Neighbors' Spillovers on High School Choice[*]

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Abstract

Do residential neighbors affect each others' schooling choices? We exploit oversubscription lotteries in Chile's centralized school admission system to identify the effect of nearby neighbors on application and enrollment decisions. A student is 9-12% 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. Spillovers are larger in neighborhoods with better accessibility to green areas, recreation centers, and other public services, consistent with a social interactions mechanism. In terms of school characteristics, we find positive spillovers of around 0.03σ on school climate but no detectable effects on school average performance in standardized tests or value-added. Our findings highlight that the indirect effects of school choice policies are relevant but might decay rapidly for later cohorts.

Keywords: spillovers, high school choice, centralized school systems

JEL Codes: I21, I24

1 Introduction

The determinants of school choice behavior have been a focus of policy interest and academic research for several decades. While differences in access to schools can be partially attributed to parental preferences for observable attributes, such as distance or cost, growing evidence suggests that information also plays a significant role in explaining racial and socioeconomic gaps in access to high-quality schools (Hastings and Weinstein, 2008; Andrabi et al., 2017; Corcoran et al., 2018; Ainsworth et al., 2023). School choices not only directly impact children's academic outcomes and future life opportunities, but they may also spillover to future cohorts of students, potentially exacerbating segregation patterns or widening achievement gaps if more disadvantaged families disproportionately attend low-performing schools. Understanding how local environments shape families' preferences for schools is important, given that neighborhoods and communities influence other relevant economic margins, such as the likelihood of working in the same location (Bayer et al., 2008; Hellerstein et al., 2011) or engaging in criminal activity (Glaeser et al., 1996; Billings et al., 2019). However, providing credible evidence on whether residential neighbors influence school decisions can be challenging in most educational contexts due to nonrandom sorting into schools and the presence of unobserved neighborhood attributes.

In this paper, we study the importance of close neighbors on families' high school application and enrollment decisions. Using data from Chile's centralized school assignment processes between 2019 and 2022, we link applicants to their nearby residential neighbors and show that shocks to neighbors' enrollment decisions spillover to applicants in the next year, affecting their choices of applying to and attending the same schools. From a policy perspective, taking into account these dynamic responses to neighbors' enrollment has important implications for the design and evaluation of school choice interventions.

Identifying spillover effects using observational data is subject to two empirical problems, known in the literature as the reflection problem and the existence of correlated effects (Manski, 1993). To surmount these two challenges, we take advantage of the school assignment rules, featuring a deferred acceptance mechanism and lottery tie-breakers in oversubscribed schools. Building on earlier work (Abdulkadiroğlu et al., 2011, 2017; Gray-Lobe et al., 2023) we employ an instrumental variables strategy to identify the effect of neighbors' enrollment on applicants' decisions. We exploit the exogeneity introduced by the tie-breaking rules in a large number of oversubscribed schools to simulate counterfactual school allocations and estimate the effect of shocks to neighbors' expected assignment on applicants' decisions (Borusyak and Hull, 2023). To overcome the reflection problem, we focus on the effect of shocks to neighbors' assignment in the *previous* round on the probability of applying to the same school in the *current* round.

We find that nearby neighbors influence future applicants' behavior. First, using a sample of applicants linked to their closest neighbors, we find increases of 2.2 and 1.4 percentage points in the probability of applying to the same school and rank it as the top-choice when the closest neighbor attends their target school. These estimates represent increases of 6% and 9% relative to the average for the non-treated group. In terms of school attendance, the probability of enrolling in the same school increases by 1.2 percentage points (12%). Second, we construct an extended sample linking each applicant to the set of neighbors located within a 0.1-mile radius. In this case, our analysis requires defining a focal school to assess whether applicants are more likely to be affected by neighbors' decisions. Our preferred estimates, which consider the reference school as the closest neighbor's top-ranked choice, are almost similar to ones obtained using the closest-neighbor sample. Using the most preferred school among all nearby neighbors as the focal school has little impact on our estimates.

We conduct a series of heterogeneity analyses to investigate how these estimates vary by observable characteristics. Firstly, we find stronger spillover effects when the applicant is male, is classified as low-SES status, and performs relatively lower in standardized tests. However, these differences are not statistically significant across subgroups and are more prominent in the extended sample considering all neighbors within 0.1 miles. Secondly, we find that neighborhood characteristics play a key role in determining the strength of the spillover effects. Our estimates are larger in neighborhoods characterized by a larger number of households and green areas, and improved access to public services. In a different exercise, we test whether spillovers decay with distance. Consistent with prior evidence, we find that spillovers are relevant only for the set of neighbors located within 0.2 miles. As the distance increases, our estimates decay and become not statistically distinguishable from zero. Taken together, our results provide evidence supporting that social interactions are the main driver of our results and that they operate at a very local level.

Guided by recent literature studying parental preferences for schools (Burgess et al., 2015; Abdulkadiroğlu et al., 2020; Beuermann et al., 2023; Ainsworth et al., 2023), we supplement our analysis with available data to characterize schools across different dimensions, such as distance, average test scores, school value-added, and school climate. We employ these metrics to analyze heterogeneous effects by school traits. We find that applicants are more likely to mimic their closest neighbors' choices when the schools they attend locate closer and rank higher in the college enrollment value-added distribution. Spillover effects on application decisions decay almost completely for schools located 5 miles away from the average distance. At the same time, we find that applicants are unresponsive to neighbors enrolling in schools that improve test scores.

Next, we move to the question of whether neighbors' decisions also shape the characteristics of the schools applicants select and whether they also impact applicants' academic outcomes. We employ our set of school traits to quantify changes in the characteristics of schools applicants choose. We find positive spillovers on school climate. Moving the closest neighbor's attended school from the average to the 84th percentile in the school climate distribution induces an increase of 0.03σ in the applicant's attended school climate. At the same time, we find that neighbors enrolling in schools with better average performance in tenth-grade standardized tests and higher value-added on college enrollment and high school graduation have modest and statistically insignificant impacts on the attributes of the schools chosen by applicants. After documenting these patterns, we assess whether changes in the characteristics of neighbors' attended schools impact applicants' short-term academic outcomes and find suggestive evidence of improvements on grade progression and performance in tenth-grade language standardized tests. However, most of our estimates are uniformly small and imprecise.

This paper contributes primarily to the literature studying spillover effects on human capital decisions.[1] Previous work related to the effects of social networks on educational choices has focused mostly on siblings' effects at the secondary level (Joensen and Nielsen (2018) for Denmark, Dustan (2018) for Mexico, and Dahl et al. (2023) for Sweden) and at the college level (Goodman et al., 2015; Aguirre and Matta, 2021; Altmejd et al., 2021).[2] By contrast, evidence about neighbors' effects on educational decisions is less common and has focused primarily on post-secondary decisions (Barrios-Fernández, 2022; Avdeev et al., 2024; Backes and Kovac, 2024).[3] 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. Our paper differentiates from these two studies in several ways. First, while Bobonis and Finan (2009) and Lalive and Cattaneo (2009) focus on extensive margin changes in enrollment, we also study neighbors' spillovers on the quality of high schools chosen by students enrolled in eighth grade and their short-term consequences on academic outcomes. Studying spillover effects on these margins can shed light on the determinants of disparities in the quality of schools attended by socioeconomic groups.[4] Second, our study em-

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[1] 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 (Glaeser et al., 1996; Billings et al., 2019), or perceptions about well-being (Luttmer, 2005).

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

[3] One exception is Goux and Maurin (2007) who study neighbor spillovers on grade progression for high school students.

[4] There is vast evidence showing that parents choose schools based on different attributes depending on their socioeconomic status. See for example Neilson (2023) for Chile, Hastings et al. (2010) for the U.S., and Burgess and

ploys data from a centralized school assignment system covering around 90% of the population of eighth-grade students in Chile. By contrast, PROGRESA is a conditional cash transfer program, targeting a specific subgroup of marginalized families in rural areas with low rates of school attendance. Therefore, our results are more likely to be generalizable to other educational systems in developed and middle-income countries.

We also contribute to the literature examining the indirect effects of centralized school choice mechanisms. We extend prior work related to the short-term (Cullen et al., 2006; Hastings et al., 2006; Dobbie and Fryer, 2011; Abdulkadiroğlu et al., 2018; Lincove et al., 2018) and long-term impacts (Deming, 2011; Deming et al., 2014; Dustan et al., 2017; Gray-Lobe et al., 2023) of winning admission to an oversubscribed school by studying how applicants' decisions spillover to future cohorts. Understanding the magnitude of these indirect effects and the mechanisms at play is important for at least two reasons. First, it has implications for the design of interventions and policies in school choice contexts, including recently implemented plans such as the Zones of Choice program in Los Angeles Unified School District (Campos and Kearns, 2024), as well as for existing centralized school systems around the globe. Spillovers might amplify the population directly targeted by school choice programs through social interactions, for example by shaping parents' information sets or reinforcing the credibility of new information made available to them (Campos and Kearns, 2024; Campos, 2024). Second, as pointed out by Angelucci and De Giorgi (2009), it is necessary to consider the spillover effects generated by treated units to evaluate these interventions accurately. In school choice contexts, spillover effects are relevant for analyzing the cost-effectiveness of competing alternatives to improve educational outcomes, such as reallocation or resource augmenting policies (Abdulkadiroğlu et al., 2025). Our analysis corroborates this idea by showing that indirect effects in centralized school systems are meaningful.

Taken together, we show that, conditional on their choice sets, neighbors' enrollment decisions propagate to applicants in later years. Our results suggest that these spillovers are a consequence of applicants reducing search and decision-making costs at the moment of choosing a high school. As a consequence, residential neighbors' choices may partly explain the persistence of unequal access to high-quality schools and subsequent achievement gaps.

Briggs (2010) for the U.K.

2 Institutional Background and Data

2.1 Institutional Background

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* or School Inclusion Bill, enacted in 2015, introduced stark changes to how parents applied to schools through the implementation of the *Sistema de Admisión Escolar* or School Admission System for all schools receiving total or partial public funds. Before the law's passing, voucher schools could charge tuition add-ons and run admission processes independently, while public schools faced more restrictions. By 2017, public and voucher schools concentrated 36% and 55% of the nationwide enrollment, respectively. Private schools, which account for 9% of total enrollment, were not included in the reform and do not participate in the centralized assignment mechanism.

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

Two important features of this centralized system are worth mentioning. First, some groups of students receive priority in the assignment rule. There are four priority groups served in strict order: (i) students with siblings enrolled at the school, (ii) students with a parent working at the school,

(iii) former students previously enrolled at the school, and (iv) all other applicants (Correa et al., 2022). Furthermore, the system includes special quotas for vulnerable students, and some schools can select a fraction of their seats based on admission tests.[5] In the former case, disadvantaged students are given the second highest priority after (i). In the latter case, the system first fills these quotas by assigning students based on their admission test scores, and the remaining seats are assigned following the priority groups (i)-(iv). Online Appendix Figure A.4 summarizes the seat classification within schools and the priorities in each case.[6] Second, ties are broken randomly within each priority group in oversubscribed schools. Figure 2 shows the proportion of schools receiving more first-rank applications than vacant seats. This figure shows that in ninth grade, more than 30% of the schools participating in the system are oversubscribed.

Crucially for our purposes, the administrative records include the geocoded location of every applicant. For confidentiality purposes, these locations contain a small amount of noise. Specifically, each location is displaced using the "random direction, random distance method" (Burgert et al., 2013).[7] Additionally, not all addresses in the data correspond to actual residences. We take on a number of steps to discard unreliable geographic locations. First, we drop all students with imputed addresses.[8] Second, we drop applicants whose registered location indicates one region, but their school enrollment records in the same year indicate a different one. This sample selection drops around 20% of all applicants.

2.2 Sample Construction

We assemble two analysis samples using data for ninth-grade applicants observed in the 2019-2022 application rounds linked to nearby neighbors whom we observe applying to ninth grade in the previous year within a given distance. As Figure 1 shows, 2019 is the first year when we observe applications for each region in the country. Our main analysis focuses on the effect of the nearest neighbor's decisions. In this case, we restrict the pooled sample to contain only one neighbor per

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

[6]An additional issue relates to the fact that students might receive different priorities depending on which group they are considered. For example, a disadvantaged student with a working parent fits into two different seat categories (disadvantaged and no trait). In these cases, the allocation mechanism assumes that students have preferences over contracts that specify the school and the type of seat to be used, whereas schools have preferences over contracts specifying the student and the type of seat (shown in Online Appendix Figure A.4). See Correa et al. (2022) for additional details.

[7]For every location, a random angle (between 0 and 360 degrees) and a random distance (between 50 and 350 meters) are generated following a uniform distribution. As a result, the observed locations are displaced between 50 and 350 meters from their actual positions, with a median displacement of 175 meters. Since the added noise is random, we cannot ascertain the specific distance by which each address has been displaced.

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

applicant. Throughout the paper, we refer to this sample as the "closest-neighbor sample." Second, we also analyze spillovers considering all neighbors within a radius of 0.1 miles—after excluding the cases described at the end of the previous section. We refer to this sample as the "0.1-mile sample." On average, each applicant is linked to 10.1 neighbors.

To distinguish between close neighbors and members of the same family applying in different years, we employ anonymized parent identifiers and discard siblings or pairs of students associated with the same adult responsible for the application.[9] Finally, we exclude from the estimation sample neighbors who took an admission test in their most preferred school.[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 each applicant to enrollment records in the next year to observe which school they ultimately attend.

We supplement our samples with three additional sources of information. Firstly, we link students' previous math and language test scores in standardized national exams (SIMCE tests).[11] We can merge these records to 80% of applicants and 82% of neighbors in our sample. Online Appendix Table A.2 summarizes the grade from which we can observe these records for each cohort. Secondly, we incorporate available student- and school-level data for the 2015-2018 cohorts of tenth-grade students to construct different proxies of school quality, such as average tenth-grade SIMCE scores, school value-added, and school climate. Thirdly, we use information about neighborhood characteristics at the sub-municipal level made available by the Ministry of Social Development and the Chilean Statistics Agency. We employ these supplementary records in our heterogeneity analysis presented in section 4.5.

2.3 Sample Description

Our analysis focuses on eighth-grade students who apply to a high school using the centralized admission system. Students enrolled in K-12 schools can choose to participate if they want to switch, while students enrolled in K-8 schools necessarily need to participate unless they prefer to enroll in a private school. Table 1 compares observable characteristics of applicants relative to the universe of eighth-grade students enrolled in public and voucher schools. Column (1) shows the av-

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[9]Unfortunately, we do not observe student names in any of the administrative records we employ in this study.

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

[11]SIMCE—an acronym of *Sistema de Medición de la Calidad de la Educación*—is the Chilean national learning outcome assessment system. It was created in 1988 and has been the primary indicator to identify effective schools (Mizala and Urquiola, 2013) or intervene ineffective ones (Chay et al., 2005).

erage characteristics of all students enrolled in K-12, non-private schools, while column (2) restricts the sample to students enrolled in K-8 schools. Column (3) shows the characteristics of students participating in the centralized system, which correspond to approximately 49% of all eighth-grade students in the country. Relative to column (1), the subset of applicants is more disadvantaged, measured by the total fraction of low-income students (*prioritario* and *preferente* statuses), prior performance in SIMCE tests, and parents' education.[12] The comparison of the number of students in columns (2) and (3) shows that a small fraction of students enrolled in K-12 schools choose to apply to a different school in ninth grade.

Column (4) presents summary statistics for the subset of students in our main estimation sample, defined as applicants who are linked to their closest neighbors with a probability of receiving an offer at their top-ranked school strictly between zero and one. The comparison of columns (3) and (4) suggests that our estimation sample is representative of the population of applicants.[13] Panel A shows that around 53% of applicants in our estimation sample are girls, 63% have a disadvantaged (*prioritario*) status, and 59% attended a public school in eight grade. The average baseline math and language test scores are -0.25σ and -0.18σ, respectively, reflecting differences in achievement between students enrolled in public and private schools. Around 19% of applicants' mothers have a college degree, and 9% of applicants' families report a monthly income higher than CLP800k ($\approx$ US$1,000 in year 2021). Similarly, Panel B summarizes application metrics for all applicants and randomized applicants. Students in our estimation sample apply on average to 3.6 schools, and around 60% of them submit three schools or less. When comparing application outcomes to the universe of applicants, our estimation sample exhibits only minor differences. Applicants in our sample submit 0.08 more schools on average and the proportion submitting four or more schools is only 1.5 percentage points larger.

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

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[12]The *prioritario* and *preferente* statuses were introduced in 2008 by the *Ley de Subvención Escolar Preferencial* or SEP bill, which established a new targeted voucher to transfer additional resources to schools receiving these students. Each status is determined based on household economic hardship, income, and mother's education. See Mizala and Torche (2017), Feigenberg et al. (2017), and Neilson (2023) for additional details about the implementation of the bill and its consequences on student outcomes.

[13]However, a formal joint F-test of significance rejects that these characteristics are balanced across the two groups at the 5% level.

3 Empirical Strategy

In this section, we describe our empirical strategy to identify the impact of close neighbors' enrollment on applicants' decisions. Our strategy leverages variation in neighbors' enrollment induced by random admission offers in oversubscribed schools, conditional on their choice sets.

3.1 Neighbor Spillovers

Consider an individual i linked to a set of neighbors among year $t-1$ participants. We are interested in studying how neighbors' enrollment in a given school s_i affects applicant i's decisions in year t. Throughout the paper, we refer to s_i as the "reference school." We describe the causal relationship between both variables using the following model:

$$y_i = \alpha + \beta x_i + \epsilon_i \tag{1}$$

In this simple model, y_i is a binary indicator equal to one if i applied to (or enrolled in) the reference school s_i. Notice that we include a subscript in the reference school since this varies for each applicant regardless of the definition used. x_i denotes the treatment variable, which can be defined in various ways depending on the set of nearby neighbors linked to each applicant. Our preferred estimates designate the reference school as the top-ranked school submitted by the closest neighbor. However, in our robustness checks we also consider the modal top-ranked school among neighbors located in the 0.1-mile vicinity.

Conditional on our definition of s_i, in the closest-neighbor sample x_i is a binary variable equal to one if the nearest neighbor enrolls in it. In the 0.1-mile sample, we consider two definitions of the treatment variable. First, we employ x_i as an indicator equal to one if there is at least one neighbor enrolling in the reference school. Second, we define x_i as the number of nearby neighbors enrolled. Our parameter of interest in equation (1) is β, which captures neighbor spillovers on i's behavior. In order to obtain an unbiased estimate of this parameter, we need to address both the reflection and correlated effects problems (Manski, 1993). The former is addressed by considering the effect of neighbors' enrollment on the next cohort of applicants. For the latter, we need to isolate the effect of x_i from other determinants contained in ϵ_i, which are common across neighbors living in the same areas, such as preferences, choice sets, or shocks. To surmount this econometric problem, we leverage exogenous variation in the likelihood of receiving an offer at the most preferred school

generated by tie-breaking rules in oversubscribed schools.

3.2 Identification

3.2.1 Centralized School Assignment

As discussed in Section 2, all applicants within a priority group who rank the same oversubscribed school as their first choice have the same probability of receiving an admission offer to that school. We characterize an applicant $i \in \mathcal{A}$ by a vector θ_i that includes a list of preferences over schools included in the set $\mathcal{S}$ and priorities at each of them. Let Θ be the collection of preferences and priorities for all applicants, $\Theta = \{\theta_i\}_{i \in \mathcal{A}}$. School assignment is a function of Θ and a collection of random tie-breakers Ω that follow a known distribution F. Let $a_i \in \mathcal{S}$ denote the school where student i is assigned. Then, we can write:

$$a_i = a_i(\Theta, \Omega) \tag{2}$$

Borusyak and Hull (2023) note that in the presence of network spillovers, estimating β in Equation (1) might suffer from omitted variable bias due to the existence of nonrandom exposure to shocks. Intuitively, due to residential sorting or other type of unobserved factors, some applicants might receive systematically different values of x_i. To the extent that this variation is correlated with ϵ_i, this will generate bias. They propose to control for confounding effects by simulating the data-generating process from which the shocks are drawn and adjusting for the expected treatment across units to eliminate omitted variable bias. In our context, since x_i is endogenous, their approach implies simulating the centralized school assignment described by equation (2) multiple times and computing the expected value of an available valid instrument z_i (e.g., how many neighbors are assigned to the reference school).

To fix ideas, assume that our instrument z_i is the number of neighbors assigned to the reference school. Let $\lambda_i = \mathbb{E}[z_i | \Theta]$ be the expected value of the instrument conditional on the set of applicants' types Θ (i.e., the expected number of neighbors assigned to the reference school). The observed value of z_i is a random deviation from its expected value λ_i. By making this adjustment, the parameter β is identified by comparing outcomes of applicants who had more neighbors receiving an offer than expected to those who had fewer than expected assigned neighbors.

In our analysis we employ three definitions of the instrument z_i. Our first definition corresponds to the total number of neighbors located within 0.1 miles who are assigned to the reference school. We denote $\mathcal{N}$ to the set of applicants who participated in the centralized system in the previous

year. Conditional on the reference school, the instrument corresponds to:

$$z_i = \sum_{j \in \mathcal{N}_i} \mathbb{1}(a_j = s_i), \qquad (3)$$

where $\mathcal{N}_i = \{j \in \mathcal{N} \text{ s.t. } d_{ij} \leq 0.1\}$ is the set of neighbors who reside within the 0.1-mile vicinity from i and s_i is the reference school. Second, we define z_i as a binary variable equal to one if the closest neighbor receives an offer at their top-ranked school:

$$z_i = \mathbb{1}(a_{k(i)} = s_i), \qquad (4)$$

where $k(i) = \text{argmin}_j\{d_{ij}\}$. Notice that in this case λ_i will be a continuous variable equal to the probability of receiving an offer at that school. This object is also known in the literature as the assignment propensity score (Abdulkadiroğlu et al., 2017; Angrist et al., 2024). Third, we can characterize neighbors' influence by exposure to at least one nearby neighbor enrolling in s_i. In this case, z_i corresponds to a binary variable equal to one if there is at least one neighbor receiving an offer at the reference school:

$$z_i = \max_{j \in \mathcal{N}_i} \mathbb{1}(a_j = s_i) \qquad (5)$$

In each case, the expected value of the instrument is expressed as:

$$\lambda_i = \mathbb{E}\left[z_i | \mathcal{N}_i, \Theta\right] \qquad (6)$$

3.2.2 Empirical Validation

We conduct a validation exercise by computing λ_i using the definition of equation (6) through simulation. We replicate the assignment algorithm 1,000 times, defining the reference school as the modal top-ranked school among neighbors located within 0.1 miles to each applicant. In each iteration l, we change the random tie-breaker and obtain counterfactual school allocations while holding preferences and priorities fixed. We use the simulated allocation to compute each of the instruments described in equations (3)-(5). We then average across simulations to obtain an estimate of the expected value of each instrument, $\hat{\lambda}_i$.

We present the results from this procedure in Online Appendix Figures A.7 and A.8. Online Appendix Figure A.7 presents the distribution of the simulated $\hat{\lambda}_i$ across applicants. The left figure displays the distribution of the probability of the closest neighbor receiving an offer at the reference school. The center and right figures consider the simulated expected value of the instrument using

the 0.1-mile sample. The center and right figures display the distribution of the probability of having at least one nearby neighbor and the expected number of nearby neighbors receiving an offer at the reference school, respectively. Online Appendix Figure A.8 shows that our simulation predicts the observed values of the instrument. As it can be seen in each subplot, the bins almost perfectly follow the 45-degree line, illustrating that our replication matches the observed allocations.

3.2.3 Identifying Assumptions

Identification of spillovers requires admission offers to affect neighbors' school enrollment (i.e., z_i predicts x_i), as well as the following conditional independence assumption:

$$z_i \mid \lambda_i \perp \epsilon_i \tag{7}$$

To explain the intuition behind this assumption, consider that the treatment variable corresponds to the number of neighbors enrolled in the most popular school in the neighborhood. Assumption (7) means that conditional on the expected number of neighbors receiving an offer at this school, the observed number of neighbors who obtain an offer must be independent of unobserved factors affecting i's enrollment, contained in the error term ϵ_i in equation (1). Independence with respect to ϵ_i relies on an exclusion restriction, i.e., we need z_i to affect y_i exclusively through its effect on x_i. In other words, we need to assume that admission offers made to neighbors do not affect applicants choices unless they affect neighbors' actual enrollment. This assumption rules out the possibility that an offer can affect the applicant even if the neighbor does not attend that school. In section 4 we present evidence that supports the exclusion restriction.

3.2.4 Estimation

Our baseline specification uses the following two-stage least squares (2SLS) system to estimate neighbor spillovers:

$$y_i = \alpha + \beta x_i + \phi \lambda_i + \varepsilon_i \tag{8}$$
$$x_i = \gamma + \delta z_i + \pi \lambda_i + \nu_i, \tag{9}$$

where y_i is a binary indicator that equals one if i applied to (or enrolled in) the reference school and x_i is the endogenous variable capturing the influence of nearby neighbors. Following our previous discussion, we conduct our analysis using three definitions of x_i. Our main analysis focuses on the closest neighbor's enrollment. In this case, x_i is an indicator equal to one if they enroll in their most preferred school. Our instrument, z_i, is an indicator equal to one if they receive an offer at

that school. When using the 0.1-mile sample, the endogenous variable is an indicator equal to one if there is at least one neighbor enrolled at the reference school. We also consider the number of neighbors enrolled. In both cases, the instrument corresponds to the same object represented by x_i, but using assignment instead of enrollment.

λ_i is the average value of z_i across simulations. While our preferred specification displayed in equations (8)-(9) controls linearly for λ_i, Borusyak and Hull (2023) propose using a recentered instrument $\tilde{z}_i = z_i - \lambda_i$. In our robustness checks, we employ the latter approach as well as an alternative specification adding a fully saturated set of indicators for λ_i. In this case, ϕ and π vary for each element of the support of λ_i. Overall, we find very small differences between these alternatives compared to our preferred specification. To estimate heterogeneous effects by observable characteristics, we add interactions with the instrument z_i, endogenous variable x_i, and expected value of the instrument λ_i to avoid omitted variable bias stemming from model misspecification (Feigenberg et al., 2025).

Finally, in all of our specifications we cluster standard errors to account for correlation in ε_i and ν_i across units. We cluster at the reference-school × applicant-priority-group × year level to flexibly allow for correlated shocks among applicants with the same *ex-ante* probability of receiving an offer at the reference school. Our results are robust to the choice of other clustering alternatives.

3.3   Balance Tests

Before presenting our main results, we examine the validity of our empirical strategy. Under the exclusion restriction, admission offers to each neighbor should be uncorrelated with other determinants of applicants' school attendance after conditioning on their probability of assignment. We present a balance test based on the closest neighbors' offer status in Table 2. Panel A shows that applicants' observable characteristics are balanced based on neighbors' offers. We test differences across several individual and family characteristics. Specifically, we consider gender, socioeconomic status, high-achieving status (defined as being in the top quintile of the eight-grade GPA distribution), baseline test scores, parents' education, college expectations, and family income for each applicant. Columns (1) and (2) show estimates of a separate OLS regression of the observable characteristic onto an offer indicator, conditioning on the propensity score. All but two of the estimates are not statistically significant at the 10% level. The only imbalanced covariates are gender and baseline math test scores. For the latter, we find that applicants whose neighbors were admitted to their target school scored 0.015σ below those linked to neighbors who did not received an offer. The remaining covariates display small differences not statistically different from zero at

the 10% level. We also conduct a joint significance test where we regress the offer indicator onto all background variables listed above and test the hypothesis that all coefficients are jointly zero. The p-value of 0.349 provides further evidence that the likelihood of a neighbor receiving an offer is exogenous to applicants' observable characteristics.

Analogously, we test whether neighbors' observable characteristics are balanced between offered and non-offered individuals. Panel B of Table 2 shows the estimates of regressions on the same set of observable characteristics as well as the p-value from a joint significance test. As expected, the estimates show that student attributes do not explain seat assignment after conditioning on the assignment propensity score. Finally, the last row shows no statistically significant differences in the geographic distance between each applicant and their closest neighbor.

4 Results

4.1 Neighbors' Spillovers on School Applications and Enrollment

Table 3 presents our intent-to-treat (ITT) and 2SLS estimates of the influence of the closest neighbor on applicants' behavior. Column (1) shows the estimate of the first-stage coefficient δ in equation (9). This estimate shows that neighbors who receive an offer at their top-ranked school are 69 percentage points (p.p.) more likely to attending it in ninth grade. Column (2) indicates that the probability of including this school in the application list increases by 1.5 p.p. on average if the closest neighbor receives an offer. Relative to the sample mean, this estimate represents an increase of 4%, displayed in brackets below the standard errors. Column (3) shows that the probability of applying to the same school as top choice increases by 1 p.p. (6%). Column (4) shows the ITT estimate on school attendance. We find an increase of 0.8 p.p. (5%) in the probability of attending neighbors' most preferred alternative.

Columns (5)-(7) show our 2SLS estimates using assignment as an instrument for observed attendance. The probability of applying to the same school in any preference increases by 2.2 p.p. and the probability of ranking it as the top choice increases by 1.4 p.p. Both estimates correspond to increases of 6% and 9%, respectively, relative to the mean for compliers in the untreated state (Abadie, 2002). Finally, column (7) shows that the closest neighbor's enrollment in their most preferred school also increases the probability of an applicant attending it by 1.2 p.p. (12%). The F-statistic is large, demonstrating the strength of our instrument. As a consequence, our estimates do not suffer from some of the concerns related to inference based on t-ratios in IV settings similar

to ours (Lee et al., 2022).[14] The correction proposed by Lee et al. (2022) leaves our standard errors and confidence intervals unchanged.

Comparison to OLS estimates: We report OLS estimates from unadjusted specifications (i.e., not controlling for λ_i) in Online Appendix Table A.3. Using the same estimation sample, we find an increase of 6-7 p.p. in the probability of applying to the closest neighbor's top-ranked school. These figures are more than four times larger than the 2SLS estimate reported in Table 3. Similarly, the OLS estimate for enrollment is 9.4 percentage points, around eight times larger than our 2SLS estimate. The upshot of these comparisons is that not properly accounting for correlated effects vastly overstates the magnitude of neighbor spillovers.

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 using the closest neighbor are not directly comparable to these estimates. First, these studies report the change in the likelihood of attending a school when the peer group's enrollment rate increases by 1 percentage point, while our treatment variable is defined only by the closest neighbor's enrollment. In addition, our sample is not restricted to a particular subpopulation (such as the villages participating in the PROGRESA program) and includes applicants from different backgrounds. For these reasons, we also consider how our estimates relate to siblings' effects on school choices at the secondary level. Overall, our estimates align with the effects reported by other work in this literature.[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 percentage points in the probability of choosing the same college-major combination.

Timing of effects: We present 2SLS estimates based on the timing of exposure in Figure 3. The x-axis represents the number of years between the participation of applicants and neighbors in the

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[14]Lee et al. (2022) show that conducting inference based on t-ratios in single-variable just-identified IV models might lead to over-rejection and under-covered confidence intervals. They propose using an adjusted t-ratio depending on the value of the first-stage F-statistic and 2SLS estimates (tF critical values).

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

[16]Joensen and Nielsen (2018) find an increase of 7 percentage points in the likelihood of applying to the same math-science major as the older sibling from a pilot program in Denmark. Dustan (2018) finds an increase of 7 percentage points in the likelihood of applying to the same school in Mexico. Dahl et al. (2023) find that younger siblings are 2.4 percentage points more likely to choose the same high school major as their older sibling in Sweden.

centralized system. Each time period corresponds to a different sample where we link applicants and their closest neighbors applying to schools within that time difference. For example, $t = 1$ corresponds to our main estimates presented in Table 3, while $t = 2$ investigates the persistence of spillover effects by linking each applicant to their closest neighbor who participated in the centralized assignment system two years before. The remaining periods $t \in \{-2, -1, 0\}$ correspond to placebo tests where we match each applicant to their closest neighbor in t, $t + 1$, and $t + 2$, respectively, and test whether there is an effect of neighbors' enrollment on applications observed in the *same* or *previous* years.

We begin by discussing the results related to persistence. Panel A shows that the effect of applying to the same school in any rank two years later is 1.1 p.p. $(p > 0.1)$. Although the estimate is statistically indistinguishable from zero we cannot reject the null hypothesis of equal effects between $t = 1$ and $t = 2$ $(p = 0.538)$. In contrast, separate tests reject the hypothesis of equality of effects between $t = 1$ and $t = 0$ $(p = 0.013)$, $t = 1$ and $t = -1$ $(p = 0.061)$, and $t = 1$ and $t = -2$ $(p = 0.043)$. We reach similar conclusions when we estimate differences across time periods regarding the likelihood of ranking the reference school as top choice, as shown in Panel B. We also do not find evidence of persistent spillovers on enrollment. While the estimate for $t = 2$ displayed in panel C is 0.4 p.p. $(p > 0.1)$ two years later, we cannot rule out the equality of effects between $t = 1$ and $t = 2$ $(p = 0.208)$, but we reject the equality of effects between $t = 1$ and $t = 0$ $(p = 0.007)$, $t = 1$ and $t = -1$ $(p = 0.008)$, and $t = 1$ and $t = -2$ $(p = 0.083)$. The brevity of the spillover effect might be surprising; however, it is not an uncommon finding in other contexts. One potential reason is that age differences are an important determinant of the degree of interactions among neighbors. Following this intuition, Avdeev et al. (2024) focus on neighbors and lottery participants with at most a two-year age difference to analyze spillovers on post-secondary field of study, precisely to maximize the likelihood of them interacting with each other. Likewise, Barrios-Fernández (2022) finds stronger spillovers among neighbors with less than a one-year age difference.

Our placebo tests displayed in Figure 3 show that future or contemporaneous choices do not affect current behavior. Considering the timeline of the assignment process presented in Online Appendix Figure A.3, by the time neighbors receive an offer in November, their decision to accept or decline should not determine which schools applicants submit before October. As expected, estimates for $t = 0$ are of smaller magnitude than our main estimates and not statistically different from zero at the 10% level. However, it could still be possible that neighbors' offers affect attendance to the same school by prompting applicants to reject their initial assignment in the main round in hopes of securing a seat in the complementary round, which starts in December. We investigate this potential concern by estimating whether a neighbor's offer causes an applicant to accept their assignment or participate in the complementary round. Columns (3) and (4) in Online Appendix

Table A.4 demonstrate that this behavior does not occur. As a result, column (5) reports no effect of same-year or next-year neighbors' offers on school enrollment. Overall, the results of the placebo tests provide additional support to our identification strategy.

To summarize, our estimates show economically important effects relative to the baseline levels. On average, our results show that applicants are more likely to rank a school as their top choice and enroll in it when the closest neighbor is also enrolled. We next turn to examining spillovers employing a broader set of nearby neighbors.

4.2   Multiple Neighbors

In the previous section we focused on the effect of the school attended by the closest neighbor. However, this definition of treatment warrants consideration. First, since we observe distances with noise, the student identified in the sample presented in Table 3 might not coincide with the true closest neighbor. Second, this neighbor is potentially one among several members of each applicant's social network. One analogy corresponds to work in the job search literature related to the importance of residential neighbors (Bayer et al., 2008; Hellerstein et al., 2011; Schmutte, 2015), where it is assumed that one peer is an indirect proxy of each applicant's network.

To incorporate these issues into our analysis, we turn to the 0.1-mile sample. Our baseline specification considers the effect of exposure to at least one neighbor enrolled in the reference school, defined as the closest neighbor's top choice. Our choice of the treatment variable takes into account differences in the number of neighbors being admitted to their top-ranked choices and helps us compare our estimates to prior work (Avdeev et al., 2024). In the robustness section, we present our results using an alternative reference school, consisting of the modal top-ranked school among all neighbors linked to an applicant within the 0.1-mile radius.

Table 4 presents our results for this extended sample. Compared to the sample of closest neighbors, the number of observations decreases since the probability of being exposed to at least one neighbor enrolled in the reference school is larger, implying a higher number of applicants with a degenerate distribution of simulated counterfactual instruments z_i equal to 1. Columns (5)-(7) present very similar results compared to our 2SLS estimates displayed in Table 3. The probability of applying to the reference school in any rank increases by 1.7 p.p. (5%) and the probability of ranking it as the top alternative increases by 1.1 p.p. (8%). Column (7) shows that having at least one neighbor enrolled also translates into a positive effect of 0.9 p.p. on the likelihood of attending the same school (11%). For comparison, Avdeev et al. (2024) find a 0.4 p.p. increase in the probability

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of enrollment in a lottery post-secondary field of study when there is at least one older neighbor enrolled in the same field of study.

These results have two main takeaways. First, the fact that the estimates for both the closest-neighbor and 0.1-mile samples are very similar suggests that the social influence of neighbors operates at a very local level. The literature on neighbors' effects in other contexts is consistent with this claim. For example, Grinblatt et al. (2008) find that the effect of a neighbor's automobile purchase is strongest for the nearest neighbor and then declines sharply for subsequent neighbors.[17] Likewise, Avdeev et al. (2024) find very similar spillover effects between samples that consider only one lottery participant within a neighborhood versus samples that define treatment as exposure to at least one treated individual in the neighborhood.[18] Finally, Goux and Maurin (2007) use a similar argument to focus on geographical units of about 20 adjacent households in France to analyze the effect of the proportion of close neighbors being held back on grade progression at the end of junior high school.

Second, our results suggest that additional individuals in the network enrolling in the reference school do not increase spillover effects. One possible explanation is that once a school is made salient to the applicant, additional neighbors located at a greater distance from the applicant who choose the same school do not add new information about its characteristics. Thus, our results focusing only on the closest neighbor accurately capture the overall neighborhood effect. Alternatively, it is also plausible that spillovers are primarily determined by social connections (e.g., a close friend) rather than by geographical proximity. However, the validity of this hypothesis is challenging to evaluate without more detailed data including friendships or relationships among network members. We further explore the relevance of the number of neighbors enrolled in the reference school using a non-linear specification in section 4.4.

4.2.1 Heterogeneity by Distance

Prior studies have demonstrated that neighbors' social influence has a very local nature across a wide range of economic outcomes, such as the likelihood of engaging in criminal behavior (Bacher-Hicks et al., 2019), changes in consumption behavior (Grinblatt et al., 2008; Agarwal et al., 2021), or enrolling in a university (Barrios-Fernández, 2022). We analyze whether this pattern is also observed in our context by estimating neighbors' effects—measured by the number of neighbors

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[17]In their study, treatment varies by neighbors' geographical proximity and the timing of the purchase. They report variation in their estimates binned by time × distance rank combinations.

[18]In their study, a neighborhood is defined as a geographical unit known as *buurtcode*. On average, this geographical unit consists of 1,400 individuals.

enrolled within a given distance—as we move farther from each applicant.

To conduct this analysis, we extend the sample to link each applicant to all neighbors located within 0.8 miles and categorize them into four equally spaced distance groups. Then, we estimate equations (8) and (9) using the total number of neighbors enrolled in the reference school within 0.2-miles bins as the endogenous variable. Consistent with this definition, the instrument z_i corresponds to the number of neighbors within each bin who receive an offer at the reference school. Its expected value, λ_i, corresponds to the average value of z_i across simulations.

Figure 4 presents our estimates by distance category. In this exercise, β is interpreted as the average effect of exposure to an additional neighbor enrolling at the reference school within each distance group. For neighbors located within 0.2 miles, the average effect of an additional enrolled neighbor results in approximately a 0.5 p.p. increase for each of the outcomes of interest. As the figure shows, estimates for the remaining groups drop and are not statistically different from zero. We formally test the heterogeneity across the four distance groups using a joint test of equality of estimates. We obtain p-values of 0.099 for applying to the reference school, 0.029 for applying as a top-ranked choice, and 0.115 for enrolling in the reference school. We conclude that, as expected, the influence of neighbors is very local and diminishes quickly with distance. Moreover, their magnitudes resemble spillover effects found in other contexts. For example, Barrios-Fernández (2022) finds that only neighbors located closer than 0.12 miles contribute to explain spillovers on college enrollment.

4.3 Additional Results

4.3.1 Effects on the Types of Schools Attended by Applicants

Our previous sections demonstrate that applicants follow neighbors to their preferred schools. In this section, we examine the implications of this behavior on additional dimensions of school choice. Specifically, we investigate whether mimicking choices implies a causal relationship between the characteristics of schools attended by neighbors and those chosen by applicants. Analyzing how spillovers affect this margin is relevant from a number of reasons. Firstly, similar to trends observed in other countries, disadvantaged students in Chile enroll in less effective schools. We describe this pattern in Online Appendix Table A.5, where we estimate the association between socioeconomic (SES) status and the average performance of the schools they select, using simple OLS regressions. Low-SES families apply to fewer schools and choose those that, on average, perform between 0.206 and 0.211σ lower in standardized tests compared to economically more advantaged applicants with similar characteristics. The gap increases to $0.271\text{-}0.278\sigma$ when we analyze their top-ranked choices.

Secondly, prior work has demonstrated the multidimensional nature of school quality and its relevance to several outcomes that extend beyond test score gains (Jackson et al., 2020; Beuermann et al., 2023). As such, neighbors might exert an indirect influence on applicants' future trajectories by shaping the characteristics of the schools they attend.

We analyze this relationship leveraging the same source of variation used to identify spillovers on application and enrollment decisions, focusing instead on school attributes as the objects of interest. Formally, we estimate the following specification using 2SLS:

$$w_i = \alpha + \beta q_i + \phi \lambda_i + \epsilon_i \tag{10}$$

$$q_i = \kappa + \rho q_i^z + \pi \lambda_i + \eta_i \tag{11}$$

The dependent variable, w_i, corresponds to one of the following attributes of the school in which the applicant enrolls: (i) average tenth-grade standardized test scores, (ii) school value-added on high school graduation, (iii) school value-added on college enrollment, and (iv) school climate. Each variable is standardized to have mean zero and standard deviation one including all schools in the country. The endogenous variable q_i is the characteristic of the school attended by the closest neighbor. We instrument this variable using the same attribute from the school where they are assigned, which we label q_i^z. Similar to our main specification, λ_i corresponds to the expected value of the instrument, computed using simulation.[19] We present a validation exercise of the association between the simulated and observed values of the instruments q_i^z in Online Appendix Figure A.9. As in the case of the binary instruments, our simulation replicates almost exactly the observed school characteristics. Finally, we interpret β as the change in the attributes of an applicant's top-ranked and attended school when the closest neighbor enrolls in a school that is 1σ above the mean in the same attribute distribution.

Table 5 shows our results. The sample sizes differ compared to our main results due to missing values in the attributes of some schools. Column (1) reports estimates of the first-stage coefficient ρ from equation (11). Since receiving an offer increases attendance significantly (69 p.p. as shown in Table 3), there is a strong relationship between the characteristics of the schools where neighbors are assigned and later enroll. Columns (3) and (4) display the ITT and 2SLS estimates of spillover effects on school characteristics for each applicant's target school. With the exception of average tenth-grade test scores, our results suggest that neighbors have a positive spillover on school attributes, but these estimates are small and none are statistically distinguishable from zero. For

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[19]Specifically, in equations (10)-(11), λ_i corresponds to:

$$\lambda_i = \mathbb{E}(q_i^z | \Theta, \mathcal{N}_i)$$

example, our 2SLS estimates indicate that a neighbor attending a school that is 1σ above the mean in the college enrollment value-added distribution induces an increase of 0.019σ in the applicant's target school. The last row shows a positive spillover of a similar magnitude on the school climate index. Conversely, our estimates for high school graduation value-added and average test scores do not provide evidence of a causal link between the attributes of the schools in which the closest neighbor enrolls and the applicant ranks as top choice. Finally, columns (6) and (7) display ITT and 2SLS estimates of the effects on the characteristics of schools where applicants ultimately enroll. For this outcome, we find a positive and statistically significant spillover of 0.028σ on school climate. The remaining attributes are not statistically significant at conventional levels.

These findings suggest that neighbor spillovers on school quality are at best modest. One potential explanation is that although applicants are more likely to follow neighbors when they enroll in their top-ranked choices, the counterfactual school they could attend does not differ substantially in terms of quality. However, we find statistically significant spillovers on school climate, suggesting that applicants may prioritize non-academic attributes over more traditional proxies of school quality, such as average standardized test scores or value-added. Next, we analyze the impact of these changes on the short-term academic performance of applicants to assess whether neighbor spillover effects result in any improvements on this margin.

4.3.2   Effects on Short-Term Academic Outcomes

As a result of spillovers in enrollment, neighbors might also indirectly influence academic outcomes. In our data, we can track applicants' GPA and grade progression in ninth and tenth grades. We also observe standardized test scores for tenth-grade math and language in 2022 and 2023, corresponding to the 2020 and 2021 application cohorts. These various outcomes provide a picture of the potential short-term impacts of neighbors' school characteristics on applicants' outcomes. To analyze this margin, we estimate equations (10) and (11) using applicants' academic outcomes as the dependent variable.

Table 6 presents 2SLS estimates of spillovers on ninth- and tenth-grade outcomes using the closest-neighbor sample. Overall, we find some positive effects on applicants' performance in ninth grade, although we lack statistical precision to rule out non-zero effects in most cases. For example, panel A shows that neighbors who attend a school 1σ above the mean in the test score value-added distribution induce an increase of 0.6 p.p. in the likelihood of promotion for applicants who participate in the centralized system in the next round. Similarly, panels B and C show that neighbors who attend schools 1σ above the mean in the college enrollment and high school graduation value-added distribution improve applicants' ninth-grade GPA by 0.12 points and the likelihood of promotion

by 11 p.p., respectively. In columns (5) and (6) we present evidence that also suggests improvements in tenth-grade standardized test scores, with the caveat that these estimates consider only two cohorts of applicants. Neighbors enrolling in schools 1σ above the average in the test score value-added and school climate distribution induce gains of around 0.024-0.028σ in language test scores. We do not find evidence of improvements in math test scores on any of the dimensions of school quality we analyze.

Our results are broadly consistent with the absence of spillover effects in school characteristics described in Table 5. At the same time, the absence of strong evidence in favor of improvements in academic outcomes echoes other studies of the causal effect of attending students' preferred schools. For example, Beuermann and Jackson (2022) show that attending sought-after schools does not improve test scores across several countries and educational systems. However, it is important to highlight that these short-term effects on academic outcomes might not fully capture school effects in other dimensions (Cullen et al., 2006; Deming, 2011). Unfortunately, we cannot evaluate spillover effects on a broader range of outcomes where attending a better high school might be relevant, such as teenage parenthood or involvement in criminal activity. The relationship between neighbor spillovers, school quality, and non-academic outcomes is an important direction for future research.

4.4   Non-linear Spillovers

Our main results can be rationalized by different explanations. One is that once the reference school is made salient to the applicant, additional neighbors attending that school are less relevant. This channel is consistent with informal networks serving as a source of information when search or decision-making is costly or there are information frictions for parents. The available evidence in the school choice literature has consistently shown that parents lack full information about school attributes, making informal networks relevant.[20] If salience is the main driver of our results, we should expect that, conditional on exposure to one neighbor enrolled in the reference school, additional ones do not increase the likelihood of applying to that school.

Alternatively, if the marginal effect of exposure to enrolled neighbors is constant, this would suggest that applicants derive higher utility from attending the same school with a larger number of neighbors. For example, families could apply to the same schools attended by other residential

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

neighbors to improve school-parent communication, avoid exposure to crime if the routes to school are unsafe, or simply be part of the same community. Lastly, findings from the siblings' effects literature (Goodman et al., 2015; Altmejd et al., 2021; Aguirre and Matta, 2021) convey the idea that there could be intrinsic value in following a sibling's path by enrolling in a particular college or major. This mechanism might also explain why applicants are more likely to follow neighbors.

To examine the existence of nonlinearities, we focus on the number of close neighbors enrolling in the reference school within 0.1 miles. We employ an extended specification in which we first estimate the average spillover effect, and then allow it to vary depending on whether the applicant is exposed to at least one or at least two neighbors enrolled in the 0.1-mile vicinity. To make our estimates comparable across specifications, we restrict the sample to applicants with an expected probability that at least two neighbors receive an offer strictly between 0 and 1. Table 7 presents our findings.

Panel A presents the results employing the reference school from our baseline analysis (i.e., the closest neighbor's top-ranked school). Column (1) shows that, on average, an additional neighbor enrolled at the reference school increases the probability of that school being included in applicants' preference list by 1.2 p.p. (3.4%). In columns (2) and (3), we decompose this estimate by examining the exposure effect of having at least one and at least two neighbors enrolled. The comparison between these two columns shows that the enrollment of the first neighbor is more important in increasing the probability of applying to the same school. Column (3) shows that, conditional on exposure to one neighbor enrolled, each additional neighbor has a marginal effect of 1.5 p.p. (4.3%). However, this estimate is imprecise and not statistically different from zero at conventional significance levels.

The pattern becomes clearer when we examine the probability of selecting the reference school as the top choice. While the average effect displayed in column (4) is indistinguishable from zero, columns (5) and (6) show that it is the first neighbor that drives this effect. Column (6) indicates that exposure to one neighbor enrolled in the reference school increases the probability of ranking the same school as the top choice by 1.6 p.p. (10%). Additional neighbors enrolled in the same school have a minimal additional effect of 0.1 p.p. (0.6%). Columns (7)-(9) exhibit a similar pattern, although the estimates become noisy. In this case, exposure to at least one neighbor enrolled increases the probability of enrollment by 1.2 p.p. (7.8%), but after observing one neighbor enrolled, additional peers increase that estimate by 0.3 p.p.

Panel B presents a similar analysis defining the reference school as the modal school among all neighbors within the 0.1-mile radius. In this case, we continue observing many of the patterns

found in panel A. The enrollment of the first neighbor accounts for most of the average spillover effects observed in the two application outcomes. However, when we examine the modal school among neighbors, we do not find evidence of spillover effects, either on average or when allowing the effect to vary depending on the number of neighbors enrolled.

Taken together, this analysis suggests that spillover effects are more consistent with the hypothesis of salience driven by one member of the network attending the reference school, reflecting the existence of search costs or application frictions for families. The available evidence suggests that both are present in the Chilean school assignment system. For example, Arteaga et al. (2022) find that (i) the search process is costly in terms of the steps required to acquire information about schools and (ii) families have limited knowledge about the options they submit.[21] At the same time, previous research documents small changes in school segregation and the proportion of vulnerable students across schools before and after the reform was implemented (Kutscher et al., 2023; Honey and Carrasco, 2023), suggesting that other structural factors might also play a role in determining spillover effects among neighbors in Chile.

4.5 Heterogeneous Spillovers

In this section we examine whether neighbor spillovers vary across applicant, neighbor, and school characteristics. To conduct these analyses, we estimate equations (8)-(9) adding interaction terms with specific attributes.

4.5.1 Heterogeneity by Applicant and Neighbor Characteristics

The literature on spillover effects often finds important differences in treatment effects across subgroups of individuals. For example, the literature focusing on siblings' effects on college and major choice shows that spillovers decrease with the age gap but reports mixed results regarding differences by gender (Goodman et al., 2015; Joensen and Nielsen, 2018; Aguirre and Matta, 2021; Altmejd et al., 2021). In the case of neighbor spillovers in higher education, Avdeev et al. (2024) find no differences by family income or GPA scores on the likelihood of choosing the same field of study, while Barrios-Fernández (2022) shows that spillovers on college enrollment are stronger for applicant-neighbor pairs who are similar in terms of socioeconomic status and age.

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

We begin by considering heterogeneous effects by background characteristics in our closest-neighbor sample, which we report in Table 8. We probe for effect heterogeneity by gender, socioeconomic status, and academic performance—measured by the average math-language score in standardized tests taken in prior grades. Differences in applicant characteristics are statistically insignificant at conventional levels for each of the key outcomes. Panel A presents small estimates of the interaction term for gender, SES status, or past achievement. Similarly, panel B of Table 8 displays estimates of heterogeneity by the closest neighbor's same characteristics. In this case, there is some evidence of stronger spillovers when the neighbor is female. However, the estimates of the interaction terms are imprecise and we cannot rule out that they are statistically different from zero. Combined with the insignificant differences in Panel A, these results suggest that spillovers are not determined by one specific group of the population. Instead, they are similar across different types of students who use the centralized system to apply to public schools.

We conduct additional exercises to assess whether pairs who share the same characteristics display larger effects, although in most cases we lack statistical power to find meaningful differences across subgroups. Online Appendix Table A.6 shows no evidence of differences in spillover effects for the subgroup of applicants and neighbors with similar gender or SES status. We find evidence of stronger spillovers for the subgroup of students scoring below the average in national standardized exams. Panel C of Online Appendix Table A.6 indicates a spillover effect of 0.053 p.p. ($p < 0.01$) and 0.023 p.p. ($p < 0.05$) on the likelihood of applying to the same school and ranking it as top choice, respectively. For enrollment, the point estimate is 0.02 p.p. ($p < 0.05$); however we cannot rule out that spillover effects are statistically different from the rest of the population. Finally, panel D shows heterogeneity based on whether applicant and neighbor both attended the same school in eighth-grade. As before, the magnitude of the interaction term is suggestive of stronger effects for this subgroup, but the point estimates are too imprecise to draw any informative conclusion. In Online Appendix Table A.7 we report differences by applicant background characteristics using the 0.1-mile sample. While this sample considers a different treatment status and set of relevant neighbors, our analysis leads to very similar results compared to Table 8. In particular, none of the differences across subgroups is statistically significant at conventional levels.

4.5.2 Heterogeneity by Neighborhood Characteristics

In addition to background characteristics, variation in neighborhood attributes might induce heterogeneity in spillover effects. Prior work studying neighbor spillovers in other economic outcomes has found that environments that foster or facilitate social interactions are a key determinant of the strength of these effects. For example, Bayer et al. (2008) find stronger job referral effects in denser neighborhoods and Glaeser et al. (1996) find that the degree of social interactions explains

a significant portion of the variation in crime rates across cities.

To conduct this analysis, we use supplementary information about amenities available at the sub-municipal level, in geographical units known as *unidades vecinales* or neighborhood units. These units were defined by the Ministry of Social Development and group together one or more neighborhoods sharing common territorial, social, and urban characteristics.[22] The median unit comprises around 700 households and an area of 0.66 square miles. The supplementary data includes rich information about population, housing characteristics, and access to services, such as schools, recreation centers, pharmacies, and grocery stores.[23] We observe valid information for 3,558 *unidades vecinales* from 109 municipalities. We can link this information to approximately 93% of the applicants considered in our closest-neighbor sample of Table 3.

We find that neighborhood characteristics—such as housing-to-population ratio, accessible recreational areas, and availability of public services—explain the presence of spillover effects. We arrive at this result in Table 9, which reports estimates of heterogeneous spillovers across four neighborhood amenities. Each attribute is centered around its mean and the corresponding measurement unit is described at the top of each panel. Panel A investigates differences by housing units per-capita in each neighborhood unit. The point estimates show that communities characterized by a larger number of houses and apartments—adjusted by population size—increase spillover effects. Applicants living in communities with one additional house or apartment per-resident above the mean are 0.32 and 0.2 p.p. more likely to rank the reference school as their top choice, respectively.

Panels B and C present estimates based on variation in the availability of urban green spaces—measured in square yards per inhabitant—and the per-capita number of public and private recreation centers, respectively. Neighborhood units with a greater availability of community parks, playgrounds, and similar spaces exhibit larger spillover effects. Similarly, a higher number of gyms, public sport centers, or sport fields, also contributes to stronger effects. Both panels indicate that these amenities, which facilitate interaction among community members, increase the magnitude of spillovers on application decisions. For example, the results in panel C show that an additional recreational center per hundred inhabitants boosts the likelihood of ranking the reference school by 0.06 percentage points. For enrollment, we do not find evidence of heterogeneity, likely due to congestion effects in denser areas.

Finally, in panel D we employ a summary index that incorporates several services and amenities available in each neighborhood unit. Specifically, we consider the following variables, adjusted by

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[22]The information is available at http://datovecino.gob.cl
[23]Unfortunately, we do not have information about the area covered by each unit.

population size: (i) park areas, (ii) number of public transportation stops, (iii) number of primary care centers, (iv) number of recreational centers, and (v) number of grocery stores. The index is calculated using principal component analysis to derive a single factor from all attributes. We then standardize the index to have mean zero and standard deviation one. The positive and statistically significant estimate of the interaction term for the application outcomes is consistent with the hypothesis of stronger spillovers in communities where social interactions are more likely to occur.

4.5.3 Heterogeneity by School Characteristics

In this section, we investigate whether spillovers also depend on the characteristics of the school neighbors choose. Following recent evidence about parental preferences in the school choice literature (Burgess et al., 2015; Abdulkadiroğlu et al., 2017; Beuermann et al., 2023; Ainsworth et al., 2023), we study heterogeneity along the following dimensions: (i) distance to school, (ii) school value-added on tenth-grade test scores, (iii) school value-added on college enrollment, and (iv) school climate. To conduct this analysis, we focus on the school ranked as top-choice by the closest neighbor in the previous round.

To characterize schools across these attributes, we employ data between 2015 and 2018. We construct school value-added using information from the cohorts of tenth-graders linked to post-secondary enrollment records and prior standardized tests. We discuss our estimation approach and the distribution of school value-added in Online Appendix A.2. Additionally, we employ a school climate index created by the Ministry of Education. This index uses student, teacher, and parental surveys to capture attitudes and perceptions about non-academic dimensions of schools across three dimensions: (i) respectful relationships between school members, (ii) rules, norms, and conflict resolution protocols, and (iii) school safety.[24] While there is not a unique definition of school climate (Thapa et al., 2013), prior work has shown that indexes based on student and teacher surveys are strong predictors of school effectiveness (Porter et al., 2023) and that providing this sort of information to families affects residential sorting (Crespin, 2025). We standardize the index to have mean zero and standard deviation one every year. The distribution of school value-added and school climate consider public and private schools so that these proxies of effectiveness capture differences across all high schools in the country. Online Appendix Figure A.10 shows that schools with higher indexes are more demanded. Each index strongly associates with the number of top-ranked submissions per vacancy.

We estimate equations (8)-(9) employing each school attribute as an interaction term. We report

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[24]Specifically, this index uses the students, teachers, and parents questionnaires included in the SIMCE examinations to create a school-grade score using a 0-100 scale, where values closer to 100 reflect more positive perceptions.

the main estimates and interaction terms in Table 10, separately by attribute. Panel A shows that distance to school attenuates the importance of spillovers. The probability of applying to the reference school and enrolling in it decreases by 0.6 and 0.4 p.p., respectively, when the school where the closest neighbor is assigned locates 1 additional mile farther from the applicant. Since the median distance to this school is 1.2 miles, these estimates imply that the spillover effect decays completely for schools located around 4 to 6 miles away from the applicant.

Panels B and C analyze whether applicants are more likely to mimic choices when schools are characterized by higher effectiveness—proxied by test score and college enrollment value-added. We find mixed evidence about the importance of school effectiveness. On the one hand, we do not find evidence of heterogeneity based on test scores value-added. Since this information is not available to families, this result could be interpreted as evidence against an explanation based on parents learning from neighbors' previous experiences. On the other hand, panel C shows that applicants are more likely to consider neighbors' schools when their contribution to college enrollment is higher, which suggests that parents might be more responsive to other traits beyond test score gains.

Finally, panel D shows that spillovers do not increase for schools scoring higher in the climate index. Interestingly, since this composite is constructed using parents' perceptions about school experiences, a positive interaction term would suggest that neighbors transmit information that is harder to acquire for parents and might influence their choices. However, our results do not support this claim. Instead, the mixed patterns are more indicative of families prioritizing distance and dimensions of school quality not necessarily related to test score gains (Ainsworth et al., 2023). Furthermore, our finding that families do respond to some school traits is somewhat different to the existing evidence in the siblings' effects literature. For example, Altmejd et al. (2021) find that an older sibling's admission to their target college-major increases the probability that the younger sibling applies to the same college, independent of the quality of the older sibling's target. This contrast suggests that neighbor spillovers are not purely determined by preferences for sharing the same environment. Although there could be utility gains from attending the same school as nearby neighbors—for example by improving school-parent communication, avoiding exposure to crime if school surroundings are unsafe, or sharing commuting costs—these are not the primary determinants of the spillovers we document.

4.6 Robustness Checks

In this section we test the robustness of our main results to the choices of the key elements of the estimation strategy. Concretely, we examine whether our results hold when we consider alternative choices of the reference school, econometric specification, and sample.

Alternative Reference School: We replicate our main results from Table 4 using a different definition of the reference school. For each applicant, we employ the modal top-ranked school among all neighbors located within 0.1 miles. Online Appendix Table A.8 indicates that our estimates of spillover effects are similar when we consider this alternative definition. Columns (6) and (7) show that the effect of exposure to at least one student enrolling in the modal top-ranked school increases the likelihood of application by 2 p.p. and 1.1 p.p., respectively. Our estimate for enrollment is almost exact to the one displayed in Table 4 but less precise.

Alternative Specifications: Our baseline estimation in equations (8)-(9) controls linearly for λ_i. We test whether our results change when we estimate neighbor spillovers using: (i) a saturated set of fixed effects for λ_i, (ii) a quadratic polynomial in λ_i, and (iii) a recentered instrument where we control for deviations from the expected treatment using $\lambda_i - z_i$. Online Appendix Table A.9 presents our main results for the closest-neighbor and 0.1-mile samples. As shown, all specifications yield almost identical estimates.

Alternative Sample: Finally, we evaluate how our results change when we restrict the sample to applicants and neighbors who were enrolled in K-8 schools and therefore were forced to enroll in a different school. Notice that this sample excludes individuals enrolled in K-12 schools who decided to switch,x even though they could have remained at their current school. Online Appendix Table A.10 replicates our main analysis using this restricted sample. We find slightly smaller differences in magnitude compared to the benchmark findings of Table 3, although our estimates for the likelihood of applying at any rank are no longer statistically significant at the 10% level. Conversely, the estimates presented in columns (6) and (7) are practically unchanged.

To summarize, these additional exercises reveal a robust pattern of neighbor spillover effects for different choices made in our estimation.

5 Discussion and Conclusion

In this paper, we investigate the influence of nearby neighbors on school application and enrollment decisions. We utilize comprehensive administrative data from Chile's centralized school admission system, spanning application cohorts from years 2019 to 2022. The large number of oversubscribed schools in ninth grade and the features of the centralized assignment system motivate an instrumental variables design, enabling us to identify causal effects. We apply the framework of Borusyak and Hull (2023) to exploit variation in shocks to the expected assignment of nearby neighbors in their most preferred schools in the prior year. To our knowledge, no prior research has examined this type of spillover effects within centralized school systems.

Our findings indicate that shocks to nearby neighbors' enrollment spillover to applicants' school applications and enrollment decisions in the next round. On average, when the nearest neighbor enrolls in their most preferred school, applicants are more likely to rank the same school as their top choice and attend it by 1.4 and 1.2 p.p., respectively. These figures represent increases of 9% and 12% relative to the non-treated rates. We find similar effects when we consider all neighbors located within 0.1 miles and estimate the impact of exposure to at least one peer enrolled in the nearest neighbor's top choice (1.1 p.p. and 0.9 p.p., respectively). The fact that our estimates do not vary substantially after expanding the pool of peers is consistent with the idea that only neighbors who reside in very close proximity drive our estimated effects. We corroborate this result by estimating a nonlinear specification that decomposes the average effect of an additional neighbor enrolled. We find that, conditional on exposure to one neighbor enrolled in the reference school, additional peers attending that school do not substantially increase their likelihood of applying to it.

We provide evidence that social interactions are the primary channel through which neighbor spillovers operate. First, we document the local nature of these effects, aligning with prior work on neighbors' effects on educational outcomes (Goux and Maurin, 2007; Barrios-Fernández, 2022; Avdeev et al., 2024). The importance of the number of nearby neighbors enrolling in the reference school decays rapidly for those located farther than 0.2 miles from the applicant. Second, we find that these effects are short-lived, suggesting that stronger connections—likely characterized by age proximity—are more relevant. We find that our main estimates based on the closest neighbor's decisions reduce by half when we consider a neighbor who participated in the centralized system two years earlier. Third, a novelty of our analysis is showing that neighborhood characteristics play a key role in shaping spillover effects. Communities with improved access to green spaces and recreation centers exhibit larger effects, reinforcing the idea that environments that facilitate informal interactions lead to greater spillover effects.

We conduct several analyses to shed light on the potential mechanisms behind the spillover effects we document. For the most part, our results primarily suggest that spillovers result from applicants reducing search and decision-making costs. However, we cannot fully rule out the possibility that they also are a consequence of information transmission. On the one hand, our results that spillovers diminish with distance to the school where the closest neighbor was assigned suggest that parents do not always consider neighbors when selecting the schools to which they apply. On the other hand, we find that applicants are more likely to follow neighbors based on specific school attributes, such as college enrollment value-added, which is consistent with information transmission. Since this kind of attribute is not readily available, we interpret this result as evidence of the use of informal channels to learn about school characteristics. Simultaneously, the lack of heterogeneous effects observed for other traits associated with academic quality or school climate suggest that families might value a mix of school characteristics and trade-off test score gains over other characteristics (Ainsworth et al., 2023). Of course, it might very well be that families value other school traits that we are not able to fully capture in our characterization. Lastly, our analysis does not support the hypothesis of utility gains from applying to the same school attended by nearby neighbors. Our findings indicate that one enrolled neighbor in the vicinity is sufficient to drive the overall spillover effect and that additional peers do not increase the probability of mimicking choices.

Beyond spillovers on application and enrollment decisions, we do not find conclusive evidence of neighbor effects on applicants' GPA, grade progression, or tenth-grade standardized test scores. This is likely a consequence of the uniformly small estimated spillover effects on school attributes. However, we caution against interpreting these estimates as evidence of the absence of effects on applicants' outcomes. On the one hand, our finding of positive spillovers on school climate might induce improvements in noncognitive skills, which are an important mediator of long-term effects (Heckman et al., 2013). On the other hand, the lack of positive effects is broadly consistent with prior work showing that attending a sought-after school does not improve short-term outcomes (Cullen et al., 2006; Beuermann and Jackson, 2022). Future work could expand the set of outcomes we analyze to incorporate non-academic dimensions, such as teenage parenthood, involvement with the criminal justice system, and others.

Taken together, our results provide new insights into the importance of the indirect effects of policies designed to increase the number of schools available to families—such as school choice programs, centralized systems, or information interventions. While we confirm that neighbors' choices induce changes in behavior for families, our findings indicate that spillover effects stemming from social networks are meaningful, but their persistence is limited. At the same time, families' preferences for distance play a crucial role in determining the extent of these effects. We document a rapid decay in spillover effects as the distance to the school where the closest neighbor was assigned

increases. All in all, the indirect benefits of school choice interventions could be constrained by the existing supply of schools located in the neighborhoods where these policies are implemented. These considerations are relevant when evaluating the cost-effectiveness of reallocation versus resource augmentation policies to improve school systems (Abdulkadiroğlu et al., 2025).

Finally, we acknowledge two significant limitations of our data that affect our ability to explore mechanisms in greater detail. First, we do not observe school preferences before exposure to neighbors' influence. This limitation prevents us from separately identifying effects on the composition of choice sets, changes in preferences, or a combination of both. Second, we lack data on the level of proximity or friendship between applicant-neighbor pairs. As a consequence, we cannot evaluate whether closer relationships are more relevant. Future research could explore these channels further to deepen our understanding of spillover effects among residential neighbors. Such information would be valuable to leverage the indirect effects of targeted interventions to improve the allocation of educational investments.

6   Figures and Tables

Figure 1: Implementation of the Centralized School Choice System

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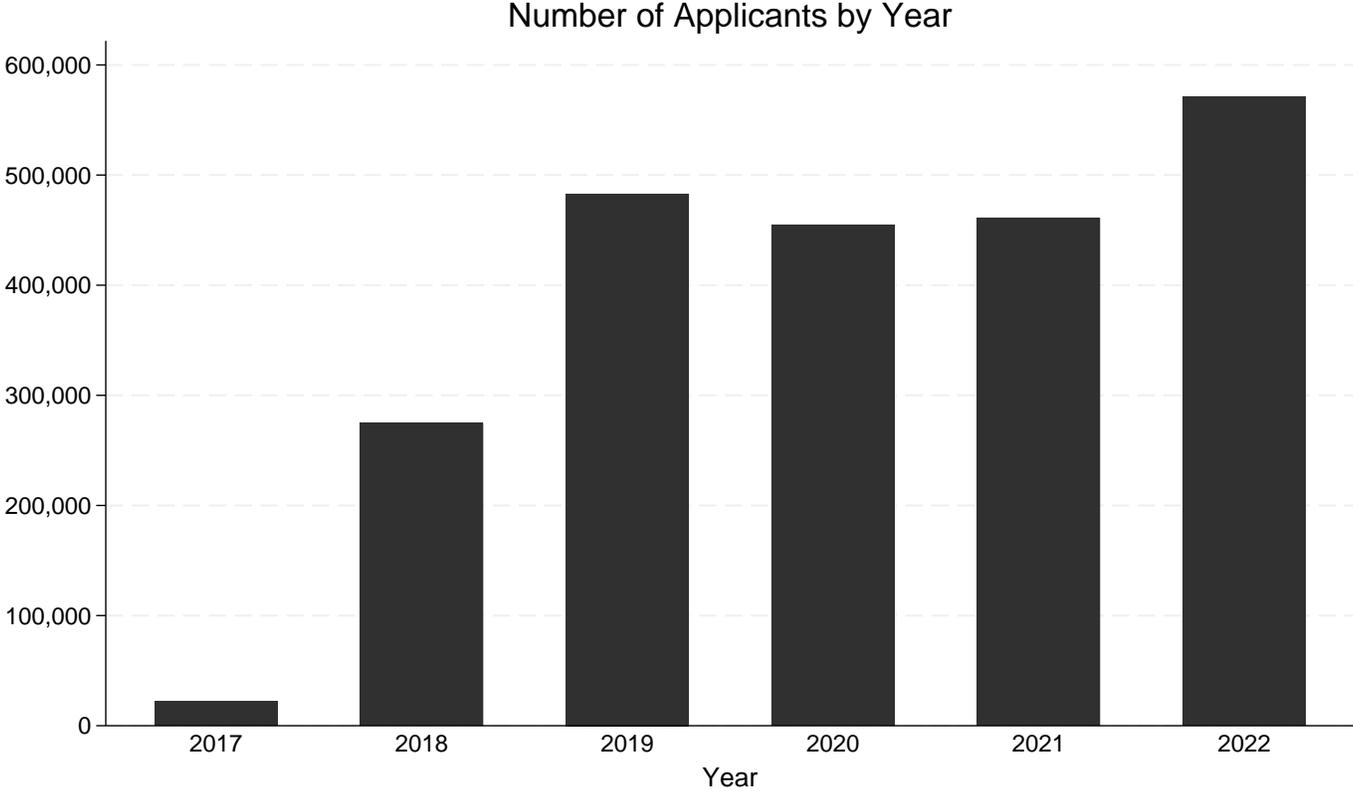
Notes: This plot shows the number of applicants observed in the centralized system between 2017 and 2022. Rollout was staggered across regions and grades. Starting in 2017, each year a new set of regions was incorporated to the system. By 2019, the centralized admission system is used for admission to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

Figure 2: Oversubscribed Schools

Share of Oversubscribed Schools by Grade (2019-2022)
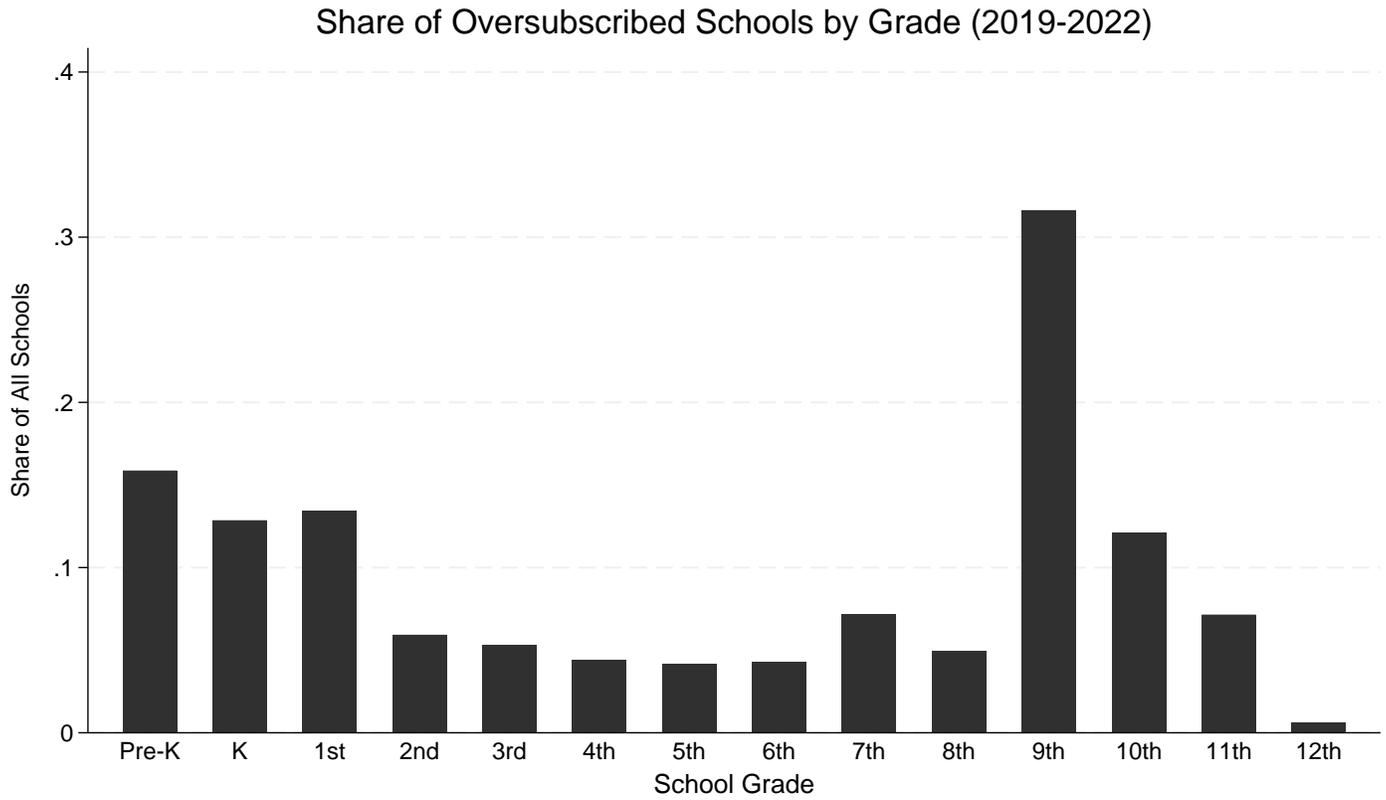
Notes: This plot shows the share of schools where the number of applicants submitting the school as first option surmounts the number of vacant seats in the corresponding grade. The share is computed pooling the 2019-22 application rounds.

Figure 3: Impacts of Neighbors From Different Time Horizons: Separate 2SLS Estimates
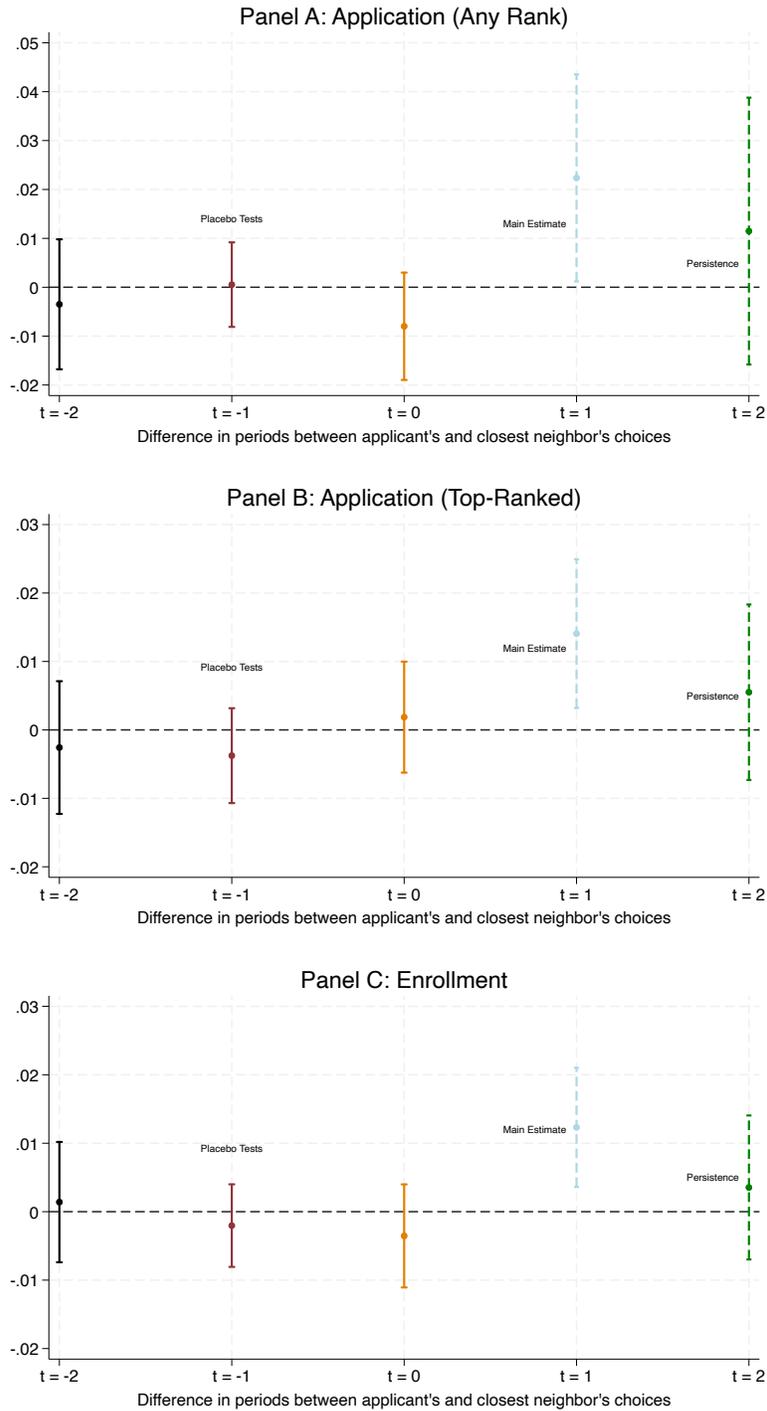
Notes: This figure illustrates how spillover effects vary depending on the number of periods used to construct applicant-closest neighbor pairs. Each plot reports separate 2SLS estimates for each sample and outcome using our baseline equations (8) and (9). Panels A and B present estimates for an indicator that equals if the applicant ranks the same school as their closest neighbor, either in any preference (panel A) or as their top-ranked choice (panel B). In panel C the outcome is an indicator equal to one if the applicant attends the same school as their closest neighbor. Neighbor's enrollment is instrumented with an indicator that equals one if the neighbor is assigned to their top-ranked school. All models control for the probability of each neighbor being assigned to their target school.

Figure 4: Heterogeneous Neighbor Spillovers: by Distance
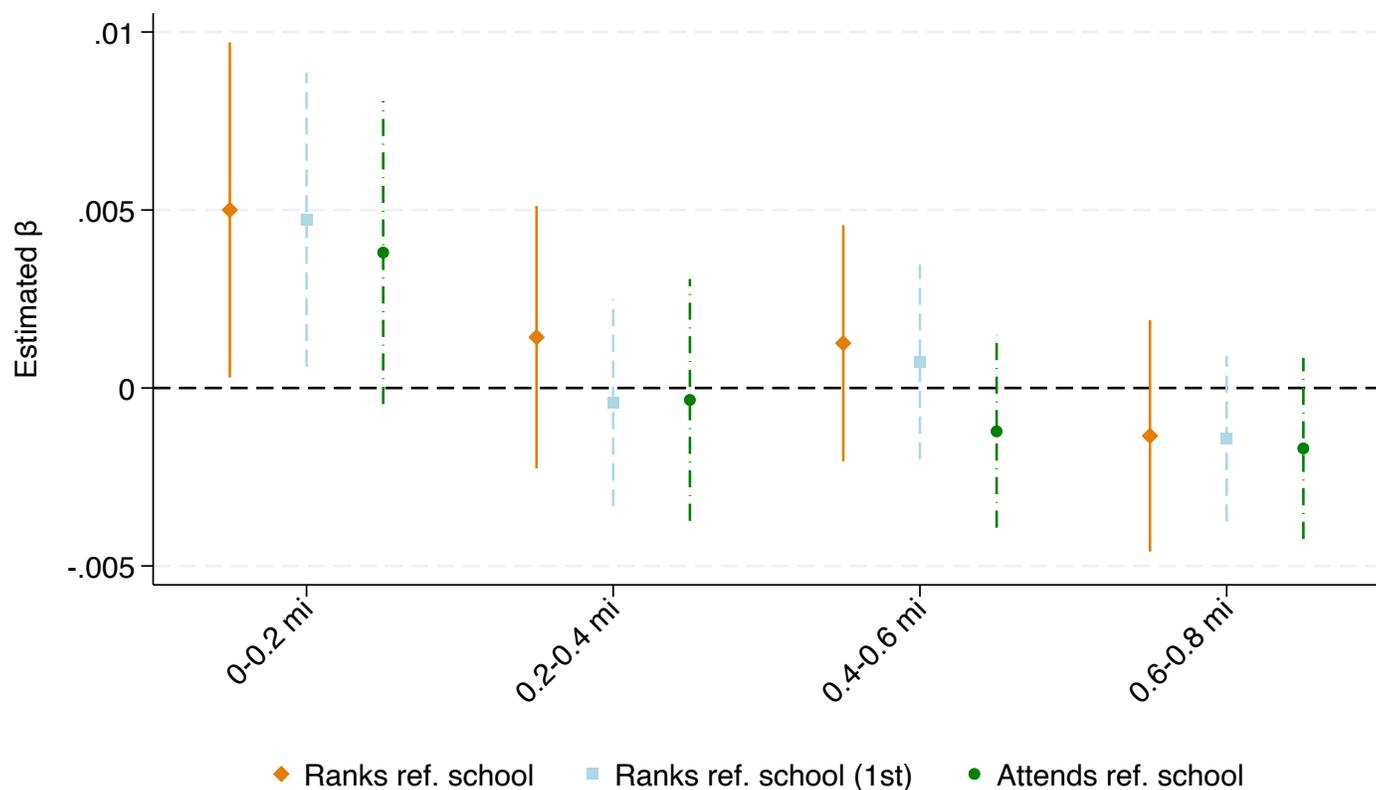
Notes: This figure shows how spillover effects vary with the distance between each applicant and their nearby neighbors. The sample consists of all applicants linked to neighbors who participated in the school assignment system in the prior year and are located within a 0.8-miles radius. We estimate equations (8) and (9) using the number of neighbors enrolled at the closest neighbor's top choice as the endogenous variable conditioning on the number of expected neighbors who are assigned to the reference school. The endogenous variable is instrumented with the number of neighbors who received an offer at the reference school. Standard errors are clustered at the reference-school $\times$ applicant-priority $\times$ year level. The p-values of a test of equality of estimates across distance groups is 0.099, 0.029, and 0.115 for applying to the reference school, applying as top choice, and attending, respectively.

Table 1: Summary Statistics

	All 8th-Grade Students (1)	All 8th-Grade in K-8 Schools (2)	All Applicants (3)	Estimation Sample (4)
Panel A: Background Characteristics				
Girl	0.512	0.532	0.504	0.529
Prioritario	0.529	0.639	0.610	0.630
Preferente	0.294	0.250	0.265	0.266
Public School	0.382	0.599	0.545	0.594
SIMCE (Math)	-0.037	-0.266	-0.210	-0.250
SIMCE (Language)	-0.022	-0.194	-0.152	-0.181
Missed SIMCE	0.156	0.193	0.178	0.177
Father Education: College	0.278	0.159	0.187	0.164
Father Education: Less than HS	0.673	0.784	0.758	0.780
Mother Education: College	0.308	0.186	0.215	0.187
Mother Education: Less than HS	0.673	0.791	0.763	0.790
College Expectations	0.756	0.671	0.692	0.677
Family Income >CLP800,000	0.185	0.088	0.111	0.089
Panel B: Application Characteristics				
Number of applications		3.473	3.530	3.612
Submits one school		0.030	0.026	0.015
Submits two schools		0.280	0.280	0.253
Submits three schools		0.336	0.318	0.340
Submits four schools or more		0.354	0.376	0.391
Observations	840,755	367,045	410,412	150,337

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

Table 2: Balance Tests

Variable	Average		Difference	p-value	Observations	
	Offered (1)	Non-offered (2)	(3)	(4)	Offered (5)	Non-offered (6)
Panel A: Applicant Covariates						
Girl	0.474	0.467	0.006*	0.080	58,225	69,860
Prioritario	0.654	0.657	−0.003	0.387	58,225	69,860
High Achiever	0.301	0.301	−0.000	0.945	58,225	69,860
SIMCE (Math)	-0.263	-0.248	−0.015*	0.056	46,716	55,538
SIMCE (Language)	-0.184	-0.183	−0.001	0.939	46,457	55,340
Father Education: College	0.161	0.164	−0.003	0.282	44,612	52,464
Father Education: Less than HS	0.782	0.780	0.003	0.445	44,612	52,464
Mother Education: College	0.186	0.186	−0.000	0.937	44,889	52,844
Mother Education: Less than HS	0.792	0.791	0.001	0.762	44,889	52,844
College Expectations	0.682	0.675	0.006	0.112	44,570	52,441
Family Income > CLP800,000	0.088	0.089	−0.002	0.516	45,050	53,020
Joint orthogonality F-test				0.349		
Panel B: Neighbor Covariates						
Girl	0.488	0.489	−0.001	0.802	32,750	40,313
Prioritario	0.595	0.589	0.006	0.233	32,750	40,313
High Achiever	0.314	0.316	−0.002	0.684	32,750	40,313
SIMCE (Math)	-0.197	-0.181	−0.016	0.100	26,690	33,440
SIMCE (Language)	-0.114	-0.110	−0.004	0.693	26,575	33,256
Father Education: College	0.175	0.181	−0.006	0.114	25,088	31,074
Father Education: Less than HS	0.768	0.758	0.009**	0.040	25088	31,074
Mother Education: College	0.203	0.210	−0.007	0.127	25,266	31,264
Mother Education: Less than HS	0.773	0.766	0.007	0.125	25,266	31,264
College Expectations	0.700	0.700	0.000	0.956	25,100	31,019
Family Income > CLP800,000	0.099	0.101	−0.002	0.490	25,301	31,299
Joint orthogonality F-test				0.501		
Distance between neighbors	0.063	0.065	−0.002	0.124	58,225	69,860

Notes: This table presents balance tests of observable characteristics between applicants and neighbors depending on the neighbors' offer status. Each row shows the estimate of a regression of the corresponding covariate onto an indicator equals to one if the closest neighbor received an offer in their most preferred school, controlling for the assignment propensity score. The propensity score is computed through simulation using the definitions of equations (4) and (6). Panel A displays estimates of applicant characteristics, while panel B shows estimates of neighbor characteristics. Joint orthogonality shows the p-value of a F-test of joint significance of all covariates listed in the corresponding panel. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 3: ITT and 2SLS Estimates of Neighbor Spillovers

In $t-1$, closest neighbor:	First Stage	ITT			2SLS		
	In $t-1$,	In t, Applicant:			In t, Applicant:		
	Closest neighbor enrolled in ref. school (1)	Ranks ref. school (2)	Ranks ref. school 1st (3)	Attends ref. school (4)	Ranks ref. school (5)	Ranks ref. school 1st (6)	Attends ref. school (7)
Assigned to Ref. School	0.691*** (0.005)	0.015** (0.007) [4.3]	0.010** (0.004) [6.0]	0.008*** (0.003) [5.2]			
Enrolled in Ref. School					0.022** (0.011) [6.2]	0.014** (0.006) [9.4]	0.012*** (0.004) [11.5]
Mean	0.565	0.351	0.166	0.153	0.351	0.166	0.153
F-Statistic					5,725	5,725	5,725
N-Obs	81,992	150,337	150,337	150,337	150,337	150,337	150,337
N-Clusters	11,069	12,789	12,789	12,789	12,789	12,789	12,789

Notes: This table reports intent-to-treat (ITT) and two-stage least squares (2SLS) estimates of neighbor spillovers on applicant decisions. The sample includes all applicants linked to their closest neighbor who participated in the centralized school system in the prior year and have a probability of receiving an offer strictly between zero and one. Column (1) presents the OLS estimate from a regression where the dependent variable is an indicator equal to one if the neighbor enrolled in ninth grade in the same school where they received an offer and the explanatory variable is the offer receipt. Columns (2)-(4) display OLS estimates of regressions where the variable of interest is an indicator equal to one if the closest neighbor is assigned to their most preferred school. Columns (5)-(7) report 2SLS coefficients instrumenting neighbors' enrollment with assignment. The numbers in brackets indicate the percent change relative to the mean outcome for compliers in the untreated state, computed following Abadie (2002). All models control for the probability of each neighbor's assignment to their target school. Standard errors are clustered at the reference-school × applicant-priority × year level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 4: Multiple Neighbors: Baseline Results

	First Stage	ITT			2SLS		
	In $t-1$,	In t, Applicant:			In t, Applicant:		
In $t-1$, at least 1 neighbor:	At least 1 neighbor enrolled in ref. school (1)	Ranks ref. school (2)	Ranks ref. school 1st (3)	Attends ref. school (4)	Ranks ref. school (5)	Ranks ref. school 1st (6)	Attends ref. school (7)
Assigned to Ref. School	0.675*** (0.005)	0.011*** (0.004) [3.2]	0.007** (0.003) [4.4]	0.006*** (0.002) [4.1]			
Enrolled in Ref. School					0.017*** (0.006) [5.0]	0.011** (0.004) [8.4]	0.009*** (0.004) [11.4]
Mean	0.507	0.351	0.166	0.153	0.351	0.166	0.153
F-Statistic					15,656	15,656	15,656
N-Obs	109,592	109,592	109,592	109,592	109,592	109,592	109,592
N-Clusters	11,988	11,988	11,988	11,988	11,988	11,988	11,988

Notes: This table reports intent-to-treat (ITT) and two-stage least squares (2SLS) estimates of neighbor spillovers on applicant decisions. The sample includes all applicants linked to neighbors located within 0.1 miles who participated in the centralized school system in the prior year. Column (1) presents the OLS estimate from a regression where the dependent variable is an indicator equal to one if there is at least one neighbor enrolled in the reference school (defined as the school ranked as top choice by the closest neighbor) and the explanatory variable is an indicator equal to one if there is at least one neighbor receiving an offer at that school. Columns (2)-(4) display OLS estimates of regressions where the variable of interest is an indicator equal to one if at least one neighbor receiving an offer at the reference school. Columns (5)-(7) report 2SLS coefficients instrumenting neighbors' enrollment with the offer. All models control for the probability of observing at least one neighbor enrolled in the reference school. Standard errors are clustered at the reference-school × applicant-priority × year level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 5: Effects on Characteristics of Applicants' Target and Enrolled Schools

	First Stage	Top-Ranked School			Attended School		
		Mean	ITT	2SLS	Mean	ITT	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
10th Grade Scores	0.714***	-0.153	-0.004	-0.005	-0.471	-0.001	-0.002
	(0.006)		(0.008)	(0.012)		(0.007)	(0.010)
F-Statistic				3,025			2,984
N-Obs	76,498		141,866	138,689		138,323	135,262
School Value-Added: College Enrollment	0.712***	-0.053	0.014	0.019	-0.081	0.010	0.014
	(0.007)		(0.009)	(0.012)		(0.008)	(0.011)
F-Statistic				7,895			7,902
N-Obs	74,840		139,001	135,864		135,509	132,490
School Value-Added: HS Graduation	0.625***	0.012	0.004	0.005	-0.001	0.001	-0.001
	(0.010)		(0.006)	(0.010)		(0.007)	(0.010)
F-Statistic				3,921			3,873
N-Obs	73,460		136,319	133,263		132,913	129,977
School Climate Index	0.686***	-0.010	0.012	0.019	-0.249	0.018**	0.028**
	(0.007)		(0.008)	(0.012)		(0.008)	(0.011)
F-Statistic				7,425			7,387
N-Obs	76,548		141,959	138,786		138,415	135,357

Notes: This table reports 2SLS estimates of the effect of the closest neighbor's school characteristics on the applicant's top-ranked and enrolled school characteristics using equations (10) and (11). Columns (2)-(4) display effects on the characteristics of the applicant's target school, while columns (5)-(7) show estimates for the characteristics of the schools where the applicant enrolls. For each outcome, column (1) shows the first stage (coefficient ρ in equation (11)). Columns (2) and (5) show sample mean outcomes. Columns (3) and (6) display coefficients from regressions of outcomes on the characteristics of the school where the neighbor received an offer. Columns (4) and (7) report 2SLS coefficients instrumenting the neighbor's attendance with the offer. All models control for the neighbor's probability of receiving an offer. Clustered standard errors at the reference-school $\times$ applicant-priority $\times$ year level. See the main text for details about the construction of each outcome. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 6: Neighbor Spillovers on Applicants' Short-Term Academic Outcomes

In $t-1$, closest neighbor:	Promoted (0/1)		GPA Scores		10th Grade Test Scores (σ)	
	9th Grade (1)	10th Grade (2)	9th Grade (3)	10th Grade (4)	Language (5)	Math (6)
Panel A: Test score value-added						
Enrolled in top-ranked school	0.006**	0.003	0.007	−0.009	0.024*	−0.001
	(0.003)	(0.003)	(0.010)	(0.012)	(0.014)	(0.013)
F-Statistic	3,068	3,068	2,977	2,846	6,598	6,515
N-Obs	140,037	140,037	135,738	124,844	51,187	51,534
Panel B: College enrollment value-added						
Enrolled in top-ranked school	0.036	0.039	0.123*	0.006	0.207	−0.020
	(0.022)	(0.028)	(0.070)	(0.069)	(0.127)	(0.111)
F-Statistic	7,934	7,934	7,738	7,296	4,832	5,179
N-Obs	137,167	137,167	132,976	122,302	50,214	50,553
Panel C: HS graduation value-added						
Enrolled in top-ranked school	0.107**	0.040	0.091	−0.175	0.235	0.190
	(0.049)	(0.062)	(0.173)	(0.204)	(0.274)	(0.246)
F-Statistic	3,934	3,934	3,838	3,750	2,771	2,594
N-Obs	134,548	134,548	130,456	120,009	49,194	49,527
Panel D: School climate index						
Enrolled in top-ranked school	0.004*	0.001	0.010	0.006	0.028**	0.002
	(0.002)	(0.003)	(0.008)	(0.008)	(0.013)	(0.012)
F-Statistic	7,453	7,453	7,427	7,163	5,369	5,336
N-Obs	140,129	140,129	135,834	124,945	51,212	51,555
Mean	0.915	0.861	5.539	5.697	-0.238	-0.289

Notes: This table reports 2SLS estimates of the effect of the closest neighbor's school characteristics on the applicant's academic outcomes. Each panel displays 2SLS estimates from equations (10)-(11) where the outcome corresponds to each variable presented in columns (1)-(6). The sample corresponds to all applicants linked to their closest neighbor with a probability of receiving an offer at their top-ranked school strictly between zero and one. All models control for neighbors' probability of receiving an offer. All models control for the neighbor's probability of receiving an offer. Clustered standard errors at the reference-school × applicant-priority × year level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 7: Multiple Neighbors: Nonlinear Spillovers

	In t, applicant...								
	Ranks School Any			Ranks School 1st			Attends School		
Neighbors (≤ 0.1 mi) in $t-1$:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Ref. school is closest neighbor's top choice									
N enrolled	0.012**			0.005			0.005		
	(0.005)			(0.004)			(0.003)		
N$\geq$ 1 enrolled		0.025**	0.019*		0.016**	0.016**		0.012*	0.011
		(0.010)	(0.011)		(0.007)	(0.008)		(0.006)	(0.007)
N$\geq$ 2 enrolled			0.015			0.001			0.003
			(0.013)			(0.010)			(0.009)
Mean	0.351	0.351	0.351	0.166	0.166	0.166	0.153	0.153	0.153
N-Obs	43,967	43,967	43,967	43,967	43,967	43,967	43,967	43,967	43,967
Panel B: Ref. school is modal top choice									
N enrolled	0.007*			0.004			0.001		
	(0.004)			(0.003)			(0.003)		
N$\geq$ 1 enrolled		0.019**	0.020**		0.010	0.012		−0.000	0.000
		(0.008)	(0.009)		(0.007)	(0.008)		(0.006)	(0.007)
N$\geq$ 2 enrolled			−0.001			−0.006			−0.001
			(0.011)			(0.009)			(0.008)
Mean	0.487	0.487	0.487	0.254	0.254	0.254	0.228	0.228	0.228
N-Obs	56,612	56,612	56,612	56,612	56,612	56,612	56,612	56,612	56,612

Notes: This table reports nonlinear spillover effects on applicants' behavior. The sample corresponds to applicants linked to all neighbors located within 0.1 miles such that the probability of exposure to at least two neighbors enrolled is strictly between zero and one. Columns (1), (4), (7) present 2SLS estimates of the average effect of an additional neighbor enrolled at the reference school from equations (8)-(9). Columns (2), (5), and (8) display 2SLS estimates of the effect of at least one neighbor enrolled while columns (3), (6), and (9) include estimates of the effect of at least two neighbors enrolled in the reference school. Panel A presents results defining the reference school as the closest neighbor's top-ranked school. Panel B uses the modal top-ranked school among all neighbors located within 0.1 miles. All models control for the probability of observing at least one neighbor enrolled in the reference school. Standard errors are clustered at the reference-school × applicant-priority × year level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 8: Heterogeneous Spillovers: By Applicant and Closest Neighbor Characteristics

In $t-1$, closest neighbor:	In t, Applicant:		
	Ranks School Any (1)	Ranks School 1st (2)	Attends School (3)
Panel A: Applicant characteristics			
Enrolled in top-ranked school	0.022*	0.015**	0.014**
	(0.011)	(0.007)	(0.005)
Enrolled × Female	0.001	−0.002	−0.003
	(0.010)	(0.007)	(0.006)
Enrolled in top-ranked school	0.037**	0.018**	0.016**
	(0.018)	(0.009)	(0.007)
Enrolled × Low SES Status	−0.022	−0.006	−0.006
	(0.022)	(0.011)	(0.009)
Enrolled in top-ranked school	0.025**	0.014**	0.012**
	(0.012)	(0.006)	(0.005)
Enrolled ×	−0.001	−0.002	−0.000
Prior academic achievement	(0.007)	(0.005)	(0.004)
Panel B: Neighbor characteristics			
Enrolled in top-ranked school	0.017*	0.010*	0.008
	(0.009)	(0.006)	(0.005)
Enrolled × Female	0.011	0.009	0.010
	(0.021)	(0.011)	(0.009)
Enrolled in top-ranked school	0.023	0.015	0.014*
	(0.021)	(0.010)	(0.008)
Enrolled × Low SES Status	−0.001	−0.002	−0.004
	(0.023)	(0.011)	(0.009)
Enrolled in top-ranked school	0.024**	0.015**	0.012***
	(0.012)	(0.006)	(0.005)
Enrolled ×	−0.010	−0.001	−0.005
Prior academic achievement	(0.014)	(0.007)	(0.005)

Notes: This table reports heterogeneous spillover effects based on applicant and neighbor observable characteristics. The sample corresponds to all applicants linked to their closest neighbors who participated in the centralized school system in the prior year and had a probability of assignment to their top-ranked school strictly between zero and one. Enrollment is instrumented with an indicator equal to one if the nearest neighbor is assigned to their top-ranked school. All models control for the closest neighbor's probability of assignment to their top-ranked school. Standard errors are clustered at the reference-school × applicant-priority × year level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 9: Heterogeneous Spillovers: by Neighborhood Characteristics

	In t, Applicant:		
	Ranks School Any	Ranks School 1st	Attends School
Neighbors (≤ 0.1 mi) in $t-1$, :	(1)	(2)	(3)
Panel A: Housing characteristics			
N Enrolled in ref. school	0.014***	0.008**	0.006**
	(0.004)	(0.003)	(0.003)
N Enrolled in ref. school ×	0.775***	0.324***	0.106***
number houses/population	(0.072)	(0.044)	(0.041)
N Enrolled in ref. school ×	0.425***	0.204***	0.046
number apartments/population	(0.057)	(0.034)	(0.030)
F-statistic	2,347	2,347	2,347
N-Obs	139,191	139,191	139,191
Panel B: Park areas (sq yards)			
N Enrolled in ref. school	0.021***	0.010***	0.006**
	(0.005)	(0.003)	(0.003)
N Enrolled in ref. school ×	0.001***	0.001***	0.000
park area/population	(0.000)	(0.000)	(0.000)
F-statistic	3,651	3,651	3,651
N-Obs	139,191	139,191	139,191
Panel C: Recreation centers			
N Enrolled in ref. school	0.015***	0.008***	0.006**
	(0.004)	(0.003)	(0.003)
N Enrolled in ref. school ×	0.135***	0.058***	0.001
number centers/population	(0.026)	(0.012)	(0.008)
F-statistic	3,464	3,464	3,464
N-Obs	139,191	139,191	139,191
Panel D: Amenities index (σ)			
N Enrolled in ref. school	0.012***	0.007**	0.006**
	(0.004)	(0.003)	(0.003)
N Enrolled in ref. school ×	0.181***	0.078***	0.000
amenities index	(0.039)	(0.021)	(0.021)
F-statistic	3,378	3,378	3,378
N-Obs	139,171	139,171	139,171

Notes: This table presents heterogeneous spillover effects based on neighborhood characteristics. Each panel displays 2SLS estimates from equations (8) and (9) including the interaction terms displayed in each panel. The sample corresponds to all applicants linked to neighbors located within 0.1 miles who participated in the centralized assignment system in the previous year. The number of enrolled neighbors is instrumented with the number of neighbors assigned to the reference school. All models control for the expected number of nearby neighbors assigned to the reference school. Standard errors are clustered at the reference-school × applicant-priority × year level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 10: Heterogeneous Spillovers: by School Characteristics

	In t, Applicant:		
	Ranks School Any	Ranks School 1st	Attends School
In $t-1$, closest neighbor:	(1)	(2)	(3)
Panel A: Distance to school (miles)			
Enrolled in top-ranked school	0.030***	0.021***	0.018***
	(0.008)	(0.006)	(0.005)
Enrolled × Distance	−0.006**	−0.004**	−0.004***
	(0.003)	(0.002)	(0.002)
F-statistic	6,963	6,963	6,963
N-Obs	131,574	131,574	131,574
Panel B: Test scores value-added (σ)			
Enrolled in top-ranked school	0.025***	0.015***	0.013***
	(0.009)	(0.005)	(0.004)
Enrolled × School VA	−0.032	−0.008	0.003
	(0.042)	(0.021)	(0.015)
F-statistic	2,092	2,092	2,092
N-Obs	148,941	148,941	148,941
Panel C: College enrollment value-added (σ)			
Enrolled in top-ranked school	0.028**	0.018***	0.013***
	(0.013)	(0.006)	(0.005)
Enrolled × School VA	0.157**	0.094**	0.021
	(0.080)	(0.044)	(0.035)
F-statistic	3,724	3,724	3,724
N-Obs	148,955	148,955	148,955
Panel D: School climate index (σ)			
Enrolled in top-ranked school	0.028***	0.015***	0.013***
	(0.009)	(0.005)	(0.005)
Enrolled × Index	−0.022	−0.005	−0.001
	(0.018)	(0.009)	(0.005)
F-statistic	2,102	2,102	2,102
N-Obs	148,941	148,941	148,941

Notes: This table presents 2SLS estimates from equations (8) and (9) to investigate heterogeneity based on the characteristics of neighbors' target school. Distance (measured in miles) corresponds to the euclidean distance between the applicant's residence and the neighbor's school. Average tenth-grade scores uses math and language scores in 2017 and 2018. School value-added on tenth-grade scores and college enrollment are constructed using data from the 2015-2018 cohorts of tenth-grade students (see the main text for details). The school climate index is created by the Ministry of Education and refers to students', teachers', and parents' perceptions about the school environment. Average tenth-grade scores and the school climate index are standardized to be mean zero and unit variance using all public and private schools with positive ninth-grade enrollment. All models control for the neighbor's probability of receiving an offer. Clustered standard errors at the reference-school × applicant-priority × year level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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A Online Appendix

Neighbors' Spillovers on High School Choice

Juan Matta   Alexis Orellana

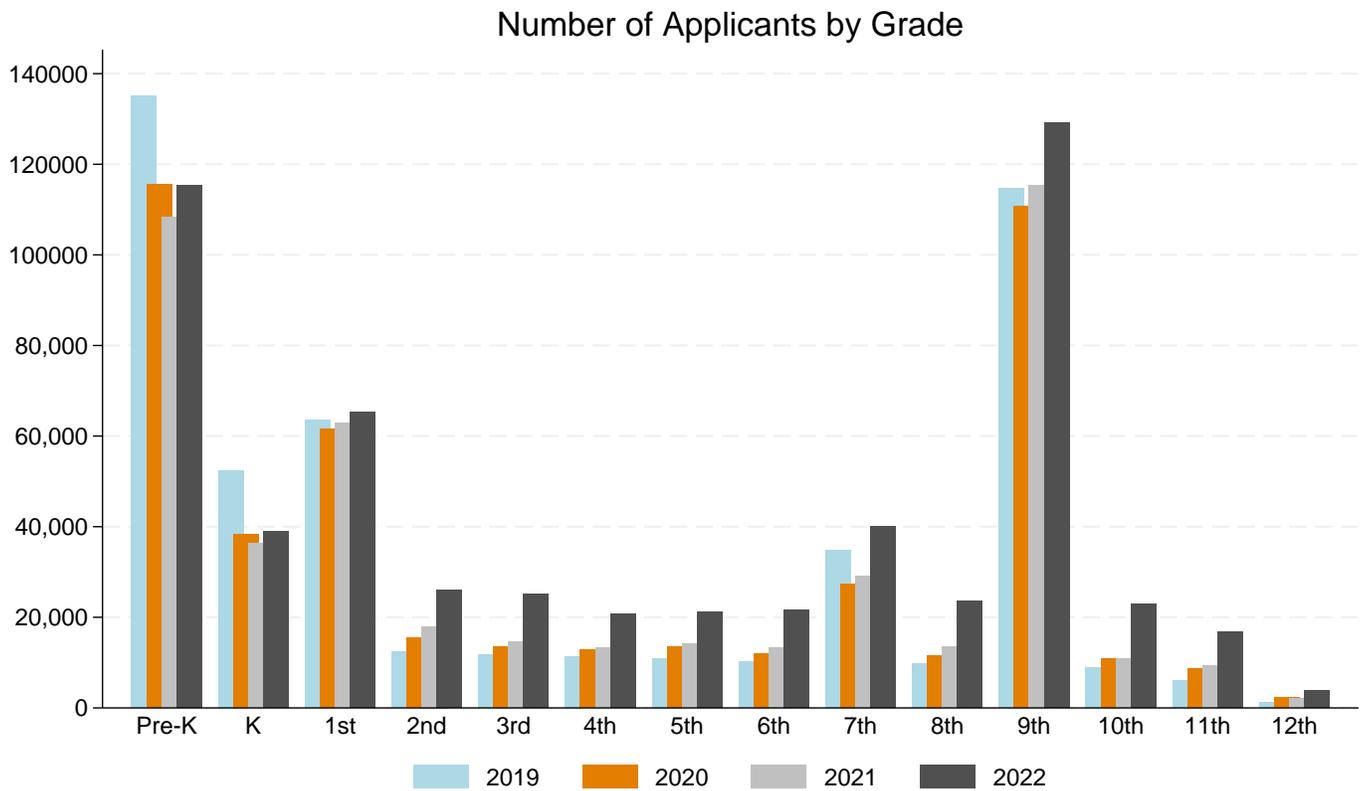## A.1 Additional Figures

Figure A.1: Number of Applicants by Grade

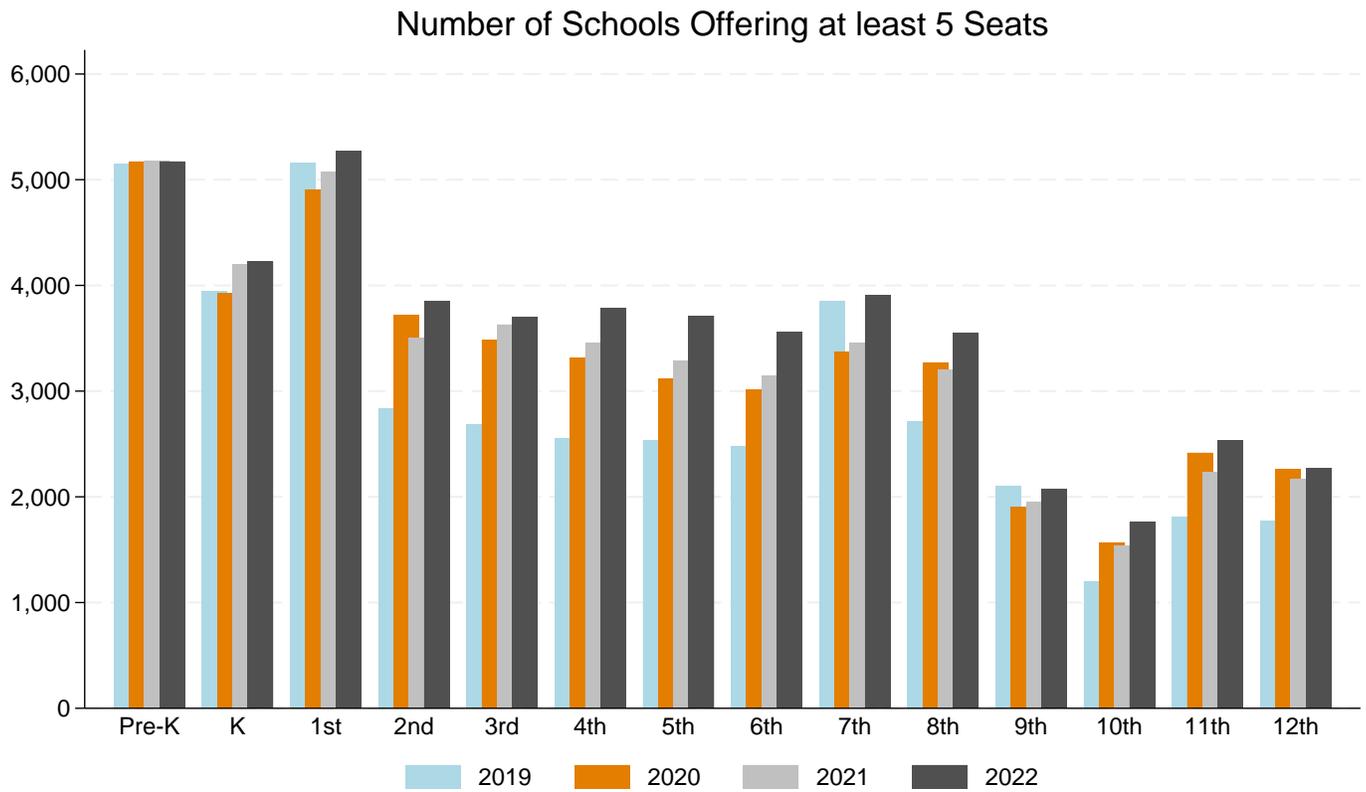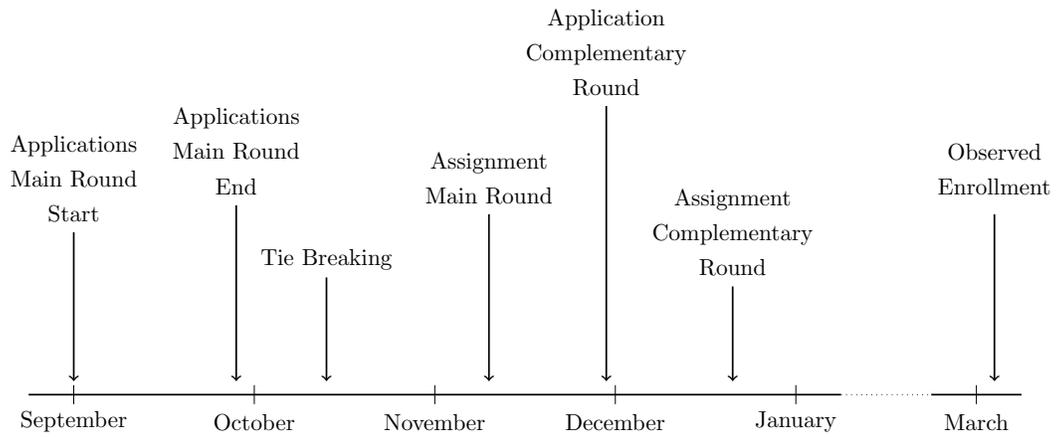Figure A.2: Number of Participating Schools by Grade

Number of Schools Offering at least 5 Seats

Figure A.3: Timeline of the Application Process

Source: Correa et al. (2022)

Figure A.4: Priority Groups

Table 1. Weak Priorities by Type-Specific Seats

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

Note. Lower numbers indicate higher priority.

Source: Correa et al. (2022)

Figure A.5: Distribution of School Applications
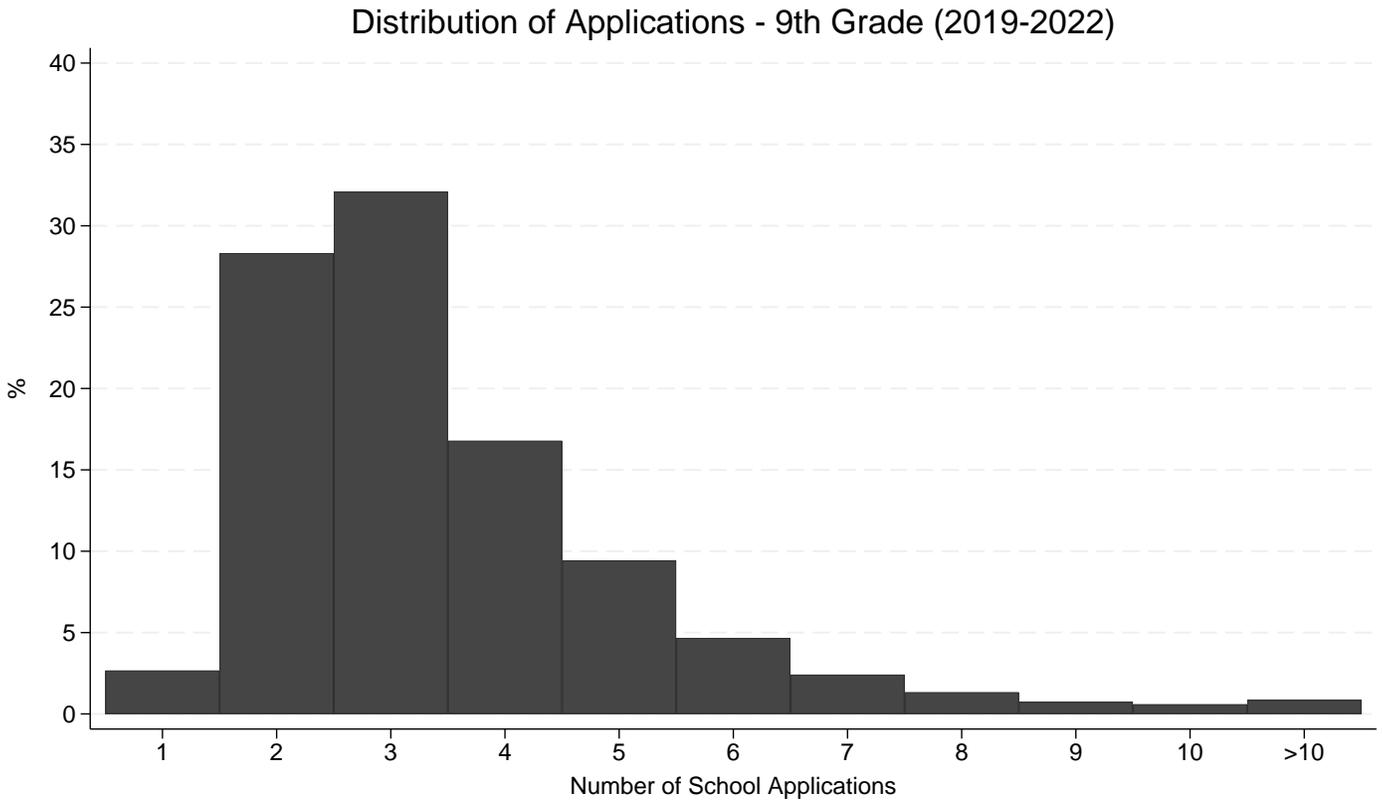
Notes: This plot shows the distribution of the number of schools submitted by ninth-grade applicants pooling across the 2019-22 application rounds.

Figure A.6: Applicant/Seats Ratio Across Schools

Distribution of Applications by School (2019-2022)
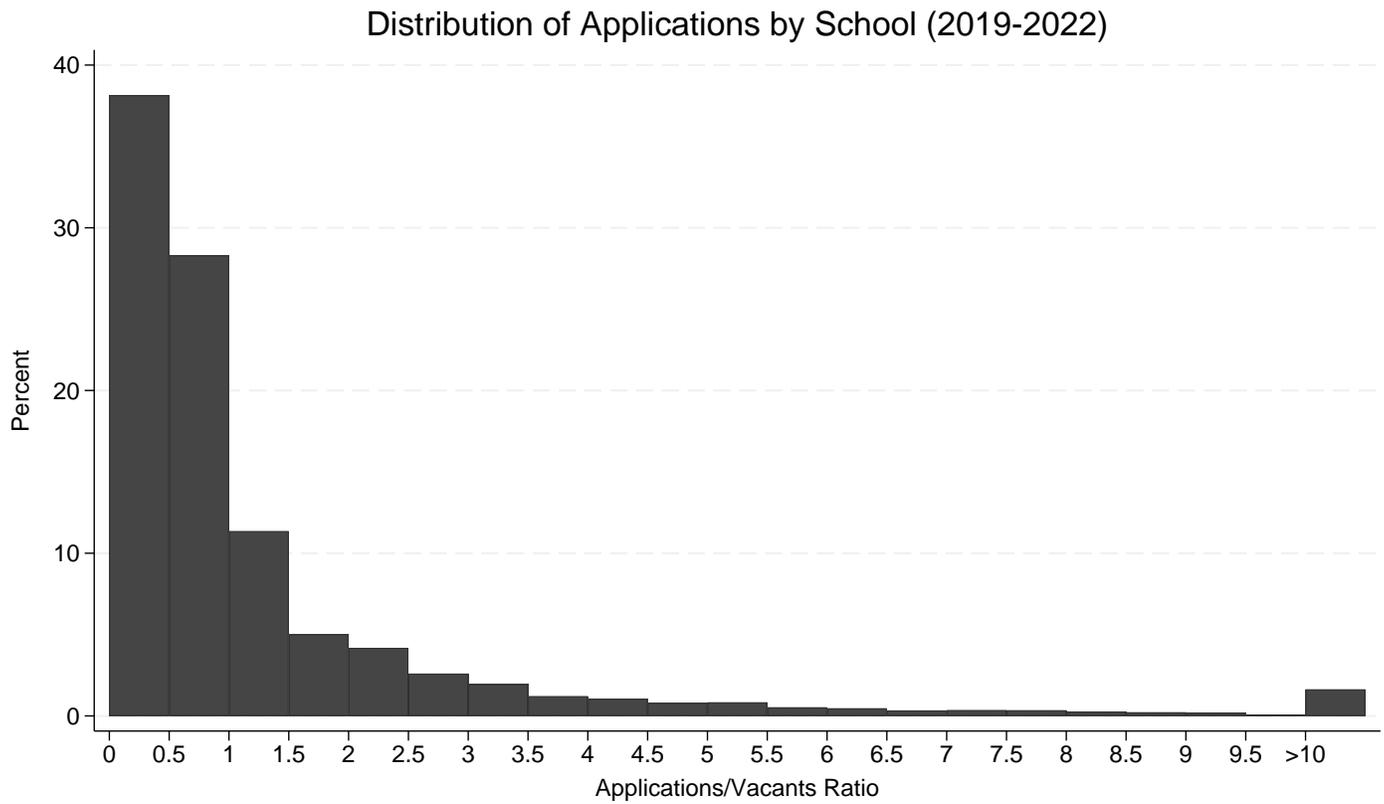
Notes: This plot shows the distribution of the applicants/seats ratio for schools offering ninth grade, restricted to schools offering at least five vacant seats. The number of applicants considers only first-rank preferences.

Figure A.7: Density of Simulated School Allocations
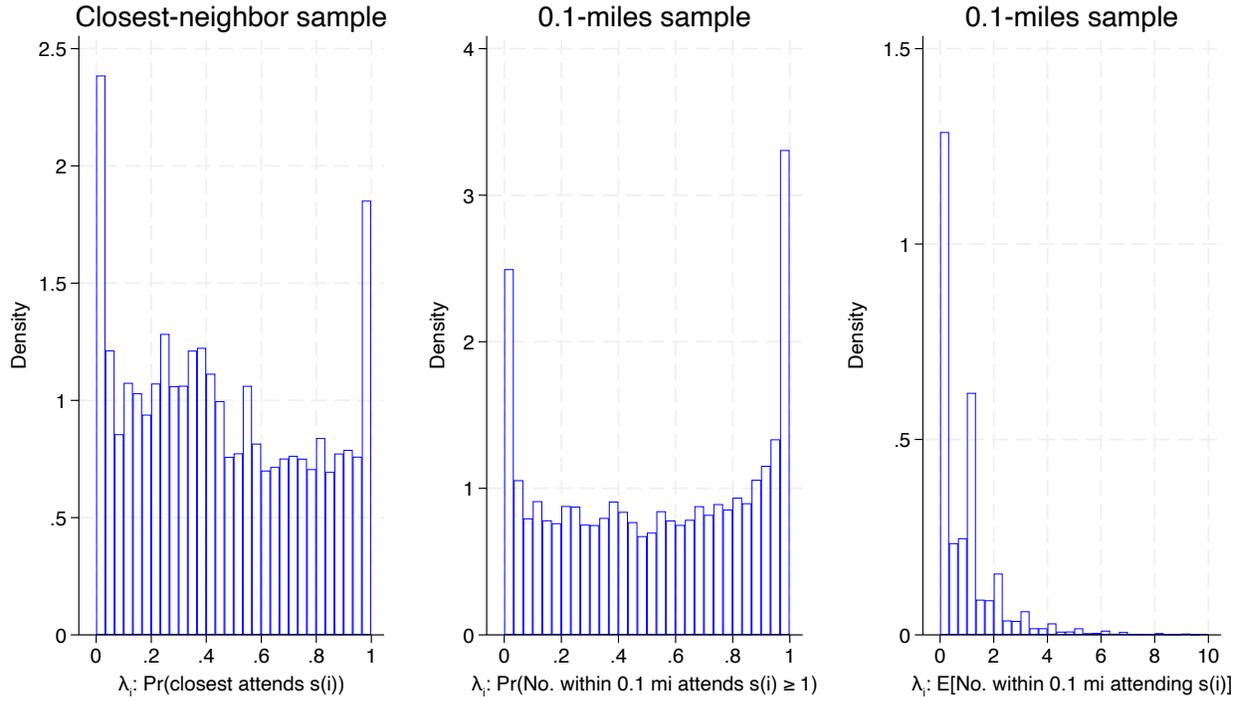
Notes: This plot presents the distribution of simulated values of λ_i for all applicants between 2019 and 2022. We simulate the assignment system 1,000 times, varying the random tie-breaking number in each iteration while keeping preferences and priorities fixed. In each iteration, we record school allocations and calculate: (i) the closest neighbor's propensity score, (ii) the probability of having at least one neighbor within 0.1 miles assigned to the reference school, and (iii) the expected number of neighbors within 0.1 miles assigned to the reference school. For the center and right figures, the reference school is defined as the modal top-ranked school among all neighbors within a 0.1-mile radius.

Figure A.8: Simulated and Observed School Allocations
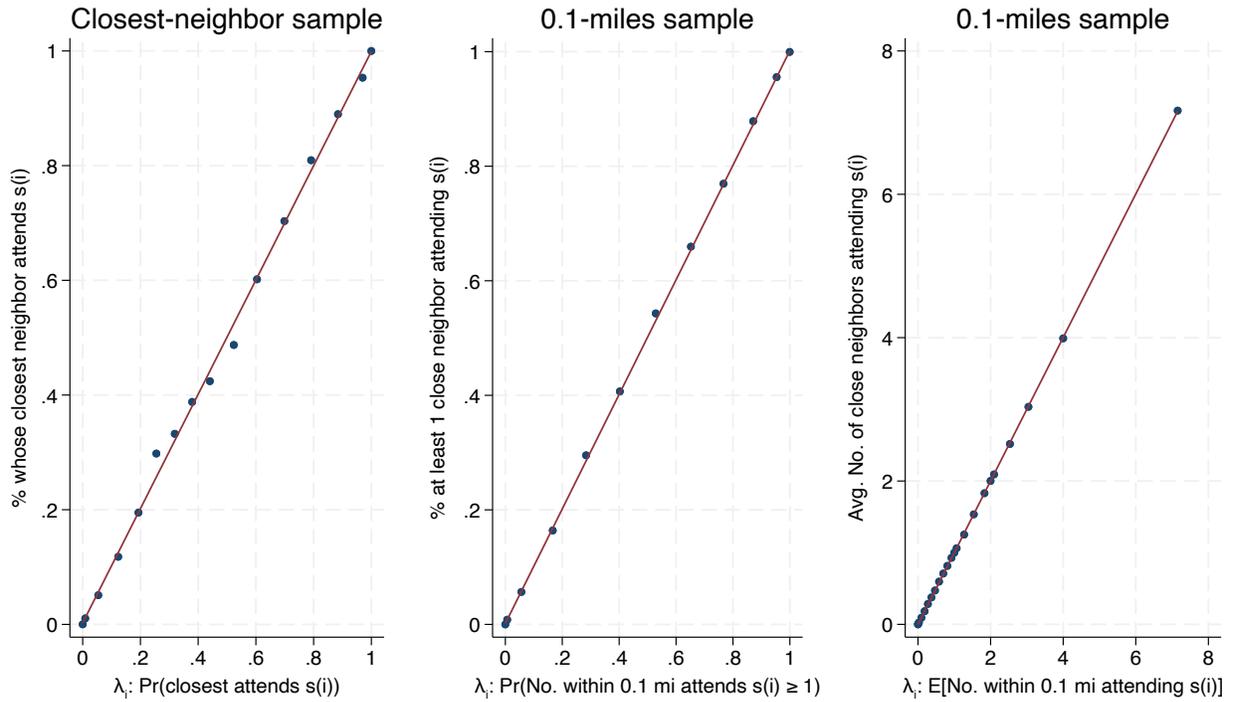
Notes: This plot presents the relationship between the simulated values of λ_i and the observed values of the instrument z_i. We simulate the assignment system 1,000 times, varying the random tie-breaking number in each iteration while keeping preferences and priorities fixed. In each iteration, we record school allocations and calculate: (i) the closest neighbor's propensity score, (ii) the probability of having at least one neighbor within 0.1 miles assigned to the reference school, and (iii) the expected number of neighbors within 0.1 miles assigned to the reference school. For the center and right figures, the reference school is defined as the modal top-ranked school among all neighbors within a 0.1-mile radius.

Figure A.9: Simulated and Observed School Characteristics
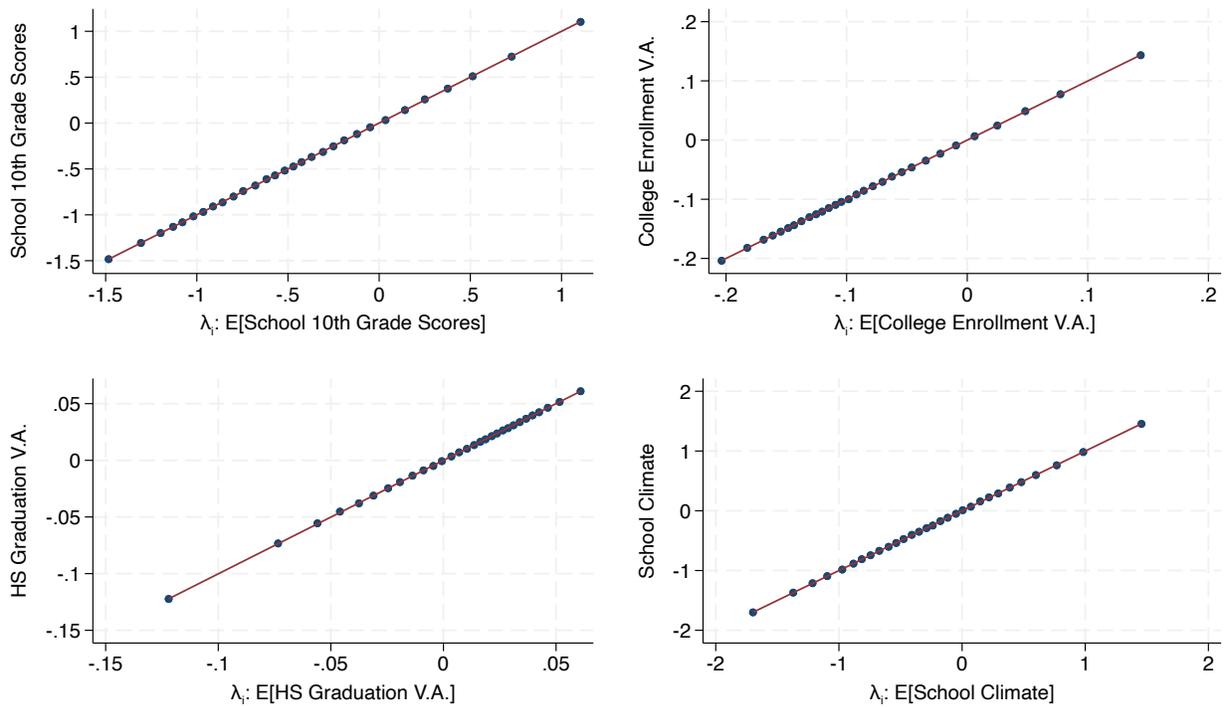
Notes: This plot presents the relationship between the simulated values of λ_i and the observed values of the instrument z_i, defined as characteristics of schools where nearby neighbors are assigned. We simulate the assignment system 1,000 times, varying the random tie-breaking number in each iteration while keeping preferences and priorities fixed. In each iteration, we record school characteristics and calculate λ_i as the average value across simulations.

A.2 School Value-Added

We use information from tenth-grade cohorts between 2015 and 2018 to construct a proxy of school effectiveness for high school graduation and college attendance. For the 2015, 2016, and 2017 cohorts we observe test scores in eight grade, while for the 2018 cohort we observe the same variables in fourth grade. Based on this information, we estimate school value-added models of the form:

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

Our outcomes y_{ist} are tenth-grade standardized test scores, indicators equal to one when student i in cohort t graduated on time from high school s and attended college the next year, respectively. The vector X_{ist} includes a third-order polynomial in math and language lagged test scores, GPA in eighth and seventh grades, and indicators for gender, and repetition in eighth and seventh grades. θ_t corresponds to cohort fixed effects. We estimate equation (12) and recover the raw school fixed effects $\hat{\theta}_s$.

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

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

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

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

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

In practice, we construct the sample estimates of the hyperparameters μ_θ and Σ_θ using the distribution of estimated fixed effects $\{\hat{\theta}_s\}_{s=1}^S$, while we employ the standard error of $\hat{\theta}_s$ to estimate Ω_s. We plug $\hat{\Omega}_s, \hat{\mu}_\theta, \hat{\Sigma}_\theta$ into (15) and use the EB posterior means as regressors in our analysis of heterogeneity effects by school characteristics in section 4.5.3.

A.3 School Attributes and Demand

Figure A.10: Correlation between School Attributes and Demand
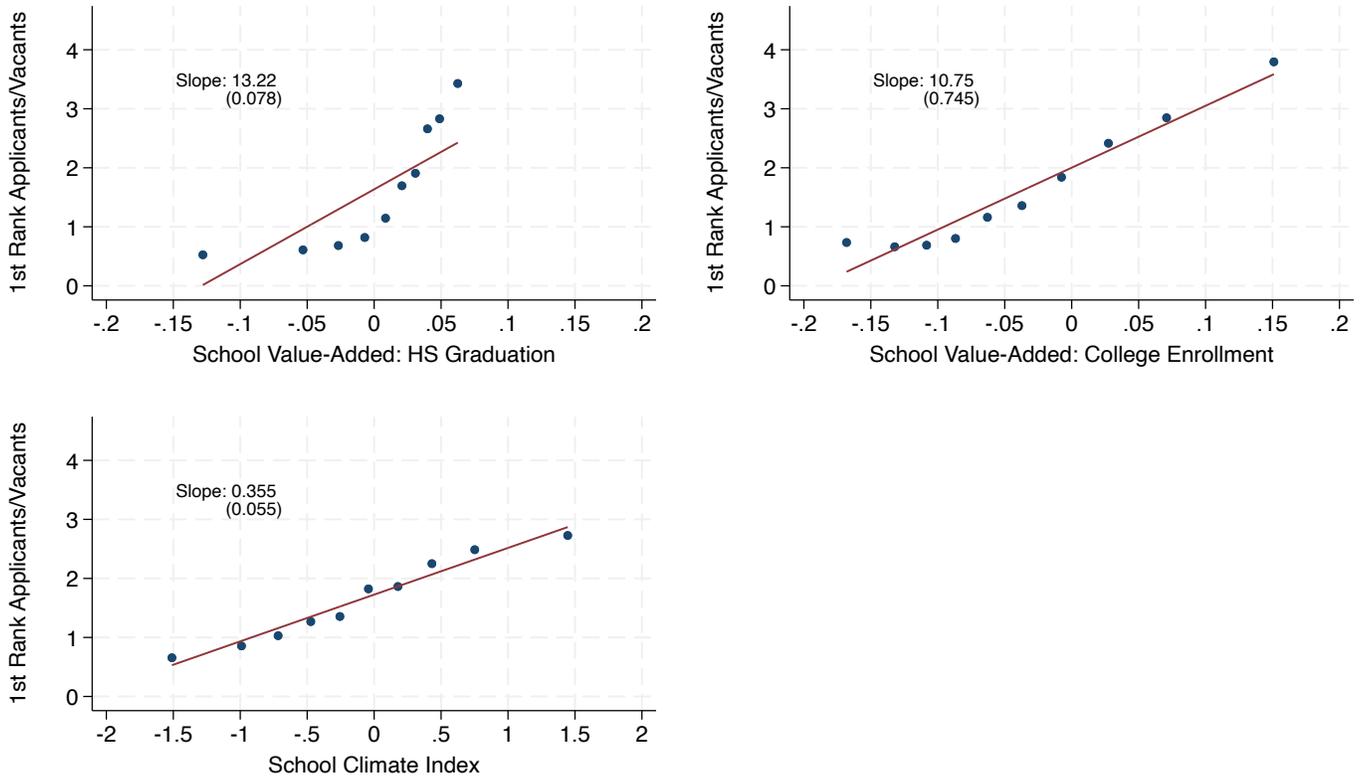
Notes: This plot shows the correlation between school characteristics described in section 4.5.3 and the ratio of first-rank applicants to the number of seats available in ninth grade in each school. The distribution of each attribute is computed using all schools in the country with positive ninth-grade enrollment, excluding schools with less than five vacant seats. School value-added on high school graduation and college attendance are computed using the methodology described in section A.2. The school climate index is reported by the Ministry of Education and refers to students', teachers', and parents' perceptions about the school environment.

A.4 Additional Tables

Table A.1: Summary of Acceptances by School Grade

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

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

Table A.2: Application Cohorts and Data Availability

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

Notes: This table presents data availability for different cohorts of eighth-graders. Grey cells represent the eighth-grade cohorts participating in the school assignment under the Deferred Acceptance mechanism. For each row, grades in bold indicate when we observe previous test scores and background information for the respective cohort.

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Table A.3: OLS Estimates of Neighbor Spillovers

| | In t, Applicant: | | |
| | Ranks School Any | Ranks School 1st | Attends School |
In $t-1$, Neighbor:	(1)	(2)	(3)
Enrolled in 1st Choice	0.069***	0.060***	0.094***
	(0.004)	(0.002)	(0.002)
	[19.7]	[39.7]	[79.4]
Mean	0.353	0.150	0.119
N-Obs	128,194	128,194	128,194
N-Clusters	73,255	73,255	73,255

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

Table A.4: Placebo Tests

	In t, Applicant:				
	Ranks School Any (1)	Ranks School 1st (2)	Accepts Same (3)	Complementary Round (4)	Attends School (5)
Panel A: In $t+1$, closest neighbor:					
Admitted to 1st Choice	0.001	0.004	0.003	0.000	0.001
	(0.004)	(0.003)	(0.002)	(0.002)	(0.003)
N-Obs	141,992	141,992	141,992	141,992	141,992
N-Clusters	82,269	82,269	82,269	82,269	82,269
Panel B: In t, closest neighbor:					
Admitted to 1st Choice	0.002	−0.000	−0.000	−0.002	−0.002
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
N-Obs	174,508	174,508	174,508	174,508	174,508
N-Clusters	125,835	125,835	125,835	125,835	125,835

Notes: This table shows placebo tests of neighbor spillovers. In Panel A, we link each applicant in year t to their closest neighbor applying in year $t+1$ and present estimates of OLS regressions of applicants' outcomes onto an indicator equal to one if the closest neighbor receives a seat offer at their most preferred school. In Panel B, we do a similar exercise, linking applicants in year t to neighbors applying in the same period. The estimates reported in columns (1) and (5) correspond to those shown in Figure 3. Standard errors are clustered at the reference-school × applicant-priority × year level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.5: Application Patterns by Socioeconomic Status

	Number of Applications (1)	All Schools Ranked		Top-Ranked School	
		10th Grade Scores (Language) (2)	10th Grade Scores (Math) (3)	10th Grade Scores (Language) (4)	10th Grade Scores (Math) (5)
Low-SES	−0.434***	−0.206***	−0.211***	−0.278***	−0.271***
	(0.009)	(0.004)	(0.004)	(0.005)	(0.005)
Girl	0.050***	0.133***	0.055***	0.154***	0.070***
	(0.011)	(0.003)	(0.003)	(0.005)	(0.005)
High Achiever	0.037***	0.186***	0.189***	0.236***	0.236***
	(0.011)	(0.004)	(0.004)	(0.005)	(0.005)
Distance to school		0.003***	0.011***	0.000	0.009***
		(0.001)	(0.001)	(0.001)	(0.001)
Low-SES × High Achiever	0.044***	-0.005	−0.010**	-0.009	−0.017***
	(0.013)	(0.005)	(0.005)	(0.007)	(0.006)
Low-SES × Girl	0.033***	-0.004	-0.003	−0.021***	−0.032***
	(0.012)	(0.004)	(0.004)	(0.006)	(0.006)
Low-SES × Distance to school		−0.006***	−0.012***	-0.002	−0.008***
		(0.001)	(0.001)	(0.001)	(0.001)
Mean Outcome	3.37	-0.06	-0.04	-0.14	-0.10
Priority Controls	Yes	Yes	Yes	Yes	Yes
Number of applications	No	Yes	Yes	No	No
N-Obs	394,189	1,307,240	1,307,151	389,574	389,565
R-Squared	0.04	0.04	0.04	0.06	0.06

Notes: This table reports OLS estimates of differences in the number and quality of schools ranked by ninth-grade applicants based on socioeconomic status and other background characteristics. The outcome in column (1) is the number of schools submitted to the centralized system. Outcomes in columns (2)-(3) measure the average school-level tenth-grade test scores across all schools included in the application. Columns (4) and (5) restrict the sample to consider only top-ranked school for each applicant. Priority controls include indicators for having a sibling enrolled in the school, a parent working in the school, and applying to a school previously attended. Students with special needs are excluded from the sample. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.6: Heterogeneous Spillovers: By Similarity Between Applicant and Closest Neighbor

	In t, Applicant:		
	Ranks School Any	Ranks School 1st	Attends School
In $t-1$, closest neighbor:	(1)	(2)	(3)
Panel A: Same gender			
Enrolled in top-ranked school	0.028**	0.014**	0.011**
	(0.011)	(0.006)	(0.005)
Enrolled × Same gender	−0.011	−0.000	0.002
	(0.010)	(0.007)	(0.006)
Panel B: Same SES status			
Enrolled in top-ranked school	0.028	0.013	0.011
	(0.018)	(0.009)	(0.007)
Enrolled × Same SES Status	−0.010	0.003	0.001
	(0.022)	(0.011)	(0.009)
Panel C: Same academic performance			
Enrolled in top-ranked school	0.009	0.009	0.007
	(0.016)	(0.008)	(0.006)
Enrolled ×	0.044**	0.014	0.013
Both below average score	(0.021)	(0.012)	(0.010)
Panel D: Same 8th-grade school			
Enrolled in top-ranked school	0.021*	0.013**	0.012***
	(0.012)	(0.006)	(0.005)
Enrolled ×	0.009	0.014	0.005
Same 8th-grade school	(0.015)	(0.011)	(0.010)

Notes: This table reports heterogeneous spillover effects based on the similarities between applicants and neighbors in observable characteristics. The sample corresponds to all applicants linked to their closest neighbors with a probability of assignment to their top-ranked school strictly between zero and one. All 2SLS estimates are derived from equations (8) and (9) including the interaction terms displayed in each panel. Enrollment is instrumented with an indicator equal to one if the nearest neighbor is assigned to their top-ranked school. All models control for the closest neighbor's probability of assignment to their top-ranked schools. Standard errors are clustered at the reference-school × applicant-priority × year level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.7: Heterogeneous Spillovers: Multiple Neighbors

Treatment: At least one neighbor enrolled in ref. school	By Gender		By SES Status		By Prior Academic Achievement	
	Boys	Girls	Prioritario	Not Prioritario	> Average	< Average
	(1)	(2)	(3)	(4)	(5)	(6)
Apply to ref. school (Any rank)	0.026***	0.009	0.015*	0.021**	0.019**	0.014*
	(0.009)	(0.008)	(0.008)	(0.010)	(0.008)	(0.008)
p-value for equal effects	0.148		0.606		0.661	
N-Obs	51,589	58,003	70,786	38,806	56,914	52,678
Apply to ref. school (Top choice)	0.015***	0.007	0.013**	0.007	0.006	0.015***
	(0.006)	(0.006)	(0.005)	(0.007)	(0.006)	(0.006)
p-value for equal effects	0.330		0.536		0.279	
N-Obs	51,589	58,003	70,786	38,806	56,914	52,678
Attend ref. school	0.010**	0.009*	0.010**	0.008	0.008	0.011**
	(0.005)	(0.005)	(0.004)	(0.006)	(0.005)	(0.005)
p-value for equal effects	0.890		0.745		0.731	
N-Obs	51,589	58,003	70,786	38,806	56,914	52,678

Notes: This table reports heterogeneous spillover effects based on applicants' observable characteristics. The sample corresponds to all applicants linked to their neighbors located within 0.1 miles. All 2SLS estimates are derived from equations (8) and (9). Enrollment is instrumented with an indicator equal to one if at least one nearby neighbor was assigned to the reference school, defined as the closest neighbor's top-ranked school. All models control for the simulated expected probability of exposure to at least one nearby neighbor assigned to the reference school. Standard errors are clustered at the reference-school × applicant-priority × year level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.8: Robustness 1: Alternative Definition of the Reference School

In $t-1$, at least 1 neighbor:	First Stage	ITT			2SLS		
	In $t-1$,	In t, Applicant:			In t, Applicant:		
	At least 1 neighbor enrolled in ref. school (1)	Ranks ref. school (2)	Ranks ref. school 1st (3)	Attends ref. school (4)	Ranks ref. school (5)	Ranks ref. school 1st (6)	Attends ref. school (7)
Assigned to Ref. School	0.688*** (0.008)	0.014*** (0.005) [2.8]	0.008** (0.004) [3.0]	0.005 (0.003) [2.3]			
Enrolled in Ref. School					0.020*** (0.007) [4.1]	0.011** (0.006) [4.9]	0.008 (0.005) [5.0]
Mean	0.457	0.495	0.256	0.227	0.490	0.229	0.151
F-Statistic					6,930	6,930	6,930
N-Obs	101,849	101,849	101,849	101,849	101,849	101,849	101,849
N-Clusters	6,560	6,560	6,560	6,560	6,560	6,560	6,560

Notes: This table reports 2SLS estimates .

Table A.9: Robustness 2: Alternative Specifications

Specification	Ranks ref. school (1)	Ranks ref. school (top choice) (2)	Attends ref. school (3)
		Outcomes	
Panel A: Closest neighbor			
Baseline: linear in λ_i	0.022**	0.014**	0.012***
	(0.011)	(0.006)	(0.004)
Fixed effects	0.024***	0.015***	0.013***
	(0.008)	(0.005)	(0.004)
Quadratic in λ_i	0.022**	0.014**	0.012***
	(0.011)	(0.006)	(0.005)
Recentered instrument $(\lambda_i - z_i)$	0.022*	0.013**	0.011**
	(0.011)	(0.006)	(0.005)
Panel B: All neighbors within 0.1 miles			
Baseline: linear in λ_i	0.017***	0.011**	0.009***
	(0.006)	(0.004)	(0.004)
Fixed effects	0.016***	0.011**	0.009***
	(0.006)	(0.004)	(0.004)
Quadratic in λ_i	0.016***	0.011**	0.009***
	(0.006)	(0.004)	(0.004)
Recentered instrument $(\lambda_i - z_i)$	0.017***	0.011**	0.010***
	(0.006)	(0.004)	(0.004)

Notes: This table reports 2SLS estimates .

Table A.10: Robustness 3: Alternative Sample Selection

| | First Stage | ITT | | | 2SLS | | |
| | In $t-1$, | In t, Applicant: | | | In t, Applicant: | | |
In $t-1$, closest neighbor:	At least 1 neighbor enrolled in ref. school (1)	Ranks ref. school (2)	Ranks ref. school 1st (3)	Attends ref. school (4)	Ranks ref. school (5)	Ranks ref. school 1st (6)	Attends ref. school (7)
Assigned to Ref. School	0.690*** (0.005)	0.013 (0.008) [3.5]	0.009** (0.004) [5.0]	0.008*** (0.003) [5.1]			
Enrolled in Ref. School					0.019 (0.011) [5.0]	0.013** (0.006) [8.2]	0.012*** (0.005) [11.0]
Mean	0.588	0.369	0.176	0.166	0.369	0.176	0.166
F-Statistic					6,358	6,358	6,358
N-Obs	70,405	128,063	128,063	128,063	128,063	128,063	128,063
N-Clusters	10,178	11,780	11,780	11,780	11,780	11,780	11,780

Notes: This table reports 2SLS estimates .