Educational Attainment and School Desegregation: Evidence from Randomized Lotteries*

PRELIMINARY AND INCOMPLETE: PLEASE DO NOT CIRCULATE

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This paper studies the long-run impact of a court-ordered desegregation ruling on education outcomes. This ruling mandates that seven school districts, which serve higher-income, predominantly-white families, accept a fixed number of minority elementary school students each year who apply to transfer from a nearby, predominantly minority school district. The fixed number of slots are allocated to families via lottery. The offer to transfer increases the number of students who enroll in college by 7 percentage points. This result is driven by greater attendance to two-year and public colleges, though there are substantial heterogeneous effects. A secondary analysis provides suggestive evidence that peer enrollment matters. Increases in the share of Hispanic students who receive an offer to transfer impacts the likelihood of college attendance among other students who receive the same offer.
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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; Johnson, 2011; Vigdor and Ludwig, 2007). Yet it is unclear to what degree neighborhood and school segregation contributes to poor education outcomes. The Moving to Opportunity housing-mobility experiment generated large reductions in neighborhood poverty rates and modest reductions in share minority for the treatment group, but had small impacts on school environment and subsequent academic outcomes (Sanbonmatsu et al., 2006). Other analyses on the impact of neighborhoods on education outcomes find positive effects (Rosenbaum, 1991; Schwartz, 2010). A complementary experiment would help answer an important question for human capital development: Holding neighborhood characteristics constant, does access to low-minority share, higher-income school districts improve educational attainment?

These questions are 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-ordered 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 find that access to low-minority share, higher-income districts has a large impact on educational enrollment. Overall, the offer to transfer school districts increases the likelihood of attending college by 7 percentage points. This effect is concentrated in attendance to public, two-year colleges. There is no overall effect on the likelihood of attending private colleges. These impacts are heterogeneous. Black students become more likely to attend two-year colleges rather than no college while enrollment impacts are small and insignificant for Hispanic students. Further heterogeneity exists by gender. Effects are large and positive for male students, with zero effect on female students' overall enrollment and possibly negative effects on their likelihood of attending a selective college or a four-year college.

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 attainment for Black students (Guryan, 2004; Reber, 2010; Johnson, 2011). 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 minority receiving 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 and 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 school district, which offers school choice, the opportunity to attend a first-choice high school improves post-secondary outcomes. These studies measure the impact of high-quality middle and high schools, which this paper complements by studying the effect of lottery-based access to schools at much earlier ages when children begin elementary school and human capital investments might be particularly important (Cunha and Heckman, 2007; Heckman and Carneiro, 2003).

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 and Section V 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. Under this restriction, large-scale busing programs often shifted students 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 largely offset by increases in interdistrict segregation. 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 two counties was unconstitutional. Ten years later, a court decision 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 *vise versa*.[1] Minority students originating from the Ravenswood School District may apply to transfer to one of seven school districts: Palo Alto, Las Lomitas, Menlo Park, Portola Valley, Belmont, 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."[2] The court ordered each district to receive a fixed number of students according to their size 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 entering kindergarten class for Ravenswood

[1]Over the entire history of the transfer program, only two students have transferred into the Ravenswood School District.

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

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

was 580 students in 2000. 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.[4] 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 School District boundaries. If a student leaves the program they are not permitted to return.

The application and assignment process proceeds as follows.[5] Applications are available in English and Spanish and are made available online and in the past have been distributed to schools. 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. The county then sorts students by sibling priority group and grade and assigns a lottery number. 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.

Figure 1 shows the geography of the sending and receiving districts. Ravenswood School District is predominantly located in East Palo Alto and adjacent to the San Francisco Bay.

[4] 6% of families reapply

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

Menlo Park and Palo Alto share district boundaries with Ravenswood. Ravenswood serves grades K-8 and students' default high school is located in Redwood City. All receiving districts serve 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 maximum 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. These districts are nearest to Ravenswood, which may factor into family choices despite the fact that free transportation is provided. 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 transfer off to any other district. Nearly 90% of families do not mark a seventh choice. Palo Alto is both the largest receiving district and the district most often ranked first. Moreover, many families do not make third choices. These facts are 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. This distribution of preferences and student allocations also imply that by-school-district effects and receiving-a-first-choice effects are driven by Palo Alto and are similar to the overall impact of a transfer offer (Appendix Tables A.1 and A.2).

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 percentiles, the next-lowest performing district has a percentile rank more than twice as high as Ravenswood. Palo Alto ranks three times higher. Most districts far out spend Ravenswood as well. Menlo Park and Palo Alto, which receive the most students from Ravenswood, spend 62% more per pupil than Ravenswood School District.[6]

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 project draws data from two sources. The first is transfer application data from 1998 until 2008. The county could not provide records prior to 1998. 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. Using names and birth dates from the applications, students are linked to National Student Clear-

[6]Note 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.

inghouse data. National Student Clearinghouse data have information on college attended, length of enrollment, enrollment status, and degree obtained. This information is supplemented by classifying colleges into selectivity tiers defined by Barron's *Profile of American Colleges*.

While I do not have individual-level data on the actual enrollment in the receiving districts conditional on receiving an offer, 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 2012-2013 school year does not cover students studied in this paper but it may nonetheless be informative. 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 reason for leaving is moving (28 students), never enrolled (10 students), other (14 students) and returned to Ravenswood School District (6 students).

The data also do not have information on student gender. Student gender is therefore inferred. Three people independently marked students as female, male or uncertain based on each student's first name only (no other data were provided). 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.

There are 1,400 applications–1,294 unique–for students age 15 or older at the time data were linked to the college outcomes in the Fall of 2013. This restriction allows for coverage of dual enrollment students as well.[7] Figure 2 shows the distribution of age among applicants in the sample. 81% of the sample is 20 years of age or younger. The distribution implies most students will not have had the opportunity to attend multiple semesters of college.

Table 3 further 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

[7]Restricting the sample to older ages increases point estimates.

32%, most of whom enroll in two-year public colleges. Note that some students attend both private and public colleges and both two-year and four-year colleges at various points in time. 24% of students persist through three or more semesters of college and this number is 76% conditional on ever enrolling in college. For comparison purposes, 55% of 19 and 20 year olds in the sample have enrolled in college and 42% have persisted three or more semesters in 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 4% of the sample.

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.[8] The estimating equation is

$$y_i = \beta_0 + \beta_1 \text{offer}_i + f(X_i) + \varepsilon_i \qquad (1)$$

The dependent variables are college-attendance outcomes described in Table 3. Persistence is defined as attending three or more semester of college. These variables are regressed on an indicator for the offer to transfer (offer_i) and a function $f(X_i)$ determining the probability of receiving a transfer offer as function of application characteristics. The baseline specification includes application year interacted with applicant first-choice district preferences, sibling-by-year fixed effects, and separable indicators for grade-level at application, remaining district preferences and the number of times applied. These variables fully determine the probability of admission, but a saturated model yields more parameters than observations. I therefore examine a variety of other specifications for robustness in Table A.3. Indicators for race are included as well. The regression is weighted to ensure that each student receives equal weight regardless of applying a second time. Standard errors are clustered by applicant risk set (i.e grade-by-district-choices-by-year). Heterogeneity is

[8]If the 2012-2013 transfer enrollment rate held for previous years, this would imply a first-year enrollment rate above 90%.

measured by interacting the offer_i variable with indicators for Hispanic, Black or female.

The lottery-based assignment should ensure that those who receive offers are similar, on average, to those who do not receive offers. Baseline data are restricted to variables drawn from application data.[9] Table 4 provides evidence that baseline characteristics are balanced across lottery winners and losers. A joint test of these variables as predictors of the offer variable has a p-value equal to 0.643.

In addition to the main-effects analysis above, I also conduct a secondary analysis on peer effects. The peer-effect estimation aims to understand the effects of a transfer student's transfer-cohort composition, which is randomly assigned, on outcomes. Transfer cohort is defined as the group of students assigned to a given district in a given year to a given grade. Importantly, the aim is not to estimate the impact of transfer students on receiving students, as I have no data on receiving students.

To assess the impact of peer composition, I estimate the following equation

$$(y_{ijkl}|\text{offer}_i = 1) = \gamma_0 + \gamma_1 \text{fraction race}_{(-i)jkl} + Z_i'\gamma_3 + \eta_i \tag{2}$$

This equation restricts the sample to those offered the opportunity to transfer to districts with one elementary school. These districts are smaller overall and receive fewer transfer students—a maximum of 14 students. The rationale for this restriction is to ensure that all transfer students are assigned to the same school within a district. District-by-grade-by-year shares of a particular race average 22% Black and 62% Hispanic. Identifying variation between share Hispanic and share Black is relative to the remaining makeup of the cohort, which is roughly 17% Asian/Pacific Islander.

The outcomes remain the same as in equation (1), but the independent variable of interest is the share of students in grade j of race k admitted to district l excluding student i. The vector Z_i contains indicators for year, district preference, grade, race, sibling status, and number of times applied. Identification arises from the random assignment of students to

[9]The lottery occurs at ages before schools administer state exams.

districts, which implies the composition of the group of students assigned to a given district, grade and year is random within a given year. This type of identification is not systematically constructed to identify peer effects, however, which may lead to bias (Angrist, 2014).

IV Results

A Transfer-Opportunity Effects

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 7 percentage points, which varies little with addition of controls. All remaining tables of results are shown with controls and appendix Table A.3, discuss in the following section, shows the sensitivity of estimates to alternative specifications. This effect is concentrated within two-year, public colleges.[10] There is no effect on attending either four-year colleges or private colleges. The coefficient on persistence is positive and insignificant (p-value¡.12), but the sample is skewed young, which means sufficient time may not have passed to accurately assess persistence. 28% of the sample is 20 years of age or older. An interaction between the offer variable and student age is significant (results not shown), but the overall effect on persistence will be most accurately measured as students in the sample get older.

Table 6 shows there is important heterogeneity in the results. The interaction terms show whether there are significantly different results by race and the marginal effect examines whether the main effect plus the interaction term is significant. Panel A presents effects for Black students. These effects are large and driven by two-year college attendance. Interestingly, while overall attendance increases significantly, there is evidence of a reduction in the relative likelihood that this occurs at a four-year school. Again, the impact is restricted to public colleges. For Black students, the increase in the likelihood of attending three or more

[10]The estimate on two-college effects is smaller than the overall impact because there is a shifting students away from attending both two-year and four-year colleges.

semesters of college is positive but not insignificant (p-value¡.16).

In contrast, Panel B shows that effects are negligible for Hispanic students. The marginal effects across most outcomes are small and insignificant. Relative to other students, who are primarily Black, the overall effect on Hispanic students is significantly smaller.

The effects are further concentrated among male students (top row of Panel C). For males, the impact is large and significant across all college enrollment outcomes, including attendance at a private university and persisting three or more semesters. The enrollment impact is 30% larger than mean male college attendance at age 18. There is no effect on female college attendance overall. In fact, there is evidence of a significant shift away from private college attendance and a comparable, though not statistically significant, shift from four-year college attendance.

There is no overall impact on college selectivity (Table 7, Panel A), which is not surprising given that the majority of students attend local community colleges. However, there is evidence of positive effects on attending a more selective school for male students and possibly a negative effect for female students (p-value¡.12). There is no impact 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 Robustness

While the application data record all variables that determine the lottery, a model that completely interacts all district preferences yields more parameters than observations. To reduce this dimensionality, I impose some measures of separability to estimate the function $f(X_i)$ in 1. The baseline specification interacts year with first-choice preferences and separately adds indicators for grade level, sibling status and remaining district preferences. Table A.3 presents a range of alternative specifications. The base specification presents the previously estimated effect on college attendance. The adjacent two columns exclude siblings and include a sibling-by-year interaction, respectively. In the bottom row, the separable column enters all variables without any interactions. The adjact column interacts each district preference by year. The final column reduces dimensionality by restricting the sample to those who choose Palo Alto as their first choice (two thirds of the sample) and then includes district-by-grade-by-year interactions for district choice two and district choice three as well as sibling-by-year interactions. The point estimates across all specifications are consistent and significant.

Table A.1 and A.2 look at differential impacts by district and by first-choice district. The first column of Table A.1 shows that Palo Alto has a larger, but not significantly larger, effect compared to all other districts (the total impact is the Palo Alto coefficient plus the offer coefficient). Unsurprisingly given its size and demand, the second column shows the magnitude of the Palo Alto's impact is similar to the overall main effects. The remaining coefficents are noisy estimates; Woodside, which has a large positive effect, and Portola Valley, which was a large negative effect, are also the two smallest receiving districts.

C Discussion and Limitations

Thus the overall impacts are positive on college attendance. 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.

However, these positive effects overall mask significant heterogeneity. Hispanic and female students receive little benefit from the transfer program. In contrast, Black-male students, often deemed an at-risk population, experience large gains in terms of college enrollment. One possible reason for the heterogeneous effects is the language skills of Hispanic applicants. Receiving districts, with small shares of students with Limited English Proficiency, may not provide adequate resources for these students. For instance, in the 2013-2014 school year, San Mateo County cited the availability of dual-language programs as frequently-asked question from transferring families. Currently, only Palo Alto and Menlo Park offer a Spanish immersion program for *all* district students with capped enrollments and Portola Valley offers English language support. Ravenswood opened a K-7 Dual Immersion School in 2012.

These results contrast with the results of random assignment studies some fifty years ago, which examine short-run outcomes and demonstrate small or zero impact of desegregation on academic outcomes (Cook, 1984). One source of this difference may be the obvious disparity in time periods between the studies in Cook (1984) and the analysis presented here. In addition, interventions that occur early in childhood can have long-run effects 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.

Recent research has studied the longer-run impacts of MTO on youth. For youth aged 15 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. In terms of college enrollment, there is no impact for for the treatment group that was offered a voucher conditional on moving to a census tract with a poverty rate less than 10%. For those families who received a voucher unconditionally, there is also no overall impact and a statistically significant negative impact for males. This heterogeneity differs with the results found in the voluntary-transfer program, which causes positive impacts on enrollment for male students.

Comparing desegregation-transfer effects to charter-school and school-choice impacts, the effect sizes are similar. 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 terms of heterogeneity, the authors find 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 near 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 are driven entirely to female students.

There are several important limitations to these results. First, this paper studies one desegregation-transfer program that provides opportunities for a particular population to enroll in a new district. Especially given the heterogeneous effects, the results from this program may not extrapolate to other transfer programs in other settings. Moreover, the results only pertain to families who apply for the transfer program. These families may be exceptionally involved with their child's education, savvy about navigating the education system, and particularly amenable to a desegregation program. To the extent that these families differ from other families in the Ravenswood school district who do not apply, the results may not generalize to the entire district.

Second, it is difficult to discern the mechanisms underlying these effects. Johnson (2011) describes three primary mechanisms that could underlie the potential effects of desegregation on education outcomes: the ability to attend schools with greater resources, positive peer effects engendered by high-achieving peers, and changes in school culture that increase expectations for educational attainment. Receiving districts spend more per pupil than Ravenswood. Interviews with students indicate that the other two mechanisms may play role in this context as well. In a newspaper program interview 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). This paper does provide evidence regarding which of these mechanisms matters for longer-run outcomes.

Lastly, these are intent-to-treat effects, which differ from the impacts of attending the receiving school districts for particular lengths of time.

D Peer Composition

While the main goal of this study is to determine the impact of the transfer offer on long-run education outcomes, the exploratory analysis in this section examines how the composition of a transfer student's transfer cohort affects his or her college enrollment. Results suggesting that peers matter would have implications for scaling such a transfer model. Depending on the program objective, it could be optimal to distribute students evenly over a wide number of schools.[11] This attention to allocation is pertinent to districts with several elementary schools and the freedom to assign students across them. The results below suggest this composition is worthy of further study.

Table 9 Panel A shows the effect of the share Black on the likelihood a student attends any college. The effect is negative and not significant. In contrast, the effects of share Hispanic (Panel B) are significantly positive. These effects imply a 10 percentage point increase in share Hispanic increases the likelihood of college attendance by 3.6 percentage points. While there is no significant impact on four-year college attendance, there are significant effects on two-year, public and private-school attendance, and persistence.

Tabel 10 examines the interaction effects of these shares. None of the effects is significant, which is not surprising given the sample size. With this qualification in mind, Panel A shows that the interaction effects of share Black with a transfer student who is Black is generally negative and reinforces the effects. This pattern is similar to that found in Hanushek et al. (2009) and Hoxby (2000). The interaction of share Hispanic with Hispanic tends to slightly offset the main effect (Panel B).

While the effects for Hispanic students are significant, their interpretation is less clear. Several important caveats must be emphasized. First, the sample size is small, which is a caution despite the significant findings; the results should be interpreted as suggestive and perhaps worthy of further investigation. Second, race likely proxies for a number of socioeconomic characteristics, such as income or parents' education, which makes the mechanisms

[11] Examples of research on optimal assignment under peer influence include Bhattacharya (2009) and Carrell et al. (2013).

difficult to parse. For instance, homophily might induce students to identify with members of their own race and self-segregate within a school such that positive peer interactions with families whose children are college bound are mitigated. A similar mechanism is hypothesized in Carrell et al. (2013). Third, peer composition may affect students not through interactions of students with each other, but through school reactions to peer composition. If, for example, a larger share of Hispanic students induces a school to provide language services for those students, this could drive positive effects. This would imply that Hispanic students would benefit most from an increase in the share Hispanic, however Tabel 10 shows there is no differential effect.

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 overall, but driven entirely by Black and male students. The latter experience gains across a range of college-related outcomes: two-year and four-year enrollment, private and public-school enrollment, and persistence through multiple semesters. These results also imply 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.

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). The positive effects found in this instance imply minority students could benefit from an expansion of these programs. However, varying spillovers due to within-transfer-group-peer effects should be studied further. This information is important for decisions about the optimal allocation of transfer students to receiving districts.

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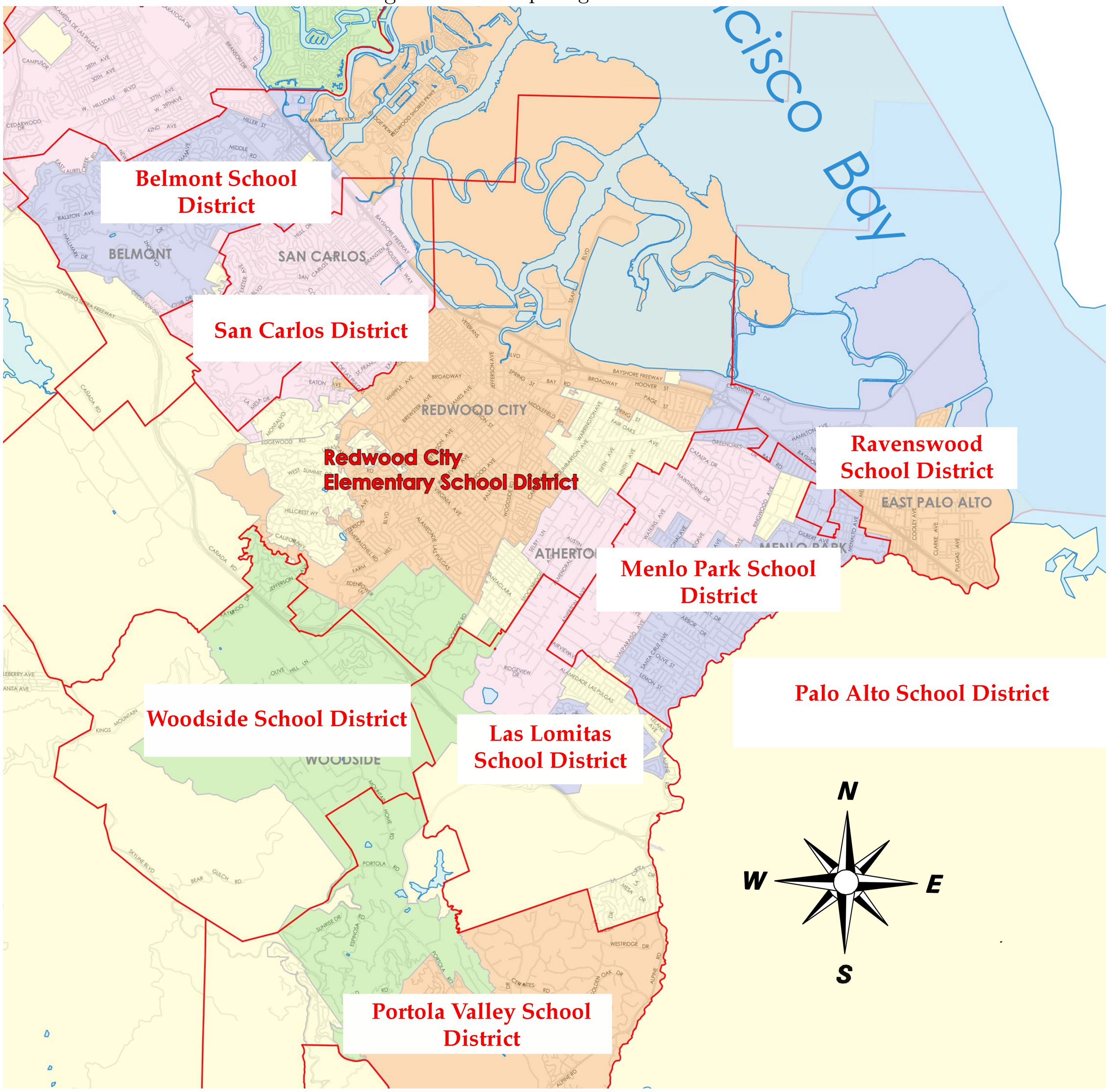
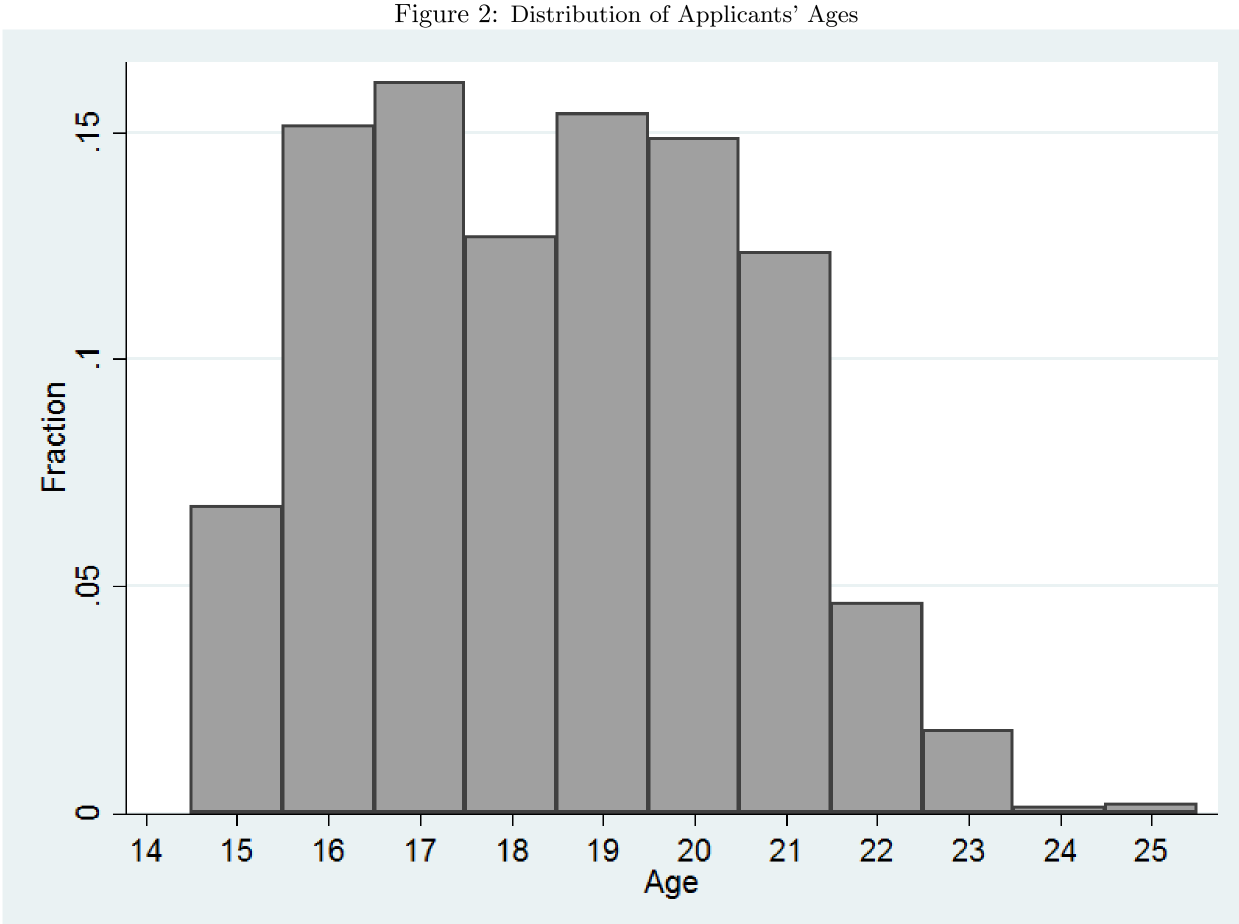
Figure 1: Participating School Districts



This map shows the geographic location of participating school districts in the California Bay Area. Ravenswood School District is the sending district. The other districts highlighted with white backgrounds are receiving districts. This map is an edited version of a map drawn by San Mateo County GIS.

Figure 2: Distribution of Applicants' Ages



This figure shows the distribution of applicants' ages using application records.

Table 1: Distribution of Family Preferences over Districts

	1st	2nd	3rd	4th	5th	6th	7th
None	0%	31%	56%	75%	84%	87%	88%
Belmont	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	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 form San Mateo County records.

Table 2: District and Household-Level Summary Statistics

Panel A.	District Information				
	Student/Teacher	Special Ed.	LEP	Spending/Pupil	Ave. Percentile
Ravenswood	**19.2**	**7%**	**65%**	**7,413**	**28**
Belmont	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.	Demographic Information				
	White	Black	Asian	Hispanic	A/PI
Ravenswood	**1%**	**24%**	**1%**	**64%**	**10%**
Belmont	64%	3%	16%	11%	1t%
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	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
<u>Demographics</u>		
Age	18.4	1,294
Female	51.5%	1,211
Black	27.2%	1,294
Hispanic	59.1%	1,294
Asian/Pacific Islander	12.4%	1,294
<u>College Enrollment</u>		
Ever Enrolled	32%	1,294
4-year ever enrolled	12%	1,294
2-year ever enrolled	24%	1,294
Private School ever enrolled	6%	1,294
Public School ever enrolled	28%	1,294
Persistence	24%	1,294
Top Three Selectivity Tiers	4%	1,294
Transfer	4%	1,294

Data come from transfer applications 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 15 and older in Fall 2013.

Table 4: Balance at Baseline

	Age	Female	Black	Hispanic	A/PI
Offer	0.043	0.023	0.007	-0.028	0.015
	(0.034)	(0.036)	(0.047)	(0.049)	(0.026)
Joint-Test P Value	0.643				
Observations	1,400	1,400	1,400	1,400	1,400

Regressions control for application year interacted with applicant district preferences, grade-level at application, indicators for the number of times applied, sibling status and indicators for race. Data come from transfer applications and the National Student Clearinghouse for eligible applicants age 15 and older. Cluster-robust standard errors shown in parentheses.

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

Table 5: Main Effects

	College Outcomes					
	Enrollment	Any 2 yr.	Any 4 yr.	Public	Private	Persistence
No Controls						
Offer	0.072**	0.062	-0.004	0.070**	-0.010	0.050
	(0.031)	(0.038)	(0.026)	(0.032)	(0.023)	(0.033)
With Controls						
Offer	0.072**	0.063*	-0.000	0.071**	-0.009	0.048
	(0.030)	(0.037)	(0.026)	(0.032)	(0.022)	(0.033)
Observations	1,400	1,400	1,400	1,400	1,400	1,400

Regressions control for grade-level at application, indicators for the number of times applied, sibling status and race, as well as application year interacted with applicant district preferences. Data come from transfer applications and the National Student Clearinghouse for eligible applicants age 15 and older. Cluster-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**					
	Enrollment	Any 2 yr.	Any 4 yr.	Public	Private	Persistence
Offer	0.042	0.034	0.027	0.050	-0.014	0.039
	(0.034)	(0.036)	(0.026)	(0.037)	(0.026)	(0.036)
Offer×Black	0.123**	0.109	-0.091	0.083	0.026	0.045
	(0.063)	(0.079)	(0.068)	(0.066)	(0.053)	(0.065)
Marginal Effect	0.165***	0.143*	-0.064	0.133**	0.012	0.084
	(0.056)	(0.079)	(0.060)	(0.057)	(0.046)	(0.060)
Panel B.	**College Outcomes: Hispanic Students**					
	Enrollment	Any 2 yr.	Any 4 yr.	Public	Private	Persistence
Offer	0.147***	0.136*	-0.041	0.115**	0.013	0.066
	(0.054)	(0.071)	(0.049)	(0.054)	(0.037)	(0.051)
Offer×Hispanic	-0.114*	-0.114	0.070	-0.067	-0.032	-0.023
	(0.063)	(0.074)	(0.057)	(0.069)	(0.044)	(0.061)
Marginal Effect	0.033	0.021	0.029	0.048	-0.019	0.043
	(0.035)	(0.035)	(0.027)	(0.041)	(0.027)	(0.039)
Panel C.	**College Outcomes: Female Students**					
	Enrollment	Any 2 yr.	Any 4 yr.	Public	Private	Persistence
Offer	0.155***	0.093*	0.065*	0.125**	0.057**	0.119**
	(0.054)	(0.051)	(0.038)	(0.052)	(0.024)	(0.054)
Offer×Female	-0.169**	-0.062	-0.134**	-0.111	-0.135***	-0.146**
	(0.071)	(0.063)	(0.064)	(0.076)	(0.052)	(0.070)
Marginal Effect	-0.015	0.030	-0.070	0.013	-0.078*	-0.027
	(0.037)	(0.046)	(0.045)	(0.047)	(0.043)	(0.042)
Observations	1,400	1,400	1,400	1,400	1,400	1,400

Regressions control for grade-level at application, indicators for the number of times applied, sibling status and race, as well as application year interacted with applicant district preferences. Data come from transfer applications and the National Student Clearinghouse for eligible applicants age 15 and older. Cluster-robust standard errors shown in parentheses.

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

Table 7: College Selectivity

Panel A.	**College Selectivity**			
	Most Competitive	Highly Competitive	Very Competitive	Competitive
Offer	0.002	-0.014	0.006	0.011
	(0.008)	(0.010)	(0.006)	(0.020)
Panel B.	**College Selectivity by Demographic**			
	Top Three Tiers	Top Three Tiers	Top Three Tiers	Top Three Tiers
Offer	-0.005	-0.009	0.010	0.026*
	(0.014)	(0.017)	(0.020)	(0.016)
Offer×Black		0.014		
		(0.031)		
Offer×Hispanic			-0.025	
			(0.029)	
Offer×Female				-0.065**
				(0.030)
Marginal Effect		0.005	-0.014	-0.039
		(0.024)	(0.019)	(0.024)
Observations	1,400	1,400	1,400	1,400

Regressions control for grade-level at application, indicators for the number of times applied, sibling status and race, as well as application year interacted with applicant district preferences. Data come from transfer applications and the National Student Clearinghouse for eligible applicants age 15 and older. Cluster-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	-0.006	0.010	-0.031	-0.004
	(0.021)	(0.014)	(0.048)	(0.033)
Offer×Black		-0.059		
		(0.063)		
Offer×Hispanic			0.039	
			(0.048)	
Offer×Female				-0.005
				(0.038)
Observations	1,400	1,400	1,400	1,400

Regressions control for grade-level at application, indicators for the number of times applied, sibling status and race, as well as application year interacted with applicant district preferences. Data come from transfer applications and the National Student Clearinghouse for eligible applicants age 15 and older. Cluster-robust standard errors shown in parentheses.

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

Table 9: Peer Effects

Panel A.	**College Outcomes: Share Black Effects**					
	Enrollment	Any 2 yr.	Any 4 yr.	Public	Private	Persistence
Share Black	-0.093	-0.056	-0.037	-0.163	-0.116	-0.166
	(0.231)	(0.225)	(0.189)	(0.209)	(0.166)	(0.197)
Panel B.	**College Outcomes: Share Hispanic Effects**					
	Enrollment	Any 2 yr.	Any 4 yr.	Public	Private	Persistence
Share Hispanic	0.368***	0.329***	0.112	0.347***	0.159**	0.227**
	(0.110)	(0.079)	(0.079)	(0.094)	(0.070)	(0.102)
Observations	200	200	200	200	200	

Regressions control for grade-level at application, indicators for the number of times applied, sibling status and race, as well as application year interacted with applicant district preferences. Data come from transfer applications and the National Student Clearinghouse for eligible applicants age 15 and older. Cluster-robust standard errors shown in parentheses.

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

Table 10: Peer Effects: Interactions

Panel A.	**College Outcomes: Share Black Interaction**					
	Enrollment	Any 2 yr.	Any 4 yr.	Public	Private	Persistence
Share Black	-0.197	-0.064	-0.139	-0.205	-0.184	-0.196
	(0.254)	(0.268)	(0.186)	(0.235)	(0.169)	(0.240)
Share Black×Black	0.323	0.025	0.322	0.133	0.211*	0.095
	(0.248)	(0.221)	(0.206)	(0.236)	(0.112)	(0.273)
Panel B.	**College Outcomes: Share Hispanic Interaction**					
	Enrollment	Any 2 yr.	Any 4 yr.	Public	Private	Persistence
Share Hispanic	0.359*	0.566***	-0.081	0.489**	-0.022	0.388*
	(0.182)	(0.181)	(0.165)	(0.203)	(0.105)	(0.202)
Share Hispanic×Hispanic	0.013	-0.332	0.272	-0.199	0.254*	-0.226
	(0.226)	(0.232)	(0.244)	(0.231)	(0.148)	(0.244)
Observations	200	200	200	200	200	

Regressions control for grade-level at application, indicators for the number of times applied, sibling status and race, as well as application year interacted with applicant district preferences. Data come from transfer applications and the National Student Clearinghouse for eligible applicants age 15 and older. Cluster-robust standard errors shown in parentheses.

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

Appendix

Table A.1: District Effects

	College Enrollment	
Palo Alto	0.026	0.067*
	(0.038)	(0.034)
Menlo Park		0.015
		(0.048)
Las Lomitas		0.089
		(0.064)
Woodside		0.198*
		(0.105)
San Carlos		0.030
		(0.071)
Belmont		0.084
		(0.080)
Portola Valley		-0.079
		(0.088)
Offer	0.048	
	(0.040)	
Observations	1,400	1,400

Regressions control for grade-level at application, indicators for the number of times applied, sibling status and race, as well as application year interacted with applicant district preferences. Data come from transfer applications and the National Student Clearinghouse for eligible applicants age 15 and older. Cluster-robust standard errors shown in parentheses.

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

Table A.2: First and Second-Choice Effects

	College Enrollment	
Offer	0.078*	0.070**
	(0.043)	(0.033)
First Choice	-0.019	
	(0.037)	
First or Second Choice		-0.017
		(0.038)
Observations	1,400	1,400

Regressions control for grade-level at application, indicators for the number of times applied, sibling status and race, as well as application year interacted with applicant district preferences. Data come from transfer applications and the National Student Clearinghouse for eligible applicants age 15 and older. Cluster-robust standard errors shown in parentheses.

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

Table A.3: Robustness

	Any College		
	Base	No Sibling	Sibling Interaction
Offer	0.072***	0.090***	0.077***
	(0.030)	(0.034)	(0.031)
Observations	1,400	1,073	1,400
No. of Parameters	120	117	227
	Separable	All Dist.×Yr.	Dist 2 & 3×Yr.×Gr. |Dist1=PA
Offer	0.065**	0.064*	0.071**
	(0.032)	(0.041)	(0.035)
Observations	1,400	1,400	934
No. of Parameters	83	578	629

Base specification controls for grade-level at application, indicators for the number of times applied, sibling status and race, as well as application year interacted with applicant first-choice preferences. No sibling estimates the base specification excluding anyone with a sibling participating in the program. Sibling interaction estimates the base specification including a year-by-first-choice-by-sibling interaction. Separable is the base specification with no interaction terms. All district specification includes year-by-district-choice interactions for each of the seven district choices. The last specification conditions on students with a first choice of Palo Alto and then interacts students' second choice with their third choice and grade and year. Data come from transfer applications and the National Student Clearinghouse for eligible applicants age 15 and older. Cluster-robust standard errors shown in parentheses.

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