Neighbors' Spillovers on High School Choice*

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Abstract

Do 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 7-10% 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 applicant-neighbor pairs with lower education, college expectations, and prior academic achievement, measured by previous scores in national standardized tests. Lower-achieving applicants are more likely to follow neighbors to schools with better attributes when their closest neighbor's test scores are higher. Our findings suggest the existence of frictions that prevent some families from learning about all available schools. 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 choices, centralized school systems

JEL Codes: I21, I24

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1 Introduction

Many cities in the U.S. and in 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. To the extent that more disadvantaged families employ informal sources of information to make decisions, due to search costs or other frictions, these families will be less likely to consider all the options offered by the centralized system, potentially leading them to attend schools with inferior quality. 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 article, we study the importance of close neighbors on families' high school application and attendance decisions. Using data from the Chilean school assignment system between 2018 and 2021, we link applicants to their closest residential neighbors and show that shocks to neighbors' enrollment decisions spillover to applicants in the next year, affecting their probability 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 have important implications for the design and evaluation of school choice interventions.

Estimating 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 exploit the implementation of a centralized school admission system in Chile. Under this system, student assignment is determined using the Deferred Acceptance mechanism and tie-breaking rules in oversubscribed schools. Building on earlier work (Abdulkadiroğlu et al., 2011, 2017; Gray-Lobe et al., 2021) these features motivate an instrumental variables strategy to identify the effect of the closest neighbor's school choice on applicants' decisions. 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 school admission 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 2SLS estimates, show that an applicant exposed to a neighbor who attends their target school is 1.2 and 1 percentage points more likely to include this school in the application list and to rank it as the most preferred school, respectively. These estimates represent an increase of 4% and 7% relative to the average levels. In terms of school attendance, the presence of a neighbor attending a given school increases by 1.2 percentage points the probability of enrolling in the same school in 9th grade.

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

This estimate corresponds to a 10% increase relative to the average level. Our empirical strategy is supported by the institutional features determining how tie-breaking rules are implemented, discarding the possibility of manipulation. To rule out other potential threats to identification, we conduct several balance and placebo tests and find no evidence of imbalances between neighbors who received and did not receive a seat offer.

We conduct a series of heterogeneity analyses to investigate how these average effects vary by observed characteristics. We show that these effects are much larger for applicants from more disadvantaged backgrounds. We find that families characterized by lower mother's educational level, college expectations, family income, and previous achievement in national standardized tests are more likely to mimic neighbors' previous choices. Then, we analyze how this influence varies according to relative differences between applicant and neighbor using previous achievement in standardized test scores. We find that spillover effects are *negative* when applicants' scores are above the median, but neighbors' scores are not. This pattern holds when we consider other correlates of high achievement, such as college expectations or family income.

Three potential mechanisms might explain our findings: learning from neighbors' previous choices, avoiding search costs, and finding better school matches. Among these mechanisms, we find that the first is most plausible. Applicants who have lower baseline test scores than their nearest neighbors are 3 percentage points more likely to rank the school attended by the closest neighbor as their top-choice. Doing so leads to applying to schools with higher average 10th grade test scores and a higher fraction of high-achieving peers. As the difference between applicants' and neighbors' test scores becomes positive, applicants are less likely to consider the school attended by the closest neighbor. However, for high-achieving applicants, neighbors' previous choices do not impact the school characteristics where applicants attend. These patterns suggest that neighbors convey information about school attributes valued by families and that applicants interpret these signals based on ability differences. This additional information leads to applicants to include (exclude) neighbors' schools in their choice sets when neighbors are perceived as relatively higher-achieving (lower-achieving) students. In examining the other two potential mechanisms, we do not find evidence supporting them. Using the number of schools submitted as a proxy of search intensity, we do not find evidence supporting that more disadvantaged families avoid decision-making costs by following their neighbors. We also evaluate the effect of following neighbors on student achievement at the end of 9th grade for the first cohort of students enrolled under the new system, and find no evidence of effects on grade progression, attendance, or GPA.

This paper contributes 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

[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).

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 they focus on extensive margin changes in enrollment, we are interested on 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 generalizable to other educational systems in developed and middle-income countries. Second, the school admission system involves around 90% of students 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 residential proximity. We are aware of no 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 impacts (Cullen et al., 2006; Hastings et al., 2006; Abdulkadiroğlu et al., 2018) and long-term impacts (Deming, 2011; Deming et al., 2014; Dustan et al., 2017; Gray-Lobe et al., 2021) of being offered a seat in an oversubscribed school, 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., 2022) 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 a larger number of schools in their choice sets, there exist frictions that prevent some families from learning about all the options available to them. These decisions propagate to other applicants and amplify their consequences on more disadvantaged families. These spillover effects

[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`

may partly explain the persistence in 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 Law), 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 passing of the law, 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 reform was fully implemented by 2019, and since then around 400,000 applicants have participated every year. Figure 3 shows the number of applicants by year. In 2021, 400,000 students were registered in the centralized system. Figure 4 shows the distribution of applicants by grade in the last three rounds. Most applications are in school transition grades (pre-K, first, and ninth grades). In the Chilean educational system, a number of secondary flagship schools (*liceos emblemáticos*) start in seventh grade, which explains the large number of applications observed in this level. Figure 1 summarizes the main stages of the admission process (Correa et al., 2022). 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. Table 1 shows the acceptance rates for each round at different school levels. On average, 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% obtain 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 that are 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 disadvantaged students and some schools can select a fraction of their seats based on admission tests.[5] In the former case,

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

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). Figure 2summarizes how seats are classified and the priority groups in each case.[6] Second, whenever schools are oversubscribed, ties are broken randomly within each priority group. Figure 5 shows the proportion of schools receiving more first-rank applications than vacant seats. This figure shows that in ninth grade more than 30% of schools participating in the system are oversubscribed.

Crucially for our purposes, parents are required to provide their addresses at the moment of applying. The administrative records include the geocoded location of every applicant. For confidentiality purposes, these locations contain a small amount of noise.[7] The presence of noise implies a potential mis-classification error if we incorrectly match applicants and neighbors. 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 imputed addresses. Second, we drop applicants whose registered location indicates one region but their school enrollment records in the same year indicate a different region. Third, to distinguish between close neighbors and members of the same family applying in different years, we employ anonymized parent identifiers to discard siblings or pairs of students associated to the same adult responsible of the application.

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) taken in 4th, 6th or 8th grades.[8] These records contain rich information about family characteristics. We also employ information about the location of public, voucher, and private schools from administrative records to characterize the geographical market conditions for each applicant. Specifically, we consider the number of public, voucher, and private schools around 2 miles of each applicant's address.

2.1 Sample Construction

We build our sample focusing on ninth grade applicants observed in the 2018-2021 application rounds. We match each applicant to the closest neighbor who applied to the same level in the previous round. For each applicant, we select the closest neighbor after excluding cases following the criteria described at the end of the previous section. This procedure drops cases where we suspect

in our analysis.

[6]We discuss how we consider different priority groups in our analysis in section 3.

[7]The median distance between the address we observe and the original is 175 meters. The maximum distance is 300 meters.

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

there exists a mechanical relationship between applications over two consecutive years. For example, students who are not siblings but live with or are linked to the same responsible adult. We also drop observations where the distance between the applicant and neighbor is higher than 15 miles.[9] Finally, we also exclude from the estimation sample neighbors who took an admission test in their most preferred school and had a positive probability of being offered a seat in this priority group.[10] 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. We link applicants to enrollment records in the next year to observe which school they finally attend. For applicants participating in the 2021 round, we use attendance records between March and September of 2022 to construct the enrollment variable.

We merge this sample of students to two additional sources of information. First, we merge students' math and language test scores in previous standardized national exams from administrative records. These contain survey information about family characteristics, such as reported income, parents' education, and college expectations. Table A1 in the Appendix summarizes the grade where we can merge information for each cohort. As a second complementary source of information, we incorporate additional geographical information by linking each individual's georeferenced location to the neighborhood unit where she resides, employing information from the Ministry of Social Development. Finally, we construct estimates of school value-added linking high-school graduation and college enrollment to previous test scores for 2015 and 2017 eight-grade cohorts.

2.2 Sample Description

Table 2 presents summary statistics for all applicants and the sample we employ in our empirical analysis, defined as applicants linked to neighbors whose seat offers were subject to randomization. Columns (1) and (2) show that our estimation sample is representative of the total applicant population. Panel A shows that around 48% of applicants are girls, 62% have priority status, and around 31% are high-achievers. Priority status and high-achiever status are determined by the ministry of Education to classify students in one of the categories displayed in Figure 2.[11] Average baseline math and language test scores are -0.2σ and -0.15σ, respectively. The magnitudes and negative signs reflect the differences in achievement between students enrolled in public and private schools. Around 20% of applicants' mothers have a college degree and 10% of applicants' families report a monthly income higher than US$1,000. The average distance to the closest neighbor is 0.25 miles. Similarly, Panel B displays application characteristics for both groups. The average number of

[9] Including these set of outliers in our main analysis does not change our results.

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

[11] Priority status is determined on the basis of households economic hardship, income, and mother's education. Schools serving these students receive additional resources from the government. High-achieving status is defined as students in the top quintile of the previous year GPA distribution.

schools ranked is 3 and around 64% of applicants submit three schools or less. Overall, our estimation sample also exhibits small differences with the total population when comparing these outcomes.

Figure 6 shows the distribution of applications pooling across 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. Figure 7 shows the distribution of the applicant-vacant ratio. For each school s and year t, we compute the number of students applying to this school as first choice A_{st} and the vacant seats offered by the school V_{st}. The ratio A_{st}/V_{st} is a measure of the excess demand for each school. Around 30% of schools display a ratio $A_{st}/V_{st} > 1$.

Figure 8 shows differences in the number of applications and school characteristics chosen by families across socioeconomic groups in 2021. The upper-left panel shows the distribution of the total number of applications submitted to the school assignment platform. We observe that priority students are more likely to apply to a lower number of schools. The blue bars show that more than 50% of priority students apply to less than four schools. The upper-right panel shows differences in the school-level math 10th grade scores, based on national standardized tests taken in 2018. Conditional on submitting the same number of schools, priority students apply to schools with significantly lower average scores. Conditional on applying to three schools, the average gap is 0.25σ. We find the same pattern for Language average scores: after conditioning on the number of applications, families from lower-SES apply to schools with lower average test 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).

Overall, these patterns are consistent with previous findings on heterogeneous preferences for school attributes across socioeconomic groups in Chile (Neilson, 2021). The rest of this paper focuses on analyzing whether this differential behavior impacts future cohorts' application decisions. With this objective in mind, we start by presenting our empirical strategy to identify neighbors' spillover effects on school applications.

3 Empirical Strategy

In this section we describe our empirical strategy to estimate the impact of close neighbors' attendance on applicants' decisions. Let i denote an *applicant* in period t, and s' be i's most preferred school in the application list. Let $j(i)$ denote the *closest neighbor* to i, measured by the euclidean distance between both, d_{ij}. To identify the effect of j's choices on i's observed applications, we exploit the quasi-random variation generated by the tie-breaking rules used for oversubscribed schools. Let s denote the school ranked first by the neighbor j in the application process and let x_{js} be an indicator if the neighbor j attends school s in year t. We define z_{js} equals to one if j received a seat offer in school s. Conditional on j's priority group, z_{js} is exogenous and it is a valid instrument for

j's observed attendance decision.

Our analysis focuses primarily on two outcomes. First, we use an indicator equals to one if i ranks s first (i.e., $s' = s$) in the next application round. Second, we also employ an indicator equals to one if i attends s in ninth grade. Formally, we estimate the following set of linear probability models using two stage least squares (2SLS):

$$y_{ijs} = \alpha + \beta x_{js} + \phi_{jl} + \epsilon_{ijs} \tag{1}$$

$$x_{js} = \delta + \lambda z_{js} + \varphi_{jl} + \eta_{js} \tag{2}$$

Equations (1) and (2) describe our baseline specification. ϕ_{jl} and φ_{jl} correspond to lottery fixed effects in the oversubscribed school ranked by j as their top choice. We define a lottery as a *year-school-priority group* combination. By including lottery fixed effects, our identifying variation comes from individuals applying to the same school and categorized into the same priority group. Based on the characteristics of the admission system, each priority group includes students with the same high-achieving status, disadvantaged status, former student status, and number of siblings enrolled in the school. Therefore, each student in a given priority group has the same probability of receiving an offer. In all our specifications, we cluster standard errors at the neighbor level to account for the fact that one neighbor can be linked to multiple applicants.

3.1 Identifying Assumptions

To be able to interpret β as a local average treatment effect (LATE) we need to satisfy the assumptions discussed in Imbens and Angrist (1994). We discuss each of them in Appendix A.2. Additionally, in order to claim that we identify the causal effect of neighbor j on applicant i, we also need to assume that there is no influence of neighbor j to applicant i through other peers who might indirectly affect i's decisions. In the next section, we present evidence ruling out contemporaneous effects as well as neighbors' influence in $t-2$.[12] Following the discussion in Barrios-Fernández (2022), if this assumption does not hold β can be interpreted as a reduced form parameter capturing both the direct effect of neighbor j as well as the indirect impact through other peers.

Under the assumptions discussed above, the estimate of β can be interpreted as a weighted average of local average treatment effects for applicants induced to mimic neighbors' decisions on the basis of random offers. As Aguirre and Matta (2021) and Altmejd et al. (2021) discuss, the LATE estimate captures a treatment effect relative to a combination of the next-best alternatives for each neighbor. To gain a better understanding of this counterfactual scenario, we compute differences in school

[12] As in Barrios-Fernández (2022), we consider three possible cases where this indirect channel could operate. To facilitate the exposition of the argument, assume each applicant i is linked to a set of $\mathcal{J}_{i,\tau}$ neighbors who apply in rounds $\tau \leq t$. First, neighbor j could affect other applicants in year t ($k \in \mathcal{J}_{i,t}$) who also impact i contemporaneously. Second, j could influence other neighbors in $t-1$ ($k \in \mathcal{J}_{i,t-1} \backslash \{j\}$). Finally, j's effect could also incorporate previous influence from neighbors applying in $\tau < t-1$ ($k \in \mathcal{J}_{i,t-\tau}$).

characteristics between the first and second options. For applicants who submit only one school, we compare it to the current in case ninth grade is offered at this school.[13] On average, schools ranked as second choices are very similar to the most preferred ones. They are located 0.2 miles further to applicants' residences. In terms of school attributes, they have 0.03σ lower school-level 10th grade scores, 0.9% higher fraction of students at the bottom quartile of the test score distribution and 0.6% lower proportion of students at the top quartile of the test score distribution.

4 Results

4.1 Balance Tests

Before presenting our main results, we turn to examining the validity of our empirical strategy. The exclusion restriction requires that, conditional on lottery fixed effects, an offer should be uncorrelated to other determinants of neighbors' school attendance. Panel A of Table 3 checks whether observable characteristics of applicants are balanced between students whose closest neighbor received or not an offer. We test differences across several individual and family characteristics. Specifically, we consider gender, priority status, high-achieving status, baseline test scores, parents' education, and family income for each applicant. Columns (1) and (2) show the 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 one estimates are not statistically significant at the 10% level. The only exception is baseline math test scores. In this case, we find that applicants whose neighbors were admitted to their target school scored 0.026σ lower than applicants with neighbors who did not obtain an offer. For the remaining covariates, differences are small and not statistically significant. We also show the results of a joint significance test where we regress the offer indicator onto all the background variables listed above and test the hypothesis that all coefficients are jointly zero. The large p-value provides further evidence that the likelihood of a neighbor receiving an offer is exogenous to applicants' observable characteristics.

Analogously, we also test whether neighbors' observable characteristics are balanced between offered and non-offered individuals. Panel B of Table 3 shows the estimates of regressions on the same set of observable characteristics as well as the p-value from a joint significance test. As would be expected from random assignment, the estimates show that, conditional on lottery fixed effects, student attributes do not explain seat assignment. Finally, the last row shows that there are no statistically significant differences in the euclidean distance between each applicant and their closest neighbor.

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

4.2 Neighbors' Spillovers on School Applications and Enrollment

Table 4 shows our intent-to-treat (ITT) and 2SLS estimates of the influence of neighbors on applicants' high school applications and enrollment decisions. Columns (1)-(2) show the ITT estimates on the probability of applying to the same school ranked first by the closest neighbor in the previous year. Column (1) shows that the probability of applying to this school in first to third order increases by 0.8 p.p. on average if the closest neighbor receives an offer. This estimate represents an increase of 2.5% relative to the unconditional probability (31%). Column (2) shows that the probability of applying to the same school as top priority increases by 0.7 p.p. (or an increase of 5% relative to the baseline level). Column (3) in Table 4 shows the ITT estimate on school attendance. We find an increase by 0.8 p.p. in the probability of attending the same school as the neighbor's most preferred alternative. Relative to the baseline level (0.12), this estimate corresponds to an increase of 7%. Column (4) 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.

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 between the first and third preference order increases by 1.2 p.p. and the probability of ranking this school in the first place increases by 1 p.p. These estimates represent increases of 4% and 7% relative to the baseline levels, respectively. Column (7) shows that the closest neighbor's enrollment in their most preferred school increases the probability of an applicant attending it by 1.2 percentage points. This estimate, which is statistically significant at the 1% level, implies an increase of 10% relative to baseline level.

Although we cannot separately identify changes on the number of options considered from changes in rankings, we can analyze how the number of submissions changes as a result of observing neighbors' previous choices. Table A2 in the Appendix shows 2SLS estimates on the number of applications, and the probability of submitting one, at least two, at least three, or at least four schools. Overall, we find modest changes on these margins. Column (1) shows that, on average, the number of additional submissions increases by 0.03, or 0.9% of the baseline number. Columns (2)-(5) show that this increase is driven by a rise of 1.6 percentage points in the probability of applying to three or more schools. Since around 73% of applicants consider at least three schools this estimate represents a modest increase of only 2% relative to the average. Overall, these estimates suggest that neighbors' enrollment have a minor impact on the number of options considered by applicants and that the average effects discussed above might reflect changes in how families rank schools already included in their choice sets.

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 conducting inference based on 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 employing their adjustment method for tests with a significance level of 0.05 and 0.01.[14] Given the large value of our reported F-statistics in Table 4, standard errors and confidence intervals remain unchanged.

Comparison to OLS estimates: We report OLS estimates from specifications not including lottery fixed effects in Table 5. Using the same estimation sample, we find an increase of 3.9 percentage points in the probability of applicants mimicking their closest neighbor's top-ranked school. This is around four times larger than the 2SLS estimate. Similarly, the OLS estimate of enrollment is 6.8 percentage points, around six times larger than the 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.[15] 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 are in line with the effects reported by this literature.[16] 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 combination in the first preference. Similarly, Aguirre and Matta (2021) find an increase of 1.9 p.p. in the probability of choosing the same college-major combination.

Placebo Tests: In addition to the balance tests presented in Table 3, a second test exploits the fact that applicants should be influenced only by neighbors' previous choices. If our results were driven by neighbors' influence, 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

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

[15] Bobonis and Finan (2009) find an increase in secondary school enrollment rate of 5 p.p. 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 p.p. in peer group school attendance leads to a 5 p.p. increase in individual attendance.

[16] Joensen and Nielsen (2018) find an increase of 7 p.p. 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 p.p. in the likelihood of applying to the same school in Mexico. Dahl et al. (2020) find that younger siblings are 2.4 p.p. more likely to choose the same high-school major as their older sibling in Sweden.

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 7 and 6 show the ITT estimates of the offer indicator in $t+1$ and t on outcomes observed one year before and the same year, respectively. In both cases, our estimates are one order of magnitude lower to 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: Our main results are robust to the inclusion of neighborhood-unit fixed effects. We define neighborhoods using the smallest geographical unit where each applicant is observed (*unidades vecinales*) to account for unobserved heterogeneity which might influence school preferences (for example, persistent differences in public transportation quality, crime rates, or other neighborhood amenities). In this case, we identify spillover effects by comparing outcomes of applicants who reside in the same local area and whose neighbors' assignment was determined by the tie-breaking rules in the previous round. Table A3 in the Appendix shows 2SLS estimates using two-way fixed effects. Although substantially more demanding in terms of statistical power, we do not find differences in our results. In addition, one might be concerned about the possibility that some students are not observed in our sample because they are already enrolled in K-12 schools and therefore they choose not to participate in the school assignment process. To check the robustness of our results to this type of selection, we restrict our analysis to eight grade applicants enrolled in K-8 schools who necessarily need to apply to get a seat in ninth grade. Table A4 in the Appendix shows that our results also do not change when we impose this additional restriction. Finally, we also check whether our results change when we focus on urban locations. Urban and rural areas differ in geography and access to different types of schools. Consequently, we can check whether these differences drive our main results by employing Census population data at the municipality (*comuna*) level.[17] For each municipality, we compute the proportion of urban population and classify them in quartiles. Table A5 in the Appendix shows our estimates after restricting our sample to municipalities with more than 48% of urban population (above first quartile) and more than 65% of urban population (above the median). In both cases, we find minor differences in our 2SLS estimates relative to the main results reported in Table 4.

Fade-out Effects: We also investigate how persistent spillover effects are by estimating (1) and (2) in a sample where each applicant is linked to the closest neighbor who participated in the assignment process two years before. Figure 10 shows these estimates alongside our main results and placebo tests. Panel A and B show that spillover effects fade-out quickly. For spillover effects on application decisions, the estimate of the effect after two years is 0.6 p.p. Nevertheless, it is not statistically significant at the 10% level. Similarly, for enrollment decisions, the estimate is almost zero two years after.

Considering the baseline levels, our estimates show economically important effects. On average, we find that neighbors' assignment outcomes affect applicants' behavior in the next admission process.

[17] We use population data for 2017.

Applicants are more likely to rank a school as top-choice and to enroll in it when the closest neighbor also enrolled in it. In the next section we turn to examining differences in both margins by applicant and neighbor characteristics.

4.3 Heterogeneity by Applicant Characteristics

The results from the previous section show that, on average, neighbors influence future applicants' school ranks and, as a consequence, which schools they attend. In this section, we study whether this influence varies according to observed characteristics. To do so, we augment our baseline specification (1)-(2) with an interaction term $x_{js} \cdot x_{ij}$ that allows to analyze how the average effect varies across applicants with different characteristics or when applicant and neighbor have similar characteristics.

We start by examining heterogeneity by applicants' observables ($x_{ij} = x_i$). Specifically, we consider gender and proxies of socio-economic status to test whether some groups of applicants are more likely to be influenced by their neighbors' previous decisions. We estimate the following set of equations using 2SLS:

$$y_{ijs} = \alpha + x_{js}(\beta_1 + \beta_2 x_i) + \gamma x_i + \phi_{jl} + \epsilon_{ijs} \tag{3}$$

$$x_{js} = \delta + z_{js}(\lambda_1 + \lambda_2 x_i) + \kappa x_i + \varphi_{jl} + \eta_{ijs} \tag{4}$$

Table 8 shows our results. Columns (1) and (2) show 2SLS estimates of the main effect and its interaction with an indicator if the applicant is female. The estimates show that the probability of ranking the same school as top-choice and attending it is stronger for boys. The first row shows that, for this group, these probabilities increase by 1.6 p.p. and 1.5 p.p., respectively. For girls, these estimates are 0.3 p.p. and 0.9 p.p., respectively, although only the estimate for enrollment in statistically significant at the 5% level. Columns (3)-(8) examine how estimates vary by applicants' socioeconomic status. We find that the probability of mimicking previous choices is stronger by more disadvantaged applicants, measured by priority status, parents' college expectations, and family income.

For each of these variables, the first row shows a positive and statistically significant estimate. At the same time, the interaction term is negative and of similar size, implying that the effect is no longer statistically significant for more advantaged groups. We find large differences between families expecting applicants to attend college and families that do not. Columns (5) and (6) show that when the closest neighbor enrolls in their most preferred school, the probability of applying to the same school as their top-choice and attending it increases by 4.1 and 2.7 p.p., respectively. The effect is close to zero and not statistically significant at the 10% level for applicants whose parents expect them to attend college. We find a similar pattern when considering differences in applicants' family income. The likelihood of mimicking the closest neighbor's application and enrollment decision increases by 2.4 p.p. and 2.1 p.p., respectively, when the reported family income is CLP 300k or lower, which roughly corresponds to the bottom tercile of the family income distribution in our sample.

Taken together, these estimates reveal that the average effects we document in the previous section are stronger for boys and applicants from more disadvantaged backgrounds. We next turn to an analysis of how these patterns vary when applicants and neighbors are similar in observed characteristics.

4.4 Applicant-Neighbor Matching

As a second heterogeneity analysis, we investigate the importance of matching to explain spillover effects. We start by examining the importance of matching on academic performance, measured by baseline math and language test scores. We compute the median achievement in each subject every year and define four groups based on applicant and neighbor performance: $(i, j) \in \{below, above\} \times \{below, above\}$. Table 9 shows 2SLS estimates of the differential effects of matching based on previous achievement. The first group $(below, below)$ includes applicant-neighbor pairs where both scored below the median, and corresponds to the reference category in all our regressions. We find evidence of a strong positive assortative matching for pairs located at the bottom of the skill distribution. Columns (1) and (4) in Table 9 show that, when the closest neighbor attends their top-choice school, an applicant is 4 p.p. and 2 p.p. more likely to apply to the same school when both students belong to the bottom half of the math or language test score distribution, respectively.

The likelihood of mimicking neighbors' choices decreases but it is still positive when the applicant scored below the median but the neighbor scored above the median. The effect on application for the category $(i, j) = \{below, above\}$ is 2.9 p.p. and 2.2 p.p. for math and language scores, respectively. These differences show that the spillover effects displayed in 8 for low-SES groups are stronger when applicants and neighbors have lower academic performance levels.

When the applicant belongs to the top half of the test score distribution the patterns are substantially different. The interaction of enrolled and $(i, j) = \{above, below\}$ is large and negative, implying that an applicant does not submit the same school when the neighbor belongs to the bottom half of the test score distribution. For math, the likelihood of considering the school in the first-third ranks decreases by 2.9 p.p. (p-value$<$0.01) while for language decreases by 2.2 p.p. (p-value$<$0.05). Finally, when the applicant-neighbor pair belongs to the top half of the test score distribution there are no spillover effects. The size of the interaction term $(i, j) = \{above, above\}$ implies that the effects for this group are almost zero and not statistically significant at the 10% level.

We find a similar pattern when we analyze effects on submitting the same school as top-choice and attending it. Columns (3) and (6) show that the probability of attending the same school increases by 1.6 p.p. and 1.9 p.p., respectively, when applicant and neighbor scored below the median. When the applicant belongs to the bottom half and the neighbor belongs to the top half, the effect is 1.6

p.p. (p-value<0.05) and 1.8 p.p. (p-value<0.01), respectively. However, for applicants who scored above the median, the effect is closer to zero and not statistically significant in most cases.

We also analyze how influence varies by gender. Table 10 shows how estimates vary by applicants' and neighbors' gender. The first and fourth rows show that spillover effects are larger for same-gender pairs. When applicant and neighbor are boys (girls) the likelihood of submitting the same school in first rank increases by 3.5 p.p. (1.4 p.p.). When the applicant is a boy and the closest neighbor is a girl, the effect is not statistically significant. Conversely, when the applicant is a girl and the closest neighbor is a boy, the probability of ranking the same school in the first position decreases by 1.6 p.p. (p-value<0.01). Column (3) displays our estimates on the effect of attending the same school. Overall, we observe a similar pattern in terms of matching.

Table A7 in the Appendix investigates whether this pattern holds for other background variables. Panel A shows our estimates when we consider applicant's and neighbor's families' college expectations and Panel B shows the heterogeneity by family income. In this case, we classify families according to their reported monthly income being above or below CLP$500,000 (≈ US$720 in 2018). This figure represents the 75th percentile in the income distribution in our sample. In both cases, we find a largely consistent picture relative to the matching patterns observed when we consider previous test scores: families from lower socioeconomic backgrounds are more likely to follow neighbors, regardless of their observed characteristics. On the other hand, this effect goes on the opposite direction when the applicant is relatively more advantaged than the closest neighbor. When both applicant and neighbor belong to the more advantaged categories, the influence is the smallest across all subgroups.

4.5 Heterogeneity by Distance

In this section, we analyze whether neighbors' influence varies as a function of distance. We construct quartiles of distance d_{ij} and interact each of them with neighbor's enrollment x_{js}. Then we estimate equations (3) and (4) using 2SLS to recover the estimates for each quartile.

As expected, the influence of the closest neighbor decreases with physical distance. Figure 9 shows our estimates of neighbors' spillovers on application decisions for each quartile. In the horizontal axis, we include the distance (in miles) covered in each quartile. The largest effect is observed for distances in the first quartile. The magnitude of the estimates decrease monotonically as the distance increases. The left-side plot shows that for distances greater than 0.06 miles (≈ 100 meters) the estimate is no longer statistically significant. Alternatively, we also employ a specification where we interact the main effect with distance. In this case, we also find a negative and statistically significant estimate for the interaction term..

4.6 Do Spillover Effects Vary By School Characteristics?

In this section, we investigate whether spillovers vary depending on school attributes. Guided by recent evidence about parental preferences in the school choice literature (Abdulkadiroğlu et al., 2017; Beuermann et al., 2022), we study whether patterns vary along the following attributes: i) proxies of school effectiveness, ii) peer composition, and iii) indicators of personal and social development. To conduct this analysis, we first classify neighbors' first-ranked schools in two groups (above and below the median) and estimate spillover effects on application and enrollment.[18]

Table 11 summarizes our findings. Each panel presents estimates of spillover effects depending on the value of each school's attribute. For all panels, columns (1) and (2) show our estimates for below-median schools while columns (3) and (4) display results for above-median schools. Panels A and B show differences by school effectiveness. We employ the standardized average math and language test scores (SIMCE scores) obtained by tenth grade students in 2017 and 2018. We merge 2017 scores to the 2018 application round and 2018 scores to the next rounds. In the Chilean educational system, SIMCE average scores are a largely used metric to rank schools. Moreover, the application platform includes this metric among the characteristics parents can observe. In both panels, we find that spillover effects are larger for schools below the median. Columns (3) and (4) show that for schools above the median the estimates effects are substantially lower and not statistically significant at the 10% level. Since aggregate scores at the school level are a combination of true school effects and students ability, they are likely a biased proxy of school effectiveness. To investigate this potential concern, we construct school value-added on high school graduation and college enrollment using information from the 2016 and 2018 cohorts of ninth graders. Appendix A.7 discusses how we estimate school value-added on high school graduation and college enrollment. Table A8 in the Appendix shows that employing these alternative measures reveals the same pattern observed for average test scores.

Panels C, D, and E show our estimates for peer characteristics. We employ the composition of the ninth-grade cohort in the application year to construct the proportion of students in the top-quartile test scores distribution, the proportion of students whose families report college expectations, and the proportion of students with college-educated mothers for each school. Panel C shows spillover effects by the share of college-educated mothers in each school. We find that spillover effects are stronger on schools with a lower fraction of college-educated mothers. The size of the spillover effects is very similar when we consider the fraction of students in the top quartile of average test scores or the share of families with college expectations in panels D and E, respectively.

Finally, we also consider an additional set of measures related to personal and social development. Unfortunately, we do not observe neighbors' personal experiences in ninth grade. As suggested by the literature examining siblings spillover effects (Dustan, 2018; Altmejd et al., 2021; Aguirre

[18] We use information from public and private schools to compute the median value in each category.

and Matta, 2021), neighbors' experiences (e.g., bullying, unsatisfactory parent-teacher relationships, episodes of school violence) could be transmitted to future applicants. As an alternative, we employ an index of school motivation reported by the ministry of Education. This index is constructed using parental surveys for tenth grade students in 2017 and 2018, capturing attitudes and perceptions about non-academic dimensions of schools.[19] We merge information from the latest available survey to each application round. Panel F in Table 11 shows that spillover effects concentrate on school with worse indexes of school motivation. Similarly to each of the previous indicators, spillover effects concentrate on schools ranked below the median value.

Overall, we find that spillover effects concentrate on schools with lower average tenth-grade scores, more disadvantaged peers, and worse indicators of personal development. Considering our previous results, this implies that students from lower socioeconomic status, by following neighbors, are more likely to enroll in schools with inferior characteristics. This pattern differs partially from siblings' effects in college enrollment. Altmejd et al. (2021) find that younger siblings are more likely to follow older siblings independently of college quality, measured by expected earnings, peer quality or retention rates. In our context, these results show how spillover effects might exacerbate patterns of unequal access to high-quality schools.

5 Exploring Mechanisms

In this section, we investigate the mechanisms behind the spillover effects we document in the previous sections. 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.[20] Furthermore, we do not observe school preferences before exposure to neighbors' influence, so we are not able to separately identify effects on the consideration of alternative options and changes in preferences. One analogy to our setting 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 estimates could be capturing more than one causal channel.

In the rest of the section, we investigate the plausibility of three explanations. First, we investigate whether neighbors provide signals about unobserved school attributes, which are internalized differently depending on applicants' relative academic performance. Second, we explore whether mimicking neighbors' choices reduces decision-making costs. Since each family can apply to any

[19] Among other topics, parents are asked multiple questions about relationships between school members, episodes of discrimination, conflict or violence incidents, and school responses to situations of conflict.

[20] 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 directly 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. (2021). We leave this task for future research.

school with vacancies, searching information for multiple schools, evaluating their attributes, and ranking these options can be a complex process for families without the adequate resources to navigate the admission process. Finally, we assess whether following neighbors improves school-student matches by analyzing short-term impacts on academic outcomes.

5.1 Ability Differences

We start by exploring the possibility that neighbors convey information that is internalized by applicants depending on academic performance differences. Under this hypothesis, applicants with lower relative academic performance will be more likely to mimic previous choices because they are more likely to think this school would be a good fit for them as well. 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 school quality (or other attribute) is low. To analyze the plausibility of this hypothesis, for each applicant-neighbor pair we compute the difference between each applicant's and neighbor's baseline standardized test scores. Then, we classify this variable in quintiles Q_k and estimate the following specification using 2SLS:

$$y_{ijs} = \alpha + \beta x_{js} + \sum_{k=1}^{5} \gamma_k (x_{js} \times Q_k) + \sum_{k=1}^{5} \delta_k Q_k + \phi_{jl} + \epsilon_{ijs} \tag{5}$$

$$x_{js} = \kappa + \rho z_{js} + \sum_{k=1}^{5} \lambda_k (z_{js} \times Q_k) + \sum_{k=1}^{5} \tau_k Q_k + \varphi_{jl} + \eta_{ijs} \tag{6}$$

Our estimates of interest are γ_k, which represent how the average effect of neighbor j enrolling in school s varies across different quintiles of Q_k. The definition of Q_k implies that the top quintile includes applicants with relatively higher standardized scores. Figure 11 shows our estimates when we consider differences in math test scores. For each plot, the x-axis shows the range of test score differences in each quintile while the y-axis shows the estimate and the 95% confidence intervals of γ_k. The superior and inferior panels show our estimates of γ_k when we consider differences in math and language test scores, respectively. We find a monotonic pattern as the test score difference between applicant and neighbor shifts from negative to positive. The left plot of Figure 11 shows that when the difference between applicant and neighbor is in the first quintile (lower than -1.1σ) the likelihood of mimicking previous choices increases by around 7 percentage points. As this difference becomes positive, the influence of neighbors decreases. We observe that, for score differences in the third quintile, that is for differences between -0.3σ and 0.4σ the estimate is closer to zero. For applicant-neighbor pairs observed in the fourth quintile, that is, when the applicant's score surpasses the closest neighbor's, the likelihood of considering the same school in the next round becomes negative. In this case, observing the closest neighbor receiving an offer in their most preferred school decreases the likelihood of including this choice in the list of rankings by around 3 p.p. Finally, applicants who score more than 1.1σ than the closest neighbor are 6 p.p. less likely to consider their closest neighbor's most preferred school.

We observe the same pattern for first-rank applications and school attendance. The center plot of Figure 11 shows that spillover effects range between 3 p.p. and -4 p.p. As discussed in section 4, applicants are most likely to enroll in the schools where they received an offer. Therefore, it is not surprising that the patterns between first-rank applications and enrollment are similar in magnitude. Although we observe a smaller gradient for enrollment, the overall pattern shows significant differences across quintiles. Figure 12 shows a similar analysis using differences in language test scores. The patterns are magnitudes of the estimates are almost identical. Figure 12 displays similar patterns when we consider language test scores.

We examine whether these differences translate into changes in the type of schools applicants choose. If high-achieving neighbors convey information about better schools, we should expect that applicants in the next round are more likely to apply to schools with better characteristics. We test this implication by looking at differences in some of the school characteristics presented in Table 11. Specifically, we estimate the following set of regressions using 2SLS:

$$w_i = \alpha + \beta x_{js} + \sum_{k=1}^{5} \gamma_k (x_{js} \times Q_k) + \sum_{k=1}^{5} \delta_k Q_k + \phi_{jl} + \epsilon_{ijs} \tag{7}$$

$$x_{js} = \kappa + \rho z_{js} + \sum_{k=1}^{5} \lambda_k (z_{js} \times Q_k) + \sum_{k=1}^{5} \tau_k Q_k + \varphi_{jl} + \eta_{ijs} \tag{8}$$

Where w_i corresponds to an attribute of i's most preferred school. Figure 13 shows our results when quintiles Q_k is defined by differences in math scores. Each plot shows the change in the value of the corresponding characteristic for each quintile. We find statistically significant changes mostly for applicant-neighbor pairs located in the two first quintiles. This implies that these applicants are following neighbors to better schools, measured by average 10th grade test scores or peer composition. For example, the estimates show that applicants in the first quintile rank schools with 0.04σ in the average test score school distribution as their first option. In terms of college attendance, we find a similar effect for the first three quintiles. The estimate in this case is 0.003, representing an increase of 0.04σ (=0.003/0.08) in the value-added distribution. Similar results follow when we consider the proportion of students with higher achievement. The estimate of 0.01 implies an increase of 4% (=0.01/0.25) in the proportion of students located in the top-quartile of the average test scores distribution. Figure A2 in the Appendix shows similar patterns when we consider a measure of high-school graduation value-added and additional peer characteristics. When neighbors' scores are lower, we do not find changes in the types of schools applicants choose. For the top two quintiles our estimates are closer to zero and not statistically significant at the 5% level. We interpret these results as evidence supporting that high-achieving neighbors make schools more salient to applicants, particularly those with lower achievement. However, applicants with relatively higher scores do not incorporate neighbors' actions in their school choice set. This does not affect the types of schools they apply to. The null effect of the closest neighbor attending a given school on applicants' choices for the two top quintiles shows that neighbors' actions are irrelevant in this case.

5.2 School Matches

One alternative hypothesis to explain the spillover effects is that following a neighbor induces better school matches. Given the heterogeneity across observable characteristics of applicant-neighbor pairs, it could be possible that these social interaction effects convey information about better learning environments. Unfortunately, a complete analysis about impacts on student outcomes is not possible. The onset of the Covid-19 pandemic in 2020 poses a challenge in interpreting any estimate on academic outcomes observed in 2020 and 2021.[21] Additionally, we do not have data about students' perceptions about schools or learning environments. These restrictions preclude us to conduct a more thorough analysis about the relevance of better school matches as a mechanism to explain our findings.

As a feasible alternative, we restrict our analysis to the subset of students who participated in the application process in 2018 and were observed in ninth grade in 2019. We consider this exercise only as suggestive evidence since this subset of students represents only 5% of our final sample. Table A6 in the Appendix shows our estimates of impacts on ninth-grade GPA, proportion of days attended, and grade progression. As controls for predetermined characteristics, we use previous test scores, gender, and priority status. Although all estimates have the expected sign, their magnitudes are small and not statistically significant. Column (1) shows an increase of 0.035 points in 9th grade GPA in a 1-7 scale. Column (2) shows an increase of 0.24 percentage points in attendance while column (3) shows that the probability of promotion to tenth grade increases by 1.2 percentage points. Column (4) includes a different proxy to test the relevance of school matches. We use an indicator equals to one if the applicant is observed participating in another school assignment process after 2019. Since families are not restricted to participate in a new process if the school does not meet their expectations, if following neighbors leads to better matches we would expect a decrease in the likelihood of observing the student in a different process. However, the estimate is zero and not statistically significant at the 10% level.

On balance, these results show only modest improvements in academic outcomes. Nevertheless, this analysis is restricted both in terms of the outcomes we can observe and the sample size. Thus, we cannot rule out the importance of better school matches as a potential driver of our main results.

[21] In Chile, the Ministry of Education mandated schools to close in March 2020 and started a reopening process during August 2021 (start of the spring semester). Nevertheless, attendance was not mandatory until March 2022.

5.3 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 determine which schools to consider.[22] In our context, additional factors, such as the complexity of the new system, or residential segregation could be additional elements that incentivize the use of informal networks.[23] Unfortunately, we do not observe a direct measure of search effort (such as time invested in gathering information about school characteristics) to test directly how they relate to neighbors' decisions. However, we can consider the number of schools included in each application as a proxy of search intensity. If neighbors' influence reduces search effort, we should expect a reduction in the number of schools submitted.

Table 12 shows estimates of the effect of neighbors' enrollment on the number of schools submitted by applicants in the next period. Overall, we do not find evidence supporting a decrease in search intensity. Column (1) shows that there are no changes in the probability of applicants submitting only one school in their applications. We test differences across families by including an interaction term with two proxies of socioeconomic disadvantage. Panel A shows estimates when we include an indicator equals to one if the mother's education is high school or lower, while Panel B shows estimates when we use an indicator of family income lower than US$750. In both cases, we do not find differences across groups. Columns (2) and (3) show estimates on the probability of applicants ranking at least 2 or at least 3 schools, respectively. We find evidence that neighbors' enrollment increases the likelihood of submitting at least three schools for families with higher income. The size and negative sign of the interaction term shows that this effect disappears for disadvantaged families, implying that search behavior is not affected by neighbors' enrollment. Finally, column (4) shows no effect on the likelihood of submitting at least four schools. Although we are not able to isolate the exact contribution of this mechanism, we interpret these findings as evidence against the hypothesis that spillovers are driven by reductions in search costs.

6 Conclusion

In this paper, we investigate the influence of close neighbors on school application and enrollment decisions. To overcome the empirical challenges associated to this question, we employ data from the Chilean centralized school admission system, which started its implementation in 2017. The large

[22] 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 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.

[23] Consistent with this hypothesis, Honey and Carrasco (2022) report small changes in the proportion of priority students across schools before and after the implementation of the reform. They argue that other structural characteristics of the educational system (e.g., residential segregation and the unequal distribution of school quality across neighborhoods) are responsible of the small variation in enrollment distribution patterns.

proportion of oversubscribed schools in ninth grade and the use of tie-breaking rules to determine assignment in these cases allow us to identify causal effects. We are not aware of previous work studying this type of spillover effects in centralized school assignment systems.

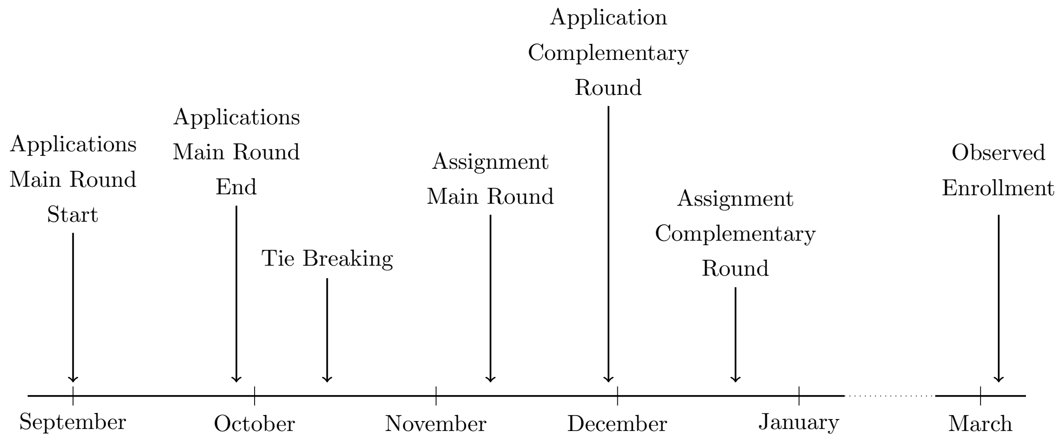
Our results show meaningful spillover effects on school applications and enrollment. On average, having a close neighbor assigned to their most preferred school in the previous round increases the likelihood of an applicant ranking that school in the first preference by 0.8 percentage points and attending this school in 9th grade by 1.2 percentage points. These estimates represent an increase of 7% and 10% relative to the average levels. Our heterogeneity analysis shows that these effects are larger for boys and applicants from disadvantaged families, measured by an index of socioeconomic status, parental college expectations, and family income.

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. The opposite pattern is observed when applicants' test scores surpass neighbors'. Our finding of larger effects on lower-SES families suggests that although information is seemingly available to all families, it is not incorporated into the decisions made by more disadvantaged families. This conclusion has been found in other settings (Hastings and Weinstein, 2008; Dizon-Ross, 2019) and suggests that targeted interventions could be useful to reduce frictions leading to a potentially inefficient allocation of educational investments.

Unfortunately, we are not able to incorporate measures of academic performance, such as tenth grade test scores, high school graduation, or college enrollment into our analyses. One reason is that the earliest cohorts assigned under the new admission system are still enrolled in high school. Another reason is that the administration of standardized test scores was canceled in Chile in 2020 and 2021 due to the Covid-19 pandemic. Studying spillover effects on academic and non-academic outcomes and assessing whether they are mediated by better school matches is an important topic we plan to address in future research.

7 Figures and Tables

Figure 1: Timeline of the Application Process



Source: Correa et al. (2022)

Figure 2: 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 3: Implementation of the Centralized School Choice System

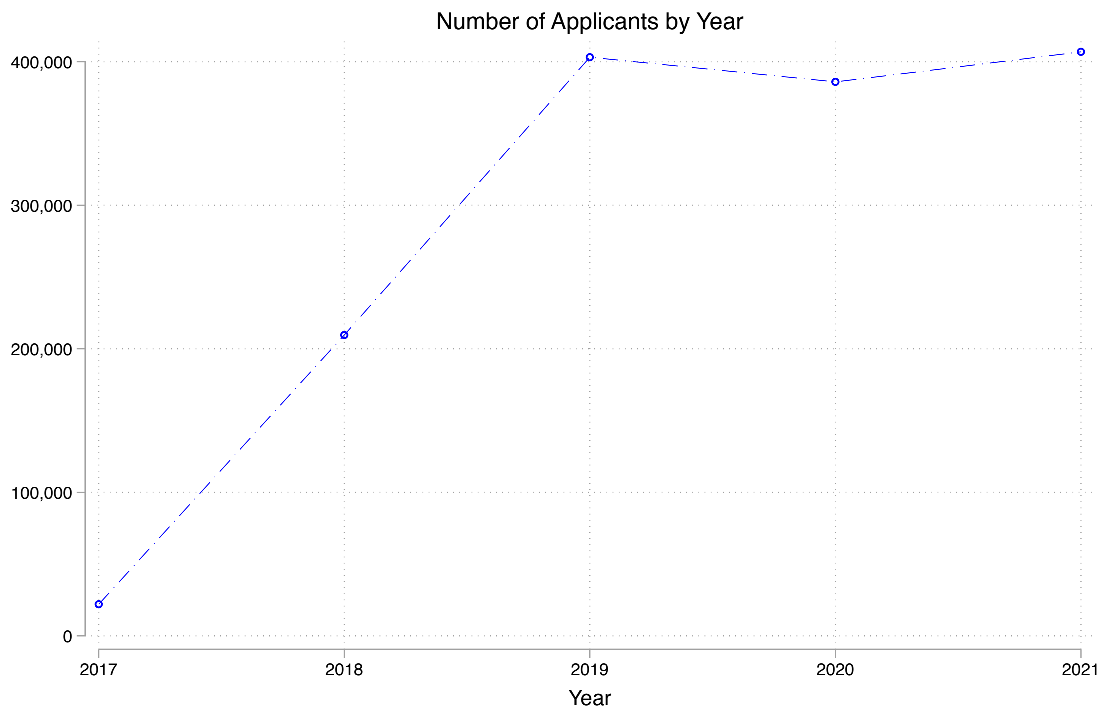


Figure 4: Distribution of Applicants by Grade

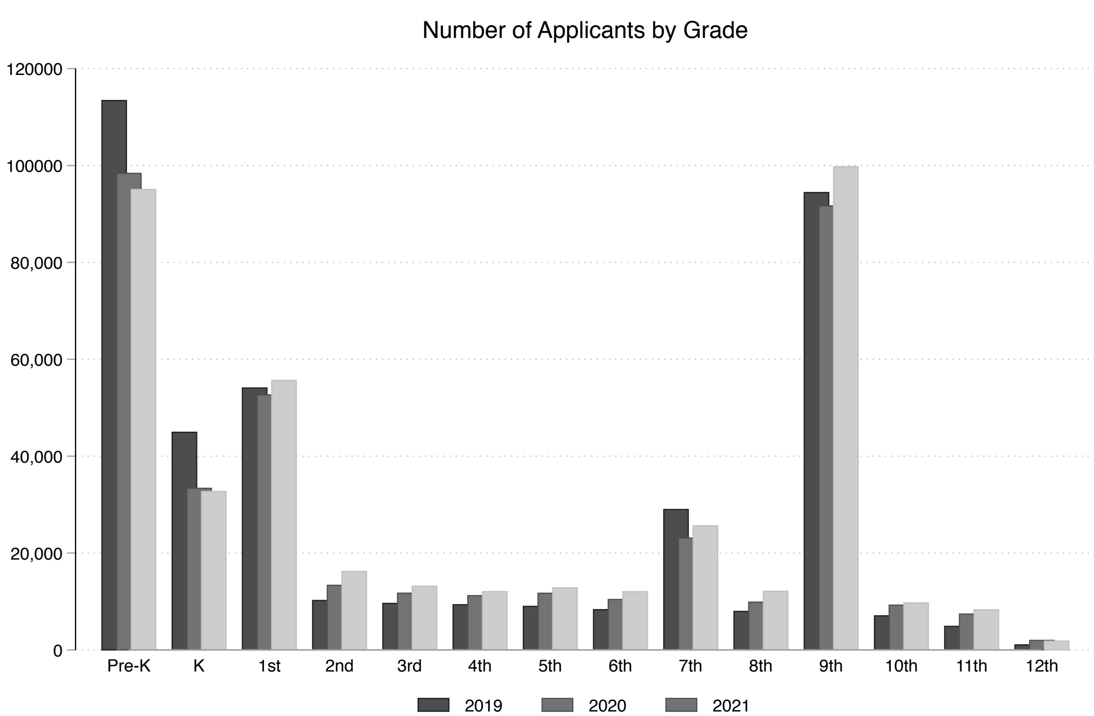
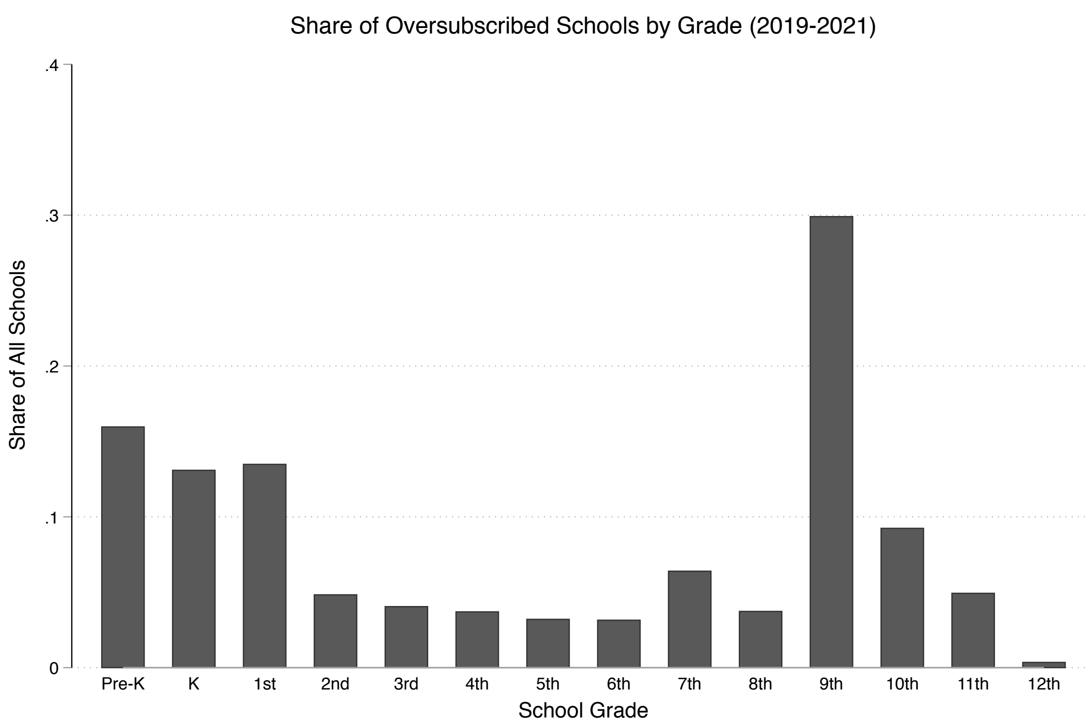
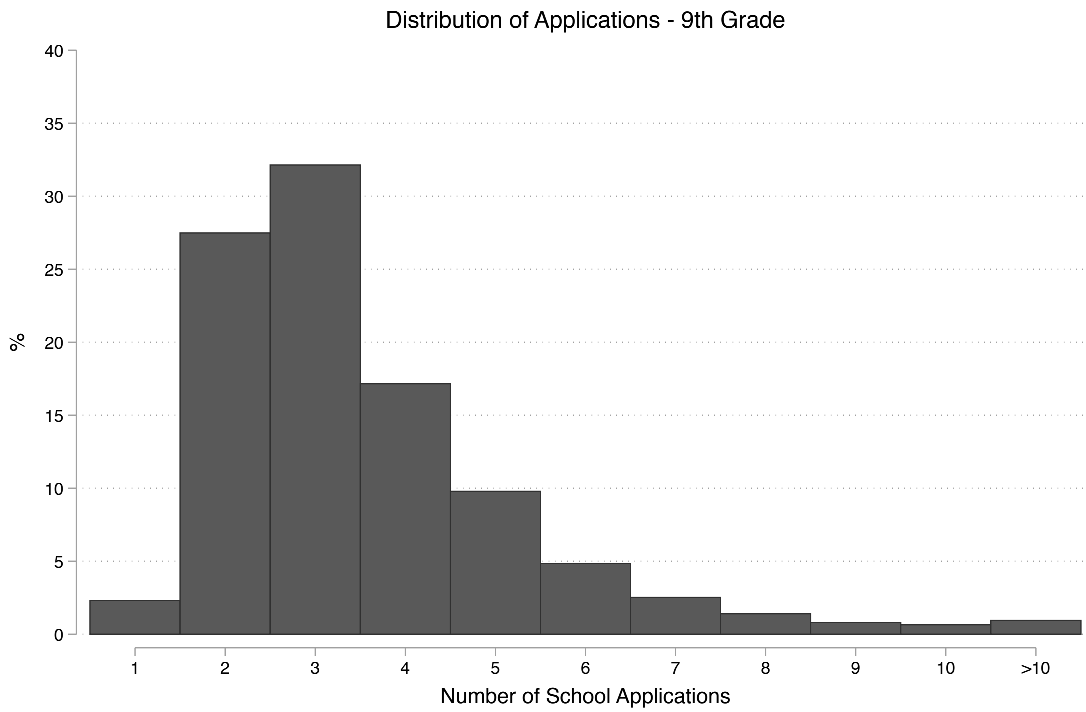


Figure 5: Oversubscribed Schools



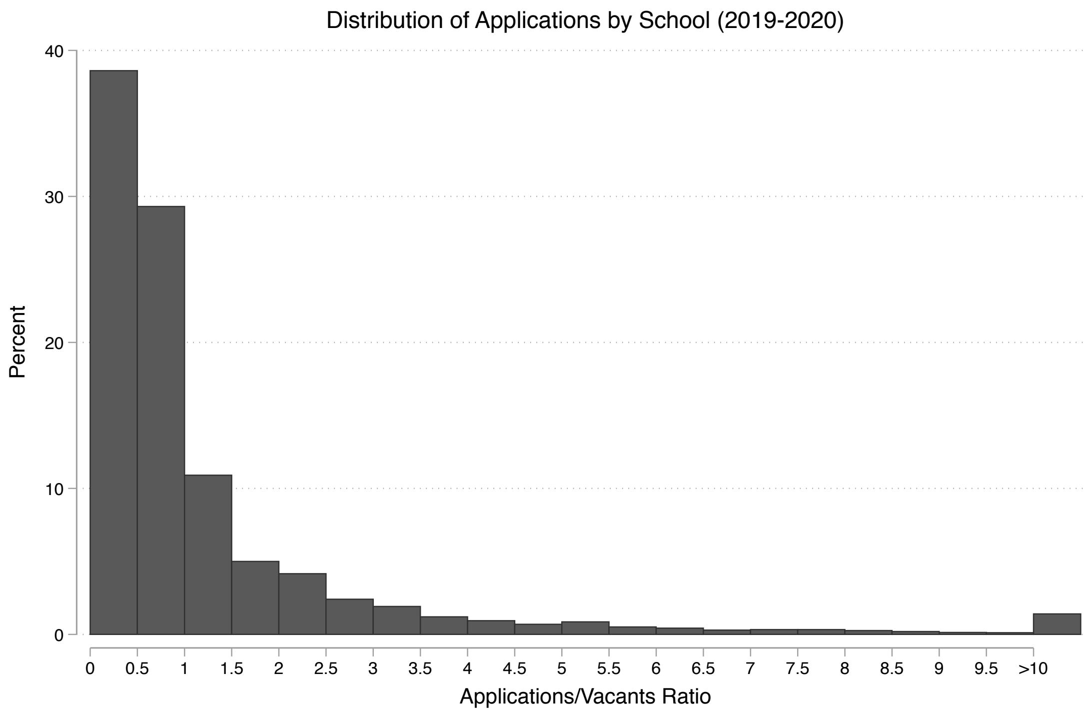
Notes: This plot shows the share of schools where the number of applicants submitting the school as first option surpasses the number of vacant seats in the corresponding grade. The share is computed pooling the 2019 and 2020 application rounds.

Figure 6: Distribution of School Applications - 9th Grade



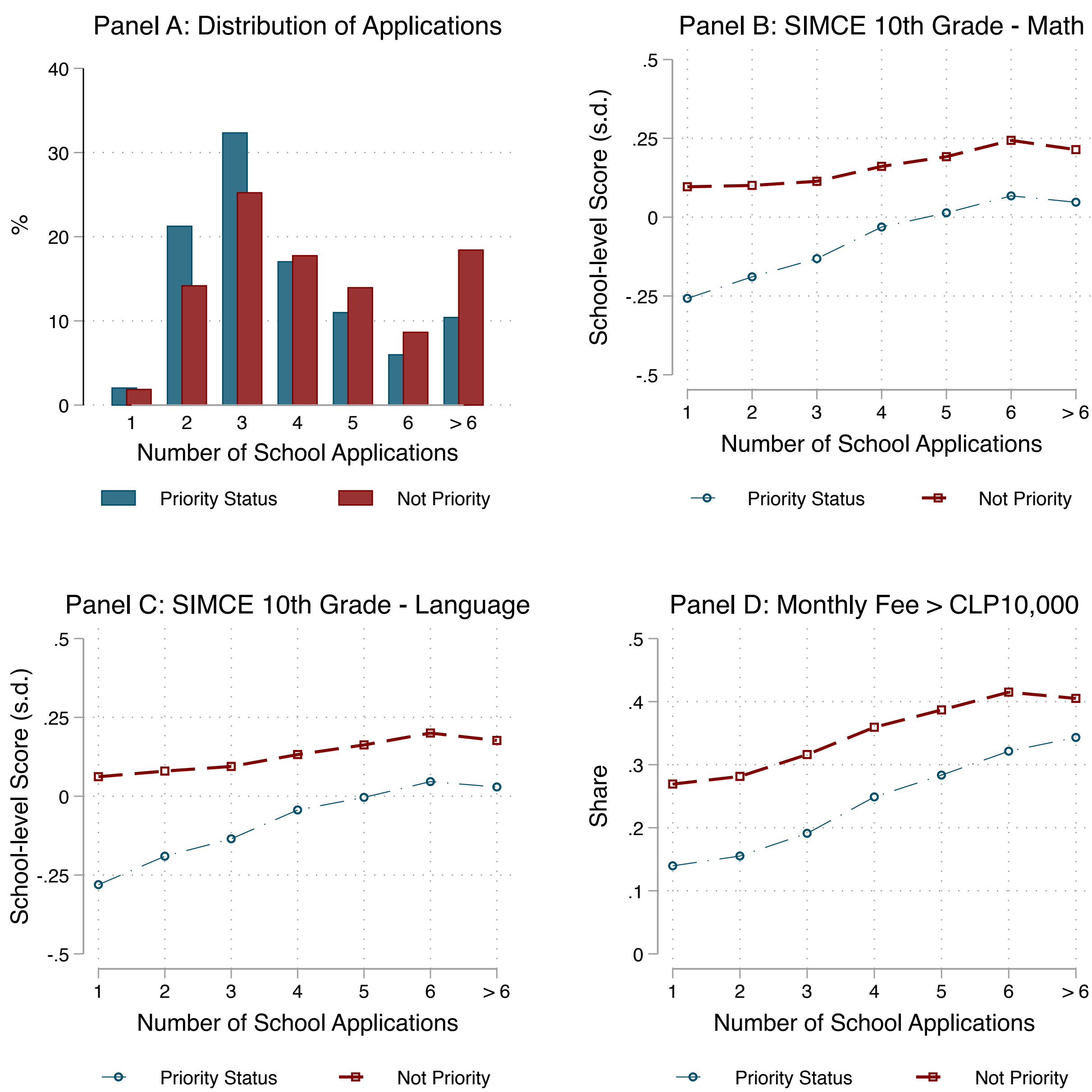
Notes: This plot shows the distribution of the number of schools 9th grade applicants submit. This plot pools the 2019 and 2020 application rounds.

Figure 7: Applicants/Seats Ratio Across Schools



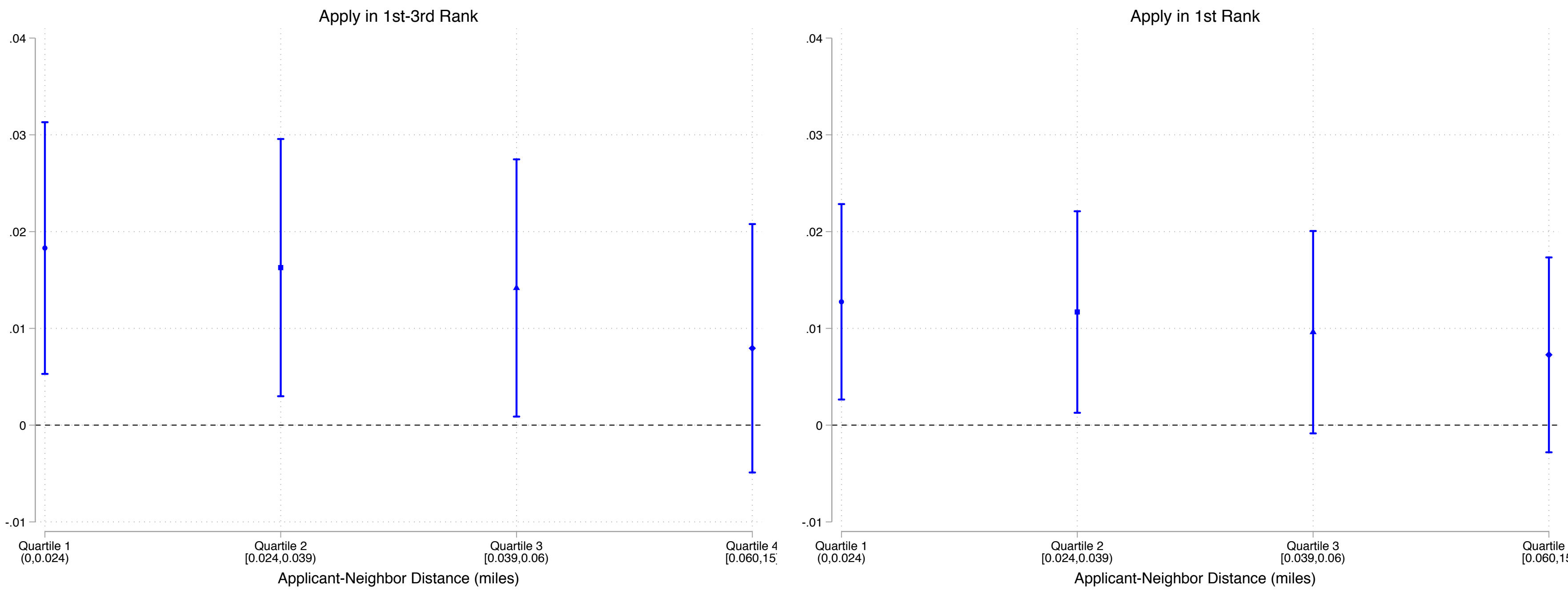
Notes: This plot shows the distribution of the applicants/seats ratio for schools offering 9th grade, restricted to schools offering at least five vacant seats. The number of applicants considers only first-rank preferences.

Figure 8: Differences in Applications and High School Characteristics by Student Priority Status



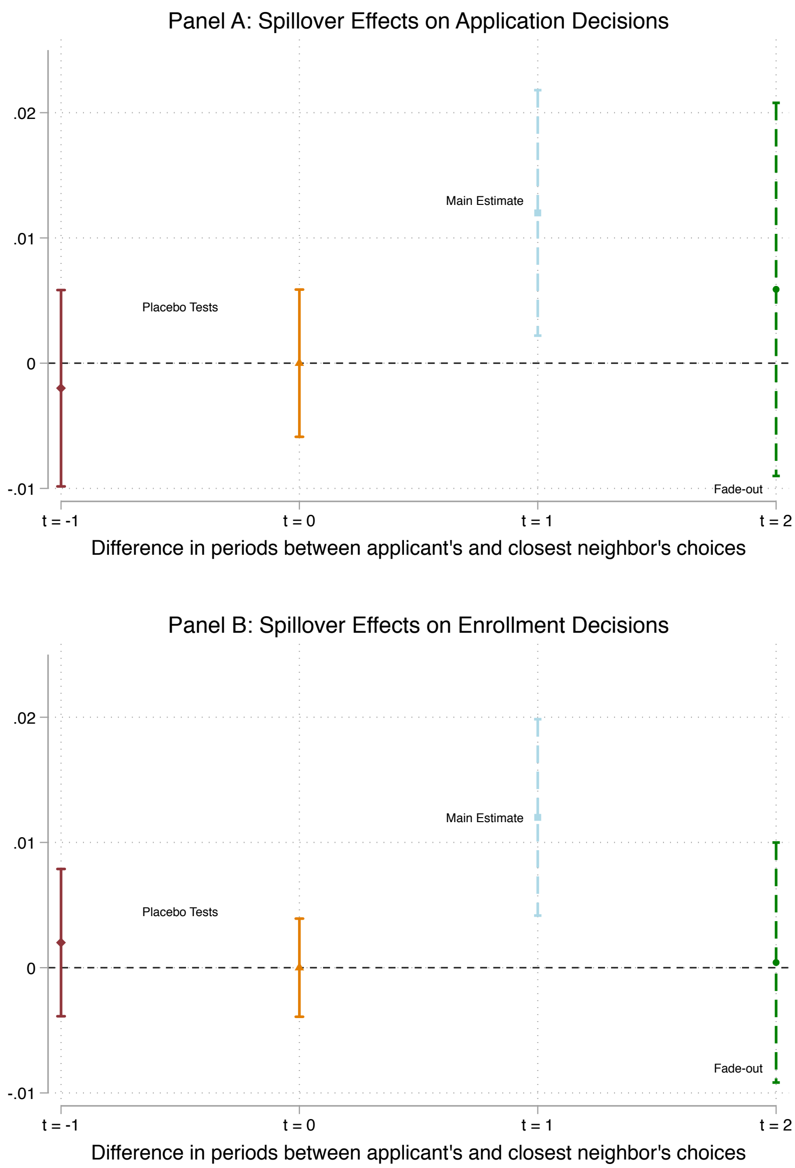
Notes:

Figure 9: Spillover Effects on Application Decisions by Distance



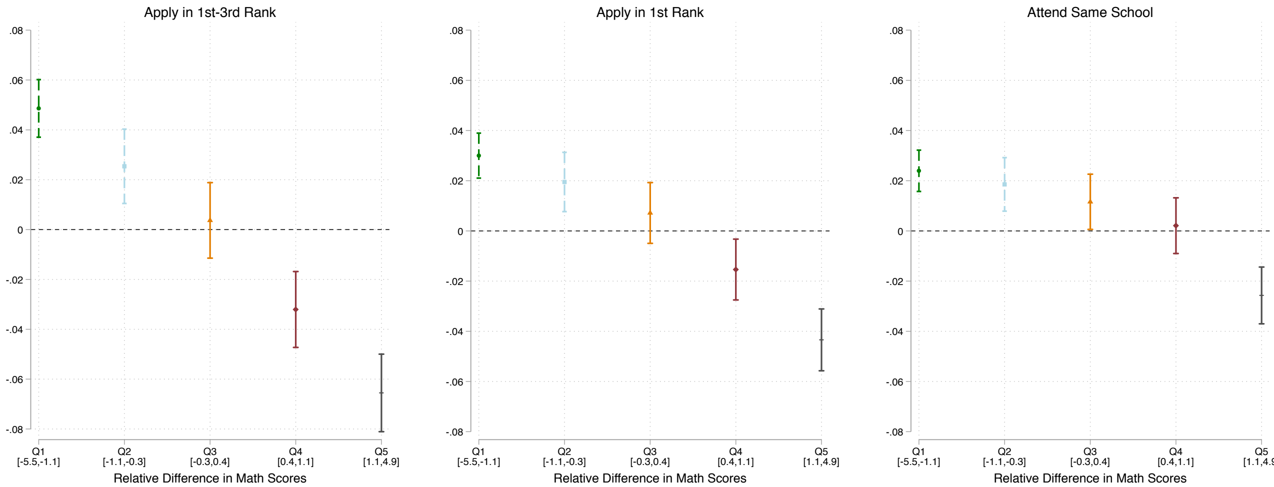
Notes: This figure shows how spillover effects vary with the distance between each applicant and the closest neighbor. We classify the euclidean distance in four groups and estimate (3) and (4) using the interaction between neighbor's enrollment and an indicator for each distance group. Neighbor's enrollment is instrumented with an indicator equals to one if the neighbor got an offer in their most preferred school. Our specification includes lottery fixed effects and standard errors are clustered at the neighbor level.

Figure 10: Impacts of Neighbors From Different Time Horizons: Separate Regressions



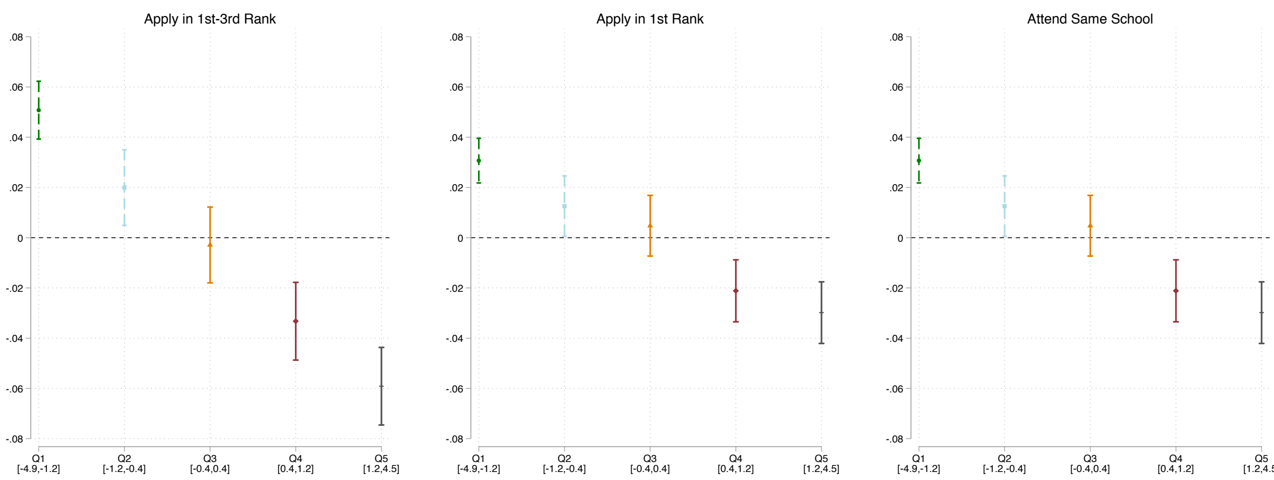
Notes: This figure shows how spillover effects vary based on the number of periods used to link each applicant with their closest neighbor. Each plot reports estimates from separate 2SLS regressions as described in equations (1) and (2). Panel A uses as outcome an indicator equals one if the applicant ranks the same school attended by the closest neighbor in first-third preference, while panel B uses as outcome an indicator equals one if the applicant attends the same school. Neighbor's enrollment is instrumented with an indicator equals 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 11: Heterogeneity by Score Differences in Math



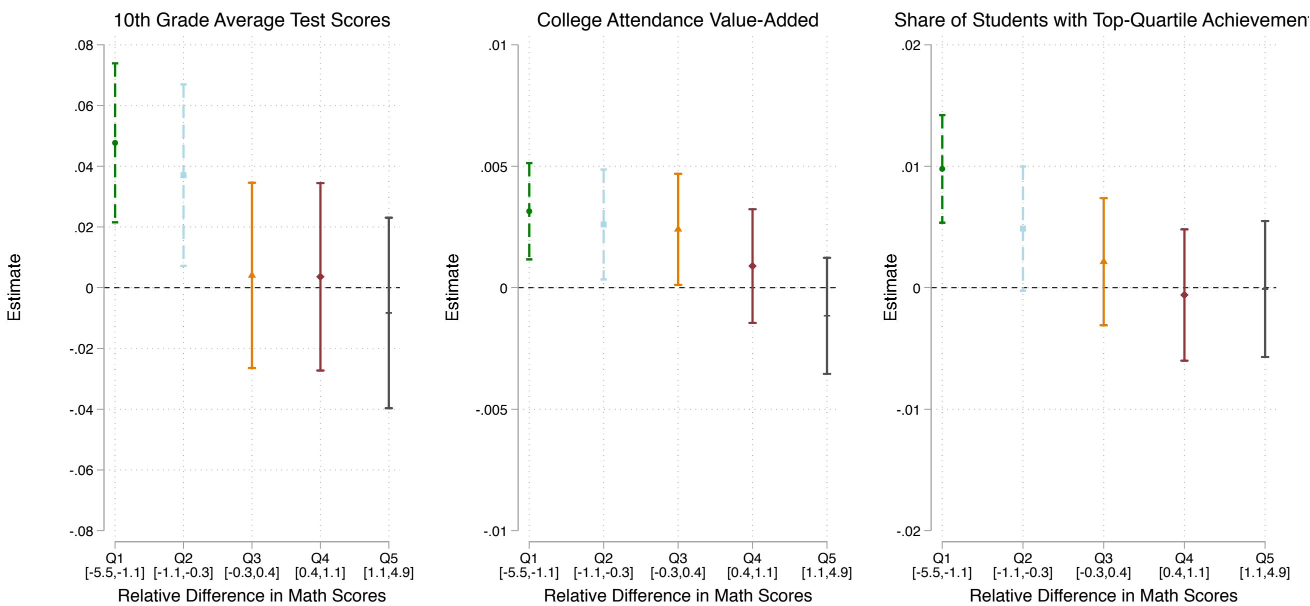
Notes: Each plot presents 2SLS estimates of the effect of the closest neighbor attending a given school on applicants decisions and enrollment, separately by the quintile of the difference in previous math test scores between applicant and neighbor. Enrollment is instrumented with an indicator equals to one if the neighbor got an offer in their most preferred school in the previous round. All models include lottery fixed effects (see equations (5) and (6)). Standard errors are clustered at the neighbor level.

Figure 12: Heterogeneity by Score Differences in Language



Notes: Each plot presents 2SLS estimates of the effect of the closest neighbor attending a given school on applicants decisions and enrollment, separately by the quintile of the difference in previous language test scores between applicant and neighbor. Enrollment is instrumented with an indicator equals to one if the neighbor got an offer in their most preferred school in the previous round. All models include lottery fixed effects (see equations (5) and (6)). Standard errors are clustered at the neighbor level.

Figure 13: Effect of Neighbors' Enrollment on Schools Chosen by Applicants



Notes: Each plot presents 2SLS estimates of the effect of the closest neighbor attending a given school on applicants' most preferred schools' characteristics. We divide applicant-neighbor pairs in quintiles by differences in previous math test scores. Enrollment is instrumented with an indicator equals to one if the neighbor got an offer in their most preferred school in the previous round. All models include lottery fixed effects (see equations (7) and (8)). Standard errors are clustered at the neighbor level.

Table 1: Summary of Acceptances by School Grade

	2019		2020		2021	
	Accepted in any of 1st-3rd options	Accepted in 1st option	Accepted in any of 1st-3rd options	Accepted in 1st option	Accepted in any of 1st-3rd options	Accepted in 1st option
School Level						
Pre-K and K	85%	59%	91%	68%	92%	70%
Elementary	76%	38%	78%	39%	79%	40%
Middle School	81%	42%	83%	44%	81%	42%
High School	87%	60%	87%	59%	86%	57%

Notes:

Table 2: Summary Statistics

	(1) All Applicants	(2) Estimation Sample
Panel A: Background Characteristics		
Girl	0.481	0.480
Priority status	0.627	0.614
High-achiever	0.316	0.309
Baseline math scores (s.d.)	-0.227	-0.221
Baseline language scores (s.d.)	-0.148	-0.143
Baseline science scores (s.d.)	-0.192	-0.192
Father has college degree	0.175	0.177
Mother has college degree	0.200	0.199
Family income > US$1,000	0.099	0.102
Distance to neighbor (miles)	0.247	0.233
Panel B: Application Characteristics		
Number of applications	3.448	3.589
Submits one school	0.026	0.013
Submits two schools	0.286	0.256
Submits three schools	0.325	0.329
Submits four or more schools	0.364	0.402
Observations	258124	109069

Notes: This table displays average characteristics for 9th grade applicants between 2018 and 2021. Panel A shows results for applicant demographics and Panel B shows results for application characteristics. Column (1) shows average values for all applicants who have non-imputed geographic location and are linked to a neighbor who applied to 9th grade in the previous year. Column (2) shows average values after restricting the sample to applicants whose closest neighbor's top-choice was an oversubscribed school (seat offer was determined by tie-breaking rules).

Table 3: Balance Tests

Variable	Average		Difference	p-value	Observations	
	Offered	Not Offered			Offered	Not Offered
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Applicant Covariates						
Girl	0.48	0.48	-0.001	0.749	45952	56320
Priority status	0.61	0.62	-0.005	0.178	45952	56320
High-achiever	0.31	0.31	-0.004	0.224	45952	56320
Test scores (Math)	-0.27	-0.24	-0.026^{***}	0.006	24442	31141
Test scores (Language)	-0.15	-0.14	-0.009	0.345	24353	31050
Test scores (Science)	-0.20	-0.18	-0.020	0.143	12343	16122
Father's education: College	0.17	0.17	-0.001	0.753	23455	29651
Father's education: Less than HS	0.77	0.77	-0.001	0.845	23455	29651
Mother's education: College	0.19	0.19	-0.003	0.548	23614	29812
Mother's education: Less than HS	0.79	0.78	0.004	0.374	23614	29812
College expectations	0.67	0.66	0.004	0.474	23438	29623
Family income > US$1,000	0.10	0.10	-0.002	0.623	23642	29877
Joint orthogonality F-test				0.666		
Panel B: Neighbor Covariates						
Girl	0.49	0.49	-0.001	0.876	23440	29270
Priority status	0.57	0.57	0.001	0.842	23440	29270
High-achiever	0.27	0.27	-0.003	0.548	23440	29270
Test scores (Math)	-0.22	-0.22	-0.003	0.809	12385	16411
Test scores (Language)	-0.12	-0.14	0.018	0.160	12324	16292
Test scores (Science)	-0.17	-0.18	0.008	0.563	11553	15170
Father's education: College	0.16	0.17	-0.004	0.437	11538	15166
Father's education: Less than HS	0.77	0.76	0.009	0.138	11538	15166
Mother's education: College	0.19	0.19	-0.002	0.731	11622	15264
Mother's education: Less than HS	0.78	0.78	0.003	0.624	11622	15264
College expectations	0.68	0.67	0.001	0.890	11550	15172
Family income > US$1,000	0.09	0.09	-0.002	0.711	11597	15212
Joint orthogonality F-test				0.750		
Distance between neighbors	0.21	0.21	0.001	0.947	45952	56320

Notes: 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 the estimates using applicants characteristics, while panel B shows the results for neighbors characteristics. Joint orthogonality shows the p-value of a F-test of joint significance of all covariates listed in the corresponding panel.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 4: ITT and 2SLS Estimates of Neighbors Effects on Applicants Decisions

	Applicant Outcome in Round t						
	ITT			First Stage	2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Neighbor Outcome in Round $t-1$:	Applies Same (1st-3rd Rank)	Applies Same (1st Rank)	Attends Same	Enrolled	Applies Same (1st-3rd Rank)	Applies Same (1st Rank)	Attends Same
Seat Offered	0.008**	0.007**	0.008***	0.678***			
	(0.004)	(0.003)	(0.003)	(0.004)			
Enrolled					0.012**	0.010**	0.012***
					(0.005)	(0.004)	(0.004)
Average Outcome	0.32	0.15	0.12	0.50	0.32	0.15	0.12
F-Statistic					19428	19428	19247
Lottery FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	102028	102028	100018	52696	102028	102028	100018
R^2	0.21	0.14	0.15	0.60			

Notes: Clustered standard errors at the neighbor level in parenthesis. Seat offered and Enrolled are indicators equal to one if the closest neighbor received an offer and enrolled at her most preferred school, respectively. Enrollment corresponds to the actual school where applicant and neighbor are observed in 9th grade in the next year.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 5: OLS Estimates on School Applications

	(1) Applies Same (1st-3rd Rank)	(2) Applies Same (1st Rank)	(3) Attends Same
Neighbor Enrolled	0.045***	0.039***	0.068***
	(0.004)	(0.003)	(0.002)
Average Outcome	0.32	0.15	0.12
Lottery FE	No	No	No
Controls	No	No	No
N	108750	108750	106606
R2	0.01	0.00	0.01

Notes: Clustered standard errors at the neighbor level
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 6: Placebo Test of Neighbors Effects: Next Year

	Applicant Outcome in Round t		
Neighbor Outcome in round $t+1$	(1) Applies Same (1st-3rd Rank)	(2) Applies Same (1st Rank)	(3) Attends Same
Seat Offered	-0.002	0.004	0.002
	(0.004)	(0.003)	(0.003)
Average Outcome	0.34	0.16	0.13
Lottery FE	Yes	Yes	Yes
N	54857	54857	54857
R^2	0.28	0.33	0.21

Notes: Clustered standard errors at the neighbor level in parenthesis. Each column shows the estimate of 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 her most preferred school.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 7: Placebo Test of Neighbors Effects: Same Year

	Applicant Outcome in Round t		
Neighbor Outcome in round t	(1) Applies Same (1st-3rd Rank)	(2) Applies Same (1st Rank)	(3) Attends Same
Seat Offered	-0.000	-0.003	0.000
	(0.003)	(0.002)	(0.002)
Average Outcome	0.34	0.17	0.13
Lottery FE	Yes	Yes	Yes
Controls	No	No	No
N	94181	94181	94181
R2	0.28	0.33	0.22

Notes: Clustered standard errors at the neighbor level in parenthesis. Each column shows the estimate of 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 her most preferred school.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 8: Estimates of Neighbors' Effects - Heterogeneity by Applicant Characteristics

	Applicant Outcome in Round t							
Neighbor Outcome in Round $t-1$	(1) Applies Same (1st Rank)	(2) Attends Same	(3) Applies Same (1st Rank)	(4) Attends Same	(5) Applies Same (1st Rank)	(6) Attends Same	(7) Applies Same (1st Rank)	(8) Attends Same
Enrolled	0.016***	0.015***	0.019***	0.015***	0.041***	0.027***	0.024***	0.021***
	(0.005)	(0.005)	(0.005)	(0.004)	(0.007)	(0.006)	(0.006)	(0.006)
Enrolled ×	-0.013**	-0.005						
$\mathbb{1}$(Applicant=Girl)	(0.006)	(0.006)						
Enrolled ×			-0.023***	-0.007				
$\mathbb{1}$(Applicant=Priority)			(0.006)	(0.006)				
Enrolled ×					-0.049***	-0.023***		
$\mathbb{1}$(College expectations)					(0.008)	(0.007)		
Enrolled ×							-0.029***	-0.015***
$\mathbb{1}$(Family Income > CLP 300k)							(0.007)	(0.007)
Average Outcome	0.15	0.12	0.15	0.12	0.15	0.12	0.15	0.12
F-Statistic	9712	9621	9656	9567	8400	8325	8453	8381
Lottery FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	102028	100018	102028	100018	77930	76989	78661	77710

Notes: This table reports 2SLS estimates from equations (3) and (4) of the effects of each applicant's closest neighbor attending her most preferred school. Each column reports the main estimate and an interaction between the main effect and an indicator variable of applicant characteristics. 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.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 9: Estimates of Neighbors Effects - Heterogeneity by Applicant-Neighbor Matching

	Applicant Outcome in Round t					
	Lagged Math Scores			Lagged Language Scores		
Neighbor Outcome in Round $t-1$	(1) Applies Same (Any Rank)	(2) Applies Same (1st Rank)	(3) Attends Same	(4) Applies Same (Any Rank)	(5) Applies Same (1st Rank)	(6) Attends Same
Enrolled	0.043***	0.023***	0.016**	0.021**	0.022***	0.019***
	(0.010)	(0.008)	(0.007)	(0.010)	(0.008)	(0.007)
Enrolled $\times$	-0.014	-0.007	0.000	0.002	-0.012	-0.006
$\mathbb{1}$(Applicant$<$Median, Neighbor$>$Median)	(0.011)	(0.009)	(0.009)	(0.011)	(0.009)	(0.009)
Enrolled $\times$	-0.085***	-0.046***	-0.023***	-0.054***	-0.039***	-0.023***
$\mathbb{1}$(Applicant$>$Median, Neighbor$<$Median)	(0.010)	(0.009)	(0.008)	(0.010)	(0.009)	(0.008)
Enrolled $\times$	-0.046***	-0.026**	-0.011	-0.022*	-0.019*	-0.018*
$\mathbb{1}$(Applicant$>$Median, Neighbor$>$Median)	(0.013)	(0.011)	(0.010)	(0.013)	(0.011)	(0.010)
Average Outcome	0.32	0.15	0.12	0.32	0.15	0.12
F-Statistic	4030	4030	3999	3996	3996	3968
Lottery FE	Yes	Yes	Yes	Yes	Yes	Yes
N	69672	69672	68841	69041	69041	68224

Notes: Clustered standard errors at the neighbor level

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 10: Estimates of Neighbors Effects - Heterogeneity by Gender

	(1) Applies Same (1st-3rd Rank)	(2) Applies Same (1st Rank)	(3) Attends Same
Neighbor Enrolled	0.049***	0.035***	0.034***
	(0.008)	(0.005)	(0.006)
Neighbor Enrolled ×	-0.069***	-0.041***	-0.042***
$\mathbb{1}$(Applicant=Boy, Neighbor=Girl)	(0.010)	(0.008)	(0.007)
Neighbor Enrolled ×	-0.070***	-0.051***	-0.040***
$\mathbb{1}$(Applicant=Girl, Neighbor=Boy)	(0.009)	(0.007)	(0.007)
Neighbor Enrolled ×	-0.012	-0.011	-0.007
$\mathbb{1}$(Applicant=Girl, Neighbor=Girl)	(0.012)	(0.009)	(0.009)
Average Outcome	0.32	0.15	0.12
F-Statistic	4714	4714	4684
Lottery FE	Yes	Yes	Yes
N	102028	102028	100018

Notes: Clustered standard errors at the neighbor level
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 11: 2SLS Estimates of Neighbors Effects by School's Characteristics

	Below Median		Above Median	
	(1) Applies Same (1st-3rd Rank)	(2) Applies Same (1st Rank)	(3) Applies Same (1st-3rd Rank)	(4) Applies Same (1st Rank)
Panel A: By 10th Grade Average Language Scores				
Neighbor Enrolled	0.021***	0.014**	0.005	0.008
	(0.008)	(0.006)	(0.007)	(0.005)
F-Statistic	7459	7459	13090	13090
N	50109	50109	51377	51377
Panel B: By 10th Grade Average Math Scores				
Neighbor Enrolled	0.032***	0.019***	-0.000	0.003
	(0.009)	(0.007)	(0.007)	(0.005)
F-Statistic	5751	5751	15668	15668
N	43899	43899	57587	57587
Panel C: By Share of Students with College-Educated Mothers				
Neighbor Enrolled	0.014**	0.011**	0.008	0.008
	(0.007)	(0.005)	(0.008)	(0.006)
F-Statistic	11057	11057	9336	9336
N	65152	65152	36855	36855
Panel D: By Share of Top-Quartile Students' Average Test Scores				
Neighbor Enrolled	0.020**	0.016**	0.005	0.004
	(0.008)	(0.006)	(0.007)	(0.005)
F-Statistic	7773	7773	13706	13706
N	52920	52920	49807	49807
Panel E: By Share of Families with College Expectations				
Neighbor Enrolled	0.017**	0.012**	0.007	0.007
	(0.007)	(0.006)	(0.007)	(0.006)
F-Statistic	8840	8840	12801	12801
N	59541	59541	42466	42466
Panel F: By School Motivation Score				
Neighbor Enrolled	0.012*	0.014**	0.006	0.002
	(0.007)	(0.006)	(0.008)	(0.007)
F-Statistic	12393	12393	9478	9478
N	56343	56343	43680	43680

Notes: This table presents 2SLS estimates of the effects of the closest neighbor attending a given school on applicants' ranks, separately by the closest neighbor's most preferred school's characteristics. Enrollment is instrumented with an indicator equals one if the neighbor obtained an offer in her most preferred school in the previous round. All models include lottery fixed effects. Each number corresponds to the 2SLS estimate in a given cell (school characteristic × value above/below the median). Standard errors are clustered at the neighbor level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 12: 2SLS Estimates on School Applications

	(1) Submissions =1	(2) Submissions ≥ 2	(3) Submissions ≥ 3	(4) Submissions ≥ 4
Panel A: By Mother's Education				
Neighbor Enrolled	-0.002	0.002	0.011	0.002
	(0.002)	(0.002)	(0.008)	(0.009)
Neighbor Enrolled $\times$	0.000	-0.000	-0.000	-0.001
Mother's Education $\leq$ HS	(0.002)	(0.002)	(0.009)	(0.010)
Mean Dep. Var.	0.01	0.99	0.73	0.40
F-Statistic	8590	8590	8590	8590
Lottery FE	Yes	Yes	Yes	Yes
N	78452	78452	78452	78452
Panel B: By Family Income				
Neighbor Enrolled	-0.003	0.003	0.021**	0.007
	(0.002)	(0.002)	(0.009)	(0.010)
Neighbor Enrolled $\times$	0.002	-0.002	-0.014	-0.008
Family Income $\leq$ CLP 500k	(0.002)	(0.002)	(0.010)	(0.011)
Mean Dep. Var.	0.01	0.99	0.73	0.40
F-Statistic	8505	8505	8505	8505
Lottery FE	Yes	Yes	Yes	Yes
N	78661	78661	78661	78661

Clustered standard errors at the neighbor level in parentheses

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

A Appendix

A.1 Data Availability

Table A1: 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

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.

A.2 Identifying Assumptions

In this section we discuss the set of assumptions required to interpret the estimate of interest as a local average treatment effect (Imbens and Angrist, 1994). From section 3, we are interested in estimating the following model:

$$y_{ijs} = \alpha + \beta x_{js} + \phi_{jl} + \epsilon_{ijs}$$

$$x_{js} = \delta + \lambda z_{js} + \varphi_{jl} + \eta_{js}$$

Our main identification results rely on the following assumptions:

Independence: This assumption implies that z_{js} is independent to both x_{js} and y_i. In our setting, this assumption is satisfied by the tie-breaking rules and the use of lottery fixed effects ϕ_{jl}. Our balance tests in Table 3 provide further evidence to support this assumption.

Relevance: We show the existence of a first stage in Table 4. The instrument z_{js} changes significantly neighbors' enrollment decision.

Exclusion Restriction: This condition implies that z_{js} affects future applicants' decisions only through its effect on neighbors' enrollment. The characteristics of the school assignment mechanism, specifically how tie-breaker numbers are determined, provide support to this assumption.

Monotonicity: This assumption requires that receiving an offer weakly increases the likelihood of enrollment for all neighbors. In our context, this assumption rules out the case of neighbors who are dissuaded from enrolling in school s if they received an offer.

Additionally, as discussed in Altmejd et al. (2021) and Aguirre and Matta (2021), we require that the instrument does not induce neighbors to enroll in a different school s'. Formally, we denote $x_{js'} = x_{js'}(z_{js})$ as the probability of enrollment in school s' as a function of the offer $z_{js} \in \{0, 1\}$ received in school s. Then, we require the following condition to hold:

$$x_{js'}(1) \leq x_{js'}(0) \quad \forall s' \neq s$$

A.3 Additional Application Outcomes

Table A2: 2SLS Estimates on the Number of Applications

	(1) Total Submissions	(2) Submissions $= 1$	(3) Submissions ≥ 2	(4) Submissions ≥ 3	(5) Submissions ≥ 4
Neighbor Enrolled	0.030	-0.002	0.002	0.016***	0.005
	(0.019)	(0.001)	(0.001)	(0.005)	(0.005)
Average Outcome	3.59	0.01	0.99	0.73	0.40
Lottery FE	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No
N	102028	102028	102028	102028	102028

Clustered standard errors at the neighbor level in parentheses

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

A.4 Additional Robustness Checks

Table A3: Estimates of Neighbors Effects on School Applications

	(1) Applies Same (1st-3rd Rank)	(2) Applies Same (1st Rank)	(3) Attends Same
Neighbor Enrolled	0.010*	0.008**	0.011***
	(0.005)	(0.004)	(0.004)
Average Outcome	0.32	0.15	0.12
F-Statistic	22514	22514	22279
Lottery FE	Yes	Yes	Yes
Neighborhood FE	Yes	Yes	Yes
N	101333	101333	99321

Notes: This table presents 2SLS estimates from equations (1) and (2) including lottery and neighborhood fixed effects. Clustered standard errors at the neighbor level are shown in parenthesis.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A4: Estimates of Neighbors Effects on School Applications

	(1) Applies Same (1st-3rd Rank)	(2) Applies Same (1st Rank)	(3) Attends Same
Neighbor Enrolled	0.013**	0.010**	0.012***
	(0.005)	(0.004)	(0.004)
Average Outcome	0.32	0.16	0.12
F-Statistic	19310	19310	19310
Lottery FE	Yes	Yes	Yes
N	100620	100620	98924

Notes: This table presents 2SLS estimates from equations (1) and (2) restricting the sample to applicants enrolled in K-8 schools. In this case, eight graders necessarily need to enroll in a different high school in the following year. Clustered standard errors at the neighbor level are shown in parenthesis.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A5: Estimates of Neighbors Effects on School Applications

	Excluding Bottom Quartile			Excluding Below-Median		
	(1) Applies Same (1st-3rd Rank)	(2) Applies Same (1st Rank)	(3) Attends Same	(4) Applies Same (1st-3rd Rank)	(5) Applies Same (1st Rank)	(6) Attends Same
Neighbor Enrolled	0.014***	0.010**	0.013***	0.015***	0.009**	0.011***
	(0.005)	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)
Average Outcome	0.31	0.15	0.12	0.30	0.15	0.11
F-Statistic	19139	19139	18962	17943	17943	17799
Lottery FE	Yes	Yes	Yes	Yes	Yes	Yes
N	96644	96644	96644	88364	88364	86511

Notes: This table presents 2SLS estimates from equations (1) and (2) restricting the sample to municipalities whose proportion of urban population is above the first quartile (columns (1)-(3)) and above the median value (columns (4)-(6)). Clustered standard errors at the neighbor level are shown in parenthesis.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

A.5 Academic Outcomes

Table A6: 2SLS Estimates of Neighbors Effects on 9th Grade Academic Outcomes

	(1) GPA	(2) Attendance (%)	(3) Promotion to 10th Grade	(4) Participates in another process
Neighbor Enrolled	0.035	0.238	0.012	0.001
	(0.033)	(0.539)	(0.015)	(0.019)
Average Outcome	5.39	90.09	0.90	0.18
F-Statistic	558	558	558	567
Lottery FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	5788	5788	5788	6193

Notes: Each column corresponds to a 2SLS regression where an indicator of the closest neighbor's enrollment in her most preferred school is instrumented with an indicator equals to one if the neighbor received an offer in this school. All outcomes are observed at the end of ninth grade. We restrict our sample to applicants observed in ninth grade during 2019 (see section 5.2 for details). All regressions include controls for applicant's and neighbor's gender, priority status, math and language previous test scores. Clustered standard errors at the neighbor level in parenthesis.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

A.6 Additional Results for Applicant-Neighbor Matching

Table A7: Estimates of Neighbors Effects - Heterogeneity by Applicant-Neighbor Matching

	(1) Applies Same (Any Rank)	(2) Applies Same (1st Rank)	(3) Attends Same
Panel A: Interaction with Applicant's and Neighbor's Parental College Expectations			
Neighbor Enrolled	0.064***	0.043***	0.038***
	(0.014)	(0.011)	(0.010)
Neighbor Enrolled ×	-0.026*	-0.014	-0.015
$\mathbb{1}$(Applicant = No, Neighbor = Yes)	(0.015)	(0.012)	(0.011)
Neighbor Enrolled ×	-0.092***	-0.057***	-0.032***
$\mathbb{1}$(Applicant = Yes, Neighbor = No)	(0.013)	(0.011)	(0.010)
Neighbor Enrolled ×	-0.072***	-0.047***	-0.031***
$\mathbb{1}$(Applicant = Yes, Neighbor = Yes)	(0.016)	(0.013)	(0.012)
F-Statistic	3510	3510	3489
Lottery FE	Yes	Yes	Yes
N	61864	61864	61146
Panel B: Interaction with Applicant's and Neighbor's Family Income			
Neighbor Enrolled	0.020***	0.012*	0.018***
	(0.008)	(0.007)	(0.006)
Neighbor Enrolled ×	-0.007	-0.011	-0.002
$\mathbb{1}$(Applicant $<$ CLP500k, Neighbor $>$ CLP500k)	(0.013)	(0.010)	(0.009)
Neighbor Enrolled ×	-0.053***	-0.019**	-0.022**
$\mathbb{1}$(Applicant $>$ CLP500k, Neighbor $<$ CLP500k)	(0.012)	(0.010)	(0.009)
Neighbor Enrolled ×	-0.046***	-0.027**	-0.016
$\mathbb{1}$(Applicant $>$ CLP500k, Neighbor $>$ CLP500k)	(0.017)	(0.014)	(0.013)
F-Statistic	3625	3625	3592
Lottery FE	Yes	Yes	Yes
N	62807	62807	62080

Notes: Clustered standard errors at the neighbor level
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

A.7 School Value-Added

We use information from ninth-grade cohorts in 2016 and 2018 to construct a proxy of school effectiveness for high school graduation and college attendance. For both cohorts we observe test scores and family background in eight grade. Using this information we estimate a simple school value-added model of the form:

$$y_{ist} = X'_{ist}\beta + \phi_s + \phi_t + \xi_{ist} \tag{9}$$

Our outcomes y_{ist} are an indicator equals to one when student i in cohort t graduated on time from high school s and an indicator equals to one if student i attended college. 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. We estimate equation (9) and recover the raw school fixed effects $\hat{\phi}_s$.

As it is common practice in the value-added literature (Kane and Staiger, 2008; Chetty et al., 2014; Bacher-Hicks et al., 2019), we generate empirical Bayes shrunken estimates of $\hat{\phi}_s$ to account for sampling error and minimize mean square prediction errors. We construct residuals $\hat{\xi}_{ist}$ from equation (9) and assume these can be decomposed into a component attributable to schools (ϕ_s), school-year variation (θ_{st}), and student-level idiosyncratic error (ϵ_{ist}). Using these variance components, we generate empirical Bayes shrunken estimates of school effects following Kane and Staiger (2008). Specifically, we multiply the weighted average of school-year level residuals by an estimate of its reliability, accounting for the number of observations in each school-year cell:

$$\hat{\phi}_s^{EB} = \overline{\xi}_s \times \frac{\hat{\sigma}^2_\phi}{\hat{\sigma}^2_\phi + \left(\sum_t \hat{\sigma}^2_{st}\right)^{-1}} \tag{10}$$

Where:

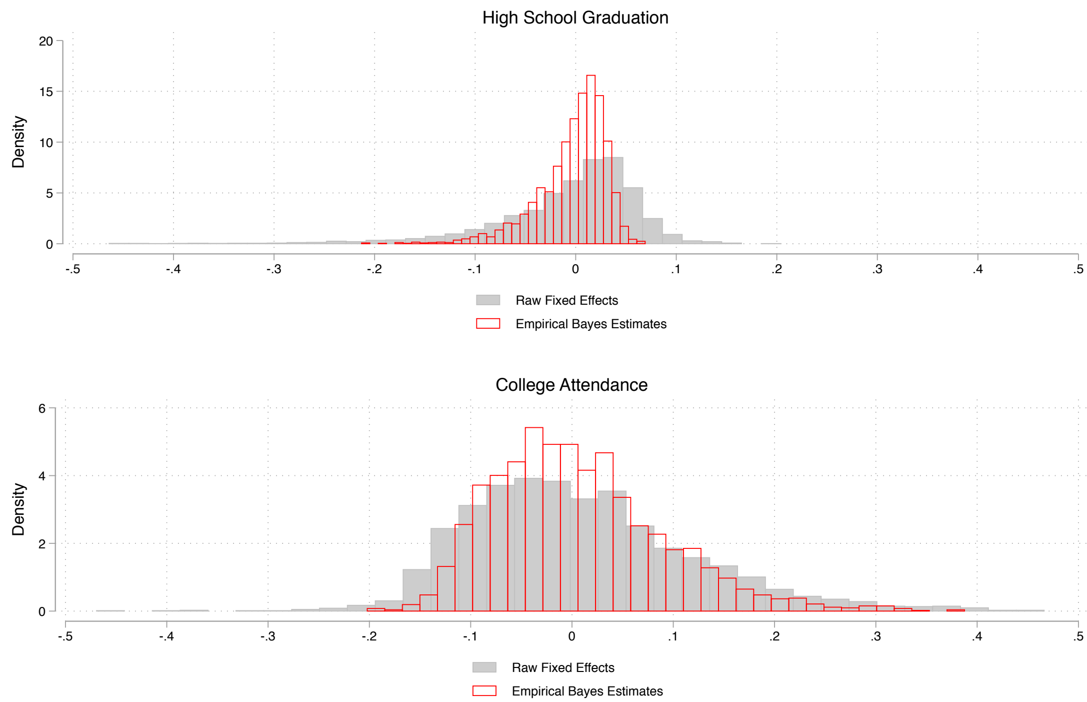
$$\overline{\xi}_s = \sum_t \overline{\xi}_{st} \times \frac{\hat{\sigma}^2_{st}}{\sum_l \hat{\sigma}^2_{sl}} \tag{11}$$

$$\hat{\sigma}^2_{st} = \left(\hat{\sigma}^2_\theta + \frac{\hat{\sigma}^2_\xi}{N_{st}}\right)^{-1} \tag{12}$$

In equations (10), (11), and (12), the school-level variance $\hat{\sigma}^2_\phi$ corresponds to the year-to-year covariance in school-year average residuals $\hat{\sigma}^2_\phi = cov(\overline{\xi}_{st}, \overline{\xi}_{st'})$. We estimate the student-level idiosyncratic variance $\hat{\sigma}^2_\epsilon$ as the variance in within-school deviations in student outcomes. Finally, we estimate the year-to-year school-level variation as the remainder of the total variation: $\hat{\sigma}^2_\theta = Var(\xi_{ist}) - \hat{\sigma}^2_\phi - \hat{\sigma}^2_\epsilon$.

Figure A1 shows the distribution of the raw fixed effects ($\hat{\phi}_s$) and the Empirical Bayes estimates ($\hat{\phi}_s^{EB}$). The standard deviation of the raw school fixed effects for high school graduation is 0.076 while the standard deviation of the empirical Bayes estimates is 0.035. For college completion, these standard deviations are 0.11 and 0.08, respectively.

Figure A1: Empirical Bayes Estimates for High School Graduation and College Attendance



Notes: This plot shows the distribution of estimates of school value-added on high school graduation and college enrollment obtained from equation (9). Each subplot shows the distribution of the raw school fixed effects ($\hat{\phi}_s$) and the empirical Bayes estimates ($\hat{\phi}_s^{EB}$), constructed following Kane and Staiger (2008). These estimates are used to characterize schools in our analysis of section 5.

A.8 Additional Results for Differences Across School Attributes

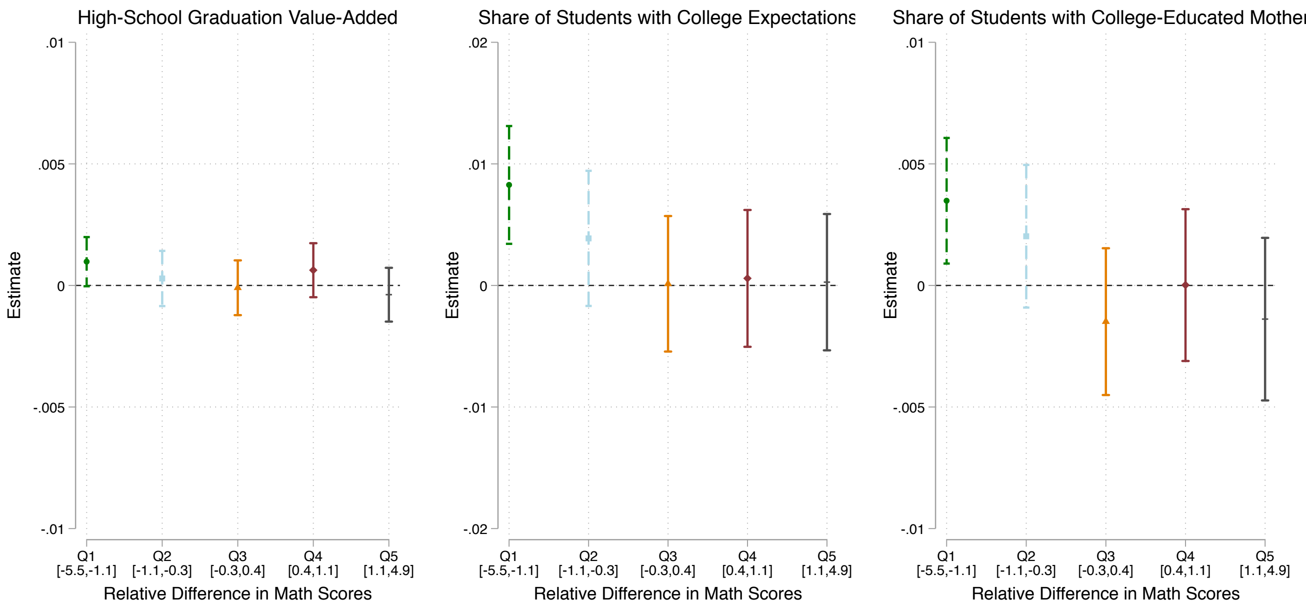
Table A8: 2SLS Estimates of Neighbors Effects by School's Characteristics

	Below Median		Above Median	
	(1) Applies Same (1st-3rd Rank)	(2) Applies Same (1st Rank)	(3) Applies Same (1st-3rd Rank)	(4) Applies Same (1st Rank)
Panel A: By School Value-Added on High-School Graduation				
Neighbor Enrolled	0.018**	0.017***	0.008	0.003
	(0.008)	(0.006)	(0.007)	(0.006)
F-Statistic	8091	8091	11602	11602
N	50720	50720	50848	50848
Panel B: By School Value-Added on College Attendance				
Neighbor Enrolled	0.017**	0.014**	0.008	0.005
	(0.008)	(0.006)	(0.007)	(0.005)
F-Statistic	8180	8180	12091	12091
N	50703	50703	50865	50865

Notes: This table presents 2SLS estimates of the effects of the closest neighbor attending a given school on applicants' ranks, separately by closest neighbor's most preferred school's characteristics. School value-added is constructed using 2016 and 2018 eight-grade cohorts. See section A.7 for details. Enrollment is instrumented with an indicator equals one if the neighbor obtained an offer in her most preferred school in the previous round. All models include lottery fixed effects. Each number corresponds to the 2SLS estimate in a given cell (school characteristic $\times$ value above/below the median). Standard errors are clustered at the neighbor level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

A.9 Additional Results for Effects on School Characteristics

Figure A2: Effect of Neighbors' Enrollment on Schools Chosen by Applicants



Notes: Each plot presents 2SLS estimates of the effect of the closest neighbor attending a given school on applicants' most preferred schools' characteristics. We divide applicant-neighbor pairs in quintiles by differences in previous math test scores. Enrollment is instrumented with an indicator equals to one if the neighbor got an offer in their most preferred school in the previous round. All models include lottery fixed effects (see equations (7) and (8)). Standard errors are clustered at the neighbor level.

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