GETTING DOWN TO FACTS II

Technical Report

Money and Freedom: The Impact Of California's School Finance Reform On Academic Achievement And The Composition Of District Spending

Rucker C. Johnson
University of California, Berkeley

Sean Tanner
Learning Policy Institute

September 2018

About: The *Getting Down to Facts* project seeks to create a common evidence base for understanding the current state of California school systems and lay the foundation for substantive conversations about what education policies should be sustained and what might be improved to ensure increased opportunity and success for all students in California in the decades ahead. *Getting Down to Facts II* follows approximately a decade after the first Getting Down to Facts effort in 2007. This technical report is one of 36 in the set of *Getting Down to Facts II* studies that cover four main areas related to state education policy: student success, governance, personnel, and funding.

Stanford University

PACE
Policy Analysis for California Education

Money and Freedom: The Impact Of California's School Finance Reform On Academic Achievement And The Composition Of District Spending

Rucker C. Johnson

Goldman School of Public Policy
University of California, Berkeley

Sean Tanner

Learning Policy Institute[1]

[1] Sean Tanner is a senior research associate with the Center for School Accountability and Performance (CSAP) Program at WestEd. He previously served as a senior researcher for the Learning Policy Institute, where he wrote the report with Johnson.

Abstract

California's recent major school finance reform, the Local Control Funding Formula (LCFF), attempts to address resource inequity by reallocating school finances on the basis of student disadvantage (rather than district property wealth) and relinquishing many of the restrictions on how revenue can be spent. Beyond a uniform "base grant" given to all districts, the LCFF reallocates additional district revenues based almost entirely on the proportion of disadvantaged students (e.g., low-income, limited English proficiency) in each district. We show LCFF significantly increased per-pupil spending, and the state now has among the most progressive funding formulas in the country. This study is among the first to provide evidence of LCFF's impacts on student outcomes. For cohorts born between 1990 and 2000, we constructed a school-by-cohort-level panel data set of school-age years of per-pupil spending, high school graduation rates, and student achievement in high school in math and reading, for all public schools in California.

We examine how simultaneous changes in spending levels and extent of categorical restrictions of state funding impact school inputs and the distribution and composition of district per-pupil spending. Using detailed annual district finance data (1995-2016), we find that LCFF-induced increases in district revenue led to a significant reduction in the average school-level student-to-teacher ratio and led to significant increases in average teacher salaries and instructional expenditures.

Our research design employs an instrumental variables approach in an event-study framework, using the LCFF funding formula as instruments, to isolate the effects of increases in district per-pupil spending on student outcomes. The empirical strategy compares changes in average student outcomes across cohorts from the same school before and after LCFF-induced changes in district per-pupil revenue (over and beyond statewide, cohort-specific time trends).

We find that LCFF-induced increases in school spending led to significant increases in high school graduation rates and academic achievement, particularly among poor and minority students. A $1,000 increase in district per-pupil spending experienced in grades 10-12 leads to a 5.9 percentage-point increase in high school graduation rates on average among all children, with similar effects by race and poverty. On average among poor children, a $1,000 increase in district per-pupil spending experienced in 8th through 11th grades leads to a 0.19 standard-deviation increase in math test scores, and a 0.08 standard-deviation increase in reading test scores in 11th grade. These improvements in high school academic achievement closely track the timing of LCFF implementation, school-age years of exposure and the amount of district-specific LCFF-induced spending increase. In sum, the evidence suggests that money targeted to students' needs can make a significant difference in student outcomes and can narrow achievement gaps.

I. Introduction

Children born into socioeconomically disadvantaged families in the United States face numerous obstacles in obtaining a high-quality primary and secondary education, leading to underinvestment in postsecondary education and lower lifetime earnings. To take teacher quality as one example, African-American and Hispanic students, English language learners, and students from lower socioeconomic families attend schools whose teachers are less experienced, perform worse on licensure exams, earn lower salaries, and have lower value-added scores than their more advantaged counterparts (Clotfelter, Ladd, & Vigdor, 2005; Goldhaber, Lavery, & Theobald, 2015; Lankford, Loeb, & Wyckoff, 2002). Residential segregation by race and socioeconomic status exacerbates such differential exposure, as teacher sorting is most pronounced across districts and schools, rather than between classrooms within schools (Goldhaber et al., 2015). This inequitable distribution of teacher quality is likely responsible for a portion of the inequality in standardized achievement tests (Clotfelter, Ladd, & Vigdor, 2010), as gaps by socioeconomic and racial status have persisted, and in some cases grown, since the early 1970s despite a contemporaneous reduction in school finance inequality (Corcoran & Evans, 2015; Reardon, 2011). The links between elementary and secondary school quality and adult outcomes such as postsecondary attainment, earnings, and criminal involvement are becoming increasingly well documented (Chetty et al., 2011; Chetty, Friedman, & Rockoff, 2013), reinforcing the lifelong handicap conferred upon disadvantaged children through lower educational quality.

While reallocating school revenues so that districts serving disadvantaged children can offer equal (or superior) educational quality to that of districts serving more advantaged children, the economic and policy literature is divided as to whether increases in per-pupil spending will meaningfully bolster student achievement. Some high-profile studies have found weak relationships between per-pupil expenditures and student achievement (Coleman et al., 1966; Hanushek, 2003), yet these have failed to identify causal relationships because they do not sufficiently account for selection bias, the demographic composition of schools, and family background factors that influence both school spending and student achievement.

Several studies have more plausibly isolated the causal effect of increased school spending on student success by analyzing major school finance reforms, both legislative and judicially mandated. These reforms increased expenditures per pupil and, consequently, narrowed gaps in performance on standardized tests, increased high school graduation rates, and bolstered adult success in the labor market (Candelaria & Shores, 2015; Card & Payne, 2002; Jackson, Johnson, & Persico, 2016; Johnson and Jackson, 2017; Lafortune, Rothstein, & Schanzenbach, 2015). While this body of research reveals a clear link between per-pupil spending and student outcomes on average, there is some evidence that historically disadvantaged students are not the prime beneficiaries of many school finance reforms. This is primarily because reforms have typically sought to equalize spending across levels of district property wealth, which is only partially correlated with student-level disadvantage (Hoxby, 2001; Hyman, 2013; Lafortune, Rothstein, Schanzenbach, 2015).

Moreover, many reforms transferred considerable fiscal power to state governments, that could place restrictions on how district revenues could be spent. It is unclear how efficacious this strategy is in enhancing student achievement. While several studies have addressed the impact of particular categories of funding, such as capital improvements (Cellini, Ferreira, & Rothstein, 2010) or pedagogical technology (Leuven, Lindahl, Oosterbeek, & Webbink, 2007), there remains a dearth of well-identified studies of the effects of restricted funding generally.

California's recent major school finance reform, the Local Control Funding Formula (LCFF) signed into law in 2013, provides an opportunity to separately test for the effects of 1) a substantial change in the levels of funding, and 2) the extent of restrictions on school financial resources, within a policy directed specifically at disadvantaged students rather than district property wealth. The LCFF, detailed in the next section, is both a massive investment in districts serving disadvantaged students and a modest relaxation of restrictions on district expenditures. It is California's attempt to overcome decades of legal and economic turmoil that had placed the state's average district revenues, just prior to the policy change, among the nation's lowest (California Budget Report, 2017). The policy reallocates district revenues based almost entirely on the proportion of unduplicated disadvantaged students in each district -- those who qualify for free- or reduced-price lunch, have limited English proficiency, are in foster care, or are homeless. Moreover, the state relinquished many of the restrictions on how districts could spend their revenues, creating a great deal more flexibility for some districts but not others. Given the magnitude and heterogeneous nature of changes to school finance that resulted from LCFF, the policy provides a test of how financial resources and flexibility can each shape student achievement.

For cohorts born between 1990 and 2000, we constructed a school-by-cohort-level panel data set of per-pupil spending, high school graduation rates, and student achievement in high school in math and reading, for all public schools in California. Through an analysis of the first four years of LCFF (2013 through 2016-17), we show LCFF significantly increased per-pupil spending and the state now has among the most progressive funding formulas in the country. This study is among the first to provide evidence of LCFF's impacts on student outcomes. Our research also contributes to the school finance reform literature by providing the first estimates of how simultaneous changes in the levels and restricted nature of state funding impact academic achievement, the composition of district spending, and markers of teacher quality.

The context of our study enables us to provide fresh evidence of the impacts of a school finance reform explicitly targeted toward disadvantaged students as opposed to district property wealth, and to explore how greater financial flexibility impacts district spending and student achievement. Using school and district fixed effects as well as dynamic changes in the decade leading up to the policy, we leverage the heterogeneous, abrupt changes in funding induced by LCFF in a two-stage least squares event-study framework. We present event-study figures that show no evidence of pre-existing time trends in student outcomes (conditional on controls), which supports the validity of the research design.

Our empirical framework hypothesizes that the effects of LCFF on student outcomes are a function of 1) the number of school-age years of "exposure" to the policy (which takes into account the staggered rollout of LCFF wherein it did not first became near-fully funded until the 2015-16 school year); and 2) the district-specific "dosage" students are exposed to (when fully funded), which is captured primarily by the LCFF-induced increase in district per-pupil revenues (based on the funding formula parameters) and, secondarily, by the corresponding reduction in the proportion of funding subject to restrictions on how revenues can be spent. If California's new school finance policy has causal beneficial impacts on student outcomes, we expect to find a dose-response relationship with outcomes improving more for students who experienced greater school-age years of "exposure" and larger spending increases ("dosage"), respectively. Our research design employs an instrumental variables approach, using the LCFF funding formula as instruments, to isolate the effects of increases in district per-pupil spending on student outcomes. The empirical strategy compares changes in average student outcomes across cohorts from the same school before and after LCFF-induced changes in district per-pupil revenue (over and beyond statewide, cohort-specific time trends). We simultaneously account for potential impacts of releasing funding from restrictions on how it is spent, using a district's pre-LCFF reliance on restricted funding as an instrument for the proportion of district revenue that is subject to restrictions (interacted with post-LCFF years). In this way, we are able to jointly test the impact of increases in per-pupil spending and impact of greater district discretion in how it is spent, independently of one another, in the same model.

To preview the results, we find that LCFF-induced increases in school spending led to significant increases in high school graduation rates and academic achievement, particularly among poor and minority students. A $1,000 increase in district per-pupil spending experienced in grades 10-12 leads to a 5.9 percentage-point increase in high school graduation rates on average among all children, with similar effects by race and poverty. On average among poor children, a $1,000 increase in district per-pupil spending experienced in 8th through 11th grades leads to a 0.19 standard-deviation increase in math test scores, and a 0.08 standard-deviation increase in reading test scores in 11th grade. These improvements in high school academic achievement closely track the timing of LCFF implementation, school-age years of exposure and the amount of district-specific LCFF-induced spending increase (and are independent of the effects of changes in the proportion of funding that is subject to restrictions).

The remainder of the paper is organized as follows. Section II provides greater detail about the Local Control Funding Formula, followed by a section highlighting prior related studies. Sections IV and V describe the data and detail our identification strategy. Section VI presents the descriptive patterns and regression results. Section VII concludes with a summary discussion of the findings, policy implications, and directions for future research.

Section II: Local Control Funding Formula

Beginning in 2013, the Local Control Funding Formula (LCFF) reallocated significant state funding to disadvantaged districts while also releasing a great deal of that funding from restrictions on how it could be spent. The policy was California's attempt to replace a highly centralized and complex school finance system, laden with myriad categorical funding programs directed to specific purposes, with a system that is simple, transparent, and (proponents claim) more equitable (Wolf & Sands, 2016).

Several landmark events help explain the state's school finance landscape at the dawn of the LCFF era. California is notable for being the first state in which reform advocates, pressing for more equity in school finance, prevailed in a state supreme court and for subsequently enacting one of the most stringent cross-district equalization plans (Sonstelie, Brunner, & Ardon, 2000, pp. 33–65). This momentous state Supreme Court decision of 1971 was followed seven years later by a major tax revolt, with voters overwhelmingly approving severe limits on property tax increases (known as Proposition 13). Accelerating what was already a national trend (Corcoran & Evans, 2015), Proposition 13 dramatically increased the state's role in funding California's schools. A decade later, voters also approved a proposition requiring the state to spend a particular percentage of the state budget on K-12 schools. None of these events helped to shield school district revenues from the impacts of the Great Recession, during which district budgets dropped by 20 percent over two years, a fall from which they had not meaningfully recovered in 2012, just prior to LCFF.

Allocation in the pre-LCFF system was achieved by the state supplementing local property taxes in order to bring each district up to a "revenue limit," a mostly uniform per pupil funding allotment that depended on state economic conditions, with some differentiation allowed for certain purposes but little explicit weighting for student demographic characteristics. For districts whose property tax wealth was insufficient to meet the revenue limit, the state complemented local property taxes until the limit was reached, so that funding was equalized across such districts despite changes to local tax revenues.

Concurrently, concern that the state's stringent finance equalization policy would harm districts facing higher operation costs led to the expansion of "categorical" aid programs that directed resources to particular expenditure categories such as transportation and special education (Weston, 2011, pp. 7–14). From the early 1980s to 2008, the state created 90 such categories through which it allocated roughly a quarter of its school revenues. Timar (1994) attributes the steady growth in these restricted categories to political patronage rather than district need, while others have criticized restricted revenue as orthogonal to student achievement (Kirst, Goertz, & Odden, 2007) and potentially stifling to innovation (Grubb, 2009). A 2009 policy suspended the lion's share of these restrictions in lieu of increased revenue, but many remained through 2012. These categorical programs did not count toward districts' revenue limits.

California's major shift in school finance reform, which first took effect in the 2013-2014 school year, replaced revenue limits with LCFF base funding differentiated by grade span, and it requires the student-to-teacher ratio in the early elementary grades (K-3) to not exceed 24 to 1 once LCFF is fully implemented. The three core components of the LCFF are (1) base grant, (2) supplemental grant, and (3) concentration grant. There is a guaranteed minimum equal to the amount received in 2012-13, adjusted for changes in average daily attendance (ADA) and local revenue. Roughly 100 "Basic Aid" districts have local revenue per pupil in excess of LCFF targets and receive nothing. For a few districts, there is also "economic recovery target" funding to restore pre-recession funding levels. Roughly 10 percent of state funding is outside the LCFF in the form of special education, Home-to-School Transportation and Targeted Instructional Improvement block grants, and school lunches.

The LCFF Base Grants establish a uniform grant that is based on average daily attendance (ADA) and varies by grade level. These grade-specific grants are adjusted for meeting K-3 class-size requirements (10.4%) and to support 9-12 college/career standards (2.6%). These 2015-16 base grants per ADA (including adjustments) were as follows:

Grades K-3	Grades 4-6	Grades 7-8	Grades 9-12
$7,820	$7,189	$7,403	$8,801

Source: http://www.cde.ca.gov/fg/aa/pa/pa1516rates.asp

Under LCFF, in addition to the uniform per-pupil base grant that depends only on grade-level enrollment proportions (K-3, 4-6, 7-8, 9-12), school districts receive a per-pupil supplemental grant that is a weighted function of student demographics in the district, and a concentration grant for districts with a high proportion of disadvantaged students. The official formula for district *d* is given in equation 1 below:

$$F_d = 0.2 \times G_d \times H_d + 0.5 \times G_d \times max[H_d - 0.55, 0] \qquad (1)$$

where G_d is the per-pupil base grant given by the state and H_d is the unduplicated proportion of disadvantaged students: those eligible for free or reduced-price lunch, with limited English proficiency, in foster care, or homeless.

The state's allocation of Supplemental and Concentration Grants is the focal point of our use of the funding formula to isolate exogenous changes in district-level revenue caused by the state policy changes. As noted, LCFF defines high-need ("unduplicated") pupils as free-or reduced-price lunch eligible, English learners, and foster youth. The Supplemental Grant is 20% of the base grant X the high-need share of enrollment. The Concentration Grant is 50% of the base grant X the high-need share of enrollment above 55%. Concentration grants begin when a district has 55% or more high-need students.

This creates a nonlinear formula with a kink at 55% disadvantaged, which we will exploit as an alternative identification strategy (2SLS-IV regression kink design) to tease out the effects of LCFF-induced increases in per-pupil spending among students in high-poverty schools (as discussed in detail in Section VI). Figure 1 shows the impact of student disadvantage on total

LCFF per-pupil funding (left-hand panel) and on the supplemental and concentration grants per pupil (right-hand panel). For the latter, the slope is $1600.6 when the proportion of disadvantaged students in the district is between zero and fifty-five percent, then jumps to $5323.5 when that proportion climbs above 55%. This should be considered the intended increase, as the state initially lacked the resources to fully fund districts at their target level. The discrepancy between target and realized funding dropped precipitously over the first three years of the policy, by which time most of the target funding had been secured (Figure 2a). Figure 3 (top left-hand panel) contains the distribution of disadvantage across districts in the 2012-2013 school year, wherein 62% of the students in the median district were classified as disadvantage (the corresponding student-enrollment weighted distribution is shown in the bottom left-hand panel). The distribution of LCFF funding four years later, when the policy was nearly fully funded, is shown in Figure 3 (top right-hand panel shows distribution across districts, and bottom right-hand panel shows corresponding student enrollment-weighted distribution). The median district received $9,192 per pupil in unrestricted LCFF revenue from the state, representing roughly 75% of total funding (Figure 2b).

State regulations require concentration grants be used to "increase or improve" services for high-need pupils "as compared to services provided to all pupils". Districts with 55% or more high-need students may spend these resources districtwide. If a school serves 40% or more high-need students, resources can be expended school wide. However, the district's Local Control Accountability Plan (LCAP) must identify these services and how they are principally directed to high-need students.

The new, dramatically overhauled system of school finance mandates that each district devise a Local Control Accountability Plan (LCAP), which is akin to the recipe and ingredients they will use to prepare a nutritious, equitable learning meal for every student beginning in preschool through 12th grade (high school graduation). But, there are minimal reporting requirements in Local Control Accountability Plans (LCAP). Some have expressed concerns that LCFF's granting greater autonomy over what services and programs the new funding supports will result in a set of unintended consequences. This alternative view posits that, without sufficient accountability, the "no-strings-attached" provision will result in money not reaching the students in greatest need; and, according to this view, the new funding will be allocated inequitably toward more affluent students and schools within districts. This is a long-standing debate between the advantages and disadvantages of a fiscally-centralized funding system that accounts for heterogeneous local schools and student needs. This study informs that debate.

It has been argued that a "one-size-fits-all" approach to school funding constrains local innovation and hampers the efficient use of resources to maximize student performance. K12 school leaders have long advocated for fiscal sovereignty, rather than categorical restrictions, that allows the tailored use of resources that best meets local needs and improves student outcomes. High-needs students are the fastest growing group of children in California and across the country: more than 60% of the state's public school students are low-income; more than one-quarter are English learners; concentrated poverty and segregation are widespread; and there is a large achievement gap by race and class.

The architects of LCFF reasoned that the answer to this rapid growth in pupil needs requires the interdependence of having more money *and* having greater autonomy over how funding is distributed to meet those needs. To address the deleterious effects of concentrated poverty, the funding formula allocates concentration grants to school districts with more than 55 percent of high-needs students (as defined by low-income, foster-youth, or English learners). The LCFF, which went into effect in the 2013-14 school year with a multi-year phase-in period, replaces the complex web of regulations and rules with a more transparent and progressive school funding system.

The second major component of the LCFF policy is the removal of restrictions from nearly all sources of state funding. Unlike the state's pre-LCFF basic funding allotment, an increasing list of categorical aid programs was not equalized across districts. Just prior to a 2009 reform, the state distributed over 20% of district revenues through approximately 60 categories. Many of these supported highly specific, sometimes voluntary school programs, such as counseling for grades 7 through 12, class size reduction for grade 9, incentives for physical education teachers, oral health assessments, and school library improvements (Weston, 2011). In response to severe financial strain due to the recession, the state enacted "Categorical Flexibility", a policy that suspended a great deal of restrictions starting in the 2009-2010 school year. Between that year and the year just prior to LCFF (2012-2013), approximately 12% of state funding was subject to categorical restrictions. That figure was cut to 9% by the third year of LCFF (2015-2016), as can be seen in Figure 4. Though LCFF nominally removed a large number of categorical programs, in reality the bulk of these programs had been suspended in 2009 and were no longer relevant. Consequently, LCFF's impact on restricted funding was far more modest than the prior policy.

These general trends toward fewer restrictions and increased funding had different implications for districts across and within levels of student disadvantage. For example, Tamalpais Union High serves an extraordinarily affluent suburban area north of San Francisco. Because of its comparatively small population of disadvantaged students and low reliance on restricted revenue sources, Tamalpais Union High witnessed few changes in the levels of or restrictions on its funding over the LCFF period. Conversely, Compton Unified, which serves a lower-income city south of Los Angeles, witnessed large increases in state funding as well as substantial reductions in restrictions on how the funding could be spent. Other districts, such as Fremont Unified in the San Francisco Bay Area and Kerman Unified in the Central Valley, saw one aspect of its funding change markedly but not the other. As Figure 5 illustrates, these four districts are not extreme outliers and their experience over the LCFF period exemplifies the variation in changes brought about by the policy's two main components.

California's new accountability context is also an important, potentially relevant element of the LCFF policy. The Local Control and Accountability Plan (LCAP) replaces the state's centralized accountability system with one that relies on individual district accountability plans, written to address specific goals in promoting student achievement.

The way a school reform rolls out is an important facet of the policy design. Traditionally, reforms roll out incrementally over time, which allows for the manifold adjustments (from personnel to curriculum) to be made at the local and district levels. However, while more immediate dispersal of funding is attractive, it often precludes a district's ability to enact bold, transformative curricular reform that can span a decade amid the constant uncertainty of available funding from year-to-year. This circumstance is typical for many districts, but particularly common for urban and low-income districts. Such fiscal uncertainty in a district is similar to the instability families that live paycheck-to-paycheck experience, which leads to suboptimal investments, rather than sustained, high-quality investments that lead to continual improvement. LCFF aims to change all of this with a $18 billion commitment in increased state support over 8 years.

Section III: School Finance Reforms

Despite the seemingly simple proposition that increasing funding to school districts will enhance the educational achievement of students, a long history of education finance scholarship suggests otherwise. Starting with the foundational Coleman Report (Coleman et al., 1966), observational work has routinely failed to find a meaningful correlation between school expenditures and student achievement. This massive study of school resources and student performance, undertaken in response to the Civil Rights Act of 1964 to assess racial disparities in schools, surveyed over 639,000 students, teachers, and principals in a representative sample of schools in the United States in 1965. While the report found substantial achievement disparities across racial groups and within racial groups across schools, very little of the variance in these measures of achievement could be accounted for by school resources. These early cross-sectional results have been reflected in aggregate time series comparisons, where National Assessment of Educational Progress (NAEP) scores only slightly increased since the 1970s despite substantial concurrent increases in school resources (Hanushek, 2003). Debates over this basic finding, that student achievement varies considerably across schools and teachers but not because of identifiable resources, have been a mainstay of education policy research for the past 50 years (Burtless, 1996; Goldhaber, 2015; Hanushek, 2011; Hanushek, Rivkin, & Taylor, 1996). A common criticism of this literature is that observational studies on the link between school resources and student achievement lack causal warrant due to the endogenous selection of students into schools and the potentially compensatory nature of school finance; compelling evidence on the impacts of school resources should come from field or natural experiments (Murnane & Willett, 2011, pp. 5–7). This challenge of identification has motivated a focus on the impacts of school finance reforms, where sharp changes in funding have been imposed on districts in a more plausibly arbitrary manner (Jackson, Johnson, & Persico, 2015).

Proponents of state-level school finance reforms seek to redress the disadvantage in school resources many children face due to the historic reliance of school districts on local revenues (Howell & Miller, 1997, p. 42; Hoxby, 1996, p. 69). Because of vast differences in local jurisdictions' wealth and preferences for education spending, children face a substantial amount of inequality in school resources across states and districts. For example, the ratio of the 95th percentile of district-level per-pupil spending to the 5th percentile was 2.73 in the early 1970s, the dawn of the first major school finance reform era. This relationship fell for decades, hitting a nadir of 1.98 in 2000, then climbed back to 2.55 by 2011 (Corcoran & Evans, 2015, p. 358). Though residential segregation across racial and socioeconomic lines has increased since the early 1970s (Clotfelter, 2004, Chapter 3), between-district inequality is largely a function of household incomes and property wealth, so that the largest inequalities across student demographic groups are based on district-level household income averages rather than individual student poverty status or ethnicity. In the early 1970s, expenditures per pupil were 1.4 times higher for pupils in wealthy districts than those in poorer districts, but only 1.08 times higher for non-poor vs. poor students and only 1.02 for whites vs. non-whites (Corcoran, Evans, Godwin, Murray, & Schwab, 2004, p. 440). Successive waves of school finance reforms attenuated these figures but did not change their ordinal relationship- average wealth in the district is still a better predictor of between-district disparities than individual student demographics.

Court-mandated and legislated school finance reforms have sought to either equalize or ensure an adequate level of school resources (Koski & Hahnel, 2015). The "equity era" began with the California Supreme Court's 1971 decision in *Serrano v. Priest* that the current system of local school finance ran afoul of constitutional guarantees of equal protection, a judicial victory for what reform advocates labeled "Proposition One": the quality of a child's schooling should not be a function of wealth within a state (Springer, Houck, & Guthrie, 2015, p. 10). Despite a subsequent setback in the United States Supreme Court in 1973, the ensuing equity era witnessed successful challenges to unequal district funding formulas in 10 states between 1971 and 1988 (Jackson et al., 2016). Recognizing that equality could be achieved through a mere reduction in expenditures at the top of the distribution, thus undermining their underlying goal of educational enhancement, reform advocates began litigating on the basis of educational adequacy (Clune, 1994). A major shift in strategy, this "adequacy era" was heralded by a 1989 Kentucky Supreme Court decision that the state's constitution guaranteed an adequate level of educational resources rather than merely equal resources across students. Comparable court rulings proliferated in the following decades in conjunction with similarly themed legislative decisions, with 27 states witnessing at least one school finance reform event through 2013 (Lafortune et al., 2015, pp. 66–67).

A growing body of literature has tried to assess the impact of school finance reforms on the levels and distributions of school finance and student achievement. In general, the causal identification in these studies has either leveraged reform-induced variation in funding within individual states or assumed that the onset of reforms are conditionally exogenous events in cross-state analyses, with non-reform states creating the counterfactual trends in student achievement, educational attainment, and labor market participation. The former group includes analyses of school finance reforms from the 1990s in Kentucky, Maryland, Massachusetts, Michigan, and Vermont. Guryan (2001) uses nonlinearities in a district funding formula brought about through the Massachusetts Education Reform Act of 1993 to identify the impact of state aid on district revenues and student achievement. Conditioning on a smooth function of district property wealth, Guryan uses sharp discontinuities in the state aid formula as exogenous instruments for district revenues. Within three years of reform, districts just below the state aid cutoff thresholds had spent roughly 65% of their new revenues, with concurrent increases in their 4th grade students' performance on standardized tests of math, science, and social studies. The new revenue does not appear to have affected performance on 4th grade literacy tests or any 8th grade tests in any of the four subjects. Similary, Papke (2005, 2008) uses discontinuities in Michigan's funding formula brought about by the state's 1994 reform (Proposal A) to instrument for school expenditures. Reform-induced increases in expenditures led to meaningful increases in the percentage of 4th grade students who successfully passed the state's standardized tests of numeracy, which a subsequent study reveals were concentrated in initially low-spending districts (Roy, 2011). This effect on pass rates for numeracy tests in initially low-spending districts is supported by Sherlock's (2011) analysis of Vermont's Equal Education Opportunity Act of 1997, yet that state's reform did not affect pass rates for literacy or writing. In the wake of Kentucky's 1990 reform, African-American students performed better on both the literacy and numeracy portions of the ACT, though the effect on all students is not distinguishable from zero. African-American performance on the 8th grade NAEP test appears to increase substantially (.12 standard deviations), yet the study lacks the power to distinguish this effect from zero (Clark, 2003). Moreover, reform-induced increases in expenditures in Maryland did little to increase student attainment, despite substantially reducing district spending inequality (Chung, 2015).

While these mixed results from individual state school finance reforms point to the heterogeneity in school finance systems, a concurrent literature has found mostly impressive impacts from school finance reforms generally. In this group of studies, the causal identification comes from variation in the presence of successful school finance reforms across time and states, with controls for endogenous state characteristics that are either fixed or vary in the pre-school finance reform periods. Murray, Evans, and Schwab (1998) analyze the impact of court-mandated school finance reforms in 16 states from 1971 to 1992, finding that reforms induced an 8 to 11 percent increase in spending in the bottom half of the distribution, leading to a 19 to 34 percent reduction in inequality across districts within states. Card and Payne (2002) find that successful court challenges led to a narrowing of district inequality within states, that the share of this state revenue translated into expenditures is between 30% and 65%, and that the reforms reduced the SAT test score gap across students divided by parental education, and possibly raised the SAT participation rate in the lower education group. More recently, Candelaria and Shores (2015) find that even seven years after court-ordered reforms, per-pupil revenues and graduation rates were higher for high poverty students. Expanding the treatment to both court-ordered and legislative reforms, LaFortune, Rothstein, and Schanzenbach (2015) find a gradual reduction of the income-dependence of state-level National Assessment of Educational Progress scores. While the immediate impact of reforms is insignificant, the ten-year impact of school reforms is a tenth of a standard deviation closure of the test score gap across district income levels. The immediate impact of school finance reforms on district financial equity persists over this period as well and is not attenuated by recapture of revenues by local taxpayers. Looking at longer-run outcomes, Jackson, Johnson, and Persico (2016) and Johnson and Jackson (2017) find that court-ordered school finance reforms increase educational attainment and wages, and lead to significant reductions in both the annual incidence of poverty and incarceration in adulthood, particularly for children from low-income families.

While the Jackson, Johnson, Persico (2016) study provides important evidence of the long-run beneficial impacts of earlier-era court-ordered school finance reforms, the recent LaFortune, Rothstein, Schazenbach study focuses on more recent school finance reforms during the adequacy era and documents significant impacts on test scores. If more recent school finance reforms have different effects on student learning, it is valuable to learn about the effects of the LCFF, even given the compelling results in Jackson, Johnson and Persico. As a whole, the school-finance-reform literature suggests that, *on average*, redistributive school finance reforms result in increased resources to less wealthy districts and enhanced achievement for students residing in those districts in both their academic careers and subsequent labor market outcomes.

A subsequent analysis of Michigan's reform illustrates factors that might mediate the impact of school finance reforms. Using a modification of the strategy in Papke (2008), Hyman finds that district revenues increased by only 58 cents of each dollar of increased state aid, pupil-administrator ratios decreased while pupil-teacher ratios did not, and the extra revenue was targeted to comparatively affluent schools. Though the reform ultimately led to increases in post-secondary attendance and degree attainment, these effects were also larger for comparatively affluent students. The relatively weak fly-paper effect, potentially inefficient use of new revenue, and within-district allocation favoring affluent schools present policy challenges but also frustrate the ability to draw strong inferences from school finance reforms regarding the causal link between school spending and student achievement.

The inefficient use of new funding is a particular salient issue. Skeptics claim that school administrators are not sufficiently incentivized to spend money in an efficient manner (Hanushek & Raymond, 2001, p. 381), which suggests that funding restrictions might increase student achievement if the public can identify and enforce more productive uses of resources. Many scholars and policy analysts that have supported increased school spending have noted that student achievement might be enhanced by systems that help ensure spending is allocated toward the most productive uses. However, in the case of California, increased restrictions on state funding have not been particularly effective. In the aftermath of severe finance reform in the 1970s, the state began making categorical funding available for specific purposes to acknowledge that schools face different costs to produce the same good (Weston, 2011). Over time more funding was directed to categorical programs, even as the state general revenues declined in periods of recession (Sonstelie et al., 2000, pp. 59–62). The growth in these categorical funds has been attributed to political patronage (Sonstelie et al., 2000, pp. 63–64) and the desire to circumvent collective bargaining agreements (Kirst et al., 2007, pp. 7–8), rather than student need (Timar, 1994). There is a relative dearth of studies on the causal impact of restricted funding on student achievement. However, two recent, well-identified studies of restricted spending increases are instructive. Cellini, Ferreira, and Rothstein (2010) assess the impacts of increased capital outlays by exploiting the sharp discontinuity in available capital improvement bonds due to local election results. The impacts of capital outlays on third grade standardized test scores are small and statistically indistinguishable from zero for most years in the 15-year period following a successful bond measure.[2] That test scores do not respond to capital improvement may not come as a great surprise, yet evidence from the Netherlands suggests that increased funding restricted to personnel and technology fares even worse. Leuven, Lindahl, Oosterbeek, and Webbink (2007) analyze the impact of two Dutch subsidies, one for personnel and one for computers, by exploiting the fact that only schools with over 70% disadvantaged students qualified for the increased funding. The results are dispiritingly negative across multiple specifications and achievement tests for both subsidies.

Three details of California's LCFF, together with the state's policy context, help provide an informative test of the impact of a school finance reform on district expenditures and student achievement. First, the state's school finance and property tax systems place severe constraints on the ability of local tax payers to influence their districts' revenue (Timar, 2006). Second, rather than re-allocating revenues across district property wealth, LCFF distributes funds on the basis of student disadvantage. Hoxby (2001) cautions that state responses to school finance reforms vary widely, with dramatically different consequences of state policy choices on education funding. For example, state aid formulas that distribute revenue to districts based on districts' local property tax wealth are endogenous to school finance and student achievement. These formulae can encourage an overall reduction in district revenues, potentially making the least-advantaged students worse off. Moreover, because district-level property wealth and student-level disadvantage are imperfectly correlated, a redistribution of revenue based on district wealth will imperfectly target disadvantage students. A particularly instructive example comes from the LaFortune, Rothstein, and Schanzenbach (2015) study, which reveals that the equalizing effect of school finance reforms on NAEP scores is not present across student-level racial or income gaps.

A third feature of LCFF affords an evaluation of the efficacy of state restrictions via categorical aid. California's recent reform gives districts a modest increase in discretion over expenditures by transferring revenue out of categorical aid programs and into the basic funding that districts receive from the state. This increase in fiscal freedom was felt heterogeneously across districts and thus allows for a joint test of the independent effects of increasing both per- pupil revenues and budgetary discretion. While the recent economics literature provides a fuller picture of the impacts of school finance generally, comparatively little is known about the impact of restrictions on how money can be spent.

Section IV: Empirical Strategy

The primary empirical challenge in estimating the effects of school spending on student outcomes is that spending and the quality of schools tend to be highly correlated with child family and neighborhood socioeconomic factors, due to the combination of parental choices

[2] The authors find that housing prices increase substantially and persistently after successful bond measures, suggesting that homebuyers value the new capital funds despite their weak relationship to student literacy and numeracy.

and residential location constraints (e.g., zoning policies and availability of affordable housing) that sort more advantaged children into better quality schools. Compensatory spending reforms may understate the effects of increased funding on student outcomes if the pre-existing student disadvantage that funding is targeted toward is not fully taken into account.[3]

Our research design employs an event study in combination with a simulated instrumental variables approach to circumvent this challenge using the LCFF funding formula and timing of implementation to isolate exogenous changes in district per-pupil revenue and promised availability of this funding from the state in future years as well. Our simulated instrumental variables (IV) approach for supplemental/concentration grants uses the following three funding formula parameters that determine funding: the baseline percentage of high-need students in the district *(H_d)*; the district's base grant (G_d); and the formula that allocates additional funding based on pupil needs in a given district. These three funding formula parameters are used to construct our instrument (***Z_d***):

$$Z_d = 0.2 \times G_d \times H_d^{2013} + 0.5 \times G_d \times max\left[H_d^{2013} - 0.55, 0\right]$$

Importantly, these reform-induced changes in district spending, which are credibly identified from the funding formula (and which serve as the instrumental variables), are unrelated to changes in child family and neighborhood characteristics conditional on the baseline level of disadvantage in each district. We refer to this reform-induced change in district per-pupil spending from the state as the "dosage" (in the parlance of the medical and treatment effects literature), which is district-specific. The "dosage" amount here refers to the LCFF fully funded amount. High-poverty districts are high-dosage and those with small proportions of disadvantaged students are low-dosage in accord with the funding formula.

We refer to "exposure" as the number of school-age years a child was exposed to the LCFF policy, which is birth cohort-specific, recognizing there is a phase-in period of implementation toward the formula being fully funded. For example, in models of high school graduation rates, cohorts born before 1996 are "unexposed" cohorts, as they had already reached age 18 prior to LCFF's enactment.

LCFF established a multiyear phase-in timeline to incrementally close the gap between actual funding and new target levels of funding. The research design explicitly accounts for this through the estimation of fully non-parametric event-study models that show the evolution of school inputs and student outcomes in both the years before and after the law's implementation separately for "high- and low-dosage" districts.

For this purpose, for cohorts born between 1990 and 2000, we constructed a school-by-cohort-level panel data set of school-age years of per-pupil revenue, high school graduation

---

[3] This point has been illustrated Johnson, R. C. & Jackson, C. K. (2017). "Reducing Inequality Through Dynamic Complementarity: Evidence from Head Start and Public School Spending". NBER working paper #23489 and Jackson, C. K., Johnson, R. C., & Persico, C. (2016). The Effects of School Spending on Educational and Economic Outcomes: Evidence from School Finance Reforms. *Quarterly Journal of Economics*, 131(1).

rates, and student achievement in high school in math and reading, for all public schools in California. These data are matched with LCFF school-reform variables. This paper focuses on high school graduation rates (the four-year cohort rate, which is consistently measured since 2009 for public schools in California), as well as high school achievement using 11th grade mathematics and reading standardized test scores (that are NAEP-norm adjusted as discussed in Section V).[4] Our analysis excludes charter schools.

Our inclusion of school fixed effects accounts for all time-invariant school-level factors, and the inclusion of birth-year fixed effects accounts for statewide trends in outcomes. Thus, factors such as persistent differences in teacher quality across schools, and statewide changes in economic conditions, are not a potential source of bias. The empirical strategy effectively compares changes in average student outcomes across cohorts from the same school before and after LCFF-induced changes in district per-pupil revenue (that exist over and beyond year/cohort-specific average changes over time).

If school spending has causal effects on student outcomes, we expect to find patterns of results that increase in both "dosage" (i.e., the amount of spending change) and the number of school-age years of exposure.[5] This dose-response relationship is indeed the pattern of results we find and document in this paper (Section VI). Importantly, we find no corresponding evidence of pre-existing time trends, which supports the validity of the research design to detect causal impacts. By 2016 (the most recent year for which data is presently available), the maximum number of school-age years of exposure is four, since the first year of enactment is during the 2013-14 school year. Target levels approached fully-funded status in the 2015-16 school year. So, for example, the high school graduating class of 2016 would have been potentially exposed to LCFF throughout their high school years; and similarly, student achievement in 11th grade during the 2016-17 school year corresponds with cohorts that had been potentially exposed to LCFF since the time they entered 8th grade (albeit at nearly fully-funded levels in only the last two of those years).

We identify the impact of the levels of and restrictions on state financing to school districts by leveraging the heterogeneous, conditionally exogenous changes in funding induced by the LCFF policy. The preferred estimation strategy, a two-stage least squares event study with school and district fixed-effects,[6] is robust to both fixed and dynamic endogenous selection of students into districts and districts into financing regimes. One of the key substantive innovations in this analysis is the inclusion of two exogenous treatment variables in

---

4 NAEP adjustments follow procedures outlined in Reardon, S.F., Kalogrides, D., & Ho, A. (2017). Linking U.S. School District Test Score Distributions to a Common Scale (CEPA Working Paper No.16-09). Retrieved from Stanford Center for Education Policy Analysis: http://cepa.stanford.edu/wp16-09.

5 A similar research design and empirical setup to identify the causal effects of K12 spending is used in Johnson, R. C. & Jackson, C. K. (2017). "Reducing Inequality Through Dynamic Complementarity: Evidence from Head Start and Public School Spending". NBER working paper #23489 and Jackson, C. K., Johnson, R. C., & Persico, C. (2016). The Effects of School Spending on Educational and Economic Outcomes: Evidence from School Finance Reforms. *Quarterly Journal of Economics*, 131(1), which examine earlier era court-ordered school finance reforms.

6 District fixed-effects are used where district finances are the outcome of interest; school fixed-effects are used where student achievement and high school graduation rates are the outcomes of interest.

the second stage equation: the predicted levels of per-pupil spending (as instrumented by the funding formula) and the predicted unrestricted proportion of that funding (as instrumented by the 2012 (pre-LCFF) proportion reliance on restricted funding). The ability to separate the effects of a per-pupil spending increase from the effects of a decrease in restrictions within the same model and policy environment is unique, as most prior studies have focused on either the impact of per-pupil spending increases (and equalizations) or the impact of increases in funding restricted to a particular purpose. This separation is achieved with the use of two separate sets of instruments: the formula weights (derived from a district's proportion of disadvantaged students) for per-pupil spending; and the proportion of each district's funding subject to restrictions in the year before the policy (2012) for the restricted proportion of funding. We however, show that, the main patterns of effects of per-pupil spending on student outcomes is similar with and without accounting for the proportion of revenue that is unrestricted.

As aforementioned, the key challenge of causal inference is to isolate the impact of the SFR policy changes as distinct from pre-existing trends and other coincident changes that may also affect graduation rates or any of the other outcomes of interest. Figure 7a provides visual evidence that the formula, the basis of state funding allocation, does not predict changes in funding levels in the four years leading up to the policy change. The linear fit is flat in the left panel, indicating that changes in funding levels from 2009 to 2012 are not predicted by the precise demographic weights used in the LCFF. The right panel is the same graph for the actual LCFF period, in which a distinct linear increase is visually detectable, which confirms that, at least in a simple multi-year change analysis, LCFF has increased district revenues via the formula. Figure 7b contains only the linear fits from each panel of Figure 7a together in the same graph for clarity of comparison. Table 1 contains the simple linear regressions that undergird the panels in Figures 7a and 7b. As can be seen, district formula does not predict linear changes in district finances in the years leading up to LCFF. The coefficient on formula should be interpreted as the predicted impact of moving from a formula of zero, corresponding to having no disadvantaged students, to a formula value of one, which would be more than double the actual highest formula possible: .425. Accordingly, the statistically insignificant coefficient in the first column should be read as a precise zero, which the observed R^2 value of .0004 reinforces. Conversely, the second column reveals a strong, statistically significant relationship between district formula and post-LCFF changes in funding, providing simple evidence of the conditional exogeneity of the formula.

Though the policy treatments contained in LCFF were outside of districts' control and appear at least upon graphical inspection to be exogenous, it might still be the case that graduation rates would have changed during the LCFF era due to continued economic growth of the state (or other temporally correlated events), irrespective of the changes in district finances. Periods of recession and economic growth can have different impacts on district revenues and graduation rates across the spectrum of district-level disadvantage. California's economy continued to expand in the LCFF era as the state recovered from the Great Recession. If graduation rates in less-advantaged (higher formula) districts are more responsive to the economy than are rates in advantaged (lower formula) districts, then a positive correlation between district revenues and graduation rates would be partially due to this structural economic relationship and not the new LCFF revenues. An analogous story could apply regarding categorical restrictions on district revenue.

This analysis addresses such structural economic relationships by controlling for the association between the policy treatments and graduation rates (and all other outcomes) that can be predicted by the decade of California's economic performance leading up to LCFF, 2004 to 2012. This period covers the pre-recession housing bubble, the housing crash and ensuing recession, and the recovery, providing sufficient variation in economic performance with which to predict district finances and graduation rates. A simple time-trend analysis of state funding reveals dramatic, non-linear changes over these years. Figure 2b shows a steady increase in funding during the housing bubble years (2004-2006), followed by a reduction in the recession years (2007-2009) that continues into the years prior to LCFF (2010-2012).

To model this role of business cycle fluctuations as it most closely relates to school finance, we use non-K-12 expenditures by the state of California, both overall and to all local sources. Statewide expenditures, rather than statewide revenues, are used because school districts' revenues are a function of what the state spends, which, because of smoothing over time, is not perfectly correlated with state GDP, tax receipts, or other revenue sources. We interact this state expenditure variable with a complete set of district fixed effects in order to obtain a district-specific relationship between statewide expenditures and district per-pupil revenue, and likewise for state local assistance provided, excluding education[7]; and include linear time trends and interact them with the funding formula. That is, for the pre-LCFF period (1995-2012), we regress district per-pupil revenue on the full set of interaction terms and district fixed effects, as in equation (2):

$$C_{db} = \alpha + \beta_d CAExp_b + \beta_d CALocalAssist_{db} + \theta timetrend + \gamma Formula_d * timetrend + \mu_d + \varepsilon_{db} \qquad (2)$$

where C_{db} is the district per-pupil revenue from the state for district *d* for birth cohort *b*; $CAExp_b$ is the total non-K-12 state expenditures per pupil for birth cohort *b*; $CALocalAssist_{db}$ is state local assistance provided (excluding education); $Formula_d$ is the LCFF funding formula parameter for district *d*; and μ_d is a vector of district fixed effects; ε_{db} is a stochastic error term for district *d* for birth cohort *b*. These models are run for the years 1995 through 2012, just prior to LCFF, and then used to predict the level of district per-pupil revenues from state sources for all years in the data, including the post-LCFF era (i.e., 2013 through 2016-17). We then take the predicted average of C_{db} during ages 15-17 to include in the regression models as controls.

Thus, in our models we account for these other potential district-level changes that are not driven by LCFF, with the inclusion as an additional control variable, the predicted district

[7] Total state expenditures, excluding public K-12 spending, covers categories such as health and human services, transportation, and the department of corrections. Total local assistance, excluding public K12 spending, covers categories such as medical assistance programs and social services. Both variables are adjusted to real 2015 dollars, and divided by the total state K-12 enrollment in each year.

per-pupil revenue from the state ($C_{db}^{age15-17}$), based on prior funding and state-wide California spending on non-K12 expenditures (based on pre-LCFF district-specific relationship between prior funding variables and district revenue from the state). This is an estimate of the counterfactual district revenue from the state if LCFF had not occurred. As shown in Figure 9, the prediction closely matches the actual average level of revenue in all years prior to LCFF; the significant departure of actual average revenue from its average prediction in the post-LCFF years (as expected) is plausibly attributable fully to the new LCFF formula. Including this in the primary regressions controls for dynamic, district-specific relationships between changes in economic conditions and district finances.[8]

Figure 10 shows the evolution of district per-pupil revenue from the state before and after LCFF for high-poverty (large spending increase) and low-poverty districts (small spending increase). In these figures, a "high-poverty district" receives $2,500 per-pupil revenue from the state when LCFF is fully funded, whereas a "low-poverty district" receives $500 per-pupil revenue (in accordance with the funding formula). This evidence that finds no pre-existing time trend also further supports the research design's ability to uncover causal effects.

The full first stage models are presented below in equations (3) and (4).

$$\widehat{ppe}_{db}^{15-17} = \pi_1\left(SFRExp_{db} \times \widehat{dose}_d\right) + \pi_2(SFRExp_{db} \times \widehat{unrs1}2_d) + \gamma_1 \cdot C_{db} + \theta_{d1} + \tau_{b,1} \tag{3}$$

$$\widehat{unrs}_{db}^{15-17} = \pi_3\left(SFRExp_{db} \times \widehat{unrs1}2_d\right) + \pi_4\left(SFRExp_{db} \times \widehat{dose}_d\right) + \gamma_2 \cdot C_{db} + \theta_{d2} + \tau_{b,2} \tag{4}$$

$\widehat{ppe}_{db}^{15-17}$ is average per-pupil revenue from state (in real 2015 dollars) during expected school-age years (ages 15 through 17) in an individual's childhood school district, $\widehat{unrs}_{db}^{15-17}$ is average proportion of revenue from state that is unrestricted during expected school-age years (ages 15 through 17) in an individual's childhood school district, $SFRExp_{db}$ is the number of school-age years that occurred after LCFF first implemented (0 = 17 years old, 4 = 13 years old, etc.), with each year entered as dummy indicator, $\widehat{dose}_d$ is the decile of the LCFF concentration/supplement grant*spline (based on funding formula), $\widehat{unrs1}2_d$ is the 2012 (pre-LCFF) proportion of revenue from state that was unrestricted, *s* indexes school, *d* indexes district , *b* indexes birth year, *g* indexes group (all kids; poor kids; or racial/ethnic group). Outside of the interactions with $SFRExp_{db}$, both $\widehat{dose}_d$ and $\widehat{unrs1}2_d$ are subsumed by the district or school fixed effects. Each first-stage regression provides information on how the policy levers actually altered district finances.

Overall there is a large first-stage effect of LCFF on district per-pupil spending (using only the funding formula parameter instruments), and there is a strong first-stage relationship between the 2012 (pre-LCFF) proportion of revenue that was unrestricted on subsequent changes in the proportion of revenue unrestricted in the post-LCFF period (independent of the

[8] The results are similar with and without this additional control.

funding formula). Table 2 contains the *F*-statistics from the first-stage regressions of district per-pupil spending and proportion unrestricted on the respective set of instruments, which both exceed 30. The table shows that the LCFF-related instrumental variables have a strong, statistically significant relationship to the endogenous financial variables, and have sufficient independent variation to identify their respective effects.[9] As expected, the 2012 (pre-LCFF) district proportion of revenue that was unrestricted is not predictive of per-pupil spending independent of the funding formula instruments.

The second stage is represented in equation (5):

$$Y_{gsdb} = \beta_1 \cdot \widehat{ppe}_{db}^{15-17} + \beta_2 \cdot \widehat{unrs}_{db}^{15-17} + \gamma \cdot C_{db} + \theta_{sd} + \tau_b + \varepsilon_{gsdb} \quad (5)$$

where Y_{gsdb} is the outcome of interest for group *g* in school *s* in district *d* for birth year *b*, θ_{sd} is a vector of school fixed effects and τ_b are birth year fixed effects. Because we have interest in estimating potential spending effects on average student achievement at the school-level, as well as impacts on achievement gaps, we estimate models for all children and separately for poor children ("poor" is defined in this paper as eligibility for free/reduced-price lunch), non-poor children, and by race/ethnicity. We also conduct a series of placebo (falsification) tests to ensure that the estimated effects are indeed due to the impacts of LCFF and not other coincident policy changes.

Average district per-pupil spending during ages 15-17 is inflation-adjusted using the CPI-U deflator (in real 2015 dollars) and then expressed in thousands[10], and the average proportion of district revenue that is unrestricted during ages 15-17 has been standardized, so that a one standard deviation increase is roughly 4 percentage points; in both cases this is done in order to facilitate interpretation of marginal effects and so the estimated effects are in the range we observe LCFF-induced variation in our key explanatory variables. The 2SLS-IV regressions are weighted by 2013 school enrollment. Standard errors are robust to heteroscedasticity and clustered at the district level.

Section V: Data

Our analysis relies on publicly available teacher-, school-, and district-level data from the California Education Department. Charter schools are excluded from the analysis, as are virtual and other non-traditional schools. Districts with insufficient years of data have also been removed. The final data set and analysis thus reflects traditional schools in elementary, high school, and unified school districts that have been in continuous operation in California from 1995 through 2017. Annual district financial records are available in aggregate from the

[9] We force the identifying variation in the proportion of revenue that is unrestricted to operate only through its prediction based on the 2012 pre-LCFF proportion unrestricted interacted with the post-LCFF years; and independent of and not through the funding formula parameters.

[10] For the analyses of 11th grade math and reading test scores, we examine the impacts of average per-pupil spending during ages 13-16 (i.e., 8th through 11th grades) and the corresponding impacts of the average proportion of funding that is unrestricted during ages 13-16 for cohort *b* in district *d*.

standardized account code structure (SACS) unaudited actual data files from 2003 forward, prior to which the files reflect a previous accounting structure that is similar with respect to coarse revenue and expenditure categories but not fine-grained expenditures.[11] This analysis primarily uses data going back to the 1995-1996 school year up to the most current year for which all necessary data is available (2016-2017), which include the first four post-LCFF school years: 2013-2014, 2014-2015, 2015-2016, 2016-2017. Each SACS file contains data on all general ledger financial records (both expenditures and revenues) for public school districts (Local Educational Agencies) in a given year. Each entry in the data is a particular financial record for a district aggregated for each relevant combination of "account" (revenue vs. expenditure), "fund" (general fund vs. a variety of special categories), "resource" (unrestricted vs. restriction categories), "goal" (Pre-K, K-12, Adult Education), "function" (Instruction, Special Education, etc.), and "object" (detailed source and purpose information). The previous accounting structure is less detailed in some of the finance categories, so analyses of certain expenditure categories can only go back as far as 2003.

This detailed financial data is transformed into real 2015 dollars per pupil using the consumer price index and enrollment data from the district. Figure 6a presents district expenditures per pupil for four categories over the period 2004 through 2016-17: teacher salaries, administrator salaries, buildings, and employee benefits (both health and retirement). The relative portions spent on these four categories do not change dramatically over this time period, with the exception of employee benefits. The LCFF era also witnessed a sharp rise in the amount of money districts spent on employee benefits (Figure 6a). Rather than reflecting more generous compensation packages, this increase was due to districts taking over a greater share of payments into the state teachers' retirement system. Over the same time period, the proportion of expenditures going toward instruction and teacher salaries have decreased, as can be seen in Figures 6a-6j.

The state of California's overall and local expenditures data, together comprising the underlying economic variables used in this analysis, come from the monthly statements of general fund cash receipts and disbursements made available through the State Controller's Office.[12] The June monthly statement in each fiscal year contains data for the preceding fiscal year. Expenditures by the state are broken down into two major categories: State Operations and Local Assistance. State operations cover categories such as health and human services, transportation, and the department of corrections. Local assistance covers categories such as public K-12, medical assistance programs, and social services. The two variables used in this analysis are the total state expenditures and the total local expenditures, each without the K-12 spending, adjusted to real 2015 dollars, and divided by the total state K-12 enrollment in each year. The school-level enrollment (average daily attendance) data comes from the financial records.

[11] Available here http://www.cde.ca.gov/ds/fd/fd/
[12] Available here http://www.sco.ca.gov/ard_state_cash.html

The main component of redistribution in the LCFF period is the proportion of (unduplicated) students who receive free- or reduced-price lunch, are of limited English proficiency, in foster care, or homeless. Unduplicated counts along those demographic lines are not available prior to 2012. Including both poverty and English language learner counts, which are available in each year, would severely overstate the proportion disadvantaged in many districts. Moreover, since 2013 there is an incentive for a district to endogenously classify its students as disadvantaged, such as through an increased effort to collect a student's socioeconomic status or retaining students in the limited English proficiency category. To circumvent these data limitations, the treatment is constructed from each district's proportion of unduplicated disadvantaged pupils in the first year of the policy, the 2013-2014 school year. This is used as the district's stable proportion of disadvantaged students across all years.[13]

The two endogenous regressors for which the policy changes serve as instruments, district per-pupil spending and the proportion of that funding not subject to restriction, are constructed from the SACS data. Total per-pupil spending is defined as the total expenditures divided by enrollment, and, likewise, total per-pupil revenue from the state is defined as the total revenue from all state sources (according to the "object" codes), divided by enrollment; both adjusted for inflation to represent 2015 dollars. The proportion not subject to restriction is defined by the "resource" codes and is simply the total per-pupil district revenue from all state sources under all unrestricted codes divided by the total district per-pupil revenue from the state in each district.

The data that include markers of teacher quality come from the California Department of Education as well. The state maintains an annual file of all teaching staff in each public school containing the staff members' education level, years of experience, and years working in the district, among other variables.[14] Each staff member is given a unique code that is not consistent across years, so that the staff records can only be merged with other records (schools, class assignment, etc.) within each year. We aggregate the staff records to create the following school by year variables: mean years of experience, mean years in the district, number of teachers in the school, and proportion of teachers with a master's degree or higher.

The high school graduation-rate analysis sample includes data on over 400,000 students per year in the in the 384 unified and high school districts with sufficient data across the years 2009 through 2016. We use the state's adjusted four-year cohort graduation rate, which has been available only since the 2009-2010 school year. While this figure more accurately measures high schools' performance, the lack of commensurate measures prior to 2009 means that our model is truncated for the graduation rate analysis. The veracity of certain schools' and districts' record keeping has recently been called into question (e.g., see OIG report), raising concerns that high-poverty schools are still not properly calculating the graduation rate. Though the problem was found in only a handful of schools in a single district and did not

[13] The formula can vary from zero to 0.425. Note that this is the formula weight, not the raw percent of disadvantaged students. A district with 50% disadvantaged students would receive 20% * 50% = 10% additional funding.
[14] Available here http://www.cde.ca.gov/ds/sd/df/filesstaffdemo.asp

provide direct evidence of inaccurate graduation rate data, we run our models with and without schools in the highest percentiles of poverty; the main pattern of results are unchanged.

The annual school files provide aggregate data on four-year cohort graduation rates – both the number in the cohort and the number of graduates from the cohort for each year. The district-level, four-year cohort graduation rate was created by dividing the aggregate number of graduates across all traditional schools in the district by the aggregate number of students in the cohort across all traditional schools. The yearly graduate rate figures are higher than the state totals in each year because charter and non-traditional schools, which typically have lower graduation rates, have been filtered out.

In the first year of the LCFF period, the state of California suspended its STAR testing program and began using the new "Smarter Balanced" tests in the following year (Cardine, 2013), complicating longitudinal analysis of student achievement over this time period. The new testing regime's computer-based administration and content focus are sufficiently different from the material and paper-and-pencil nature of STAR testing that the superintendent of public instruction cautioned against any comparison across the two tests after the first wave of results revealed significantly lower student performance on the new test (Noguchi, 2015). To overcome this challenge, we norm both the STAR and Smarter Balanced tests to the National Assessment of Educational Progress (NAEP), which, over this time period, has not changed and has been given to a representative sample of California students biennially. We follow the procedure in Reardon et al. (2017) but extended the norming to the school-subgroup level. Each school-subgroup score in each year thus reflects standardized performance on the NAEP scale. Because this scale does not change across the analysis time period, test scores in this normed metric can be compared both before and after the onset of LCFF. This norming enables comparable measurement over time to analyze student performance and comparisons of that performance before and after LCFF changes in spending. Changes in the testing procedures that could otherwise lead to biases are also accounted for through our inclusion of year fixed effects, which pick up average year-to-year trends in student performance that may be attributable to the changes in standardized test measurement.

Section VI: Results

We focus our discussion first on the results of the impacts of per-pupil spending on high school achievement. Table 3 and Figure 15 present the main results from the analysis of high school graduation rates. The first row of the table shows that a $1,000 increase in the average per-pupil spending experienced during ages 15-17 (i.e., 10^{th} through 12^{th} grades) increases the high school graduation rate for students overall by 5.89 percentage points, with comparable effect sizes for low-income (5.1 percentage-point increase) and Hispanic students (5.68

percentage-point increase). This effect is strongest for African-American students, at 7.71 percentage points.

The second row of Table 3 reveals that a one standard deviation increase in budgetary flexibility[15] experienced during ages 15-17 leads to a 1.41 percentage point gain in the high school graduation rate for students overall. Similar to the impact of expenditures, this effect is most pronounced among African-American students, for whom the graduation rate increase is 2.88 percentage points. The effects for all other groups are smaller and statistically insignificant.

Figure 16 and Table 4 present the results for 11th grade math and reading standardized test scores by child poverty status. We find that a $1,000 increase in the average per-pupil spending during ages 13-16 (i.e., 8th through 11th grades) leads to a 0.19 standard deviation increase in math and a 0.08 standard deviation increase in reading for poor children. The same increase leads to a 0.08 standard deviations in reading for non-poor children, for whom no impact on math achievement is detectable.

Table 5 presents similar results by ethnicity. Hispanics comprise 54% of California's public school children, and 24% of schoolchildren are non-Hispanic whites. We present results for Hispanics and non-Hispanic whites in Figure 17. Only 5.8% of California's public school children are black, so we are not able to break out the results on school-level test scores separately for black students due to missing reported information in public data when small numbers of blacks are in a school. We find, among Hispanic children, that a $1,000 increase in per-pupil spending during ages 13-16 leads to an increase of 0.19 standard deviations in math, and 0.11 standard deviations in reading. No statistically significant effects are detectable for white children.

2SLS-IV Regression Kink Design Estimates. We next explore an alternative complimentary research design that exploits the fact that the funding formula involved concentration grants for districts that have more than 55 percent of their enrollment comprised of disadvantaged students (limited English proficiency, foster child, free lunch). This funding rule creates a kink in the LCFF funding received as a function of the district proportion of disadvantaged students, and can be leveraged within a two-stage least squares regression kink design (2SLS-RKD-IV). We first present graphical depictions of the kink at 55 percent and its direct effects on per-pupil revenues from the state and per-pupil spending for large (vs small) SFR-induced spending increases for successive post-LCFF cohorts (Figure 18a). In contrast, and as a falsification check, we show that there is no positive kink relationship in per-pupil revenues (at 55%) for pre-LCFF cohorts—it is indeed flat and statistically insignificant (Figure 18b). The identification assumption of the research kink design is that, absent the additional LCFF revenue, there would be no associated kink in outcomes beyond a district's 55-percent threshold of disadvantage; and thus, any kink in outcomes beyond that point can be

---

15 A one standard deviation increase in the proportion of revenue that is unrestricted is roughly 4 percentage points.

interpreted appropriately as consistent with being attributable to the causal effects of per-pupil funding on student outcomes. We find this is indeed the case, as our graphical results show for post-LCFF cohorts that the kink and resultant improvements in both high school graduation rates and high school math achievement is more pronounced for cohorts that have been exposed to the increased resources for more of their school-age years and for whom the dosage was higher (i.e., as represented by the steeper upward-sloping kink beyond 55% shown in Figure 19a). As a placebo test, we show in contrast that no such positive kink relationship is found for pre-LCFF cohorts' high school graduation rates nor high school math achievement; in fact, outcomes are decreasing in district proportion of disadvantaged students through the 55-percent thresholds for unexposed LCFF cohorts, while for exposed cohorts the trajectory turns upward (Figures 19a-d).

Table 6 presents these 2SLS-IV-RKD estimates and the previous 2SLS-IV estimates side-by-side for comparison for high school graduation and high school math achievement. With regard to interpretation, it is important to note the local average treatment associated with the 2SLS-IV-RKD estimates are more in line with the average effects of spending increases among students in high poverty schools, while we compare them with the average effects of spending we find among poor children across all schools on average using the 2SLS-IV estimates. We find that the 2SLS-IV-RKD estimates are larger though, as expected, with significantly less precision; but we find significant effects for both high school graduation rates and 11th grade math test scores using the regression kink design (insignificant 2SLS-RKD estimated effects in the case of reading). For example, the 2SLS-IV-RKD results indicate that a $1,000 increase in per-pupil spending experienced throughout high school years leads to an 8.77 percentage-point increase in high school graduation rates (Figure 20).

Exploring Potential Mechanisms. Given these results from both the 2SLS-IV and 2SLS-RKD estimates, it is natural to ask how the schools and districts achieved such improvements; yet doing such an analysis requires successfully choosing the correct subset of expenditures from an immense data set. We focus here on teacher salaries and administrator salaries, employee benefits, buildings, instruction, special education, preschool spending per 4-year old, and teacher professional development. The "buildings" category includes construction of new buildings and improvements and repairs to existing structures. The "instruction" category includes expenditures on regular K-12 education, as opposed to special, bilingual, or adult education, alternative schools, and a host of non-regular educational goals.

Table 7 presents the impact of LCFF on school inputs and the composition of district spending. Column 1 reveals that the increase in revenues caused the average school-level student-to-teacher ratio to fall by 0.2368 overall, whereas the increase in budgetary flexibility leads to a slight increase of 0.0722. The increase in flexibility also leads to a 3.8 percent increase in the likelihood that a teacher has limited experience, as can be seen in column 3. In Table 7a, column 4 shows the impacts of revenues and flexibility on district per-pupil spending.

Row 1 shows that 83 cents of every dollar is passed through as expenditures.[16] This is a relatively strong flypaper effect given the range found in the school finance reform literature generally, and is expected as Proposition 13 allows very limited scope for increases in state funding to lead to local property tax savings. Greater budgetary freedom causes a slight drop in per-pupil annual expenditures, but this may arise from a shift in accounting system requirements (e.g., reporting between general fund vs deferred maintenance fund).[17] Row 2 column 5 shows that the average teacher salary increases by 2.7 percent for every 10 percent increase in per-pupil revenues.

Row 1, columns 6 through 13 contain the proportion of the increased revenue that is spent on various expenditure categories. We find 11 percent of the increase went toward teacher salaries, 24 percent went toward instructional expenditures (including teacher salaries), 3 percent went toward administrator salaries, 12 percent went toward employee benefits, 5 percent was spent on capital improvements, and 6 percent was spent on special education.

In our final set of analyses, we attempt to provide suggestive evidence of potential mechanisms. For the regression models that explore potential mechanisms, we

instrumented for "teacher salaries per pupil", "administrative salaries per pupil", "capital expenditures per pupil" and "employee benefits per pupil" in the same model (and controlling for instrumented proportion of district revenue from state that is unrestricted).[18] The results show that LCFF-induced increases in teacher salaries per pupil (which include both increases in the number of teachers hired and increases in teacher salary) are significantly related to student achievement--for children from low-income families and Hispanic students (Figures 21a-c). On the other hand, administrative salaries, capital expenditures, and employee benefits are not found to be significantly related to student achievement (Figures 21a-c). We acknowledge these exploratory patterns are far from definitive and are meant only to be suggestive. But they are supportive of the overall pattern of results; one interpretation may be that when increased resources make it to the classroom, they may more directly influence learning outcomes.

---

16 The estimated effect is larger over a two-year period as some district revenues in a given year are applied to a future school year's expenditures in an accounting sense that this district finance data may not fully capture due to the reporting requirements with the California Department of Education.

17 In an accounting sense, districts can shift funding from one fund to another and this could lead to what looks like crowd-out when in fact it is not. For example, districts could take some state aid which is deposited into the general fund and then transfer it to the deferred maintenance fund, which would appear like crowd-out (e.g., see https://www.cde.ca.gov/fg/ac/ac/sacsminutes050614a.asp).

18 For these analyses, we put the key explanatory variables in standard deviation units to facilitate a more straightforward comparison of effect sizes, and so the estimated effects are in the range we observe LCFF-induced variation in our key explanatory variables. A one-standard deviation increase in the proportion of district revenue from state that is unrestricted is roughly 0.04; a one-standard deviation increase in teacher salaries per pupil is roughly $500; a one-standard deviation increase in administrative salaries per pupil is roughly $100; a one-standard deviation increase in capital expenditures per pupil is also about $100; a one-standard deviation increase in employee benefits per pupil is roughly $500.

Section VII: Summary Discussion

Overall, LCFF achieved its immediate purpose of increasing funding to districts with disadvantaged students. Though the policy is nearly fully funded, after three years of increases, district revenues were substantially higher than they would have been in the absence of LCFF. This was mostly due to the mechanical increase in funding to disadvantaged districts, but also to the reasonably strong flypaper effect. The inability of property owners to respond by lowering their contribution to public schools is likely a key contextual factor accounting for this result, but it is possible that some crowd out of instructional expenditures will be seen in the future due to mounting pension debt obligations (Koedel and Gassman, 2018). The policy also achieved its second immediate goal of reducing restrictions on state funding. A vast majority of state revenue is no longer subject to restrictions, though roughly 9 percent of the median district's budget is still tied up in categorical revenue streams. Spending patterns were not altered dramatically by the policy, with notable increases in spending on employee benefits being the exception to that generalization.

Increases in per-pupil spending caused by LCFF led to significant increases in high school graduation rates and student achievement. We find the effects increase in both the amount of spending increases and the number of school-age years of exposure. We find no evidence of differential pre-reform trending. Furthermore, we find a similar pattern of results across all three empirical approaches ((1) event-study difference-in-difference; (2) 2SLS-IV; (3) 2SLS-RKD-IV models), wherein the improvements in high school academic achievement closely track the timing of LCFF implementation, school-age years of exposure and the amount of district-specific LCFF-induced spending increase.

In particular, the increases in per-pupil spending led to significant increases in high school graduation rates overall by nearly six percentage points (associated with a $1,000 spending increase throughout high school), while the increase in expenditure flexibility increased graduation rates by 1.4 a percentage points for each standard deviation increase in budgetary freedom. The effects were heterogeneous across student demographic groups, being strongest for African-American students, but positive and statistically significant for all student subgroups. The increases in per-pupil spending improved test scores as well, with the additional expenditures significantly boosting literacy and numeracy for Hispanic and poor children. This more targeted effect is somewhat expected, as the policy was meant to deliver greater resources for low-income students and students with limited English proficiency.

We find, for low-income students, that a $1,000 increase in district per-pupil spending during ages 13-16 led to a 0.19 standard deviation increase in 11th grade mathematics test scores. To put this magnitude in perspective, the 0.19 standard deviation increase in high school math achievement is equivalent to 37% of the average mathematics achievement gap between poor and non-poor students in 11th grade; is equivalent to 24% of the average mathematics black-white achievement gap in 11th grade; and is equivalent to 34% of the average mathematics Hispanic-white achievement gap in 11thgrade (based on data from all CA public schools, 2003-16). On average, students gain about 0.25 standard deviations each 10 months of high school (one year), so the 0.19 standard deviation increase in high school math achievement (resultant from a 1,000 increase in district per-pupil revenue during ages 13-16) is equivalent to approximately 7 months of learning (i.e., 0.19/.25).

We find, for low-income students, that a $1,000 increase in district revenue per-pupil during ages 13-16 led to a 0.08 standard deviation increase in 11th grade reading test scores. To put this magnitude in perspective, the 0.08 standard deviation increase in high school reading achievement is equivalent to 13% of the average reading achievement gap between poor and non-poor students in 11th grade; is equivalent to 10% of the average reading black-white achievement gap in 11th grade; and is equivalent to 12% of the average reading Hispanic-white achievement gap in 11th grade (based on data from all CA public schools, 2003-16). This 0.08 standard deviation increase in high school reading achievement (resultant from a 1,000 increase in district per-pupil revenue during ages 13-16) is equivalent to approximately 3 months of learning (i.e., 0.08/.25). These are meant as rough back-of-the-envelope calculations to facilitate putting the magnitudes in perspective. In sum, the evidence suggests that money targeted to students' needs can make a significant difference in student outcomes and can narrow achievement gaps.

The magnitudes of these effects are large and broadly similar to those found in recent studies that use quasi-experimental methods. Candelaria and Shores (2015) find that, seven years after a reform event, per-pupil revenues increase by an average of 11.9% and graduation rates increase by an average of 8.4 percentage points in the poorest quartile of districts. In California, $1,000 was 11.8% of average per pupil expenditures on the eve of LCFF in 2012.[19] With three successive years of exposure to such an increase in per-pupil spending, this 11.8% increase led to a 5.89 percentage point increase in the graduation rate, a smaller effect than is present in Candelaria and Shores. However, the event-study graph contained in Figure 13 shows that this effect is increasing with duration of exposure, suggesting that a seven-year effect may be substantially larger and is in line with the previous study.

As for test scores, an increase of $1,000 in per pupil expenditures over four years raises numeracy scores for poor children by 0.19 standard deviations (event study graph shown in Figure 14). This is the precise magnitude found in a national study by Lafortune, Rothstein, and Schanzenbach (2015, p. 6), wherein this effect took ten years to manifest rather than four. Several differences in the two studies may explain the accelerated appearance of the effect in California. First, our estimates come from NAEP-normed tests given in 11th grade as compared to 4th and 8th grade NAEP tests in the national study. It may be that schools serving high school students are better able to either capture new revenues or translate them into student achievement. Second, the nature of LCFF, with its explicit focus on disadvantaged students and localized accountability structure, may have encouraged more efficient uses of the new expenditures than did the "adequacy" reforms studied previously. Third, the effect in California may exhibit diminishing returns over time, so that the 10-year effect may be similar to the 4-

---

[19] The average expense per student (in terms of average daily attendance) was $8,448 in the 2012-2013 school year. For details, see https://www.cde.ca.gov/ds/fd/ec/currentexpense.asp

year effect found here. This would be the case if districts are able to reach their long-run level of efficiency within several years rather than a decade. In any event, both the test-score and graduation-rate effects are within plausible ranges that one would expect, given recent studies.

Our estimated significant effects of per-pupil spending are robust across student outcomes and identification strategies and robust to a variety of falsification checks. On the other hand, the estimated effects of reductions in the proportion of funding with categorical restrictions exhibit a far less consistent pattern of results across outcomes and subgroups. This may simply be an artifact that the reduction in the proportion of funding with restrictions was a much more modest change, but this requires further investigation and may require more years of data before definitive conclusions can be reached on the latter.

The context of California's legal and policy environment is important to consider when making sense of these results. It bears repeating that it is extraordinarily difficult for tax payers to capture the new state revenue with lower property tax rates. This condition may not hold in other states. Second, the LCFF era followed a period of deprivation for district resources. States in which schools are well-funded may see diminishing marginal returns to budget increases of LCFF's scale. Third, the changes in budgetary restrictions are of a limited scale. These results say little about the magnitude of impacts one should expect from much larger changes or changes at a different baseline level of restrictions.

Several limitations of this study also warrant further consideration. The first is the relative recency of the policy-- unintended negative consequences such as local recapture might take longer. Though tax payers may not be able to alter their local property tax rates, there are other means through which budget offsets may occur – such as reductions in parental financial support or a reduction in the willingness of voters to approve of parcel taxes. However, it is just as likely that improvement in student achievement will also grow as school and districts adjust to the new funding environment. There is some evidence that district administrators are hesitant to invest in more permanent inputs until they are assured that LCFF will not be repealed. Second, the variables used in this analysis are school- and district-level averages that do not reflect inequality or changes across student groups within schools and districts. The heterogeneity in graduation-rate impacts suggest that within-district resource allocation should be analyzed. Third, the district financial data may not capture the proper mechanisms that enhanced graduation rates, either because the "true" mechanism is not measured by the accounting code or our selective analysis has missed it. In future work, we will analyze the effects of LCFF on student achievement in earlier grades as additional years of data become available.[20]

The impacts of the new policy are still reverberating, and the verdict is still out; but, given the magnitude of redistribution in the LCFF, the policy provides a test of how state policy and school resources can shape student achievement and reduce inequality. Notwithstanding

---

[20] An additional important direction of future research includes the examination of LCFF effects on intra-district school resource allocation decisions and resultant effects on student achievement gaps. Tom Dee (Stanford University) is exploring aspects of this for one of the other GDTFII chapters.

those limitations, this study is among the first to document impacts of LCFF on student outcomes, and jointly test the impact of a simultaneous change in school district revenues, directed toward disadvantaged students, and budgetary restrictions on how such revenues can be spent. The findings suggest that both revenue and flexibility can be productive in enhancing the academic achievement and educational attainment of disadvantaged students. These findings are particularly noteworthy in light of the fact that LCFF is a recent reform and has been gradually rolled out to become fully funded and implemented in the past year. The country is watching as it is anticipated that, if successful, the new school finance measure may lead other states to adopt similar legislation. Time will tell—in the interim, this new research evidence suggests that money targeted to the needs of students, and allocated by local districts to meet those needs, can make a difference in student outcomes.

References

Burtless, G. (1996). Introduction and Summary. In G. Burtless (Ed.), *Does Money Matter? The Effect of School Resources on Student Achievement and Adult Success.* (p. 316). Washington, D.C.: Brookings Institution Press.

Candelaria, C. A., & Shores, K. A. (2015). *Court-Ordered Finance Reform on Spending and Graduation Rates*.

Card, D., & Payne, A. A. (2002). School Finance Reform, the Distribution of School Spending, and the Distribution of SAT Scores. *Journal of Public Economics*, *83*(1), 49–82. Retrieved from http://www.nber.org/papers/w6766

Cardine, S. (2013, November 6). State revises school tests. *Los Angeles Times*. Los Angeles. Retrieved from http://www.latimes.com/tsn-vsl-state-revises-school-tests-20131106-story.html

Cellini, S. R., Ferreira, F., & Rothstein, J. (2010). The Value of School Facility Investments: Evidence from a Dynamic Regression Discontinuity Design. *Quarterly Journal of Economics*, *125*(1), 215–261.

Chetty, R., Friedman, J. N., Hilger, N., Saez, E., Schanzenbach, D. W., & Yagan, D. (2011). How does your kindergarten classroom affect your earnings? Evidence from project star. *Quarterly Journal of Economics*, *126*(4), 1593–1660. https://doi.org/10.1093/qje/qjr041

Chetty, R., Friedman, J. N., & Rockoff, J. E. (2013). *Measuring the Impacts of Teachers II: Teacher Value-Added and Student Outcomes in Adulthood* (No. 19424). Retrieved from http://www.nber.org/papers/w19424

Chung, I. H. (2015). Education finance reform, education spending, and student performance: Evidence from Maryland's Bridge to Excellence in Public Schools Act. *Education and Urban Society*, *47*(4), 412–432. https://doi.org/10.1177/0013124513498413

Clark, M. A. (2003). *Education Reform, Redistribution, and Student Achievement: Evidence From the Kentucky Education Reform Act*. Princeton, NJ.

Clotfelter, C. T. (2004). *After Brown: The Rise and Retreat of School Desegregation*. Princeton, NJ: Princeton University Press.

Clotfelter, C. T., Ladd, H. F., & Vigdor, J. (2005). Who teaches whom? Race and the distribution of novice teachers. *Economics of Education Review*, *24*(4), 377–392. https://doi.org/10.1016/j.econedurev.2004.06.008

Clotfelter, C. T., Ladd, H. F., & Vigdor, J. L. (2010). Teacher Credentials and Student Achievement in High School: A Cross-Subject Analysis with Student Fixed Effects. *Journal of Human Resources*, *45*(3), 655–681.

Clune, W. H. (1994). The Shift from Equity to Adequacy in School Finance. *Education Policy*, *8*(4), 376–394. https://doi.org/0803973233

Coleman, J. S., Campbell, E. Q., Hobson, C. J., McPartland, J., Mood, A. M., Weinfeld, F. D., & York, R. L. (1966). *Equality of Educational Opportunity*. Washington, D.C. Retrieved from http://www.eric.ed.gov/PDFS/ED012275.pdf

Corcoran, S. P., & Evans, W. N. (2015). Equity, Adequacy, and the Evolving State Role in Education Finance. In H. F. Ladd & M. G. Goertz (Eds.), *Handbook of Research in Education Finance and Policy* (2nd ed., p. 676). New York: Routledge.

Corcoran, S. P., Evans, W. N., Godwin, J., Murray, S. E., & Schwab, R. M. (2004). The Changing

Distribution of Education Finance, 1972 to 1997. In K. M. Neckerman (Ed.), *Social Inequality* (p. 1017). New York: Russell Sage Foundation.
Goldhaber, D. (2015). Teachers Clearly Matter, but Finding Effective Teacher Policies Has Proven Challenging. In H. F. Ladd & M. E. Goertz (Eds.), *Handbook of Research in Education Finance and Policy* (2nd ed., p. 676). New York: Routledge.
Goldhaber, D., Lavery, L., & Theobald, R. (2015). Uneven Playing Field? Assessing the Teacher Quality Gap Between Advantaged and Disadvantaged Students. *Educational Researcher*, *44*(5), 293–307. https://doi.org/10.3102/0013189X15592622
Grubb, W. N. (2009). *The Money Myth: School Resources, Outcomes, and Equity*. New York: Russell Sage Foundation Publications.
Guryan, J. (2001). *Does Money Matter? Regression-Discontinuity Estimates from Education Finance Reform in Massachusetts* (NBER Working Paper No. 8269). Cambridge, MA. https://doi.org/10.1007/s13398-014-0173-7.2
Hanushek, E. A. (2003). The Failure of Input-based Schooling Policies*. *The Economic Journal*, *113*(485), F64–F98.
Hanushek, E. A. (2011). The economic value of higher teacher quality. *Economics of Education Review*, *30*(3), 466–479.
Hanushek, E. A., & Raymond, M. E. (2001). The Confusing World of Educational Accountability. *National Tax Journal*, *54*(2), 365–384.
Hanushek, E. A., Rivkin, S. G., & Taylor, L. L. (1996). Aggregation and the Estimated Effects of School Resources. *Review of Economics and Statistics*, *78*(4), 611–627.
Howell, P. L., & Miller, B. B. (1997). Sources of Funding for Schools. *Future of Children*, *7*(3), 39–50. https://doi.org/10.2307/1602444
Hoxby, C. M. (1996). Are Efficiency and Equity in School Finance Substitutes or Compliments? *Journal of Economic Perspectives*, *10*(4), 51–72.
Hoxby, C. M. (2001). All School Finance Equalizations are not Created Equal. *Quarterly Journal of Economics*, *116*(4), 1189–1231. https://doi.org/10.1162/003355301753265552
Hyman, J. (2013). *Does Money Matter in the Long Run? Effects of School Spending on Educational Attainment*.
Jackson, C. K., Johnson, R. C., & Persico, C. (2015). Money Does Matter After All. Retrieved November 5, 2016, from http://educationnext.org/money-matter/
Jackson, Kirabo, Rucker C. Johnson, Claudia Persico (2016). "The Effects of School Spending on Educational & Economic Outcomes: Evidence from School Finance Reforms". *The Quarterly Journal of Economics* 131(1): 157-218.
Johnson, Rucker C. and C. Kirabo Jackson (2017). "Reducing Inequality Through Dynamic Complementarity: Evidence from Head Start and Public School Spending". NBER working paper #23489.
Kirst, M. W., Goertz, M., & Odden, A. R. (2007). *The Evolution of California's State School Finance System and Implications from Other States* (Getting Down to Facts). Palo Alto, CA. Retrieved from https://cepa.stanford.edu/content/evolution-california's-state-school-finance-system-and-implications-other-states
Koedel, Cory and Gabriel E. Gassman (2018). "Pensions and California Public Schools" (Getting Down to Facts II report).
Koski, W. S., & Hahnel, J. (2015). The Past, Present, and Possible Future of Educational Finance

Reform Litigation. In H. F. Ladd & M. E. Goertz (Eds.), *Handbook of Research in Education Finance and Policy* (2nd ed., p. 676). New York: Routledge.
Lafortune, J., Rothstein, J., & Schanzenbach, D. W. (2015). *School Finance Reform and the Distribution of Student Achievement* (NBER Working Paper No. 22011). Cambridge, MA. https://doi.org/10.1017/CBO9781107415324.004
Lankford, H., Loeb, S., & Wyckoff, J. (2002). Teacher Sorting and the Plight of Urban Schools: A Descriptive Analysis. *Educational Evaluation and Policy Analysis*, *24*(1), 37–62. https://doi.org/10.3102/01623737024001037
Leuven, E., Lindahl, M., Oosterbeek, H., & Webbink, D. (2007). THE EFFECT OF EXTRA FUNDING FOR DISADVANTAGED PUPILS ON ACHIEVEMENT. *Review of Economics and Statistics*, *89*(4), 721–736.
Murnane, R. J., & Willett, J. B. (2011). *Methods Matter: Improving Causal Inference in Educational and Social Science Research*. New York: Oxford University Press.
Murray, B. S. E., Evans, W. N., & Schwab, R. M. (1998). Education-Finance Reform and the Distribution of Education Resources. *American Economic Review*, *88*(4), 789–812.
Noguchi, S. (2015, September 9). California’s school test scores reveal vast racial achievement gap. *The San Jose Mercury News*. San Jose. Retrieved from http://www.mercurynews.com/california/ci_28782503/califs-test-scores-reveal-yawning-achievement-gap
Papke, L. E. (2005). The effects of spending on test pass rates: Evidence from Michigan. *Journal of Public Economics*, *89*(5–6), 821–839. https://doi.org/10.1016/j.jpubeco.2004.05.008
Papke, L. E. (2008). The Effects of Changes in Michigan’s School Finance System. *Public Finance Review*, *36*(4), 456–474. https://doi.org/10.1177/1091142107306287
Reardon, S. F. (2011). The Widening Academic Achievement Gap Between the Rich and the Poor: New Evidence and Possible Explanations. In G. J. Duncan & R. J. Murnane (Eds.), *Whither Opportunity? Rising Inequality, Schools, and Children’s Life Chances* (pp. 91–116). New York: Russell Sage Foundation.
Roy, J. (2011). Impact of School Finance Reform on Resource Equalization and Academic Performance: Evidence from Michigan. *Education Finance and Policy*, *6*(2), 137–167. https://doi.org/10.2139/ssrn.630121
Sherlock, M. (2011). The Effects of Financial Resources on Test Pass Rates : Evidence from Vermont ’ s Equal Education Opportunity Act. *Public Fi*, *39*(3), 331–364. https://doi.org/10.1177/1091142110396500
Sonstelie, J., Brunner, E., & Ardon, K. (2000). *For Better or For Worse ? School Finance Reform in California*. San Francisco, CA.
Springer, M. G., Houck, E. A., & Guthrie, J. W. (2015). History and Scholarship Regarding U.S. Education Finance and Policy. In H. F. Ladd & M. E. Goertz (Eds.), *Handbook of Research in Education Finance and Policy* (2nd ed., p. 676). New York: Routledge.
Timar, T. B. (1994). Politics, Policy, and Categorical Aid: New Inequities in California School Finance. *Educational Evaluation and Policy Analysis*, *16*(2), 143–160. https://doi.org/10.3102/01623737016002143
Timar, T. B. (2006). *How California Funds K-12 Education*. Palo Alto, CA. Retrieved from http://irepp.stanford.edu/documents/GDF/STUDIES/02-Timar/2-Timar(3-07).pdf
Weston, M. (2011). *California’s New School Funding Flexibility*. San Francisco, CA.

Wolf, R., & Sands, J. (2016). A Preliminary Analysis of California's New Local Control Funding Formula. *Education Policy Analysis Archives*, *24*(34), 1–37.

Figures and Tables

Figure 1.

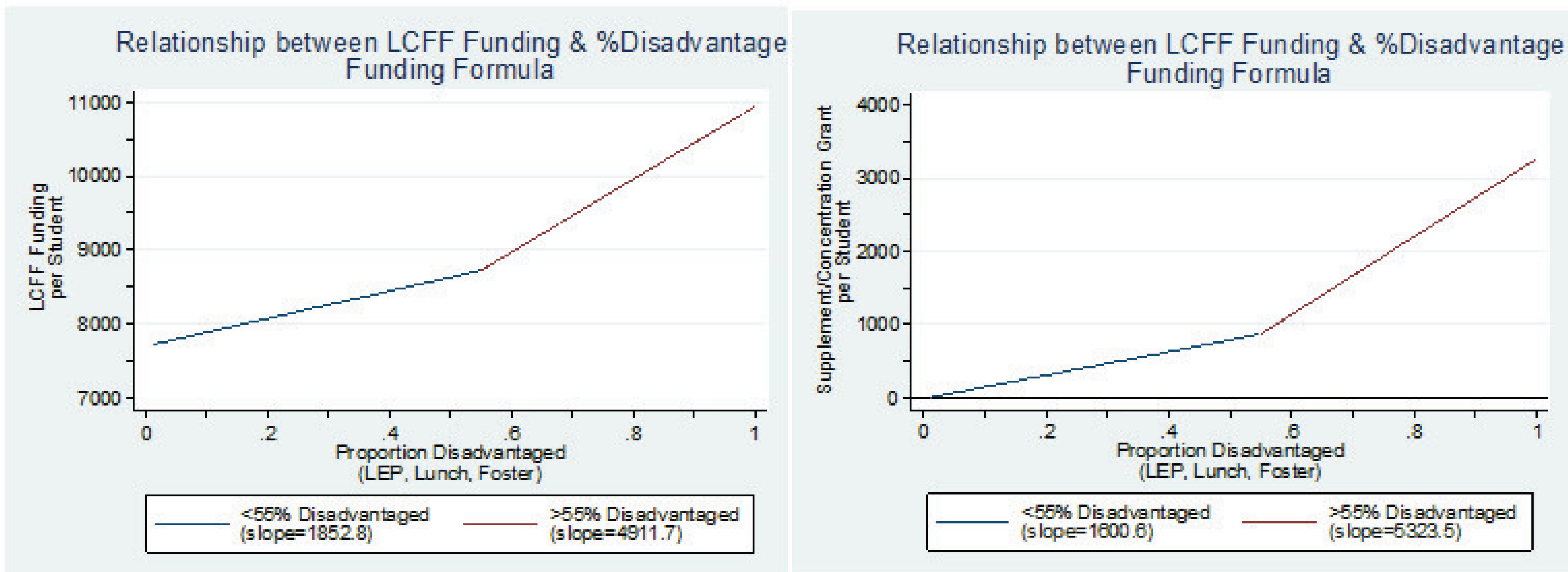


Figure 2a.

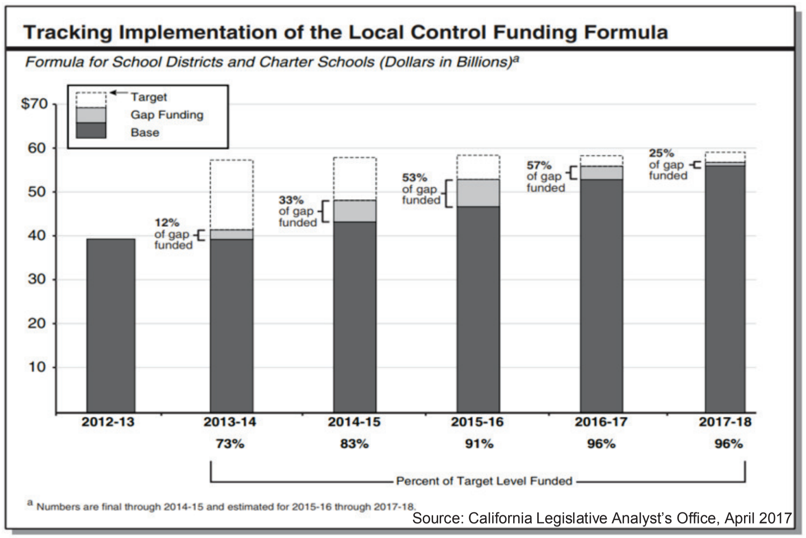


Figure 2b.

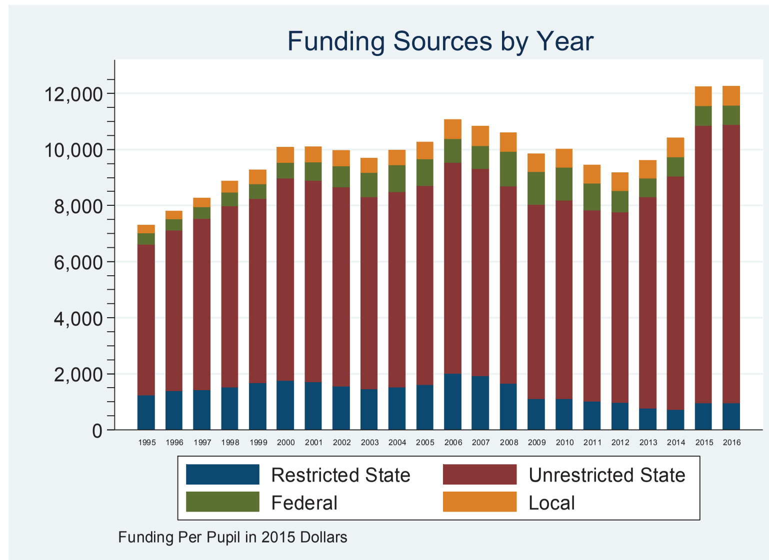
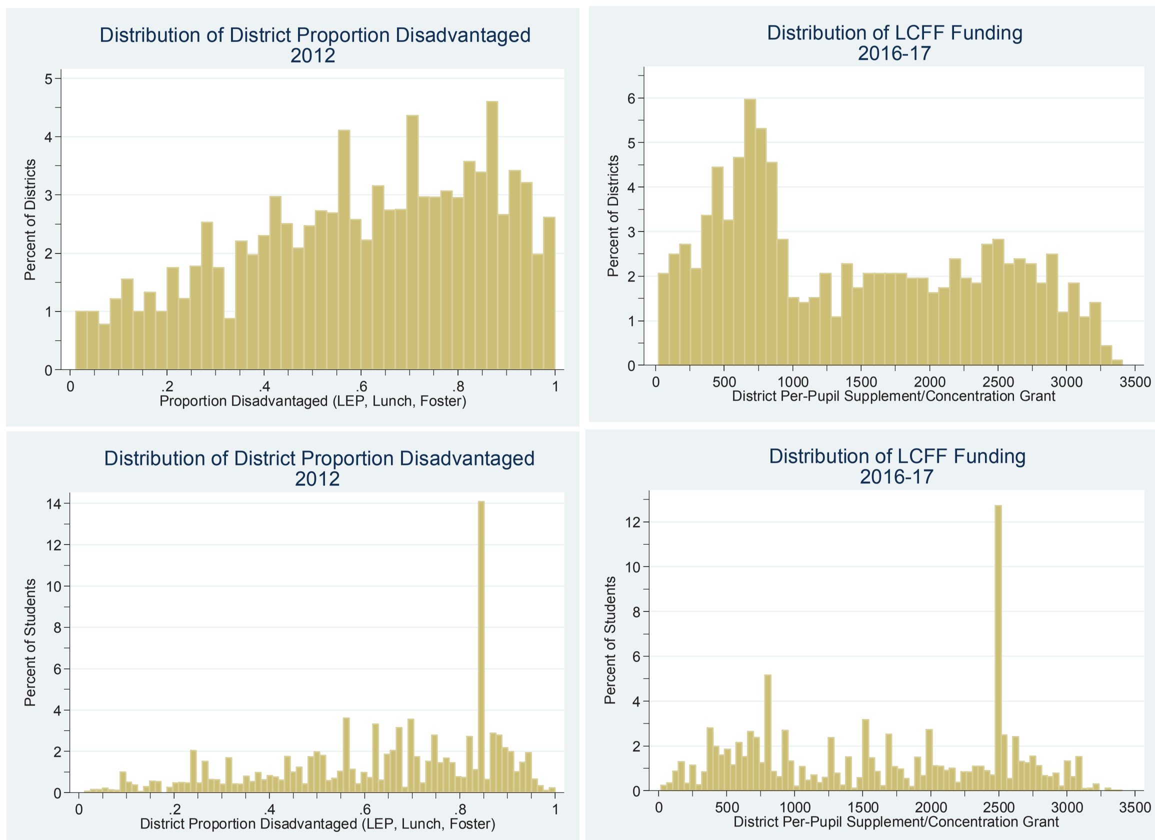


Figure 3.



Distribution of District Proportion Disadvantaged
2012
Percent of Districts
0
1
2
3
4
5
0
.2
.4
.6
.8
1
Proportion Disadvantaged (LEP, Lunch, Foster)
Distribution of LCFF Funding
2016-17
Percent of Districts
0
1
2
3
4
5
6
0
500
1000
1500
2000
2500
3000
3500
District Per-Pupil Supplement/Concentration Grant
Distribution of District Proportion Disadvantaged
2012
Percent of Students
0
2
4
6
8
10
12
14
0
.2
.4
.6
.8
1
District Proportion Disadvantaged (LEP, Lunch, Foster)
Distribution of LCFF Funding
2016-17
Percent of Students
0
2
4
6
8
10
12
0
500
1000
1500
2000
2500
3000
3500
District Per-Pupil Supplement/Concentration Grant

Figure 4.

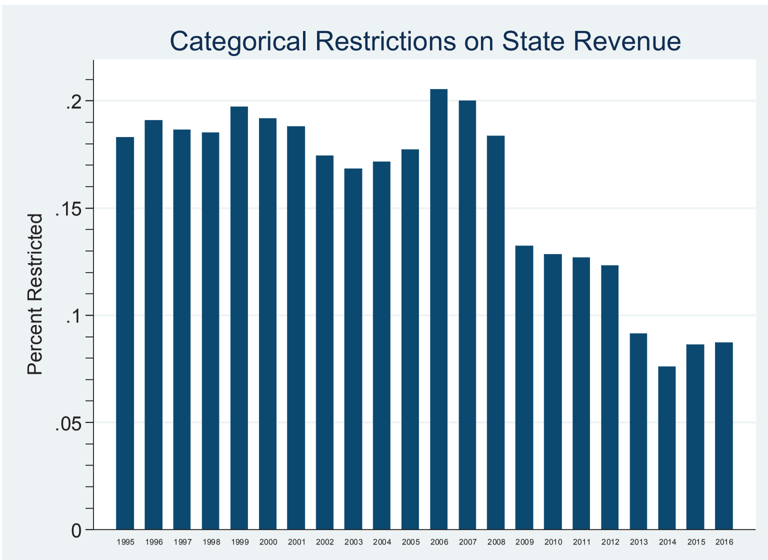


Figure 5.

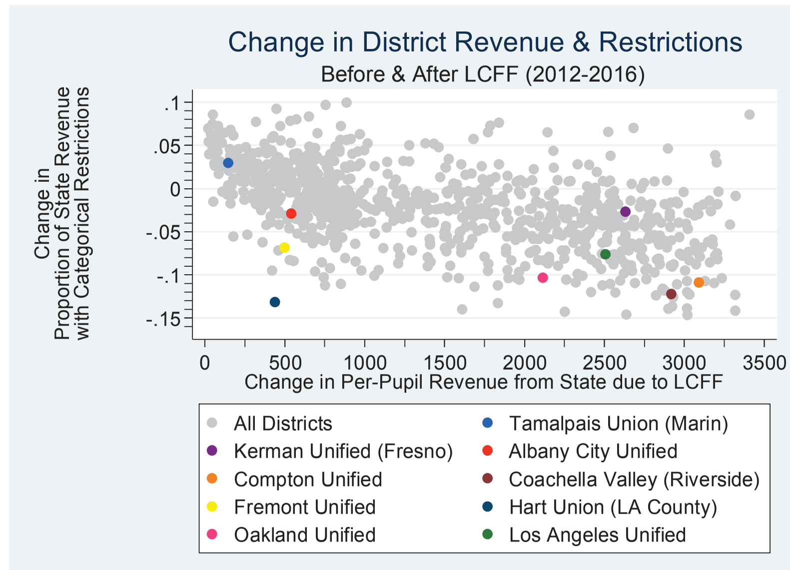


Figure 6a.

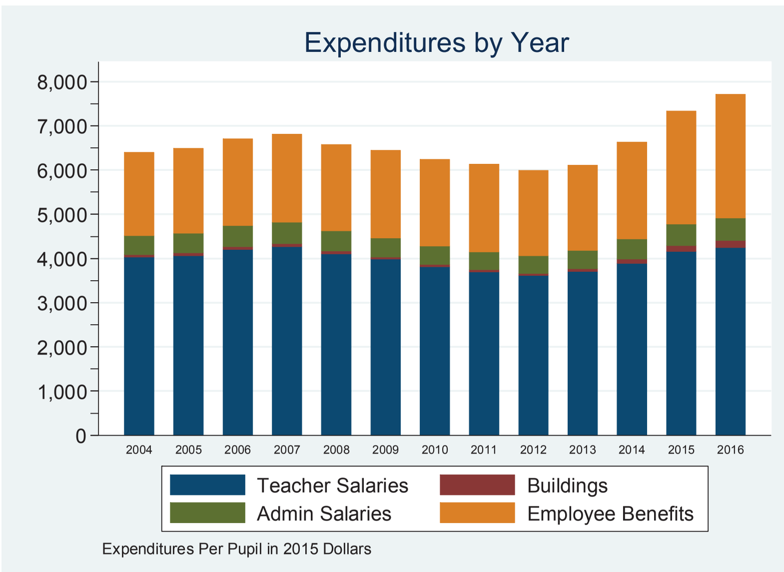


Figure 6b.

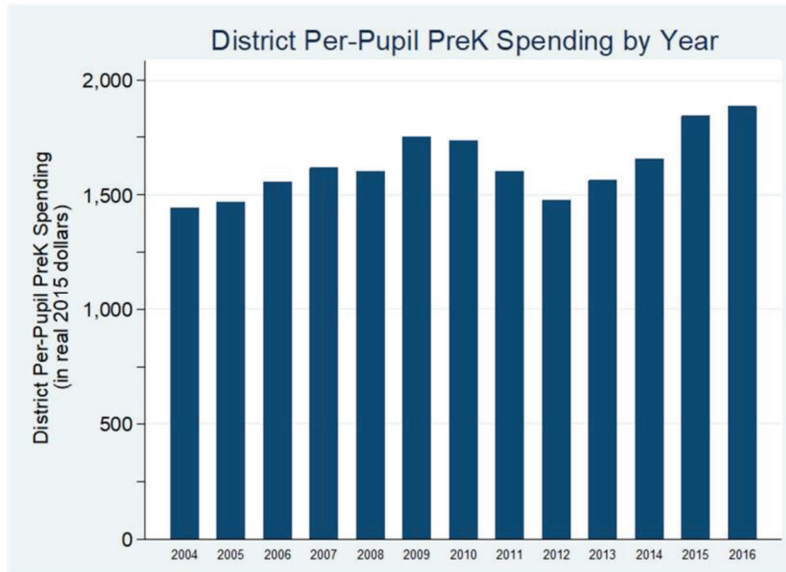


Figure 6c

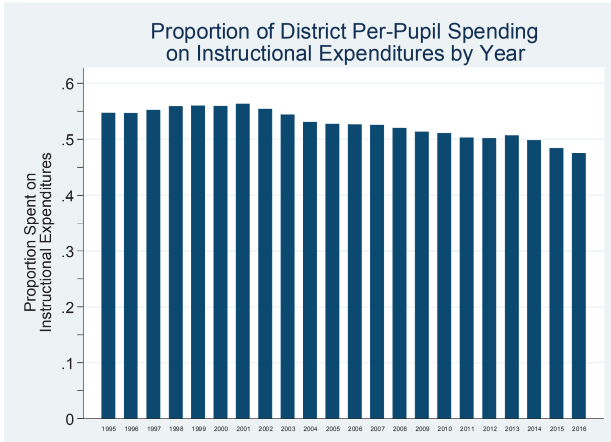


Figure 6d

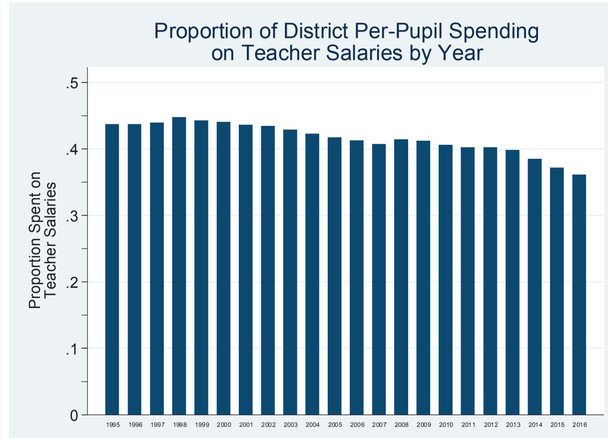


Figure 6e

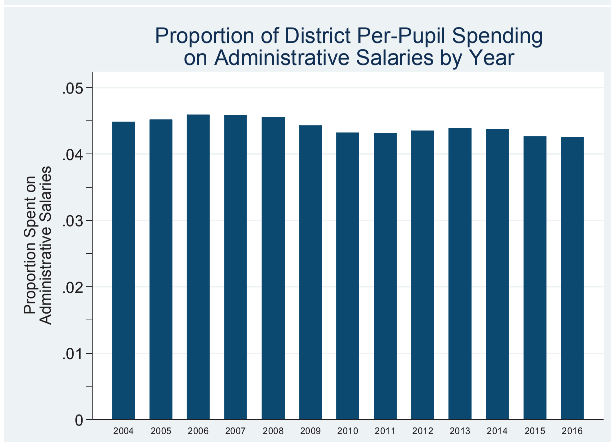


Figure 6f

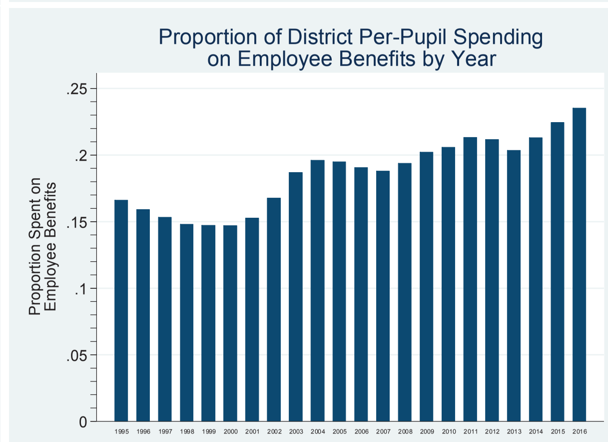


Figure 6g

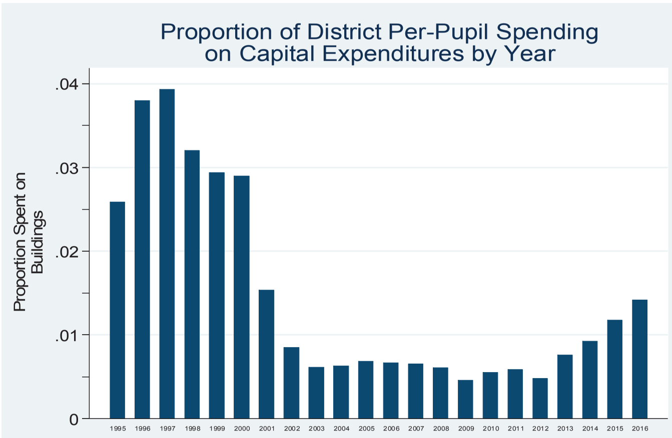


Figure 6h

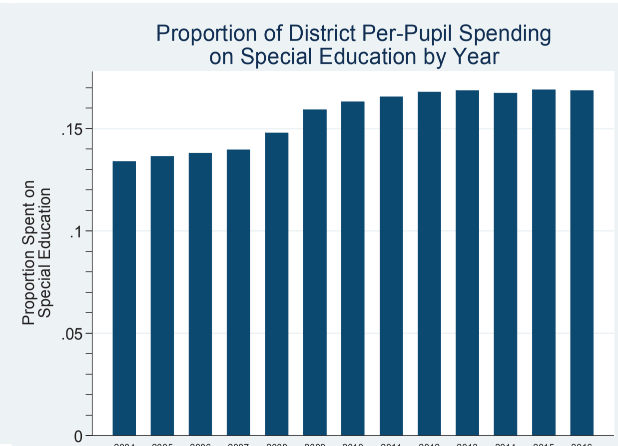


Figure 6i

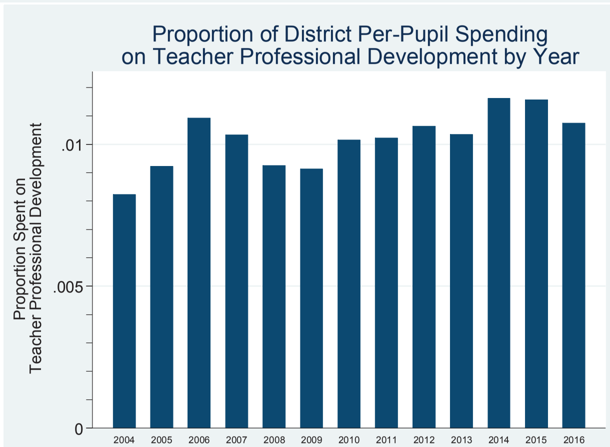
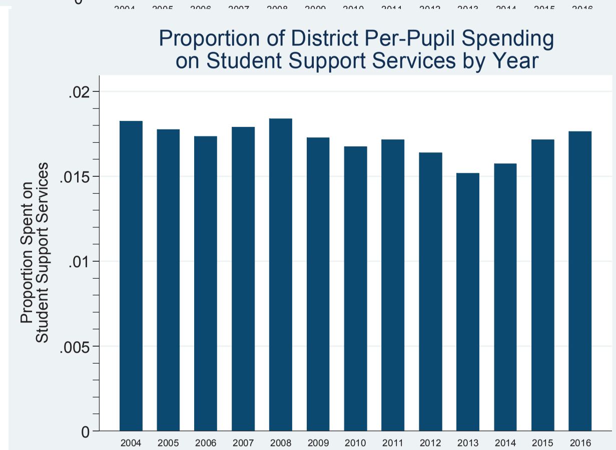
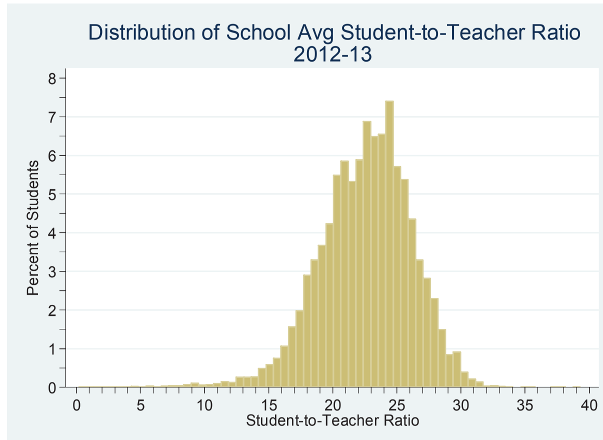
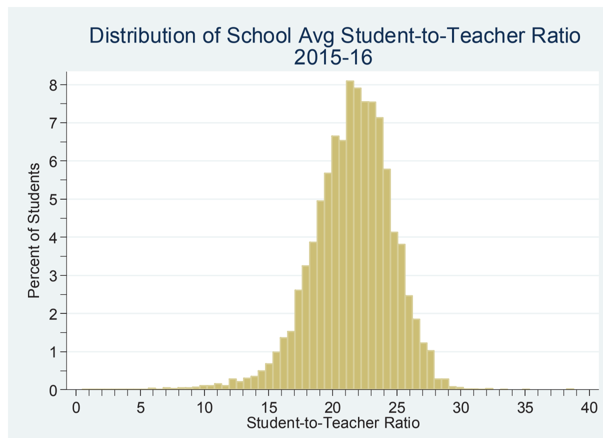
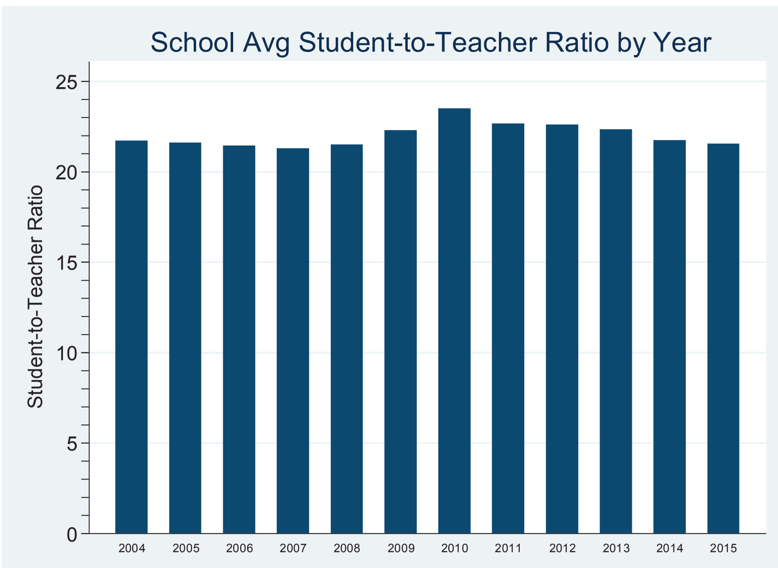


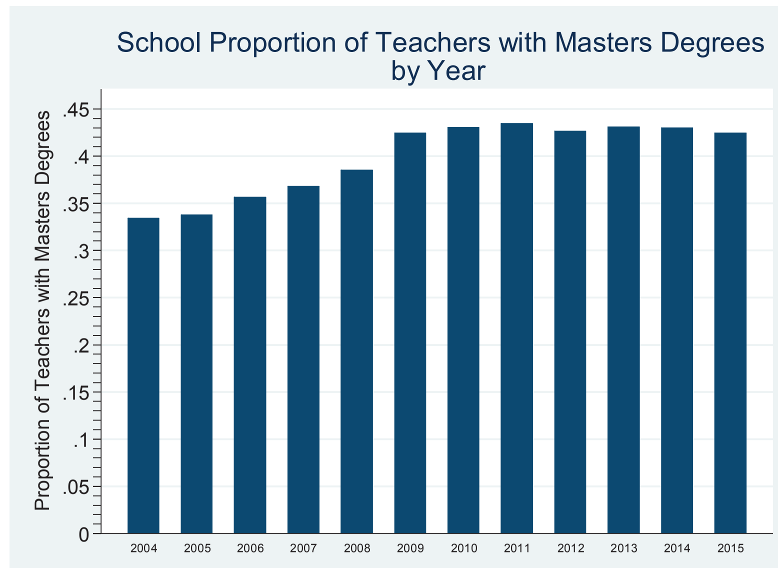
Figure 6j




Distribution of School Avg Student-to-Teacher Ratio
2012-13
Percent of Students
0
1
2
3
4
5
6
7
8
0
5
10
15
20
25
30
35
40
Student-to-Teacher Ratio


Distribution of School Avg Student-to-Teacher Ratio
2015-16
Percent of Students
0
1
2
3
4
5
6
7
8
0
5
10
15
20
25
30
35
40
Student-to-Teacher Ratio


School Avg Student-to-Teacher Ratio by Year
Student-to-Teacher Ratio
0
5
10
15
20
25
2004
2005
2006
2007
2008
2009
2010
2011
2012
2013
2014
2015


School Proportion of Teachers with Masters Degrees by Year
Proportion of Teachers with Masters Degrees
0
.05
.1
.15
.2
.25
.3
.35
.4
.45
2004
2005
2006
2007
2008
2009
2010
2011
2012
2013
2014
2015

Evidence of Treatment Exogeneity

Figure 7a.

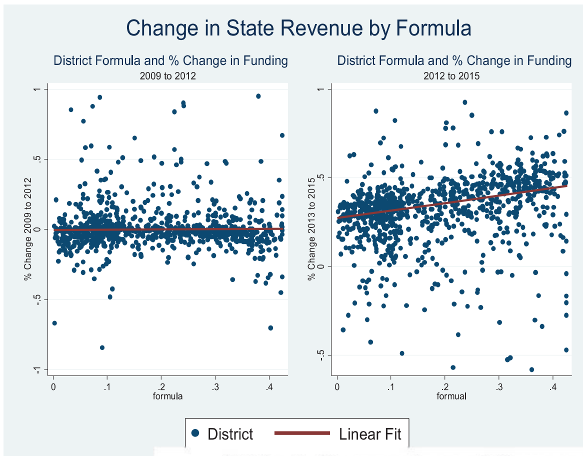


Figure 7b.

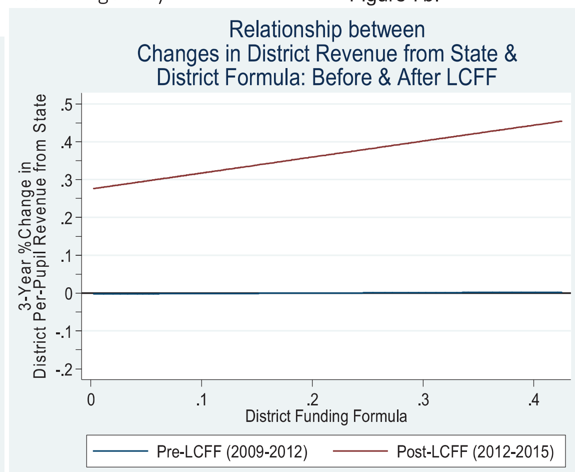


Table 1. Regressions of Revenue Change on District Disadvantage

	%Change in $$ (2009-2019)	%Change in $$ (2012-2015)
Formula	0.019	0.445
	(0.048)	(0.066)**
Constant	-0.003	0.269
	(0.009)	(0.010)**
R^2	0.0004	0.1817
N	889	905

*p<0.05

**p<0.01

Figure 9.

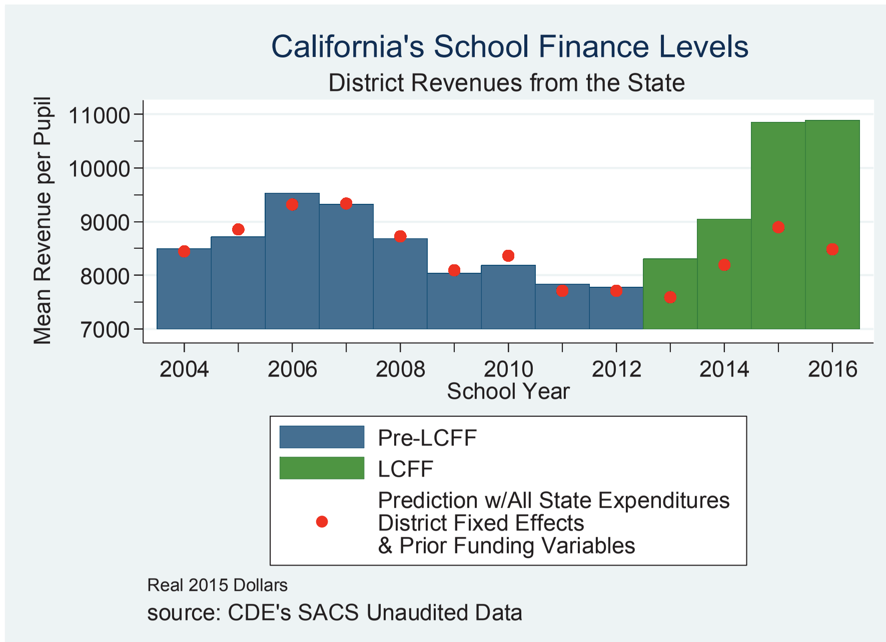
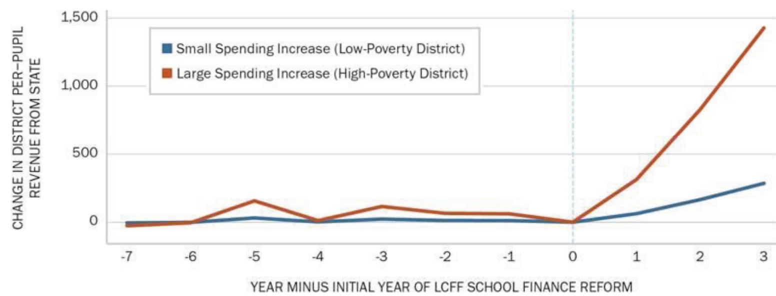

California's School Finance Levels
District Revenues from the State
Mean Revenue per Pupil
11000
10000
9000
8000
7000
2004
2006
2008
2010
2012
2014
2016
School Year
Pre-LCFF
LCFF
Prediction w/All State Expenditures
District Fixed Effects
& Prior Funding Variables
Real 2015 Dollars
source: CDE's SACS Unaudited Data

Figure 10.

Effects of LCFF on District Revenue From the State



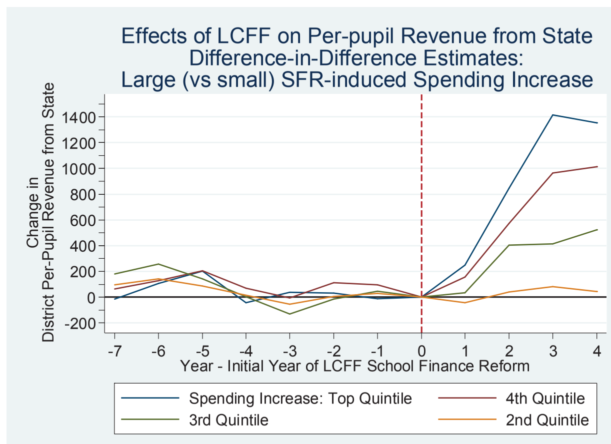


Table 2. **F-Statistics on Excluded LCFF Instruments from 1st-Stage Models of District Per-Pupil Spending & Proportion of Funding with Restrictions:**

	2SLS-IV			
	First-Stage Models, Dependent variable:			
	(1)	(2)	(3)	(4)
	District Per-pupil Spending		Proportion of Revenue from State with no Categorical Restrictions	
Model	without 2012 %Restricted	with 2012 %Restricted	without LCFF dosage*LCFF exposure	with LCFF dosage*LCFF exposure
Excluded Instruments	LCFF dosage* LCFF exposure	LCFF dosage* LCFF exposure	2012 %Restricted* LCFF exposure	2012 %Restricted* LCFF exposure
First-Stage, F-Statistic on excluded instruments	33.38	36.20	175.47	62.86
Number of Districts	920	920	920	920

Table 3. **2SLS-IV Estimates of Effects of LCFF SFR-Induced Effects of Per-Pupil Spending on High School Graduation Rates by Child Poverty & Race/Ethnicity**

	(1)	(2)	(3)	(4)	(5)
	High School Graduation Rate (4-Year Cohort)				
KEY EXPLANATORY VARIABLE:	2SLS-IV				
	All Kids	Poor Kids	Whites	Blacks	Hispanics
(SFR) Instrumented avg District Per-pupil Spending, ages 15-17 (in 000s)	0.0589***	0.0510***	0.0412**	0.0771**	0.0568***
	(0.0146)	(0.0166)	(0.0197)	(0.0323)	(0.0172)
(Instrumented) Proportion of Revenue from State Unrestricted, ages 15-17 (in std units)	0.0141**	0.0044	0.0075	0.0288**	0.0094
	(0.0059)	(0.0074)	(0.0057)	(0.0140)	(0.0080)
Number of School-Year Observations	9,882	9,438	6,829	3,325	8,527
Number of Schools	1,585	1,535	1,121	621	1,394
Number of School Districts	399	395	361	182	373

Figure 10.

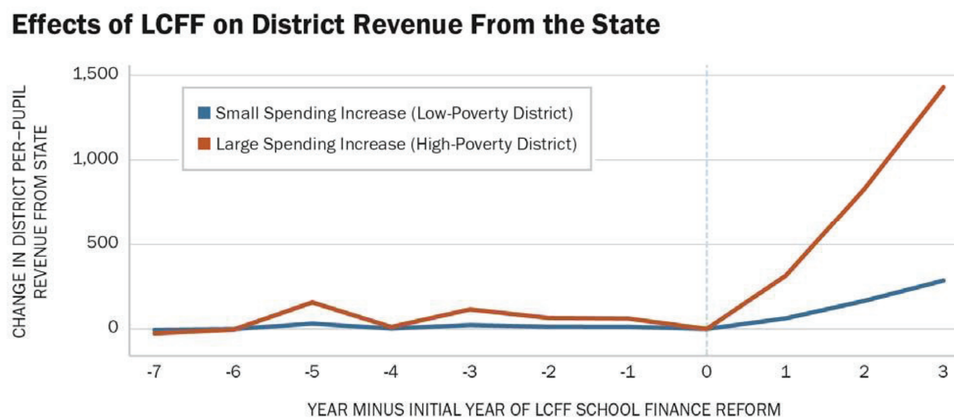


Figure 13.

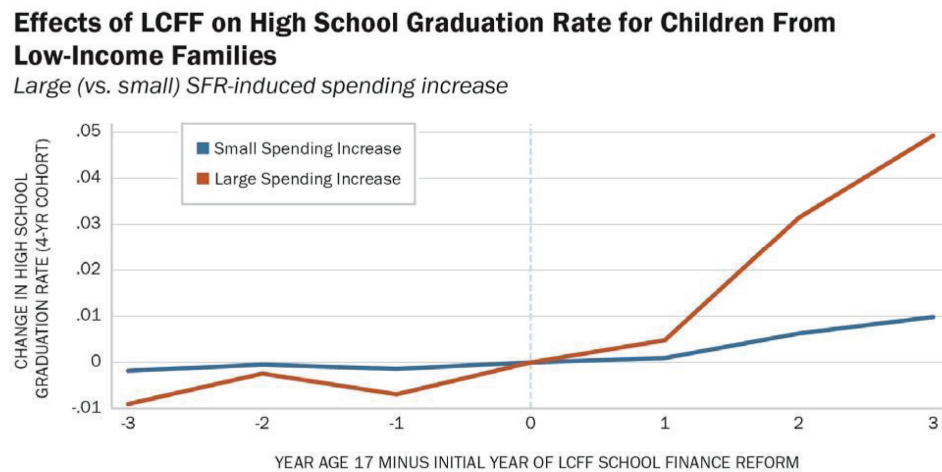


Figure 10.

Effects of LCFF on District Revenue From the State

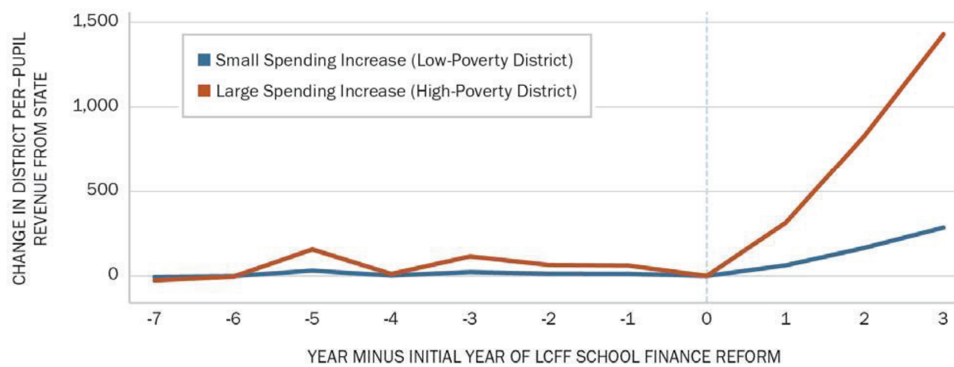


Figure 14.

Effects of LCFF on High School Mathematics Achievement for Children From Low-Income Families

Large (vs. small) SFR-induced spending increase

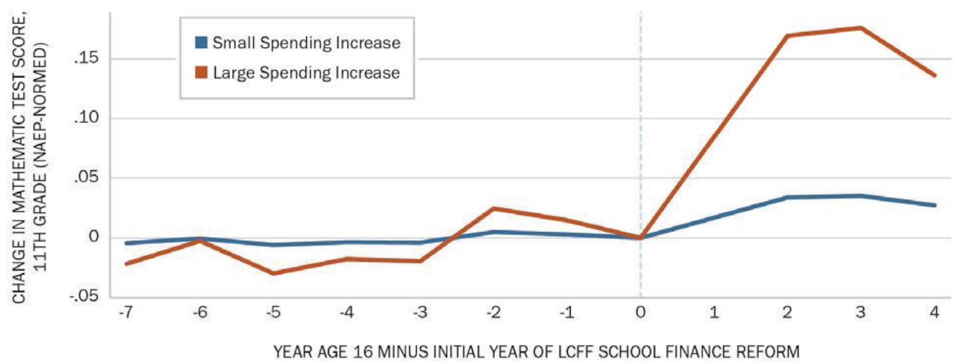


Figure 15.

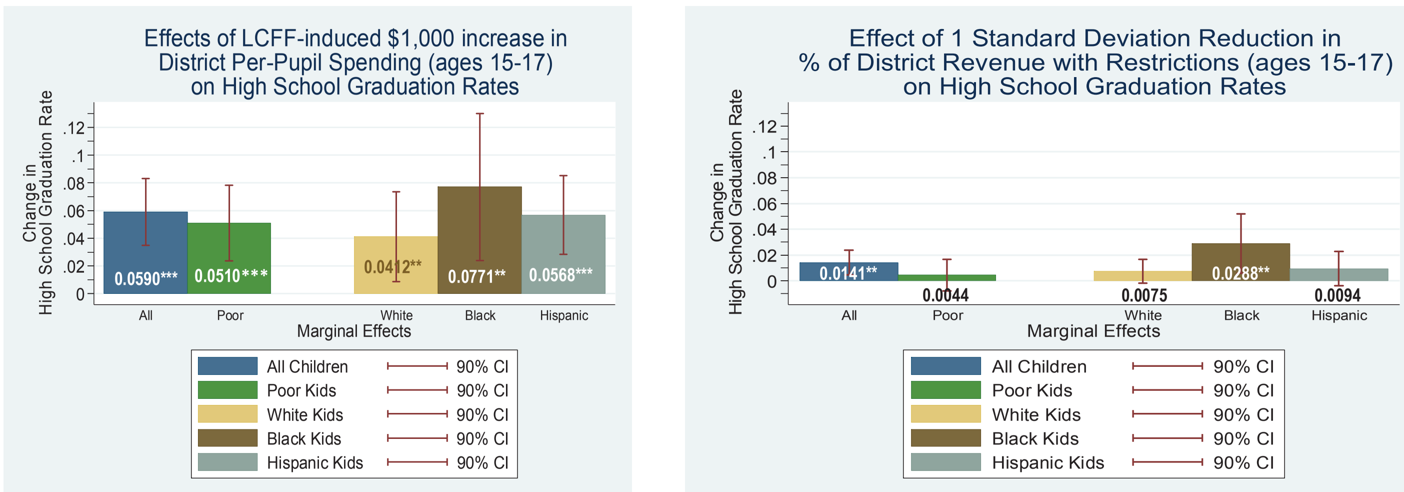

Effects of LCFF-induced $1,000 increase in District Per-Pupil Spending (ages 15-17) on High School Graduation Rates
Change in High School Graduation Rate
0.0590***
0.0510***
0.0412**
0.0771**
0.0568***
All
Poor
White
Black
Hispanic
Marginal Effects
All Children
Poor Kids
White Kids
Black Kids
Hispanic Kids
90% CI
Effect of 1 Standard Deviation Reduction in % of District Revenue with Restrictions (ages 15-17) on High School Graduation Rates
Change in High School Graduation Rate
0.0141**
0.0044
0.0075
0.0288**
0.0094
All
Poor
White
Black
Hispanic
Marginal Effects
All Children
Poor Kids
White Kids
Black Kids
Hispanic Kids
90% CI

Figure 16

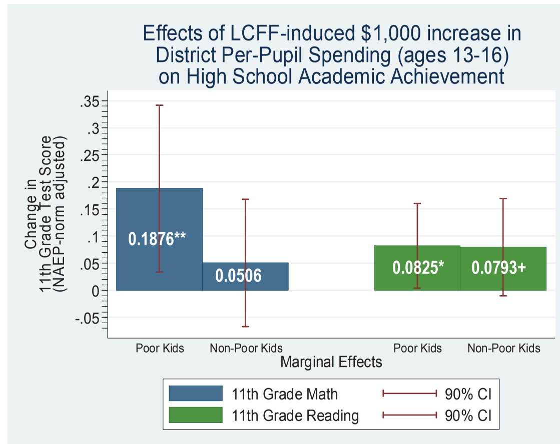
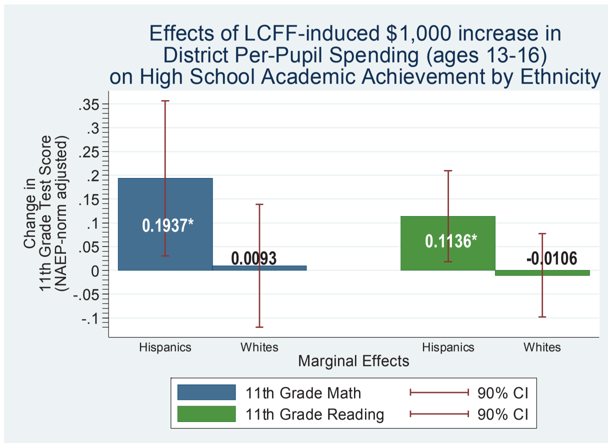


Table 4. 2SLS-IV Estimates of Effects of LCFF SFR-Induced Effects of Per-Pupil Spending on Academic Achievement by Child Poverty Status

	(1)	(2)	(3)	(4)
	Standardized Test Score (NAEP-scale normed)			
	2SLS-IV			
KEY EXPLANATORY VARIABLE	Math, 11th grade		Reading, 11th grade	
	Poor Kids	Non-Poor Kids	Poor Kids	Non-Poor Kids
(SFR) Instrumented avg District Per-pupil Spending, ages 13-16 (in 000s)	0.1876**	0.0506	0.0825*	0.0793+
	(0.0937)	(0.0713)	(0.0475)	(0.0546)
(Instrumented) Proportion of Revenue from State Unrestricted, ages 13-16 (in std units)	0.1752**	-0.2609	-0.0765**	0.2006
	(0.0751)	(0.2366)	(0.0372)	(0.1840)
Number of School-Year Observations	8,816	8,801	13,414	12,088
Number of Schools	1,426	1,228	1,540	1,421
Number of School Districts	387	367	394	384

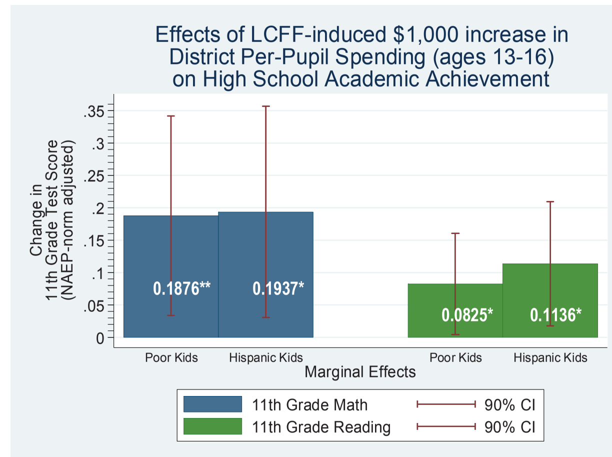
Table 5. **2SLS-IV Estimates of Effects of LCFF School Finance Reform-Induced Effects of Per-Pupil Spending on Academic Achievement by Race/Ethnicity**

	(1)	(2)	(3)	(4)
	Standardized Test Score (NAEP-scale normed)			
	2SLS-IV			
KEY EXPLANATORY VARIABLE	Math, 11th grade		Reading, 11th grade	
	Hispanics	Whites	Hispanics	Whites
(SFR) Instrumented avg District Per-pupil Spending, ages 13-16 (in 000s)	0.1937*	0.0093	0.1136*	-0.0106
	(0.0991)	(0.0786)	(0.0582)	(0.0531)
(Instrumented) Proportion of Revenue from State Unrestricted, ages 13-16 (in std units)	0.2539**	-0.5707**	-0.0962	0.0911
	(0.1184)	(0.2790)	(0.0615)	(0.2231)
Number of School-Year Observations	8,455	7,101	12,859	10,330
Number of Schools	1,353	1,002	1,463	1,204
Number of School Districts	365	351	374	373

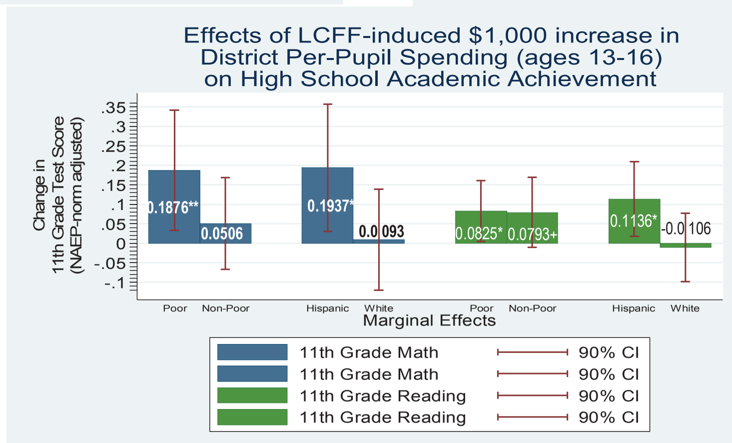


Effects of LCFF-induced $1,000 increase in
District Per-Pupil Spending (ages 13-16)
on High School Academic Achievement by Ethnicity
Change in
11th Grade Test Score
(NAEP-norm adjusted)
.35
.3
.25
.2
.15
.1
.05
0
-.05
-.1
0.1937*
0.0093
0.1136*
-0.0106
Hispanics
Whites
Hispanics
Whites
Marginal Effects
11th Grade Math
90% CI
11th Grade Reading
90% CI

Figure 17



Effects of LCFF-induced $1,000 increase in
District Per-Pupil Spending (ages 13-16)
on High School Academic Achievement
Change in
11th Grade Test Score
(NAEP-norm adjusted)
.35
.3
.25
.2
.15
.1
.05
0
0.1876**
0.1937*
0.0825*
0.1136*
Poor Kids
Hispanic Kids
Poor Kids
Hispanic Kids
Marginal Effects
11th Grade Math
90% CI
11th Grade Reading
90% CI



Effects of LCFF-induced $1,000 increase in
District Per-Pupil Spending (ages 13-16)
on High School Academic Achievement
Change in
11th Grade Test Score
(NAEP-norm adjusted)
.35
.3
.25
.2
.15
.1
.05
0
-.05
-.1
0.1876**
0.0506
0.1937*
0.0093
0.0825*
0.0793+
0.1136*
-0.0106
Poor
Non-Poor
Hispanic
White
Poor
Non-Poor
Hispanic
White
Marginal Effects
11th Grade Math
90% CI
11th Grade Math
90% CI
11th Grade Reading
90% CI
11th Grade Reading
90% CI

Figure 18a.

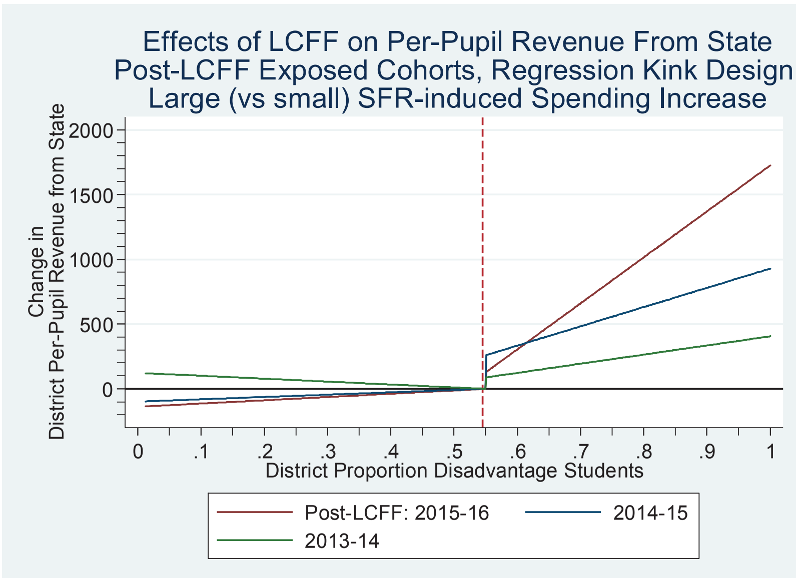


Figure 18b.

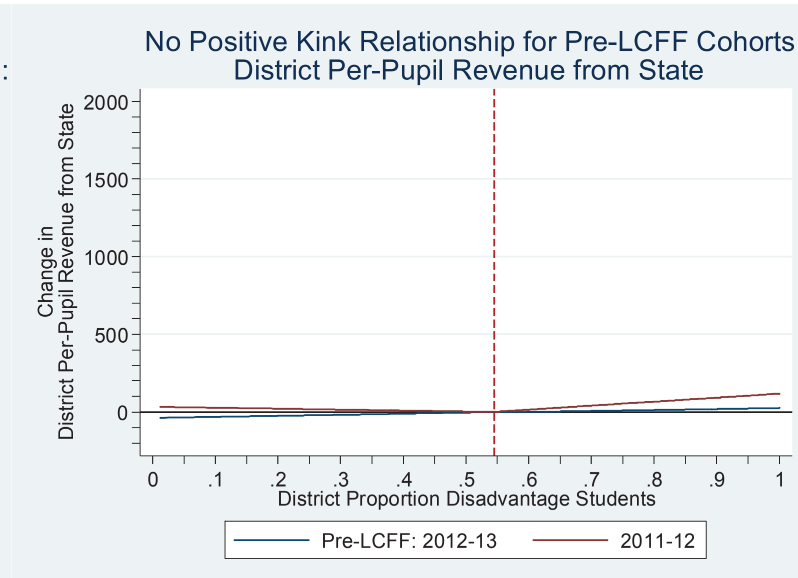


Figure 18a.

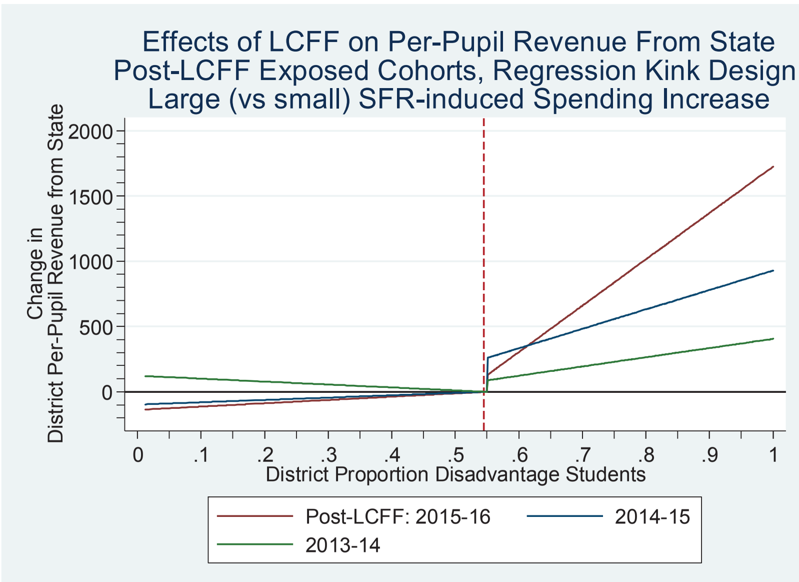


Figure 19a.

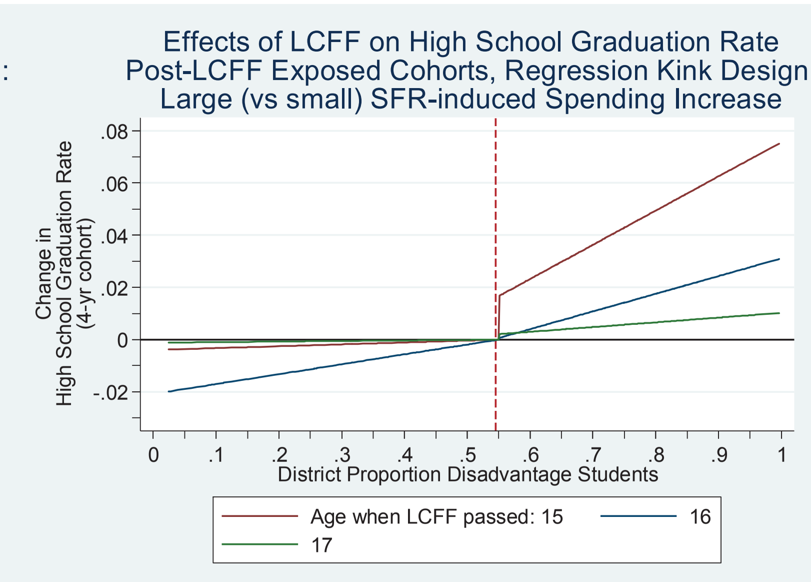


Figure 19a.

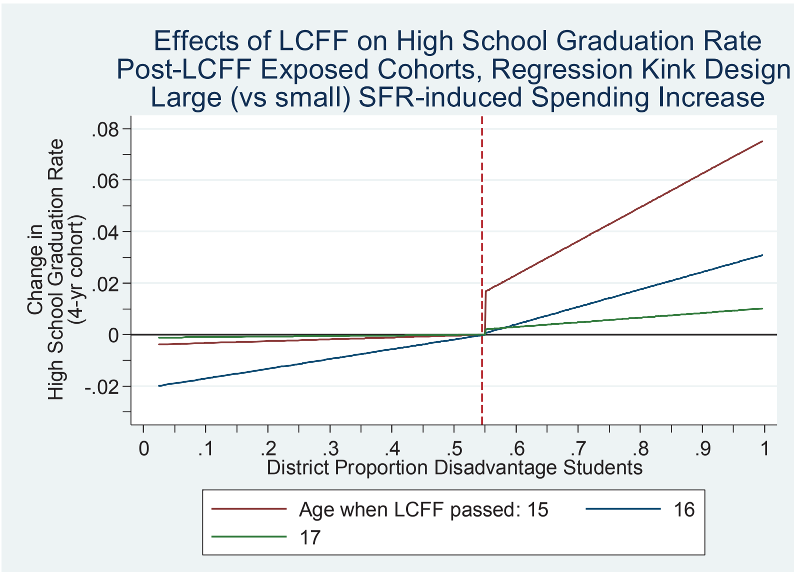


Figure 19b.

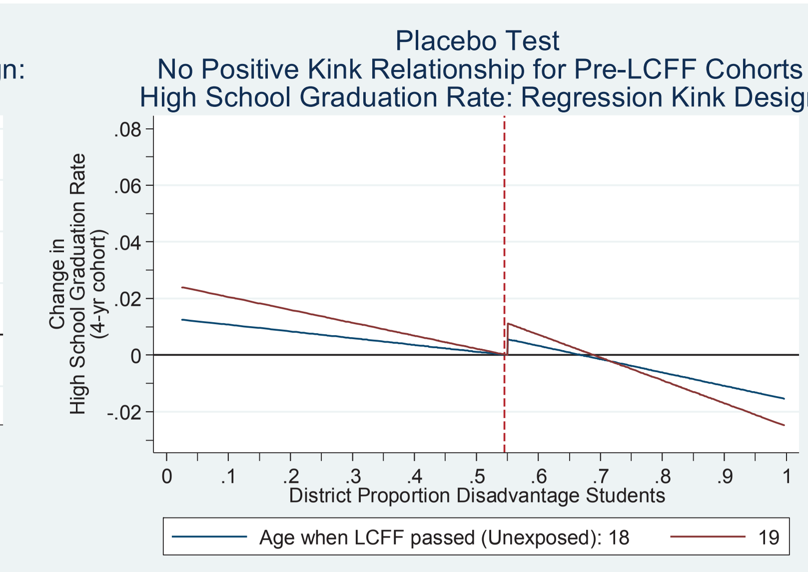


Figure 19c.

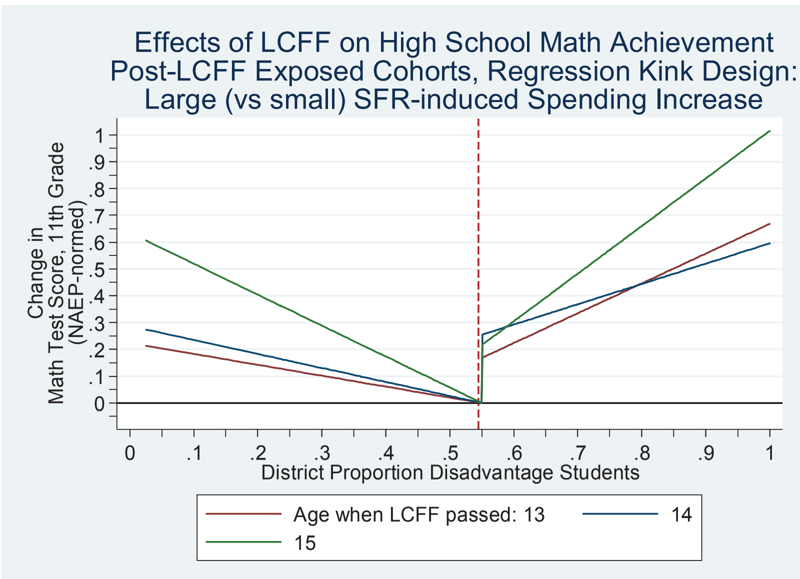


Figure 19d.

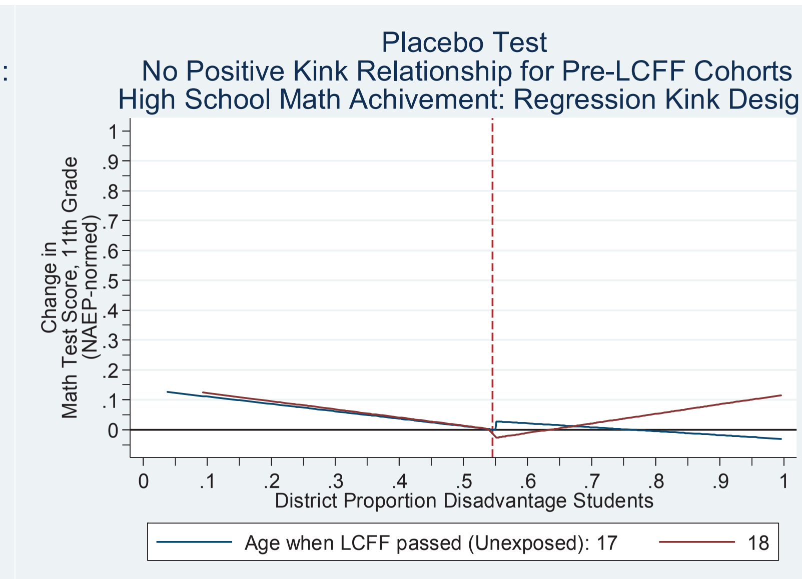


Table 6. **2SLS-IV & Regression Kink Design Estimates of Effects of LCFF School Finance Reform-Induced Effects of Per-Pupil Spending on High School Achievement**

KEY EXPLANATORY VARIABLE	(1)	(2)	(3)	(4)
	High School Graduation Rate (4-Year Cohort)		Math, 11th grade Standardized Test Score (NAEP-scale normed)	
	Poor Kids, 2SLS-IV	All Kids, 2SLS-RKD	Poor Kids, 2SLS-IV	All Kids, 2SLS-RKD
(SFR) Instrumented avg District Per-pupil Spending, ages 13-16 (in 000s)	0.0510***	0.0877*	0.1876**	0.4848***
	(0.0166)	(0.0435)	(0.0937)	(0.1834)
Number of School-Year Observations	9,438	9,882	8,816	10,790
Number of Schools	1,535	1,585	1,426	1,534
Number of School Districts	395	399	387	395

F i g u r e 2 0

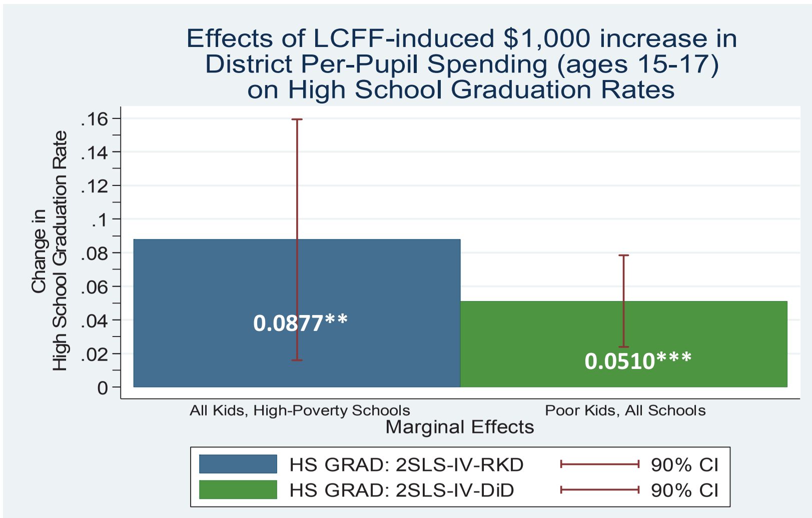


Table 7

2SLS-IV Estimates of Effects of LCFF SFR-Induced Effects of Increased Revenue & Discretion on School Inputs and the Composition of School District Spending

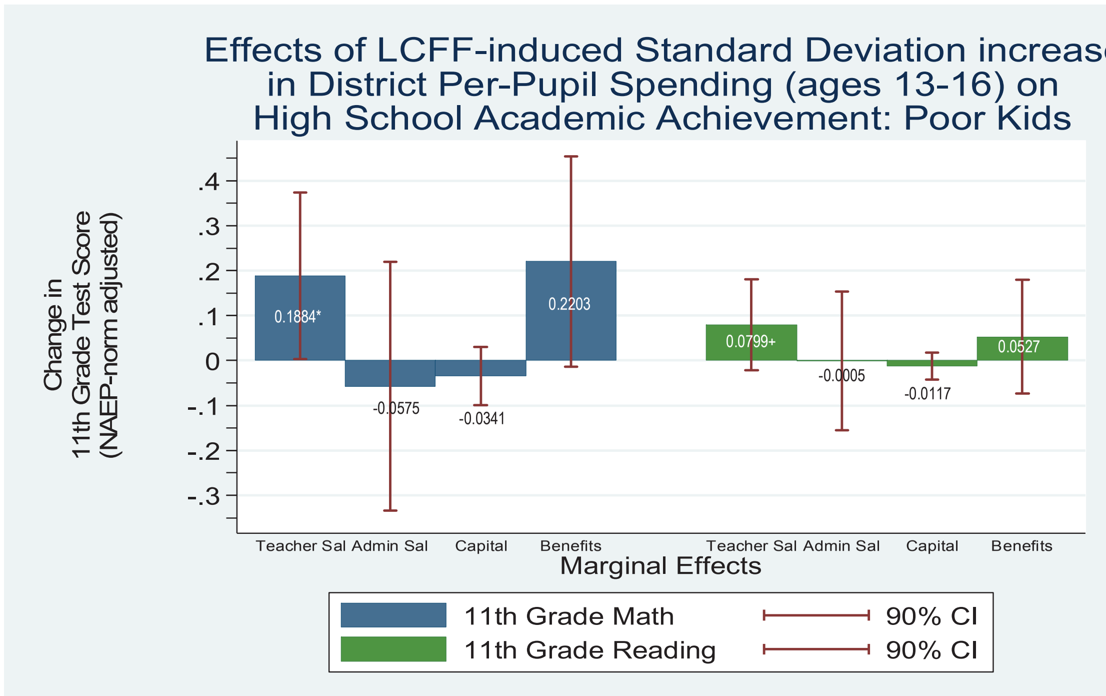
	(1)	(2)	(3)
KEY EXPLANATORY VARIABLE:	School-level Inputs		
	Avg Student-to-Teacher Ratio	Proportion of Teachers w/ Masters	Prob(avg Teacher has Limited Experience)
(Instrumented) avg District Per-pupil Revenue from State	**-0.2368*****	**0.0023**	**0.0062**
	(0.0549)	**(0.0028)**	**(0.0067)**
(Instrumented) Percent of Revenue from State Unrestricted	**0.07221*****	**0.0002**	**0.0038****
	(0.0148)	**(0.0007)**	**(0.0017)**
Number of Schools	9,231	9,231	9,231
Number of School Districts	920	920	920

Table 7a

2SLS-IV Estimates of Effects of LCFF SFR-Induced Effects of Increased Revenue & Discretion on School Inputs and the Composition of School District Spending

	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
KEY EXPLANATORY VARIABLE:	District-level	District-level per-pupil spending composition								
	Per-pupil Expenditures	Ln(Avg Teacher Salary)	Teacher Salaries	Instructional Expenditures	Administrative Salaries	Employee Benefits	Capital Expenditures	Special Education	PreK spending per 4-year old	Teacher Professional Development
(Instrumented) District Per-pupil Revenue from State	0.8347***		0.1096***	0.2362***	0.0335***	0.1233***	0.0502***	0.0642***	0.2059***	0.0120
	(0.0505)		(0.0177)	(0.0244)	(0.0084)	(0.0360)	(0.0188)	(0.0245)	(0.0583)	(0.0114)
Ln(Instrumented District Per-pupil Revenue from State)	--	0.2722***	--	--	--	--	--	--	--	--
		(0.0802)								
(Instrumented) Percent of Revenue from State Unrestricted	-71.0185***	-0.0015	-23.2624***	-28.4604***	-2.6946	11.5816	-7.9266	16.5176**	53.6850*	-7.3262**
	(16.9661)	(0.0025)	(7.4919)	(10.1954)	(2.2873)	(11.0462)	(5.3107)	(7.6381)	(28.3767)	(3.4514)
Number of Schools	--	--	--	--	--	--	--	--	--	--
Number of School Districts	920	920	920	920	920	920	920	920	920	920

Figure 21a.



Effects of LCFF-induced Standard Deviation increase in District Per-Pupil Spending (ages 13-16) on High School Academic Achievement: Poor Kids
Change in 11th Grade Test Score (NAEP-norm adjusted)
.4
.3
.2
.1
0
-.1
-.2
-.3
0.1884*
-0.0575
-0.0341
0.2203
0.0799+
-0.0005
-0.0117
0.0527
Teacher Sal
Admin Sal
Capital
Benefits
Teacher Sal
Admin Sal
Capital
Benefits
Marginal Effects
11th Grade Math
90% CI
11th Grade Reading
90% CI

Figure 21b.

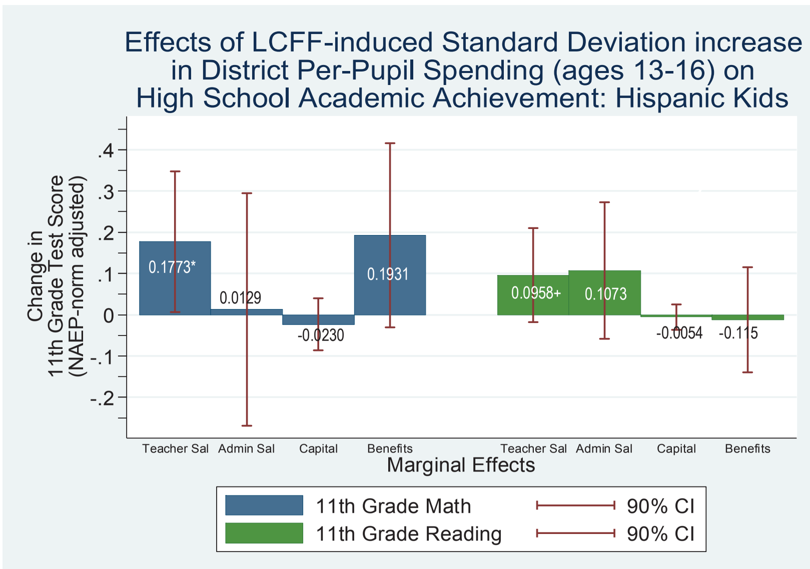


Figure 21c.