Educational Attainment, Peer Effects and School Desegregation: Evidence from Randomized Lotteries*

PRELIMINARY AND INCOMPLETE

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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 Black or 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 little impact on school environment and subsequent academic outcomes (Sanbonmatsu et al., 2006). A complementary experiment would help answer important questions for human capital development and segregation: Holding neighborhood characteristics constant, does access to low-minority share, higher-income school districts improve educational attainment? And holding schools constant, how does peer enrollment affect 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 children about to enter kindergarten, first or second grade can apply for a transfer to one of seven receiving districts. Older students are not eligible to apply.

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 90 students are assigned to each district each year. Once assigned, students can remain in the district as long as they do not move from the sending neighborhood. 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 attainment. 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 and Hispanic students become relatively more likely to attend four-year colleges rather than two-year colleges. Further heterogeneity exists by gender. Positive effects are driven entirely by male students, with zero effect on female students' overall enrollment and a negative effect on their likelihood of four-year attendance relative to two-year attendance.

A secondary analysis suggests that peer enrollment has a significant effect on educational attainment and is worthy of further exploration. Conditional on the offer to transfer, a 10 percentage point increase in the fraction of Hispanic students offered admission in a particular transfer cohort increases the likelihood of a student in the same cohort of attending any form of college by 3 percentage points. An increase the share of Black students has the converse effect. Two caveats to the analysis of peers are the small sample size and that it is unclear what race proxies for in the results. For instance, Black students may also be of lower income, which cannot be separately identified in the data. Nonetheless, these results motivate the need to further study peer effects in this context as they have implications for the expansion of transfer programs.

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 the specific case of Charlotte-Mecklenburg schools and find increases in segregation and disparities between minorities and whites. In particular, terminating school integration modestly decreased test scores, increased crime among minority males and lowered attainment. This paper uses random assignment to study the long-run effects of access to low-minority share, higher-resource districts. 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 Angrist and Lang (2004), who conducted a detailed study on peer effects of the Metco program, which is a similar, though not lottery-based, transfer program across districts. The authors focus on the effects of transfer students on receiving students and find little impact, but there are small negative effects on minority receiving students. This paper instead examines the effect of transferring peers on other transfer students, all of whom are minority students. A larger literature use exogenous variation in peers to study the effect of composition on a number of education outcomes.[1]

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), using admission lotteries and survey data, find a similar effect on college attendance and also a reduction in teen pregnancy for students who attend schools in the Harlem

[1] Peer effects have been studied at a number of school levels across a variety of outcomes (Carrell et al., 2008, 2013; De Giorgi et al., 2010; Ding and Lehrer, 2007; Duflo et al., 2011; Epple and Romano, 2011; Garlick, 2014; Hanushek et al., 2009; Sacerdote, 2001).

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, which induced "white flight" (Welch and Light, 1987; Reber, 2005)—rapid changes in white enrollment patterns—that limited the potential for school integration. Coleman et al. (1966) 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 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 lawsuit brought by parents against a group of districts in Northern California in 1976 (Jones, 2006). Ten years later, a court decision mandated the details of the transfer program, which offers minority students from a predominantly-minority school district the opportunity to transfer to a district that is white majority, and *vise versa.* 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. Applications are restricted to rising kindergarten, first and second-grade students. Families rank the seven possible school districts according to preference. Each district has a fixed number of slots per year, ranging from roughly 5 to 60 students, with an average of 25 students. If applications exceed the total available slots in a district, students are selected by lottery. 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 School District boundaries.

Figure 1 shows the geography of the sending and receiving districts. Ravenswood School District is 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 is located in Redwood City. Portola Valley, Las Lomitas and Woodside are elementary school districts with a single elementary school; Belmont, Menlo Park and San Carlos serve grades K-8; and Palo Alto serves grades K-12. Redwood City, which also shares a boundary, does not participate in the program because more than 60% of students are part of a minority-racial group.

Table 1 provides summary statistics for each district using data from the 2000 census and state test information in 2000, which is around when children in the sample were in the sample districts. Panel A shows district-level information for grade five and Panel B shows household-level information for families with children ages zero to four. 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 and academic proficiency level: 65% of students have Limited English Proficiency and proficiency across math and English averages to 37%. The numbers from other districts that are closest to these statistics are 6% and 79%, respectively. Most districts far out spend Ravenswood as well. Palo Alto, which receives the most students from Ravenswood, spends 62% more per pupil than Ravenswood School District. In terms of test scores, the next-lowest performing district has a percentile rank more than twice as high. Palo Alto ranks three times higher.

Demographically, the differences between Ravenswood and other districts are stark. Ravenswood is predominantly Hispanic (50%) and Black (10%) with almost no White or Asian children less than age 5. In contrast, Palo Alto children are 58% White, 20% Asian, 2% Black and 8% Hispanic. 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. These application data are recorded on spreadsheets dating back to 1998 and contain 2,410 applications in all. 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 Clearinghouse data. National Student Clearinghouse data have information on college attended, length of enrollment, enrollment status, and degree obtained. There are no data on the actual enrollment in the receiving districts conditional on receiving an offer, though initial take up is high according to the county officials.

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 1,401 applications–1,294 unique–for students age 15 or older at the time data were linked to the college outcomes. This restriction allows for coverage of dual enrollment students as well.[2] Table 2 summarizes these data. Most applicants are Hispanic or Black, followed by Asian/Pacific Islander. The percent of students who have ever enrolled in college is 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; this number is 76% conditional on ever enrolling in college.

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 offer effect, which is an Intent-to-Treat (ITT) effect, because I have no data tracking enrollment. The estimating equation is

$$y_i = \beta_0 + \beta_1 \text{offer}_i + X_i' \beta_3 + \varepsilon_i \tag{1}$$

The dependent variables are college-attendance outcomes: ever attended college, ever attended a two-year college, ever attended a four-year college, ever attended a public college, ever attended a private college and persistence. Persistence is defined as attending three or more semester of college.[3] These variables are regressed on an indicator for the offer to transfer (offer_i) and controls (X_i) for application year interacted with applicant district

[2]Restricting the sample to older ages increases point estimates.
[3]This is the same definition as in Angrist et al. (2013).

preferences, grade-level at application, indicators for the number of times applied and sibling status. These variables fully determine the probability of admission. 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. As in Angrist et al. (2013), standard errors are clustered at the grade-by-school-by-year level. Heterogeneity is measured by interacting the offer_i variable with indicators for Hispanic, Black or female. Marginal effects are reported using average-marginal effects.

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.[4] Table 3 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 0.579.

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 this particular set of peer effects, 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 equations 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. This small size contributes to substantial district-by-grade-by-year variation in the share of a particular race, which averages 22% Black and 62% Hispanic. Identifying variation between share Hispanic and share Black is relative to the

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

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 districts, which implies the composition of the group of students assigned to a given district, grade and year is random within a given year.

IV Results

A Transfer-Opportunity Effects

The main effects of the offer to transfer on college outcomes are shown in Table 4. Overall, the offer increases the probability of attending college by 7.1 percentage points. This effect is concentrated within two-year, public colleges. There is no effect on attending either four-year colleges or private colleges. The coefficient on persistence is positive and insignificant, but the sample is skewed young. An interaction between the offer variable and student age is significant (results not shown), but this could be due to the linear extrapolation and the overall effect on persistence will be most accurately assessed as students in the sample get older.

Table 5 shows there is important heterogeneity in the results. Panel A presents effects for Black students. These effects are large, and particularly so for two-year college attendance. Interestingly, while overall attendance increases significantly, there is evidence of a reduction in the likelihood that this occurs at a four-year school. While the probability of attending a private school is significantly more positive for Black students relative to other students, the overall effect is not significant. For Black students, the increase in the likelihood of attending three or more semesters of college is large enough to be significant. At 9 percentage points, this is an 18% increase over the mean rate of persistence for Black students 20 years of age or older.

Panel B shows that effects are statistically smaller for Hispanic students. The marginal effects across most outcomes are small and insignificant. Attendance to public colleges is the only evidence of a positive marginal effect. Relative to other students, there is a positive effect on four-year college attendance, but again, the marginal effect is insignificant.

The effects are further concentrated among male students (Panel C). For males, the impact is large and significant across all college outcomes, including attendance to 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 a significant shift away from four-year college attendance and a comparable shift from private-college attendance.

Thus the overall impacts are positive on college attendance. However, these positive effects overall mask significant heterogeneity. Hispanic and female students receive little benefits. Interestingly, black-male students, often deemed an at-risk population, experience large gains. One caveat is that most students in this sample were not yet old enough to have completed college. Lastly, these are intent-to-treat effects, so the impacts are likely larger for those who actually attend receiving districts.

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 (Cook, 1984). However, 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.

It is difficult to compare these results directly to the Moving-to-Opportunity (MTO) experiment because research has studied short-run MTO impacts on test scores and child behaviors. It is interesting to note, however, that MTO shows evidence of small or negative impacts on male outcomes with any positive benefits realized only for females. This heterogeneity contrasts with the results found here.

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. Deming et al. (2014) find small overall college-enrollment effects of receiving an offer to attend a first-choice school, which are attributed entirely to female students.

B Peer Effects

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 attainment. The results below suggest this composition matters and is worthy of further study.

Table 6 Panel A shows the effect of the share Black on the likelihood a student attends any college. The effect is negative and significant. A 10 percentage point increase in share Black reduces the likelihood of college attendance by 2.7 percentage points. The effect is negative across outcomes, and significantly so (at the 10% level) for public and private-school attendance.

In contrast, the effects of share Hispanic are significantly positive (Table 6, Panel B). These effects are slightly larger in magnitude than the effects above and imply a 10 percentage point increase in share Hispanic increases the likelihood of college attendance by 3.6 percentage points.[5] While there is no significant impact on four-year college attendance, there are significant effects on two-year, public and private-school attendance.

Tabel 7 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 here are significant, interpretation is less clear. Several caveats must be made explicit: first, the sample size is small, which is a caution despite the significant findings, and second, race likely proxies for a number of socioeconomic characteristics, such as income or parents' education, which makes mechanisms 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).

Results suggesting that peers matter would have implications for scaling such a transfer model. For instance, depending on one's social-welfare function, it could be optimal to distribute students evenly over a wide number of schools.[6] This attention to allocation is pertinent to districts with several elementary schools and the freedom to assign students across them.

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

[5] Though the sample is roughly 20% Asian/Pacific Islander, the similarity in magnitude with the effect size for share Black could partly be due to a lack of identifying variation between share Black and share Hispanic.

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

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 Iowa, Massachusetts, Missouri, Ohio, Texas, Pennsylvania and Wisconsin. 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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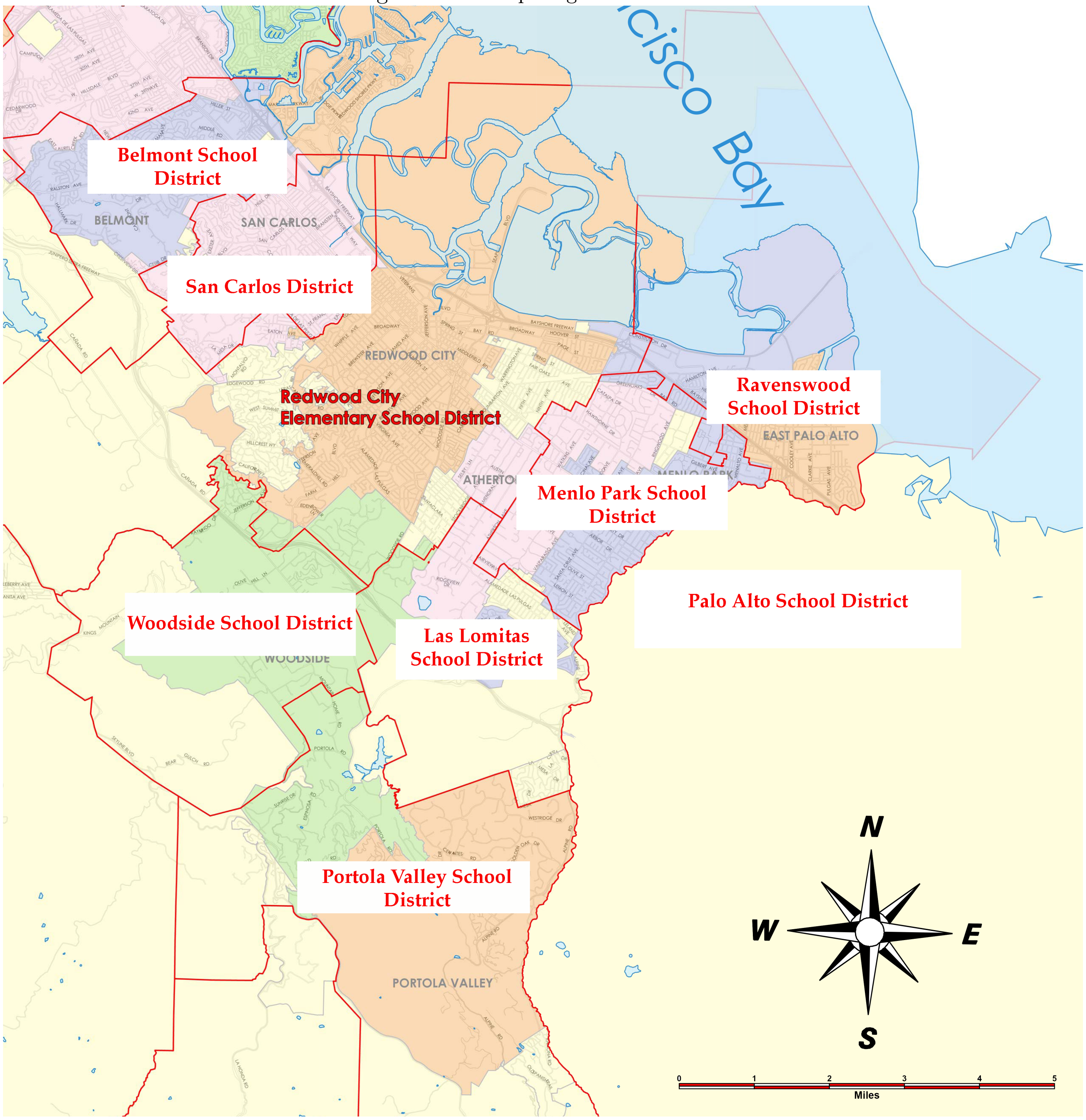
Figure 1: Participating School Districts



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.

Table 1: 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
Ravenswood	**1%**	**10%**	**0%**	**50%**
Belmont	53%	1%	26%	11%
Las Lomitas	76%	0%	9%	6%
Menlo Park	79%	0%	8%	4%
Palo Alto	58%	2%	20%	8%
Portola Valley	89%	0%	6%	0%
San Carlos	70%	1%	5%	11%
Woodside	80%	4%	2%	6%

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%

All summary statistics are drawn from the year 2000 census, except for percentile scores. Percentile scores are taken from the California Department of Education data on state exams from the year 2000. The average percentile score is the average of grade five math and reading percentile scores. Demographic and household summary statistics are tabulated for families with children younger than age 5.

Table 2: Applicant Summary Statistics

Variable	Mean	Observations
Demographics		
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
College Enrollment		
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

Data come from transfer applications and the National Student Clearinghouse. Gender is inferred from student names. These numbers are for unique, eligible applicants age 15 and older.

Table 3: Balance at Baseline

	Age	Black	Hispanic	Asian/Pacific Islander	Female
Offer	0.080	0.030	0.022	-0.039	0.004
	(0.098)	(0.029)	(0.032)	(0.035)	(0.020)
Joint-Test P Value	0.579				
Observations	1,401	1,401	1,401	1,401	1,401

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 4: Main Effects

	College Outcomes					
	Enrollment	Any 2 yr.	Any 4 yr.	Public	Private	Persistence
Offer	0.071***	0.055**	0.001	0.060***	0.000	0.019
	(0.024)	(0.023)	(0.022)	(0.022)	(0.016)	(0.023)
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 5: Main Effects: Heterogeneity by Demographics

Panel A.	**College Outcomes: Black Students**					
	Enrollment	Any 2 yr.	Any 4 yr.	Public	Private	Persistence
Offer	0.038*	0.027	0.028	0.041**	-0.012	-0.007
	(0.022)	(0.023)	(0.020)	(0.020)	(0.014)	(0.021)
Offer×Black	0.116***	0.101***	-0.095***	0.068***	0.042**	0.095***
	(0.027)	(0.025)	(0.028)	(0.025)	(0.019)	(0.029)
Marginal Effect	0.154***	0.128***	-0.067*	0.109***	0.031	0.087**
	(0.037)	(0.032)	(0.035)	(0.033)	(0.026)	(0.037)
Panel B.	**College Outcomes: Hispanic Students**					
	Enrollment	Any 2 yr.	Any 4 yr.	Public	Private	Persistence
Offer	0.144***	0.128***	-0.043	0.101***	0.024	0.071**
	(0.033)	(0.033)	(0.030)	(0.031)	(0.022)	(0.031)
Offer×Hispanic	-0.118***	-0.118***	0.071***	-0.066**	-0.039**	-0.083***
	(0.027)	(0.026)	(0.020)	(0.027)	(0.015)	(0.026)
Marginal Effect	0.025	0.010	0.028	0.035*	-0.015	-0.013
	(0.023)	(0.022)	(0.019)	(0.021)	(0.014)	(0.023)
Panel C.	**College Outcomes: Female Students**					
	Enrollment	Any 2 yr.	Any 4 yr.	Public	Private	Persistence
Offer	0.147***	0.084***	0.060**	0.108***	0.062***	0.067**
	(0.031)	(0.029)	(0.023)	(0.029)	(0.015)	(0.029)
Offer×Female	-0.168***	-0.064***	-0.131***	-0.106***	-0.134***	-0.106***
	(0.030)	(0.023)	(0.023)	(0.025)	(0.016)	(0.021)
Marginal Effect	-0.021	0.020	-0.070***	0.002	-0.071***	-0.040*
	(0.026)	(0.023)	(0.024)	(0.022)	(0.019)	(0.023)
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: Peer Effects

Panel A.	**College Outcomes: Share Black Effects**				
	Enrollment	Any 2 yr.	Any 4 yr.	Public	Private
Share Black	-0.269**	-0.121	-0.139	-0.212*	-0.139*
	(0.125)	(0.101)	(0.084)	(0.109)	(0.080)
Panel B.	**College Outcomes: Share Hispanic Effects**				
	Enrollment	Any 2 yr.	Any 4 yr.	Public	Private
Share Hispanic	0.316***	0.221***	0.086	0.262***	0.116**
	(0.096)	(0.071)	(0.076)	(0.083)	(0.051)
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 7: Peer Effects: Interactions

Panel A.	**College Outcomes: Share Black Interaction**				
	Enrollment	Any 2 yr.	Any 4 yr.	Public	Private
Share Black	-0.204	-0.081	-0.137	-0.110	-0.172*
	(0.160)	(0.141)	(0.091)	(0.144)	(0.096)
Share Black ×Black	-0.221	-0.136	-0.006	-0.347	0.112
	(0.294)	(0.251)	(0.203)	(0.267)	(0.126)
Panel B.	**College Outcomes: Share Hispanic Interaction**				
	Enrollment	Any 2 yr.	Any 4 yr.	Public	Private
Share Hispanic	0.326**	0.372***	-0.039	0.355**	0.064
	(0.143)	(0.126)	(0.129)	(0.135)	(0.073)
Share Hispanic × Hispanic	-0.015	-0.219	0.181	-0.135	0.076
	(0.189)	(0.168)	(0.173)	(0.168)	(0.116)
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