Immigrant Exposure and Native Students' Educational Outcomes[†]

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Abstract

International migration is among the most contentious issues in receiving countries, where immigrant inflows into schools are often perceived as disruptive. We provide causal evidence to the contrary, exploiting Chile's rapid 2015–2020 immigration shock and administrative records for the universe of K–12 students, linked to parent, teacher, and student surveys. Instrumenting younger siblings' immigrant exposure with the share they would have faced at their eldest sibling's school, we find that a one-standard-deviation increase in immigrant share—3.1 percentage points—raises fourth-grade test scores by 0.03–0.04 standard deviations. These gains persist into eighth and tenth grade and translate into higher rates of on-time graduation and higher-education enrollment. The improvements appear to operate through peer effects, as immigrants are positively selected relative to native classmates in cognitive and non-cognitive skills, and native students benefit particularly from exposure to immigrants who score above the school-cohort median on an index of motivation and grit, our proxy for peer quality. We find no evidence that immigrant exposure crowds out school resources or induces compensatory responses by schools, as teacher–student ratios and teacher quality remain unchanged in classrooms with more immigrants.

Keywords: Immigration, education, peer effects.
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1 Introduction

International migration has become one of the most contentious policy issues in receiving countries, with immigrant inflows often perceived as disruptive across multiple domains, including education (Ajzenman et al., 2023; Alesina et al., 2023, 2024; Betts and Fairlie, 2003; Cascio and Lewis, 2012). A growing body of evidence shows that, at least in the short run, greater exposure to immigrant classmates improves native students' test scores (Figlio and Özek, 2019; Figlio et al., 2024; Ohinata and Van Ours, 2013). Yet we still know little about whether these gains persist over time or about the mechanisms behind them. Answering these questions is central to the design of immigration and education policy.

This paper provides causal evidence that exposure to immigrant classmates improves native students' educational outcomes in both the short and long run. It also shows that peer effects seem to be an important driver of these impacts and that the effects vary with immigrant characteristics. We study this in Chile, a setting particularly well suited to answering these questions because the country experienced a rapid and largely unanticipated immigration wave between 2015 and 2020, generating substantial variation in exposure across schools and cohorts. In addition, administrative records covering the universe of K–12 students allow us to follow students throughout their school trajectory and into higher education. The institutional data environment is also unusually rich. Chile's quality-assurance system administers annual tests in fourth grade and periodic tests in later grades, and these exams are paired with surveys of students, parents, and teachers. Together, these features allow us to trace outcomes over time, characterize peer composition beyond academic achievement, and study school adjustments that go beyond observable resource inputs.

Estimating these effects poses an important identification challenge. Exposure to immigrants is not randomly assigned. Immigrant students may sort systematically into particular schools, and schools and students exposed to larger immigrant inflows may differ along dimensions that independently affect achievement. Much of the existing literature has addressed this challenge by exploiting within-school variation, and more recently within-family variation (see for instance Figlio and Özek, 2019; Figlio et al., 2024).

Although family fixed effects account for many confounders not captured by school

fixed effects, estimates may still be biased if families select schools differentially for their children. Building on Figlio et al. (2024), we explicitly address both identification challenges within a unified empirical framework. Our preferred specification relies on mother fixed effects and an instrumental variable approach that instruments each child's immigrant exposure with the exposure they would have faced had they attended their eldest sibling's school. This design exploits within-family variation while accounting for differential school choices across siblings.

Naive comparisons yield negative correlations between immigrant exposure and native achievement. However, once we account for both immigrant sorting and differential school choices within family, we find that exposure to immigrant peers has positive and statistically significant effects on native students' outcomes. Our preferred estimates indicate that a one-standard-deviation increase in immigrant exposure—approximately three percentage points, or roughly one additional immigrant student per classroom—raises fourth-grade test scores by between 0.03 and 0.04 standard deviations in both language and mathematics. These gains persist into later educational stages, increasing on-time high school completion by 2.6 percentage points and post-secondary enrollment by 1.1 percentage points. We also show that the effects are nonlinear, driven primarily by schools with relatively large immigrant shares rather than by those experiencing only marginal increases in immigrant exposure.

Conceptually, immigrant inflows can affect native students through two broad channels. One channel is peer effects. Immigrant classmates may shape study norms, aspirations, and classroom behavior, with effects that can be positive or negative depending on how immigrants compare to their native peers. The other channel operates through resources, as changes in school composition may shift how teachers, funding, and instructional resources are allocated across students.

To investigate these mechanisms, we conduct a set of exercises that build on our main empirical approach.

We first examine the peer channel by descriptively showing that although immigrants have lower average test scores than natives in the full population, they sort into schools where they compare favorably to local peers and are positively selected along non-cognitive dimensions such as academic motivation, grit, and self-efficacy. These sorting patterns are not unique to Chile; indeed, Dustmann et al. (2024); Card (2001); Saiz (2007); Glitz

(2014) describe similar immigrant selection and sorting into neighborhoods in Germany, the United Kingdom, and the United States.

To understand whether this sorting pattern helps explain our findings, we proxy for immigrant peer quality using an index of academic motivation and grit built from student survey responses collected alongside the national exams, and classify immigrants by whether they rank above or below the native school-cohort median on this index. Instrumenting exposure to high- and low-index immigrants using the same identification logic as in our baseline analyses, we find that native outcomes improve with exposure to above-median immigrants and worsen with exposure to below-median immigrants, both in the short and in the mid-term. We interpret this pattern as evidence that peer quality matters, rather than as evidence that motivation and grit themselves are the specific trait driving native outcomes. Because immigrants score on average above their cohort median on this index, the net effect is positive.

We next examine whether immigrant inflows affected school resources, and in which direction. Because the per-student voucher system funds schools equally regardless of whether students are native or immigrant, immigration does not mechanically reduce per-student resources. At the same time, schools could in principle respond to immigrant arrivals by directing additional resources or their most effective teachers toward more exposed classrooms, which would also help explain positive effects on native students. We find no evidence of either pattern. Class sizes, teacher–student ratios, and teacher characteristics—test scores, experience, and certification—remain broadly unchanged as immigrant exposure rises, indicating that the gains for native students reflect changes in peer composition rather than changes in the level or targeting of school resources.

Our findings contribute to two main literatures. First, we contribute to the literature on immigration and education by showing that positive effects in short-term outcomes—already documented in a variety of countries and institutional contexts (Ohinata and Van Ours, 2013; Geay et al., 2013; Schneeweis, 2015; Jensen and Rasmussen, 2011; Ballatore et al., 2018; Figlio and Özek, 2019; Figlio et al., 2024; Assaad et al., 2023)—persist over time, with effects evident in mid-term and long-run outcomes including on-time high school completion and post-secondary enrollment. Most immigrants to Chile arrive from other Spanish-speaking countries, which might make it easier for them to integrate into native classrooms. Yet several of the settings cited above feature substantial language dif-

ferences between immigrants and natives and still document comparable gains for native students, indicating that linguistic proximity is not a precondition for these gains to arise.

We also add to this literature by showing, as in Figlio et al. (2024), that failing to account for differential school choice within families can bias the estimates. Contreras and Gallardo (2022)—a paper that also studies the Chilean setting—is one of the few that finds negative effects of immigrant exposure on native outcomes. We show that when their empirical approach is adjusted to account for potentially endogenous school switches the estimated effects turn positive (see Online Appendix D for further details).We further extend this literature by providing evidence on mechanisms. We show that the Chilean school system absorbed the immigration inflow without reducing per-student resources—similar to what Assaad et al. (2023) argues is key to understanding the positive effects they document—and immigrants sorted into schools where their characteristics and academic motivation made them, on average, better peers relative to the native population. These elements jointly explain our positive effects and likely account for our divergence with Gould et al. (2009), one of the few papers that have looked at long-term effects of immigration (i.e., performance in a high-stakes exam taken at the end of high school).[1] Relative to the broader peer effects literature—see Barrios-Fernández (2023) and Sacerdote (2014) for reviews—our magnitudes are closer to the upper bound and comparable to estimates in closely related settings such as Figlio et al. (2024). They are also comparable to magnitudes associated with removing disruptive peers from the classroom, even outside immigration contexts (Carrell et al., 2018).

Second, we contribute to a growing literature showing that perceptions of immigration do not always align with its realized effects across different domains (Ajzenman et al., 2022, 2023; Alesina et al., 2023). Education is not an exception. In school settings, immigrants are often associated with negative stereotypes (Alesina et al., 2024), and changes in immigrant shares can trigger responses by native families, including native flight (Betts and Fairlie, 2003; Cascio and Lewis, 2012; Figlio and Özek, 2019; Figlio et al., 2024). Our results add to this literature by showing that the average causal effects of immigrant exposure on native students are positive once selection is properly addressed. The contrast between specifications that do and do not account for selection might help explain why public perceptions do not match the underlying impacts.

[1]Note that the empirical strategy in Gould et al. (2009) does not directly account for immigrant sorting or endogenous native responses. Thus, differences in findings might also stem from this.

The remainder of the paper is organized as follows. Section 2 describes the institutional context of the Chilean school system and recent immigration patterns. Section 3.1 presents the data and summary statistics. Section 3.2 outlines the empirical strategy. Section 4 presents the main results. Section 5 explores mechanisms. Section 6 concludes.

2 Institutional Background

This section provides background on recent immigration trends in Chile and describes the K–12 school system, focusing on the demographic and institutional context that shapes native students' exposure to immigrant peers.

2.1 Immigration to Chile

Chile's immigrant population grew from a negligible share of the population to levels comparable to moderately diverse U.S. states such as North Carolina, though still well below high-immigration states such as California, Florida, and Texas (U.S. Census Bureau, 2025a,b,c,d). Within the K–12 system, the share of foreign-born students rose from about 1% in 2004 to roughly 8% by 2022. There was a steady increase from 2008 onward, but the largest change took place between 2015 and 2020, when immigrant shares roughly tripled. This rise sharply increased native students' exposure to immigrant peers, as the share of native students with at least one immigrant classmate rose from approximately 10% to 65% between 2004 and 2022.

Immigrant settlement was spatially concentrated, with particularly large inflows into northern regions and the Metropolitan Region. Both the timing and magnitude of inflows appear to be linked primarily to economic and political shocks in origin countries, rather than to contemporaneous changes in Chilean institutions or in the school system. The composition of immigration also evolved over time. Inflows were initially dominated by migrants from neighboring countries, most notably Peru, before shifting markedly toward Venezuelan migrants as the democratic crisis in Venezuela intensified.

Immigrant students and their families appear positively selected along educational dimensions, including higher parental schooling and educational expectations (Section 5.1 documents these patterns in detail). Despite this positive selection, immigrant students are more likely to report discrimination within schools, particularly on dimensions directly

linked to migrant status. Online Appendix A provides additional details on immigration trends, including regional patterns, changes in origin-country composition, and evidence on public attitudes toward immigrants.

2.2 K–12 School System in Chile

The Chilean school system is organized into two cycles, primary education (grades 1–8) and secondary education (grades 9–12). Education is provided by three types of schools. Public and private subsidized schools together serve 93% of students and are funded through a voucher system that provides both regular and targeted subsidies based on student socioeconomic status and school location. Private schools, which serve the remaining 7% of students, are funded exclusively through tuition fees. An important feature of the Chilean system is that both native and immigrant students are eligible for the voucher, ensuring broad access to subsidized schools regardless of citizenship status.

The Chilean system gives both native and immigrant students substantial freedom to choose schools. Since 2006, the General Education Law has limited schools' ability to select students. It requires admissions to be transparent and non-discriminatory, and it prohibits using academic, socioeconomic, or family background information to restrict access.[2] In 2016, the School Inclusion Law (*Ley de Inclusión*) introduced centralized admissions based on a deferred acceptance algorithm. Under this system, priorities are limited to siblings, disadvantaged students (up to 15% of seats), children of school workers, and former students. There is no priority for native students over immigrants. Recent immigrants may still face tighter constraints, however, because they are less likely to have siblings already enrolled or parents working at schools, and those who arrive after first grade must find schools with available seats. Still, administrative admissions data for 2024 show that only 11% of schools were oversubscribed, suggesting substantial scope for school choice. This high-choice environment makes it easier for families to differentially select schools for their children, something we take into account in our empirical strategy (see Section 3.2 for details).

These admissions rules operated in a system already undergoing major demographic change. Sustained fertility decline had left schools with substantial excess capacity well

[2] A few high schools were allowed to continue selecting students based on prior academic performance. In addition, with decentralized admissions, enforcement was difficult and there is evidence that some schools continued selecting students using criteria that were formally prohibited (Carrasco et al., 2017).

before immigrant enrollment began to grow. Figure 1 shows that native fourth-grade enrollment declined steadily throughout the 2000s and early 2010s, reflecting a sharp and persistent fall in fertility rates. Chile's current fertility rate, around 0.99 children per woman, is well below replacement and substantially lower than that of the United States. This decline contracted the size of native cohorts entering elementary school and generated excess capacity across the system. The contraction was particularly pronounced in public schools, as many families shifted toward voucher schools, which experienced a slight increase in enrollment (see Online Appendix Figure A.II). This movement from public schools to other subsidized schools is not unique to Chile. Campos et al. (2025) document a similar pattern in the United States. As immigrant enrollment expanded in the late 2010s, inflows entered a system with considerable spare capacity, especially in the public sector.

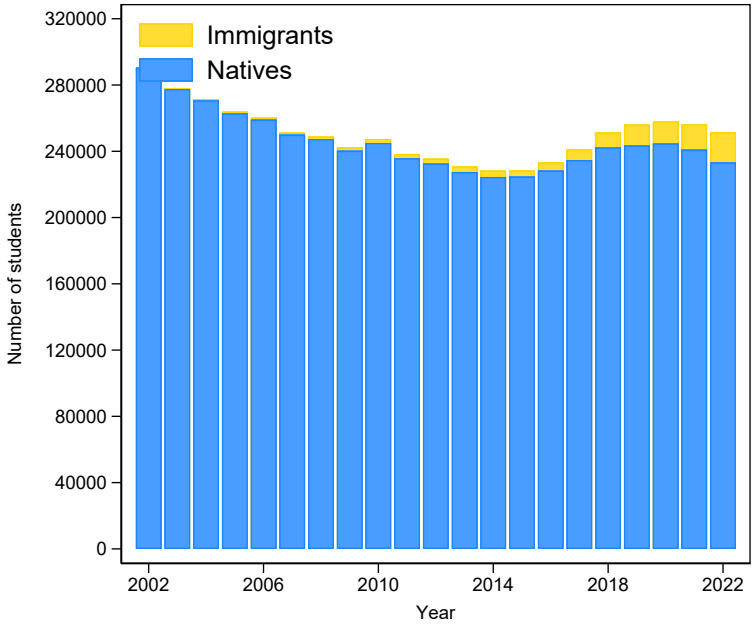


Figure 1: Enrollment evolution

Notes: The figure illustrates the evolution of total enrollment among fourth-grade native students (blue) and fourth-grade immigrant students (yellow) from 2002 to 2022. School enrollment data are from Ministry of Education administrative records.

3 Data and Identification Strategy

3.1 Data and Sample Construction

We combine rich student-level administrative records from multiple Chilean agencies using a common student identifier.

Students Administrative Records: Longitudinal enrollment data from the Ministry of Education cover the universe of students enrolled in Chile's K–12 system, including public, voucher, and private schools, and include detailed information on students and schools. These records are linked to standardized test data from the Education Quality Agency, which administers nationwide assessments annually in grade four and on a regular basis in other grades. The linked data also include enrollment in higher education from Ministry of Education records and student performance in university admission exams from the Department of Educational Measurement (DEMRE). Together, these sources allow us to track students across educational stages, from primary school through the transition to higher education.

Students, Parents and Teachers Survey Data: All administrative records described above are combined with survey data from questionnaires administered by the Education Quality Agency to students, parents, and teachers alongside standardized assessments. These data are used to characterize immigrant students, compare them with native peers, and study changes in classroom composition and the learning environment.

Immigrant Exposure Measure: Using these data, we construct immigrant exposure at the school-cohort level and define outcomes at three horizons. Short-run outcomes are grade-four SIMCE test scores. Medium-run outcomes are grades eight and ten SIMCE test scores. Long-run outcomes are high-school completion and post-secondary enrollment. Because data availability differs across stages of the schooling trajectory, the estimation sample also differs by outcome. For short- and medium-run outcomes, the sample includes students reaching grade four between 2008 and 2018. For high-school completion and higher-education enrollment, the sample is restricted to students reaching grade four between 2008 and 2015. In the empirical analysis, immigrant exposure is defined at the grade-four school-cohort level, and many outcome and mechanism exercises are anchored in variation at that grade because it is the most frequently tested grade.

Family-link Administrative Records: These data are complemented with family-link administrative records used in Altmejd et al. (2021); Barrios-Fernández (2022); Barrios Fernández et al. (2024) to identify siblings. These family links are essential for implementing the empirical strategy, as they allow us to control for family fixed effects and to

construct the instrumental variable discussed in Section 3.2.

Table 1 characterizes native and immigrant students in fourth grade. The table shows that immigrant (foreign-born) students are substantially more likely than natives to enroll in public schools (58% versus 40%), a pattern consistent with Figure 1, while at the same time they are more likely to come from households with higher maternal education (28% versus 24%). Despite this more favorable observed parental background, immigrants display weaker average academic outcomes in the raw data. However, in Section 5.1, we show that these average differences reverse when immigrants are compared with the native peers they actually face within the same school-cohort, a pattern consistent with sorting into more disadvantaged schools (Online Appendix Figure C.I illustrates this sorting).

3.2 Empirical Approach

We aim to examine the effect of exposure to immigrant peers on native students' educational outcomes. A central identification challenge is that exposure to immigrants is not randomly assigned. Immigrant students sort non-randomly across schools, and therefore schools with higher immigrant shares likely differ systematically along dimensions that also affect student achievement (Online Appendix Figure C.I documents these patterns).

Controlling for family fixed effects addresses many potential confounders by absorbing all time-invariant household factors correlated with school choices and outcomes. However, family fixed effects may still be insufficient if parents differentially select schools for their children. Few papers account for this additional layer of selection. A notable exception is Figlio et al. (2024), who study immigration in Florida and adopt an instrumental variable approach similar to the one we describe below.

To address these identification challenges, we exploit within-family variation in immigrant exposure across siblings and estimate the following specification:

$$Y_{ist} = \beta_0 + \beta_1 I_{ist} + \mu_{f(i)} + \phi_t + \varepsilon_{ist}, \tag{1}$$

where $\mu_{f(i)}$ denotes family fixed effects and ϕ_t cohort fixed effects.

Actual exposure to immigrant peers, I_{ist}, is instrumented using the immigrant share that student i would have faced in year t had she attended the school where her eldest

Table 1: Summary Statistics

	All students (1)	Immigrant students (2)	Native students (3)
A. Demographic characteristics			
Female = 1	0.488	0.450	0.489
Age in fourth grade	9.246	9.363	9.244
B. Socioeconomic background			
Public school = 1	0.411	0.584	0.407
Voucher school = 1	0.508	0.339	0.511
Private school = 1	0.081	0.077	0.081
School SES = Low	0.405	0.370	0.406
School SES = Mid	0.344	0.410	0.343
School SES = High	0.250	0.219	0.250
Mother education: less than high school	0.294	0.273	0.294
Mother education: completed high school	0.458	0.412	0.459
Mother education: higher education	0.248	0.315	0.247
Books at home: less than 10	0.371	0.432	0.371
Books at home: 10–50	0.429	0.364	0.430
Books at home: 50–100	0.118	0.106	0.118
Books at home: more than 100	0.081	0.097	0.081
C. Academic performance			
Fourth-grade GPA [1–7]	5.878	5.676	5.882
Fourth-grade attendance [%]	92.429	87.159	92.538
Spanish score (σ_n)	0.016	-0.046	0.017
Math score (σ_n)	0.017	-0.081	0.018
Grade retention = 1	0.026	0.069	0.025

Notes: This table presents summary statistics for fourth-grade students. Column (1) reports averages for the full sample. Column (2) restricts to immigrant students, defined as those not born in Chile. Column (3) reports averages for native students. GPA is measured on a 1–7 scale, and attendance is measured as a percentage. Spanish and math scores are standardized using the native-student distribution. School SES refers to the socioeconomic classification assigned by the Education Quality Agency.

sibling began schooling. Thus, the corresponding first-stage equation is:

$$I_{ist} = \pi_0 + \pi_1 Z_{s'(i)t} + \mu_{f(i)} + \phi_t + \upsilon_{ist}, \tag{2}$$

where $s'(i)$ denotes the school in which the eldest sibling of student i was enrolled in grade one.

Standard errors are clustered at the eldest sibling's school, which is the level at which the instrument is defined. In tables and figures, we report effects in standard-deviation units for test-score outcomes and in percentage points for binary educational outcomes.

Since we use the eldest sibling in each family to build the instrument and we include family fixed effects, this design can only be estimated on families with three or more children. The identifying variation arises from differences in the timing of school entry across siblings combined with the rapid expansion of immigration to Chile. Intuitively, siblings in the same family are likely to attend the same school and thus younger siblings may face different immigrant shares simply because immigration increased between the time at which different children enter school.

If all siblings attended the same school, this approach would be equivalent to a school fixed-effects design, as in Hoxby (2000). In practice, however, a substantial fraction of families send their children to different schools, accounting for approximately 35% of the sample. Instrumenting actual exposure with the immigrant share the younger sibling would encounter at their eldest sibling's school allows us to isolate variation in exposure that is independent of endogenous school choice.

Under the assumptions discussed below, our IV estimates have a local average treatment effect interpretation. They capture the effect of higher immigrant exposure for students whose realized exposure is shifted by the predicted exposure at the eldest sibling's school. We therefore interpret the preferred estimates as LATE-type effects for compliers in our design.

Immigrant shares are highly persistent across grades within a school. As shown in Online Appendix Figure C.II, exposure in fourth grade is strongly correlated with exposure in earlier and later grades. This implies that immigrant exposure at a given grade is a good proxy for exposure throughout the student's school trajectory. Accordingly, the estimated coefficients should be interpreted as reflecting the effect of sustained exposure

to immigrant peers over multiple years of schooling, rather than a purely contemporaneous effect.

3.3 Instrument Validity

The validity of our approach rests on three key assumptions, which we discuss next: a strong first stage, the exclusion restriction, and monotonicity. Additional details are provided in Online Appendix C.

First stage. The first-stage relationship is examined in Figure C.III. The estimated coefficient π_1 is approximately 0.28, and the Kleibergen–Paap rk Wald F-statistic exceeds 600, indicating a strong first stage. The first stage coefficient indicates that an important share of younger siblings do attend the same school as their eldest sibling. However, a large fraction of families send their children to different schools, which is consistent with the idea that families differentially chose schools for their children, the source of selection our instrument is trying to address.

Exclusion restriction. The exclusion restriction in our setting requires that, conditional on the fixed effects included in specification (1), predicted immigrant exposure at the eldest sibling's school affects native students' outcomes only through the actual immigrant share they face.

A first concern is that the instrument could affect younger sibling outcomes by changing the composition of native peers at the eldest sibling's school. To assess this possibility, we examine whether predicted immigrant exposure is related to predetermined characteristics of native students at the eldest sibling's school in the cohort of the younger sibling. We find no evidence of changes in gender, age, mother education, or parental expectations (see Online Appendix Figure C.IVa for more details).

Another concern is that immigrants might sort into schools in particular good trajectories. We study this possibility by analyzing whether predicted immigrant exposure is associated with differential pre-trends in test scores at the eldest sibling's school before younger siblings began school. We find no evidence of such trends, as illustrated in Online Appendix Figure C.IVb.

We also consider the possibility that the instrument could capture neighborhood-level immigration exposure rather than school-level exposure. To explore this, we take ad-

vantage of the fact that the Chilean school system allows students to attend school at different locations—i.e., there are not catchment areas. Thus, we compare estimates for students living nearby—i.e., in the same municipality—and further away—i.e., in a different municipality—from the eldest sibling's school, and obtain similar estimates for both groups. These results suggest that the effects are actually driven by exposure to immigrants in the school, as shown in Online Appendix Figure C.IVc.

Finally, we examine whether the estimated effects are driven specifically by exposure to immigrant peers in the student's own cohort, as opposed to broader school-level changes. We show that immigrant shares in adjacent cohorts—both before and after the student's own cohort—have no impact on the outcomes we study (see Online Appendix Figure F.I). This pattern indicates that the relevant variation operates at the cohort level, consistent with within-cohort peer effects rather than school-wide shocks. Overall, these findings are reassuring as they suggest that the exclusion restriction is likely to hold.

Monotonicity. Interpreting our IV estimates as a local average treatment effect requires that higher predicted immigrant exposure at the eldest sibling's school weakly increases the immigrant share a student actually experiences. While monotonicity is an individual-level assumption and therefore not directly testable with observational data, we conduct multiple exercises to study potential violations to this assumption.

First, we examine whether the average first stage relationship is globally non-decreasing along the support of the instrument. Panel (a) of Figure C.V presents a binscatter plot of the younger sibling's actual immigrant exposure (I) against the instrument (Z). The bin means are monotonically increasing across the full support of Z, indicating that higher values of the instrument are associated with higher values of treatment on average.

We then study whether the first stage remains strong and positive across subsamples defined by observable characteristics. Panel (b) of Figure C.V shows that the first stage coefficient is always positive and stable across gender, different levels of parental education, parental expectations, and school type, suggesting that the monotonic relationship between predicted and actual exposure holds across a wide range of observable characteristics.

We continue by examining whether the probability that a younger sibling attends the same school as the eldest sibling varies with the instrument—i.e., with the predicted

share of immigrants at the eldest sibling's school in the younger sibling's cohort. This is a natural concern considering international evidence on native flight in response to immigration (Betts and Fairlie, 2003; Cascio and Lewis, 2012; Figlio and Özek, 2019; Figlio et al., 2024). However, in panel (c) of Figure C.V we show that the probability of attending the same school as the eldest sibling does not change with predicted immigrant exposure at that school.

Finally, and related to the previous point, in panels (d) to (f) of Figure C.V we show that the instrument does not change the characteristics of the native peers—e.g., gender, age, mother education, and parental expectations—-that the younger sibling faces, nor does it change the baseline performance in test scores or pre-existing trends in test scores at the school the younger sibling actually attends. These patterns together suggest that although families might choose different schools for their children, they do not do so in response to predicted shares of immigrants. Thus, defiers do not seem to be a big concern in our setting.

4 Immigrant Exposure and Native Students' Educational Outcomes

This section presents the results of our analysis on the effects of immigrant exposure on native students' short, mid and long-term educational outcomes. We begin by discussing how exposure to immigrants in fourth grade impacts test scores in the same grade, which is the most frequently tested grade and the one for which relying on survey data allow us to characterize native and immigrant students with greater level of detail. We then show that these positive average effects of immigrant exposure in fourth grade persist into later grades and translate as well into improvements in high school completion and higher education enrollment rates.

Figure 2 summarizes our results. The bars in panels (a) and (b) illustrate how estimates for reading and mathematic fourth-grade test scores vary across specifications that progressively address selection across schools and families, starting with a naive OLS specification that controls only for year fixed effects and culminating in our preferred instrumental variables specification with mother fixed effects.

The patterns in reading and mathematics are very similar. In both cases, a naive

approach that only controls for year fixed effects—i.e., assumes that immigrants are randomly assigned to schools—yields negative and statistically significant estimates. These estimates show that a one-standard-deviation increase in exposure to immigrants—i.e., a 3.1 percentage point increase in the immigrant share or roughly one extra student per class—is associated with a decline of about 0.025 standard deviation in test scores.

However, this negative association turns positive once we control for school fixed effects, which is consistent with immigrants sorting into low-performing schools (Online Appendix C.1 describes immigrants' sorting patterns in detail). This specification yields a positive and statistically significant effect of 0.007σ in language and non-significant effect of 0.001σ in mathematics. Both estimates increase once we control for mother fixed effects, suggesting that school fixed effects are insufficient to fully account for selection in exposure to immigrants. These estimates—as the ones presented in Figlio et al. (2024)—already indicate that immigrant exposure is actually beneficial for native students.

Nevertheless, these estimates might still be biased if, with the aim of maximizing each child's learning, families differentially choose schools for their children. Our preferred IV-specification, which accounts for this possibility, suggests that this is indeed the case. When instrumenting actual immigrant exposure with the one younger siblings would have faced at their eldest sibling's school, the estimated effects become substantially larger. According to these estimates a one-standard-deviation increase in immigrant exposure improves test scores between 0.03 and 0.04 standard deviations. As shown in Table 2, the first-stage relationship underlying these estimates is strong, with Kleibergen–Paap F-statistics above 600.[3]

Table 2 presents the same results of the figures just discussed, together with the number of observations used to estimate each specification, the outcome mean of each sample, and the reduced form and first stage estimates of IV specifications. Specifications only controlling for year and school fixed effects are estimated in a sample much larger than the one used in specifications with mother fixed effects, as the latter require families with at least two children. The IV specifications are estimated in an even smaller sample as to be able to build the instrument and on top of that control for family fixed effects we need families with at least three children. Thus, a natural question is whether the larger effects observed in the IV specification are just the result of sample selection. In an Online

[3]Online Appendix Figure D.I studies heterogeneity by gender and shows that immigrant exposure affects similarly both male and female native students.

Appendix table D.I we show that this is not the case. We re-estimate all specifications in the same samples used for the IV specification—i.e., the most restrictive one—and find the exact same pattern discussed here, suggesting that the larger effects observed in the IV specification are not driven by sample selection.

An additional concern that arises in the context of IV estimates is that they might reflect a causal effect for a narrow or atypical subpopulation of *compliers*. Online Appendix Section C.6 explicitly characterizes this population following the approach of Hull (2025), and shows that compliers are more likely to come from families whose eldest sibling started in a public school (85% versus 45% among other native students) and in a low-SES school (55% versus 38%), more likely to have mothers who did not complete high school (31% versus 23%), and less likely to have mothers with higher education (15% versus 27%).

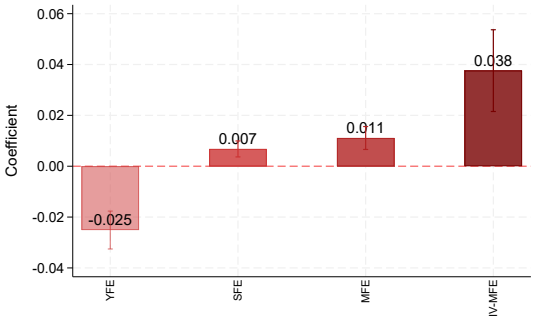
We therefore interpret our IV estimand as especially informative for students who are more exposed to the public-school margin and who come from relatively disadvantaged families. This is a relevant group of students and the target of multiple educational and social policies. This composition is consistent with the institutional environment described in Section 2, where immigrant students disproportionately sort into public schools. It is also consistent with the mechanisms discussed in Section 5, where we show that peer effects and the positive average selection of immigrants relative to natives are important drivers of our effects.

The effects we document are large. Relative to teacher quality, a one-standard-deviation increase in immigrant exposure implies gains that are of the same order as moving to a teacher with one-standard-deviation higher effectiveness (Chetty et al., 2014; Rivkin et al., 2005). At the same time, the magnitudes are not implausible. They lie within the range of effects generated by other relatively small classroom-level changes documented in the literature. For a one student class size reduction, Angrist and Lavy (1999) estimate effects slightly below 0.02σ in Israel, while Krueger and Whitmore (2001) report effects around 0.05σ in the Tennessee STAR experiment. In the peer-effects literature, our estimates are within range but closer to the upper bound (see Barrios-Fernández, 2023; Sacerdote, 2014, for detailed reviews). They are somewhat larger than the estimates in Carrell et al. (2018), where exposure to one additional disruptive student throughout elementary school lowers test scores by about 0.02σ, and close to the magnitudes reported by Figlio et al. (2024) for immigrant exposure in Florida using a related empirical design.

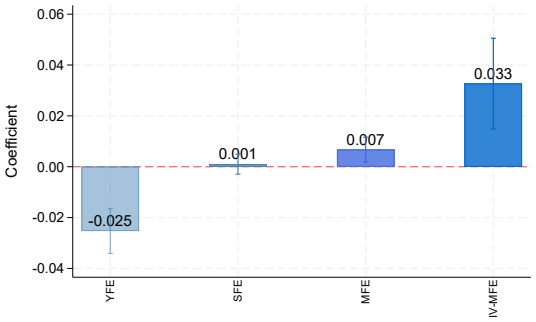
Overall, these comparisons indicate that the effects we estimate are large but consistent with credible effects from small changes in classroom composition in other contexts, which makes them plausible and policy-relevant.

An important question is whether the positive effects we document in fourth grade persist into later educational stages. To examine this, we estimate the same IV–MFE specification for medium- and long-run outcomes. As discussed in Section 3, standardized exams comparable to those in fourth grade are also administered in grades eight and ten. Because testing is less frequent in these grades, we pool scores across both grades to improve precision and estimate the effect of fourth-grade immigrant exposure on later achievement. Figure 2c shows that the positive effects observed in fourth grade persist into grades eight and ten. A one-standard-deviation increase in immigrant exposure in fourth grade raises later standardized test scores by 0.041 standard deviations in language and 0.016 in mathematics. We then examine whether these gains translate into educational attainment. A one-standard-deviation increase in fourth-grade immigrant exposure raises on-time high school graduation by 2.6 percentage points (3.2%) and higher education enrollment in the year after high school graduation by 1.1 percentage points (1.5%). We also analyze university admission exam outcomes and find no effect on the probability of taking the exam (coefficient approximately zero); among those who take it, the estimated effect on scores is 0.009 standard deviations and is not statistically significant.

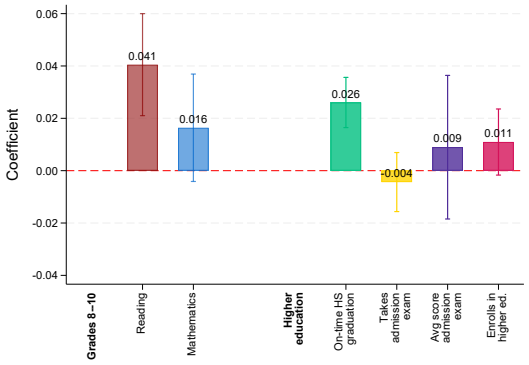
When interpreting these estimates, it is useful to note that immigrant exposure is persistent over time (see Online Appendix figure C.II). Part of the medium- and long-run effects therefore likely reflects cumulative exposure. We do not construct exposure measures beyond fourth grade because exposure up to fourth grade may affect selection into later-grade outcomes, which would introduce selection. Even with this caveat, the results indicate that immigrant exposure improves outcomes beyond contemporaneous test scores and on margins that are known to matter for long-run labor market outcomes (Card, 2001). This is especially relevant for higher education enrollment, given evidence that access to four-year colleges generates substantial returns even for academically marginal students (Zimmerman, 2014; Goodman et al., 2017).



(a) Reading



(b) Mathematics



(c) Medium- and long-term outcomes

Figure 2: Effects of immigrant exposure on educational outcomes

Notes: Panels (a) and (b) report estimated effects of a one-standard-deviation increase in the immigrant share, approximately 3.1 percentage points or roughly one additional immigrant student per classroom, on native students' fourth-grade test scores, measured in standard deviations of the native score distribution. Panel (a) reports reading and Panel (b) reports mathematics. Bars correspond, from left to right, to a naive OLS specification with year fixed effects only, OLS with school fixed effects, OLS with mother fixed effects, and the preferred 2SLS specification with mother fixed effects (IV–MFE), in which actual immigrant exposure is instrumented using the immigrant share the student would have faced at her eldest sibling's school. Panel (c) summarizes IV–MFE estimates for medium- and long-term educational outcomes, including standardized test scores in later grades, on-time high school completion, admission exam scores, and post-secondary enrollment. Binary outcomes are measured in percentage points, while test-score outcomes are measured in standard deviations of the native score distribution. Standard errors are clustered at the eldest sibling's school level; vertical lines indicate 95% confidence intervals.

Table 2: Effects of Immigrant Exposure on Test Scores

	NAIVE (1)	School FE (2)	School+Mother FE (3)	IV MFE (4)
		Panel A: SIMCE Language		
Share of Immigrants (σ)	-0.0251*** (0.0038)	0.0068*** (0.0016)	0.0111*** (0.0023)	0.0376*** (0.0082)
Reduced form				0.0107*** (0.0024)
First stage				0.2839*** (0.0139)
Observations	2945567	2945488	1154756	260907
Outcome mean	0.0177	0.0177	0.0274	-0.0751
Kleibergen–Paap F				601.20
		Panel B: SIMCE Mathematics		
Share of Immigrants (σ)	-0.0253*** (0.0045)	0.0010 (0.0020)	0.0067*** (0.0025)	0.0327*** (0.0091)
Reduced form				0.0092*** (0.0026)
First stage				0.2818*** (0.0142)
Observations	2949415	2949340	1157158	261465
Outcome mean	0.0189	0.0189	0.0497	-0.0301
Kleibergen–Paap F				619.23
Year FE	X	X	X	X
School FE		X	X	
Mother FE			X	X

Notes: Each cell reports coefficients; standard errors in parentheses. Columns (1)–(3) report OLS estimates. Column (4) reports 2SLS estimates in which 'Reduced form' gives the coefficient on the instrument in the outcome equation and 'First stage' gives the coefficient on the instrument in the first-stage equation. The treatment variable is the immigrant share standardized to have unit standard deviation (approximately 3.1 percentage points, or roughly one additional immigrant student per classroom). Outcome variables are standardized relative to the native student population in each year. Standard errors are clustered at the eldest sibling's school level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

5 What is behind the Immigrant Exposure Effects?

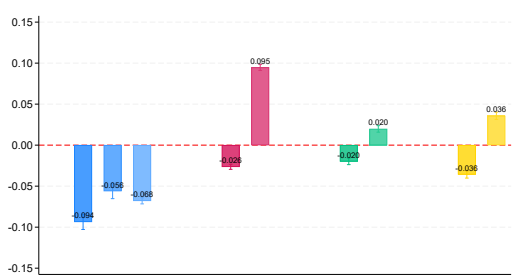
This section examines two potential mechanisms behind our main findings: peer effects and school resources. We first present evidence suggesting that peer effects are an important driver of the gains experienced by native students. We then show that higher immigrant

exposure does not meaningfully change either the quantity or the quality of school inputs, suggesting that school resources are unlikely to be a first-order driver in our setting.

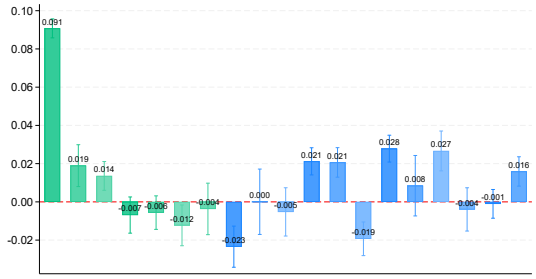
5.1 Immigrant Characteristics and Peer Effects

Characterizing Immigrant Peers: To assess whether peer effects can account for our main results, we first characterize immigrant students relative to natives along academic, sociodemographic, and multiple proxies of non-cognitive skills. Panels (a) and (b) of Figure 3 compare immigrants with the full native student population, whereas panels (c) and (d) compare immigrants with the native classmates they face within their school cohort. Relative to the overall native population, immigrants have lower test scores and grades and are older when they reach fourth grade. At the same time, they come from households with higher maternal education and higher educational expectations. On non-cognitive dimensions, immigrants score above natives on most proxies of motivation and grit, including effort, perseverance, and attitudes toward learning, but they score below natives on some proxies of self-efficacy.

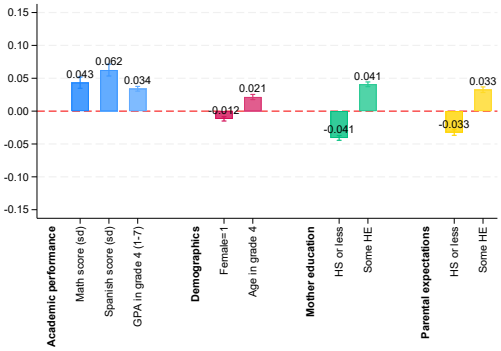
These aggregate differences do not necessarily capture the relevant peer exposure that native student face at their schools. Because immigrants sort non-randomly across schools, the natural benchmark is the school cohort. Once we compare immigrants with the native classmates they actually encounter, several patterns reverse. Immigrants tend to have higher test scores and grades, age differences at fourth grade become much smaller, and gaps in maternal education and parental expectations either widen or remain similar. The same contrast appears for non-cognitive traits. Relative to their school-cohort peers, immigrants score higher on all proxies of motivation and grit and on all measures of self-efficacy, with 14 of the 19 differences statistically significant at conventional levels. Taken together, these results indicate that immigrants are positively selected relative to the native peers they encounter within schools, which is consistent with peer effects as an important driver of our findings. Online Appendix Section E.1 provides additional detail on these exercises.



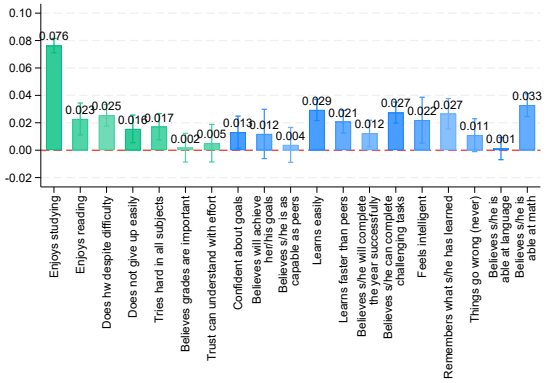
(a) Academic and demographic characteristics: overall population



(b) Cognitive and non-cognitive characteristics: overall population



(c) Academic and demographic characteristics: school cohort



(d) Cognitive and non-cognitive characteristics: school cohort

Figure 3: Immigrant students' characteristics relative to native students

Notes: The figure presents estimated differences in academic, demographic, cognitive, and non-cognitive characteristics between immigrant and native students. The left column reports academic and demographic outcomes, while the right column reports cognitive and non-cognitive outcomes. The top row compares immigrant and native students in the overall student population. Each bar in panels (a) and (b) corresponds to the coefficient on an indicator for immigrant status from a regression of the corresponding outcome on immigrant status and year fixed effects, as in equation (6). These coefficients capture average differences between immigrant and native students within the same year. The bottom row compares immigrant and native students within the same school cohort. Each bar in panels (c) and (d) corresponds to the coefficient on immigrant status from a regression of the corresponding outcome on immigrant status and school-by-year fixed effects, as in equation (7). These fixed effects absorb differences across schools and cohort-specific shocks within schools, so coefficients are identified from comparisons between immigrant and native classmates enrolled in the same school and year. Test score outcomes are standardized and coefficients can be interpreted in standard deviation units. GPA in Chile is measured in a scale that runs between 1 and 7. Female, mother's education, parental expectations, and cognitive and non-cognitive outcomes are binary indicators, so coefficients are expressed in percentage points, while age is measured in years. Cognitive and non-cognitive outcomes are constructed from student survey responses collected between 2009 and 2018. Each survey-based variable takes the value of one when the student reports the highest category of the corresponding item, or the lowest category in the case of "never" responses, and zero otherwise. Variables in the right-column panels are grouped into two domains: measures related to grit (green) and measures related to self-efficacy (blue). Vertical lines indicate 95% confidence intervals, with standard errors robust to heteroskedasticity.

Effects of Immigrant Exposure by Immigrants' Characteristics To test whether the peer-composition channel can account for our main results, we examine heterogeneity by immigrants' characteristics. Peer quality is not directly observable, so we proxy for it

using an index of academic motivation and grit built from SIMCE questionnaire items on effort, persistence, and attitudes toward learning. We focus on this index because it is available across cohorts and, unlike contemporaneous test scores, is less likely to be shaped by the peer environment we are studying, making it closer to a predetermined characteristic. We classify immigrants as below the native school-cohort median or at/above that median on this index.

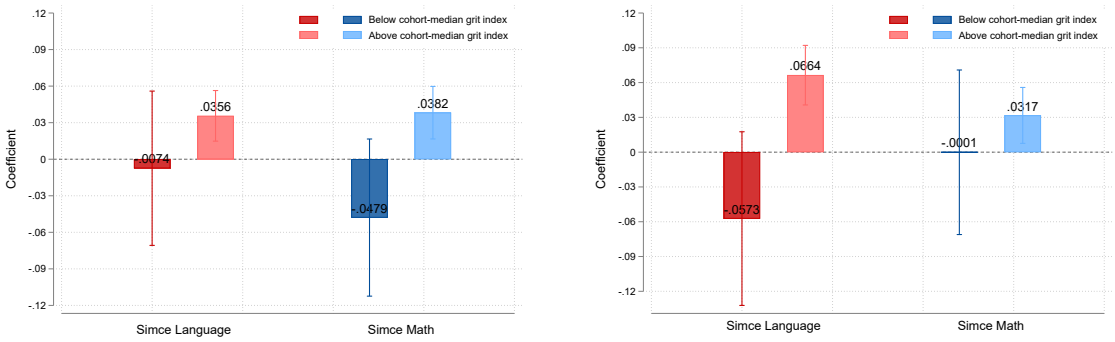
Because these survey measures are unavailable for some cohorts, precision becomes a first-order concern. We therefore use a specification that is closely related to our baseline design but differs in two respects. Instead of family fixed effects, we include fixed effects for the eldest sibling's first-grade school-by-entry-year. Thus, we exploit variation within a subset of families whose eldest child started school the same year and in the same institution. This adjustment allows us to include families with two siblings and substantially increases sample size.[4] We also define two endogenous treatments, exposure to below-median immigrants and exposure to at/above-median immigrants. Each treatment is instrumented with the corresponding predicted exposure at the school where the eldest sibling began first grade. Online Appendix E.3 provides full details.

Figure 4 shows that gains are driven by exposure to immigrants who score at or above the school-cohort median on this index, while exposure to below-median immigrants yields negative and imprecise estimates. Because immigrants are more likely to fall in the at/above-median group, average effects are positive. The evidence points to composition within immigrant exposure as a key margin, so what matters is not only the immigrant share but also which immigrants enter the cohort.

Panel (a) reports fourth-grade outcomes, where the index is measured contemporaneously. Panel (b) reports later outcomes, in eighth or tenth grade depending on data availability, and uses the fourth-grade index as a predetermined characteristic. We interpret these results as evidence that peer quality matters, with our motivation-and-grit index serving as a proxy for a broader set of favorable peer characteristics rather than pinpointing motivation and grit themselves as the operative trait. Rather than reflecting a uniform effect of immigrant presence, the results indicate that the academic influence of immigrant peers depends on the type of immigrant students to whom natives are exposed.[5]

[4] This alternative specification produces almost identical estimates to our baseline design when estimated on the same sample of families. Online Appendix E.2 presents a detailed discussion of this alternative specification and its validation.

[5] Online Appendix Figure E.III studies whether similar effects arise among native students. When we



(a) Fourth-grade test scores

(b) Eighth- or tenth-grade test scores

Figure 4: Effects of immigrant exposure by grit index

Notes: Each figure reports IV estimates of the effects of exposure to high-grit immigrants (dark bars) and low-grit immigrants (light bars) on native students' test scores. Immigrants are classified as above or below the school-cohort native median on an index of academic motivation and grit constructed from SIMCE student questionnaire items on effort, persistence, and attitudes toward studying (see Section 5 for index construction details). Both exposure shares are instrumented using the corresponding predicted shares at the eldest sibling's school, following equation (8). Effects are scaled to a one-standard-deviation increase in the relevant immigrant share and measured in standard deviations of the native test score distribution. Panel (a) reports effects on fourth-grade SIMCE language and mathematics scores; Panel (b) reports effects on eighth- or tenth-grade scores, depending on data availability for each cohort. The motivation-and-grit index in Panel (b) is measured in fourth grade and therefore serves as a predetermined characteristic relative to the later outcomes. Standard errors are clustered at the eldest sibling's school level; vertical lines indicate 95% confidence intervals.

School Environment and Teaching Practices The evidence above points to a peer-composition channel. Relative to their school cohorts, immigrant classmates are positively selected on GPA, test scores, motivation, effort, and behavior-related traits. If this channel is operative, higher immigrant exposure should also improve day-to-day classroom functioning, even without large changes in formal teacher inputs.

We next examine whether immigrant exposure is associated with changes in the school and learning environment. We use survey outcomes reported by students, parents, and teachers. As above, because survey modules are not available every year, we rely on the alternative IV design that allows us to include families with two siblings in the analysis; Online Appendix E.2 provides details and validation.

Figure 5 reports effects on school environment outcomes, including classroom disruptions, discipline, peer relations, and respect between students and teachers. The left-column panels show baseline means ($\hat{Y}$) and mean-plus-effect values ($\hat{Y} + \hat{\beta}$); the right-column panels report the corresponding coefficients with confidence intervals. Each outcome is estimated using equation (8), and variables are oriented so that higher values

examine exposure to native peers above or below the school-cohort median, we find a qualitatively similar pattern, although magnitudes are smaller.

denote better conditions.

Higher immigrant exposure is associated with fewer classroom disruptions, better peer relations, and more respectful interactions. Although not every estimate is individually precise, the pattern is consistent across outcomes and respondents. This evidence is consistent with an indirect peer channel in which improvements in classroom composition make the learning environment more conducive to instruction.

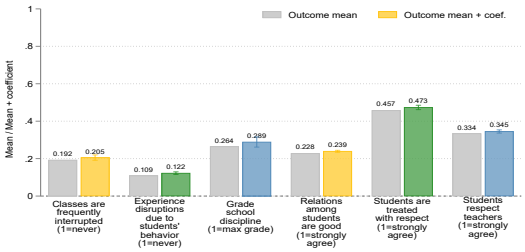
Panels (c) and (d) of Figure 5 examine teaching-practice outcomes reported by students and parents. The estimates suggest more favorable perceptions of instructional practices in higher-immigrant-exposure environments. Respondents are more likely to report that teachers pay attention to struggling students, provide feedback, and display stronger classroom management. We interpret these patterns as consistent with peer-driven improvements in classroom functioning that make existing teaching practices more effective or more salient; in Section 5.2, we show that teachers do not report changes in their own practices.

5.2 School Inputs and Resources

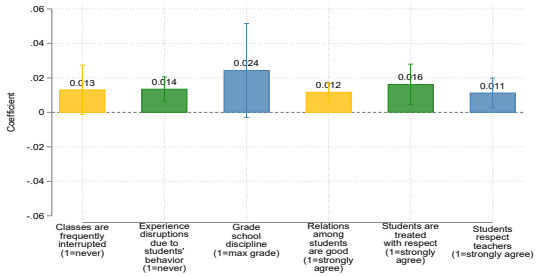
A natural hypothesis is that immigrant inflows affect native outcomes through school resources. This channel can operate in either direction. If schools are not adequately compensated for additional enrollment, resources per student may fall and native performance may deteriorate. Conversely, if schools receive support that scales with immigrant enrollment, available resources may increase, and in a setting such as Chile where immigrant students are positively selected relative to their cohorts this could reinforce achievement gains.

The institutional context in Section 2 suggests that large congestion effects are unlikely. The immigration shock occurred during a period of declining fertility and falling native demand for public schools, which left substantial slack capacity in many schools. At the same time, public and private subsidized schools operate under the same voucher system, and native and immigrant students are eligible for the same per-student funding. As a result, schools could absorb immigrant inflows while receiving additional revenues for each enrolled student, which keeps resources per student stable.

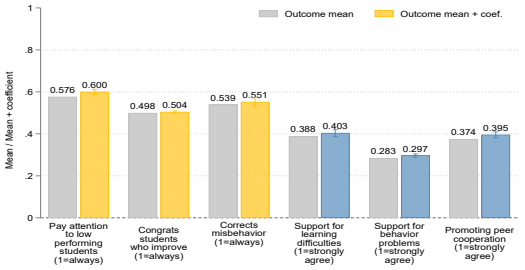
Beyond potential changes in the quantity of resources, schools may also adapt to immigrant inflows by changing resource quality or by modifying how available inputs are



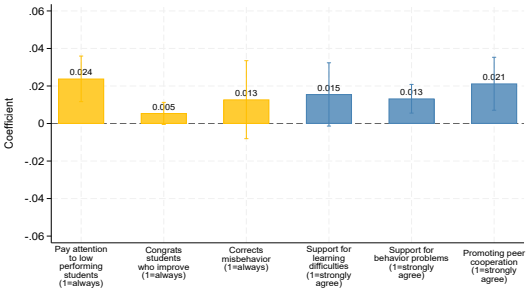
(a) School environment: $\hat{Y} + \hat{\beta}$



(b) School environment: $\hat{\beta}$



(c) Teaching practices: $\hat{Y} + \hat{\beta}$



(d) Teaching practices: $\hat{\beta}$

Figure 5: School environment and teaching practices

Notes: The figure summarizes the estimated effects of immigrant exposure on school environment and teacher practice outcomes. Panels (a) and (b) report results for school environment outcomes, while panels (c) and (d) report results for teacher practice outcomes. Panels (a) and (c) present outcomes in levels: gray bars show the baseline mean of each dependent variable, while colored bars show the corresponding baseline mean plus the estimated IV coefficient. Thus, the distance between the gray and colored bars corresponds to the estimated effect of a one-standard-deviation increase in the immigrant share. Panels (b) and (d) report the same estimated IV coefficients directly, measured in the units of each dependent variable. Vertical lines in all panels indicate 95% confidence intervals, centered around the mean-plus-coefficient estimate in panels (a) and (c), and around the coefficient estimate in panels (b) and (d). In all panels, estimates follow the alternative IV specification in equation (8), where immigrant exposure is instrumented using the predicted immigrant share based on the eldest sibling's school, and the specification includes eldest-sibling school-by-entry-year fixed effects and entry-year fixed effects. Standard errors are clustered at the eldest sibling's school level. School environment outcomes include measures of classroom disruptions, school discipline, peer relations and respect between students and teachers. All school environment outcomes are coded so that higher values reflect a better environment, such as fewer disruptions, stronger peer relations, more respectful interactions, or a friendlier school climate. Teacher practice outcomes capture instructional and classroom-management practices, including attention to low-performing students, recognition of student improvement, correction of misbehavior, support for learning difficulties, support for behavior problems, and promotion of peer cooperation. Colors indicate the SIMCE survey source: student questionnaires are shown in yellow, teacher questionnaires in green, and parent questionnaires in blue.

used in the classroom. We evaluate all these possibilities by combining administrative records on enrollment, staffing, and teacher characteristics, including teachers' university-admission scores from DEMRE, with teacher survey responses on classroom practices. Using these outcomes, we estimate equation (8) to assess adjustments in cohort size, class size, teacher–student ratios, new teacher hiring, teacher qualifications, and reported teaching practices. Across this set of outcomes, we find no evidence that immigrant exposure meaningfully changes the amount or quality of school resources, nor the practices that teachers report using in class.

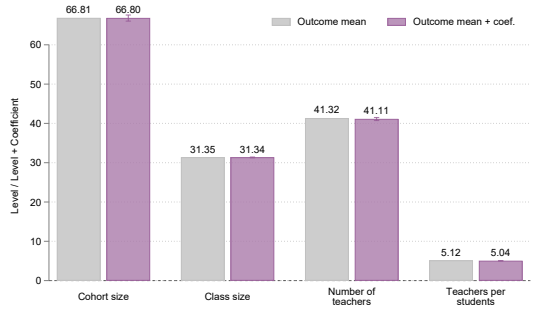
Panels (a) and (b) of Figure 6 show little evidence that schools adjust quantity margins in response to immigrant inflows. Cohort size, class size, teacher–student ratios, and new hiring remain broadly stable, with economically small effects. Panels (c) and (d) likewise show no meaningful changes in teacher quality: admission scores, certification, university graduation, and experience are largely unchanged.

An additional contrast is informative. In the previous subsection, student and parent reports suggested improvements in teaching practices. Here, panels (e) and (f) show that teacher-reported practices do not change significantly. Taken together, these findings suggest that the mechanism is unlikely to be large shifts in formal teaching inputs. Instead, they are more consistent with improved classroom conditions under which similar teaching practices are experienced as more effective by students and parents.

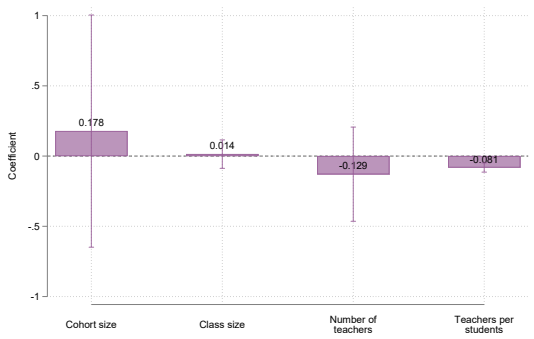
Overall, the evidence indicates that school resources and teacher quality are unlikely to be first-order drivers of our main effects. In this setting, the voucher system and the decline in native enrollment appear to have allowed schools to absorb immigrant inflows without meaningful congestion. The pattern across mechanisms therefore points primarily to peer effects.

6 Conclusion

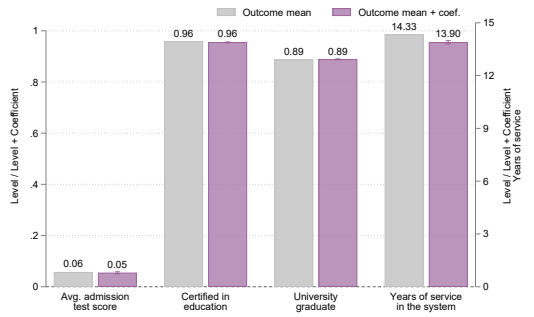
This paper provides causal evidence on how exposure to immigrant classmates affects native students' educational outcomes, exploiting a largely unanticipated immigration shock that recently affected the K–12 Chilean school system. Using population-wide administrative records linked to survey data and instrumenting each child's immigrant exposure with the exposure they would have experienced at their eldest sibling's school, we find that



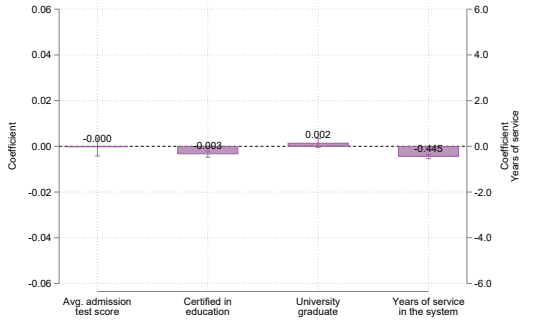
(a) School inputs: $\hat{Y} + \hat{\beta}$



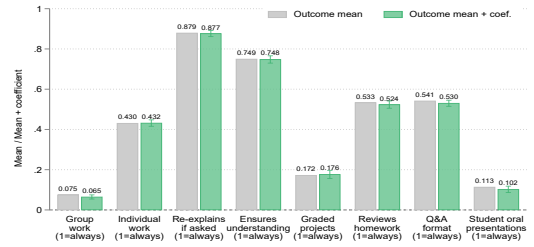
(b) School inputs: $\hat{\beta}$



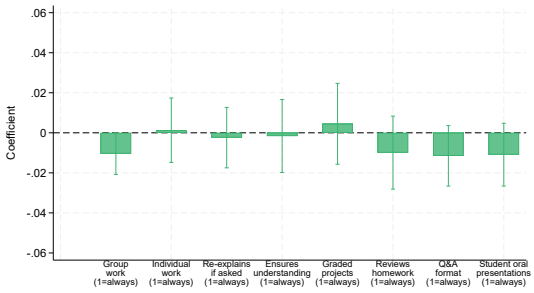
(c) Teacher quality: $\hat{Y} + \hat{\beta}$



(d) Teacher quality: $\hat{\beta}$



(e) Teacher reports: $\hat{Y} + \hat{\beta}$



(f) Teacher reports: $\hat{\beta}$

Figure 6: School resources, teacher quality, and teacher-reported practices

Notes: The figure presents IV estimates of the effect of a one-standard-deviation increase in immigrant peer exposure on school resources, teacher quality, and teacher-reported classroom practices. The left column reports outcomes in levels: gray bars show the baseline mean, while colored bars show the baseline mean plus the estimated coefficient. The right column reports the corresponding coefficients directly; vertical lines indicate 95% confidence intervals.

Panels (a)–(b) report school inputs, including cohort size, class size, the number of teachers, and the teacher–student ratio, constructed from administrative enrollment and teacher records. Panels (c)–(d) report teacher quality, including admission test scores, certification, university graduation, and years of service, constructed from teacher administrative records and teacher admission-exam records. In panel (d), coefficients for the first three outcomes are shown on the left axis, while years of service is shown on the right axis. Both axes are centered at zero and scaled to approximately one standard deviation of the relevant outcomes; bar heights across axes should therefore not be compared directly. Panels (e)–(f) report teacher-reported classroom practices, constructed from SIMCE teacher questionnaires, including group and individual work, re-explanation, checks for understanding, graded projects, homework review, Q&A activities, and oral presentations.

All estimates use the alternative IV specification in equation (8), which instruments immigrant exposure using predicted exposure at the eldest sibling's school and includes eldest-sibling school-by-entry-cohort fixed effects. Standard errors are clustered at the eldest sibling's school level. Outcomes in panels (a)–(d) are measured in natural units, while teacher-reported practices in panels (e)–(f) are binary indicators.

immigrant exposure raises native test scores and later educational attainment, including high school completion and higher education enrollment. We show that immigrants are positively selected relative to their native classmates on both cognitive and non-cognitive dimensions, suggesting that peer effects are an important driver of our findings. Consistent with this mechanism, we find that gains are concentrated among students exposed to immigrants who score above the school-cohort median on our proxy for peer quality, an index of motivation and grit. We find no evidence that schools respond to immigrant inflows by either straining or reallocating resources.

Most immigrants to Chile arrive from other Spanish-speaking countries, so language is unlikely to pose an important barrier between immigrant and native students in our setting. Comparable positive effects have nonetheless been documented in settings with substantial language differences between immigrants and natives (Geay et al., 2013; Ballatore et al., 2018; Jensen and Rasmussen, 2011; Schneeweis, 2015), suggesting that a shared language is not necessary for immigrant exposure to benefit native students.

Our findings speak to a tension that animates the broader public debate on immigration. In Chile, as in many receiving countries, public attitudes toward immigrants in schools turned increasingly negative over the period we study. Yet along the educational dimension this perception diverges sharply from the evidence, as the actual effects of immigrant exposure on native students' learning and long-run trajectories are positive.

These findings apply most directly to settings that share two features of the Chilean context—positive immigrant selection relative to local peers and school systems with sufficient capacity to absorb immigrant inflows without straining resources. In settings where either condition fails—immigrants are negatively selected or schools are genuinely congested—effects may differ or reverse. Neither condition appears to be specific to Chile. Because migration is itself a costly and selective decision, immigrants' positive selection relative to natives is a pattern documented well beyond our setting. Figlio et al. (2024) attributes positive effects of immigrants on native students in Florida to similar selection-and-sorting dynamics, and studies of labor market effects document analogous neighborhood sorting in Germany, the United Kingdom, and the United States (Dustmann et al., 2024; Card, 2001; Saiz, 2007; Glitz, 2014). In many settings, countries have found ways to accommodate rising school-age demand (Verme and Schuettler, 2021; Assaad et al., 2023), and demographic change of the kind we describe is likely to further facilitate that

adjustment (OECD, 2024).

Our mechanism results carry a direct policy implication. Because the gains we document hinge on how immigrant students compare to their native classmates, the same mechanism that raises native achievement in our setting could depress it elsewhere if immigrants compare less favorably to their local peers. Identifying schools or areas where this happens is therefore central to targeting support programs and additional resources toward integrating immigrant students into the school system without compromising native learning. Characterizing this heterogeneity across contexts, and understanding which policies can effectively respond to it, is an important avenue for future research.

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Immigrant Exposure and Native Students' Educational Outcomes

Online Appendix

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August 25, 2026

Latest Version

Contents

A Further Institutional Details

This section presents additional descriptive evidence on immigration patterns and social interactions within schools in Chile. The first part of the section characterizes the evolution, composition, and geographic concentration of immigration, as well as the resulting exposure of native students to immigrant peers. The second part documents the social environment experienced by immigrant students, with a particular focus on discrimination. Together, these descriptive patterns provide additional context for Section 2.1 in the main text.

A.1 National trends and exposure.

Panel A of Figure A.I provides a national overview of immigration dynamics and native students' exposure to immigrant peers. Figure (a) shows that the total number of immigrants in Chile increased substantially over the period, alongside a rise in the share of foreign-born students enrolled in the K–12 system. This share rose from about 1% in 2008 to roughly 8% by 2022. Thus, while immigrant presence within schools increased meaningfully, it remained moderate in aggregate terms, consistent with the levels reported in the main text.

Figure (b) translates these aggregate trends into exposure at the cohort level. The share of native students with at least one immigrant classmate increased from approximately 20% to 65% over the same period. This indicates that relatively modest changes in aggregate immigrant shares can generate large increases in peer exposure within schools. The difference between these two margins reflects the uneven distribution of immigrant students across schools, which makes changes in school composition more visible in the probability of contact than in aggregate enrollment shares.

A.2 Regional variation and exposure.

A key feature of immigration to Chile is its strong geographic concentration. Panel B of Figure A.I documents the evolution of the share of immigrant students across regions, as well as the corresponding exposure of native students to immigrant peers.

Figure (c) shows that immigrant inflows are highly uneven across regions, with particularly large increases in the northern regions and in the Metropolitan Region. Figure

(d) translates these patterns into native students' exposure, showing that the likelihood of interacting with immigrant peers increased sharply over time, especially in high-inflow areas.

This regional heterogeneity is important for two reasons. First, it highlights that the immigration shock was not uniform across the country, but instead generated substantial variation in exposure across cohorts and locations. Second, it provides suggestive evidence that the observed changes are driven by external migration dynamics rather than by endogenous responses of the education system, as inflows are concentrated in regions that are more exposed to international migration routes.

A.3 Enrollment dynamics by school type.

Figure A.II complements the aggregate enrollment patterns shown in the main text by disaggregating these trends by school type. Panel (a) shows that the decline in native fourth-grade enrollment was particularly pronounced in public schools, while immigrant enrollment increased steadily over the period. Panel (b) shows that voucher schools experienced comparatively milder changes in native enrollment, alongside a gradual rise in immigrant enrollment.

These patterns are consistent with the institutional context described in Section 2. Immigration expanded at a time when Chilean schools had substantial slack capacity due to declining fertility, but the adjustment was uneven across school sectors. Overall, the figure shows that immigrant inflows partly offset shrinking native cohorts in both public and voucher schools, with a stronger margin of adjustment in the public sector.

A.4 Country-of-origin composition.

Beyond its magnitude and spatial distribution, the composition of immigration to Chile has also changed markedly over time. Figure A.III documents the evolution of immigrant students by country of origin, revealing a substantial shift in the sources of immigration during the period of analysis.

In the earlier phase of the immigration wave—spanning the 2000s and early 2010s—inflows were largely dominated by migrants from neighboring countries, particularly Peru, which accounted for the largest share of immigrant students through the late 2010s. This composition began to shift toward the end of the 2010s and changed markedly in the early 2020s.

Over this period, the number of students from Venezuela increased rapidly, overtaking other groups and becoming the predominant source of immigration in the Chilean school system in the 2020s.

This shift in composition closely aligns with major economic and political shocks in origin countries—most notably the humanitarian crisis in Venezuela—rather than with changes in Chilean education policies. As a result, the timing and scale of these inflows appear largely driven by external factors.

These changes in country-of-origin composition are important for interpreting the empirical analysis, as different immigrant cohorts may vary in language, prior schooling experiences, and socioeconomic background. At the same time, the sharp and externally driven nature of these compositional shifts further supports the view that the immigration shock studied in this paper is plausibly exogenous to the Chilean education system.

A.5 Parental background and positive selection.

Finally, we provide evidence on the socioeconomic background of immigrant students. This exercise is motivated by the need to understand whether immigrant students are negatively or positively selected relative to the native population, as this is a key determinant of potential peer effects.

Figure A.IV reports two indicators of parental background: mothers' educational attainment and parental expectations regarding children's educational attainment.

The results show that immigrant students are more likely to have mothers with higher education and to have parents who expect them to reach higher education. These patterns are consistent with positive selection among immigrant families, particularly along dimensions related to educational aspirations and long-term investment in human capital.

This evidence helps rationalize the positive peer effects documented in the main analysis. If immigrant students are positively selected in terms of motivation and expectations, their presence in the classroom may generate beneficial spillovers for native students, especially through non-cognitive channels.

A.6 Discrimination and Social Interactions

We begin by documenting differences in reported discrimination between immigrant and native students within the same school environment. This exercise is motivated by the

fact that peer effects operate not only through academic channels, but also through social interactions. Understanding whether immigrant students face differential treatment is therefore key for interpreting the broader consequences of immigration in schools.

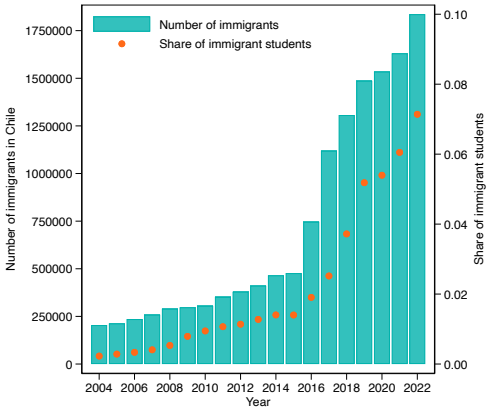
Figure A.V reports differences in self-reported discrimination between immigrant and native students attending the same school in the same year. Each coefficient is obtained from a regression where the dependent variable is a binary indicator for reporting a specific type of discriminatory experience, and the key explanatory variable is an indicator for immigrant status. All specifications include school and year fixed effects, so comparisons are made within the same school cohort.

The discrimination measures are constructed using student questionnaires from the SIMCE surveys for the years 2017 and 2018. Students are asked the following question: "Thinking about this year, have you felt rejected at your school for any of the following reasons" (*"Pensando en este año, ¿te has sentido rechazado en tu escuela por alguno de los siguientes motivos?"*) For each category, we define an indicator equal to one if the student reports having experienced that type of discrimination. The categories include discrimination based on sex, physical characteristics, personality, appearance (e.g., clothing or hairstyle), disability status, and being a foreigner. In addition, we construct an aggregate indicator equal to one if the student reports having experienced at least one of these forms of discrimination.

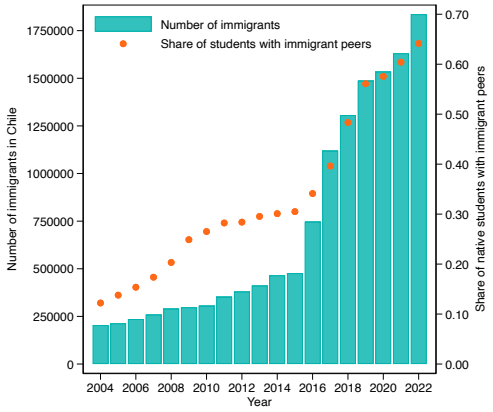
The results show that immigrant students are consistently more likely to report experiencing discrimination. The overall probability of reporting any type of discrimination is 10.9 percentage points higher for immigrant students relative to their native peers. This difference is largely driven by discrimination related to being a foreigner, with an estimated effect of 0.323. In contrast, for other types of discrimination, the differences are modest: immigrant students are slightly more likely to report discrimination based on physical characteristics (0.008), appearance (0.014), and disability (0.011). We find no meaningful differences in discrimination based on sex or personality.

These patterns indicate that immigrant students face a systematically more adverse social environment than their native peers, with particularly pronounced differences in dimensions directly linked to their migrant status. This evidence highlights that the integration of immigrant students is not frictionless. Even in a setting where we document positive peer effects on academic outcomes, immigrant students are disproportionately

exposed to negative social interactions. This coexistence of positive academic spillovers and adverse social experiences underscores the importance of considering both dimensions when evaluating the overall impact of immigration in schools.

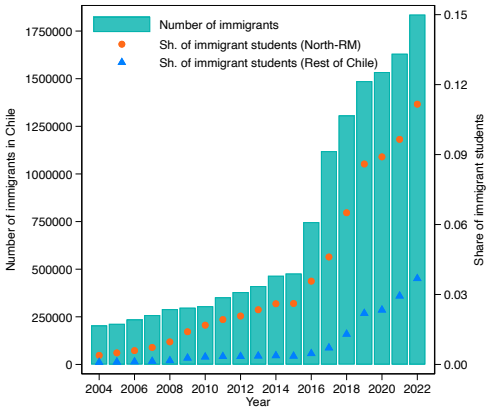


(a) Immigration evolution

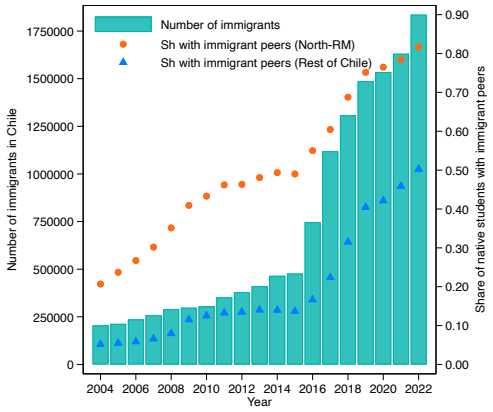


(b) Native exposure to immigrants

Panel A. National level



(c) Immigrant share evolution

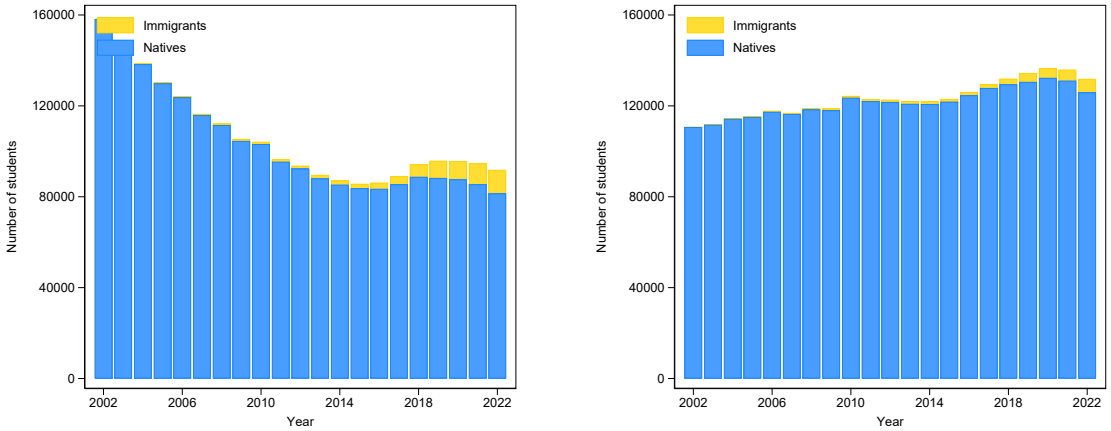


(d) Native exposure to immigrants

Panel B. Regional level

Figure A.I: Immigration trends and native exposure

Notes: Panel A reports national-level dynamics. Figure (a) plots the total stock of immigrants in Chile between 2004 and 2022 (left axis) and the share of foreign-born students enrolled in the K–12 school system (right axis). Figure (b) plots the share of native fourth-grade students with at least one immigrant classmate in their school cohort. Panel B reports regional-level dynamics. Figure (c) plots the share of foreign-born students in the K–12 system between 2008 and 2022, separately for northern regions plus the Metropolitan Region and the rest of the country. Figure (d) plots the share of native fourth-grade students with at least one immigrant classmate in their school cohort, by the same regional grouping. Immigration stock series are from the National Statistics Institute (INE). School enrollment data are from Ministry of Education administrative records covering the universe of K–12 students. Immigration status is defined based on students' country of birth.



(a) Public schools (b) Voucher schools

Figure A.II: Enrollment evolution by school type

Notes: The figure illustrates the evolution of total enrollment among fourth-grade native students (blue) and fourth-grade immigrant students (yellow) from 2002 to 2022. Panel (a) focuses on public schools, and panel (b) focuses on private subsidized schools.

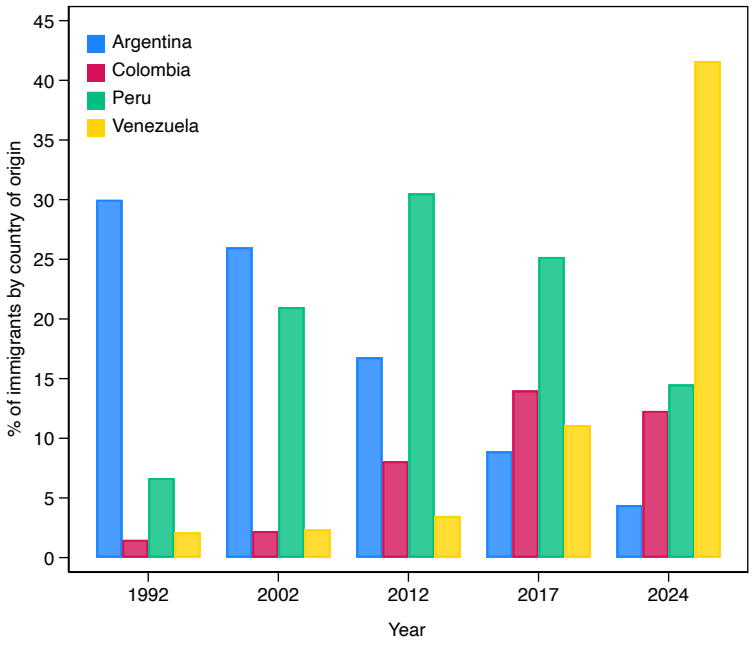
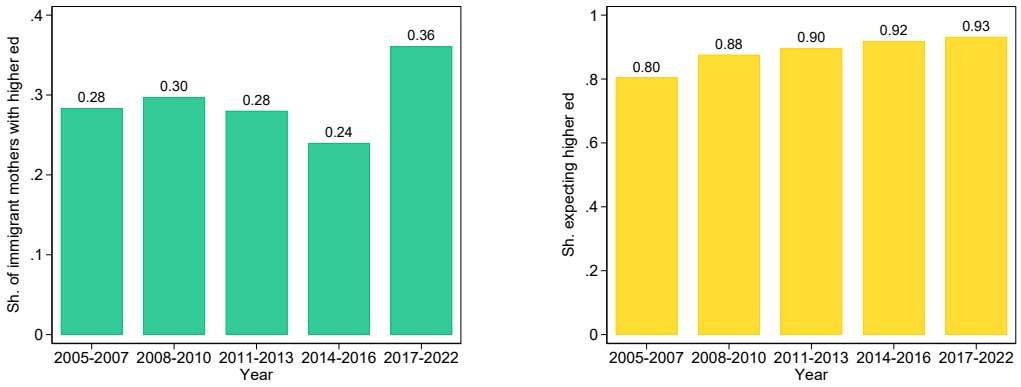


Figure A.III: Evolution of immigration to Chile by country of origin

Notes: The figure shows the evolution of the share of immigrant students by country of origin between 1992 and 2024. The series include the main sending countries to Chile—Argentina, Colombia, Peru, and Venezuela. Immigration status and country of origin are defined based on students' country of birth. All measures are constructed using administrative enrollment records from the Ministry of Education covering the universe of K–12 students.



(a) Share of immigrant students with mothers with higher education

(b) Share of immigrant students with parents expecting them to reach higher education

Figure A.IV: Parental background of immigrant students

Notes: Panel (a) reports the share of immigrant students whose mothers have completed higher education. Panel (b) shows the share of immigrant students whose parents report expecting them to attain higher education. Parental education and expectations are measured using parent surveys administered by the Education Quality Agency in fourth grade. The sample is restricted to immigrant students with non-missing survey information.

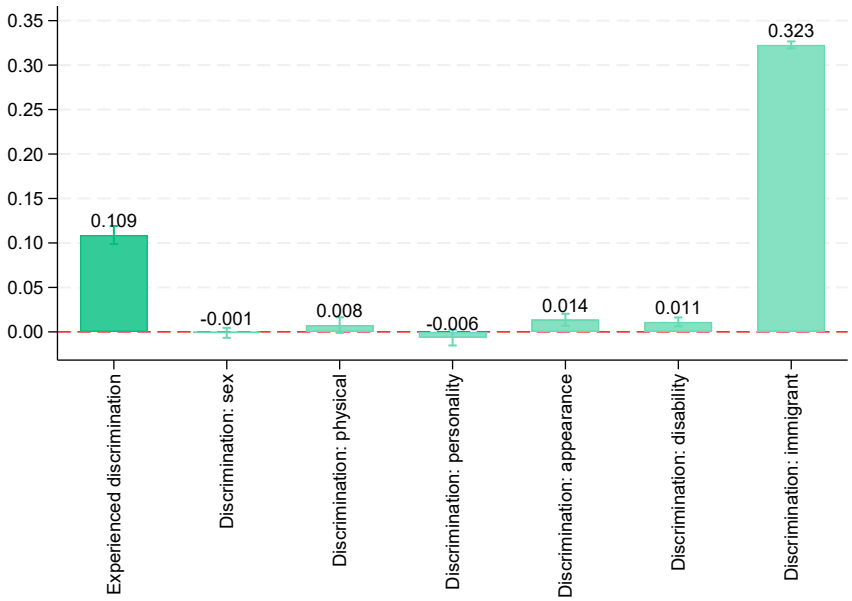


Figure A.V: Discrimination

Notes: The figure reports differences in reported discrimination between immigrant and native students within the same school cohort. Each coefficient is obtained from a regression where the dependent variable is a binary indicator equal to one if the student reports having experienced the corresponding type of discrimination, and the explanatory variable is a binary indicator for immigrant status. All regressions include school and year fixed effects, so comparisons are made between immigrant and native students attending the same school in the same year. Discrimination measures are constructed using student questionnaires from the SIMCE surveys for 2017 and 2018. Students are asked: "Thinking about this year, have you felt rejected at your school for any of the following reasons" Indicators are defined for discrimination based on sex, physical characteristics, personality, appearance (e.g., clothing or hairstyle), disability, and being a foreigner. Experienced discrimination is an indicator equal to one if the student reports any form of discrimination. Baseline rates for native students are as follows: 41.8% report any discrimination, 7.9% report discrimination based on sex, 23.6% on physical characteristics, 12.1% on appearance, 6.1% on disability, and 3.1% on being a foreigner.

B Data and Sample Construction

This appendix subsection provides additional detail on data sources, variable construction, and sample definition. Its purpose is to make transparent how outcomes and key explanatory variables are measured and how sample restrictions map into the estimands discussed in the main text.

B.1 Data sources

This project combines administrative records from the Ministry of Education, the Education Quality Agency, and the Department of Educational Measurement (DEMRE), which administers the national university admission exam.

The administrative enrollment records cover approximately 5.2 million student-year observations across more than 10,000 schools and 380 municipalities over the period 2002–2022, effectively spanning the universe of K–12 students in Chile and allowing us to follow students throughout their educational trajectories. These data are complemented by SIMCE standardized test records, which include roughly 235,000 students per cohort in fourth grade and provide near-universal coverage of academic achievement.

From the Ministry of Education, we obtain longitudinal records covering the universe of students enrolled in Chile's K–12 school system, including public, voucher, and private schools. These registers contain detailed information on schools—such as identifiers, administrative dependence, and location—and on students—such as identifiers, gender, age, grade, class section, attendance, GPA, and grade completion status. We link these records to data on standardized exams from the Education Quality Agency using a consistent student identifier.

A distinctive feature of the Chilean system is that, together with these standardized exams, rich background surveys are administered to students, their parents, and teachers. These surveys collect detailed information on household characteristics—such as parental education, expectations, and books at home—as well as on study habits, teaching practices, and the school environment. We use these data to characterize and compare immigrant and native students beyond academic performance and to explore potential mechanisms underlying our main effects.

We complement these data with DEMRE records on the national university admission

exam, which allow us to study whether exposure to immigrant peers affects the likelihood of taking the exam and native students' performance on it. In addition, we merge data from the Ministry of Education on higher education enrollment to examine whether immigrant exposure influences postsecondary attendance.

The Ministry of Education also provides detailed information on teachers, including their education, university admission exam scores, demographics, and years of experience, which we link to schools. These data allow us to investigate whether larger immigrant concentrations affect both the number and the composition of teachers within schools.

Family links are constructed using the approach in Altmejd et al. (2021); Barrios-Fernández (2022); Barrios Fernández et al. (2024), which matches siblings through shared maternal identifiers in the enrollment registry. Approximately 74% of students in the enrollment data can be linked to their mother; among them, about 67% have at least one sibling and 27% belong to families with three or more children. These statistics indicate substantial coverage of family networks in the administrative data.

Table B.I summarizes the sources, coverage, units of observation, and main variables used throughout the analysis. It also clarifies how each source contributes to the construction of the treatment, the short-, medium-, and long-run outcomes, the family-based instrument, and the mechanism exercises.

B.2 Variable construction

The data allow us to observe a broad set of outcomes across the educational trajectory. Administrative records yield binary outcomes (grade repetition, on-time high school completion, higher-education enrollment) and continuous outcomes (GPA, attendance) with near-universal coverage.

The SIMCE standardized tests provide academic achievement measures in grades four, eight, and ten, with annual administration in grade four since 2005 and regular administration in other grades since 2004.[6] Accompanying background surveys provide information on parental education, household resources, study habits, non-cognitive skills, teaching practices, and school climate.

In the K–12 enrollment data, we observe whether each student was born in Chile

[6]SIMCE administration in eight grade was in 2009, 2011, 2013, 2014, 2017 and 2019, and SIMCE administration in ten grade was in 2008, 2010, 2012, 2013, 2014, 2015, 2016, 2017, 2018, 2022, 2023 and 2024.

or abroad. Since nationality of origin is available only from 2016 onwards, we define immigrant status solely based on the foreign-born indicator.

When analyzing test scores, we standardize them using the mean and standard deviation of the native student population. Most analyses focus on grade four, the level with the most consistent testing. We define exposure to immigrants as the share of immigrant students in a student's school cohort in grade four.

Table B.II provides the exact definition, timing, source, and scale of the outcomes used in the paper. Panel A presents the main short-, medium-, and long-run educational outcomes, while Panel B describes the additional administrative and survey-based outcomes used in the descriptive and mechanism analyses. Reading and mathematics are grouped within a common row whenever they share the same source, timing, standardization, and interpretation.

B.3 Sample definition

Our main sample consists of native students who started first grade between 2005 and 2015 and who are expected to reach fourth grade between 2008 and 2018. Table 1 reports summary statistics for this sample and compares three groups: (i) all students entering school in the same cohorts, (ii) immigrant students, and (iii) native students.

Panel A of Table 1 shows demographic characteristics. Gender composition is fairly similar across groups, but immigrant students are somewhat less likely to be female (45.0%) than native students (48.9%). Age in fourth grade is also close across groups, although immigrants are slightly older on average (9.363 versus 9.244 years).

Panel B documents socioeconomic background. School type differences are sizable: immigrant students are much more likely to attend public schools (58.4% versus 40.7% among natives) and less likely to attend voucher schools (33.9% versus 51.1%). The share attending private schools is similar across groups, though slightly lower for immigrants (7.7% versus 8.1%).

The school SES distribution is consistent with these enrollment patterns. Immigrants are less represented in low-SES schools (37.0% versus 40.6%), more represented in middle-SES schools (41.0% versus 34.3%), and somewhat less represented in high-SES schools (21.9% versus 25.0%).

Parental background measures show a mixed profile. Immigrant students are less likely

to have mothers with completed high school (41.2% versus 45.9%), but more likely to have mothers with higher education (31.5% versus 24.7%). They are also slightly less likely to have mothers with less than high school (27.3% versus 29.4%). Household cultural-resource measures point to greater concentration at both tails: immigrants are more likely to report fewer than 10 books at home (43.2% versus 37.1%) and more than 100 books (9.7% versus 8.1%), while they are less represented in intermediate categories (10–50 books: 36.4% versus 43.0%; 50–100 books: 10.6% versus 11.8%).

Panel C shows academic outcomes in fourth grade. Immigrant students have lower average GPA (5.676 versus 5.882), lower attendance (87.159% versus 92.538%), and lower standardized test scores in both Spanish (-0.046 versus 0.017) and math (-0.081 versus 0.018). They also exhibit substantially higher grade retention (6.9% versus 2.5%).

Together, the table indicates that immigrant and native students differ not only in average academic performance, but also in school type, SES composition, and household background. These baseline gaps underscore the relevance of the empirical strategy in the main text, which compares students within narrowly defined school-cohort environments.

Table B.I: Data Sources and Coverage

Data source	Provider	Coverage	Unit of observation	Main variables used	Role in the analysis
K–12 enrollment records	Ministry of Education	2002–2022	Student–school–grade–year	Student and school identifiers; gender; age; nationality status; grade; school type and location	Construction of immigrant status and school-cohort immigrant exposure; sample definition
Academic records	Ministry of Education	2002–2022	Student–school–grade–year	Student and school identifiers; attendance; GPA; grade progression and completion	Construction of school histories, attendance, GPA, and grade-progression measures
Teacher administrative records	Ministry of Education	2008–2018	Teacher–school–year	Teacher identifiers; school assignment; education; certification; university graduation; experience	School-input, teacher–student-ratio, staffing, and teacher-composition analyses
Teacher admission-exam records	DEMRE	2008–2018	Teacher–exam process	University admission-exam scores	Proxy for teacher academic preparation in the teacher-quality analysis
SIMCE assessment records	Education Quality Agency	Fourth grade: 2008–2018; grades eight and ten: selected years[a]	Student–grade–year	Reading and mathematics scores; grade tested; school and cohort identifiers	Fourth-grade test-score outcomes and medium-run test-score outcomes in grades eight and ten
SIMCE background questionnaires	Education Quality Agency	Fourth grade: 2008–2018; grades eight and ten: selected years[b]	Student, parent, or teacher questionnaire response	Parental education and expectations; books at home; motivation and grit; self-efficacy; school climate; teacher practices	Description of immigrant and native students; construction of the motivation-and-grit index; peer-mechanism and school-environment analyses
High-school graduation records	Ministry of Education	2016–2024	Student–graduation year	High-school completion status; year of graduation; school of graduation	On-time high-school graduation
University admission-exam records	DEMRE	2016–2024	Student–exam process–year	Exam participation and subject-specific admission scores	Indicator for taking the admission exam and average standardized admission-exam score
Higher-education enrollment records	Ministry of Education	2017–2024	Student–institution–program–year	Enrollment in postsecondary education and year of entry	Higher-education enrollment in the year following expected high-school graduation
Family-link records	Birth registry	1992–2010	Student–mother link	Maternal identifier and sibling links	Mother fixed effects; identification of the eldest sibling; construction of predicted immigrant exposure at the eldest sibling's school

Notes: The table summarizes the administrative records, assessments, and background questionnaires used in the analysis. Student-level records are linked across sources using anonymized individual identifiers. School identifiers are used to link students to school, cohort, and teacher information. *a.* Eighth-grade SIMCE assessments were administered in 2009, 2011, 2013, 2014, 2017, and 2019. Tenth-grade assessments were administered in 2008, 2010, 2012–2018, and 2022–2024. The medium-run analyses use the assessment years that can be linked to the fourth-grade cohorts included in the estimation sample. *b.* Background questionnaires are administered alongside the SIMCE assessments. However, the questions and response categories included vary across years, grades, and respondent types. Consequently, the years of coverage and estimation samples differ across questionnaire-based measures.

Table B.II: Outcome Definitions and Construction

Outcome	Source	Timing	Definition	Scale
Fourth-grade reading and mathematics scores	SIMCE assessments	Fourth grade	Reading and mathematics test scores, standardized using the native-student distribution in the corresponding year	Standard deviations of the native-student distribution
Later-grade reading and mathematics scores	SIMCE assessments	Grades eight and ten	Reading and mathematics test scores in grades eight and ten, standardized using the native-student distribution in the corresponding year and grade	Standard deviations of the native-student distribution
On-time high-school graduation	High-school graduation records	Expected year of high-school graduation	Indicator equal to one if the student graduates from high school by the calendar year implied by normal grade progression from fourth grade, and zero otherwise	Binary outcome; coefficients are reported in percentage points
Takes university admission exam	DEMRE	At the end of the expected high-school trajectory	Indicator equal to one if the student appears in the relevant national university admission-exam process and takes both the reading and mathematics exams, and zero otherwise	Binary outcome; coefficients are reported in percentage points
Average university admission-exam score	DEMRE	Conditional on taking the exam	Average of the reading and mathematics admission-exam scores used in the analysis, standardized using the native test-taker distribution in the corresponding admission process	Standard deviations of the native test-taker distribution
Higher-education enrollment	Ministry of Education	Year following expected high-school graduation	Indicator equal to one if the student enrolls in any higher-education program in the year immediately following expected high-school graduation, and zero otherwise	Binary outcome; coefficients are reported in percentage points
School inputs	Enrollment and teacher records	School/cohort/year of fourth-grade exposure	Cohort size, average class size, number of teachers, teacher–student ratio, and newly hired teachers, measured at the school-cohort or school-year level	Natural units
Teacher quality	Teacher administrative and admission-exam records	School/year of fourth-grade exposure	Teacher admission-exam scores, certification status, university graduation, and years of experience	Natural units or binary indicators, depending on the outcome
School environment and teacher practices	SIMCE background questionnaires	Fourth grade	Survey-based measures of classroom disruptions, school discipline, peer relations, respect between students and teachers, and instructional or classroom-management practices	Binary indicators or original survey scales, oriented so that higher values indicate more favorable environments or practices

Notes: The table describes the construction of the main outcome and mechanism variables used in the analysis. Test-score outcomes are standardized using the relevant native-student distribution. Binary outcomes are multiplied by 100 when estimated, so coefficients are interpreted in percentage points. Survey-based measures are coded so that higher values correspond to more favorable school environments or more supportive teacher practices. The exact estimation sample varies across outcomes depending on data availability and the cohorts that can be linked to each outcome horizon.

C Validity and Interpretation of the IV

This appendix section provides empirical support for the identifying assumptions discussed in Section 3.2. We organize the evidence in five blocks that parallel the main text: immigrant sorting, interpretation of exposure persistence, first-stage strength, validity checks for the independence, exclusion, and monotonicity assumptions, and characterization of the complier population underlying our IV estimates.

C.1 Immigrant Sorting

This subsection provides descriptive evidence on non-random sorting of immigrant students across schools, which motivates the empirical strategy in the main text. To do so, we compare observable characteristics of schools attended by immigrant and native students within the same cohort. Specifically, we estimate:

$$Y_{ist} = \alpha_0 + \alpha_1 I_i + \phi_t + \varepsilon_{ist}, \tag{3}$$

where Y_{ist} denotes a school or student characteristic, I_i is an indicator for immigrant status, and ϕ_t are cohort fixed effects. The coefficient α_1 captures average differences in characteristics between immigrant and native students within the same cohort.

Consistent with the discussion in the main text, immigrant students are more likely to enroll in public schools and in schools with lower parental education, and they exhibit lower test scores on average. Figure C.I summarizes these systematic differences by immigration status. Immigrant students are 22.4 percentage points more likely to attend public schools, while they are 20.5 percentage points less likely to attend voucher schools and 1.9 percentage points less likely to attend private schools. They also attend schools with lower average academic performance, with gaps of 0.212 standard deviations in Spanish and 0.188 standard deviations in Math. In terms of family background, immigrant students are more likely to attend schools where mothers have less than high school education, and less likely to attend schools where mothers have higher education. Specifically, the difference is 5.8 percentage points for mothers with less than high school, close to zero for complete high school, and -5.5 percentage points for higher education. Finally, parental expectations display a different pattern: immigrant students are 3.2 percentage points less likely to have parents who expect at most high school completion and 3.2 percentage

points more likely to have parents who expect higher education for their child.

These patterns indicate that immigrant exposure is not randomly assigned across school environments, reinforcing the need for the identification strategy developed in the paper.

C.2 Interpretation

Figure C.II shows that immigrant shares are highly correlated across grades within a given school. The correlation between first- and fourth-grade exposure is close to one, and remains strong when comparing first grade with later grades such as eighth and tenth. This high degree of persistence reflects the fact that immigrant inflows into schools tend to evolve gradually rather than abruptly.

This persistence has a direct implication for the interpretation of our estimates. Because immigrant shares are highly correlated across grades, exposure in fourth grade serves as a proxy for exposure throughout the student's school trajectory. In practice, immigrant peers observed in fourth grade are likely to remain in the same cohort in subsequent grades, so estimated effects should be interpreted as the impact of sustained exposure to immigrant classmates rather than a one-time or transitory shock.

C.3 First stage

Figure C.III illustrates the first stage by plotting predicted versus actual immigrant exposure after partialling out fixed effects. The predicted exposure is constructed using the immigrant composition of the eldest sibling's school cohort, which serves as the source of exogenous variation in our instrumental variable strategy. The figure shows a strong positive relationship between predicted and actual exposure, indicating that variation in the instrument translates into meaningful differences in realized peer composition.

The estimated coefficient π_1 is approximately 0.28, implying that a one standard deviation increase in predicted exposure is associated with a 0.28 standard deviation increase in actual exposure. The Kleibergen–Paap rk Wald F-statistic exceeds 600, indicating a very strong first stage.

C.4 Independence and Exclusion restriction

In our setting, the exclusion restriction requires that—conditional on the fixed effects included in specification (1)—the share of immigrants that native students *would have faced* in their eldest sibling's school affects their outcomes only through the share of immigrants they actually face.

We assess the plausibility of this assumption through three complementary exercises that probe different potential violations of the exclusion restriction, summarized in Figure C.IV. For the first two exercises, we estimate reduced-form specifications of the form:

$$Y_{ist} = \delta_0 + \delta_1 Z_{s'(i)t} + \mu_{f(i)} + \phi_t + \varepsilon_{ist}, \tag{4}$$

where Y_{ist} denotes the outcome of interest (e.g., native peer characteristics or pre-trends in test scores), and $Z_{s'(i)t}$ is the instrument based on the share of immigrants that the student would have faced in their eldest sibling's school. The coefficient δ_1 captures whether the instrument is systematically related to alternative channels that could violate the exclusion restriction.

Panel (a) examines whether the instrument predicts changes in the composition of native peers at the eldest sibling's school. Specifically, we estimate reduced-form regressions in which predetermined characteristics of native students—such as parental education and socioeconomic composition—are regressed on the instrument. The estimates show no systematic relationship, indicating that variation in predicted immigrant exposure is not associated with shifts in native peer characteristics at the school attended by the eldest sibling, which alleviates concerns that the instrument captures changes in the composition of native peers rather than immigrant exposure.

Panel (b) evaluates whether the instrument is correlated with pre-existing trends in school quality. We test this by regressing changes in average test scores at the eldest sibling's school in the years prior to the entry of the cohorts under study on the instrument. The absence of differential pre-trends in test scores suggests that schools with higher predicted immigrant exposure were not on systematically different achievement trajectories prior to the arrival of younger siblings, supporting the view that the instrument is not proxying for underlying school quality trends.

Finally, Panel (c) explores whether the estimated effects reflect broader neighborhood-

level immigration patterns rather than school-level exposure. For this exercise, we estimate the IV specification in equation (1) separately for two subsamples: students whose families lived in the same municipality as the school at the time the eldest sibling began schooling, and students whose families lived in a different municipality. Despite differences in residential proximity, estimated effects are similar across the two groups, suggesting that our results are driven by exposure within schools rather than by correlated changes in neighborhood composition.

Overall, these results provide no evidence that the instrument is correlated with alternative channels that could directly affect outcomes, and show that the IV estimates are not driven by students residing in the same municipality as the school. This supports the plausibility of the exclusion restriction.

C.5 Monotonicity

To interpret the IV estimates as the local average treatment effect (LATE), our instrument must satisfy the monotonicity assumption: an increase in the immigrant share that a student would have faced in their eldest sibling's school should weakly increase the immigrant share they actually experience, and a decrease should weakly reduce it.

We provide evidence in support of this assumption through a series of complementary exercises summarized in Figure C.V. The figure jointly examines whether predicted exposure translates into actual exposure in a stable way, whether families respond through school switching, and whether higher predicted exposure is associated with compensatory school choices.

Panel (a) shows that the first-stage coefficient remains remarkably stable across a wide range of subsamples defined by observable characteristics, including school dependence, gender, maternal education, and parental educational expectations. The absence of substantial heterogeneity in the first stage suggests that increases in predicted exposure translate into increases in actual exposure in a consistent manner across groups, which is a necessary condition for monotonicity.

Panel (b) examines whether families respond to higher potential immigrant exposure by systematically changing schools. Specifically, we estimate reduced-form regressions in which the outcome is an indicator for whether the younger sibling attends the same school as the eldest sibling. The relationship is essentially flat, indicating that the likelihood

of remaining in the same school does not vary meaningfully with predicted immigrant exposure. This finding rules out selective school switching as a primary channel through which monotonicity could be violated.

Panel (c)–(e) examines whether monotonicity could be violated through compensatory school choices, whereby families facing higher immigrant exposure might move younger siblings to systematically higher- or lower-quality schools. Panel (c) shows that the instrument does not predict changes in native peer characteristics at the fourth-grade school, indicating that exposure to immigrants is not associated with shifts in the composition of native classmates. Panel (d) shows no relationship between the instrument and baseline test scores at the fourth-grade school, suggesting that families are not sorting into schools with systematically different prior achievement levels. Panel (e) shows that the instrument does not predict differential test score trends at the fourth-grade school prior to exposure, ruling out selective movement toward schools on distinct achievement trajectories.

These results suggest that higher predicted immigrant exposure does not induce systematic school switching toward better or worse environments. Instead, variation in the instrument translates smoothly into variation in actual exposure, supporting the monotonicity assumption required for a LATE interpretation of our IV estimates.

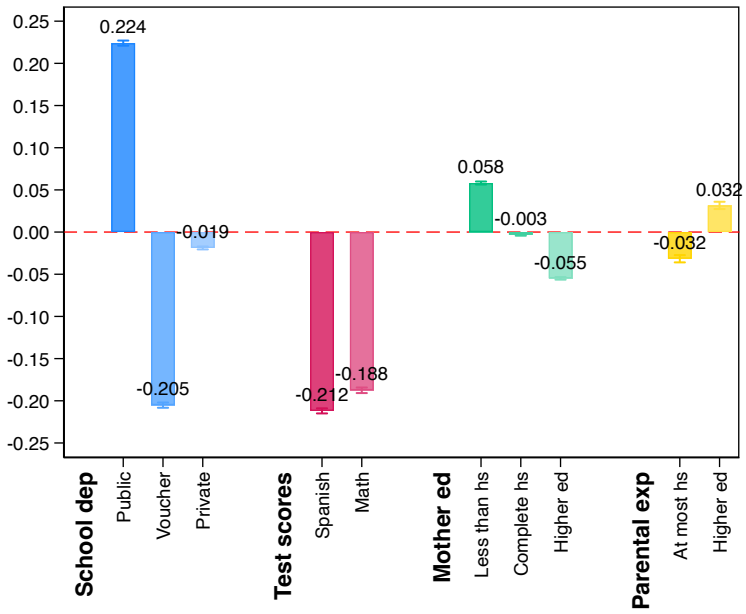
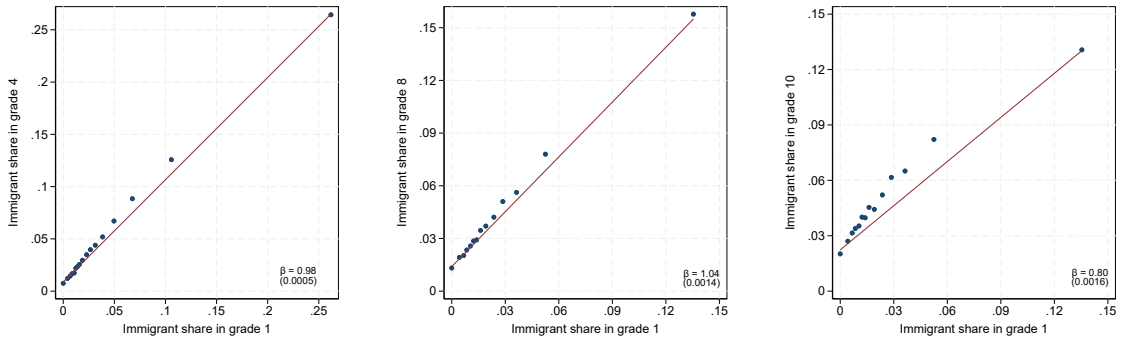


Figure C.I: Immigrant sorting

Notes: The figure presents differences in school and student characteristics between immigrant and native students. Each bar corresponds to the coefficient $\hat{\alpha}_1$ from equation 3, where the outcome is regressed on an indicator for immigrant status and cohort fixed effects. Outcomes include school dependence, average test scores, maternal education, and parental expectations. Coefficients capture average differences between immigrant and native students within the same cohort and are expressed in percentage points for binary outcomes and standard deviations for standardized test scores. Vertical lines indicate 95% confidence intervals, with standard errors robust to heteroskedasticity.



(a) 1st vs 4th grade (b) 1st vs 8th grade (c) 1st vs 10th grade

Figure C.II: Immigrant shares correlation

Notes: Each panel plots the correlation between immigrant shares measured in 1st grade and later grades within the same school. Panels (a), (b), and (c) compare immigrant shares in 1st grade with those in 4th, 8th, and 10th grade, respectively. Immigrant shares are computed using administrative enrollment records covering the universe of K–12 students. The strong correlations across grades indicate substantial persistence in immigrant exposure over time.

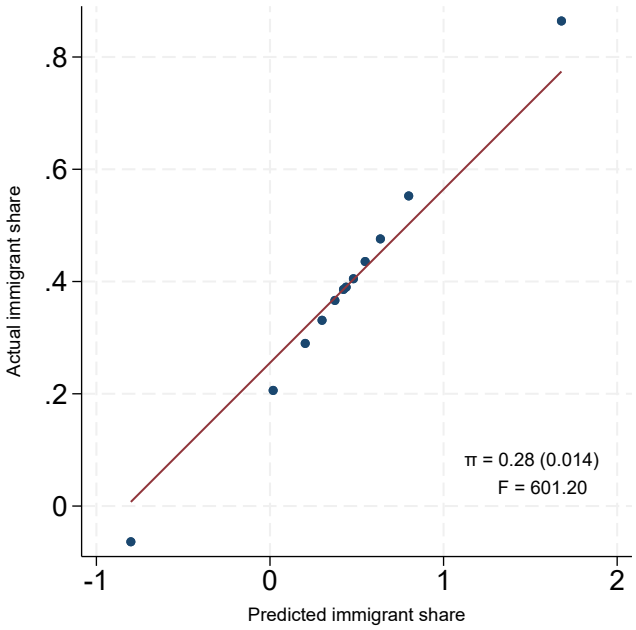
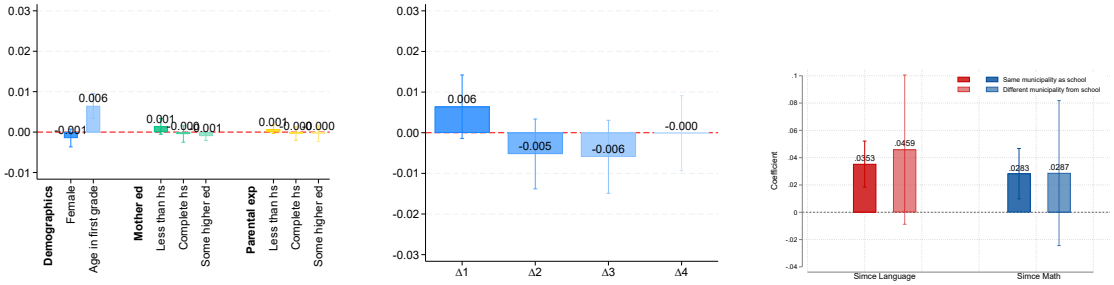


Figure C.III: Predicted versus actual exposure to immigrants

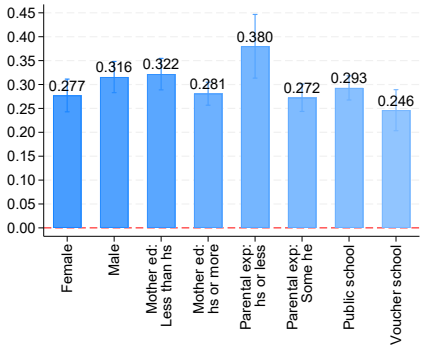
Notes: The figure plots binned averages of actual immigrant exposure against predicted exposure from the instrumental variable after partialling out mother and year fixed effects. Each point represents the average within a bin of the predicted exposure variable. The solid line shows the fitted linear relationship. π denotes the estimated first-stage coefficient, with clustered standard errors in parentheses, and F denotes the Kleibergen–Paap rk Wald F-statistic from the corresponding IV specification.



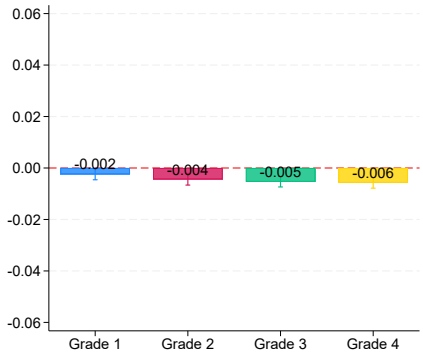
(a) Native peer characteristics (b) Test score trends (c) Distance to school

Figure C.IV: Assessing the exclusion restriction

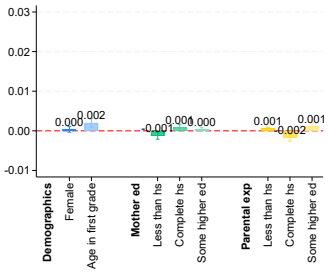
Notes: The figure presents three exercises assessing the plausibility of the exclusion restriction. Panels (a) and (b) report reduced-form estimates from regressions of the form in equation 4, where outcomes are regressed on the instrument based on the share of immigrants that students would have faced in their eldest sibling's school. Panel (a) examines whether the instrument predicts predetermined characteristics of native peers at the eldest sibling's school. Panel (b) tests whether the instrument is associated with pre-existing trends in school test scores prior to the entry of the cohorts under study. Panel (c) reports IV estimates from equation 1, estimated separately for students whose families lived in the same municipality as the school and for those whose families lived in a different municipality when the eldest sibling began schooling. The first two panels therefore capture whether the instrument is correlated with alternative channels that could directly affect outcomes, while the third panel examines whether the estimated effects are driven by broader neighborhood-level immigration patterns rather than school-level exposure. Vertical lines indicate 95% confidence intervals, with standard errors clustered at the eldest sibling's school level



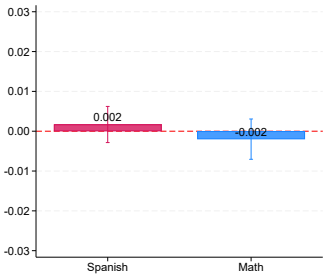
(a) First-stage across subsamples



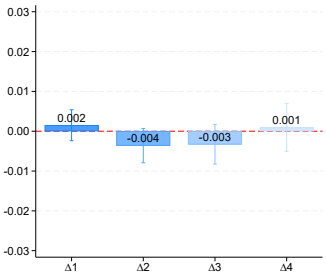
(b) Probability of attending eldest sibling's school



(c) Native peer characteristics



(d) Baseline academic performance



(e) Pre-existing test score trends

Figure C.V: Monotonicity checks

Notes: This figure presents evidence on monotonicity by examining how predicted immigrant exposure translates into actual exposure and school attendance decisions, and whether predicted exposure is associated with compensatory school choices. Panel (a) reports first-stage coefficients from regressions of actual immigrant exposure on predicted exposure, estimated separately across subsamples defined by observable characteristics (e.g., mother's education and school dependence), including year, school, and mother fixed effects. Panel (b) reports reduced-form estimates from regressions of an indicator for whether the younger sibling attends the same school as the eldest sibling on predicted immigrant exposure. Panel (c) examines native peer characteristics at the younger sibling's school, Panel (d) reports baseline test scores, and Panel (e) tests for pre-existing test-score trends. Across panels, the absence of systematic relationships supports the monotonicity assumption. Vertical lines indicate 95% confidence intervals, with standard errors clustered at the eldest sibling's school level.

C.6 Compliers

An additional concern that arises in the context of IV estimates is that they might reflect a causal effect for a narrow or atypical subpopulation of *compliers*. To address this issue, we explicitly characterize the population of compliers underlying our IV estimates. We follow the approach proposed by Hull (2025), which provides a transparent way to describe the effective population contributing to design-based estimands in settings with continuous treatments and instruments. The method consists of interacting predetermined characteristics with treatment intensity and re-estimating the IV specification using them as outcomes. This approach allows us to characterize the complier population along a rich

vector of baseline characteristics without imposing parametric assumptions.

Figure C.VI compares baseline characteristics across three groups: the complier population of our IV approach, the subset of students with at least one sibling reaching fourth grade in the period we study—i.e., the sample we use in specifications with mother fixed effects—, and the overall population of fourth grade native students—i.e., the sample behind our naive estimates. The most salient differences indicate that compliers are more socioeconomically disadvantaged than the full native population.

The clearest gaps are in school background. Compliers are substantially more likely to come from families whose eldest sibling started in a public school (85% versus 45% among other native students) and in a low-SES school (55% versus 38%). In line with this pattern, compliers are more likely to have mothers who did not complete high school (31% versus 23%) and less likely to have mothers with higher education (15% versus 27%).[7]

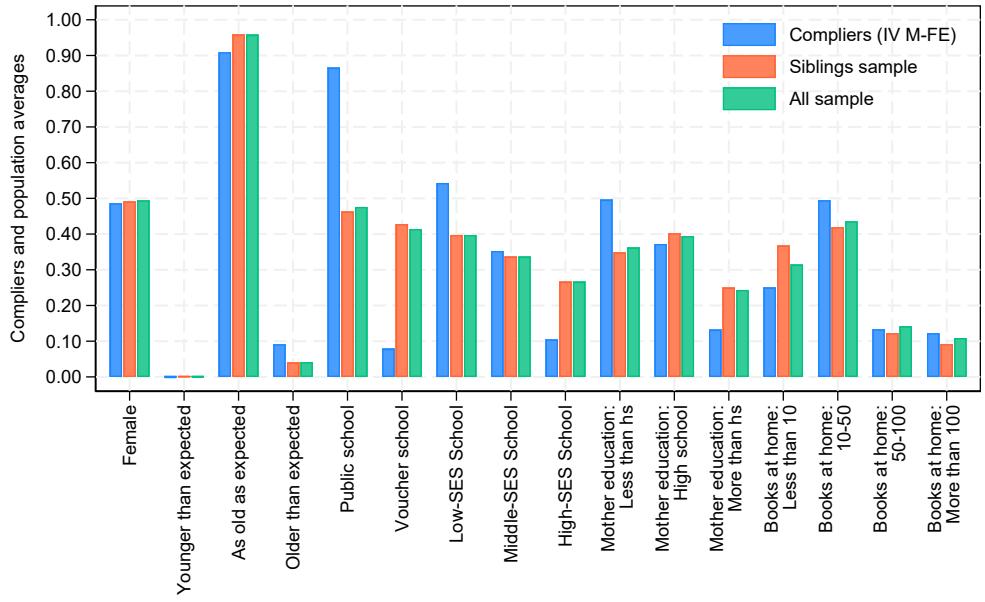


Figure C.VI: Compliers

Notes: The figure compares baseline characteristics across three groups: the complier population implied by the IV–MFE specification (blue bars), the native sibling sample used for estimation (orange bars), and the full population of native fourth-grade students (green bars). Compliers are characterized following Hull (2025), who recovers weighted averages of baseline characteristics for the complier population without imposing parametric assumptions. All characteristics are binary indicators, so the bars can be interpreted as proportions. Age categories indicate whether the student reached fourth grade younger than expected, at the expected age, or older than expected. School type and school socioeconomic status refer to the eldest sibling's school. Maternal education and books at home measure household background.

[7] Online Appendix Figure F.II reports an additional check based on fixed-effects estimates computed in complier-like subsamples. The pattern remains stable across these groups, suggesting that the IV magnitudes are not mechanically driven by sample composition.

D Additional Results

This appendix reports additional results that complement the main findings in Section 4. It focuses on heterogeneity by gender and on whether the main IV estimates remain stable when the full specification sequence is re-estimated in the same restrictive sample used by the IV–MFE specification. A related robustness check based on complier-like subsamples is reported in Appendix G.

D.1 Heterogeneity by gender

We next assess whether the effect of immigrant exposure differs by student gender using a single interacted IV specification estimated on the baseline IV sample. Following the notation of our main IV design, we estimate:

$$Y_{ist} = \beta_0 + \beta_1 I_{ist} + \beta_2 \left(F_i \times I_{ist}\right) + \mu_{m(i)} + \phi_t + \phi_t \times F_i + \varepsilon_{ist}, \tag{5}$$

where F_i is an indicator for female students, $\mu_{m(i)}$ are mother fixed effects, and $\phi_t \times F_i$ are year-by-gender fixed effects. The terms I_{ist} and $F_i \times I_{ist}$ are instrumented with $I_{s'(i)t}$ and $F_i \times I_{s'(i)t}$, respectively. This setup allows treatment effects to differ by gender while preserving a common estimation sample and identification strategy.

Figure D.I presents the corresponding estimates. The evidence indicates that effects are very similar across genders in reading. In mathematics, point estimates appear more different, with larger effects for male than female students; however, confidence intervals overlap, so we cannot reject equality of effects across genders at conventional significance levels. Overall, this evidence indicates that the benefits of exposure to immigrant peers are broadly shared and are not driven by gender-specific mechanisms. These findings are consistent with the interpretation that immigrant peers affect general aspects of the learning environment, such as classroom dynamics, behavior, or motivation, rather than channels that would differentially benefit one gender over the other.

D.2 Specification Sequence Under a Common IV Sample

Table D.I addresses the sample-composition concern raised in Section 4. In the main table, specifications with year and school fixed effects are estimated in larger samples than IV–MFE, since mother fixed effects require families with at least two children and IV–MFE

additionally requires families with at least three children to construct the instrument. This difference in estimation samples raises the question of whether larger IV coefficients could be mechanically driven by sample selection.

To address this concern, we re-estimate the full sequence of specifications (naive OLS, school FE, school+mother FE, and IV–MFE) using the same restrictive sample used by IV–MFE. Panels A and B report language and mathematics, respectively, and the table also reports reduced-form and first-stage coefficients and first-stage strength. The pattern matches the one in the main text: naive estimates are negative, fixed-effects estimates become positive and smaller, and IV–MFE remains the largest. This confirms that the larger IV estimates are not driven by differences in sample composition.

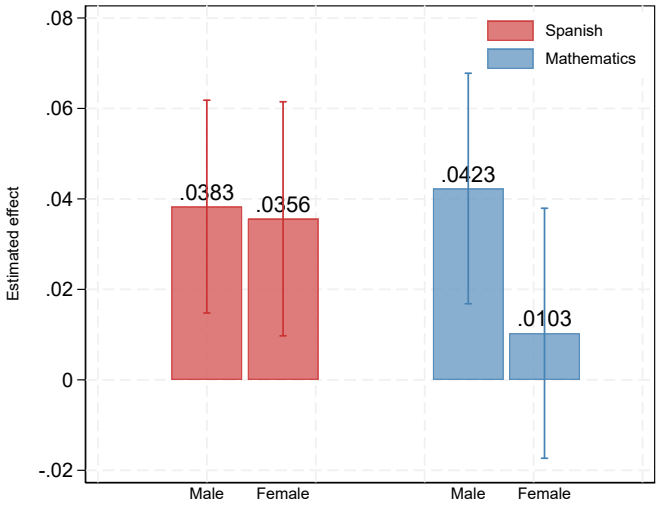


Figure D.I: Heterogeneity by gender

Notes: The figure reports gender-specific effects from an interacted IV–MFE specification estimated in the baseline IV sample. The regression includes immigrant exposure and its interaction with a female indicator, instrumented with the corresponding instrument and interaction. The coefficient for males corresponds to the baseline immigrant-exposure effect, while the coefficient for females equals the sum of the baseline and interaction coefficients. All models include mother fixed effects, year fixed effects, and year-by-gender fixed effects. Standard errors are clustered at the eldest sibling's school level; vertical lines indicate 95% confidence intervals.

	NAIVE	School FE	School+Mother FE	IV MFE
	(1)	(2)	(3)	(4)
		Panel A: SIMCE Language		
Share of Immigrants (σ)	-0.0260***	0.0134***	0.0121***	0.0376***
	(0.0045)	(0.0028)	(0.0038)	(0.0082)
Reduced form				0.0107***
				(0.0024)
First stage				0.2839***
				(0.0139)
Observations	260906	260444	260052	260907
Outcome mean	-0.0751	-0.0745	-0.0742	-0.0751
Kleibergen–Paap F				601.20
		Panel B: SIMCE Mathematics		
Share of Immigrants (σ)	-0.0271***	0.0078**	0.0078*	0.0327***
	(0.0052)	(0.0031)	(0.0043)	(0.0091)
Reduced form				0.0092***
				(0.0026)
First stage				0.2818***
				(0.0142)
Observations	261464	261000	260600	261465
Outcome mean	-0.0301	-0.0291	-0.0286	-0.0301
Kleibergen–Paap F				619.23
Year FE	X	X	X	X
School FE		X	X	
Mother FE			X	X

Notes: Each cell reports coefficients; standard errors in parentheses. All columns are estimated on the restrictive sample used by IV–MFE. Columns (1)–(3) report OLS estimates. Column (4) reports 2SLS estimates in which gives the coefficient on the instrument in the outcome equation and gives the coefficient on the instrument in the first-stage equation. The treatment variable is the immigrant share standardized to have unit standard deviation, approximately 3.1 percentage points, or roughly one additional immigrant student per classroom. Outcome variables are standardized relative to the native student population in each year. Standard errors are clustered at the eldest sibling's school level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table D.I: Effects of Immigrant Exposure on Test Scores

E Drivers

This appendix section complements the mechanisms analysis in Section 5 by providing additional details on immigrant peer characteristics and on the alternative IV design used in the mechanism exercises. We first document how immigrant students differ from native students in aggregate and within school cohorts. We then present the alternative specification in full, characterize the associated complier population relative to the baseline IV–MFE design, and use the same framework to benchmark the heterogeneity results against exposure to native peers with different levels of motivation and grit.

E.1 Characterizing Immigrant Peers

This appendix section provides additional details on the descriptive exercises used in Section 5.1. The goal is to document how immigrant students differ from native students along academic, demographic, cognitive, and non-cognitive dimensions, and to distinguish aggregate differences from within-school-cohort differences.

We begin by comparing immigrant students with the overall student population. For each outcome, we estimate:

$$Y_{it} = \beta_0 + \beta_1 I_i + \mu_t + \varepsilon_{it}, \tag{6}$$

where Y_{it} denotes an academic, demographic, cognitive, or non-cognitive characteristic of student i in year t, I_i is an indicator equal to one for immigrant students, and μ_t denotes year fixed effects. The coefficient β_1 captures the average difference between immigrant and native students within the same year. These estimates correspond to the aggregate comparisons reported in the top row of Figure 3.

This aggregate comparison is informative about how immigrant students differ from the overall population of students. However, it does not necessarily describe the peer environment experienced by native students, because immigrant students are not randomly distributed across schools. If immigrant students are disproportionately enrolled in schools with different observable or unobservable characteristics, aggregate comparisons may combine differences between immigrant and native students with differences across schools.

To isolate differences between immigrant and native students within the same educa-

tional environment, we also estimate specifications that compare students enrolled in the same school and year:

$$Y_{ist} = \beta_0 + \beta_1 I_i + \mu_s \times \mu_t + \varepsilon_{ist}, \tag{7}$$

where $\mu_s \times \mu_t$ denotes school-by-year fixed effects. These fixed effects absorb all differences across schools and all cohort-specific shocks within schools. As a result, β_1 is identified from comparisons between immigrant and native students who attend the same school in the same year. These estimates correspond to the within-school-cohort comparisons reported in the bottom row of Figure 3.

The contrast between equations (6) and (7) is useful for interpretation. The first specification describes how immigrant students compare with the overall student population. The second specification describes how immigrant students compare with their native classmates. This distinction is important because peer effects operate within schools and cohorts: the relevant comparison for native students is not necessarily the average immigrant student in the national student population, but rather the immigrant students with whom they share the school environment.

The academic and demographic outcomes include standardized test scores in Spanish and mathematics, age in fourth grade, gender, mother's education, and parental educational expectations. Test-score outcomes are standardized, so coefficients are expressed in standard deviation units. Age is measured in years. Binary outcomes, including gender, mother's education, and parental expectations, are expressed in percentage points.

We also examine cognitive and non-cognitive characteristics using student survey responses collected between 2009 and 2018. To make survey items comparable across questions, each variable is coded as a binary indicator equal to one when the student reports the most favorable category of the corresponding item, and zero otherwise. For items framed negatively, such as responses involving "never," the coding is reversed so that higher values always represent more favorable characteristics. This normalization allows all coefficients to be interpreted as percentage-point differences in the probability of reporting the most favorable response.

For exposition, the survey-based outcomes are grouped into two broad domains. The first domain captures dimensions related to conscientiousness, motivation, and persistence, which we interpret as measures related to grit. These variables include students'

reported effort, perseverance when facing difficult tasks, attitudes toward studying and reading, homework completion, and the importance assigned to grades. The second domain captures dimensions related to self-confidence and perceived learning ability, which we interpret as measures related to self-efficacy. These variables include students' self-assessed ability, perceived ease of learning, confidence in completing the school year, and expectations about achieving future goals.

The estimates show that aggregate and within-school comparisons differ substantially. In aggregate comparisons, immigrant students appear negatively selected along some academic dimensions, displaying lower test scores and older age at fourth grade. In contrast, within-school-cohort comparisons indicate that immigrant students are positively selected relative to their native classmates along several academic and survey-based dimensions. In particular, immigrant students exhibit more favorable non-cognitive characteristics across the full set of survey-based measures, especially along dimensions related to motivation, effort, perseverance, and self-efficacy. These patterns indicate that immigrant students may constitute particularly strong peers along dimensions that are likely to shape classroom dynamics and peer interactions, providing a plausible peer-based channel through which exposure to immigrant classmates may affect native students' academic outcomes.

E.2 Alternative IV Specification

This appendix subsection complements Section 5 by presenting the alternative IV specification used in the mechanism analyses. The goal of this specification is to increase statistical power for outcomes that are not available for all cohorts, including survey-based measures of peer composition, school climate, teaching practices, and school inputs.

The mechanism analysis combines administrative records with survey data from students, parents, and teachers collected by the Education Quality Agency. Although the main results document persistent effects on later academic outcomes, the mechanism exercises focus on outcomes measured closer to the period of exposure, which provide a more direct window into peer-composition, classroom, and school-resource channels. Because the set of survey questions varies over time, several of these outcomes are not consistently available for all cohorts. This reduces the effective sample size in mechanism-specific analyses.

To address this limitation, we implement a modified version of the baseline IV strategy.

The baseline IV–MFE specification uses families with at least three children and includes mother fixed effects. In contrast, the alternative specification replaces family fixed effects with fixed effects for the eldest sibling's school interacted with the year in which the eldest sibling enters school. This allows us to include families with two children, increasing the available sample size and improving power for mechanism outcomes.

For each mechanism outcome m, we estimate:

$$Y_{ist}^{m} = \beta_0^m + \beta_1^m I_{ist} + \mu_{s'(i)\times t} + \phi_t + \varepsilon_{ist}^m, \tag{8}$$

where Y_{ist}^m is a mechanism-specific outcome for native student i in school s and cohort t, I_{ist} is the immigrant share in student i's school-cohort, $\mu_{s'(i)\times t}$ are fixed effects for the eldest sibling's school interacted with the year in which the eldest sibling enters school, and ϕ_t denotes cohort fixed effects. As in Section 3.2, we instrument I_{ist} with immigrant exposure at the eldest sibling's school, $I_{s'(i)t}$. Unless otherwise stated, coefficients are interpreted as the effect of immigrant exposure on the corresponding mechanism outcome under this IV variation.

The corresponding first stage is:

$$I_{ist} = \pi_0 + \pi_1 I_{s'(i)t} + \mu_{s'(i)\times t} + \phi_t + \nu_{ist}. \tag{9}$$

Comparison with the baseline IV specification. Figure E.I compares the estimates obtained from the baseline IV–MFE specification with those obtained from the alternative IV design. The first bar reports the baseline IV–MFE estimate. The second bar estimates the alternative specification on the same sample used in the baseline IV–MFE specification. The third bar estimates the alternative specification on the full sample available under the two-sibling design.

The estimates from the alternative specification are very similar to the baseline IV–MFE estimates when both are estimated on the same sample. This indicates that the baseline results are not driven by the particular fixed-effect structure or by the sample restriction required by the mother fixed effects design. When the alternative specification is estimated on the expanded sample, the estimated effects remain positive and economically meaningful, although they are somewhat smaller. This attenuation is consistent with a change in the complier population induced by the expanded sample, as discussed below.

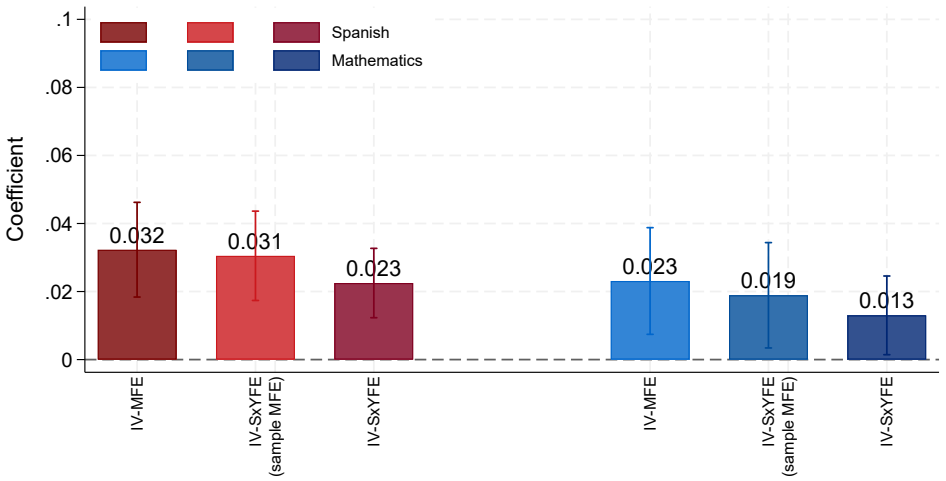


Figure E.I: Alternative IV specification

Notes: The figure reports IV estimates and corresponding confidence intervals across specifications. The first bar (IV–MFE) reports the baseline IV estimate. The second bar (IV–S×YFE, baseline IV–MFE sample) reports estimates from the alternative specification with eldest sibling's school-by-entry-year fixed effects, estimated on the baseline IV–MFE sample. The third bar (IV–S×YFE) reports estimates from the same specification using the full sample available under the alternative design. Standard errors are clustered at the eldest sibling's school level; vertical lines indicate 95% confidence intervals.

Complier comparability. Figure E.II characterizes the complier population implied by the alternative IV specification. The figure compares observable characteristics across three groups: the complier population implied by the alternative IV, the native sibling sample used for estimation, and the full population of native fourth-grade students.

The main message is that compliers under the alternative IV remain negatively selected relative to the full native population. They are more likely to attend public schools and low-socioeconomic-status schools and to come from less advantaged family backgrounds than the average native student. At the same time, comparing Figure E.II with the baseline complier profile in Figure C.VI suggests that the alternative-IV compliers are slightly less disadvantaged than those identified by the baseline IV–MFE design. Across the remaining observables, differences are generally modest and do not point to a qualitatively different complier group.

This evidence helps interpret the estimates in Figure E.I. Both IV strategies identify effects for broadly similar populations, but the alternative specification in the expanded sample shifts weight toward students who appear somewhat less disadvantaged than the baseline IV–MFE compliers. Since the main effects are larger among more disadvantaged

students, this shift in complier composition provides a plausible explanation for why the alternative-IV estimates in the full sample are somewhat attenuated while remaining positive.

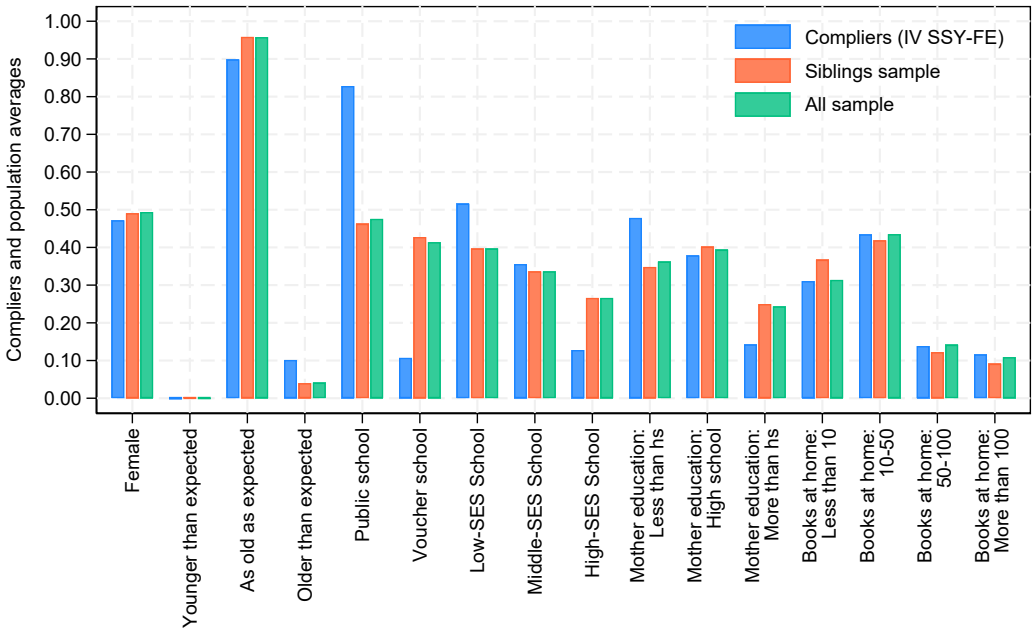


Figure E.II: Alternative-IV compliers

Notes: The figure compares baseline characteristics across three groups: the complier population implied by the alternative IV specification with eldest sibling's school-by-entry-year fixed effects, the native sibling sample used for estimation, and the full population of native fourth-grade students. Each characteristic is standardized to have mean zero and unit variance in the full native population. Compliers are characterized following Hull (2025), who recovers weighted averages of baseline characteristics for the complier population without imposing parametric assumptions.

E.3 Heterogeneous Peer Effects

We also use the alternative IV specification to examine whether the effects of immigrant exposure differ depending on the non-cognitive characteristics of immigrant peers. This exercise is motivated by the evidence in Section 5, which shows that immigrant students are positively selected relative to their native classmates along several dimensions related to motivation, effort, persistence, and attitudes toward learning.

We construct an index of academic motivation and grit using SIMCE student questionnaire items that capture effort, persistence, and attitudes toward studying and school engagement. For each cohort, we use the available standardized items that map to this domain. The index is constructed as the simple equal-weighted average of these items within each cohort, so that each component contributes equally to the overall measure. A higher

value of the index reflects stronger academic engagement, motivation, and perseverance.

We focus on this domain because the relevant survey questions are observed with enough regularity across cohorts to construct a comparable measure over time. We do not construct an analogous summary index for self-efficacy because the corresponding items do not appear with sufficient consistency across cohorts to deliver a stable measure.

Using the motivation-and-grit index, we classify immigrant peers in three steps. First, within each school cohort, we compute the median value of the motivation-and-grit index among native students. Second, we classify native classmates according to whether their index is at or above this median, or below it. Immigrants whose index is exactly equal to the native median are assigned to the above-or-equal group, except when the median corresponds to the lowest category and the immigrant also belongs to this lowest category; in those cases, immigrants are classified in the below-median group. Third, we construct two exposure variables: the share of immigrants above or equal to the native median and the share of immigrants below the native median.

We then estimate a version of equation (8) with two endogenous exposure variables:

$$Y_{ist} = \beta_0 + \beta_H I^H_{ist} + \beta_L I^L_{ist} + \mu_{s'(i)\times t} + \phi_t + \varepsilon_{ist}, \tag{10}$$

where I^H_{ist} denotes the share of immigrant peers whose motivation-and-grit index is above or equal to the school-cohort native median, and I^L_{ist} denotes the share of immigrant peers whose index is below the school-cohort native median.

Both exposure variables are instrumented using the corresponding predicted shares at the eldest sibling's school. The first stages are:

$$I^H_{ist} = \pi^H_0 + \pi^H_1 I^H_{s'(i)t} + \mu_{s'(i)\times t} + \phi_t + \nu^H_{ist}, \tag{11}$$

$$I^L_{ist} = \pi^L_0 + \pi^L_1 I^L_{s'(i)t} + \mu_{s'(i)\times t} + \phi_t + \nu^L_{ist}. \tag{12}$$

We use the same sample restrictions, fixed-effect structure, and clustering level as in the alternative mechanism specification.

The coefficients β_H and β_L capture the marginal effects of exposure to immigrant peers with high versus low motivation-and-grit profiles. This design asks whether the academic effects of immigrant exposure are driven by immigrant classmates who are more positively

selected on non-cognitive traits. Figure 4 reports these estimates in the main text.

E.4 Peer Effects Using Native Peers

Figure E.III provides an additional benchmark for interpreting the magnitude and nature of peer effects by examining exposure to native peers with different levels of motivation and grit. The construction of the motivation-and-grit index and the above/below-median classification follows the same procedure used for immigrant peers in the main analysis. The heterogeneity design below therefore asks whether effects are stronger when native peers are more positively selected on these traits.

Using this index, we proceed in three steps. First, within each school cohort we compute the median value of the motivation-and-grit index among native students. Second, we classify native classmates according to whether their index is at or above the native median, or below it. Natives with an index exactly equal to the native median are assigned to the above-or-equal group, except in cases where the median corresponds to the lowest category (as variables are categorical) and the native also belongs to this lowest category; in these cases, natives are classified in the below group. Third, instead of using only the total native share, we define two exposure variables: the share of natives at or above the median and the share of natives below the median.

We instrument each of these two shares using the same intuition as in our baseline design (applied separately to the high-index and low-index native components), and estimate the following specification:

$$Y_{ist} = \beta_0 + \beta_H N^H_{ist} + \beta_L N^L_{ist} + \mu_{s'(i)\times t} + \phi_t + \varepsilon_{ist}, \tag{13}$$

where N^H_{ist} and N^L_{ist} denote the shares of natives at or above, and below, the native median in student i's school-cohort. We instrument these endogenous variables with the corresponding predicted shares at the eldest sibling's school, $N^H_{s'(i)t}$ and $N^L_{s'(i)t}$, using:

$$N^H_{ist} = \pi^H_0 + \pi^H_1 N^H_{s'(i)t} + \mu_{s'(i)\times t} + \phi_t + \nu^H_{ist}, \tag{14}$$

$$N^L_{ist} = \pi^L_0 + \pi^L_1 N^L_{s'(i)t} + \mu_{s'(i)\times t} + \phi_t + \nu^L_{ist}. \tag{15}$$

We use the same sample restrictions, fixed effects structure, and clustering choice as in

the alternative mechanism specification.

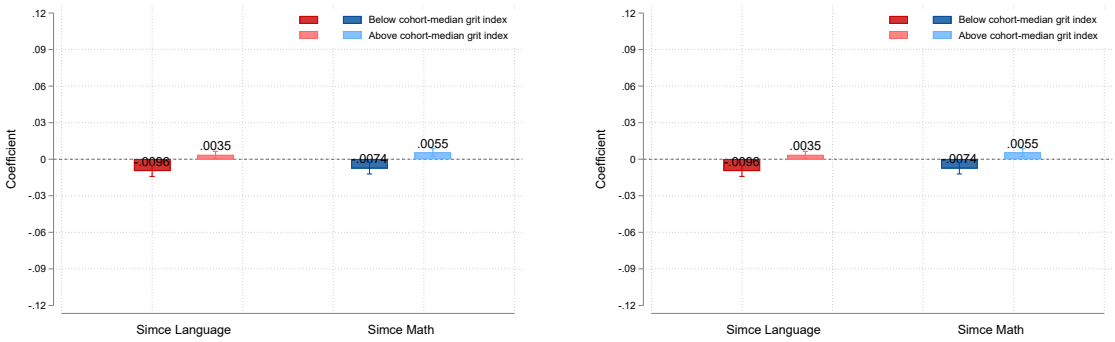
The coefficients β_H and β_L are interpreted as the marginal effects of exposure to native peers with high versus low motivation-and-grit profiles, respectively.

The purpose of this exercise is to assess whether the heterogeneity patterns documented for immigrant peers reflect a broader mechanism related to peer quality rather than immigrant status per se. If the effects are driven by non-cognitive traits such as motivation and perseverance, similar patterns should emerge when considering native peers with comparable characteristics.

Panel (a) reports effects on fourth-grade test scores, while Panel (b) reports effects on eighth- or tenth-grade test scores. In both panels, exposure to high-motivation native peers is associated with positive effects on academic achievement, whereas exposure to low-motivation peers yields effects that are small, negative, or statistically indistinguishable from zero.

The qualitative similarity between these patterns and those observed for immigrant peers is consistent with a peer-quality mechanism operating through the behavioral and motivational composition of classmates. At the same time, the magnitude of native-peer effects is substantially smaller than that of immigrant-peer effects. This suggests that motivation and perseverance explain an important share of peer influence, but do not fully account for the larger effects associated with immigrant exposure.

These results indicate that the main effects are not solely driven by exposure to high-effort peers in general, but also by characteristics that are more strongly associated with immigrant peers. The persistence of the pattern in later grades further suggests that these peer influences are not transitory and contribute to sustained improvements in academic trajectories.



(a) Fourth-grade test scores

(b) Eighth- or tenth-grade test scores

Figure E.III: Effects of exposure to native peers by grit index

Notes: Each figure reports IV estimates of the effects of exposure to high-motivation, high-grit native peers (dark bars) and low-motivation, low-grit native peers (light bars) on other native students' test scores. The motivation-and-grit index and the above/below-median classification follow the same construction used for immigrants in the main analysis. The purpose of this exercise is to benchmark the magnitude of immigrant peer effects against those of native peers with similar non-cognitive characteristics. Panel (a) reports effects on fourth-grade SIMCE language and mathematics scores; Panel (b) reports effects on eighth- or tenth-grade scores. Effects are scaled to a one-standard-deviation increase in the relevant peer share and measured in standard deviations of the test score distribution. Standard errors are clustered at the eldest sibling's school level; vertical lines indicate 95% confidence intervals.

F Robustness Checks

This appendix section presents robustness checks that complement the main identification strategy and interpretation of the IV estimates. We focus on two questions. First, we test whether the estimated effects are specific to contemporaneous peer exposure by implementing placebo exercises with immigrant shares from adjacent cohorts. Second, we assess whether the magnitude of the IV results could be mechanically driven by complier composition by re-estimating fixed-effects models on complier-like subsamples.

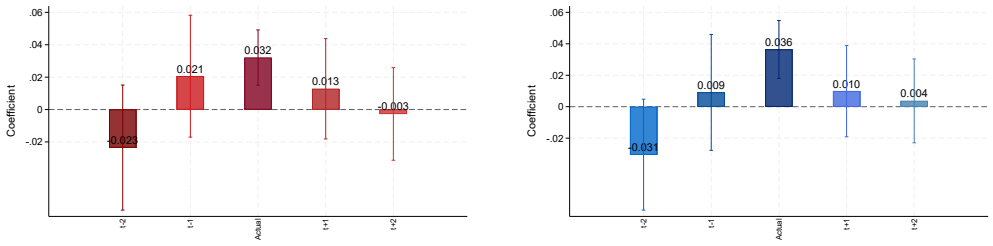
F.1 Placebo Tests Using Adjacent Cohorts

While persistence informs how exposure should be interpreted over time, we next examine whether the estimated effects are driven specifically by exposure to immigrant peers in the student's own cohort, rather than by broader school-level factors that evolve over time. To this end, we estimate placebo specifications using immigrant exposure in adjacent cohorts.

This placebo design asks whether immigrant exposure in nearby cohorts predicts outcomes once own-cohort exposure is accounted for.

If the estimated effects were capturing persistent differences across schools or general trends in immigrant composition, similar coefficients should arise when using immigrant shares from nearby cohorts. In contrast, if the effect reflects within-cohort peer interactions, only contemporaneous exposure should matter.

Figure F.I shows that coefficients for $t-2$ and $t-1$ are small and statistically indistinguishable from zero in both subjects, while the contemporaneous effect remains positive and significant. Placebo estimates using future cohorts ($t+1$ and $t+2$) are also close to zero. This pattern indicates that the estimated effects are driven by exposure to immigrant peers within the student's own cohort, rather than by school-level shocks or persistent differences across cohorts within the same school. Overall, the placebo profile supports an interpretation based on cohort-specific peer interactions rather than slowly evolving school-level confounders.



(a) Spanish Test Scores

(b) Mathematics Test Scores

Figure F.I: Placebo Estimates Using Two Adjacent Cohorts Before and After

Notes: This figure presents placebo estimates using immigrant exposure in nearby cohorts. The variables $t-2$ and $t-1$ correspond to the immigrant share in fourth grade two years before and one year before the student's own cohort, respectively. The variables $t+1$ and $t+2$ correspond to the immigrant share in fourth grade one year after and two years after the student's own cohort, respectively. The central bar corresponds to the immigrant share in the student's own fourth-grade cohort. Standard errors are clustered at the eldest sibling's school level; vertical lines indicate 95% confidence intervals.

F.2 Complier Composition vs. Selection Correction

A natural concern in IV settings is that estimated effects may be driven by the specific characteristics of the complier population rather than reflecting a general causal relationship. To assess this possibility, we estimate our baseline fixed-effects specification on subsamples designed to approximate the characteristics of likely compliers.

Figure F.II reports estimates for four alternative samples: (i) students attending low-SES schools; (ii) students attending low-SES and public schools; (iii) students whose mothers have a high school degree or less; and (iv) students satisfying both conditions. These groups capture populations that are more likely to be exposed to variation in immigrant peer composition and therefore to be relevant for the IV estimation.

As a robustness check, we re-estimate the mother fixed-effects specification on subsamples that more closely resemble the IV complier population. If the larger IV estimates were mainly driven by complier composition, these fixed-effects estimates should be close in magnitude to the IV results. Figure F.II shows that this is not the case: estimates in complier-like subsamples are positive but substantially smaller than the IV estimates. This pattern indicates that the IV magnitudes are not mechanically explained by sample composition; rather, they reflect the correction of selection bias that remains even after conditioning on mother fixed effects.

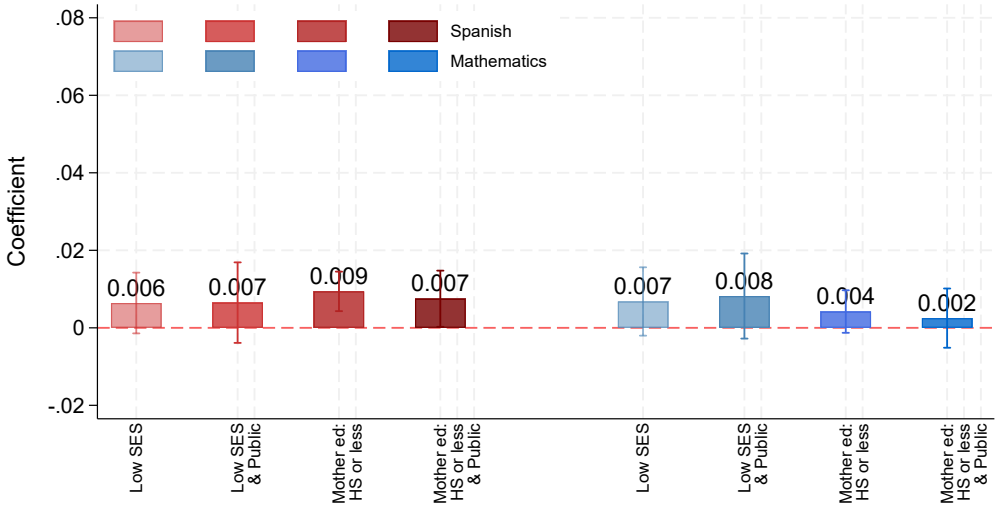


Figure F.II: Mother-FE Estimates in Complier-Like Subsamples

Notes: This figure reports estimates from a fixed-effects specification that includes year, school, and mother fixed effects. The model is estimated on subsamples designed to approximate the characteristics of IV compliers: (i) students attending low-SES schools only; (ii) students attending low-SES and public schools; (iii) students whose mothers have a high school degree or less; and (iv) students whose mothers have a high school degree or less and who attend public schools. Standard errors are clustered at the eldest sibling's school level; vertical lines indicate 95% confidence intervals.