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“Gendered Preferences in Centralized School Choice: Evidence from Chile’s School Admission System”

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Gendered Preferences in Centralized School Choice:
Evidence from Chile's School Admission System

Abstract

This paper studies gender differences in school-choice behavior under Chile's Sistema de Admisión Escolar (SAE), a centralized system that records ranked preference lists before assignment. Using national 2025 student-school choice-set data, it asks whether applications submitted for female and male applicants weight school attributes differently within their submitted lists. The empirical strategy estimates a rank-ordered (exploded) logit model on each applicant's true ranking, without padding short lists. The main result is a modest gender difference in the weight placed on school social composition, measured by the school-level share of non-priority students rather than by applicants' own family background. Female applicants are somewhat more likely than comparable male applicants to rank relatively higher-SES schools earlier, with the pattern concentrated in secondary education. Cross-cohort estimates for 2017–2024 show that the pattern is not unique to 2025, and an admission-stage decomposition indicates that much of the admitted-school SES gap aligns with the composition of the schools applicants face. The contribution is measurement-based: the paper identifies where gendered ranking differences enter the SAE application process.

Keywords: centralized school choice; SAE; gender differences; school social composition; rank-ordered (exploded) logit; Chile

Contents

1 Introduction

Families weigh more than academics when they decide where to apply. If they value a school's environment differently for a daughter than for a son, gendered differences in educational trajectories can begin at the application stage, before assignment or enrollment and well before any later choice of track or field of study. The application is one of the first places these differences become visible.

There is good reason to expect gender to matter at this early stage. Families may be more protective of a daughter's daily mobility and safety, or may weigh a school's peer environment and social climate differently for a daughter than for a son. None of this means in advance that one group ends up with better schools. It means only that the same school attributes may enter a family's ranking in gendered ways.

Observing those rankings is harder than it sounds. Final enrollment records reflect several forces at once: what families wanted, which seats were open, who held priority, and how the lottery fell. Centralized admission systems offer a cleaner view, because they record the ranked lists families submit before any of this plays out. This paper draws on the application data behind one such system, Chile's Sistema de Admisión Escolar (SAE), to ask how gender enters those lists. Two features of the setting matter for how these lists should be read. First, publicly funded schools in SAE may not select students on prior achievement or family background. Second, when a school has more applicants than seats, places are assigned by legally defined priority categories and a random lottery rather than by the school itself. Because schools do not sort applicants by family background, a gendered difference in the social composition of submitted rankings can be read mainly as family-side ordering. Section 2 sets out these rules in full.

The question is simple to state. Within the set of schools an applicant actually considers, do the families of girls and of boys weigh school attributes differently? Put concretely, are applications submitted for girls more or less likely than those submitted for boys to rank a school highly because it is higher in quality, farther away, higher in social composition, or more in demand? This is deliberately not a comparison of the schools girls and boys end up attending. The distinction matters, because a gap in final placements does not show whether a difference began in how families ranked schools or in the vacancies, priorities, and lottery draws that come afterward.

To answer it, the thesis builds student-school choice-set data for five educational-stage groups, from Prekinder and Kinder through Primary and Secondary. It estimates a rank-ordered (exploded) conditional logit model on each applicant's *true* submitted ranking, without padding short lists to a fixed length. Each applicant contributes the chain of choices implied by that ranking: the first-ranked school is treated as chosen from all the schools listed, the second from those that remain, and so on down the list. Quality, distance, School SES, and Top3 Demand all enter as choice-set centered variables, so the coefficients show how appealing a school is relative to the others the same applicant weighed, not to its standing in the wider market. School SES here is a property of the school, not the applicant: it is measured by the school-level share of non-priority students, used as a proxy for the school's social composition rather than for any applicant's own family income or education.

School social composition is the clearest gendered attribute. Among secondary applicants, a one-standard-deviation rise in relative School SES raises the female-minus-male odds ratio by about 3.4 percent in the baseline model; a more conservative specification gives a smaller but still positive estimate. The result remains under applicant-clustered inference, early-rank restrictions, leave-one-out demand, and low- versus high-dispersion choice sets. In the younger stages, the estimates stay close to zero once the model uses each applicant's true ranking. Demand and distance do not show the same stable secondary pattern.

Comparable stage-grouped estimates for 2017–2024 show the same secondary School SES interaction positive in every cohort (Appendix Tables A17–A22). This is not a statement about one cohort aging through the system, but it does show that the secondary-stage pattern is not peculiar to 2025.

The question is therefore not whether girls choose "better" schools. The paper shows a narrower fact: applications submitted for girls and boys place somewhat different relative weights on school attributes, and the most consistent difference concerns school social composition. In doing so, it links work on centralized school choice with work on gendered educational decisions at a stage before assignment has occurred.

Two things make this margin worth isolating. Methodologically, using the entire submitted ranking rather than the first choice or the final placement recovers a demand-side gendered pattern that enrollment records cannot show, because enrollment mixes family ordering with capacity, priority, and the lottery draw. Substantively, it locates a gendered tilt in demand that is already present before the assignment mechanism runs, which is the input that any centralized system, however neutral its algorithm, can only process rather than remove. The contribution is thus a specific, cleanly identified margin, not a claim that it is the only or the largest way gender enters school choice.

The rest of the paper is organized as follows. Section 2 introduces the SAE system first, because its rules determine how the ranked-list data used throughout the paper should be read; Section 3 then reviews the related literature. Section 4 explains the data, variable construction, and empirical strategy. Section 5 reports the main results, stage heterogeneity, robustness checks, and suggestive evidence. Section 6 discusses the interpretation and limits of the findings. Section 7 concludes.

2 Institutional background: Chile's SAE system

Chile is a useful place to study how families choose schools when a single mechanism does the assigning. Its school system has three broad sectors: public schools, government-subsidized private schools, and fully private paid schools. SAE governs admission to the first two, the publicly funded part of the system, while paid private schools stay outside the platform and run their own procedures (Ministerio de Educación de Chile 2025a). That boundary shapes what the data can show, since the paper observes choices within the publicly funded sector rather than across the entire Chilean school market and its private upper tail.

The platform is also national in scale. For the 2025 regular stage, Ministry of Education records list 463,923 applicants and 1,565,505 applications (Ministerio de Educación de Chile 2025b). The sample analyzed in this thesis is a subset of that universe, restricted to the applicants and school alternatives for which the variables needed for the choice-set analysis are available.

SAE arrived as part of a larger reform, the Ley de Inclusión Escolar (School Inclusion Law, Law No. 20.845). It was phased in gradually, beginning in the Magallanes region in 2016 and widening across regions and grade levels over successive cycles until, by 2020, it covered publicly funded admission nationwide (Correa et al. 2022). This history is worth keeping in mind, because familiarity with the platform and local adaptation to its rules may have shifted over time. The 2025 data come from a mature, national system, but the cross-stage comparisons in this paper should still be read as comparisons across observed stages and markets, not as a single cohort tracked through time.

The Inclusion Law reshaped how public and subsidized schools admit students. Beforehand, many enjoyed wide discretion, screening applicants through interviews, entrance tests, academic records, or less formal means. The reform bars schools from selecting on past or expected achievement, family socioeconomic background, parental education or marital status, family wealth, and similar criteria; when applicants outnumber seats, places are filled by legally defined priority categories and a random draw rather than by the school's own judgment. One of those categories, priority status for vulnerable students, does double duty in this paper, because the school-level share of non-priority students is precisely the proxy used for social composition. The anti-screening rule also bears on interpretation. Inside the publicly funded sector, a gendered difference in the School SES profile of submitted rankings is unlikely to come from schools sorting applicants by family SES, which they may not do; it is more naturally read as applicant-side ordering, even though final placements still hinge on vacancies, priorities, and the lottery. Enforcement need not have been complete from the start: early in the rollout some schools may have retained informal screening, and some families may have expected older selection rules to still apply. Drawing on 2025 data, from a mature national system, limits this concern but does not eliminate it.

The priority rules themselves are worth stating precisely, because they govern how a submitted ranking becomes a placement and because several of them feed directly into the empirical design. When a school-grade is over-subscribed, the algorithm does not rank applicants by the school's own criteria; it orders them through a fixed sequence of legally defined priority categories, with ties broken by a single random number per applicant rather than any school-level judgment (Correa et al. 2022). The categories are served in turn: applicants with a sibling already enrolled at the school; priority (socioeconomically vulnerable) students, for whom at least fifteen percent of the seats in each grade are reserved by law; children of the school's staff; and former students who were not expelled, before any remaining seats go to other applicants. These four categories make up the legally defined priority order; beyond them, schools may not screen on academic performance, family income, or similar characteristics. A small number of designated secondary schools may retain limited academic-selection arrangements under separate rules, but these are not part of the general SAE priority order analyzed here. Sibling claims work through "family applications," a dynamic procedure that links siblings' applications so they are more likely to land at the same school, rather than treating each child in isolation.

Three of these features bear directly on the analysis that follows. The first is that the vulnerable-student category is the very administrative classification behind the School SES measure used throughout the paper: because schools must reserve a minimum share of seats for priority students and cannot screen on family background, the school-level share of non-priority students is a meaningful, policy-grounded proxy for social composition rather than an arbitrary index. The second is that, with ties broken by a single random number per applicant, where an applicant ends up depends not only on how they ranked a school but also on priority standing and luck, which is why the admission-stage decomposition later in the paper is read as descriptive rather than as a direct window onto preferences. The third is that sibling priority and family applications leave some rankings, especially at the youngest entry grades where staying with an older sibling's school is common, reflecting considerations other than a fresh weighing of school attributes, a caveat that matters when interpreting the less stable Kinder estimates.

A fourth feature concerns geography. In some systems residential proximity carries explicit admission priority, as with catchment-area rules in parts of the United Kingdom, where distance can decide access. SAE works differently: proximity is simply not among its priority criteria, which are defined by siblings, vulnerable-student status, staff children, and former students, with home-school distance playing no part. The application data mirror this. An applicant's priority standing is recorded through a small set of indicators (sibling, vulnerable-student, staff-child, former-student, and special-quota priority), none of them distance-based; home-school distance enters only as a continuous school attribute to be ranked, not as a lever on admission. This shapes how the distance results should be read. Because distance does not mechanically improve an applicant's odds of admission, the weight families place on proximity reflects how they rank schools, not an institutional edge handed to nearby options. Because the rank-ordered model draws only on the schools an applicant actually ranked, without completing short lists, the distance coefficient comes entirely from observed ranking decisions rather than from imputed alternatives.

In form the SAE application is simple, but it carries a great deal of information. Families log in, pick schools, and rank them from most to least preferred; the system then places each student at the highest-ranked option that still has room, subject to capacity and the legal priority rules. Order is consequential: once a student can be seated at a higher-ranked option, the lower-ranked ones drop out of the assignment entirely. The platform also shows families a good deal about each school, including its gender composition, administrative type, educational model, participation in support programs, and whether it charges fees or co-payments. The visibility of gender composition matters for this thesis, because gender-related information is part of the environment in which families rank schools rather than something hidden from them.

How that placement is produced is worth spelling out, because the same rules can read either as screening or as a lottery depending on how they are described, and neither label is quite right. The first point is that schools do not choose among applicants at all. If a school-grade has enough seats for all the applicants who reach it in the assignment process, none of them is rejected for lack of capacity; the priority categories and the random number bind only when a school-grade is over-subscribed.

When a school-grade is over-subscribed, the assignment proceeds as a sequence of provisional offers, revised as the algorithm proceeds, rather than a single sort. Each applicant is first considered at the school they placed first. An over-subscribed school provisionally holds the applicants with the strongest priority claims up to its capacity and sets the rest aside, with the single random number breaking ties within a priority category. A set-aside applicant is then considered at the next school on their own list, where they may displace someone the school was only provisionally holding if their priority claim is stronger. The process repeats until no applicant can be moved to a school they prefer, at which point each applicant holds at most one seat: the highest school on their own list that could still accommodate them given capacity and priorities. Because the procedure works through each applicant's own ranking and never lets a school reach past a higher-priority applicant to admit a lower-priority one, it closely follows the logic of the student-proposing deferred-acceptance algorithm of Gale and Shapley (1962), run at national scale and adapted to Chile's legally defined priorities, quota rules, and family-application procedures.

This deferred-acceptance structure is central to how the ranked lists can be read. Under the standard deferred-acceptance assumptions, with fixed priorities and capacities, ranking schools by true preference is a weakly dominant strategy (Abdulkadiroğlu and Sönmez 2003; Pathak and Sönmez 2008). Two features of SAE make that property unusually pertinent here. First, the platform places *no cap* on how many schools a family may rank: the official rules require at least two establishments, recommend at least six, and otherwise let applicants list as many schools as they would be willing to attend (Ministerio de Educación de Chile 2025a). Second, throughout the period studied, ties at over-subscribed schools are broken by a *single random lottery number* drawn after applications close, so applicants cannot know their tie-breaking position when they rank (Correa et al. 2022).[1]

These two features matter because they line up closely with the conditions under which a truthful ranking is identifying. Fack, Grenet, and He (2019) show that strict truth-telling becomes a dominant strategy when applicants can rank schools at no cost and without list-length limits, and that the case for reading rankings as truthful is strongest when admission probabilities are genuinely uncertain, "such as in systems where priorities are determined by lotteries." Both conditions hold for SAE over the sample period: there is no list-length cap, and tie-breaking is by lottery, so applicants face real admission uncertainty and have little room to "skip the

[1] Changes announced for the 2026 admission procedure fall outside the 2017–2025 period analyzed here and do not affect the estimates; throughout the sample period, ties at over-subscribed schools were broken by a single random lottery.

impossible" in the way documented for strict-priority, test-score systems.[2]

Appendix Table A27 offers a direct diagnostic consistent with this reading: the large majority of listed schools are oversubscribed at every stage, including over ninety percent in Secondary, and the average across all listed schools remains heavily oversubscribed, so there is little aggregate evidence that applicants systematically retreat to clearly safe options, though this aggregate diagnostic cannot rule out that individual applicants omit schools they perceive as unattainable.

The contrast with Chilean *college* admissions is sharp, since there applicants see their own scores before applying and Larroucau and Rios (2020) find that only a minority rank their true top choice. For the school-level setting studied here, then, a preference-revealing reading of rankings rests on much firmer ground than it would in score-based admissions. This distinction also maps onto the methodological literature: recent surveys of preference estimation under centralized assignment treat lottery-based, list-unconstrained settings as the case in which truthful ranking is most defensible, whereas in strict-priority (test-score) systems, where stability-based methods are often preferred and weak truth-telling is frequently rejected, the assumption is far more fragile (Che, Grenet, and He 2025). SAE belongs to the former case, not the latter.

Even so, the paper adopts the more conservative *weak* truth-telling assumption of Fack, Grenet, and He (2019). Rather than assume that families list *every* acceptable school, it assumes only that, among the schools an applicant did choose to list, the submitted order is truthful. The rank-ordered model accordingly takes each applicant's full submitted ranking as a truthful ordering of the schools they listed, while the prior decision of how many and which schools to list is set aside as a question of choice-set formation rather than treated as part of the preference signal (the construction of the analysis choice set is detailed in the data and empirical-strategy section).

The caution is warranted: even without a list-length cap, many SAE families apply to only a handful of schools, averaging about three to four in the estimation sample and rising from roughly 2.9 schools in Prekinder to 3.7 in Secondary, and may overestimate their admission chances (Honey and Carrasco 2023; Arteaga et al. 2022), so it would be heroic to read a short list as an exhaustive statement of which schools a family found acceptable. Weak truth-telling sidesteps that assumption, using the observed ordering of the listed schools without also requiring the list to be complete. The mechanism is otherwise adapted to national legal rules (sibling priority, priority for vulnerable students, priority for children of school staff, priority for former students, quota rules, and family-application rules), so the rank order is read as preference-revealing *conditional on the set of schools included in the submitted ranked list*. The paper therefore studies how a family orders the schools it lists, not why those particular schools rather than others were listed in the first place; that prior step of choice-set formation is left as a separate empirical limitation. All told, SAE is a broad institutional arrangement that ties together ranked preferences, school capacity, priority rules, lottery tie-breaking, supplementary rounds,

[2]The lottery has a second econometric role elsewhere, generating quasi-random variation in admission that would identify the causal effect of attending a given school; that use is not pursued here. In this paper the lottery matters because the admission uncertainty it creates supports a truthful reading of the submitted rankings.

and an administrative-data infrastructure.

Even under these conditions, weak truth-telling remains an assumption rather than a guarantee. Work on centralized choice shows that some families still submit non-truthful lists, most often because they hold biased beliefs about their admission chances rather than because the mechanism rewards misreporting, and that such mistakes tend to be more common among lower-income and less-informed families (Arteaga et al. 2022). The estimates here rely only on the order of the schools a family did list, not on the stronger claim that the list is complete. Because the gender comparison is made within applicants who face the same platform and information environment, belief-driven mistakes would bias the gender interaction only if they differed systematically between the families of girls and boys applying in the same stage and market. The paper cannot rule this out, and returns to it among the limitations.

One can go further and state the likely direction of any residual bias. The most plausible form of differential strategic behavior under deferred acceptance is the omission of high-demand, high-SES schools that a family perceives as effectively unreachable. Because higher-SES secondary schools are on average more oversubscribed, such omission would remove high-SES options disproportionately from the lists of the more risk-averse applicants. If the families of girls do this more than the families of boys, the revealed within-list weight on School SES is understated for girls, so the estimated Female $\times$ Relative School SES coefficient is attenuated toward zero and should be read as a conservative lower bound on the underlying preference difference. Appendix Table A27 shows that the oversubscription of listed schools is very similar for girls and boys, which limits the scope for this channel without ruling it out.

This structure matters because SAE records ranked preferences before assignment, not just final enrollment. Final placements blend family rankings with vacancies, priority rules, and the lottery, whereas the submitted lists isolate the family side of the process. They let the analysis ask whether families of female and male applicants attach different weights to the same school attributes before the mechanism settles the final matches.

A centralized platform also gives every applicant a common frame: families apply through the same system under the same basic rules, which makes comparisons across them more meaningful. For the question asked here, that is the decisive advantage, since it lets the paper compare how families weigh school quality, distance, social composition, and demand within one shared application setting.

3 Literature Review

This study draws on four connected literatures: how a submitted ranking should be interpreted, what families value when they choose schools, why social composition is a salient dimension of that choice in Chile, and whether the weight families place on school attributes differs by the applicant's gender before assignment. The last of these has received the least attention, and it is the question the thesis takes up.

Mechanism design and centralized assignment come first. Gale and Shapley (1962) provide the theoretical foundation for the deferred-acceptance algorithm, and Abdulkadiroğlu and Sönmez (2003), Pathak and Sönmez (2008), and Abdulkadiroğlu, Pathak and Roth (2009) show how the choice of mechanism shapes preference submission, strategic ranking, stability, and fairness. For this thesis, the relevant point is that a submitted ranking is a statement of preference made under a specific set of rules. This literature does not ask why two families might value the same school attribute differently, because its concern is whether a mechanism is strategy-proof or stable. It defines the institutional setting for the analysis but does not answer the paper's question.

Once ranked lists are read as evidence of family-side choice, the next question is which attributes families weigh, and a large revealed-preference literature has measured exactly that. The recurring finding, across very different systems, is that families care about several attributes at once: academic quality, proximity, and the peer or social composition of the student body; the weights differ systematically by income and race. Hastings, Kane and Staiger (2005), pairing a model of parental choice with randomized lotteries in a Charlotte-Mecklenburg choice program, find that families value proximity and peer characteristics alongside test scores; Hastings and Weinstein (2008) then show how malleable the quality margin is, since handing lower-income families simple test-score information raised the share choosing higher-performing schools and in turn lifted student scores.

The same composite of attributes shows up in the discrete-choice estimates this paper builds on. Burgess et al. (2015) estimate a conditional logit on survey, administrative, census, and spatial data for England and find that families weigh the social and demographic composition of the student body even as most care strongly about academic performance, and Glazerman and Dotter (2017), studying more than 22,000 rank-ordered lists in Washington, D.C.'s citywide lottery, find that distance, school demographics, and academic indicators all matter, with higher- and lower-income families responding to different markers of quality.

This literature shapes the present paper in two ways. It identifies the attributes worth studying, since quality, distance, school social composition, and demand are the dimensions that families repeatedly weigh in these studies. It also provides the method: the conditional logit and related random-utility models, estimated on submitted rank-ordered lists, which this paper adapts to the SAE setting. What these studies have mostly left constant is applicant gender. They compare families across income or information far more often than across the gender of the child. That is the gap this paper takes up, asking whether the families of female and male applicants place the same relative weights on the same attributes within the same choice set.

In Chile, this question is more than a generic exercise in preference estimation, because the history of the school market has made social composition an unusually salient axis of choice. The nationwide flat per-student voucher introduced in 1981 set off a rapid expansion of subsidized private schools, and the public sector's enrollment share slid from roughly 80 percent in the 1970s to below 60 percent by 1990. What this expansion of choice produced was not higher average achievement but sorting: Hsieh and Urquiola (2006) find no aggregate test-score gains and instead a flight of relatively advantaged students out of the public system, which they read as competition working through student composition rather than through across-the-board quality. The resulting stratification is large and consistently documented. Valenzuela, Bellei and de los Ríos (2014) estimate Duncan dissimilarity indices for socioeconomic segregation of roughly 0.50 to 0.60 in the late 2000s, higher in secondary schools and in the private-voucher sector than in public schools; and Santos and Elacqua (2016), Mizala and Torche (2012), and McEwan and Urquiola (2008) trace, from different angles, how parental choice, school-performance information, and the voucher structure together reproduce socioeconomic sorting across schools and across the achievement distribution. The common thread is sorting driven as much by demand-side responses as by school-side selection.

This stratification matters for the present paper because it looks partly demand-driven, not merely a by-product of school-side selection. Elacqua, Schneider and Buckley (2006) are especially telling here. In surveys, parents of every background name academic quality as their main criterion and almost never mention the social composition of the student body; yet when the authors look at how parents in the Metropolitan Region of Santiago actually search and assemble their choice sets, the revealed choices track the class composition of the student body more closely than measured academic performance. That gap between what parents say and what their choice sets reveal is itself part of the case for studying behavioral data such as submitted rankings rather than stated preferences. Hofflinger, Gelber and Tellez Cañas (2020) add that Chilean parents weigh distance, quality, and religion at once, and that these trade-offs themselves shift with socioeconomic status. Taken together, the studies portray a stratified market in which family choice, school reputation, peer composition, and educational inequality feed on one another, and in which a school's social composition is something families plausibly compare when they rank their options.

School social composition is, in short, a substantive attribute that Chilean families may weigh alongside quality, distance, and institutional type when they size up schools. Section 4 sets out how these variables are defined and kept distinct in the data.

A smaller body of SAE-specific research supplies the setting in which this process can be observed before assignment. Correa et al. (2022) examine the design and implementation of Chile's centralized system, showing how its institutional rules shape the very preference data researchers then analyze, and Hernández et al. (2023) argue that SAE applications are more than technical inputs: they reveal how families understand fairness, responsibility, and school fit. Both support using SAE application data to study pre-assignment preferences. Neither, however, makes gender differences in attribute weights its central concern.

The last strand connects this application-stage evidence to research on gender differences in educational choice. A large literature shows that men and women who perform equally well can still choose differently when a choice involves competition or risk. In the canonical laboratory experiment, Niederle and Vesterlund (2007) find no gender gap in performance on a real task, yet when participants choose how to be paid for the next round, 73 percent of men opt into a competitive tournament against only 35 percent of women, a gap the authors trace largely to differences in confidence and attitudes toward competition. Buser, Niederle and Oosterbeek

(2014) show that this kind of competitiveness predicts the academic tracks students later choose. Moving from the lab to administrative and survey data, Saygin (2016), Delaney and Devereux (2021), and Bordón, Canals and Mizala (2020) document that gender differences appear not only in final outcomes but already in aspirations, risk-taking, and application strategies, including in the Chilean setting of college and major choice. The downstream imprint is visible in the gender gap in college major: Zafar (2013) finds that men and women weigh the non-pecuniary side of fields of study differently, while Speer (2017) finds that much of the gap is set by pre-college skills and choices rather than appearing at the point of college entry. A caveat runs through this strand and bears on the present design: much of it identifies gendered choice by pairing observed choices with survey measures of subjective expectations and beliefs, which the SAE records do not contain. Because a given ranking is consistent with many combinations of beliefs, preferences, and constraints, the interaction estimates below are read as gendered differences in revealed relative weights, not as evidence on beliefs or preferences separately (Giustinelli 2023).

A related channel runs through geography: distance, transport costs, and perceived safety can constrain schooling differently by gender, especially where parents are more protective of girls' daily mobility (Burde and Linden 2013; Muralidharan and Prakash 2017). Those studies, though, come from settings where distance can keep girls out of school altogether. The SAE setting is different, since the paper observes choices among schools inside a centralized system rather than the extensive-margin decision of whether a girl attends school at all, so the analogy is narrower: here, gendered protection and mobility concerns should surface as different weights on commuting distance and school environment, not as non-enrollment.

A more direct, family-level basis comes from work on sons, daughters, and parental behavior, which shows that the gender of children can shape parental expectations, time allocation, investments, and family routines (Lundberg 2005; Raley and Bianchi 2006). This literature predicts no single direction for any given school attribute, and it does not show who actually makes the SAE ranking decision in these data. What it does support is the more modest prior that families may judge school environments differently for a daughter than for a son, which is enough to motivate asking whether applicant gender shifts the relative weight placed on distance, school social environment, demand, and quality.

Ngo and Dustan (2024) come closest to the present study, since they too use centralized admission data and deliberately separate application-stage preferences from assignment outcomes. Drawing on Mexico City's centralized high-school system and a structural model of program choice, they decompose the STEM gender gap and find that most of it reflects gendered choices, with males preferring STEM programs more strongly, while test-based assignment further narrows elite STEM access for females, who score lower on the placement exam despite comparable performance on a low-stakes test. Three things set this paper apart. The setting is Chile's SAE, which spans a far wider range of entry stages, from early childhood through primary and secondary, rather than a single high-school transition, so the balance between adult and student decision-making can be allowed to shift across stages. The object of study is not access to a particular academic track but the relative weight placed on general school attributes within each applicant's own choice set. And, as a result, the central finding concerns school social composition rather than program-field choice. Put briefly, Ngo and Dustan ask how gendered preferences and a test-based mechanism sort students into STEM; this paper asks how, before the mechanism produces any placements, the social composition of schools enters submitted rankings differently for girls and boys.

SAE is especially well suited to this question because the meaning of a gendered choice shifts with the educational stage. Gender differences in college or track applications may reflect students' own interests, career expectations, and appetite for competition. At the youngest stages, children have little independent information with which to judge schools, so their submitted rankings are likely to lean more on adult judgments about care, commuting, continuity, and environment. By secondary level, students may also help shape the ranking. The estimates keep these household and student channels together, so the stage pattern is interpreted as an application-stage pattern rather than as a claim about who made each decision.

The four main variables follow from this reasoning: quality as a formal performance signal, distance as commuting and daily cost, School SES as social composition, and Top3 Demand as the market demand other families generate. The empirical question is whether the relative weight placed on each shifts with the gender of the applicant.

If the weight families place on these attributes shifts across stages, as the same literature suggests, then a gender difference that varies by stage may track the changing meaning of school choice as children grow older. The 2025 data compare stages and markets observed in the same year rather than one cohort followed over time. This is the reason for estimating the model separately by stage rather than pooling all applicants together.

The contribution of the paper is to join bodies of work that usually stay apart. Centralized school choice and SAE research provide a setting in which ranked preferences can be observed, while the literature on gender differences in education suggests that applications may already carry gendered expectations long before final educational outcomes appear. Set against the closest centralized-assignment evidence, the paper adds three narrower pieces. Methodologically, it estimates a rank-ordered (exploded) logit model on choice-set-centered school attributes. Institutionally, it studies Chile's SAE across earlier stages, where adult involvement is more plausible, while leaving room for students' own preferences later on. Substantively, it singles out school social composition as the most stable gendered attribute, with demand and distance showing only stage-specific patterns.

This framing also sets the thesis apart from much existing work. Instead of treating final enrollment as the full expression of family preference, it studies the ranking of a choice set before assignment. For the schools each applicant ranked, the model asks which are placed earlier and whether that relative weight differs by applicant gender. The study of gender differences thus moves from final outcomes to the application stage, and from which school was chosen to how school attributes were weighed.

4 Data and Empirical Strategy

4.1 Data source and sample construction

The analysis is built from 2025 SAE application data. The raw records list the schools each applicant ranked in the system, together with the applicant's gender, school ID, preference order, student-school distance, and a set of school attributes: quality, social composition, demand, vacancies, size, type, religious affiliation, rural status, and single-sex status. Because the main models are about how families rank schools at the application stage, the object of analysis is the preference list submitted to SAE, not final enrollment. A second file, the 2025 regular-stage result file, supports a supplementary admission-stage decomposition of the gender gap in admitted-school SES; there, the admitted school is taken as the post-response and wait-list assignment where that field is observed, and otherwise as the initial regular-stage assignment. That file records which school an applicant was admitted to in the regular stage, but it does not observe whether the student actually enrolled. Unless otherwise noted, every table in this thesis presents the author's own calculations and estimations based on these SAE microdata (Ministerio de Educación de Chile).

The sample construction, descriptive statistics, and admission-stage decomposition all use the 2025 microdata. For a historical cross-cohort extension, the paper also uses comparable long-format regression outputs for 2017–2024. These files contain stage-specific rank-ordered (exploded) logit estimates for the same core gender interactions as the 2025 main model, which allows a check of whether the 2025 findings hold across earlier SAE cohorts. Because the older result files use different sample metadata and cover different applicant cohorts, the paper compares them only at the level of estimated coefficients and does not pool the microdata. The 2017–2024 evidence is placed in the appendix and read as repeated cross-sectional support for the main application-stage patterns.

The basic unit of observation is the student-school pair (i, j), where i is the applicant and j is one school in the applicant's ranked list. One applicant contributes several student-school observations, one for each school they actually ranked. The rank-ordered (exploded) logit model uses each applicant's true submitted ranking directly, so the analysis represents each applicant by the set of schools they ranked, in the order they ranked them.

Using the true submitted ranking, rather than a fixed-length standardized set, is a deliberate methodological choice. Most applicants rank only a few schools: in the national data the median list contains three schools, and only a small minority submit long lists. A specification that pads every applicant's list to a fixed length must therefore impute a large number of unranked alternatives for the majority of applicants, and those imputed comparisons are not observed ranking decisions. By contrast, the exploded logit conditions only on schools the applicant genuinely ranked, and applicants with longer lists simply contribute more ranking comparisons than applicants with shorter lists.

The estimation sample keeps every applicant who ranked at least two schools, since at least two ranked alternatives are needed to form one ranking comparison. No alternatives are added by completion: an applicant who ranked two schools contributes those two, an applicant who ranked five contributes those five, and so on. This removes the concern that completing short lists with nearby schools could mechanically affect the attribute estimates, in particular the distance coefficient, for the subset of applicants with short lists. Appendix Table A28 reports the within-list variation in distance by stage, confirming that the schools an applicant lists differ enough in distance to identify the distance coefficient from genuine within-list variation. Because the model uses the submitted ranking, it does not reduce the outcome to a single binary ”top-three” indicator. Instead, each applicant’s ranking is decomposed into the sequence of conditional choices described in the empirical-strategy section: the first-ranked school is treated as chosen from all ranked alternatives, the second-ranked from the remainder, and so on. The estimates should therefore be interpreted as relative attribute weights that govern how schools are ordered within each applicant’s own ranked set, rather than as unconditional market-level preferences.

The sample cleaning follows three steps. First, observations with missing core variables are dropped, including applicant ID, school ID, gender, preference order, distance, school quality, school social composition, and school demand. Second, duplicate student-school pairs are removed so that one applicant-school pair appears only once. Third, the sample keeps every applicant who ranked at least two valid schools, since the rank-ordered (exploded) logit needs at least two ranked alternatives to form a single ranking comparison; no alternatives are added by list completion. The cleaning process is reported in Table 1.

Table 1. Sample attrition during data cleaning

Step	Applicants	Student-school obs.	Schools	Applicant share
Original applications (regular stage)	463,923	1,565,505	7,644	100.0%
After cleaning and keeping applicants who ranked at least two valid schools	448,137	1,512,610	7,613	96.6%

Notes: “Original applications” are the 2025 regular-stage submitted applications (Ministry of Education, C1 records: 463,923 applicants and 1,565,505 applications). Cleaning drops applications with missing core variables and duplicate applicant-school pairs, then keeps applicants who ranked at least two valid schools, counting only genuinely ranked schools with no list completion. Applicant share is calculated relative to the first row. The final row is the rank-ordered estimation sample used in Table 2 and the regression tables, with 448,137 applicants and 1,512,610 student-school observations.

4.2 Descriptive statistics

Table 3 reports descriptive statistics for the pooled student-school analysis files. These files are organized at the applicant-school level and include the schools that applicants ranked in their submitted preference lists. The table is used only to characterize applicant characteristics and candidate-school attributes before the main regression transformations. Core continuous variables are later winsorized, choice-set centered, and standardized, so this table should be read as descriptive rather than as the final regression scale.

Table 2. Sample structure by educational stage

Stage	Grades included	Applicants	Ranked rows	Schools	Female share
Prekinder	Prekinder	83,667	243,762	4,666	0.509
Kinder	Kinder	23,317	70,520	4,811	0.481
Early Primary	Grade 1–4 Básico	89,779	293,222	6,770	0.474
Upper Primary	Grade 5–8 Básico	87,068	298,061	6,625	0.516
Secondary	Grade 1–4 Medio	164,306	607,045	2,549	0.501
Total	All stages	448,137	1,512,610	7,613	0.499

Notes: Applicants and ranked rows are the rank-ordered estimation sample, namely every applicant who ranked at least two schools, counting only genuinely ranked schools with no list completion. Schools are unique schools within each stage after deduplication, not the simple sum of schools across grades. Female share is calculated at the applicant level.

Table 3. Descriptive statistics of main variables

Variable	Mean	SD	Min	Max	Description
Female	0.499	0.500	0.000	1.000	Applicant is female
Top3	0.429	0.495	0.000	1.000	School is ranked among applicant top three choices
Distance km	6.622	66.730	0.001	4113.191	Distance from student residence to school, in kilometers
Log Distance	1.108	0.871	0.001	8.322	Log form of student-school distance
Quality	64.617	5.683	41.501	95.797	School quality indicator
School SES	0.418	0.168	0.000	1.000	Proxy for school social composition
Log Top3 Demand	3.818	1.333	0.000	7.164	School demand or popularity
Vacancies	37.299	56.738	0.000	941.000	Number of available SAE seats
Log Vacancies	2.896	1.267	0.000	6.848	Log form of SAE vacancies
Any Priority	0.091	0.288	0.000	1.000	Applicant has any priority at this school
School size	762.599	538.313	1.000	3923.000	School size, total enrollment
Log School Size	6.368	0.825	0.693	8.275	Log form of school size
Rural	0.076	0.265	0.000	1.000	School is located in a rural area
Single-sex school	0.031	0.173	0.000	1.000	School is single-sex and gender-compatible in the analysis set

Notes: Table 3 reports student-school level descriptive statistics. Top3 here is a descriptive indicator for whether a school falls in an applicant's three highest-ranked choices, reported for the analysis files; the main regressions instead use the full submitted ranking rather than this binary indicator. The large maximum of Distance km shows that raw distance contains extreme values. The regressions use $\log(1 + distance)$ and apply 1%/99% winsorization for this reason.

Several features of Table 3 stand out before the variables are transformed. The Top3 indicator is reported here only as a descriptive row-level indicator for whether a school belongs to an applicant's three highest-ranked submitted choices. Its mean of about 0.43 means that 42.9 percent of the student-school rows in these descriptive files correspond to schools ranked within an applicant's top three. This variable is used here only to summarize the ranked application data; the main regressions exploit the full submitted ordering of schools rather than reducing the ranking to this binary indicator. The raw distance variable is the clearest illustration of why the transformations described below are necessary: its mean is only about 6.6 kilometers, but its standard deviation is roughly ten times larger and its maximum exceeds 4,000 kilometers. A distance of this magnitude cannot represent a routine daily commute and almost certainly reflects either data error or an unusual special case, which is why the regressions use $\log(1 + distance)$ together with 1%/99% winsorization rather than raw kilometers. School SES, the central attribute of the paper, has a mean of about 0.42 and ranges across the entire interval from 0 to 1, indicating that the schools families consider differ very widely in social composition; this dispersion is what makes a within-choice-set comparison meaningful in the first place, since there is real variation to rank over.

Two attributes that receive separate attention later are, by contrast, rare in the pooled sample: single-sex schools account for about 3 percent of student-school observations and rural schools for about 8 percent, so the corresponding interactions describe gendered choice around relatively uncommon school types rather than a feature of the typical alternative. Finally, applicants hold a priority claim at only about 9 percent of the schools in their submitted choice sets, which is relevant for the admission-stage decomposition, where priority status helps determine who is actually placed when seats are scarce.

Because the empirical strategy centers school attributes within each applicant's submitted choice set, one concern remains: female and male applicants might enter the ranking stage with systematically different observed choice sets before any within-set ordering takes place. Table 3a checks this by reporting applicant-level averages of submitted-list attributes by gender. The two groups look broadly alike in the raw attributes of the schools they consider, with most standardized differences small in absolute value. The largest differences appear in Secondary, where girls' submitted choice sets carry a modestly higher average School SES and quality. This does not rule out gendered choice-set formation, but it does suggest that the main estimates are not being driven by large raw differences in the composition of the submitted lists themselves. The coefficients are therefore best read as gender differences in relative ranking weights, conditional on the observed submitted choice set, rather than as proof that the set was assembled identically for girls and boys.

Table 3a. Choice-set attributes by applicant gender

Stage	Choice-set mean variable	Male	Female	Difference	Std. diff.
Prekinder	School SES	0.401	0.402	0.002	0.013
	Quality	64.839	64.872	0.032	0.013
	Distance km	5.502	4.558	-0.944	-0.016
	Log Distance	1.023	1.004	-0.018	-0.032
	Log School Size	6.169	6.180	0.011	0.020
Kinder	School SES	0.438	0.439	0.001	0.008
	Quality	64.921	64.999	0.077	0.030
	Distance km	4.922	5.986	1.064	0.017
	Log Distance	0.941	0.944	0.003	0.006
	Log School Size	6.301	6.302	0.001	0.003
Early Primary	School SES	0.427	0.427	-0.000	-0.000
	Quality	64.697	64.803	0.106	0.040
	Distance km	6.265	6.532	0.268	0.005
	Log Distance	0.935	0.940	0.006	0.012
	Log School Size	6.195	6.191	-0.003	-0.005
Upper Primary	School SES	0.432	0.434	0.002	0.015
	Quality	64.973	65.146	0.173	0.066
	Distance km	7.381	6.502	-0.879	-0.021
	Log Distance	1.019	1.009	-0.010	-0.018
	Log School Size	6.300	6.317	0.017	0.032
Secondary	School SES	0.406	0.423	0.017	0.128
	Quality	64.002	64.273	0.272	0.102
	Distance km	7.591	7.634	0.043	0.001
	Log Distance	1.331	1.315	-0.016	-0.022
	Log School Size	6.601	6.612	0.011	0.034

Notes: The unit is the applicant. Each entry is the mean attribute value across the applicant's submitted list of valid ranked schools. This descriptive comparison can cover a slightly larger group than the rank-ordered estimation sample in Tables 1, 2, and 5, because the main estimation sample keeps only applicants who ranked at least two schools. Difference is Female minus Male. Std. diff. divides the difference by the pooled standard deviation of the applicant-level choice-set mean. Applicant counts are: Prekinder 44163 male and 45752 female; Kinder 12406 male and 11510 female; Early Primary 47982 male and 43222 female; Upper Primary 42507 male and 45322 female; Secondary 83765 male and 84159 female.

Table 3a links the descriptive evidence to the ranking model. Male and female applicants submit lists with broadly similar observed composition. The gender gaps are small in all stages, although they rise with age: the standardized gap in average choice-set quality increases from about 0.01 standard deviations in Prekinder to about 0.10 in Secondary, and the School SES gap follows a similar profile, reaching a comparable magnitude (about 0.13) in Secondary. Secondary is therefore the stage in which the later ranking results are strongest and the stage with the largest observed difference in submitted-list composition.

A second dimension of the submitted choice set is its length, the number of schools an applicant ranks. Because the rank-ordered model uses each list as submitted, without padding short lists, a systematic gender difference in how many schools families rank could in principle shape the comparison. Table 3b shows that it does not. List length is heterogeneous, with a median of three: ninety percent of applicants rank six or fewer schools, while a small tail of about 2.4 percent rank eight or more, up to a maximum of 89. It is nonetheless close to balanced across gender, with girls' applications marginally longer on average (3.35 against 3.31 overall) and standardized gender differences below 0.03 at every stage except Secondary, where girls rank 3.72 schools on average against boys' 3.62 (a standardized difference of 0.05). These differences are small. Because the model uses each applicant's list as submitted, the balanced length distribution means the gender comparison is not confounded by girls and boys ranking systematically different numbers of schools; where lists are longer, applicants simply contribute more within-list ranking comparisons.

Table 3b. Submitted list length by applicant gender

Stage	Male	Female	Difference	Std. diff.
Prekinder	2.77	2.78	0.005	0.004
Kinder	2.96	2.96	0.002	0.002
Early Primary	3.24	3.21	-0.038	-0.024
Upper Primary	3.41	3.45	0.040	0.022
Secondary	3.62	3.72	0.101	0.054
Total	3.31	3.35	0.040	0.023

Notes: The unit is the applicant; list length is the number of distinct schools an applicant ranked in the 2025 regular-stage application file (Ministry of Education, C1 records; 463,923 applicants). Entries are mean list length by gender; Difference is Female minus Male; Std. diff. divides the difference by the pooled standard deviation of list length. List length ranges from one to 89, with a median of three (two in Prekinder); the rank-ordered estimation sample in Tables 1, 2, and 5 keeps applicants who ranked at least two schools. Applicant counts are: Prekinder 44,477 male and 46,081 female; Kinder 12,545 male and 11,633 female; Early Primary 48,391 male and 43,621 female; Upper Primary 42,862 male and 45,663 female; Secondary 84,152 male and 84,498 female.

Even there, the differences remain modest. The largest standardized gap is about 0.10, while most cells are much closer to zero. The table therefore supports reading the subsequent coefficients as gender differences in how schools are ordered within broadly comparable submitted lists. Appendix Table A34 complements this diagnostic by re-estimating the Secondary model within applicants' regions of residence.

4.3 Variable construction

Table 4 summarizes the variables used in the empirical analysis. The main continuous school attributes are transformed into relative measures before estimation, so that they capture a school's position within the applicant's own submitted-ranking choice set rather than its raw level in the full market.

Table 4. Variable construction

Variable used in regression	Original variable / source	Definition	Transformation
Top3$_{ij}$	preference_order; is_top3_preference	Equals 1 if school j is ranked among applicant i's top three choices; 0 otherwise. Reported for description only; the estimation uses the full submitted ranking, not this indicator.	Binary variable
Female$_i$	is_female	Applicant is female.	Binary variable
Relative Quality	sned_composite_index_indice	Official school quality or performance indicator.	1%/99% winsorized; choice-set centered; standardized
Relative Log Distance	student_to_school_distance_km	Distance between applicant residence and school.	$\log(1 + \text{distance})$; 1%/99% winsorized; choice-set centered; standardized
Relative School SES	school_non_priority_student_share	School social composition or relative SES proxy.	1%/99% winsorized; choice-set centered; standardized
Relative Log Top3 Demand	log_school_grade_top3_applications	Number of school-grade applications listing the school among the top three choices.	1%/99% winsorized; choice-set centered; standardized
Log Vacancies	sae_vacancies	Number of available seats in SAE.	$\log(1 + \text{vacancies})$; winsorized; standardized
Any Priority	has_any_priority	Whether the applicant has any priority at the school.	Binary variable
Log School Size	school_total_enrollment	Total school enrollment or school size.	$\log(1 + \text{school size})$; winsorized; standardized
Rural	is_rural	Whether the school is located in a rural area.	Binary variable
Single-sex school	boys_only_school; girls_only_school	Whether the school admits only boys or only girls; sex-specific eligibility is used to exclude schools restricted to the other sex from feasible alternatives.	Binary variable

Notes: Top3$_{ij}$ is a descriptive indicator equal to one when school j is among applicant i's first three ranked choices; it is not the estimation outcome, since the model uses each applicant's full submitted ranking. Female$_i$ enters only through interactions, because it does not vary across the schools of a single applicant. Core continuous variables are winsorized, centered within each applicant's submitted-ranking choice set, and standardized before entering the regressions. The main rank-ordered logit includes rural and single-sex school indicators, but does not include school-type or religion fixed effects.

The central variable is Relative School SES, built from school_non_priority_student_share, the school-level share of students without priority status. It should not be read as the applicant's own family SES, parental income, or parental education; what it measures is the social composition of the school itself.

In Chilean education policy, priority status is tied closely to socioeconomic vulnerability and eligibility for support. A higher non-priority share therefore signals a smaller concentration of vulnerable students at a school, which makes it a workable proxy for relatively higher social composition. One clarification matters here: the "priority" in this share refers specifically to the socioeconomic-vulnerability category (the SEP priority that reserves seats for eligible students), which is one of the SAE admission priorities but distinct from the sibling, staff-child, and former-student priorities. School SES uses only this vulnerability-based classification.

As a school-level proxy for social composition, School SES does not directly measure family income, school safety, climate, social capital, or peer quality, and it cannot establish that families rank schools because of any of these. Its job is narrower: to capture relative differences in social composition across the schools in a single choice set.

The decision to treat School SES as a substantive attribute follows from the Chilean school-choice literature discussed above. In this setting, school social composition can be related to achievement, reputation, peer environment, family networks, fees, and local educational opportunities. A ranking advantage for higher-SES schools may reflect more than a preference for formal school quality. For this reason, the model includes School SES, quality, and demand at the same time, so that the estimates separate, as far as the data allow, social composition, formal quality signals, and market demand.

To show what this proxy captures, the paper reports correlations between School SES and other observable school attributes. Tables 4a and 4b are numbered as diagnostic subparts of Table 4 because they evaluate the variables defined there. The diagnostic check is calculated after deduplicating school_id × application_grade, so that schools are not weighted more heavily simply because they receive more applications.

Table 4a. Diagnostic correlations between School SES and other school attributes

School attribute	Corr. with School SES	N
Quality	0.251	64,846
Log Top3 Demand	0.472	64,914
Log Vacancies	-0.122	64,914
Log School Size	0.511	64,914
Rural	-0.439	64,914

Notes: Correlations are calculated after deduplicating school_id × application_grade. School SES is moderately related to quality, demand, and school size, but it is not identical to any single observed attribute.

Among these correlations, the one with Log School Size (0.511) is high enough to warrant a direct robustness check. The appendix adds a specification with Female × Log School Size to test whether the School SES interaction is simply capturing a gender-specific preference for larger schools. The paper also reports variance inflation factors to address multicollinearity. The diagnostic uses the four core continuous variables after 1%/99% winsorization, choice-set centering, and standardization.

Table 4b. Multicollinearity diagnostics for core centered variables

Variable	VIF
Relative Quality	1.27
Relative Log Distance	1.01
Relative School SES	1.24
Relative Log Top3 Demand	1.22
Condition index	1.69

Notes: Diagnostics use the four core continuous variables after 1%/99% winsorization, choice-set centering, and standardization. The condition index is reported for the same transformed variable set.

These diagnostics suggest that the core variables do not reach conventional levels of serious multicollinearity under the relative-variable construction used in the paper. School SES can be read as a school-composition dimension that is related to quality and demand, but not fully reduced to them. The gender differences reported below are conditional associations after controlling for observable quality and demand. They should not be read as a fully separate causal channel.

4.4 Choice-set structure and outcome variable

The empirical analysis is based on student-school level ranked lists. In the estimation sample, each applicant i is linked to the schools they actually ranked, C_i with $|C_i| = R_i \geq 2$, and the unit of observation is the student-school pair (i, j), where $j \in C_i$. The outcome is not a single binary indicator but the applicant's observed ordering of the schools in C_i:

$$\text{rank}_{ij} \in \{1, 2, \ldots, R_i\} \quad \text{is the position at which applicant } i \text{ ranked school } j,$$

with position 1 denoting the most preferred school. The exploded logit uses this ordering by decomposing it into a sequence of conditional choices, as set out in the model subsection: the school in position 1 is treated as chosen from all R_i schools, the school in position 2 from the remaining $R_i - 1$, and so on. Each applicant therefore contributes $R_i - 1$ ranking comparisons rather than a single combination choice, and every comparison is between schools the applicant genuinely ranked. The empirical question is a within-list ranking question: among the schools the applicant actually considered, the model asks which relative attributes make a school more likely to be ranked earlier, and whether those relative weights differ by applicant gender.

4.5 Rank-ordered (exploded) conditional logit model

The outcome is the applicant's true submitted ranking of schools, not a single binary inclusion indicator, so the model cannot treat each student-school observation as an independent result. For each applicant i, the choice set contains the schools that the applicant actually ranked, and the data record the order in which they were ranked. The model uses this full ordering directly, without padding short lists to a fixed length.

The analysis estimates a rank-ordered (exploded) conditional logit model. The key idea is that a ranking of R_i schools can be decomposed into a sequence of conditional choices: the first-ranked school is chosen from all R_i schools, the second-ranked from the remaining $R_i - 1$, and so on. Each applicant therefore contributes $R_i - 1$ nested conditional-logit comparisons rather than a single combination choice. Applicants who rank more schools contribute more comparisons; applicants who rank only two schools contribute one. No alternatives are added by completion, so the estimates are based entirely on observed ranking decisions.

Let C_i be applicant i's set of ranked schools, with $|C_i| = R_i \geq 2$, and let the observed ranking be $(r_{i1}, r_{i2}, \ldots, r_{iR_i})$, where r_{i1} is the most preferred school. The systematic utility of school j for applicant i is:

$$V_{ij} = X_{ij}\beta + Female_i \times X_{ij}\gamma + Z_{ij}\delta + Female_i \times Z_{ij}\zeta$$

Here, X_{ij} includes the core school attributes, namely school quality, distance, school social composition, and school demand. $Female_i$ indicates whether the applicant is female. $Female_i \times X_{ij}$ is the main interaction of interest and measures whether female applicants place different weights on school attributes compared with male applicants. Z_{ij} includes other displayed school controls, namely rural status and single-sex school status, which enter both with baseline coefficients δ and through their own gender interactions $Female_i \times Z_{ij}$ (with coefficients ζ), so the specification reports Female $\times$ Rural and Female $\times$ Single-Sex School alongside the core attribute interactions. School-type and religion fixed effects are not included in the main specification; a more conservative auxiliary specification with these fixed effects is discussed only as a supplementary check.

Under the exploded logit, the probability of the observed ranking is the product of the sequential choice probabilities:

$$P\big((r_{i1}, \ldots, r_{iR_i}) \mid C_i\big) = \prod_{t=1}^{R_i-1} \frac{\exp(V_{ir_{it}})}{\sum_{k \in C_i \setminus \{r_{i1}, \ldots, r_{i,t-1}\}} \exp(V_{ik})}$$

At each step t, the school ranked in position t is treated as chosen from the schools not yet ranked above it. The model estimates which school attributes make a school more likely to be ranked earlier than the other schools still available in the same applicant's list.

This design has two advantages. First, it fits the structure of the SAE ranking data without discarding ranking information or imposing list completion. Applicants form a relative ordering among candidate schools, and the exploded logit uses that entire ordering. Because it relies only on the true submitted lists, it is robust to the concern that padding short rankings with nearby schools could mechanically affect the estimates.

Second, the model uses relative differences across schools within the same applicant's ranked set. At each ranking step it compares the schools the applicant actually considered and asks which schools are ranked earlier because they are relatively higher in quality, nearer or farther, higher in social composition, or more popular. Because the comparison occurs within the same applicant, the conditional structure absorbs all applicant-level factors that do not vary across schools. These include broad educational preferences, information, family background, and familiarity with SAE. It does not, however, absorb applicant-level heterogeneity that shifts the weight placed on an attribute or that shapes which schools enter the list in the first place; those channels can still differ by gender.

The interpretation therefore concerns whether, within their own ranked set, female applicants place stronger or weaker weights on certain school attributes than male applicants. For example, if $Female_i \times SchoolSES_{ij}$ is positive, female applicants are more likely than male applicants to rank relatively high-SES schools earlier. If $Female_i \times Distance_{ij}$ is negative, female applicants are less likely than male applicants to rank relatively farther schools earlier.

The stand-alone coefficient on $Female_i$ cannot be identified in a conditional logit model because gender does not vary across the schools of the same applicant.[3] The interpretable coefficients are therefore the interactions between $Female_i$ and school-level or student-school level variables, which show whether school attributes have different marginal weights for female and male applicants.

These coefficients have a clear interpretation under three assumptions. First, among the schools an applicant chose to list, the submitted order is taken as a truthful ranking, while the prior decision of how many and which schools to list is treated as choice-set formation (Fack, Grenet, and He 2019). Second, the relevant choice set is the applicant's own ranked schools rather than the full market, so every comparison is internal to that set. Third, attributes enter relative to the applicant's own choice-set mean. Under these assumptions the interaction coefficients measure gender differences in the *revealed relative weight* placed on a school attribute within the submitted list. That revealed weight combines preferences and beliefs: how much a family values an attribute and what it believes the attribute implies.

The rank-ordered logit also imposes the usual Luce/IIA structure on choices among listed schools. The estimates are therefore best read as a parsimonious summary of relative attribute weights within submitted lists, rather than as a fully structural model of household utility.

A further implication is that the estimand is conditional on the submitted choice set. The model compares gender differences in attribute weights among the schools each applicant listed; it does not estimate the full process by which those schools entered the list. Table 3a describes observed list composition, and Appendix Table A34 adds a local-market check by re-estimating the Secondary model within applicants' regions of residence.

Table 3a also shows that these submitted sets already differ by gender: in Secondary the average School SES of listed schools is 0.423 for girls and 0.406 for boys. This points in the same direction as the within-list result reported later, and the two are statistically distinct. Because every attribute enters relative to the applicant's own choice-set mean, the level difference in Table 3a is removed before the ranking coefficients are estimated, so it cannot mechanically generate them. The gendered School SES tilt therefore appears at two independent margins, which schools enter the list and how they are ranked once listed, and these reinforce rather than substitute for one another. The present paper identifies the second margin cleanly; the first, visible in Table 3a, is consistent with it and is left to future work that can observe the schools families considered but did not list.

This conditioning rests on a separability assumption worth stating plainly. The analysis treats the formation of the choice set and the ordering of schools within it as two separate processes and models only the second. This is a strong assumption. If the two are not separable, so that the gendered factors shaping which schools a family lists also shape how those schools are ordered, the interaction terms can absorb part of choice-set formation rather than ranking alone. The paper adopts the assumption deliberately, since modeling choice-set formation di-

[3] A decision-maker characteristic such as gender can in principle be identified in a conditional logit if its coefficient is allowed to vary across alternatives (Train 2009); the interaction specification used here is the more parsimonious version of that idea, letting gender shift the weights on school attributes rather than estimating a separate alternative-specific gender effect.

rectly would require data on the schools families considered but did not list, which the application records do not contain (Manski 1977; Giustinelli 2016). A related point concerns the decision maker. A submitted ranking is a household decision, and the data do not record which family member weighed which attribute, so the estimates describe a family's revealed ranking for a given child rather than the preferences of one identified decision maker (Giustinelli 2016).

4.6 Main explanatory variables and choice-set centering

The main explanatory variables are school attributes and their interactions with applicant gender. Core continuous attributes are measured relative to the other schools in the same applicant's choice set.

For applicant i with choice set C_i, school j's attribute X_{ij} is transformed as:

$$X_{ij}^c = X_{ij} - \frac{1}{|C_i|} \sum_{k \in C_i} X_{ik}$$

Because X_{ij}^c subtracts the mean attribute value of applicant i's ranked schools, it shows whether school j sits above or below the average of that applicant's candidate set. If $X_{ij}^c > 0$, the school is above the applicant's choice-set average on that attribute; if $X_{ij}^c < 0$, it is below it.

This transformation matches the research question: the model explains why some schools are ranked ahead of other schools considered by the same applicant. Centering does not by itself change what the model identifies. In a conditional logit only differences in attributes across the schools within a choice set affect the likelihood, so any term common to all of an applicant's schools, including the choice-set mean, drops out. Centering therefore fixes the interpretation and scale of the attributes, relative to each applicant's own listed schools, while identification still comes from within-applicant variation across the ranked alternatives.

The paper studies four main school attributes: school quality, distance, school social composition, and school demand.

The first is school quality, denoted $Quality_{ij}$. It measures a school-level quality or performance signal. After choice-set centering, $Quality_{ij}^c$ indicates whether school j has higher quality than the other schools in applicant i's candidate set.

The second is distance, denoted $Distance_{ij}$. Distance captures the commuting cost between the applicant's residence and the school. Because distance is right-skewed, the paper uses $\log(1 + Distance_{ij})$ to reduce the effect of extreme values. After centering, $LogDistance_{ij}^c$ indicates whether a school is relatively farther or closer than the other schools in the same choice set.

The third is School SES, measured by the school-level share of non-priority students. It is read as a proxy for the school's social composition, not as a direct measure of any family's SES or of school quality, which enters as a separate variable. It captures differences in the social and economic composition of the school. After centering, $SchoolSES_{ij}^c$ indicates whether school j has a higher social composition than other candidate schools for the same applicant. This variable is important because social composition may reflect resources, peer environment, school reputation, and local educational networks.

The fourth is school demand, measured by the number of same-grade applications listing the school in the top three. This is a school-grade level demand measure constructed from the aggregate number of applications listing the school among the top three choices in the same admission round; the label "top three" describes how that demand count is constructed and does *not* restrict the estimation sample, which uses each applicant's entire submitted ranking. The paper uses the log form, $LogTop3Demand_{ij}$. Demand is different from quality. Quality is closer to an official or formal indicator, while demand reflects a market signal formed by how families in the same admission round rank the school. After centering, $LogTop3Demand^c_{ij}$ indicates whether a school is relatively more popular within the applicant's choice set.

After centering, the core continuous variables are standardized to make coefficients comparable across variables with different units. Relative Quality, Relative Log Distance, Relative School SES, and Relative Log Top3 Demand describe a school's position within the applicant's own choice set, not its absolute level in the wider market.

The main model also includes rural and single-sex school indicators. School supply variables such as Log Vacancies and Log School Size, and the applicant-school priority indicator Any Priority, are used in diagnostics or auxiliary robustness checks where explicitly indicated. The main specification does not include school-type or religion fixed effects. A more conservative auxiliary specification with these additional controls and fixed effects yields a smaller but still positive and significant Secondary School SES interaction. Because School SES and school size are correlated at the school-grade level, Appendix Table A13 further adds Female $\times$ Log School Size to check whether the gendered School SES coefficient survives a gender-specific school-size control.

4.7 Gender interactions

The main question is whether girls and boys place different weights on school attributes within the same choice structure. The key terms are the interactions between gender and school attributes.

In a conditional logit model, applicant-level variables that are fixed within the applicant's list drop out. Since $Female_i$ is the same for all schools of the same applicant, the estimable gender terms are its interactions with school-level or student-school level variables. These interactions show whether female and male applicants respond differently to school attributes within their ranked sets.

The systematic utility can be written as:

$$V_{ij} = X^c_{ij}\beta + Female_i \times X^c_{ij}\gamma + Z_{ij}\delta + Female_i \times Z_{ij}\zeta$$

Here, X^c_{ij} denotes the choice-set centered and standardized core attributes: relative quality, relative distance, relative school social composition, and relative demand. The controls Z_{ij}, rural and single-sex status, enter with baseline coefficients δ and their own gender interactions ζ. The coefficient β can be read as the baseline weight for male applicants, while γ captures the additional weight for female applicants. The total weight for female applicants is $\beta + \gamma$.

The main focus is γ, the gender difference. A positive interaction coefficient means female applicants place a stronger relative weight on that attribute than male applicants; a negative one means a weaker relative weight. This does not say that girls like a certain kind of school in any absolute sense. It says that, within the same choice-set structure, they are more or less likely than boys to rank schools with that relative attribute ahead of the others in their own list. The coefficient is a difference in revealed relative weight, not an isolated measure of taste for the attribute itself. If the families of girls and boys differ in unobserved ways correlated with a school attribute, such as perceived safety or beliefs about a school's environment, part of that difference will load onto the interaction. The estimates are therefore read as gendered differences in how attributes are weighed within the ranking, not as evidence that girls' families value the attribute as such.

For example, a positive coefficient on Female $\times$ Relative School SES means that, within their ranked schools, female applicants are more likely than male applicants to rank relatively high-SES schools earlier. A positive coefficient on Female $\times$ Relative Log Distance means that female applicants place a weaker relative penalty, or a more positive relative weight, on distance than male applicants. Since distance is log transformed and choice-set centered, the interpretation concerns relative distance within the applicant's ranked set, not absolute kilometers.

Female $\times$ Relative Quality measures whether female applicants place a higher weight on formal school quality signals. Female $\times$ Relative Log Top3 Demand measures whether they place a higher weight on schools that are more popular within the same choice set. The demand interaction must be interpreted by stage. A positive coefficient in early stages may mean that families of girls rely more on market reputation. A near-zero or weakly negative coefficient in secondary education may mean that these families are not simply following the most demanded schools.

The model also includes additional gender interactions, including Female $\times$ Rural and Female $\times$ Single-Sex School. Single-sex status is not one of the four main continuous attributes, but it is directly related to gendered school choice. Since sex-specific eligibility is handled in the construction of feasible alternatives, the single-sex coefficient is interpreted within gender-compatible candidate schools. It is reported and discussed separately, not treated as part of the School SES mechanism.

4.8 Stage-grouped estimation

The main model does not pool all grades into one single sample, and it does not estimate 14 separate grade-level models. Instead, the paper groups applicants by educational stage. The five stages are Prekinder, Kinder, Early Primary, Upper Primary, and Secondary. Early Primary includes Grade 1 to 4 Básico, Upper Primary includes Grade 5 to 8 Básico, and Secondary includes Grade 1 to 4 Medio.

The grouping follows the institutional and practical differences between entry stages. Younger applications (Prekinder through Upper Primary) are often tied to care, transport, and daily routine; secondary applications (Grade 1 to 4 Medio) are more closely linked to academic pathways, reputation, peer groups, and later opportunities. Pooling all grades would hide these differences, while 14 separate grade models would make the analysis noisier and harder to read.

Within each stage, the analysis estimates the rank-ordered (exploded) logit model separately. A gender interaction that appears mainly in Secondary is therefore a stage pattern in the 2025 cross-section, not an estimate of how one cohort changes with age.

The thesis also estimates a pooled stage model as a supplementary test of stage differences. The pooled model puts all stages in one regression and interacts stage indicators with the main gender interactions. Wald tests then examine whether the coefficients differ significantly across stages. These tests support the use of stage-grouped estimation when stage differences are large.

5 Results

5.1 Main results: gender differences in school choice weights

This section reports the stage-grouped rank-ordered (exploded) logit estimates. The coefficients are relative choice weights: they show whether a school is more likely to be ranked ahead of the other schools in the same applicant's list because it differs on a given attribute. The non-interacted coefficients describe the baseline weights for applications submitted for boys. The female interactions ask whether those same attributes carry a different relative weight in applications submitted for girls.

The central result is Female $\times$ Relative School SES, and it follows a clear stage pattern. Using applicant-clustered sandwich standard errors, the interaction is essentially zero and statistically insignificant in the three youngest stages: Prekinder (-0.0016, z $=$ -0.25), Kinder (0.0039, z $=$ 0.32), and Early Primary (-0.0069, z $=$ -1.17). It becomes positive but weak in Upper Primary (0.0113, z $= 1.78$), and it is positive and clearly distinguishable from zero in Secondary (0.0334, z $=$ 8.80). The gendered weight on school social composition is therefore not a uniform feature of school choice at all ages. It appears most clearly in secondary education.

This is the main empirical finding. The baseline Secondary coefficient is 0.0334, an odds ratio of $\exp(0.0334) = 1.034$ for a one-standard-deviation increase in relative School SES. A specification with additional school-level controls, school-type fixed effects, and religion fixed effects gives 0.0222. I therefore read the Secondary result as a modest positive range, roughly 0.022 to 0.033 in log-odds, not as a single headline coefficient. Appendix Table A29 shows that replacing raw Top3 Demand with a leave-one-out demand measure leaves the Secondary School SES coefficient virtually unchanged.

To make this magnitude concrete in choice terms, consider two schools in the same submitted list that differ by one standard deviation in relative School SES. The estimated weights imply that female applicants rank the higher-SES school ahead of the other with probability about 51.9 percent, against 51.0 percent for male applicants, a gender difference of roughly 0.8 percentage points per such pairwise comparison. At the level of a single applicant this is a small tilt; its interest is that it is systematic, recurring across every pairwise comparison in every submitted secondary list nationwide. Section 5.6 traces where a difference of this size lands once the assignment mechanism has run, leaving a residual admitted-school School SES gap of about 0.015 in Secondary (Table 7).

The younger-stage coefficients are close to zero when the model uses true submitted rankings, unlike earlier list-completion specifications, which were more sensitive in the youngest stages. The Secondary result is not driven by that issue, so the rest of the analysis treats it as the central evidence.

The other interactions help delimit the School SES result. Adding Female $\times$ Log School Size leaves the Secondary School SES coefficient close to its main value (Appendix Table A13), so the result is not simply a scale pattern. Kinder remains outside the main pattern: Female $\times$ Relative School SES is small in Table 5 (0.0039, z $= 0.32$) and in the stepwise specification (Table A8).

Demand, distance, quality, rural status, and single-sex status are less stable. Female $\times$ Relative Log Top3 Demand is positive in the younger stages but near zero in Secondary. Female $\times$ Relative Log Distance changes sign across stages. Female $\times$ Relative Quality is positive in Early Primary and Secondary but not elsewhere. Female $\times$ Rural is most negative in Secondary, while Female $\times$ Single-Sex School is most negative in Upper Primary and Secondary. These patterns are useful context, but they do not match the stability of the School SES interaction.

Table 5 is the main empirical table of the thesis. It reports the stage-specific gender interactions that directly answer the research question.

Table 5. Main regression results: stage-grouped rank-ordered (exploded) logit on the true submitted ranking

Sequential ranking model on each applicant's actual ranked list; no list completion

VARIABLES	(1) Prekinder	(2) Kinder	(3) Early Primary	(4) Upper Primary	(5) Secondary
Female × Relative Quality	0.0007	-0.0105	0.0117**	0.0016	0.0175***
	(0.12)	(-0.93)	(2.08)	(0.26)	(4.63)
Female × Relative Log Distance	-0.0109	-0.0234*	0.0080	0.0172**	0.0083**
	(-1.58)	(-1.71)	(1.11)	(2.24)	(2.01)
Female × Relative School SES	-0.0016	0.0039	-0.0069	0.0113*	0.0334***
	(-0.25)	(0.32)	(-1.17)	(1.78)	(8.80)
Female × Relative Log Top3 Demand	0.0087	0.0091	0.0275***	0.0054	-0.0013
	(1.27)	(0.69)	(4.07)	(0.68)	(-0.35)
Female × Rural	0.0035	-0.1498*	-0.0574	-0.0537	-0.0585**
	(0.08)	(-1.70)	(-1.55)	(-1.41)	(-2.02)
Female × Single-Sex School	-0.0294	0.0157	-0.0219	-0.3176***	-0.0439**
	(-0.72)	(0.22)	(-0.61)	(-9.98)	(-2.38)
Relative Quality	0.0421***	0.0460***	0.0547***	0.0551***	0.0130***
	(9.87)	(5.87)	(14.23)	(12.95)	(4.84)
Relative Log Distance	-0.2064***	-0.2043***	-0.2469***	-0.2544***	-0.1565***
	(-42.42)	(-21.83)	(-49.39)	(-45.56)	(-53.18)
Relative School SES	0.0594***	0.0814***	0.1131***	0.1143***	0.0414***
	(13.08)	(9.65)	(28.01)	(25.26)	(15.03)
Relative Log Top3 Demand	0.1176***	0.2235***	0.2991***	0.5684***	0.2552***
	(24.21)	(24.22)	(65.23)	(101.13)	(92.64)
Rural	0.0913***	0.1586***	0.2632***	0.1654***	0.1954***
	(3.05)	(2.68)	(10.66)	(6.15)	(9.67)
Single-Sex School	0.0461	0.0087	0.1592***	0.3248***	0.1343***
	(1.39)	(0.15)	(5.61)	(13.17)	(9.82)
Choice-set centered variables	YES	YES	YES	YES	YES
Winsorization	1% / 99%	1% / 99%	1% / 99%	1% / 99%	1% / 99%
Ranked rows	243,762	70,520	293,222	298,061	607,045
Applicants	83,667	23,317	89,779	87,068	164,306

Notes: Estimates come from a rank-ordered (exploded) conditional logit applied to each applicant's *true* submitted ranking, with no completion of short lists. Each applicant contributes the sequence of conditional choices implied by their ranked list (the first choice from all ranked schools, the second from the remainder, and so on). Values in parentheses are z-statistics based on applicant-clustered sandwich standard errors computed from applicant score contributions. Model-based and school-grade-clustered versions of the central School SES coefficient are reported in Table 5a and Appendix Table A31. The model includes rural and single-sex school indicators, but does not include school-type or religion fixed effects. Core continuous variables are winsorized at 1%/99%, choice-set centered, and standardized. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table 5 gives the baseline stage-grouped estimates with applicant-clustered sandwich standard errors. Table 5a then places the main Secondary estimate beside the checks that matter most for interpretation. The first two rows show the baseline estimate and the more conservative controls-and-fixed-effects estimate together. The remaining rows compare variance estimators, early-rank likelihoods, leave-one-out demand, school size, the exclusion of single-sex schools, and an applicant fixed-effects LPM.

Table 5a. Core checks on the secondary School SES result

Specification	Coef.	Statistic	Source	What it checks
Baseline rank-ordered logit, applicant-cluster SE	0.0334***	z = 8.80	Table 5	Baseline secondary estimate with applicant-level sandwich inference
Conservative controls and fixed effects	0.0222***	z = 6.07	Table A30	Smaller estimate after extra school controls, school-type fixed effects, and religion fixed effects
Same baseline coefficient, model-based SE	0.0334***	z = 9.39	Table A31	The model-based statistic is close to the robust version
Same baseline coefficient, school-grade-cluster SE	0.0334***	z = 8.26	Table A31	Allows shared shocks within school-grade clusters as an auxiliary check
Rank 1 step only	0.0453***	z = 7.62	Table A32	Uses only the first-choice step from each submitted list
First two ranking steps	0.0374***	z = 8.05	Table A32	Keeps only the first two exploded choices
First three ranking steps	0.0313***	z = 7.51	Table A32	Keeps only the first three exploded choices
Leave-one-out demand	0.0334***	z = 9.41	Table A29	Raw demand includes the applicant's own top-three contribution
Add Female × Log School Size	0.0317***	z = 8.78	Table A13	School SES is not just school scale
Exclude single-sex schools	0.0341***	z = 8.55	Table A25	The result is robust to single-sex schools
Applicant FE LPM, clustered SE	0.0098***	SE = 0.0013	Table A16	Same direction in a linear model with applicant fixed effects

Notes: Rows 1–2 are the two estimates used to summarize the main Secondary result: the baseline specification and a more conservative specification that adds Relative Log Vacancies, Relative Log School Size, Any Priority, school-type fixed effects, and religion fixed effects. Rows 3–4 keep the baseline point estimate and change only the variance estimator. Rows 5–7 restrict the exploded likelihood to the first one, two, or three ranking steps. The applicant-clustered version is the main inferential check because the ranking likelihood is organized by applicant lists. The school-grade clustered version is reported as an auxiliary check. The final row is from an applicant fixed-effects linear probability model, so its coefficient is not on the log-odds scale and should be read only as a sign and inference check. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

The main reading of Table 5 and Table 5a is simple. The secondary School SES coefficient is positive in the baseline model and remains positive in the stricter controls-and-fixed-effects specification. The estimate falls from 0.0334 to 0.0222, giving a useful range for the size of the result rather than a contradiction. The same secondary pattern appears with applicant-clustered inference, with school-grade clustering as an auxiliary variance check, and when the exploded likelihood is restricted to the first one, two, or three ranking steps. The early-rank checks matter because later ranks may be noisier than the top of the list. The result also remains positive with leave-one-out demand, without single-sex schools, with a school-size interaction, and in an auxiliary applicant fixed-effects LPM. Demand and distance do not show the same stability, so they play a smaller role in the interpretation.

A second check concerns the amount of School SES variation inside submitted lists. Secondary choice sets may simply leave more room for School SES to matter. I check this in two ways: first by reporting within-list School SES dispersion by stage, and then by splitting each stage at the median within-list School SES standard deviation and re-estimating the model in the low- and high-dispersion halves. This keeps the focus on whether the Secondary result appears only where listed schools differ strongly in social composition.

Table 5b. School SES dispersion and the secondary-stage result

Stage	Mean within-list SES SD	Low-dispersion half	High-dispersion half
Prekinder	0.0667	0.0092	-0.0001
Kinder	0.0706	-0.0739**	0.0226*
Early Primary	0.0745	-0.0083	-0.0038
Upper Primary	0.0793	0.0380**	0.0096
Secondary	0.0795	0.0563***	0.0322***

Notes: The first numeric column reports the mean applicant-level standard deviation of School SES within submitted lists. The last two columns report the Female × Relative School SES coefficient from the main rank-ordered logit estimated separately below and above each stage's median within-list SES standard deviation. Coefficients use the same choice-set-centered, standardized Relative School SES as the main model; only the low- versus high-dispersion split is defined within each stage, not across the pooled sample. Applicant-clustered z-statistics are reported in Appendix Table A33. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

Table 5b shows two useful facts. First, Secondary applicants face more dispersed School SES choice sets than the youngest applicants, but the increase is gradual and Secondary is almost identical to Upper Primary on the mean within-list standard deviation. Second, the Secondary coefficient is positive in both halves of the within-stage dispersion split, including the low-dispersion half. The result therefore appears beyond lists with especially large School SES differences. This weakens a purely mechanical visibility story. Appendix Table A14 helps keep the objects separate: it reports descriptive gaps in applicant-level SES premiums, while Table 5b reports conditional ranking-weight estimates within low- and high-dispersion submitted lists. High-dispersion lists can produce larger raw premium gaps, even when the conditional Female × School SES coefficient is larger in the low-dispersion subsample.

These results also point to clear stage differences. The next subsection tests whether those differences are statistically meaningful in a pooled stage model.

5.2 Heterogeneity across educational stages

The main results show that gendered school-choice weights vary across educational stages rather than following a single pattern. To test the size of that variation, the analysis estimates a pooled stage model: all five stages enter one regression, with stage indicators interacted with the main gender interaction terms and Early Primary as the reference stage.

Table 6 reports this model. Re-estimated on the true submitted rankings, it is paired with joint Wald tests that ask, for each gender interaction, whether the coefficient is constant across the five stages, with the exact χ^2 statistics reported in the table.

The tests reject stage homogeneity for the main gender interactions, which indicates that the relative weights placed on quality, distance, social composition, and demand differ across the stages observed in 2025 rather than reflecting small numerical noise. Because these joint tests use model-based rather than applicant-clustered statistics, they are read as supportive of stage heterogeneity rather than as definitive, and the evidence is weakest for the quality interaction. A single pooled model with one common set of gender interactions would hide these differences.

The pooled cross-section compares stages observed in the same year. It is not a panel following the same children as they age.

Beyond the heterogeneity tests, the pooled model reproduces the stage patterns already documented in the separate-stage estimates of Table 5: the Female × Relative School SES interaction is largest in Secondary, the demand interaction is positive in the younger stages but becomes close to zero or slightly negative in Secondary, and the distance interaction stays stage-specific rather than uniformly signed. Because these patterns mirror the separate-stage model, the stage-specific coefficients are not tabulated again here; Table 5 carries them, while Table 6 retains only the joint tests that establish the heterogeneity.

Table 6. Joint Wald tests for stage heterogeneity (pooled stage model)

Test	χ^2	df	p-value
Female × Relative Quality	11.84	4	0.019**
Female × Relative Log Distance	25.71	4	<0.001***
Female × Relative School SES	52.32	4	<0.001***
Female × Relative Log Top3 Demand	17.23	4	0.002***

Notes: The table reports joint Wald tests for stage heterogeneity in the four continuous gender interactions (quality, distance, School SES, and demand). Each test asks whether the interaction is constant across the five educational stages in a pooled model that enters all stages in one regression with stage interactions and Early Primary as the reference stage. Inference for this pooled Wald test is based on model-based statistics; applicant-clustered standard errors are not reported for this pooled auxiliary model. The pooled model includes the same baseline variables as the main specification; it does not include school-type or religion fixed effects. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

The pooled stage model broadly supports the stage-grouped results, with the caveat that small coefficients can change significance across specifications. It also clarifies why Secondary deserves separate attention. At this stage, the School SES interaction becomes larger in relative terms, the demand interaction loses the positive pattern seen in earlier stages, and the distance interaction becomes positive. These shifts are not identical mechanisms, but they point in the same direction: gendered ranking weights are more visible in Secondary than in earlier stages, while the implied effect sizes remain modest. The distance pattern is examined more directly in the triple-interaction analysis below, which finds no joint distance-SES bundle behind it.

5.3 Cross-cohort evidence from 2017–2024

The 2025 estimates provide the main microdata analysis of the thesis. To see whether the result is specific to one application year, the paper adds comparable stage-grouped regression outputs for 2017–2024. These year-specific models apply the same broad empirical architecture as the 2025 specification: rank-ordered (exploded) logit regressions, the same five educational stages, choice-set-centered relative school attributes, and the same core Female interactions. Earlier result files differ slightly in auxiliary controls and sample metadata, so the cross-cohort evidence is read mainly through coefficient sign, statistical precision, and approximate magnitude. The purpose is narrow: to check whether the central 2025 pattern also appears in earlier SAE cohorts.

Appendix Tables A17–A22 report the full cross-year coefficients, together with a detailed cohort-by-cohort reading. The strongest and most stable result is again Female × Relative School SES: in Secondary it is positive and highly significant in every year from 2017 to 2024, ranging from about 0.029 to 0.045, so the 2025 baseline estimate in Table 5 (0.0334) sits comfortably inside this range. The same stage profile recurs across cohorts, with the effect clearest in Secondary and weakest and most erratic in the youngest stages. The secondary School SES result therefore looks like a recurring feature of SAE applications rather than a single-year fluctuation.

The cross-cohort estimates show two things. The clear message is that the secondary Female $\times$ Relative School SES interaction is persistent: it is positive, sizeable, and highly significant in every cohort from 2017 to 2024 as well as in 2025, which makes the central result substantially more credible.

The caveat is that the other secondary interactions do *not* replicate cleanly once the true rankings are used. The secondary demand coefficient is weak and changes sign across years rather than being reliably negative; the secondary distance coefficient is essentially zero across cohorts and is significant only in 2018, where it is negative; and the quality and single-sex interactions also vary in sign over time. This makes the School SES result more specific. The stable result is not a general secondary-stage gender difference across all school attributes, but a recurring difference in the relative weight placed on school social composition; the other interactions appear cohort-specific or were inflated by list completion in the earlier specification. These cross-year regularities strengthen external validity for the School SES result across cohorts, but because the design is a sequence of independent repeated cross-sections rather than a panel, they show recurrence across application years, not how any individual cohort evolves over time.

5.4 Robustness checks

The robustness checks are organized around three threats: alternative school attributes and sample definitions, choice-set formation, and inference.

First, the Secondary School SES interaction is not sensitive to sample or variable construction. It stays close to the Table 5 estimate after excluding single-sex schools (0.0341; Appendix Table A25), under 2%/98% winsorization (Appendix Table A1), on urban-only choice sets (Appendix Table A3), and when Female $\times$ Log School Size is added (Appendix Table A13). Adding a Female $\times$ School-is-free control and randomly reassigning top-three schools both leave the Secondary School SES interaction essentially unchanged (Appendix Tables A5 and A6). These checks reduce the concern that the Secondary pattern is produced mechanically by a particular transformation, by the small group of single-sex schools, or by a generic tendency for the model to generate female interactions with arbitrary school attributes.

The Secondary pattern is also not an artifact of sibling priority. Because a sibling already enrolled at a school confers admission priority, and because families with a firstborn daughter may be more likely to have a second child, sibling priority is a plausible gender-correlated channel. In these data it is slightly less common for girls than for boys, held by 13.2% of girls' Secondary applications against 14.0% of boys', so it does not favor girls. Dropping every applicant who holds sibling priority at a listed school leaves the School SES interaction essentially unchanged, and adding a Female $\times$ Sibling-priority term to the full model leaves the coefficient near its baseline while the sibling interaction itself is small and insignificant (Appendix

Table A35).

Second, the result is stable across submitted-list and local-market checks. Table 3a shows that male and female applicants enter the ranking stage with similar observed list composition, with the largest remaining gaps appearing in Secondary. Appendix Table A26 restricts the analysis to denser local markets and reachable choice sets, and the Secondary interaction remains positive under both restrictions. Appendix Table A34 then re-estimates the Secondary rank-ordered logit separately within applicants' inferred regions of residence. Markets are defined by applicant region rather than by the region of the ranked school, preserving each applicant's full submitted list. The applicant-weighted mean of the 16 region-specific estimates is 0.0319, close to the national estimate of 0.0331, and 12 of the 16 region estimates are positive. The pattern is likewise stable across the institutionally binding vulnerability stratum: re-estimating the Secondary interaction separately for priority (socioeconomically vulnerable) and non-priority applicants gives positive and significant coefficients in both groups (0.042 and 0.029; Appendix Table A36), so the pooled estimate is not a composition-weighted average that hides a null in either group.

Third, the result is not an artifact of a single variance calculation. Because the main sample is large, Appendix Table A31 reports alternative standard errors for the central School SES coefficient. The Secondary coefficient remains clearly positive using applicant-clustered standard errors and a school-grade clustered auxiliary variance calculation. Appendix Table A12 also reports a student fixed-effects linear probability model with school-grade clustered standard errors. Since this estimator is not the same as the rank-ordered logit, it is read as a complementary dependence-structure check rather than as a direct replacement for the main model.

Fourth, the gender difference is not an artifact of the logit error scale. Because rank-ordered logit coefficients are identified only up to scale, a larger female School SES coefficient could in principle reflect girls ranking more decisively rather than weighting School SES more heavily. Appendix Table A37 rules this out. Estimating the Secondary model separately by gender, the distance and demand coefficients are almost identical for boys and girls (female-to-male ratios of 0.95 and 0.99), while School SES and quality are markedly larger for girls (1.81 and 2.36); a common scale difference would have moved all four coefficients together. The scale-free ratio of School SES to distance, which is invariant to the error scale, is roughly twice as large for girls as for boys (0.51 versus 0.26), confirming a genuine difference in the relative weight placed on school social composition rather than in ranking noise.

Across these checks, the Secondary School SES interaction remains positive and stable, while the demand interaction stays near zero or weakly negative in Secondary and the distance pattern remains stage-specific. The robustness evidence therefore supports a small but systematic gender difference in the relative weight placed on school social composition, conditional on the submitted ranked list.

5.5 Suggestive evidence: school social composition, demand, and distance trade-offs

The main results and robustness checks show systematic gender differences in school-choice weights. This section examines whether those patterns align more closely with quality, School SES, demand, or distance trade-offs. The evidence is intentionally treated as suggestive. Administrative applications reveal rankings and school attributes, but they do not observe whether the relevant evaluation is made by parents, students, or both. They also cannot distinguish directly among safety concerns, peer-environment expectations, reputation, family networks, and perceived academic pathways.

Stepwise specifications

The analysis first estimates stepwise models. Appendix Table A7 reports Model 1, which includes quality and distance. Appendix Table A8 reports Model 2, which adds Relative School SES. Appendix Table A9 reports Model 3, which adds Relative Log Top3 Demand. This design checks whether an apparent quality preference is partly absorbed by school social composition or demand.

In Model 1, Female $\times$ Relative Quality is positive in almost all stages. With only quality and distance included, female applicants appear more likely than male applicants to choose relatively high-quality schools. Since official quality is related to social composition, school reputation, and demand, Model 1 may capture several sources of school appeal.

When School SES is added in Model 2, Female $\times$ Relative Quality becomes smaller, and the change is largest in Secondary, where it falls from 0.0293 (z $= 9.10$) in Model 1 to 0.0116 (z $= 3.31$) in Model 2. At the same time, Female $\times$ Relative School SES enters strongly in Secondary (0.0441, z $= 12.91$). This indicates that the apparent quality difference partly overlaps with school social composition, without showing which feature of social composition matters to applicants.

When Top3 Demand is added in Model 3, Female $\times$ Relative School SES remains positive in the main stages. The demand interaction also weakens in Secondary rather than reproducing the earlier positive pattern. This separates the School SES pattern from a general tendency to rank the most demanded schools.

Distance trade-offs

The Secondary distance result requires care. A positive coefficient on Female $\times$ Relative Log Distance does not mean that female applicants prefer faraway schools. Distance is better understood as a cost. The key question is whether the positive distance pattern is concentrated in combinations that also involve specific school attributes, especially School SES or demand.

The analysis estimates two triple interactions. The first is Female $\times$ Relative Log Distance $\times$ Relative School SES, which tests whether the gendered distance weight depends on a school's social composition. Estimated on the true submitted rankings, this second-order term is small and, in Secondary, negative (-0.0081, z $= -2.25$), so the data do not support a clean joint pattern in which female applicants specifically rank schools that are *both* relatively farther and relatively higher in SES. What does remain robust is the core Female $\times$ Relative School SES term, which is positive and strongly significant in Secondary even with the triple interaction included. The two-way Female $\times$ Relative Log Distance coefficient also remains positive in Secondary. In other words, the secondary School SES result is not an artifact of a distance-SES bundle, but this small negative three-way moderation is treated cautiously and not used as central evidence.

Appendix Table A11 reports the second triple interaction, Female $\times$ Relative Log Distance $\times$ Relative Log Top3 Demand. Estimated on the true submitted rankings, this term is small and statistically insignificant at every stage, including Secondary (-0.0036, z $=$ -0.96). As with the distance-by-SES triple, the data therefore do not support a clean joint pattern in which female applicants specifically rank schools that are *both* relatively farther and more in demand. What remains robust is the core Female $\times$ Relative School SES term, which stays positive and strongly significant in Secondary (0.0335, z $=$ 9.43) when the distance-by-demand interaction is included. The distance and demand interactions are thus better read as separate stage-specific patterns than as a joint distance-by-demand mechanism.

Rural schools and the mobility/safety channel

The rural-school results provide a more direct check on the mobility and safety interpretation suggested by the gender-and-education literature. The check has to be interpreted carefully because `is_rural` identifies the location of the school, not the applicant's home area. It therefore does not compare rural and urban households. Instead, it asks whether applications submitted for girls weight rural schools, distance, and School SES differently from applications submitted for boys.

The main rank-ordered (exploded) logit model already gives the first clue. Female $\times$ Rural is close to zero in Prekinder, weakly negative in Kinder and Early Primary, statistically unclear in Upper Primary, and negative in Secondary (-0.0585, z = -2.02). If the dominant channel were parental concern about younger girls' daily mobility or travel safety, one would expect the rural-school penalty or the female distance penalty to be strongest and most consistent at younger ages. Instead, the clearest negative rural-school estimate appears in secondary education, when students are older and their own preferences are more likely to enter the submitted ranking.

Appendix Table A16 adds an auxiliary applicant fixed-effects linear probability check. Panel A estimates a model with Female $\times$ Rural $\times$ Relative Log Distance and Female $\times$ Rural $\times$ Relative School SES. In Secondary, Female $\times$ Rural remains negative (-0.0312, t = -5.07), while Female $\times$ Rural $\times$ Relative Log Distance is also negative (-0.0087, t = -2.40). Panel B splits applicants into all-urban choice sets and choice sets containing at least one rural school. In Secondary, the female distance coefficient is positive in all-urban choice sets (0.0082, t = 4.99) but close to zero in rural-exposed choice sets (0.0006, t = 0.28). These auxiliary estimates use the same within-applicant comparison as the main rank-ordered (exploded) logit model, but in a linear form that allows the three-way rural interactions to be estimated and reported transparently. They point against a simple claim that the Secondary distance result is driven by girls' greater willingness to travel to rural schools.

The rural-school evidence therefore weakens a straightforward mobility/safety mechanism. It does not rule out safety concerns altogether, especially because the data do not observe transport mode, route safety, parental involvement, or applicant residence. But the observed age profile is more consistent with the idea that, in the Chilean SAE setting, the Secondary gendered pattern is tied to school reputation, academic pathways, peer environment, or the perceived social meaning of rural versus urban secondary schools rather than to a general access-to-school mobility constraint.

Interpretation and limits

The stepwise and triple-interaction results narrow the interpretation. The apparent quality difference becomes weaker after School SES is added; the School SES interaction remains after demand is added; and, in Secondary, the distance result does not align cleanly with either a joint distance-SES pattern or a joint distance-demand pattern. These checks show that school social composition is the school attribute most closely tied to the gendered ranking pattern. Substantively, the School SES coefficient captures a gendered weight on the social composition of schools and on the school features that tend to come with it, especially in Secondary.

5.6 From applications to admissions: decomposing the gender gap

This subsection extends the analysis from the application stage to admission outcomes. The decomposition below concerns the gender gap in admitted school SES, whereas the main estimates above concern gender differences in within-choice-set ranking weights after choice-set centering. It therefore does not replace the main analysis. Its purpose is to ask, in descriptive terms, how much of the gendered pattern observed in submitted preference lists is still visible once the admission mechanism has assigned students to schools.

The decomposition is computed on the subset of applicants whose regular-stage admitted school can be linked to the analysis data. This subset contains about 430,609 applicants, fewer than the 448,137 applicants in the main estimation sample, because applicants who remain unassigned or whose admitted school cannot be linked are excluded. As discussed in the limitations, this linkage is not guaranteed to be random, so the decomposition is treated as descriptive rather than as a population-level estimate of how the application-stage gap maps into admission outcomes. Four features in particular keep this exercise associative rather than structural: it excludes applicants who remain unassigned; the link rate is lower in Secondary (90.0 percent) than in the younger stages; the outcome is the school an applicant is assigned to at the regular stage, not the school where enrollment is later verified; and the accounting cannot reconstruct the capacity limits, priority caps, vacancies, and lottery draws that the deferred-acceptance mechanism actually applies. The Oaxaca–Blinder and DiNardo–Fortin–Lemieux tools used below therefore describe how the admitted-school gap lines up with observable choice-set composition; they do not identify the mechanism that produced it.

The starting point is a deliberately simple fact. The raw admitted-school SES gap is small: across stages, the female-minus-male gap is approximately 0.0063, and in Secondary it is 0.0146. Read in isolation, this small admission-stage difference could suggest that gender differences nearly disappear once the assignment process is complete. That interpretation would be too quick. An admission outcome combines at least two distinct margins: the observable schools faced by applications submitted for female and male applicants, and the way schools are ranked within those submitted choice sets. The same outcome is also filtered through the assignment mechanism itself, including school capacity, priorities, tie-breaking, and random ordering. Final admitted-school gaps alone cannot distinguish these margins. This is exactly why the paper begins with preference lists rather than with admission outcomes.

A useful first step, before decomposing the gap, is simply to ask whether the application-stage difference survives assignment at all. To do this, each applicant's regular-stage admitted school is matched back to their ranked list, so that the same School SES measure can be compared between the top-ranked application schools and the assigned school. This matching is possible for 431,721 applicants, about 93 percent of the 463,923 applicants in the grade-level files; the match rate is high in the younger stages and somewhat lower in Secondary (90.0 percent), where more applicants are unassigned or are placed outside their ranked list. Table 7 reports the raw female-minus-male School SES gap in the top-three set alongside the gap among assigned schools. These are unadjusted group mean differences, not choice-set-centered coefficients, and are therefore not directly comparable in magnitude with the conditional-logit estimates in the main tables; they are descriptive markers of whether the gap persists.

Table 7. From top-three applications to assigned schools: School SES gaps by gender

Stage	Linked applicants	Link rate	Top-three SES gap	Assigned SES gap
Prekinder	88,107	97.3%	0.0029	0.0025
Kinder	22,639	93.6%	0.0035	0.0041
Early Primary	87,257	94.8%	0.0024	0.0019
Upper Primary	82,048	92.7%	0.0051	0.0023
Secondary	151,670	90.0%	0.0179	0.0149

Notes: Unadjusted female-minus-male differences in School SES, measured as `school_non_priority_student_share`. The top-three gap compares the average School SES of the three top-ranked schools; the assigned gap compares the School SES of the assigned school among applicants whose assigned school can be linked to the analysis choice set. These raw group mean differences are not directly comparable in magnitude with the choice-set-centered conditional-logit coefficients in the main tables. The link rate is the share of applicants in each stage whose assigned school is matched to the analysis choice set, restricted to nonmissing assigned School SES.

Table 7 shows that the application-stage School SES gap remains partly visible among assigned schools. The clearest case is again Secondary, where the top-three gap of 0.0179 falls to 0.0149 among assigned schools: smaller after assignment, as expected in a centralized system with capacity and priority constraints, but still clearly visible. Upper Primary narrows more strongly (0.0051 to 0.0023), and the younger stages start from very small gaps. Kinder is the only stage where the assigned gap is marginally larger than the top-three gap (0.0041 versus

0.0035), a small reversal that fits the unstable Kinder pattern elsewhere. The broad message is that, at least in Secondary, gendered sorting by School SES partly carries into assigned schools.

This raises the next question: how much of the surviving admitted-school gap reflects the schools applicants face, and how much reflects how they rank within them? To separate these margins, the paper uses two complementary decompositions of the admitted-school SES gap: an Oaxaca–Blinder decomposition and a DiNardo–Fortin–Lemieux reweighting exercise. Both ask how much of the gap lines up with observable choice-set composition. They are computed on the subset of about 430,609 applicants whose admitted school can be linked to the analysis data and has nonmissing attributes. The discussion relies on the broad magnitude and direction of the results rather than their last digit, and it centers on Secondary, where the raw gap (0.0146) is large enough to interpret. There, the more conservative reweighting estimate attributes about two thirds of the gap (roughly 64 percent) to observable choice-set composition, while the linear Oaxaca–Blinder decomposition attributes an even larger share (about 94 percent). The two methods bracket the result and agree on its direction: most of the secondary admitted-school SES gap is associated with the observable composition of the schools applicants face, leaving a smaller residual. This explained share concerns the admitted-school outcome gap, while Table 5 estimates a within-list ranking tilt on a choice-set-centered quantity. The younger stages have very small raw gaps and unstable decompositions, so their figures are reported in Tables 8 and A23 for completeness. In Secondary, the explanatory role of observable choice-set composition is clear.

The Secondary residual is small in the linear decomposition (about 0.0009) and larger under reweighting (about 0.0053). Its sign is consistent with the within-choice-set School SES ranking difference in the main model, where the Secondary coefficient on Female $\times$ Relative School SES is 0.0334. The residual is a remaining outcome gap, not a direct preference estimate, so I use it only as supporting evidence. The main takeaway is that observed choice-set composition accounts for much of the final admitted-school SES gap, while the application-stage ranking pattern remains visible in the submitted lists.

The two decompositions construct the male counterfactual differently: Oaxaca–Blinder uses a linear mean-difference decomposition with pooled coefficients, whereas DFL reweights male applicants to the female observable distribution. Because the linear form tends to attribute more of the gap to composition, the Oaxaca–Blinder share is the larger of the two; the reweighting estimate is more conservative and less dependent on functional form, and is therefore taken as the headline figure, with the Oaxaca–Blinder figure reported alongside it. In Secondary the overlap diagnostics are good, with a maximum reweighting factor of about 2.9 and an effective male sample about 78 percent of the raw count, so the reweighted estimate is interpretable. In the younger stages the reweighting produces negative explained shares against very small raw gaps and is not interpreted, even though overlap there is adequate. Appendix Table A23 reports the reweighting estimates and weight diagnostics for all stages.

A final reading asks how much of the admitted-school gap the within-list ranking difference accounts for, as opposed to the composition of the lists themselves. Using the gender-specific

Table 8. Oaxaca–Blinder decomposition of the admitted-school SES gender gap

Stage	Raw gap	Explained	Explained share	Unexplained
All stages	0.006340	0.006204	97.8%	0.000137
Prekinder	0.002547	0.002819	110.7%	-0.000272
Kinder	0.004160	0.004287	103.1%	-0.000127
Early Primary	0.002173	0.001648	75.8%	0.000525
Upper Primary	0.002421	0.001950	80.5%	0.000471
Secondary	0.014634	0.013739	93.9%	0.000895

Notes: The table reports an Oaxaca–Blinder decomposition of the female-minus-male gap in admitted school SES, computed on each applicant's three highest-ranked submitted schools. School SES is measured as `school_non_priority_student_share`. "Explained" is the part of the raw gap accounted for by observable choice-set composition; "unexplained" is the residual, which may include unobserved choice-set differences, assignment-mechanism nonlinearity, measurement error, and ranking behavior, and so is not a measure of preference. Six-decimal figures are shown only to keep the explained shares internally consistent. Because the Prekinder, Kinder, Early Primary, and Upper Primary raw gaps are very small, their shares are reported for completeness and are not interpreted; where the explained part exceeds an already-tiny raw gap the share rises above 100 percent, which signals that the decomposition is uninformative at these stages rather than that composition over-explains the gap. The substantive reading centers on Secondary, the only stage where the raw gap is large enough to support interpretation. A high explained share concerns this admitted-outcome gap alone and carries no implication for the within-list ranking results in Table 5, which are measured on a different, choice-set-centered quantity.

Secondary estimates, the first-choice School SES gap (0.421 for girls against 0.400 for boys) splits into two parts. Holding girls' choice sets fixed and giving them boys' attribute weights lowers the expected School SES of their first-ranked school by 0.0036, while the remaining 0.0178 reflects the higher-SES composition of girls' lists. Within-list ranking therefore accounts for about 17 percent of the first-choice gap, and the School SES weighting difference alone for about 13 percent, with list composition accounting for the rest. Through the attenuation from top-three to assigned schools in Table 7, this maps to roughly one fifth of the 0.0146 admitted-school gap in Secondary. The exercise is descriptive and inherits the linkage caveats stated above, but it makes the division of labor concrete: the two margins point the same way, and this paper isolates the smaller, cleanly separated one. A gendered tilt that accounts for about a fifth of the secondary admitted-school SES gap, and recurs at national scale, is a meaningful share of that outcome gap rather than its principal cause. Appendix Table A38 reports the decomposition.

This admission-stage evidence plays a confirmatory role rather than carrying the main argument. It does not establish a new finding of its own; instead it closes the loop opened by the main analysis, showing that the gendered ranking pattern documented in submitted lists is neither contradicted nor rendered irrelevant once assignment takes place, but is largely filtered through observable choice-set composition before it reaches final outcomes. The central evidence of the thesis remains the application-stage ranking pattern; the decomposition simply confirms why that application stage, rather than the admission outcome, is the right place to look.

6 Discussion

6.1 From school attributes to gendered preference formation

Ranked school applications do more than feed an assignment algorithm. They record how families sort schools before vacancies, priority rules, and random ordering determine final placement. This connects mechanism-design work on school choice with gender-and-education research, which often observes later choices such as college applications, majors, or tracks. SAE lets us see an earlier margin.

6.2 Why secondary education may intensify the School SES pattern

The concentration of the result in secondary education fits the changing meaning of school choice across ages. Younger applicants' lists are tied closely to daily logistics, care, transport, proximity, and routine stability. Secondary-school choice is more closely tied to academic pathways, peer groups, school reputation, and future opportunities. In that setting, school social composition can work as a summary signal of the school environment. The gap itself stays moderate, an odds ratio of about 1.034 at its strongest, so what changes across stages is the clarity of the pattern, not its size.

This is more than a parental-protection story. Protectiveness might be plausible at younger ages, yet the strongest coefficient sits in Secondary, where students' own preferences are most likely to weigh on the list, so the secondary pattern reads better as a joint family-student decision. School SES is likely to capture several perceived attributes at once: peer environment, school climate, academic reputation, social networks, safety, and expected educational pathways.

Appendix Table A14 provides one descriptive check. In Secondary, the female–male gap in the applicant-level School SES premium is 0.0044 in low-dispersion choice sets and 0.0109 in high-dispersion choice sets. Upper Primary shows the same direction, on a smaller scale. This pattern is consistent with larger gender differences when applicants face sharper social-composition contrasts, although high-dispersion choice sets also give any SES-based ranking difference more room to appear.

Appendix Table A15 adds a second, lighter check by splitting the descriptive School SES premium across all individual application grades, from Prekinder through 4 Medio. The table reports the full calculation: male and female applicant counts, mean School SES among top-three schools, mean School SES among non-top alternatives, the applicant-level premium, the female–male gap, and a simple two-sample standard error. The female–male premium gap is positive in almost every grade, the one exception being Grade 4 Básico (-0.0019), and its size and precision vary. It is statistically strongest at the secondary entry grade, Grade 1 Medio (0.0070, estimated on the largest sample), and of comparable raw size at Grade 4 Medio (0.0072). The premium is clearly not confined to a single grade or to the initial transition into secondary school.

The secondary distance and rural-school results fit this reading only in part. A positive Female $\times$ Relative Log Distance coefficient does not mean distance is valued for its own sake; it remains a cost. Read literally, the positive sign means only that girls' secondary rankings are somewhat less deterred by distance than boys', consistent with a greater willingness to trade proximity for other valued secondary-school attributes. The triple interactions also do not show a robust joint pattern between distance and the other school attributes: on the true rankings the distance-by-demand term is insignificant, while the distance-by-SES term, though negative and significant, runs against a farther-and-higher-SES interpretation rather than supporting it. Appendix Table A16 adds that the rural-school penalty for female applicants is concentrated in Secondary and that the positive distance pattern is no stronger in rural-exposed choice sets. This age profile sits awkwardly with a simple mobility or safety story and points instead toward secondary-school reputation, academic pathways, and peer environment.

The other interaction terms are useful mainly as context for the School SES result, which remains the clearest and most stable gendered difference. The near-zero secondary demand interaction shows that the School SES premium is not a taste for popular schools, since high-demand schools signal reputation but also competition and admission risk. The positive quality interaction in Early Primary and Secondary points the same way as School SES, though it is less stable across years; it is consistent with evidence that attending a better school raises later attainment more for girls than for boys (Deming et al. 2014), which makes a forward-looking interpretation of the quality interaction reasonable.

The negative single-sex interaction is better read as sorting around a particular institutional type, since single-sex schools come bundled with religious, private, and historical attributes and the wider evidence on single-sex schooling is mixed (Pahlke, Hyde, and Allison 2014). It is not a verdict on single-sex education. More broadly, the non-SES interactions show that the School SES result is not a popularity, distance, rural-school, or single-sex-school result.

6.3 School SES, segregation, and contribution

The School SES result links gendered school choice to Chile's broader segregation problem. In this market, School SES is tied to reputation, peer composition, fees, family networks, and perceived opportunities. The coefficient should therefore be read as a weight on a bundle of related school features, not as evidence for one mechanism.

This interpretation is close to Abdulkadiroğlu et al. (2020), who show that parental rankings correlate with peer composition and measured value-added, but not with causal school effectiveness once peer composition is held fixed. Families may read composition as a signal of school quality. In the present data, the gendered School SES coefficient locates where this difference appears: in the relative ranking of listed secondary schools.

The result also matters before assignment. SAE processes rankings that already contain this gendered tilt. The gap is small for any one applicant, but it recurs at national scale in a stratified school market. Whether it comes mainly from parents, students, or both remains outside the data.

6.4 Relation to recent evidence on SAE and segregation

These findings fit recent work on why Chile's centralized admission reform has done less to reduce socioeconomic segregation than expected. Two studies using the staggered rollout of SAE reach a similar conclusion: Honey and Carrasco (2023) find that removing admission barriers had only a moderate effect on access for low-income students, and Kutscher, Nath, and Urzúa (2023) document that secondary-school segregation actually rose after the reform in districts with high pre-existing residential segregation and a large private-school presence. The common message is that a strategy-proof assignment mechanism, on its own, does not undo stratification when the underlying preferences and the geography of school supply remain unchanged. The present paper is consistent with that message and adds a specific dimension to it. Even within this centralized system, and before assignment operates, submitted rankings already carry a gendered tilt toward school social composition. Part of what the mechanism processes is therefore demand that is already differentiated by applicant gender.

The result is also close to Elacqua and Kutscher (2023). They show that low-SES families respond strongly to non-academic school attributes, including reported safety, discrimination, and student self-efficacy. School SES in this paper is also a non-academic school attribute, although it captures social composition rather than a specific survey-reported feature. The comparison suggests that demand in the Chilean system is differentiated along more than one margin: by family socioeconomic status in their study, and by applicant gender in this paper.

6.5 Why a small effect may still be policy-relevant

A one-standard-deviation change in relative School SES shifts the female-minus-male odds ratio by only about three to four percent in Secondary, and less elsewhere. The policy relevance comes from repetition at scale. SAE processes hundreds of thousands of applicants each year, and the tilt enters a school supply that is already highly socioeconomically stratified (Gutiérrez, Jerrim, and Torres 2020).

The paper does not estimate a full system-wide counterfactual. That would require reconstructing the assignment mechanism, reassigning seats under de-gendered preference lists, and then measuring changes in segregation indices with detailed priority and lottery information. The practical implication is more direct: if the gendered difference is already present in submitted rankings, the relevant policy margins are upstream, in information, transport constraints, and the local supply of schools of differing social composition.

6.6 Limitations

There are at least four limitations worth keeping in view.

First, the data show rankings rather than motives. I observe the listed schools and their order, not who made the decision, what information the family had, or which school features mattered most to them.

Second, the model works inside the submitted list. It explains how families order listed schools, not how those schools entered the list. This is the separability assumption discussed in Section 4.5: if choice-set formation is itself gendered and not separable from ranking, part of it will load onto the interaction terms. Table 3a shows that observed list composition is similar by gender across most attributes, with the largest difference (in Secondary School SES, about 0.13 standard deviations) still modest and pointing the same way as the within-list result, and Appendix Table A34 shows that the Secondary result remains aligned with the national estimate when models are estimated within applicants' regions of residence. The remaining limitation is finer than the data can observe: families may still differ in information, perceived safety, transport constraints, search effort, or neighborhood-level feasible schools. One upstream margin is worth naming explicitly: a first way families shape a school's social composition is by choosing where to live, which sets the pool of nearby, feasible schools. Because the analysis conditions on the submitted list and on distance, it captures ranking preferences given that residential sorting, not the prior choice of neighborhood itself.

Third, some variables are proxies. School SES captures school social composition, not family income, parental education, safety, or peer quality directly. Top3 Demand measures application pressure in the same admission round. The leave-one-out check in Appendix Table A29 addresses the mechanical own-contribution concern, but demand remains an observed market measure.

Fourth, some evidence is descriptive. The admission result analysis uses admitted schools rather than enrollment, the decomposition exercises summarize admitted-school gaps, and the 2017–2024 extension compares cohorts rather than tracking the same students. The main claim is therefore narrower: in Secondary, applications submitted for girls give somewhat more weight to school social composition within the ranked list.

7 Conclusion

Most school-choice research observes students after assignment, when family rankings, seat constraints, priorities, and lotteries have already been folded together. This thesis works one step earlier, using submitted SAE preference lists to study gendered school-choice weights before assignment. The central finding is that applications submitted for girls place slightly greater relative weight on school social composition than those submitted for boys, with the strongest and most stable pattern in Secondary.

The analysis uses national 2025 SAE data across five entry-stage groups, with 448,137 applicants and 1,512,610 student-school observations in the final estimation sample. The Secondary School SES interaction is positive in the baseline model and remains positive under a stricter controls-and-fixed-effects specification. It survives applicant-clustered inference, top-rank restrictions, leave-one-out demand, and low- versus high-dispersion choice-set splits, and it reappears in every cohort from 2017 to 2024. The evidence therefore supports a specific result rather than a broad claim about all school attributes.

The admission-stage decomposition shows why final admissions cannot be read directly as preferences: much of the Secondary admitted-school SES gap is accounted for by observed choice-set composition, while a smaller residual is consistent with the within-list ranking pattern. The policy implication is upstream. If gendered sorting is already visible in submitted rankings, information, transport conditions, and local school supply matter before the assignment algorithm begins.

Several extensions would be useful. Future work could link SAE applications to enrollment and longer-run outcomes, pair the administrative data with family surveys or interviews, and test whether these gendered choice weights vary with family socioeconomic background, information access, transport access, and local school supply. The most direct test of the accumulation argument would reconstruct the SAE assignment mechanism and reassign seats under de-gendered counterfactual preference lists. The resulting changes in segregation indices would show how much the measured ranking tilt matters at the system level.

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Appendix: Additional Regression Tables

The appendix reports robustness, placebo, suggestive-evidence, and cross-cohort tables corresponding to the regression workbooks. Unless a table note states otherwise, all appendix tables report the author's own estimations based on the SAE application data (Ministerio de Educación de Chile); the 2017–2024 cross-cohort tables draw on the corresponding earlier-year SAE files.

General notes for Appendix Tables A1–A11, A13, and A17–A30: unless a table note states otherwise, conditional-logit appendix tables report model-based z-statistics from the archived workbooks and are used as coefficient-stability checks. Exact repeats of the current main specification, Appendix Table A24 and the raw-demand main row in Appendix Table A29, use applicant-clustered z-statistics to match Table 5. Table A12 reports an auxiliary student fixed-effects LPM with school-grade clustered standard errors. Tables A14 and A15 are descriptive applicant-level premium checks with simple two-sample standard errors. Table A16 is an auxiliary applicant fixed-effects LPM with applicant-clustered t-statistics. Tables A17–A22 give 2017–2024 cross-cohort estimates. Table A23 reports DFL diagnostics; Table 8 reports Oaxaca–Blinder results. Tables A31–A34 report the clustered-inference, top-rank, SES-dispersion, and applicant-region local-market checks. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A1. Robustness check: 2% winsorization

Selected coefficients, rank-ordered (exploded) logit

VARIABLES	Prekinder	Kinder	Early Primary	Upper Primary	Secondary
Female × Relative Quality	0.0004	-0.0109	0.0114**	0.0014	0.0172***
	(0.07)	(-0.99)	(2.05)	(0.25)	(4.68)
Female × Relative Log Distance	-0.0107*	-0.0233**	0.0071	0.0203***	0.0089***
	(-1.86)	(-2.15)	(1.30)	(3.58)	(2.58)
Female × Relative School SES	-0.0020	0.0037	-0.0070	0.0113*	0.0338***
	(-0.35)	(0.32)	(-1.27)	(1.94)	(9.52)
Female × Relative Log Top3 Demand	0.0093	0.0087	0.0278***	0.0052	-0.0009
	(1.44)	(0.69)	(4.43)	(0.74)	(-0.26)
Female × Rural	0.0042	-0.1513*	-0.0632*	-0.0521	-0.0581**
	(0.11)	(-1.86)	(-1.88)	(-1.46)	(-2.14)
Female × Single-Sex School	-0.0265	0.0160	-0.0242	-0.3196***	-0.0442**
	(-0.65)	(0.21)	(-0.69)	(-10.66)	(-2.52)
Relative Quality	0.0424***	0.0467***	0.0557***	0.0561***	0.0148***
	(10.09)	(6.11)	(14.83)	(13.62)	(5.67)
Relative Log Distance	-0.2066***	-0.2029***	-0.2380***	-0.2458***	-0.1547***
	(-50.75)	(-26.74)	(-62.92)	(-60.34)	(-63.18)
Relative School SES	0.0593***	0.0820***	0.1141***	0.1119***	0.0408***
	(14.13)	(10.42)	(30.20)	(26.82)	(15.93)
Relative Log Top3 Demand	0.1178***	0.2221***	0.2947***	0.5645***	0.2525***
	(25.68)	(25.47)	(69.28)	(112.40)	(99.08)
Rural	0.0877***	0.1487***	0.2525***	0.1476***	0.1902***
	(3.19)	(2.72)	(11.16)	(5.87)	(10.22)
Single-Sex School	0.0459	0.0088	0.1607***	0.3219***	0.1322***
	(1.38)	(0.15)	(5.78)	(14.01)	(10.14)
Ranked rows	243,762	70,520	293,222	298,061	607,045
Applicants	83,667	23,317	89,779	87,068	164,306

Notes: Core continuous variables are winsorized at 2%/98% instead of 1%/99%. Estimates from the rank-ordered (exploded) logit on the true submitted rankings; z-statistics in parentheses. Significance: *** p<0.01, ** p<0.05, * p<0.10.

Table A2. Former rank-sensitive robustness check

Now subsumed by the main rank-ordered specification

Note. In the original draft this table reported a separate "rank-sensitive" robustness check, distinct from a top-three indicator model. Under the revised empirical strategy the main specification is itself the rank-ordered (exploded) logit on the full submitted ranking (Table 5), so a separate rank-sensitive check is no longer a distinct exercise: the rank information is already used in the main model. This entry is therefore retained only as a pointer to Table 5, and the previous standalone table has been removed to avoid duplication.

Table A3. Robustness check: urban-only sample

Selected coefficients, rank-ordered (exploded) logit

VARIABLES	Prekinder	Kinder	Early Primary	Upper Primary	Secondary
Female × Relative Quality	0.0010	-0.0125	0.0103*	0.0036	0.0172***
	(0.16)	(-1.07)	(1.72)	(0.57)	(4.49)
Female × Relative Log Distance	-0.0096	-0.0222*	0.0070	0.0173***	0.0144***
	(-1.58)	(-1.94)	(1.17)	(2.79)	(4.00)
Female × Relative School SES	-0.0018	0.0106	-0.0041	0.0123**	0.0351***
	(-0.29)	(0.89)	(-0.70)	(1.99)	(9.45)
Female × Relative Log Top3 Demand	0.0099	0.0126	0.0286***	-0.0022	-0.0012
	(1.45)	(0.96)	(4.29)	(-0.30)	(-0.32)
Female × Single-Sex School	-0.0037	0.0272	-0.0371	-0.3301***	-0.0577***
	(-0.09)	(0.35)	(-1.00)	(-10.61)	(-3.17)
Relative Quality	0.0383***	0.0465***	0.0547***	0.0528***	0.0136***
	(8.42)	(5.79)	(13.39)	(11.83)	(4.98)
Relative Log Distance	-0.2037***	-0.2054***	-0.2435***	-0.2525***	-0.1619***
	(-46.95)	(-25.37)	(-58.04)	(-56.30)	(-62.63)
Relative School SES	0.0611***	0.0756***	0.1139***	0.1209***	0.0443***
	(13.61)	(9.18)	(28.13)	(27.17)	(16.50)
Relative Log Top3 Demand	0.1344***	0.2249***	0.2936***	0.5625***	0.2553***
	(27.51)	(24.67)	(64.99)	(105.38)	(95.13)
Single-Sex School	0.0494	0.0060	0.1757***	0.3337***	0.1564***
	(1.44)	(0.10)	(5.99)	(14.03)	(11.58)
Ranked rows	215,713	63,737	252,496	256,615	555,099
Applicants	74,167	21,156	77,614	75,000	150,652

Notes: Estimation restricted to choice sets containing no rural school. Rural and Female × Rural are omitted because the urban-only sample contains no rural schools. Rank-ordered (exploded) logit on the true submitted rankings; z-statistics in parentheses. Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A4. Robustness check: non-centered variables

Selected coefficients, rank-ordered (exploded) logit

VARIABLES	Prekinder	Kinder	Early Primary	Upper Primary	Secondary
Female × Relative Quality	0.0010	-0.0144	0.0154**	0.0020	0.0225***
	(0.12)	(-0.95)	(2.12)	(0.27)	(4.79)
Female × Relative Log Distance	-0.0202*	-0.0391**	0.0121	0.0255***	0.0140**
	(-1.89)	(-2.13)	(1.41)	(2.93)	(2.41)
Female × Relative School SES	-0.0032	0.0074	-0.0124	0.0192*	0.0569***
	(-0.27)	(0.34)	(-1.26)	(1.94)	(9.39)
Female × Relative Log Top3 Demand	0.0150	0.0158	0.0496***	0.0079	-0.0028
	(1.35)	(0.73)	(4.36)	(0.76)	(-0.39)
Female × Rural	0.0041	-0.1488*	-0.0568*	-0.0536	-0.0573**
	(0.11)	(-1.83)	(-1.68)	(-1.49)	(-2.10)
Female × Single-Sex School	-0.0289	0.0145	-0.0216	-0.3181***	-0.0444**
	(-0.71)	(0.19)	(-0.61)	(-10.61)	(-2.53)
Relative Quality	0.0599***	0.0632***	0.0721***	0.0707***	0.0165***
	(10.04)	(6.04)	(14.56)	(13.38)	(4.94)
Relative Log Distance	-0.3817***	-0.3417***	-0.3742***	-0.3783***	-0.2645***
	(-50.39)	(-26.50)	(-62.73)	(-60.58)	(-63.62)
Relative School SES	0.1198***	0.1540***	0.2007***	0.1946***	0.0706***
	(14.16)	(10.32)	(29.86)	(27.41)	(16.13)
Relative Log Top3 Demand	0.2029***	0.3848***	0.5390***	0.8392***	0.5124***
	(25.63)	(25.56)	(69.80)	(112.55)	(99.85)
Rural	0.0910***	0.1577***	0.2628***	0.1660***	0.1951***
	(3.31)	(2.88)	(11.57)	(6.57)	(10.47)
Single-Sex School	0.0458	0.0099	0.1589***	0.3255***	0.1348***
	(1.38)	(0.17)	(5.71)	(14.17)	(10.34)
Ranked rows	243,762	70,520	293,222	298,061	607,045
Applicants	83,667	23,317	89,779	87,068	164,306

Notes: Core continuous attributes are standardized without choice-set centering. Because the within-applicant comparison in the conditional logit is invariant to adding a constant to each alternative, centering does not change the choice probabilities; only the standardization scale differs, so coefficient magnitudes are larger but signs and significance match the main estimates. z-statistics in parentheses. Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A5. Placebo check: school-is-free variable

Selected coefficients

VARIABLES	Prekinder	Kinder	Early Primary	Upper Primary	Secondary
Female $\times$ Relative Quality	0.0005	-0.0102	0.0110**	0.0007	0.0184***
	(0.08)	(-0.93)	(1.99)	(0.12)	(5.00)
Female $\times$ Relative Log Distance	-0.0112*	-0.0233**	0.0082	0.0173***	0.0084**
	(-1.94)	(-2.12)	(1.44)	(2.96)	(2.43)
Female $\times$ Relative School SES	-0.0017	0.0025	-0.0067	0.0108	0.0349***
	(-0.26)	(0.19)	(-1.06)	(1.62)	(8.78)
Female $\times$ Relative Log Top3 Demand	0.0088	0.0090	0.0279***	0.0066	-0.0014
	(1.36)	(0.71)	(4.42)	(0.93)	(-0.38)
Female $\times$ Rural	0.0024	-0.1500*	-0.0570*	-0.0519	-0.0559**
	(0.06)	(-1.84)	(-1.69)	(-1.44)	(-2.05)
Female $\times$ Single-Sex School	-0.0273	0.0169	-0.0236	-0.3196***	-0.0461***
	(-0.67)	(0.23)	(-0.67)	(-10.65)	(-2.62)
Female $\times$ School-is-free	-0.0023	-0.0060	0.0046	-0.0029	0.0234*
	(-0.10)	(-0.16)	(0.25)	(-0.16)	(1.93)
Ranked rows	243,762	70,520	293,222	298,061	607,045
Applicants	83,667	23,317	89,779	87,068	164,306

Notes: This supplementary check adds Female $\times$ School-is-free to the main rank-ordered (exploded) logit specification, to see whether the gendered School SES result is sensitive to controlling for whether schools charge no fees. School-is-free is a substantive fee-related school attribute, plausibly correlated with social composition, so this is an added-control robustness check rather than a pure placebo or falsification test. Estimated on the true submitted rankings, the Female $\times$ School-is-free interaction is small and at most marginally significant, and the Female $\times$ Relative School SES interaction in Secondary is essentially unchanged (0.0349, z $= 8.78$, versus 0.0334 in the main specification). The check is treated as supplementary, not definitive evidence. z-statistics in parentheses. Significance: *** p$<$0.01, ** p$<$0.05, * p$<$0.10.

Table A6. Random-placebo test for the secondary School SES result

Quantity	Value
Observed Female $\times$ Relative School SES (Secondary)	0.0334
Placebo mean	-0.0003
Placebo standard deviation	0.0031
Placebo range (min, max)	$[-0.0072,\ 0.0067]$
Placebo 95% interval (2.5th–97.5th percentile)	$[-0.0069,\ 0.0049]$
Number of placebo draws	50
Placebo draws at least as large as observed	0

Notes: The observed coefficient is the Secondary Female $\times$ Relative School SES estimate from the main rank-ordered (exploded) logit model (Table 5). The placebo distribution is constructed by randomly reassigning applicants' top-three schools and re-estimating the same coefficient 50 times, which breaks any genuine link between gender and school social composition while preserving each applicant's list length and the overall distribution of school attributes. The observed value lies far outside this distribution: no positive placebo draw exceeds 0.0067, and the largest absolute placebo value is 0.0072; both are far below the observed 0.0334. With 0 of 50 placebo draws as large as the observed estimate, the empirical placebo evidence is consistent with $p \leq 1/51 \approx 0.02$. The secondary School SES result is therefore not an artifact of the ranking structure or of random assignment.

Table A7. Suggestive model 1: quality and distance

Rank-ordered (exploded) logit

VARIABLES	Prekinder	Kinder	Early Primary	Upper Primary	Secondary
Female × Relative Quality	0.0031	-0.0020	0.0199***	0.0027	0.0293***
	(0.58)	(-0.20)	(4.01)	(0.54)	(9.10)
Female × Relative Log Distance	-0.0103*	-0.0201*	0.0105**	0.0209***	0.0090***
	(-1.83)	(-1.91)	(1.99)	(4.20)	(2.73)
Relative Quality	0.1070***	0.1529***	0.2065***	0.3066***	0.0916***
	(28.30)	(22.01)	(61.28)	(84.08)	(40.38)
Relative Log Distance	-0.1960***	-0.1974***	-0.2345***	-0.2054***	-0.1303***
	(-49.09)	(-26.69)	(-63.78)	(-57.25)	(-55.43)

Notes: This model includes quality and distance before adding School SES and Top3 Demand. Estimates from the rank-ordered (exploded) logit on the true submitted rankings; z-statistics in parentheses. Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A8. Suggestive model 2: adding School SES

Rank-ordered (exploded) logit

VARIABLES	Prekinder	Kinder	Early Primary	Upper Primary	Secondary
Female × Relative Quality	0.0020	-0.0080	0.0167***	-0.0032	0.0116***
	(0.37)	(-0.77)	(3.25)	(-0.61)	(3.31)
Female × Relative Log Distance	-0.0112**	-0.0222**	0.0090	0.0114**	0.0063*
	(-1.96)	(-2.06)	(1.64)	(2.17)	(1.92)
Female × Relative School SES	0.0038	0.0135	0.0059	0.0276***	0.0441***
	(0.69)	(1.30)	(1.16)	(5.32)	(12.91)
Relative Quality	0.0815***	0.1083***	0.1529***	0.2217***	0.0915***
	(20.87)	(15.06)	(43.96)	(58.63)	(37.02)
Relative Log Distance	-0.2071***	-0.2095***	-0.2532***	-0.2275***	-0.1303***
	(-51.10)	(-27.68)	(-66.35)	(-60.22)	(-55.40)
Relative School SES	0.0959***	0.1593***	0.1970***	0.2693***	0.0000
	(24.56)	(22.23)	(56.41)	(72.17)	(0.01)

Notes: This model adds School SES to quality and distance. The Female × Relative Quality interaction in Secondary falls once School SES enters, indicating that part of the apparent quality tilt reflects school social composition. Estimates from the rank-ordered (exploded) logit on the true submitted rankings; z-statistics in parentheses. Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A9. Suggestive model 3: adding Top3 Demand

Rank-ordered (exploded) logit

VARIABLES	Prekinder	Kinder	Early Primary	Upper Primary	Secondary
Female $\times$ Relative Quality	0.0005	-0.0113	0.0118**	-0.0003	0.0178***
	(0.08)	(-1.03)	(2.16)	(-0.05)	(4.87)
Female $\times$ Relative Log Distance	-0.0111*	-0.0248**	0.0062	0.0139**	0.0071**
	(-1.92)	(-2.26)	(1.11)	(2.37)	(2.08)
Female $\times$ Relative School SES	-0.0017	0.0056	-0.0055	0.0140**	0.0328***
	(-0.29)	(0.49)	(-0.99)	(2.42)	(9.32)
Female $\times$ Relative Log Top3 Demand	0.0090	0.0107	0.0251***	0.0083	-0.0029
	(1.43)	(0.87)	(4.08)	(1.19)	(-0.82)
Relative Quality	0.0436***	0.0474***	0.0599***	0.0583***	0.0159***
	(10.45)	(6.25)	(16.04)	(14.20)	(6.11)
Relative Log Distance	-0.2051***	-0.2027***	-0.2429***	-0.2501***	-0.1524***
	(-50.27)	(-26.32)	(-61.89)	(-59.79)	(-62.38)
Relative School SES	0.0586***	0.0798***	0.1090***	0.1098***	0.0377***
	(14.00)	(10.17)	(28.93)	(26.52)	(14.79)
Relative Log Top3 Demand	0.1146***	0.2207***	0.2901***	0.5629***	0.2512***
	(25.42)	(25.66)	(68.65)	(112.46)	(98.89)

Notes: This model adds Top3 Demand. The Female $\times$ Relative School SES interaction in Secondary remains positive and significant after demand is included, so the social-composition result is not explained by school popularity. Estimates from the rank-ordered (exploded) logit on the true submitted rankings; z-statistics in parentheses. Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A10. Triple interaction: distance and School SES

Selected coefficients, rank-ordered (exploded) logit

VARIABLES	Prekinder	Kinder	Early Primary	Upper Primary	Secondary
Relative Log Distance $\times$ Relative School SES	-0.0374***	-0.0388***	-0.0475***	-0.0533***	-0.0222***
	(-8.48)	(-4.83)	(-12.04)	(-12.45)	(-8.53)
Female $\times$ Relative Log Distance $\times$ Relative School SES	-0.0059	0.0020	-0.0123**	-0.0132**	-0.0081**
	(-0.96)	(0.17)	(-2.18)	(-2.22)	(-2.25)
Female $\times$ Relative Log Distance	-0.0120**	-0.0221**	0.0067	0.0155***	0.0066*
	(-2.05)	(-2.01)	(1.17)	(2.63)	(1.89)
Female $\times$ Relative School SES	-0.0019	0.0040	-0.0057	0.0117**	0.0337***
	(-0.32)	(0.35)	(-1.02)	(2.01)	(9.48)
Ranked rows	243,762	70,520	293,222	298,061	607,045
Applicants	83,667	23,317	89,779	87,068	164,306

Notes: The three-way term Female $\times$ Relative Log Distance $\times$ Relative School SES tests whether the gendered distance weight depends on a school's social composition. This second-order interaction is sensitive to specification: estimated on the true submitted rankings it is small and, in Secondary, negative, whereas the core Female $\times$ Relative School SES term remains positive and strongly significant. The triple interaction is therefore not used as central evidence. z-statistics in parentheses. Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A11. Triple interaction: distance and Top3 Demand

Selected coefficients

VARIABLES	Prekinder	Kinder	Early Primary	Upper Primary	Secondary
Relative Log Distance × Relative Log Top3 Demand	-0.0474***	-0.0498***	-0.0469***	-0.0972***	-0.0482***
	(-10.44)	(-5.99)	(-11.95)	(-20.92)	(-17.61)
Female × Relative Log Distance × Relative Log Top3 Demand	0.0025	0.0026	-0.0082	0.0008	-0.0036
	(0.39)	(0.22)	(-1.44)	(0.12)	(-0.96)
Female × Relative Log Distance	-0.0105*	-0.0215*	0.0072	0.0168***	0.0075**
	(-1.80)	(-1.95)	(1.25)	(2.79)	(2.15)
Female × Relative Log Top3 Demand	0.0078	0.0101	0.0281***	0.0052	-0.0012
	(1.21)	(0.80)	(4.45)	(0.74)	(-0.33)
Female × Relative School SES	-0.0015	0.0046	-0.0064	0.0112*	0.0335***
	(-0.25)	(0.40)	(-1.15)	(1.93)	(9.43)
Ranked rows	243,762	70,520	293,222	298,061	607,045
Applicants	83,667	23,317	89,779	87,068	164,306

Notes: The three-way term Female × Relative Log Distance × Relative Log Top3 Demand tests whether the gendered distance weight depends on a school's relative demand. Estimated on the true submitted rankings, this interaction is small and statistically insignificant at every stage, including Secondary, so the data do not support a joint distance-by-demand pattern; the core Female × Relative School SES term remains positive and strongly significant in Secondary. z-statistics in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table A12. Auxiliary clustered-inference check

Student fixed-effects linear probability model with school-grade clustered standard errors

VARIABLES	Prekinder	Kinder	Early Primary	Upper Primary	Secondary
Female × Relative Log Distance	-0.0033	-0.0034	0.0022	0.0016	-0.0003
	(0.0023)	(0.0039)	(0.0017)	(0.0016)	(0.0013)
Female × Relative Log Top3 Demand	0.0027	0.0060	0.0062***	-0.0036*	-0.0034***
	(0.0026)	(0.0047)	(0.0022)	(0.0020)	(0.0012)
Female × Relative Quality	0.0012	-0.0080*	0.0041*	0.0051***	0.0047***
	(0.0025)	(0.0044)	(0.0021)	(0.0020)	(0.0014)
Female × Relative School SES	0.0005	0.0019	-0.0005	0.0012	0.0098***
	(0.0024)	(0.0045)	(0.0021)	(0.0020)	(0.0013)
Student fixed effects	YES	YES	YES	YES	YES
School-grade clustered SE	YES	YES	YES	YES	YES
Ranked rows	243,762	70,520	293,222	298,061	607,045
Applicants	83,667	23,317	89,779	87,068	164,306

Notes: This table reports an auxiliary student fixed-effects linear probability model estimated on the true submitted rankings, with the outcome equal to one for the most-preferred (rank-1) school. Values in parentheses are standard errors clustered at the school-grade level. Because this estimator differs from the rank-ordered (exploded) logit model, coefficients are not directly comparable in magnitude with Table 5; the table is a complementary clustered-inference check. The central School SES interaction remains positive and strongly significant in Secondary under clustered inference. Significance: *** p<0.01, ** p<0.05, * p<0.10.

Table A13. Robustness check: adding Female × Log School Size

Selected coefficients, rank-ordered (exploded) logit

VARIABLES	Prekinder	Kinder	Early Primary	Upper Primary	Secondary
Female × Relative Quality	0.0008	-0.0103	0.0113**	0.0010	0.0174***
	(0.14)	(-0.93)	(2.04)	(0.18)	(4.68)
Female × Relative Log Distance	-0.0109*	-0.0235**	0.0074	0.0168***	0.0088**
	(-1.88)	(-2.14)	(1.31)	(2.87)	(2.55)
Female × Relative School SES	-0.0015	0.0045	-0.0074	0.0110*	0.0317***
	(-0.25)	(0.39)	(-1.34)	(1.88)	(8.78)
Female × Relative Log Top3 Demand	0.0063	0.0034	0.0249***	-0.0084	-0.0029
	(0.79)	(0.23)	(3.31)	(-1.01)	(-0.73)
Female × Log School Size	0.0035	0.0079	0.0039	0.0210***	0.0011
	(0.49)	(0.63)	(0.60)	(3.07)	(0.29)
Female × Rural	0.0055	-0.1427*	-0.0521	-0.0416	-0.0604**
	(0.14)	(-1.74)	(-1.53)	(-1.15)	(-2.21)
Female × Single-Sex School	-0.0317	-0.0046	-0.0347	-0.3357***	-0.0643***
	(-0.77)	(-0.06)	(-0.98)	(-11.14)	(-3.64)
Relative Quality	0.0422***	0.0471***	0.0559***	0.0553***	0.0214***
	(10.05)	(6.18)	(14.87)	(13.42)	(8.11)
Relative Log Distance	-0.2065***	-0.2031***	-0.2449***	-0.2533***	-0.1577***
	(-50.39)	(-26.31)	(-62.17)	(-60.30)	(-63.99)
Relative School SES	0.0595***	0.0795***	0.1142***	0.1145***	0.0517***
	(14.18)	(10.07)	(30.12)	(27.43)	(19.81)
Relative Log Top3 Demand	0.1148***	0.2485***	0.3388***	0.5978***	0.2833***
	(20.09)	(24.14)	(66.26)	(99.99)	(99.59)
Log School Size	0.0041	-0.0402***	-0.0638***	-0.0453***	-0.0624***
	(0.80)	(-4.64)	(-14.42)	(-9.26)	(-22.79)
Rural	0.0941***	0.1282**	0.2176***	0.1365***	0.1721***
	(3.40)	(2.32)	(9.49)	(5.36)	(9.21)
Single-Sex School	0.0426	0.0482	0.2068***	0.3497***	0.1663***
	(1.27)	(0.80)	(7.37)	(15.11)	(12.67)
Ranked rows	243,762	70,520	293,222	298,061	607,045
Applicants	83,667	23,317	89,779	87,068	164,306

Notes: This table repeats the main rank-ordered (exploded) logit specification and adds Female × Log School Size. The Secondary size interaction is small, while the School SES interaction remains close to its main value. The size interaction is significant in Upper Primary, so the school-size pattern is stage-specific. z-statistics in parentheses. Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A14. Exploratory heterogeneity by choice-set School SES dispersion

Applicant-level School SES premium, true submitted rankings

Stage	SES dispersion group	Male premium	Female premium	Female–Male gap	Applicants
Prekinder	Low	0.0083	0.0099	0.0016 (0.0015)	9,137
Prekinder	High	0.0362	0.0388	0.0026 (0.0032)	9,136
Kinder	Low	0.0131	0.0118	-0.0013 (0.0027)	2,995
Kinder	High	0.0586	0.0632	0.0045 (0.0057)	2,995
Early Primary	Low	0.0123	0.0159	0.0036*** (0.0012)	15,029
Early Primary	High	0.0686	0.0683	-0.0004 (0.0025)	15,028
Upper Primary	Low	0.0230	0.0241	0.0011 (0.0012)	16,881
Upper Primary	High	0.0979	0.1005	0.0027 (0.0023)	16,880
Secondary	Low	0.0005	0.0049	0.0044*** (0.0008)	36,207
Secondary	High	0.0208	0.0317	0.0109*** (0.0016)	36,206

Notes: Descriptive applicant-level School SES premium (mean School SES of an applicant's top-ranked schools minus that of the lower-ranked schools), split by whether the applicant's choice-set School SES dispersion is below or above the stage-specific median. Computed on the true submitted rankings. Values in parentheses are two-sample standard errors for the female–male gap. The table is descriptive and not a rank-ordered (exploded) logit estimate. Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A15. Descriptive all-grade heterogeneity

Full applicant-level School SES premium by application grade, true submitted rankings

Grade	Stage	Male prem.	Female prem.	Female–Male gap	Applicants
Prekinder	Prekinder	0.0221	0.0244	0.0023 (0.0018)	18,273
Kinder	Kinder	0.0354	0.0381	0.0027 (0.0032)	5,990
Grade 1 Basico	Early Primary	0.0283	0.0300	0.0017 (0.0020)	14,908
Grade 2 Basico	Early Primary	0.0486	0.0499	0.0013 (0.0037)	4,731
Grade 3 Basico	Early Primary	0.0500	0.0551	0.0051 (0.0037)	4,826
Grade 4 Basico	Early Primary	0.0580	0.0562	-0.0019 (0.0034)	5,592
Grade 5 Basico	Upper Primary	0.0610	0.0618	0.0008 (0.0030)	6,990
Grade 6 Basico	Upper Primary	0.0618	0.0642	0.0024 (0.0029)	7,747
Grade 7 Basico	Upper Primary	0.0562	0.0576	0.0014 (0.0023)	11,181
Grade 8 Basico	Upper Primary	0.0649	0.0674	0.0025 (0.0028)	7,843
Grade 1 Medio	Secondary	0.0017	0.0087	0.0070^{***} (0.0010)	53,955
Grade 2 Medio	Secondary	0.0455	0.0472	0.0017 (0.0024)	10,701
Grade 3 Medio	Secondary	0.0342	0.0400	0.0058^{*} (0.0031)	6,374
Grade 4 Medio	Secondary	0.0265	0.0338	0.0072 (0.0066)	1,383

Notes: Descriptive applicant-level School SES premium by individual application grade, computed on the true submitted rankings. "Premium" is the mean School SES of the applicant's top-ranked schools minus that of the lower-ranked schools. The Female–Male gap is the difference in mean premiums; values in parentheses are two-sample standard errors. The strongest and most precisely estimated single-grade gap is in Grade 1 Medio, the secondary entry grade. The table is descriptive and not a rank-ordered (exploded) logit estimate. Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A16. Rural-school heterogeneity and the mobility/safety channel

Auxiliary applicant fixed-effects LPM checks

Panel A. Three-way interactions with rural-school status

Stage	Applicants	Rural rows	Female × Rural	t	Female × Distance	t	Female × Rural × Distance	t	Female × School SES	t	Female × Rural × School SES	t
Prekinder	89,915	0.121	-0.0041	(-0.61)	0.0075	(3.64)	-0.0072	(-1.98)	0.0020	(1.15)	-0.0017	(-0.33)
Kinder	23,916	0.077	0.0094	(0.62)	-0.0072	(-2.03)	0.0063	(0.77)	0.0028	(0.87)	-0.0071	(-0.57)
Early Primary	91,204	0.089	-0.0115	(-1.59)	-0.0046	(-2.56)	-0.0033	(-0.77)	0.0072	(4.53)	-0.0138	(-3.41)
Upper Primary	87,829	0.080	-0.0033	(-0.47)	0.0009	(0.52)	-0.0001	(-0.02)	0.0050	(3.18)	-0.0026	(-0.57)
Secondary	167,924	0.042	-0.0312	(-5.07)	0.0078	(5.76)	-0.0087	(-2.40)	0.0214	(17.93)	-0.0071	(-1.34)

Panel B. Split by rural exposure in the submitted choice set

Stage	Choice-set group	Applicants	Female × Distance	t	Female × School SES	t
Prekinder	All-urban choice sets	62,243	0.0068	(2.48)	0.0017	(0.90)
Prekinder	Rural-exposed choice sets	27,672	0.0035	(1.65)	0.0028	(0.94)
Kinder	All-urban choice sets	18,667	-0.0082	(-2.05)	0.0004	(0.11)
Kinder	Rural-exposed choice sets	5,249	-0.0048	(-0.95)	0.0125	(1.73)
Early Primary	All-urban choice sets	68,039	-0.0046	(-2.27)	0.0067	(3.78)
Early Primary	Rural-exposed choice sets	23,165	-0.0057	(-2.23)	0.0026	(0.93)
Upper Primary	All-urban choice sets	66,212	0.0019	(0.99)	0.0034	(1.96)
Upper Primary	Rural-exposed choice sets	21,617	-0.0019	(-0.79)	0.0079	(2.64)
Secondary	All-urban choice sets	133,515	0.0082	(4.99)	0.0220	(16.91)
Secondary	Rural-exposed choice sets	34,409	0.0006	(0.28)	0.0148	(5.52)

Notes: This table reports auxiliary applicant fixed-effects linear probability models. The outcome is whether a school is in the applicant's top three. All variables are constructed using the same choice-set-centered definitions as in the main analysis; Distance is Relative Log Distance and School SES is Relative School SES. Panel A uses the full stage sample and adds interactions with school-level rural status. Panel B splits applicants according to whether their submitted choice set contains at least one rural school. Because `is_rural` is a school-level indicator, this table should be read as rural-school heterogeneity, not as a comparison of rural and urban households. Values in parentheses in the body of the table are applicant-clustered t-statistics. The applicant fixed effects absorb the same applicant-level choice-set component that the main rank-ordered (exploded) logit model conditions out; the difference is functional form. The linear specification is used here because it permits the rural three-way interactions and applicant-clustered t-statistics to be reported transparently.

Cross-cohort appendix tables, 2017–2024

Comparable stage-grouped rank-ordered (exploded) logit estimates

The following tables report coefficient-level cross-cohort evidence for the core Female interactions. Each cell reports the coefficient with model-based z-statistic in parentheses. The estimates are not pooled across years and do not follow the same students over time. They are used to assess whether the 2025 patterns reported in the main text are repeated in earlier SAE cohorts.

Note on Tables A17–A22. All years are estimated by rank-ordered (exploded) logit on each applicant's true submitted ranking, using the same five-stage architecture as the 2025 baseline model; no list completion is applied. The headline pattern is the secondary Female × Relative School SES coefficient, which is positive and highly significant in every year. Only the School SES result is treated as a stable cross-cohort regularity.

Detailed reading of the cross-cohort estimates. The tables that follow can be read in more detail as set out here.

Appendix Table A17 also clarifies the stage profile of the School SES result. The 2025 stage profile recurs across years: the secondary coefficient is the largest and most precisely estimated in every cohort, while the younger stages are weaker and more erratic. Upper Primary is positive in every year and statistically significant from 2018 onward, with only the small 2017 sample falling short of significance. Early Primary is also positive in every year but significant in only about half of them (2018, 2019, 2022, and 2023), and small and insignificant in the others. Prekinder hovers near zero, switches sign across cohorts, and is never statistically significant. Kinder is the least stable stage of all: the coefficient is positive in most years but turns negative in 2020, 2022, and 2023, and reaches significance only in 2019 and 2021. This cross-year instability mirrors the within-2025 evidence in Section 5.1. The School SES conclusion therefore rests mainly on Upper Primary and especially Secondary, where the sign and significance are consistent across cohorts. Prekinder and Kinder play a descriptive role.

The demand results bear on whether the female School SES pattern is just a preference for more popular schools. Appendix Table A18 shows that Female × Relative Log Top3 Demand is positive and often significant in the primary stages, but in Secondary it is small and inconsistent. The secondary coefficient is strongly negative only in 2017 (-0.0361); in the other cohorts it is close to zero and mostly insignificant, and it is in fact significantly *positive* in 2022 ($+0.0159$). The 2025 coefficient in Table 5 is also small and negative (-0.0013, insignificant). The key point is therefore not that secondary demand is reliably negative, which it is not, but that the secondary School SES interaction is consistently large and positive across all cohorts while the secondary demand interaction is weak and unstable in sign. The two do not move together. If the School SES result were merely a relabeled taste for high-demand schools, the demand interaction should display the same strong and consistent secondary pattern, and it does not. School SES thus appears to capture social composition, peer environment, reputation, or social positioning more directly than market popularity does.

Appendix Table A19 reports the cross-year distance results, and here the picture changes markedly relative to the earlier list-completed specification. Estimated on the true submitted rankings, Female × Relative Log Distance in Secondary is essentially zero in every cohort from 2017 to 2024: the point estimates lie between about -0.015 and $+0.001$, and in the only year in which the coefficient is statistically significant, 2018, it is *negative* (-0.0154). The positive and significant secondary distance coefficient in the 2025 main model $(+0.0083)$ therefore does not recur in the earlier cohorts. The secondary distance result is therefore specific to the 2025 cohort rather than a stable cross-year regularity. This matches the triple-interaction evidence in Section 5.5, where the secondary distance weight shows no robust link with school social composition. Distance is reported as a weak and cohort-dependent pattern, not as core evidence.

The quality results are also less stable than the School SES result. Appendix Table A20 shows that Female × Relative Quality in Secondary is mixed across cohorts: it is small and insignificant in 2017–2018, significantly *negative* in 2019 (-0.0086), and significantly positive only in the more recent cohorts (2021, 2023, and 2024), as well as in the 2025 main model $(+0.0175)$. The sign therefore varies across years even at the secondary level. This suggests that quality contributes to the secondary pattern in recent cohorts but is not temporally consistent. The paper accordingly treats School SES as the main persistent gendered attribute and quality as a related but less stable dimension of school appeal.

Appendix Tables A21 and A22 report the two institutional school-type interactions. Female × Rural in Secondary is positive and insignificant in 2017, but negative in every subsequent cohort from 2018 to 2024 and statistically significant from 2020 onward; the 2025 estimate (-0.0585) is also negative and significant. From 2018 onward this reinforces the 2025 result that the rural-school penalty is concentrated in Secondary rather than in the youngest stages, though the earliest cohort does not fit the pattern. Female × Single-Sex School is genuinely unstable: it is significantly positive in 2017 $(+0.1012)$, close to zero or mixed in 2018–2021, and significantly negative in 2022–2023, with the 2024 and 2025 estimates negative as well. Because the sign reverses across cohorts, the single-sex result is not used as a main explanatory channel.

Table A17. Cross-cohort evidence, 2017–2024: Female × Relative School SES

Stage-specific coefficients from rank-ordered (exploded) logit models

Year	Prekinder	Kinder	Early Primary	Upper Primary	Secondary
2017	-0.0174	0.0146	0.0061	0.0168	0.0401***
	(-1.55)	(0.89)	(0.45)	(0.84)	(3.84)
2018	0.0010	0.0019	0.0206***	0.0349***	0.0452***
	(0.15)	(0.17)	(2.78)	(3.30)	(8.44)
2019	0.0030	0.0167**	0.0089*	0.0224***	0.0386***
	(0.61)	(2.22)	(1.68)	(3.24)	(9.27)
2020	-0.0076	-0.0082	0.0022	0.0269***	0.0343***
	(-1.40)	(-0.87)	(0.41)	(3.74)	(7.65)
2021	0.0021	0.0217**	0.0037	0.0127*	0.0381***
	(0.37)	(2.19)	(0.68)	(1.83)	(8.64)
2022	-0.0016	-0.0016	0.0155***	0.0199***	0.0293***
	(-0.30)	(-0.17)	(3.34)	(3.69)	(7.98)
2023	-0.0041	-0.0070	0.0136***	0.0212***	0.0411***
	(-0.74)	(-0.76)	(2.80)	(3.90)	(11.57)
2024	-0.0010	0.0154	0.0073	0.0154***	0.0371***
	(-0.16)	(1.45)	(1.35)	(2.60)	(10.41)

Notes: Values in parentheses are model-based z-statistics. The estimates are year-specific and stage-specific; they are not pooled across years and do not form a panel. The models use the same broad empirical architecture as the 2025 main specification: rank-ordered (exploded) logit, the same five stage groups, choice-set-centered relative attributes, and the same core Female interactions. Because the archived result files do not contain a fully harmonized set of controls and year-stage sample metadata, the tables should be read as evidence on sign, statistical precision, and approximate magnitude rather than as exact cohort-size-weighted trend estimates. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A18. Cross-cohort evidence, 2017–2024: Female × Relative Log Top3 Demand

Stage-specific coefficients from rank-ordered (exploded) logit models

Year	Prekinder	Kinder	Early Primary	Upper Primary	Secondary
2017	0.0305**	0.0051	0.0182	0.0476**	-0.0361***
	(2.57)	(0.27)	(1.19)	(1.99)	(-3.79)
2018	-0.0050	0.0315***	0.0170**	0.0187	-0.0054
	(-0.76)	(2.68)	(2.09)	(1.51)	(-1.03)
2019	-0.0093*	0.0107	0.0322***	0.0349***	-0.0038
	(-1.72)	(1.27)	(5.34)	(4.30)	(-0.94)
2020	0.0146**	0.0258**	0.0272***	0.0229***	0.0053
	(2.46)	(2.49)	(4.40)	(2.79)	(1.26)
2021	0.0005	0.0124	0.0168***	0.0142*	0.0047
	(0.07)	(1.14)	(2.72)	(1.76)	(1.13)
2022	0.0056	0.0271***	0.0158***	0.0109*	0.0159***
	(0.93)	(2.68)	(2.97)	(1.69)	(4.30)
2023	0.0087	0.0290***	0.0279***	-0.0015	-0.0035
	(1.43)	(2.78)	(4.94)	(-0.23)	(-0.96)
2024	-0.0016	0.0162	0.0159**	0.0078	0.0060*
	(-0.25)	(1.39)	(2.56)	(1.09)	(1.69)

Notes: Values in parentheses are model-based z-statistics. The estimates are year-specific and stage-specific; they are not pooled across years and do not form a panel. The models use the same broad empirical architecture as the 2025 main specification: rank-ordered (exploded) logit, the same five stage groups, choice-set-centered relative attributes, and the same core Female interactions. Because the archived result files do not contain a fully harmonized set of controls and year-stage sample metadata, the tables should be read as evidence on sign, statistical precision, and approximate magnitude rather than as exact cohort-size-weighted trend estimates. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A19. Cross-cohort evidence, 2017–2024: Female × Relative Log Distance

Stage-specific coefficients from rank-ordered (exploded) logit models

Year	Prekinder	Kinder	Early Primary	Upper Primary	Secondary
2017	-0.0050	0.0114	-0.0039	-0.0014	-0.0121
	(-0.47)	(0.74)	(-0.32)	(-0.07)	(-1.34)
2018	-0.0002	0.0091	0.0028	0.0070	-0.0154***
	(-0.04)	(0.97)	(0.43)	(0.74)	(-3.11)
2019	0.0027	-0.0032	-0.0092*	-0.0048	-0.0018
	(0.60)	(-0.47)	(-1.86)	(-0.73)	(-0.45)
2020	-0.0027	0.0119	-0.0160***	0.0083	-0.0001
	(-0.55)	(1.34)	(-2.83)	(1.14)	(-0.03)
2021	-0.0015	0.0000	-0.0014	0.0142*	0.0001
	(-0.28)	(0.00)	(-0.23)	(1.84)	(0.03)
2022	-0.0005	-0.0093	0.0033	-0.0048	0.0006
	(-0.10)	(-1.06)	(0.70)	(-0.90)	(0.16)
2023	-0.0052	-0.0194**	-0.0110**	0.0101*	0.0014
	(-0.96)	(-2.11)	(-2.20)	(1.83)	(0.40)
2024	0.0053	-0.0138	0.0098*	0.0212***	0.0009
	(0.93)	(-1.35)	(1.78)	(3.59)	(0.27)

Notes: Values in parentheses are model-based z-statistics. The estimates are year-specific and stage-specific; they are not pooled across years and do not form a panel. The models use the same broad empirical architecture as the 2025 main specification: rank-ordered (exploded) logit, the same five stage groups, choice-set-centered relative attributes, and the same core Female interactions. Because the archived result files do not contain a fully harmonized set of controls and year-stage sample metadata, the tables should be read as evidence on sign, statistical precision, and approximate magnitude rather than as exact cohort-size-weighted trend estimates. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A20. Cross-cohort evidence, 2017–2024: Female × Relative Quality

Stage-specific coefficients from rank-ordered (exploded) logit models

Year	Prekinder	Kinder	Early Primary	Upper Primary	Secondary
2017	-0.0131	-0.0010	0.0082	0.0159	0.0040
	(-1.18)	(-0.06)	(0.59)	(0.80)	(0.38)
2018	0.0077	0.0050	-0.0047	-0.0186*	-0.0053
	(1.34)	(0.51)	(-0.67)	(-1.85)	(-0.97)
2019	-0.0022	-0.0046	0.0085*	-0.0022	-0.0086**
	(-0.48)	(-0.64)	(1.66)	(-0.34)	(-1.98)
2020	0.0026	-0.0066	0.0124**	-0.0075	0.0070
	(0.48)	(-0.76)	(2.34)	(-1.07)	(1.51)
2021	0.0002	0.0101	0.0131**	-0.0167**	0.0098**
	(0.04)	(1.09)	(2.49)	(-2.45)	(2.15)
2022	-0.0019	-0.0091	0.0162***	-0.0074	0.0016
	(-0.35)	(-1.05)	(3.56)	(-1.41)	(0.42)
2023	-0.0035	0.0030	0.0002	0.0038	0.0125***
	(-0.64)	(0.34)	(0.04)	(0.72)	(3.34)
2024	0.0081	0.0003	0.0066	0.0018	0.0095***
	(1.41)	(0.03)	(1.24)	(0.31)	(2.58)

Notes: Values in parentheses are model-based z-statistics. The estimates are year-specific and stage-specific; they are not pooled across years and do not form a panel. The models use the same broad empirical architecture as the 2025 main specification: rank-ordered (exploded) logit, the same five stage groups, choice-set-centered relative attributes, and the same core Female interactions. Because the archived result files do not contain a fully harmonized set of controls and year-stage sample metadata, the tables should be read as evidence on sign, statistical precision, and approximate magnitude rather than as exact cohort-size-weighted trend estimates. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A21. Cross-cohort evidence, 2017–2024: Female × Rural

Stage-specific coefficients from rank-ordered (exploded) logit models

Year	Prekinder	Kinder	Early Primary	Upper Primary	Secondary
2017	-0.0582	0.0071	0.0658	-0.2896***	0.0695
	(-1.12)	(0.09)	(1.04)	(-2.94)	(1.22)
2018	-0.0497	-0.0311	0.0153	-0.0106	-0.0049
	(-1.40)	(-0.47)	(0.38)	(-0.20)	(-0.14)
2019	-0.0333	0.0316	-0.0629*	-0.0692*	-0.0141
	(-0.99)	(0.55)	(-1.86)	(-1.67)	(-0.47)
2020	0.0115	0.0651	-0.0324	-0.0725*	-0.0851***
	(0.31)	(0.97)	(-0.94)	(-1.67)	(-2.59)
2021	-0.0993**	-0.0889	-0.0011	-0.0482	-0.0582*
	(-2.43)	(-1.23)	(-0.03)	(-1.14)	(-1.78)
2022	-0.0527	-0.0227	-0.0050	-0.0573*	-0.0865***
	(-1.39)	(-0.35)	(-0.17)	(-1.68)	(-3.11)
2023	-0.0688*	0.0067	0.0188	-0.0086	-0.0465*
	(-1.81)	(0.10)	(0.63)	(-0.25)	(-1.73)
2024	-0.0129	-0.0955	-0.0639**	-0.0859**	-0.0786***
	(-0.33)	(-1.30)	(-1.97)	(-2.37)	(-2.93)

Notes: Values in parentheses are model-based z-statistics. The estimates are year-specific and stage-specific; they are not pooled across years and do not form a panel. The models use the same broad empirical architecture as the 2025 main specification: rank-ordered (exploded) logit, the same five stage groups, choice-set-centered relative attributes, and the same core Female interactions. Because the archived result files do not contain a fully harmonized set of controls and year-stage sample metadata, the tables should be read as evidence on sign, statistical precision, and approximate magnitude rather than as exact cohort-size-weighted trend estimates. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A22. Cross-cohort evidence, 2017–2024: Female × Single-Sex School

Stage-specific coefficients from rank-ordered (exploded) logit models

Year	Prekinder	Kinder	Early Primary	Upper Primary	Secondary
2017	-0.0345	-0.0031	-0.0372	-0.4580***	0.1012**
	(-0.37)	(-0.03)	(-0.51)	(-5.54)	(2.31)
2018	-0.1646***	0.1059*	-0.0390	-0.5350***	-0.0126
	(-3.83)	(1.75)	(-0.90)	(-10.07)	(-0.49)
2019	-0.1204***	-0.1074**	0.0514*	-0.2115***	0.0005
	(-3.57)	(-2.44)	(1.68)	(-7.21)	(0.03)
2020	-0.1347***	-0.0268	-0.0883***	-0.3008***	0.0128
	(-3.56)	(-0.49)	(-2.74)	(-9.32)	(0.65)
2021	-0.1089***	0.0691	-0.1148***	-0.2783***	0.0318
	(-2.72)	(1.20)	(-3.52)	(-8.98)	(1.61)
2022	-0.0465	0.0458	0.0116	-0.3530***	-0.0344*
	(-1.15)	(0.78)	(0.38)	(-13.14)	(-1.91)
2023	-0.0510	0.0520	0.0238	-0.3819***	-0.0416**
	(-1.21)	(0.79)	(0.71)	(-13.10)	(-2.25)
2024	-0.0490	0.0703	-0.0026	-0.3316***	-0.0302
	(-1.05)	(0.93)	(-0.07)	(-10.42)	(-1.62)

Notes: Values in parentheses are model-based z-statistics. The estimates are year-specific and stage-specific; they are not pooled across years and do not form a panel. The models use the same broad empirical architecture as the 2025 main specification: rank-ordered (exploded) logit, the same five stage groups, choice-set-centered relative attributes, and the same core Female interactions. Because the archived result files do not contain a fully harmonized set of controls and year-stage sample metadata, the tables should be read as evidence on sign, statistical precision, and approximate magnitude rather than as exact cohort-size-weighted trend estimates. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A23. DFL reweighting decomposition

Reweighting estimates and weight diagnostics

Stage	DFL explained	Residual after reweighting	Weight diagnostics	Interpretation
All stages	0.0017	0.0047	Max wt 2.58; ESS 87%	Pooled across stages
Prekinder	-0.0021	0.0046	Max wt 1.29; ESS 95%	Tiny raw gap; not interpreted
Kinder	-0.0007	0.0049	Max wt 1.65; ESS 94%	Tiny raw gap; not interpreted
Early Primary	-0.0045	0.0066	Max wt 2.08; ESS 92%	Tiny raw gap; not interpreted
Upper Primary	-0.0048	0.0073	Max wt 1.99; ESS 87%	Tiny raw gap; not interpreted
Secondary	0.0094	0.0053	Max wt 2.91; ESS 78%	Composition dominates (≈64%)

Notes: The DiNardo–Fortin–Lemieux reweighting exercise reweights male applicants to the female observable distribution, defined by the applicant's observable choice-set composition (the choice-set means of school quality, distance, School SES, and demand), and is reported here as the more conservative, less functional-form-dependent of the two decompositions. "DFL explained" is the part of the raw gap closed by the reweighting; "residual" is what remains. In Secondary the overlap diagnostics are good (maximum weight about 2.9, effective male sample about 78 percent of the raw count), so the reweighted estimate is interpretable: it attributes about 64 percent of the gap to observable choice-set composition, somewhat less than the linear Oaxaca–Blinder figure of about 94 percent in Table 8. In the younger stages the raw gaps are very small and the reweighting overshoots, producing negative explained shares; despite adequate overlap at every stage, these estimates are uninformative and are not interpreted.

Table A24. Influence diagnostic: no flagged observations

Selected coefficients, rank-ordered (exploded) logit

VARIABLES	Prekinder	Kinder	Early Primary	Upper Primary	Secondary
Female × Relative Quality	0.0007	-0.0105	0.0117**	0.0016	0.0175***
	(0.12)	(-0.93)	(2.08)	(0.26)	(4.63)
Female × Relative Log Distance	-0.0109	-0.0234*	0.0080	0.0172**	0.0083**
	(-1.58)	(-1.71)	(1.11)	(2.24)	(2.01)
Female × Relative School SES	-0.0016	0.0039	-0.0069	0.0113*	0.0334***
	(-0.25)	(0.32)	(-1.17)	(1.78)	(8.80)
Female × Relative Log Top3 Demand	0.0087	0.0091	0.0275***	0.0054	-0.0013
	(1.27)	(0.69)	(4.07)	(0.68)	(-0.35)
Female × Rural	0.0035	-0.1498*	-0.0574	-0.0537	-0.0585**
	(0.08)	(-1.70)	(-1.55)	(-1.41)	(-2.02)
Female × Single-Sex School	-0.0294	0.0157	-0.0219	-0.3176***	-0.0439**
	(-0.72)	(0.22)	(-0.61)	(-9.98)	(-2.38)
Ranked rows	243,762	70,520	293,222	298,061	607,045
Applicants	83,667	23,317	89,779	87,068	164,306

Notes: This check applies an influence diagnostic to the ranked observations and re-estimates the main rank-ordered (exploded) logit specification after removing any flagged observations. No observations exceeded the influence threshold, so the estimation sample and estimates coincide with Table 5. Values in parentheses are applicant-clustered z-statistics, as in Table 5. Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A25. Robustness check: excluding single-sex schools

Selected coefficients, rank-ordered (exploded) logit

VARIABLES	Prekinder	Kinder	Early Primary	Upper Primary	Secondary
Female × Relative Quality	0.0065	-0.0039	0.0072	-0.0055	0.0244***
	(1.04)	(-0.33)	(1.21)	(-0.86)	(5.88)
Female × Relative Log Distance	-0.0100	-0.0242**	0.0071	0.0215***	0.0040
	(-1.63)	(-2.08)	(1.18)	(3.30)	(1.04)
Female × Relative School SES	-0.0047	-0.0003	-0.0026	0.0191***	0.0341***
	(-0.76)	(-0.03)	(-0.43)	(2.96)	(8.55)
Female × Relative Log Top3 Demand	0.0009	0.0079	0.0285***	0.0060	0.0046
	(0.13)	(0.60)	(4.25)	(0.76)	(1.16)
Female × Rural	-0.0314	-0.1850**	-0.0347	-0.0416	-0.0844***
	(-0.78)	(-2.20)	(-0.99)	(-1.11)	(-2.86)
Ranked rows	219,116	63,044	259,074	248,313	484,246
Applicants	76,961	21,269	80,984	74,647	136,559

Notes: This check removes all single-sex schools from the applicants' choice sets and re-estimates the main rank-ordered (exploded) logit specification on the reduced sample. Because no single-sex schools remain in the choice sets, the Female × Single-Sex School interaction is not estimable and is omitted. The Female × Relative School SES interaction in Secondary remains positive and highly significant (0.0341, model-based $z = 8.55$), close to the main estimate of 0.0334, confirming that the secondary School SES result does not depend on the presence of single-sex schools in the choice set. Estimates are on the true submitted rankings; values in parentheses are model-based z-statistics from the archived robustness file. Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A26. Geographic-market robustness for the secondary School SES result

Female × Relative School SES in Secondary, under local-market and reachable-set restrictions

Specification	Coefficient	z	Applicants	Ranked rows
Baseline (leaner specification)	0.0383***	10.75	164,306	607,045
Dense 5 km markets (≥200 applicants)	0.0440***	10.90	119,965	460,836
Dense reachable set (ranked ≥3 schools)	0.0356***	9.33	118,690	515,813

Notes: The table re-estimates the secondary Female × Relative School SES interaction on samples that restrict applicants to more comparable local options, to check whether the result is attenuated once local opportunities are held more nearly fixed rather than reflecting gendered differences in which schools are listed. The second row keeps only dense local markets defined on a 5 km coordinate grid with at least 200 applicants; the third keeps applicants who ranked at least three schools (a reachable-set proxy). This specification uses a leaner control set than the main model, so its baseline (0.0383) differs slightly from Table 5. A full within-market fixed-effects stratification was too granular to estimate stably across the 171 dense markets and is therefore not reported. Across specifications the secondary School SES interaction stays positive and highly significant. z-statistics shown are model-based statistics from the archived geographic-market check. Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A27. Application behavior and the "skip the impossible" concern

Crowding and oversubscription of listed schools, by stage and gender

	All applicants			Female − Male	
Stage	% oversub. listed	Crowd rank 1	Crowd all listed	Rank-1 oversub.	Listed-school SES
Prekinder	86.8	2.9	3.0	+0.005	+0.002
Kinder	68.3	7.7	7.2	+0.008	+0.004
Early Primary	73.2	10.9	9.7	+0.007	+0.003
Upper Primary	79.7	17.6	14.5	+0.014	+0.004
Secondary	92.1	25.0	27.2	−0.004	+0.016

Notes: "Crowd" is the ratio of applicants to seats at a school-grade, so values above one indicate oversubscription; a listed school is counted as oversubscribed when this ratio exceeds one. "% oversub. listed" is the share of an applicant's listed schools that are oversubscribed, averaged over applicants. The last two columns report the female-minus-male difference in the share of top-ranked listed schools that are oversubscribed and in the mean School SES of listed schools. The diagnostic speaks to the "skip the impossible" concern: if applicants systematically retreated to safer options, one would expect substantially lower oversubscription among listed schools, especially beyond the first-ranked option. Instead, listed schools are predominantly oversubscribed at every stage, including more than 90% in Secondary, and the average across all listed schools remains heavily oversubscribed. This pattern is consistent with applicants continuing to list competitive schools rather than retreating to clearly safe options. The female-minus-male difference in the share of oversubscribed schools listed is close to zero, while the female-minus-male gap in listed-school SES is largest in Secondary, mirroring the main School SES result. Computed on the 2025 submitted rankings across all five stages.

Table A28. Within-list distance variation, by stage

Do listed schools vary enough in distance to identify the distance coefficient?

Stage	List length	Nearest school	Within-list SD	Within-list range	Within-list var. share
Prekinder	2.9	0.73	0.61	1.08	0.16
Kinder	3.0	0.75	0.68	1.23	0.44
Early Primary	3.3	0.73	0.81	1.56	0.55
Upper Primary	3.4	0.80	1.01	2.00	0.54
Secondary	3.7	1.04	1.11	2.30	0.50
All	3.4	0.85	0.89	1.73	0.51

Notes: The table summarizes how far apart the schools on an applicant's list are, to check whether distance varies enough within lists to identify the choice-set-centered distance coefficient. "List length" is the mean number of schools ranked. "Nearest school", "within-list SD", and "within-list range" (maximum minus minimum) are medians across applicants of the corresponding within-list statistic, measured in kilometres; medians are reported because the distance distribution has a long right tail. "Within-list var. share" is the share of total squared distance variation that lies within applicant lists rather than between applicants (a one-way variance decomposition). Applicants concentrate on nearby schools, with the nearest listed school a median of 0.7–1.0 km away, but their lists still span a meaningful distance range, and the within-list share of distance variation rises from 16% in Prekinder to about 50% in the primary and secondary stages. The distance coefficient is therefore identified from genuine within-applicant variation. Computed on the 2025 submitted rankings.

Table A29. Leave-one-out demand robustness

Female × Relative School SES under raw and leave-one-out Top3 Demand

Specification	Prekinder	Kinder	Early Primary	Upper Primary	Secondary
Main specification, raw Top3 Demand	-0.0016	0.0039	-0.0069	0.0113*	0.0334***
	(-0.25)	(0.32)	(-1.17)	(1.78)	(8.80)
Leave-one-out Top3 Demand	-0.0015	0.0035	-0.0068	0.0117**	0.0334***
	(-0.25)	(0.31)	(-1.24)	(2.01)	(9.41)
Difference: LOO minus main	0.0001	-0.0004	0.0001	0.0004	0.0000

Notes: This table checks whether the main Female × Relative School SES result is affected by the mechanical fact that the raw Top3 Demand measure can include the applicant's own top-three list. The leave-one-out specification subtracts the applicant's own contribution from the school-grade top-three demand count whenever the school appears among that applicant's top three choices, and then applies the same log transformation, winsorization, choice-set centering, and standardization as in the main specification. The model otherwise follows the same rank-ordered (exploded) logit specification used in Table 5. The secondary coefficient is unchanged to four decimal places, and the pattern across stages is substantively the same. This supports treating leave-one-out demand as a robustness check: it removes the direct mechanical self-inclusion concern without changing the central School SES conclusion. To keep the duplicated main row aligned with Table 5, its parentheses report applicant-clustered z-statistics. The leave-one-out row reports the model-based z-statistics from the archived leave-one-out estimation file and is used here as a coefficient-stability check. Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A30. Conservative controls and fixed-effects specification

Female × Relative School SES with additional school controls and fixed effects

Specification	Prekinder	Kinder	Early Primary	Upper Primary	Secondary
Controls + school-type/religion FE	0.0023	0.0057	-0.0095*	0.0011	0.0222***
	(0.37)	(0.50)	(-1.65)	(0.18)	(6.07)

Notes: This table reports a stricter version of the rank-ordered logit used as a supplementary check. It keeps the true submitted rankings and adds Relative Log Vacancies, Relative Log School Size, Any Priority, school-type fixed effects, and religion fixed effects. The Secondary School SES coefficient becomes smaller than in Table 5, but it remains positive and highly significant. Values in parentheses are z-statistics. Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A31. Robust standard-error check for the main School SES coefficient

Female × Relative School SES under alternative variance estimators

Stage	Coef.	Model SE	Model z	Applicant SE	Applicant z	School-grade SE	School-grade z
Prekinder	-0.0016	0.0059	-0.27	0.0064	-0.25	0.0053	-0.30
Kinder	0.0039	0.0114	0.34	0.0123	0.32	0.0106	0.37
Early Primary	-0.0069	0.0055	-1.25	0.0059	-1.17	0.0052	-1.35
Upper Primary	0.0113	0.0058	1.94	0.0063	1.78	0.0060	1.88
Secondary	0.0334	0.0036	9.39	0.0038	8.80	0.0040	8.26

Notes: This table uses the same rank-ordered (exploded) logit specification as Table 5 and reports only the Female × Relative School SES coefficient. The coefficient is unchanged across columns; only the variance estimator changes. "Applicant SE" is a sandwich standard error based on applicant-level score contributions and is the version used for the z-statistics in Table 5. "School-grade SE" sums row-level score contributions by school-grade cluster and is reported as an auxiliary check, since the ranking likelihood is naturally organized by applicant list. The Secondary estimate remains clearly positive under both robust variance calculations.

Table A32. Top-rank robustness for the main School SES coefficient

Female × Relative School SES when the exploded likelihood is restricted to early ranking steps

Specification	Prekinder	Kinder	Early Primary	Upper Primary	Secondary
Rank 1 step only	0.0035	0.0120	-0.0061	0.0008	0.0453***
	(0.41)	(0.71)	(-0.73)	(0.09)	(7.62)
First two ranking steps	-0.0001	0.0093	0.0003	0.0049	0.0374***
	(-0.01)	(0.67)	(0.04)	(0.66)	(8.05)
First three ranking steps	0.0015	0.0101	-0.0031	0.0081	0.0313***
	(0.22)	(0.78)	(-0.50)	(1.21)	(7.51)
Full exploded ranking	-0.0016	0.0039	-0.0069	0.0113*	0.0334***
	(-0.25)	(0.32)	(-1.17)	(1.78)	(8.80)

Notes: This table uses the same specification as Table 5 and changes only how much of the submitted ranking is used in the exploded likelihood. "Rank 1 step only" uses only the first-choice step, where the first-ranked school is chosen from all ranked schools. "First two ranking steps" and "First three ranking steps" keep only the first two or first three exploded choices. "Full exploded ranking" is the main specification. Values in parentheses are applicant-clustered z-statistics from applicant score contributions. The Secondary School SES coefficient is positive in all early-rank versions, so the main result is not generated only by lower-ranked comparisons in longer lists.

Table A33. School SES dispersion and split-sample estimates

Within-list School SES dispersion and Female × Relative School SES by stage

Stage	Mean SD	Median SD	Low coef.	Low z	High coef.	High z
Prekinder	0.0667	0.0593	0.0092	0.45	-0.0001	-0.01
Kinder	0.0706	0.0641	-0.0739**	-2.03	0.0226*	1.67
Early Primary	0.0745	0.0684	-0.0083	-0.50	-0.0038	-0.58
Upper Primary	0.0793	0.0749	0.0380**	2.22	0.0096	1.38
Secondary	0.0795	0.0766	0.0563***	5.43	0.0322***	7.72

Notes: Mean SD and Median SD describe the applicant-level standard deviation of School SES within submitted lists. Low and high groups split applicants at the median within-list School SES standard deviation within each stage. The reported coefficient is Female × Relative School SES from the same rank-ordered logit specification used in Table 5, estimated separately within each split. As in the main model, the continuous variables are choice-set centered (relative to each applicant's submitted list) and standardized within stage before the split model is estimated. Values in the z columns use applicant-clustered sandwich standard errors. The Secondary coefficient remains positive in both low- and high-dispersion halves. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A34. Local admission-market robustness by applicant region

Female × Relative School SES in Secondary rank-ordered logit models

Applicant region	Applicants	Ranked rows	Coef.	Applicant SE	z	p-value
TPCA	4,722	14,696	0.0777**	0.0333	2.33	0.020
ANTOF	7,915	25,098	0.0756***	0.0220	3.44	0.001
ATCMA	4,010	12,859	0.0262	0.0249	1.05	0.294
COQ	8,150	26,812	0.0338*	0.0176	1.92	0.054
VALPO	14,913	51,719	0.0588***	0.0128	4.58	0.000
LGBO	9,897	31,989	0.0324*	0.0182	1.78	0.075
MAULE	10,897	37,995	0.0150	0.0145	1.04	0.300
BBIO	14,470	50,201	0.0384***	0.0112	3.44	0.001
ARAUC	10,874	35,062	0.0049	0.0153	0.32	0.751
LAGOS	9,092	28,630	0.0464***	0.0174	2.66	0.008
AYSEN	642	1,956	-0.0412	0.0677	-0.61	0.543
MAG	1,346	4,279	-0.0052	0.0533	-0.10	0.923
RM	57,306	215,803	0.0269***	0.0064	4.20	0.000
RIOS	3,613	11,588	-0.0050	0.0272	-0.18	0.856
AYP	2,764	8,148	-0.0428	0.0319	-1.34	0.179
NUBLE	3,693	12,211	0.0349	0.0241	1.45	0.148
National Secondary baseline	164,304	568,845	0.0331***	0.0038	8.69	0.000

Notes: This table reports the coefficient on Female × Relative School SES from Secondary-stage rank-ordered logit models estimated separately by applicants' inferred region of residence, using `student_region_code`. Markets are defined by applicant region rather than by the region of the ranked school, so each applicant's full submitted ranking list is preserved. Continuous school attributes use the same Secondary-stage transformations as the baseline model: 1%/99% winsorization, applicant choice-set centering, and stage-level standardization. Standard errors are applicant-clustered sandwich standard errors. The national baseline uses the same transformed Secondary sample with valid applicant-region information, which explains the small difference from Table 5. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Distribution statistic across 16 applicant regions	Value
Unweighted mean coefficient	0.0236
Median coefficient	0.0297
Applicant-weighted mean coefficient	0.0319
Standard deviation of coefficients	0.0380
Interquartile range	-0.0003 to 0.0485
Positive region estimates	12 / 16
Regions significant at 10%	8 / 16
Regions significant at 5%	6 / 16
Regions significant at 1%	5 / 16

Notes: The distribution summarizes the 16 applicant-region estimates in the table above. The applicant-weighted mean uses the number of Secondary applicants in each region as weights. Because regional samples are smaller than the national sample, the distribution of estimates is more informative here than the significance of each individual region coefficient.

Table A35. Robustness to sibling priority (Secondary)

Selected coefficients, rank-ordered (exploded) logit; independent re-estimation

	(1) Baseline (all Secondary)	(2) Drop sibling priority applicants	(3) Add Female × Sibling
Female × Relative School SES	0.0351***	0.0384***	0.0363***
	(9.28)	(9.62)	(9.52)
Female × Sibling priority	–	–	-0.0054
			(-0.18)
Applicants	164,306	141,957	164,306
Ranked rows	607,045	535,042	607,045

Notes: The table checks whether the Secondary Female × Relative School SES interaction is confounded by sibling priority, a channel that could link applicant gender to school choice through family size. Column (1) re-estimates the baseline Secondary rank-ordered (exploded) logit; column (2) drops every applicant holding sibling priority at a listed school; column (3) adds Female × Sibling priority and a sibling-priority main effect (main effect not shown) to the full sample. Sibling priority is held by 13.2% of girls' and 14.0% of boys' Secondary applications. Estimates are from an independent re-estimation of the model; the column (1) baseline (0.0351) is consistent with the main Table 5 estimate (0.0334). z-statistics in parentheses use applicant-clustered standard errors. Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A36. Robustness to applicant vulnerability stratum (Secondary)

Female × Relative School SES by priority status; rank-ordered (exploded) logit, independent re-estimation

	(1) All Secondary	(2) Priority (vulnerable)	(3) Non-priority applicants
Female × Relative School SES	0.0351***	0.0419***	0.0292***
	(9.28)	(8.60)	(4.83)
Applicants	164,306	108,622	55,684

Notes: The Secondary rank-ordered (exploded) logit is re-estimated separately for applicants with and without socioeconomically vulnerable (priority) status, the classification that underlies the School SES measure. Female × Relative School SES is positive and highly significant in both strata; the two bracket the full-sample estimate, and a share-weighted combination ($0.665 \times 0.0419 + 0.335 \times 0.0292 \approx 0.037$) reproduces it, so the pooled coefficient is not a composition-weighted average that masks a null in either group. Priority status is balanced by gender, held by 66.5% of girls' and 65.7% of boys' Secondary applications. Estimates are an independent re-estimation; the column (1) baseline is consistent with the main Table 5 estimate (0.0334). z-statistics in parentheses use applicant-clustered standard errors. Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A37. Scale (decisiveness) diagnostic (Secondary)

Gender-specific coefficients and scale-free ratios; rank-ordered (exploded) logit, independent re-estimation

	(1) Male	(2) Female	(3) Female/Male
Relative Quality	0.0129***	0.0306***	2.36
	(4.82)	(11.38)	
Relative Log Distance	-0.1570***	-0.1487***	0.95
	(-53.18)	(-51.01)	
Relative School SES	0.0415***	0.0750***	1.81
	(15.04)	(28.63)	
Relative Log Top3 Demand	0.2561***	0.2547***	0.99
	(92.65)	(94.03)	
Applicants	81,784	82,522	

Scale-free coefficient ratios (invariant to the logit error scale)

	Male	Female
School SES / |Distance|	0.265	0.505
School SES / Demand	0.162	0.295
Quality / |Distance|	0.082	0.206

Notes: Rank-ordered logit coefficients are identified only up to the scale of the logit error term, so a uniformly larger set of female coefficients could reflect girls ranking more decisively (a smaller error scale) rather than different preferences. This table separates the two channels. The Secondary model is estimated separately by gender (four main effects shown; rural and single-sex controls included but omitted from the panel). If the gender gap were pure scale, the female-to-male ratio in column (3) would be a single constant across all rows. Instead it ranges from 0.95 (distance) and 0.99 (demand) to 1.81 (School SES) and 2.36 (quality): the amenities that plausibly proxy the peer environment move far more than the near-mechanical distance and congestion terms. The lower panel reports scale-free ratios, each a quotient of two coefficients from the same model, so the common error scale cancels exactly; girls weight School SES relative to distance almost twice as heavily as boys (0.505 versus 0.265). The gender-specific School SES gap ($0.0750 - 0.0415 = 0.0335$) reproduces the main Female $\times$ Relative School SES interaction (0.0334, Table 5). Estimates are an independent re-estimation. z-statistics in parentheses use applicant-clustered standard errors. Significance: *** p$<$0.01, ** p$<$0.05, * p$<$0.10.

Table A38. Counterfactual decomposition of the Secondary first-choice School SES gap

Set composition versus within-list ranking; gender-specific rank-ordered (exploded) logit

	Value	Share of gap
First-choice School SES, girls	0.421	
First-choice School SES, boys	0.400	
Female–male gap (model)	0.0214	100%
Set composition (which schools listed)	0.0178	83%
Within-list ranking (all attributes)	0.0036	17%
of which School SES weighting	0.0028	13%

Implied contribution to the admitted-school gap (via Table 7 attenuation)

	Contribution	Share of 0.0146
Within-list ranking channel	0.0030	21%
School SES weighting alone	0.0024	16%

Notes: The Secondary rank-ordered (exploded) logit is estimated separately by gender. For female applicants, the expected School SES of the first-ranked school is $\sum_j p_{ij}$ SchoolSES_{ij}, where p_{ij} is the model probability that school j is ranked first within applicant i's submitted set. The counterfactual holds each girl's choice set fixed and replaces her attribute weights with the male estimates; the resulting fall in expected first-choice School SES (0.0036) is the within-list ranking channel, and the remainder of the female–male gap (0.0178) is attributed to the higher-SES composition of girls' lists. Swapping only the School SES weight (0.0750 to 0.0415) isolates the School SES tilt (0.0028). The model reproduces the observed first-choice gap (0.0214 predicted versus 0.0212 observed). The admitted-school column applies the Secondary top-three-to-assigned attenuation from Table 7 ($0.0149/0.0179 = 0.832$) and is illustrative only, since the assignment mechanism itself is not reconstructed. The split is consistent with the Oaxaca–Blinder and DiNardo–Fortin–Lemieux decompositions (Tables 8 and A23), which attribute most of the admitted gap to observable list composition. The exercise is descriptive and inherits the linkage caveats in Section 5.6.