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Neighbors' Spillovers on High School Choice

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Neighbors' Spillovers on High School Choice*

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Abstract

Do residential neighbors affect each others' schooling choices? We exploit oversubscription lotteries in Chile's centralized school admission system to identify the effect of close neighbors on application and enrollment decisions. A student is 5-7% more likely to rank a high school as their first preference and to attend that school if their closest neighbor attended it the prior year. These effects are stronger among boys and applicants with lower parents' education and prior academic achievement, measured by previous scores in national standardized tests. Lower-achieving applicants are more likely to follow neighbors when their closest neighbor's test scores are higher. A neighbor enrolling in a school with 1σ higher school effectiveness, peer composition, or school climate induces increases of 0.02-0.04σ in the applicant's attended school. Our findings suggest that targeted policies aimed at increasing information to disadvantaged families have the potential to alleviate these frictions and generate significant multiplier effects.

Keywords: spillovers, high school choice, centralized school systems

JEL Codes: I21, I24

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1 Introduction

Many cities in the United States and other countries have implemented centralized school choice systems in an attempt to give families access to schools that more closely align with their preferences while, at the same time, increasing access to better schools.[1] One crucial assumption to achieve this goal is that parents are fully informed about the availability of schools and their characteristics. However, search costs or inaccurate beliefs about admission chances can influence families' choice sets even when the assignment mechanism is strategy-proof (Arteaga et al., 2022). Families, particularly those from more disadvantaged backgrounds, might be less likely to consider all the options the centralized system offers, potentially leading them to an inefficient allocation of human capital investments. Moreover, through social interactions, their choices could spillover to future applicants, thus exacerbating segregation patterns or gaps in access to high-quality schools.

In this paper, we study the importance of close neighbors on families' high school application and enrollment decisions. Using data from the Chilean school assignment system between 2019 and 2022, we link applicants to their closest residential neighbor and show that shocks to neighbors' enrollment decisions spillover to applicants in the next year, affecting their choices of applying to and attending the same schools. Understanding how local environments shape families' decisions is relevant since most school choice models do not consider this influence. From a policy perspective, taking into account these dynamic responses has important implications for the design and evaluation of school choice interventions.

Identifying spillover effects using observational data is subject to two empirical problems, known in the literature as the reflection problem and the existence of correlated effects (Manski, 1993). To surpass these two challenges, we take advantage of the implementation of a centralized school admission system in Chile. Under this system, student assignment is determined using the Deferred Acceptance mechanism and lottery tie-breakers in oversubscribed schools. Building on earlier work (Abdulkadiroğlu et al., 2011, 2017; Gray-Lobe et al., 2023) these features motivate an instrumental variables strategy to identify the effect of the closest neighbor's school choice on an applicant's decision. We exploit the exogeneity introduced by the tie-breaking rules in a large number of oversubscribed schools to overcome the correlated effects problem. Regarding the reflection problem, we employ multiple rounds of the centralized system and focus on the effect of the closest neighbor being offered a seat in the *previous* round on the probability of applying to the same school in the *current* round.

We find that close neighbors influence future applicants' behavior. Our main results, based on

[1] For example, Boston, Chicago, New York City, New Haven, Amsterdam, Barcelona, and New Orleans.

two-stage least squares (2SLS) estimates, show that an applicant exposed to a neighbor who attends their target school is 1.0 and 0.8 percentage points more likely to include this school in the application list and rank it as the most preferred school, respectively. These estimates represent an increase of 3% and 5% relative to the average for the non-treated group. In terms of school attendance, the presence of a neighbor attending a given school increases by 0.8 percentage points the probability of enrolling in the same school in ninth grade. This estimate corresponds to a 7% increase relative to the average for the non-treated group.

We conduct a series of heterogeneity analyses to investigate how these estimates vary by applicants' and neighbors' observable characteristics. First, in terms of gender, we find these effects are much larger when the applicant and neighbor are boys. In addition, applicants from more disadvantaged backgrounds, measured by socioeconomic status (SES) and parents' educational level, are more likely to mimic neighbors' previous choices and the likelihood of following a neighbor does not change substantially after conditioning on the neighbor's SES. By contrast, applicants from more advantaged families are less likely to follow neighbors' previous choices. In a different exercise, we test whether spillovers decay with distance using an extended sample where we link each applicant to their ten closest neighbors. We find that only the two closest have a meaningful effect on applicants' decisions. As the distance order increases, our estimates become imprecise and not statistically distinguishable from zero.

Guided by recent literature studying parental preferences for schools (Burgess et al., 2015; Abdulkadiroğlu et al., 2017; Beuermann et al., 2022; Ainsworth et al., 2023), we supplement our analysis with available data to characterize schools across different dimensions, such as average test scores, school value-added on high-school graduation and college enrollment, student composition, and school climate. We employ these measures to analyze heterogeneous effects by school traits. We find that applicants are more likely to rank the same school as their top preference when the distance to the school is shorter and each of the school quality proxies are higher. When we consider the influence of all these traits simultaneously, our estimates are statistically significant at conventional levels only for distance and school climate.

Then we move to the question of whether neighbors' decisions also impact the characteristics of the schools applicants choose. We employ our set of school traits to quantify changes in the characteristics of schools applicants attend as a consequence of following neighbors. We find that a neighbor enrolling in their most preferred school induces applicants to attend schools with better average academic characteristics and more advantaged peers. A neighbor enrolling in a school one standard deviation (σ) above the average in the tenth-grade test score distribution induces an increase of 0.02σ in the school where the applicant enrolls. We find effects of similar magnitude when we consider peers' characteristics and school climate.

We provide evidence supporting that learning from neighbors' previous choices is one of the mechanisms explaining our results. For applicants with a baseline math test score below the median and their closest neighbor scored above the median the probability of ranking the same school as top choice increases by 1.9 percentage points (13%). By contrast, applicants who scored above the median but their neighbors did not are 1.7 percentage points (9%) *less likely* to consider the same school as their top choice. This pattern suggests that neighbors convey information about school quality or other attributes valued by families and that applicants internalize these signals based on ability differences. While we view these findings as evidence consistent with learning, we cannot completely rule out the relevance of other potential mechanisms, such as reducing search costs, or increased preferences for other traits.

This paper contributes primarily to the literature studying spillover effects on human capital decisions.[2] Previous work related to the effects of social networks on educational choices has focused mostly on siblings effects at the secondary level (Joensen and Nielsen (2018) for Denmark, Dustan (2018) for Mexico, and Dahl et al. (2020) for Sweden) and at the college level (Goodman et al., 2015; Aguirre and Matta, 2021; Altmejd et al., 2021).[3] By contrast, evidence about neighbors' effects on educational decisions is less common. One recent study is Barrios-Fernández (2022), who estimates neighbors' spillovers on college attendance. Most related to this paper, Bobonis and Finan (2009) and Lalive and Cattaneo (2009) show evidence of neighbors' effects on school enrollment in primary grades leveraging variation from the implementation of the PROGRESA program in Mexican rural communities. This paper differentiates from these studies in two important ways. First, while Bobonis and Finan (2009) and Lalive and Cattaneo (2009) focus on extensive margin changes in enrollment, we are interested in application decisions for students already attending eighth grade. According to the 2021 *Education at a Glance* report, Chile has an attendance rate of 82% for students aged 15-19, similar to the average 84% in OECD countries.[4] Thus, our results are likely to be generalizable to other educational systems in developed and middle-income countries. Second, the school admission system is available to all students enrolling in non-private schools, which account for around 90% of total enrollment in Chile. By merging application records to a rich set of background characteristics, we examine how families respond to their closest neighbors' decisions across several dimensions, such as socioeconomic status, previous achievement, and resi-

[2] A related literature has studied the effects of residential proximity on other economic outcomes and decisions, such as the effects of working on a specific job or establishment (Bayer et al., 2008; Hellerstein et al., 2011), consumption choices (Grinblatt et al., 2008; Angelucci and De Giorgi, 2009; Kuhn et al., 2011; Agarwal et al., 2021), engaging in youth criminal activity (Billings et al., 2019), or perceptions about well-being (Luttmer, 2005).

[3] See Qureshi (2018), Nicoletti and Rabe (2019), and Gurantz et al. (2020) for siblings spillover effects on student achievement.

[4] https://doi.org/10.1787/b35a14e5-en

dential proximity. We are not aware of such type of analysis in previous work.

We also contribute to the literature examining the indirect effects of centralized school choice mechanisms. Unlike previous literature studying the short-term (Cullen et al., 2006; Hastings et al., 2006; Dobbie and Fryer, 2011; Abdulkadiroğlu et al., 2018; Lincove et al., 2018) and long-term impacts (Deming, 2011; Deming et al., 2014; Dustan et al., 2017; Gray-Lobe et al., 2023) of winning admission to an oversubscribed school under a lottery-based design, this paper focuses on how applicants' decisions spillover to future cohorts. Our heterogeneity analyses show that applicants from lower socioeconomic status are significantly more likely to be influenced by the decisions made by close neighbors in previous rounds. We quantify these differences and show that spillover effects vary significantly across observable dimensions. Understanding how these indirect effects vary across families is important for at least two reasons. First, it has implications for the design of information interventions (Andrabi et al., 2017; Neilson et al., 2019; Ainsworth et al., 2023) or other policies, such as introducing (or expanding) quotas for specific groups. Second, as pointed out by Angelucci and De Giorgi (2009), to assess the effectiveness of these interventions correctly, it is necessary to consider spillover effects generated by treated units. Our analysis confirms that these effects are meaningful in school choice contexts.

Taken together, our findings suggest that although the centralized admission system allows families to include an unrestricted number of schools in their choice sets, frictions prevent some families from learning about all available options. These decisions propagate to other applicants and amplify their consequences on more disadvantaged families. These spillover effects may partly explain the persistence of unequal access to high-quality schools and subsequent achievement gaps.

2 Institutional Background and Data

In 2016, Chile started a transition from a decentralized admission system to a centralized system based on the Deferred Acceptance mechanism (Gale and Shapley, 1962). The *Ley de Inclusión Escolar* (School Inclusion Bill), enacted in 2015, introduced stark changes to how parents applied to schools through the implementation of the *Sistema de Admisión Escolar* (School Admission System) for all schools receiving total or partial public funds. Before the law's passing, voucher schools could charge tuition add-ons and run admission processes independently, while public schools faced more restrictions. By 2017, public and voucher schools concentrated 36% and 55% of the nationwide enrollment, respectively. Private schools, which account for 9% of total enrollment, were not included in the reform and do not participate in the centralized assignment mechanism.

The implementation of the policy was staggered across regions and grades. Starting in 2017, every year an additional group of regions were incorporated for Pre-K, K, first, seventh and ninth grades, adding the remaining grades in the following year. For ninth grade applicants, the reform was fully implemented by 2019. Figure I shows the number of applicants observed each year and Online Appendix Figure A.1 shows the distribution of applicants by grade in the 2019-2022 rounds. Most applications are observed in school transition grades (pre-K, first, and ninth grades). In the Chilean educational system, some secondary flagship schools (*liceos emblemáticos*) start in seventh grade, which explains the high number of applications observed at this level. In Online Appendix A.1 we present additional features related to the implementation of the new system. Figure A.2 shows the number of participating schools by grade. Between 2019 and 2022, around 2,000 high schools offer at least five seats for ninth-grade students, with an average of 69 vacant seats. Figure A.3 summarizes the main stages of the admission process. Each year, families submit their school preferences between September and October. After receiving all applications, the main assignment round is conducted and families observe the outcomes around November. There is a complementary round where unassigned applicants or families who did not participate in the main round are allowed to submit a new application. Students that result unassigned in this complementary round are assigned to the closest tuition-free school with available seats. The process ends in late December when all students have received an assignment. Online Appendix Table A.1 shows the acceptance rates for each round at different school levels. Considering the 2019-2022 rounds, more than 80% of applicants obtain a seat in any of their three most preferred schools. Depending on the school level, between 40% and 60% obtained a seat in their most preferred alternative.

Two important features of this centralized system are worth mentioning. First, some groups of students receive priority in the assignment rule. There are four priority groups served in strict order: (i) students with siblings enrolled at the school, (ii) students with a parent working at the school, (iii) former students previously enrolled at the school, and (iv) all other applicants (Correa et al., 2022). Furthermore, the system includes special quotas for vulnerable students, and some schools can select a fraction of their seats based on admission tests.[5] In the former case, disadvantaged students are given the second highest priority after (i). In the latter case, the system first fills these quotas by assigning students based on their admission test scores, and the remaining seats are assigned following the priority groups (i)-(iv). Online Appendix Figure A.4 summarizes seats classification within schools and the priorities in each case. We discuss how we consider different priority groups in our analysis in section 3.[6] Second, ties are broken randomly within each priority

[5]Some schools incorporate a quota reserved for special-needs students. We do not incorporate this last group of students in our analysis.

[6]An additional issue relates to the fact that students might receive different priorities depending on which priority group they are considered. For example, a disadvantaged student with a working parent fits into two different seat categories in Online Appendix Figure A.4 (disadvantaged and no trait). In these cases, the allocation mechanism

group in oversubscribed schools. Figure II shows the proportion of schools receiving more first-rank applications than vacant seats. This figure shows that in ninth grade more than 30% of the schools participating in the system are oversubscribed.

Crucially for our purposes, the administrative records include the geocoded location of every applicant. For confidentiality purposes, these locations contain a small amount of noise.[7] Additionally, not all addresses in the data correspond to actual residences. We take on a number of steps to discard unreliable geographic locations. First, we drop all students with imputed addresses.[8] Second, we drop applicants whose registered location indicates one region, but their school enrollment records in the same year indicate a different region. This sample selection drops around 20% of all applicants.

Although the centralized platform includes information about the location and preferences of each applicant, it does not collect family background characteristics, such as household income or parents' education. We can observe these by linking ninth-grade applicants to previous records from the national standardized tests (SIMCE tests) taken when they were enrolled in fourth, sixth, or eighth grades.[9] We can link these records to 80% of applicants and 82% of neighbors in our sample. Online Appendix Table A.2 presents the information available for each cohort.

2.1 Sample Construction

We build our sample using data for ninth-grade applicants observed in the 2019-2022 application rounds. As Figure I shows, 2019 is the first year when we observe applications for each region in the country. We match each applicant to the closest neighbor who applied to ninth grade in the previous year, after excluding cases following the criteria described at the end of the previous section. Since our location data contain noise, the closest neighbor we identify needs not be the actual closest neighbor. However, using administrative data of actual distances between applicants in 2018, we verify that most neighbors in our sample live within one or two blocks of distance.[10] To distinguish between close neighbors and members of the same family applying in different years,

assumes that students have preferences over contracts that specify the school and the type of seat to be used, whereas schools have preferences over contracts specifying the student and the type of seat (shown in Figure A.4). See Correa et al. (2022) for additional details.

[7]Observed locations are displaced between 50 and 350 meters from the actual ones, with a median value of 175 meters.

[8]Applicants whose residential address was not accurately captured are assigned the location of the municipality where they live.

[9]SIMCE is an acronym of *Sistema de Medición de la Calidad de la Educación* (National System of Quality Measurement). It was created in 1988 and has been the primary indicator to identify effective schools (Mizala and Urquiola, 2013) or intervene ineffective ones (Chay et al., 2005).

[10]See Online Appendix A.2 for details.

we employ anonymized parent identifiers and discard siblings or pairs of students associated with the same adult responsible for the application. We also drop observations where the (noisy) distance between the applicant and neighbor is higher than 0.5 miles. Finally, we exclude from the estimation sample neighbors who took an admission test in their most preferred school and keep neighbors with an *ex-ante* probability of getting an offer between zero and one.[11] For all applicants, we observe the outcome of the first round of the assignment process. At this stage, parents can accept the designation, accept it conditionally on not receiving an offer from a more preferred school, or reject it and apply to a private school. Finally, we link each applicant to enrollment records in the next year to observe which school they finally enrolled.

We supplement our analysis sample with two additional sources of information. Firstly, we link students' previous math and language test scores in standardized national exams. These administrative records also contain survey information about family characteristics, such as reported income, parents' education, and college expectations. Online Appendix Table A.2 summarizes the grade from which we can observe these records for each cohort. Using this information we construct estimates of school value-added linking high-school graduation and college enrollment to previous test scores for the 2016 sixth-grade and 2015 and 2017 eighth-grade cohorts.[12] Secondly, we incorporate available data at the school level to consider additional attributes, such as distance to school, average tenth-grade SIMCE scores, or school climate.

2.2 Sample Description

Our analysis sample consists of eight-grade students who apply to a high school using the centralized system. Students enrolled in K-12 schools can choose to participate if they want to move to a new school, while students enrolled in K-8 schools necessarily need to participate unless they prefer to switch to a private school. Table I compares observable characteristics of applicants relative to the universe of eight-grade students enrolled in non-private schools. Column (1) shows average characteristics of all students enrolled in K-12, non-private schools, while column (2) restricts the sample to students enrolled in K-8 non-private schools. Column (3) shows the characteristics of students participating in the centralized system. Relative to column (1), the subset of applicants is more disadvantaged, measured by the fraction of low-income students (*prioritario* and *preferente* statuses), previous performance, and parents' education.[13] The comparison of the number of

[11] In our sample, 4% of all assigned seats correspond to schools authorized to select applicants based on admission tests.

[12] See Online Appendix A.3 for details about the estimation of school value-added measures.

[13] The *prioritario* and *preferente* statuses were introduced in 2008 by the *Ley de Subvención Escolar Preferencial* or SEP bill, which established a new targeted voucher to transfer additional resources to schools receiving these students. Each status is determined based on household economic hardship, income, and mother's education. See

students in columns (2) and (3) shows that a small fraction of students enrolled in K-12 schools chooses to apply to a different school in ninth grade.

Column (4) presents summary statistics for the subset of students in our estimation sample, defined as applicants linked to neighbors whose top choice was an oversubscribed school. The comparison of columns (3) and (4) shows that our estimation sample is representative of the total applicant population. Panel A shows that around 52% of applicants in our estimation sample are girls, 62% have a disadvantaged (*prioritario*) status, and around 60% attended a public school in eight grade. Average baseline math and language test scores are -0.25σ and -0.18σ, respectively. The magnitudes and negative signs reflect the differences in achievement between students enrolled in public and private schools. Around 16% of applicants' mothers have a college degree, and 9% of applicants' families report a monthly income higher than CLP800k ($\approx$ US$1,000 in year 2021). Similarly, Panel B summarizes application metrics for both groups. The average number of schools ranked is 3.6, and around 60% of applicants submitted three schools or less. When comparing these outcomes, our estimation sample exhibits only minor differences with the universe of applicants. Specifically, applicants in our sample submit on average 0.07 more schools and are the fraction of them submitting four or more schools is 1.4 percentage points larger.

We present additional descriptive results of the admission process in Online Appendix A.1. Online Appendix Figure A.5 shows the distribution of applications pooling all rounds. We observe that the modal number of applications is three and that less than 25% of families apply to more than five schools. Online Appendix Figure A.6 shows the distribution of the applicant-vacant ratio for schools offering ninth grade. For each school s and year t, we compute the number of students applying to this school as their first choice A_{st} and the vacant seats offered by the school V_{st}. The ratio A_{st}/V_{st} summarizes the excess demand for each school. Online Appendix Figure A.6 shows that around 30% of schools display a ratio $A_{st}/V_{st} > 1$.

Figure III shows differences in the number of applications and school characteristics chosen by families across socioeconomic groups in 2021, using each student's disadvantaged (*prioritario*) status as a measure of economic hardship.[14] The upper-left panel shows the distribution of the total number of applications submitted to the school assignment platform. We observe that disadvantaged students are more likely to apply to fewer schools. The blue bars show that more than 50% of low-SES students apply to less than four schools. The upper-right panel shows differences in school-level math tenth grade scores, based on the most recent round of national standardized tests observed

the work of Mizala and Torche (2017), Feigenberg et al. (2017), and Neilson (2023) for additional details about the implementation of the bill and its consequence on student outcomes.

[14] We find similar patterns for the remaining years.

at this level (2018). Conditional on submitting the same number of schools, more disadvantaged students apply to schools with significantly lower average scores. For example, conditional on applying to three schools, the average gap is 0.25σ. We find the same pattern for language average scores. Finally, the lower-right panel shows the proportion of families applying to schools charging a monthly fee of at least CLP10,000 ($\approx$ US$12.5 in 2021).

These patterns are consistent with previous findings about heterogeneous preferences for school attributes across socioeconomic groups in different countries and educational systems (Hastings et al., 2005; Hastings and Weinstein, 2008; Burgess et al., 2015; Neilson, 2023). Our objective for the rest of the paper is to analyze whether this differential behavior impacts future cohorts' application decisions. In the next section, we present our empirical strategy to identify neighbors' spillover effects on applications and enrollment.

3 Empirical Strategy

In this section, we describe our empirical strategy to estimate the impact of close neighbors' attendance on applicants' decisions. Our strategy leverages variation in neighbors' enrollment induced by random admission offers.

3.1 Admission Lotteries

As discussed in Section 2, all applicants in the same priority group who rank a given oversubscribed school as their first option in the same year have the same probability of receiving an admission offer to that school. Formally, we say that that an applicant i participates in lottery l (denoted $l(i) = l$) if the following conditions hold: first, they applied in year t_l; second, ranked school s_l as their first choice; and third, had priority status p_l at this school. Hence, all applicants in lottery l are equally likely to be offered admission in school s_l.

3.2 Neighbors' Spillovers

Consider an individual i applying to school in year t, and let n be i's nearest neighbor among year $t-1$ applicants. We are interested in studying how n's school enrollment affects the decisions of applicant i. Formally, we estimate the following model using two-stages least squares (2SLS):

$$y_{in} = \alpha + \beta x_n + \phi_{l(n)} + \varepsilon_{in} \tag{1}$$

$$x_n = \gamma + \delta z_n + \varphi_{l(n)} + \nu_n \tag{2}$$

where y_{in} is a binary indicator that equals one if i applied to (or enrolled in) school $s_{l(n)}$ (i.e., the school ranked first by n), x_n is a binary indicator that equals one if neighbor n enrolled in $s_{l(n)}$, and the instrument z_n is a binary indicator for whether n was offered admission to $s_{l(n)}$. Our parameter of interest is β, which captures the causal effect of n's enrollment in school $s_{l(n)}$ on the probability that i applies (or enrolls) in the same school. The terms $\phi_{l(n)}$ and $\varphi_{l(n)}$ are lottery fixed effects.

In our estimation, we use the sample of applicants whose nearest neighbors participate in oversubscribed lotteries, that is, lotteries l such that $0 < Pr(z_n = 1 \mid l(n) = l) < 1$. In all of our specifications, we cluster standard errors at the neighbor level to account for the fact that one neighbor can be linked to multiple applicants.

3.3 Identifying Assumptions

Identification of spillovers requires admission offers to affect n's school enrollment (i.e., $\delta \neq 0$), as well as the following conditional independence assumption:

$$z_n \mid l(n) \perp \varepsilon_{in}, \nu_n \tag{3}$$

This assumption means that conditional on the neighbor's lottery $l(n)$, admission offers made to n must be independent of unobserved factors affecting i and n enrollment. Independence with respect to ν_n is guaranteed by the fact that all neighbors in the same lottery have the same probability of being offered admission to $s_{l(n)}$. Independence with respect to ε_{in} further relies on an exclusion restriction, i.e., we need z_n to affect y_{in} exclusively through its effect on x_n. In other words, we need to assume that admission offers made to the neighbor do not affect the applicant's choices unless they affect the neighbor's actual enrollment.

Our framework can be extended to accommodate the possibility of heterogeneous effects. Under additional assumptions, our 2SLS estimate of β can be interpreted as a weighted average of local average treatment effects (Imbens and Angrist, 1994) for applicants within each lottery l. As Aguirre and Matta (2021) and Altmejd et al. (2021) discuss, the lottery-l LATE captures the average effect among compliers of attending s_l instead of the next-preferred school, which may be different for different neighbors. To better understand this counterfactual scenario, we compute differences in school characteristics between the first and second options. For applicants who submit only one school, we compare it to the school where each applicant is currently enrolled (conditional on grade nine being served).[15] On average, schools ranked as second choices are similar to the most preferred ones. They are located 0.3 miles further from applicants' residences, have 0.036σ higher average tenth-grade scores, their fraction of students at the bottom and top quartiles of the test score distribution is 0.9 p.p. less and 0.6 p.p. more, respectively. Each of these differences is statistically significant at the 1% level.[16]

Our strategy allows us to identify the causal effect of neighbor n's enrollment on applicant i's decision. This effect should be interpreted as a reduced form parameter capturing both the direct influence of n over i and any indirect effects of n operating through other applicants who might be affected by n's enrollment and affect i's decisions (Barrios-Fernández, 2022). However, in the next section we present evidence ruling out contemporaneous effects, which favors an interpretation of β as the direct effect of the nearest neighbor applying in the previous round.

4 Results

4.1 Balance Tests

Before presenting our main results, we examine the validity of our empirical strategy. Under the exclusion restriction, admission offers to each neighbor should be uncorrelated with other determinants of applicants' school attendance conditional on each lottery $l(n)$. Panel A of Table II shows that applicants' observable characteristics are balanced based on neighbors' offers. We test differences across several individual and family characteristics. Specifically, we consider gender, socioeconomic status, high-achieving status, baseline test scores, parents' education, college expectations, and family income for each applicant. Columns (1) and (2) show estimates of a separate OLS regression of the observable characteristic onto an offer indicator, including a full set of lottery fixed effects. Conditional on these, all but two of the estimates are not statistically significant at the 10% level. The only exceptions are gender and baseline math test scores. For the latter, we find that applicants whose neighbors were admitted to their target school scored 0.018σ below applicants whose neighbors who did not obtain an offer. For the remaining covariates, the differences are small and not statistically significant at the 10% level. We also show the results of a joint

[15] The assignment system secures enrollment in the current school if the applicant does not get a seat in one of their submitted choices.

[16] We also find small differences in other dimensions of school quality we employ in our analysis. Fallback schools have 0.007σ and 0.001σ higher college attendance and high-school graduation value-added, respectively, and 0.6 p.p. more college-educated mothers.

significance test where we regress the offer indicator onto all background variables listed above and test the hypothesis that all coefficients are jointly zero. The p-value provides further evidence that the likelihood of a neighbor receiving an offer is exogenous to applicants' observable characteristics.

Analogously, we test whether neighbors' observable characteristics are balanced between offered and non-offered individuals. Panel B of Table II shows the estimates of regressions on the same set of observable characteristics as well as the p-value from a joint significance test. As expected, the estimates show that student attributes do not explain seat assignment after conditioning on lottery fixed effects. Finally, the last row shows no statistically significant differences in the geographic distance between each applicant and their closest neighbor.

4.2 Neighbors' Spillovers on School Applications and Enrollment

Table III shows our intent-to-treat (ITT) and 2SLS estimates of the influence of neighbors on applicants' behavior. Columns (1)-(2) show ITT estimates on the probability of applying to the same school ranked first by the closest neighbor in the previous year. Column (1) shows the estimate of the first-stage coefficient λ in equation (2). This estimate shows that an offer at the top-ranked school increases the probability of attending it in ninth grade by 68 percentage points. Column (2) shows that the probability of including this school in the application list increases by 0.7 percentage points on average if the closest neighbor receives an offer. To contextualize the magnitude of each estimate, we use the estimate of the average outcome in the untreated state for the group of compliers, following Abadie (2002). Relative to the mean for non-treated compliers (i.e., applicants whose closest neighbor did not get an offer), this estimate represents an increase of 2%. Column (3) shows that the probability of applying to the same school as top choice increases by 0.6 percentage points (or 4% relative to the mean for non-treated compliers). Column (4) in Table III shows the ITT estimate on school attendance. We find an increase of 0.5 percentage points in the probability of attending the same school as the neighbor's most preferred alternative, corresponding to a 4% increase.

Columns (5)-(7) show our 2SLS estimates using the neighbor's offer receipt as an instrument for attendance. The probability of applying to a school in any preference increases by 1 percentage point and the probability of ranking this school as the top alternative increases by 0.8 percentage points. These estimates represent increases of 3% and 5% relative to the mean for non-treated compliers, respectively. Finally, column (7) shows that the closest neighbor's enrollment in their most preferred school also increases the probability of an applicant attending it by 0.8 percentage points. This estimate is equivalent to an increase of 7% relative to the baseline level (12%). We report the Kleibergen-Paap F-statistic in all tables.

Standard errors: In recent work, Lee et al. (2022) show that conducting inference based on t-ratios in IV studies might lead to over-rejection and under-covered confidence intervals. They propose using an adjusted t-ratio depending on the value of the first-stage F statistic and 2SLS estimates (tF critical values). We examine whether our estimates are robust to this correction by employing their adjustment method for tests with a significance level of 0.05 and 0.01.[17] Considering the large values of our reported F-statistics in Table III, standard errors and confidence intervals remain unchanged.

Comparison to OLS estimates: We report OLS estimates from specifications not including lottery fixed effects in Online Appendix Table A.3. Using the same estimation sample, we find an increase of 5.8 percentage points in the probability of applicants mimicking their closest neighbor's top-ranked school. This number is almost six times larger than the 2SLS estimate we report. Similarly, the OLS estimate for enrollment is 8.9 percentage points, around ten times larger than our 2SLS estimate. The upshot of these comparisons is that not properly accounting for endogenous peer effects vastly overstates the magnitude of the spillover effects.

Comparison to previous literature: Previous research on neighbors' spillovers in school enrollment decisions (Bobonis and Finan, 2009; Lalive and Cattaneo, 2009) has documented the relevance of peers living in the same community.[18] However, our results are not directly comparable to these estimates. First, these studies report the change in the likelihood of attending a school when the peer group's enrollment rate increases by 1 percentage point, while our treatment variable is defined only by the closest neighbor's enrollment. In addition, our sample is not restricted to a particular subpopulation (such as the villages participating in the PROGRESA program) and includes applicants from different backgrounds. For these reasons, we also consider how our estimates relate to siblings' effects on school choices at the secondary level. Overall, our estimates align with the effects reported by other work in this literature.[19] These orders of magnitude are also observed for siblings' effects on college major choices. For example, Altmejd et al. (2021) show that the probability of a younger sibling applying to the same college in first preference increases by 3.3 to 6.3 percentage points and by 0.6 to 1.2 percentage points by applying to the same college-major

[17] See pages 3271 and 3272 in Lee et al. (2022).

[18] Bobonis and Finan (2009) find an increase in secondary school enrollment rate of 5 percentage points in ineligible households of treated villages in the PROGRESA program, relative to ineligible households in control villages. Lalive and Cattaneo (2009) find that an increase of 10 percentage points in peer group school attendance leads to a 5 percentage points increase in individual attendance.

[19] Joensen and Nielsen (2018) find an increase of 7 percentage points in the likelihood of applying to the same math-science major as the older sibling from a pilot program in Denmark. Dustan (2018) finds an increase of 7 percentage points in the likelihood of applying to the same school in Mexico. Dahl et al. (2020) find that younger siblings are 2.4 percentage points more likely to choose the same high-school major as their older sibling in Sweden.

combination in the first preference. Similarly, Aguirre and Matta (2021) find an increase of 1.9 percentage points in the probability of choosing the same college-major combination.

Placebo Tests: In addition to the balance tests presented in Table II, a second test exploits the fact that applicants should be influenced only by neighbors' previous choices. If neighbors' influence drives our results, future choices should not affect current behavior. To conduct this falsification exercise, we first match each applicant in year t to their closest neighbor in $t+1$ or t and test whether there is an effect of the offer received by this neighbor on applications observed in the *previous* or the *same* year. Tables IV and V show 2SLS estimates of the offer indicator in $t+1$ and t on outcomes observed one year before and the same year, respectively. In both cases, the estimates are of smaller magnitude than our main estimates and not statistically different from zero at the 10% level. These tests provide additional support to our identification strategy.

Additional Robustness Checks: In addition to the placebo and balance tests, we consider two robustness checks. First, one might be concerned that our sample does not include students enrolled in K-12 schools who choose to participate in the assignment process. To check the robustness of our results to this type of selection, we include all applicants enrolled in K-12 schools in our estimation sample. Online Appendix Table A.4 shows that our results remain qualitatively similar to the main results reported in Table III. Second, in our sample we employ enrollment data to determine which school each applicant and neighbor attended in ninth grade. The enrollment data span the first months of the year and does not include information about schools attended if a student transfers or moves during the year. To account for this potential misclassification, we also use student-level academic achievement data, which includes all schools where a student was registered, allowing us to see if a student transferred to another school during the year. We repeat our main analysis defining the school attended as the school where each student was enrolled the largest number of days each year. Online Appendix Table A.5 shows very small differences when we consider this alternative definition of school attendance.

Fade-out Effects: We also investigate how persistent spillover effects are by estimating equations (1) and (2) in a different sample where we link each applicant to the closest neighbor who participated in the centralized assignment process two years before. Figure IV shows these estimates alongside our main results and placebo tests. Panels A and B show that spillover effects fade out quickly. For application decisions, the estimate of the effect after two years is 0.4 percentage points. However, it is not statistically significant at the 10% level. Similarly, the estimate for enrollment decisions is only 0.1 percentage points two years later.

To summarize, our estimates show economically important effects relative to the baseline levels. On average, neighbors' assignment outcomes affect applicants' behavior in the next admission process. Applicants are more likely to rank a school as their top choice and enroll in it when the closest neighbor is also enrolled. In the next section, we examine differences in both margins by applicant and neighbor characteristics.

4.3 Heterogeneous Spillovers

The results from the previous section show that, on average, neighbors influence which schools applicants choose and, consequently, which schools they attend. In this section, we study whether this influence varies according to the characteristics of applicants, neighbors, and schools. To do so, we augment our baseline specification (1)-(2) with interaction terms that allow us to analyze how the average effect varies by observable characteristics. Specifically, we estimate the following set of equations using 2SLS:

$$y_{in} = \alpha_0 + w_{in}'\alpha_1 + (\beta_0 + w_{in}'\beta_1)x_n + \phi_{l(n)} + \varepsilon_{in} \tag{4}$$

$$x_n = \gamma_0 + w_{in}'\gamma_1 + (\delta_0 + w_{in}'\delta_1)z_n + \varphi_{l(n)} + \nu_n \tag{5}$$

Where w_{in} is a vector of variables varying at the applicant and/or neighbor levels. Heterogeneity is captured by the vector of parameters β_1. The parameter β_0 can be thought of as the effect conditional on $w_{in} = 0$.

4.3.1 Heterogeneity by Applicant and Neighbor Characteristics

We start by considering heterogeneous effects by gender. Table VI shows that spillover effects are stronger when applicant and neighbor have the same gender. For boys, the effect on applying to the same school as top choice is 1.3 p.p. (7% increase) and the effect of attending the same school increases by 1.9 p.p. (16% increase). We observe a similarly large effect for girls only for attendance. In this case, the estimate shows an increase of 1.6 p.p. (13% increase). Table VII presents estimates of spillover effects across socioeconomic statuses, using parents' education as a proxy. Using the reports included in the SIMCE questionnaires, we classify students according to whether at least one parent attended college. Therefore, we estimate heterogeneous effects alongside four sub-groups. Overall, Table VII reports two main findings. Firstly, spillover effects are positive when applicants' parents did not attend college but negative for applicants with more educated parents. Secondly, this difference does not change substantially depending on neighbors' parental education. These patterns could be interpreted as applicants being more likely to mimic previous choices if they (or their families) interpret neighbors' choices as signals of school quality or peer composition and base their judgment on relative differences in socioeconomic status. We obtain similar results when we employ the *prioritario* status as a proxy. Online Appendix Table A.6 shows that spillovers are positive for applicants from low-educated families and negative for applicants coming from families where at least one parent attended college. Using this classification we observe even larger differences across groups in application behavior.

4.3.2 Heterogeneity by Distance

Our main results show that the closest neighbor plays an important role in applicants' decisions. However, the closest neighbor is one among potentially multiple members of each applicant's social network. To analyze whether neighbors' influence varies with distance we augment our sample to include the seven closest neighbors within 0.5 miles for each applicant. Using the pooled sample, we augment our baseline specification by allowing the effect of each neighbor $n = 1, \ldots, 7$ to vary by distance order. Specifically, we estimate the following set of equations using 2SLS:

$$y_{in} = \alpha + \beta_n x_n + \phi_{l(n)} + \varepsilon_{in} \tag{6}$$

$$x_n = \gamma + \delta_n z_n + \varphi_{l(n)} + \nu_n \tag{7}$$

We recover the set of estimates $\{\beta_n\}$ for each outcome and present them in Figure V.[20] The horizontal axis shows the order of each neighbor n and the y-axis plots the corresponding estimate β_n for each outcome. We exclude the confidence intervals and show the statistical significance instead for clarity and ease of exposition. Overall, we find that when pooling across neighbors only the first and second ones seem to influence applicants' decisions while the next neighbors have a much smaller and imprecise effect. Our estimates are statistically different from zero only for the closest neighbor and we find a decreasing effect for each outcome. This pattern suggests that neighbors' influence works at a very local level, similar to what previous literature has documented (Barrios-Fernández, 2022).

Online Appendix A.2 discusses the implications of identifying neighbors with noisy location measures. We have access to administrative data on actual distances between students applying to schools in 2018. Unfortunately, these records span only one year and we are unable to match applicant-neighbor pairs or replicate our main analysis using actual distances. However, we can characterize the actual distribution of distances and compare it to the distribution obtained using noisy locations. Online Appendix Figures A.7 and A.8 display how the distributions vary and the correlation between both variables.

[20] Our results remain unchanged when we consider a set of ten or twenty neighbors instead.

4.3.3 Heterogeneity by SIMCE Scores

In addition to proxies of socioeconomic status, we employ data from previous math and language test scores to assess whether neighbors' influence depends on relative differences in past academic performance. In doing so, we further explore the possibility that our results can be explained by neighbors conveying information about schools' characteristics. For instance, applicants with lower relative academic performance could be more likely to mimic previous choices because they might consider this school would also be a good fit for them. By contrast, applicants with relatively better performance will be less willing to consider the school where the neighbor is enrolled because they might infer that quality is low. To test the plausibility of this argument, we construct indicators equal to one if the student scored above the median in the corresponding test score distribution, and include interaction terms of applicants' and neighbors' performance in our specification (4)-(5).

Table VIII shows our estimates for previous math test scores. We find substantial heterogeneity depending on applicant's and neighbor's past performance. Similarly to the results shown in Table VII, we do find large differences when the applicant and neighbor do not belong to the same performance group. For each outcome, the first two rows consider applicants scoring below the median. The first row shows that when the neighbor also scored below the median spillovers increase by 3.1 p.p. This estimate does not change substantially when the neighbor has better performance. By contrast, the third row shows that when the applicant scored above the median but the neighbor did not, the probability of mimicking choices decreases by 2.7 p.p. When both applicant and neighbor score above the median the estimate is -1.8 but only statistically significant at the 10% level. As a consequence, column (3) shows that the probability of also attending the same school is larger for low-achieving applicants. Online Appendix Table A.7 shows a similar pattern when we employ language tests. In particular, that low-achieving applicants are more likely to follow neighbors and that the probability decreases when an applicant performed relatively better in previous standardized tests.

4.3.4 Heterogeneity by School Characteristics

The results from the previous sections show that applicants from more disadvantaged backgrounds are more likely to follow neighbors. In this section, we investigate how the probability of following neighbors also depends on school attributes. Following recent evidence about parental preferences in the school choice literature (Burgess et al., 2015; Abdulkadiroğlu et al., 2017; Ainsworth et al., 2023; Beuermann et al., 2022), we study heterogeneity along the following dimensions: i) distance to school, ii) average test scores, iii) school effectiveness, iv) peer composition, and v) school climate.

To conduct this analysis, we first construct a standardized index for each category using principal component analysis and a set of school-level attributes. For average test scores, we use math and language tenth-grade test scores between 2017 and 2018. For school effectiveness, we employ the average math and language scores and measures of school value-added on high school graduation and college enrollment using information from the 2016, 2018, and 2019 cohorts of ninth graders.[21] For peer composition, we characterize each school's ninth-grade cohort in the application year using average lagged test scores, the proportion of students whose parents expect them to attend college, and the proportion of students with college-educated mothers. Finally, we employ a school climate index reported by the Ministry of Education. This index uses parental surveys from tenth-grade students in the 2017 and 2018 cohorts, capturing attitudes and perceptions about non-academic dimensions of schools.[22] We merge information from the latest available survey to each application round. For each index, we employ information from public and private schools so that our indexes capture differences across all high schools in the country. We then standardize each index to have mean zero and unit variance.[23] Online Appendix Figure A.11 shows that schools with higher indexes are more demanded. Each index strongly associates with the first-rank submissions to vacancies ratio.

We denote θ_s^q to the index q associated to the school where the neighbor enrolls, $s(n)$. Then, we estimate the following set of equations using 2SLS:

$$y_{in} = \alpha + x_n(\beta_1 + \sum_q \beta_q \theta^q_{s(n)}) + \sum_q \gamma_q \theta^q_{s(n)} + \phi_{l(n)} + \epsilon_{in} \tag{8}$$

$$x_n = \delta + z_n(\lambda_1 + \sum_q \lambda_q \theta^q_{s(n)}) + \sum_q \kappa_q \theta^q_{s(n)} + \varphi_{l(n)} + \eta_{in} \tag{9}$$

The parameters β_q allow households' preferences to depend on each index q, conditional on n attending school s. We present our estimates in Table IX, which indicate that, on average, families react to school distance and other attributes, although we obtain precise estimates only for distance and school climate. Column (1) shows that, conditional on the neighbor's enrollment, the probability of ranking the same school as the top choice decreases by 0.1 percentage points ($p < 0.01$) when the distance to this school increases by 1 mile (around 0.17σ). Columns (2)-(5) investigate families' responsiveness to each of the indexes described above. We find that neighbors enrolling

[21]We focus on these cohorts because for each of them we simultaneously observe previous test scores and post-secondary outcomes (see Online Appendix Table A.2 for details). In Online Appendix A.3 we discuss our estimation approach and the distribution of school value-added on high-school graduation and college enrollment.

[22]Parents are asked multiple questions about relationships between school members, episodes of discrimination, conflict or violence incidents, and school responses to conflict situations.

[23]The predicted index for school effectiveness equals 0.56(School-level 10th Grade Scores) + 0.26(HS Graduation Value-Added) + 0.54(College Enrollment Value-Added) while the predicted index for peer composition equals 0.58(Lagged Scores) + 0.24(Mother's Education) + 0.38(College Expectations).

in schools with 1σ higher average tenth grade scores, effectiveness, or school climate increase the likelihood of applicants choosing the same school as top choice by around 1 percentage point, although only average test scores and school climate are statistically significant at the 10% level. However, column (4) shows that peer composition barely changes the likelihood of choosing the same school as top choice. An increase of 1σ in the value of this index increases the likelihood of ranking the same school as the top choice only by 0.1 percentage points. Finally, column (6) show estimates from a horse race specification where we include the full set of school attributes. Using this specification, we find that families seem to be more responsive to distance and school climate. In both cases, we do not find substantial changes in the magnitude or standard errors. By contrast, the remaining attributes decrease their magnitude and are not statistically different from zero.

Previous research studying siblings effects documents a similar pattern. In particular, Altmejd et al. (2021) find that an older sibling's admission to their target college-major increases the probability that the younger sibling applies to the same college, independent of the quality of the older sibling's target. While we find similar results when consider typical measures of school quality, such as average academic performance or peer composition, we also note that households are less likely to follow neighbors if this school is distant and more likely to follow them if it offers a better learning environment.

4.4 Effects on Schools Attended by Applicants

Finally, in this section, we examine whether spillover effects impact the characteristics of schools chosen by applicants. Specifically, we estimate the causal relationship between neighbor's and applicant's school characteristics (e.g., average tenth grade scores, peer composition, school climate). We employ the same source of variation used to estimate equations (1)-(2) but focus on school attributes rather than binary decisions as the outcomes of interest. Formally, we estimate the following equations using 2SLS:

$$w_{s(i)} = \alpha + \beta w_{s(n)} + \phi_{l(n)} + \epsilon_i \tag{10}$$

$$w_{s(n)} = \kappa + \rho w_{s(n)}^{offer} + \varphi_{l(n)} + \eta_n \tag{11}$$

Where the indexes $s(i)$ and $s(n)$ refer to the schools where i and n enroll, respectively. We instrument the characteristics of the school where each neighbor goes, $w_{s(n)}$, using the same attributes of the school where she received an offer, $w_{s(n)}^{offer}$. The outcome $w_{s(i)}$ corresponds to each of the school attributes shown in Table IX: tenth grade average scores in language and math, school effectiveness, peer composition, and school climate indexes. As before, the estimate of interest is β, which represents the effect of an increase of one standard deviation in the attribute of the school where n

enrolled on the value of the same attribute in the school ranked and attended by the applicant.

Table X shows our results. Each panel displays estimates for each of the school attributes shown in Table IX. Column (1) reports estimates of the first-stage coefficient ρ in equation (11). Since the receipt of an offer increases attendance by a large fraction (68 percentage points according to Table III), we also observe a strong relationship between the characteristics of the school where each neighbor received an offer and enrolled. Columns (3) and (4) show the reduced form and 2SLS estimates of spillover effects on school characteristics for each applicant's target. These estimates suggest that neighbors have a positive influence on the type of schools applicants consider. For average test scores, our 2SLS estimates show that a neighbor attending a school 1σ higher than the average increases by 0.024σ the same attribute in the applicant's top choice. We find a similar magnitude when we estimate the effects on the school effectiveness index, which also considers school value-added on high school graduation and college enrollment in addition to average test scores. The last two panels show the effect of neighbors attending schools with higher peer composition and school climate indexes. In both cases, we also find that following neighbors lead applicants to choose schools with better attributes. A neighbor attending a school with an index 1σ above the average leads an applicant to rank as top choice a school with 0.038σ and 0.029σ above the average, respectively. Columns (6) and (7) present estimates using the characteristics of schools where applicants enroll in ninth grade as the outcomes of interest. These estimates show similar effects, between 0.019-0.045σ, for each attribute we analyze. These findings are noteworthy since they suggest that, by means of spillover effects, information interventions conducted in a given period might induce changes in school preferences, allowing applicants to enroll in schools with higher academic performance and more advantaged peers. We next turn to an analysis of potential mechanisms to explain these patterns.

5 Discussing Mechanisms

This section investigates the mechanisms behind the spillover effects we document. It is worth remarking that we employ exogenous variation in the likelihood of receiving an offer for one of potentially multiple members of each applicant's network.[24] Furthermore, we do not observe school preferences before exposure to neighbors' influence, so we cannot separately identify effects on increasing awareness of alternative options from changes in preferences. One analogy to our setting

[24]In addition to exposure to one particular neighbor, another important treatment corresponds to the share of close neighbors who obtain a seat in their most preferred school. Unfortunately, we cannot apply our framework to a larger number of close neighbors. This type of analysis would require a different empirical strategy, for example, by simulating the admission system and computing a propensity score for each neighbor, as in Abdulkadiroğlu et al. (2017) and Gray-Lobe et al. (2023). We leave this task for future research.

corresponds to work in the job search literature related to the importance of neighbors (Bayer et al., 2008; Hellerstein et al., 2011). As in their context, we assume the closest neighbor acts as an indirect proxy of each applicant's network. Considering these data limitations, our results could be capturing multiple causal channels. For these reasons, we discuss the plausibility of three explanations: learning from neighbors' choices, reducing decision-making costs, and utility gains.

5.1 Learning from Neighbors

We start by exploring the possibility that neighbors convey information internalized by applicants depending on relative socioeconomic status and academic performance. Under this hypothesis, applicants with lower relative academic performance will be more likely to mimic previous choices because they might infer this school would also be a good fit for them. By contrast, applicants with relatively better performance will be less willing to consider the school where the neighbor is enrolled because they infer that school quality (or other attribute they value) is low. Our results from Tables VII and VIII are consistent with this explanation. Alongside these two dimensions, we find that disadvantaged applicants are more likely to follow advantaged neighbors and the opposite when we consider advantaged applicants and disadvantaged neighbors. However, at the same time we find that high-SES applicants are less likely to choose the same school regardless of neighbors' parental education.

An additional piece of evidence supporting the learning hypothesis comes from the heterogeneity results by school characteristics in Table IX. Although we find imprecise heterogeneity effects for the academic and peer indexes, we find that families are more likely to consider neighbors' choices when schools have a better school climate. Since this type of information is not reported in the application platform and is based on previous parents' reports, we interpret this finding as evidence of parents reacting to previous experiences about learning environments. Unfortunately, we do not observe families perceptions about schools in our data. Such information, collected in 10th grade tests, would be useful to test whether applicants who follow neighbors are more likely to have a positive valuation of the school where they enroll.

5.2 Search Costs

If searching for schools is more costly for disadvantaged families or there are information frictions, households could primarily rely on social networks and other informal sources to choose where to apply. Parents lacking information about school attributes has been extensively documented in the school choice literature.[25] Our results in Table VII showing that spillover effects concentrate on disadvantaged applicants might be explained by these or additional factors, such as the admission system's complexity or residential segregation, all of them motivating the use of informal networks. Consistent with this hypothesis, Arteaga et al. (2022) show for the Chilean context that (i) the search process is costly in terms of the steps required to acquire information about schools and (ii) families have limited knowledge about the options they submit.[26] In addition, previous research documents small changes in school segregation levels and the proportion of vulnerable students across schools before and after implementing the reform (Kutscher et al., 2023; Honey and Carrasco, 2023). Therefore, we cannot rule out that search costs or other frictions stemming from the structural characteristics of the Chilean educational system could explain our results.

5.3 Preferences

Finally, there is the possibility that applicants and neighbors simply have the same preferences for school traits unavailable in our data or that applicants derive utility from sharing the same environment with residential neighbors. For example, families could apply to the same schools where other residential neighbors attend to improve school-parent communication, avoid exposure to crime if the routes to school are unsafe, or simply be part of the same community. Findings from the siblings effects literature (Goodman et al., 2015; Altmejd et al., 2021; Aguirre and Matta, 2021), suggesting that there could be intrinsic value in following the path of a sibling who enrolls in a particular college or major, might also be relevant in our case to explain why applicants are more likely to follow neighbors, particularly when schools are closer. In addition, recent work by Ainsworth et al. (2023) shows that, after providing information about school value-added, families are responsive but a significant fraction still leave value-added "on the table". Thus, preferences or other unobserved school traits might be a relevant factor in explaining neighbors' spillovers.

To summarize, there are a number of reasons that could explain a causal impact of a residential neighbor impacting applicants' future decisions. Although we show evidence consistent with the idea of applicants learning about schools characteristics based on who enrolls in them, other alternative explanations might rationalize our results. Although recent work has shown that infor-

[25] Hastings and Weinstein (2008) show evidence of parents lacking information about schools and their characteristics in the Charlotte-Mecklenburg school choice program, while Jensen (2010) shows evidence that families underestimate the returns to secondary school from an experimental intervention in the Dominican Republic. Using surveys from applicants in New Haven, Kapor et al. (2020) find that families' beliefs about their admission chances are off by 30 percentage points on average.

[26] In one question asking parents about what they needed to know about a school to feel that they knew it well, 79% of applicants answered that "asking for references from current families" is a relevant step to know a school. In addition, when asked about how much they knew about the schools submitted, 64% of applicants declared that they "knew well" their target school.

mation and preferences are relevant to understanding families' choices, further research is required to quantify the importance of these different mechanisms in explaining how choices spillover to future cohorts.

6 Conclusion

In this paper, we investigate the influence of close neighbors on school application and enrollment decisions. We employ data from the Chilean centralized school admission system between 2019-2022. The large proportion of oversubscribed schools in ninth grade and the use of lottery-based tiebreakers to determine assignments allow us to identify causal effects. We are unaware of previous work studying this type of spillover effects in centralized school systems.

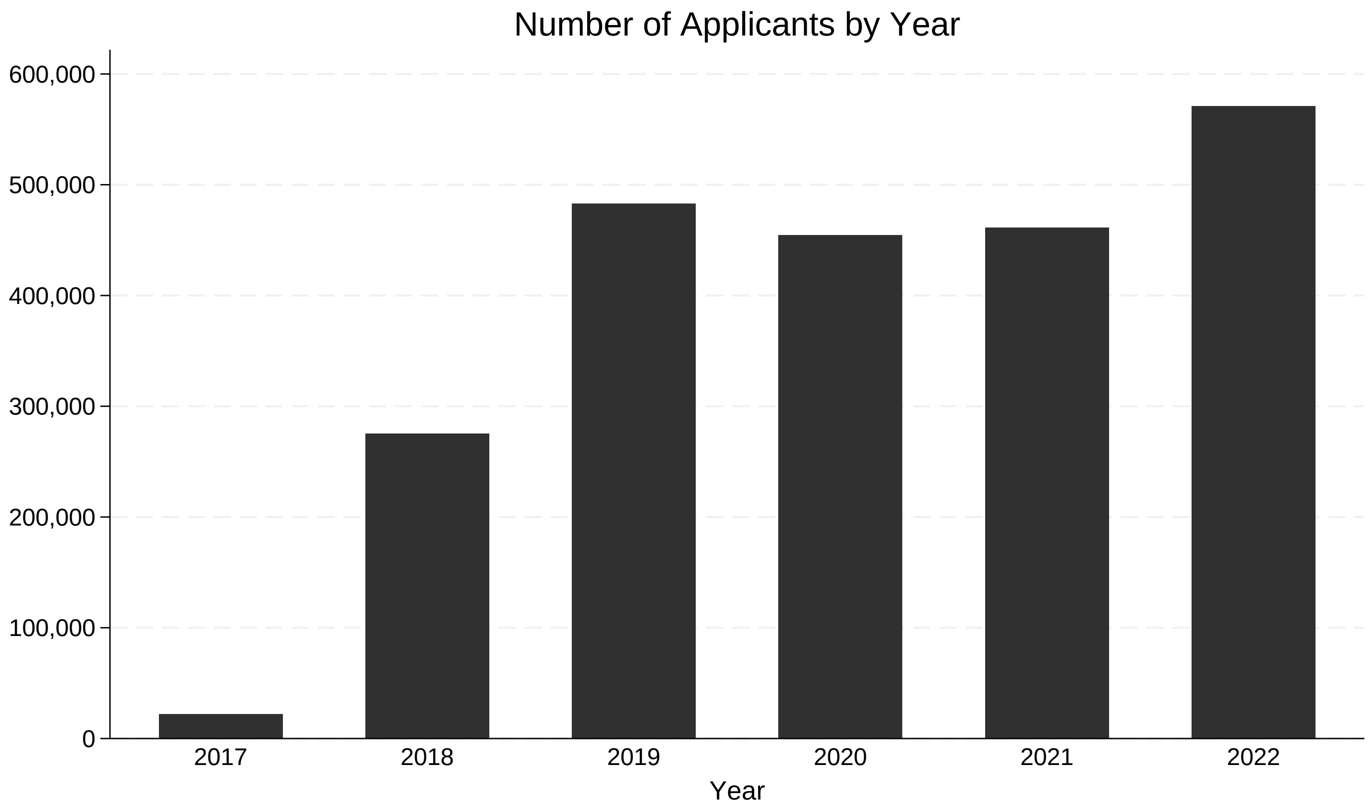
Our results show meaningful spillover effects on school applications and enrollment. On average, having a close neighbor assigned to their most preferred school in the previous round increases the likelihood of an applicant ranking that school in the first preference and attending it by 0.8 percentage points. These estimates represent an increase of 5% and 7% relative to the non-treated rates. Our heterogeneity analysis reveals that these effects are larger when applicant and neighbor are boys and when both belong to disadvantaged families, measured by socioeconomic status or parents' education. Neighbors attending schools with higher average tenth-grade test scores and school climate increase the probability of applicants choosing these schools as their top choices in the next year. Finally, we show that following neighbors has consequences on the quality of schools applicants attend. We find that applicants enroll in schools 0.02-0.04σ higher in tenth-grade average test scores, school effectiveness, peer composition, and school climate.

We find evidence supporting information transmission as one mechanism driving our results. Specifically, applicants who obtain lower baseline test scores than neighbors are more likely to rank as top-choice and attend the same school where the neighbor enrolled. The opposite pattern emerges when applicants' test scores surpass neighbors'. Our findings of larger effects on more disadvantaged students suggests that although the introduction of the centralized system seemingly made information available to all families, it has not been incorporated into the decisions made by this group of families. This conclusion has been found in other settings (Hastings and Weinstein, 2008; Dizon-Ross, 2019) and points out the role of targeted interventions to reduce frictions and improve the allocation of educational investments.

One important question relates to spillover effects beyond ninth-grade enrollment, for example on high school graduation, performance in college-admission tests, or college major choice. As data about these outcomes becomes available, future research could explore how residential neighbors might also affect these and other longer-term outcomes.

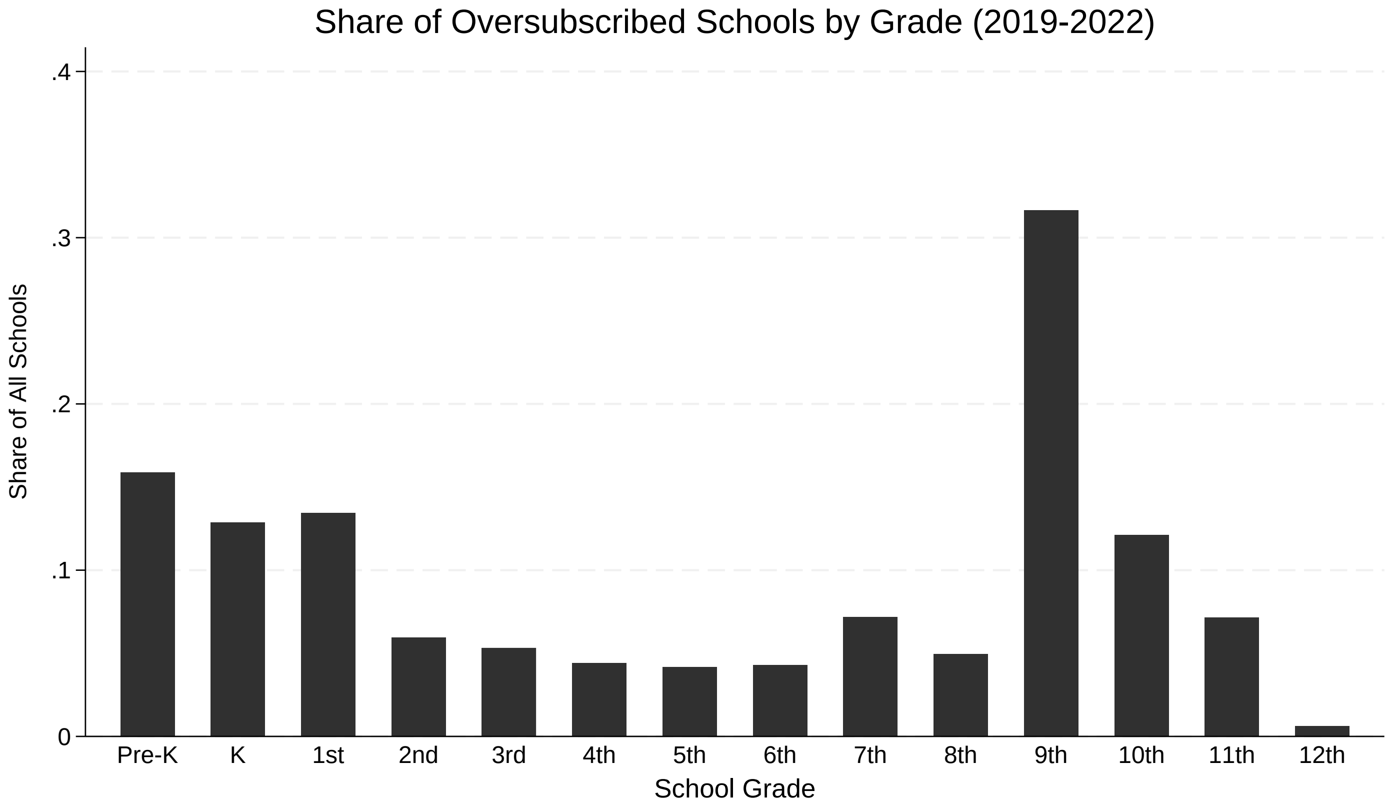
7 Figures and Tables

Figure I: Implementation of the Centralized School Choice System



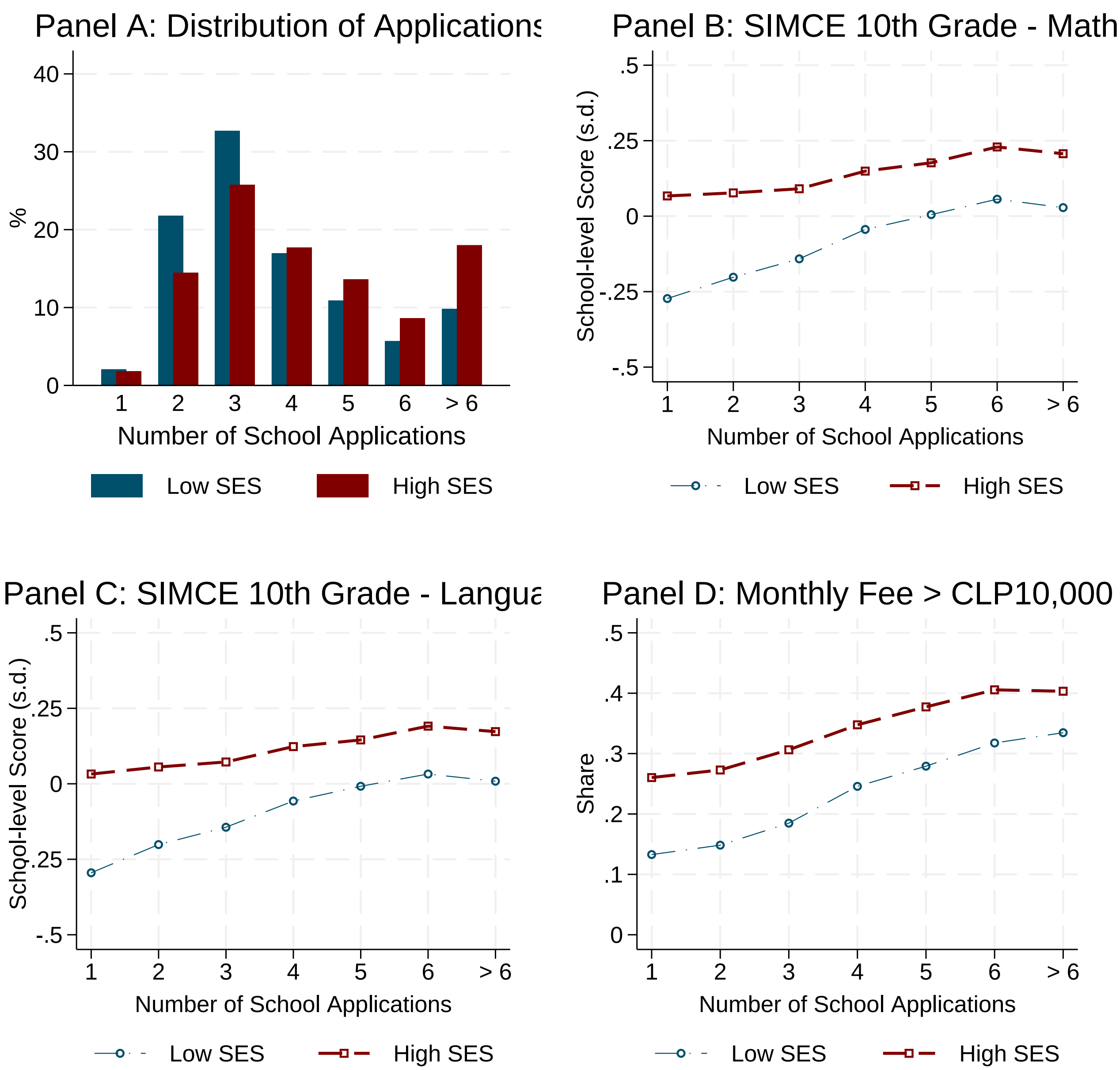
Notes: This plot shows the number of applicants observed in the centralized system between 2017 and 2022. Rollout was staggered across regions and grades. Starting in 2017, each year a new set of regions was incorporated to the system. By 2019, the centralized admission system is used for admission to ninth grade in all public and private voucher schools.

Figure II: Oversubscribed Schools



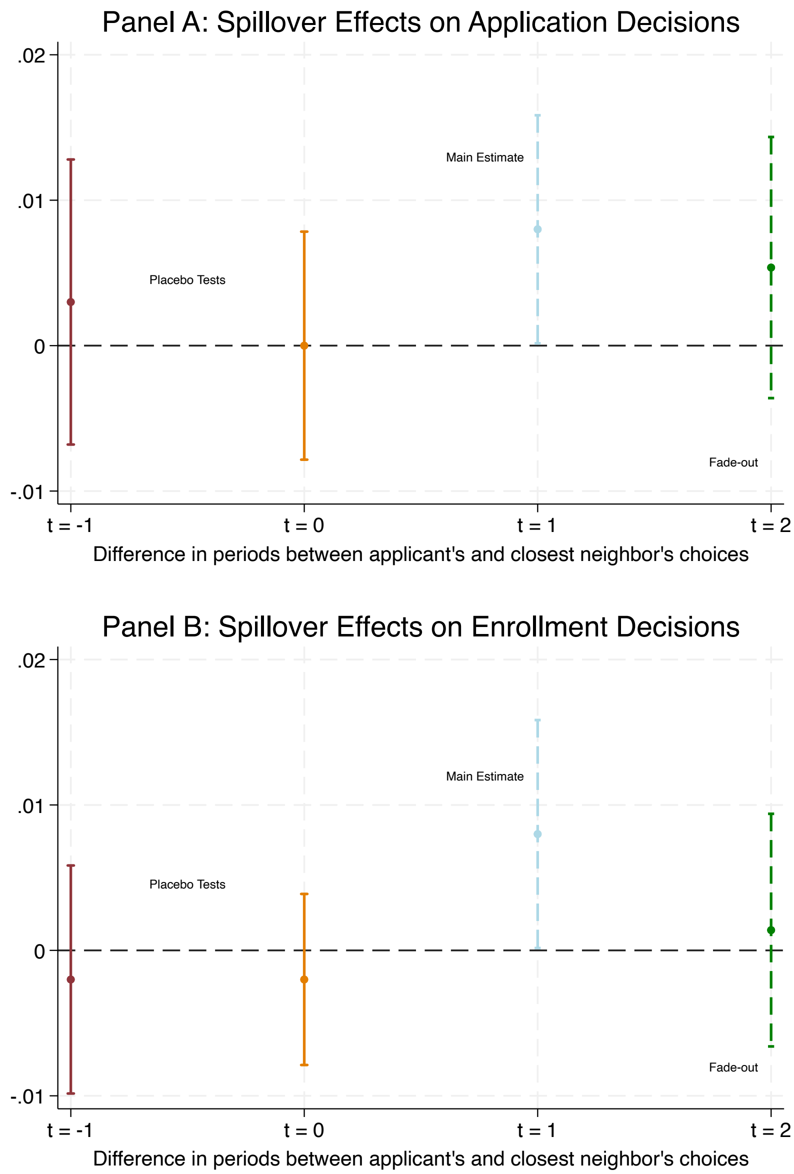
Notes: This plot shows the share of schools where the number of applicants submitting the school as first option surpasses the number of vacant seats in the corresponding grade. The share is computed pooling the 2019-22 application rounds.

Figure III: Differences in Applications and High School Characteristics by Students' SES



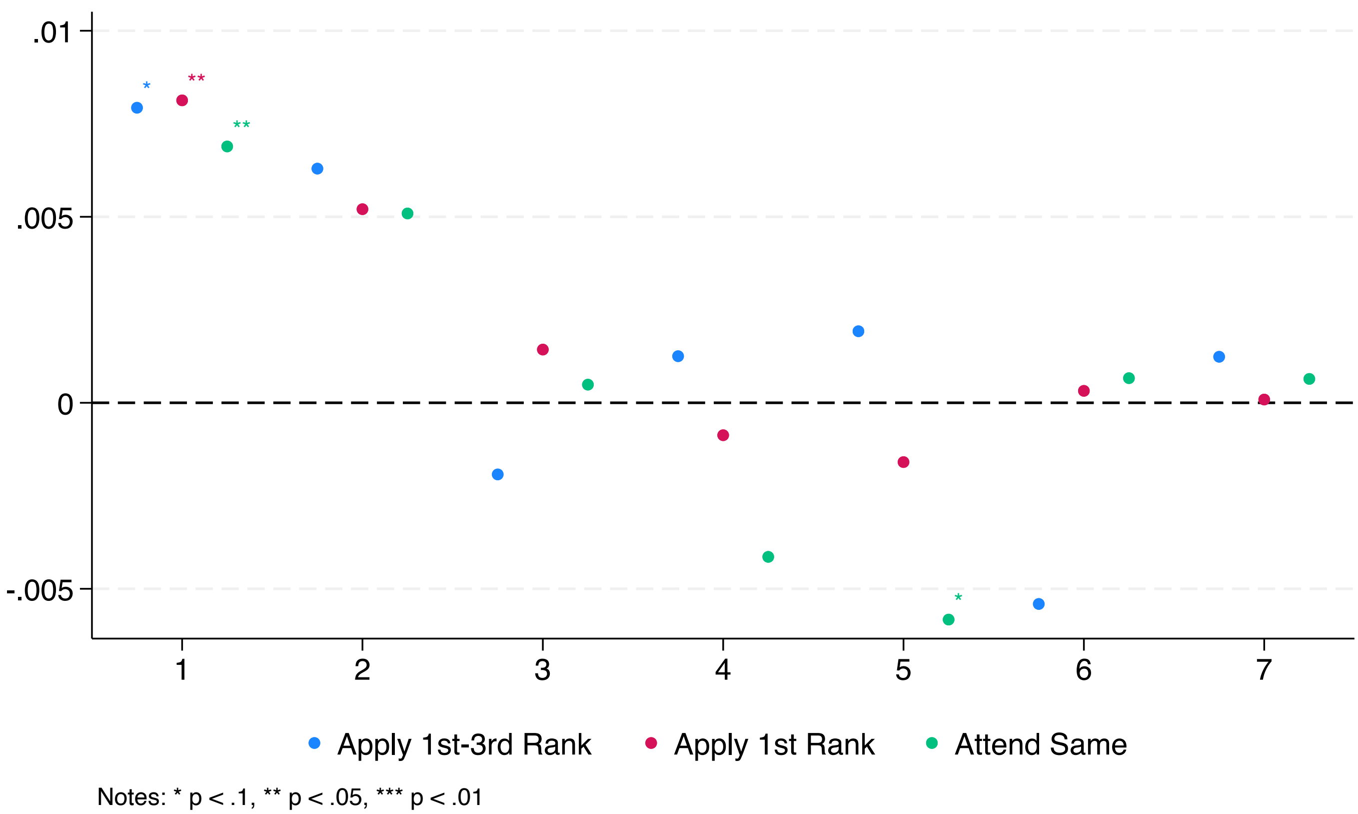
Notes: This figure shows differences in the distribution of applications and school characteristics between priority and regular ninth-grade applicants. Priority status is defined by the Ministry of Education on the basis of economic hardship. Panel A shows the distribution of applications. Panels B and C show differences in average tenth-grade test scores for math and language, respectively. Panel D shows the fraction of students applying to schools charging a monthly add-on higher than CLP10,000 ($\approx$ US\$12.5 in 2021).

Figure IV: Impacts of Neighbors From Different Time Horizons: Separate Regressions



Notes: This figure shows how spillover effects vary depending on the number of periods used to link each applicant with their closest neighbor. Each plot reports estimates from separate 2SLS regressions as described in equations (1) and (2). Panel A uses as outcome an indicator equal to one if the applicant ranks the same school attended by the closest neighbor between in any preference, while panel B uses as outcome an indicator equal to one if the applicant attends the same school. Neighbor's enrollment is instrumented with an indicator equal to one if the neighbor got an offer in their most preferred school. All models include lottery fixed effects and standard errors are clustered at the neighbor level.

Figure V: Heterogeneous Neighbor Spillovers: by Distance



Notes: This figure shows how spillover effects vary with the distance between each applicant and the seven closest neighbors. Neighbor's enrollment is instrumented with an indicator equals to one if the neighbor received an offer in their most preferred school. Our specifications include lottery fixed effects. Standard errors are clustered at the neighbor level.

Table I: Summary Statistics

	All 8th-Grade Students (1)	All 8th-Grade in K-8 Schools (2)	All Applicants (3)	Estimation Sample (4)
Panel A: Background Characteristics				
Girl	0.512	0.532	0.504	0.531
Prioritario	0.529	0.639	0.610	0.636
Preferente	0.294	0.250	0.265	0.263
Public School	0.382	0.599	0.545	0.602
SIMCE (Math)	-0.037	-0.266	-0.210	-0.260
SIMCE (Language)	-0.022	-0.194	-0.152	-0.190
Missed SIMCE	0.156	0.193	0.178	0.180
Father Education: College	0.278	0.159	0.187	0.158
Father Education: Less than HS	0.673	0.784	0.758	0.785
Mother Education: College	0.308	0.186	0.215	0.182
Mother Education: Less than HS	0.673	0.791	0.763	0.796
College Expectations	0.756	0.671	0.692	0.675
Family Income Above CLP800k	0.185	0.088	0.111	0.086
Panel B: Application Characteristics				
Number of Applications			3.530	3.562
Submits One School			0.026	0.015
Submits Two Schools			0.280	0.259
Submits Three Schools			0.318	0.347
Submits Four Schools or More			0.376	0.379
Observations	840,755	367,045	410,412	115,148

Notes: This table presents average characteristics of the estimation sample relative to 8th grade students who participate in the centralized system between 2019 and 2022 and all students enrolled in K-12 public schools. Column (1) shows average characteristics for all 8th-grade students; column (2) restricts the sample to students enrolled in K-8 schools. Column (3) displays average characteristics for all applicants with a valid (non-imputed) geographic location. Column (4) shows average values after restricting the sample to applicants whose closest neighbor's top-choice was an oversubscribed school (e.g., seat offer was determined by the tie-breaking rules).

Table II: Balance Tests

Variable	Average		Difference	p-value	Observations	
	Offered	Non-offered			Offered	Non-offered
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Applicant Covariates						
Girl	0.474	0.465	0.008**	0.024	52,838	62,310
Prioritario	0.657	0.662	−0.005	0.181	52,838	62,310
High Achiever	0.298	0.299	−0.000	0.975	52,838	62,310
SIMCE (Math)	-0.267	-0.251	−0.017**	0.036	42,276	49,459
SIMCE (Language)	-0.188	-0.189	0.001	0.866	42,069	49,285
Father's Education: College	0.156	0.160	−0.004	0.224	40,361	46,760
Father's Education: Less than HS	0.786	0.784	0.002	0.519	40,361	46,760
Mother's Education: College	0.181	0.182	−0.001	0.774	40,612	47,091
Mother's Education: Less than HS	0.796	0.795	0.001	0.766	40,612	47,091
College Expectations	0.678	0.672	0.006	0.131	40,318	46,745
Family Income	0.085	0.087	−0.002	0.331	40,750	47,255
Joint orthogonality F-test				0.282		
Panel B: Neighbor Covariates						
Girl	0.483	0.484	−0.001	0.825	29,433	35,562
Prioritario	0.601	0.598	0.002	0.367	29,433	35,562
High Achiever	0.304	0.304	−0.000	0.971	29,433	35,562
SIMCE (Math)	-0.202	-0.191	−0.012	0.202	23,888	29,358
SIMCE (Language)	-0.123	-0.123	−0.000	0.964	23,748	29,187
Father Education: College	0.170	0.174	−0.004	0.319	22,411	27,269
Father Education: Less than HS	0.772	0.766	0.007	0.153	22,411	27,269
Mother Education: College	0.196	0.201	−0.005	0.218	22,586	27,448
Mother Education: Less than HS	0.779	0.775	0.004	0.332	22,586	27,448
College Expectations	0.696	0.691	0.005	0.333	22,448	27,229
Family Income	0.094	0.095	−0.001	0.757	22,615	27,477
Joint orthogonality F-test				0.425		
Distance between neighbors	0.065	0.065	−0.000	0.591	52,838	62,310

Notes: This table presents balance tests of observable characteristics between applicants and neighbors depending on neighbors' offer status. Each row shows the estimate of a regression of the corresponding covariate onto an indicator equals to one if the closest neighbor received an offer in her most preferred school and a set of lottery fixed effects. Panel A displays estimates of applicants characteristics, while panel B shows estimates of neighbors characteristics. Joint orthogonality shows the p-value of a F-test of joint significance of all covariates listed in the corresponding panel. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table III: ITT and 2SLS Estimates of Neighbor Spillovers

	First Stage	ITT			2SLS		
	In $t-1$, Neighbor:	In t, Applicant:			In t, Applicant:		
In $t-1$, Neighbor:	Enrolled in 1st Choice (1)	Ranks School Any (2)	Ranks School 1st (3)	Attends School (4)	Ranks School Any (5)	Ranks School 1st (6)	Attends School (7)
Admitted to 1st Choice	0.677***	0.007*	0.006**	0.005**			
	(0.003)	(0.004)	(0.003)	(0.002)			
Enrolled in 1st Choice					0.010*	0.008**	0.008**
					(0.005)	(0.004)	(0.004)
Mean (Not Enrolled)	0.169***	0.364***	0.156***	0.122***	0.385***	0.163***	0.118***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.004)	(0.003)	(0.003)
F-Statistic					25,738	25,738	25,738
N-Obs	64,995	115,148	115,148	115,148	115,148	115,148	115,148
N-Clusters		65,244	65,244	65,244	65,244	65,244	65,244

Notes: This table reports intent-to-treat (ITT) and two-stage least squares (2SLS) estimates of neighbors' spillovers on applicants' decisions made the following year. Columns (1)-(3) display OLS estimates of regressions where the variable of interest is an indicator equal to one if the closest neighbor received an offer in their most preferred school. Column (4) presents the OLS estimate from a regression where the dependent variable is an indicator equal to one if the neighbor enrolled in ninth grade in the same school where they received an offer and the explanatory variable is the offer receipt. Columns (5)-(7) report 2SLS coefficients instrumenting neighbors' enrollment with the offer. All models include lottery fixed effects. Standard errors are clustered at the neighbor level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table IV: Placebo Test: Effect of Neighbor's Next Year Choices

	In t, Applicant:		
In $t+1$, Neighbor:	Ranks School Any (1)	Ranks School 1st (2)	Attends School (3)
Enrolled in 1st Choice	0.007	0.007	-0.001
	(0.006)	(0.005)	(0.004)
Mean (Not enrolled)	0.380***	0.166***	0.133***
	(0.004)	(0.003)	(0.003)
F-Statistic	22,084	22,084	22,084
N-Obs	92,732	92,732	92,732
N-Clusters	57,790	57,790	57,790

Notes: This table presents a placebo test where we regress the outcome of an applicant in period t onto an indicator equal to one if the closest neighbor applying in round $t+1$ receives a seat offer at their most preferred school. Clustered standard errors at the neighbor level are reported in parenthesis. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table V: Placebo Test: Effect of Neighbor's Contemporaneous Choices

	In t, Applicant:		
	Ranks School Any	Ranks School 1st	Attends School
In t, Neighbor:	(1)	(2)	(3)
Enrolled in 1st Choice	0.004	0.001	-0.003
	(0.005)	(0.004)	(0.003)
Mean (Not enrolled)	0.395***	0.177***	0.129***
	(0.003)	(0.003)	(0.002)
F-Statistic	41,888	41,888	41,888
N-Obs	133,682	133,682	133,682
N-Clusters	100,828	100,828	100,828

Notes: This table shows a placebo test where we regress the outcome of an applicant in period t onto an indicator equal to one if the closest neighbor applying in round t receives a seat offer at their most preferred school. Clustered standard errors at the neighbor level are reported in parenthesis. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table VI: Heterogeneous Neighbor Spillovers: by Gender

	In t, Applicant:		
	Ranks School Any	Ranks School 1st	Attends School
	(1)	(2)	(3)
Enrolled in 1st Choice			
Both male	0.018**	0.013**	0.019***
	(0.008)	(0.006)	(0.006)
Male applicant, female neighbor	0.011	0.008	−0.002
	(0.008)	(0.006)	(0.006)
Female applicant, male neighbor	−0.001	0.002	−0.004
	(0.008)	(0.006)	(0.006)
Both female	0.009	0.008	0.016**
	(0.009)	(0.007)	(0.006)
Complier Mean			
Both male	0.408***	0.177***	0.124***
	(0.006)	(0.004)	(0.004)
Male applicant, female neighbor	0.366***	0.150***	0.112***
	(0.006)	(0.004)	(0.004)
Female applicant, male neighbor	0.363***	0.150***	0.114***
	(0.006)	(0.004)	(0.004)
Both female	0.402***	0.174***	0.121***
	(0.006)	(0.005)	(0.004)
F-Statistic	6,283	6,283	6,283
N-Obs	115,148	115,148	115,148
N-Clusters	65,244	65,244	65,244

Notes: This table reports 2SLS estimates from equations (4) and (5) of the effects of each applicant's closest neighbor attending her most preferred school depending on applicant and neighbor gender. Each column reports the main estimate and an interaction between the main effect and an indicator variable of applicant and neighbor gender. Enrollment is instrumented with an indicator equals to one if the closest neighbor received a seat offer. All models control for lottery fixed effects, defined as a school-year-priority group combination (see the main text for details). Clustered standard errors at the neighbor level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table VII: Heterogeneous Neighbor Spillovers: by Parents' Education

	In t, Applicant:		
	Ranks School Any (1)	Ranks School 1st (2)	Attends School (3)
Enrolled in 1st Choice			
None attended college	0.015*	0.015**	0.017***
	(0.008)	(0.007)	(0.006)
Only neighbor's attended college	0.028**	0.018*	0.003
	(0.013)	(0.010)	(0.008)
Only applicant's attended college	−0.053***	−0.022**	−0.008
	(0.012)	(0.010)	(0.009)
Both attended college	−0.065***	−0.039***	0.002
	(0.018)	(0.014)	(0.013)
Complier Mean			
None attended college	0.410***	0.175***	0.129***
	(0.006)	(0.004)	(0.004)
Only neighbor's attended college	0.360***	0.155***	0.115***
	(0.009)	(0.006)	(0.005)
Only applicant's attended college	0.413***	0.181***	0.132***
	(0.009)	(0.007)	(0.006)
Both attended college	0.428***	0.196***	0.126***
	(0.012)	(0.009)	(0.007)
F-Statistic	4,406	4,406	4,406
N-Obs	68,787	68,787	68,787
N-Clusters	42,622	42,622	42,622

Notes: This table reports 2SLS estimates from equations (4) and (5) of the effects of each applicant's closest neighbor attending her most preferred school depending on applicant's and neighbor's parents' education. Each column reports the main estimate and an interaction between the main effect and an indicator variable of whether at least one parent obtained a college degree. Enrollment is instrumented with an indicator equals to one if the closest neighbor received a seat offer. All models control for lottery fixed effects, defined as a school-year-priority group combination (see the main text for details). Clustered standard errors at the neighbor level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table VIII: Heterogeneous Neighbor Spillovers: by Previous Math Scores

	In t, Applicant:		
	Ranks School Any (1)	Ranks School 1st (2)	Attends School (3)
Enrolled in 1st Choice			
Both below median score	0.031***	0.025***	0.013*
	(0.010)	(0.008)	(0.007)
Only applicant below median score	0.042***	0.019**	0.009
	(0.010)	(0.008)	(0.007)
Only neighbor below median score	−0.027***	−0.017**	−0.004
	(0.010)	(0.008)	(0.008)
Both above median score	−0.020**	−0.007	0.009
	(0.010)	(0.008)	(0.007)
Complier Mean			
Both below median score	0.382***	0.163***	0.122***
	(0.007)	(0.005)	(0.005)
Only applicant below median score	0.354***	0.151***	0.112***
	(0.007)	(0.005)	(0.004)
Only neighbor below median score	0.418***	0.186***	0.138***
	(0.007)	(0.006)	(0.005)
Both above median score	0.420***	0.184***	0.126***
	(0.007)	(0.005)	(0.004)
F-Statistic	4,804	4,804	4,804
N-Obs	75,619	75,619	75,619
N-Clusters	46,475	46,475	46,475

Notes: This table reports 2SLS estimates from equations (4) and (5) of the effects of each applicant's closest neighbor attending her most preferred school depending on applicant's and neighbor's previous math test scores. Each column reports the main estimate and an interaction between the main effect and an indicator variable of whether applicant and neighbor scored above the median in the corresponding test score distribution. Enrollment is instrumented with an indicator equals to one if the closest neighbor received a seat offer. All models control for lottery fixed effects, defined as a school-year-priority group combination (see the main text for details). Clustered standard errors at the neighbor level are reported in parenthesis. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table IX: Heterogeneous Neighbors Spillovers: by School Characteristics

	Outcome: Applicant Ranks Same School (1st)					
In $t-1$, Neighbor:	(1)	(2)	(3)	(4)	(5)	(6)
Enrolled in 1st Choice	0.034***	0.006	0.010*	0.007	0.008*	0.025***
	(0.005)	(0.006)	(0.006)	(0.004)	(0.004)	(0.008)
Enrolled × Distance (miles)	−0.009***					−0.009***
	(0.001)					(0.001)
Enrolled ×		0.017**				0.027
Average 10th Grade Scores (s.d.)		(0.008)				(0.019)
Enrolled ×			0.013			0.009
Effectiveness Index (s.d.)			(0.008)			(0.019)
Enrolled ×				0.001		−0.026
Peer Composition Index (s.d.)				(0.008)		(0.018)
Enrolled ×					0.015***	0.013*
School Climate Index (s.d.)					(0.005)	(0.007)
Mean (Not Enrolled)	0.172***	0.161***	0.156***	0.154***	0.162***	0.163***
	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)	(0.005)
F-Statistic	13,616	1,893	1,766	3,249	3,315	550
N-Obs	106,659	83,079	83,079	111,694	110,095	78,768
N-Clusters	61,288	47,000	47,000	63,304	62,412	45,223

Notes: This table presents 2SLS estimates from equations (8) and (9) to investigate how the effect of the closest neighbor attending a given school on applicants' ranks depends on neighbors' school characteristics. Distance (measured in miles) corresponds to the euclidean distance between the applicant and neighbor's school. Average tenth grade scores uses math and language scores in 2017 and 2018. School effectiveness, peer composition, and school climate are indexes that summarize schools characteristics along these dimensions. The school effectiveness index uses the average math and language school-level scores in tenth grade, school value-added on high-school graduation, and school value-added on college attendance. The peer composition index uses cohort-level average math and language scores, the proportion of students with college-educated mothers, and the proportion of students whose parents expect them to attend college. The school climate index is created by the Ministry of Education and refers to students', teachers', and parents' perceptions about the school environment. The effectiveness and peer composition indexes are constructed using a principal component model and the Bartlett method. Average tenth grade scores and each of the indexes described above are standardized to be mean zero and unit variance using all public and private schools with positive ninth-grade enrollment. In each specification the variables defining the interaction are included as controls. In addition, all models include lottery fixed effects. Standard errors are clustered at the neighbor level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table X: Estimates of Effects on Applicants' Attended Schools

	First Stage	Top-Ranked School			Attended School		
		Mean (Compliers)	ITT	2SLS	Mean (Compliers)	ITT	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
10th Grade Scores (s.d.)	0.761***	−0.171***	0.017***	0.022***	−0.501***	0.017***	0.022***
	(0.005)	(0.006)	(0.005)	(0.007)	(0.005)	(0.005)	(0.006)
F-Statistic				22,041			21,646
N-Obs	55,232		99,426	97,137		97,053	94,845
Effectiveness Index (s.d.)	0.784***	−0.284***	0.022***	0.027***	−0.617***	0.023***	0.028***
	(0.006)	(0.007)	(0.007)	(0.010)	(0.007)	(0.007)	(0.009)
F-Statistic				13,770			12,081
N-Obs	39,091		60,152	56,101		54,425	50,881
Peer Composition Index (s.d.)	0.732***	−0.333***	0.027***	0.039***	−0.559***	0.028***	0.041***
	(0.005)	(0.005)	(0.006)	(0.008)	(0.005)	(0.005)	(0.007)
F-Statistic				17,141			16,917
N-Obs	55,351		99,747	97,585		97,153	95,077
School Climate Index (s.d.)	0.708***	0.041***	0.021***	0.030***	−0.137***	0.031***	0.044***
	(0.005)	(0.007)	(0.005)	(0.007)	(0.007)	(0.005)	(0.007)
F-Statistic				14,104			13,695
N-Obs	54,430		97,385	94,823		94,908	92,445

Notes: This table reports 2SLS estimates from equations (10) and (11) of neighbors' school characteristics on applicants' top-ranked and enrolled school characteristics. Columns (2)-(4) display estimates on the applicant's top-ranked school characteristics, and columns (5)-(7) show estimates on the applicant's attended school. For each outcome, column (1) shows the first stage (coefficient ρ in equation (10)). Columns (2) and (5) show mean outcomes for compliers computed following Abadie (2002). Columns (3) and (6) show coefficients from regressions of outcomes on the characteristics of the school where the neighbor received an offer. Columns (4) and (7) report 2SLS coefficients instrumenting the neighbor's attendance with the offer. All models include lottery fixed effects and cluster standard errors at the neighbor level. See the main text for details about the construction of each outcome. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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A Online Appendix

Neighbors' Spillovers on High School Choice

Juan Matta Alexis Orellana

A.1 Additional Figures

Figure A.1: Number of Applicants by Grade

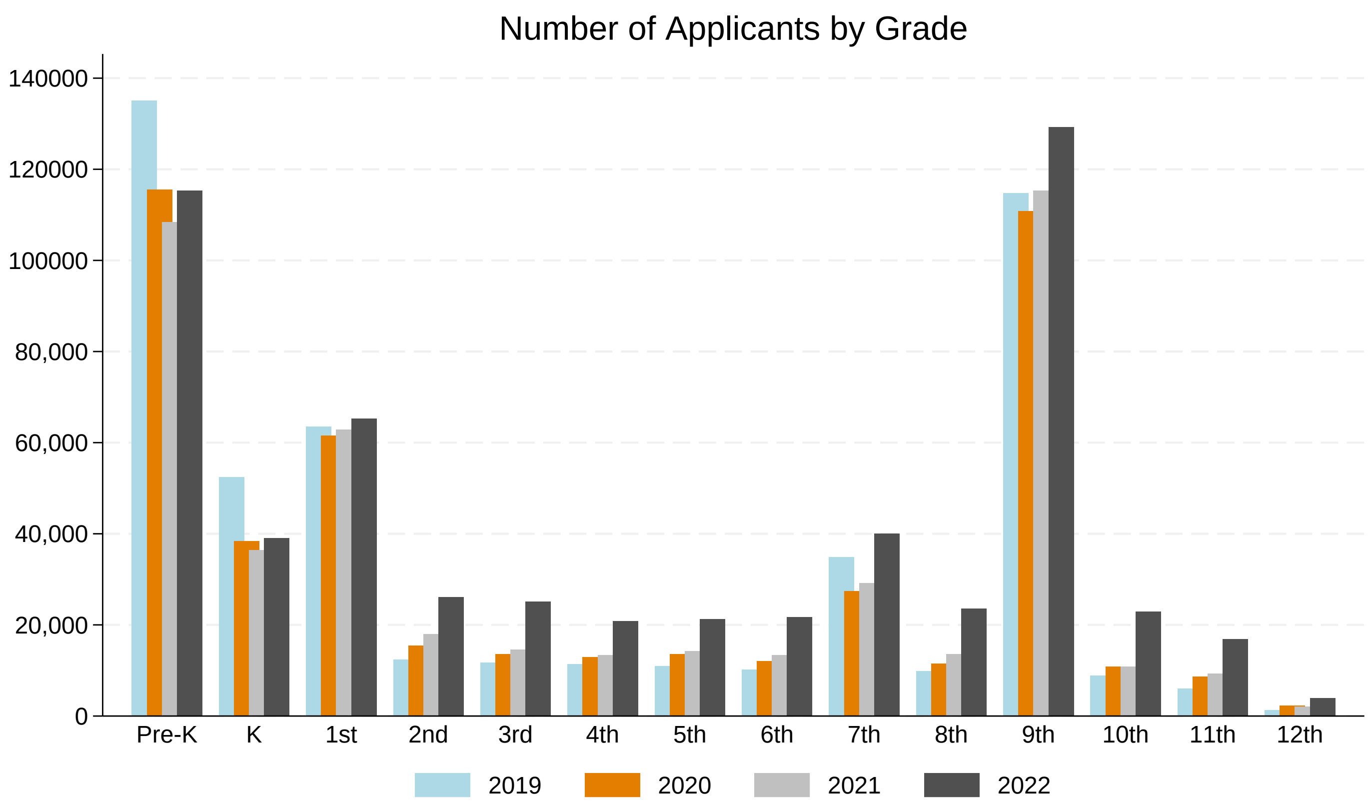


Figure A.2: Number of Participating Schools by Grade

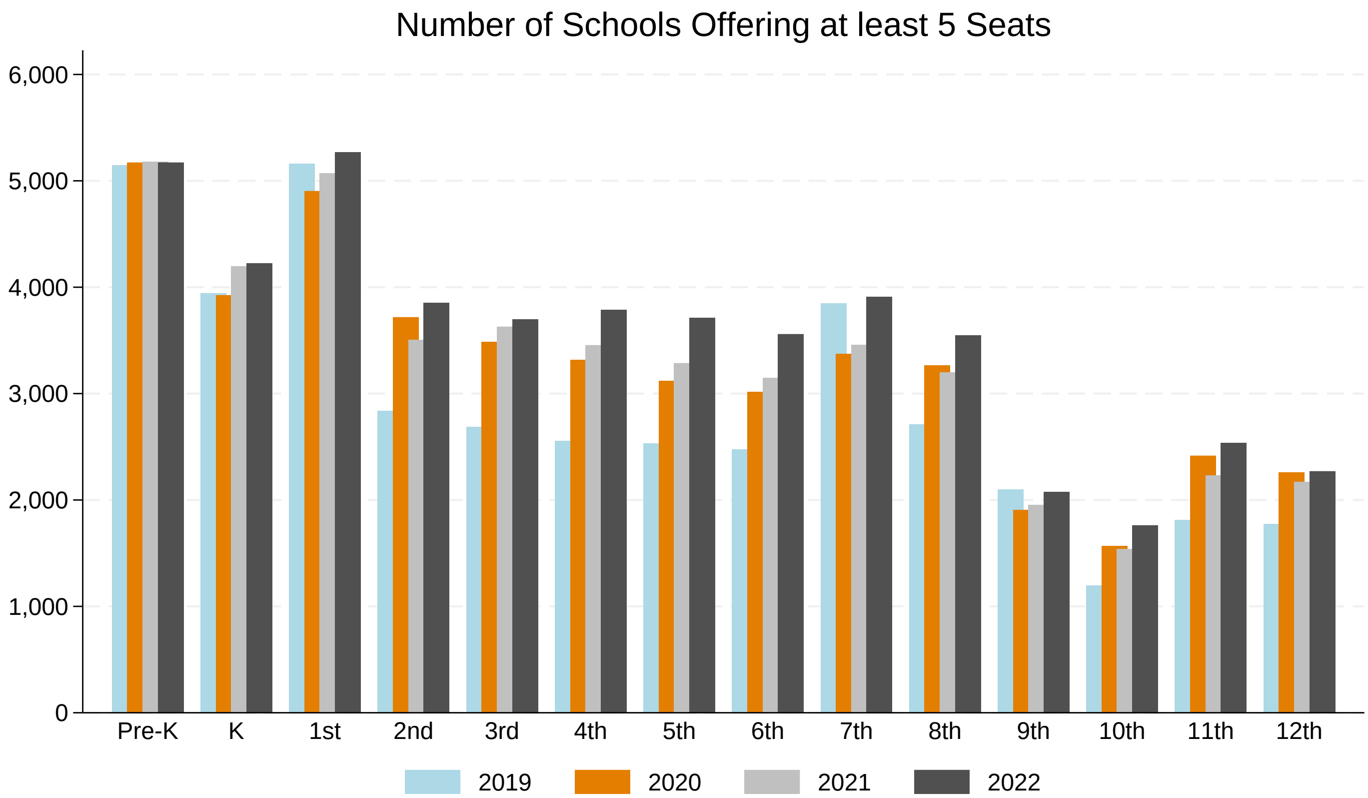
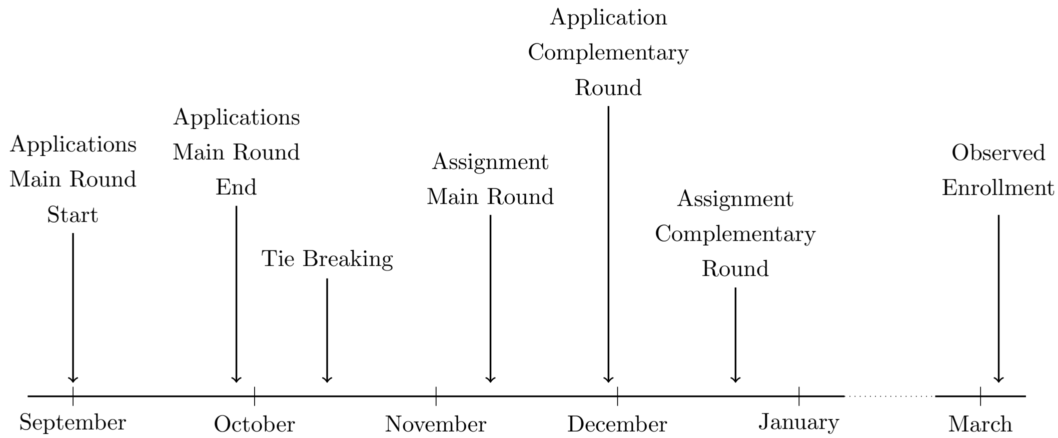


Figure A.3: Timeline of the Application Process



Source: Correa et al. (2022)

Figure A.4: Priority Groups

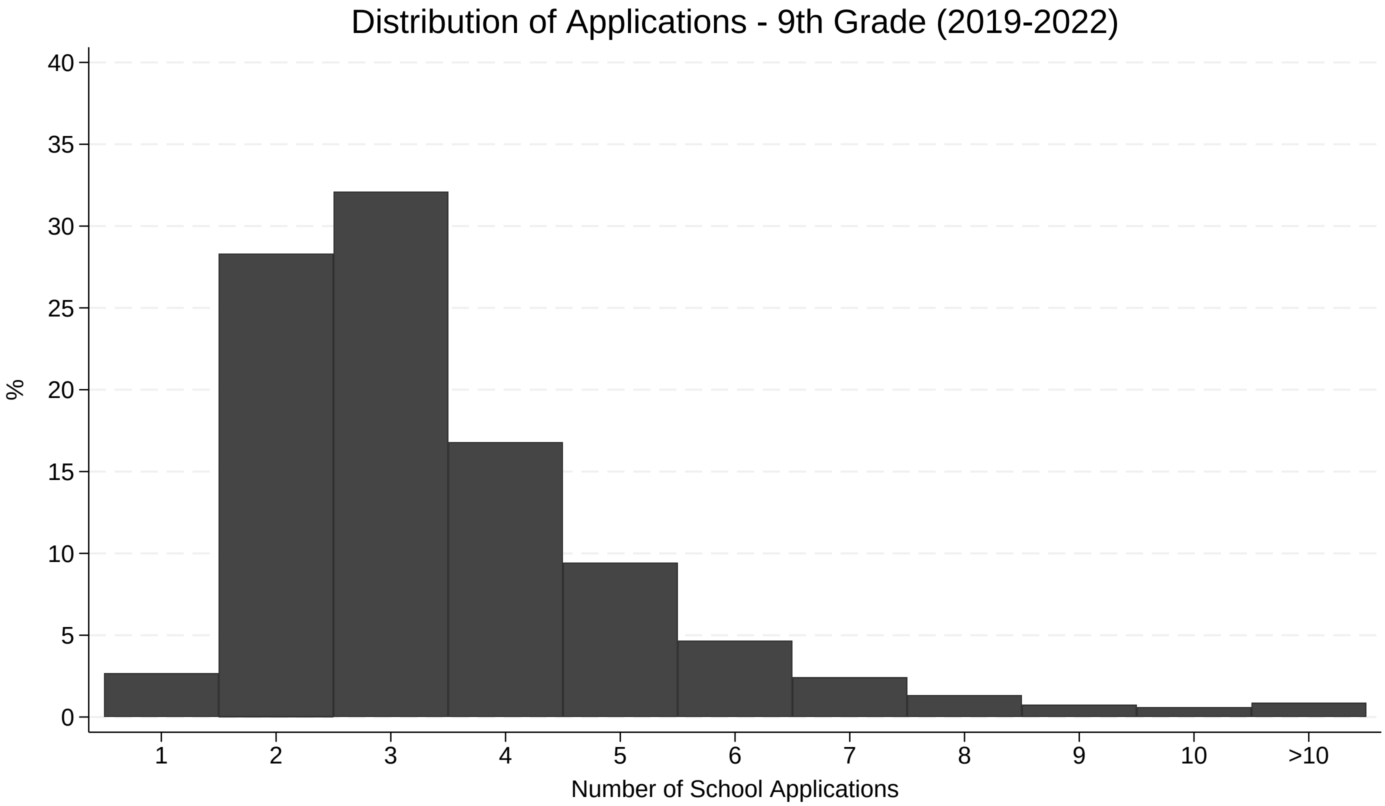
Table 1. Weak Priorities by Type-Specific Seats

Priority	Special needs	Academic excellence	Disadvantaged	No trait
1	Current school	Current school	Current school	Current school
2	Special needs	Academic excellence	Siblings	Siblings
3	Siblings	Siblings	Disadvantaged	Working parent
4	Working parent	Working parent	Working parent	Returning students
5	Returning students	Returning students	Returning students	No priority
6	No priority	No priority	No priority	

Note. Lower numbers indicate higher priority.

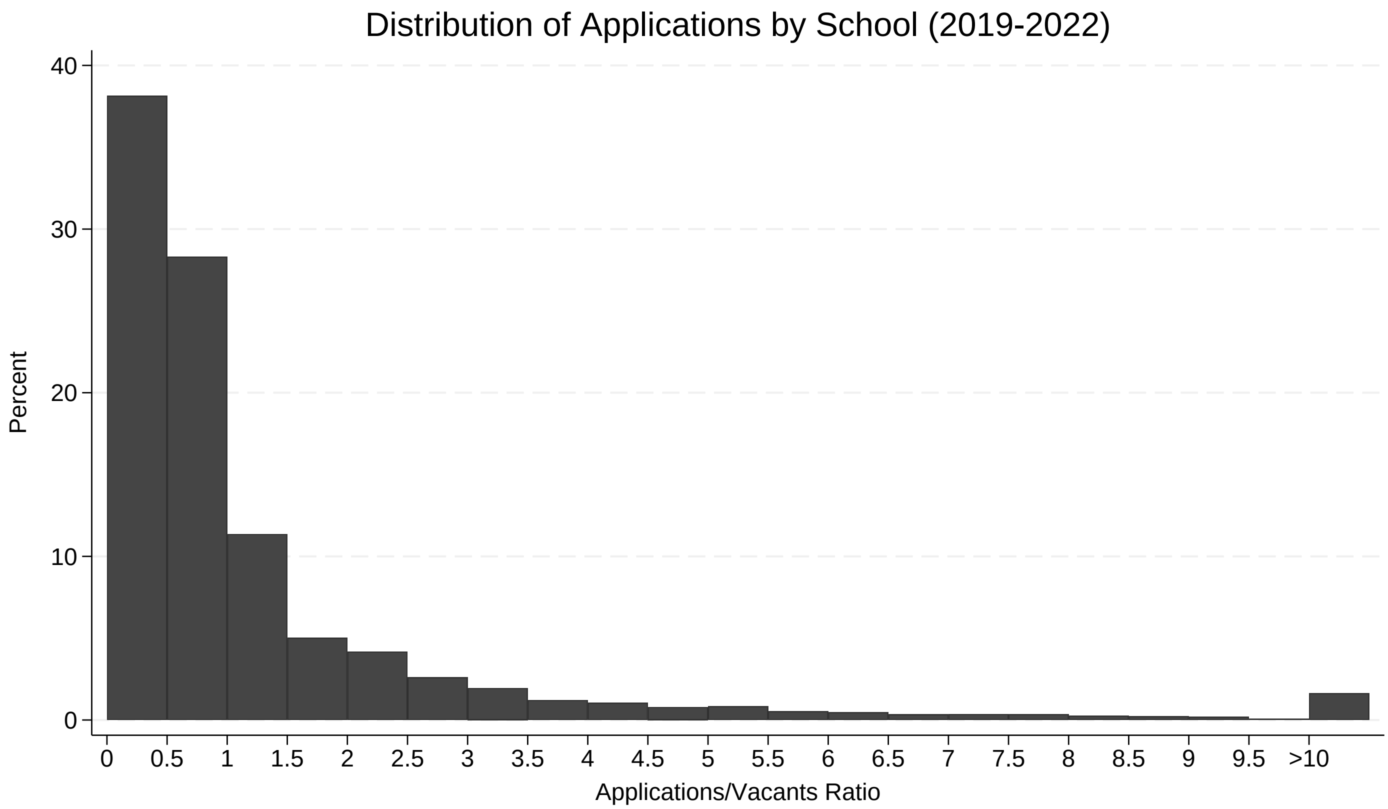
Source: Correa et al. (2022)

Figure A.5: Distribution of School Applications - 9th Grade



Notes: This plot shows the distribution of the number of schools submitted by ninth-grade applicants. This plot pools the 2019-22 application rounds.

Figure A.6: Applicants/Seats Ratio Across Schools



Notes: This plot shows the distribution of the applicants/seats ratio for schools offering ninth grade, restricted to schools offering at least five vacant seats. The number of applicants considers only first-rank preferences.

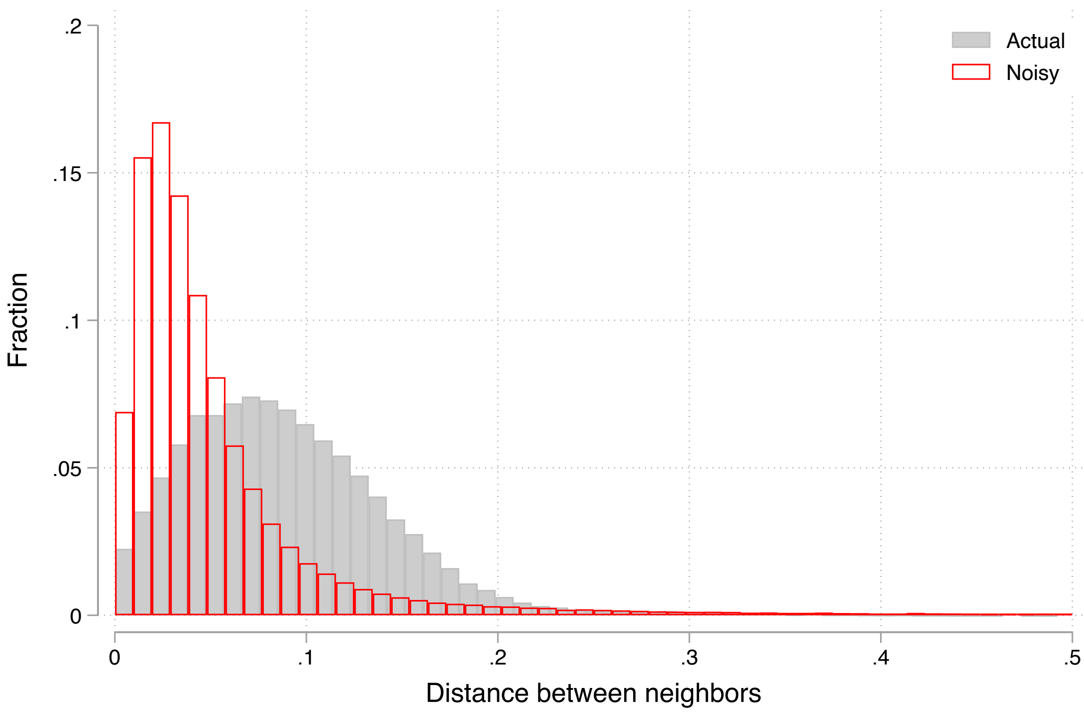
A.2 Distance Between Neighbors

We assess the implications of identifying neighbors with noisy location measures by using administrative data on actual distances between students applying to schools in 2018. The Ministry of Education provided this restricted dataset containing the distance between each pair of applicants in that year. Using these records allows us to characterize the actual distribution of distances and compare it to the one we obtain from noisy locations. However, since this additional data spans only one year, we are unable to match applicant-neighbor pairs or replicate our 2SLS estimates using actual distances.

Using the restricted data, we consider all applicants to ninth grade in 2018 and then drop applicants with imputed locations in our sample. We then use the noisy locations in our sample to identify the closest neighbor applying in the same year (since we don't have data of actual distances for previous years). For each pair, we thus observe both the actual and the noisy measures of distance. Figure A.7 shows histograms with the distribution of both variables. The distribution of the noisy distance is more right-skewed than the actual distance. The mean of the actual distance distribution is 0.089 miles, while the median and the 90th percentile are 0.083 and 0.156 miles, respectively. These numbers suggest that most neighbors in our sample live within one or two blocks of distance.

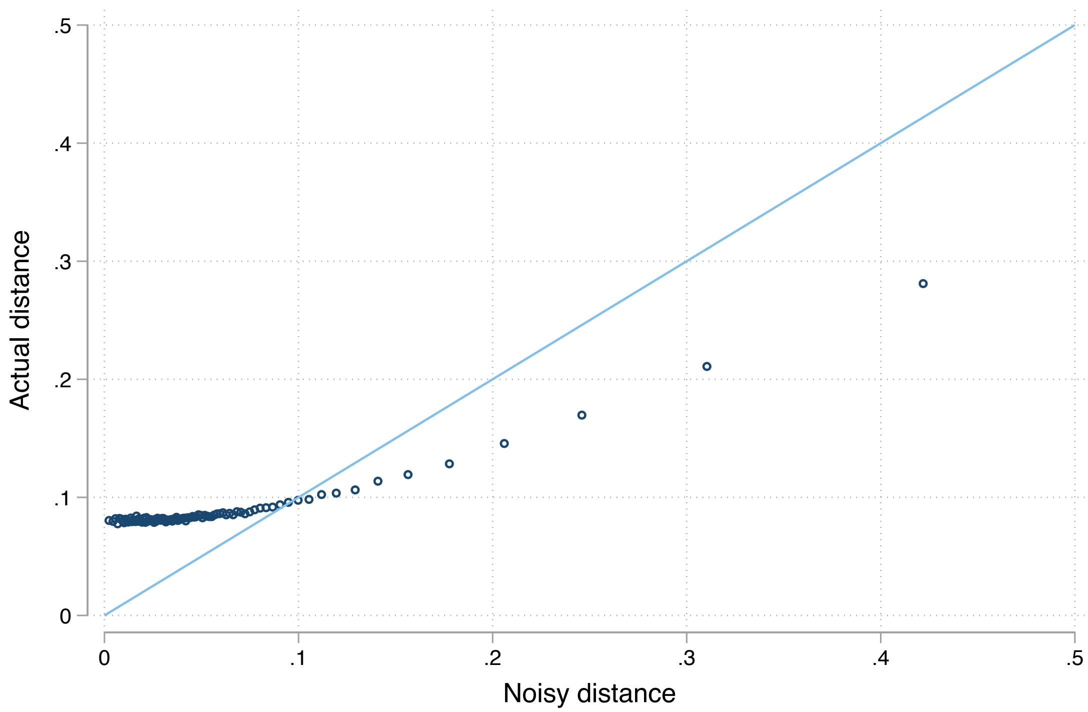
Figure A.8 shows a binscatter plot of actual vs. noisy distance between neighbors, using our sample of closest neighbors. Although both measures are positively associated, the curve is almost flat for noisy distances below 0.1 miles. While our previous exercise shows that our noisy location data allows us to identify close neighbors, this analysis suggests that our noisy measure of distance is not a very good proxy of the actual distance between individuals in our sample. For this reason, when studying heterogeneous spillovers by distance, we prefer to focus on the influence of the n-th closest neighbor, for $n = 1, \ldots, 7$.

Figure A.7: Distribution of Distance Between Neighbors (Actual vs. Noisy)



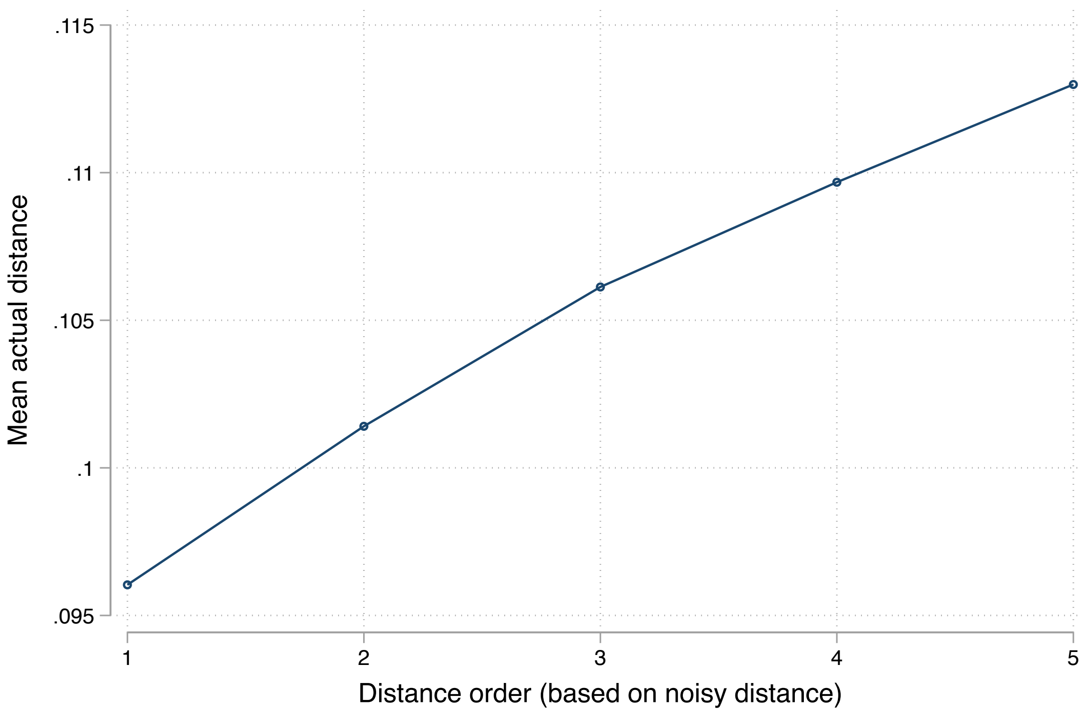
Notes: The figure shows histograms of actual and noisy distance between closest neighbors identified from noisy locations, for applicants to ninth degree in 2018.

Figure A.8: Actual vs. Noisy Distance Between Neighbors



Notes: This binscatter plot characterizes the expected value of the actual distance between neighbors given the measure of noisy distance, for our sample of closest neighbors identified from noisy locations. Both the applicant and its neighbor are applicants to ninth grade in 2018.

Figure A.9: Mean Distance Between Neighbors for the n-th Nearest Neighbor



Notes: This plot shows the expected value of the actual distance between the applicant and its n-th nearest neighbor (identified from noisy locations) as a function of n. Both the applicant and its neighbors are applicants to ninth grade in 2018.

A.3 School Value-Added

We use information from ninth-grade cohorts in 2016, 2018, and 2019 to construct a proxy of school effectiveness for high school graduation and college attendance. For the 2016 and 2018 cohorts we observe test scores and family background in eight grade, while for the 2019 cohort we observe the same variables in sixth grade. Based on this information, we estimate a simple school value-added model of the form:

$$y_{ist} = X'_{ist}\beta + \theta_s + \theta_t + \xi_{ist} \tag{12}$$

Our outcomes y_{ist} are indicators equal to one when student i in cohort t graduated on time from high school s and attended college the next year, respectively. The vector X_{ist} includes a third-order polynomial in math and language lagged test scores, the interaction of both, and indicators for gender, family income, and mother's education. θ_t corresponds to cohort fixed effects. We estimate equation (12) and recover the raw school fixed effects $\hat{\theta}_s$.

As it is common practice in the teacher and school value-added literature (Kane and Staiger, 2008; Chetty et al., 2014; Bacher-Hicks et al., 2019), we generate empirical Bayes (EB) shrunken estimates of $\hat{\theta}_s$ to account for sampling error and minimize mean square prediction errors. Following Abdulkadiroğlu et al. (2020), we assume that the distribution of the true school-specific parameters θ_s is given by the following hierarchical Bayesian model:

$$\hat{\theta}_s|\theta_s \sim N(\theta_s, \Omega_s) \tag{13}$$

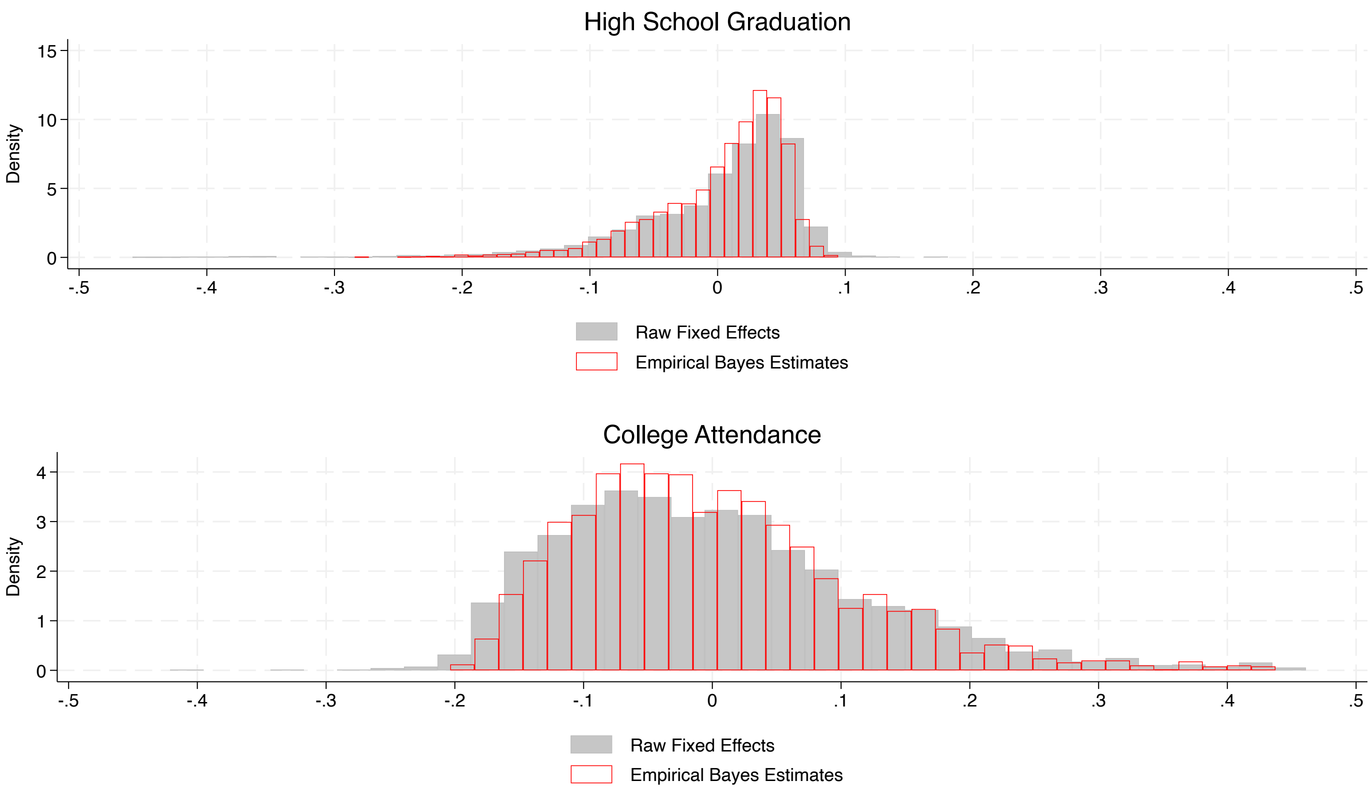
$$\theta_s \sim N(\mu_\theta, \Sigma_\theta) \tag{14}$$

Where Ω_s is the sampling variance of the estimator $\hat{\theta}_s$, while μ_θ and Σ_θ are the mean and variance of the distribution of the underlying parameters θ_s. We compute the posterior mean for each school, $\hat{\theta}_s^{EB}$ as the weighted average of the OLS estimate and the prior mean, where the weight corresponds to the signal-to-noise ratio:

$$\hat{\theta}_s^{EB} = \frac{\Omega_s^{-1}}{\Omega_s^{-1} + \Sigma_\theta^{-1}}\hat{\theta}_s + \frac{\Sigma_\theta^{-1}}{\Omega_s^{-1} + \Sigma_\theta^{-1}}\mu_\theta \tag{15}$$

In practice, we construct the sample estimates of the hyperparameters μ_θ and Σ_θ using the distribution of estimated fixed effects $\{\hat{\theta}_s\}_{s=1}^S$, while we employ the standard error of $\hat{\theta}_s$ to estimate Ω_s. We plug $\hat{\Omega}_s, \hat{\mu}_\theta, \hat{\Sigma}_\theta$ into (15) and use the EB posterior means as regressors in our analysis of heterogeneity effects by school characteristics in section 4.3.4. Figure A.10 shows the distribution of the raw fixed effects ($\hat{\theta}_s$) and the EB estimates ($\hat{\theta}_s^{EB}$) for both outcomes. The standard deviation of the raw school fixed effects for high school graduation is 0.07 while the standard deviation of the empirical Bayes estimates is 0.05. For college completion, these standard deviations are 0.12 and 0.11, respectively.

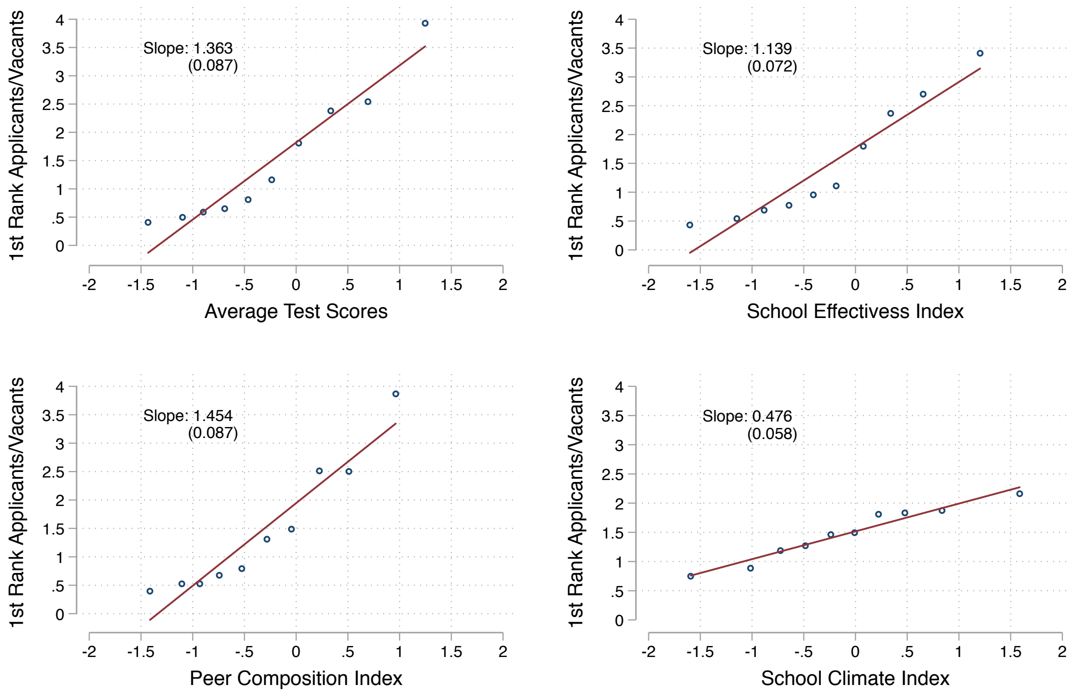
Figure A.10: Empirical Bayes Estimates for School Value-Added



Notes: This plot shows the distribution of estimates of school value-added on high school graduation and college enrollment obtained from equation (12). Each subplot shows the distribution of the raw school fixed effects ($\hat{\theta}_s$) and the empirical Bayes estimates ($\hat{\theta}_s^{EB}$), constructed following Abdulkadiroğlu et al. (2020). Empirical Bayes estimates are used to characterize schools in our analysis of section 4.3.4.

A.4 School Attributes and Demand

Figure A.11: Correlation between School Attributes and Demand



Notes: This plot shows the correlation between each school index described in section 4.3.4 and the ratio of first-rank applicants to the number of seats available in ninth grade in each school. We exclude schools with less than five vacant seats. Average tenth grade scores correspond to the school-level math and language scores observed in each school in 2018 (last available year). The school effectiveness index uses the average math and language school-level scores in tenth grade, school value-added on high-school graduation and college attendance. The peer composition index uses cohort-level average math and language scores, the proportion of students with college-educated mothers, and the proportion of students whose parents expect them to attend college. The school climate index is reported by the Ministry of Education and refers to students', teachers', and parents' perceptions about the school environment. The effectiveness and peer composition indexes are constructed using a principal component model and the Bartlett method. We standardize each outcome to be mean zero and unit variance using all public and private schools with positive ninth-grade enrollment.

A.5 Additional Tables

Table A.1: Summary of Acceptances by School Grade

	2019		2020		2021		2022	
	Accepted in 1st-3rd options (1)	Accepted in 1st option (2)	Accepted in 1st-3rd options (3)	Accepted in 1st option (4)	Accepted in 1st-3rd options (5)	Accepted in 1st option (6)	Accepted in 1st-3rd options (7)	Accepted in 1st option (8)
School Level								
Pre-K and K	85%	59%	91%	68%	92%	70%	92%	68%
Elementary	76%	38%	78%	39%	79%	40%	77%	35%
Middle School	81%	42%	83%	44%	81%	42%	76%	32%
High School	87%	60%	87%	59%	86%	57%	82%	49%

Notes: This table summarizes the assignment process for different school levels. Columns (1), (3), (5), and (7) show the proportion of applicants who were allocated and accepted a seat in one of their top three choices. Columns (2), (4), (6), and (8) show the proportion of applicants who accepted a seat in their top choice.

Table A.2: Application Cohorts and Data Availability

	Calendar Year						
Application Cohort	2016	2017	2018	2019	2020	2021	2022
2016	8th	9th	10th	11th	12th	post-HS	post-HS
2017	7th	8th	9th	10th	11th	12th	post-HS
2018	6th	7th	8th	9th	10th	11th	12th
2019	5th	6th	7th	8th	9th	10th	11th
2020	4th	5th	6th	7th	8th	9th	10th
2021	3th	4th	5th	6th	7th	8th	9th
2022	2nd	3rd	4th	5th	6th	7th	8th

Notes: This table presents the availability of data for different cohorts of eight-graders. Grey cells represent cohorts participating in the school assignment under the Deferred Acceptance mechanism. Black cells denote the years and grades in which we observe previous test scores and background information for each cohort.

Table A.3: OLS Estimates of Neighbor Spillovers

	In t, Applicant:		
In $t-1$, Neighbor:	Ranks School Any (1)	Ranks School 1st (2)	Attends School (3)
Enrolled in 1st Choice	0.058***	0.055***	0.089***
	(0.004)	(0.003)	(0.002)
Mean (Not enrolled)	0.362***	0.134***	0.081***
	(0.009)	(0.004)	(0.004)
N-Obs	115,486	115,486	115,486
N-Clusters	65,582	65,582	65,582

Notes: This table shows OLS estimates of neighbors' spillovers on applicants' decisions observed the following year, excluding lottery fixed effects. Enrolled is an indicator equal to one if the closest neighbor enrolled at their most preferred school. Clustered standard errors at the neighbor level are reported in parenthesis. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.4: Estimates of Neighbors Spillovers: K-12 Schools

	In t, Applicant:		
	Ranks School Any	Ranks School 1st	Attends School
In t, Neighbor:	(1)	(2)	(3)
Enrolled in 1st Choice	0.008	0.008**	0.007**
	(0.005)	(0.004)	(0.003)
Mean (Not enrolled)	0.369***	0.157***	0.113***
	(0.004)	(0.003)	(0.002)
F-Statistic	28,016	28,016	28,016
N-Obs	137,119	137,119	137,119
N-Clusters	72,431	72,431	72,431

Notes: This table presents 2SLS estimates from our main specification (1)-(2) using all K-12 schools offering at least five vacancies in each application year. All models include lottery fixed effects. Standard errors clustered at the neighbor level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.5: Estimates of Neighbors Spillovers: Using Attendance Days to Define Enrollment

	In t, Applicant:			
In $t-1$, Neighbor:	Ranks School 1st (1)	Ranks School 1st-3rd (2)	Ranks School Any (3)	Attends School (4)
Enrolled to 1st Choice	0.009**	0.011*	0.011*	0.007*
	(0.004)	(0.005)	(0.006)	(0.004)
Mean (Not Enrolled)	0.162***	0.338***	0.385***	0.116***
	(0.003)	(0.004)	(0.004)	(0.003)
F-Statistic	20,916	20,916	20,916	20,916
N-Obs	115,148	115,148	115,148	115,148
N-Clusters	65,244	65,244	65,244	65,244

Notes: This table presents 2SLS estimates from our main specification (1)-(2) considering the school where each student was enrolled the largest number of days during each year. Standard errors clustered at the neighbor level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.6: Heterogeneous Neighbor Spillovers: by *SEP* Status

	In t, Applicant:		
	Ranks School Any (1)	Ranks School 1st (2)	Attends School (3)
Enrolled in 1st Choice			
Both SEP students	0.017**	0.015**	0.009
	(0.008)	(0.006)	(0.005)
SEP applicant, non-SEP neighbor	0.033***	0.016**	0.011
	(0.010)	(0.008)	(0.007)
Non-SEP applicant, SEP neighbor	−0.025**	−0.007	0.006
	(0.010)	(0.008)	(0.008)
None is SEP student	−0.030**	−0.006	0.008
	(0.013)	(0.010)	(0.009)
Complier Mean			
Both SEP students	0.400***	0.170***	0.128***
	(0.006)	(0.004)	(0.004)
SEP applicant, non-SEP neighbor	0.359***	0.150***	0.113***
	(0.007)	(0.005)	(0.004)
Non-SEP applicant, SEP neighbor	0.417***	0.181***	0.128***
	(0.008)	(0.006)	(0.005)
None is SEP student	0.400***	0.166***	0.110***
	(0.009)	(0.007)	(0.005)
F-Statistic	4,533	4,533	4,533
N-Obs	92,107	92,107	92,107
N-Clusters	53,624	53,624	53,624

Notes: This table reports 2SLS estimates from equations (4) and (5) of the effects of each applicant's closest neighbor attending her most preferred school depending on applicant's and neighbor's socioeconomic status. Each column reports the main estimate and an interaction between the main effect and an indicator variable of whether neighbor or applicant is classified as disadvantaged by the Ministry of Education in the corresponding year. Enrollment is instrumented with an indicator equals to one if the closest neighbor received a seat offer. All models control for lottery fixed effects, defined as a school-year-priority group combination (see the main text for details). Clustered standard errors at the neighbor level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.7: Heterogeneous Neighbor Spillovers: by Previous Language Scores

	In t, Applicant:		
	Ranks School Any	Ranks School 1st	Attends School
	(1)	(2)	(3)
Enrolled in 1st Choice			
Both below median score	0.025**	0.010	0.008
	(0.010)	(0.008)	(0.007)
Only applicant below median score	0.031***	0.021**	0.012*
	(0.010)	(0.008)	(0.007)
Only neighbor below median score	−0.019*	−0.007	−0.001
	(0.010)	(0.008)	(0.008)
Both above median score	−0.015	−0.002	0.005
	(0.010)	(0.008)	(0.007)
Complier Mean			
Both below median score	0.387***	0.175***	0.128***
	(0.007)	(0.006)	(0.005)
Only applicant below median score	0.362***	0.148***	0.104***
	(0.007)	(0.005)	(0.004)
Only neighbor below median score	0.415***	0.179***	0.136***
	(0.007)	(0.006)	(0.005)
Both above median score	0.418***	0.184***	0.129***
	(0.007)	(0.005)	(0.004)
F-Statistic	4,877	4,877	4,877
N-Obs	75,057	75,057	75,057
N-Clusters	46,182	46,182	46,182

Notes: This table reports 2SLS estimates from equations (4) and (5) of the effects of each applicant's closest neighbor attending her most preferred school depending on applicant's and neighbor's previous language test scores. Each column reports the main estimate and an interaction between the main effect and an indicator variable of whether applicant and neighbor scored above the median in the corresponding test score distribution. Enrollment is instrumented with an indicator equals to one if the closest neighbor received a seat offer. All models control for lottery fixed effects, defined as a school-year-priority group combination (see the main text for details). Clustered standard errors at the neighbor level are reported in parenthesis. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.