]California is used due to its data availability and large sample size.

[8]Specifically, a fuzzy RD design is used due to imperfect compliance to the threshold rule and to account for the fact that not all schools choose to opt-in to the decentralized funding mechanism.

although smaller effects cannot be ruled out due to large standard errors. Additionally, all estimates should be construed as a local average treatment effect, which may not be representative of the impact of decentralized federal funding for all schools. Of note, the results point to even greater improvements for the socioeconomically disadvantaged student subgroup.

In placing the significant effects of Title I decentralization alongside the prior literature, I propose a new explanation for the seemingly negligible effects of Title I. Prior work suggests that the ineffectiveness of Title I may be due to school resources not affecting student achievement (Hanushek, 1998) or Title I crowding out other state and local funding sources (Gordon, 2004; Matsudaira et al., 2012). While these channels likely contribute to Title I's ineffectiveness, if they were the sole explanations, then comparing non-Title I schools to any school receiving Title I should generate negligible results. Using a regression-discontinuity design to compare non-Title I schools to decentralized Title I schools, I show that decentralized Title I generates large improvements in student achievement. The most likely explanation for the ineffectiveness of federal funds under centralization involves wide-ranging federal regulations, which force school administrators to sacrifice productivity through high compliance costs and undertake ineffective classroom policies – for instance, creating disadvantaged student-only classes.

Consistent with this, estimated model parameters indicate that centralized funds are highly inefficient.[9] Based on the parameter estimates, over ninety percent of Title I funds are squandered when delivered in the centralized form relative to the decentralized form. Estimates based on the model also suggest that over seventy percent of decentralized Title I funds are captured by the advantaged. Nonetheless, decentralization improves the performance of the disadvantaged due to the extreme inefficiency of centralization. In fact, the estimates suggest that the achievement gap between the advantaged and disadvantaged narrows under decentralization.

This paper provides evidence that, at the current threshold, decentralization substantially raises student achievement, suggesting policymakers should lower the decentralization threshold. Given that centralized funds consist of approximately twenty percent of California's $1.7 billion in Title I funding, providing more funds in the decentralized form is likely to generate significant gains in student achievement. However, lowering the threshold exacerbates public resource capture by

[9]Centralized funding can be inefficient relative to decentralized funding due to a loss of either productive or allocative efficiency. In this paper, the two types of inefficiencies cannot be disentangled; therefore, the inefficiency parameter combines both forms.

local elites. Balancing the two, I use the estimated parameters to determine that the achievement of the disadvantaged is maximized when the decentralization threshold is set to a school poverty rate of approximately fifteen percent. Moreover, the striking extent of waste under centralized funding suggests that policymakers should reconsider the onerous federal mandates embedded in the centralized mode of provision and their continued use.

The rest of the paper is organized as follows: The next section describes the Title I funding program. Section III sets out the model, and Section IV provides an empirical framework for the study and discusses the data set used. Results are presented in Section V and are placed in a broader context in Section VI. Section VII concludes.

II. Institutional Background

In 1964, President Johnson proposed a 'War on Poverty' in order to address a national poverty rate hovering around nineteen percent. Education was a cornerstone of this policy, as policymakers argued that children from lower socioeconomic backgrounds were disadvantaged in two respects. First, they often had fewer non-school learning opportunities. Second, they tended to enroll in schools that, for a variety of reasons, were less able to provide support and enriching experiences relative to more affluent schools (Coleman et al., 1966). Title I of the Elementary and Secondary Education Act was passed in an attempt to decrease the gap in school quality and reduce inequality by providing additional funding to the nation's poorest schools. To alleviate concerns that Title I funds would be used to benefit students of higher socioeconomic standing, policymakers centralized Title I by forcing schools to use Title I funds exclusively for students from disadvantaged backgrounds. The centralized model was thereafter called 'targeted assistance,' as the funds were targeted exclusively at eligible students.

The distribution of Title I funds from the federal government to individual schools has remained more or less the same over time and can be thought of as involving a three-stage allocation procedure. First, Title I funds are allocated to each State Education Agency by multiplying the number of school-aged children from low-income families by the average educational expenditure in the state. Next, the State Education Agency allocates funding to Local Education Agencies (LEAs) using similar criteria to the federal government. Third, the LEAs distribute the Title I funds to schools in their district. To be eligible for any Title I funds, a school must report the number of socioeconomically disadvantaged children enrolled to the LEA. Schools are then ranked according to the percentage of the student body that is socioeconomically disadvantaged, and funds are allocated according to that ranking so that, within a district, higher poverty schools receive an equal or higher per-disadvantaged student Title I allocation than lower poverty schools.[10] Title I allocations are approximately linear in the number of disadvantaged students at a school, because a majority of schools receive Title I allocations on a strict per-disadvantaged pupil basis, though some districts do provide additional funding for schools with very high poverty rates.[11] To receive any Title I funding, a school must either have a poverty rate greater than the district average or in excess of thirty-five percent, though some exemptions exist.[12]

In the 1980s, a push from the Reagan administration to reduce government regulation and devolve control to local jurisdictions led to the development of the so-called 'schoolwide program' when Title I was reauthorized in 1988. The schoolwide program was voluntary and allowed schools opting in to receive their Title I funds without conditions attached. To prevent schools from using Title I for the benefit of the socioeconomically advantaged subgroup, an eligibility threshold was set. In particular, a school with a poverty rate less than seventy-five percent had to receive Title I funds in their centralized form. Once a school's poverty rate exceeded seventy-five percent, the school could opt into the schoolwide program and receive its Title I funds in a decentralized form.

This eligibility threshold has been lowered subsequently, most recently as part of the No Child Left Behind Act of 2001, to a student poverty rate of forty percent.[13] Nationally, the lowering of the eligibility threshold and increased awareness of the program among administrators has caused the percentage of eligible schools opting into the schoolwide program to increase from ten percent in 1994-1995 to fifty-eight percent in 2004-2005 (Herrin, 2011). A detailed description comparing Title

[10] For the thirty percent of California LEAs that are unified, the per-disadvantaged student allocations can be varied by grade span.

[11] In Section III, Title I allocations are assumed to be on a strict per-disadvantaged pupil basis. The fact that some schools with very high poverty rates receive additional district funding is immaterial here because this paper focuses on schools with poverty rates around or below the decentralization threshold of forty percent.

[12] The most significant exemption is the grandfather rule which allows schools currently receiving Title I to be eligible for an additional year if they drop below the eligibility criteria. Other exemptions include desegregation provisions and exceptionally poor school performance. These same exemptions apply to receiving Title I in its decentralized form.

[13] Schoolwide program eligibility does not guarantee Title I eligibility. High poverty districts may set Title I eligibility thresholds in excess of the schoolwide program threshold of forty percent. For example, the Title I eligibility threshold in the San Juan Unified School District for 2010-11 was set by the district at seventy percent, meaning any school below a seventy percent poverty rate did not receive Title I.

I under targeted assistance and the schoolwide program is provided in Appendix A. In essence, targeted assistance forces schools to prove that all Title I funds are used solely for the benefit of disadvantaged students. In contrast, the schoolwide program allows schools to directly incorporate Title I funds into their regular budget.

A school's poverty rate is key to the analysis that follows in this paper. Typically, the number of socioeconomically disadvantaged children is calculated by using Free and Reduced Price Meals (FRPM) eligibility.[14] Other measures to calculate the number of socioeconomically disadvantaged children at a school are rarely used. For example, schools may also count students from a family eligible for Temporary Assistance for Needy Families (TANF) or Medicaid as socioeconomically disadvantaged. However, as the threshold for FRPM eligibility is more generous than these programs, students from a TANF or Medicaid family are automatically enrolled in FRPM through the State.[15] The other method of calculating the school poverty rate utilizes school attendance area Census data. In this method, a child who resides in the school's attendance area but does not attend the school can be counted toward the school's poverty rate. However, Census data are not available at the school attendance zone level so the use of this measure appears exceedingly difficult in practice. Henceforth, this paper will use FRPM eligibility to determine poverty rates due to its prevalence in measuring student poverty.

III. Conceptual Framework

In this section, I propose a theoretical model of federal resource allocation to capture the key trade-off between the allocative and productivity enhancing benefits of decentralization versus resource capture by local elites. Following the current practice under Title I, federal funds are allocated to each school based on a linear function of the number of disadvantaged students. Given these allocations, the planner decides whether the funds are provided in a centralized or decentralized manner. When funds are decentralized, advantaged students can capture a significant share of the resources intended for the disadvantaged. Centralization alleviates this moral hazard problem,

[14] In a 2004-05 sample of 281 districts, ninety-two percent of schools use FRPM eligibility for poverty rate calculations (Chambers et al., 2009).

[15] Enrollment in FRPM is automatic for many state or federal assistance program, including Healthy Families, Medi-Cal, Medicaid, State Children's Health Insurance Program, TANF, Food Stamps, Food Distribution Program on Indian Reservations, and CalWorks.

but imposes both a direct bureaucracy cost and an indirect compliance cost in that schools must meet inflexible mandates, likely leading to resource misallocation.[16]

There are S schools, each with N students. Each school has public resources R_s at its disposal. Each student must receive a minimum amount of public resources r_{min} by law. There are two types of students: a total of D socioeconomically disadvantaged, indexed by d, and A advantaged, indexed by a. The two student types are mutually exclusive, so $A + D = N$. Schools differ in demographic composition across the two types. The proportion of disadvantaged students within a school, $\frac{D}{N}$, is henceforth referred to as a school's poverty rate. The achievement technology for student $i \in \{a, d\}$ is given by:[17]

$$t_i = f_i(\alpha_i, r_i),\ i \in \{a, d\} \tag{III.1}$$

where t_i is the achievement of a type-i student, α_i represents both student ability and private resources devoted to a type-i student,[18] and r_i are the public resources devoted to a type-i student. The function $f_i(\cdot)$ is strictly increasing and concave in its arguments.

The planner, in keeping with the spirit of Title I, wants to aid the disadvantaged.[19] To that end, I assume that the planner cares solely about the achievement of the disadvantaged. After allocating r_{min} to each student, each school s has some additional public resources remaining which are topped-up by a federal resource allocation of R^{Fed} per disadvantaged student. All the additional resources at the disposal of school s are then targeted to specific students to give them an 'achievement boost.'

In the first-best solution, the planner is able to costlessly allocate the federal resources to

[16]In the context of centralized Title I, the bureaucracy costs and resource misallocations are described in Appendix A.

[17]The achievement technology presented excludes peer effects. Peer effects can be added by specifying the achievement technology as $t_i = f(\alpha_i, r_i, \bar{t}_s)$ where $\bar{t}_s$ is the average achievement at school s. However, the peer effect portion will not affect parameter estimation due to the linearity assumption. Thus, for simplicity, peer effects are excluded.

[18]In essence, α_i captures all factors influencing student achievement excluding public resources. This includes ability, investment by parents and broader non-public investment such as community investment. α_i may be a substitute for or complement to public resources.

[19]The stated goal of Title I is to narrow the achievement gap between the advantaged and disadvantaged. In practice, this can be accomplished either through helping the disadvantaged or harming the advantaged. As Title I is not intended to impair the advantaged, its goals are likely adequately captured by having the planner care only for the disadvantaged.

individual students within every school. Hence, for each school s, the planner solves:

$$\max_{\{r_a, r_d\}} Dt_d, \qquad \text{subject to:} \qquad Ar_a + Dr_d \leq DR^{Fed} \tag{III.2}$$

where R^{Fed} is the total federal resources allocated per disadvantaged student. In the first-best case, the planner's solution is:

$$Ar_a = 0; \qquad r_d = R^{Fed} \tag{III.3}$$

Thus, in the first-best case, the planner allocates all federal resources to the disadvantaged. This optimum is infeasible because the school-level resource allocation is determined by each school, which causes a proportion of the federal resources to be allocated to advantaged students because of resource capture, described below. The planner can prevent advantaged students from receiving federal resources under centralization, but loses some fraction of federal resources in bureaucracy and misallocation costs.

III.A. The Decentralized and Centralized Cases

Under decentralization, involved parents, who are disproportionately advantaged, are assumed to capture the school's additional resources. This assumption is supported by two pieces of evidence. First, parental school involvement is closely related to socioeconomic status (Lee and Bowen, 2006). Second, advantaged parents use their additional bargaining power "in crafty ways to secure the best of what schools have to offer for [their] own children" (Brantlinger, 2003). The model coalesces these two facts: the disproportionate involvement of advantaged parents in school decision-making gives them more bargaining power, allowing them to systematically reallocate school resources to their children.[20]

Formally, under decentralization public resources in excess of the total minimum allocation requirement, Nr_{min}, are allocated within each school s among the children of involved parents.[21] Each parent becomes involved in the school allocation decision with some probability. Let p_a and p_d

[20]While I model the public resource capture by the advantaged through parental involvement, any situation where advantaged students get disproportionately more resources than disadvantaged students under decentralization generates the moral hazard problem that is key to the model.

[21]Hence, each school s allots additional resources of $R_s - Nr_{min} + DR^{Fed}$ to involved parents.

denote the respective probabilities that parents of advantaged and disadvantaged students become involved in the school allocation decision. Involvement probabilities are strictly positive and are closely related to socioeconomic position, so $p_a \geq p_d$. All children of the involved parents receive the same fraction of the additional public resources.[22] The fraction of additional resources allocated to the disadvantaged is given by $\frac{D*p_d}{D*p_d+A*p_a}$.

For a fixed relative probability of involvement, $\frac{p_a}{p_d}$, Figure I graphs the fraction of additional resources distributed to the disadvantaged in school s as a function of that school's poverty rate. For any $p_a > p_d$, the advantaged capture proportionately more public resources than the disadvantaged. The problem gets exacerbated as $\frac{p_a}{p_d}$ increases; for example, when $\frac{p_a}{p_d} = 2$ and $\frac{p_a}{p_d} = 5$ a school with a poverty rate of forty percent allocates twenty-five percent and twelve percent of the additional public resources to the disadvantaged, respectively. Additionally, the convex nature of the relationship means the disadvantaged receive few resources when the poverty rate is low. For instance, a school with a poverty rate of ten percent and $\frac{p_a}{p_d} = 2$ only allocates five percent of its additional resources to the disadvantaged.

The achievement of the disadvantaged under decentralization is given by:

$$D * (1 - p_d) * f_d(\alpha_d, r_{min}) + D * p_d * f_d\left(\alpha_d, r_{min} + \frac{R_s - Nr_{min} + DR^{Fed}}{p_a * A + p_d * D}\right) \tag{III.4}$$

If the planner chooses to centralize R^{Fed} for school s, then school s must earmark all federal resources *solely* for the disadvantaged. However, a fraction of the federal resources, governed by the parameter $\psi \in [0, 1]$, are absorbed as a compliance cost,[23] capturing the feature that these centralized resources are used to monitor compliance and can be misallocated. The achievement of

[22] This assumes that involved parents get the same additional resources regardless of socioeconomic background. This is supported in Lee and Bowen (2006), where parents with different characteristics involved at the school derive the same benefit in terms of their children's achievement.

[23] For simplicity, both ψ and $\frac{p_a}{p_d}$ are assumed to be independent of the poverty rate. In a more complex model, the intuitive assumptions are that ψ is decreasing with the poverty rate, as schools have more of their student body being Title I eligible, and $\frac{p_a}{p_d}$ is increasing with the poverty rate because disadvantaged parents could become discouraged by their minimal representation in the richer schools. Both of these assumptions would suggest a *lower* optimal decentralization threshold. Therefore, the independence assumptions cause an overestimate of the optimal decentralization threshold.

the disadvantaged in this case is given by:

$$D * (1 - p_d) * f_d (\alpha_d, r_{min}) + D * p_d * f_d \left(\alpha_d, r_{min} + \frac{R_s - N r_{min}}{p_a * A + p_d * D} + \frac{(1-\psi) D R^{Fed}}{p_d * D}\right) \quad \text{(III.5)}$$

III.B. Second-Best Solution

The planner, facing a trade-off between the resource capture under decentralization and the inefficient nature of centralization, chooses whether to centralize or decentralize the federal resources in school s by comparing the achievement of the disadvantaged in Equation III.5 to Equation III.4. Whether a planner chooses to centralize or decentralize in a given school will depend on the school's poverty rate. The second-best solution for the planner is described in Proposition 1 below. Formal proofs are provided in Appendix B.

Lemma 1 *The achievement of the disadvantaged increases monotonically under decentralization relative to centralization as the poverty rate increases.*

Proof. As the poverty rate rises, resource capture falls while the inefficiency of centralization remains constant. The costs of decentralization to the disadvantaged is thus falling while its benefits are (relatively) constant. The Lemma follows immediately. ■

Proposition 1 *For any* $0 < \psi < 1$, *a unique optimal decentralization threshold exists.*

Proof. The proof contains three parts. First, for a school with a poverty rate near zero, centralization is always optimal due to extreme resource capture. Second, decentralization is always optimal for a school with a poverty rate near a hundred percent as the resource capture problem disappears. Combining the above two parts with Lemma 1 implies the existence of a fixed point. Uniqueness follows from the monotonicity in Lemma 1. ■

By Proposition 1, the planner maximizes the achievement of the disadvantaged by specifying a decentralization threshold. The optimal threshold is set at the school poverty rate when the achievement of the disadvantaged under centralization is equal to their achievement level under decentralization. To illustrate, the resulting achievement levels under various federal funding schemes are graphed in Figure II,[24] where the decentralization threshold will be optimally set at a school poverty rate of twenty-five percent.

The advantaged are indifferent between no federal funding or centralized funding as in each case they receive no additional resources, but they are always better off under decentralization as they capture some federal resources. In contrast, the disadvantaged are always better off receiving centralized federal funding than no funding. Depending on the school's poverty rate, the disadvantaged may be better or worse off under decentralized compared to centralized funding. For the above parameters, disadvantaged students are better off under decentralization only if the school's poverty rate exceeds twenty-five percent.

To visualize how the optimal decentralization threshold varies with ψ and $\frac{p_a}{p_d}$, Figures IIIa and IIIb plot the achievement level of the disadvantaged for $\psi \in [0,1]$ and $\frac{p_a}{p_d} \in [1,5]$ under various decentralization thresholds.[25] Intuitively, an increase in ψ raises the benefit of decentralization, making the optimal threshold fall. This is seen in Figure IIIa where the decentralization threshold that maximizes the achievement level of the disadvantaged falls as ψ increases. Conversely, Figure IIIb shows that the decentralization threshold that maximizes the achievement level of the disadvantaged rises as $\frac{p_a}{p_d}$ increases, because a higher $\frac{p_a}{p_d}$ makes decentralization costlier by increasing the fraction of captured resources. The effect of R^{Fed} on the optimal decentralization threshold is ambiguous and depends on the relative magnitude of the cost of centralization and the degree of resource capture.[26]

III.C. Parameter Identification

The parameters of interest are the cost of centralization, ψ, and the involvement probabilities of the advantaged relative to the disadvantaged, $\frac{p_a}{p_d}$.[27] The model suggests the existence of three

[24] Figure II is graphed for the following model parameters: $f_{i\in\{a,d\}}(\cdot) = (\alpha_i^\rho + r_i^\rho)^{\frac{1}{\rho}}$, $\rho = -2$, $p_a = 0.5$, $p_d = 0.4$, $\psi = 0.2$, $\alpha_a = 1.2\ \forall a$, $\alpha_d = 1\ \forall d$, $r_{min} = 1$, $R_s = 1100$, $R^{Fed} = 0.25 * D$ and $N = 1000$. The school-level funding levels, R_s and R^{Fed}, are measured in r_{min} units, with r_{min} normalized to one.

[25] Both Figures IIIa and IIIb are based on one thousand schools with poverty rates uniformly distributed between zero and a hundred percent.

[26] Formally, if $1 - \psi > \frac{p_d * D * \psi}{p_a * A}$ then the optimal decentralization threshold falls (and vice versa). For a one-unit increase in R^{Fed}, the left-hand represents the increased resources to the disadvantaged under centralization, while the right-hand side represents their increased resources under decentralization multiplied by the increased waste of centralization induced by the additional unit of R^{Fed}.

[27] p_a and p_d cannot be separately identified, because identification comes from the fraction of funds that is allocated to the disadvantaged, which only depends on $\frac{p_a}{p_d}$.

empirical points of comparison, implying that each parameter can be estimated twice using different empirical moments. To identify the parameters, N, D, $f(\cdot)$, R^{Fed} and α_d, $\forall d$ must be known. The ability distribution is unknown in practice, but if $f(\cdot)$ is assumed to be linear, then the α's are no longer required for identification. While this assumption may be strong, linearity allows for the estimation of two parameters with three equations, allowing for a check on the reliability of the model by estimating each parameter twice.

From Figure IIa, the respective β's highlight the differences between the achievement levels under the different funding schemes for a given poverty rate: β_1 compares centralized funding to no funding, β_2 compares decentralized funding to no funding, and β_3 compares decentralized funding to centralized funding. Assuming linearity, ψ is identified using the difference in the achievement of the disadvantaged under centralized funding relative to no funding. The difference is given by:

$$\frac{\beta_1}{p_d} = \frac{(1-\psi)DR^{Fed}}{p_d * D} \tag{III.6}$$

The relative involvement probability, $\frac{p_a}{p_d}$, is identified through the difference in the achievement of the disadvantaged under decentralized versus no funding, which is given by:

$$\frac{\beta_2}{p_d} = \frac{DR^{Fed}}{p_a * A + p_d * D} \tag{III.7}$$

Third, in conjunction with Equations III.6 and III.7, both model parameters are also identified through the difference in the achievement of the disadvantaged under decentralized versus centralized funding, given by:

$$\frac{\beta_3}{p_d} = \frac{DR^{Fed}}{p_a * A + p_d * D} - \frac{(1-\psi)DR^{Fed}}{p_d * D} \tag{III.8}$$

In Section V, estimates of β_1, β_2 and β_3 are generated. Using these estimates, I then recover the two model parameters, ψ and $\frac{p_a}{p_d}$. Both model parameters are estimated twice using different equations. Using the estimated model parameters, estimates of β_3 are generated for various poverty levels. Since the decentralization threshold that maximizes the achievement of the disadvantaged occurs when $\beta_3 = 0$, the optimal decentralization threshold is determined. Estimates are provided in Section VI.A, following the results section.

IV. Econometric Strategy

The effect of Title I and the schoolwide program on student achievement is ultimately an empirical question. I apply two very similar regression discontinuity (RD) designs: a schoolwide program RD design and a Title I RD design. The schoolwide program design identifies the impact of receiving Title I in its decentralized, relative to centralized, form. The Title I design follows the prior literature (Van der Klaauw, 2008) and identifies the impact of receiving Title I versus no Title I funds. Because the Title I design has been extensively used, this paper focuses its discussion on the schoolwide program design.

To estimate the effect on a school's average student achievement by receiving Title I via the schoolwide program relative to targeted assistance, we wish to estimate:

$$y_{st} = \alpha + \beta_{SWP}\mathbb{1}\{SWP_{st}\} + \phi X_{st} + \epsilon_{st} \tag{IV.1}$$

for school s at time t, where y_{st} is a school-level academic achievement measure, $\mathbb{1}\{SWP_{st}\}$ is an indicator for whether school s operates a schoolwide program at time t, X_{st} is a set of student and school characteristics, and ϵ_{st} is the error term. The parameter of interest is β_{SWP} and represents the effect of the schoolwide program on student achievement. As the schoolwide program only operates for Title I schools, the effect of the schoolwide program is identified relative to targeted assistance.[28]

The major econometric concern with Equation IV.1 is that the choice to operate a schoolwide program is endogenous. It is not random which schools operate a schoolwide program as schools have the option to continue to receive targeted assistance. For example, only technically proficient schools may have the confidence to opt-in to the schoolwide program. If these schools also have better student outcomes, OLS estimates of β_{SWP} will be upward biased. Hence, $\hat{\beta}_{SWP}$ will not provide the causal impact of the schoolwide program on student achievement. To address the endogeneity problem, a regression-discontinuity approach is used, which exploits a policy rule affecting whether or not a school can operate a schoolwide program. Under the No Child Left Behind Act, schools can operate a schoolwide program only if their poverty rate exceeds forty percent.[29]

[28]Hence, in the schoolwide program design, only Title I schools are included in the analysis.

[29]Schoolwide program-eligible schools that choose to continue to receive Title I in the centralized form are remark-

The essence of the empirical strategy is to compare students' outcomes in schools with poverty rates slightly less than forty percent to those with slightly more. To illustrate the idea, consider a school with a poverty rate of thirty-nine percent to one with a poverty rate of forty percent. It is unlikely that schools with a poverty rate of thirty-nine percent differ much from schools with a poverty rate of forty percent. However, due to the policy rule, the school with the forty percent poverty rate is eligible to opt-in to the schoolwide program, while the school with a thirty-nine percent poverty rate is not. As the schools are likely to be similar in other dimensions, we can compare outcomes between the two schools to examine the effect of the schoolwide program. In implementation, this neighborhood around the cutoff is expanded to gain more precise estimates. As the cutoff expands, possible relationships between the poverty rate and student achievement may appear, making it is necessary to control for the poverty rate within the school through some function. To keep with the spirit of comparing schools close to the cutoff, all the below regressions are estimated within a bandwidth determined by Imbens and Kalyanaraman (2012) optimal bandwidth procedure. Further, all equations will be estimated using local linear estimation allowing for different functions on either side of the threshold.[30]

As the schoolwide program is voluntary, the policy rule is not perfectly adhered to, making it necessary to implement a fuzzy regression-discontinuity design (Imbens and Lemieux, 2008; Lee and Lemieux, 2010) in order to obtain an estimate of β_{SWP} that has a causal interpretation. This method is a 2SLS estimation where I first estimate:

$$\mathbb{1}\{SWP_{st}\} = \alpha_1 + \delta_{SWP}\mathbb{1}\{Above40_{st}\} + \gamma g(FRPMrate_{st} - c) + \phi X_{st} + \epsilon_{st} \tag{IV.2}$$

where c is the cutoff for schoolwide program eligibility, $g(FRPMrate_{st} - c)$ is a flexible control for the school's poverty rate, and $\mathbb{1}\{Above40_{st}\}$ is an indicator for the school's poverty rate being greater than or equal to forty percent. The parameter of interest in this first-stage equation is δ_{SWP}. This parameter captures whether the policy rule induces some schools to opt into the schoolwide program that would have otherwise received targeted assistance.

ably similar to schools with similar poverty rates opting to receive Title I in its decentralized form. It is thus unclear why an eligible school would refuse the decentralized form of Title I since the costs are minimal and the benefits appear substantial.

[30]For brevity, the interaction term between the poverty rate and the threshold dummy – which allows for different functions on either side of the threshold – is suppressed in this section.

Next, the potentially endogenous regressor in Equation IV.1, $\mathbb{1}\{SWP_{st}\}$, is instrumented with its predicted value from Equation IV.2. This gives a reduced-form equivalent of Equation IV.1:

$$y_{st} = \alpha_{RF} + \beta_{RF}\mathbb{1}\{\widehat{SWP_{st}}\} + \gamma f(FRPMrate_{st} - c) + \phi X_{st} + \epsilon_{st} \tag{IV.3}$$

The reduced-form effect, β_{RF}, indicates the causal effect of the schoolwide program compared to targeted assistance on student achievement. The fuzzy RD design identifies the local average treatment effect (LATE)[31] for schools close to the forty percent poverty threshold. As these schools must receive Title I to be in the sample, and must be close to the threshold, the reduced-form estimates provided may not reflect the average treatment effect, nor can they be generalized to other schools that are not near the poverty threshold. These reduced form estimates, however, provide empirical inputs to estimate parameter values from the theoretical model presented in Section III.

For the Title I research design, I follow a similar 2SLS approach. Equation IV.2 becomes

$$\mathbb{1}\{TitleI_{st}\} = \alpha_2 + \delta_2\mathbb{1}\{AboveCutoff_{st}\} + \gamma g(FRPMrate_{st} - c) + \theta\mathbb{1}\{GrantType_{st}\} + \phi X_{st} + \epsilon_{st} \tag{IV.4}$$

where $\mathbb{1}\{TitleI_{st}\}$ is an indicator for whether the school received Title I, $\mathbb{1}\{AboveCutoff_{st}\}$ is an indicator for whether the school is above the district-specific cutoff and $\mathbb{1}\{GrantType_{st}\}$ is a Title I grant type eligibility indicator (basic, concentrated, or targeted) used as an additional control.[32] These district-specific cutoffs are defined as the minimum of the district average FRPM rate or thirty-five percent.[33] Next, taking the predicted value from Equation IV.4, this value is used similarly to Equation IV.3 to get a reduced-form effect of Title I by regressing:

$$y_{st} = \alpha + \beta_{TitleI_{RF}}\mathbb{1}\{\widehat{TitleI_{st}}\} + \gamma f(FRPMrate_{st} - c) + \theta\mathbb{1}\{GrantType_{st}\} + \phi X_{st} + \epsilon_{st} \tag{IV.5}$$

[31] Identification of the LATE also requires a monotonicity assumption that schools crossing the eligibility threshold do not become more likely to choose targeted assistance. This assumption holds by construction in this case because schools have only one funding option before crossing the threshold – targeted assistance.

[32] The grant type indicator controls for the fact that schools become eligible for concentrated and targeted grants at a poverty rate of ten and fifteen percent, respectively. These grants increase the amount of per Title I student funding to the school.

[33] Explicit district cutoffs are defined for the ten largest school districts, who explicitly announce Title I cutoff rules. These school districts often announce a Title I cutoff greater than thirty-five percent. This correction improves the first stage of the regression-discontinuity, but does not significantly alter the results.

where $\beta_{TitleI_{RF}}$ is the parameter of interest and represents the reduced-form effect of the school receiving Title I on student achievement. The reduced-form effect identifies a LATE. Accordingly, the effect of Title I is identified from schools receiving Title I in the form of targeted assistance as schools close to the district cutoff are rarely eligible for the schoolwide program.

IV.A. Data

The data for both research designs are drawn from three sources. The first source, the California Department of Education (CDE), provides Free and Reduced Price Meal eligibility rates, school characteristics, teacher characteristics and student characteristics. For the Title I research design, the CDE also provides a direct measure of the school district average FRPM. In addition, the CDE provides outcome measures through the Standardized Testing and Reporting (STAR) results. These mandatory state-administered tests are reported for schools if at least ten students took the test in that grade for that year,[34] implying small schools are not included in the sample.[35] The National Center for Education Statistics (NCES) provides data on whether a school received Title I funding and whether it was in the form of targeted assistance or the schoolwide program. Additionally, it provides data on student characteristics including Free and Reduced Price Meal eligibility rates.

Unfortunately, the reported Free and Reduced Price Meal (FRPM) eligibility rates differ at times between the CDE and NCES data sources. While the measure from the CDE is preferred, as it is what is used to determine eligibility, at times the public CDE data source has a missing or a zero value for some schools because they did not report their FRPM rate to the CDE in time for the annual October data release.[36] As this variable is key to my regression-discontinuity design, any measurement error may cause a significant loss in efficiency. To alleviate this concern, a third data source is used, Ed-Data.[37] With these data sources, I construct the FRPM variable using the

[34] Test results with less than ten students are not reported due to privacy concerns.

[35] Schools designated special education were also excluded as they have different curriculum and performance standards than other California schools. While they use different instructional strategies (such as independent study, community-based education and flexible scheduling), alternative schools were included as they face identical curriculum and performance measures as traditional schools.

[36] A failure to report the FRPM in a timely manner to the state results in a missing or zero value for the FRPM variable in the CDE data. Since Title I allocations are made at the district level, late reporting to the state is unlikely to affect Title I allocations.

[37] Ed-Data is a collaboration of the California Department of Education, EdSource and the Fiscal Crisis & Management Assistance Team. It uses data from these sources to provide access to information to education stakeholders.

CDE measure as the school FRPM measure which can be overridden by the NCES measure if the CDE and Ed-Data measure disagree substantially.[38] About ten percent of observations receive a correction to their FRPM variable; however, the results are not sensitive to these changes. These corrections are predominantly for schools with a missing or non-sensible zero FRPM rate in the CDE data. The construction of a singular FRPM variable is not necessary as all three FRPM variables could be used simultaneously as instruments. However, this complicates the graphical analysis of the regression discontinuity design and for this paper the constructed FRPM variable is used. Table VII.B reports the main results of the paper if all three measures of FRPM are used simultaneously as instruments in the 2SLS estimation procedure described in Section IV. The constructed FRPM variable delivers nearly identical results to using all three FRPM measures simultaneously as instruments.

The different data sources for this study were matched using the school identification code and school-year. STAR test results are reported at the test-grade-year level. To compress the STAR test results into a single schoolwide subject measure, all California Standards Tests and Califonia Modified Assessment Tests[39] taken in a specific school for a specific subject in a given year are collapsed into a single school-year score by weighting them by the number of students examined.

Results are reported for five outcome variables under each subject. The primary outcome of interest is the Mean Scaled Score, which is the arithmetic mean of the scaled scores for all students taking the test without modifications. The scaled scores range from a low of 150 to a high of 600. Scaled scores are determined by the California Department of Education each year and are used to equate the tests from year-to-year and to determine performance levels.[40] There are four possible performance levels: advanced, proficient, basic and below basic.[41] The student is placed into one of the performance categories according to his or her scaled score: below basic (200-300), basic

[38]Specifically, I override the CDE measure under two conditions: (1) when the CDE and NCES measure disagree by greater than two percentage points and the NCES and Ed-Data are within one percentage point, or (2) when a variation in FRPM eligibility within a school of greater than twenty percent is reported between consecutive years and the NCES and Ed-Data measure would not exhibit such a wide variation.

[39]Two additional tests are given in California: the California Alternate Performance Assessment and the Standards-based Tests in Spanish. These tests were excluded as they were taken by relatively few students and are only given to students with learning disabilities or with limited English comprehension. In addition, these tests do not contribute to a school's accountability measure.

[40]This is done by changing half of the test questions from the previous year and adjusting for any differences in the difficulty of the test caused by the new questions. This allows scaled scores and performance levels to be compared year-to-year.

[41]The CDE actually reports five performance categories, but since so few students perform far below basic, these students are combined with the below basic category.

(300-350), proficient (350-400) and advanced (400-600).[42] The percent of students attaining these scores in a school is used as an outcome to understand the type of students affected.

Table I presents average school characteristics for non-Title I and Title I schools for the three school years used. These characteristics are at the school level – not the test taker level. However, test taking rates appear continuous around the thresholds (see Figure Vh and Appendix Figure D.IIh) used in this paper and thus school level characteristics are representative of test taker characteristics.[43] School characteristics are presented for middle and high schools only due to the exclusion of elementary schools in the main analysis (see Section V.A below). Due to their exclusion in the main analysis, I only discuss summary statistics for middle and high schools. Approximately 60 percent of middle and high schools receive Title I, with seventy-five percent of those Title I schools being a schoolwide program school. By design, Title I schools have a larger percentage of FRPM-eligible students. This coincides with large differences in racial composition, with Title I schools having about 8 percent less whites and 10 percent more hispanics. Accordingly, Title I schools also have much lower accountability scores measured by API and are less likely to pass the federal accountability scheme (AYP). However, Title I schools do not appear to differ significantly from non-Title I school in average school size, class size, teacher experience, or their test taking rate.

Table II presents summary statistics for the outcome measures analyzed in this paper. These statistics are subdivided into an all student group and a disadvantaged subgroup. Unsurprisingly, the disadvantaged subgroup fares worse in test outcomes in both math and English by about 0.35 to 0.4 of a standard deviation. While students have similar mean scaled scores in both math and English, there is a much higher variance in math scores. Looking at the distribution of student designations, the four designation categories are about equally distributed in English, while math has almost a third of students being designated below-basic and only sixteen percent being designated advanced.

[42] The scaled scores required for an advanced label actually vary slightly year-to-year but generally are around 400.

[43] Specifically, this alleviates any concern that the decentralized funding causes schools to exclude some students from taking the test relative to centralized funding. While these exclusions have been noted in the context of NCLB (see Jacob (2005)), there is no reason NCLB should affect decentralized or centralized schools differentially.

V. Results

The validity of the two regression discontinuity designs introduced in Section IV are discussed. Results are then presented for both designs. Section VI then places these results in the context of the theoretical model in Section III.

V.A. Tests of the Validity of the Regression Discontinuity Design

Because decentralization decreases misallocation and compliance costs, there is an incentive for schools to manipulate their poverty rate to become eligible for the schoolwide program. As schools that manipulated their poverty rate to become schoolwide program-eligible may systematically differ from those that do not, such manipulation may invalidate the RD design.

Section II describes how Free and Reduced Price Meal eligibility (FRPM) is used to calculate school poverty. However, schools have the ability to manipulate the number of FRPM-eligible students if they can identify and encourage FRPM-eligible children who are not enrolled to apply. Other sources of manipulation, such as enrollment manipulation, is difficult in California as all age eligible students[44] within a school's attendance boundary must be able to enroll in that school. Under-age children are enrolled at the school's discretion, but do not count towards a school's poverty rate. Using intra- or inter-district transfers to manipulate poverty levels may be possible but seems unlikely.[45]

Manipulation of FRPM eligibility may be feasible at the elementary school level because parents can be uninformed of the FRPM program when their child begins school. However, once a student is enrolled in FRPM, the following year's FRPM enrollment application is resent directly to the parent's house by the district. Hence, by middle school the child's parents should know whether they are FRPM-eligible, making school level FRPM manipulation challenging.

In order to test whether the school poverty rate is manipulated, a visual check is provided in Figure IVa which plots the distribution of elementary schools by poverty rate. A discontinuity in the density of schools around the forty percent poverty threshold suggests possible manipulation of the forcing variable. The visual inspection gives the impression that there is excess density after the

[44] To be age-eligible a child must be five years of age on or before October 1 of the school year.

[45] Schools do determine the number of transfer spots themselves; however, transfer spots are filled via a district run lottery making precise selection of FRPM-eligible children difficult.

forty percent threshold is crossed, suggesting possible manipulation. However, since elementary schools are hypothesized to be likelier to manipulate their poverty rate, Figure IVb plots the distribution of middle and high schools by poverty rate. This histogram displays no jumps in the density of middle and high schools around the cutoff. A formal test of continuity in the density around the threshold (McCrary, 2008) confirms our visual analysis: the null hypothesis of continuity at the cutoff is rejected when elementary schools are considered. However, when middle and high schools are considered, the null hypothesis of continuity at the cutoff is not rejected. Due to the failure of the density test for elementary schools, elementary schools will be excluded from the sample for the main results. However, results including elementary schools will be provided in tables for comparison purposes.

Another method to determine the validity of the regression-discontinuity design is to check for discontinuities in observable characteristics at the forty percent poverty cutoff. Figure V graphs the mean covariates by school poverty rate for middle and high schools. These covariates include race, class size, school size, teacher experience and whether the school is a charter school. For all covariates, smooth distributions around the cutoff are observed. More formally, Table III estimates the following equation for middle and high schools:[46]

$$Covariate_{st} = \alpha + \delta \mathbb{1}\{Above40_{st}\} + \psi PercentFRPM_{st} + \gamma \mathbb{1}\{Above40_{st}\} * PercentFRPM_{st} + \epsilon_{st} \tag{V.1}$$

Confirming the visual evidence, the variable of interest, δ, is only statistically significant at ten percent for one of the covariates. A seemingly unrelated regression (SURE) is estimated and the null hypothesis that δ is jointly equal to zero for all covariates is tested. The p-value from this test is 0.37, so the null hypothesis cannot be rejected.

A similar check of the observables is also provided for elementary schools in Appendix Figure C.I and Appendix Table C.I. Similar to the middle and high school sample, there are no significant differences in observables. This may indicate that the regression-discontinuity design is valid for all schools even with the failure of the density test.[47] For this reason, results are reported for all schools

[46]For consistency, this equation is estimated for a bandwidth of 18 percent – the same bandwidth selected by Imbens and Kalyanaraman (2012) optimal bandwidth procedure for the main results.

[47]While the failure of the density test is indicative of self-selection and a failure of the regression-discontinuity design, the density test can fail even when there is no failure of identification (McCrary, 2008). This can occur, for

jointly and for middle and high schools separately. In general, results are similar but, due to the failure of the density test, this paper only discusses results for middle and high schools. Finally, for brevity, the validity of the Title I regression-discontinuity design is shown in Appendix Figure D.I and D.II. No excess density is observed in the histogram at the cutoff with the statistical McCrary test not rejecting the null hypothesis of continuity at the cutoff (p-value 0.71). Additionally, the covariates do not appear to have any discontinuities at the threshold.[48]

V.B. First-Stage

For the regression discontinuity to be valid, crossing the forty percent poverty rate threshold must cause some schools to opt into the schoolwide program that would have otherwise received targeted assistance. If this policy is followed, then there will be a jump in the probability of being in the schoolwide program at the forty percent threshold. The statistical significance of δ_{SWP} in Equation IV.2 indicates whether such a jump occurs. Figure VI plots the average share of schools that are in the schoolwide program by school FRPM rates. As predicted, there is a jump in take-up at the forty percent FRPM cutoff. Due to some exceptions to the policy rule,[49] perfect adherence to the policy is not observed. A formal test of the statistical significance of the jump in Figure VI is done by estimating Equation IV.2. These first-stage results are reported above Figure VI. The coefficient is large and significant at the one percent level.

Similarly, for the Title I RD design, Appendix Figure D.III plots the average share of schools receiving Title I by the distance from the district Title I cutoff. There is a significant jump in the figure at the district cutoff. As in the schoolwide program design, adherence to the policy is imperfect but, as reported in Appendix Figure D.III, the first-stage generates a large and statistically significant jump in the probability of a school receiving Title I. Hence, the policy rule in each case generates sufficient variation for the fuzzy RD design.

example, if elementary schools below the threshold send out FRPM application forms, but whether these forms are filled out is random. Then some schools just to left of the threshold would move to the right of the threshold while those to the right of the threshold take no action. This creates additional density to the right of the threshold. In this plausible scenario, schools remain randomized around the threshold while the density test fails.

[48] A SURE test of the null hypothesis that all covariates are jointly equal to zero is tested with a bandwidth of 18 percent and yields a p-value of 0.27.

[49] The most significant of these is the grandfather rule. If the grandfather rule were not in place the number of schools in the schoolwide program to the left of the cutoff would be half the current measure.

V.C. Results of the Schoolwide Program Design

Figure VII indicates a reduced-form relationship between the percent of students on FRPM in a school and mean standardized test scores for math and English. For all outcomes, significantly higher achievement is observed among students in schools above the forty percent decentralization threshold. Point estimates suggests an one to two-ninth standard deviation improvement in both math and English test scores occurs when the decentralization threshold is crossed. These estimates are construed as local intent-to-treat (ITT) estimates because treatment refers to all schools with the option to opt-in to the schoolwide program.

Local average treatment effect (LATE) estimates are obtained by dividing the local ITT point estimates by the estimated increase in the probability of a school operating a schoolwide program determined in the first-stage. LATE estimates represent the effect of Title I decentralization among schools in the schoolwide program. Additional controls are also included. These estimates are reported in the columns marked (2) of Table IV. A two-third standard deviation improvement in math scores is reported for all students while a full standard deviation improvement is reported for the socioeconomically disadvantaged subgroup. For English, a one-third standard deviation improvement is reported for all students while a half standard deviation improvement is reported for the socioeconomically disadvantaged subgroup. Point estimates have large standard errors but are statistically significant at the five percent level.

The magnitude of the estimated increase in student achievement induced by the schoolwide program may appear implausibly large. However, the reader should note that smaller effects cannot be ruled out by the ninety-five percent confidence interval. Additionally, these estimates should be interpreted as a LATE. Decentralization for a school with a poverty rate of forty percent may be particularly advantageous because Title I funds are substantial and elite resource capture may be particularly problematic because advantaged parents are still a majority in the school. Also, many schools around the forty percent poverty rate are on the cusp of passing accountability measures, incentivizing the principal to utilize the decentralized funds to maximize student achievement. Finally, a forty percent poverty rate may be particularly problematic under peer effects as centralized funding can induce schools to create disadvantaged only classes, which could generate significant negative peer effects relative to a decentralized funding counterfactual of (relatively) evenly mixed classes of advantaged and disadvantaged students.

Table IV reports the change in the percentage of students in various achievement categories induced by opting into the schoolwide program. The results indicate the improvement in test scores is driven by the extremes of the achievement distribution. For both English and math, there is a significant improvement in the percentage of students in the advanced category with a corresponding drop in the below-basic category. For math, a reduction of approximately two-thirds of a standard deviation of students in the below-basic category is observed, with a corresponding increase in the advanced and proficient category. For English, a one-third of a standard deviation reduction is observed in the below basic category with a corresponding one-third standard deviation increase in the advanced category.

V.D. Results of the Title I Design

Repeating the above analysis for the Title I RD design, Appendix Figure D.IV provides evidence of the reduced-form relationship between the Title I district cutoffs and mean standardized test scores for math and English. For all outcomes, negative effects are reported. These figures exclude controls; when controls are added, point estimates turn positive but are statistically indistinguishable from zero.

Fuzzy RD point estimates for the Title I design are reported in Table V. For each outcome two columns are reported: column (1) reports the fuzzy RD design from Equation IV.5 with column (2) adding district-year fixed effects. As cutoffs are district-year-specific, a fixed effect specification may be desirable to identify the discontinuity only from variation across district-year cutoffs (Manacorda, 2012). Results for both columns are similar; however, column (2) is more precisely estimated so those results are discussed.

All results for the Title I design are statistically indistinguishable from zero. This result supports the findings of Van der Klaauw (2008) that Title I has little effect on the marginal school. As in Van der Klaauw (2008), because marginal Title I schools are not schoolwide program-eligible, the LATE is identified off schools receiving centralized Title I funds. However, a small subset of schools do have the Title I district cutoff and the schoolwide program cutoff coincide. These special cases and their implications will be discussed in Section VI.

V.E. Sensitivity Analysis

The bandwidth used for both regression-discontinuity designs is determined using the Imbens and Kalyanaraman (2012) optimal bandwidth procedure.[50] Figure VIII tests the sensitivity of estimates for the schoolwide program design with respect to the bandwidth. The figures plot the point estimates for the subject score as a function of the chosen bandwidth. Point estimates do not vary substantially and, as expected, precision improves as the bandwidth increases. A similar result is obtained for the Title I design, and figures are available upon request.

An underlying assumption of the regression-discontinuity design is that outcomes would be continuous through the threshold in the absence of the schoolwide program. No direct test of this assumption is possible, but smoothness of outcomes over placebo FRPM eligibility thresholds may be suggestive of smoothness at the true threshold. Figure IX reports the results of running a sharp RD design[51] over a range of placebo FRPM cutoffs. In general, the plurality of random cutoffs yield an estimate near zero and the discontinuity in outcomes at the forty percent cutoff is greater than the majority of estimates at the placebo cutoffs.[52] Similarly, in the Title I design, the outcome displays smoothness over the placebo cutoffs and figures are available upon request.

Two additional robustness checks are performed for the schoolwide program design. In Table VII.A, different functional forms are used: quadratic, cubic and a triangular kernel. Standard errors increase under these different functional forms, but point estimates are relatively stable and always positive. Next, the validity of the constructed FRPM variable, as described in Section IV.A, is checked using the three FRPM measures simultaneously as instruments. Table VII.B reports the results of running this regression. Results are essentially unchanged. This approach also allows for an indirect test of instrument validity. Since the regression uses three instruments, the validity of the identifying instruments can be indirectly tested using an overidentification test. The test uses one instrument and then checks whether the other instruments are correlated to the error term. A rejection of the null hypothesis would cast doubt on instrument validity, but as the null is not

[50] For consistency and comparability, the bandwidth for each regression-discontinuity design is held constant. Hence, the chosen bandwidth for a single regression may differ slightly from the optimal bandwidth determined through Imbens and Kalyanaraman (2012)'s procedure.

[51] A sharp RD design is required in this instance as a first-stage does not exist for the placebo cutoffs.

[52] Jumps in the outcomes for all students in math and English around a cutoff of thirty-one percent do exceed the jump in outcome at the forty percent schoolwide program threshold. However, estimates from this cutoff have high standard errors and are not significant at the 5% level.

rejected at a five percent level for any regression, the evidence supports the assumption that FRPM is a valid identifying instrument.

VI. Discussion of the Results

The results indicate that schools receiving Title I in the decentralized form do significantly better than schools receiving Title I in the centralized form. These results do not contradict the finding in the prior literature that Title I has minimal effects on student achievement because, relative to this study, prior research on Title I used data from a time period where schools were far less likely to be on the schoolwide program. This paper shows that receiving Title I funds in the decentralized, relative to the centralized, form leads to significant improvements in student achievement. Further, decentralization actually benefits the disadvantaged more than the advantaged, suggesting that decentralization can also narrow the achievement gap – the main goal of Title I. However, empirical estimates are LATEs and thus are only valid around school poverty rates of forty percent for schools receiving Title I.[53]

There are three leading reasons for the minimal impact of Title I funds in the centralized form: crowd-out of other funding sources, the negligible impact of funding on student outcomes, or the centralized nature of the funding. I provide evidence that the latter reason is predominantly responsible for the observed negligible effect of Title I by using a convenient feature of the data, namely that some districts in California have their Title I and schoolwide program cutoffs coincide. Running a regression-discontinuity design on this subset of schools identifies the effect of decentralized Title I relative to no Title I.[54]

Table VI reports the results of a regression-discontinuity design for the subset of districts whose Title I and schoolwide program cutoffs coincide.[55] Appendix Figure E.I provides visual evidence of the first-stage of the design, while Appendix Figure E.II provides visual evidence of the reduced form relationship. Due to the small number of observations, the optimal bandwidth is large and point

[53]Section VI.A uses the LATEs to estimate model parameters and provide credible estimation away from forty percent cutoff.

[54]The subset of districts that have the cutoffs coincide may not be representative of California as a whole as these are some of the largest and poorest school districts in the state.

[55]The three districts this occurs in are Los Angeles Unified, Sacramento Unified, and San Diego Unified. San Diego Unified is used only for the 2008-09 and 2009-10 school years due to the district moving the Title I eligibility threshold in 2010-11.

estimates are imprecise. However, results are statistically significant for math at the ten percent level and at the five percent level for the disadvantaged subgroup. The point estimates suggest that receiving Title I in its decentralized form relative to no Title I gives rise to an improvement in math scores comparable to that from receiving decentralized relative to centralized Title I. If Title I funds were crowded-out or had negligible impacts on student achievement, then we might expect similar results when comparing non-Title I schools to centralized or decentralized Title I schools. Since this is not the case, the minimal impact of Title I on student achievement found throughout the literature is likely caused by the centralized nature of Title I funds.

VI.A. Identifying Model Parameters

In the model developed in Section III, both $\frac{p_a}{p_d}$, the probability of advantaged relative to disadvantaged parents being involved in the school resource allocation decision, and ψ, the inefficiency parameter governing centralized funding, are identified through Equations III.6, III.7 and III.8. For parameter identification, the federal resources allocated to schools per disadvantaged student, R^{Fed}, must be known. In California, Title I allocations average around \$1300 per disadvantaged student compared to average per-pupil funding of \$8000. If the average per-pupil funding is the minimal resource allocation, r_{min}, then federal funds are 0.1625 per disadvantaged student.[56] However, the Title I allocation is likely higher as the average per-pupil funding includes Title I allocations and non-r_{min} school-level funds. The model is estimated for R^{Fed} values of 0.1625, 0.25 and 0.5 per disadvantaged student.

Using the estimated $\beta_{TitleI_{RF}}$ from Section V, Equation III.6 estimates ψ by relating the increase in the achievement of the disadvantaged under centralized Title I relative to no Title I. Similarly, Equation III.7 uses the estimate from Section VI relating the increase in the achievement of the disadvantaged under decentralized Title I relative to no Title I to estimate the ratio $\frac{p_a}{p_d}$. Finally, the estimated β_{SWP} from Section V enters into Equation III.8, which corresponds to the increase in the achievement of the disadvantaged under decentralized relative to centralized Title I. Both ψ and $\frac{p_a}{p_d}$ are estimated from this equation and are denoted by ψ_2 and $\frac{p_a}{p_d}_2$.[57] Because the empirical estimate

[56]Recall that r_{min} is normalized to one and R^{Fed} is measured in r_{min} units. Additionally, R^{Fed} in school s is normalized by the poverty rate by multiplying it by the term $\frac{1000}{N}$.

[57]Estimation of ψ_2 requires the estimated $\frac{p_a}{p_d}$ from Equation III.7, and estimation of $\frac{p_a}{p_d}_2$ requires the estimated ψ from Equation III.6.

used in Equation III.7 is calculated only using the all schools sample, I use the estimated β_{SWP} and $\beta_{TitleI_{RF}}$ from the all school sample. The school poverty rate is set at forty percent because, with the exception of the Title I design,[58] the RD designs give estimates for schools near a forty percent poverty rate. Let μ denote the proportion of federal funds going to the advantaged.[59]

Parameter estimates are reported in Table VIII. The parameters are similar regardless of which equation is used. This is because the empirical estimates concur with each other; so comparing decentralized Title I to both centralized Title I and no Title I yield similar estimates. Since the differences in achievement levels are fixed at their estimated levels, the parameter estimates increase for higher levels of R^{Fed} because more inefficiency and resource capture are required to generate the estimated achievement differences. Estimates of ψ are close to one, indicating that centralized funding is highly inefficient. Estimates of $\frac{p_a}{p_d}$ suggest advantaged parents are considerably more involved in the school resource allocation decision than disadvantaged parents.

The optimal decentralization threshold occurs at a school poverty rate where there is no difference in the achievement of the disadvantaged under decentralization relative to centralization. Given the parameters estimated in Table VIII, Figure X graphs[60] this difference for various levels of R^{Fed}. Regardless of the level of R^{Fed}, the achievement difference between decentralization and centralization is zero around a poverty rate of fifteen percent,[61] suggesting that policymakers should set the Title I decentralization threshold at that level.

VII. Conclusion

Alleviating poverty is a central policy goal in countries throughout the world, with policymakers often trying to tackle the root causes of poverty. For instance, in a U.S. setting, Title I provides additional education funding for the disadvantaged so they can escape poverty, as reflected in President Johnson (1964)'s State of the Union speech: "Very often a lack of jobs and money is

[58]On average, the Title I design estimates are identified near a thirty percent poverty rate. Prior literature using this design (Van der Klaauw, 2008; Weinstein et al., 2009; Matsudaira et al., 2012) found similar estimates at much higher poverty rates, suggesting Title I estimates are similar for different poverty rates.

[59]For brevity, I only report the μ estimated using $\frac{p_a}{p_d}$. Estimates of μ are similar if estimated using $\frac{p_a}{p_d}_2$.

[60]Figure X has its x-axis restricted to school poverty rates below fifty percent for viewing purposes. The lines representing the different funding levels begin to fan out afterwards due to the high costs of centralization for poor schools.

[61]The optimal decentralization threshold ranges between thirteen and fifteen percent depending on the level of federal funding. Unfortunately, standard errors are too high to rule out almost any decentralization threshold as suboptimal.

not the cause of poverty, but the symptom. The cause may lie deeper in our failure to give our fellow citizens a fair chance to develop their own capacities, in a lack of education and training." However, providing additional public resources to schools may be problematic, as funds intended for the disadvantaged can be captured by local elites. In response, policymakers have often tried to eliminate resource capture through the imposition of centralization.

This paper proposed a new model of public good funding that captures the key trade-off between the benefits of decentralization and resource capture by local elites. Since the resource capture is exacerbated in schools with low rates of poverty, I show that a decentralization threshold is optimal, consistent with current practice whereby a threshold of forty percent is observed: schools with poverty rates exceeding the threshold can receive decentralized funding while those below the threshold cannot. Using a regression-discontinuity design, I show that, at the current threshold, decentralization both improves student achievement *and* narrows the achievement gap between advantaged and disadvantaged students. Accordingly, this indicates that policymakers should lower the current decentralization threshold. Further, I provide guidance as to where the new threshold should be situated by simulating my model under its estimated parameters: based on that exercise, the decentralization threshold should be set at fifteen percent.

Looking forward, the analysis suggests that policymakers should carefully consider the consequences of centralization. In the case of Title I, the disadvantaged benefit under decentralization even under substantial resource capture by local elites. This is in all likelihood caused by inflexible mandates imposed by the central planner under centralization, leading to both high bureaucracy costs and resource misallocation. Further research should look beyond the mode of public resource delivery and into the design of centralized mandates: carefully designed mandates may alleviate the burdens of centralization, making it more effective.

APPENDIX

A. Centralized versus Decentralized Title I

The inequality-narrowing intent of Title I motivated policymakers to provide safeguards for the disadvantaged when a school opted to decentralize. A school that opts to decentralize[62] must select a planning team and technical assistance provider[63] to complete a decentralization plan called the 'comprehensive needs assessment.' A successful plan demonstrates how the school will meet the needs of disadvantaged students after the funds are decentralized. The district reviews the plan and, if the plan is satisfactory, approves it. Once approved, the school's Title I funds become decentralized.

After decentralization, the school has discretion regarding the use of Title I funds. In contrast, under centralization, a school must carefully document that Title I funds are used *solely* for Title I students. In order to comply with these requirements, schools have developed two models to direct Title I funding to eligible students only: the in-class and the pullout models.[64]

Under the in-class model, the school uses Title I funding to hire specialized Title I teachers. For this to be an allowable expense, teachers must possess a Title I certificate and teach a class composed of only disadvantaged students. The rationale is that, as these teachers require further certification, they are better prepared to provide for the specific needs of the disadvantaged. Note that the segregation of all Title I students into a single classroom may have significant consequences if peer effects are present.[65] Under the pullout model, schools hire additional math or English coaches to work with both teachers and eligible students. To work with the students in a one-on-one environment, coaches pull eligible students out of regular classes. Education leaders have mentioned this as a point of concern as students miss regular class time; to circumvent this, some schools have implemented after-school tutoring. Often, schools use a combination of these

[62]The decision to decentralize Title I varies from school-to-school, but staff and parent involvement is strongly encouraged. For example, the local planning team which creates the comprehensive needs assessment must include parents and teachers.

[63]The technical assistance provider may be an expert from the district office, the county office of education, an external provider, or a representative from higher education.

[64]See Bruce (2009) for a discussion of these and other Title I targeted assistance delivery methods.

[65]The segregation of students by disadvantaged status induced by centralized Title I and its subsequent effects on student achievement is an avenue of further research I intend to explore. My ability to investigate these peer effects in the California context is limited by the absence of classroom-level data. However, student-level data should allow for an investigation of this mechanism.

two models. Centralized Title I funds can also be used to purchase equipment for disadvantaged students. Schools must prove the equipment is solely for the benefit of disadvantaged students, and the equipment is tracked for five years[66] after purchase to ensure it remains in the use of disadvantaged students only. In practice, the compliance requirements force Title I equipment funds to be earmarked for a Title I classroom or a specific group of disadvantaged students.

Centralized Title I is likely to harm student achievement in several ways. First, schools face onerous compliance requirements that likely directly waste public resources. Second, in order to comply with the inflexible mandates of centralized Title I, schools are often forced to misallocate pupils and teachers across classrooms which may have adverse consequences such as negative peer effects. In addition, restrictions may result in inefficient capital investments as large capital expenditures are often unacceptable as they affect non-Title I students. These costs of centralization are all incorporated into the ψ parameter presented in the theoretical framework in Section III.

[66] The equipment is tracked by the district but is subject to federal reporting requirements. The tracking requirement is for any purchase exceeding $200.

B. Formal Proofs

Lemma 1 *The achievement of the disadvantaged monotonically increases under decentralization relative to centralization as the poverty rate increases.*

Proof. Let N_I be the total number of involved parents such that $N_I = p_a * A + p_d * D$. For a fixed N, as the school's poverty rate increases, N_I decreases because $p_a * A$ decreases faster than $p_d * D$ increases as $p_a \geq p_d$. Take Equation III.5 and subtract Equation III.4 and differentiate this expression w.r.t. N_I. Achievement under decentralization responds more to a decrease in N_I because $\frac{\partial f_d(\cdot)}{\partial r} \frac{R_s - N r_{min}}{N_I^2} < \frac{\partial f_d(\cdot)}{\partial r} \frac{R_s - N r_{min} + DR^{Fed}}{N_I^2}$. Thus, as a school's poverty rate increases, N_I decreases and the achievement for the disadvantaged under decentralization increases more than under centralization. ■

Proposition 1 *For any* $0 < \psi < 1$, *a unique optimal decentralization threshold exists.*

Proof. To prove the proposition I invoke the intermediate value theorem. Let $F = f_d(\textit{decentralized}) - f_d(\textit{centralized})$. First, I show $F(1-\epsilon) > 0$ and $F(\epsilon) < 0$ for some positive ϵ close to zero. For $F(1-\epsilon) > 0$, consider a school with a poverty rate of $1-\epsilon$ for some $\epsilon > 0$. Then, since $\frac{\partial f_d(\cdot)}{\partial r} > 0$ and $\psi > \frac{(1-\psi)p_a \epsilon}{p_d(N-\epsilon)}$ for some ϵ close to zero and $\psi > 0$, it can be shown that:

$$f_d\left(\alpha_d, r_{min} + \frac{R_s - N r_{min}}{p_a \epsilon + p_d(N-\epsilon)} + \frac{DR^{Fed}}{p_a \epsilon + p_d(N-\epsilon)}\right) > f_d\left(\alpha_d, r_{min} + \frac{R_s - N r_{min}}{p_a \epsilon + p_d(N-\epsilon)} + \frac{(1-\psi)DR^{Fed}}{p_d(N-\epsilon)}\right) \quad \text{(B.1)}$$

The left and right-hand side of Equation B.1 represents the achievement of the disadvantaged under decentralization and centralization, respectively. Hence, the disadvantaged always perform better under decentralization in schools with poverty rates close to one. To show $F(\epsilon) < 0$, consider a school with a poverty rate of ϵ. Since $\frac{\partial f_d(\cdot)}{\partial r} > 0$ and $(1-\psi) > \frac{p_d \epsilon}{pa(N-\epsilon)+p_d \epsilon}$ I can show that the disadvantaged always perform better under centralization when $\psi < 1$ and ϵ is close to zero. Finally, Lemma 1 implies that $F' > 0$. This together with $F(1-\epsilon) > 0$ and $F(\epsilon) < 0$ is sufficient to establish the existence of a unique optimal decentralization threshold under the intermediate value theorem.[67] ■

[67] I also require $F(\cdot)$ to be continuous w.r.t. the poverty rate which is true under a large N.

C. Data Appendix

C.A. Constructing Title and SWP Status

C.B. Test Data

I construct outcome measures for each school by aggregating the grade-level test data into an aggregate school level measure. For English, the California Standards Test (CST) is administered in grades 2-11. Since this test's grade span covers all schools, the grade level measure is directly aggregated into a school measure. First, all test scores are normalized to have mean zero and standard deviation of one at each grade level (weighted by the number of test takers in that grade). Then, the school level measure is created by averaging all (normalized) grade level test scores to the school level (weighted by the number of test takers in each grade).

For math, the outcome measure for grades 2-6 is recovered in the same fashion as for English. Grades 7-11 measures, however, are more difficult to obtain because students choose which test math test they take in grades 7-9 (though they are required to take at least one per year over that time period). Further, past grade 9 students are not required to take any math test. In fact, the only math test that must be passed for a student to graduate is Algebra I. However, the state has incentivized schools to encourage their students to the courses in the following order: CST in grade 7, Algebra I in grade 8, Geometry in grade 9 and Algebra II in grade 10. All of these courses must be passed for admission to any university in California.

I then show robustness for this result by showing that results do not dramatically differ if I just use Algebra I a course that must be taken in order to graduate.

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Table I: Summary Statistics by School Type

	All Schools		Title I Schools	
	Full Sample	Middle and High Schools	Full Sample	Middle and High Schools
Mean (S.D.) Among School Characteristics				
% Title I	68.86	60.05	100	100
	(46.31)	(48.98)		
% FRPM	58.43	56.93	71.68	69.54
	(29.61)	(28.30)	(22.01)	(22.45)
% Schoolwide Program	53.47	44.36	78.28	74.80
	(49.88)	(49.69)	(41.23)	(43.42)
% Meeting AYP	38.89	20.67	32.26	16.04
	(48.75)	(40.50)	(46.75)	(36.70)
API	796.6	760.8	772.0	735.2
	(84.3)	(101.5)	(72.7)	(92.9)
% Charter	6.95	13.87	6.04	12.70
	(25.43)	(34.57)	(23.82)	(33.30)
% Alternative Schools [1]	2.53	8.66	1.89	7.59
	(15.69)	(28.13)	(13.61)	(26.49)
% Taking test (Math)	73.22	87.86	72.70	89.45
	(17.74)	(25.88)	(17.04)	(23.94)
% White	28.53	29.97	20.73	21.56
	(25.86)	(25.92)	(22.97)	(22.99)
% Hispanic	48.94	47.58	59.16	57.35
	(29.80)	(28.61)	(27.89)	(27.78)
% Black	6.97	8.15	7.71	9.20
	(11.23)	(12.30)	(12.31)	(13.44)
% Asian	10.17	9.29	7.70	7.52
	(14.34)	(13.27)	(12.01)	(11.74)
School Size	596.2	760.3	585.2	756.3
	(316.5)	(488.1)	(307.5)	(489.6)
Average Class Size	22.79	24.15	22.51	24.04
	(11.90)	(18.24)	(13.83)	(22.60)
Avg. Teacher Experience	13.88	12.83	13.66	12.26
	(3.75)	(3.92)	(3.74)	(3.83)
Observations (School-Year)	21,263	4,924	14,562	2,931

[1] Alternative schools often use different instructional strategies, such as independent study, community-based education and flexible scheduling, but face identical curriculum and performance measures as traditional schools.

Notes: Each school-year for school years 2008-09 to 2010-11 is a separate observation. Sample is limited to observations for schools with non-missing math STAR test data. STAR test results are not reported for tests with 10 or fewer students.

Table II: Summary Statistics for the Outcome Measures

School Outcome Measures Mean(S.D.)		*All Students* Full Sample	*All Students* Middle and High Schools	*Socioeconomically Disadvantaged* Full Sample	*Socioeconomically Disadvantaged* Middle and High Schools
Math	Scaled Score	373.7 (38.43)	343.3 (36.36)	356.8 (28.90)	330.6 (27.34)
	% Advanced	29.99 (17.23)	15.59 (13.19)	-	-
	% Proficient[1]	28.21 (6.56)	26.97 (9.90)	50.38 (15.99)	35.11 (15.72)
	% Basic	21.81 (7.41)	26.47 (7.41)	-	-
	% Below Basic	20.01 (13.96)	30.97 (19.06)	-	-
English	Scaled Score	354.9 (27.59)	349.0 (31.92)	340.7 (18.51)	336.4 (22.97)
	% Advanced	24.79 (15.59)	22.58 (16.22)	-	-
	% Proficient[1]	28.72 (6.28)	27.15 (8.30)	43.90 (13.41)	41.05 (15.66)
	% Basic	26.86 (8.20)	27.35 (8.77)	-	-
	% Below Basic	19.64 (12.91)	22.94 (17.61)	-	-

[1] % proficient refers to only students attaining the proficiency label on STAR for all students but refers to students attaining the proficient and advanced label (i.e. proficient or above) for the disadvantaged.

Notes: Each school-year for school years 2008-09 to 2010-11 is a separate observation. Sample is limited to observations for schools with non-missing STAR test results for that subject. Students are placed into one of four categories: advanced, proficient, basic or below basic. These categories should sum to 100 percent, however may not due to rounding conventions. Only scaled scores and proficient and above categories are reported for the socioeconomically disadvantaged as the other categories are not reported for this subgroup.

Table III: Tests of Discontinuities in Observable Covariates

Student Characteristics	Percent White (1)	Percent Hispanic (2)	Percent Asian (3)	Percent Black (4)	Percent ESL (5)
Percent FRPM<cutoff	-1.39	-2.87	2.29	1.10	-1.97
	(3.26)	(2.87)	(1.57)	(1.67)	(1.50)
Constant (control mean)	48.91***	32.99***	6.96***	4.55***	11.46***
	(2.82)	(2.28)	(1.99)	(0.88)	(1.29)
Observations	625	625	625	625	493
Joint Significance Test (Prob>χ^2)		0.56			
School Characteristics	School Size (6)	Avg Teacher Experience (7)	Percent Charter (8)	Percent Alternative (9)	Percent Taking Test (Math) (10)
Percent FRPM<cutoff	157	0.32	-11.72*	-3.47	6.21
	(118)	(0.67)	(6.32)	(4.59)	(3.91)
Constant (control mean)	644***	13.60***	24.72***	8.45**	86.40***
	(88)	(0.66)	(6.18)	(3.42)	(3.50)
Observations	625	625	625	625	625
Joint Significance Test (Prob>χ^2)		0.15			
Student and School Characteristics					
Joint Significance Test (Prob>χ^2)		0.37			

Notes: Unit of observation is at the school-year level. Estimates are based on local linear regressions allowing for different functions on either side of the cutoff with a bandwidth of 18 percent. The sample is restricted to Title I schools with non-missing math STAR test results. Sample is restricted to middle and high schools only due to possible manipulation of the forcing variable in elementary schools. Percent English learners is missing observations; however the probability of being missing is not discontinuous at the cutoff. The relevant joint significance tests exclude all observations with missing English learner status. The p-values when these observation are included are 0.67 and 0.31 for student characteristics and for student and school characteristics, respectively. Standard errors are clustered at the school level. Significance levels: *** 1 percent; ** 5 percent; * 10 percent.

Table IV: Estimates of Effect of Schoolwide Program on Student Achievement

Outcomes:	Math		English	
	All Schools (1)	Middle & High Only (2)	All Schools (1)	Middle & High Only (2)
A. RD Point Estimates for All Students				
Mean Score	23.28**	25.31**	5.23	11.65**
	(9.70)	(12.67)	(5.52)	(5.57)
Advanced	11.26**	8.33	2.46	5.52*
	(4.88)	(5.09)	(3.73)	(3.13)
Proficient	0.33	6.97*	1.27	1.90
	(1.93)	(3.72)	(1.82)	(1.79)
Basic	-5.14**	-2.80	-1.55	-1.01
	(2.42)	(3.14)	(1.98)	(1.75)
Below Basic	-6.50**	-12.53**	-2.09	-6.25**
	(2.97)	(6.24)	(2.36)	(3.07)
Observations	2,949	625	2,954	628
B. RD Point Estimates for Socioeconomically Disadvantaged Students				
Mean Score	15.84*	33.01***	2.80	16.86***
	(9.62)	(12.29)	(6.20)	(5.40)
Proficient	10.44*	20.39***	-0.31	9.67*
(or above)	(5.34)	(7.82)	(4.88)	(5.34)
Observations	2,808	607	2,813	612

Notes: Column (1) represents all schools while column (2) represents middle and high schools only due to possible bunching for elementary schools. Each cell represents results for a separate local linear regression allowing for different functions on either side of the cutoff. The bandwidth used is 18 percent and was determined with consideration to Imbens and Kalyanaraman (2012) but keeping the bandwidth constant for all regressions for consistency. Controls for student demographics and school characteristics are included. Standard errors are clustered at the school level. Elementary corresponds to grades K-5; and middle and high schools correspond to grades 7-12. Grade 6 can be considered elementary or middle school depending on the school district. K-12 schools are considered middle schools for these results. Students are placed into one of four categories: advanced, proficient, basic or below basic. The sum of the effect of the schoolwide program on these categories should be zero, however may not exactly equate due to rounding conventions. Significance levels: *** 1 percent; ** 5 percent; * 10 percent.

Table V: Estimates of Effect of Title I on Student Achievement

Outcomes:	Math		English	
	(1)	(2)	(1)	(2)
A. RD Point Estimates for All Students				
Mean Score	9.46* (5.65)	5.94 (4.28)	4.51 (2.87)	-0.30 (2.13)
Advanced	4.61 (2.81)	3.58 (2.20)	2.89 (1.89)	-0.02 (1.39)
Proficient	-1.01 (1.07)	-1.71 (1.07)	0.22 (0.91)	0.29 (0.78)
Basic	-1.55 (1.19)	-1.11 (1.03)	-1.52 (1.00)	-0.56 (0.76)
Below Basic	-2.10 (1.72)	-0.77 (1.33)	-1.62 (1.12)	0.28 (0.90)
Observations	5,656	5,656	5,675	5,675
B. RD Point Estimates for Socioeconomically Disadvantaged Students				
Mean Score	5.31 (6.68)	4.86 (5.39)	1.03 (3.74)	0.32 (2.98)
Proficient (or above)	1.96 (3.82)	2.09 (3.08)	0.03 (2.76)	0.16 (2.15)
Observations	5,127	5,127	5,126	5,126
District-Year FE	No	Yes	No	Yes

Notes: Column (1) represents a basic RD specification while column (2) includes district-year fixed effects so the discontinuity is identified only from variation across the cutoff within district-years. Sample is restricted to observations with non-missing outcomes measures and with districts with greater than 5 percent poverty. The latter restriction is due to only districts with poverty greater or equal to 5 percent poverty receive subsequent Title I apportionments which represent the majority of Title I funding. Sample also does not include districts with only one school per grade span or with fewer than 2,500 students due to Title I eligibility being calculated differently for schools in these districts. Each cell represents results for a separate local linear regression allowing for different functions on either side of the cutoff. Controls for student demographics and school characteristics are included. Additional dummies for district poverty being greater than 10 and 15 percent are also included as these are the thresholds for Title I targeted grants and concentration grants, respectively. The bandwidth used is 18 percent and was determined with consideration to Imbens and Kalyanaraman (2012) but keeping the bandwidth constant for all regressions for consistency. Standard errors are clustered at the district level. Students are placed into one of four categories: advanced, proficient, basic or below basic. The sum of the effect of Title I on these categories should be zero, however may not exactly equate due to rounding conventions. Significance levels: *** 1 percent; ** 5 percent; * 10 percent.

Table VI: Estimates of the Effect of Title I on Student Achievement for Districts with Identical Title I and Schoolwide Program Cutoffs

Outcomes:	Math (1)	English (2)
A. RD Point Estimates for All Students		
Mean Score	18.93* (10.03)	7.11 (5.16)
Advanced	8.70* (4.78)	4.09 (3.16)
Proficient	-0.09 (1.71)	0.38 (1.45)
Basic	-3.64* (2.00)	-1.62 (1.77)
Below Basic	-4.87* (2.93)	-2.80 (2.12)
Observations	665	671
B. RD Point Estimates for Socioeconomically Disadvantaged Students		
Mean Score	21.02** (10.65)	6.87 (5.70)
Proficient (or above)	9.96* (5.24)	3.83 (4.10)
Observations	643	643

Notes: Schools included are from Los Angeles Unified, Sacramento Unified, and San Diego Unified (2008-09 and 2009-10 school years only for San Diego Unified schools). Each cell represents results for a separate local linear regression allowing for different functions on either side of the cutoff. Controls for student demographics and school characteristics are included. The bandwidth used is larger than in other designs – 30 percent – due to smaller sample sizes and was determined with consideration to Imbens and Kalyanaraman (2012) but keeping the bandwidth constant for all regressions for consistency. Standard errors are clustered at the school level. Students are placed into one of four categories: advanced, proficient, basic or below basic. The sum of the effect of Title I and the schoolwide program on these categories should be zero, however may not exactly equate due to rounding conventions. Significance levels: *** 1 percent; ** 5 percent; * 10 percent.

Table VII.A: Robustness: Main Result Estimates Under Different Functional Forms

Outcomes:	Math		English	
	All Schools (1)	Middle & High Only (2)	All Schools (1)	Middle & High Only (2)
A. RD Point Estimates for All Students (Mean Score)				
Linear	23.28** (9.70)	25.31** (12.67)	5.23 (5.52)	11.65** (5.57)
Quadratic	14.40 (24.48)	0.34 (11.63)	-10.17 (16.48)	4.68 (9.45)
Cubic	36.81 (26.49)	14.29 (13.84)	7.50 (14.09)	11.78 (10.42)
Triangular Kernel	20.40* (12.21)	13.94 (10.21)	1.01 (7.47)	9.14 (6.24)
B. RD Point Estimates for Socioeconomically Disadvantaged Students (Mean Score)				
Linear	15.84* (9.62)	33.01*** (12.29)	2.80 (6.20)	16.86*** (5.40)
Quadratic	26.38 (24.89)	7.23 (10.10)	7.38 (16.81)	13.93 (9.67)
Cubic	30.15 (23.12)	12.25 (10.88)	19.65 (15.45)	20.67* (10.94)
Triangular Kernel	17.57 (12.26)	21.81** (9.85)	3.56 (8.32)	16.05*** (6.08)

Notes: Column (1) represents all schools while column (2) represents middle and high schools only due to possible bunching for elementary schools. The estimate cell represents results for a local polynomial regression allowing for different functions on either side of the cutoff with a bandwidth of 18 percent. Each row represents a different polynomial used. Controls for student demographics and school characteristics are included. Standard errors are clustered at the school level. Elementary corresponds to grades K-5; and middle and high schools correspond to grades 7-12. Grade 6 can be considered elementary or middle school depending on the school district. K-12 schools are considered middle schools for these results. Significance levels: *** 1 percent; ** 5 percent; * 10 percent.

Table VII.B: Robustness: Using All Three FRPM Measures as IVs

Outcomes:	Math		English	
	All Schools (1)	Middle & High Only (2)	All Schools (1)	Middle & High Only (2)
A. RD Point Estimates for All Students (Mean Score)				
Estimate	21.09**	16.28*	5.48	12.92**
	(8.67)	(8.55)	(4.87)	(5.95)
Overid Test (p-value)	0.9038	0.1863	0.6413	0.1241
B. RD Point Estimates for Socioeconomically Disadvantaged Students (Mean Score)				
Estimate	16.32*	26.67***	4.56	18.35***
	(8.39)	(8.32)	(5.19)	(5.87)
Overid Test (p-value)	0.2462	0.2876	0.9124	0.0691

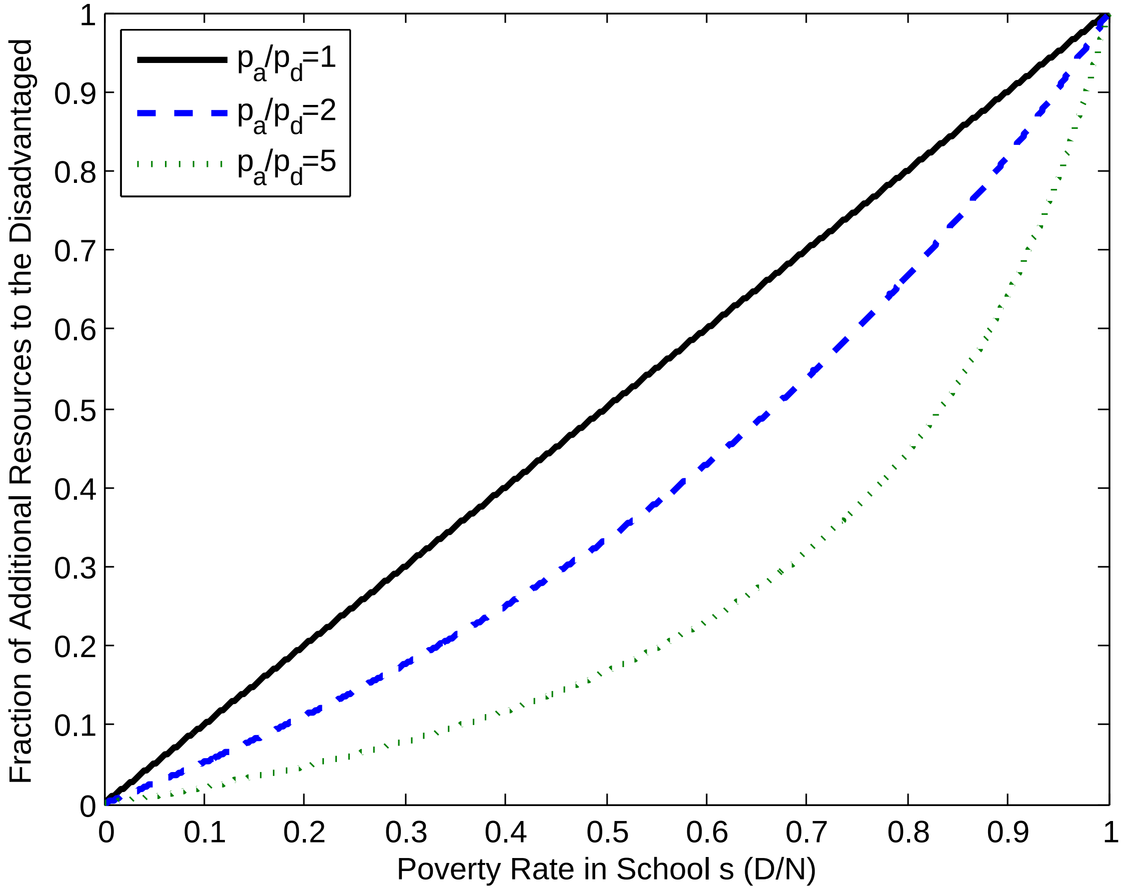
Notes: Column (1) represents all schools while column (2) represents middle and high schools only due to possible bunching for elementary schools. The estimate cell represents results for a local linear regression allowing for different functions on either side of the cutoff with a bandwidth of 18 percent. Controls for student demographics and school characteristics are included. Standard errors are clustered at the school level. Elementary corresponds to grades K-5; and middle and high schools correspond to grades 7-12. Grade 6 can be considered elementary or middle school depending on the school district. K-12 schools are considered middle schools for these results. The instruments used are the three measures of FRPM from the California Department of Education, the National Center for Education Statistics, and Ed-Data. The overidentification test tests the joint null hypothesis that the instruments are uncorrelated to the error term. Significance levels: *** 1 percent; ** 5 percent; * 10 percent.

Table VIII: Estimated Model Parameters

	Federal Resources		
	R^{Fed}=0.1625	R^{Fed}=0.25	R^{Fed}=0.5
ψ	0.918***	0.947***	0.974***
	(0.10)	(0.07)	(0.03)
ψ_2	0.920***	0.948***	0.974***
	(0.19)	(0.13)	(0.07)
$\frac{p_a}{p_d}$	1.382	2.485	5.638*
	(1.10)	(1.70)	(3.40)
$\frac{p_a}{p_d}_2$	1.395	2.505	5.677
	(1.54)	(2.38)	(4.75)
μ	0.675***	0.789***	0.894***
	(0.15)	(0.10)	(0.05)

Notes: All parameters are estimated at a school poverty rate of forty percent. To identify the parameters, RD estimates from the 'all school' sample in math are used, though similar estimates are generated using RD estimates from English. The '2' subscript indicates that the parameter was estimated using Equation III.8. μ denotes the proportion of federal funds going to the advantaged. Standard errors are based on the delta method. Significance levels: *** 1 percent; ** 5 percent; * 10 percent.

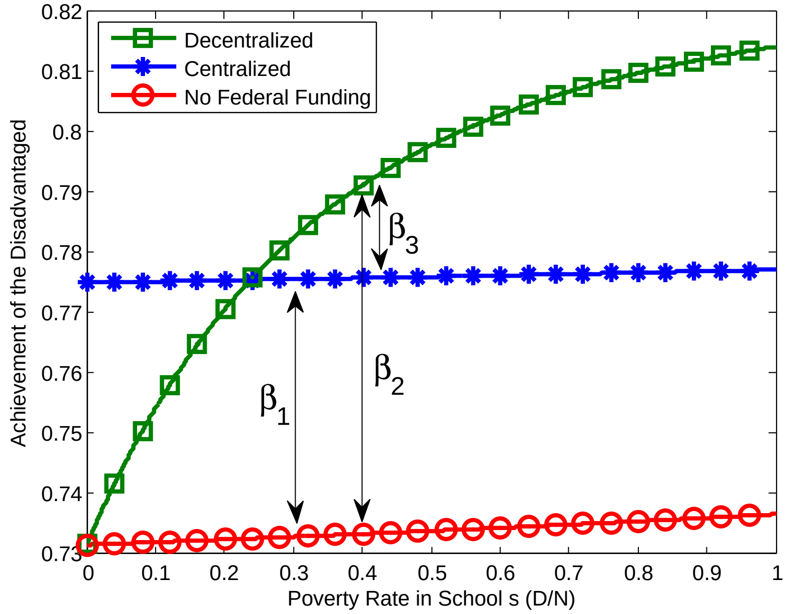
Figure I: Poverty Rate and Resource Allocation



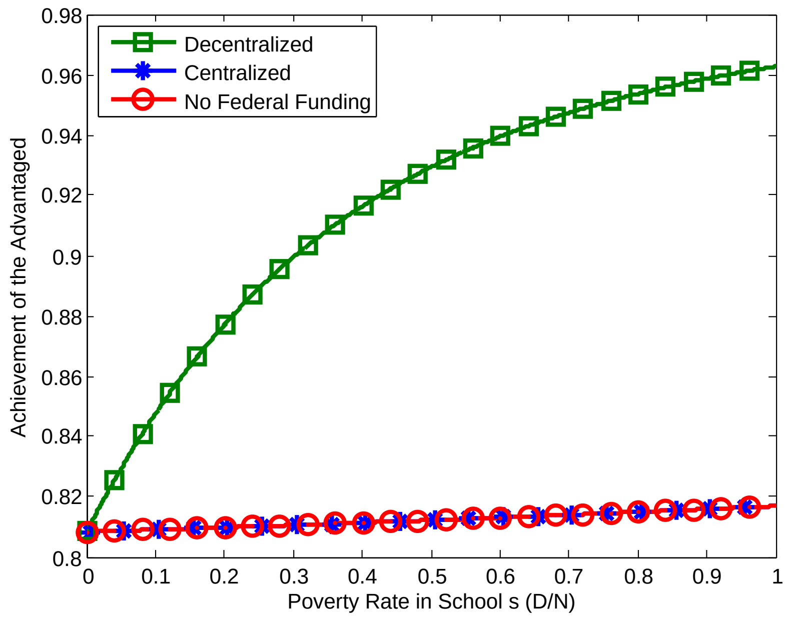
Notes: $\frac{p_a}{p_d}$ represents the probability that an advantaged parent is involved in the school allocation decision relative to a disadvantaged parent.

Figure II: Achievement under Centralization and Decentralization

(a) Disadvantaged Students



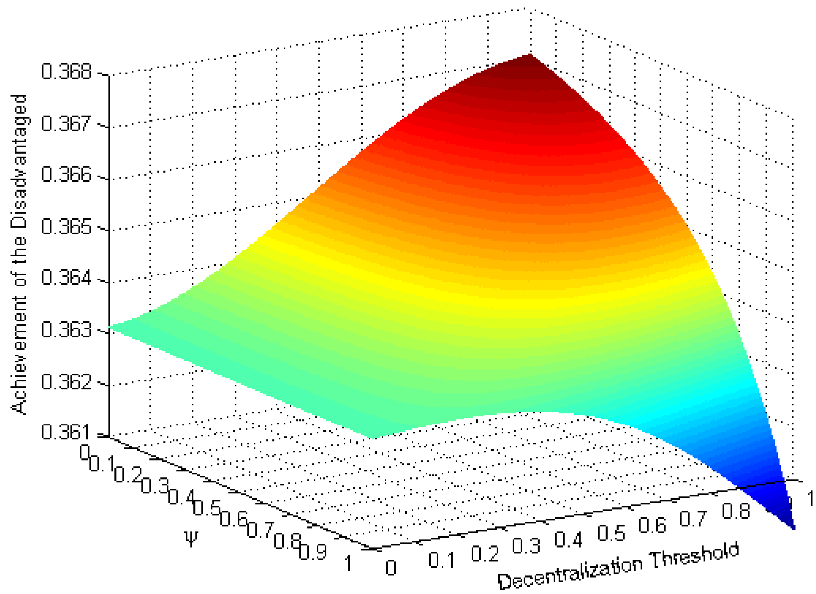
(b) Advantaged Students



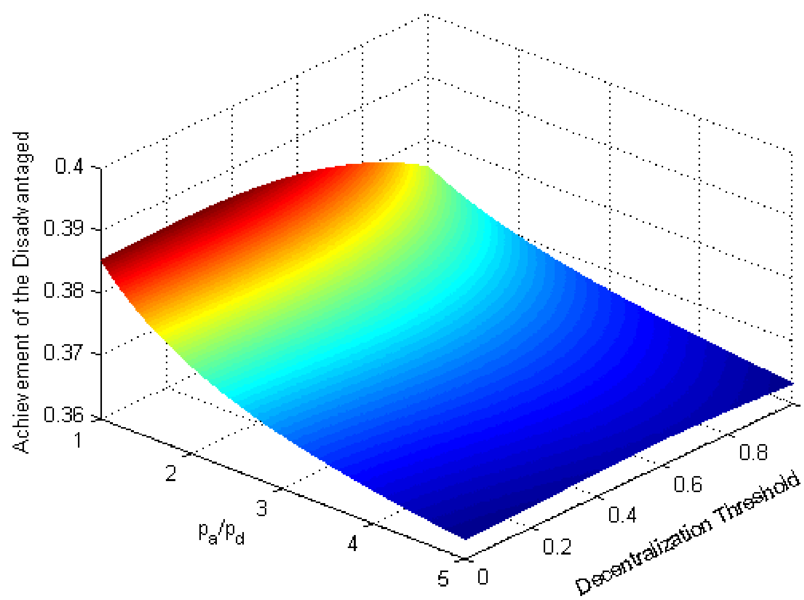
Notes: In Figure IIa the βs compare the achievement of the disadvantaged under different funding regimes: β_1 compares centralized funding to no funding, β_2 compares decentralized funding to no funding, and β_3 compares decentralized funding to centralized funding. The figures are graphed for the following model parameters: $f_{i\in\{a,d\}}(\cdot) = (\alpha_i^\rho + r_i^\rho)^{\frac{1}{\rho}}$, $\rho = -2$, $p_a = 0.5$, $p_d = 0.4$, $\psi = 0.2$, $\alpha_a = 1.2\ \forall a$, $\alpha_d = 1\ \forall d$, $r_{min} = 1$, $R_s = 1100$, $R^{Fed} = 0.25 * D$ and $N = 1000$.

Figure III: Decentralization Thresholds and Parameters

(a) ψ



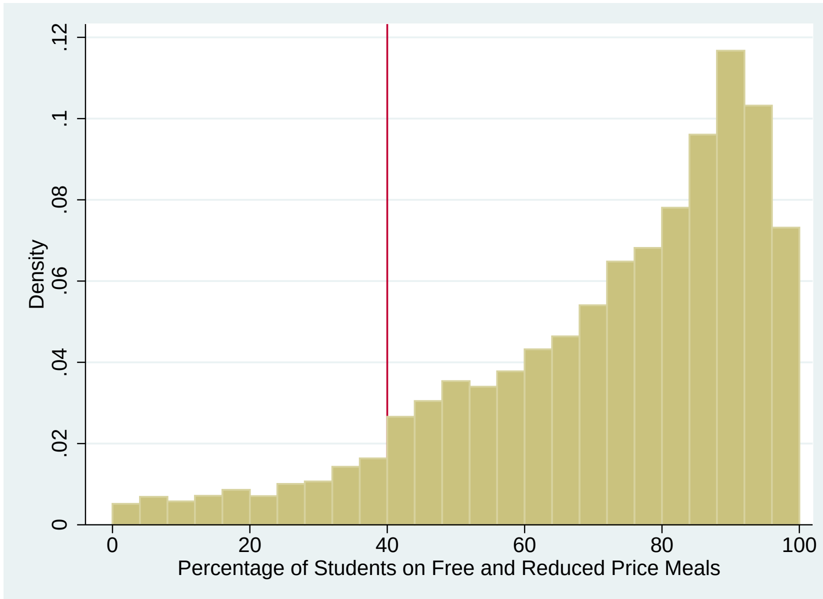
(b) $\frac{p_a}{p_d}$



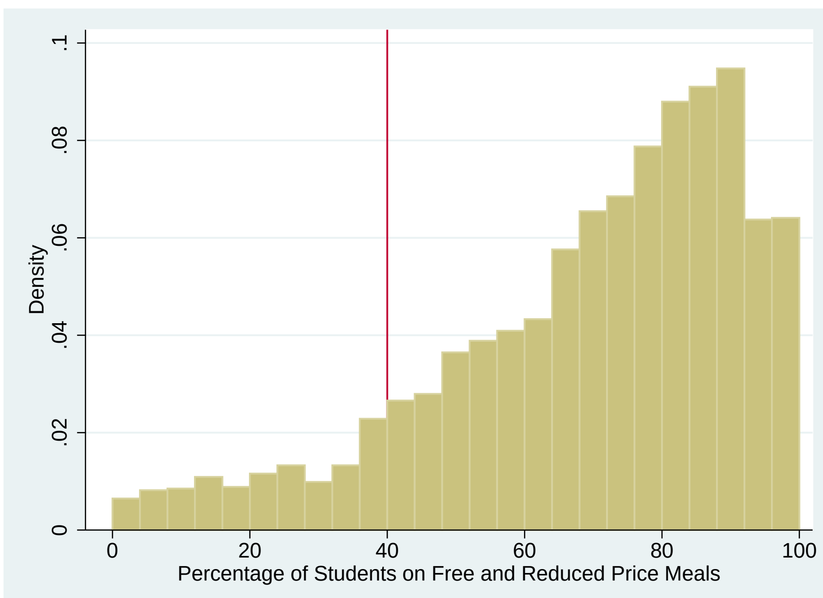
Notes: Figure IIIa and IIIb plot the achievement level of the disadvantaged under various decentralization thresholds for $\psi \in [0, 1]$ and $\frac{p_a}{p_d} \in [1, 5]$, respectively. The total achievement level in each figure is based on one thousand schools with poverty rates uniformly distributed between zero and a hundred percent. The model parameters used are the same as in Figure II.

Figure IV: Distribution of Schools by Free and Reduced Price Meal Eligibility, 2008-09 to 2010-11

(a) Elementary Schools: McCrary density test p-value: 0.00



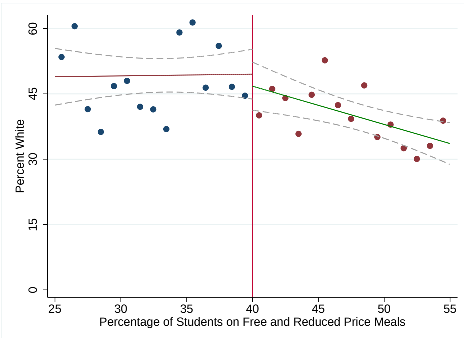
(b) Middle and High Schools: McCrary density test p-value: 0.537



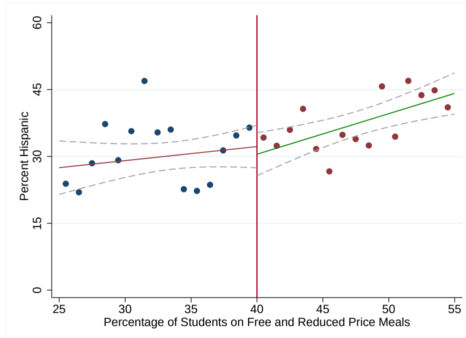
Notes: Unit of observation is at the school-year level and figures are based on 11,619 and 2,933 observations respectively for elementary schools and middle and high schools only. Only Title I schools are included. Histograms have bin widths of four percent. The vertical line corresponds to the cutoff threshold for schoolwide program eligibility.

Figure V: Observable Covariates by Percent of FRPM in Middle and High Schools

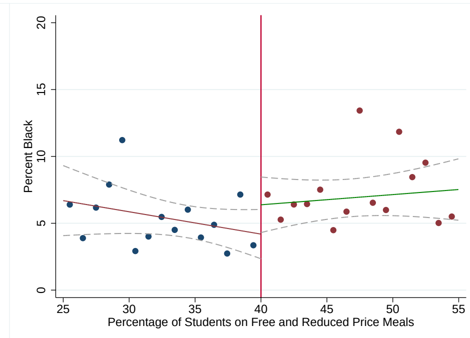
(a) White
Discontinuity: -2.76 (3.30).



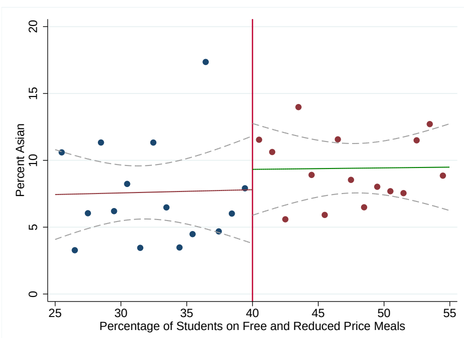
(b) Hispanic
Discontinuity: -1.71 (3.03).



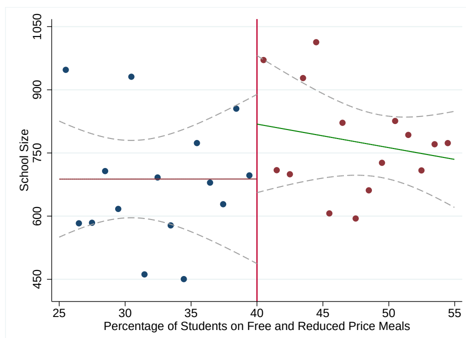
(c) Black
Discontinuity: 2.19* (1.22).



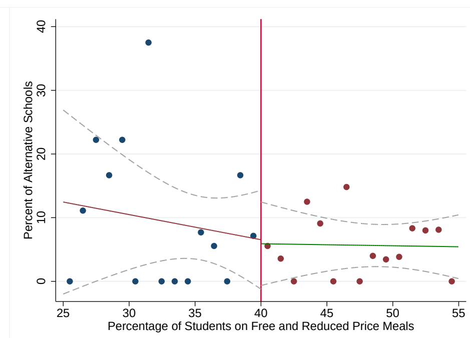
(d) Asian
Discontinuity: 1.52 (1.62).



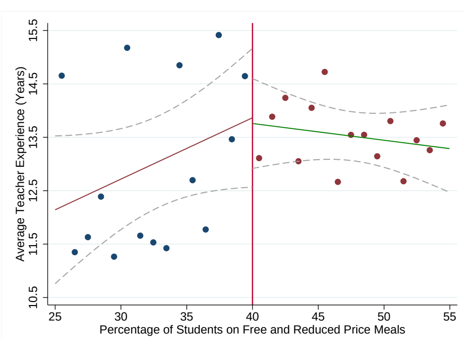
(e) Average School Size
Discontinuity: 130 (127).



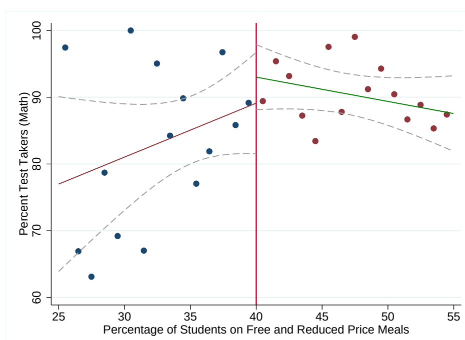
(f) School is Alternative
Discontinuity: -0.63 (5.21).



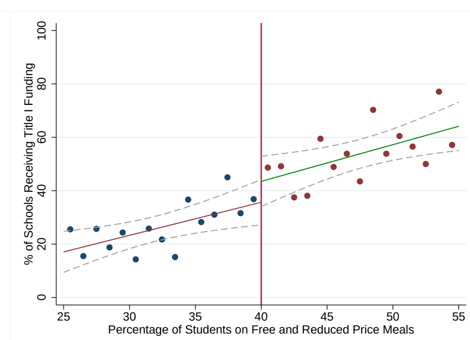
(g) Average Teacher Experience
Discontinuity: -0.11 (0.67).



(h) Test Taking Rate (Math)
Discontinuity: 3.89 (4.34).



(i) School Receives Title I
Discontinuity: 7.82 (5.48).

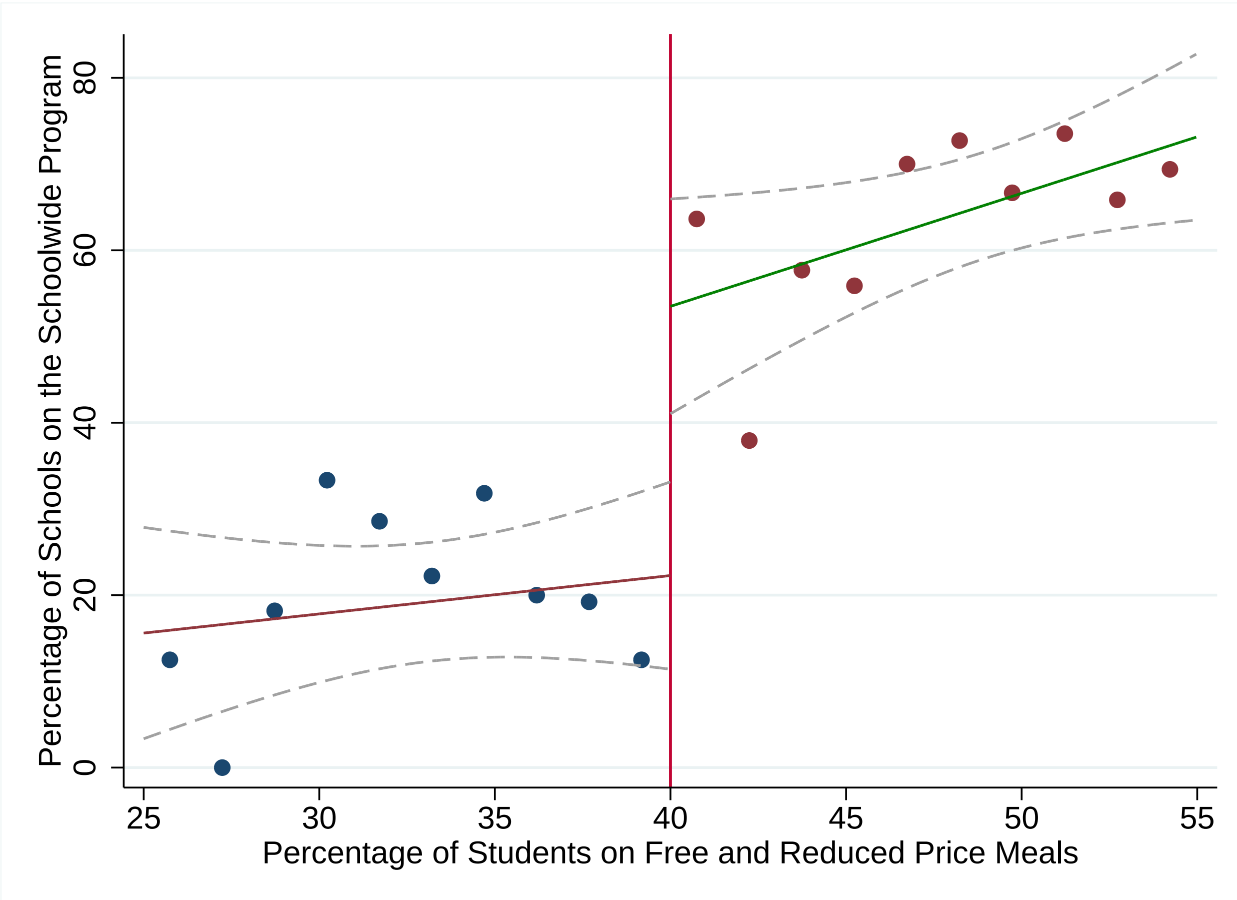


Notes: Unit of observation is at the school-year level. Figures only include Title I schools and are based on 519 observations with the exception of the Title I figure which is based on 1,279 observations and includes non-Title I schools. Sample is restricted to middle and high schools only due to possible manipulation of the forcing variable in elementary schools. The dashed lines represent the 95% confidence intervals of a local linear fit on either side of the threshold. Point estimates are from local linear regressions without controls allowing for different functions on either side of the cutoff. A bandwidth of 15 percent is used and standard errors are clustered at the school level. Significance levels: *** 1 percent; ** 5 percent; * 10 percent.

Figure VI: First-Stage - Schoolwide Program Design

(a) School-Year Has a Schoolwide Program.

Estimate of the discontinuity: 31.22***(7.39).

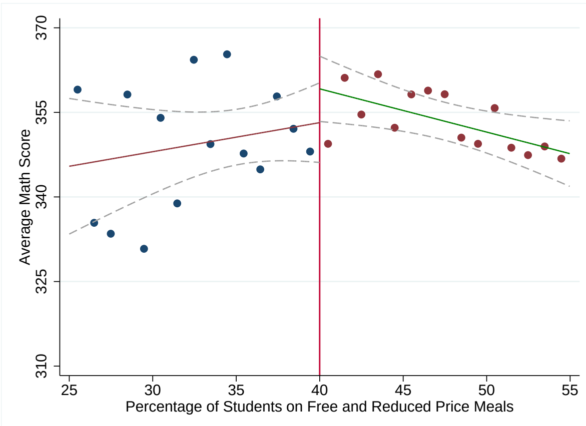


Notes: Unit of observation is at the school-year level and is based on 519 observations. Only Title I middle and high schools are included. Standard errors are in parentheses and are clustered at the school level. Dashed lines represent the 95% confidence interval. Estimates are from local linear regressions without controls and a bandwidth of 15 percent allowing for different functions on either side of the cutoff. Significance levels: *** 1 percent; ** 5 percent; * 10 percent.

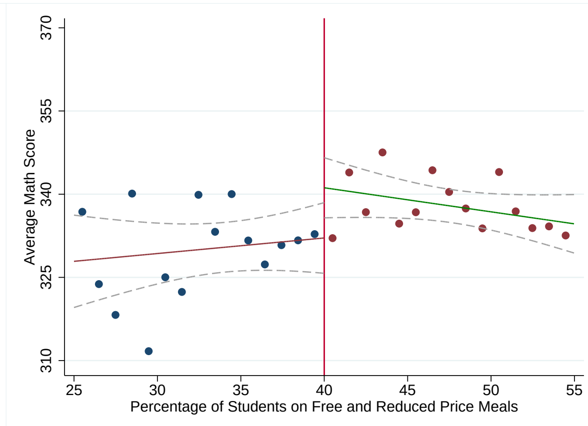
Figure VII: Reduced Form - Schoolwide Program Design

Math (Mean Scaled Score)

(a) Math - All Students:
Estimate of the discontinuity: 5.99 (4.35).

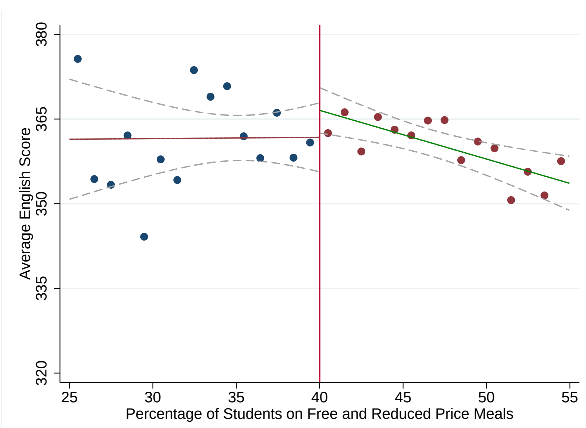


(b) Math - Disadvantaged:
Estimate of the discontinuity: 9.05** (3.89).

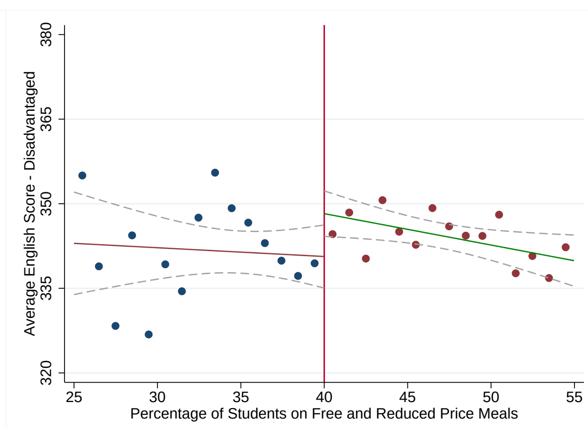


English (Mean Scaled Score)

(c) English - All Students:
Estimate of the discontinuity: 4.78 (3.71).



(d) English - Disadvantaged:
Estimate of the discontinuity: 7.60** (3.39).

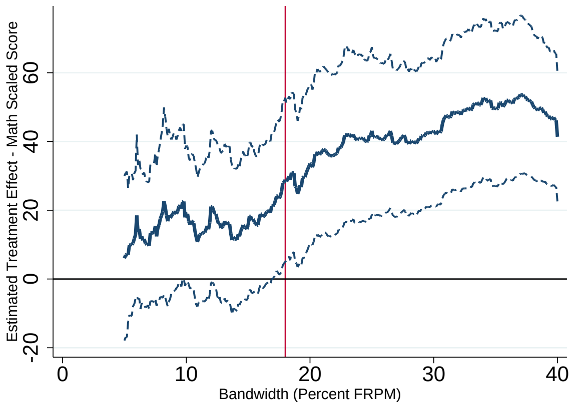


Notes: Unit of observation is at the school-year level and is based on 519, 501, 522 and 506 observations for Figures VIIa, VIIb, VIIc and VIId, respectively. Only Title I middle and high schools are included. Figures are based on a linear fit with dashed lines representing 95% confidence intervals. Standard errors are in parentheses and are clustered at the school level. Estimates are from local linear regressions without controls and a bandwidth of 15 percent allowing for different functions on either side of the cutoff. Point estimates in these figures correspond to those in Table IV - however the point estimates in Table IV are scaled by the first-stage estimate and include covariates. Significance levels: *** 1 percent; ** 5 percent; * 10 percent.

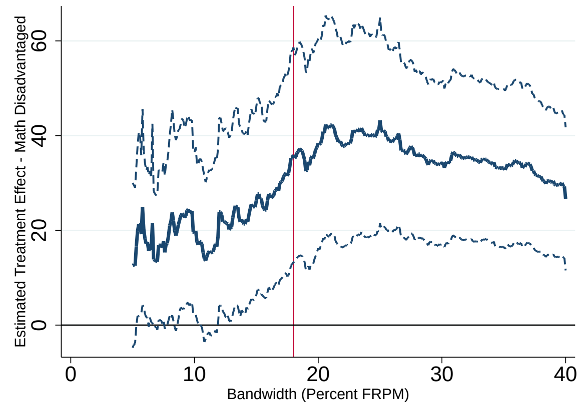
Figure VIII: Bandwidth Sensitivity

Math (Mean Scaled Score)

(a) All Students

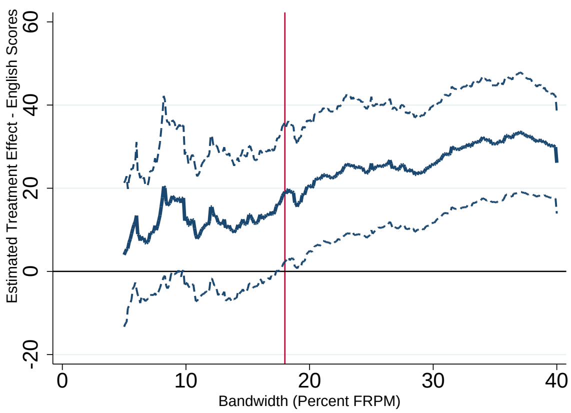


(b) Socioeconomically Disadvantaged

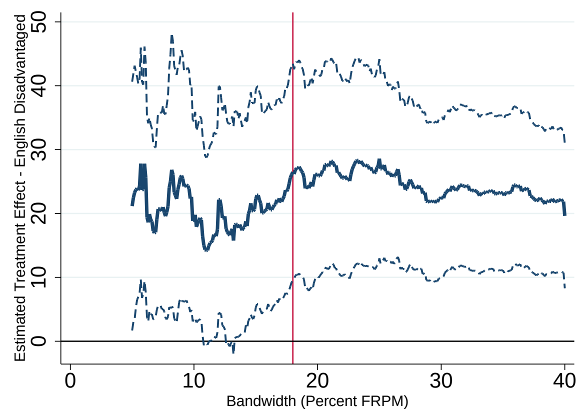


English (Mean Scaled Score)

(c) All Students



(d) Socioeconomically Disadvantaged

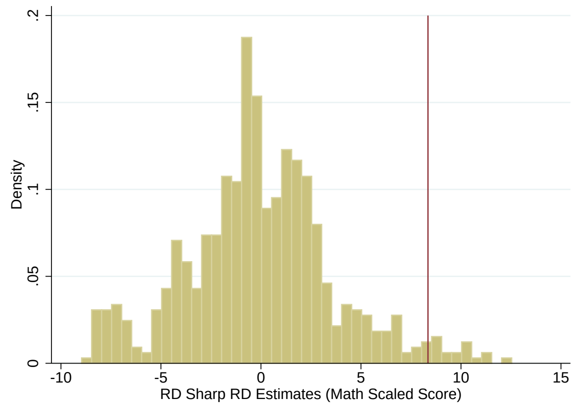


Notes: These figures display the sensitivity of RD estimates for middle and high schools to different bandwidth choices. The x-axis represents the size of the bandwidth used while the y-axis represents the estimated treatment effect. The solid line represents RD point estimates while the dashed lines represent the 90% confidence bands. RD estimates come from local linear regressions allowing for different functions on either side of the cutoff. Controls for student demographics and school characteristics are included and standard errors are clustered at the school level. The vertical line represents the chosen bandwidth.

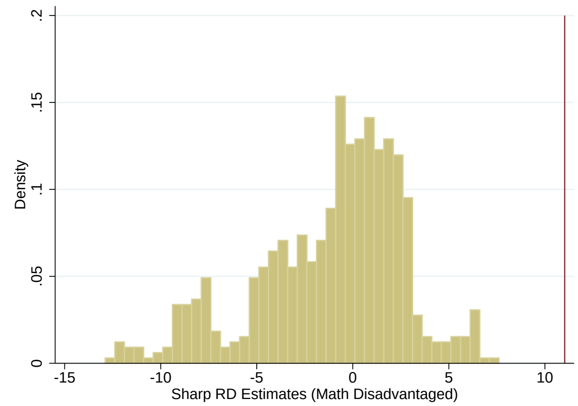
Figure IX: Estimates of Outcome (Mean Scaled Score) Discontinuity for Placebo Thresholds

Math (Mean Scaled Score)

(a) All Students

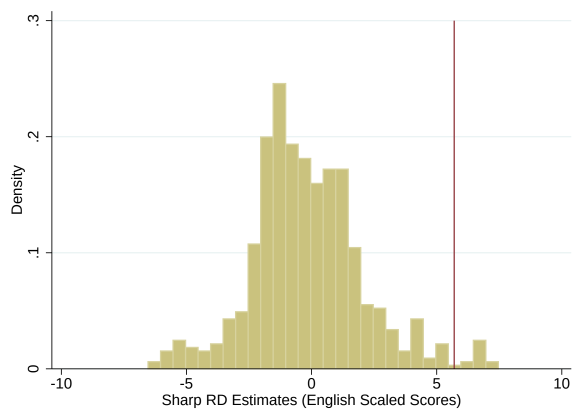


(b) Socioeconomically Disadvantaged

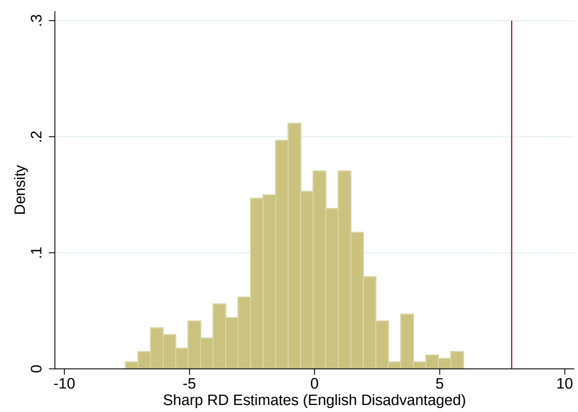


English (Mean Scaled Score)

(c) All Students

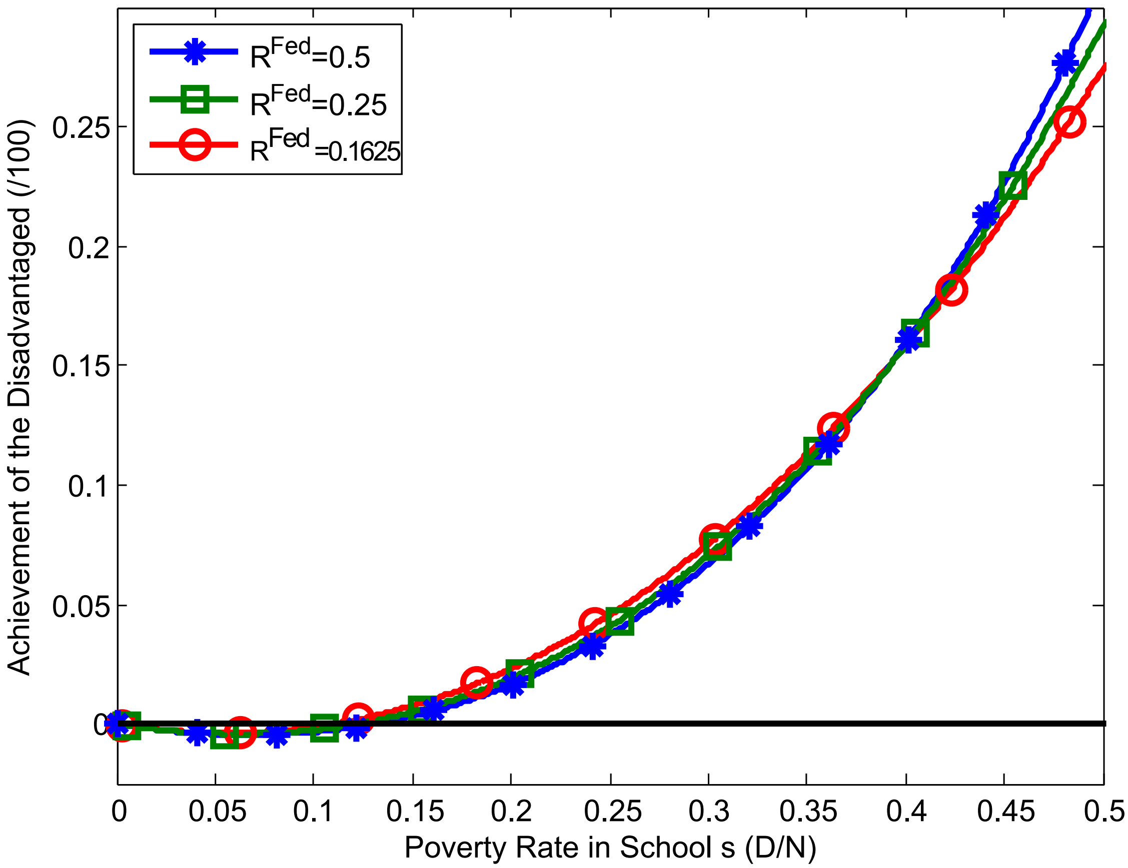


(d) Socioeconomically Disadvantaged



Notes: These histograms shows the result of assigning a random threshold to a sharp RD regression for middle and high schools only. Hence, the x-axis represents RD estimates of a local regressions with the density of obtaining that estimate on the y-axis. The vertical line represents the sharp RD regression estimate at the true threshold. Sharp RD is required due to no discontinuity for the schoolwide program for the placebo thresholds. RD estimates come from local linear regressions allowing for different functions on either side of the cutoff. Controls for student demographics and school characteristics are included. Random thresholds were restricted to between 18% and 90% FRPM and thresholds between 35% and 42% were excluded due to proximity to the true threshold and to the Title I poor district cutoff. Histogram points to the right of the sharp RD point estimate for English and math are near thirty-one percent FRPM and are not significant at 5% due to higher standard errors. Histograms are drawn with bin widths of 0.5.

Figure X: Optimal Decentralization Thresholds



Notes: The figure plots the achievement of the disadvantaged under decentralization relative to centralization using parameters $\frac{p_a}{p_d}$ and ψ estimated in Table VIII. R^{Fed} represents different levels of per disadvantaged student funding relative to base funding.

D. Covariates for Elementary Schools

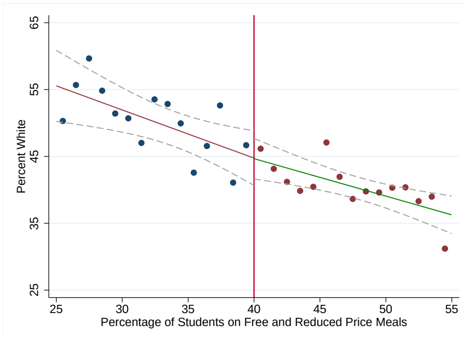
Table C.I: Tests of Discontinuities in Observable Covariates

Student Characteristics	Percent White (1)	Percent Hispanic (2)	Percent Asian (3)	Percent Black (4)	Percent ESL (5)
Percent FRPM<cutoff	-0.75	1.13	-0.24	0.86	0.42
	(2.16)	(1.59)	(1.64)	(0.62)	(1.35)
Constant (control mean)	46.09***	31.16***	10.49***	3.91***	17.36***
	(1.97)	(1.33)	(1.59)	(0.42)	(1.17)
Observations	2,324	2,324	2,324	2,324	2,044
Joint Significance Test (Prob>χ^2)		0.35			
School Characteristics	School Size (6)	Avg Teacher Experience (7)	Percent Charter (8)	Percent Alternative (9)	Percent Taking Test (Math) (10)
Percent FRPM<cutoff	21.5	-0.15	-1.65	0.42	-0.69
	(21.0)	(0.35)	(2.42)	(0.52)	(1.14)
Constant (control mean)	471.0***	14.84***	6.09**	0.26	68.12***
	(18.2)	(0.31)	(2.41)	(0.39)	(1.05)
Observations	2,324	2,324	2,324	2,324	2,324
Joint Significance Test (Prob>χ^2)		0.59			
Student and School Characteristics					
Joint Significance Test (Prob>χ^2)		0.46			

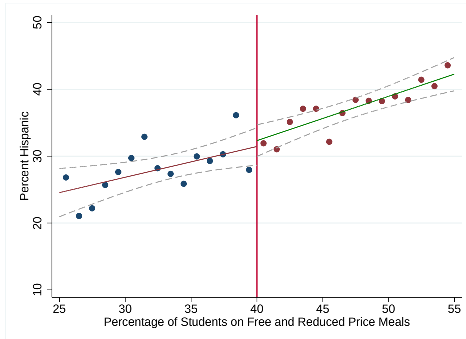
Notes: Unit of observation is at the school-year level. Estimates are based on local linear regressions allowing for different functions on either side of the cutoff with a bandwidth of 18 percent. The sample is restricted to elementary Title I schools with non-missing math STAR test results. Percent English learners is missing observations; however the probability of being missing is not discontinuous at the cutoff. The relevant joint significance tests exclude all observations with missing English learner status. The p-values when these observation are included are 0.48 and 0.65 for student characteristics and for student and school characteristics, respectively. Standard errors are clustered at the school level. Significance levels: *** 1 percent; ** 5 percent; * 10 percent.

Figure C.I: Observable Covariates by Percent of FRPM in Elementary Schools

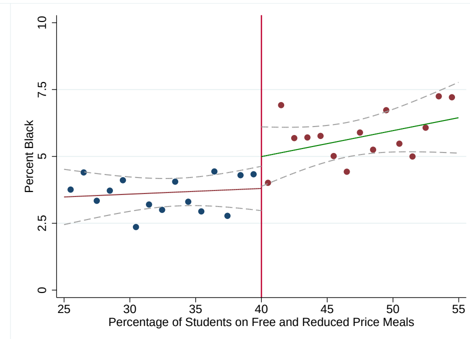
(a) White
Discontinuity: -0.08 (2.26).



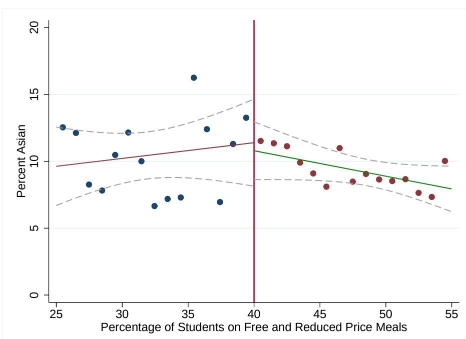
(b) Hispanic
Discontinuity: 0.88 (1.67).



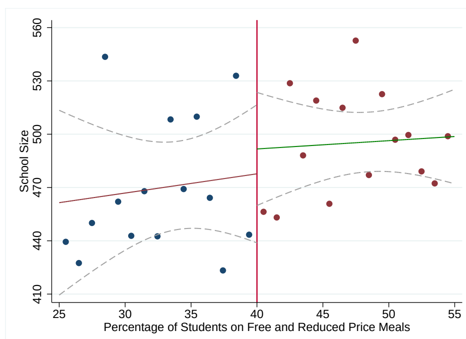
(c) Black
Discontinuity: 1.19** (0.61).



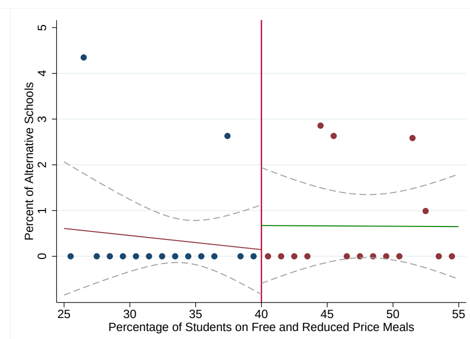
(d) Asian
Discontinuity: -0.60 (1.69).



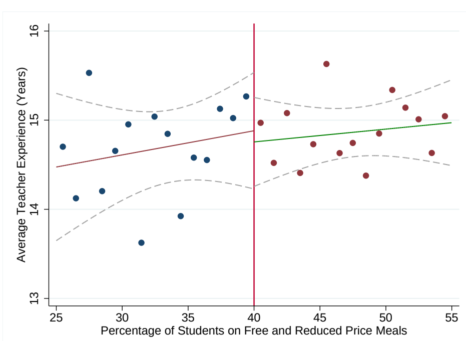
(e) Average School Size
Discontinuity: 14.00 (22.54).



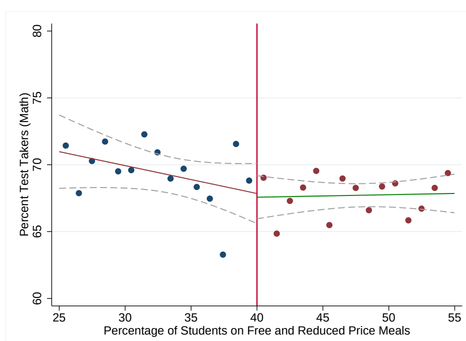
(f) School is Alternative
Discontinuity: 0.53 (0.58).



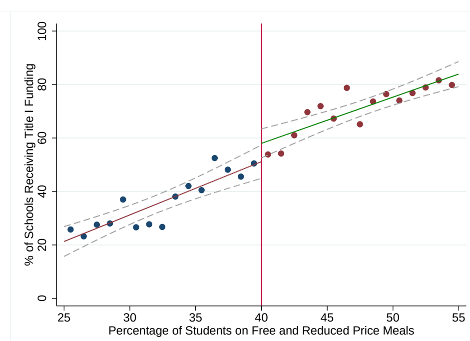
(g) Average Teacher Experience
Discontinuity: -0.13 (0.38).



(h) Test Taking Rate (Math)
Discontinuity: -0.28 (1.25).



(i) School Receives Title I
Discontinuity: 6.85* (3.66).



Notes: Unit of observation is at the school-year level. Figures only include Title I elementary schools and are based on 1,931 observations with the exception of the Title I figure which is based on 3,512 observations and includes non-Title I schools. The dashed lines represent the 95% confidence intervals of a linear fit on either side of the threshold. Estimates are from local linear regressions without controls and a bandwidth of 15 percent allowing for different functions on either side of the cutoff. Standard errors are clustered at the school level. Significance levels: *** 1 percent; ** 5 percent; * 10 percent.

E. Title I RD Figures

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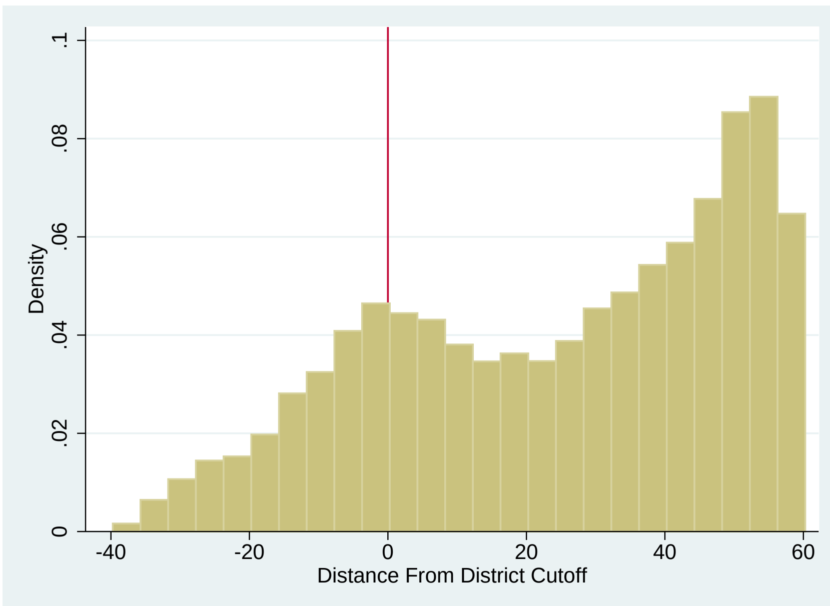
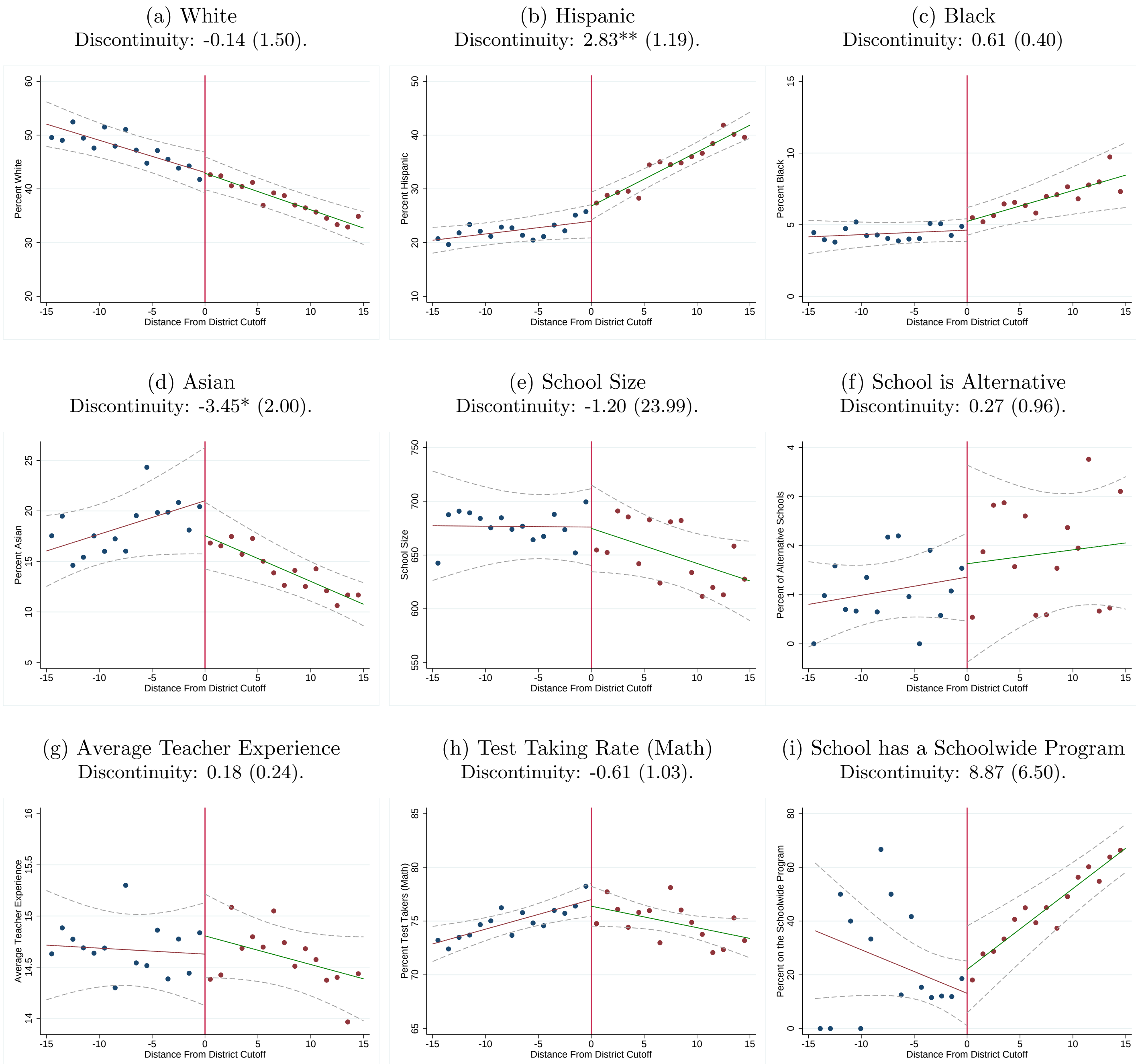
Figure D.I: Distribution of Schools by Distance From District Title I Cutoff

(a) McCrary density test p-value: 0.71



Notes: Unit of observation is at the school-year level and is based on 16,594 observations. Histogram has a bin width of four percent. The vertical red line corresponds to the district level cutoff for Title I eligibility.

Figure D.II: Observable Covariates by Distance From District Cutoff



(a) White
Discontinuity: -0.14 (1.50).

(b) Hispanic
Discontinuity: 2.83** (1.19).

(c) Black
Discontinuity: 0.61 (0.40)

(d) Asian
Discontinuity: -3.45* (2.00).

(e) School Size
Discontinuity: -1.20 (23.99).

(f) School is Alternative
Discontinuity: 0.27 (0.96).

(g) Average Teacher Experience
Discontinuity: 0.18 (0.24).

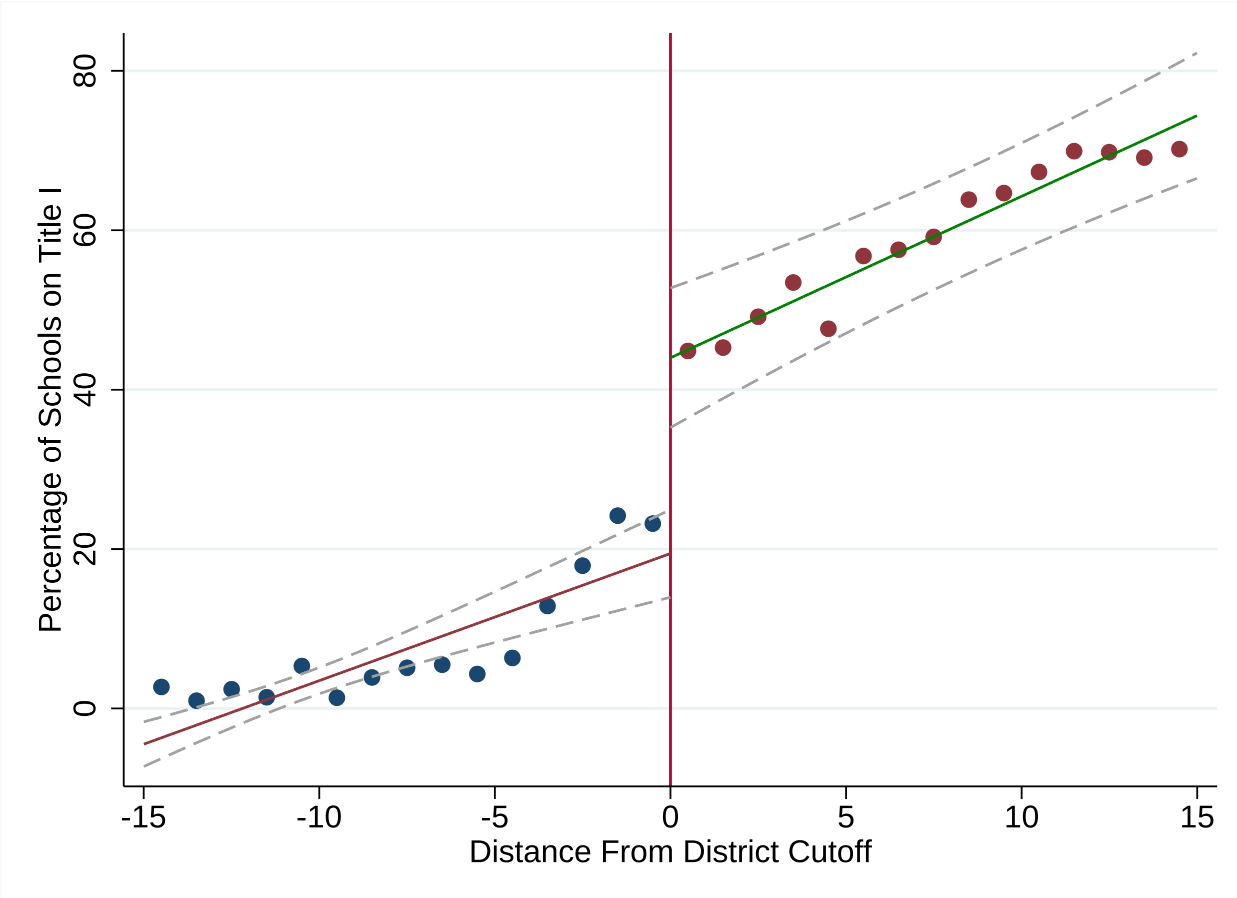
(h) Test Taking Rate (Math)
Discontinuity: -0.61 (1.03).

(i) School has a Schoolwide Program
Discontinuity: 8.87 (6.50).

Note: Unit of observation is at the school-year level and figures are based on 4,888 observations with the exception of the schoolwide program figure which is based on 1,644 observations as it only includes Title I schools. Figures are based on a linear fit with dashed lines representing 95% confidence intervals. Standard errors are in parentheses and are clustered at the district level. Estimates are from local linear regressions without controls and a bandwidth of 15 percent allowing for different functions on either side of the cutoff. Significance levels: *** 1 percent; ** 5 percent; * 10 percent.

Figure D.III: First-Stage - Title I Design

(a) School Received Title I. Estimate of the discontinuity: 24.55*** (3.62).

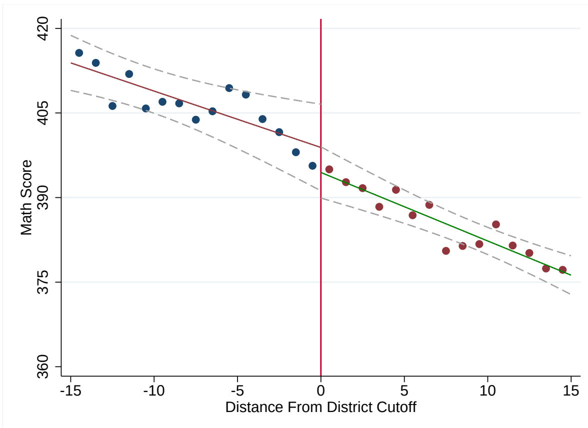


Notes: Unit of observation is at the school-year level and is based on 4,874 observations. Dashed lines represent 95% confidence intervals. Standard errors are in parentheses and are clustered at the district level. Estimates are from local linear regressions without controls and a bandwidth of 15 percent allowing for different functions on either side of the cutoff. Significance levels: *** 1 percent; ** 5 percent; * 10 percent.

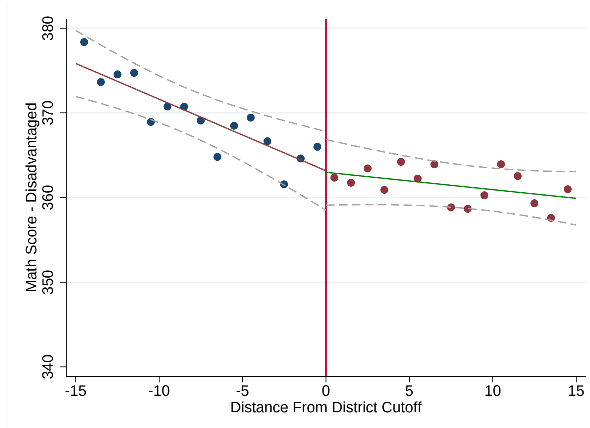
Figure D.IV: Reduced Form - Title I Design

Math

(a) Math - All Students:
Estimate of the discontinuity: -4.43 (2.91).

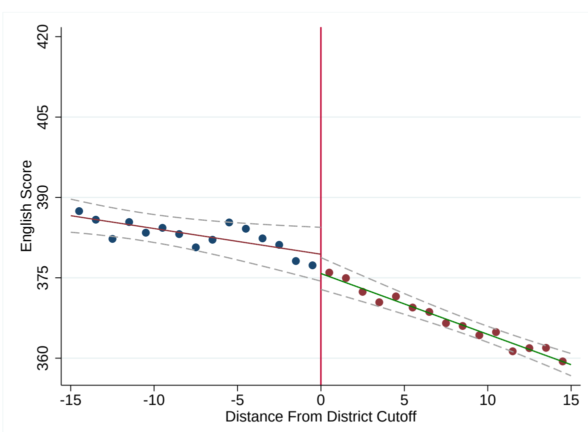


(b) Math - Disadvantaged:
Estimate of the discontinuity: -0.18 (2.07)

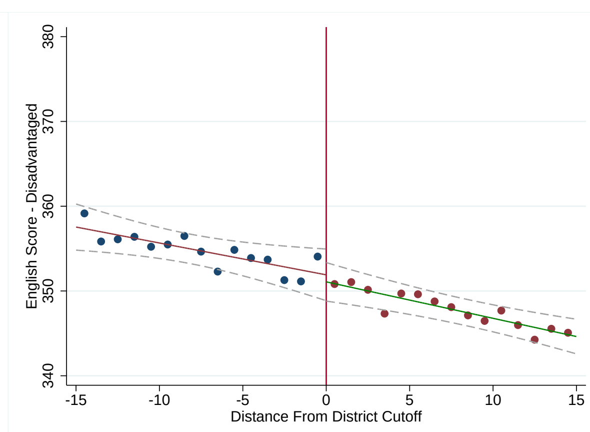


English

(c) English - All Students:
Estimate of the discontinuity: -3.61** (1.68).



(d) English - Disadvantaged:
Estimate of the discontinuity: -0.83 (1.16).



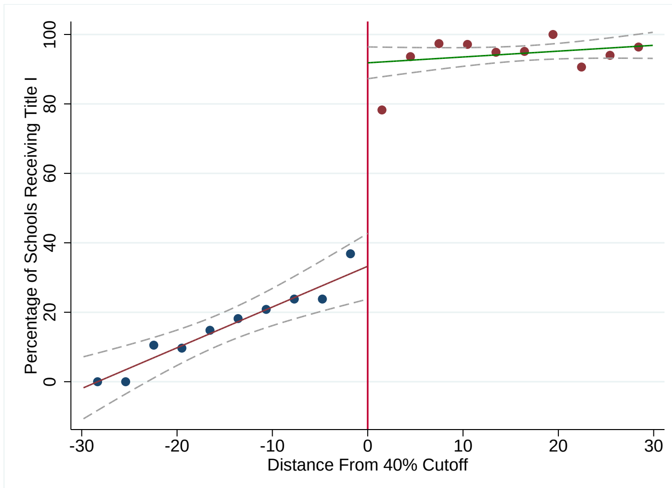
Note: Unit of observation is at the school-year level. Figures D.IVa, D.IVb, D.IVc and D.IVd are based on 4,888, 4,430, 4,906 and 4,430 observations, respectively. Dashed lines represent 95% confidence intervals. Standard errors are in parentheses and are clustered at the district level. Estimates are from local linear regressions without controls and a bandwidth of 15 percent allowing for different functions on either side of the cutoff. Point estimates in these figures correspond to those in Table V. However the point estimates in Table V are scaled by the first-stage estimates of the probability of receiving Title I and include covariates – which may switch the sign of the estimated effect. Significance levels: *** 1 percent; ** 5 percent; * 10 percent.

F. RD Figures for Districts with 40% Title I Cutoff

Figure E.I: First-Stage - Districts with 40% Title I Cutoff

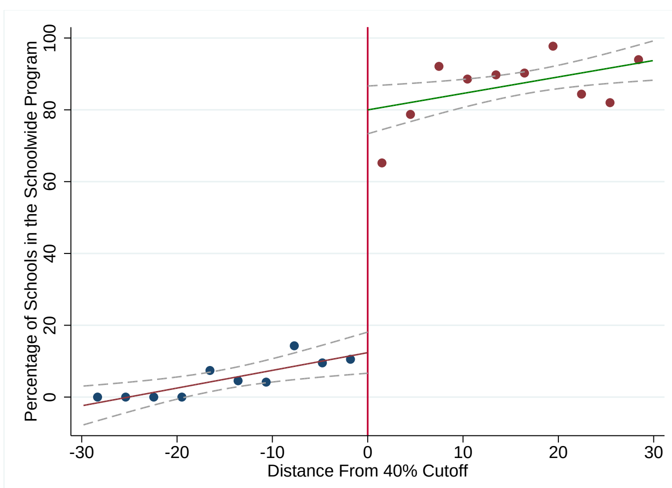
(a) School Received Title I.

Estimate of the discontinuity: 58.59***(6.40).



(b) School Operates a Schoolwide Program.

Estimate of the discontinuity: 67.61***(5.52).

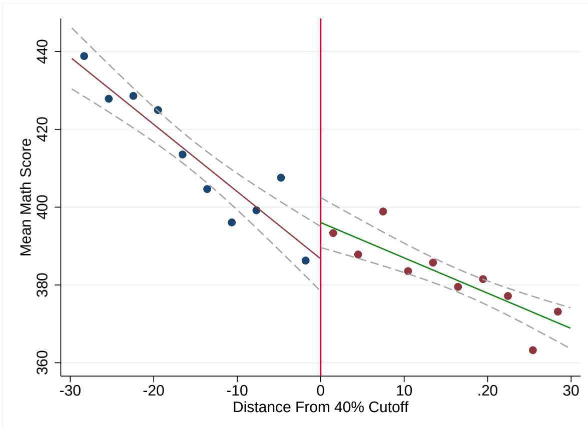


Note: Unit of observation is at the school-year level. All schools are included for Figure E.Ia, while only Title I schools are used for Figure E.Ib. Figures E.Ia and E.Ib are based on 665 and 445 observations, respectively. The schools included are from the following districts: Los Angeles Unified, Sacramento Unified, and San Diego Unified. San Diego Unified was not included for school year 2010-11 due to its Title I threshold moving to 45%. Dashed lines represent 95% confidence intervals. Standard errors are in parentheses and are clustered at the school level. Estimates are from local linear regressions without controls allowing for different functions on either side of the cutoff. The bandwidth used is larger than in other designs – 30 percent – due to smaller sample sizes and was determined with consideration to Imbens and Kalyanaraman (2012) but keeping the bandwidth constant for all regressions for consistency. Significance levels: *** 1 percent; ** 5 percent; * 10 percent.

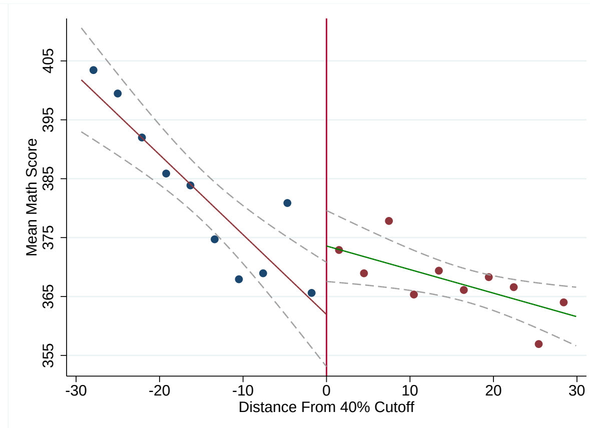
Figure E.II: Reduced Form - Districts with 40% Title I Cutoff

Math

(a) Math - All Students:
Estimate of the discontinuity: 9.33(6.56).

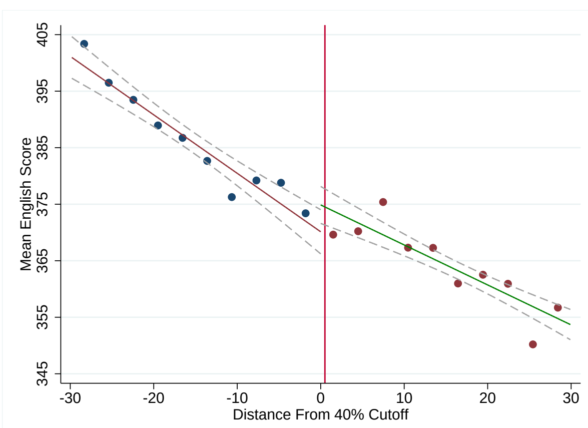


(b) Math - Socioeconomically Disadvantaged:
Estimate of the discontinuity: 11.63*(6.86).

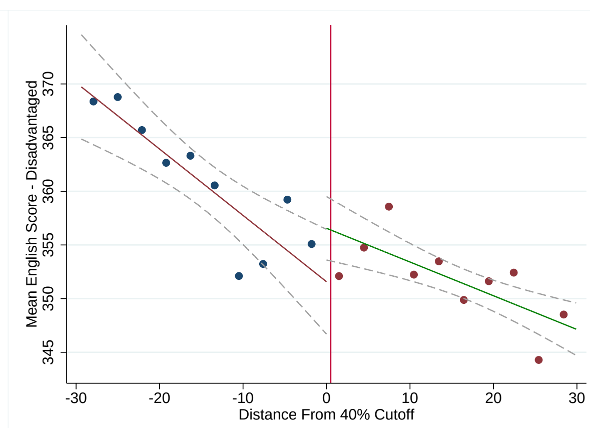


English

(c) English - All Students:
Estimate of the discontinuity: 4.75(3.16).



(d) English - Socioeconomically Disadvantaged:
Estimate of the discontinuity: 4.98(3.50).



Note: Unit of observation is at the school-year level. Figures E.IIa, E.IIb, E.IIc and E.IId are based on 665, 643, 671, and 643 observations, respectively. The schools included are from the following districts: Los Angeles Unified, Sacramento Unified, and San Diego Unified. San Diego Unified was not included for school year 2010-11 due to its Title I threshold moving to 45%. Dashed lines represent 95% confidence intervals. Standard errors are in parentheses and are clustered at the school level. Estimates are from local linear regressions without controls allowing for different functions on either side of the cutoff. The bandwidth used is larger than in other designs – 30 percent – due to smaller sample sizes and was determined with consideration to Imbens and Kalyanaraman (2012) but keeping the bandwidth constant for all regressions for consistency. Point estimates in these figures correspond to those in Table VI - however the point estimates in Table VI are scaled by the first-stage estimates of the probability of receiving Title I and include covariates. Significance levels: *** 1 percent; ** 5 percent; * 10 percent.