



# **HOW DECENTRALIZING SCHOOL FINANCE MAY NARROW ACHIEVEMENT GAPS –**

## **Uneven Progress in California after \$41 Billion**



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# How Decentralizing School Finance May Narrow Achievement Gaps – Uneven Progress in California after \$41 Billion

## Summary

Several governors have moved to distribute school dollars based on pupil weights, recognizing that disadvantaged students require fresh resources to reach state proficiency bars. This policy strategy, after winning broad support in California, balances adequate dollars for all students with the additional cost of hoisting children held back by family poverty or limited English.

California's rendition of weighted-pupil funding, approved in 2013, provides *concentration grants* to districts in which at least 55 percent of enrollment consists of disadvantaged children. Local districts enjoy discretion over whether the new funds generated by these children – equaling \$41 billion to date when *supplemental grants* are included – go to schools that serve them.

We find that schools in districts receiving concentration grants during the initial two years of Local Control Funding did engage in organizational change that parallels gains in pupil achievement, compared with schools in almost identical districts not receiving concentration grants. These benefits were largely experienced by Latino students and not by other groups at significant levels. We exploit a regression-discontinuity design, capturing this large natural experiment and allowing for stronger causal inferences.

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## How Decentralizing School Finance May Narrow Achievement Gaps – Uneven Progress in California after \$41 Billion

State policy makers have turned to *weighted pupil formulae*, going back four decades, to advance sufficient and equitable funding for public schools. Many states had moved away from reliance on property tax to finance schools by the 1980s in the wake of California's *Serrano* decision and similar cases. This litigation had revealed gross disparities in per pupil spending among districts. To equalize state support between rich and poor districts, or to better serve specific groups, most states centralized funding, then regulated a bevy of "categorical aid" programs from their state capital. This reform agenda dramatically narrowed disparities in per pupil spending among local districts, whether they enjoyed ample tax bases or not (Wirt & Kirst, 1972; Odden & Picus, 2014).

But as governors and legislatures put in place aggressive school accountability regimes through the 1990s – expecting all students to reach rigorous proficiency standards – the differential cost of lifting low-achieving students came into focus. Severe declines in school spending, especially in the wake of the Great Recession, also prompted the additional question of whether local schools were adequately funded, and then whether all students enjoyed a well-resourced opportunity to learn

Weighted-pupil funding (WPF) has gained appeal among a variety of policy activists, under which students from disadvantaged backgrounds are assigned a larger weight in the state funding formula. This results in additional dollars for districts (not necessarily schools) that serve higher concentrations of these youngsters. Base grants to districts, where all pupils are weighted equally, along with supplemental grants weighted for poor students, serve to balance the tandem policy aims of adequacy and equity (Augenblick, Myers, & Anderson, 1997; Leppert & Routh, 1980). Several states had created WPF finance structures by the 1990s, including Florida, Oklahoma, and Texas (King, 1983; Berne & Stiefel, 1979).

Long-term data suggest that pro-equity finance reforms overall have contributed to gains in student achievement, even lifting the performance of poor children (Jackson, Johnson, & Persico, 2015; Lafortune, Rothstein, & Schanzenbach, 2016). The efficiency with which equity reforms have raised achievement or narrowed disparities remains a key question (Hanushek & Woessmann, 2017). Nor do we understand whether WPF strategies – banking on decentralized governance – lead to organizational changes inside schools that strengthen pupil engagement and learning. Advocates often remain satisfied with the redistributive effects of WPF reforms, whether they alter schools in ways that elevate learning, or not.

We focus on one element of California's rendition of WPF reform, what Gov. Jerry Brown came to call the Local Control Funding (LCF) Formula. Approved in the summer of 2013, it recast how local districts were to be funded, kicking-in just two months later. State aid tied to weighted pupils – those from poor or non-English speaking families, and those in foster care – grew by \$41 billion during the initial four years of implementation (California, 2016). This paper

estimates the extent to which one portion of the funding apparatus – about \$3.3 billion in annual *concentrations grants* – are associated with school-level changes that may shape student learning, along with actual differences in pupil test scores, compared with almost identical districts that did not enjoy this extra funding.

Under the California reform, the state awards concentration grants to local districts when more than 55% of their enrollments consist of pupils in the weighted categories (poor, English learners, or kids in foster care). We exploit this discrete cutpoint to estimate changes in school organizations and, in turn, pupil achievement levels in districts just above the 55% threshold and those just below but otherwise almost identical in their pupil composition, which could confound the discrete influence of receiving this extra funding. Note that the additional funding becomes substantial only as a district's enrollment reflects many more disadvantaged students above the 55% line.

In short, we find that districts receiving concentration grants, during the initial two years of LCF implementation assigned significantly fewer instructional periods to teachers and provided more advanced courses at the high school level, compared with districts just below the cutoff for receiving these grants. Those winning the extra funding did not move toward smaller classes on average. Student performance of pupils, grades 3-8, also ranged higher when attending schools in districts that received concentration grants. These benefits were experienced by Latino students, but not by other groups at statistically significant levels. Future work will formally assess the extent to which these and other school-level organizational changes mediate observed gains in achievement.

## **A Hazy Theory of Organizational Change**

### **Why the Rise of Weighted-Pupil Funding?**

Affection for WPF strategies stems from the confluence of three policy developments. First, there's the recognition by policy makers that – after setting demanding learning standards or proficiency bars – it will cost more to lift many poor children over these hurdles. Children may be disadvantaged by family poverty, language, or unstable homes situations. Growing up in neighborhoods with concentrated poverty may constrain the capacity of children to engage and grow within classrooms (Bryk, Sebring, Kerbow, Allensworth, & Easton, 2010). “Because not all students come to school with the same individual, family, or neighborhood advantages, some need more resources than others to meet a given achievement standard,” as argued by initial designers of California's LCF reform (Bersin, Kirst, & Liu, 2008:5).

Second, the accretion of centrally regulated categorical aid fell into disfavor by the late 1990s. California officials, for instance, monitored over 65 separate funding streams, most requiring bureaucratic oversight at state and district levels. One survey of school principals detailed the substantial time spent simply completing budgets and forms, trying to keep pace with disparate regulations tied to multiple funding streams (Fuller, Loeb, Arshan, Chen, & Yi, 2007). In California, Gov. Schwarzenegger pushed through legislation in 2009 to consolidate



many of the state's categorical programs, a move welcomed by local school boards and union leaders (placing more dollars on the bargaining table), while worrying equity advocates who feared diminished accountability over how targeted funds would be spent locally (Fuller, Marsh, Stetcher, & Timar, 2011). The original argument for state-run categorical aid was that dollars for poor children might otherwise go to schools situated in politically stronger communities.

The consolidation of categorical aid into block grants mirrors a third policy shift that's gained momentum over the past half-century: delegating greater authority to school principals over budget and personnel. Rising distrust of education bureaucracies, along with faith in school-level control, had already been exploited by charter school advocates, including early advocates of neighborhood control, largely on the political Left (Finn, Manno, & Wright, 2016; Fuller, 2015). Two California districts, Oakland and San Francisco, had employed weighted-pupil formulae to more progressively distribute dollars to schools – one facet of decentralized fiscal control out to principals. Results in terms of altering school staffing or quality, however, proved mixed (Chambers, Shambaugh, Levin, Muraki, & Poland, 2008).

Still, the decentralizing of school finance and budget decision-making down to school districts feels consonant with the shift toward school-site management. At the same time, designers of California's WPF reform applied student weights only to allocations from Sacramento out to the state's nearly one thousand districts. Nothing in Gov. Brown's initiative – beyond his rhetoric and the reform's pro-equity spirit – requires districts to allocate new dollars to schools that serve higher concentrations of disadvantaged students. Legal actions have been filed in three major urban districts, claiming that new dollars have been diverted from the intended students.

### **When Does Decentralized Finance Touch Schools?**

WPF financing unfolds high above the inner workings of schools. This funding structure does offer a simple way of granting resources to *district-level* leaders, the actors that gain discretion under California's LCF initiative, not necessarily principals or school-level leaders. One sage activist punctuates this pivotal facet of California's reform, asking whether it operates as a *dump truck* or a *backpack*. That is, does the state drive up to a school district office and dump new dollars onto the loading dock, no questions asked, or did policy makers believe they were strapping funds onto the backs of disadvantaged students, with dollars flowing to their particular schools?<sup>1</sup>

Nothing in California law or regulations under LCF requires the backpack. School boards do *not* need to take into account pupil weights when distributing dollars among schools within their district. Some feel ethically obliged to do so, but that's not the letter of the law. Indeed, between-school distributions shaped by weighting pupils have yielded mixed results. Hawaii's statewide reform did result in larger allocations to schools that served greater shares of disadvantaged students (Levin et al., 2013). But an ambitious WPF experiment in Prince George's County, Maryland, unearthed only slight redistribution based on student attributes after the district moved to boost per-pupil spending among schools. The district "unlocked"

only certain school-level posts that otherwise remained centrally controlled. Overall progress toward equalizing spending per pupil was slight (Malen, Dayhoff, Egan, & Croninger, 2015).

Hawley Miles and Rosa (2006) similarly found that the devil lies in particular details, after studying WPF programs in Cincinnati and Houston: the share of district budgets to which pupil weights are applied, fine-grain elements of the formula, and prior, often institutionalized, ways of distributing teaching posts and fungible dollars. Between-school allocations in New York City under former mayor Michael J. Bloomberg did become weighted toward disadvantaged students, but only when federal Title I and other non-state revenues were included. State and city dollars were not progressively allocated and more experienced teachers were disproportionately assigned (or seniority rules eased migration) to schools serving fewer poor students (Rubenstein, Schwartz, Stiefel, & Amor, 2007).

California's LCF legislation does require that each local district establish a baseline level of support in "supplemental services" provided disadvantaged pupil (that is, prior to passage of LCF). Then, districts receiving so-called *supplemental* or *concentration* grants must increase spending on weighted students in proportion to the amount of new dollars generated by these pupils (the *proportionality requirement*; California, 2013). Some districts responded by over-estimating what they had earlier spent on weighted pupils, by as much \$450 million per year in the case of the Los Angeles Unified School District (LAUSD), later challenged by the state Department of Education. This allows the district to allocate fewer new dollars for services that benefit the weighted students than the level required if they had accurately reported their pre-LCF baseline level of support (United Way, 2017).

We also arrive at the theoretical vagaries of decentralized financing, namely the lack of specificity in *how* district flexibility, blended with extra funding for certain students, will alter school organizations. Under WPF regimes, state officials often reassert their faith in local school boards, the capacity of district leaders to craft and discern effective school practices. But in the California case, these district officials are not required to move new funding to schools that serve disadvantaged students. Nor is there any requirement that districts report hard evidence on what school-level changes are predictive of achievement gains. (To be fair, state-run categorical-aid programs often met the first criterion yet fell miserably short on the second.)

The maldistribution of California's large infusion of new funding – or at least erratic channeling of pupil-weighted dollars to their schools – may stem from collateral fiscal pressure to backstop employee benefits and pension liabilities, fixed facility costs, and ongoing pressure from middle-class and affluent parents within districts to protect their schools. Research inside LAUSD also reveals a lack of analytic capacity inside this particular headquarters to determine which schools benefit from new state funding, a reticence to even ask the question (Partnership, 2017; United Way, 2017). Whatever the underlying forces, the lack of progressive distribution of funding out to schools serving the kids that generate the new monies manifests one weak link in the WPF theory of action. If districts fail to progressively target dollars on high-needs schools, how can weighted funding nudge organizational changes inside schools that are proximal to teaching and learning?

## The California Case

The state's LCF reform swept aside scores of categorical aid programs in the summer of 2013, retaining about seven major programs. It also replaced earlier local revenue limits (put in place by Proposition 13 in 1978) with an elegantly simple WPF program. Gov. Brown's legislation set forth three funding tiers. The *base grant* provides equal dollars per pupil, varying by grade levels, in amounts set at about \$6,900 per K-6 student, \$7,200 for middle school pupils, and \$8,300 for each high school student, adjusted for inflation each year (California, 2013). *Supplemental grants* provide districts an additional 20% for each student from a low-income family, designated English learners, and those in foster care. *Concentration grants* further boost per-pupil distributions by an amount equal to 50% of the base grant, kicking-in for the first targeted student after the district reaches 55% of its total enrollment falling into one of the weighted categories.<sup>2</sup>

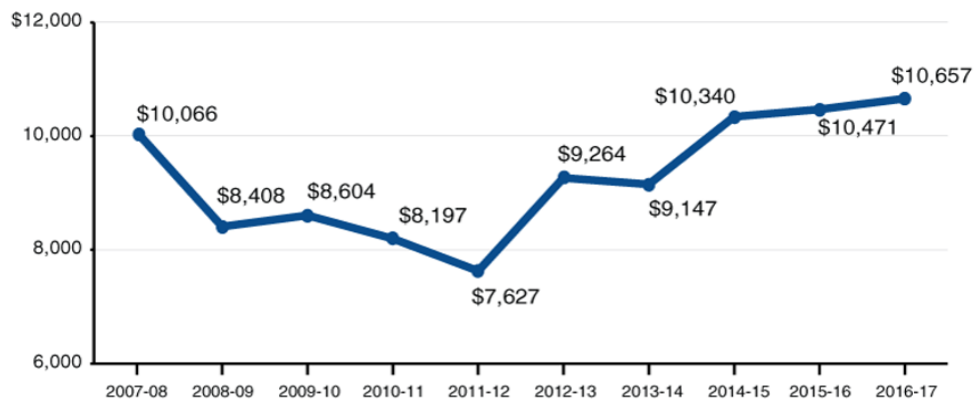
At the same time, districts only receive the 50% addition to their base grant for each student enrolled over the 55% level of targeted students in the district. So, a district that hypothetically enrolls just 10 students in one of the weighted categories above 55% of all weighted students would receive very little additional funding. Thus our analysis focuses on what analysts call the "intent-to-treat" – that is, all districts over the 55% cutpoint receive some level of additional funding, no matter how large or small their augmentation. Future work will compare the present results with the estimated effect of actual dollar augmentations tied to receipt of concentration grants.

### Rising School Spending, with Targeting on Poor Children

The LCF reform went into effect soon after being signed by the Gov. Brown for the 2013-14 school year. "We are bringing government closer to the people, to the classroom where real decisions are made, and directing the money where the need and the challenge is greatest," he said (Brown, 2013). California's resurging economy and the constitutionally required allocation for K-12 education was already spurring new spending on public schools. California's set-aside for schools equaled \$63.6 billion by 2016-17, about 88% flowing to local districts through the LCF mechanism, the remaining 12% via surviving categorical programs (California, 2016).

Overall spending rebounded to just above pre-recession levels by 2016-17 after adjusting for inflation, reaching \$10,657 per pupil (Figure 1, California, 2016). This level still places California in the bottom third of all states nationwide. The bulk of LCF funding is tied to the non-weighted base grant, equaling \$45.3 billion in the same year, compared with \$5.7 and \$3.3 billion in supplemental and concentration grants, respectively. Still, districts with large shares of disadvantaged pupils already enjoy the biggest gains in state support; this will continue into 2018-19 when LCF will likely be fully implemented. Districts with enrollments that include more than 25% weighted students will see per-pupil spending rise 5% over six years, compared with about a one-third gain in spending for districts in which 80% of all students fall into at least one of the disadvantaged categories (EdSource, 2016).

## K-12 Proposition 98 Funding Per Student Over Last Decade<sup>a</sup>



<sup>a</sup> Reflects inflation-adjusted amounts. In 2016-17 dollars.  
Posted July 2016.

Figure 1. Post-recession recovery in per pupil spending, California schools

The LCF initiative intends to widen civic participation in local budget discussions, a process resulting in a three-year Local Control Accountability Plan (LCAP). District leaders must consult a variety of stakeholders and devise a blueprint that specifies spending patterns that aim to advance the state's eight priorities and improve services for disadvantaged students (Wolf & Sands, 2016).<sup>3</sup> Districts do not have to report on the between-school distribution of dollars.

### Does Local Control Funding Improve Schools?

Studies of LCF implementation have relied mostly on interviews with district officials or school principals. The Governor's Office and state Board of Education have shown little interest in supporting rigorous evaluation of their massive experiment (Baker, 2013).<sup>4</sup> Since the inception of LCF, two teams have conducted qualitative research inside district offices to learn about implementation. They find that district leaders do exercise fiscal discretion by widening civic participation in budget discussions, and a variety of new efforts are often mounted to better engage disadvantaged students (Fuller & Tobben, 2014). District officials express confusion over whether, and if so how, to allocate supplemental and concentration grants, including how to improve pedagogy or supports for weighted students (Wolf & Sands, 2016). Some district leaders report seeing the LCAP's nudge for participatory budget as a compliance exercise, not one that spurs inventive thinking or serious assessment of what's working inside schools to lift achievement.

Concern has spread, four years into LCF implementation, over whether dollars generated through supplemental and concentration grants are being directed to schools that serve larger concentrations of weighted students. One large district, Fresno Unified, sought to use supplemental and concentration grant dollars to help fund teacher salary increases, even a



gunshot tracking program pushed by the city police department. This diversion of LCF funds was discouraged by the state schools chief following a legal complaint filed by the ACLU (Fensterwald, 2015). This local board reversed its earlier decision. The state’s chief educator has also ruled that LAUSD over estimated how much it spent on weighted students prior to the governor’s reform, allowing the district to under specify by about \$245 million in what it’s legally required to spend annually for services aimed at disadvantaged pupils (Kholi, 2016).

## Research Questions and Analytic Strategy

Overall, little is known about the extent to which the LCF reform alters the social organization of schools, especially in ways that elevate student learning over time. Figure 2 advances a simple causal sequence by which targeted aid – if allocated to schools that serve disadvantaged students – might spur organizational change. We assume that districts vary in the extent to which they progressively distribute new dollars to schools that host greater shares of low-achieving pupils (detailed for LAUSD in United Way, 2017). Racial or class interests inside districts, or the lack of technical capacity, may act to preserve institutionalized ways in which teachers and dollars are allocated among schools (Fuller, Marsh, Stetcher, & Timar, 2011; Hawley Miles & Rosa, 2006; Shipp, 2006).

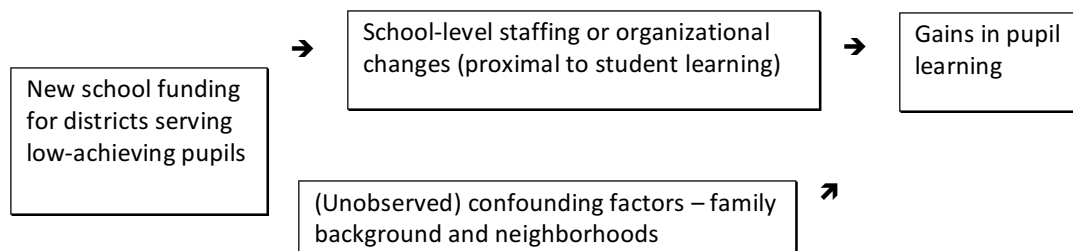


Figure 2. Causal logic for how targeted funding may affect school organizations, mediating achievement effects

When district leaders do allocate new dollars to schools with greater shares of weighted pupils, learning gains can only result if organizational change or gains in teacher quality result from these fresh allocations. Such organizational changes must be proximal to student engagement or learning. Given the data available in California, we can assess whether districts receiving additional funding tend to reduce average class size, enroll more high school pupils in rigorous courses, or improve working conditions by assigning teachers to fewer instructional periods each day. What’s key is that such changes in the social organization of schooling or the qualities of teachers must presumably be observed before achievement gains can be expected.

We focus on whether organizational shifts or achievement differences can be observed in when districts surpass the 55% threshold in terms of disadvantaged pupil enrollments. We compare these winners of concentration grants to districts, almost identical in terms of pupil demographics, but falling short of the 55% cutoff. This natural experiment allows for a regression-discontinuity design (RDD) in which confounding factors among districts are held

constant. The only factor that's changing in discrete fashion is the "intent-to-be-treated," the receipt of concentration grants.

This captures how LCF policy reflects a treatment in binary fashion, below or above the 55% cutpoint. The actual treatment is more continuous in that districts receive the addition funding for each pupil enrolled above the 55% composition of targeted students (see Appendix). Our intent-to-treat approach yields insight into the net impact of all of possible changes potentially induced by assigning districts to the treatment group. Future work and other researchers will examine that discrete effect of rising per pupil spending above the 55% cutpoint.

The statistical analysis specifically informs these empirical questions for the concentration-grant portion of California's Local Control Funding –

**RQ1.** How do school districts compare descriptively when falling on either side of the 55% threshold, triggering concentration grants, based on their share of students who are disadvantaged and weighted?<sup>5</sup>

**RQ2.** Do schools display organizational differences that are proximal to student learning when situated in districts receiving concentration grants, compared with schools in otherwise identical districts that do not receive concentration grants?

We focus on organizational features that all schools must report to the state Department of Education, including (a) the number of newly hired teachers, (b) count of class assignments for teachers, and (c) the percentage of high school courses meeting A-G college-entry requirements in California.

**RQ3.** Do students display stronger achievement on standardized tests when situated in districts receiving concentration grants, compared with peers in otherwise similar districts that did not receive concentration grants?

We look at both ends of the test-score distribution, including the share of students, enrolled in grades 3-8, falling below standard for their grade level, and those exceeding the standard, in mathematics and English language arts.

We also identify which groups of students benefit most from the infusion of concentration grants, and whether differing gains may reflect a narrowing of achievement gaps, defined along lines of race or ethnicity. Ideally, concentration grants narrow learning disparities between disadvantaged and better-off children statewide and among groups within districts.

## Methods

### Data

To inform these questions we draw data from three main sources: the 2013-14 LCF “funding snapshots,” 2014-15 LCF “state priorities snapshots,” and 2014-15 staff assignment and course-taking data submitted by districts to the California Department of Education (CDE). These data reports are newly required under the 2013 finance legislation, offering a wealth of funding and school-performance information, posted at <http://www.cde.ca.gov/ds>. We merged these data to provide complete accounts for 949 districts statewide, allowing us to test for organizational and achievement effects observed among district that received concentration grants.

*LCF funding snapshot data.* This includes LCF funding allocations, along with counts of differing types of weighted students from which funding levels are derived for the 2013-14 fiscal year. These data specify the percentage of unduplicated counts of weighted students for each district, those classified by districts as English learners, coming from poor families, and those in foster care.

Our assignment-to-treatment variable – the percentage of a district’s enrollment consisting of unduplicated weighted pupils (UPP) – is calculated by dividing each district’s weighted pupil count by total district enrollment. The UPP is used in determining eligibility for a concentration grant, when exceeding 55%, then calculating the concentration grant level. This makes for a clear assignment variable for our regression discontinuity design. Figure 3 shows the distribution of UPP shares among all California school districts in three recent years.

*LCFF state priorities snapshot data.* Current law requires districts to annually update their LCAP, which must include data on 26 elements, including measures of student achievement, student engagement, and school climate. The CDE then compiles the LCAP data coming from districts, generating the so-called state priorities snapshot data. We draw measures of student achievement for each district from the general 2014-15 CDE file, utilizing standardized tests administered to students in spring, 2015.

These student outcomes, aggregated to the district level, include (a) the percent of students who passed an Advanced Placement (AP) Exam with a score of 3 or higher, (b) the percent of students, grades 3 to 8, who scored Standard Exceeded in the state’s Smarter Balanced exams in mathematics and English-language arts (ELA), and (c) the percent of third to eighth-grade pupils who scored Standard Not Met.

Although the test results for ELA and mathematics are categorized by four levels of performance (Standard Not Met, Standard Nearly Met, Standard Met, and Standard Exceeded), we focus on the highest and the lowest achievement ranks, testing how concentration grants may be related to pupil performance at the low and high ends, lending insight into change in achievement disparities. Given that spring 2015 was the first year of Smarter Balanced

assessments, we gauge possible achievement effects two school years after the initial concentration grants were awarded to eligible districts.

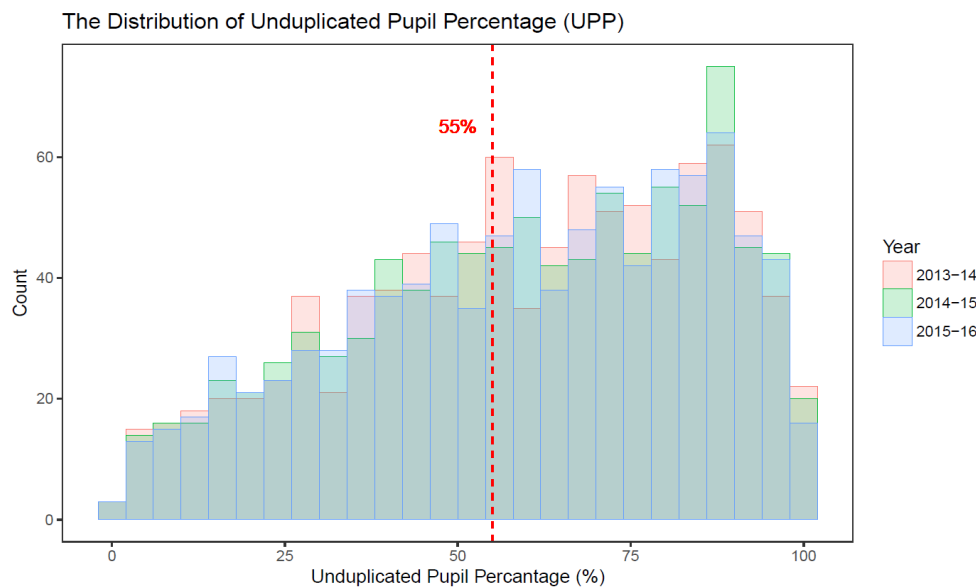


Figure 3. The distribution of districts' unduplicated percentage of weighted students statewide

*Staff assignment and course data.* To measure organizational change within schools (aggregated to the district), we exploit data on staffing and course-taking compiled by CDE. These data set include each course taken by high school students, including courses meeting college admission requirements of the University of California, known as the A-to-G course sequence, along with information on completion of AP courses. Students completing A-G or AP courses are generally viewed as traveling a more rigorous curriculum path.

The school staffing and course-taking data allowed us to generate measures of organizational features that may foster higher achievement. These measures include (a) the number of newly hired first year teachers in the district, (b) the average number of class periods assigned to teachers, one dimension of working conditions, and (c) the percent of courses meeting A-G requirements. The staff assignment and course data were collected in October 2014, which fell between the distribution of LCF funding (August 2013) and the administration of state achievement tests (April 2015). Table 1 shows descriptive statistics for the assignment variable and the outcome variables discussed above.

### Quasi-Experimental Analysis – Regression Discontinuity Design

We employ an RDD analysis, defined by Imbens and Lemieux (2008), to estimate the magnitude with which concentration grants may shape school-level organizational change or pupil achievement when aggregated to the district level. Our strategy takes advantage of the fact that receipt of a concentration grant is triggered when district enrollment is made up of

55% weighted students, plus one. Figure 4 shows how concentration grants arrive when the unduplicated pupil percentage (UPP) exceeds 55%, then varies widely based on the unduplicated count of weighted students.

Table 1. Descriptive statistics for assignment and outcome variables used in regression discontinuity design

Variable	N	Mean	SD	Min	Max
<i>Assignment variable</i>					
Unduplicated pupil percentage	949	0.60	0.25	0.01	1.00
<i>Outcome variables 1. Social-organizational features of high schools</i>					
The number of newly hired first-year teachers in the district	414	11.7	24.76	0	414
The average number of class periods assigned to teacher	414	5.5	1.32	2.67	14.97
The percent of classes meeting A to G requirements	414	57.3	15.32	0.00	86.71
<i>Outcome variables 2. Student achievement</i>					
The percent of students who passed an Advanced Placement (AP) Exam with a score of 3 or higher	400	57.4	19.11	0	100
The percent of 3-8 grade students who have scored "Standard Exceeded" in SBA ELA test	859	13.9	11.77	0	62
The percent of 3-8 grade students who have scored "Standard Not Met" in SBA ELA test	859	32.1	16.15	0	100
The percent of 3-8 grade students who have scored "Standard Exceeded" in SBA Math test	859	13.6	12.73	0	71
The percent of 3-8 grade students who have scored "Standard Not Met" in SBA Math test	859	34.8	16.81	0	100

The policy's strict cutoff allows us to utilize the so-called "sharp" regression discontinuity design. In sharp RDD, the probability of receiving the treatment equals 1 above a given threshold and 0 below, that is  $P(T = 1|x > x^*) = 1$  and  $P(T = 1|x < x^*) = 0$ . In our setting the assignment variable  $x$  is the percentage of UPP and the threshold  $x^*$  is 55%. The treatment  $T$  is whether to receive the concentration grants.

We assume that the treatment-assignment mechanism is determined only by the observable  $x$ , so that the discontinuity approximates a randomized experiment around the threshold  $x^*$ . School districts on either side of the threshold and close to it are expected to be very similar. Therefore, a difference in outcome variables can be attributed to the treatment.

The estimator of average treatment effect (ATE) is:

$$ATE = E(Y|x \in [x^*, x^* + \delta]) - E(Y|x \in [x^*, x^* - \delta]),$$

where  $Y$  is an outcome variable. Since this ATE is only identified when  $\delta$  is a very small value (i.e., for  $\delta \rightarrow 0$ ), the ATE is quantified in the difference between regression lines right at the

cutoff value  $x^*$ . In other words, we estimate the jump in the regression line relating the unduplicated pupil percentage to outcome variables right at the UPP cutoff of 55% at which the treatment assignment switches. This ATE is interpreted as an intent-to-treat (ITT) effect of assigning districts to the treatment group, those receiving concentration grants.

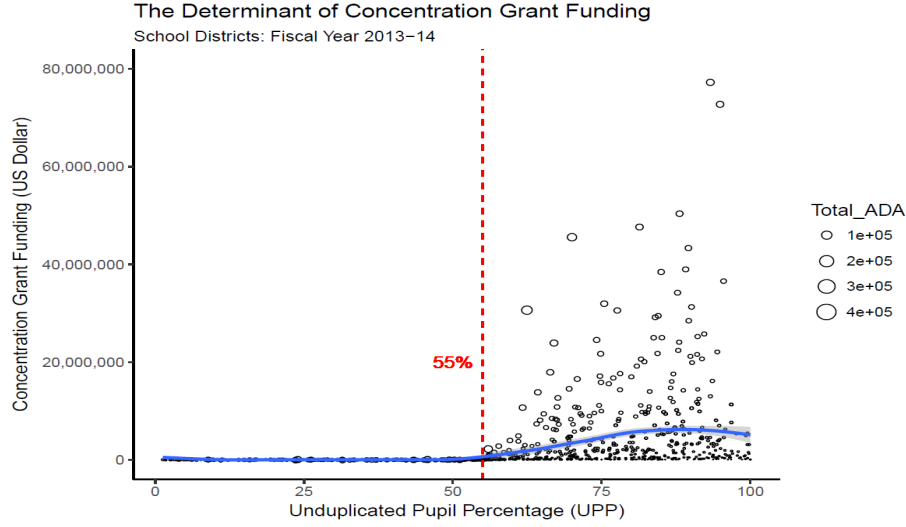


Figure 4. The distribution of California school districts' concentration grants, stemming from percentage of unduplicated weighted students

*Estimation and bandwidth selection.* We fit the following model to estimate the treatment effect of district receipt of concentration grants:

$$Y_i = f(x_i^c) + \theta T_i + \epsilon_i$$

In this model,  $Y_i$  is the value of an outcome for the  $i$ th school district, and the assignment variable  $x_i^c$  is the unduplicated pupil percentage in the  $i$ th district, re-centered so that it has a value of zero at the cutoff of 55% ( $x_i^c = x_i - x^*$ ). Since  $T_i$  indicates whether the  $i$ th district received a concentration grant (1 = received concentration grant, 0 = otherwise), its associated regression parameter  $\theta$  represents the treatment effect of the concentration grants on the outcome, estimated at the unduplicated pupil percentage of 55%.

$f(x_i^c)$  is a generic representation of the functional form of the hypothesized relationship between the outcome and the centered assignment variable  $x_i^c$ .<sup>6</sup> We choose to use nonparametric local linear regression for the  $f(x_i^c)$ , which primarily considers data points located closely around the 55% cutoff. The weighted local linear regression model with the Epanechnikov kernel function is fitted on either side of the cutoff, imposing higher weights to school districts which are closer to the cutoff. To determine the weights of the local linear regression at the cutoff, we use the Imbens–Kalyanaraman (Imbens & Kalyanaraman, 2009) bandwidth selection algorithm (IK hereafter). The IK algorithm considers the sample size as well as the variation and functional form of the outcome variable near the discontinuity in generating an ideal bandwidth which minimizes the mean square error.



Figure 5 shows the estimated kernel weights for local linear regression using IK bandwidth selection when the outcome is the percent of 3-8 grade students who have scored Standard Not Met in the state ELA test. Since the estimated IK bandwidth is  $7.98$ , data points outside the range of  $55 \pm 7.98$  are excluded, giving zero weights to those points. For data points receiving non-zero kernel weights, we see that kernel weights for the districts closer to the 55% cutoff are estimated higher.

We also check the robustness of our findings by using different bandwidth selection algorithm developed by Calonico, Cattaneo, and Titiunik (2015). Their bandwidth selection algorithm attempts to correct for bias due to under smoothing, which might emerge because the functional form of the regression lines at the cutoff is not well approximated. In the finding section, we call their estimator as a *bias-corrected* RDD estimator. We also present *robust* estimates which correct standard errors due to uncertainty in the bias correction. See Colonic, Cattaneo, and Titiunik (2015) for more detail.

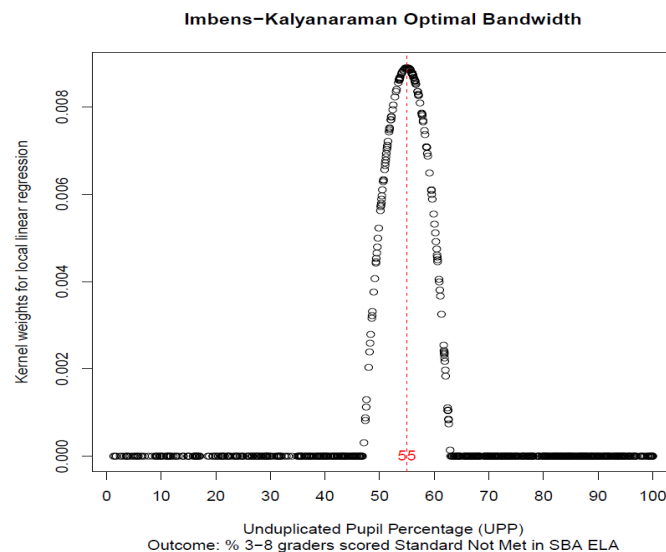


Figure 5. The estimated kernel weights for local linear regression using Imbens–Kalyanaram bandwidth selection when the outcome is the percent of 3-8 grade students who have scored “Standard Not Met” on ELA test.

**Checking assumptions.** Identification of treatment effects relies on the assumption that the treatment-assignment mechanism behaved as assumed. For example, there may be concerns that school districts with slightly lower UPPs could manipulate their records to receive concentration grants. In that case, the distribution of the assignment variable could be discontinuous at the cutoff, with surprisingly many districts just barely eligible for receiving concentration grants and surprisingly few failing to eligibility. These discontinuities or bumps in the density of assignment variable can be visually inspected or formally tested with a hypothesis test. We perform the McCrary test (McCrary, 2008) to assess potential discontinuities at the cutoff of the assignment variable (UPP). Figure 6 shows the results from the McCrary test, suggesting no significant discontinuity of density around the 55% cutoff.

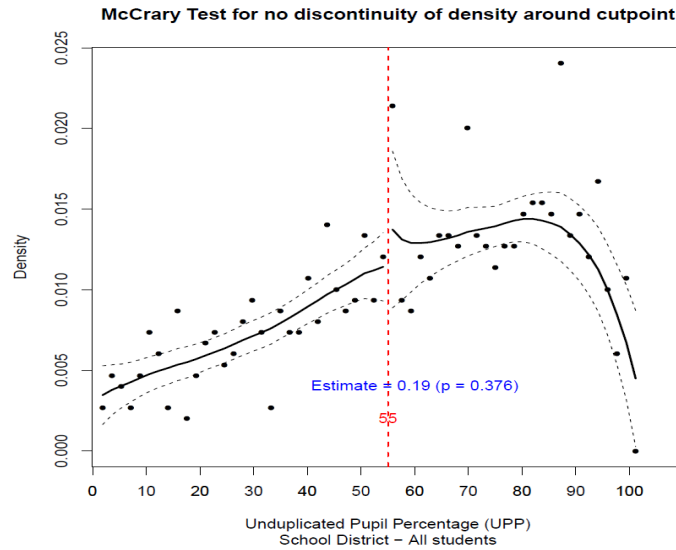


Figure 6. The McCrory test for checking discontinuities around the cutoff of the assignment variable

Another assumption check is to replace the actual outcome in the RDD analysis with non-outcome covariates, particularly those collected prior to treatment implementation. If we would observe a treatment effect on a pre-treatment covariate, doubt would be cast on the validity of the RDD analysis. A desired result is that no effect at the cutoff is found in any of the pretreatment covariates. We chose two sets of variables — average daily attendance in each grade span and enrollment by ethnicity and EL status. These variables can clearly be regarded as pre-treatment covariates, because they are measured and reported before distributing concentration grants.

## Findings

We first describe how districts compare when falling on either side of the 55% cutpoint. Table 2 presents estimated differences in the pre-treatment covariates between treatment and control groups. Note that pre-treatment covariates were transformed by their natural logarithm to mitigate against the extremely heavy right tails of the distributions of enrollment variables. The first column gives raw differences, which are mean differences between all districts located on the right-hand side of the cutoff and all districts on the left-hand side. It shows that districts in treatment group have significantly more kindergarten, elementary, middle, and high school students on average than districts in the control group, while no differences were found in 4-6 grade students. The difference in size becomes statistically insignificant when we restrict our sample to observations within the 1K bandwidth.

The second set of estimates uses Imbens and Kalyanaraman (2009)'s bandwidth selection algorithm, providing raw mean difference for districts within selected range and *weighted* local conditional mean difference within the range. The latter indicates RDD estimates. As shown, none of the RDD estimates is statistically significant. This suggests that districts falling on either

side of the 55% threshold for receiving concentration grants do not differ significantly based on their student enrollment for each school level or grade span.

Table 2. Estimated differences between treatment and control groups in (log-transformed) pre-treatment covariates

Variable	Descriptive statistics	Raw difference		Imbens & Kalyanaraman (2009)'s bandwidth selection algorithm				
	Mean (Std.Dev)	Total N	Mean diff.	Band width	Band range (non-zero weights)	N within band	Mean diff.	RDD Estimate (Std.Err)
<i>Average Daily Attendance (ADA) in each grade span (log transformed)</i>								
Total ADA	7.15 (2.00)	949	0.02 (0.13)	5.18	[49.90, 60.10]	122	0.11 (0.34)	0.87 (0.68)
ADA K-3	5.63 (2.50)	949	0.30* (0.17)	5.15	[49.87, 60.06]	122	-0.51 (0.44)	0.61 (0.79)
ADA 4-6	5.33 (2.49)	949	0.23 (0.17)	4.91	[50.20, 59.90]	120	-0.46 (0.44)	0.91 (0.90)
ADA 7-8	4.69 (2.65)	949	0.39** (0.18)	4.81	[50.20, 59.60]	118	-0.35 (0.46)	0.51 (0.94)
ADA 9-12	5.63 (2.50)	949	0.30* (0.17)	5.15	[49.87, 60.06]	122	-0.51 (0.44)	0.61 (0.79)
<i>Enrollment by student subgroups (log transformed)</i>								
Total enrollment	7.18 (2.02)	937	0.02 (0.13)	5.23	[49.90, 60.20]	123	0.11 (0.34)	0.81 (0.67)
English learner	5.50 (2.26)	866	1.28*** (0.15)	5.91	[49.10, 60.90]	128	0.33 (0.42)	0.74 (0.73)
Latino/Hispanic	6.17 (2.33)	925	0.89*** (0.15)	5.66	[49.40, 60.60]	133	0.09 (0.38)	0.84 (0.71)
Black	3.63 (2.29)	767	0.10 (0.16)	6.00	[49.10, 60.90]	119	0.29 (0.42)	1.02 (0.84)

Note. \*\*\* $p \leq .01$ , \*\* $.01 < p \leq .05$ , \* $.05 < p \leq .10$

Districts in the treatment group tend to have more Latino students and English Learners, but these differences no longer persist, becoming insignificant, when the sample is restricted to districts within the 1K band and when we give more weight to districts closer to the 55% cutoff. Thus, we can conclude that, at least on observed pre-treatment student composition, the conditional expectations are smooth at the discontinuity. This boosts our confidence in the validity of any estimated treatment effects.

Table 3 displays a sample of 10 unified or high school districts located close to the 55% cutoff. They vary in terms of enrollment size and the share of students designated as English learners. Yet otherwise, pupil compositions look quite similar across these illustrative districts. Note also that the amount of concentration grants received varies greatly, which likely conditions the magnitude of possible effects. Our analytic method tests only for whether receipt of a concentration grant, not the amount, is predictive of organizational change or achievement differences.

Table 3. Ten school districts around the 55% threshold

District	Enrollment	%EL	%Latino	%Black	UPP	Concentration Grant (CG)	Per-pupil CG	Per-pupil Total Funding
San Rafael City High	2,365	16.6	53.3	2.3	48.0	0	0	8,355
Hanford Joint Union High	3,845	8.6	61.2	5.8	53.3	0	0	9,262
Escalon Unified	2,695	20.5	47.7	0.4	53.7	0	0	8,855
Santa Barbara Unified	14,291	30.6	60.4	0.9	54.1	0	0	8,394
Imperial Unified	3,898	23.1	80.9	1.5	54.9	0	0	8,278
East Side Union High	23,685	14.8	46.5	2.9	55.2	196,879	15	9,640
Tracy Joint Unified	15,761	25.8	49.9	6.4	55.5	323,896	37	8,704
Elk Grove Unified	62,196	17.3	25.9	14.1	56.0	2,218,799	64	8,350
Grossmont Union High	17,580	12.8	34.1	5.9	56.2	845,789	86	9,522
Chaffey Joint Union High	24,598	10.1	63.5	8.1	57.7	2,828,169	199	9,614

### Effects on Social-Organizational Features of High Schools

Table 4 summarizes estimated differences in outcome variables between treatment and control groups, districts falling above or below the 55% cutoff, respectively. The first set of outcome variables pertain to social-organizational features of high schools, as reported by 412 districts with complete data. *Raw differences* between all treatment and control districts are notable. We see almost no difference in the count of new teachers hired between districts falling below versus above the 55% cutoff (1.4 teachers). However, districts above the cutoff assign their high school teachers about one-third more class periods each day, on average, compared with district falling below the 55% marker. The former districts offer almost 9% fewer courses meeting A-G criteria than the latter districts.

But results are quite different when estimating the discrete effect of receiving concentration grants *after pruning districts far from the 55% cutoff*. The *IK* regression-discontinuity estimates show that weighted districts above cutoff were able to hire 10 additional teachers beyond the average for weighted districts falling below the 55% cutoff. The districts receiving concentration grants assigned teachers one fewer class periods each day on average, and offered a greater share of courses that met A-G requirements (about 10% higher), compared with weighted districts not receiving concentration grants.

Figure 7 vividly illustrates the bump experienced by districts with high schools that benefited from concentration grants. Each dot on this pair of plots represents a school district, placed in relation to its UPP and the outcome of interest: new teachers hired or class periods

Table 4. Estimated differences between treatment and control groups for organizational features of schools and student achievement

Variable	Raw difference		IK algorithm					CCT algorithm	
	Total N	Mean diff. (Std.err.)	Band width	Band range (non-zero weights)	N within band	Mean diff. within IK band (Std.err.)	IK RDD Estimate (Std.err.)	Bias-corrected RDD Estimate (Std.err.)	Robust RDD Estimate (Std.err.)
<i>Outcome variables 1. Social-organizational features of high schools</i>									
The number of newly hired first-year teachers in the district	412	1.36 (2.12)	11.58	[43.90, 66.50]	122	2.21 (3.04)	9.87** (3.96)	13.62*** (4.43)	13.62** (5.39)
The average number of class periods assigned to teacher	412	0.30** (0.12)	5.31	[50.20, 60.20]	56	-0.44 (0.30)	-1.12 (0.73)	-1.12** (0.50)	-1.12* (0.58)
The percent of classes meeting A to G requirements	412	-8.75*** (1.42)	10.24	[45.10, 64.70]	109	-1.39 (2.92)	10.31* (5.26)	12.34** (5.26)	12.34** (6.16)
<i>Outcome variables 2. Student achievement</i>									
The percent of students who passed an Advanced Placement (AP) Exam with a score of 3 or higher	400	-18.29*** (1.73)	9.36	[45.80, 64.40]	98	-3.01 (3.18)	17.21*** (6.08)	22.46*** (7.08)	22.46*** (8.14)
The percent of 3-8 grade students who have scored "Standard Exceeded" in SBA ELA test	859	-14.98*** (0.76)	7.61	[47.40, 62.60]	157	-1.30 (1.00)	3.61** (1.51)	3.31** (1.35)	3.31** (1.54)
The percent of 3-8 grade students who have scored "Standard Not Met" in SBA ELA test	859	20.26*** (0.86)	7.98	[47.20, 62.90]	160	1.33 (1.53)	-6.11*** (1.99)	-5.34*** (1.89)	-5.34** (2.17)
The percent of 3-8 grade students who have scored "Standard Exceeded" in SBA Math test	859	-16.05*** (0.83)	8.04	[47.20, 63.00]	161	-2.13* (1.13)	3.01* (1.75)	2.67* (1.50)	2.67 (1.74)
The percent of 3-8 grade students who have scored "Standard Not Met" in SBA Math test	859	21.94*** (0.88)	8.96	[46.20, 63.80]	175	1.71 (1.59)	-4.49** (2.21)	-4.61** (2.03)	-4.61** (2.34)

Note. \*\*\* $p \leq .01$ , \*\* $.01 < p \leq .05$ , \* $.05 < p \leq .10$ ; IK = Imbens & Kalyanaraman (2009), CCT = Calonico, Cattaneo, and Titiunik (2015) bandwidth selection algorithm.

assigned to teachers (left and right-hand panels). The dark lines trace the estimated association between the two variables (a regression line). The light gray lines represent the confidence interval with which the regression estimates are calculated. The sharp breaks at the 55% cutoff illustrates the jump in new teachers hired (left panel) and fewer class period assigned (right panel) indicate the effects of concentration grants on districts' organizational behaviors.

## Effects on Student Achievement

Table 4 also shows differences in student achievement levels for district above or below the 55% cutoff. We obtained California's standardized test scores in math and ELA for students enrolled in grades 3-8, with  $n=859$  districts reporting complete data. In addition, we report on the performance of secondary school pupils on AP exams, scoring 3 or higher, with  $n=412$  districts operating high schools and providing complete data. We again display raw differences between the two sets of district, then estimate differences between weighted districts laying with the bandwidth around the 55% cutoff.

Let's first look at achievement differences in ELA among pupils, grades 3-8. We see that over 20% (20.3) more students fail to meet the proficiency standard ("Standard Not Met") in districts above the 55% cutoff, compared with peers attending schools in districts below. This is not surprising given that the former group of students is more likely to come poor or non-English speaking homes. This can be clearly seen in the upper-right panel of Figure 8. The upwardly sloping regression line indicates that the percentage of students failing to meet proficiency climbs rapidly as UPP shares rise.

But note the drop in the regression lines at the 55% cutoff: detecting an interruption in the expected association between pupil background and achievement. Back on Table 2 we see that the share of pupils failing to meet the ELA standard was about 6% (6.1) less when compared with weighted districts below the 55% cutoff. Similarly, the share of students meeting the ELA standard was about 4% (3.6) higher in districts above the cutoff and thus receiving concentration grants (seen in the upper-left panel, Figure 8). These results for ELA are consequential and statistically significant.

Differences between districts below and above the 55% are less remarkable when it comes to math. The lower-left panel in Figure 8 shows the share of pupils meeting the math standard, grades 3-8, plotted against UPP enrollment shares. As expected, the percentage of students exceeding the math standard falls as UPP shares rise. But near the 55% cutoff we see a jump up in the estimated percentage of students clearing the math proficiency bar. This advantage equals 3%. But the difference is only marginally significant (at  $p<.10$ ) and not significant for one of the tests in mean differences (Table 4). On the other hand, the 4.5% fewer students who fail to meet the math standard is statistically significant. Overall, math achievement effects stemming from concentration grants are less consistent than the encouraging effects on pupils' ELA performance in districts above the 55% cutoff.



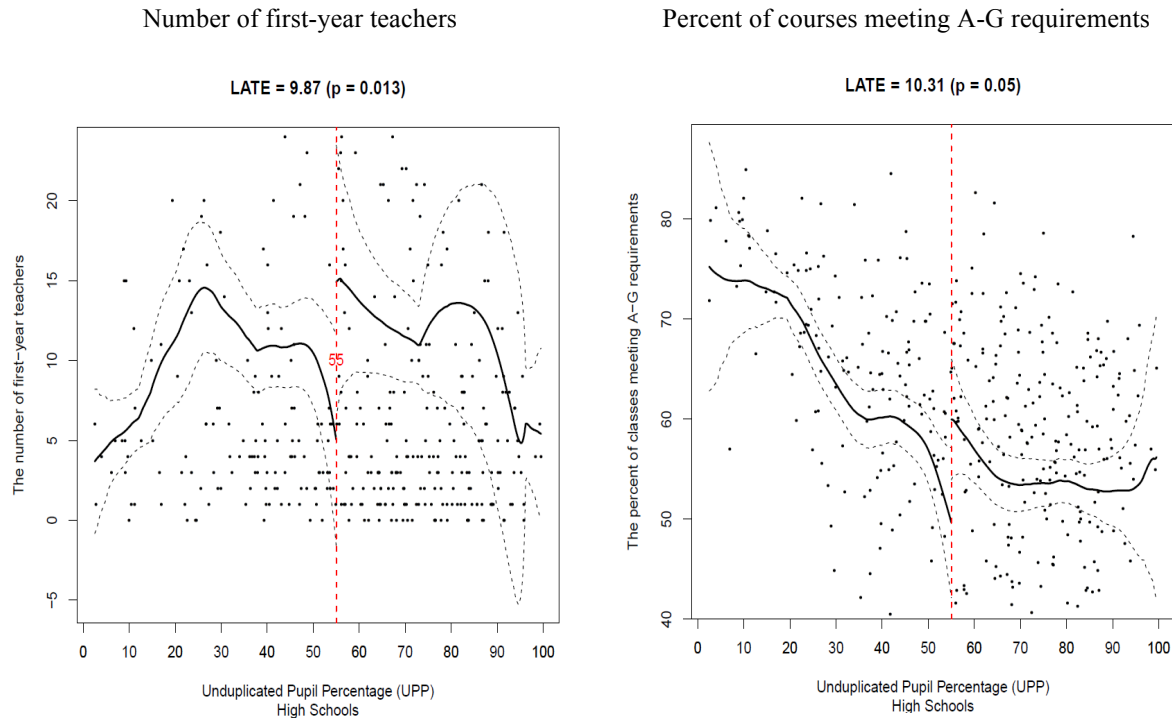


Figure 7. Positive organizational outcomes for districts close to the 55% cutoff. Lines represent local linear regressions using an Epanechnikov kernel and IK bandwidth. Dots represent school districts.

Finally, we see that the share of high schoolers scoring a 3 or higher on AP exams was considerable greater for the average weighted district above the 55% cutoff. The percentage of pupils scoring 3 or higher equaled 17% percent (17.2) greater than the average weighted district above the line, compared with the average district below. This difference is highly significant and consequential in pragmatic terms.

We also tested for heterogeneous treatment effects by estimating separate models for specific racial or ethnic groups. Table 5 shows that most significant effects, stemming from receipt of concentration grants, are felt by Latino students and not by other groups. This is true for the percent of high school students passing AP courses with a score of 3 or higher, as well as test score results for pupils in grades 3 through 8.

This specificity of student achievement effects may not be surprising, given that districts with more than 55% UPP are dominated by Latino pupils. On the other hand, the lack of effects for Black students is worrisome. The lack of positive effects on White students – a distinct minority in districts winning concentration grants – does suggest that the treatment overall may narrow achievement gaps between Latino and White students. More work is required to understand how gap-closing works among schools within districts.

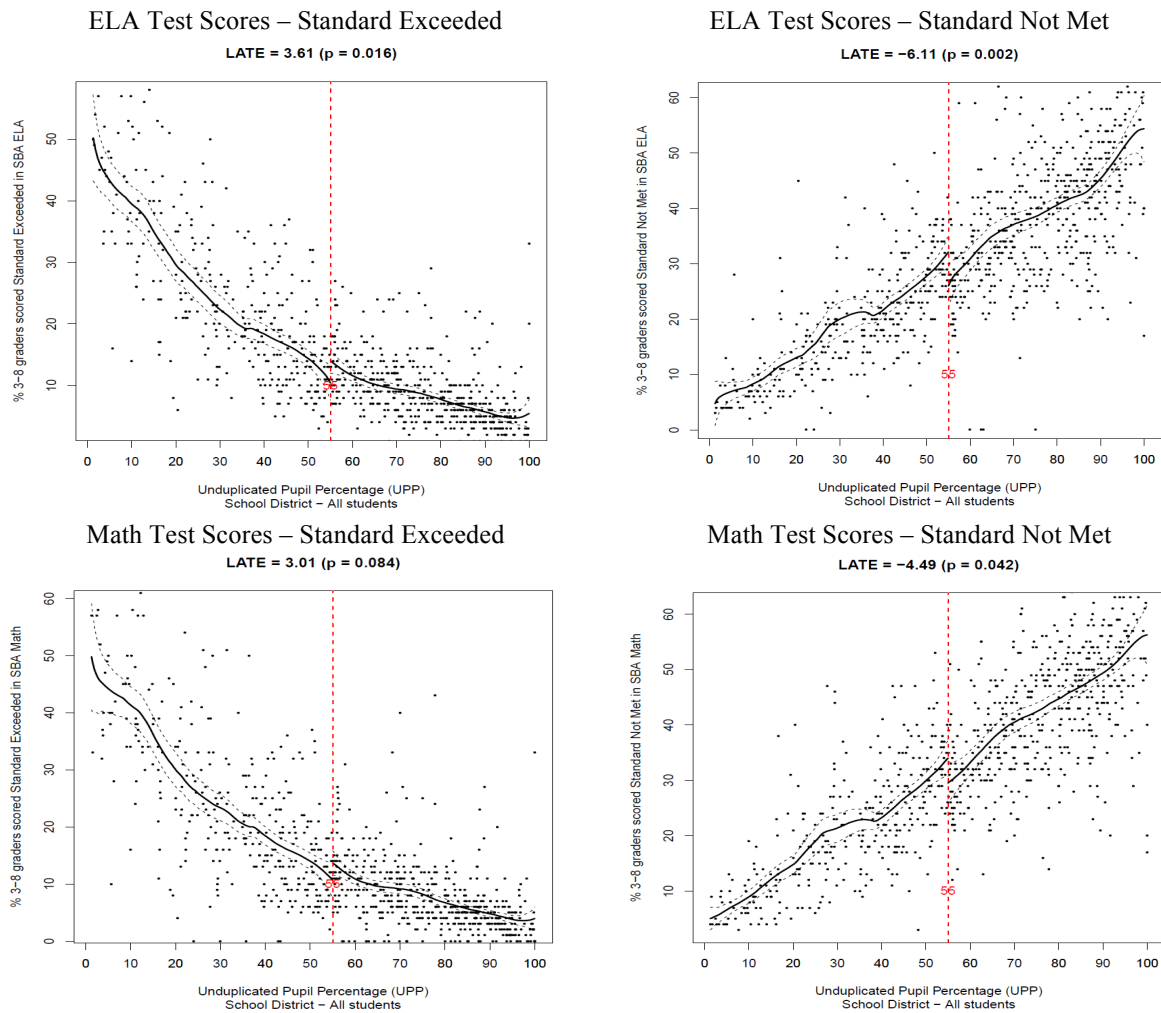


Figure 8. Each California district placed in relation to percentage of students meeting or not meeting proficiency standard and UPP. Lines represent local linear regressions using an Epanechnikov kernel and IK bandwidth.

## Discussion and Policy Implications

California's wager on restoring budget authority to local boards – then awarding many with \$41 billion in progressively distributed funding in recent years – offers a huge experiment in decentralized public finance. The Local Control Funding initiative stemmed from growing frustration with divergent streams of categorical aid, fragmented programs that were centrally designed and tightly regulated. Collateral faith in site-based management, freeing principals to select their own staff, manage their budgets, and perhaps focus on instructional gains, also shifted the policy discourse in California over the past generation.

Table 5. Heterogeneous effects of concentration grants on student achievement by ethnicity subgroups

Variable	Estimate	Combined	White	Latino	Black
The percent of students who passed an Advanced Placement (AP) Exam with a score of 3 or higher	N (Bandwidth)	400 (9.36)	380 (10.80)	388 (8.92)	241 (9.59)
	<i>IK</i> RDD Estimate (Std.err.)	17.21*** (6.08)	6.04 (3.77)	20.13*** (7.72)	11.37 (16.97)
The percent of 3-8 grade students who have scored “Standard Exceeded” in SBA ELA test	N (Bandwidth)	859 (7.61)	690 (8.75)	681 (6.47)	293 (8.68)
	<i>IK</i> RDD Estimate (Std.err.)	3.61** (1.51)	1.10 (1.84)	2.14** (1.01)	2.19 (2.52)
The percent of 3-8 grade students who have scored “Standard Not Met” in SBA ELA test	N (Bandwidth)	859 (7.98)	690 (8.27)	681 (8.77)	293 (10.33)
	<i>IK</i> RDD Estimate (Std.err.)	-6.11*** (1.99)	-2.92 (2.21)	-3.67* (2.20)	-4.31 (4.28)
The percent of 3-8 grade students who have scored “Standard Exceeded” in SBA Math test	N (Bandwidth)	859 (8.04)	690 (8.77)	680 (6.59)	293 (7.24)
	<i>IK</i> RDD Estimate (Std.err.)	3.01* (1.75)	0.52 (2.20)	0.14 (1.17)	-1.89 (2.90)
The percent of 3-8 grade students who have scored “Standard Not Met” in SBA Math test	N (Bandwidth)	859 (8.96)	690 (8.90)	680 (8.60)	293 (10.45)
	<i>IK</i> RDD Estimate (Std.err.)	-4.49** (2.21)	-2.79 (2.62)	-3.81** (1.90)	-3.50 (6.91)

Note. \*\*\*p≤.01, \*\*p≤.05, \*p≤.10; IK = Imbens & Kalyanaraman (2009) bandwidth selection algorithm.

The Golden State devised a hybrid finance structure: devolving budget authority within a framework of eight state policy goals, progressively distributing dollars among districts, and mandating a seemingly democratic process for devising budgets locally. Many advocates argue that two pieces are missing: holding districts accountable for whether they channel dollars to the kids that generate new revenue through supplemental and concentration grants; and an explicit strategy to evaluate the school-level effects of this massive public investment.

### Positive Organizational and Achievement Effects

We do find encouraging results from the concentration-grant element of the LCF reform. Districts with more than 55% of their enrollments in the weighted-pupil categories – thus winning concentration grants from Sacramento – are more likely to engage in organizational

changes that may mediate learning gains. Additional analysis will tell us whether improved working conditions for teachers (e.g., by teaching fewer class periods) or a more rigorous curriculum (offering a higher share of A-G courses) helps to explain the achievement gains that we detected. The present analysis at least establishes that concentration grants offered fresh resources that prompted organizational change at the school level.

Following along this hypothesized causal pathway, we estimated higher achievement in ELA for students enrolled in districts that received concentration grants. We see gains at both the low and high ends of pupil performance. That is, the share of students, grades 3-8, who failed to meet the state's proficiency standard eased among districts above the 55% cutpoint. And the share that did meet standard ranged higher, compared with (weighted) districts falling below 55%, failing to win concentration grants. These achievement benefits were felt by Latino students. Pupils in other ethnic groups did not show commensurate gains in test scores.

### **Limitations and Persisting Questions**

Our analysis is limited to the first two years of LCF implementation. The initiative is about to enter its fifth year of implementation. As data become available, we can test for whether these organizational and pupil-achievement gains persist over time. We hope to identify additional organizational features of schools – including assignment of new teachers – which may help to explain changes in pupil achievement. We can further disaggregate how student subgroups may differentially benefit from infusions of concentration grants.

The fact that gains were felt by Latino students is encouraging, suggesting that achievement gaps are narrowing vis-à-vis White peers. More work is required, however, to understand whether concentration grants or parallel elements of LCF are closing disparities among student groups *within* districts, and through what organizational changes. Our inability to detect gains for Black students is worrisome.

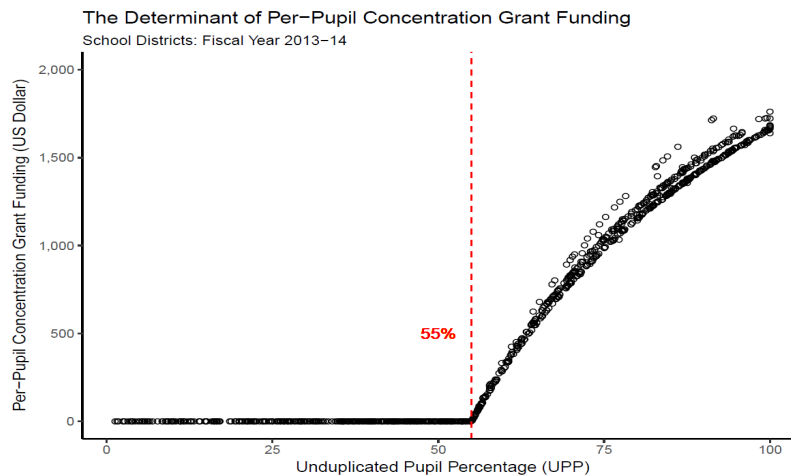
Staffing data can be further exploited to better understand possible effects on the kinds of teaching or student support positions added to school payrolls, along with related organization-level change. Our work in Los Angeles reveals that new LCF funding has led to shrinking shares of dollars going for teaching posts in high schools, as dollars go for counselors, assistant principals, and pupil support staff (United Way, 2017).

The effects we estimate stem solely from the concentration-grant portion of LCF, which equaled \$3.3 billion statewide in 2016-17, or about one-sixth of the governor's program. We could estimate how supplemental grants contribute to these effects – perhaps interacting with levels of concentration grants – although such estimates could be contaminated by confounders proxied by the level of supplemental grants. In addition, per pupil spending tied to concentration grants is not as discontinuous as whether grants are given or not (discussed above and see the Appendix). Further analysis is required to learn how spending gains shape school-level organizational change and achievement.

This paper offers one method for understanding one element of this massive experiment in progressive public finance. We emphasize how organizational change or shifts in teacher qualities logically precedes gains in student engagement and learning. Simply associating new funding with pupil outcomes does little to build theory over *how* decentralizing finance may set in motion practices on the ground that alter schools and pupil motivation, or not. Nor do good intentions or high-sounding rhetoric about helping poor children suffice. When the political stars do align to progressively fund schools, we must rigorously dig into whether these well-meaning initiatives truly advance fairness and, if so, through what practices inside schools?

## Appendix

The dollar increase per pupil received by districts that receive concentration grants, seen below for 2013-14, is not as discontinuous at the 55% cutpoint relative to the overall intent-to-treat discontinuity. Future work might use “kinked regression” techniques to associate spending changes with organizational practices and differences in student achievement.



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## Endnotes

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<sup>1</sup> Thanks go to Bill Lucia of the EdVoice organization in Sacramento for this vivid analogy.

<sup>2</sup> The state counts weighted students just once, so-called “unduplicated pupils”, when they fall into more than one category of disadvantage.

<sup>3</sup> The state’s centrally articulated goals offer a mix of intentions, which emphasize implementation of Common Core State Standards; improving parent participation, school climate, and basic services; widening “course access” and deepening student engagement, along with raising student achievement and “other student outcomes” (California, 2013).

<sup>4</sup> The medium-term assessment of Gov. Schwarzenegger’s consolidation of categorical aid programs did find a warm reception from district officials, many reporting that they welcomed the new fiscal discretion. At the same time, districts displayed limited capacity overall in assessing school-level change or achievement effects stemming from new or recast programs aimed at lifting students (Fuller, Marsh, Stetcher, & Timar, 2011).

<sup>5</sup> That is, the percentage of a district’s enrollment that includes students from low-income families, English learners, or those in foster care.

<sup>6</sup> We included additional covariates such as the natural log of total enrollment and the type of school district (unified, elementary, or high school) in the model specification. This is not necessary for treatment-effect identification. Still, it can be useful in improving precision. The results with or without including covariates did not differ significantly.