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The Effects of School Integration: Evidence from a Randomized Desegregation Program

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Abstract

This paper studies the impact of a desegregation ruling on several medium-run outcomes. This ruling mandates that seven school districts, which serve higher-income, predominantly-white families, accept a group of minority elementary school students who apply to transfer from a nearby, predominantly-minority school district. Slots are allocated via lottery. The offer to transfer raises college enrollment by 10 percentage points. This is due to greater attendance at two-year colleges and particularly for male students. There is evidence male students are also more likely to vote. In contrast, transferring increases the likelihood of arrest. This is driven by increases in non-violent offenses.

JEL-Codes: I200, I210, I240, I280.

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I Introduction

More than 50 years after the Supreme Court ruled that school integration must proceed with “all deliberate speed,” segregation across neighborhoods persists (Reardon and Owens, [Forthcoming](#)). This fact is unsettling because segregation is correlated with a number of adverse outcomes, such as higher rates of poverty and crime (Ludwig et al., 2012). Across schools, segregation is associated with fewer resources, lower peer quality and poor performance (Card and Rothstein, 2007; Reber, 2010; ?; Vigdor and Ludwig, 2007). Yet it is unclear to what degree neighborhood and school segregation contributes to these poor education outcomes. The Moving to Opportunity (MTO) housing-mobility experiment generated large reductions in neighborhood poverty rates and modest reductions in share minority for the experimental group, but had small impacts on school environment and short-run academic outcomes overall (Sanbonmatsu et al., 2006). Other analyses on the impact of neighborhoods on children’s outcomes find heterogeneous effects (Rosenbaum, 1991; Gould et al., 2011) and beneficial effects for children whose families moved when they were young (Schwartz, 2010; Chetty et al., 2016). A complementary experiment would help answer an important question for child development: Holding neighborhood characteristics constant, does access to low-minority share, higher-income school districts improve child outcomes?

This question is difficult to answer because most school-choice and school-integration plans shift students between schools *within* a district, but the largest determinant of segregation is *across* school districts (Fiel, 2013). Moreover, it is difficult to find exogenous variation in access to low-minority-share schools for minority students. This paper addresses these difficulties by studying an on-going, court-settled desegregation program. In contrast to the typical integration plan, the program studied here offers to transfer a small population of minority students from a low-income, predominantly Black and Hispanic-attended school district to school districts that serve a higher income, predominantly-white demographic. Each year, families with minority children about to enter kindergarten, first or second grade

are eligible for a transfer to one of seven receiving districts. Students in other grades are ineligible. Importantly, the program is oversubscribed so a fixed number of applicants are selected at random and assigned to a receiving district. In the years studied here, no more than 200 students are assigned across the seven districts each year. Once assigned, students can remain in the district as long as they do not move from the sending district boundaries. Thus students who win the lottery gain access to higher-resource, majority-white schools at an early age, but cannot change neighborhoods without being removed from the program.

I link application data to several medium-run outcomes for participants including college enrollment, public arrest records, and voter records. I find that access to low-minority share, higher-income districts has a large impact on college enrollment. Overall, the offer to transfer school districts increases the likelihood of attending college by 10 percentage points. The effects are concentrated in attendance to public, two-year colleges. There is no overall effect on the likelihood of attending private colleges.

These impacts are heterogeneous. Male students become more likely to attend two year, four year, public or private colleges, while enrollment impacts are significantly smaller for female students. In line with the larger impacts on college enrollment, there is some evidence male students are also more likely to vote as well. There is no effect on the likelihood of voting overall however.

In striking contrast, there is a significant overall increase in the likelihood of arrest. Again, this is particularly true for male students, with no effects on female students. This increase in arrests is primarily driven by offenses that are non-violent, such as driving with a suspended license followed by drug and property-related offenses. A significant share of arrests occur outside the sending district. I present evidence that the integration program causes students to traverse areas farther from home, which could expose them to greater risk of arrest for non-violent or driving-related offenses.

A number of papers have studied the effects of district-wide, court-ordered desegregation, primarily implemented during the 1960s and 1970s, and found evidence of increased attain-

ment for Black students (Guryan, 2004; Reber, 2010; Johnson, 2015). More recently, Lutz (2011) and Billings et al. (2013) study the end of court-ordered desegregation. Billings et al. (2013) examine Charlotte-Mecklenburg schools and find increases in segregation and disparities between minorities and whites. Terminating school integration modestly decreased test scores, increased crime among minority males and lowered attainment. Interestingly, older random-assignment studies of desegregation show no effect on short-run academic achievement (Cook, 1984; Rivkin and Welch, 2006).

This paper also relates to research by Angrist and Lang (2004), who studied peer effects of the Metco program, which is a similar, though not lottery-based, transfer program across districts. The authors focus on the impact of transfer students on receiving students and find little impact, but there are small negative effects on certain minority receiving students. Thus a key distinction between this paper and the research by Angrist and Lang (2004) is that they study the effects on receiving students while this paper studies the effects on the transferring students.

In addition to studying the long-run impacts of a desegregation program, this paper also contributes to research on how schools impact longer-run outcomes for students. Several papers examine the effects of high-performing charter schools on college attendance and non-academic outcomes. Angrist et al. (2013) use admission lotteries to show significant, causal effects of a group of Boston-area charter schools attendance on college attendance, which shift students from attending two-year colleges to four-year colleges. Likewise, Dobbie and Fryer (2013) gathered admission lottery and survey data to study a range of students' medium-run outcomes, including college enrollment and risky behaviors. They find a similar effect on college attendance and also a reduction in teen pregnancy for students who attend schools in the Harlem Children's Zone. Booker et al. (2014) use non-experimental estimates that suggest charter schools impact post-secondary attainment and earnings. Deming et al. (2014) show that in Charlotte-Mecklenburg Schools, which offers school choice, the opportunity to attend a first-choice high school improves post-secondary outcomes.

In general, these studies measure the impact of high-quality middle and high schools. This paper complements the research above by studying the effect of lottery-based access to schools at much earlier ages, when children begin elementary school, which is potentially a critical period of human capital investment ([Cunha and Heckman, 2007](#); [Heckman and Carneiro, 2003](#)).

More broadly, researchers have examined neighborhood effects on a number of longer-run outcomes of children. Revisiting the Moving to Opportunity housing mobility experiment, [Chetty et al. \(2016\)](#) find substantial increases in college enrollment and earnings for families with young children who moved to low-poverty neighborhoods. However [Gennetian et al. \(2012\)](#) show evidence of an increase in the likelihood of arrest for property-related crimes, and [Kling et al. \(2005\)](#) find increases in property-related arrests for males. Lastly, [Magre et al. \(2014\)](#) looks at how MTO impacted voting behavior and finds evidence that MTO results in *lower* voter participation rates.

There has been less evidence using experimental variation in schooling to study voting behaviors. [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.

The rest of this paper is organized as follows. Section II provides background information on the transfer 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 limited the scope for school integration based on within-district policies. Coleman et al. (1975) find that while within-district segregation decreased during this time period, it was partly offset by increases in interdistrict 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).

In contrast to more common intradistrict-desegregation programs, the desegregation program studied in this paper is an interdistrict, voluntary-transfer program. This program is borne out of a court ruling 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 district participation in a transfer program if less than 60% of that district 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 *vice versa*.¹ Minority students originating from the Ravenswood City School District may apply to transfer to one of seven school districts: Palo Alto Unified, Las Lomas, Menlo Park, Portola Valley, Belmont-Redwood Shores-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,

¹Over the entire history of the transfer program, only two students have transferred into the predominantly minority school district.

and other San Mateo County School Districts.”² 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.³ 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, primarily for entering kindergarten students, and the kindergarten class for Ravenswood was 580 students in 2000.⁴ Typical enrollment in the transfer program is roughly 150 students.

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.⁵ 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. Importantly, 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.

²This statement is an excerpt from the Palo Alto School District website describing the program: <http://pausd.org/parents/programs/VoluntaryTransfer/>

³More slots may open if students who have transferred leave the program in later years.

⁴Enrollment information from [Ed-Data](#).

⁵This description of the lottery process is based on documentation provided by San Mateo County and the consultant hired to code the lottery program.

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 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 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 serves 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 bound for mandatory participation in the program.

Table 1 shows the distribution of families’ district preferences and the minimum number of slots districts are mandated to make available to transfer students. Overwhelmingly families choose Palo Alto as their first-choice district, followed by Menlo Park. Interestingly, 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 12th grade. Students enrolled in other districts would revert back to the neighborhood school by default, which is in Redwood City.⁶

Table 2 provides summary statistics for each district using demographic and fifth 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 five 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 ranks, the next-lowest performing district has a percentile rank 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:

⁶Recently, 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.

in the 9th grade, average percentile rank in math and reading is 84 in the former and 40 in the latter.

Most districts far out spend Ravenswood as well.⁷ Menlo Park and Palo Alto, which receive the most students from Ravenswood, spend 62% more per pupil than Ravenswood City School District.⁸

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

A Data

This study draws data from several sources. The first is 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. This does not include either enrollment data or district transcript data, which I do not have. The application data have identifiable information, including name, date of birth, and demographic information.

While I do not have individual-level data on the actual enrollment in the receiving districts, San Mateo and Santa Clara County provided records of student attrition for the 2012-2013 school year and the proximate reason students left the transfer program. The

⁷This is possible because a district like Palo Alto opts 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.

⁸Note that Palo Alto is a unified school district serving grades K-12, which implies that per-pupil spending numbers are not directly comparable to Ravenswood.

2012-2013 school year does not cover students studied in this paper but it may be 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). San Mateo County also has information on aggregate enrollment in more recent years. Based on these data, the first-year enrollment rate conditional on receiving an offer was 94% in 2011. From 2011 to 2012, kindergarten cohorts between 2003-2004 and the 2007-2008 experienced between two and four percentage point attrition. Assuming four percent annual attrition after a 94% initial enrollment rate, a cohort of students receiving an offer to enroll in Palo Alto Unified School District remains there for 9 years on average.

The data also do not have information on student gender. Student gender is therefore inferred. Three independent raters marked students as female, male or uncertain based only 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." 6% of the sample is marked as uncertain.

Using names and birth dates from the applications, students are linked 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 and 98% of all students in public and private institutions across the United States. Importantly, community colleges local to the sample are in the National Student Clearinghouse data as well. This information is supplemented by classifying colleges into selectivity tiers defined by Barron's *Profile of American Colleges*.

Restricting the sample to students age 16 or older at the time data were linked to the college outcomes in the spring of 2015, there are 1,493 applications—1,383 students. This restriction allows for coverage of dual enrollment students as well. Dual enrollment implies students are enrolled in a college-level course at the same time as taking a high school course.

Figure 2 shows the distribution of age among applicants in the sample. 72% of the sample is age 18 or older at the time data were merged to college outcomes.

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 as freedom-of-information-act requests. Arrests after the age of 18 are public record. 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.⁹ Application data are matched to arrest records using full name and birth date.¹⁰ There are 1,205 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 non-exclusive indicators for property-related offenses (vandalism and theft), drug-related offenses (possession or sale of drugs) and violent offenses (assault or battery). Other offenses typically indicate driving with a suspended license. For 88% of the arrests I can also determine the city in which the arrest occurred as well.

Lastly, voting outcomes are from California administrative data. These data record whether an individual voted, their voting history in the past seven elections, whether an individual registered to vote for the 2012 election as well the individual’s name and birth date. Application data are therefore matched using name and birth date to the voting record information. There are 675 students eligible for voter registration by the 2012 presidential elections.

Table 3 summarizes the data. Most applicants are Hispanic or Black, followed by Asian-Pacific Islander. 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

⁹The counties not covered in the data are Modoc County, Sierra County and Siskiyou County.

¹⁰Specifically, students are matched using exact birth date but a “fuzzy,” Soundex matching algorithm for matching first and last names. Middle or second names are omitted.

colleges and both 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, followed by property and drug-related arrests. Males are more likely to be arrested than female students; 11% of male students have been arrested while 6% of female students have been arrested.

B Empirical Strategy

I measure the impact of the desegregation program by estimating the effect of a transfer offer on college outcomes. I study the effect of an offer, which is an Intent-to-Treat (ITT) effect, because I do not have detailed enrollment data.

While district preferences and sibling status fully determine the probability of admission, a completely saturated model yields many more parameters than observations. To reduce dimensionality, I simulate 200,000 lotteries to generate admission probabilities for each applicant, as do [Abdulkadiroglu et al. \(2015\)](#) to estimate the effects of charter schools in Denver. Conditioning on the probability of admission to each district is sufficient to eliminate selection bias ([Rosenbaum and Rubin, 1983](#)). To account for heterogeneous treatment effects by district, I estimate separate district effects conditioned on probabilities of admission, compute the weighted average of effects according to enrollment shares, and conduct hypothesis tests based on this statistic. The estimating equation is as follows:

$$y_i = \beta_0 + \mathbf{Dist}_i\beta_1 + \mathbf{Admission}_i\beta_2 + \mathbf{X}_i\beta_3 + \varepsilon_i$$

Where \mathbf{Dist}_i is a vector of district assignments and $\mathbf{Admission}_i$ is a vector of associated admission probabilities for each applicant. \mathbf{X}_i is a vector of controls, entered separately, for

applicants' race, gender and age. Inference is conducted using conventional, robust standard errors as in [Abdulkadiroglu et al. \(2015\)](#).

[Abdulkadiroglu et al. \(2015\)](#) note that simulated propensity scores tend to take on more distinct values than the actual score. As a robustness check, they recommend considering various rounded estimates of the score and non-parametrically controlling for these probabilities by using dummy variables for each value it takes on. I conduct this analysis in the section on robustness.

The lottery-based assignment should ensure that those who receive offers are similar, on average, 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 could be a strong predictor of program participation, so I calculate applicants' distance from the Palo Alto Unified School District (measured in miles) as well. 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 0.616.

IV Results

A College Enrollment Outcomes

The main effects of the offer to transfer on college outcomes are shown in Table 5. The top row shows estimates without controls and the lower panel adds controls for race. Overall, the offer increases the probability of attending college by 10 percentage points. This effect is not sensitive to the addition of controls for race and gender. All remaining tables of results are shown with controls.

The enrollment effect is concentrated within two-year, public colleges. There is no effect on attending either four-year colleges or private colleges.¹¹ In terms of persistence, there is a 6 to 7 percentage point impact on the likelihood students attend three or more semesters of

¹¹There is also no effect on attending four-year public colleges.

college, which is significant at the 10% level. Though not shown, students are highly likely to persist two or more semesters, which is statistically significant at the 5% level.

Table 6 explores important heterogeneity in the results. This table shows the results of splitting the sample by race and gender and re-estimating the effects on college enrollment. Panel A shows effects for Black students. These effects show a similar pattern to the overall results, but are muted in magnitude. For instance, the effect on enrollment is smaller at 7 percentage points and the effect on persistence is slightly negative.

Panel B shows that effects are for Hispanic students more in line with the overall results, which is not surprising given the results above and because Hispanic students are the largest demographic group in the sample. Hispanic students are 9 percentage points more likely to enroll in college.

Panels C and D show the starkest contrasting results, which are by gender. The effects are concentrated among male students (Panel D). For males, the impact is large and significant across all college-enrollment outcomes, including attendance at a private university. College enrollment increases by 13 percentage points. Attendance at four-year colleges is 7 percentage points and significant at the 5% level. The enrollment impact implies a 33% increase over mean college attendance at age 18 for males.

There is a smaller (5 percentage point) effect on female college attendance overall (Panel C). All coefficients are not significant and generally smaller than the other results. This implies that the offer to transfer districts substantially closes the gender gap in college attendance. For context, the regression-adjusted enrollment gap between male and female students in the sample is 15 percentage points in favor of female students.

There is little evidence of an overall impact on college selectivity. Panel A of Table 7 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 is a slight shift from attending “highly competitive” schools to “very competitive” schools. However, most of the schools students attend are less selective and do not fit into any of

these categories, and there is no overall effect on selectivity (Panel B, column 1). Nor are there differential effects by race; all coefficients are small and not significant, though there is some evidence male students attend more selective schools.

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 either overall or for any demographic subgroup on the likelihood of attending a two-year college followed by a four-year college (Table 8).

B Arrest Outcomes

Table 9 shows the effects of a transfer offer on the likelihood of ever being arrested after the age of 18. There is an increase in the likelihood of arrest by 5 percentage points. The remaining columns show that these effects are primarily driven by arrests related to “other offenses.” Based on the descriptions for each arrest code, these other offenses are not drug related or property related or violent. Overwhelmingly the most common of these is driving related, such as driving with a suspended license. There is also a significant positive effect of drug-related arrests, which include both possession and sale.

The point estimates for violent and property-related arrests are near zero and not significant, though there is heterogeneity in the latter, which is discussed below. Moreover, there is no effect on the *number* arrests, which implies that the program causes an increase in arrest on the extensive margin, and particularly for non-violent offenses and non-property related offenses.

The heterogeneity in effects is largely in line with the heterogeneity found for college enrollment. Table 10 shows the most significant departure is that Black students experience the largest positive increase in arrests (significant at the 10% level). Though the results lack precision, this is primarily accounted for by increases in “other”-related offenses.

Again, there are small effects for female students, and large, positive effects on male

students. Masked in the overall effects on arrests, there is a statistically significant, positive effect on property-related arrests for male students, which is offset by negative effect on these arrests for female students. Similar to the overall results however, the largest reason for arrest fits into the “other” category.

C 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 11 shows the results for the entire sample. The effect on registering to vote is 4 percentage points and the effect on voting is 1 percentage point. Neither effect is statistically insignificant.

If the positive effects on college enrollment reflect positive effects in other domains such as voting, larger impacts would be expected for male students. There are indeed larger and marginally significant effects for males. There are opposing, though not significant, effects female students. The point estimates are all close to zero for Hispanic and Black students. These subgroup results are only suggestive however as much of the sample was too young to be eligible to either register to vote or vote in the 2012 election.

D Robustness

Abdulkadiroglu et al. (2015) 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.1 presents the main results with scores rounded to either the nearest tenth, hundredth, or thousandth in Panels A, B and C respectively. The magnitudes are consistent across specifications and all results remain significant at the 5% level, though the standard errors do increase as the controls become more flexible.

Table A.2 looks at differential impacts by district for the main outcomes. Unsurprisingly

given its enrollment share and family preferences, the results for Palo Alto largely mirror the results overall. The effects on college enrollment and arrests are particularly large for Belmont-Redwood Shores and San Carlos, respectively. None of the patterns or significance of the results is significantly altered if these districts are excluded from each of these analyses (results available upon request).

E Discussion

The impacts are positive on college attendance though these overall positive effects on college outcomes mask significant heterogeneity. Female students receive less benefit from the transfer program in terms of college enrollment in contrast to male students, who experience large gains in college enrollment.

Given that these impacts are concentrated in two-year colleges, one question is whether these meaningfully could affect earnings. [Jepsen et al. \(2014\)](#) find that degrees or diplomas from community colleges result in a \$1,500 increase in quarterly earnings for men. Similarly, [Stevens et al. \(2014\)](#) find earnings returns to a degree ranging from 10% to 20% for males. Lastly, [Belfield and Bailey \(2011\)](#) review the evidence on the returns to community college attendance, which shows a 9% earnings return even to credits that do not lead to a degree.

These enrollment results contrast with the results of random assignment studies some fifty years ago, which examine short-run outcomes and demonstrate small or negligible impact of desegregation on academic outcomes ([Cook, 1984](#)). One obvious potential source for this difference is disparity in time periods between the studies in [Cook \(1984\)](#) and the analysis presented here. A second factor may be that interventions occurring early in childhood can have effects on important long-run outcomes despite the fact that short-run cognitive impacts fade ([Duncan and Magnuson, 2013](#)). This is the case for programs such as Perry Preschool, Head Start and Nurse Family Partnerships ([Currie and Thomas, 1995](#); [Deming, 2009](#); [Heckman et al., 2013](#); [Olds, 2006](#)), and could be the case for the transfer program as well. Students transfer relatively early in childhood, albeit at slightly older ages than

children participating in the programs mentioned above.

The school-integration-transfer effects are not directly comparable to charter-school and school-choice impacts for several reasons. The potential 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 are larger. [Dobbie and Fryer \(2013\)](#) report an ITT effect on college attendance equal to 5 percentage points, which primarily shifts students from attendance at two-year schools to attendance at four-year schools. [Angrist et al. \(2013\)](#) show a similar size and pattern of results for college outcomes.

In general, there seems to be no consistent pattern to the heterogeneous effects of schools or neighborhoods on academic outcomes. [Angrist et al. \(2013\)](#) find slightly stronger effects for males on the likelihood of attending any college and similar effects across gender for four-year college attendance, though the disparity is not nearly the size found in this transfer program. [Deming et al. \(2014\)](#) find overall college-enrollment effects of receiving an offer to attend a first-choice school, which stem from the gains accrued by female students.

Recent research has studied the longer-run impacts of the Moving to Opportunity project on children. For youth aged 13 to 20, a similar age group to the youth studied in the transfer program, [Gennetian et al. \(2012\)](#) find there was no impact on math and reading assessments. [Chetty et al. \(2016\)](#) find a 2.5 percentage point increase in on-time college attendance for families with children under the age of 13 who participated in MTO. Interestingly, Chetty, Hendren and Katz do not find differential impacts for gender, which contrasts with the shorter-run impacts which generally found more positive effects for female children and than male children ([Kling et al., 2007](#)).

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. They do not analyze heterogeneous effects. I find

small, insignificant effects on voting overall, though offsetting effects for male and female students. More conclusive results would be found by tracking voting behaviors in the upcoming 2016 presidential election.

The positive effects on college enrollment are dampened by the increased likelihood of arrest. A closer look at the arrest codes reveals a bit more information about the nature of these arrests. In particular, it is instructive to break down the “other”-related arrests to a finer level. Conditional arrest, just over half (52%) of all students have an arrest in this latter category. The bulk (40%) of these “other” 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.

To break down the treatment effects further, I create an arrest variable that is a zero for any arrest solely related to these two categories and a one for an arrest that has any relation to drugs, property or violence. Table A.3 shows the overall effect on arrests, which drops by roughly 50% compared to the previous results and becomes marginally insignificant (p-value=0.12). The effect for female students is now zero, and the effect for male students is 40% smaller. In results not shown, the remainder of arrests for male students are primarily driven by property-related offenses.

Given that a significant portion of the arrests are caused by driving and intoxication-related offenses, this may be caused by greater police resources to address these potential crimes compared to where non-transfer students travel (Cook and Goss, 1996; Gennetian et al., 2012). Alternatively, there may be discrimination against minority males that increases their likelihood of being stopped relative to students not receiving the transfer offer (Kling et al., 2005). It is difficult to distinguish between these two hypotheses, but I can check if transfer students were more likely to go to a college far away (greater than 15 miles from the Ravenswood City School District) or closer (less than 15 miles away from the Ravenswood

City School District). The median distance from Ravenswood City School District to the colleges attended by applicants is 28 miles. Table A.4 shows that the bulk of the overall effect and the effects on male students is driven by attendance to colleges farther away, as there little effect on the likelihood of attendance to colleges close by.

Lastly, I also examine where the increase arrests occurred. If arrests correspond to a locational shift of the students, then it is likely that the arrests are in Palo Alto or, given that 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.5 presents the results of this analysis. Column (1) shows that program participants are 2 percentage points more likely to be arrested in Palo Alto. There is no effect on 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, these pattern of effects are consistent with the integration program causing students to traverse areas farther from the Ravenswood area, which possibly exposes them to greater risk of being arrested for non-violent offenses as a result.

V Conclusion

Significant segregation across neighborhoods and schools raises important questions about the effects of neighborhood and school segregation on human-capital development. This paper presents evidence on the effects of a natural experiment that creates random variation in access to higher-resource, low-minority share school districts while approximately holding neighborhood characteristics constant. The impacts on college enrollment are large and significant and benefit male students in particular. The latter experience gains across a range of college-related outcomes: two-year and perhaps four-year enrollment, as well as

private and public-school enrollment, and persistence through multiple semesters. These results suggest that when segregation impedes access to schools, on the margin there are large, deleterious effects on human-capital outcomes for students often deemed most at risk.

? cites improvements in peer quality and changes in school expectation for achievement as possible mechanisms for integration to increase educational attainment. Interviews with students indicate these mechanisms may play role in this context as well. In a local newspaper interview with two former transfer students, one student recalls a counselor connecting her with a Palo Alto High School graduate currently enrolled in college for guidance ([Kenrick, 2012](#)). Other students express pride in doing work “above grade level” ([Bischoff, 2014](#)).

However, there is a corresponding increase in the likelihood of arrest concentrated among male students. The modal arrest is for a driving-related offense, which is the type of offense that would be consistent with effects generated by some combination of contextual change and actual behavioral changes induced by the program. I show evidence that the former is likely an important determinant.

The transfer program discussed here is not unique; similar programs exist in Connecticut, Indiana, Massachusetts, Minnesota, Missouri, New York and Wisconsin ([Wells et al., 2009](#)). Policies that aim to integrate schools, such as the White House’s \$120 million dollar proposal to incentivize the integration of schools, could reap long-run benefits in college enrollment. However these policies should simultaneously consider programs to mitigate the potential risks for participating students as well.

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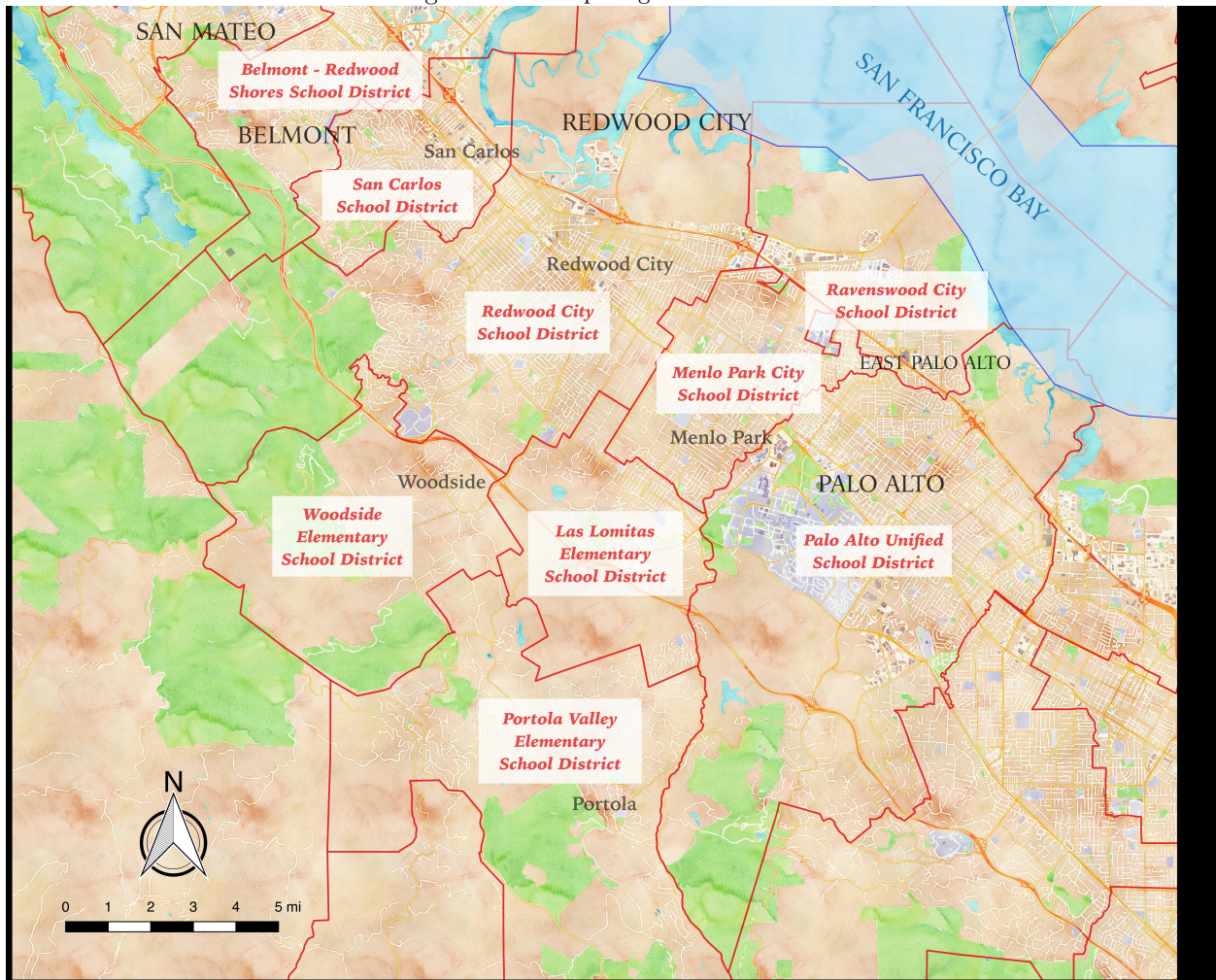
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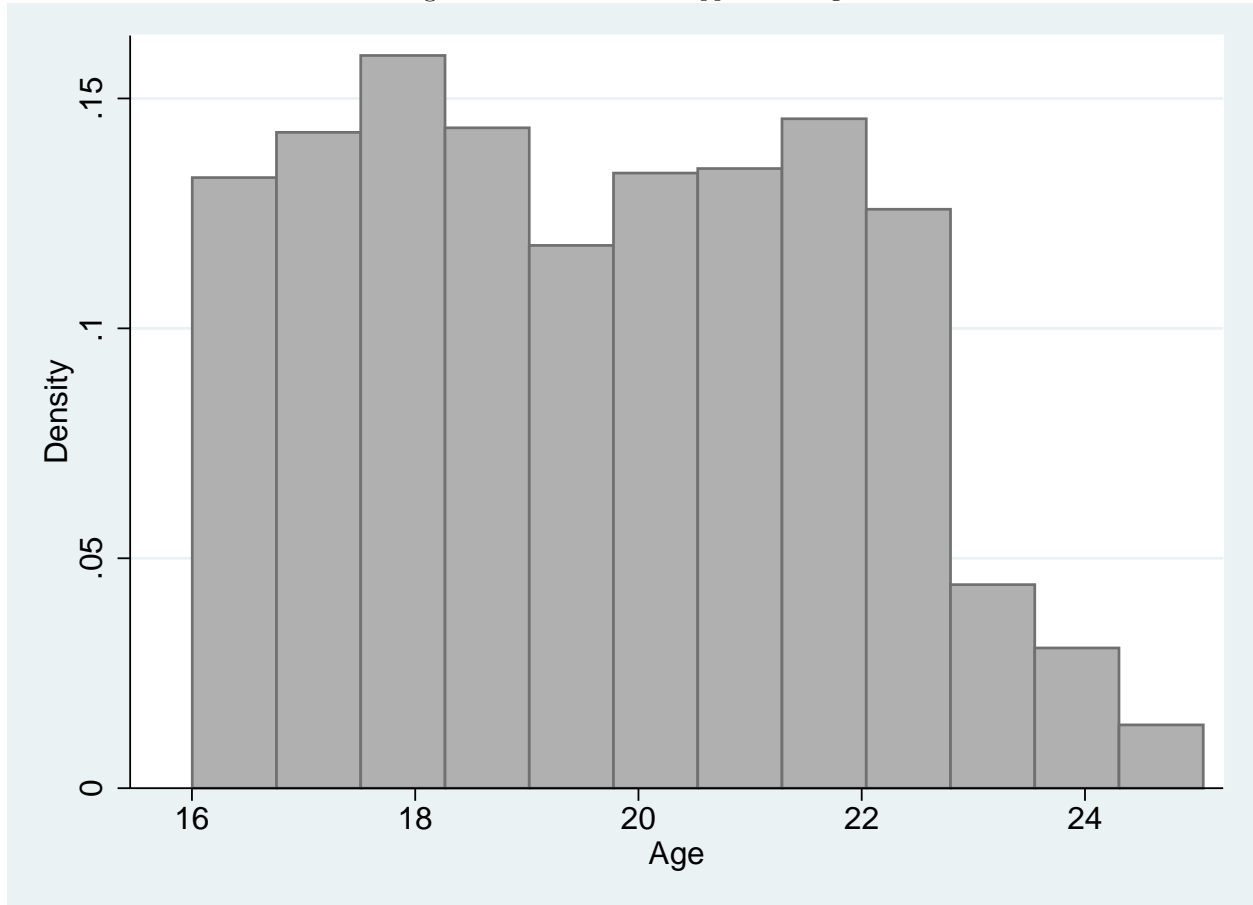
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Figure 1: Participating School Districts



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: Distribution of Applicants' Ages



This figure shows the distribution of applicants' ages using application records based on the date application records were merged to college enrollment data.

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 Lomas	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 Lomas	Menlo Park	Palo Alto	Portola Valley	San Carlos	Woodside
Seats	31	12	24	60	8	26	5

This table shows 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					
	<u>Student/Teacher</u>	<u>Special Ed.</u>	<u>LEP</u>	<u>Spending/Pupil</u>	<u>Ave. Percentile</u>
Ravenswood	19.2	7%	65%	7,413	28
Belmont-Redwood Shores	17.9	10%	4%	7,196	72
Las Lomas	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					
	<u>White</u>	<u>Black</u>	<u>Asian</u>	<u>Hispanic</u>	<u>A/PI</u>
Ravenswood	1%	24%	1%	64%	10%
Belmont-Redwood Shores	64%	3%	16%	11%	1%
Las Lomas	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					
	<u>Family Size</u>	<u>Median Income</u>	<u>Below Poverty</u>	<u>No H.S. Diploma</u>	
Ravenswood	3.8	\$45,573	20%	54%	
Belmont-Redwood Shores	2.3	\$87,267	2%	5%	
Las Lomas	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
<u>Demographics</u>		
Age	20	1,380
Female	52%	1,294
Black	27%	1,380
Hispanic	59%	1,380
Asian/Pacific Islander	12%	1,380
<u>College Enrollment</u>		
Ever enrolled	39%	1,380
4-year ever enrolled	17%	1,380
2-year ever enrolled	31%	1,380
Persistence	34%	1,380
Private school ever enrolled	7%	1,380
Public school ever enrolled	36%	1,380
Top three selectivity tiers	5%	1,380
Transfer	7%	1,380
<u>Arrest</u>		
Ever Arrested	8.7%	1,205
Property Offense	2.5%	1,205
Drug Offense	2.0%	1,205
Violent Offense	1.7%	1,205
Other Offense	4.6%	1,205
<u>Voting</u>		
Registered	34%	675
Voted	23%	675

Data come from application data 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 2012 presidential election.

Table 4: Balance at Baseline

	Age	Female	Black	Hispanic	A/PI	Distance
Offer	0.049 (0.197)	0.028 (0.042)	-0.035 (0.036)	0.014 (0.041)	0.000 (0.027)	0.050 (0.057)
Joint-Test P Value	0.616					
Observations	1,380	1,380	1,380	1,380	1,380	1,380

Regressions control for district admission probabilities. See text for exact specification. Data come from program applications and the National Student Clearinghouse for eligible applicants age 15 and older. Robust standard errors shown in parentheses.

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

Table 5: College Outcomes

College Enrollment Outcomes						
	<u>Enrollment</u>	<u>Any 2 yr.</u>	<u>Any 4 yr.</u>	<u>Public</u>	<u>Private</u>	<u>Persistence</u>
<u>No Controls</u>						
Offer	0.101*** (0.039)	0.074* (0.038)	0.028 (0.028)	0.092** (0.039)	0.015 (0.019)	0.063* (0.036)
<u>With Controls</u>						
Offer	0.096*** (0.034)	0.067** (0.033)	0.030 (0.028)	0.087*** (0.034)	0.016 (0.019)	0.061* (0.032)
Observations	1,380	1,380	1,380	1,380	1,380	1,380

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 the National Student Clearinghouse for eligible applicants age 16 and older. Robust standard errors shown in parentheses.

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

Table 6: Main Effects: Heterogeneity by Demographics

Panel A. College Outcomes: Black Students						
	<u>Enrollment</u>	<u>Any 2 yr.</u>	<u>Any 4 yr.</u>	<u>Public</u>	<u>Private</u>	<u>Persistence</u>
Offer	0.066 (0.071)	0.024 (0.074)	-0.043 (0.064)	0.059 (0.073)	0.027 (0.045)	-0.036 (0.074)
Observations	372	372	372	372	372	372
Panel B. College Outcomes: Hispanic Students						
	<u>Enrollment</u>	<u>Any 2 yr.</u>	<u>Any 4 yr.</u>	<u>Public</u>	<u>Private</u>	<u>Persistence</u>
Offer	0.089** (0.039)	0.078** (0.039)	0.044 (0.032)	0.093** (0.040)	0.005 (0.022)	0.064* (0.037)
Observations	819	819	819	819	819	819
Panel C. College Outcomes: Female Students						
	<u>Enrollment</u>	<u>Any 2 yr.</u>	<u>Any 4 yr.</u>	<u>Public</u>	<u>Private</u>	<u>Persistence</u>
Offer	0.050 (0.047)	0.041 (0.049)	-0.016 (0.043)	0.064 (0.048)	-0.026 (0.035)	0.025 (0.047)
Observations	666	666	666	666	666	666
Panel D. College Outcomes: Male Students						
	<u>Enrollment</u>	<u>Any 2 yr.</u>	<u>Any 4 yr.</u>	<u>Public</u>	<u>Private</u>	<u>Persistence</u>
Offer	0.132*** (0.047)	0.087* (0.046)	0.071** (0.036)	0.107** (0.048)	0.050*** (0.019)	0.094** (0.043)
Observations	714	714	714	714	714	714

Regressions control for district admission probabilities, race, gender and age. See text for exact specification. Data come from transfer applications and the National Student Clearinghouse for eligible applicants age 16 and older. Robust standard errors shown in parentheses.

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

Table 7: College Selectivity

Panel A. College Selectivity				
	<u>Most Competitive</u>	<u>Highly Competitive</u>	<u>Very Competitive</u>	<u>Competitive</u>
Offer	0.001 (0.011)	-0.014 (0.009)	0.017** (0.007)	0.025 (0.020)
Panel B. College Selectivity by Demographic				
	<u>Top Three Tiers</u>	<u>Top Three Tiers</u>	<u>Top Three Tiers</u>	<u>Top Three Tiers</u>
Offer	0.003 (0.015)	0.006 (0.017)	0.003 (0.028)	0.033* (0.020)
Offer×Black		-0.017 (0.039)		
Offer×Hispanic			-0.002 (0.034)	
Offer×Female				-0.066** (0.033)
Observations	1380	1380	1380	1380

Regressions control for district admission probabilities, race, gender and age. See text for exact specification. Data come from transfer applications and the National Student Clearinghouse for eligible applicants age 16 and older. Selectivity tiers are from *Baron's*. Robust standard errors shown in parentheses.

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

Table 8: Effects on Transfer from Two-Year to Four-Year College

Likelihood of Transfer				
Offer	0.002 (0.021)	0.018 (0.021)	-0.027 (0.038)	0.032 (0.027)
Offer×Black		-0.062 (0.051)		
Offer×Hispanic			0.043 (0.051)	
Offer×Female				-0.070* (0.041)
Observations	1380	1380	1380	1380

Regressions control for district admission probabilities, race, gender and age. See text for exact specification. Data come from transfer applications and the National Student Clearinghouse for eligible applicants age 16 and older. Transfer is defined as first attending a two-year college and subsequently attending a four-year college. Robust standard errors shown in parentheses.

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

Table 9: Effects on Arrests and Offenses

Arrests						
	<u>Ever Arrested</u>	<u>Arrests</u>	<u>Violent</u>	<u>Property</u>	<u>Drugs</u>	<u>Other</u>
Offer	0.052*** (0.018)	0.019 (0.104)	0.003 (0.008)	0.004 (0.012)	0.018** (0.007)	0.028** (0.014)
Observations	1,205	1,205	1,205	1,205	1,205	1,205

Regressions control for district admission probabilities, race, gender and age. See text for exact specification. Violent, Property, Drugs and Other are indicators for ever being arrested for an offense in each respective category. The sample is students age 18 and older at the time the application data were merged to arrest records. Robust standard errors shown in parentheses.

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

Table 10: Arrest Effect Heterogeneity by Demographics

Panel A.		Arrest Outcomes: Black Students				
	<u>Ever Arrested</u>	<u>Arrests</u>	<u>Violent</u>	<u>Property</u>	<u>Drugs</u>	<u>Other</u>
Offer	0.082* (0.046)	-0.103 (0.290)	0.022 (0.017)	0.010 (0.037)	0.002 (0.011)	0.043 (0.041)
Observations	333	333	333	333	333	333
Panel B.		Arrest Outcomes: Hispanic Students				
	<u>Ever Arrested</u>	<u>Arrests</u>	<u>Violent</u>	<u>Property</u>	<u>Drugs</u>	<u>Other</u>
Offer	0.046** (0.023)	0.036 (0.128)	-0.006 (0.012)	0.008 (0.012)	0.026*** (0.010)	0.015 (0.017)
Observations	706	706	706	706	706	706
Panel C.		Arrest Outcomes: Female Students				
	<u>Ever Arrested</u>	<u>Arrests</u>	<u>Violent</u>	<u>Property</u>	<u>Drugs</u>	<u>Other</u>
Offer	0.023 (0.029)	-0.087 (0.158)	0.018 (0.011)	-0.026 (0.023)	0.010* (0.006)	0.025 (0.023)
Observations	582	582	582	582	582	582
Panel D.		Arrest Outcomes: Male Students				
	<u>Ever Arrested</u>	<u>Arrests</u>	<u>Violent</u>	<u>Property</u>	<u>Drugs</u>	<u>Other</u>
Offer	0.077*** (0.025)	0.080 (0.138)	-0.005 (0.014)	0.024** (0.011)	0.017 (0.011)	0.033* (0.020)
Observations	622	622	622	622	622	622

Regressions control for district admission probabilities, race, gender and age. See text for exact specification. Violent, Property, Drugs and Other are indicators for ever being arrested for an offense in each respective category. The sample is students age 18 and older at the time the application data were merged to arrest records. Robust standard errors shown in parentheses.

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

Table 11: Voting Outcomes

Panel A.	All Students		Black Students	
	<u>Registered</u>	<u>Voted</u>	<u>Registered</u>	<u>Voted</u>
Offer	0.038 (0.048)	0.009 (0.044)	-0.020 (0.111)	-0.033 (0.099)
Observations	675	675	207	207
Panel B.	Hispanic Students		Female Students	
	<u>Registered</u>	<u>Voted</u>	<u>Registered</u>	<u>Voted</u>
Offer	0.004 (0.062)	-0.007 (0.057)	-0.061 (0.087)	-0.113 (0.084)
Observations	378	378	323	323
Panel B.	Male Students			
	<u>Registered</u>	<u>Voted</u>		
Offer	0.102* (0.056)	0.082* (0.047)		
Observations	352	352		

Regressions control for district admission probabilities, race, gender and age. See text for exact specification. The sample is students age 18 and older at the time of the 2012 presidential election. Robust standard errors shown in parentheses.

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

Appendix

Table A.1: Robustness

Panel A.	Rounded to nearest tenth		
	<u>College Enrollment</u>	<u>Every Arrested</u>	<u>Voted</u>
Offer	0.097*** (0.034)	0.060*** (0.020)	0.026 (0.048)
Observations	1,383	1,205	675
Panel B.	Rounded to nearest hundredth		
	<u>College Enrollment</u>	<u>Every Arrested</u>	<u>Voted</u>
Offer	0.078** (0.038)	0.067*** (0.024)	0.020 (0.054)
Observations	1,383	1,205	675
Panel C.	Rounded to nearest thousandth		
	<u>College Enrollment</u>	<u>Every Arrested</u>	<u>Voted</u>
Offer	0.108** (0.047)	0.060** (0.027)	0.034 (0.068)
Observations	1,383	1,205	675

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. Robust standard errors shown in parentheses.

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

Table A.2: District Effects

	(1)	(2)	(3)
	<u>College Enrollment</u>	<u>Every Arrested</u>	<u>Voted</u>
Palo Alto	0.099*** (0.038)	0.036 (0.022)	0.004 (0.048)
Menlo Park	0.090* (0.049)	0.041 (0.028)	0.046 (0.064)
Las Lomas	0.139 (0.064)	0.017 (0.043)	-0.140* (0.072)
Woodside	0.131 (0.099)	0.024 (0.048)	-0.005 (0.103)
San Carlos	0.034 (0.077)	0.197*** (0.062)	0.085 (0.090)
Belmont-Redwood Shores	0.235*** (0.080)	0.093 (0.060)	-0.103 (0.077)
Portola Valley	-0.033 (0.094)	0.067 (0.046)	0.146 (0.127)
Observations	1,383	1,205	675

Regressions control for district admission probabilities, race, gender and age. See text for exact specification. Robust standard errors shown in parentheses.

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

Table A.3: Arrests related unrelated to driving or alcohol

Non-driving and alcohol-related offenses			
	<u>All Students</u>	<u>Female Students</u>	<u>Male Students</u>
Offer	0.025 (0.016)	-0.002 (0.025)	0.047** (0.022)
Observations	1,205	582	623

Regressions control for district admission probabilities, race, gender and age. See text for exact specification. The dependent variable is an indicator for arrest if that arrest is not related to driving with a suspended license, driving under the influence, or being drunk in public. Robust standard errors shown in parentheses.

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

Table A.4: Proximity of College Attended

Panel A. Attend college >25 miles away			
	<u>All Students</u>	<u>Female Students</u>	<u>Male Students</u>
Offer	0.073** (0.035)	0.051 (0.053)	0.091* (0.048)
Observations	1,205	582	623
Panel B. Attend college <15 miles away			
	<u>All Students</u>	<u>Female Students</u>	<u>Male Students</u>
Offer	0.021 (0.029)	-0.001 (0.046)	0.045 (0.038)
Observations	1,205	582	623

Regressions control for district admission probabilities, race, gender and age. See text for exact specification. The dependent variable is an indicator for attending a college either more than 15 miles from Ravenswood City School District or less than 15 miles from the district. Robust standard errors shown in parentheses.

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

Table A.5: Location of Arrest

Ever Arrested in a given Location			
	<u>Palo Alto</u>	<u>East Palo Alto</u>	<u>Neither P.A. nor E.P.A.</u>
Offer	0.018* (0.009)	0.002 (0.007)	0.024* (0.015)
Observations	1,193	1,193	1,193

Regressions control for district admission probabilities, race, gender and age. See text for exact specification. The dependent variable is an indicator for attending a college either more than 15 miles from Ravenswood City School District or less than 15 miles from the district. Robust standard errors shown in parentheses.

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