ESSAYS ON SCHOOL CHOICE, INFORMATION, AND TEXTBOOK FUNDING

by

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A DISSERTATION

Presented to the Department of Economics
and the Graduate School of the University of Oregon
in partial fulfillment of the requirements
for the degree of
Doctor of Philosophy

June 2014

DISSERTATION APPROVAL PAGE

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Title: Essays on School Choice, Information, and Textbook Funding

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Degree awarded June 2014

DISSERTATION ABSTRACT

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Doctor of Philosophy

Department of Economics

June 2014

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The second chapter examines the impact of information about school quality on student enrollment. I use a regression discontinuity design to estimate the effects of a school choice program in California that provides families with signals of low school quality. I find that signals of low quality decrease school enrollment by 14.3% relative to enrollment in the previous year and 23.6% over two years. Despite the large changes in enrollment, student demographics are not affected. Additionally, the effects of school-quality signals are largest when families have alternative school choices that are nearby. I also find some evidence that student achievement in elementary schools declines, although I cannot separately identify the degree to which this is caused by changes in student composition.

The third chapter examines the effect of textbook funding on student performance. Evidence on the effects of school resources on student achievement is mixed, but quasi-experimental methods suggest that interventions like class size reductions improve student achievement. This is the first study to consider the effect of textbook funding on student achievement by using a quasi-experimental setting in the U.S. I focus on a large class action lawsuit in California that provided a one-time payment of $96.90 per student for textbooks if schools fell below a threshold of academic performance in the previous year. Exploiting this variation with a regression discontinuity design, I find that textbook funding has significant positive effects on student achievement. The low cost of textbooks relative to class size reduction implies that these effects have a very high benefit-per-dollar.

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To Lynette, for her endless love, encouragement, and support.

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CHAPTER I

INTRODUCTION

In the following chapters, I present empirical research on questions of information and school choice, and textbook funding. Each chapter addresses a separate research question and together they further our understanding of the economics of education.

The second chapter examines the impact of information about school quality on student enrollment. In general, the proponents of school choice argue that schooling options will cause schools to compete to attract student by improving academic performance. However, this argument depends crucially on the family's access to information about school quality. If families do not have access to information about school quality, then schools that raise academic performance may not attract students and school-choice programs may not lead to better outcomes. To investigate the dependence of student transfers on school quality information, I use a Regression Discontinuity design to show that schools that are labeled low performing experience large declines in enrollment. This suggests that many families do not use school choice programs because they do not have information on school quality. This lack of information may inhibit school choice programs from creating competition between schools to attract students by improving academic achievement.

The third chapter examines the effect of textbook funding on student achievement. Student achievement is a key determinate of outcomes later in life, such as wages and health, and U.S. education policy has focused on improving student outcomes by allocating additional resources to schools. While policies like class size reduction or teacher training are well studied, very little is known about the effects of capital in the education production function. In particular, I focus on the effects of the William's Case settlement that allocated $138 million of additional funding for textbooks. Using a threshold for school eligibility, I estimate the causal effect of textbook funding on student test performance with a Regression Discontinuity design. I find that this funding improves student test scores for elementary school students while costing significantly less than comparable class size reduction policies.

CHAPTER II

SIGNALS OF SCHOOL QUALITY AND SCHOOL CHOICE: REGRESSION DISCONTINUITY EVIDENCE FROM CALIFORNIA'S OPEN ENROLLMENT PROGRAM

Introduction

Forty-six states have public school choice programs that allow families to choose the school their child attends without changing their residence.[1] This widespread adoption of school-choice programs has created a renewed interest among researchers to understand the effects of school choice. This literature focuses on many different aspects of school-choice, including competition between schools (see Hoxby (2003) and Ladd (2003) for summaries), performance gains for transfer students (Cullen, Jacob and Levitt (2006); Hastings and Weinstein (2008)) and impacts on the racial composition of schools (Bifulco and Ladd (2007)). Despite covering a wide range of topics, few papers have focused on the importance of information about school quality on student transfers. In general, proponents argue that school choice programs could cause schools to compete to attract students by improving academic performance and thus raise the academic performance of all schools. However, if families do not have information about school quality, then schools that raise academic performance may not attract students and school-choice programs may not lead to better outcomes.

A similar question exists in the literature on accountability programs. With the expansion of school choice, targeted stigma and school voucher threats are widely used by policy makers to motivate schools to improve academic achievement. These programs label schools as low-performing and provide the option for students to transfer, explicitly using the household's decision to transfer schools as a punishment for schools that fail to meet performance standards. Researchers have shown mixed, but promising evidence that these programs may improve student achievement, but we do not know the extent to which households are responding to the signal of quality about their school versus using the new school choice options. This paper aims to fill this important gap in the literature by examining the effect of new information about school quality on school enrollment. Specifically, I examine California's Open Enrollment program, which provides signals of low-school quality while only having a minimal effect on transfer options.

[1] This information comes from the Center for Education Reform, The Heritage Foundation, Choices in Education and the Education Commission of the States: School Choice State Laws

Beginning in 2010, the Open Enrollment program labels 1,000 low-performing schools each year based on their average test scores. Families who have children attending a low-performing school are notified by their school district that the school is low performing. Though the family is also informed that they have the option to transfer to any school with higher average test scores, the Open Enrollment program is rarely necessary for a student to transfer because California's intradistrict transfer program allows any student to transfer within the school district. In particular, the Open Enrollment program only introduces one new type of transfer option: it allows a student at a low-performing school to transfer outside their school district to a school with higher average test scores. Furthermore, I show that my results are not being driven by families that use the Open Enrollment program to transfer across school districts, which suggests that the Open Enrollment program does not affect school enrollment by introducing new transfer options. As such, the Open Enrollment program affects school enrollment by providing families with information about school quality.

I examine the effects of labeling a school as low-performing using a regression discontinuity (RD) design. In particular, any school with fewer than 100 "valid" test takers in 2009 cannot be assigned to the Open Enrollment list.[2] This eligibility rule creates a discontinuity (at 100 valid test scores) in the probability that a school is labeled low-performing.[3] This source of variation is particularly appealing because the number of valid test scores in 2009 and 2010 were determined before the eligibility rule was announced in 2010. Therefore, it is virtually impossible that eligibility has been manipulated by schools.

I find that the low-performing label causes school enrollment to decline 14.3% from the previous year, and by 23.6% over two years. This suggests that families are very responsive to labeling despite publicly available measures of school quality and that accountability programs that label schools are potentially placing massive pressure on schools separate from providing an ability to transfer.[4]. In short, labelling schools as low performing can induce households to transfer schools, and greatly reduce enrollment at a labelled school. With the rise in school choice, policies that

[2]A test taker is "valid" if they complete the exam and receive no special assistance. I discuss the relationship of valid test takers, test takers, and enrollment in Section 2.

[3]The observant reader may have noticed the potential for a RD design at the "1,000" school threshold. As it turns out, the probability of being labelled low-performing does not change sharply at this threshold due to the nature of other exclusion rules. I discuss this point further in Section 2.

[4]For more on accountability threats and school choice, see Hoxby (2003)

provide information about quality can be easily implemented to encourage sorting between schools, but care should be taken when identifying schools as the effects on enrollment are very large.

I also study the potential effects of the low-performing label on school-level academic achievement. I find that signals of low-school quality significantly reduce student achievement in elementary schools, although I am cannot separately identify the degree to which this is caused by changes in student composition. It is interesting to note that school accountability programs do not attempt to identify changes in student demographics, and thus, may not accurately reflect the efforts of labeled schools to improve because these programs focus on average performance while student demographics may be significantly affected by student transfers. For example, if high performing students are more responsive than low-performing students to labeling, then average performance at the school may decline. This would diminish the observed efforts of a school's attempt to improve student achievement. To explore this possibility, I investigate how signals of low school quality affect student demographics through the changes in school enrollment. I find that, despite the large changes in enrollment, signals of low school quality do not change the percent of minority students or low-income students in low-performing schools. To the extent to which academic performance is related to ethnicity and income, this suggests that higher performing students are not more likely to transfer in response to signals of school quality, and therefore the low-performing label reduces student achievement.

This work is closely related to Hastings and Weinstein (2008) who show that low-income families apply to schools with higher academic performance when they are given information about school test scores. They use a natural experiment caused by No Child Left Behind legislation and a field experiment that randomly supplied families with school test scores and find that families apply to schools with higher academic achievement when they receive test score information. The experimental setting provides an excellent source of exogenous variation in information at the household level. My paper helps expand this literature by focusing on variation in labeling at the school level. This level of variation speaks to the effects of school accountability programs that focus on improving low-performing schools that use the stigma of a low-performing label or the threat of student transfers improve academic achievement. While initial research on Florida's accountability system suggests large improvements in response to voucher threats (Greene (2001)), additional work suggests that a large portion of these gains were caused by measurement error, changing compositions of students, and the stigma of being identified as low performing (Kane and Staiger (2001), Camilli and Bulkley (2001), Figlio and Rouse (2006)). While California's Open Enrollment program provides a similar stigma for schools that are labeled low performing, my results suggest that the low-performing label created pressure through changes in enrollment, but reduced the academic performance of labelled schools.

This paper is also related to work that focuses on the effects of school quality information on property values and suggests that information about school quality is not widely known. In particular, Figlio and Lucas (2004) find that a signal of high quality for a school raises nearby housing prices. Similarly, Imberman and Lovenheim (2012) use a differences-in-differences identification strategy to examine the effects of providing households with information on teacher and school value added estimates. They find that housing prices are not effected by this information.

The remainder of this paper proceeds as follows. Section 2 describes California's Open Enrollment Program in detail, and Section 3 discusses the data used in my empirical analysis. My empirical strategy is discussed in Section 4 and Section 5 presents the main results. Section 6 concludes.

Background

The History of California's Open Enrollment Program

This paper focuses on California's Open Enrollment program, which was implemented in 2010. The complete timeline for this program is shown in Figure 1 (see Appendix A for all figures). The Open Enrollment program was initially proposed by California State senator Gloria Romero on December 15, 2009. and was designed to "improve student achievement...and to enhance parental choice in education by providing additional options to pupils to enroll in public schools throughout the state without regard to the residence of their parents." By providing parents with information on school performance and the ability to transfer, policy makers were attempting to "hold schools accountable" and encourage schools to improve performance. On July 9, 2010, the California Department of Education announced the rules that determine how schools are identified as low performing, and the Open Enrollment program was active for the first time for the 2010-2011 school year. However, most transfer options offered by the Open Enrollment program are also provided by a previous school choice program.

In 1994, California introduced the *Intradistrict* transfer program. The purpose of this program is simple: it allows students to transfer to any school in their school district of residence. The Intradistrict transfer program has been active before and after the Open Enrollment program, and therefore provides many of the options to transfer provided by the Open Enrollment program. Additionally, the Intradistrict transfer program should not be obscure to parents because each year parents are informed of this option through the annual notice of rights and responsibilities. An example of this notice for the Irvine Unified School District in the 2008-2009 school year is included in Appendix B. In the section "Transfers between Schools," it states that parents may transfer their children to any school within their school district. Furthermore, this letter must be signed by the parents or guardians of each student.

In addition to the signal about quality, it is possible that the Open Enrollment program could affect enrollment by (1) allowing new types of transfers, (2) making it easier for students to transfer into crowded schools or (3) informing families about an existing transfer option. However, there is evidence to suggest that none of these mechanisms are likely to be relevant in practice. First, the Open Enrollment program provides only one new type of transfer: it allows a student at a school labeled low-performing to transfer outside their school district to a school with higher average test scores. In Section 5, I provide evidence that the effect of a negative signal of school quality declines as the distance a family would have to travel increases, and the distance between school districts is further than most families are willing to travel.

Furthermore, both programs follow the same method for allowing students into crowded schools; even the wording of both programs is nearly identical.[5] In fact, the programs are so similar that several school districts use identical forms for within district transfers, regardless of the program that students use to transfer. Moreover, the California School Boards Association has suggested that the Open Enrollment law be rewritten to avoid confusion. This suggests that it is unlikely that the Open Enrollment program effects school enrollment by making it easier to transfer into crowded schools.

Lastly, the information contained in the Open Enrollment letter can be classified as (1) information about the ability to transfer and (2) information about school quality. However, it

[5]For example, both programs require that the selection of transfer students must use a "random, unbiased process that prohibits an evaluation of whether a pupil should be enrolled based upon his or her academic or athletic performance," if the school is at full capacity.

is unlikely that information about the ability to transfer causes changes in enrollment, because all families are informed annually about the ability to transfer by the annual notice of rights.[6] Thus, families that do not receive the signal of school quality should have similar information about their transfer options as families that receive a signal of school quality. In total, each of these points suggest that the Open Enrollment program does not affect school enrollment except through the low-performing label it gives to schools.

Assignment of the Low-Performing Label

Each year, California's Open Enrollment program identifies 1,000 schools as low performing. Schools are ranked according to average test score performance, as measured by California's Academic Performance Index, and the lowest ranked schools are labeled low performing with two exceptions.[7] First, no school district can have more than ten percent of its schools labeled low performing. Second, no school with fewer than 100 valid test scores can be labeled low performing.[8]

Figure 2 illustrates the labeling of schools according to 2009 API. The vertical line shows the highest possible API score a school can have and still be labeled. Schools with API scores below this point are labeled with some positive probability. If they have fewer than 100 valid test scores or 10% of the schools in the district are labeled, then they are not labeled. There are two points to note from this figure. First, school performance (as measured by API) is correlated with the low performing label and simple correlations between labeling and enrollment may reflect underling school characteristics instead of information provided by the label. Therefore, it is crucial that my identification strategy can account for underling school performance. Figure 2 also shows a discrete change in the probability of labeling across the API threshold.

While the "1000" school threshold provides potentially appealing variation in labeling, this estimate is very imprecise. While it is not inconsistent with the main estimates, it is not very informative as the 95 percent confidence interval includes effects twice as large as a schools actual enrollment. One reason for this lack of precision is substantial variation in enrollment. Near the

[6] While parents are required to sign the annual notice of rights, they may choose to ignore the contents of this document. However, it seems likely that such a parent would also ignore the Open Enrollment letter as well.

[7] California's Academic Performance Index is a numeric scale from 200 to 1,000 that measures a school's performance and growth in standardized tests for English, math, science and history.

[8] Appendix B provides a more detailed description of the identification of low-performing schools.

threshold, average enrollment is 693 with a standard deviation of 482. This issue is compounded by the small change in the probability of labeling, which causes the local average treatment effect to be very imprecisely estimated. Because of these issues, I focus on the valid test score rule that is also used to exclude schools from labeling.

Figure 3 illustrates the labeling of schools according to the valid test score threshold. In particular, the eligibility rule prevents schools with less than 100 valid test scores in 2009 from being labelled as low performing.[9] Conversely, schools that have 100 or more valid test scores can be labeled low-performing. This suggests that a fuzzy RD design can be used to estimate the causal effect of signals of low school quality on enrollment. Furthermore, this threshold at 100 valid test scores provides an appealing source of variation because its timing makes manipulation nearly impossible. As shown in Figure 1, the number of valid test scores for each school in 2009 and 2010 were determined before the eligibility threshold was announced. This suggests that schools were not able to manipulate the reported number of valid test scores to avoid the Open Enrollment list because the threshold was not known when the number of valid test scores were determined.

School districts that have one or more schools on the Open Enrollment list are required to notify families by mail if their child's school is on the Open Enrollment school list.[10] While some school choice programs, like Title 1 No Child Left Behind, try to use positive language when referring to identified schools, the laws that implemented the Open Enrollment program refers to schools on the Open Enrollment list as "low achieving" or "low performing" and this language is used in the Open Enrollment letters. Additionally, as discussed in Section 2.1, this letter notifies parents that they can transfer their child to any school that has a higher average test scores.[11]

Data

The main source of data for this project is the California Department of Education's Academic Performance Index (API) files.[12] The API data were originally collected as a result of the Public

[9] Valid test scores do not include any students in kindergarten, first or 12th grade. Additionally, only students who have attended the school for the whole year are counted.

[10] See California Code of Conduct Section 4702 part (a)

[11] Discretion is left to each school district about "the most appropriate method and language for accomplishing parent notification." A sample letter from San Lorenzo Unified School District is included in Appendix C to show how a letter may be structured. For example, the San Lorenzo Unified School District also provided the letter in Spanish.

[12] This data is available on the CDE's website: www.cde.ca.gov/ta/ac/ap/

Schools Accountability Act of 1999. The Open Enrollment program uses these data to measure school performance and to exclude any school with fewer than 100 valid test scores from being labeled low performing.

I use the California Basic Educational Data System (CBEDS) for information on enrollment, race, free and reduced lunch, and pupil to teacher ratios, which are collected annually in October for all schools in California.[13] In addition to CBEDS, the California Department of Education also provides a directory with the latitude and longitude of each school in California. I use this information to approximate the distance a transfer student would have to travel to a new school by calculating the distance between each school in California and its closest available alternative.[14]

The descriptive statistics for the sample are shown in Table 1 (see Appendix A for all tables). For the RD design, I focus on schools near the 100 valid test score threshold and the characteristics of these schools are listed in column two. Notably, these schools have much lower enrollment compared to the average school in California, but not so small as to be unimportant for policy. For example, 17 percent of schools in California are smaller than 200 students, and 20 percent of schools in the U.S. have fewer than 200 students enrolled.[15] Despite their small size, schools near the threshold are fairly comparable in other observable characteristics, such as percent white and Hispanic, free and reduced lunch eligibility, and school type.

Empirical Strategy

I begin by examining the variation that the eligibility threshold causes in the labeling of schools as low performing. The probability that a school is labeled low performing is a non-deterministic function of valid test scores that can be expressed as:

$$P(LABEL_i = 1) = \begin{cases} f(X_i) & \text{if } VALID_i > 99 \\ 0 & \text{if } VALID_i \leq 99 \end{cases} \tag{2.1}$$

[13]This data is also available on the CDE's website: www.cde.ca.gov/ds/sd/sd/.

[14]I calculate the distance using Nichols (2007) STATA program "vincenty" which implements Vincenty's (1975) algorithm for calculating distances.

[15]NCES Table 99. Average enrollment and percentage distribution of public elementary and secondary schools, by type and size: Selected years, 1982-83 through 2008-09.

where X_i is a vector of school characteristics that influence the assignment of the low-performing label. Note that schools are only labeled low-performing if they have 100 or more valid test scores. Therefore, if $f(X_i)$ is non-zero at the threshold, the eligibility rule creates variation in the labeling of schools.

This source of variation is appealing because schools with low test performance tend to be labeled low-performing. If average test scores are also related to school size, correlations between the low-performing label and school enrollment will not reflect the true relationship between information about school quality and school enrollment. In contrast, assignment around the eligibility threshold is independent of school characteristics.[16]

Following Hahn, Todd and Van der Klaauw (2001), I use this threshold to instrument for the low-performing label, estimating the average causal effect of information about school quality on school enrollment using a two stage least squares estimator:

$$\widehat{LABEL}_i = \alpha_1 + \delta_1 * 1(VALID_i > 99) + m(VALID_i) \tag{2.2}$$

$$Y_i = \alpha_3 + \delta_3 * \widehat{LIST}_i + m(VALID_i) + u_i \tag{2.3}$$

where $m(\cdot)$ is a flexible, continuous function of a school's number of valid test scores. The coefficient of interest is δ_3, the average causal effect of information about school quality on enrollment at the threshold. Identification relies on the assumption that the underlying, potentially endogenous relationship between school outcomes and the number of valid test scores is fully captured by the flexible function $m(\cdot)$.

As suggested by Imbens and Lemieux (2008), I estimate the discontinuity using local linear regressions with rectangular kernel weights, robust standard errors, and a bandwidth of fifty valid test scores and show that these estimates are similar for a wide range of bandwidths. Additionally, I present estimates with standard errors clustered at the district level and estimates with standard errors clustered on the running variable as suggested by Lee and Card (2008).

In this setting, the fuzzy RD design estimates a local average treatment effect that is also the effect of treatment on the treated, because no school below the threshold can be labeled low performing, and therefore schools can not be either a "defier" or an "always-taker" Thus, the local

[16] In Section 5 I present evidence that issues like non-random sorting do not cause school characteristics to be related to being labeled low-performing.

average treatment effect contains only "compliers," in this setting, and measures the difference in enrollment between schools that are low-performing and those that would have been labeled low-performing in the absence of the eligibility rule.

Results

I present the results of my analysis in several parts. I begin by testing the validity of the RD design and present the main results, the effect of negative signals of school quality on enrollment. I also present subsequent analysis for the effects by distance, effects on academic performance, and the effects on the income and racial composition of schools.

Tests of the Validity of the RD Approach

To overcome potential bias caused by the relationship between labelling and API, I focus on RD estimates that exploit the structure of the labelling process. These results are appealing because we can verify that observable characteristics, like API score, are smooth through the threshold.

Non-random sorting is the main concern in RD designs in which those who could be affected by the policy under consideration know the eligibility cutoff. However, non-random sorting is virtually impossible in this setting because a school's eligibility is determined before the threshold for eligibility was announced, and school administrators did not know that schools with fewer than 100 valid test scores would be ineligible for the Open Enrollment list.

While non-random sorting is not likely, there are other issues that could threaten the validity of the RD design. One possibility would be if policy makers chose 100 valid test scores as the cutoff because schools around this threshold vary substantially in the absence of the Open Enrollment program. To investigate this possibility, I perform a falsification test using school characteristics from the year before the Open Enrollment program was implemented to verify that school characteristics are smooth through the threshold.

Figure 4 shows the level of school enrollment in the 2009-2010 school year as a function of the number of valid test scores in 2009. This figure shows that school enrollment is smooth through the threshold, and therefore schools across the threshold had similar enrollment before the Open Enrollment program was implemented. The first row of Table 2 uses a local linear regression with a bandwidth of fifty valid test scores to estimate the effect of the Open Enrollment program on 2009-

2010 enrollment as a function of the number of valid test scores in 2009. Each column of Table 2 shows the estimated effect on enrollment for a particular year. These estimates are insignificant, suggesting that differences in enrollment across the threshold are not driven by pre-existing differences in enrollment for these schools.

Figure 5 shows several other observable characteristics in the 2009-2010 school year as a function of the number of valid test scores. This figure shows that these school characteristics are smooth through the threshold, and thus schools around the threshold have similar characteristics. Rows two and below of Table 2 use local linear regressions with a bandwidth of fifty valid test scores to estimate any discontinuities in observable characteristics in the 2009-2010 school year. These estimates show that there are no significant discontinuities in these characteristics, and therefore schools on each side of the cutoff have similar characteristics before the Open Enrollment program.

If sorting were a problem, we might expect to see a discontinuity in the distribution of schools at the cutoff, as a disproportionate number of schools would fall just below the threshold to avoid the low-performing label.[17] To investigate this possibility, Figure 6 shows the distribution of schools across the cutoff using three different bin sizes. This figure shows that the distribution of schools is smooth through the cutoff. Using each of these cells as an observation, the Table 3 shows estimates from local linear regressions with rectangular kernel weights and a bandwidth of 50.[18] The estimated discontinuity is not significant, and the distribution of schools is smooth across the cutoff.

As a whole, these tests support the validity of the research design. School enrollment, ethnicity, test scores, pupil teacher ratios and free or reduced lunch eligibility before the Open Enrollment program was implemented are all smooth through the cutoff. As such, the changes in school outcomes across the threshold presented in subsequent sections can be attributed to the Open Enrollment program's negative signals of school quality.

The Low-Performing Label and School Enrollment

As discussed in Section 4, the identification strategy uses the valid test score threshold to instrument for the low-performing label. Figure 3 shows the assignment of the low-performing school label as a function of the number of valid test scores. As discussed in Section 2, this figure

[17]If schools prefer to be labeled low-performing and sorting is present, then we would expect to see a disproportionate number of schools just above the threshold.

[18]This is similar to the test proposed by McCrary (2008)

shows that the Open Enrollment program does not label any school with less than 100 valid test scores as low-performing. It also shows that the Open Enrollment program labels a sizeable share of the schools with 100 or more valid test scores low-performing.

Panel A of Table 4 presents estimates of the effect of the eligibility threshold on the probability that the Open Enrollment program labeled a school as low performing using Equation (1). Each entry presents the estimated discontinuity in the probability of labeling at the threshold. Across the four columns, the table shows a range of functional form choices and the columns present estimates for various bandwidths. Overall, these estimates suggests that about 44% to 65% of the schools with just under 100 valid test scores are prevented from being labeled low-performing. Therefore, the valid test score cutoff causes substantial variation in the labeling of schools as low-performing. As shown in the previous section, this variation is not driven by non-random sorting or pre-existing differences. This suggests that differences in outcomes across the cutoff can be attributed to signals of school quality. The remainder of this chapter is motivated by the notion that if parents respond to information about school quality, then we may see different levels of enrollment for schools on each side of the threshold.

Panels A and B of Figure 7 show enrollment as a function of the number of valid test scores in 2009. Panel A shows enrollment for all years prior to the program's implementation in 2010 and visual inspection of panel suggests that enrollment is very similar across the threshold before the program is implemented. Panel B suggests a discontinuous change in enrollment, after the program is implemented, for schools that are labelled with positive probability. In panel B of Table 4, I investigate the potential effects on enrollment by estimating effects with OLS and instrumenting for the low-performing label with the 100 valid test score threshold. For both OLS and IV, the left column shows the estimated effect on enrollment prior to the program's implementation in 2010, while the right column shows the estimated effect on enrollment after the program's implementation. OLS estimates suggest a reduction in enrollment of around 36 to 44 students after the program is implemented. The IV estimates suggest a decrease in enrollment somewhere between 78 to 99. While the placebo effects are not significant for any model, they are fairly large compared to the treatment effect, and all estimates are fairly noisy. Using the change in enrollment is an appealing way to potentially reduce the noise in these estimates.

Panels A, B and C of Figure 8 show the difference in school enrollment from 2010 to 2011, 2010 to 2012, and 2010 to 2013 as a function of the number of valid test scores. Panel A shows that the initial effect of school quality information on enrollment between 2011 and 2010 is large, but somewhat noisy. Schools to the right of the cutoff have fewer students than in 2010 while schools to the left of the threshold have grown since 2010. This suggests that the signals of school quality cause changes in school enrollment. Furthermore, these changes are more pronounced in 2012 and 2013. Panel B shows that schools to the right of the cutoff have lose more students than schools to the right of the cutoff in the second year of the Open Enrollment program. Panel C of Figure 8 shows a similar effect on the change in enrollment from 2010 to 2013.

Panel A of Table 5 presents estimates of the effect of signal of school quality on the change in enrollment from 2012 and 2010. Column (1) presents estimates for the effect of information across all schools, including schools that are not labeled low-performing. This estimate suggests that average enrollment was reduced by 24.03 students. Column (2) shows that this estimate is similar when controlling for school characteristics.[19]

In columns (3) and (4), I use the eligibility rule as an instrument for the low-performing label. As discussed in section 4, these estimates are a local average treatment effect that is also the effect of treatment on the treated. The estimate in Column (3) suggests that labeling a school as low-performing decreases enrollment by 54.7 students. Column (4), which includes controls for school characteristics, yields a similar estimate. For schools near the threshold, this represents 23.6% change in enrollment from the 2009-2010 school year.[20]

Panels B and C of Table 5 present estimates for changes in enrollment from 2010 to 2011 and for changes in enrollment from 2011 to 2012. Panel B presents estimates for the effect of signals of school quality on the difference in enrollment from 2011 and 2010. These point estimates are smaller than the overall change in enrollment, which suggests that some families take more than one year to transfer in response the signal of low school quality. However, these estimates are also more than half of the two year estimates, suggesting that the majority of transfers occurs in the first year. For schools near the threshold, this represents a 14.3% change in enrollment from the 2009-2010 school

[19]These characteristics include test scores, percent of white and Hispanic students and free or reduced lunch eligibility.

[20]Recall that the estimates are a measure of treatment on the treated. For the same level of valid test scores, treatment schools tend to be larger than the average school. In 2010, low-performing schools near the threshold had an average enrollment of 231.71.

year. Panel C uses the assignment of low-performing labels in the 2011-2012 school year that are based on 2010 valid test scores to estimate the one year change in enrollment from 2012 and 2011. These estimates are similar in magnitude to those estimated in Panel B, suggesting that the short run change in enrollment is similar in both implementations of the policy.

Figure 9 explores the sensitivity of the estimated treatment effect to different choices of bandwidth. This figure shows that the point estimates from Table 5 are quite robust across different bandwidths, and the bandwidth of 50 chosen for Table 6 yields an estimate that is very similar to estimates based on larger or smaller bandwidths.

It is important to note that the effect on enrollment does not necessarily correspond to the number of transfers caused by the negative signal of school quality because I observe school level enrollment and my results suggest that there is a net effect on enrollment. This net effect will overstate the number of transfers if students at treated schools transfer into schools below the eligibility threshold, and furthermore, students may have a higher probability of choosing to transfer to these schools if they avoid schools that are identified as low-performing. At a maximum, the net difference could only overstate the number of transfers by a factor of 2.[21]

Open Enrollment and Transfers Between Districts

It is crucial for the interpretation of the main results that information about school quality is causing changes in enrollment and not access to new transfer options. As discussed in Section 2, the Open Enrollment program provides only one new transfer option: students who attend a low-performing school can transfer to a school outside their current school district if the school they transfer into has a higher API score. In this section, I present results that suggest that families do not use this transfer option.

To illustrate, panel A of Figure 10 shows the estimated treatment effect for schools that have a transfer option that is closer than 3 miles and the estimated effect for schools where the closest transfer option is more than 3 miles away. This figure suggests that schools with options within 3 miles are affected by the signals of school quality while schools that have no option within 3 miles are unaffected.

[21] If there is no effect, then there would be no transfers out of low-performing schools and we would observe no reduced form effect. If students only choose to transfer into schools with less than 100 valid test scores, then each student would be counted twice, once as a transfer out and again as an incoming transfer.

Figure 11 shows the estimates of the effect on the change in enrollment from 2010 to 2012 as the distance to the closest transfer option increases.[22] This figure suggests that when the distance between schools is 1.2 miles or more, the OE program has no effect on enrollment. Intuitively, I find that the effect of the Open Enrollment program declines as the distance to a transfer school increases.

The average distance between a school labeled low-performing and the closest school outside the district with a higher API score is 6.5 miles. Furthermore, only 3% of the schools in the sample have this type of transfer option within 1.2 miles. Furthermore, Figure 9 shows that there is no statistically significant effect on enrollment for schools that are more than 1.2 miles apart. It is highly unlikely that transfers between districts are responsible for the estimated effect on enrollment and the Open Enrollment program does not effect enrollment by allowing a new type of transfer.

Open Enrollment and Average Test Scores

The primary goal of school-choice programs is to encourage schools to compete to attract students by improving academic performance. Several studies have focused on estimating the gains in academic achievement for students that transfer (Cullen, Jacob and Levitt (2006); Hastings and Weinstein (2008)) while others such as Bifulco, Ladd and Ross (2009) examine outcomes for students that do not transfer. The main results presented in the previous section suggests that information about school quality causes changes in enrollment, but it is not clear how information about school quality will affect academic performance at low-performing schools.

In theory, negative signals of school quality could either increase or decrease academic achievement at low-performing schools. For example, these signals may improve average test scores if these schools avoid being labelled low-performing in the following year by improving student test scores relative to other schools. However, the signals of low school quality could also reduce average test scores at low-performing schools if students who are induced to transfer by the signals of low school quality tend to have higher test scores than the students that are not induced to transfer.

In Figure 12, I explore this issue by examining the STAR test scores of elementary, middle and high schools as functions of valid test scores. Test scores have significantly more noise compared to other characteristics so far, so to visually compare outcomes, Figure 12 has larger bins and includes

[22]The black line shows the point estimate from a local linear regression using a bandwidth of 35. This smaller bandwidth is less sensitive to the curvature in enrollment caused by restricting the sample as seen in Figure 8.

a wider range of valid test score values. Visual inspection of math and reading scores in elementary schools suggests that average test performance falls in response to signals of low-school quality while middle and high school test scores are not affected. Similarly, in Table 6 I present estimates of the effects on student achievement. Each entry shows the estimated effect of signals of quality on math and reading scores. The first columns shows effects for 2008-2010, before the Open Enrollment program is implemented. If the identification strategy is valid in this setting, then there should be no significant effects for any of these estimates. The second column shows estimated effects after the program is implemented in 2011. Signals of school quality reduce performance in elementary schools, but have no significant affect on middle or high school test scores.

In the previous results, I cannot control for changes in student composition, which may drive part of this effect. While student transfers are not observable in the data, academic performance tends to be strongly related to both race and income. If transfer students tend to be either high or low income, we would also expect to see a change in the racial composition and income distribution of schools.

Effects of Information about Quality on Income and Racial Composition

Recent work finds that school choice programs can drastically change the demographic composition of schools. In particular, many papers focusing on how school-choice impacts the income and racial composition of schools find that higher-income students are more likely to use school-choice programs to transfer than lower-income students (see Epple, Figlio, and Romano, (2004); Fairlee (2006); Figlio and Stone (2001); Long and Toma (1988); Lankford, Lee, and Wyckoff (1995)). Similarly, signals of school quality could change the average family income of students at a school if higher-income students are especially responsive. To examine how information about school quality affects the average income level of students, I consider the fraction of students who are eligible for free and reduced lunch eligibility.

Figure 13 shows the fraction of students in the school who qualify for free or reduced lunch as a function of valid test scores. Using free or reduced lunch eligibility as a proxy for income, this figure suggests that there is no effect of information about school quality on the income distribution of students. The first row of Table 7 shows the estimate of the potential reduced form effect of information about school quality on the percent of students who qualify for free or reduced lunch.

This estimate is insignificant and suggest that negative signals of school quality do not affect the proportion of students who are eligible for free and reduced lunch.

In addition to school-choice research on the distribution of income, studies regularly find that school-choice programs contribute to racial segregation. For instance, Bifulco and Ladd (2007) find that North Carolina charter schools "increased the racial isolation of both black and white students, and has widened the achievement gap." If signals of school quality disproportionately cause white students to transfer, then we would expect to see a smaller proportion of white students in low-performing schools. To investigate how information about school quality affects racial segregation, I examine the racial composition of schools across the cutoff.

Figure 13 shows the percent of White, Hispanic and Black students in each school as a function of valid test scores. This figure suggests that there is no effect of information about school quality on racial composition. The last three rows of Table 7 show estimates of the reduced form effects on the percent of White, Hispanic and other ethnicities. These estimates suggest that information about school quality does not disproportionately cause white students to transfer.[23]

In general, the results in Table 7 suggest that the demographics of schools are not effected by the signals of school quality.

Conclusion

In this paper I identify the effects of signals of low school quality on enrollment using an eligibility threshold that prevents schools below the threshold from being labeled low performing. I estimate that negative signals of school quality cause enrollment to fall by 19.2%. This effect on enrollment suggests that a lack of school quality information prevents many families from using school-choice programs. Thus, school-choice programs may fail to create competition between schools to attract students if families are not provided with school quality information. However, school quality information should be provided with caution, as it can cause very large changes in enrollment.

While information about school quality causes large changes in enrollment, I find no evidence that it affects the demographics of schools and some evidence that student achievement falls in elementary schools. To the extent to which academic performance is related to ethnicity and income,

[23]It is important to note that this test does not rule out a special case of sorting where the students who do not transfer to higher performing schools (and lower total enrollment) sort into schools of similar ethnicities in a way that preserves the percent of ethnicity *across schools*. However, this special case is very unlikely.

this suggests that student transfers are not driving the academic performance of schools, and that the Open Enrollment Program reduced the achievement of students in labelled schools.

CHAPTER III

BUY THE BOOK? EVIDENCE ON THE EFFECT OF TEXTBOOK FUNDING ON STUDENT ACHIEVEMENT

Introduction

Evidence on the effect of school resources on student achievement is mixed. In a series of influential reviews, Hanusheck (1981, 1986, 2003) argues that most research on the education production function finds little evidence that improvements in pupil teacher ratios, teacher experience and teacher qualifications cause improvements in student achievement. In contrast, a growing body of literature uses experimental or quasi-experimental methods to provide evidence that school resources affect student achievement. Using these methods, Krueger (1999), Angrist and Levy (1999) and many others provide evidence that reducing pupil-teacher ratios increases student achievement.[1] Similarly, improvements in teacher quality and increasing instructional hours have been shown to improve student achievement (Rockoff, 2004; Hanushek, Kain, and Rivkin, 2005; Lee and Barro, 1997; Eren and Millimet, 2007; Hansen and Marcotte, 2010). However, experimental and quasi-experimental settings are rare and virtually no work has focused on capital-related inputs.[2] This paper seeks to fill part of this gap by analyzing the effect of textbook spending on student achievement using a quasi-experiment.

While researchers think that textbooks can affect student achievement in the right setting (for example see Hanushek, 1995), textbooks have remained unstudied in the U.S. for three reasons.[3] First, identifying the causal impact of a school input is difficult. If, for example, textbook shortages are associated with local poverty levels, then the fact that schools without textbooks have lower student achievement could actually reflect a causal relationship between poverty and student achievement. Second, there is virtually no data on the stocks of textbooks in schools, meaning even simple correlations between textbooks and student achievement can not be studied directly. Third,

[1]See also Ding and Lehrer (2010), Urquiola (2006), Dustmann, Rajah and Soest (2003), Jepsen and Rivkin (2009).

[2]Notable exceptions include the research on school facility investments; see Cellini, Ferreira, and Rothstein, (2010); Jones and Zimmer, (2001); and Schneider, (2002) and the research on classroom computers by Angrist and Lavy (2002b).

[3]Most work textbooks and student achievement come from developing countries. See Glewwe, Kremer, and Moulin, 2009; Heyneman, Jamison, and Montenegro, (1984); Jamison, Searle, Galda, and Heyneman, (1981).

and perhaps most significantly, researchers believe that shortages of textbooks are not common in the U.S. because the cost of textbooks are a small portion of total spending on education.

If textbooks affect education, then the reported shortages of textbooks in the U.S. are cause for concern. California has experienced several reports of textbook shortages in 1996, 2000 and 2010 (California Community Foundation, 1998; Oakes, 2004; San Francisco Examiner, 2010). Textbook shortages in 2000 were severe enough to motivate students in California schools to file a large lawsuit against the state. Furthermore, textbook shortages are not limited to California. New York City schools experienced sharp declines in textbook funding and corresponding shortages in textbooks in following years.[4] In 1997, 24 percent of New York teachers reported that they could not assign homework because of a lack of textbooks and 21 percent indicated that their classes are disrupted because students had to share textbooks in class.[5] Similar issues have been reported in the Houston Independent School District (Houston Chronicle, 2012). Schools in Denver, Detroit, New Orleans, Indianapolis and Rochester have also reported "serious problems" with providing textbooks to students (Prescott Courier, 1988). These reports suggest that textbook shortages are a common and reoccurring problem in many major school districts throughout the U.S., yet no one has estimated the impact these shortages have on student achievement.

Textbooks may affect student achievement through several mechanisms. When reporting shortages, teachers tend to emphasize that textbooks enable a student to complete homework. Descriptive evidence suggests that teachers assign less homework when their students do not have textbooks to take home and experimental evidence suggests that homework improves student achievement (Paschal, Weistein and Walberg, 1984).[6] Therefore a shortage of textbooks may affect student achievement by reducing the amount of homework assigned to students. Additionally, because textbooks facilitate study outside of the classroom, it is likely that the presence of textbooks may have an effect similar to increasing instructional hours in the classroom, an input that is known to increase student achievement. Furthermore, Houtenville and Conway (2007) suggest that parental involvement has large effects on student achievement and if parents use textbooks to help their children learn at home, then textbook shortages may reduce the effectiveness of parental effort.

[4]NYS Division of the Budget, 2004.

[5]Stringer,(2002)

[6]Descriptive evidence comes from teacher and student testimony in the Williams Case.

To my knowledge, this is the first paper to estimate the causal effect of textbook funding on student achievement in the U.S. I estimate these effects by exploiting a quasi-experiment generated by a large lawsuit settlement in California. As part of the Williams Settlement, the state allocated a one-time payment of $96.90 per student for textbooks to schools with average test scores below a threshold. My identification strategy leverages this sharp cutoff for textbook funding for a panel of California public schools over a period of 8 years (2002-2009).

A key part of this analysis is to verify that textbook funding affects student performance through the purchase of additional textbooks and not through changes in other school inputs. I find no evidence that the additional textbook funding causes fiscal substitution, i.e., that it was used to replace existing textbook funding, by estimating the effects of this funding on other school inputs. Additionally, I use detailed financial records for school districts to show that the Williams funding significantly affected district level spending on textbooks.

The analysis reveals that textbook funding significantly increases standardized test scores for math and reading. This effect is strongest for young children in elementary schools. My preferred estimates indicate that a one-time increase in funding of $96.90 per student improves student test scores by 0.14 standard deviations.[7] Furthermore, this estimate is robust across a number of different specifications and corresponds to the timing of the states disbursal of textbook funding. While the magnitude of this estimate is comparable to estimates of other education interventions, the textbook intervention had a very high benefit-per-dollar ratio.

This paper is related to a large literature examining the effect of litigation on school finance and student achievement. Lawsuits are primarily responsible for changes in state education finance systems and many studies find that litigation raises district resources and reduces funding inequality (see Corcoran and Evans, (2007); Springer et al. (2009); Sims, (2011)) but also find mixed evidence of the effect of these resources on student achievement (positive effects are found by Card and Payne, (2002); Downs and Figlio, (1998), and negative or zero effects are found by Hoxby, (2001) and Husted and Kenny, (1997)). This paper provides evidence that the Williams Case increased school resources for textbooks and also improved student achievement which suggests that increases in school finance that target particular school inputs may be more successful than increases in general funding.

[7] In terms of books per student, an additional textbook raises test scores by 0.07 standard deviations.

This paper also speaks to growing concerns about the ability to implement large scale policies and achieve outcomes similar to small scale quasi-experiments. For example, Jepsen and Rivkin (2009) provide evidence that the large scale class size reduction program in California resulted in unanticipated tradeoffs between class size reduction and teacher quality. Similarly, Sims (2008) suggests that California schools strategically structured classes to receive cash payments for class size reduction. Both of these responses result in smaller effects for reducing class size than small scale quasi-experiments suggest. In contrast, this paper uses variation in textbook funding that affected 20 percent of the schools in California and suggests that a large scale policy for textbook funding can impact student achievement.

The remainder of this paper proceeds as follows. In Section 2, I discuss the quasi-experiment that provided textbook funding. I describe the data and empirical methodology in Section 3. In Section 4, I present the main results for the analysis of textbook funding on student achievement and investigate potential mechanisms for the estimated effects. Finally, I conclude in Section 5.

Background on the Williams Settlement

This study provides evidence on the effects of textbook funding by focusing on a quasi-experiment generated by Eliezer Williams, et al., vs. State of California (commonly referred to as Williams case), which was a class action lawsuit filed on May 17, 2000. Plaintiffs testified that conditions were very poor in the 72 public schools involved in the Williams case. These schools lacked textbooks, qualified instructors, and safely maintained buildings.

Evidence from the trial suggests that textbooks were in very short supply in all schools named in the lawsuit. Most teachers only had a classroom set, i.e. one set of books for all of a teacher's students, which required multiple students to share the same textbook in class. This lack of textbooks also prevented students from taking textbooks home with them, and several teachers reported that they had to assign less homework as a result. Testimony from students noted that most students shared textbooks in class, sometimes with three to four students per book. The condition of the books was notably poor and significantly outdated. For example, the social studies text that Luther Burbank Middle school students used was so old that it did not reflect the breakup of the former Soviet Union.

The state of California agreed to a settlement in 2004. The settlement established a new standard that "each pupil, including English learners, has a textbook or instructional materials, or both, to use in class and to take home to complete required homework assignments."[8] The state also provided two sources of funding to help schools meet these new standards for textbooks. The first source of funding was the Instructional Materials Fund, which provided all schools with an annual fund for textbooks. Although the program was scheduled to end in 2006, the Williams settlement continued the program with an allocation of $380.3 million dollars. This increased funding for textbooks from $25 per student to $54.22 per student. The second fund, called Instructional Materials–Williams Case (IMWC), was designed to provide additional support for low-performing schools. In addition to the $54.22 per student that every school received, $138 million was allocated to low-performing schools for textbooks.

This paper focuses on the $138 million in IMWC funding that was distributed to low-performing schools. In particular, the settlement used a sharp cutoff for eligibility that provides potentially exogenous variation in the amount of textbook funding for schools. The cutoff restricted IMWC funding to schools in the first two deciles of the academic performance index (API) in 2003. For each type of school—elementary, middle and high school—a particular API score is chosen as the upper limit for each decile. For example, in 2003, all elementary schools with an API score of 643 or less were within the first or second decile. Thus, a school's API score precisely determined if the school received IMWC funding. Importantly, the Williams settlement did not use this threshold to allocate other types of funding for monitoring, building repair, or other services.[9]

The Williams settlement provides a unique opportunity to examine the effects of textbook funding in a setting where the spending of this money was highly monitored. The Department of Education adopted two oversight policies to ensure that the new textbook standard was met. First, the Uniform Complaint Process allowed students, parent, teachers and others to submit complaints about textbook insufficiencies and appeal if the complainant was not satisfied. Second, low-performing schools were visited annually by the County Superintendent for an inspection of the stock of textbooks. If the school failed to meet the standards for textbooks, then they had to follow a series of steps to purchase the required books. In particular, the Department of Education required

[8]SB 550, section 18, ECS 60119(c).

[9]If eligibility for other types of funding, such as facility repair funds, also depended on this threshold, then the mechanism for improvements in student achievement would be ambiguous.

county oversight for schools within the first three deciles.[10] The county office staff visited schools annually to determine if students had a textbook to use in class and take home for every subject. The staff visits were very thorough; the ACLU estimates that around 98 percent of schools in the first three deciles were visited by county office staff. These visits make fiscal substitution particularly unlikely because any missing textbooks must be replaced by the school within a certain amount of time. However, if the school has no shortages of textbooks *before* the funding is distributed, then the inspections will not guarantee that textbook funding does not cause fiscal substitution. To investigate this potential issue, I focus on financial records for school districts.

In California, school districts are the financially responsible entity within the educational system and the state records all finances at the district level. In contrast, the state determined eligibility for IMWC funding for each *school*. In Panel A of Figure 14, I examine how IMWC funding affected textbook spending by comparing school districts with no qualifying schools to districts with at least one qualifying school. The vertical axis shows average district spending on textbooks per student and the vertical line indicates that IMWC funding was distributed in 2005. Before 2005, districts with no eligible schools spend very similar amounts on textbooks compared to districts with at least one eligible school. After IMWC textbook funding was distributed, districts with at least one eligible school have persistently higher spending on textbooks relative to districts with no eligible schools. This indicates IMWC textbook funding affected textbook spending. I further test for fiscal substitution by using cross sectional variation in school level characteristics. I examine if IMWC textbook funding affected other school inputs, such as pupil teacher ratios, staff FTE and teacher experience. Additionally, textbooks are relatively inexpensive compared to other school interventions. If IMWC funds caused fiscal substitution and the hiring of more teachers, it would only reduce average class size by around a quarter of a student. Previous literature suggests that class size reduction would have an implausibly large effect in this setting.

Figure 14 also illustrates that the state did not impose time constraints on the use of IMWC funds. Approximately 65 percent ($90 million) of the IMWC funding is spent in the first three years and about 10 percent is spent between 2008 and 2010. For this reason, I estimate the combined effect over the sample period as well as the effects on student achievement for each year.

[10]The cutoff for oversight does not coincide the cutoff for IMWC funding.

Data and Empirical Strategy

The data for this paper are the universe of public schools in California with the exception of charter schools and alternative schools which were unaffected by the Williams Settlement. For each school, from 2002 to 2009, the data contains yearly records of detailed school characteristics. In particular, I focus on five categories of data for these schools: Standardized testing in California provides test scores to measure student achievement; API data determines school eligibility for additional textbook funding; race and enrollment data are provided by the California Department of Education; the Common Core of Data provides the fraction of students who are eligible for free and reduced student lunch; and staff data and district financial data also comes from the California Department of Education.

Student Achievement and Standardized tests

I measure student achievement with standardized test scores from California's Standardized Testing and Reporting (STAR) program. STAR tests are taken by students annually in either April or May, depending on the start date of the school. There are four types of tests in the STAR program: the California Standards (CST), the California Achievement Test (CAT/9), the California Alternate Performance Assessment (CAPA) and the Spanish Assessment of Basic Education (SABE). I focus on the CST to measure student achievement because all students (with the exception of students with disabilities) are required to take it. The other STAR tests are taken by fewer students: the SABE tests Spanish-speaking English learners and these students are required to take the CST in English as well; the CAT/9 only tests grades 3 and 11 for the 2002 to 2009 time period, while the CST tests grades 2 through 11.

The CST covers English-language arts (referred to as reading), mathematics, science, and history-social science for second through eleventh grade. The California Department of Education reports mean-scale scores for schools in addition to the percent of students within the school that meet performance criteria: far below basic, below basic, basic, proficient, and advanced. I modify these categories to be cumulative–e.g., percent of students at or below the advanced standard, percent at or below basic performance– so that the direction of the predicted effects is clear. I use the mean-scale scores to measure school level improvements and the percent meeting performance standards to examine how the distribution of student ability changes within the school.

I normalize the mean-scale scores to a mean of zero and a standard deviation of one to compare my results to other school interventions. To measure overall achievement, I average math and reading scores (called average score).

API, Textbook Funding, and Fiscal Substitution

The most important data topic in this paper is to determine how textbook funding is distributed to schools, which is determined by California's API score. A school's API score is a weighted average of test scores from the STAR program and it is used to measure a school's accountability to Adequate Yearly Progress (AYP) of the No Child Left Behind program with a API of 800 being the target performance level. It is also used to roughly compare a school's academic performance relative to other schools for California programs (such as the California Open Enrollment program examined in Holden (2013)). The Williams Settlement used API scores in 2003 to allocate funding for textbooks to "low-performing" schools.

The Williams settlement distributed textbook funding as a deterministic function of California's Academic Performance Index in 2003. All schools received a base amount of $54.22 per student for textbooks each year, and elementary, middle and high schools with API scores at or below 643, 600, and 584, respectively, receive an additional one-time payment of $96.90 per student. I merge the 2003 API file to the STAR test score data so that schools have the same API score for each year of STAR test data.

I use data from the California Department of Education's API files to identify each school's API score relative to the cutoff for funding. The probability that a school qualifies for additional funding is a deterministic function of API score in 2003:

$$FundingPerStudent_{i2005} = \begin{cases} \$54.22 + \$96.90 & \text{if } APINORM_{i2003} \leq 0 \\ \$54.22 & \text{if } APINORM_{i2003} > 0 \end{cases} \tag{3.1}$$

where I define $APINORM_{i2003}$ as the distance between school i's API score and the API cutoff for the 20th percentile.[11] To construct the "first stage" of the funding for textbooks to schools, I use

[11]This cutoff is 643 for elementary schools, 600 for middle schools and 584 for high schools

publically available data from Los Angeles County schools.[12] In section 4, I use this data to show how funding was distributed and that the API threshold was strictly enforced.

A key part of this analysis relies on verifying that IMWC funding did not cause fiscal substitution and I use two data sets to investigate this issue. First, I use the Standardized Account Code Structure (SACS) to identify district level spending on textbooks. Second, I use a rich set of school characteristics to investigate the possibility of fiscal substitution. In particular, I examine full-time equivalency (FTE), experience, qualifications and overtime for teachers, administrators and pupil-service staff.

Estimating Effects on Student Achievement

I use the "sharp" RD implied by (1) to estimate effects of textbook funding on student outcomes at the eligibility cutoff (Trochim, 1984). I use the following regression equation to estimate the effects of funding for textbooks on school outcomes:

$$TestScore_{ist} = \alpha + \delta * 1(APINORM_{i2003} \leq 0) + m(APINORM_{i2003}) + u_{it} \tag{3.2}$$

where $m(\cdot)$ is a flexible, continuous function of a school's normalized API score in 2003. The coefficient of interest is δ, the estimated impact of textbook funding.[13] As mentioned previously, school districts spend the funding at different points in time. As such, my estimates can be interpreted as a sharp intent to treat effect and I estimate equation (2) separately for each year following the settlement.

One practical issue is how to model $m(\cdot)$. As suggested by Imbens and Lemieux (2008), I estimate the discontinuity using local linear regressions with rectangular kernel weights and heteroskedasticity-robust standard errors and show that these estimates are similar for a wide range of bandwidths.[14] In Appendix A, I present similar results for local quadratic regressions and controls for observable school characteristics. Additionally, given the discrete nature of API scores, I present

[12] Data on IMWC funding is not available for all California schools. The CDE is willing provide this data, however, they have not located the records as of this date.

[13] Positive estimates of δ correspond to improvements in performance.

[14] My regression equation is given by $TestScore_{it} = \alpha + \delta * 1(APINORM_{i2003} \leq 0) + \beta * APINORM_{i2003} + \gamma * APINORM_{i2003} * 1(APINORM_{i2003} \leq 0) + u_{it}$ and restricted to a bandwidth of 15 API above and below the cutoff.

estimates with standard errors clustered on the running variable as suggested by Lee and Card (2008) and estimates with standard errors clustered by school district.

My empirical approach is motivated by the idea that schools with API scores just above the cutoff provide a good counterfactual for schools with API scores just below the cutoff. More precisely, identification relies on the assumption that school characteristics should be smooth through the cutoff (Porter, 2003). This assumption is plausible, as the bill that introduced the cutoff was proposed on August 24, 2004 and 2003 API scores were determined from tests taken in spring of 2003. Thus, it is unlikely that the cutoff was known before API in 2003 was determined. However, as it is possible that information about the cutoff was available before the bill was introduced, I investigate the smoothness of observable school characteristics through the threshold. The conclusion from this analysis is that my estimates for textbook funding are valid RD estimates.

The identification strategy provides estimates that are local to the eligibility threshold, and in this setting, schools that are near the cutoff are important for policy because they are regularly targeted for academic improvement. Table 8 compares the full sample of California schools to schools within a 15 API bandwidth of the cutoff. The first three rows show that the cutoff was chosen to provide resources for low-performing schools; particularly, schools near the threshold have lower math and reading scores as well as lower API scores. As we may expect, these low-performing schools tend to have higher enrollment and more Hispanic students than the average California school. Additionally, there are more students who are eligible for free or reduced school lunch. This high fraction of FRLP eligibility suggests that schools near the threshold tend to be in higher poverty areas, suggesting a correlation between low-performance and poverty. The larger schools require more teaching, administrative and pupil-service staff, and have slightly larger class sizes.

Results

I present the results of my analysis in three parts. I begin by examining the assignment of textbook funding to schools and the corresponding effects on textbook spending. Next, I present the main results, the effect of textbook funding on student achievement. Lastly, I examine the validity of the RD design in this setting and explore potential mechanisms for improvements in student achievement.

Assignment of Textbook Funding

I begin by showing the effect of the API cutoff on the allocation of textbook funding to school districts. Panel B of Figure 14 shows textbook funding from the Williams Case in Los Angeles County as a function of API in 2003. Consistent with the program's description in Section 2, there is a sharp change in the allocation of textbook funding across the cutoff. Schools at or below their API cutoff receive the IMWC one-time payment of $96.90 per student. The Williams Settlement also allocated $54.22 of textbook spending per student each year for all schools, regardless of API score in 2003. Of the 1,509 schools in Los Angeles County, only one school did not receive funding according to the schedule, which suggests that compliance was very high.

Next, I investigate the effect on textbook spending. In Panel A of Figure 14, I examine aggregate textbook spending for two types of districts: the solid line shows spending on textbooks for districts with at least one school that qualified for IMWC textbook funding and the dashed line shows spending on textbooks for districts with no qualifying schools and therefore should not be affected by IMWC textbook funding. Before the funding was distributed in 2005, both types of districts have similar trends in textbook spending. After the funding was distributed, districts with at least one qualifying school had persistently higher textbook spending relative to districts that have no qualifying schools. Therefore, IMWC funding has an effect on textbook spending. Also note that the difference in spending declines over time as schools spend their one-time payment.

Main Results: Student Achievement in Elementary Schools

In Table 9 and Figure 15, I present the main results for the effect of textbook funding on student achievement. In Table 9, each row presents estimates for a different dependent variable measuring student achievement.[15] The estimate in Row (1) and Column (1) indicates that textbook funding improves average test scores by 4.48 points in the five years following the distribution of textbook funding. The second and third rows show estimates with similar magnitudes for math and reading scores. Visual inspection of Figure 15 suggests corresponding effects for average test scores, math scores, and reading scores as a function of API in 2003.

[15] Each column presents estimates for a different year. All effects are estimated with local linear regressions with a bandwidth of 15 API and heteroskedasticity-robust standard errors. Estimates from flexible quadratic regressions, available in Appendix A, provide similar estimates with slightly larger estimates for math and smaller estimates for reading.

As mentioned in Section 2, spending of the one-time payment is concentrated in the first three years following the Williams Settlement. For this reason, I also examine the effects on student achievement over time. In Figure 16 and Columns (2) - (5) of Table 9 I show the effect on student achievement over time. The estimates in Column (2) indicate an effect on student achievement in the first year that is statistically significant at the 5 percent level for average scores. The estimated effect increases to 5.62 in 2006 and peaks at 5.87 in 2007 before declining in 2008 and 2009. While the effect on reading is statistically significant at the 5 percent level and the effect on math is not due to the considerable increase in standard errors for math scores in 2009.

Why do estimated effects fall off in 2008 and 2009? First, the pattern of effects on student achievement is similar to pattern the spending of additional textbook funding. Spending of the additional textbook funding is high in 2005, 2006 and 2007, and falls to around 5 percent in 2008 and 2009. This suggests that a constant source of funding of textbooks may be required to prevent large changes in the stock of textbooks. While detailed information on the loss rate of textbooks is not available, L.A. County has a general rule that 10 percent of the textbook stock will need to be replaced every year. Back of the envelope calculation suggests that around $16,000 would be needed each year to maintain the stock of textbooks. In fact, we may be concerned if a one-time payment for textbooks lead to persistent differences in test scores. Second, it may be that the marginal benefits of textbook funding may decrease quickly as the stock of textbooks increases. During this period, all schools receive an additional $54.22 each year for textbooks, and thus, schools that did not qualify for IMWC funding may be increasing their stock of textbooks as well. After four years, all schools may have sufficient numbers of textbooks, eliminating the advantage of IMWC textbook funding.

How large are these estimates compared to other school interventions? Two studies are ideal for comparing estimates of textbooks to class size reduction. Jepsen and Rivkin (2009) and Unlu (2005) estimate the effect of a massive class size reduction program for the same set of elementary schools in California. Jepsen and Rivkin (2009) find that a ten-student reduction in class size improved reading and math scores by 0.10 and 0.06 standard deviations of the school test distribution while Unlu finds larger estimates of 0.3 and 0.2 standard deviations. I find an effect of 0.14 standard deviations of improvement in student achievement. While both interventions have roughly similar effects on students, the class size reduction program exceeded 1.7 billion dollars (or $1,024 per pupil) annually while the entire IMWC program cost 138 million dollars (or $96.90 per pupil). In terms of benefit per \$1,000, class size reduction provides 0.16 standard deviations per \$1,000 while the textbook intervention provides a 1.4 standard deviations per \$1,000. If class size interventions are cost effective policies, this suggests that textbook interventions should also be implemented.

In Table 10 and Figure 17, I explore the distribution of the effect on student achievement by examining the percent of students that meet various performance standards. In Panel A, each column of Table 10 shows the estimated effect on the percent of students in the school that achieve the relevant performance standard. The estimate in Column (1) indicates that textbook funding reduces the percent of students that are far below the basic standard by 0.96 percent. Similarly, the estimate in Column (2) shows that 1.3 percent fewer students are classified as below the basic standard. The estimate in Column (5) indicates that textbook funding causes a 1.7 percent increase in students meeting the highest performance standard.[16] These estimates suggest that textbook interventions not only improve test performance for low-performing students, but they also increase the percent of high performing students in the school. Figure 17 shows corresponding scatter plots for the percent of students in each performance category as a function of API score in 2003 and visual inspection suggests similar results as Table 10.[17]

For robustness, Figure 18 shows the sensitivity of the estimated effect to different choices of bandwidth. This figure shows that the point estimates from Table 9 are quite robust across different bandwidths, and the bandwidth of 15 yields an estimate that is very similar to estimates based on larger or smaller bandwidths.

Main Results: Middle and High Schools

In Table 11, I present estimated effects on middle school and high school student achievement. Columns (1) - (5) show estimated effects on math scores and Columns (6) -(10) show estimated effects on reading scores. The estimates for middle schools in Panel A indicate there is no statistically significant effect on student achievement for any year. Point estimates for math scores fluctuate between positive and negative effects and have very large standard errors while reading scores are

[16]The predicted effect of textbook funding on intermediate performance categories is ambiguous. If textbooks improve student achievement, we may find a reduction in the percent of students in the basic category because these students are now proficient or advanced, or an increase in percent basic because of improvements in far below basic. Similarly, visual inspection of Figure 17 suggests a highly non-linear function in the advanced category, suggesting that there is no effect on the percent advanced.

[17]Panel B of Table 10 shows estimates for the percent of students at or below each performance standard. These estimates suggest that textbook funding reduces the percent of students at or below each performance category.

negative. For high schools, estimated effects are shown in Panel B. Again, there are no statistically significant effects for any year.

Why are there no effects for middle and high schools? Notably, there are far fewer middle and high schools compared to elementary schools. In California, there are roughly five times as many elementary schools than high schools or middle schools and the small sample size is further reduced when focusing on a bandwidth of observations near the cutoff.[18] As a result, the standard errors of the estimated effects are very large and we can not rule out economically significant effects on middle schools or high schools.

On the other hand, older students may not have benefited from textbook provision. This could occur for several reasons. Older students may not have developed the study habits necessary to use textbooks because they did not have access to textbooks in earlier grades. Teachers of older students may not adapt their curriculum to include new textbooks. Or, perhaps, middle schools and high schools place greater importance on maintaining the stock of textbooks and additional textbook funding did not increase student access to textbooks.

Validity and Mechanisms

Non-random sorting is the main concern in RD designs in which those who could be affected by the policy under consideration know the eligibility cutoff. In this case, non-random sorting would occur if schools just above the cutoff actively influenced their 2003 API score to receive additional textbook funding. Non-random sorting is unlikely in this setting because the individuals affected by the policy did not know the eligibility cutoff. In particular, the bill that introduced the cutoff was proposed on August 24, 2004, and API scores were determined from tests taken in the spring of 2003. However, information about the cutoff may have been spread before the announcement of the bill in 2004. Alternatively, policy makers may have chosen the 20th percentile as the cutoff because schools around this cutoff vary substantially in the absence of the Williams settlement. For these reasons, I investigate the identifying assumption that school characteristics are smooth through the threshold.

A key part of my analysis is to leverage the timing of the Williams Settlement. If the RD design is valid in this setting, student achievement and school characteristics should be smooth

[18] Several researchers note that RD designs have low statistical power; Deke and Dragoset, 2012, Schochet, 2008

through the cutoff before the before textbook funding is disbursed in 2005. In Table 12, I present the estimated effects on student achievement and school characteristics before and after textbook funding is disbursed in 2005. Each row shows an estimated effect for a different dependent variable. Column (1) shows effects over 2005 to 2009. Columns (2) - (4) show estimated effects before the Williams Settlement and Columns (5) - (9) show estimated effects after the Williams Settlement. In Panel A, I present the estimated effects on student achievement over time. In Columns (2) - (4), we can see that there is no effect on student achievement before textbook funding is allocated.[19] After funding is distributed in 2005 we find the positive effects on student achievement discussed in Table 9. Thus, the improvement in student achievement corresponds with the threshold for eligibility *and* the timing of textbook funding disbursal.

In Panel B of Table 12 and Figure 19, I show estimated effects on the composition of students, including enrollment, ethnicity and free or reduced lunch eligibility. Across all of the Columns, there are no statistically significant effects and student characteristics are smooth through the threshold before the settlement. While the smoothness of characteristics before the settlement speak to validity, effects after the settlement may speak to potential mechanisms. For example, the literature on school choice programs suggest that students are attracted to schools with high academic achievement (Hastings and Weinstein, 2003; Holden, 2013). Thus, elementary schools with higher student achievement may eventually have higher enrollment and changes in student characteristics. Estimated effects in Columns (5) - (9) suggest that elementary schools did not attract additional students or experience changes in student composition.

Panel C of Table 13 shows estimated effects on staff characteristics. Each row displays estimated effects on the Full-time equivalency (FTE) for the related staff type. Estimates in Columns (2) - (4) are not statistically significant which further supports the validity of the RD design in this setting. Estimates in Columns (5) - (9) are also not statistically significant, which suggests that textbook funding did not cause fiscal substitution.[20] Panel D presents estimates for years of experience and years in school district. Of the 80 coefficients of characteristics in Table 12 and Table

[19]California changed the reporting of scores in 2002 to mean scaled scores to make test outcomes comparable across years. I have also examined placebo effects for 2001, where the reported outcome is percent correct. Using an approximate scaling, 2001 test scores show similar sized point estimates to 2002 that are also insignificant.

[20]While statistically insignificant, the magnitude on teacher FTE may merit further discussion. In particular, it is unlikely that this caused by fiscal substitution because there is a similar magnitude before the settlement. Additionally, this difference in teacher FTE does not explain the timing of the effects on student achievement in Panel A.

13, only two are significant at the 10 percent level. Figure 20 shows FTE for teachers, administrators and pupil service staff as a function of API score. Visual inspection of these figures suggests that FTE is smooth through the threshold, with the possible exception of administrator FTE in 2007. However, administrator FTE is very noisy and there is no statistically significant effect. Additionally, administrator FTE can not explain positive effects on student performance in 2005 or 2006.

If non-random sorting was a problem, we would expect to see a discontinuity in the distribution of schools at the cutoff, as a disproportionate number of schools would fall just below the cutoff relative to the number of schools just above the cutoff. Figure 21 shows the distribution of schools around the cutoff as a function of API score in 2003. This figure shows that the distribution of schools is smooth through the cutoff. Using each of these cells as an observation, the Table 14 shows estimates from local linear regressions with rectangular kernel weights for various bandwidths and bin sizes.[21] The estimated discontinuity is not significant, and the distribution of schools is smooth across the cutoff.

As a whole, these tests support the validity of the research design. Pre-treatment test scores, student characteristics, staff characteristics, and the distribution of schools are all smooth through the cutoff. As such, the changes in school outcomes across the threshold presented in previous sections can be attributed to the funding for textbooks provided by the Williams Settlement.

Conclusion

In this paper, I identify the effect of textbook funding on student achievement by focusing on a quasi-experiment generated by the Williams Settlement. Exploiting an eligibility threshold for textbook funding, I use a RD design to estimate the effect of a one-time payment of $96.90 per student for textbook funding on student achievement. My findings suggest that textbook funding has a significant, positive affect on student achievement. I also find that the benefits of the one-time payment decline four years after funding is disbursed. Textbook funding improves student achievement at all performance levels and I find no evidence that textbook funding caused fiscal substitution.

My results also indicate that textbooks are a very cost-effective way to improve test scores when compared to class size interventions. The estimates from Kruger (1999) suggest that reducing

[21] This is similar to the test proposed by McCrary (2008)

class size leads to a 0.2 standard-deviation improvement in reading and math scores and costs approximately $3,501 per year for each student (Kruger (2003)). In comparison, textbooks improve test scores by 0.14 standard deviations and cost only $96.90 per year for each student. While both programs have roughly the same benefit, textbook provision appears to have a very high benefit-to-cost ratio.

The purpose of this paper, however, is not to discourage class size reductions. We still know very little about the mechanisms that lead textbooks to affect student achievement. In particular, the marginal benefit textbook interventions may be zero when students have a book to use in class and one to take home. Instead of suggesting substitution between school inputs, I view these results as suggesting that the benefit of providing textbooks appears to exceed the very low cost of provision. In fact, Kruger (2003) argues that the benefits of reducing class size are greater than the high costs when future earnings of students are calculated.

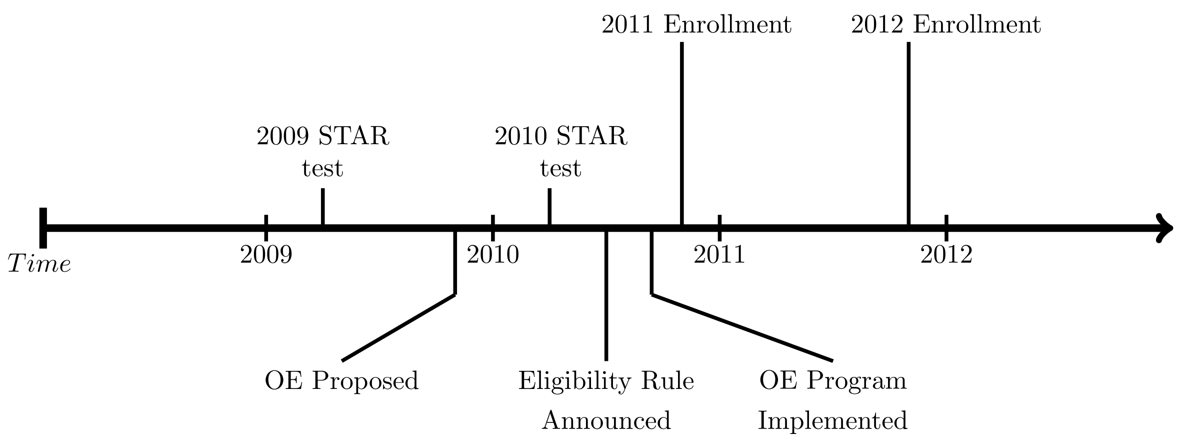
With such high spending per student, why are there textbook shortages in the U.S.? These shortages of textbooks appear to be closely related to fluctuations in textbook funding. Figure 22 shows the spending per pupil for textbooks and teachers in California. Textbook spending appears to be counter-cyclical and highly volatile compared to spending on teacher salaries. Following the decline in textbook funding, schools in San Diego school district reported they were missing tens of thousands of textbooks.

I view this paper as a first step towards understanding the effects of textbooks in the U.S. There is still much to learn about textbook interventions, including further study of the effects of textbook provision on middle and high schools. Additionally, schools considered in this paper are heavily monitored; further research can investigate if monitoring is an important policy to include with textbook provision.

APPENDIX A

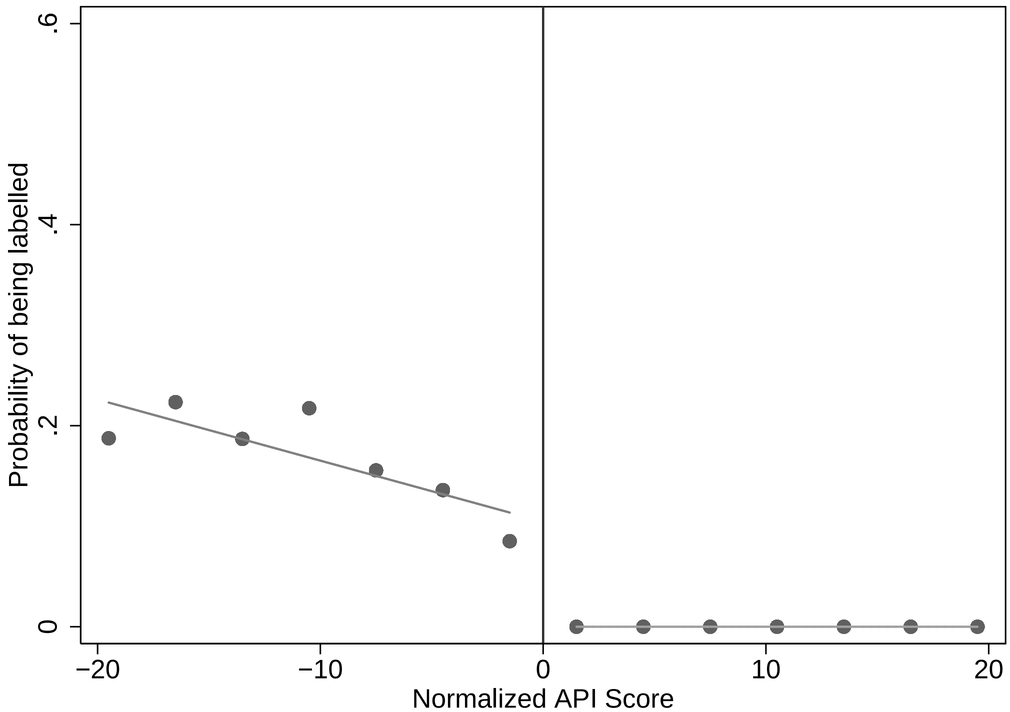
FIGURES AND TABLES

FIGURE 1. Timeline of the Open Enrollment Program



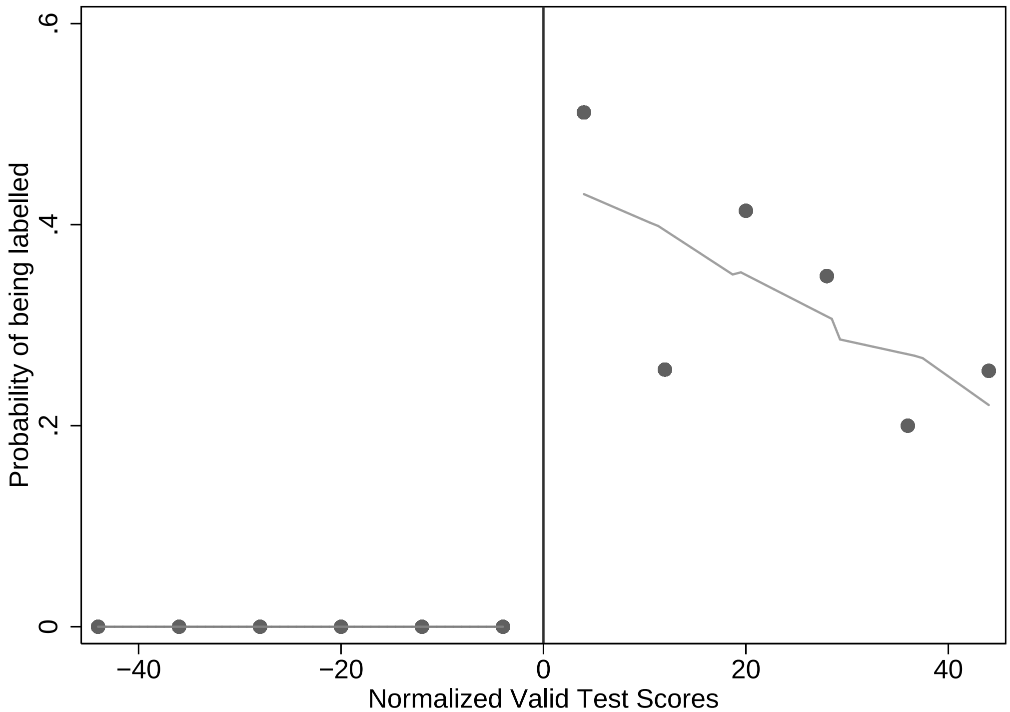
Notes: The 2009 STAR test determines the number of valid test scores and school eligibility in the Open Enrollment program for the 2010-2011 school year. Similarly, the 2010 STAR test determines the number of valid test scores and school eligibility in the Open Enrollment program for the 2011-2012 school year.

FIGURE 2. Probability of labeling by API



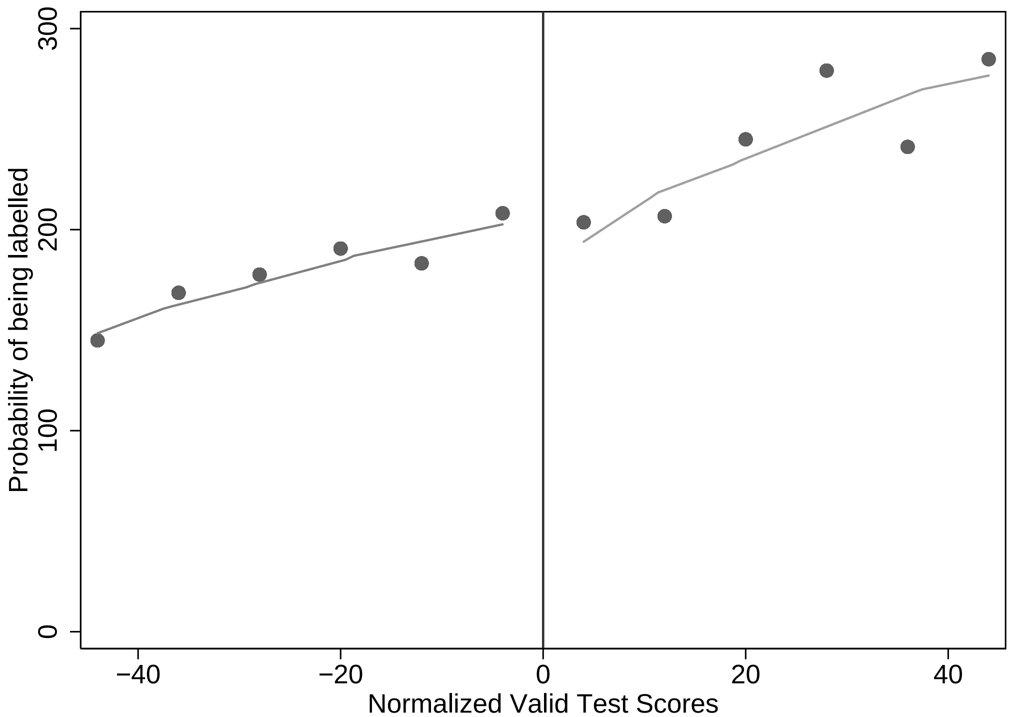
Notes: The figure shows an indicator equal to one if the a school is labelled low-performing as a function of API, normalized for Elementary, Middle, and High Schools. Each dot represents the average value of the indicator for bins of three API. Predicted lines are constructed using local-linear regressions.

FIGURE 3. Probability of labeling by valid test scores



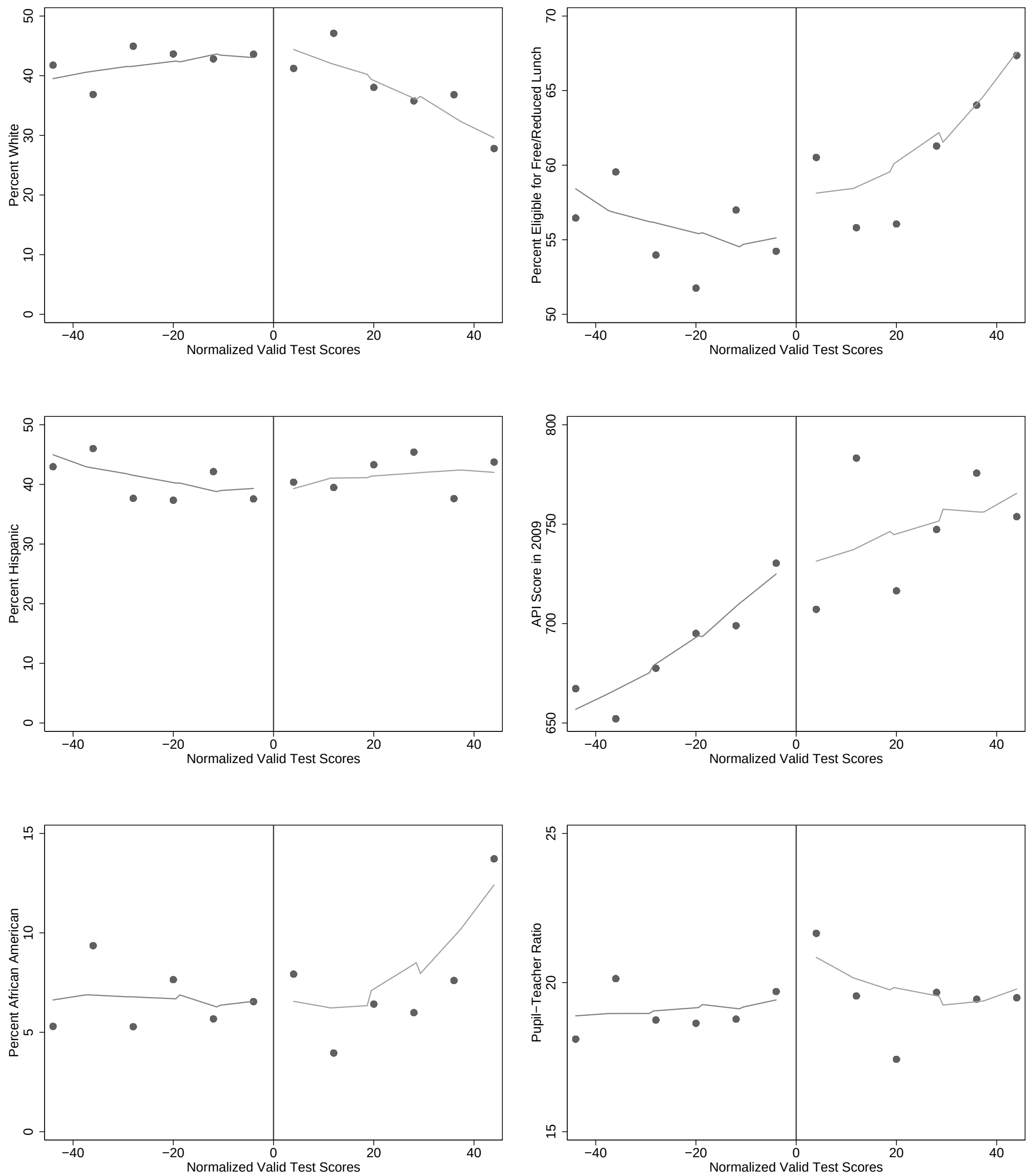
Notes: The figure shows an indicator equal to one if the a school is labelled low-performing as a function of valid test scores in 2009, normalized to the cutoff at 100 valid test scores. Each dot represents the average value of the indicator for bins of 8 valid test scores. The scale of the x-axis reflects a similar number of schools compared to Figure 2. Predicted lines are constructed using local-linear regressions.

FIGURE 4. The level of enrollment before the OE program was implemented



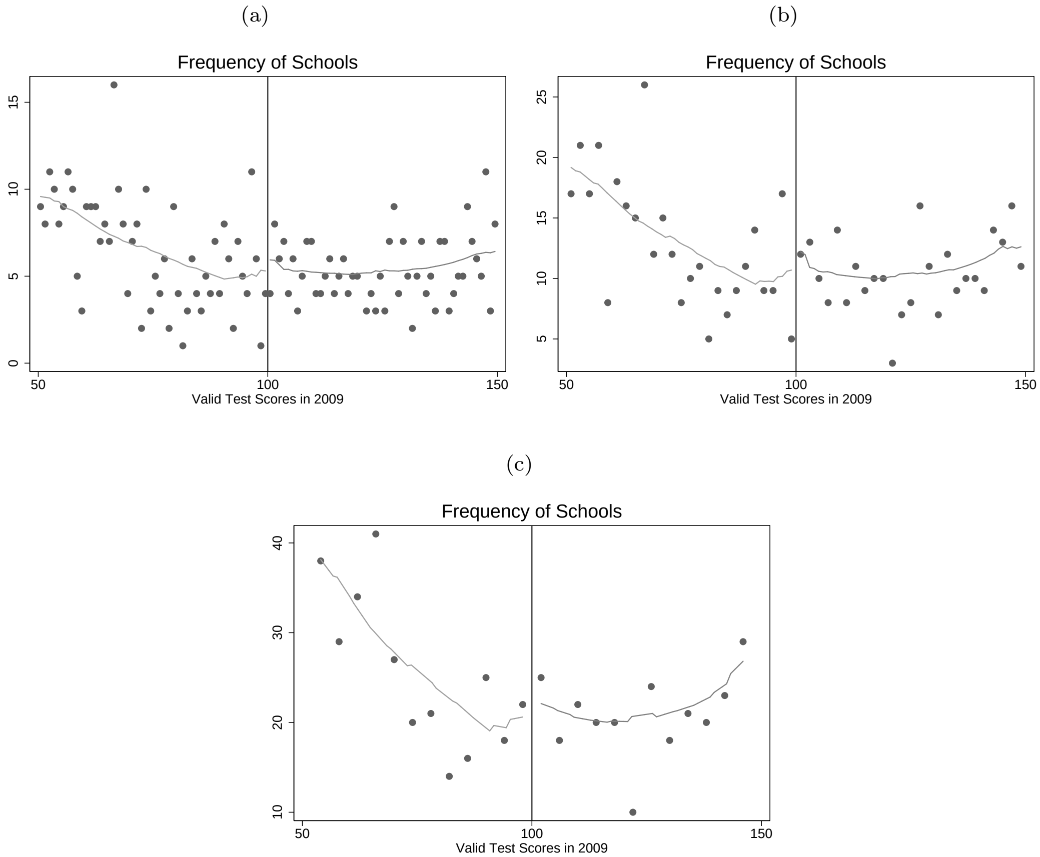
Notes: The figure shows the level of enrollment in 2010 as a function of valid test scores in 2009. Each dot represents the average enrollment of schools within a bin of eight valid test scores. Predicted lines are constructed using local linear regressions.

FIGURE 5. Characteristics of schools before the OE program was implemented



Notes: The figure shows school characteristics in 2010 as a function of valid test scores in 2009. Each dot represents the average of a characteristic for schools within a bin of eight valid test scores. Predicted lines are constructed using local linear regressions.

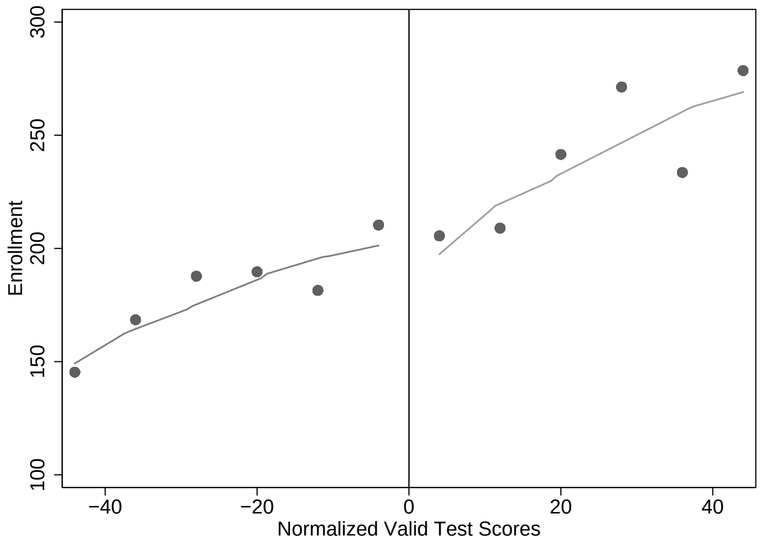
FIGURE 6. The distribution of schools is smooth through the threshold



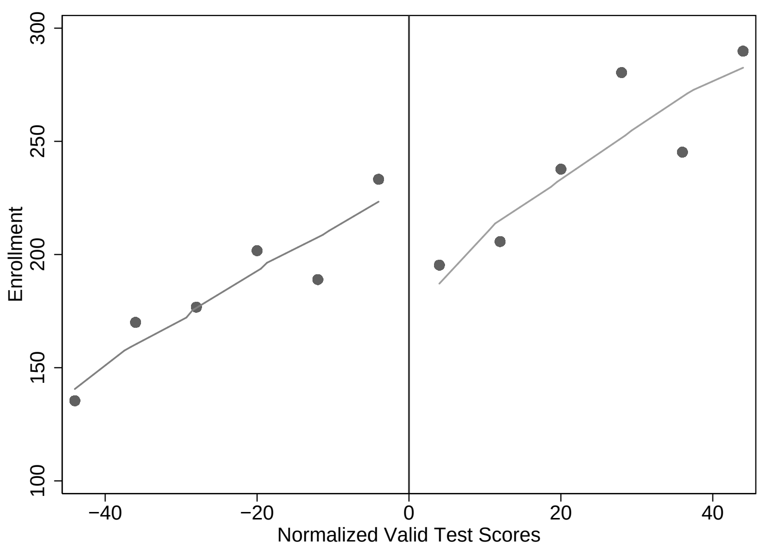
Notes: The figure shows the number of schools as a function of valid test scores in 2009. Each dot represents the average number of schools within a one, two, and four valid test score bin. Predicted lines are constructed using local linear regressions.

FIGURE 7. Effects of labelling on the level of enrollment

(a) Enrollment before 2010



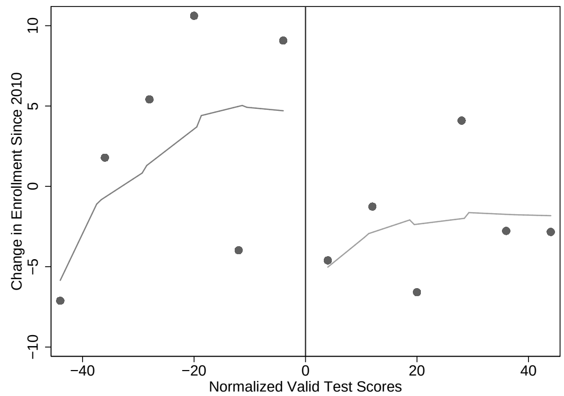
(b) Enrollment after 2010



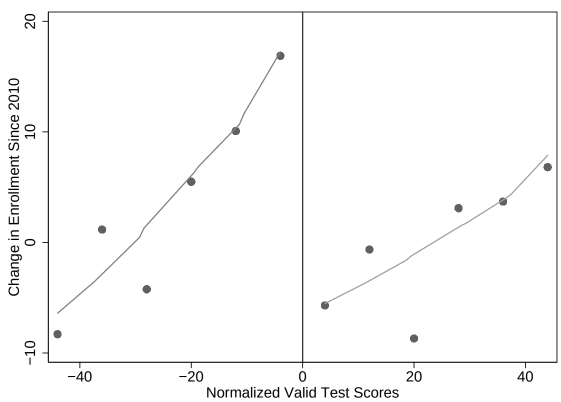
Notes: The upper figure shows enrollment for observations from 2008, 2009 and 2010. The lower figure shows enrollment after the OE program is introduced, in 2011, 2012, and 2013. Each dot represents the average enrollment for schools within a bin of eight valid test scores. Predicted lines are constructed using local linear regressions.

FIGURE 8. Effects on change in enrollment during the OE program

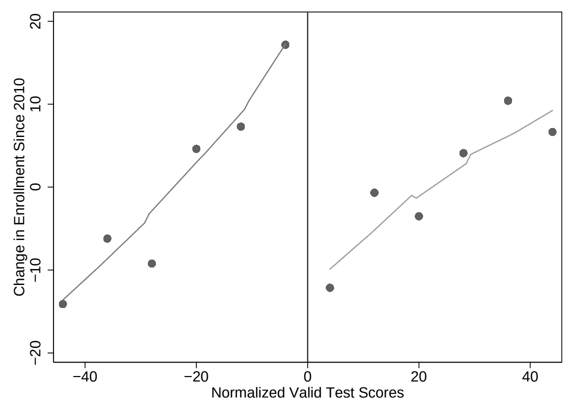
(a) Enrollment change from 2010 to 2011



(b) Enrollment change from 2010 to 2012



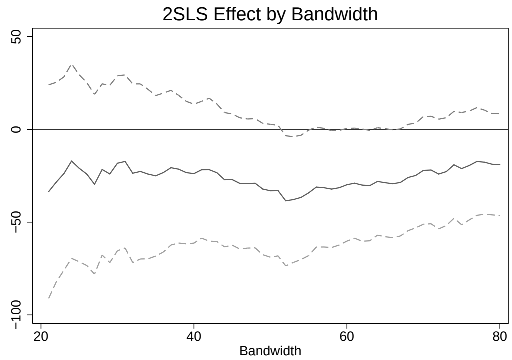
(c) Enrollment change from 2010 to 2013



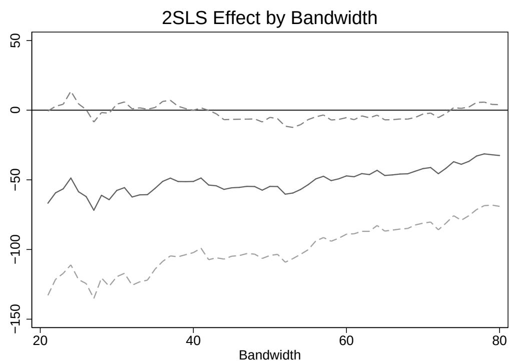
Notes: The figure shows the difference in enrollment between 2010 and the indicated year. Each dot represents the average difference in enrollment for schools within a bin of eight valid test scores. Predicted lines are constructed using local linear regressions.

FIGURE 9. Bandwidth sensitivity analysis for the change in enrollment

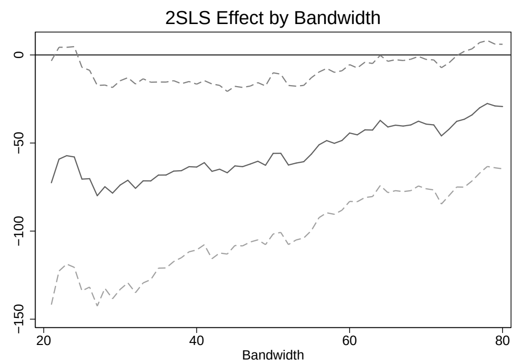
(a) Enrollment in 2011 - Enrollment in 2010



(b) Enrollment in 2012 - Enrollment in 2010



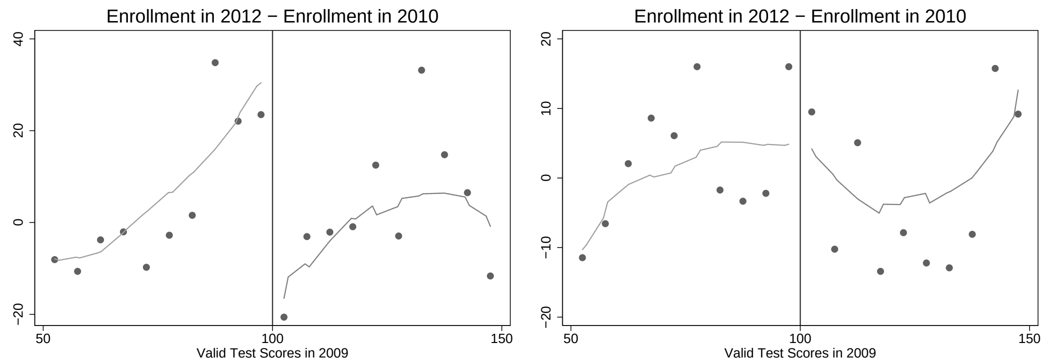
(c) Enrollment in 2013 - Enrollment in 2010



Notes: The black line indicates the point estimate from a local linear regression or 2SLS regression using the associated bandwidth on the x-axis. The dashed lines indicate 95% confidence intervals for the point estimate.

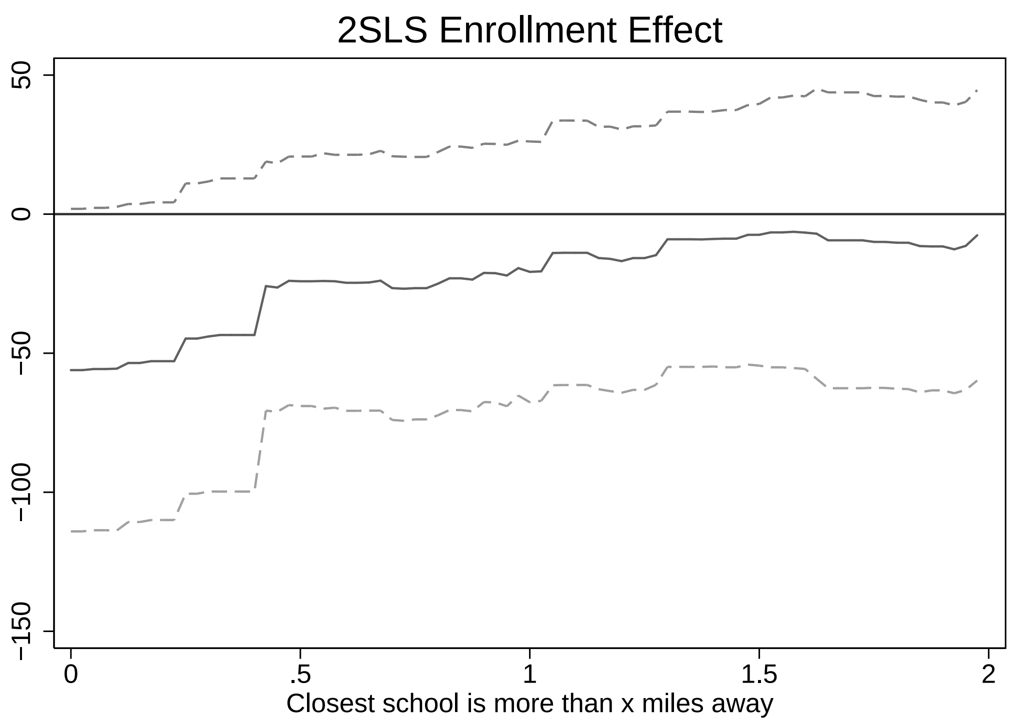
FIGURE 10. Effects on enrollment for schools by distance to nearest school

(a) Less than 3 miles (b) More than 3 miles



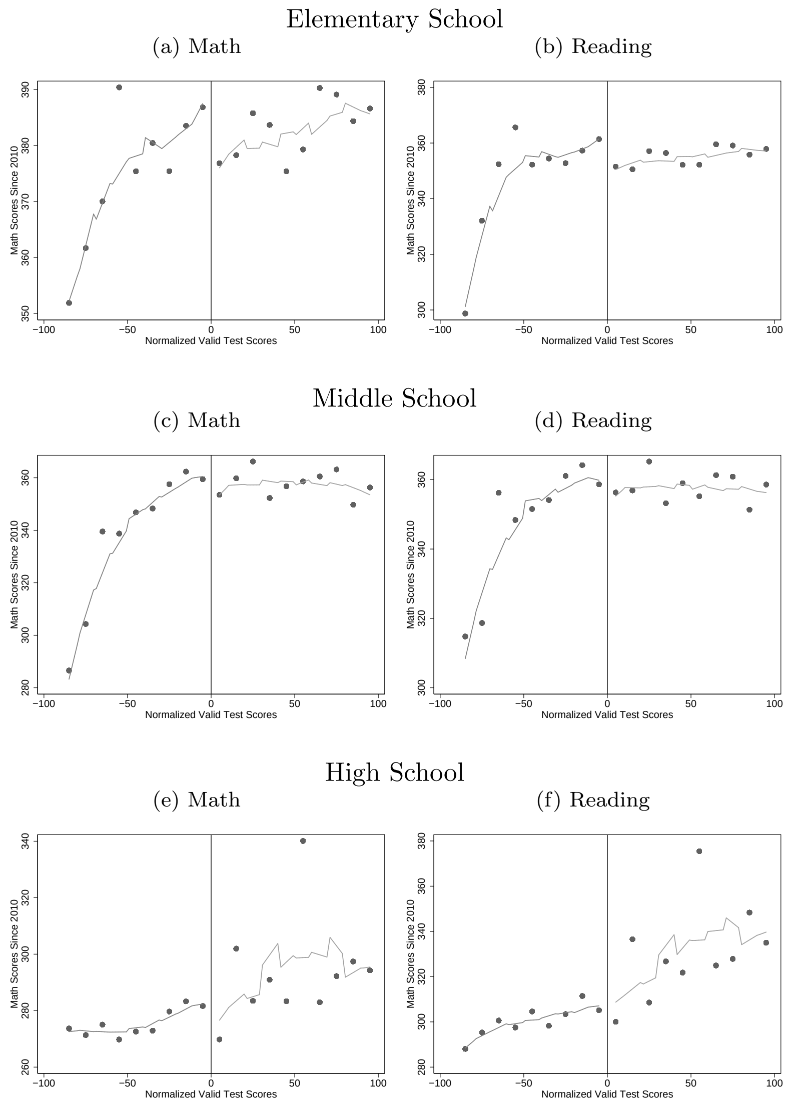
Notes: The figure on the left shows the difference in enrollment for observations whose closest, compatible school is within three miles. The figure on the right shows the difference in enrollment for observations whose closest, compatible school is further than three miles. Predicted lines are constructed using local linear regressions.

FIGURE 11. Estimate of the discontinuity in enrollment by distance between schools



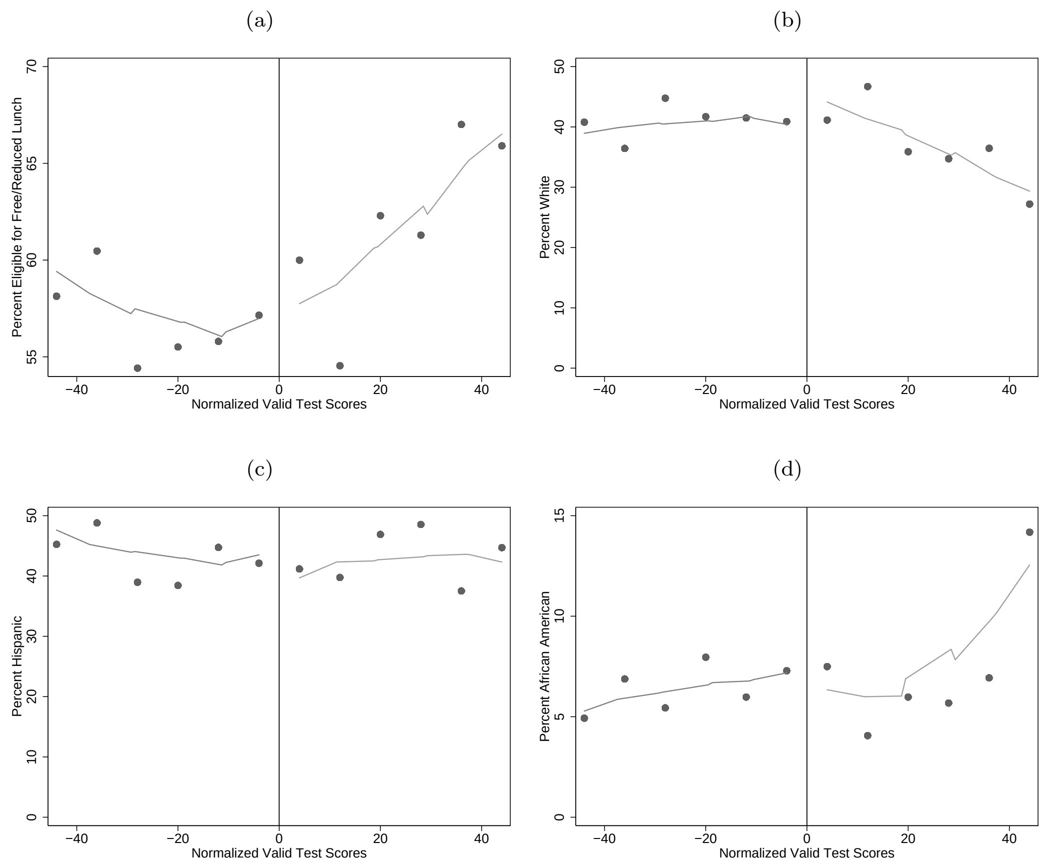
Notes: The black line indicates the point estimate from a local linear regression on the change in enrollment from 2010 to 2012 for schools where the closest available transfer option is further away than the associated distance on the x-axis. The dashed lines indicate 95% confidence intervals for the point estimate using robust standard errors. A bandwidth of 35 is used to estimate each regression.

FIGURE 12. Student achievement during the OE program



Notes: Each figure shows school-level average mean scaled score from California's STAR exams. Each dot represents the average outcome for schools within a eight valid test score bin. Predicted lines are constructed using local linear regressions.

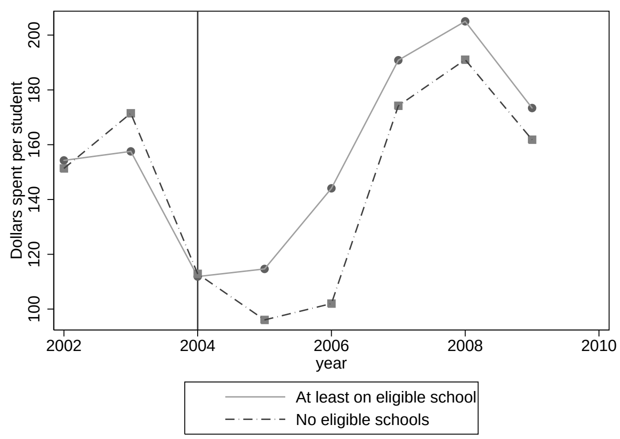
FIGURE 13. Effects on the percent of free lunch and ethnicities during the OE program



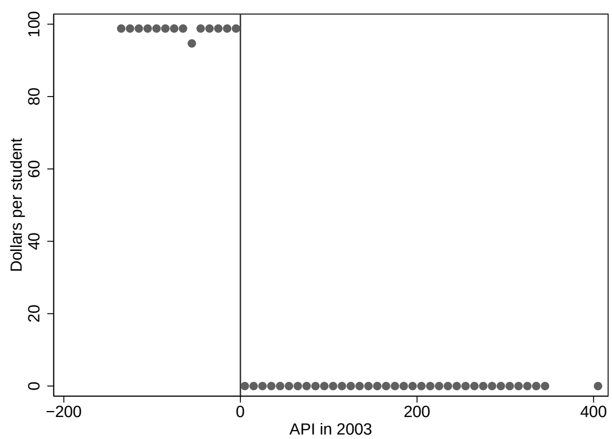
Notes: Each figure shows a school demographic for the 2011 school year. Each dot represents the average characteristic for schools within a eight valid test score bin. Predicted lines are constructed using local linear regressions.

FIGURE 14. Assignment and spending of textbook funding–school level eligibility, district level spending

Panel A: District level spending on textbooks per student by number of IMWC schools



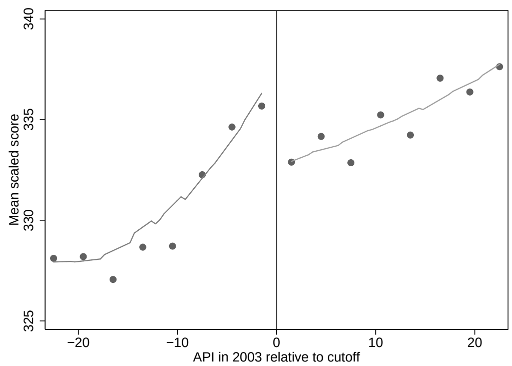
Panel B: School level assignment of IMWC textbook funding per student in 2005



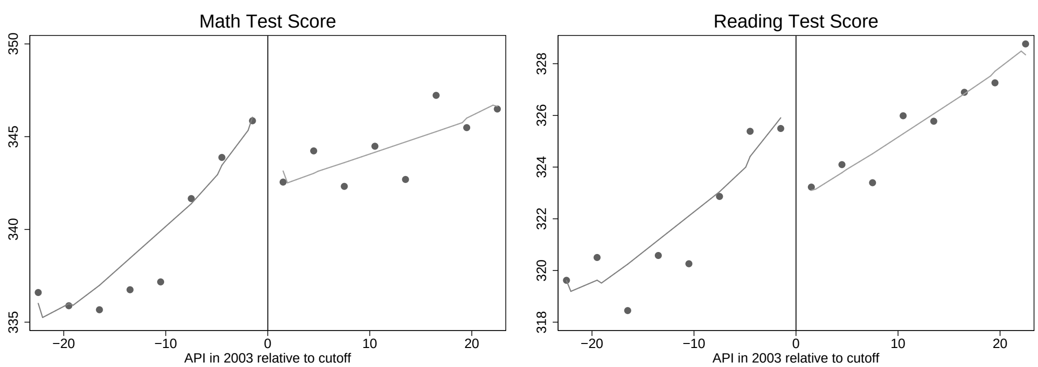
Notes: Panel A shows district level spending on textbooks per student over time. The solid line shows spending in districts with at least on school that qualifies for IMWC funding. The dashed line shows spending in districts with no qualifying schools. The vertical line indicates the date that IMWC funding was allocated. Panel B shows Williams Settlement textbook funding in LA county schools as a function of API score in 2003 normalized to zero at the threshold. The vertical line shows the threshold for eligibility. Schools at or below the threshold receive an additional \$ 96.90 per student from the IMWC fund. Schools are averaged into five API score bins.

FIGURE 15. Effect of textbook funding on student achievement in elementary schools for all years after disbursal

Panel A: Effects on the average of math and reading scores



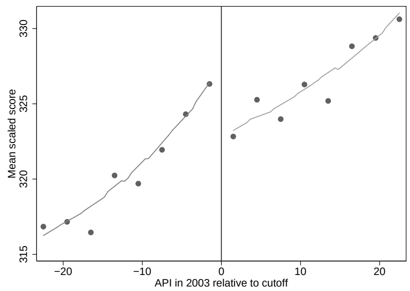
Panel B: Effects on the math and reading scores separately



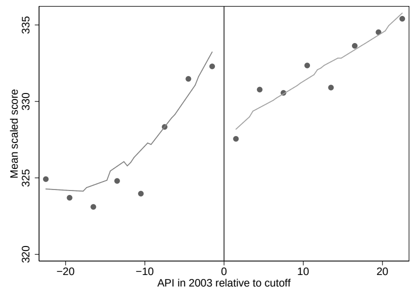
Notes: Panel A shows the average test score at the school as a function of API score in 2003 normalized to zero at the threshold. The vertical line shows the threshold for eligibility. Similarly, Panels B and C show math and reading test scores. Each dot shows an average over 3 API scores. Predicted lines are constructed using local linear regressions.

Panel A estimated discontinuity = 4.48*** with a standard error of (1.09), bandwidth of 15. Panel B estimated discontinuity = 4.93*** with a standard error of (1.28), bandwidth of 15. Panel C estimated discontinuity = 4.03*** with a standard error of (1.49), bandwidth of 15.

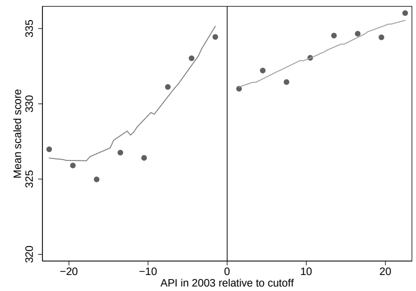
FIGURE 16. Effect of textbook funding on student achievement in elementary schools by year



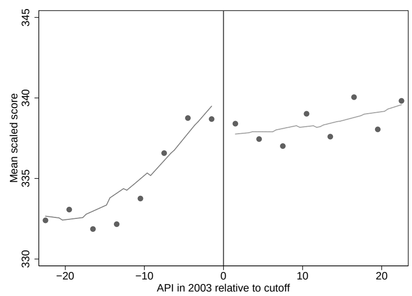
(a) 2005



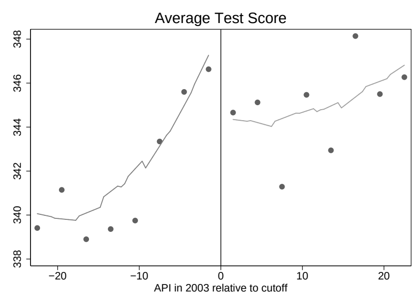
(b) 2006



(c) 2007



(d) 2008



(e) 2009

Notes: Each panel shows the average test score in each year as a function of API score in 2003 normalized to zero at the threshold. The vertical line shows the threshold for eligibility. Each dot shows an average over 3 API scores. Predicted lines are constructed using local linear regressions.

2005 estimated discontinuity = 3.84** with a standard error of (1.78), bandwidth of 15.

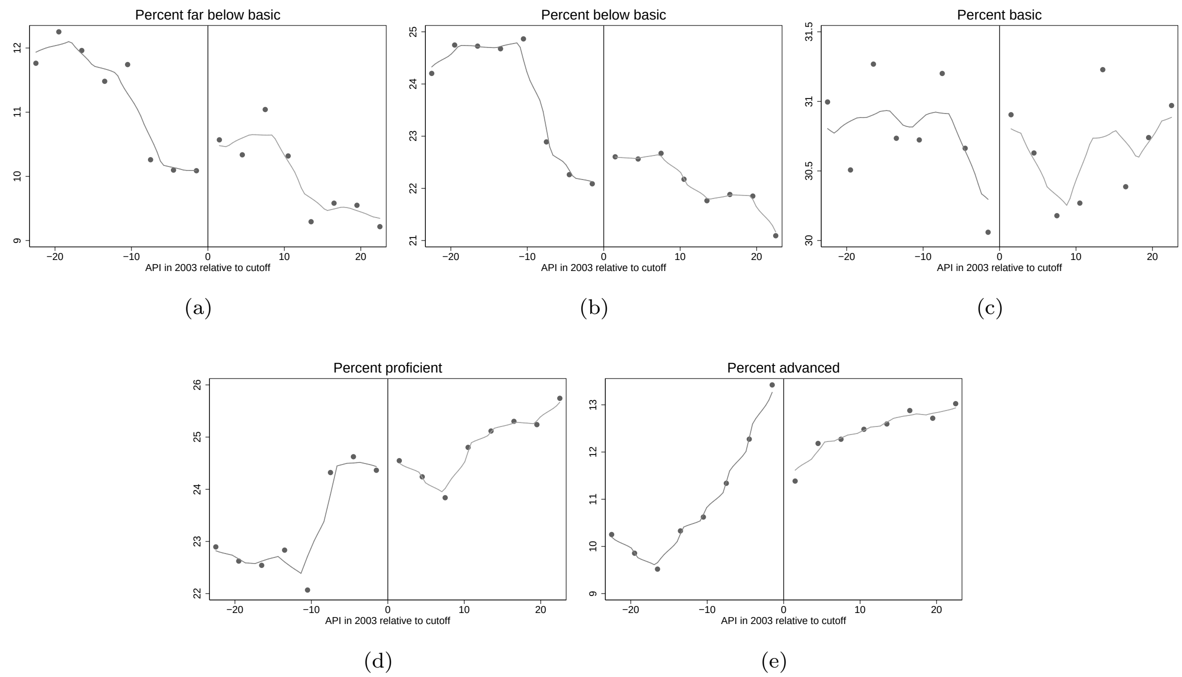
2006 estimated discontinuity = 5.62*** with a standard error of (2.05), bandwidth of 15.

2007 estimated discontinuity = 5.87*** with a standard error of (2.12), bandwidth of 15.

2008 estimated discontinuity = 2.86 with a standard error of (2.29), bandwidth of 15.

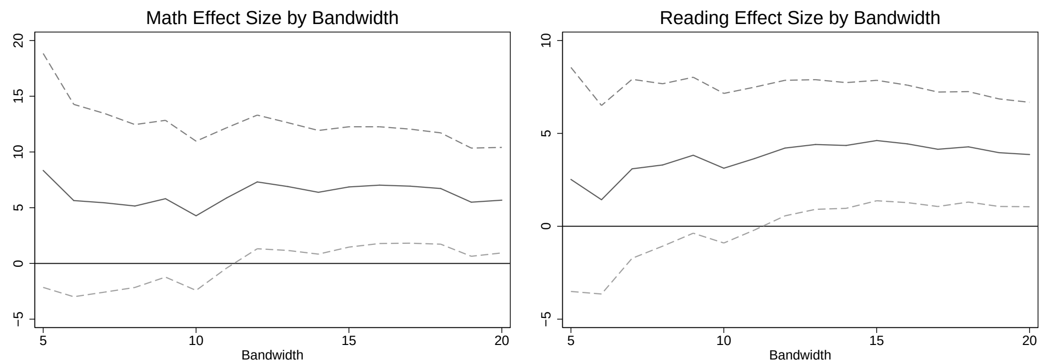
2009 estimated discontinuity = 4.21* with a standard error of (2.42), bandwidth of 15.

FIGURE 17. Effect of textbook funding on the distribution student achievement in elementary schools–percent of students in performance category



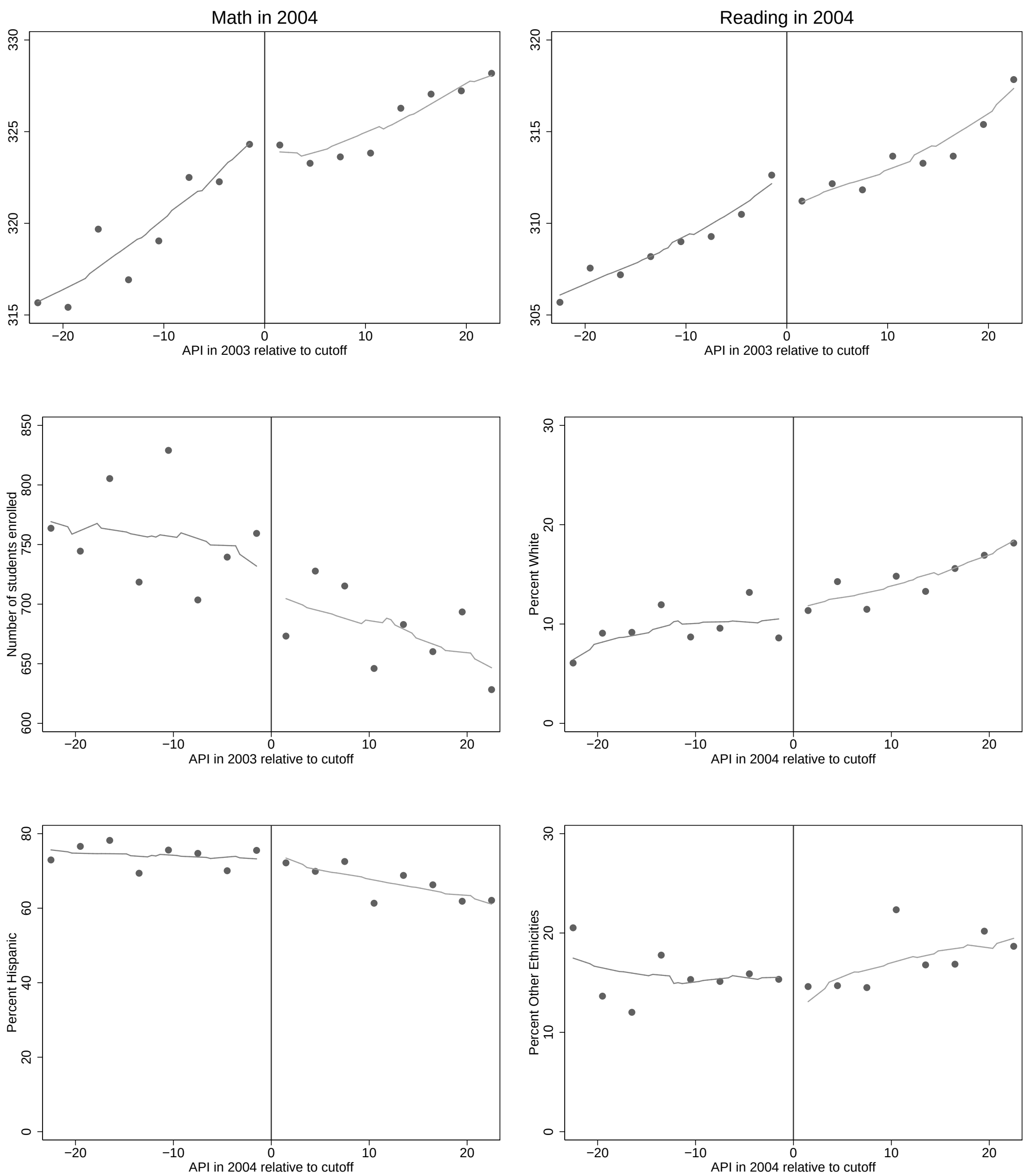
Notes: Each panel shows the percent of students in the school that are in the relevant performance category: Far below basic, below basic, basic, proficient, advanced; as a function of API score in 2003 normalized to zero at the threshold. The vertical line shows the threshold for eligibility. Each panel shows raw cell means (points) for each API score. Predicted lines are constructed using local linear regressions.

FIGURE 18. Robustness of regression discontinuity estimates in elementary schools to bandwidth choice



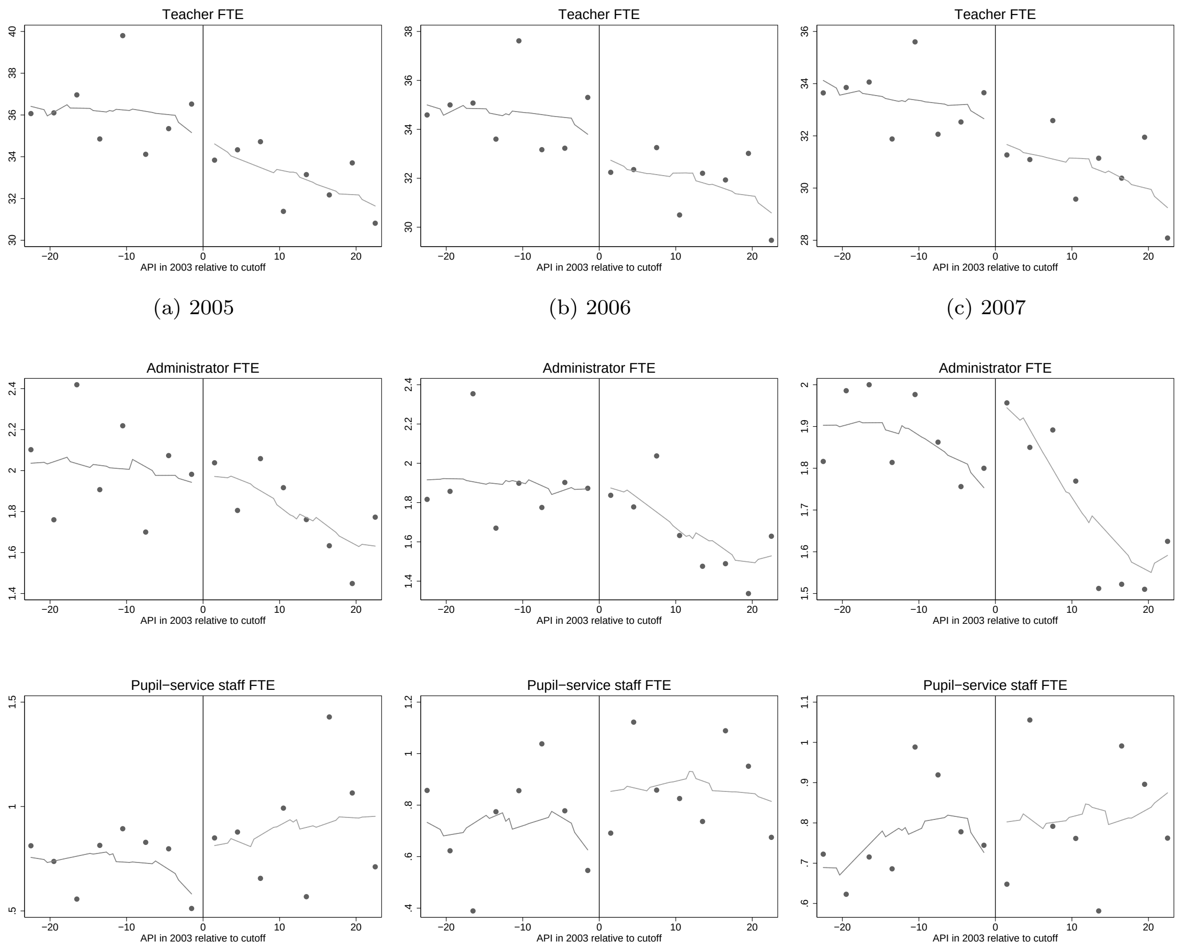
Notes: The black line indicates the estimated effect on student achievement from a local linear regression with a bandwidth on the x-axis. The dashed lines indicate 95% confidence intervals for the point estimate using robust standard errors.

FIGURE 19. Smoothness of school characteristics before disbursal of textbook funding in elementary schools



Notes: Each panel shows an observable school characteristic in 2004 as a function of API score in 2003 normalized to zero at the threshold. The vertical line shows the threshold for eligibility. Each dot shows an average over 3 API scores. Predicted lines are constructed using local linear regressions. None of the estimated effects are significant, supporting the validity of the RD design in this setting.

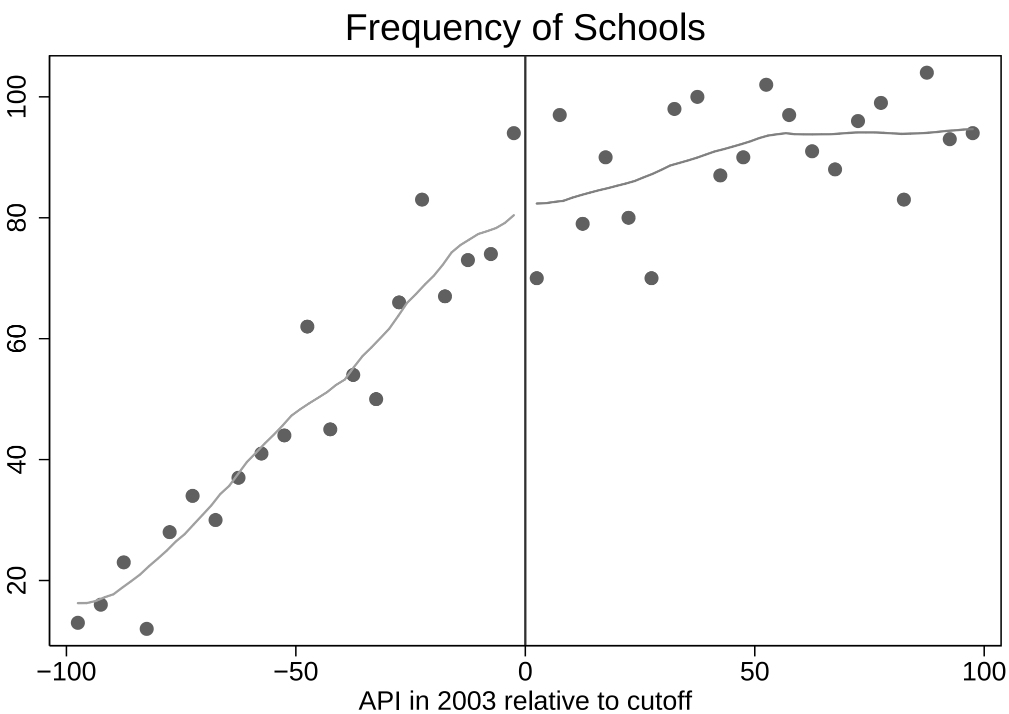
FIGURE 20. Potential effects of textbook funding on staff characteristics in elementary schools



(a) 2005 (b) 2006 (c) 2007

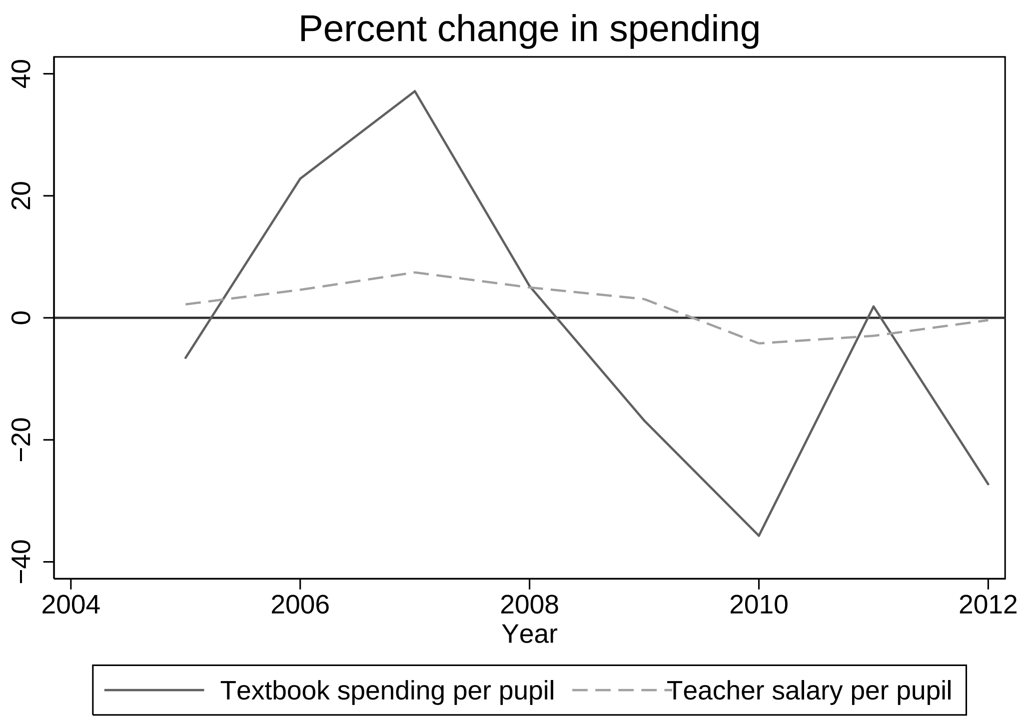
Notes: Each panel shows an observable staff characteristic as a function of API score in 2003 normalized to zero at the threshold. The vertical line shows the threshold for eligibility. Each dot shows an average over 3 API scores. Predicted lines are constructed using local linear regressions. None of the estimated effects are significant, suggesting that fiscal substitution did not occur.

FIGURE 21. Frequency of schools around eligibility threshold



Notes: Shows the number of schools with an API score of "x" in 2003. The vertical line shows the threshold for eligibility. Predicted lines are constructed using local linear regressions.

FIGURE 22. Percent change in total spending on textbooks and teacher salaries in California



Notes: Percent change in average district spending per pupil from 2005 to 2012.

TABLE 1. Summary statistics for Chapter II

	All Schools	Schools near the threshold
Total Enrollment	628.55	209.55
Percent White	30.16	39.71
Percent Hispanic	48.07	44.09
Percent Black	7.13	5.62
Percent free or reduced lunch	53.09	53.84
API score	768.54	712.41
Number of Valid Test Scores	509.18	95.72
Percent Elementary School	65.86	57.24
Percent Middle School	19.27	39.35
Percent High School	14.88	3.41
Observations	7417	534

Notes: The sample includes 20,247 school-year observations for each public school in California from the 2010-2011 school year and the 2011-2012 school year. Data on total enrollment, percent White, percent Hispanic, percent Black, percent Asian, and percent of students eligible for free or reduced lunch are provided by the California Basic Educational Data System (CBEDS). API score is an index of standardized test scores for California schools and this data is available from the California Department of Education' Academic Performance Index files. The number of valid test scores is also available from the Academic Performance Index files, and is used to prevent schools with fewer than 100 valid test scores from being labeled low performing.

TABLE 2. RD estimates of characteristics of schools before the OE program was implemented

		Years before program		
Outcome:	All Pre-program years	2008	2009	2010
Enrollment	-9.55	-9.42	-11.85	-7.3
	(10.06)	(17.16)	(17.34)	(17.87)
Percent White	0.64	0.7	-0.04	1.26
	(2.89)	(4.97)	(4.97)	(5.07)
Percent Hispanic	1.52	1.6	1.11	1.89
	(2.74)	(4.77)	(4.74)	(4.76)
Free and Reduced	1.6	1.57	0.78	2.34
Price Lunch	(2.56)	(4.48)	(4.39)	(4.42)
Teacher FTE	-1.38	-0.95	-1.76	-1.43
	(0.91)	(1.54)	(1.74)	(1.41)
Admin. FTE	-0.14	-0.1	-0.2	-0.12
	(0.09)	(0.17)	(0.15)	(0.13)

Notes: Each entry is an estimated effect from a linear regression with flexible slops, rectangular kernel weights, and a bandwidth of 50 valid test scores. Robust standard errors are displayed in parentheses.
***Significant at the 1 percent level
**Significant at the 5 percent level
*Significant at the 10 percent level

TABLE 3. Estimates of the potential discontinuity in the distribution of schools

	Bin size = 1		Bin size = 2		Bin size= 4	
Bandwidth:	50	75	50	75	50	75
	(1)	(2)	(3)	(4)	(5)	(6)
1(Valid >99)	1.172	0.019	2.50	-0.829	6.10	0.805
	(2.00)	(2.49)	(4.37)	(5.509)	(9.20)	(9.836)
Constant (control mean)	8.23***	-	16.47***	-	32.41***	-
	(1.65)	-	(3.70)	-	(7.11)	-
Observations	100	150	50	74	24	38

Notes: Robust standard errors are displayed in parentheses. Estimates for small bandwidths are based on linear regressions with rectangular kernel weights. Estimates for large bandwidths are based on quadratic regressions with rectangular kernel weights.
***Significant at the 1 percent level
**Significant at the 5 percent level
*Significant at the 10 percent level

TABLE 4. Effects on the probability of being labeled and the level of enrollment

Panel A: Effects on the Probability of Labeling

Bandwidth	Linear	Quadratic	Cubic	Quartic
50	0.45***	0.50***	0.50***	0.63***
	(0.08)	(0.08)	(0.11)	(0.14)
40	0.47***	0.47***	0.61***	0.58***
	(0.06)	(0.09)	(0.13)	(0.17)
30	0.43***	0.56***	0.57***	0.64***
	(0.05)	(0.12)	(0.16)	(0.22)

Panel B: Effects of labeling on Enrollment

	OLS		IV: 1(Valid >99) for Labelled	
	Pre-treatment	Post-treatment	Pre-treatment	Post-treatment
Bandwidth				
50	-9.55	-36.81***	-22.84	-84.75***
	(10.06)	(11.19)	(24.44)	(27.42)
40	-4.53	-36.34***	-10.08	-78.11***
	(11.18)	(12.60)	(25.03)	(28.63)
30	-11.80	-44.64***	-27.28	-99.73***
	(13.75)	(15.37)	(32.45)	(37.05)

Notes: Each entry in Panel A represents the estimated discontinuity in the probability of labeling using a regression with flexible slops, rectangular kernel weights, and the indicated functional form. Entries for OLS in Panel B represent estimated discontinuities in enrollment for all observations either before or after the program is implemented in 2010. These effects are estimated using linear regressions with flexible slopes and rectangular kernel weights. Entries for IV in Panel B represent estimated effects of labeling using the threshold as an instrument. Robust standard errors are displayed in parentheses.
***Significant at the 1 percent level
**Significant at the 5 percent level
*Significant at the 10 percent level

TABLE 5. Effects on changes in enrollment since 2010

	OLS		IV	
	No Controls	Controls	No Controls	Controls
	(1)	(2)	(3)	(4)
Panel A: Effects on Enrollment in 2012 - Enrollment in 2010				
$1(Valid_{2009}>99)$	-24.03**	-22.98**	-54.70**	-52.12**
	(10.69)	(10.20)	(25.29)	(24.12)
	[10.27]	[9.49]	[22.93]	[21.64]
	[10.57]	[9.99]	[25.20]	[23.86]
Constant	18.76**	-	18.77**	-
(control mean)	(8.91)	-	(8.88)	-
Observations	565	565	565	565
Panel B: Effects on Enrollment in 2011 - Enrollment in 2010				
$1(Valid_{2009}>99)$	-14.53*	-14.22*	-33.07*	-32.24*
	(7.88)	(7.75)	(18.28)	(18.06)
	[7.23]	[6.98]	[15.88]	[15.65]
	[7.79]	[7.62]	[18.46]	[18.15]
Constant	10.70**	-	10.70**	-
(control mean)	(5.26)	-	(5.23)	-
Observations	565	565	565	565
Panel C: Effects on Enrollment in 2012 - Enrollment in 2011				
$1(Valid_{2010}>99)$	-12.36**	-11.86**	-30.70**	-29.01**
	(5.61)	(5.65)	(14.33)	(14.21)
	[5.03]	[4.97]	[11.92]	[11.80]
	[5.77]	[5.79]	[14.47]	[14.29]
Constant	4.30	-	4.30	-
(control mean)	(3.66)	-	(3.64)	-
Observations	565	565	565	565

Notes: Robust standard errors are displayed in parentheses, standard errors clustered on the running variable and standard errors that are clustered at the district level are reported below. Estimates are based on linear regressions with rectangular kernel weights and a bandwidth of 50. Controls include average test score, percent white, Hispanic, and other races and the percent of students eligible for free or reduced lunch.
***Significant at the 1 percent level
**Significant at the 5 percent level
*Significant at the 10 percent level

TABLE 6. Effects on average student achievement

	All Pre years	All Post Years	Years before program			Years after program		
School and test:			2008	2009	2010	2011	2012	2013
Elementary math	2.82	-7.83*	4.05	-0.41	4.25	-2.46	-8.31	-12.75
	(4.20)	(4.20)	(6.94)	(7.30)	(7.34)	(7.00)	(7.15)	(7.75)
Elementary reading	-2.27	-9.15***	-0.34	-5.11	-1.77	-7.69	-9.34*	-10.42*
	(3.11)	(3.06)	(5.10)	(5.41)	(5.47)	(5.15)	(5.30)	(5.49)
Middle math	-4.01	-7.69	-10.73	-9.86	10.2	-3.03	-12.53	-6.97
	(5.41)	(5.77)	(8.45)	(9.57)	(9.94)	(9.73)	(10.23)	(10.17)
Middle reading	-4.3	-6.47	-8.93	-5.65	2.72	-2.69	-8.62	-8.04
	(4.36)	(4.31)	(6.98)	(7.67)	(7.82)	(7.55)	(7.54)	(7.43)
High math	-3.8	-3.00	-1.45	-9.35	-0.43	-3.73	-6.48	1.14
	(4.42)	-4.87	(7.47)	(7.46)	(8.14)	(8.44)	(9.08)	(7.88)
High reading	3.19	8.58	1.13	2.52	5.85	8.68	7.36	9.52
	(6.01)	(6.07)	(10.15)	(10.52)	(10.69)	(10.61)	(10.52)	(10.60)

Notes: Each entry represents a estimated effect on mean scale scores from California's STAR tests using a linear regression with rectangular kernel weights and a bandwidth of 50. Robust standard errors are displayed in parentheses.
***Significant at the 1 percent level
**Significant at the 5 percent level
*Significant at the 10 percent level

TABLE 7. Effects on school demographics

			Years before program			Years after program		
Outcome var.:	All Pre years	All Post Years	2008	2009	2010	2011	2012	2013
Free and Reduced	1.60	1.72	1.57	0.78	2.34	1.41	0.64	3.14
Price Lunch	(2.56)	(2.52)	(4.48)	(4.39)	(4.42)	(4.32)	(4.35)	(4.45)
Percent White	0.64	1.93	0.70	-0.04	1.26	3.37	1.78	0.58
	(2.89)	(2.87)	(4.97)	(4.97)	(5.07)	(5.00)	(4.92)	(5.00)
Percent Hispanic	1.52	-0.57	1.60	1.11	1.89	-0.64	-1.04	-0.01
	(2.74)	(2.79)	(4.77)	(4.74)	(4.76)	(4.78)	(4.84)	(4.90)
Percent Black	0.35	-0.07	-0.42	0.53	0.95	-0.73	-0.13	1.06
	(1.51)	(1.48)	(2.61)	(2.65)	(2.60)	(2.67)	(2.62)	(2.43)

Notes: Robust standard errors are displayed in parentheses. Estimates are based on linear regressions with rectangular kernel weights and a bandwidth of 50.
***Significant at the 1 percent level
**Significant at the 5 percent level
*Significant at the 10 percent level

TABLE 8. Summary statistics for Chapter III

Variable	Full sample	15 API bandwidth around eligibility cutoff
Math Score	354.35	328.32
Reading Score	340.80	318.16
API score in 2003	727.18	634.56
Total Enrollment	659.79	745.96
Percent Hispanic	45.63	71.27
Percent White	32.97	12.11
Percent Other	19.13	15.41
Percent Eligible for Free or Reduced lunch	53.33	80.00
FTE for Teachers	31.64	36.27
FTE for Admin	1.75	2.12
FTE for Pupil Service	1.17	1.28
Pupil/Teacher	21.034	20.26
Funding for Textbooks per student*	78.00	108.01
Observations	45517	4190

Notes: Listed means are not weighted. The full sample includes 45,517 school-year observations for public schools in California from 2002 to 2009. The restricted sample shows statistics of schools within 15 API from the cutoff for additional textbook funding. API score, as constructed by the California Department of Education, is a weighted average of standardized test scores.

*This data comes from LA County and includes 1450 observations for the full sample and 180 for the restricted sample.

TABLE 9. Effects of textbook funding on student achievement in elementary schools

	All Years	Post-treatment				
		2005	2006	2007	2008	2009
	(1)	(2)	(3)	(4)	(5)	(6)
Effects of Textbook Funding on Student Achievement						
Average score	4.48***	3.84**	5.62***	5.87***	2.86	4.21*
	(1.09)	(1.78)	(2.05)	(2.12)	(2.29)	(2.42)
Math	4.93***	4.66*	7.05**	6.55**	2.17	4.24
	(1.28)	(2.54)	(2.83)	(2.87)	(3.05)	(3.27)
Reading	4.03***	3.02**	4.20**	5.20***	3.5*	4.17**
	(1.49)	(1.49)	(1.66)	(1.70)	(1.85)	(1.97)

Notes: Each entry is an estimated effect from a linear regression with flexible slopes, rectangular kernel weights and a bandwidth of 15. Robust standard errors are displayed in parentheses. Estimates in All Years pool all observations from 2005 to 2009 for a total of 2055 observations. Columns (2) - (6) show estimated effects for individual years, with 411 observations each. Average score is school average of math and reading scores.
***Significant at the 1 percent level
**Significant at the 5 percent level
*Significant at the 10 percent level

TABLE 10. Effects of textbook funding on the distribution of student achievement in elementary schools

	Far Below	Below	Basic	Proficient	Advanced
	(1)	(2)	(3)	(4)	(5)
Panel A: Percent Meeting Standard					
	-0.96***	-1.32***	-0.43	0.99***	1.70***
	(0.31)	(0.39)	(0.28)	(0.35)	(0.43)
Panel B: (Cumulative) Percent at or Below Standard					
	-0.96***	-2.28***	-2.71***	-1.72***	-
	(0.31)	(0.65)	(0.72)	(0.43)	-

Notes: Each entry is an estimated effect from a linear regression with flexible slopes, rectangular kernel weights and a bandwidth of 15. Each Column shows estimates for a dependent variable; in Panel A, the percent of students in that performance category. In Panel B, the percent of students at or below that performance category. Robust standard errors are displayed in parentheses. Estimates pool all observations from 2005 to 2009 for a total of 2055 observations.
***Significant at the 1 percent level
**Significant at the 5 percent level
*Significant at the 10 percent level

TABLE 11. Effects on student achievement in middle schools and high schools

Subject	Math Scores					Reading Scores				
Year	2005	2006	2007	2008	2009	2005	2006	2007	2008	2009
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Effects of Textbook Funding on Student Achievement in Middle Schools										
	0.57	5.13	-2.75	2.52	2.61	-2.53	-0.56	-1.70	-4.26	-3.10
	(8.57)	(8.06)	(7.82)	(8.42)	(9.53)	(1.92)	(2.32)	(2.60)	(2.99)	(3.31)
Panel B: Effects of Textbook Funding on Student Achievement in High Schools										
	1.83	3.22	2.18	1.21	6.32	3.56	7.13	4.54	2.30	4.10
	(5.67)	(5.42)	(6.08)	(6.48)	(7.40)	(4.08)	(4.31)	(4.27)	(5.00)	(4.40)

Notes: Each entry is an estimated effect from a linear regression with flexible slopes, rectangular kernel weights and a bandwidth of 15. Robust standard errors are displayed in parentheses. Each column shows estimates for math scores or reading scores in a particular year, with 105 observations for middle schools and 62 observations for high schools.
***Significant at the 1 percent level
**Significant at the 5 percent level
*Significant at the 10 percent level

TABLE 12. Effects of textbook funding on pre-treatment student achievement, student characteristics of elementary schools

Years	All Post Years	Pre-treatment			Post-treatment				
		2002	2003	2004	2005	2006	2007	2008	2009
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Student Achievement Pre-treatment and Post-treatment									
Average score	4.48***	1.22	0.42	1.94	3.84**	5.62***	5.87***	2.86	4.21*
	(1.09)	(0.67)	(1.34)	(1.78)	(1.78)	(2.05)	(2.12)	(2.29)	(2.42)
Math	4.93***	1.34	0.92	2.18	4.66*	7.05**	6.55**	2.17	4.24
	(1.28)	(1.70)	(1.41)	(2.04)	(2.54)	(2.83)	(2.87)	(3.05)	(3.27)
Reading	4.03***	1.09	-0.07	1.70	3.02**	4.20**	5.20***	3.5*	4.17**
	(1.49)	(1.27)	(0.85)	(1.22)	(1.49)	(1.66)	(1.70)	(1.85)	(1.97)
Panel B: Potential Effects on Student Characteristics									
Enrollment	48.19	30.04	37.34	49.76	47.24	52.64	51.40	47.94	41.72
	(53.37)	(59.74)	(60.16)	(59.92)	(55.79)	(49.79)	(46.55)	(44.63)	(42.49)
Fraction White	-2.64	-2.40	-2.83	-2.76	-2.86	-2.19	-2.82	-2.67	-2.67
	(1.71)	(2.77)	(2.66)	(2.50)	(2.37)	(2.24)	(2.17)	(2.12)	(2.11)
Fraction Hispanic	2.02	1.26	1.44	1.82	2.14	1.48	2.14	2.04	2.32
	(2.97)	(4.43)	(4.36)	(4.29)	(4.19)	(4.08)	(4.05)	(3.99)	(4.06)
Fraction other	1.42	0.94	1.70	2.00	1.66	1.61	1.28	1.31	1.22
	(1.84)	(3.44)	(3.45)	(3.41)	(3.32)	(3.22)	(3.07)	(3.01)	(3.05)
Free or Reduced Lunch	-0.02	0.01	0.01	-0.03	-0.03	-0.02	-0.04	-0.02	-0.01
	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.02)

Notes: Each entry is an estimated effect from a linear regression with flexible slopes, rectangular kernel weights and a bandwidth of 15. Robust standard errors are displayed in parentheses. Estimates in All Years pool all observations from 2005 to 2009 for a total of 2055 observations. Columns (2) - (9) show estimated effects for individual years, with 411 observations each. Average score is school average of math and reading scores.
***Significant at the 1 percent level
**Significant at the 5 percent level
*Significant at the 10 percent level

TABLE 13. Effects on pre-treatment staff characteristics of elementary schools

	All Post Years	Pre-treatment			Post-treatment				
		2002	2003	2004	2005	2006	2007	2008	2009
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel C: Full-Time Equivalence (FTE) by Staff									
Teachers	1.74	1.44	1.40	1.72	1.63	2.07	1.76	1.56	1.70
	(2.55)	(3.00)	(2.94)	(2.87)	(2.72)	(2.42)	(2.20)	(2.15)	(2.04)
Administrators	-0.10	-0.03	0.14	-0.14	-0.04	-0.03	-0.23	-0.06	-0.14
	(0.18)	(0.16)	(0.18)	(0.19)	(0.24)	(0.20)	(0.20)	(0.20)	(0.21)
Service Staff	-0.23	0.33	0.05	-0.19	-0.33	-0.22	-0.01	-0.16	-0.41
	(0.19)	(0.21)	(0.21)	(0.21)	(0.26)	(0.22)	(0.19)	(0.21)	(0.20)
Panel D: Other Teacher Characteristics									
Years exper.	-0.69	-1.02*	-0.93	-0.60	-0.67	-1.00*	-0.78	-0.58	-0.40
	(0.50)	(0.54)	(0.57)	(0.58)	(0.58)	(0.53)	(0.55)	(0.54)	(0.53)
Years in district.	-0.54	-0.57	-0.75	-0.40	-0.62	-0.72	-0.70	-0.40	-0.25
	(0.47)	(0.48)	(0.49)	(0.52)	(0.53)	(0.48)	(0.51)	(0.53)	(0.52)

Notes: Each entry is an estimated effect from a linear regression with flexible slopes, rectangular kernel weights and a bandwidth of 15. Robust standard errors are displayed in parentheses. Estimates in All Years pool all observations from 2005 to 2009 for a total of 2055 observations. Columns (2) - (9) show estimated effects for individual years, with 411 observations each. Average score is school average of math and reading scores.
***Significant at the 1 percent level
**Significant at the 5 percent level
*Significant at the 10 percent level

TABLE 14. Effects of textbook funding on the smoothness of the distribution of schools

API cells	1	1	2	2	5	5
Bandwidth	20	50	20	50	20	50
	(1)	(2)	(3)	(4)	(5)	(6)
1(API in 2003 >643)	-2.91	-1.50	-5.57	-3.04	-16.55	-8.48
	(2.67)	(1.63)	(3.76)	(2.60)	(15.91)	(9.09)
Constant	18.33***	17.06***	36.63***	34.18***	90.35***	85.37***
(control mean)	(1.03)	(0.66)	(2.85)	(2.15)	(6.67)	(5.20)
Observations	40	100	20	50	8	20

Notes: Robust standard errors are displayed in parentheses. Estimates are based on linear regressions with rectangular kernel weights and a bandwidth of 50.
***Significant at the 1 percent level
**Significant at the 5 percent level
*Significant at the 10 percent level

APPENDIX B

SAMPLE OF NOTICE OF PARENT'S RIGHTS

School Year 2008/09

ANNUAL NOTIFICATION OF PARENTS' AND STUDENTS RIGHTS

Dear Parent/Guardian:

State and federal laws require school districts notify parents and guardians of minor pupils of their parental and student rights. This law requires the parents or guardians sign a notification form and return it to school. The signature is an acknowledgment that the parents or guardians have been informed of their rights, but does not indicate that consent to participate in any particular program has been either given or withheld.

Some legislation requires additional notification to the parents or guardians during the school term or at least 15 days prior to a specific activity.(A separate letter will be sent to parents or guardians prior to any of these specified activities or classes, and the student will be excused whenever the parents file with the principal of the school a statement in writing requesting that their child not participate.) Other legislation grants certain rights that are spelled out in this notification.

The following rights, responsibilities, and protections are provided (when used in this notification "parent" includes a parent or legal guardian):

...

TRANSFERS BETWEEN SCHOOLS (EC 35160.5c, 48980(h), and 46600): Provides for parents of each school-age child who is a resident in the district the opportunity to select the schools the child shall attend irrespective of the family's residence. Limitations are space availability, ethnic balance, no displacement of students within the attendance area, and no transportation. A more detailed notification is available at each school site and will be sent to parents in January, 2008.

APPENDIX C

SAMPLE LETTER FROM SAN LORENZO UNIFIED.

Dear Hillside Elementary School Parents/Guardians:

On January 7, 2010, former Governor Schwarzenegger signed into law the California Open Enrollment Act which establishes a list of 1,000 low achieving schools for each school year. The identification method for the 1,000 Open Enrollment schools is based upon a formula that references the Academic Performance Index (API). Excluded from the list are charter schools, court, community and community day schools, schools that are not of a district of residence, and schools with less than 100 state test scores. Additionally, no district will have more than 10% of its schools on the list. The Open Enrollment Act is not to be confused with School Choice, related to Program Improvement.

The intent of the Open Enrollment Act is to provide parents options for student attendance. The parents of students at one of the 1,000 Open Enrollment schools identified for the school year have the option to request transfer to another school with a higher API score within the district or outside of the district. Once enrolled by the school of choice, the student may remain until the highest grade served by that school without the need to reapply. *Transportation is not provided.*

In October of 2011, the list of 1,000 Open Enrollment schools for the 2012-2013 school year based on the 2011 API scores was published on the California Department of Education website. *Hillside Elementary School* is on the list with an API score of 659. Students attending the identified school within the San Lorenzo Unified School District may request to transfer to another school within the District or may leave the District and request to attend another school outside of the District with a higher API score and that is not on the list.

The Open Enrollment Act allows districts to adopt specific, written standards for acceptance and rejection of applications so long as students who are selected are done so through a random, unbiased process. A district may take into account capacity of a program, class, grade level, school building, or adverse financial impact in determining if and what number of Open Enrollment requests it can approve. If you choose to apply to a school within a district other than the San Lorenzo Unified School District you will need to contact that district for application information, and submit the application directly to that district.

The San Lorenzo Unified School District is committed to continued improvement in providing an educational program and learning environment that meets the needs of its students, families, and community. Should you have any questions or need additional information regarding the Open Enrollment Act, please contact ****** ******, Director of Student Support Services at ************ or *********.

Cordially,

********* ******,

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