The Risks and Benefits of School Integration for Participating Students: Evidence from a Randomized Desegregation Program*†

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Over the last 40 years, efforts to desegregate schools have largely been undone and intra-district programs have limited scope to stem the resulting rise in segregation. This is the first paper to study the short-run and long-run impacts of an *inter-district* desegregation program on the minority students given an opportunity to transfer to majority-white school districts. Students who are given the opportunity to transfer districts attend schools that are 73 percentage points more white than schools they would have attended. Transferring students have higher test scores, and, over the longer run, an increase in college enrollment by 8 percentage points. At the same time, there is an increase in special education classification and arrests, which are largely for non-violent offenses. Both the benefits and the risks of the desegregation program accrue to male students.

JEL Codes: I20, I21, I24, I28.

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I Introduction

Arguably, one of the most important education policy and civil rights accomplishments of the 20$^{\text{th}}$ century is the desegregation of public schools. Between the 1960s and 1980s, integration programs shifted children across schools to reduce the racial isolation of students. While several Supreme Court decisions constrained these to intra-district efforts, they led to substantial decreases in school segregation and raised educational attainment for Black students (Welch and Light, 1987; Rossell and Armor, 1996; Guryan, 2004; Reber, 2010; Johnson, 2015).

But many of these plans are being undone. Their removal has increased school segregation and lowered educational attainment for minority students (Reardon et al., 2012; Lutz, 2011; Billings et al., 2013; Cook, 2018). Meanwhile, "white flight" and suburban migration have undermined the ability of remaining intra-district programs to integrate schools (Coleman et al., 1975; Welch and Light, 1987; Reber, 2005). As a result, school segregation persists (Reardon and Owens, 2014). In 1968, 77% of Black students and 55% of Hispanic students attended majority-minority public schools. Four decades later, 74% of Black students and 80% of Hispanic students attended majority-minority schools (Thompson Dorsey, 2013). The typical minority student today attends a school with fewer white students than their counterpart would have in 1970 (Fiel, 2013). At the same time, the Black-white test-score gap, which had been closing, steadied during the 1980s (Vigdor and Ludwig, 2008; Heckman, 2011).

This paper is the first to look at the short-run and long-run effects of an inter-district integration program. The program, which is ongoing, offers to transfer a population of minority students from a district that serves predominantly low-income Black and Hispanic students to school districts that serve high-income, predominantly-white students. Each year, families with children about to enter kindergarten, first or second grade are eligible for a transfer to one of seven receiving districts. The program is oversubscribed, so a fixed number of applicants are selected at random and assigned to a receiving district. Once assigned, students can remain in the district as long as they do not move from the sending district's boundaries.

The ability to transfer students *across* school districts is critical to integration because the largest determinant of segregation occurs between, and not within, districts (Fiel, 2013). Shifting students within a single district has limited scope to change the demographic characteristics of the schools students attend. For example, when students win admission to their first-choice school in Chicago or in Charlotte Mecklenberg County, the difference in the share of Black or Hispanic students at the school they attend changes by 4 percentage points (Cullen et al., 2006; Deming, 2011). In Chicago, 84% of district students are Black or Hispanic; a recent change to their choice system to make selective schools more diverse can only reduce racial and socio-economic isolation by so much (Ellison and Pathak, 2016).[1] In contrast, the inter-district program studied in this paper causes minority students to attend schools that are 73 percentage points more white than they would have otherwise attended.

Despite their ability to reduce racial isolation substantially, the evidence on these programs is limited. One prominent study on an inter-district integration plan, METCO, provides valuable evidence on the impacts of transferring students on *receiving* students' test scores, which were minimal (Angrist and Lang, 2004).[2] The lack of evidence on the effects for transferring students makes it difficult to assess the value of moving minority students to majority-white school districts.

At the outset, it is unclear whether such a program will help or hurt participating students. On the one hand, access to high-performing schools can increase post-secondary enrollment (Deming et al., 2014) and reduce crime (Cullen et al., 2006; Deming, 2011).[3] Moreover, while that research studies access to high schools, this paper studies access beginning in elementary

[1] Washington, D.C. is similar: only one high school has more than 1% white students.

[2] Rao (2019) studies the effects of a class-based, school-integration program on receiving students in India, and finds substantial improvements in pro-social behavior and small negative impacts on test scores.

[3] Similarly, high-performing charter schools can increase four-year college attendance (Dobbie and Fryer, 2015; Angrist et al., 2016), reduce risky behaviors (Dobbie and Fryer, 2015), and increase earnings (Booker et al., 2014; Dobbie and Fryer, 2016).

school, when investments have the potential for high returns (Heckman and Carneiro, 2003; Heckman and Masterov, 2007; Cunha and Heckman, 2007). On the other hand, there may be risks. In contrast to within-district choice, integrating across school districts often sends minority children to schools in majority-white neighborhoods. Prior research suggests this change could increase the incidence of risky behaviors and arrests, particularly among male youth (Kling et al., 2005; Clampet-Lundquist et al., 2011; Gennetian et al., 2012; Odgers et al., 2015; Boyd and Clampet-Lundquist, 2018). Researchers have examined several hypotheses for this increase, such as additional harassment by police due to profiling, greater police resources in higher-income areas, and changing comparative advantages in certain types of crime (Kling et al., 2005; Clampet-Lundquist et al., 2011; Boyd and Clampet-Lundquist, 2018). These risks may be relevant to students who cross district lines to attend school.

I find that access to low-minority share, higher-income school districts introduces risks and benefits to participating students. In the short run, test scores increase in several subjects. In the longer run, the offer to transfer raises college enrollment by 8 percentage points, which is due to greater attendance at two-year colleges. I present evidence that these results are not driven by changes in school resources alone. There is no effect on the overall likelihood of voting. However, the opportunity to transfer introduces several potential risks. Students are significantly more likely to be classified as requiring special education and significantly *more* likely to be arrested. This is driven primarily by increases in arrests for non-violent offenses. A significant share of these arrests are due to driving-related offenses and occur outside the sending district. I present evidence that is consistent with the integration program causing students to traverse areas outside their home district, which could expose them to greater risk of arrest if these areas have more police resources or higher likelihoods of profiling minority students.

The impacts are heterogeneous and overwhelmingly driven by effects on male students. In the short run, test score impacts and special education classification rates are larger for male youth. In the longer run, male students are more likely to attend college and enrollment effects are significantly smaller for female students. In line with the larger impacts on college enrollment, the effects on arrests are also entirely driven by male students. Male students are more likely to register and vote as well.[4]

Lastly, these results also provide insight to how neighborhoods versus schools affect youth outcomes (Fryer and Katz, 2013). The Moving to Opportunity (MTO) housing-mobility experiment caused families to move to neighborhoods with much lower poverty rates, but had smaller impacts on school environment and short-run academic outcomes overall (Sanbonmatsu et al., 2006).[5] Complementing this work, the program studied here induces large changes in the schools participating children attend while families remain in their original neighborhood.

The rest of this paper is organized as follows. Section II provides background information on the integration program and participating school districts. Section III describes the data and empirical strategy. Section IV presents the results, Section V provides discussion, and Section VI concludes.

II Background

While the 1954 *Brown v. Board of Education* decision mandated the end of racial segregation in schools, *Milliken v. Bradley* (1974) impeded the ability of policymakers to integrate schools across district boundaries (Fiel, 2013). Under this restriction, large-scale busing programs often shifted students across schools within districts. However, factors such as suburban migration and "white flight" (Welch and Light, 1987; Reber, 2005) led to changing white enrollment shares within districts that impeded school integration based on within-

[4]A few paper study the causal effect of schooling on voting outcomes. Milligan et al. (2004) and Dee (2004) use instrumental variables to identify causal effects of schooling on voting behaviors, and they find positive impacts. Sondheimer and Green (2010) analyze several education-related interventions on voting behaviors, including the "I Have a Dream" promise scholarship program, the Perry Preschool experiment and the Tennessee STAR class-size reduction experiment. The authors also find a positive impact on voter participation.

[5]Research studying the impact of neighborhoods on children's outcomes finds heterogeneous effects on education outcomes, risky behaviors and mental health (Rosenbaum, 1991; Kling et al., 2007; Gould et al., 2011). Other, recent research has found beneficial short-run and longer-run effects for children whose families moved when they were young (Schwartz, 2010; Chetty et al., 2016).

district policies. Coleman et al. (1975) find that, while within-district segregation decreased during this time period, it was partly offset by increases in inter-district segregation (Reber, 2005). The sorting of families across neighborhoods, and in turn district boundaries, became central to interracial contact in schools (Rivkin and Welch, 2006).

Like Boston's METCO program, the desegregation program studied in this paper is an inter-district, voluntary-transfer program. The program is born out of a court case in 1976. Following racially-motivated fights in local high schools and the contentious drawing of district boundaries, parents filed a class-action lawsuit against a group of school districts and two counties in Northern California (Jones, 2006). The plaintiffs argued that the racial segregation in eight school districts across the two counties was unconstitutional. Ten years later, a court settlement mandated the eight districts' participation in a transfer program if less than 60% of their students is composed of minority students.

This program offers minority students from a predominantly minority school district the opportunity to transfer to districts that are majority white, and *vise versa*.[6] Minority students originating from the Ravenswood City School District may apply to transfer to one of seven school districts: Palo Alto Unified, Las Lomitas, Menlo Park, Portola Valley, Belmont-Redwood Shores, Woodside and San Carlos. The program has the explicit goal of reducing "the racial isolation of students of color in the Palo Alto, Ravenswood, and other San Mateo County School Districts."[7] The court ordered each district to receive a fixed number of students according to their student enrollment at the time of the settlement. Palo Alto receives the most students, 60, and Woodside receives five students, which is the fewest.[8] Per-pupil funding for these students is divided between Ravenswood and the receiving districts, with 70% going to the receiving district. To put these numbers in perspective, the program offers 166 slots, roughly 70% of were for rising kindergarten students. The kindergarten class for

[6]Over the entire history of the transfer program, only two students have transferred into the predominantly minority school district.

[7]This statement is an excerpt from the Palo Alto School District website describing the program: http://pausd.org/parents/programs/VoluntaryTransfer/

[8]More slots may open if students who have transferred leave the program in later years.

Ravenswood was 580 students in 2000.[9]

Applications are restricted to rising kindergarten, first and second-grade students. Students are assigned to districts via a lottery. Once accepted, districts have discretion over which particular school that child attends if more than one elementary school operates within that district. If a student is not accepted, the family may reapply the following year if they are still in an eligible grade. Once a student has transferred, the student may remain in the receiving school district throughout all of the grades the district offers, so long as they reside within the Ravenswood City School District boundaries. If a student leaves the program after the second grade, they are not permitted to return.

The application and assignment process proceeds as follows.[10] Applications are available in English and Spanish, are made available online, and are distributed to schools and via mass mailing. Families fill out an application in which they write down their district preference rankings (1st choice through 7th choice), their child's grade, their child's race, and whether another sibling is enrolled in the program. Families are only eligible to transfer to a district they list on their application. If, for example, a family only writes down two choices, they only have a chance for admission to those two districts. Families mail or hand deliver this application to the San Mateo County Office of Education.

The county uses a mechanism known to the school-assignment literature as "serial dictatorship" to assign students to district slots. Importantly, this assignment mechanism is strategy proof (Pathak, 2011). Strategy proof implies that it is suboptimal to "game" the system in that the optimal strategy for a parent to receive their preferred choice is to reveal their true preferences on the application form. Accordingly, the county sorts students by sibling priority group and grade and assigns a lottery number. Within a priority group, students assigned a low lottery number are likely to receive their first choice. If slots are all filled for a student's first choice, the process moves down to their second choice; if the slots

[9] Enrollment information from Ed-Data.

[10] This description of the lottery process is based on documentation provided by San Mateo County and the consultant hired to code the lottery program.

for their second choice are filled as well, the process moves down to their subsequent choice (if listed), and so on. Then the process moves to the person with the lottery number one greater. Roughly 80% of students win an offer to transfer. As discussed below, this process has implications for the empirical strategy.

Figure 1 shows the geography of the sending and receiving districts. Ravenswood City School District is predominantly located in East Palo Alto and adjacent to the San Francisco Bay. Menlo Park and Palo Alto share district boundaries with Ravenswood. Ravenswood serves grades K-8 and students' default high school for the sample in this study is located in Redwood City. All receiving districts offer grades K-8 with the exception of Palo Alto, which offers grades K-12. Redwood City, which also shares a boundary, has not participated in the program since 1994 because more than 60% of students are part of a minority-racial group, which is the upper bound for mandatory participation in the program.

Table 1 shows the distribution of families' district preferences and the minimum number of slots that districts are mandated to make available to transfer students. Overwhelmingly families choose Palo Alto as their first-choice district, followed by Menlo Park. 56% of families do not mark a third choice, which implies that, if they do not receive an offer to transfer to either Menlo Park or Palo Alto, they will receive no offer to transfer to any other district. Nearly 90% of families do not mark a seventh choice. That Palo Alto is both the largest receiving district and the district most often ranked first is important for interpreting treatment effects. The impact of a transfer offer will largely identify the effect of receiving an offer to Palo Alto Unified School District.

There are several reasons families might rank Palo Alto and Menlo Park at the top of their preference list. First, these districts are nearest to Ravenswood, which may factor into family choices despite the fact that free transportation is provided. Proximity is a powerful determinant of choice; for instance, Hastings et al. (2005) find that an additional mile of driving distance reduces the odds of choosing a school by 30%. Schools in Woodside and Portola Valley are 11 to 15 miles from Ravenswood—roughly a 30 minute drive away without traffic.

Second, Palo Alto Unified School District has the benefit of offering enrollment through the 12th grade. Students enrolled in other districts would revert back to the neighborhood school by default, which is in Redwood City.[11]

Table 2 provides summary statistics for each district using demographic and 5th grade test-score information from the California Department of Education, district finance information from the Common Core of Data, and census data, all from the year 2000, which is around when children in the sample entered school. Panel A shows district-level information for grade 5 and Panel B shows household-level information for families attending participating districts. Ravenswood has the second-highest student-teacher ratio, the lowest proportion of students classified as special education, the highest students classified as Limited-English Proficiency (LEP), the second-lowest per-pupil spending, and the lowest average proficiency level (Panel A). Ravenswood stands out particularly for LEP status: 65% of students have Limited English Proficiency. The next closest district has 6% of students classified as LEP.

In terms of test scores, which average math and reading state-wide percentile scores, the next-lowest performing district has a percentile score more than twice as high as Ravenswood. Palo Alto ranks three times higher. Though not shown, these test-score disparities are similar between Palo Alto High School and the neighborhood high school for Ravenswood students: in the 9th grade, average percentile rank in math and reading is 84 in the former and 40 in the latter.[12]

Most districts far out spend Ravenswood as well. This is possible because a district like Palo Alto raises more in local property taxes than they would receive from the state. The district opts out of most state funding in favor of local financing primarily through property taxes. Menlo Park and Palo Alto, which receive the most students from Ravenswood, spend

[11] Recently, the school boundaries assigning the neighborhood high school have shifted for some students in the Ravenswood area, but this shift does not apply to the sample studied here.

[12] Test score differences between the assigned high schools for Ravenswood, which is Sequoia Union High School, and Palo Alto are also stark. According to the California Department of Education, 48% passed English-Language Arts and 33% passed math in the 11th grade High School Exit Exam for Sequoia Union High School compared to 86% and 95% for Palo Alto High Schools in the year 2000.

62% more per pupil than Ravenswood City School District.

Demographically, the differences between Ravenswood and other districts are stark. The former is predominantly Hispanic (64%) and Black (24%) with almost no white or Asian children. In contrast, Palo Alto children are 68% white, 19% Asian, 5% Black and 7% Hispanic, which includes Ravenswood transfer students. The median income of Ravenswood residents is just over half of the median income for next poorest district ($45,573 compared to $87,267). Overall, these numbers imply that students who win an offer to transfer may attend schools with significantly greater resources, wealthier surrounding families, and a student body that is largely white.

III Data and Empirical Strategy

The sample frame for this study is based on program application data from 1998 until 2008. Records prior to 1998 are unavailable. These application data are recorded on spreadsheets and contain 2,410 applications. The application data have identifiable information, including name, date of birth, and demographic information, but do not record enrollment or gender. To identify student gender, three independent raters marked students as female, male or uncertain based on each student's first name. If two or more of the raters agreed on male or female, that mark is imputed as a student's gender. Otherwise gender is coded as 0 with an indicator variable for "uncertain."[13]

These data are merged to short-run and longer-run data sources described below.

A Short-Run Outcomes

Test Scores

For the more recent cohorts of the sample, I merge application records to test-score data from the state from grade 2, when testing begins, through grade 8, using students' names and

[13] 6% of the sample is marked as uncertain.

birth dates.[14] The match rate is 76% for students who receive a transfer offer and 74% for students who do not receive an offer, which do not significantly differ (p=0.54). For context, the rate of primary-school attrition from the state data, for instance because students move to private schools or leave the state, is 8% per year. These data contain information on test scores in math, English, science and history. While math and English are tested every year from grade 2 through grade 8, science is only tested in grades 5 and 8, and history is only tested in grade 8. For 85% of applicants, the years of available test score data span the applicant's entire education trajectory through the 8th grade; for the remaining 15%, the available years cover part of their trajectory (e.g. from grade 2 to grade 6).

Enrollment Information

Enrollment information also comes from the state-wide test score data. These data have student-level information on grade level, district enrollment, and school enrollment. Testing begins in grade 2, hence enrollment information begins at grade 2 as well. Enrollment information does not go beyond grade 8. Palo Alto Unified School District offers grades K-12 and all of the other districts offer grades K-8.[15]

Special and Gifted Classification

The test-score data also have indicators for special education and gifted classification. For special education, the data categorize students by the type of disability (e.g. specific learning disability, emotion disturbance, autism, etc.). Special education status has implications for the services afforded to a student, but is often associated with larger achievement gaps and disproportionate representation by minority students (Skiba et al., 2008).

Previous research suggests the effects of these classifications are unclear. Hanushek et al. (2002) finds that special-education classification increases math achievement, while Setren

[14] Unfortunately, test score data for earlier cohorts were not available.
[15] These data do not have information on high school graduation or drop out.

(2017) finds that highly-effective charter schools tend to reduce special education classification at the same time as increasing student achievement.

The impact of gifted programs on gifted-classified students' test scores is also mixed, and minority students are often underrepresented in these programs (Card and Giuliano, 2016). For instance, Bui et al. (2014) and Card and Giuliano (2014) find little impact on test scores, but Cohodes (2015) and Chan (2018) find increases in enrollment in advanced high-school coursework and college.

B Longer-Run Outcomes

College Enrollment

I also link application records to National Student Clearinghouse data. National Student Clearinghouse data have information on college attended, length of enrollment, enrollment status, and degree obtained for more than 3,600 public and private institutions across the United States covering 98% of all students. Importantly, community colleges local to the sample are in the National Student Clearinghouse data. I supplement this information by classifying colleges into selectivity tiers defined by Barron's *Profile of American Colleges*.

For college enrollment, I restrict the sample to students age 16 or older at the time data were linked to the college outcomes, which leaves 1,492 applications—1,353 students. This restriction allows for coverage of dual enrollment students as well, though the results are not sensitive to higher age cutoffs.[16] Dual enrollment occurs when students are enrolled in a college-level course at the same time as taking a high school course, which is offered by Palo Alto High School.

Arrest Records

Data on arrest records come from United Reporting, a private firm that obtains public arrest records through agreements with law enforcement agencies across California as well

[16] 72% of the sample is age 18 or older at the time data were merged to college outcomes.

as Freedom of Information Act requests. Arrest records are public in California for those aged 18 and above. These records cover all but three of California's 58 counties. The three counties not covered are in rural areas far from where the program takes place and they represent less than 1% of the entire population of California.[17] United Reporting matched application data to arrest records using name and birth date.[18] There are 1,178 students aged 18 or over at the time of the data merge.

The arrest records also document the arrest codes, which describe the ostensible reason an individual was arrested. The former are coded into indicators for property-related offenses (vandalism and theft), drug-related offenses (possession or sale of drugs) and violent offenses (assault or battery). Other offenses often indicate driving with a suspended license. For 88% of the arrests I can also determine the city in which the arrest occurred as well.

Voting Records

Lastly, voting outcomes are from California administrative data. These data record whether an individual voted, their voting history in the past seven elections, and whether an individual registered to vote for the 2016 election. Application data are matched using name and birth date to the voting record information. There are 1,465 students eligible for voter registration by the 2016 presidential elections.

C Summary Statistics

Table 3 summarizes the data. Most applicants are Hispanic or Black. The remaining applicants are primarily Pacific Islander. 14% of students are ever classified as special education and 6% are ever classified as gifted.

The percent of students who have ever enrolled in college is 39%, most of whom enroll in two-year public colleges. Some students attend both private and public colleges and both

[17] The counties not covered in the data are Modoc County, Sierra County and Siskiyou County.

[18] Specifically, students are matched using exact birth date but a" fuzzy," Soundex-matching algorithm for matching first and last names.

two-year and four-year colleges at various points in time. Unconditional on enrollment, 27% of students persist through three or more semesters of college. I also define "transfer" as an indicator for whether a student first enrolled in a two-year college and then enrolled in a four-year college, which occurs for 7% of the sample.

Roughly 9% percent of the sample has been arrested at some point after the age of 18. Most arrests fall in the "other" category—they are not violent, drug related, or property related—followed by property and drug-related arrests.

D Empirical Strategy and Enrollment Effects

Estimating Equation

I measure the impact of the desegregation program by estimating the effect of a transfer offer on the short-run and longer-run outcomes described above. The admission process complicates the estimation of this effect. While families' district preferences and sibling status fully determine the probability of admission, a completely saturated model yields many more parameters than observations. Fortunately, the county's assignment process is straightforward and replicable given the application data: admission is based entirely on a set of pre-assignment characteristics observable in the application data. Knowledge of the assignment process, combined with the application data, means that admission offers for all applicants can be generated for any given assignment of lottery numbers. This means the lottery can be simulated many times, holding students' preferences fixed, and recording each time when a student is offered admission to a receiving district. Dividing the number of times a student is offered admission by the total number of simulations yields a probability of admission for each student.

This procedure allows for dimension reduction in the estimating equation. For each student, the fraction of times they are admitted is their propensity score. Unlike many settings where it may be uncertain whether the propensity score estimation satisfies functional-form and selection-on-observables assumptions, the estimator in this setting is unbiased. The applications contain all determinants of admission and the lottery process defines how these variables are used. These probabilities can be constructed with an arbitrary amount of precision, which is determined by the number of lottery simulations (Abdulkadiroğlu et al., 2017). I simulate the lottery 200,000 times to generate the probabilities of admission to any district as well as the probability of admission to each district for every applicant. Importantly, the probability of admission sufficiently characterizes families' preferences and district priority groups such that students' potential outcomes are independent of their program admission (Rosenbaum and Rubin, 1983).

Abdulkadiroğlu et al. (2017) use this strategy to identify causal effects in a similar school-choice set up. In one specification, they enter the probability of admission linearly, as in the following estimating equation, which I estimate:

$$y_i = \beta_0 + \beta_1 \text{Offer}_i + \beta_2 \text{Pr}(\text{Admission})_i + \mathbf{X}_i \beta_3 + \varepsilon_i$$

Where Offer_i is an indicator for student i receiving a transfer offer and $\text{Pr}(\text{Admission})_i$ is the admission probability for student i. $\mathbf{X}_i$ is a vector of control variables for applicants' race, indicators for their district choices, gender, age, grade, year and distance from Palo Alto Unified School District. These controls add additional precision.

When the score is known, using the score via regression and other methods are all likely to reduce bias effectively (Hirano et al., 2003; Imbens, 2004; Imbens and Rubin, 2015). Nevertheless, these methods rely on smoothness assumptions in the regression function and propensity score. Moreover, it is difficult to compare participants with the *exact* same probabilities of admission. As Abdulkadiroğlu et al. (2017) discuss, the simulated propensity score converges to the true propensity score, but, with a finite number of simulations, the estimated score takes on more values than the true score. This occurs because we cannot simulate all possible lottery assignments. Abdulkadiroğlu et al. (2017) recommend rounding the score and then non-parametrically controlling for it by using indicator variables for each value. I show that these alternative specifications yield similar estimates of the effects to the primary specification above.

Standard errors are clustered at the level of treatment assignment: district-by-cohort-by-grade status. When students are observed multiple times for a given test subject across years, I use all of those observations in the estimation to improve precision. The lottery-based assignment should ensure that those who receive offers are similar, in expectation, to those who do not receive offers. Table 4 provides evidence that participants' race, gender and age is balanced across lottery winners and losers. Distance to the school district is a strong predictor of program participation, so I calculate applicants' distance from the Palo Alto Unified School District (measured in miles) as well. The regression is as specified as above, but with controls only for applicants' probability of admission and district choices on the application. There are neither large nor significant differences across lottery winners and losers. A joint test of these variables as predictors of receiving any offer has a p-value equal to 0.70.

Enrollment Effects

Table 5 shows the impacts of receiving an offer on second-grade enrollment for each district. As stated above, the enrollment data come from state test scores, which begin in grade 2. The impact is large and significant for every district, generally ranging from 75 to 85 percentage points, though it is smaller (45 percentage points) for Belmont, which is also the least-preferred school district. The effect on enrollment for Palo Alto Unified School District, which comprises twice the enrollment share of the second-most receiving receiving district, is 82 percentage points. The overall impact on second-grade enrollment to any program school district is 62 percentage points.

Table A.3 shows enrollment effects from grade 2 through grade 8. They remain roughly steady across grades—around 60%. If this rate holds from K-8, then the effect of a program offer on years of enrollment is 5.4 years of additional enrollment relative to students who did not receive a transfer offer.[19]

San Mateo and Santa Clara County provided aggregate records of student attrition for the 2012-2013 school year and the proximate reason students left the transfer program. The 2012-2013 school year does not cover students studied in this paper but is informative nonetheless. Among 1,128 students participating in the program throughout all grade levels and districts in that school year, 58 students left the transfer program (4.7%). The most commonly cited reasons for leaving are moving (28 students), never enrolled (10 students), other (14 students) and returned to Ravenswood City School District (6 students).

IV Results

I separate the results into short-run outcomes, which occur from grade 2 until grade 8, and longer-run outcomes, which occur primarily after high school. As the results are mixed in terms of their benefits to students, I present the findings in their entirety, provide context for the magnitudes, and then I discuss their interpretation and mechanisms after all of the findings have been presented.

A Short-Run Outcomes

Special Education and Gifted Classification

Table 7 shows the effects on special-education and gifted and talented classification and impacts on test scores. Students are 8 percentage points more likely to ever be classified as special needs. This effect is a 57% increase over the mean for lottery losers, which is 14%. Overwhelmingly this is due to students classified as having a "specific learning disability"' (not shown). In general, this is the most common special-education classification,

[19] 83 students in the enrollment sample applied for a transfer as a rising second grader. The first-stage impact for this subset of students is almost identical to the impact for students applying in earlier grades, which is evidence that the roughly 60% impact of an offer is similar for those initially admitted in earlier grades.

and includes dyslexia and dysgraphia. In contrast, there is no significant impact of a transfer offer on gifted and talented classification, and 5% of lottery losers are classified as gifted and talented.

For comparison, Setren (2017) finds that, for new students, traditional public elementary schools classified 2% of students as special needs. Charter school enrollment *reduces* this rate by 1 percentage point. At the same time, these charter schools increase student test scores relative to the sample of lottery losers. The following section examines test-score impacts of the integration program.

Test-Scores

The remaining columns of Table 7 show the effects of winning the lottery on math, English, science and history standardized test scores. There is no detectable effect on math test scores, though this is not a precise zero. However, scores increase in several other subjects: English scores increase by 0.20 standard deviations, science scores increase by 0.15 standard deviations, and history scores increase by 0.28 standard deviations. Where applicable, these results do not vary significantly by grade level, though this is difficult to test with precision (results available upon request).

For comparison, Cullen et al. (2006) find no evidence of winning a school choice lottery in Chicago Public Schools on test scores. Effective charter schools in cities such as Boston, New York and Washington D.C., tend show effects on the order of 0.30 standard deviations for math and smaller impacts on English scores, which typically range from 0.05 to 0.25 standard deviations for district standardized tests (Angrist et al., 2010; Tuttle et al., 2010; Dobbie and Fryer, 2011; Abdulkadiroğlu et al., 2011; Curto and Fryer Jr, 2014). Dobbie and Fryer (2015) use the Woodcock-Johnson test as one measure of learning, and estimate offer effects from the Harlem Children's Zone charter school of 0.28 and 0.12 standard deviations on math and reading, respectively. While fewer papers study the effects of charter schools on science and history, Cohodes (2016) finds that Boston charter schools increase grade-8 science scores by 0.41 standard deviations.

The results found in this paper contrast with studies of small integration programs some 50 years ago, which generally found negligible impacts on short-run academic outcomes (Cook, 1984). One obvious potential source for this difference is the disparity in time periods and contexts between the studies in Cook (1984) and the analysis presented here. I next examine whether these short-run gains in test scores translate into impacts on longer-run outcomes.

B Longer-Run Outcomes

College Enrollment

The main effects of the offer to transfer on college outcomes are shown in Table 8. Panel A shows that an offer to transfer increases the probability of attending college by 8 percentage points. The enrollment effect is concentrated at two-year, public colleges. There is no effect on attending either four-year colleges or private colleges.[20] In terms of persistence, there is a 4 percentage point impact on the likelihood students attend three or more semesters of college, which is significant at the 5% level.

There is little evidence of an overall impact on college selectivity. Panel B of Table 8 shows the effects of the transfer offer on indicators of selectivity, which are ordered with "most competitive" as the most selective and "competitive" schools as less selective. There appears to be a shift from attending "highly competitive" schools to "very competitive" schools, which could be due to the increased number of students going to college and a resulting change in the composition of those who attend. However, most of the schools students attend are less selective and do not fit into any of these categories.

Given the effects on students attending community colleges, there is a potential these students go on to transfer to four-year institutions. Roughly 9 percent of students aged 18 and older transfer in this fashion in the data. However, there is no effect on the likelihood of attending a two-year college followed by a four-year college, as shown in Panel B of Table

[20] There is also no effect on attending four-year public colleges.

8 as well.

These effects are not directly comparable to prior charter-school and school-choice research contexts for several reasons: the enrollment length in this context is much longer and the timing begins at an earlier age than typically studied in charter schools, which usually begin in middle or high school. With these caveats in mind, the effects found here have similarities and differences. Dobbie and Fryer (2015) report an ITT effect on college attendance equal to 5 percentage points. Angrist et al. (2016) do not find significant overall enrollment effects from Massachusetts charter schools. Unlike the integration program, these charter schools tend to shift students from two-year school enrollment to four-year school enrollment.

Given the college enrollment effects are concentrated in two-year colleges, one question is whether these effects could meaningfully affect earnings. Jacobson et al. (2005), Jepsen et al. (2014) and Belfield and Bailey (2011) summarize the evidence on returns, which is typically on the order of 10% for a year of community college credits, even if those credits do not lead to a degree. Thus, there is evidence that the observed enrollment impacts could lead to future earnings increases.

Arrest Outcomes

Table 9 shows the effects of a transfer offer on the likelihood of ever being arrested after the age of 18.[21] There is an increase in the likelihood of arrest by 4 percentage points. The remaining columns show that a significant share of these are arrests stem from "other offenses." Based on the descriptions for each arrest code, these other offenses are not drug related or property related, nor are they violent offenses. Overwhelmingly, the most common of these is driving related, such as driving with a suspended license. There is also a significant, positive effect on drug-related arrests, which include both possession and sale. The point estimates for violent and property-related arrests are near zero and not significant.

These results contrast sharply with the broader literature on school quality and arrest

[21] The impact on college enrollment of a transfer offer is similar for the arrest sample as it is overall: 9 percentage points ($p<0.01$).

outcomes. Cullen et al. (2006) find that effect of winning a school-choice lottery to a high-quality school in Chicago reduces the likelihood of being arrested in the past year by 5 percentage points. Deming (2011) and Dobbie and Fryer (2015) also find reductions in crime-related outcomes as a result of winning an admission lottery to a high-quality school. I delve further into why the results found here may be so different in the discussion section.

Civic Participation Outcomes

Lastly, there are small effects on the likelihood of registering to vote or voting. The first two columns in Panel A of Table 10 shows the results for the entire sample. The effect on registering to vote is 1 percentage points and the effect on voting is negative 3 percentage points. Neither effect is statistically significant.

There has been less evidence using experimental variation in schooling to study voting behaviors. Sondheimer and Green (2010) analyze three education-related interventions on voting behaviors and find 2-9 percentage point reduced-form impacts on the likelihood of voting, depending on the intervention, with Perry Preschool yielding the largest effect. Several other papers use non-experimental variation to study the impacts of education on civic participation, such as Dee (2004), and Milligan et al. (2004) who find positive impacts of educational attainment on voting in the United States.

C Robustness

I follow the empirical strategy in Abdulkadiroğlu et al. (2017), who note that simulated propensity scores tend to take on more distinct values than the actual score and suggest considering various rounded estimates of the score. As a robustness check, they suggest non-parametrically controlling for these probabilities by using dummy variables for each value the scores takes on. Table A.4 and Table A.5 present the short and longer-run results with scores rounded to either the nearest tenth or hundredth in Panels A and B, respectively. The magnitudes are consistent if a bit larger across these alternative specifications, and the results remain statistically significant. Perhaps the primary notable difference in the results is that the effects of gifted classification, which was previously negative and not statistically significant, becomes statistically significant and remains negative.

V Mechanisms and Heterogeneous Effects

One question is whether impacts are entirely driven by resource disparities. Interestingly, spending per pupil varies widely across receiving school districts. Several receiving districts have comparable spending per pupil to Ravenswood. For instance, Ravenswood spends $7,413 per pupil, whereas Belmont-Redwood Shores spends $7,196. Las Lomitas spends $9,151 per pupil. Menlo Park and Palo Alto spend roughly 60% more than Ravenswood, and Woodside spends more than double the amount per pupil that Ravenswood spends. If all the effects are driven by school spending, school quality can be conceptualized by spending per pupil. I assign each student the spending per pupil of the district they are randomly admitted to and I regress college enrollment on this variable conditional on a student's probability of admission to each district and the control variables described in previous regressions.[22] Figure 2 shows the results. The slope of the line is positive but not significantly different from zero; a move from Ravenswood's spending per pupil to Palo Alto's, which is 62% greater, implies an increase in college enrollment equivalent to three percentage points, which is less than half of the effect size found for program admission on college enrollment. This is evidence that school resources, though possibly important, are not the sole driver of impacts.[23]

Perhaps the most important question why arrests increased. A closer look at the arrest codes reveals more information about the nature of these arrests. In particular, it is instructive to break down the "other"-related arrests further. Conditional on arrest, nearly 50% of all students have an arrest in this latter category. The bulk (40%) of these "other"

[22] Formally, I estimate the following: $college_i = \beta_0 + \beta_1 spending_i + \sum_{j=1}^{7} \gamma_i j Probability_i j + X_i \beta_3 + \varepsilon_i$.

[23] The remaining district characteristics are not so smoothly distributed across Ravenswood and the receiving districts. For example, Ravenswood is 1% white, while the next nearest district is 64% white.

arrests have some relation to driving-related offenses. For instance, driving with a suspended license, driving under the influence of alcohol or driving at a high speed. Breaking this category down further, another 10% (conditional on arrest) of offenses are for being "drunk" in public, according to the arrest code.

The increase in arrests may occur for several reasons. First, this could be the result of greater police presence in areas where transfer students spend time compared to where non-transfer students spend time, which may be particularly pertinent given that a significant portion of the arrests are comprised of driving and intoxication-related offenses (Cook and Goss, 1996; Gennetian et al., 2012). Second, there may be greater discrimination or profiling against minority students, emphasized by the large demographic changes induced by the program, and nearly 90% of the sample is Black or Hispanic (Kling et al., 2005; Darity et al., 2015).[24] It is difficult to distinguish between these two hypotheses, but I can examine whether transfer students were more likely to go to a college farther away (greater than 10 miles from the Ravenswood City School District) or closer (less than 10 miles away from the Ravenswood City School District). I can also examine where the increase in arrests occurred. If arrests correspond to a locational shift of the students, then it is likely that the arrests are in Palo Alto, or, if students attend college farther from home, even outside of Palo Alto and East Palo Alto. I study the effects on an indicator for ever being arrested in Palo Alto, another indicator for ever being arrested in East Palo Alto, and a final indicator for ever being arrested outside of Palo Alto and East Palo Alto.

Table A.6 presents the results on the college distance. Table A.6 shows that the most of the overall effect is driven by attendance to colleges farther away. The median distance from Ravenswood City School District to the colleges attended by applicants is 28 miles.

Table A.7 shows the effects on arrest location. Column (1) shows that program participants are 2 percentage points more likely to be arrested in Palo Alto. There is no effect on

[24] For instance, in 2008, the Palo Alto police chief left her position following public comments she made about profiling Black men in Palo Alto (Bulwa, 2008). After a spate of robberies in Palo Alto, the police chief said, "The one suspect around the California Avenue train station was wearing a doo-rag. If my officers see an African-American who has a doo-rag on his head, absolutely the officers will be stopping and asking who that person is" (Keller, 2008).

the likelihood of being arrested in East Palo Alto. Column (3) shows that there is another 2 percentage point increase in the likelihood of being arrested outside of Palo Alto and East Palo Alto. Overall, this pattern of effects is consistent with the integration program causing students to traverse areas outside the Ravenswood area, which leads to greater risk of arrest.[25]

These findings are distinct from the research findings on the effects of school quality. Access to certain charter schools and traditional public schools typically has positive effects on college enrollment (Deming et al., 2014; Dobbie and Fryer, 2015; Angrist et al., 2016), and, if anything, reduces impacts on arrests or incarceration (Cullen et al., 2006; Deming, 2011; Dobbie and Fryer, 2015). Instead, the results of this program tend to blend some of the academic benefits due to higher school quality with the risks—in terms of higher arrest rates—associated with moves to low-poverty neighborhoods for minority children or resegregation (Kling et al., 2005; Gennetian et al., 2012; Billings et al., 2013).

A Effects by Gender

Previous research has shown that changes in where youth live or spend time can increase the prevalence of risky behaviors for male youth (Kling et al., 2005; Gennetian et al., 2012). Kling et al. (2005) suggested that male youth may have had a new-found comparative advantage in certain risky behaviors due to their moves. Qualitative research found that this heterogeneity may arise from differential responses to neighborhoods across gender, such as male youth's increased time spent time in public spaces combined with additional harassment and stops by the police, as well as difficulty navigated varying neighborhood contexts (Clampet-Lundquist et al., 2011). Boyd and Clampet-Lundquist (2018) documented how males who changed neighborhoods were particularly likely to report frequent stops by the police while driving, and more males than females reported regular police questioning overall.

I find significant effect heterogeneity by gender as well.[26] Table 11 shows the results.

[25] There is no significant heterogeneity in effects by distance to Palo Alto Unified School District.

[26] Table A.1 and Table A.2 show that pre-random-assignment characteristics are balanced within male and female students.

Treatment effects are estimated as before, but with the addition of an offer-by-gender interaction to show whether the effects for female students are significantly different from the effects for male students. On the whole, the benefits—and the risks—of the integration program accrue almost entirely to male students. In the short run, female students are less likely to be classified as gifted (significant at the 10% level) and experience much smaller treatment effects on science and history test scores than male students. In the long run, all of the increase in college enrollment is for male students. So is the increase in arrests. There is also evidence that male students are more likely to vote and female students are less likely to vote.[27] Nonetheless, as in previous research on neighborhood changes, it is difficult to disentangle to what extent one particular mechanism may account for these effects over another.

VI Conclusion

Significant segregation across neighborhoods and schools raises important questions about the effects of neighborhood and school segregation on human-capital development, and policies to attenuate this segregation. This paper presents evidence on the effects of a program that creates random variation in the access to higher-resource, low-minority share school districts while approximately holding participants' neighborhood contexts constant. For instance, a shift from the sending district, Ravenswood, to Palo Alto Unified School District would result in a 67 percentage point increase in the share of white students in schools.[28]

From the standpoint of students' wellbeing, the results are decidedly mixed. The impacts on test scores and college enrollment are positive and significant. These results suggest that when segregation impedes access to schools on the margin, there are large, deleterious effects on human-capital outcomes for students. Importantly, there are also increases in the

[27]There is no difference in enrollment rates by gender either overall or for enrollment in Palo Alto Unified specifically (results available upon request).

[28]For comparison, MTO generated modest changes in racial composition of the schools and neighborhoods (Kling et al. find a 6 and 7 percentage point increase the share of white students in schools and neighborhoods, respectively, for the experimental group) relative to the demographic shifts in school composition due to the integration program.

likelihood of arrest. The modal arrest is for a driving-related offense, which is the type of offense that would be consistent with effects generated by either increases in profiling or police resources, or behavioral changes as a result of the program. I present evidence that the former are likely important determinants.

One limitation of this study is that the results may not extrapolate to different settings. Moreover, the results may only pertain to families who apply for this program. These families may be exceptionally involved with their child's education, savvy about navigating the education system, or particularly amenable to school integration. To the extent that these families differ from other families in the Ravenswood school district who do not apply, the results may not generalize.

That said, the integration program discussed here is not unique; similar programs exist in Connecticut, Indiana, Massachusetts, Minnesota, Missouri, New York and Wisconsin (Wells et al., 2009). Wake County, in North Carolina, has implemented an income-based integration program, which reduced racial segregation in schools (McMillian et al., 2015). The Century Foundation released a report documenting 100 school districts and charter schools—representing 9% of public school enrollment across the country—that are pursuing school integration plans (Kahlenberg, 2016). This paper suggests that these policies could lead to longer-run benefits in college enrollment. However, unlike the policies that provide improvements in school quality within a given school district, integration programs should simultaneously consider how to mitigate the outside-school risks participants may encounter.

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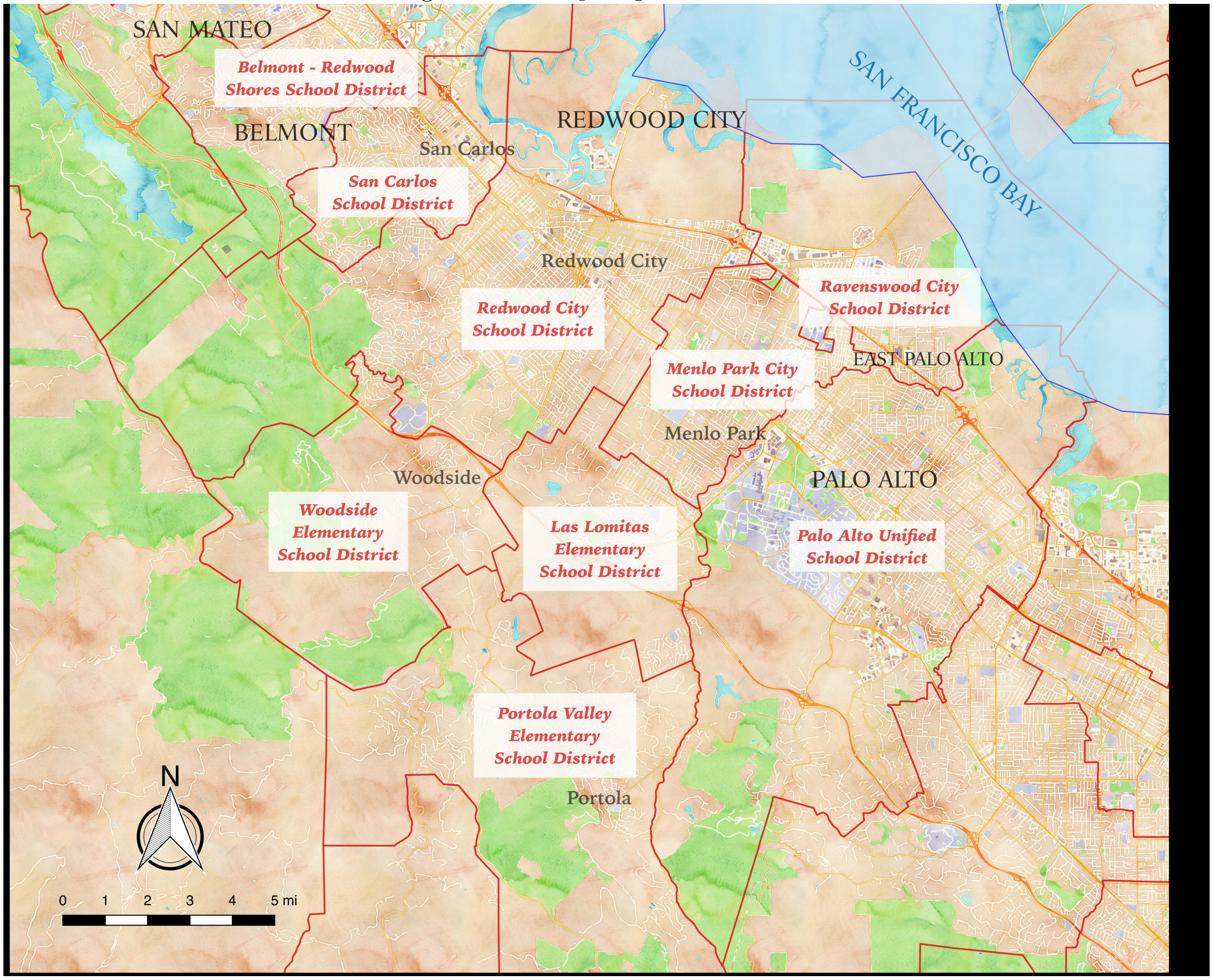
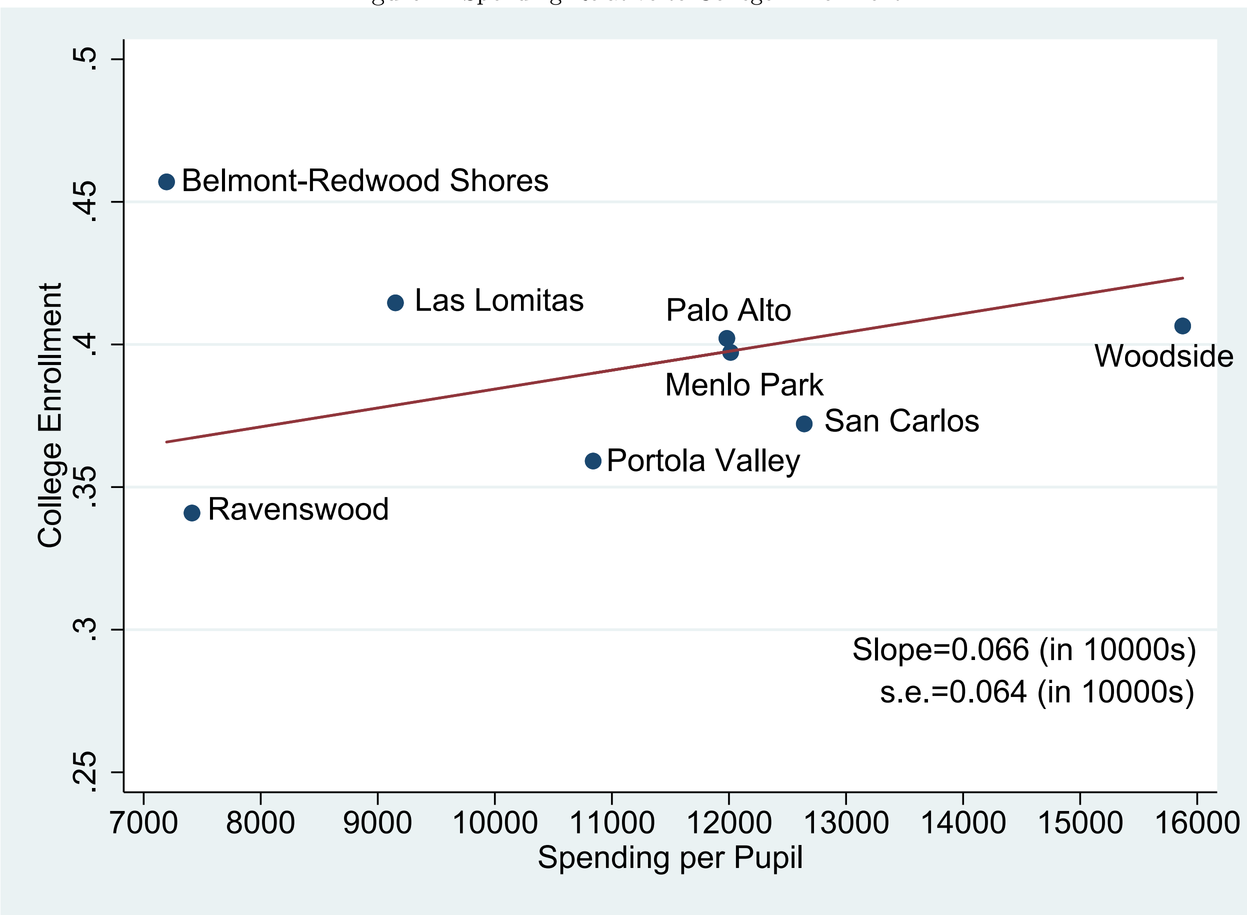
Figure 1: Participating School Districts



This map shows the geographic location of participating school districts in the California Bay Area. Ravenswood City School District is the sending district. The other districts highlighted with white backgrounds are receiving districts. The shape file for this map comes from the National Center for Education Statistics.

Figure 2: Spending Relative to College Enrollment



This figure plots residualized college enrollment on the y-axis and residualized district spending per pupil on the x-axis. The residuals are from regressing an indicator for a college enrollment or spending per pupil on each student's admission probability to each district, race, gender, distance to Palo Alto and age with means added subsequently. The fitted line is the result from a regression of this residualized college enrollment on the residualized spending per pupil of the district students' received a randomly-assigned offer to attend. The coefficient and standard error on spending, multiplied by 10,000, are shown below the line.

Table 1: Distribution of Family Preferences over Districts

	1st	2nd	3rd	4th	5th	6th	7th
None	0%	31%	56%	75%	84%	87%	88%
Belmont-R.S.	2%	2%	4%	4%	4%	3%	3%
Las Lomitas	6%	7%	12%	3%	2%	2%	2%
Menlo Park	16%	40%	7%	2%	0%	1%	0%
Palo Alto	67%	14%	5%	1%	0%	0%	0%
Portola Valley	3%	2%	4%	4%	3%	3%	3%
San Carlos	4%	3%	5%	5%	4%	4%	1%
Woodside	2%	2%	8%	6%	4%	1%	2%

	Belmont-Redwood Shores	Las Lomitas	Menlo Park	Palo Alto	Portola Valley	San Carlos	Woodside
Seats	31	12	24	60	8	26	5

This table choices the share of families marking a particular district as their first through seventh choice within in the sample period for children aged 15 years or older as of fall 2013. This information is constructed from San Mateo County records.

Table 2: District and Household-Level Summary Statistics

Panel A.	**District Information**				
	Student/Teacher	Special Ed.	LEP	Spending/Pupil	Avg. Percentile
Ravenswood	**19.2**	**7%**	**65%**	**7,413**	**28**
Belmont-Redwood Shores	17.9	10%	4%	7,196	72
Las Lomitas	16.8	10%	6%	9,151	90
Menlo Park	18.0	11%	6%	12,014	85
Palo Alto	17.7	11%	5%	11,982	87
Portola Valley	15.8	13%	1%	10,840	89
San Carlos	20.6	7%	2%	12,643	71
Woodside	13.8	8%	4%	15,876	88

Panel B.	**Race\Ethnicity Information**				
	White	Black	Asian	Hispanic	Other Race
Ravenswood	**1%**	**24%**	**1%**	**64%**	**10%**
Belmont-Redwood Shores	64%	3%	16%	11%	1%
Las Lomitas	80%	3%	9%	7%	1%
Menlo Park	78%	4%	6%	8%	3%
Palo Alto	68%	5%	19%	7%	1%
Portola Valley	87%	3%	5%	4%	2%
San Carlos	80%	2%	6%	9%	1%
Woodside	85%	2%	3%	9%	1%

Panel C.	**Household Information**			
	Family Size	Median Income	Below Poverty	No H.S. Diploma
Ravenswood	**3.8**	**$45,573**	**20%**	**54%**
Belmont-Redwood Shores	2.3	$87,267	2%	5%
Las Lomitas	2.4	$125,360	0%	4%
Menlo Park	2.3	$100,827	5%	3%
Palo Alto	2.3	$87,549	4%	4%
Portola Valley	2.7	$162,027	2%	3%
San Carlos	2.4	$87,459	3%	5%
Woodside	2.7	$149,062	0%	7%

Percentile scores and ethnicity are from the California Department of Education data from the year 2000. The average percentile score is the average of grade five math and reading percentile scores. The remaining information in Panel A is from the Common Core of Data. All summary statistics in Panel C are drawn from the year 2000 census.

Table 3: Applicant Summary Statistics

Variable	Mean	Observations
Demographics		
Age	20	1,411
Female	52%	1,403
Black	27%	1,493
Hispanic	59%	1,493
Other Race	14%	1,493
Grade 2 - 8 Outcomes		
Special Education	23%	1,085
Gifted	6%	1,085
College Enrollment		
Ever enrolled	39%	1,492
4-year ever enrolled	17%	1,492
2-year ever enrolled	31%	1,492
Persistence	26%	1,493
Private school ever enrolled	7%	1,492
Public school ever enrolled	36%	1,492
Top three selectivity tiers	5%	1,492
Transfer	7%	1,492
Arrest		
Arrested	8.7%	1,305
Property Offense	2.3%	1,305
Drug Offense	1.9%	1,305
Violent Offense	1.7%	1,305
Other Offense	3.6%	1,305
Voting		
Registered 2016	45%	1,367
Voted 2016	26%	1,367

Data come from application data, state test scores, United Reporting, and the National Student Clearinghouse. Top Three Selectivity Tiers are college selectivity categories defined by Barron's *Profiles of American Colleges*. Transfer is defined as any enrollment in community college prior to attending a four-year college. Gender is inferred from student names. These numbers are for unique, eligible applicants age 16 and older in Fall 2013. Arrest records are from United Reporting and the sample consists of students who were aged 18 and older at the time the data were merged. Voting records are from public California administrative data for any person who registered to vote in the 2016 presidential election.

Table 4: Balance at Baseline

	Age	Female	Black	Hispanic	Other Race	Distance
Offer	0.068	0.029	-0.024	0.001	0.024	0.013
	(0.277)	(0.028)	(0.028)	(0.032)	(0.016)	(0.034)
Joint-Test P Value	0.699					
Observations	1,492	1,492	1,492	1,492	1,492	1,492

All regressions control for the probability of admission to a participating district, the number of applications a student submitted from kindergarten through second grade, and indicators for students' district preferences. See text for exact specification. Data come from program applications for applicants age 16 and older. This information is collected prior to district assignment. Cluster-robust standard errors shown in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 5: Impacts of an Offer on Grade 2 Enrollment, by District

	Palo Alto	Menlo Park	Las Lomitas	Woodside	Belmont	San Carlos	Portola Valley	Any District
Offer	0.82***	0.75***	0.77***	0.85***	0.45***	0.57***	0.84***	0.62***
	(0.02)	(0.04)	(0.06)	(0.08)	(0.11)	(0.07)	(0.06)	(0.04)
Observations	869	869	869	869	869	869	869	869

Each column in the table presents the results from an individual regression. The dependent variable is an indicator for grade-2 enrollment in a particular district. The Offer variable is an indicator for receiving an offer to the district listed in the column header. All regressions control for the admission probability to each district. Additional controls are race, gender, distance to Palo Alto and age. See text for exact specification. Data come from program applications and K-8 test score information from 2000-2008. Cluster-robust standard errors shown in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 6: Impacts of an Offer on District Characteristics

Panel A.	District Academic Indicators				
	Students per Teacher	% Special Edu.	% L.E.P.	Per Pupil Spending ($)	Avg. Percentile
Offer	-1.59***	3.51***	-60.2***	4,240***	57.3***
	(0.12)	(0.15)	(0.12)	(150)	(0.67)
Observations	869	869	869	869	869

Panel B.	District Racial Composition			
	% White	% Black	% Asian	% Hispanic
Offer	72.7***	-20.0***	11.8***	-56.5***
	(1.07)	(0.18)	(1.18)	(0.14)
Observations	869	869	869	869

Each cell in the table presents the results from an individual regression with cluster-robust standard errors shown in parentheses below the coefficient estimate. Each outcome is a district-level characteristic. The Offer variable is an indicator for admission to any receiving district. All regressions control for the probability of admission to a participating district. Additional controls are race, gender, distance to Palo Alto and age. See text for exact specification. Data come from program applications and the California Department of Education. L.E.P. stands for Limited English Proficiency. Avg. Percentile is the average percentile of the state math and English scores of a given district.

*** p<0.01, ** p<0.05, * p<0.1

Table 7: Impacts of an Offer on K-8 Outcomes

	Special Ed.	Gifted	Math	English	Science	History
Offer	0.09**	-0.02	0.05	0.20**	0.15*	0.28***
	(0.04)	(0.02)	(0.09)	(0.06)	(0.09)	(0.10)
Observations	1,085	1,085	5,054	5,054	1,064	432

Each column in the table presents the results from an individual regression. Special education is an indicator for an individual student's special education status. Gifted is an indicator for an individual student's gifted status. Math, English, Science and History are test scores standardized to be mean zero and standard deviation one according to students who did not receive admission to a receiving district. All regressions control for district admission probabilities and additional controls are race, gender, distance to Palo Alto and age. See text for exact specification. Data come from program applications and K-8 test score information from 2000-2008 from the state of California. Cluster-robust standard errors shown in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 8: College Outcomes

Panel A.	**College Enrollment Outcomes**					
	Enrollment	Any 2 yr.	Any 4 yr.	Public	Private	Persistence
Offer	0.08***	0.06**	0.01	0.07***	0.01	0.04**
	(0.03)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)
Observations	1,492	1,492	1,492	1,492	1,492	1,492

Panel B.	**College Selectivity and Transfer**				
	Most Competitive	Highly Competitive	Very Competitive	Competitive	Transfer
Offer	-0.00	-0.01	0.01**	0.01	-0.00
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Observations	1,492	1,492	1,492	1,492	1,492

Each column in a panel presents the results from an individual regression. Enrollment is an indicator for enrollment in any college. Any 2yr, 4yr, public and private are indicators for enrollment in a two-year, four-year, public or private college, respectively. Persistence is an indicator for attending 3 or more semesters of college. Mostly competitive, highly competitive, very competitive and competitive are measures of college selectivity as defined by Barron's *Profile of American Colleges*. All regressions control for the probably of admission to a receiving district. Additional controls are race, gender, distance to Palo Alto and age. See text for exact specification. Data on college outcomes come from the National Student Clearinghouse for eligible applicants age 16 and older and Barron's *Profile of American Colleges*. Cluster-robust standard errors shown in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 9: Effects on Arrests and Offenses

	Arrested	Violent	Property	Drugs	Other
Offer	0.04***	-0.00	0.00	0.02***	0.03***
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	1,305	1,305	1,305	1,305	1,305

Each column presents the results from an individual regression. Arrested is an indicator for ever being arrested; Violent, property, drugs and other are indicators for violent, property, drug or other-related offenses as coded from the arrest codes. Regressions control for the probability of admission to a receiving district, race, gender, distance to Palo Alto and age. See text for exact specification. Data come from state-wide records on arrests in California collected by United Reporting. The sample is students age 18 and older at the time the application data were merged to arrest records. Cluster-robust standard errors shown in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 10: Voting Outcomes

	Registered	Voted
Offer	0.01	-0.03
	(0.03)	(0.02)
Observations	1,367	1,367

Each column presents the results of an individual regression. Registered is an indicator for ever registering to vote and voted is an indicator for ever voting in the 2016 presidential election. Regressions control for the probability of admission to a receiving district, race, gender, distance to Palo Alto and age. See text for exact specification. The sample is students age 18 and older at the time of the 2016 presidential election. Data on voting and registration come from the state of California. Cluster-robust standard errors shown in parentheses.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 11: Heterogeneity by Gender

Panel A.	Short-Run Outcomes					
	Special Ed.	Gifted	Math	English	Science	History
Offer	0.11**	-0.01	0.05	0.15**	0.22**	0.45***
	(0.04)	(0.01)	(0.06)	(0.06)	(0.10)	(0.12)
Offer×Female	-0.04	-0.02*	-0.01	0.11	-0.17*	-0.48***
	(0.03)	(0.01)	(0.08)	(0.08)	(0.10)	(0.13)
Observations	1,085	1,085	5,054	5,054	1,064	432

Panel B.	Longer-Run Outcomes			
	College	Arrested	Registered	Voted
Offer	0.16***	0.07***	0.09***	0.06**
	(0.03)	(0.02)	(0.03)	(0.03)
Offer×Female	-0.16***	-0.05***	-0.17	-0.18***
	(0.03)	(0.01)	(0.03)	(0.02)
Observations	1,492	1,305	1,367	1,367

Each column in a panel presents the results from an individual regression. Special education is an indicator for an individual student's special education status. Gifted is an indicator for an individual student's gifted status. Math, English, Science and History are test scores standardized to be mean zero and standard deviation one according to students who did not receive admission to a receiving district. College is an indicator for ever enrolling in college; arrested is an indicator for ever being arrested; registered and voted are indicators for ever registering and ever voting in the 2016 presidential election, respectively. Regressions control for the probability of admission to a receiving district, race, gender, distance to Palo Alto and age. See text for exact specification. All regressions control for district admission probabilities and additional controls are race, gender, distance to Palo Alto and age. See text for exact specification. Data on short-run outcomes come from program applications and K-8 test score information from 2000-2008. Data on arrests come from state-wide records on arrests in California collected by United Reporting. Data on college enrollment come from the National Student Clearing House. Data on voting come from the state of California. Cluster-robust standard errors shown in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Appendix

Table A.1: Balance at Baseline for Female Students

	Age	Black	Hispanic	Other Race	Distance
Offer	0.269	0.004	0.022	-0.025	0.009
	(0.318)	(0.037)	(0.039)	(0.021)	(0.051)
Joint-Test P Value	0.710				
Observations	725	725	725	725	725

Each column presents the results of an individual regression. All regressions control for the probability of admission to a participating district, the number of applications a student submitted from kindergarten through second grade, and indicators for students' district preferences. See text for exact specification. The sample is restricted to female students. Data come from program applications for applicants age 16 and older and exclude applicants for whom gender could not be identified. This information is collected prior to district assignment. Cluster-robust standard errors shown in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table A.2: Balance at Baseline for Male Students

	Age	Black	Hispanic	Other Race	Distance
Offer	0.028	-0.041	0.012	0.030	0.016
	(0.304)	(0.047)	(0.047)	(0.018)	(0.048)
Joint-Test P Value	0.769				
Observations	678	678	678	678	678

Each column presents the results of an individual regression. All regressions control for the probability of admission to a participating district, the number of applications a student submitted from kindergarten through second grade, and indicators for students' district preferences. The sample is restricted to male students. Data come from program applications for applicants age 16 and older and exclude applicants for whom gender could not be identified. This information is collected prior to district assignment. Cluster-robust standard errors shown in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table A.3: Impacts of an Offer on Enrollment, by Grade

	Grade 2	Grade 3	Grade 4	Grade 5	Grade 6 6	Grade 7	Grade 8
Offer	0.62***	0.61***	0.59***	0.60***	0.60***	0.56***	0.57***
	(0.04)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)
Observations	869	829	791	676	588	492	393

Each column in the table presents the results from an individual regression. The dependent variable is an indicator for enrollment in a receiving district for the grade level in the column header. The Offer variable is an indicator for receiving an offer to any district. All regressions control for the admission probability to any district. Additional controls are race, gender, distance to Palo Alto and age. See text for exact specification. Data come from program applications and K-8 test score information from 2000-2008. Cluster-robust standard errors shown in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table A.4: Robustness: Short-Run Outcomes

Panel A.	Rounded to nearest tenth					
	Special Ed.	Gifted	Math	English	Science	History
Offer	0.07**	-0.02**	0.04	0.20***	0.16*	0.27***
	(0.03)	(0.01)	(0.05)	(0.06)	(0.08)	(0.10)
Observations	1,085	1,085	5,054	5,054	1,064	432

Panel B.	Rounded to nearest hundredth					
	Special Ed.	Gifted	Math	English	Science	History
Offer	0.07*	-0.03**	0.07	0.25***	0.24***	0.36***
	(0.04)	(0.01)	(0.05)	(0.06)	(0.08)	(0.10)
Observations	1,085	1,085	5,054	5,054	1,064	432

Panel A shows results non-parametrically controlling for the probability of admission by rounding them to the nearest tenth and controlling for indicators of each value. Panel B uses the same procedure but rounds these probabilities to the nearest hundredth and controls for each value. Special education is an indicator for an individual student's special education status. Gifted is an indicator for an individual student's gifted status. Math, English, Science and History are test scores standardized to be mean zero and standard deviation one according to students who did not receive admission to a receiving district. Regressions also control for race, gender, distance to Palo Alto and age. Cluster-robust standard errors shown in parentheses.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A.5: Robustness: Longer Run Outcomes

Panel A.	Rounded to nearest tenth		
	College Enrollment	Arrested	Voted
Offer	0.09***	0.05***	-0.02
	(0.02)	(0.01)	(0.03)
Observations	1,492	1,305	1,367
Panel B.	**Rounded to nearest hundredth**		
	College Enrollment	Arrested	Voted
Offer	0.10***	0.06***	-0.01
	(0.02)	(0.02)	(0.03)
Observations	1,492	1,305	1,367

Each column in a panel presents the results of an individual regression. Panel A shows results non-parametrically controlling for admission probabilities by rounding them to the nearest tenth and controlling for indicators of each value. Panel B uses the same procedure but rounds these probabilities to the nearest hundredth and controls for each value. College is an indicator for ever enrolling in college; arrested is an indicator for ever being arrested; voted is an indicator for ever voting in the 2016 presidential election. Regressions also control for race, gender, distance to Palo Alto and age. Cluster-robust standard errors shown in parentheses.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A.6: Proximity of College Attended

	>10 miles away	<10 miles away
Offer	0.08***	0.02
	(0.02)	(0.01)
Observations	1,492	1,492

Each column in a panel presents the results of an individual regression. Regressions control for the probability of admission to a district, race, gender, distance to Palo Alto and age. See text for exact specification. The dependent variable is an indicator for attending a college either more than 10 miles from Ravenswood City School District or less than 10 miles from the district. College information is from the National Student Clearing House. Robust standard errors shown in parentheses.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A.7: Location of Arrest

	Ever Arrested in a given Location		
	Palo Alto	East Palo Alto	Neither
Offer	0.02*	0.00	0.02*
	(0.01)	(0.01)	(0.02)
Observations	1,193	1,193	1,193

Each column in a panel presents the results of an individual regression. Regressions control for the probability of admission to a district, race, gender, distance to Palo Alto and age. See text for exact specification. The dependent variable is an indicator for being arrested in Palo Alto, East Palo Alto, and Neither Palo Alto nor East Palo Alto in columns 1-3, respectively. Data on arrest location is from United Reporting.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$