When Schools Are Full: Capacity Constraints and the Absorption of a Migration Shock*

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Abstract

Between 2017 and 2019 Peru's primary schools absorbed 46,000 Venezuelan children, four fifths of them in ten cities, into a public system whose capacity is fixed a year ahead by teaching posts allocated at thirty pupils per section. We use the universe of enrollment records and school censuses to ask how a school system with no slack absorbs an unanticipated demand shock and how the families already in it respond. Public schools near the inflow grew, and they made room almost entirely by enlarging sections: nine tenths of the additional pupils sat in existing sections, the share of sections above the norm rose by seven percentage points in the most exposed cities, and teaching posts remained unchanged within the three years. Private schools did not respond on any margin, neither fees, nor posts, nor sections, and lost a few pupils. Families in exposed grades moved to other schools, toward schools with fewer migrants, and a full school did not slow them down. Capacity mattered on a different margin. Full public schools that received migrants held back fewer pupils and increased dropouts. We write down a model of school choice with a public sector that allocates posts by rule and rations seats, and a private sector with fixed capacity, whose primitives are the resourcing rate and the rationing rule we estimate, and we set out the counterfactuals it will answer: full compliance with the staffing norm, resources that follow pupils, vouchers into a private sector at capacity, and the placement of migrant children across schools with room.

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1 Introduction

School systems plan a year ahead. Teaching posts, sections and buildings for a given school year are set in the previous one, on the basis of the children the system expects. When more children arrive than were expected, a school has few options: it can put them in the sections it already has, it can turn them away, or it can wait for the posts that the rules say it is owed. Which of these happens, and what it does to the children already in the school, depends on how capacity is set and on who is allowed to say no. These questions are old in the economics of education, where class-size rules and the sorting they induce have been studied at length (Angrist and Lavy, 1999; Urquiola and Verhoogen, 2009; Lazear, 2001), but they have rarely been observed in the presence of a shock large enough to test them, and almost never in a system where public schools can neither refuse a child by law nor create a section without a post.

In this paper we study how Peru's primary schools absorbed the Venezuelan migration. Between 2017 and 2019 the country received close to a million Venezuelans, and its primary schools enrolled 46,000 Venezuelan children, four fifths of them in ten cities. The inflow was sudden, spatially concentrated, and landed on public schools that were already full. We use the universe of enrollment records, which follow every child in every licensed school from 2014 to 2022, and the annual school censuses, to measure three things: how schools made room, how the families already in them responded, and whether the response depended on whether the school had any room to make. We then write down a model in which families choose among schools that can fill up, public schools receive posts by rule and ration seats when the rule lags, and private schools set fees and capacity, and we set out the counterfactual policies the model is built to evaluate.

Four descriptive facts frame the analysis. The inflow was a big-city phenomenon: the ten largest urban markets enroll 38 percent of primary pupils and held between 82 and 88 percent of the Venezuelan pupils in every year. The schools that received it were full: in 2016, before the migrants arrived, the median urban public school ran at 75 percent of the thirty-pupils-per-section norm, one in five was at or above 95 percent, and those schools held 37 percent of urban public pupils. Sections got larger rather than more numerous: between 2017 and 2019 public enrollment in urban schools grew by 5.4 percent and the number of sections by 1.4, the share of sections above the norm rose from 21 to 27 percent, and the share of pupils sitting in such sections from 29 to 36 percent, while in private schools nothing moved. And full public schools turned families away: in the 2019 census one urban public school in four, and one in two in the ten largest markets, reported that some applicants did not obtain a place, almost always for lack of capacity, and whether a school refused was predicted by how full it had been in 2016 and not by how many migrants it had.

Motivated by these facts, we estimate the response of schools and of families to local exposure to the inflow, using the 2014 settlement pattern of the few hundred Venezuelan families then in Peru, combined with the national inflow, as the instrument, and comparing schools with themselves over time. A public school whose neighborhood gained one Venezuelan pupil per school enrolled 5.6 more pupils, or 25 in a typical big-city school, and it made room for nine tenths of them by enlarging its sections: pupils per section rose by 0.37 per unit of exposure and the share of sections above the norm by 1.6 percentage points, while sections rose by 0.02 and teaching posts by 0.03, an estimate not distinguishable from zero. At the point estimate, one post arrived for every 180 marginal pupils, against the one per thirty the norm implies. Private schools did not respond on any margin. Fees, teaching posts, sections and teacher credentials did not move within school, and enrollment fell slightly. The earlier reduced-form work on this episode, including our own, found large fee and staffing responses; we show that those were differences between the districts that received the migrants and those that did not, present before the migrants arrived, and that they vanish with school fixed effects.

Families moved. Ten more Venezuelan pupils in a child's grade at her previous school raised the probability that she was in a different school by 2 percentage points from a public school, on a base of 9 percent, and the induced moves went toward schools with fewer migrants, smaller sections and higher marks; families in private schools moved too, at a rate that is larger per unit of migrant share and smaller per migrant child, because private grades are a quarter the size. The moves were not toward richer or higher-scoring peers: by the socioeconomic index and the test scores of the destination, the induced moves are evenly split between up and down, the moves toward fewer migrants happen within socioeconomic strata, and families of every level of mother's education move toward fewer migrants at the same rate. Marks fell by about one hundredth of a standard deviation per ten points of share, test scores in the national assessment did not fall at all, at the school or at the pupil level, and failing and permanent exit did not change on average. Whether the school was full did not reduce switching: under every measure of fullness we try, families in full schools moved at least as much as families in schools with room. It made a difference to what happened to the children who stayed. In a full public school that received migrants, the probability of failing the year fell by 0.6 percentage points per ten points of share, on a base of 3.6 percent, and the probability that the year was the child's last in the registered system rose by 0.5 points, on a base of 0.8 percent; in private schools neither moved. The extra exits are children who passed, not children who failed; they are age-appropriate, more often girls, and more often weak pupils; they are not children who reappear under a new identifier; and they are not more frequent where the neighboring schools were full too. A full public school that receives migrants does not select who enters through the families that leave, since capacity leaves that margin untouched; it changes who stays, by holding back fewer of the weak and by losing some of them.

The model of Section 8 organizes these findings. Households choose among schools valuing quality, price, distance, crowding and the composition of the school; public schools enroll everyone up to a tolerance above the norm and ration beyond it, receive posts the following year at a rate we estimate, and lose quality as sections grow; private schools have fixed capacity and fees in the short run and choose whom to admit. The estimated primitives are the household preferences, the crowding penalty in the quality technology, the resourcing rate, and the rationing rule, and the last two are read directly from the reduced form. The counterfactuals ask what the same inflow would have cost incumbent children under full compliance with the staffing norm, under resources that follow pupils, under vouchers into a private sector that is itself at capacity, and under a rule that places migrant children in schools with room. The structural estimates are not yet in this draft; Section 9 sets out the identification and the tables they will fill.

This paper builds on a rich prior literature. Dinerstein et al. (2023) is possibly our closest predecessor: they estimate an equilibrium model of school choice in the Dominican Republic to evaluate a public school construction program, and they maintain that public capacity does not bind. Our setting is one in which it does, and our contribution is to measure what binding looks like, in the sections, the posts and the children. Urquiola and Verhoogen (2009) show that class-size caps induce schools and families to sort around them; Peru has a norm rather than a cap, and we show that a norm without a splitting rule absorbs a demand shock by letting sections grow past it. Allende (2019), Neilson (2013) and Bau (2022) provide the demand and supply framework we adapt, and Ryan (2012) the logic of estimating a constraint from the behavior it induces rather than from its cost. Allende et al. (2026) is the closest antecedent for the counterfactual exercise. They scale an experimental demand shift through a model of Chilean primary schools in which capacities enter through a centralized assignment and private schools re-optimize quality, and they find that congestion at popular schools removes two fifths of the demand-side gain while the quality response of schools puts it back. Peru differs in the two respects that matter for that arithmetic: there is no assignment mechanism, so a full school rations at the door and, as we show, in what it does with the pupils who stay; and the shock we study is not a demand shift toward better schools but an inflow into schools chosen by where migrants could live. Their finding that capacity classified at 95 percent utilization changes a school's incentives is also the reason we treat fullness at that threshold as a regime rather than a covariate. The literature on immigration and native schooling (Betts and Fairlie, 2003; Cascio and Lewis, 2012; Figlio and Özek, 2019; Hunt, 2017; Tumen, 2019) has asked whether native families leave when immigrant children arrive; we find that they do, at rates that capacity does not reduce, and we add the margin that capacity does govern. The reaction of a destination to the people who arrive is the subject of Derenoncourt (2022), who shows that the northern cities that received the Great Migration changed in ways that lowered what growing up in them delivers to Black children two generations later: incumbents withdrew from shared neighborhoods and public schools, and local governments spent more on police and no more on schools. We measure the same reactions in the first three years of a shock, in the schools where they begin, and we find the school-level counterpart of both: families left the exposed grades, and the posts that the rule ties to enrollment did not arrive. Our shift-share design follows Goldsmith-Pinkham et al. (2020) and Borusyak et al. (2022), and our treatment of the school-level data corrects two features of earlier work on this episode: a retention variable that counted only failures in the recovery period, and an exposure measure whose denominator moved with the families that left.

The rest of the paper proceeds as follows. Section 2 describes the inflow and the rules that set capacity in Peruvian primary schools. Section 3 describes the data and the measures. Section 4 presents the four descriptive facts. Section 5 lays out the empirical strategy. Sections 6 and 7 present the school-side and family-side results. Section 8 presents the model, Section 9 its estimation and the counterfactuals, and Section 10 concludes.

2 The Context

2.1 The Venezuelan inflow

The collapse of Venezuela's economy produced the largest displacement episode in the recent history of Latin America. Peru became the second destination after Colombia. Fewer than ten thousand Venezuelans lived in Peru in 2015; by 2019 the stock exceeded 860,000, and by 2021 it was close to 1.3 million, about four percent of the country's population (R4V, 2021). The inflow accelerated in 2017, when Peru created a temporary residence permit (*Permiso Temporal de Permanencia*) that gave Venezuelans legal residence for a year and access to formal work and public services, including schooling for their children. Entry requirements tightened in 2018 and 2019, but the stock kept growing until the pandemic closed the border.

Three features of the inflow matter for our purposes. First, it was sudden and unanticipated by the school system. School budgets and teaching posts for a given school year are set in the previous year, and the migrants who arrived between 2017 and 2019 were not in any of the enrollment projections that those budgets were based on. Second, it was spatially concentrated. Migrants settled overwhelmingly in Lima and in a handful of coastal cities, where labor markets are largest, and within those cities in the districts where Venezuelans had already settled before the crisis (INEI, 2019). Third, it brought children. Peru registered the first Venezuelan pupils in its school system in 2014, when they numbered fewer than a thousand. By 2019 there were about 46,000 Venezuelan children in primary school alone, and more than four in five of them sat in schools in the ten largest urban markets. Section 4 documents this concentration with our data.

2.2 Primary schooling in Peru

Peru enrolls about 3.5 million children in six grades of primary education, in roughly 38,000 schools. Three quarters of the children attend public schools, which charge no tuition. The remaining quarter attend private schools, which range from small fee-charging establishments in poor urban neighborhoods to elite institutions, and which set their own fees and admission rules. Public and private schools operate side by side in every city, and the private sector is largest precisely where the migrants settled: in Lima, close to half of primary pupils attend a private school.

School choice is not tied to residence. Families may apply to any school with an available place. Public schools give priority to siblings and to children who live nearby, but there is no catchment-area assignment, and once a child is enrolled, re-enrollment in the same school for the next grade is automatic. Transfers between schools require a place in the destination school and an administrative transfer between the two institutions, both of which are recorded in the Ministry of Education's student information system (SIAGIE). Switching schools is common: about one primary pupil in ten changes school from one year to the next, outside of any migration episode. Public schools are obliged to accept any child who requests a place, subject to available places; private schools may not use entrance examinations, but they choose whom to admit.

Venezuelan children were entitled to enroll in public schools from the start of the episode. In 2017 the Ministry instructed schools to enroll migrant children even when their documentation was incomplete, and to place them in the grade corresponding to their age or to a placement test. In practice, migrant families applied to the schools near where they lived and were placed where there were seats. Because they arrived throughout the year and mostly in already dense neighborhoods, the inflow hit particular schools rather than the system as a whole.

2.3 How capacity is set: sections, posts, and places

The unit of capacity in a Peruvian primary school is the section (*sección*): a group of pupils of the same grade taught by one teacher in one classroom for the whole year. Public schools do not choose their number of sections freely. The Ministry's teacher-post rationalization norms define a reference number of pupils per section, and the number of teaching posts a school is entitled to follows from it at one teacher per section. During our estimation window the operative norms were RSG N° 1825-2014-MINEDU, in force from October 2014 to December 2018 and hence for the 2017 and 2018 school years, and RM N° 721-2018-MINEDU, which governed 2019. Both set the same reference: 30 pupils per section in urban and 25 in rural fully staffed (*polidocente completo*) primary schools, with a tolerance of five pupils in either direction "depending on classroom size or duly justified reasons."[1]

Two features of these rules are central to what follows. First, the reference is a target for allocating posts, not a cap on enrollment. No norm requires a school to open a new section when a section exceeds 30 pupils, and none prevents it from enrolling a thirty-first pupil. There is no analogue of the Maimonides rule that mechanically creates sections at multiples of a threshold (Angrist and Lavy, 1999; Urquiola and Verhoogen, 2009). A new section requires a funded post, which the school must request from its local education authority (UGEL) in the rationalization process of the previous year. In the short run, therefore, a public school that receives more pupils than it planned for has two options: enlarge its sections, or turn applicants away.

Second, the two sectors ration differently. A public school is obliged in principle to enroll any child, but the enrollment norm in force in 2019 (RM N° 665-2018-MINEDU) has schools publish the number of places per classroom before the enrollment period and admit up to that number. When the places are gone, the school reports that there are no vacancies, and the family looks elsewhere. A private school faces the same physical constraint but also chooses whom to admit and what to charge. In the 2019 school census, one public primary school in four in urban areas reported having turned applicants away, almost all of them for lack of capacity; in the ten largest markets it was one in two (Section 4).

These rules define the constraint set that the migration shock ran into. Capacity in the year of the shock was the number of staffed sections, fixed by decisions taken before the migrants arrived. Whether a school enlarged its sections, added a section, or refused entry is the supply response we study in Section 6. How families in schools that were already full

[1] RSG N° 1825-2014-MINEDU, numeral 6.5.2 d), and RM N° 721-2018-MINEDU, numeral 10.19, Cuadro 07, both with note (**). The norms are reproduced in Appendix E. Multigrade schools have a reference of 25 (urban) and 20 (rural), and single-teacher rural schools 20. The rationalization norms are the instrument through which the Ministry reallocates funded posts across schools each year; enrollment per section is verified in SIAGIE (RM 721-2018, numeral 10.21).

reacted, and what happened to the children who could not stay, is the demand response we study in Section 7.

3 Data

We build every dataset in the paper from two administrative sources of the Peruvian Ministry of Education, both covering the universe of schools and pupils: the student information system SIAGIE (*Sistema de Información de Apoyo a la Gestión Educativa*) and the annual school census (*Censo Escolar*) with its school roster (*Padrón de Instituciones Educativas*). We describe what we take from each, how we measure capacity, exposure and the family outcomes, and the samples. Appendix D gives the construction in detail and reconciles our counts with published figures.

3.1 Sources

SIAGIE records every enrollment in every licensed school in Peru, public and private, since 2011. For each pupil and year it reports the school, the grade, the section (the classroom group), sex, date of birth, nationality, the end-of-year result (passed, failed, withdrew) and the report-card marks by subject. A pupil identifier links the records over time and across schools. We use the 2014–2022 files, 62 million pupil-years, to build a register of every child in initial, primary and secondary education; the primary rows for 2014–2019 are the family-side analysis file. Because the identifier follows the child through the whole registered system, we observe whether a child who leaves a school reappears in another school anywhere in the country in any later year through 2022.

The school census is an annual questionnaire completed by every school. From the roster we take the school's sector (public or private), whether it is urban, its type (single-teacher, multigrade or fully staffed), the shifts it runs, the district, the building it occupies and, for 2016, 2018 and 2019, its coordinates. From the teacher module we take the number of teaching posts by position and by tenure, and the share of teachers with a pedagogical degree. From the building module we take the number of classrooms. From the 2019 census we take two questions that were asked only that year and that are central to our argument: whether every family that requested a place obtained one and, if not, why. A third source is the national student assessment (ECE), a standardized test in reading and mathematics with pupil identifiers that link to SIAGIE: a census of second graders in 2014–2016 and of fourth graders in 2016 and 2018, and samples in 2019. We use it for test-score outcomes, for school value added, and, through the socioeconomic index the Ministry computes for the tested pupils in 2018, for a school-level measure of socioeconomic status. The family questionnaire that accompanies the test reports the mother's and father's education and household assets for 1.3 million tested pupils, which we link to the register through the pupil identifier. From the Ministry's cost survey of private schools we take monthly fees and enrollment fees for 2016–2019.

3.2 Capacity

Our measure of capacity is the number of staffed sections, counted in SIAGIE as the number of distinct sections with at least one enrolled pupil in each grade of each school and year.[2] Pupils per section is enrollment divided by that count. Utilization in 2016, the last pre-shock year, is

$$u_{j,2016} = \frac{\text{enrollment}_{j,2016}}{30 \times \text{sections}_{j,2016}},$$

the ratio of enrollment to what the school could hold at the norm. A school is full ("low slack") if $u_{j,2016} \geq 0.95$. We also use the share of sections above 30 pupils and the share of pupils sitting in such sections.

3.3 Exposure

Venezuelan pupils are identified by the nationality field in SIAGIE. For the school-side analysis, exposure is the number of Venezuelan primary pupils per school among the other primary schools within three miles of the school, using the roster coordinates; we also use one, five and ten miles. The shift-share instrument multiplies the neighborhood's share of the country's Venezuelan pupils in 2014 by the national count in year t. For the family-side analysis, exposure is the number of Venezuelan pupils in the current year in the pupil's grade at the school she attended the year before, divided by the size of that grade at that school the year before. The denominator is predetermined on purpose: if it were the current size of the grade, every incumbent who left for any reason would raise the share mechanically, and Section 7 shows that this matters.

[2]Section labels are free text. We normalize them by removing accents, spaces and punctuation and by mapping the single-section labels ("Única", "U", "Sección única") to one value. Appendix D shows that the resulting counts match the teacher counts in the census: in urban fully staffed public schools, enrollment regressed on sections has an R^2 of [0.93] and a slope close to the norm, while enrollment regressed on classrooms has an R^2 of [0.56], and one school in four runs more sections than it has classrooms by using two shifts. Sections, not rooms, are what constrain enrollment.

3.4 Family outcomes

A pupil switches school in year t if her school in t differs from her school in $t-1$, within primary education; the variable is defined for pupils observed in primary in both years, hence for grades two to six. Where a switcher goes is described by the characteristics of the destination school relative to the origin school, both measured in $t-1$ so that the move itself cannot change them: the Venezuelan share, pupils per section, the sector, the average standardized mark of the school's pupils and, for private schools, the fee. A pupil fails the year if her end-of-year result is "failed" and not reversed in the recovery period. She repeats if she is in the same grade in $t+1$. She exits the registered system permanently in year t if t is her last year in the 2014–2022 register in any school and any level; because a child who leaves and later re-enrolls does so under the same identifier, this is a stronger notion than dropping out of a school, and we verify in Section 7 that it is not an artifact of identifier changes. Because the pandemic year 2020 removed a large number of children from the register, we measure permanent exit for 2017 and 2018 and report 2019 separately. Report-card marks in mathematics and language are letter grades that we convert to a numeric scale and standardize within grade and year on the distribution of non-Venezuelan pupils. Age is measured at 31 March of the school year; a pupil is over-age if she is two or more years above the modal age for her grade.

3.5 Samples

The analysis is restricted to primary education. The school-side sample is every primary school in the roster and in SIAGIE in 2015–2019; the family-side sample is every non-Venezuelan primary pupil in 2017–2019 with a baseline school in the previous year. We report results for all of Peru and for three subsamples: urban fully staffed schools, which are the schools the class-size norm applies to and which enroll [X] percent of urban pupils; the ten largest urban markets, defined as Lima in four subregions, Callao, Arequipa, Trujillo, Cusco, Huancayo and Ica, which comprise 94 districts; and the intersection of the two. Table 1 describes the schools in 2016, before the shock, by sector; Section 4 describes the shock.

4 Four Facts About Capacity Under Stress

Before estimating anything, we document four facts about where the inflow landed and what it met. Each is a simple tabulation of the administrative data, and together they fix the setting for the estimates that follow: the shock was concentrated in the largest cities, it hit schools that were already full, those schools absorbed it by enlarging sections rather than

Table 1: Primary schools in 2016, before the inflow

	Urban fully staffed		All primary schools	
	Public	Private	Public	Private
Schools	5,965	6,346	29,388	8,515
Enrollment	318.1	132.4	89.5	107.6
Sections	13.0	7.1	6.9	6.7
Pupils per section	22.2	16.2	8.8	13.7
Share of sections above 30	0.154	0.061	0.032	0.046
Share of pupils in sections above 30	0.175	0.072	0.037	0.055
Teaching posts	14.2	8.0	4.3	6.7
Classroom teachers	13.1	7.4	4.0	6.2
Classrooms (building)	15.2	13.0	5.7	11.5
Utilization (enrollment / 30 x sections)	0.739	0.540	0.292	0.456
25th percentile	0.572	0.333	0.089	0.217
median	0.752	0.538	0.172	0.422
75th percentile	0.911	0.733	0.406	0.667
Share of schools full (utilization $\geq$ 0.95)	0.190	0.063	0.039	0.047
Share of pupils in full schools	0.370	0.183	0.268	0.169
Share running an afternoon shift	0.276	0.081	0.067	0.092
Share with any Venezuelan pupil	0.082	0.091	0.017	0.073
Monthly fee, private (2019 soles)	–	191.4	–	172.5

Notes: Means across primary schools (school code by campus) observed in the 2016 school roster and in SIAGIE. Sections are counted in SIAGIE as classroom groups with at least one enrolled pupil; teaching posts and classrooms are from the 2016 school census; classrooms are counted at the building. Utilization is 2016 primary enrollment divided by thirty times the number of sections; a school is full if its utilization is at least 0.95. Urban fully staffed schools are those the roster classifies as urban *polidocente completo*, the schools to which the 30-pupil norm applies. The monthly fee is the private-school fee from the Ministry's cost survey, in 2019 soles.

adding them, and when they could not, they turned families away.

Fact 1. The inflow is a big-city phenomenon. Table 2 splits primary schools into three groups: the ten largest urban markets, other urban areas, and rural areas. The ten markets enroll 38 percent of primary pupils. In every year from 2014 to 2019 they held between 82 and 88 percent of the Venezuelan pupils in the country. By 2019 the Venezuelan share of primary enrollment was 2.7 percent in the ten markets, 0.5 percent in other urban areas and 0.03 percent in rural areas, and more than half of the primary schools in the ten markets had at least one Venezuelan pupil, against one in five elsewhere in urban Peru. Within the ten markets, the inflow went to the public sector: in urban fully staffed public schools the Venezuelan share reached 4.2 percent and 85 percent of schools had a Venezuelan pupil. Figure 1 shows the time path. Two implications follow. Any national estimate of the effect of the inflow is identified almost entirely from the comparison of these 94 districts with the rest of the country, which is why we report the ten-market results throughout. And the 2014 settlement shares that our instrument uses were already concentrated in the same places: the average school in the ten markets had 45 Venezuelan pupils within three miles in 2014, against one in other urban areas.

Table 2: Where the Venezuelan pupils are: primary education by market group, 2016–2019

Year	Group	Share of national enrollment	Venezuelan pupils	Venezuelan share of enrollment	Share of national Venezuelan pupils	Share of schools with any Venezuelan
2016	10 markets	0.365	1,586	0.12%	0.867	0.131
	Other urban	0.447	242	0.02%	0.132	0.022
	Rural	0.188	2	0.00%	0.001	0.000
2017	10 markets	0.370	3,313	0.25%	0.883	0.214
	Other urban	0.448	432	0.03%	0.115	0.037
	Rural	0.182	8	0.00%	0.002	0.000
2018	10 markets	0.374	14,992	1.11%	0.878	0.408
	Other urban	0.451	2,058	0.13%	0.120	0.102
	Rural	0.175	32	0.01%	0.002	0.001
2019	10 markets	0.379	37,902	2.71%	0.824	0.551
	Other urban	0.453	7,954	0.48%	0.173	0.198
	Rural	0.168	166	0.03%	0.004	0.004

Notes: All primary schools. The ten markets are Lima (four subregions), Callao, Arequipa, Trujillo, Cusco, Huancayo and Ica, 94 districts; other urban and rural follow the roster's classification of the school. Venezuelan pupils are identified by the nationality field in SIAGIE.

Fact 2. The schools that received the migrants were already full. Table 1 describes primary schools in 2016, the last year before the inflow. Urban fully staffed public schools had on average 318 pupils in 13 sections, or 22 pupils per section, against 16 in private schools. That average hides a wide dispersion. Utilization, enrollment divided by what the school could hold at 30 pupils per section, has a median of 0.75 in public schools and an upper quartile of 0.91; 19 percent of urban fully staffed public schools were at or above 0.95, and because full schools are the large ones, 37 percent of the pupils in that sector sat in a full school. Fifteen percent of public sections were already above the 30-pupil norm, holding 17 percent of pupils. Private schools had more room: a median utilization of 0.54, six percent of schools full, six percent of sections above 30. The public sector also runs its buildings harder: 28 percent of urban fully staffed public schools run an afternoon shift, against 8 percent of private schools, and a public school has on average 13 classroom teachers for 15 classrooms in its building, which it often shares.[3]

Fact 3. Sections got larger, not more numerous. Table 3 follows all sections in urban fully staffed schools from 2014 to 2019. In public schools the number of pupils grew by 102,000 between 2017 and 2019, 5.4 percent, while the number of sections grew by 1,100, 1.4 percent. Mean section size rose from 24.4 to 25.3, the median from 25 to 27, and the share of sections above 30 from 21 to 27 percent. The share of pupils sitting in a section above 30 rose from 29 to 36 percent. The mass at exactly 30 pupils, the norm, also grew, from 5.7 to 6.8 percent of sections. Private schools moved the other way: pupils and sections both grew by about one percent, mean section size stayed at 18.5, and the share of sections above 30 fell from 8.7 to 8.0 percent. Figure 2 shows the section-size distribution in 2016 and 2019 by sector: in public schools the whole distribution shifted right, with a bulge at and just above 30; in private schools it did not move. This is the shape the institutional rules predict. A public school that receives more pupils than it has posts for enlarges its sections, because it cannot open a section without a post and cannot refuse a child who has a legal claim to a seat as long as it has one; a private school at capacity does not enroll the marginal child.

Fact 4. Full public schools turned families away, and fullness predates the shock. The 2019 school census asked every principal whether all the families that had requested a place in the enrollment period obtained one. Table 4 reports the answers. One urban fully staffed public school in four said no, and 92 percent of those cited a lack of capacity; in the ten markets, one in two said no. Private schools refused less often, 16 percent, and gave a different mix of reasons: 80 percent no capacity, 19 percent a preference for a low teaching

[3]Sections, not classrooms, are the binding unit. Among urban fully staffed public schools that are alone in their building, enrollment regressed on the number of sections has an R^2 of 0.93 and a slope of 31.6 pupils per section; regressed on the number of classrooms it has an R^2 of 0.41, and conditional on sections the coefficient on classrooms is 0.7 pupils (Figure 5). A school with more sections than classrooms runs two shifts.

Table 3: Sections and section sizes, urban fully staffed primary schools, 2014–2019

	Sections	Pupils	Pupils per section			Share of sections		Share of pupils
		(thousands)	mean	median	p90	= 30	> 30	in sections > 30
Public schools								
2014	76,363	1836	24.0	24	34	0.044	0.212	0.304
2016	77,657	1897	24.4	25	34	0.053	0.218	0.304
2017	78,014	1902	24.4	25	33	0.057	0.211	0.293
2018	78,426	1940	24.7	26	34	0.062	0.235	0.322
2019	79,093	2004	25.3	27	34	0.068	0.270	0.361
Private schools								
2014	42,750	800	18.7	19	31	0.025	0.108	0.202
2016	45,314	840	18.5	19	30	0.025	0.093	0.174
2017	45,334	839	18.5	19	30	0.026	0.087	0.161
2018	45,555	845	18.6	19	30	0.027	0.080	0.148
2019	45,688	849	18.6	19	30	0.026	0.080	0.147

Notes: All primary sections in schools classified as urban *polidocente completo* in the 2016 roster, by sector. A section is a classroom group with at least one enrolled pupil in SIAGIE. The norm is 30 pupils per section with a tolerance of five.

load, the answer that signals selection rather than a constraint. Refusal is a function of how full the school was before the migrants arrived. Among public schools with 2016 utilization below 0.75, 7 percent refused an applicant in 2019; between 0.75 and 0.95, 33 percent; between 0.95 and 1.10, 59 percent; above 1.10, 73 percent (Figure 4). Within a district, a one-unit increase in 2016 utilization raises the probability of refusing by 80 percentage points, while the school's own 2019 Venezuelan share has no additional predictive power, with a coefficient of 0.00 and a standard error of 0.23. The same pattern holds in private schools with a slope of 0.38. The obligation of public schools to accept every child is real on paper; a full public school says there are no vacancies, and the family looks elsewhere.

These four facts define the empirical question. The inflow was an unanticipated demand shock, concentrated on a set of public schools that had little slack to begin with, and those schools' first response was to let sections grow. What we want to know is how much of the inflow was absorbed by new sections and how much by larger ones, whether anything else about the schools changed, and how the families already in those schools responded when the school was full.

5 Empirical Strategy

We measure two responses to the same shock with two designs: how a school's inputs and organization change when Venezuelan pupils arrive in its neighborhood, and how the families already in a school react when Venezuelan pupils join their child's grade. The two designs

Table 4: Schools that turned applicants away in 2019

	Public		Private	
	Share refusing	Schools	Share refusing	Schools
A. By school type				
Urban fully staffed	0.255	6,051	0.155	6,171
Other primary schools	0.010	23,179	0.042	2,147
of which citing no capacity	0.925		0.804	
of which citing a preference for a low load	0.016		0.187	
B. Urban fully staffed schools, by market group				
10 markets	0.512	1,553	0.165	4,221
Other urban	0.166	4,498	0.135	1,950
C. Urban fully staffed schools, by 2016 utilization				
Below 0.75	0.069	2,983	0.105	4,287
0.75 to 0.95	0.327	1,881	0.265	1,021
0.95 to 1.10	0.594	876	0.445	265
1.10 or more	0.735	260	0.553	114
D. Refusal regressed on 2016 utilization and 2019 migrant share, district fixed effects				
2016 utilization	0.796***	(0.039)	0.377***	(0.027)
2019 Venezuelan share of enrollment	0.002	(0.229)	-0.052	(0.108)

Notes: The 2019 school census asked principals whether every family that requested a place in the enrollment period obtained one and, if not, why (no capacity; capacity but classrooms at risk; a preference for a low teaching load). Panels A–C report the share of primary schools answering that some applicants did not obtain a place, and the number of schools. Panel D reports coefficients from a regression of that indicator on the school's 2016 utilization and its 2019 Venezuelan share, with district fixed effects and standard errors clustered by district; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

share the source of variation, the timing and the samples, so that their answers can be read together. Throughout, the estimation window is the 2017–2019 school years, the years in which the inflow reached the schools, with 2014–2016 as the pre-shock baseline.

5.1 School responses

For school j in district d and year t we estimate

$$y_{jt} \;=\; \beta\, E_{jt} \;+\; \mu_j \;+\; \tau_t \;+\; \epsilon_{jt}, \tag{1}$$

where y_{jt} is an outcome such as enrollment, teaching posts, sections, pupils per section or fees, E_{jt} is local exposure, defined as the number of Venezuelan pupils per primary school among the other schools within three miles of j, and μ_j and τ_t are school and year effects.[4] The school effects absorb everything that is fixed about a school and its neighborhood, including the fact, documented in Section 4, that migrants settled in districts whose schools were already large, public and full. Identification comes from changes in exposure within a school over time.

Exposure is not randomly assigned even within a school: migrants may have moved toward neighborhoods where schools were expanding, or away from those that were closing. We instrument E_{jt} with a shift-share prediction that combines the neighborhood's share of the country's Venezuelan pupils in 2014, before the inflow, with the national count of Venezuelan pupils in year t:

$$Z_{jt} \;=\; \frac{V_{j,2014}}{V_{2014}} \times V_t, \tag{2}$$

where $V_{j,2014}$ is the number of Venezuelan pupils in the other schools within three miles of j in 2014 and V_t is the national total. The exclusion restriction is that, conditional on school and year effects, the 2014 settlement pattern of the few hundred Venezuelan families then in Peru predicts where the later inflow went but not where school inputs would otherwise have changed (Goldsmith-Pinkham et al., 2020; Borusyak et al., 2022). The logic is the one Card (2001) and Derenoncourt (2022) use for immigrant and Great Migration inflows: newcomers settle where earlier arrivals from their community settled, so pre-period settlement predicts the geography of a later inflow whose size is set elsewhere. The shares are the identifying variation here: there is a single national shift, so the design is a comparison of neighborhoods with high and low 2014 shares before and after the inflow, and the year effects absorb the shift itself. We show in Appendix B that the results are unchanged with a 2016 base, and

[4]We report results for radii of one, five and ten miles in Appendix B. Exposure is measured over the other schools in the neighborhood so that a school's own enrollment does not enter its treatment; the own-school migrant share is an outcome.

that the first stage is strong in every sample we use.

We report three specifications for every outcome: no fixed effects beyond year, district and year effects, and school and year effects. The comparison is informative. The earlier reduced-form work on this episode estimated (1) without school effects and found large responses of enrollment, teachers and private fees to exposure. Most of that variation is between districts: the districts that received migrants had larger schools, more teachers and higher fees before the migrants arrived. With school effects the coefficients on quantities fall by four fifths and the coefficient on fees goes to zero. We treat the school-effects estimates as the response to the shock and the others as a description of where the shock landed.

Standard errors are clustered by district, the level at which the 2014 settlement shares vary. We estimate (1) separately for public and private schools, since the two sectors face different constraints, and for four samples: all primary schools, urban fully staffed (*polidocente completo*) schools, which are the schools the class-size norm applies to and where the migrants are, the ten largest urban markets, and the intersection of the two.

5.2 Family responses

For a non-Venezuelan pupil i who attended school b in year $t-1$ and is in grade g in year t, we estimate

$$y_{it} \;=\; \delta\, M_{bgt} \;+\; X_{it}'\gamma \;+\; \eta_{bt} \;+\; \eta_g \;+\; \epsilon_{it}, \tag{3}$$

where M_{bgt} is the number of Venezuelan pupils in year t in grade g at school b, the cell the pupil would sit in by staying, divided by the number of pupils in that cell in year $t-1$. This is the family's exposure at the moment of decision, and it is assigned by the school she was in, not by the school she chose. The denominator is the cell's size before the migrants and before any incumbent left. With the current size in the denominator, as in earlier work on this episode, the share rises mechanically whenever incumbents leave, and the switching coefficient is three times larger (Appendix C). The outcomes are whether she switched school between $t-1$ and t, where she went, her end-of-year marks, whether she failed the year, and whether year t was her last year in the registered system. X_{it} contains age, sex and the pupil's standardized mark in $t-1$. The baseline-school-by-year effects η_{bt} absorb everything that happened to school b in year t, including the total inflow the school received and its supply response; the grade effects absorb the age profile of switching and failing. Identification comes from the fact that within a school and year, the Venezuelan pupils were not spread evenly across grades: a fourth-grade family in a school where migrants entered mostly in first and second grade is the comparison for a second-grade family in the same school. Standard errors are clustered by school and grade, the level of the treatment.

Two features of the design deserve comment. First, the treatment is a share on a zero-to-one scale, so a coefficient of δ means that raising the migrant share of the grade from zero to one hundred percent changes the outcome by δ. The shares that matter are much smaller: the median exposed cell has a share below five percent. We report effects per ten percentage points, $\delta/10$, and we show the response by bins of exposure to make sure the linear specification is not driven by the tail. Second, because the treatment is a cell-level variable and the fixed effects are school-by-year, the design cannot be run within a single grade; the within-school comparison across grades is the design.

To ask whether the response depends on capacity, we interact M_{bgt} with an indicator that school b was full before the shock, $F_b = \mathbb{1}[u_{b,2016} \geq 0.95]$, where $u_{b,2016}$ is enrollment in 2016 divided by thirty times the number of sections. The main effect of F_b is absorbed by η_{bt}; the coefficient on the interaction measures how much larger or smaller the response to the same exposure is in a school that had no room. We show that the answer does not depend on the cutoff, on using the utilization rate continuously, on a 25-pupil norm, or on measuring fullness as the share of sections above thirty.

5.3 Samples and the ten markets

The two designs run on the same schools. The family-side sample is every non-Venezuelan primary pupil in 2017–2019 with a baseline school in the previous year, which excludes first graders from the specifications that control for the lagged mark; Appendix C reports the versions that include them. The school-side sample is every primary school observed in the Census and in SIAGIE. Both are run on all of Peru and on the ten largest urban markets that the structural model of Section 8 uses. Section 4 shows that the ten markets hold more than four fifths of the Venezuelan pupils, so the national estimates are largely identified from them; the ten-market estimates are the ones the model is disciplined against.

6 How Schools Absorb the Inflow

This section asks what a school does when the neighborhood around it fills with migrant children. We estimate equation (1) for the quantities a school controls in the short run, enrollment, teaching posts, sections, pupils per section, the share of sections above the norm, shifts, and for the prices and inputs a private school controls, fees and teacher credentials. Table 5 reports the estimates with school and year fixed effects for urban fully staffed schools, public and private; Table 6 repeats them for the ten largest markets; and Table 21 and Figure 6 show what happens to the same coefficients as fixed effects are added. Appendix B has the full set of outcomes, samples and radii.

6.1 Scale

The coefficients are per unit of exposure, one Venezuelan pupil per neighboring primary school within three miles. Exposure was essentially zero before 2017. By 2019 the average urban fully staffed public school had 1.7 Venezuelan pupils per neighboring school, with a standard deviation of 2.6 and a 90th percentile of 6.5; in the ten largest markets the average was 4.6. The change in exposure between 2016 and 2019 in the ten markets is therefore about 4.5 units, and we use that figure to translate coefficients into what happened to a typical big-city school. The first stage is strong everywhere: the shift-share prediction explains exposure with a Kleibergen–Paap F above 200 in every sample and specification we report, and the coefficients on the headline outcomes are stable across radii of one to ten miles (Tables 31 and 27).

6.2 Public schools: larger sections, not more of them

Table 5: How schools absorb the inflow: urban fully staffed primary schools, school and year fixed effects

	Enrollment	Teaching posts	Classroom teachers	Sections	Pupils per section	Share of sections > 30	Afternoon shift
Public schools							
Exposure	5.55***	0.031	-0.015	0.024*	0.373***	0.016***	0.000
	(0.49)	(0.020)	(0.022)	(0.013)	(0.027)	(0.003)	(0.002)
Mean of outcome	323.49	14.53	13.16	13.12	22.34	0.16	0.28
Observations	29,665	29,665	29,665	29,665	29,665	29,665	29,665
First-stage F	392.1	392.1	392.1	392.1	392.1	392.1	392.1
Private schools							
Exposure	-1.35***	-0.005	-0.024	-0.028***	-0.090***	-0.002***	-0.000
	(0.23)	(0.020)	(0.025)	(0.009)	(0.023)	(0.001)	(0.001)
Mean of outcome	138.67	8.21	7.36	7.40	16.61	0.06	0.08
Observations	29,323	29,323	29,323	29,323	29,323	29,323	29,323
First-stage F	296.1	296.1	296.1	296.1	296.1	296.1	296.1

Notes: Each cell is the 2SLS coefficient on exposure, the number of Venezuelan primary pupils per school among the other primary schools within three miles, instrumented with the shift-share prediction that combines the neighborhood's share of national Venezuelan pupils in 2014 with the national count in the year. School-years 2015–2019. Standard errors clustered by district in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. First-stage F is the Kleibergen–Paap rk Wald statistic. Sample: schools classified as urban *polidocente completo* in 2016; specifications include school and year fixed effects.

The first row of Table 5 shows that the migrants did enroll. One more Venezuelan pupil per neighboring school raises a public school's own enrollment by 5.6 pupils, with a standard error of 0.5; at the big-city change in exposure this is 25 pupils on a base of 323, or almost eight percent. The rest of the row shows how the school made room. Teaching posts rose by 0.03 per unit of exposure, an estimate that is not distinguishable from zero and whose upper confidence bound, 0.07, would still be less than one post for every eighty pupils. Classroom teachers did not move at all. Sections rose by 0.024, with a standard error of 0.013: one new section for every forty units of exposure, or about 0.5 pupils' worth of new capacity for every 5.6 pupils who arrived. The remaining nine tenths went into existing sections. Pupils per section rose by 0.37 per unit of exposure, which at the big-city change is 1.7 pupils on a base of 22, and the share of sections above the 30-pupil norm rose by 1.6 percentage points per unit, or seven percentage points on a base of sixteen. Afternoon shifts, the other way to add capacity without posts, did not change. Nor did the number of classrooms in the building.

The arithmetic is simple. A school with 13 sections that enlarges each by 0.37 pupils adds 4.9 pupils of capacity; the 0.024 new sections add another 0.5. Together these account for the 5.6 pupils the school enrolled. The Peruvian public school absorbed the migration shock almost entirely through crowding, and the resourcing rule of Section 2.3 did not catch up within the window: at the point estimate, one post arrived for every 180 marginal pupils, against the one per 30 the norm implies. This is the empirical content of the resourcing rate ρ in the model of Section 8. Within the three years of the shock it was close to zero for urban public schools. Derenoncourt (2022) reads the same absence in the northern cities that received the Great Migration, whose governments did not raise school spending as the share of children from disadvantaged backgrounds rose, as telling in itself. Here the absence has a mechanism, a rule that allocates posts on the previous year's enrollment; but the lag in the rule is one year, and the posts did not arrive in three.

Two further results sharpen the picture. First, the schools that grew did so at their existing capacity: the coefficient on the share of sections above 35, the upper limit of the norm's tolerance, is a fifth of that on the share above 30 (Table 21), so sections grew past the norm but rarely past its tolerance. Second, contract teachers, the flexible margin of the public teaching force, did not rise; if anything they fell slightly, by 0.08 posts per unit, and the share of contract among all teachers fell by half a percentage point. Whatever posts arrived came through the regular rationalization, not through emergency hiring.

6.3 The ten largest markets

Table 6 restricts the sample to the ten markets that received 82 percent of the migrants and where, as Section 4 showed, one public school in two was turning families away by 2019. The enrollment response of a public school there is about half the national one, 3.0 pupils per unit of exposure with a standard error of 0.8, and the responses of sections, pupils per

Table 6: How schools absorb the inflow in the ten largest markets, school and year fixed effects

	Enrollment	Teaching posts	Classroom teachers	Sections	Pupils per section	Share of sections > 30	Afternoon shift
Public schools							
Exposure	2.96***	0.042	-0.012	0.036	0.083	0.004	-0.000
	(0.76)	(0.034)	(0.041)	(0.023)	(0.056)	(0.004)	(0.005)
Mean of outcome	410.45	16.68	14.98	15.06	25.28	0.28	0.42
Observations	8,247	8,247	8,247	8,247	8,247	8,247	8,247
First-stage F	231.7	231.7	231.7	231.7	231.7	231.7	231.7
Private schools							
Exposure	-1.38***	-0.002	-0.038	-0.035***	-0.092***	-0.002**	-0.000
	(0.33)	(0.023)	(0.032)	(0.011)	(0.036)	(0.001)	(0.002)
Mean of outcome	123.21	7.40	6.66	7.10	14.98	0.05	0.06
Observations	25,000	25,000	25,000	25,000	25,000	25,000	25,000
First-stage F	170.7	170.7	170.7	170.7	170.7	170.7	170.7

Notes: Each cell is the 2SLS coefficient on exposure, the number of Venezuelan primary pupils per school among the other primary schools within three miles, instrumented with the shift-share prediction that combines the neighborhood's share of national Venezuelan pupils in 2014 with the national count in the year. School-years 2015–2019. Standard errors clustered by district in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. First-stage F is the Kleibergen–Paap rk Wald statistic. Sample: primary schools in the ten markets (Lima in four subregions, Callao, Arequipa, Trujillo, Cusco, Huancayo, Ica); specifications include school and year fixed effects.

section and the share above the norm are all small and imprecise. The migrants in these markets were not absorbed by the schools nearest to where they settled growing by a lot; they were absorbed by many schools each growing by a little, and by families being sent from full schools to schools with room. Within a market, exposure moves a school's enrollment by less because the schools with the highest exposure had the least room. This is what the rationing rule in the model produces, and it is why the family-side evidence in the next section is organized around whether the school was full.

6.4 Private schools: no response on any margin

The lower panel of Table 5 shows the private sector. Private schools near the migrants did not grow; they shrank. Enrollment fell by 1.4 pupils per unit of exposure, with a standard error of 0.2, one percent of the mean, and sections and pupils per section fell in proportion. Teaching posts, classroom teachers and credentials did not change. Fees did not change either: the annual fee rose by 22 soles per unit of exposure with a standard error of 14, on a base of 2,370, and the log fee fell by 0.4 percent with a standard error of 0.6. Table 32 shows that this null is not a matter of specification. Without fixed effects the same regression yields an increase of 370 soles, or twelve percent, per unit of exposure; with district effects the increase is 55 soles; with school effects it is zero. Private schools in the districts the migrants settled in charged more before the migrants arrived. They did not raise their fees when they arrived, they did not hire, and they did not add sections. They enrolled slightly fewer children.

6.5 What the fixed effects reveal

Table 21 lines up the three specifications. Without fixed effects, exposure predicts 32 more pupils, one more teaching post and 0.8 more sections in a public school, and 370 soles more in a private fee. Four fifths of each of those coefficients is between districts: the neighborhoods that received the migrants had larger public schools, more posts and dearer private schools in 2014. Adding district effects removes most of it, and adding school effects removes the rest, except for the two quantities that did move within schools, enrollment and pupils per section. The earlier reduced-form literature on this episode, including our own previous drafts, read the no-fixed-effects estimates as responses. They are a description of where the shock landed. The response is the school-effects column, and it says that the public school got bigger sections and the private school got nothing.

7 How Families Respond

We now turn to the families already in the schools. Section 6 showed that a public school absorbs the inflow by enlarging its sections, so an incumbent family in an exposed school experiences three things at once: new classmates, a larger class, and, if the school was full, a school with no room to maneuver. This section measures how those families responded, using equation (3) with the migrant share of the child's grade at her baseline school as the treatment. We first report the average response on switching, achievement, failing and exit, then ask where the switchers went, and then split every response by whether the baseline school was full before the shock. The last split is the paper's central result on the family side: switching does not depend on capacity, but retention and exit do.

7.1 Switching, marks, failing and exit

Table 8 reports the coefficients for urban fully staffed baseline schools, pooled and by sector of the baseline school; Appendix C has the other samples, the version that includes first graders, and the response by quintile of exposure. Families move. A migrant share of one in the child's grade raises the probability that she is in a different school this year by 38 percentage points, with a standard error of 1.3; per ten percentage points of migrant share, the effect is 3.8 points on a base of 12 percent. The response is larger from private baseline schools, 4.5 points per ten points of share, than from public ones, 1.2 points, but the comparison depends on

Table 7: School responses with and without fixed effects, urban fully staffed primary schools

	Public			Private		
	No FE	District, year	School, year	No FE	District, year	School, year
Enrollment	31.90***	3.67**	5.55***	4.66***	-1.71***	-1.35***
	(4.82)	(1.51)	(0.49)	(1.75)	(0.51)	(0.23)
Teaching posts	0.976***	0.006	0.031	0.378**	0.012	-0.005
	(0.146)	(0.045)	(0.020)	(0.151)	(0.032)	(0.020)
Classroom teachers	0.802***	-0.046	-0.015	0.261***	-0.014	-0.024
	(0.126)	(0.043)	(0.022)	(0.098)	(0.023)	(0.025)
Contract teachers	-0.001	-0.092***	-0.076***	0.346**	0.116**	0.104*
	(0.042)	(0.027)	(0.025)	(0.137)	(0.053)	(0.054)
Sections	0.768***	-0.008	0.024*	0.146***	-0.037**	-0.028***
	(0.123)	(0.042)	(0.013)	(0.056)	(0.016)	(0.009)
Pupils per section	1.182***	0.313***	0.373***	0.137*	-0.138***	-0.090***
	(0.127)	(0.027)	(0.027)	(0.071)	(0.030)	(0.023)
Share of sections > 30	0.031***	0.012***	0.016***	0.002	-0.003***	-0.002***
	(0.007)	(0.002)	(0.003)	(0.002)	(0.001)	(0.001)
Share of pupils in > 30	0.034***	0.012***	0.016***	0.001	-0.003***	-0.002***
	(0.007)	(0.002)	(0.003)	(0.002)	(0.001)	(0.001)
Any section > 30	0.073***	0.012***	0.015***	0.000	-0.010***	-0.007***
	(0.008)	(0.003)	(0.004)	(0.003)	(0.002)	(0.002)
Afternoon shift	0.049***	0.000	0.000	-0.016***	-0.001	-0.000
	(0.005)	(0.002)	(0.002)	(0.003)	(0.001)	(0.001)
Classrooms	1.234***	0.060	0.093	0.728**	-0.281**	-0.252
	(0.167)	(0.078)	(0.077)	(0.288)	(0.129)	(0.192)
Monthly fee (2019 S/)	–	–	–	33.55***	5.10*	2.13
				(9.03)	(2.63)	(1.35)
Annual fee (2019 S/)	–	–	–	367.81***	55.27*	22.36
				(99.82)	(28.82)	(14.40)
log annual fee	–	–	–	0.121***	0.015**	-0.004
				(0.020)	(0.007)	(0.006)
Share with pedagogical degree	0.003***	0.002***	0.002***	-0.022***	0.003	0.002
	(0.001)	(0.000)	(0.000)	(0.004)	(0.002)	(0.002)
Share with any degree	0.003***	0.002***	0.001***	-0.022***	0.002	0.001
	(0.001)	(0.000)	(0.000)	(0.004)	(0.002)	(0.002)

Notes: Each cell is the 2SLS coefficient on exposure, the number of Venezuelan primary pupils per school among the other primary schools within three miles, instrumented with the shift-share prediction that combines the neighborhood's share of national Venezuelan pupils in 2014 with the national count in the year. School-years 2015–2019. Standard errors clustered by district in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. First-stage F is the Kleibergen–Paap rk Wald statistic. Sample: urban *polidocente completo* primary schools. Fees are observed for private schools only. Contract teachers are observed for public schools only.

Table 8: How families respond: switching, marks, failing and exit, urban fully staffed baseline schools

	All	Public baseline school	Private baseline school
Switched school	0.378***	0.115***	0.449***
	(0.013)	(0.014)	(0.014)
per 10 points	0.0378	0.0115	0.0449
mean of outcome	0.121	0.090	0.193
observations	6,831,226	4,731,239	2,099,987
Math mark (SD)	-0.081***	-0.116**	-0.028
	(0.019)	(0.054)	(0.020)
per 10 points	-0.0081	-0.0116	-0.0028
mean of outcome	0.038	-0.008	0.144
observations	6,779,806	4,700,260	2,079,546
Language mark (SD)	-0.098***	-0.175***	-0.038*
	(0.019)	(0.051)	(0.020)
per 10 points	-0.0098	-0.0175	-0.0038
mean of outcome	0.047	-0.004	0.165
observations	6,777,367	4,698,362	2,079,005
Failed the year	0.012***	0.009	0.002
	(0.002)	(0.008)	(0.002)
per 10 points	0.0012	0.0009	0.0002
mean of outcome	0.027	0.036	0.008
observations	6,831,227	4,731,240	2,099,987
Left the register (2017–18)	-0.005***	-0.002	-0.004**
	(0.002)	(0.008)	(0.002)
per 10 points	-0.0005	-0.0002	-0.0004
mean of outcome	0.008	0.008	0.006
observations	4,516,857	3,123,011	1,393,846
Repeats next year	0.008***	0.016	-0.001
	(0.003)	(0.010)	(0.003)
per 10 points	0.0008	0.0016	-0.0001
mean of outcome	0.036	0.046	0.011
observations	5,501,595	3,797,014	1,704,581

Notes: Each cell is the coefficient on the migrant share (0–1): the number of Venezuelan pupils in year t in the pupil's grade at the school she attended in $t-1$, divided by the size of that grade at that school in $t-1$; from a regression with baseline-school-by-year and grade fixed effects and controls for age, sex and the standardized math mark in $t-1$. Non-Venezuelan primary pupils in grades 2–6, 2017–2019 (2017–2018 for permanent exit). Standard errors clustered by baseline school and grade; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Per 10 points* is the coefficient divided by ten, the effect of a ten-percentage-point higher migrant share. Sample: pupils whose baseline school is urban *polidocente completo*.

the unit. A ten-point share is two migrant children in the typical private grade of twenty pupils and eight in the typical public grade of eighty; per ten Venezuelan pupils who join the grade, the response is 2.0 points from a public school and 0.7 points, not significant, from a private one (Table 44). Table 37 shows the response by quintile of exposure: in public schools it rises with exposure, from nothing in the lowest quintile, where the migrant share is below one percent, to 1.3 points in the top quintile, where it averages 13 percent; in private schools it is concentrated in the top quintile, where the share averages a third and the effect is five points.

Marks fall a little. Per ten points of share, the standardized mathematics mark falls by 0.008 standard deviations and the language mark by 0.010, pooled; in public baseline schools the effects are 0.012 and 0.018, in private ones 0.003 and 0.004 and not significant. Failing rises by 0.12 percentage points per ten points of share on a base of 2.7 percent, an effect that is significant pooled but not within either sector, and the probability that year t is the child's last in the registered system falls by 0.06 points, with a standard error of 0.02, on a base of 0.8 percent. Averaged over all incumbents, then, the inflow moved families across schools and lowered marks slightly; it did not push children out.

7.2 Where the switchers go

Table 9 decomposes the switching effect by the characteristics of the destination relative to the origin, both measured the year before the move so that the move itself cannot change them. From a public baseline school, a full migrant share induces 12 extra moves per hundred pupils: 9 to a school with a lower migrant share the year before and 2 to a school with a higher one; 7 to a school with smaller sections; 7 to a school whose pupils had higher marks; 7 to another public school and 4 to a private school. From a private baseline school, the 45 induced moves per hundred go 35 to another private school and 11 to a public school, 11 to a school with smaller sections and 22 to a school with higher marks; among the private-to-private moves with fee information, 27 are to a school with a higher fee and 16 to one with a lower fee. The migrant-share comparison is less informative for private origins because most private schools had no Venezuelan pupils the year before, so origin and destination are often tied at zero.

The induced moves from public schools are not random reshuffling. Four fifths of them go to schools with fewer migrants, three fifths to schools with smaller sections and three fifths to schools with higher marks, and the destinations are on average only modestly less crowded and only modestly higher-achieving than the origins. This is the pattern the composition and crowding terms in the model of Section 8 are there to reproduce, and it is why a

Table 9: Where the switchers go, urban fully staffed baseline schools

	Coefficient on the migrant share			Mean of outcome		
	All	Public	Private	All	Public	Private
to a school with fewer migrants	0.059***	0.089***	0.053***	0.024	0.021	0.030
	(0.009)	(0.012)	(0.012)			
to a school with more migrants	0.070***	0.021***	0.086***	0.024	0.014	0.045
	(0.006)	(0.007)	(0.007)			
to a school with smaller sections	0.096***	0.068***	0.106***	0.062	0.058	0.071
	(0.012)	(0.010)	(0.014)			
to a school with higher marks	0.188***	0.075***	0.221***	0.058	0.047	0.081
	(0.013)	(0.011)	(0.016)			
to a public school	0.102***	0.071***	0.110***	0.068	0.066	0.072
	(0.007)	(0.012)	(0.008)			
to a private school	0.276***	0.043***	0.344***	0.050	0.023	0.113
	(0.013)	(0.007)	(0.015)			
public to public	0.019***	0.071***	0.000	0.046	0.066	0.000
	(0.003)	(0.012)	(0.000)			
public to private	0.012***	0.043***	-0.000	0.016	0.023	0.000
	(0.002)	(0.007)	(0.000)			
private to public	0.083***	0.000***	0.110***	0.022	0.000	0.072
	(0.007)	(0.000)	(0.008)			
private to private	0.265***	0.000***	0.345***	0.035	0.000	0.113
	(0.013)	(0.000)	(0.015)			
to a private school with a higher fee	0.267***		0.267***	0.056		0.056
	(0.017)		(0.017)			
to a private school with a lower fee	0.159***		0.159***	0.058		0.058
	(0.011)		(0.011)			
Observations	6,804,884	4,725,176	2,079,708			

Notes: Each cell is the coefficient on the migrant share (0–1): the number of Venezuelan pupils in year t in the pupil's grade at the school she attended in $t-1$, divided by the size of that grade at that school in $t-1$; from a regression with baseline-school-by-year and grade fixed effects and controls for age, sex and the standardized math mark in $t-1$. Non-Venezuelan primary pupils in grades 2–6, 2017–2019 (2017–2018 for permanent exit). Standard errors clustered by baseline school and grade; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The effect of a ten-percentage-point higher share is the coefficient divided by ten. Each outcome is an indicator equal to one if the pupil switched to a school with the stated characteristic, measured in $t-1$ for both origin and destination, and zero otherwise (including for stayers), so the coefficients decompose the switching effect in the first row of Table 8. Observations are those of the first outcome. The two fee outcomes are defined for pupils whose baseline school reports a fee (1,880,972 pupils, nearly all in private schools); cells with fewer than 1,000 observations are left blank.

model without a composition term cannot: crowding and marks alone would not deliver the concentration of moves toward schools with fewer migrants.

7.3 Fewer migrants, or richer peers?

Table 10: Where the switchers go: migrants, socioeconomic status and achievement of the destination

	Public baseline school			Private baseline school		
	Coefficient	(s.e.)	Mean	Coefficient	(s.e.)	Mean
A. Switching and the destination's migrant share (year before the move)						
Switched school	0.115***	(0.014)	0.090	0.449***	(0.014)	0.193
to a school with fewer migrants	0.089***	(0.012)	0.021	0.053***	(0.012)	0.030
to a school with more migrants	0.021***	(0.007)	0.014	0.085***	(0.007)	0.045
to a school with the same share	0.004	(0.007)	0.053	0.317***	(0.013)	0.110
B. The destination's socioeconomic status (2018 ISE) and achievement (2016 grade-4 score)						
to a higher-SES school	0.052***	(0.010)	0.047	0.166***	(0.018)	0.057
to a lower-SES school	0.056***	(0.010)	0.036	0.262***	(0.019)	0.113
to a higher-scoring school	0.042***	(0.010)	0.037	0.271***	(0.018)	0.091
to a lower-scoring school	0.065***	(0.010)	0.044	0.167***	(0.015)	0.074
C. Migrant share and socioeconomic status jointly						
fewer migrants and higher SES	0.044***	(0.007)	0.009	0.011**	(0.005)	0.010
fewer migrants and lower or equal SES	0.040***	(0.008)	0.011	0.036**	(0.016)	0.018
more or equal migrants and higher SES	0.008	(0.007)	0.037	0.155***	(0.018)	0.047
more or equal migrants and lower or equal SES	0.016***	(0.006)	0.025	0.226***	(0.015)	0.095
D. By socioeconomic tercile of origin and destination						
to a higher SES tercile	0.041***	(0.010)	0.046	0.115***	(0.017)	0.030
to the same SES tercile	0.063***	(0.009)	0.034	0.280***	(0.019)	0.120
to a lower SES tercile	0.005	(0.004)	0.004	0.033***	(0.006)	0.023
fewer migrants, higher SES tercile	0.024***	(0.007)	0.009	0.012***	(0.004)	0.004
fewer migrants, same SES tercile	0.055***	(0.008)	0.010	0.034**	(0.016)	0.021
fewer migrants, lower SES tercile	0.004	(0.004)	0.002	0.001	(0.002)	0.004
E. Among switchers: destination minus origin, on the migrant share						
Difference in SES index (SD)	0.037	(0.048)	0.080	0.038**	(0.016)	-0.220
Difference in 2016 grade-4 score (SD)	0.000	(0.036)	-0.066	-0.022*	(0.012)	0.060
Difference in migrant share	-0.002	(0.002)	-0.001	-0.001***	(0.000)	0.001
Difference in migrant share, conditional on the SES difference	-0.002	(0.002)	-0.001	-0.000	(0.000)	0.001

Notes: The specification of Table 8, urban fully staffed baseline schools. In panels A–D each outcome is an indicator equal to one if the pupil switched to a school with the stated characteristic relative to her origin school (zero for stayers), so the coefficients decompose the switching effect. Socioeconomic status is the school's mean MINEDU socioeconomic index among its grade-4 pupils in the 2018 census; achievement is the school's mean standardized grade-4 score in 2016; the migrant share is measured the year before the move. Comparisons are defined when both schools have the attribute. Panel E regresses the destination-minus-origin difference on the migrant share among switchers only. Standard errors clustered by baseline school and grade.

A move toward a school with fewer migrants could be a move toward richer or higher-achieving peers that the migrant share merely proxies. Table 10 tests this with two predetermined school attributes: the mean of the Ministry's socioeconomic index among the school's fourth graders in the 2018 census, which we use as the school's socioeconomic status, and the school's mean grade-4 score in the 2016 national assessment. Across urban public schools the two are correlated at 0.56, and the migrant share is correlated with socioeconomic status at 0.27: the migrants went to the better-off public schools, not the poorer ones.

The induced moves are not toward richer peers. From a public baseline school, a full migrant share induces 11.5 moves per hundred pupils, of which 5.2 are to a school of higher socioeconomic status and 5.6 to one of lower status; 4.2 are to a higher-scoring school and 6.5 to a lower-scoring one. Among switchers, the destination-minus-origin difference in the socioeconomic index does not respond to the migrant share (a coefficient of 0.04 with a standard error of 0.05), and neither does the difference in test scores (0.00 with a standard error of 0.04). The moves toward fewer migrants, on the other hand, are concentrated within socioeconomic strata: of the 8.9 induced moves to a school with fewer migrants, 5.6 stay in the same socioeconomic tercile as the origin, 2.4 go up a tercile and 0.4 go down; and the split by joint cells is even, 4.4 moves to fewer migrants and higher status against 4.0 to fewer migrants and equal or lower status. Families in exposed public schools moved sideways in socioeconomic and achievement terms, and toward fewer migrants within their stratum. From private baseline schools the induced moves go, if anything, toward lower-status and higher-scoring schools, and most are to schools with the same, usually zero, migrant share the year before. We read this as evidence that the composition term the model needs is about the presence of migrant children in the grade, or about what came with them in the year of arrival, rather than about the socioeconomic sorting that dominates school choice in normal times.

The family questionnaire of the assessment lets us go one step further, for the cohorts that were tested: it reports the mother's education of 1.3 million pupils, and from it we build a second school-level measure, the share of a school's tested pupils whose mother has post-secondary education, which is correlated at 0.80 with the socioeconomic index. Table 11 splits the family-side estimates by the mother's education. Families of all three groups leave exposed public grades, and all three move toward schools with fewer migrants at similar rates, 0.12, 0.13 and 0.11 per unit of share for mothers with primary or less, secondary, and post-secondary education. The overall switching response is largest for the least educated, 0.19 against 0.12 for the most educated, and the pooled interaction with post-secondary education is −0.10 with a standard error of 0.02. Where they go differs modestly: the least educated families that leave move up in socioeconomic terms more often than down, 0.14 against 0.03 per unit of share, and the most educated are roughly even, 0.06 against 0.04. Table 12 repeats the destination decomposition with the educated-mother share as the socioeconomic measure, in the questionnaire sample: moves toward more educated peers outnumber moves toward less educated ones, 0.09 to 0.05, but among switchers the educated-mother share of the destination relative to the origin does not respond to the migrant share (−0.01 with a standard error of 0.03), the fewer-migrant moves split evenly between more

Table 11: Family responses by the mother's education

	Public baseline school			Private baseline school		
	Primary or less	Secondary	Post-secondary	Primary or less	Secondary	Post-secondary
Switched school	0.192**	0.128***	0.124**	0.446***	0.490***	0.503***
	(0.079)	(0.046)	(0.060)	(0.086)	(0.039)	(0.034)
	mean 0.061	mean 0.079	mean 0.101	mean 0.204	mean 0.200	mean 0.152
to fewer migrants	0.119**	0.133***	0.108**	0.016	0.041	0.048*
	(0.053)	(0.040)	(0.048)	(0.030)	(0.037)	(0.026)
to more migrants	-0.011	-0.026	-0.003	0.034	0.123***	0.064***
	(0.026)	(0.018)	(0.028)	(0.059)	(0.024)	(0.018)
to higher SES (ISE)	0.137***	0.070***	0.061	0.374**	0.180***	0.129***
	(0.050)	(0.027)	(0.044)	(0.157)	(0.040)	(0.028)
to lower SES (ISE)	0.034	0.053	0.044	-0.069	0.291***	0.305***
	(0.061)	(0.036)	(0.041)	(0.129)	(0.047)	(0.038)
to more educated mothers	0.109*	0.081***	0.070	0.132	0.187***	0.120***
	(0.058)	(0.030)	(0.046)	(0.094)	(0.037)	(0.024)
to less educated mothers	0.062	0.048	0.023	0.119	0.294***	0.316***
	(0.052)	(0.034)	(0.040)	(0.115)	(0.044)	(0.038)
Math mark (SD)	0.026	-0.234*	-0.268*	-0.163	0.049	0.045
	(0.184)	(0.121)	(0.138)	(0.219)	(0.067)	(0.057)
Language mark (SD)	-0.188	-0.131	-0.161	-0.068	-0.070	0.115**
	(0.186)	(0.117)	(0.140)	(0.209)	(0.071)	(0.057)
Failed the year	0.030	-0.005	-0.002	-0.040	0.011	0.005
	(0.036)	(0.020)	(0.018)	(0.041)	(0.010)	(0.006)
	mean 0.038	mean 0.022	mean 0.010	mean 0.014	mean 0.008	mean 0.003
Left the register	0.037	0.000	-0.003	-0.007	0.005	-0.002
	(0.039)	(0.014)	(0.014)	(0.005)	(0.004)	(0.002)
	mean 0.008	mean 0.003	mean 0.002	mean 0.004	mean 0.003	mean 0.003
Pooled, interaction of the migrant share with an indicator for post-secondary mother's education						
Switched school	0.179***	× post-sec: -0.102***		0.497***	× post-sec: 0.001	
	(0.039)	(0.024)		(0.030)	(0.006)	
to fewer migrants	0.091***	× post-sec: 0.094***		0.046*	× post-sec: 0.003	
	(0.031)	(0.023)		(0.026)	(0.005)	
Math mark (SD)	-0.187*	× post-sec: -0.128**		0.061	× post-sec: -0.061*	
	(0.100)	(0.061)		(0.046)	(0.032)	
Failed the year	-0.003	× post-sec: 0.000		0.004	× post-sec: -0.000	
	(0.016)	(0.009)		(0.006)	(0.003)	
Left the register	0.006	× post-sec: 0.008		0.001	× post-sec: -0.002	
	(0.011)	(0.011)		(0.003)	(0.001)	

Notes: The specification of Table 8, estimated separately by the education of the pupil's mother as reported in the family questionnaire of the national assessment (pupils tested in grade 2 in 2015–2016 or grade 4 in 2016–2018, or in the 2019 samples; 2017–2019 rows of those cohorts). Destination characteristics as in Table 10; *more educated mothers* means a higher share of tested pupils with a post-secondary-educated mother in the destination than in the origin. The bottom panel pools the groups and interacts the migrant share with an indicator for post-secondary education. Standard errors clustered by baseline school and grade.

and less educated destinations, 0.05 and 0.06, and the migrant-share gap between destination and origin is the same for educated and less educated families. The socioeconomic tilt of the induced moves is a feature of who switches, not of what the migrant share does to where they go.

7.4 Test scores

The report-card marks of Table 8 are coarse and teacher-assigned. The national assessment gives a comparable measure, for the grades and years in which it was administered: second grade in 2014–2016, fourth grade in 2016 and 2018, and samples of both in 2019. Two things can be asked of it. First, did exposure to the inflow change a school's test scores? Table 13 regresses a school's mean fourth-grade scores, and its value added, on exposure with school and year fixed effects, as in Section 6. It did not: the coefficients on reading and mathematics are -0.002 and 0.003 standard deviations per unit of exposure, with standard errors of 0.007 and 0.008, in urban public schools, and zero in private schools, while in the same schools and years section size rose by 0.39 pupils per unit. Value added, the school-by-year effect from a regression of the fourth-grade score on the same pupils' second-grade score two years earlier, rose by 0.018 with a standard error of 0.011. Without fixed effects the coefficients are large and positive, 0.11 and 0.09, because the migrants went to the higher-scoring public schools.

Second, did larger sections lower learning? Table 14 estimates the crowding penalty of the model directly, at the pupil level, with the lagged score, school and year fixed effects, and the school's section size instrumented by the shift-share exposure. The point estimates are small and, if anything, positive: 0.025 standard deviations per additional pupil per section in public schools, with a standard error of 0.017, and -0.035 with a standard error of 0.027 in private schools. Within the range the shock produced, one to two extra pupils in sections of 22 to 25, we find no loss of learning. We record this as an estimate of the crowding penalty ϕ that is not distinguishable from zero, and we note that it is silent on what larger increases would do.

7.5 Does the response depend on whether the school was full?

Table 15 interacts the migrant share with the indicator that the baseline school was full in 2016, separately for public and private baseline schools; Figure 8 plots the coefficients, and Table 41 repeats the exercise with a cutoff of 1.00, with utilization entered continuously, with a 25-pupil norm, with the 2016 share of sections above 30, and with the child's own section size the year before.

Capacity does not reduce switching. In public baseline schools the coefficient on the

Table 12: Where the switchers go: migrants and the education of the destination's mothers

	Public baseline school		Private baseline school	
	Coefficient	Mean	Coefficient	Mean
Switched school		0.080		0.170
	(0.038)		(0.030)	
to fewer migrants		0.016		0.022
	(0.030)		(0.026)	
to more migrants		0.012		0.035
	(0.014)		(0.015)	
to a school with more educated mothers		0.042		0.052
	(0.027)		(0.021)	
to a school with less educated mothers		0.033		0.102
	(0.026)		(0.030)	
fewer migrants and more educated mothers		0.008		0.008
	(0.018)		(0.006)	
fewer migrants and less or equally educated mothers		0.008		0.014
	(0.022)		(0.011)	
to a higher tercile of mothers' education		0.035		0.023
	(0.025)		(0.015)	
to the same tercile		0.033		0.102
	(0.024)		(0.027)	
to a lower tercile		0.008		0.033
	(0.014)		(0.020)	
fewer migrants, higher tercile		0.007		0.003
	(0.017)		(0.005)	
fewer migrants, same tercile		0.007		0.015
	(0.019)		(0.009)	
fewer migrants, lower tercile		0.002		0.004
	(0.013)		(0.007)	
Among switchers: destination minus origin, on the migrant share				
Difference in the educated-mother share		0.040		-0.098
	(0.0346)		(0.0128)	
Difference in the migrant share		-0.001		0.001
	(0.0032)		(0.0004)	
post-secondary mother (level)		-0.001		0.001
	(0.0000)		(0.0001)	
post-secondary mother × migrant share		-0.001		0.001
	(0.0027)		(0.0003)	

Notes: As Table 10, with the school's socioeconomic status measured as the share of its tested pupils (2016 and 2018 national assessments, schools with at least ten) whose mother has post-secondary education; sample restricted to pupils with a family questionnaire. Standard errors clustered by baseline school and grade.

Table 13: Exposure and school-level test scores, grade 4, 2016–2019

		Public		Private	
Sample	Outcome	No FE	School, year	No FE	School, year
Urban fully staffed	Mean reading score (SD)	0.112***	-0.002	0.049***	-0.001
		(0.011)	(0.007)	(0.016)	(0.007)
Urban fully staffed	Mean math score (SD)	0.086***	0.003	0.020	0.004
		(0.010)	(0.008)	(0.014)	(0.007)
Urban fully staffed	Value added (SD)	0.072***	0.018	0.034***	0.008
		(0.007)	(0.011)	(0.008)	(0.009)
Urban fully staffed	Pupils per section	1.283***	0.391***	0.303***	-0.156***
		(0.154)	(0.039)	(0.098)	(0.056)
Ten markets	Mean reading score (SD)	0.005	-0.008	0.057***	-0.004
		(0.013)	(0.010)	(0.020)	(0.009)
Ten markets	Mean math score (SD)	-0.017	-0.010	0.026	0.004
		(0.013)	(0.011)	(0.018)	(0.009)
Ten markets	Value added (SD)	0.003	0.003	0.030***	0.003
		(0.010)	(0.016)	(0.010)	(0.010)
Ten markets	Pupils per section	0.110	0.143*	0.296*	-0.124*
		(0.182)	(0.084)	(0.154)	(0.070)
All	Mean reading score (SD)	0.166***	-0.001	0.050***	-0.000
		(0.013)	(0.006)	(0.015)	(0.007)
All	Mean math score (SD)	0.137***	0.003	0.022	0.006
		(0.012)	(0.008)	(0.014)	(0.007)
All	Value added (SD)	0.093***	0.011	0.035***	0.009
		(0.007)	(0.011)	(0.007)	(0.009)
All	Pupils per section	2.473***	0.431***	0.368***	-0.176***
		(0.213)	(0.039)	(0.104)	(0.053)

Notes: 2SLS coefficient on exposure (Venezuelan pupils per other primary school within three miles, instrumented with the 2014-base shift-share), school-years 2016, 2018 and 2019, schools with at least ten tested grade-4 pupils. Outcomes are the school's mean standardized grade-4 scores in the national assessment, its value added (the school-by-year effect from a regression of the grade-4 score on the pupil's grade-2 score two years earlier, sex and age), and pupils per section. Standard errors clustered by district.

Table 14: Test scores and section size: the crowding penalty

	Public			Private		
	Reading	Math	Mean	Reading	Math	Mean
OLS: own section size, with lagged score	0.0038***	0.0050***	0.0044***	-0.0022***	-0.0023***	-0.0022***
	(0.0007)	(0.0009)	(0.0007)	(0.0007)	(0.0007)	(0.0007)
IV: school section size, with lagged score	0.0106	0.0403**	0.0254	-0.0016	-0.0693**	-0.0348
	(0.0164)	(0.0186)	(0.0168)	(0.0291)	(0.0314)	(0.0272)
	[F = 84]	[F = 84]	[F = 84]	[F = 20]	[F = 20]	[F = 20]
Reduced form: neighborhood exposure	0.0045	0.0201**	0.0123*	0.0026	0.0131**	0.0077
	(0.0071)	(0.0083)	(0.0075)	(0.0065)	(0.0067)	(0.0063)
OLS: own section size, no lagged score	0.0112***	0.0126***	0.0119***	0.0004	0.0002	0.0003
	(0.0008)	(0.0009)	(0.0008)	(0.0008)	(0.0008)	(0.0007)
IV: school section size, no lagged score	0.0038	0.0324**	0.0183	0.0666*	-0.0038	0.0319
	(0.0131)	(0.0158)	(0.0138)	(0.0347)	(0.0265)	(0.0281)
	[F = 91]	[F = 90]	[F = 90]	[F = 23]	[F = 23]	[F = 23]

Notes: Pupil-level regressions of the standardized grade-4 score in the national assessment (ECE 2016 and 2018 censuses) on section size, with school and year fixed effects and controls for sex and age; rows with a lagged score add the pupil's grade-2 score two years earlier. Own section size is the number of pupils in the child's section in SIAGIE; in the IV rows the school's mean section size is instrumented with the 2014-base shift-share exposure. Non-Venezuelan pupils in urban fully staffed schools. Standard errors clustered by school (OLS) or district (IV); Kleibergen–Paap F in brackets.

Table 15: Does the response depend on whether the school was full? urban fully staffed baseline schools

	Switched	Math (SD)	Language (SD)	Failed	Left the register
public baseline schools					
Migrant share	0.103***	-0.133**	-0.229***	0.018**	-0.010
	(0.016)	(0.056)	(0.053)	(0.009)	(0.009)
Migrant share × full school	0.088***	0.118	0.374**	-0.061**	0.050***
	(0.034)	(0.178)	(0.172)	(0.024)	(0.019)
Mean of outcome	0.090	-0.008	-0.004	0.036	0.008
Observations	4,731,239	4,700,260	4,698,362	4,731,240	3,123,011
private baseline schools					
Migrant share	0.443***	-0.027	-0.036*	0.002	-0.004**
	(0.014)	(0.020)	(0.020)	(0.002)	(0.002)
Migrant share × full school	0.273***	-0.019	-0.085	-0.013	0.009
	(0.097)	(0.129)	(0.120)	(0.013)	(0.011)
Mean of outcome	0.193	0.144	0.165	0.008	0.006
Observations	2,099,987	2,079,546	2,079,005	2,099,987	1,393,846

Notes: The specification of Table 8 with the migrant share interacted with an indicator that the baseline school was full in 2016 (utilization, enrollment over thirty times the number of sections, at or above 0.95). The main effect of the indicator is absorbed by the baseline-school-by-year fixed effects. Sample: urban fully staffed baseline schools. Standard errors clustered by baseline school and grade; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

migrant share is 0.10 and the coefficient on its interaction with the full-school indicator is +0.09 with a standard error of 0.03: families in full schools move at least as much as families in schools with room. The interaction is not robust in size, +0.05 with a cutoff of 1.00, −0.03 with the continuous utilization rate, +0.04 with the 25-pupil norm, +0.09 with the share of sections above 30, none of them distinguishable from zero, but under no measure is it negative and large. In private baseline schools the interaction is +0.27 with a standard error of 0.10. Whatever a full school does to the families that want to leave it, it does not stop them from leaving. Switching is about who the classmates are, and the model needs a composition term for that reason; it does not need capacity to explain flight.

Retention and exit do depend on capacity, and only in public schools. In a public school that was not full, a higher migrant share raises failing slightly, by 0.2 percentage points per ten points of share on a base of 3.6 percent, and does not change exit. In a public school that was full, the same migrant share lowers failing by 0.4 points per ten points of share and raises the probability that the year is the child's last in the register by 0.4 points, on a base of 0.8 percent. The interaction coefficients are −0.061 with a standard error of 0.024 on failing and +0.050 with a standard error of 0.019 on exit, and they keep their signs under every alternative measure of fullness: with the continuous utilization rate they are −0.08 and +0.09, with the share of sections above 30 they are −0.07 and +0.09, with the 25-pupil norm −0.01 and +0.02. The child's own section being above 30 the year before lowers failing too, by 0.03, but does not raise exit; it is the school's fullness, not the child's classroom, that governs the exit margin. In private baseline schools both interactions are zero. At the big-city migrant shares of five to ten percent, a full public school held back one child in thirty fewer and lost one child in two hundred more than a public school with room. Neither response is visible in the average of Table 8, because full schools hold a third of urban public pupils and the two responses have opposite signs in the two groups.

7.6 What the exit margin is made of

Fewer failures and more exits in the same schools invite one reading: a full school that holds a child back must find her a seat next year, so it passes her instead, and the child who is passed against her record leaves. Table 16 tests this and rejects it. First, the extra exits are not the children who failed. Among pupils who passed the year, the interaction of the migrant share with the full-school indicator on exit is 0.061 with a standard error of 0.017, and among pupils who failed it is 0.13 with a standard error of 0.24; the exits come from the 96 percent of pupils who passed. Second, failing itself falls in full exposed schools, by 0.14 per unit of share, and every pupil who fails repeats: among failers who stay, the probability

Table 16: The exit margin in full public schools: passers, failers and promotion

	All public		Urban fully staffed	
	Migrant share	× full	Migrant share	× full
Left the register, among pupils who passed	-0.009***	0.108***	-0.013	0.061***
	(0.003)	(0.021)	(0.009)	(0.017)
	mean 0.011	N 4,092,620	mean 0.007	N 3,008,437
Left the register, among pupils who failed	-0.003	0.088	0.034	0.134
	(0.013)	(0.238)	(0.096)	(0.244)
	mean 0.033	N 187,719	mean 0.037	N 113,472
Failed the year	0.041***	-0.232***	-0.009	-0.145***
	(0.004)	(0.045)	(0.017)	(0.047)
	mean 0.046	N 4,289,127	mean 0.037	N 3,123,011
Passed the year	-0.039***	0.223***	0.014	0.140***
	(0.005)	(0.048)	(0.019)	(0.050)
	mean 0.947	N 4,289,127	mean 0.957	N 3,123,011
Promoted anyway, failers who stayed (grades 2–5)	-0.000	-0.002	-0.002	-0.001
	(0.001)	(0.009)	(0.011)	(0.014)
	mean 0.001	N 170,695	mean 0.001	N 103,320
Repeated, failers who stayed (grades 2–5)	0.000	0.002	0.002	0.001
	(0.001)	(0.009)	(0.011)	(0.014)
	mean 0.999	N 170,695	mean 0.999	N 103,320
Repeated, passers who stayed (grades 2–5)	0.002	-0.006	0.008	-0.012
	(0.002)	(0.014)	(0.007)	(0.015)
	mean 0.005	N 3,274,390	mean 0.004	N 2,408,449

Notes: The specification of Table 15, public baseline schools, 2017–2018, estimated on the stated subsample. *Left the register* means that year t is the pupil's last year in SIAGIE through 2022. Standard errors clustered by baseline school and grade; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

of being promoted anyway is 0.07 percent and does not respond to exposure or fullness. Peru has no social promotion in primary school, and the shock did not create it. Third, passers who stay in a full exposed school are not repeating more either; the interaction on repetition among passers is -0.01 with a standard error of 0.02.

What the table does say is that a full public school with migrants marks fewer of its pupils as failing and loses more of the pupils it passed. Table 17 and Figure 9 describe who leaves. The exit response in full schools is present among age-appropriate pupils, 0.045 with a standard error of 0.013, and absent among over-age ones; it is larger among girls, 0.070, than among boys, 0.032; it is larger in grades 4–6 than in grades 2–3; and it is largest among pupils in the lowest tercile of the previous year's marks, 0.10 with a standard error of 0.06, against 0.04 in the middle tercile and 0.01 in the top. The children who leave the registered system when a full public school receives migrants are, disproportionately, weaker pupils who were not held back. Switching shows no such gradient in any group.

Table 17: Who leaves: the exit response in full public schools by pupil group

	Left the register			Switched school		
	Migrant share	× full	Mean	Migrant share	× full	Mean
Grades 2–3	-0.008	0.022	0.004	0.048**	0.095*	0.103
	(0.010)	(0.022)		(0.024)	(0.054)	
Grades 4–6	-0.026**	0.046*	0.011	0.070***	0.089**	0.081
	(0.013)	(0.027)		(0.021)	(0.043)	
Age-appropriate	0.001	0.045***	0.004	0.102***	0.090***	0.089
	(0.006)	(0.013)		(0.016)	(0.034)	
Over-age (two or more years)	-0.106	-0.522*	0.104	0.148**	0.064	0.105
	(0.114)	(0.317)		(0.066)	(0.190)	
Boys	-0.014	0.032	0.009	0.117***	0.063	0.090
	(0.011)	(0.025)		(0.020)	(0.046)	
Girls	-0.004	0.069***	0.008	0.087***	0.116***	0.089
	(0.013)	(0.025)		(0.020)	(0.044)	
Baseline mark: low tercile	0.008	0.096*	0.009	0.076***	0.209***	0.107
	(0.025)	(0.058)		(0.024)	(0.061)	
Baseline mark: middle tercile	-0.009	0.044	0.012	0.166***	-0.042	0.077
	(0.015)	(0.034)		(0.031)	(0.058)	
Baseline mark: high tercile	-0.012	0.013	0.003	0.076***	0.051	0.085
	(0.010)	(0.024)		(0.028)	(0.057)	

Notes: The specification of Table 15, public urban fully staffed baseline schools, estimated separately by pupil group. Standard errors clustered by baseline school and grade; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Two checks complete the picture. Table 42 asks whether the exit response is larger where the neighboring schools are full too, as it would be if exits were failed attempts to switch in a saturated market. It is not: the triple interaction of the migrant share, the own school's fullness and the district's share of full schools is -0.11 with a standard error of 0.15. What district fullness does is dampen switching, by 0.25 per unit of share with a standard error of 0.15; where every school is full, families have fewer places to go. Table 43 asks whether the exits are children who reappear under a new identifier. We match every exiter to the pupils who first appear in the register the following year with the same sex and date of birth, in her grade or the next, and compare with a placebo birth date shifted by a week. Anywhere in Peru the match rate is 40.6 percent against a placebo of 40.7, pure chance; in the same district it is 1.2 percent against 0.9, and in the same school 0.27 percent against 0.03. Reclassifying the same-school matches as non-exits leaves the interaction on exit at 0.049; reclassifying the same-district matches lowers it to 0.040 with a standard error of 0.018. At most a fifth of the exit response is administrative. The rest is children who were in a full public school when the migrants arrived and were not in any Peruvian school afterwards.

We cannot see where they went. The profile, age-appropriate, lower-achieving, passed rather than failed, is not the profile of a child leaving school for work, and the register covers every licensed school in the country. The candidates are emigration, a move to an unlicensed school, or non-attendance. What the evidence establishes is the margin on which a full public school adjusts: it does not stop the families that want to leave, and it does not select at the door among incumbents; it changes who stays, by not holding back the weak and by losing some of them.

8 A Model of School Choice with Capacity Constraints

The reduced-form results describe how one school system absorbed one shock. To say what would have happened under different rules, or what a policy that adds capacity, subsidizes private seats, or changes how migrants are placed would do, we need a model in which families choose among schools that can fill up, public schools receive resources through a rule rather than a market, and private schools set prices and capacity. This section writes down that model. It is built to be estimated with the data of Section 3, and its primitives map one to one onto the objects the reduced form measures: the resourcing rate ρ of Section 6, the rationing rule of Section 7, and the substitution patterns behind the switching results. Estimation and the counterfactuals are set out in Section 9. We keep the exposition to what the counterfactuals require; extensions are noted where they matter.

8.1 Environment

A market m is a city.[5] In market m there is a set $\mathcal{J}_m$ of primary schools, each either public ($j \in \mathcal{J}_m^G$) or private ($j \in \mathcal{J}_m^P$), and a continuum of households i of mass N_m, each with one child entering a given grade. A household is described by its residence, which determines the distance d_{ij} to every school, by observable characteristics Z_i (the mother's education and a proxy for income), by whether the child is a Venezuelan migrant, and by unobserved tastes. Households choose one school or the outside option $j = 0$, which we interpret as leaving the registered system: the margin that Section 7 shows the shock operates on.

Time is discrete and a period is a school year. Within a year the sequence is: (i) public schools' staffed sections s_j and private schools' prices p_j and capacities K_j are fixed, having been set at the end of the previous year; (ii) households apply and are admitted or rationed; (iii) enrollment x_j is realized and the school's crowding and composition follow; (iv) at the end of the year, public schools receive posts for the next year according to the resourcing rule, and private schools update prices and capacity. The migration shock is an unanticipated increase in the mass of households between (i) and (ii). This timing is what makes capacity a constraint rather than a choice in the year of the shock.

8.2 Households

Household i values school j according to

$$u_{ij} \;=\; \alpha_i\, p_j \;+\; \beta_i\, q_j \;+\; \gamma_i\, n_j \;+\; \kappa_i\, z_j \;+\; \lambda_i\, d_{ij} \;+\; x_j'\theta \;+\; \xi_j \;+\; \varepsilon_{ij}, \qquad u_{i0} = \varepsilon_{i0}, \tag{4}$$

where p_j is the annual fee (zero in public schools), q_j is the school's value added, n_j is crowding measured as pupils per section, z_j is the share of migrant pupils in the school, d_{ij} is distance, x_j collects other observed characteristics, ξ_j is a school unobservable, and ε_{ij} is an idiosyncratic taste with the type-I extreme value distribution. The coefficient vector $\vartheta_i = (\alpha_i, \beta_i, \gamma_i, \kappa_i, \lambda_i)$ varies across households,

$$\vartheta_i \;=\; \bar{\vartheta} \;+\; \Pi Z_i \;+\; \Sigma \nu_i, \qquad \nu_i \sim F_\nu, \tag{5}$$

so that price sensitivity, the taste for quality and the tolerance for crowding differ with the mother's education and income, and unobserved heterogeneity ν_i generates the substitution patterns that a plain logit rules out (Berry et al., 1995; Berry and Haile, 2021).

[5] The ten markets are Lima, split into four subregions, and the six next-largest cities: Callao, Arequipa, Trujillo, Cusco, Huancayo and Ica. Together they hold 38 percent of primary enrollment and 82 percent of the Venezuelan pupils in 2019 (Section 4).

Three terms deserve comment. The crowding term $\gamma_i n_j$ lets families dislike large sections for their own sake; the reduced form finds that switching does not respond to the school's slack, and the estimate of γ will tell us whether that is because families do not value smaller sections or because they cannot act on it. The composition term $\kappa_i z_j$ is a reduced-form preference over peers. We include it because the switching results cannot be reproduced without it: families that leave an exposed school move to schools with fewer migrants (Section 7), and a model in which composition does not enter utility would have to attribute those moves to quality and price alone. We do not interpret κ as a taste against migrants. It absorbs whatever families expect a change in composition to bring, including language, peer learning, information about the school and the anticipated crowding that Section 6 documents. It is also a preference at the moment of choice, not a statement about what contact produces later: Bailey et al. (2026) find that natives who shared a school cohort with a refugee form more ties with refugees as adults. What matters for the counterfactuals is the elasticity of choices to composition, not its source. Finally, q_j is the school's contribution to learning, which we measure as a value-added fixed effect from the student-level achievement data and treat as a sufficient statistic for the quality families perceive; we discuss the assumption in Section 9.

Let $\delta_j = \bar{\alpha} p_j + \bar{\beta} q_j + \bar{\gamma} n_j + \bar{\kappa} z_j + x_j' \theta + \xi_j$ be the mean utility of school j and μ_{ij} the deviation from it induced by Z_i, ν_i and distance. The probability that household i demands school j is

$$\sigma_{ij}(\delta, \mu) \;=\; \frac{\exp(\delta_j + \mu_{ij})}{1 + \sum_{k \in \mathcal{J}_m} \exp(\delta_k + \mu_{ik})}, \tag{6}$$

and demand for school j is $D_j = N_m \int \sigma_{ij} \, dF(Z_i, \nu_i, d_i)$. Crowding n_j and composition z_j are outcomes of the choices of all households. We assume that households hold correct expectations about them, so that in equilibrium

$$n_j = \frac{x_j}{s_j}, \qquad z_j = \frac{\int \sigma_{ij} \, \mathbb{1}[i \text{ migrant}] \, dF}{\int \sigma_{ij} \, dF}, \tag{7}$$

where x_j is realized enrollment after rationing. Equations (6) and (7) define a fixed point in (n, z) for given (δ, μ), of the kind studied by Allende (2019) and Bau (2022) for peer composition; crowding adds a second endogenous attribute.

8.3 Public schools: a resourcing rule and a rationing rule

A public school does not maximize anything in our model. Its behavior is described by two rules that the institutional setting of Section 2.3 imposes and that the reduced form measures.

Capacity and crowding. School j enters the year with s_j staffed sections, one teacher each, and a norm of $\bar{n} = 30$ pupils per section in urban schools. Its nominal capacity is $\bar{n}s_j$. If demand D_j is below nominal capacity the school enrolls everyone and sections are smaller than the norm; if it is above, the school either enlarges its sections beyond $\bar{n}$ or rations. We let the school enlarge sections up to a tolerance $\bar{n} + \tau$, with $\tau = 5$ in the norm, and ration beyond that:

$$x_j = \min\{D_j,\ (\bar{n} + \tau)s_j\}, \qquad n_j = x_j/s_j. \tag{8}$$

Quality. Quality is what the school adds to a child's learning. We define it through the achievement production function

$$Y_{ijt} \ = \ X'_{ijt}\beta \ + \ \theta_{jt} \ + \ \varepsilon_{ijt}, \tag{9}$$

where Y_{ijt} is the end-of-year achievement of child i in school j in year t, X_{ijt} contains her achievement the year before and her characteristics, and θ_{jt} is the school's value added. Quality in utility is $q_j = \theta_j$, the value added a family expects, and it responds to crowding through a technology

$$\theta_{jt} \ = \ \bar{q}_j \ - \ \phi\, n_{jt}, \tag{10}$$

where $\bar{q}_j$ is the school's underlying value added and $\phi \geq 0$ is the crowding penalty. This is the channel through which a demand shock lowers the quality of the schools that absorb it, and it is the only channel: teachers, curricula and buildings do not change within the year. Section 7.4 estimates ϕ and finds it not distinguishable from zero for the section-size changes the shock produced; the technology is kept because the counterfactuals that add capacity or remove it move section sizes over a wider range, and because the quality accounting requires it even when $\phi = 0$. Quality is also the metric in which the counterfactuals of Section 9.4 are reported: the value added of the school each child attends, and the loss of it that crowding imposes on the children who stay.

Resourcing. At the end of the year the school requests posts for the following year on the basis of its realized enrollment, and the local education authority grants a fraction of what the norm implies. We write the rule as

$$s'_j \ = \ s_j \ + \ \rho \cdot \left\lfloor \frac{(x_j - \bar{n}s_j)_+}{\bar{n}} \right\rfloor, \tag{11}$$

where s'_j is next year's number of sections and $\rho \in [0,1]$ is the resourcing rate: the fraction of the sections that full compliance with the norm would add that the system actually adds. With $\rho = 1$ the school returns to the norm after one year; with $\rho = 0$ sections never adjust and crowding is permanent. Section 6 estimates ρ from the response of sections and teaching posts to the migration shock. In urban public schools nationally, every 5.6 pupils that arrived brought 0.024 new sections and 0.03 posts, which at the point estimates is $\rho \approx 0.15$, and neither response is distinguishable from zero; in the ten largest markets the point estimate is about 0.4, with a confidence interval that spans zero and one. Within the three years of the shock the rule in (11) barely operated, and the counterfactual with $\rho = 1$ is therefore far from the observed allocation.

Rationing. When D_j exceeds $(\bar{n} + \tau)s_j$, some households that demand the school do not get in. Section 7 shows that this margin is not about who is admitted at the door only; full schools also fail fewer pupils and lose more of their incumbents from the registered system. We model rationing as a rule that acts on the excess demand $e_j = D_j - (\bar{n} + \tau)s_j$: a fraction π_R of it is met by not holding back pupils who would otherwise repeat the grade, and a fraction π_X by incumbents who leave the registered system, with the remainder refused at entry and re-optimizing among the other schools. The pair (π_R, π_X) is estimated from the retention and exit responses in Section 7. Rationed households are reassigned according to their preferences over the remaining schools with capacity, which generates the spillover from full schools to their neighbors that the model needs to reproduce the switching patterns.[6]

8.4 Private schools

A private school j has a capacity K_j, a fee p_j and a quality q_j, and chooses whom to admit. Within the year of the shock, K_j and p_j are fixed: Section 6 shows that fees, teachers, sections and credentials in private schools did not respond to local exposure once school fixed effects are included, so we treat the private sector as a set of fixed-capacity, fixed-price providers in the short run,

$$x_j = \min\{D_j, K_j\}, \tag{12}$$

[6]The reassignment rule is the deferred-acceptance-like sequence used in Dinerstein et al. (2023): rationed households are removed from the full school and their demand is redistributed across schools according to (6) restricted to the schools with remaining places, iterating until no school exceeds its capacity. Because public schools in Peru have no priorities other than siblings and proximity, we treat rationing as random among applicants conditional on Z_i.

with admission among applicants governed by a selection rule that favors non-migrant and higher-Z households.[7]

Over the longer run, a private school chooses p_j and q_j to maximize profit,

$$\pi_j = (p_j - c_j)\, x_j(p, q), \qquad c_j = w_j'\gamma + \gamma_q q_j + \omega_j, \tag{13}$$

where marginal cost depends on local input prices w_j, on quality, and on an unobservable ω_j. The first-order conditions are the price condition and the quality condition

$$p_j \;=\; c_j + \frac{x_j}{-\partial x_j/\partial p_j}, \qquad (p_j - c_j)\,\frac{\partial x_j}{\partial q_j} \;=\; \gamma_q\, x_j, \tag{14}$$

so that a private school raises quality until the enrollment it attracts, valued at the margin, equals its marginal cost (Allende, 2019). We use these conditions in the counterfactuals in which private schools re-optimize (vouchers, and the long-run response to the shock), not to rationalize the short-run data, where the response is zero.

8.5 Equilibrium

Given the primitives $(\bar{\vartheta}, \Pi, \Sigma, \phi, \rho, \pi_R, \pi_X)$, the school attributes $(\bar{q}_j, \xi_j, x_j, d_{ij})$, the capacities (s_j, K_j), the fees p_j and the mass and composition of households, a within-year equilibrium is a vector of enrollments x, crowding n, composition z and rationing outcomes such that (i) each household demands its preferred school given (n, z, p, q); (ii) enrollment in each school satisfies (8) or (12); (iii) rationed households are reassigned by the rationing rule; and (iv) (n, z) satisfy (7) at the realized enrollment. The equilibrium is a fixed point in (n, z) nested inside the rationing loop; we compute it by iterating on (7) for given assignments and on the assignments for given (n, z), and we check uniqueness numerically by starting from the no-rationing allocation and from the pre-shock allocation. A long-run equilibrium adds the resourcing rule (11) for public schools and the first-order conditions of (13) for private schools.

The model nests the cases the literature has studied. With $\rho = 1$, $\tau = 0$ and no rationing it is the demand model of Allende (2019) with public schools that never bind, the assumption maintained in Dinerstein et al. (2023). With $\tau = 0$, a deferred-acceptance assignment in place of the rationing rule, and private schools at capacity choosing quality to attract a more profitable composition rather than more pupils, it is the capacity-constrained

[7] The 2019 school census shows that private schools that turned applicants away cited "a preference for a low teaching load" in one case out of five, against one in fifty for public schools, which is the observable trace of selection. We model the admission rule as a probability of admission that depends on migrant status and Z_i, estimated from the composition of admitted versus applying households in the structural sample.

counterfactual of Allende et al. (2026); the difference in our setting is that no mechanism allocates the scarce seats, so the rationing rule and its consequences for retention and exit are objects to be estimated rather than imposed. With $\kappa = 0$ it is a pure quality-and-crowding model, and the migration shock affects incumbents only through n_j and q_j. The estimates in Section 9 will say which of these restrictions the data reject.

8.6 What is estimated and what is read from the data

The model has three groups of objects. The preference parameters $(\bar{\vartheta}, \Pi, \Sigma)$ are estimated from choices. The public-sector rules are measured outside the model: ρ from the supply-side regressions, (π_R, π_X) from the demand-side regressions, and ϕ from the value-added regressions (9)–(10), with section size instrumented by the shock. Value added itself, θ_{jt}, is estimated on the tested grades from the national assessment and the report-card marks, and it is the quality measure that enters both the choice model and the welfare accounting. The capacities s_j and K_j, the norm $\bar{n}$ and the tolerance τ are taken from the data and the legal sources. Nothing about private-school costs needs to be estimated for the short-run counterfactuals, which is deliberate: the paper's contribution is to measure the capacity constraint and the behavior it induces, not the cost function of private schools, and the reduced form shows that the latter did not move.

9 Estimation, Identification and Counterfactual Policies

This section sets out how the model of Section 8 is taken to the data and what we will do with it. The structural estimates are not yet in this draft; the tables below are placeholders with the layout the estimates will fill.

9.1 Sample and measurement

The structural sample is the set of children entering first grade in 2019 in the ten largest urban markets, about [N] households choosing among [J] schools. First grade is the largest cohort and the grade at which the school choice is made afresh, and 2019 is the year of greatest Venezuelan presence. We restrict to markets because the choice set must be a set of schools a household could plausibly attend; within a market the choice set is every primary school within [R] miles of the household's residence, which we recover for the subsample of children matched to the 2017 population census.[8]

[8]The match rate is 73 percent of 2019 enrollment nationally; the census provides a geocoded residence for about a third of the matched children, concentrated in the urban districts that the migrants settled in.

School attributes are measured as follows. Value added q_j is the school fixed effect in a regression of end-of-year achievement on lagged achievement and student characteristics, estimated on the tested grades and treated as a sufficient statistic for the quality families perceive. Crowding n_j is pupils per staffed section in the year of choice, from the section file. Composition z_j is the share of migrant pupils across all primary grades of the school, not in the child's own grade, so that an individual choice cannot move it. Fees p_j are annual private-school fees from the Ministry's cost survey. Distance is great-circle distance from residence to school. Household characteristics Z_i are the mother's education, from the census and from the household questionnaire that accompanies the national test, and an income proxy from the census.

9.2 Identification

The preference parameters are identified from the covariation of choices with school attributes across households, conditional on school unobservables ξ_j, which are recovered by inverting the shares (Berry et al., 1995). Three attributes are endogenous in the sense that ξ_j is correlated with them: fees, composition and crowding. We use one source of variation for each.

For composition and crowding we use the migration shock itself. The shift-share instrument of Section 5 predicts each school's exposure to Venezuelan pupils from the 2014 settlement pattern and the national inflow, and it shifts both the migrant share z_j and, through the mechanism documented in Section 6, pupils per section n_j. Because the instrument moves both attributes, we separate them with the pre-shock slack of the school: at a given exposure, a school that started with small sections absorbs the inflow with little change in n_j, whereas a full school absorbs it entirely through n_j. The interaction of exposure with baseline slack is the excluded instrument for crowding, and exposure itself is the excluded instrument for composition. This is the "double duty" of the shock: the same variation that identifies the reduced-form responses identifies the structural elasticities, which is what allows the model to be held to the reduced form.

For fees we have no discontinuity of the kind available in voucher settings. We rely on cost shifters at the school level (the local wage of teachers, from the teacher payroll and the household survey) and on the differentiation instruments of Gandhi and Houde (2019), which exploit the position of a school in attribute space relative to its competitors. The random coefficients are identified from the way substitution patterns vary with Z_i and with distance, following Berry and Haile (2021).

See [Appendix D] for the match and its selection.

Two objects are measured outside the choice model and enter it as constants: the resourcing rate ρ, from the supply-side regressions of Section 6, and the rationing rule (π_R, π_X), from the demand-side regressions of Section 7. The crowding penalty ϕ in the quality technology is estimated in Table 14, from the fourth-grade scores of the national assessment with the pupil's second-grade score two years earlier, school and year effects, and section size instrumented by the shift-share exposure; the estimate is not distinguishable from zero.

9.3 Estimation

Estimation proceeds in three steps. First, we estimate value added q_j and the crowding penalty ϕ from the student-level achievement data of the national assessment (Section 7.4; the school-by-year value-added estimates for 2016 and 2018 are already built). Second, we estimate the demand parameters by the generalized method of moments, nesting the fixed point in (n, z) of equation (7) inside the share inversion, so that the composition and crowding that households respond to are the ones the model generates. Third, we recover the private-school marginal costs from the price first-order condition, for use in the counterfactuals in which private schools re-optimize. Standard errors are computed by the block bootstrap over markets.

Before using the model we check that it reproduces the reduced form. We simulate the 2017–2019 inflow through the estimated model, holding public capacity at its 2016 level and applying the resourcing and rationing rules, and compare the simulated switching, retention and exit responses to those in Section 7. A model that could not reproduce the switching effect of [X] percentage points per ten points of migrant share, or its concentration in schools with fewer migrants, would not be a credible basis for counterfactuals.

Table 18: Demand estimates (placeholder)

	Mean	$\times$ Mother HE	$\times$ Income	SD
Fee (thousand soles)	[ ]	[ ]	[ ]	[ ]
Value added (SD)	[ ]	[ ]	[ ]	[ ]
Pupils per section	[ ]	[ ]	[ ]	[ ]
Migrant share	[ ]	[ ]	[ ]	[ ]
Distance (km)	[ ]	[ ]	[ ]	[ ]
Households		[ ]		
Schools		[ ]		

Notes: To be filled with the GMM estimates of (4)–(5). Standard errors from the block bootstrap over markets.

Table 19: Public-sector rules and technology (placeholder)

	Estimate	Source
Resourcing rate ρ, urban public	[ ]	Section 6
Resourcing rate ρ, ten markets	[ ]	Section 6
Rationing: retention share π_R	[ ]	Section 7
Rationing: exit share π_X	[ ]	Section 7
Crowding penalty ϕ (SD per pupil)	0.025 (0.017)	Table 14

Notes: To be filled. The norm $\bar{n} = 30$ and tolerance $\tau = 5$ are taken from the legal sources in Appendix E.

9.4 Counterfactual policies

The counterfactuals ask what the same inflow would have done under different rules, and what a policy maker who anticipates such a shock could do. Each is computed as a within-year equilibrium of Section 8.5, followed by one application of the resourcing rule, and reported for the incumbent and migrant children separately, in terms of the value added of the school attended (the quality measure of equations (9)–(10), so that the crowding loss $\phi\,\Delta n_j$ is part of it), crowding, the share who switch, the share rationed out, and consumer surplus by mother's education. Reporting outcomes in units of value added, that is in test-score standard deviations, lets the counterfactuals be compared with the achievement effects in the literature without a price on learning.

1. *No shock.* The 2019 market without the Venezuelan households. This is the benchmark against which the cost of the shock to incumbents is measured.

2. *Full compliance with the norm* ($\rho = 1$, $\tau = 0$). Every public school receives the posts the norm implies in the year of the shock. This isolates the cost of the lagged, partial resourcing that the data show.

3. *Resources that follow pupils.* A per-pupil transfer to public schools proportional to the inflow, spent on posts; equivalent to $\rho = 1$ with a one-year lag, plus the fiscal cost.

4. *Vouchers for private seats.* A subsidy that lowers the private fee for migrant or for all households, with private capacities K_j fixed in the short run and with private schools re-optimizing prices in the long run. The comparison shows how much of a voucher is absorbed by rationing and by prices when the private sector is at capacity. It is the analogue, for a price subsidy, of the congestion-versus-supply-response decomposition in Allende et al. (2026), where two fifths of a demand shift toward better schools was lost to congestion and recovered through schools' quality response; here the evidence of Section

6 is that the within-school quality and capacity response of private schools was zero, so the congestion term is the one that survives.

5. *Placement of migrants.* A rule that assigns migrant children to schools with slack within their district, rather than to the nearest school with a seat. This uses the model's estimate of the crowding and composition responses to ask whether spreading the inflow lowers its cost to incumbents. Spreading the inflow also changes which incumbents share a classroom with a migrant, and in what size of class, and that has a value our measure does not count. Bailey et al. (2026) show that German natives placed by a birth-date cutoff in a school cohort with a Syrian refugee have 40 percent more Syrian friends by age 21 than natives in the adjacent cohort, and that the effect is a third smaller in cohorts one standard deviation larger. We therefore also report, under each rule, the share of incumbents with a migrant classmate and the size of their sections, so that a reader who values contact can weigh it without our pricing it.

Table 20: Counterfactual outcomes (placeholder)

	Quality	Pupils/section	Switch (%)	Rationed (%)	Surplus
Observed 2019	[ ]	[ ]	[ ]	[ ]	[ ]
No shock	[ ]	[ ]	[ ]	[ ]	[ ]
Full compliance	[ ]	[ ]	[ ]	[ ]	[ ]
Resources follow pupils	[ ]	[ ]	[ ]	[ ]	[ ]
Vouchers, fixed capacity	[ ]	[ ]	[ ]	[ ]	[ ]
Vouchers, long run	[ ]	[ ]	[ ]	[ ]	[ ]
Migrant placement	[ ]	[ ]	[ ]	[ ]	[ ]

Notes: To be filled. Outcomes are averages over incumbent first-graders in the ten markets; surplus in soles per household relative to the no-shock benchmark.

10 Conclusion

A school system that plans a year ahead cannot absorb an unanticipated inflow without straining something. We have shown what strained in Peru. The migrants went to the cities and, within them, to public schools that were already full. Those schools grew by enlarging their sections, past the norm and toward its tolerance, while the posts that the rule ties to sections did not arrive within the three years of the shock. Private schools, which could have raised fees, added sections or hired, did none of these things. Families in the exposed grades moved to other schools, toward schools with fewer migrants and smaller sections, and a full school did not stop them. Capacity showed up elsewhere: in the full public schools that received migrants, fewer pupils were held back and more pupils who had passed left the registered system for good.

Three lessons follow for policy. First, the binding constraint in a public system with a staffing norm is the post, not the classroom, and a rule that allocates posts on last year's enrollment guarantees that a demand shock is absorbed by crowding in the year it arrives. A resourcing rule that responds within the year would have changed the size of every section in the exposed schools; the model of Section 8 is built to say by how much, and at what fiscal cost. Second, the obligation of public schools to accept every child is real only where there are seats. Where there are none, the school says so, and the family looks elsewhere; the children who bear the cost of a full school are not the ones who leave for another school but the ones who stay in it. Third, the private sector did not act as the safety valve that a voucher policy would count on. It was at capacity where the migrants settled, and it did not expand. A subsidy for private seats in that setting is a transfer to the families who already hold them unless capacity responds, and the counterfactuals we set out are designed to measure that response.

Two questions remain open. We do not know where the children who left the registered system went; the register covers every licensed school, and the profile of the leavers does not fit dropping out for work, so emigration, unlicensed schooling and non-attendance are all possible, and only a link to migration and civil records would tell them apart. And the structural estimates that will turn the reduced-form responses into welfare and policy counterfactuals are not yet in this draft. The identification is in hand; the estimation is the next step.

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A Additional figures

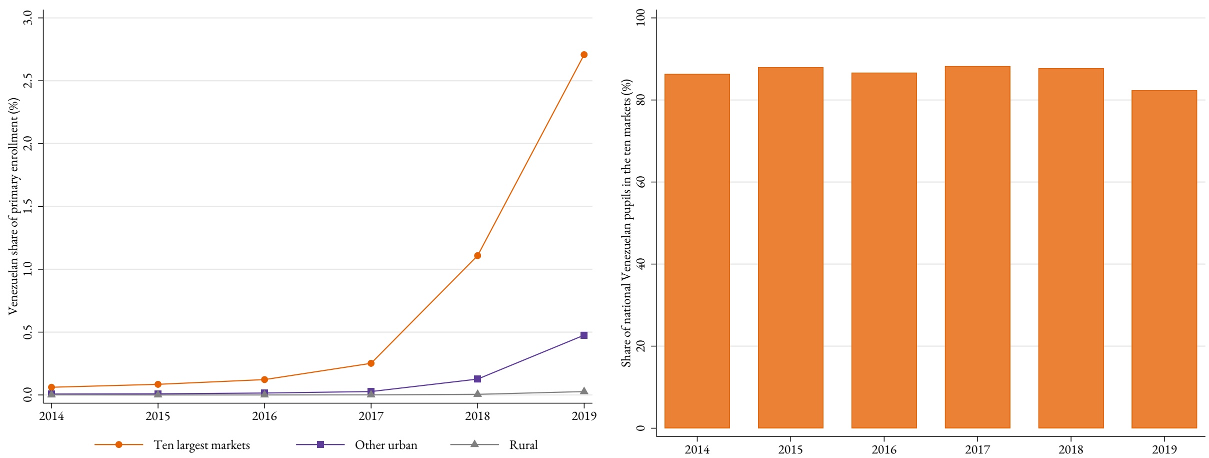

Figure 1: Where the Venezuelan pupils are
Notes: Left: Venezuelan pupils as a share of primary enrollment, by market group. Right: share of the country's Venezuelan primary pupils enrolled in the ten largest markets. All primary schools, SIAGIE.

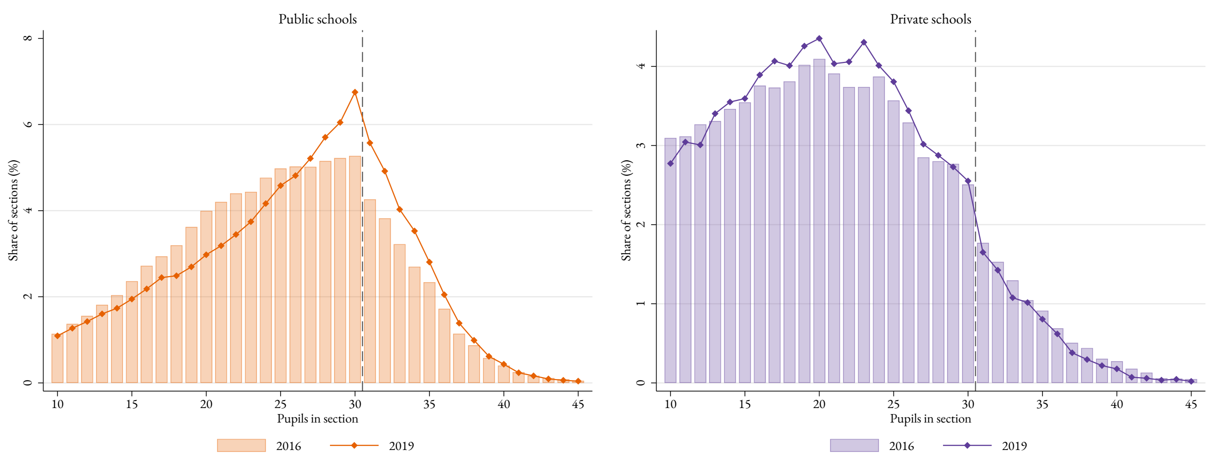

Figure 2: The distribution of section sizes in 2016 and 2019, urban fully staffed schools
Notes: Share of primary sections by number of pupils, in schools classified as urban *polidocente completo* in 2016. Bars: 2016; line: 2019. The dashed line marks the 30-pupil norm.

B School responses: additional results

This appendix reports the school-side estimates behind Section 6: the same outcomes without fixed effects and with district fixed effects (Tables 22 and 23), the side-by-side comparison of

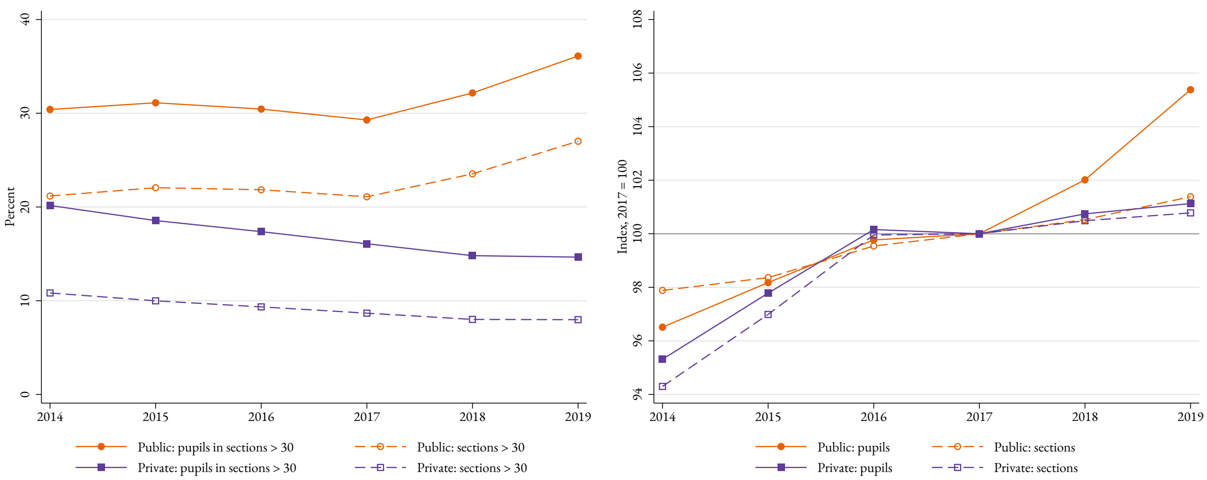

Figure 3: Sections above the norm and enrollment versus sections, 2014–2019
Notes: Left: share of sections with more than 30 pupils and share of pupils sitting in such sections, by sector. Right: pupils and sections indexed to 2017 = 100. Urban fully staffed schools.

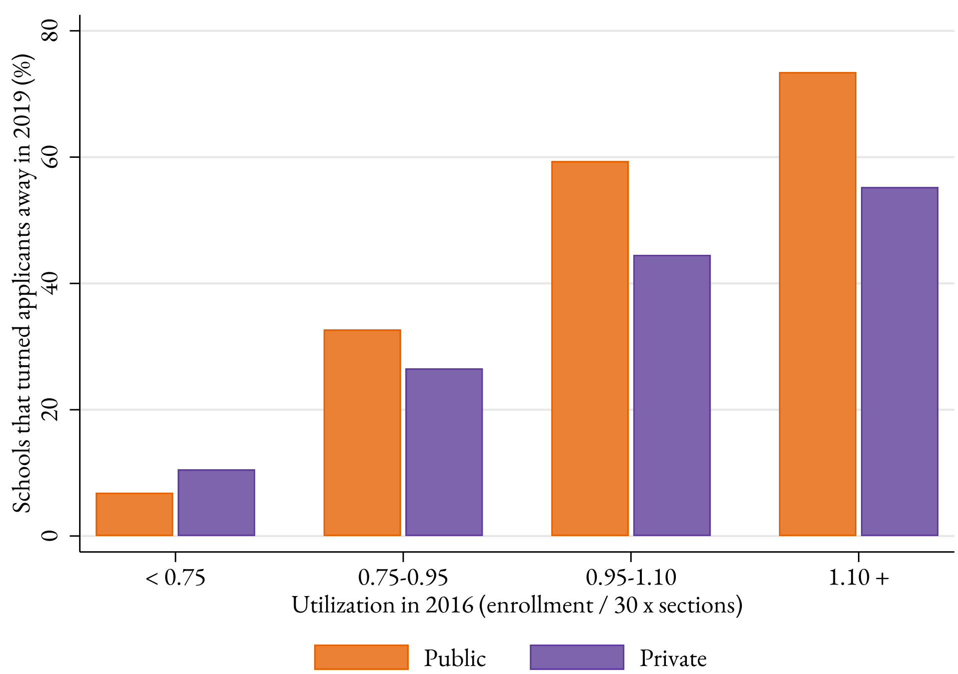

Figure 4: Schools that turned applicants away in 2019, by utilization in 2016
Notes: Share of urban fully staffed primary schools reporting in the 2019 school census that some applicants did not obtain a place, by bin of 2016 utilization (enrollment over thirty times the number of sections).

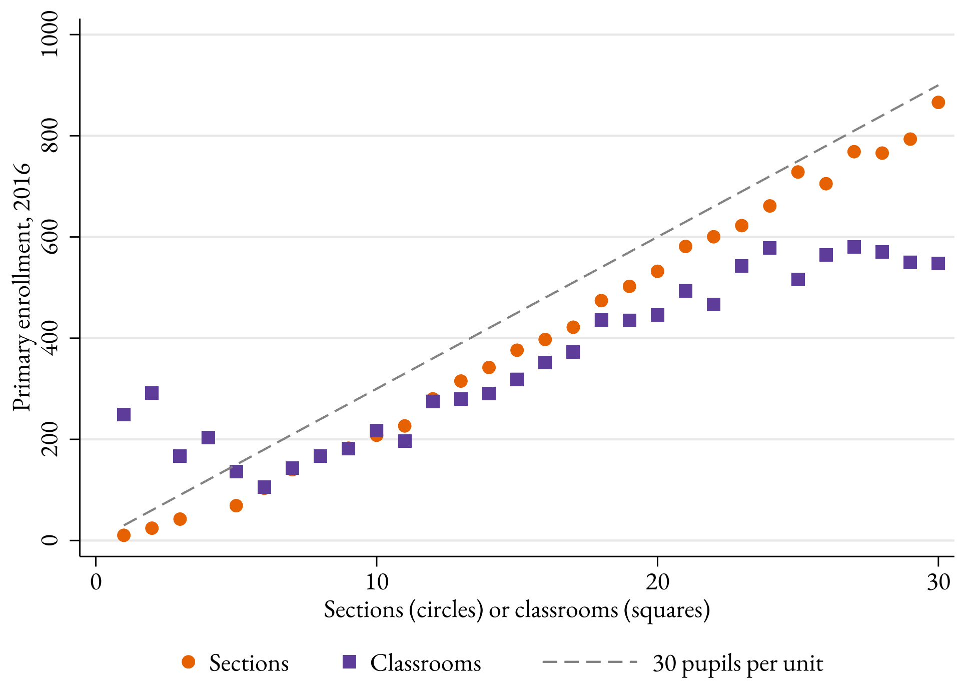

Figure 5: Enrollment against sections and against classrooms, 2016

Notes: Mean primary enrollment by number of sections (circles) and by number of classrooms in the building (squares), urban fully staffed public schools that are the only school in their building. The dashed line is 30 pupils per unit.

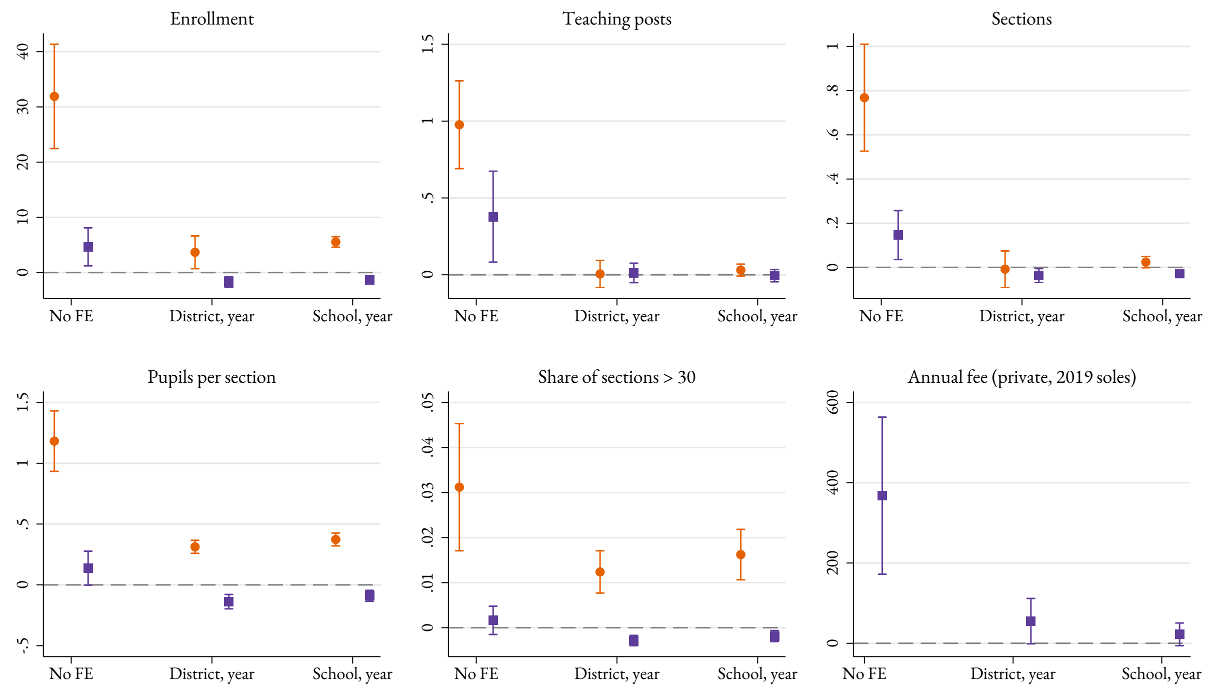

Figure 6: School responses with and without fixed effects
Notes: 2SLS coefficient on exposure with 95 percent confidence intervals, urban fully staffed schools, by specification. Circles: public; squares: private. The fee is observed for private schools only.

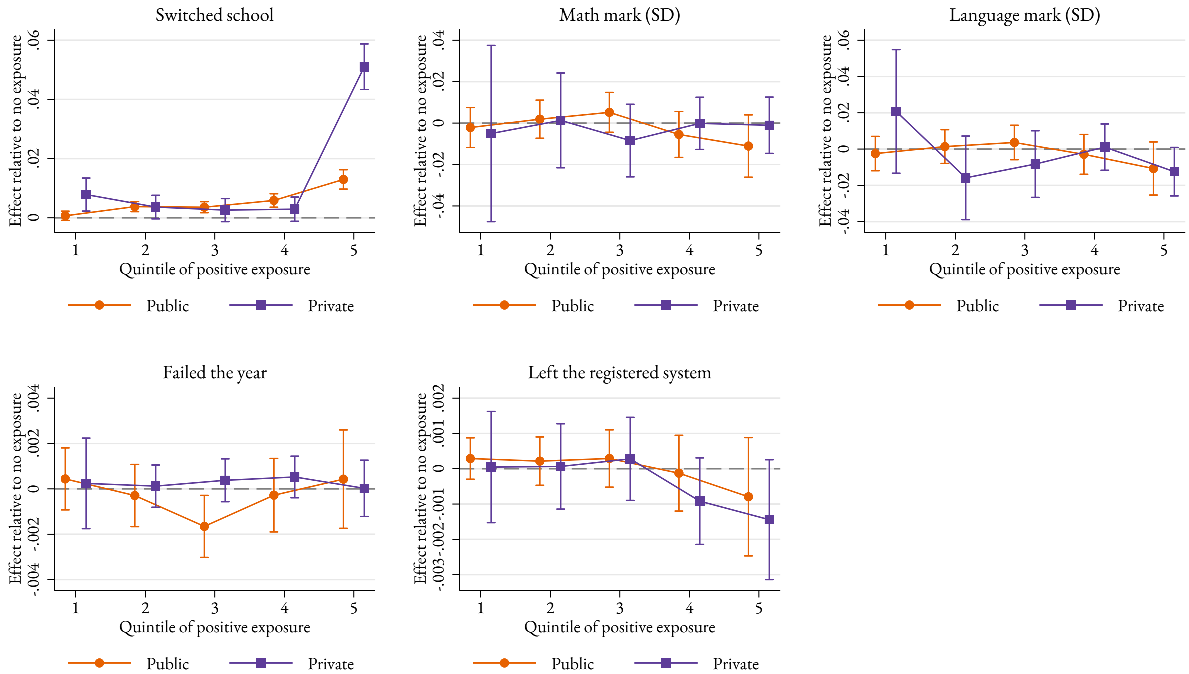

Figure 7: Family responses by quintile of exposure
Notes: Coefficients on quintile indicators of the migrant share among exposed cells, relative to unexposed cells, with 95 percent confidence intervals. Urban fully staffed baseline schools.

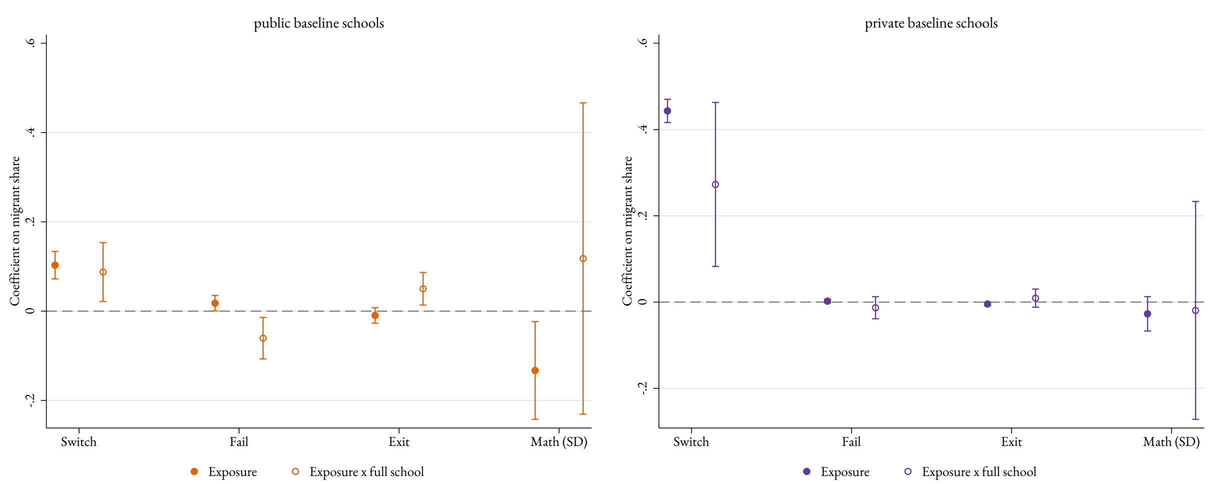

Figure 8: Family responses in full and non-full schools
Notes: Coefficient on the migrant share (filled) and on its interaction with the indicator that the baseline school was full in 2016 (hollow), with 95 percent confidence intervals. Urban fully staffed baseline schools.

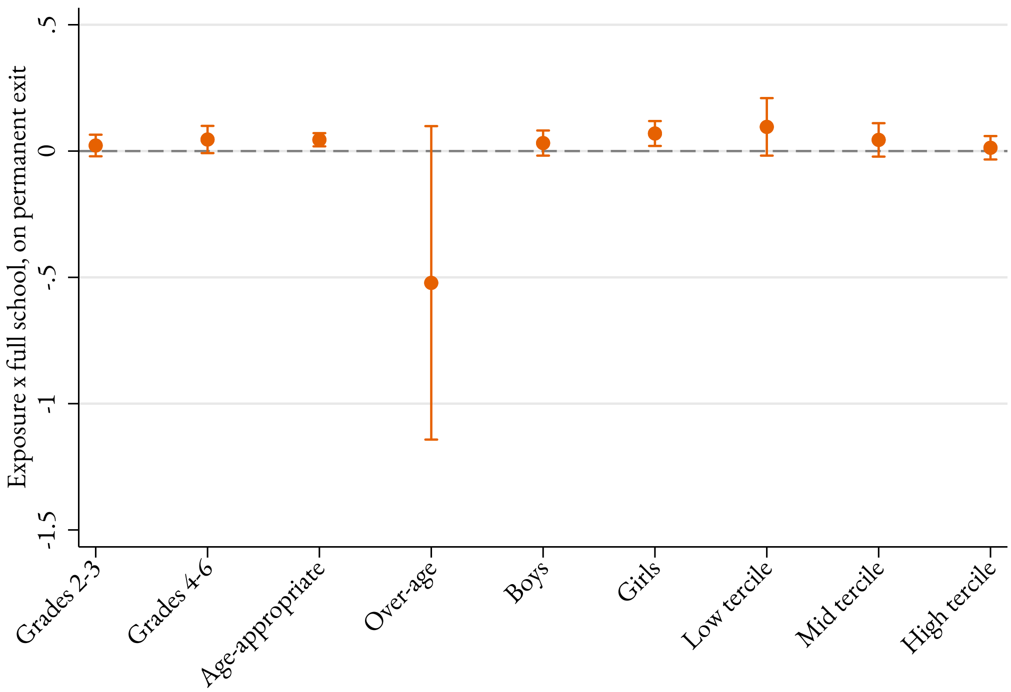

Figure 9: The exit response in full public schools, by pupil group

Notes: Coefficient on the interaction of the migrant share with the full-school indicator, on permanent exit, estimated separately by pupil group, with 95 percent confidence intervals. Public urban fully staffed baseline schools, 2017–2018.

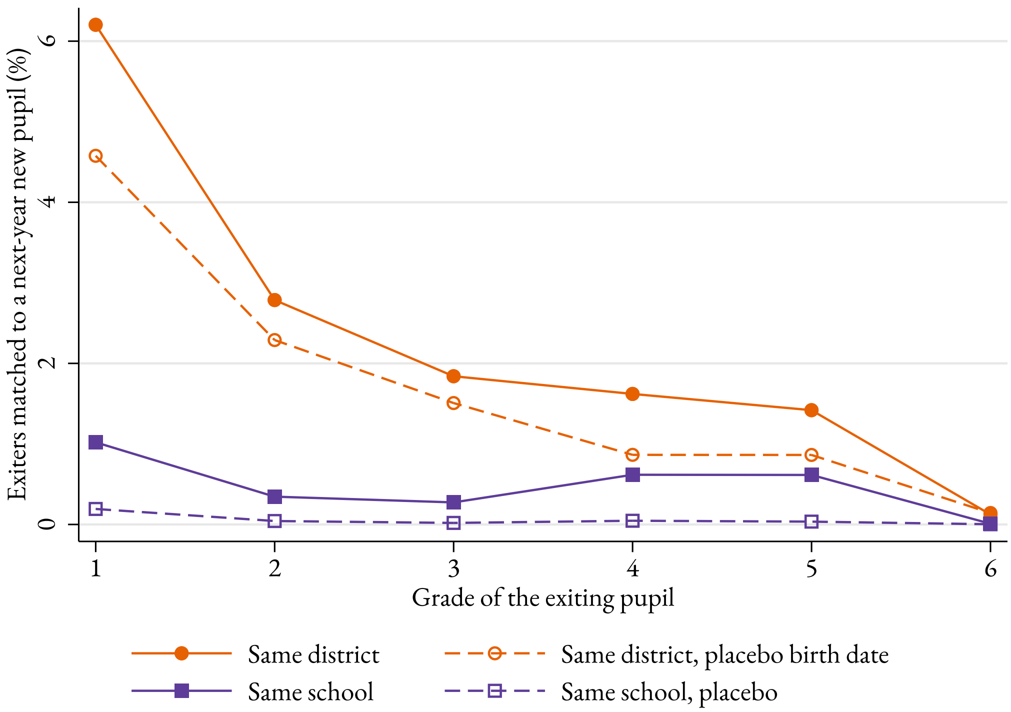

Figure 10: Exiters matched to new pupils the following year, actual and placebo
Notes: Share of exiting pupils for whom a pupil with the same sex and birth date first appears in SIAGIE the next year in the same district or the same school, by grade of the exiter; the placebo shifts the birth date by seven days.

specifications (Table 21), the full list of outcomes for each sample (Tables 26–29), the first stage by radius (Table 31) and the private-fee grid (Table 32).

Table 21: School responses with and without fixed effects, urban fully staffed primary schools

	Public			Private		
	No FE	District, year	School, year	No FE	District, year	School, year
Enrollment	31.90***	3.67**	5.55***	4.66***	-1.71***	-1.35***
	(4.82)	(1.51)	(0.49)	(1.75)	(0.51)	(0.23)
Teaching posts	0.976***	0.006	0.031	0.378**	0.012	-0.005
	(0.146)	(0.045)	(0.020)	(0.151)	(0.032)	(0.020)
Classroom teachers	0.802***	-0.046	-0.015	0.261***	-0.014	-0.024
	(0.126)	(0.043)	(0.022)	(0.098)	(0.023)	(0.025)
Contract teachers	-0.001	-0.092***	-0.076***	0.346**	0.116**	0.104*
	(0.042)	(0.027)	(0.025)	(0.137)	(0.053)	(0.054)
Sections	0.768***	-0.008	0.024*	0.146***	-0.037**	-0.028***
	(0.123)	(0.042)	(0.013)	(0.056)	(0.016)	(0.009)
Pupils per section	1.182***	0.313***	0.373***	0.137*	-0.138***	-0.090***
	(0.127)	(0.027)	(0.027)	(0.071)	(0.030)	(0.023)
Share of sections > 30	0.031***	0.012***	0.016***	0.002	-0.003***	-0.002***
	(0.007)	(0.002)	(0.003)	(0.002)	(0.001)	(0.001)
Share of pupils in > 30	0.034***	0.012***	0.016***	0.001	-0.003***	-0.002***
	(0.007)	(0.002)	(0.003)	(0.002)	(0.001)	(0.001)
Any section > 30	0.073***	0.012***	0.015***	0.000	-0.010***	-0.007***
	(0.008)	(0.003)	(0.004)	(0.003)	(0.002)	(0.002)
Afternoon shift	0.049***	0.000	0.000	-0.016***	-0.001	-0.000
	(0.005)	(0.002)	(0.002)	(0.003)	(0.001)	(0.001)
Classrooms	1.234***	0.060	0.093	0.728**	-0.281**	-0.252
	(0.167)	(0.078)	(0.077)	(0.288)	(0.129)	(0.192)
Monthly fee (2019 S/)	–	–	–	33.55***	5.10*	2.13
				(9.03)	(2.63)	(1.35)
Annual fee (2019 S/)	–	–	–	367.81***	55.27*	22.36
				(99.82)	(28.82)	(14.40)
log annual fee	–	–	–	0.121***	0.015**	-0.004
				(0.020)	(0.007)	(0.006)
Share with pedagogical degree	0.003***	0.002***	0.002***	-0.022***	0.003	0.002
	(0.001)	(0.000)	(0.000)	(0.004)	(0.002)	(0.002)
Share with any degree	0.003***	0.002***	0.001***	-0.022***	0.002	0.001
	(0.001)	(0.000)	(0.000)	(0.004)	(0.002)	(0.002)

Notes: Each cell is the 2SLS coefficient on exposure, the number of Venezuelan primary pupils per school among the other primary schools within three miles, instrumented with the shift-share prediction that combines the neighborhood's share of national Venezuelan pupils in 2014 with the national count in the year. School-years 2015–2019. Standard errors clustered by district in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. First-stage F is the Kleibergen–Paap rk Wald statistic. Sample: urban *polidocente completo* primary schools. Fees are observed for private schools only. Contract teachers are observed for public schools only.

C Family responses: additional results

This appendix reports the family-side estimates behind Section 7 for the other samples and specifications: all baseline schools and the ten markets (Tables 33 and 34), the version without the baseline mark that includes first graders (Table 35), the destinations of switchers

Table 22: School responses without fixed effects (urban fully staffed)

	Enrollment	Teaching posts	Classroom teachers	Sections	Pupils per section	Share of sections > 30	Afternoon shift
Public schools							
Exposure	31.90***	0.976***	0.802***	0.768***	1.182***	0.031***	0.049***
	(4.82)	(0.146)	(0.126)	(0.123)	(0.127)	(0.007)	(0.005)
Mean of outcome	323.48	14.53	13.16	13.12	22.34	0.16	0.28
Observations	29,666	29,666	29,666	29,666	29,666	29,666	29,666
First-stage F	565.4	565.4	565.4	565.4	565.4	565.4	565.4
Private schools							
Exposure	4.66***	0.378**	0.261***	0.146***	0.137*	0.002	-0.016***
	(1.75)	(0.151)	(0.098)	(0.056)	(0.071)	(0.002)	(0.003)
Mean of outcome	138.44	8.20	7.35	7.40	16.59	0.06	0.08
Observations	29,380	29,380	29,380	29,380	29,380	29,380	29,380
First-stage F	508.9	508.9	508.9	508.9	508.9	508.9	508.9

Notes: Each cell is the 2SLS coefficient on exposure, the number of Venezuelan primary pupils per school among the other primary schools within three miles, instrumented with the shift-share prediction that combines the neighborhood's share of national Venezuelan pupils in 2014 with the national count in the year. School-years 2015–2019. Standard errors clustered by district in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. First-stage F is the Kleibergen–Paap rk Wald statistic. Sample: urban *polidocente completo* schools; year fixed effects only.

Table 23: School responses with district and year fixed effects (urban fully staffed)

	Enrollment	Teaching posts	Classroom teachers	Sections	Pupils per section	Share of sections > 30	Afternoon shift
Public schools							
Exposure	3.67**	0.006	-0.046	-0.008	0.313***	0.012***	0.000
	(1.51)	(0.045)	(0.043)	(0.042)	(0.027)	(0.002)	(0.002)
Mean of outcome	323.48	14.53	13.16	13.12	22.34	0.16	0.28
Observations	29,666	29,666	29,666	29,666	29,666	29,666	29,666
First-stage F	548.2	548.2	548.2	548.2	548.2	548.2	548.2
Private schools							
Exposure	-1.71***	0.012	-0.014	-0.037**	-0.138***	-0.003***	-0.001
	(0.51)	(0.032)	(0.023)	(0.016)	(0.030)	(0.001)	(0.001)
Mean of outcome	138.45	8.20	7.35	7.40	16.59	0.06	0.08
Observations	29,376	29,376	29,376	29,376	29,376	29,376	29,376
First-stage F	424.1	424.1	424.1	424.1	424.1	424.1	424.1

Notes: Each cell is the 2SLS coefficient on exposure, the number of Venezuelan primary pupils per school among the other primary schools within three miles, instrumented with the shift-share prediction that combines the neighborhood's share of national Venezuelan pupils in 2014 with the national count in the year. School-years 2015–2019. Standard errors clustered by district in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. First-stage F is the Kleibergen–Paap rk Wald statistic. Sample: urban *polidocente completo* schools; district and year fixed effects.

Table 24: How schools absorb the inflow: all primary schools, school and year fixed effects

	Enrollment	Teaching posts	Classroom teachers	Sections	Pupils per section	Share of sections > 30	Afternoon shift
Public schools							
Exposure	7.20***	0.121***	0.001	0.073***	0.431***	0.018***	0.000
	(0.44)	(0.019)	(0.019)	(0.012)	(0.027)	(0.003)	(0.002)
Mean of outcome	90.84	4.41	4.08	6.95	8.76	0.04	0.07
Observations	144,895	144,895	144,895	144,895	144,895	144,895	144,895
First-stage F	383.2	383.2	383.2	383.2	383.2	383.2	383.2
Private schools							
Exposure	-1.06***	-0.010	-0.034	-0.023***	-0.072***	-0.002***	-0.000
	(0.22)	(0.016)	(0.022)	(0.008)	(0.022)	(0.000)	(0.001)
Mean of outcome	114.27	6.97	6.28	6.89	14.20	0.05	0.09
Observations	38,850	38,850	38,850	38,850	38,850	38,850	38,850
First-stage F	351.1	351.1	351.1	351.1	351.1	351.1	351.1

Notes: Each cell is the 2SLS coefficient on exposure, the number of Venezuelan primary pupils per school among the other primary schools within three miles, instrumented with the shift-share prediction that combines the neighborhood's share of national Venezuelan pupils in 2014 with the national count in the year. School-years 2015–2019. Standard errors clustered by district in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. First-stage F is the Kleibergen–Paap rk Wald statistic. Sample: all primary schools; specifications include school and year fixed effects.

Table 25: How schools absorb the inflow: urban fully staffed schools in the ten markets

	Enrollment	Teaching posts	Classroom teachers	Sections	Pupils per section	Share of sections > 30	Afternoon shift
Public schools							
Exposure	2.35***	0.022	-0.017	0.022	0.093*	0.002	-0.001
	(0.83)	(0.038)	(0.045)	(0.023)	(0.052)	(0.004)	(0.005)
Mean of outcome	442.03	17.91	16.07	15.89	26.77	0.30	0.46
Observations	7,576	7,576	7,576	7,576	7,576	7,576	7,576
First-stage F	221.9	221.9	221.9	221.9	221.9	221.9	221.9
Private schools							
Exposure	-1.59***	0.002	-0.040	-0.035***	-0.111***	-0.002**	0.000
	(0.33)	(0.028)	(0.035)	(0.013)	(0.036)	(0.001)	(0.002)
Mean of outcome	142.19	8.33	7.46	7.52	16.78	0.06	0.05
Observations	20,281	20,281	20,281	20,281	20,281	20,281	20,281
First-stage F	162.7	162.7	162.7	162.7	162.7	162.7	162.7

Notes: Each cell is the 2SLS coefficient on exposure, the number of Venezuelan primary pupils per school among the other primary schools within three miles, instrumented with the shift-share prediction that combines the neighborhood's share of national Venezuelan pupils in 2014 with the national count in the year. School-years 2015–2019. Standard errors clustered by district in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. First-stage F is the Kleibergen–Paap rk Wald statistic. Sample: urban *polidocente completo* schools in the ten markets; school and year fixed effects.

Table 26: School responses, all outcomes: all primary schools, school and year fixed effects

	Public			Private		
	Coefficient	Mean	N	Coefficient	Mean	N
Enrollment		90.84	144,895		114.27	38,850
Teaching posts		4.41	144,895		6.97	38,850
Classroom teachers		4.08	144,895		6.28	38,850
Contract teachers		1.24	144,895		5.45	38,850
Share contract		0.36	123,058		0.99	30,450
Sections		6.95	144,895		6.89	38,850
Pupils per section		8.76	144,895		14.20	38,850
Share of sections > 30		0.04	144,895		0.05	38,850
Share of sections > 35		0.01	144,895		0.01	38,850
Share of pupils in > 30		0.04	144,895		0.06	38,850
Any section > 30		0.10	144,895		0.14	38,850
Largest section		12.13	144,895		19.39	38,850
Afternoon shift		0.07	144,895		0.09	38,850
Classrooms		5.84	140,366		11.74	30,146
Monthly fee (2019 S/)	–				195.89	29,751
Annual fee (2019 S/)	–				2145.32	29,751
log monthly fee	–				5.14	24,802
log annual fee	–				7.26	27,990
Enrollment fee	–				186.46	29,751
Entry fee	–				192.50	29,750
Share with pedagogical degree		0.95	123,058		0.73	38,059
Share with any degree		0.95	123,058		0.77	38,059
Share with unfinished studies		0.02	123,058		0.05	38,059

Notes: Each cell is the 2SLS coefficient on exposure, the number of Venezuelan primary pupils per school among the other primary schools within three miles, instrumented with the shift-share prediction that combines the neighborhood's share of national Venezuelan pupils in 2014 with the national count in the year. School-years 2015–2019. Standard errors clustered by district in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. First-stage F is the Kleibergen–Paap rk Wald statistic. Sample: all primary schools; school and year fixed effects.

Table 27: School responses, all outcomes: urban fully staffed schools, school and year fixed effects

	Public			Private		
	Coefficient	Mean	N	Coefficient	Mean	N
Enrollment		323.49	29,665		138.67	29,323
Teaching posts		14.53	29,665		8.21	29,323
Classroom teachers		13.16	29,665		7.36	29,323
Contract teachers		3.39	29,665		6.41	29,323
Share contract		0.26	29,652		0.98	23,312
Sections		13.12	29,665		7.40	29,323
Pupils per section		22.34	29,665		16.61	29,323
Share of sections > 30		0.16	29,665		0.06	29,323
Share of sections > 35		0.04	29,665		0.02	29,323
Share of pupils in > 30		0.19	29,665		0.07	29,323
Any section > 30		0.46	29,665		0.19	29,323
Largest section		28.58	29,665		22.22	29,323
Afternoon shift		0.28	29,665		0.08	29,323
Classrooms		15.57	28,813		13.27	22,806
Monthly fee (2019 S/)	–				215.65	22,787
Annual fee (2019 S/)	–				2366.81	22,787
log monthly fee	–				5.30	18,884
log annual fee	–				7.41	21,372
Enrollment fee	–				210.32	22,787
Entry fee	–				247.09	22,786
Share with pedagogical degree		0.98	29,652		0.75	29,154
Share with any degree		0.98	29,652		0.79	29,154
Share with unfinished studies		0.01	29,652		0.05	29,154

Notes: Each cell is the 2SLS coefficient on exposure, the number of Venezuelan primary pupils per school among the other primary schools within three miles, instrumented with the shift-share prediction that combines the neighborhood's share of national Venezuelan pupils in 2014 with the national count in the year. School-years 2015–2019. Standard errors clustered by district in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. First-stage F is the Kleibergen–Paap rk Wald statistic. Sample: urban fully staffed schools; school and year fixed effects.

Table 28: School responses, all outcomes: the ten markets, school and year fixed effects

	Public			Private		
	Coefficient	Mean	N	Coefficient	Mean	N
Enrollment		410.45	8,247		123.21	25,000
Teaching posts		16.68	8,247		7.40	25,000
Classroom teachers		14.98	8,247		6.66	25,000
Contract teachers		3.25	8,247		5.81	25,000
Share contract		0.20	8,190		0.99	19,792
Sections		15.06	8,247		7.10	25,000
Pupils per section		25.28	8,247		14.98	25,000
Share of sections > 30		0.28	8,247		0.05	25,000
Share of sections > 35		0.07	8,247		0.02	25,000
Share of pupils in > 30		0.30	8,247		0.06	25,000
Any section > 30		0.67	8,247		0.16	25,000
Largest section		31.33	8,247		20.28	25,000
Afternoon shift		0.42	8,247		0.06	25,000
Classrooms		18.51	7,976		12.60	19,486
Monthly fee (2019 S/)	–				222.27	19,492
Annual fee (2019 S/)	–				2432.80	19,492
log monthly fee	–				5.33	16,290
log annual fee	–				7.47	18,157
Enrollment fee	–				210.12	19,492
Entry fee	–				272.15	19,492
Share with pedagogical degree		0.99	8,190		0.71	24,745
Share with any degree		0.99	8,190		0.75	24,745
Share with unfinished studies		0.00	8,190		0.06	24,745

Notes: Each cell is the 2SLS coefficient on exposure, the number of Venezuelan primary pupils per school among the other primary schools within three miles, instrumented with the shift-share prediction that combines the neighborhood's share of national Venezuelan pupils in 2014 with the national count in the year. School-years 2015–2019. Standard errors clustered by district in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. First-stage F is the Kleibergen–Paap rk Wald statistic. Sample: the ten markets; school and year fixed effects.

Table 29: School responses, all outcomes: urban fully staffed schools in the ten markets, school and year fixed effects

	Public			Private		
	Coefficient	Mean	N	Coefficient	Mean	N
Enrollment		442.03	7,576		142.19	20,281
Teaching posts		17.91	7,576		8.33	20,281
Classroom teachers		16.07	7,576		7.46	20,281
Contract teachers		3.48	7,576		6.53	20,281
Share contract		0.20	7,575		0.99	16,119
Sections		15.89	7,576		7.52	20,281
Pupils per section		26.77	7,576		16.78	20,281
Share of sections > 30		0.30	7,576		0.06	20,281
Share of sections > 35		0.07	7,576		0.02	20,281
Share of pupils in > 30		0.33	7,576		0.07	20,281
Any section > 30		0.72	7,576		0.19	20,281
Largest section		33.00	7,576		22.35	20,281
Afternoon shift		0.46	7,576		0.05	20,281
Classrooms		19.65	7,325		13.66	15,825
Monthly fee (2019 S/)	–				238.42	15,865
Annual fee (2019 S/)	–				2613.51	15,865
log monthly fee	–				5.39	13,239
log annual fee	–				7.53	14,763
Enrollment fee	–				229.28	15,865
Entry fee	–				330.75	15,865
Share with pedagogical degree		0.99	7,575		0.72	20,151
Share with any degree		0.99	7,575		0.76	20,151
Share with unfinished studies		0.00	7,575		0.05	20,151

Notes: Each cell is the 2SLS coefficient on exposure, the number of Venezuelan primary pupils per school among the other primary schools within three miles, instrumented with the shift-share prediction that combines the neighborhood's share of national Venezuelan pupils in 2014 with the national count in the year. School-years 2014–2019. Standard errors clustered by district in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. First-stage F is the Kleibergen–Paap rk Wald statistic. Sample: urban fully staffed schools in the ten markets; school and year fixed effects.

Table 30: School responses, all outcomes: urban fully staffed schools, no fixed effects

	Public			Private		
	Coefficient	Mean	N	Coefficient	Mean	N
Enrollment		323.48	29,666		138.44	29,380
Teaching posts		14.53	29,666		8.20	29,380
Classroom teachers		13.16	29,666		7.35	29,380
Contract teachers		3.39	29,666		6.41	29,380
Share contract		0.26	29,653		0.98	23,389
Sections		13.12	29,666		7.40	29,380
Pupils per section		22.34	29,666		16.59	29,380
Share of sections > 30		0.16	29,666		0.06	29,380
Share of sections > 35		0.04	29,666		0.02	29,380
Share of pupils in > 30		0.19	29,666		0.07	29,380
Any section > 30		0.46	29,666		0.18	29,380
Largest section		28.58	29,666		22.19	29,380
Afternoon shift		0.28	29,666		0.08	29,380
Classrooms		15.58	28,815		13.25	22,892
Monthly fee (2019 S/)	–				215.35	22,929
Annual fee (2019 S/)	–				2363.36	22,929
log monthly fee	–				5.30	19,303
log annual fee	–				7.41	21,605
Enrollment fee	–				209.89	22,929
Entry fee	–				245.71	22,928
Share with pedagogical degree		0.98	29,653		0.75	29,220
Share with any degree		0.98	29,653		0.79	29,220
Share with unfinished studies		0.01	29,653		0.05	29,220

Notes: Each cell is the 2SLS coefficient on exposure, the number of Venezuelan primary pupils per school among the other primary schools within three miles, instrumented with the shift-share prediction that combines the neighborhood's share of national Venezuelan pupils in 2014 with the national count in the year. School-years 2015–2019. Standard errors clustered by district in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. First-stage F is the Kleibergen–Paap rk Wald statistic. Sample: urban fully staffed schools; no fixed effects.

Table 31: First stage: exposure on the shift-share prediction, by radius

		No fixed effects				School and year fixed effects			
Sample	Sector	1 mi	3 mi	5 mi	10 mi	1 mi	3 mi	5 mi	10 mi
All	public	0.011***	0.002***	0.001***	0.000***	0.011***	0.002***	0.001***	0.000***
		(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
		[458.4]	[543.6]	[346.9]	[755.3]	[268.2]	[383.2]	[264.7]	[607.3]
All	private	0.008***	0.001***	0.001***	0.000***	0.007***	0.001***	0.001***	0.000***
		(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
		[355.0]	[598.8]	[545.3]	[602.1]	[158.8]	[351.1]	[387.8]	[463.1]
Urban fully staffed	public	0.010***	0.002***	0.001***	0.000***	0.010***	0.002***	0.001***	0.000***
		(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
		[442.3]	[565.4]	[350.0]	[781.8]	[248.3]	[392.1]	[266.9]	[628.5]
Urban fully staffed	private	0.007***	0.001***	0.001***	0.000***	0.007***	0.001***	0.000***	0.000***
		(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
		[288.6]	[508.9]	[487.3]	[571.5]	[121.7]	[296.1]	[342.1]	[435.6]
Ten markets	public	0.008***	0.001***	0.000***	0.000***	0.007***	0.001***	0.000***	0.000***
		(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
		[268.1]	[366.9]	[253.2]	[317.7]	[137.7]	[231.7]	[184.6]	[232.2]
Ten markets	private	0.006***	0.001***	0.000***	0.000***	0.006***	0.001***	0.000***	0.000***
		(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
		[173.4]	[299.9]	[281.3]	[197.6]	[75.1]	[170.7]	[189.5]	[142.2]

Notes: Coefficient on the shift-share prediction in a regression of exposure (Venezuelan pupils per other primary school within the radius) on it, school-years 2015–2019; standard errors clustered by district in parentheses; the Kleibergen–Paap F statistic of the corresponding 2SLS regression of enrollment in brackets.

Table 32: Private-school fees and exposure: specifications, windows and samples

		2016–2019			2017–2019		
Sample	Outcome	No FE	District, year	School, year	No FE	District, year	School, year
All	Monthly fee (2019 S/)	30.22***	4.76**	1.70	29.59***	3.89	0.12
		(7.97)	(2.34)	(1.50)	(7.79)	(2.96)	(1.80)
All	Annual fee (2019 S/)	332.39***	52.32**	18.64	325.44***	44.09	3.14
		(88.10)	(25.45)	(15.63)	(86.16)	(32.15)	(18.45)
All	log monthly fee	0.138***	0.018**	0.003	0.135***	0.018**	0.001
		(0.020)	(0.007)	(0.003)	(0.020)	(0.008)	(0.003)
All	log annual fee	0.143***	0.016**	-0.001	0.139***	0.018**	-0.001
		(0.020)	(0.007)	(0.006)	(0.020)	(0.009)	(0.006)
Urban f.s.	Monthly fee (2019 S/)	33.55***	5.10*	2.13	32.86***	4.08	0.42
		(9.03)	(2.63)	(1.35)	(8.83)	(3.40)	(1.54)
Urban f.s.	Annual fee (2019 S/)	367.81***	55.27*	22.36	360.15***	45.72	6.06
		(99.82)	(28.82)	(14.40)	(97.60)	(37.31)	(16.10)
Urban f.s.	log monthly fee	0.110***	0.015*	-0.002	0.107***	0.015	-0.004*
		(0.020)	(0.008)	(0.004)	(0.020)	(0.010)	(0.003)
Urban f.s.	log annual fee	0.121***	0.015**	-0.004	0.118***	0.018*	-0.004
		(0.020)	(0.007)	(0.006)	(0.020)	(0.009)	(0.006)
Ten mkts	Monthly fee (2019 S/)	33.97***	7.08**	2.32	33.23***	7.68*	2.24
		(10.39)	(3.35)	(1.56)	(10.16)	(4.29)	(2.10)
Ten mkts	Annual fee (2019 S/)	374.61***	77.06**	24.45	366.32***	84.23*	24.78
		(115.00)	(36.88)	(16.64)	(112.50)	(46.95)	(21.61)
Ten mkts	log monthly fee	0.115***	0.025**	-0.001	0.112***	0.026**	-0.003
		(0.025)	(0.011)	(0.004)	(0.024)	(0.013)	(0.003)
Ten mkts	log annual fee	0.115***	0.024**	-0.005	0.112***	0.032**	0.003
		(0.025)	(0.010)	(0.008)	(0.025)	(0.013)	(0.008)

Notes: 2SLS coefficient on exposure (Venezuelan pupils per other primary school within three miles), instrumented with the 2014-base shift-share, private primary schools with a reported fee. Fees in 2019 soles. Standard errors clustered by district; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

in the full sample (Table 36), the response by quintile of exposure (Table 37), the exposure-by-fullness interaction in the other samples and with alternative measures of fullness (Tables 38, 39, 40 and 41), the district-slack interactions (Table 42) the identifier-break test (Table 43), the comparison of three definitions of exposure (Table 44), the instrumental-variables estimates (Table 45), and the test-score and peer-composition tables referenced in the text.

An instrument for the cell-level share, and why we do not use it. In a full school a migrant child can only join a grade in which a seat has opened, so the migrant share of a grade could in principle be a consequence of incumbent departures rather than a cause of them. We tried to instrument the share with the interaction of the baseline school's neighborhood exposure and the national share of Venezuelan primary pupils in the child's grade in that year, with baseline-school-by-year and grade-by-year fixed effects, so that only exposed schools in the grades the inflow concentrated in nationally identify the effect (Table 45). The first stage is strong, but the reduced form is far too large relative to it: the implied effect of a one-point higher share on switching is three points from a public school, thirty times the OLS estimate and outside any plausible range. We read this as a failure of the exclusion restriction rather than as evidence of large effects. The instrument shifts, within an exposed school, the grades in which migrants are numerous in the whole neighborhood, and families in those grades face different alternatives, different crowding in the schools they might move to, and possibly different trends, none of which run through the migrant share of their own grade. The paper's family-side estimates therefore rest on the within-school-and-year comparison across grades of Section 5, with the predetermined denominator, and on the checks in Tables 44 and 43.

D Data construction

Sources and files. The student register is built from the Ministry's SIAGIE enrollment files for 2014–2022 (*siagie_ YYYY_new*), one row per enrollment, with the school code and campus, the level, grade and section, sex, date of birth, nationality, the end-of-year result and the report-card marks. Rare within-year duplicates (a pupil enrolled in two schools in the same year) keep the row with the highest level and grade. Venezuelan pupils are those whose nationality field reads "Vene" (2014–2019) or "Venezuela" (2020–2022). Schools come from the roster (*Padrón*) of each year's school census, restricted to the primary level (`NIV_MOD` B0); sector, urban status, type, shifts, district, building and coordinates are taken as reported. Teaching posts come from the teacher module of the census (cuadro 305 or 306 depending on the year), summing the positions "docente de aula", "docente de aula con cargo

Table 33: How families respond, all baseline schools

	All	Public baseline school	Private baseline school
Switched school	0.569***	0.701***	0.454***
	(0.005)	(0.005)	(0.010)
per 10 points	0.0569	0.0701	0.0454
mean of outcome	0.118	0.087	0.202
observations	8,822,908	6,464,341	2,295,858
Math mark (SD)	-0.133***	-0.177***	-0.045***
	(0.011)	(0.015)	(0.015)
per 10 points	-0.0133	-0.0177	-0.0045
mean of outcome	0.003	-0.046	0.139
observations	8,749,163	6,416,751	2,271,045
Language mark (SD)	-0.132***	-0.182***	-0.044***
	(0.010)	(0.014)	(0.015)
per 10 points	-0.0132	-0.0182	-0.0044
mean of outcome	0.006	-0.048	0.158
observations	8,744,738	6,412,995	2,270,416
Failed the year	0.025***	0.037***	0.004**
	(0.002)	(0.003)	(0.002)
per 10 points	0.0025	0.0037	0.0004
mean of outcome	0.035	0.044	0.009
observations	8,822,910	6,464,342	2,295,859
Left the register (2017–18)	-0.006***	-0.007***	-0.001
	(0.002)	(0.002)	(0.002)
per 10 points	-0.0006	-0.0007	-0.0001
mean of outcome	0.010	0.012	0.007
observations	5,836,113	4,289,127	1,521,821
Left the register (2019)	0.002	-0.005	0.007
	(0.004)	(0.005)	(0.006)
per 10 points	0.0002	-0.0005	0.0007
mean of outcome	0.038	0.038	0.041
observations	2,986,797	2,175,215	774,038
Repeats next year	0.028***	0.045***	0.003
	(0.002)	(0.004)	(0.002)
per 10 points	0.0028	0.0045	0.0003
mean of outcome	0.045	0.057	0.013
observations	7,118,428	5,196,165	1,868,746

Notes: Each cell is the coefficient on the migrant share (0–1): the number of Venezuelan pupils in year t in the pupil's grade at the school she attended in $t-1$, divided by the size of that grade at that school in $t-1$; from a regression with baseline-school-by-year and grade fixed effects and controls for age, sex and the standardized math mark in $t-1$. Non-Venezuelan primary pupils in grades 2–6, 2017–2019 (2017–2018 for permanent exit). Standard errors clustered by baseline school and grade; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Per 10 points* is the coefficient divided by ten, the effect of a ten-percentage-point higher migrant share. Sample: all primary pupils.

Table 34: How families respond, the ten markets

	All	Public baseline school	Private baseline school
Switched school	0.348***	0.118***	0.402***
	(0.011)	(0.015)	(0.013)
per 10 points	0.0348	0.0118	0.0402
mean of outcome	0.152	0.106	0.200
observations	3,229,667	1,652,177	1,577,490
Math mark (SD)	-0.090***	-0.138**	-0.042**
	(0.018)	(0.056)	(0.018)
per 10 points	-0.0090	-0.0138	-0.0042
mean of outcome	0.040	-0.052	0.138
observations	3,200,870	1,640,481	1,560,389
Language mark (SD)	-0.109***	-0.180***	-0.056***
	(0.018)	(0.052)	(0.018)
per 10 points	-0.0109	-0.0180	-0.0056
mean of outcome	0.058	-0.036	0.157
observations	3,199,740	1,639,805	1,559,935
Failed the year	0.010***	0.015*	0.000
	(0.002)	(0.008)	(0.002)
per 10 points	0.0010	0.0015	0.0000
mean of outcome	0.022	0.035	0.009
observations	3,229,667	1,652,177	1,577,490
Left the register (2017–18)	-0.005**	-0.023***	-0.002
	(0.002)	(0.009)	(0.002)
per 10 points	-0.0005	-0.0023	-0.0002
mean of outcome	0.008	0.008	0.007
observations	2,135,218	1,086,847	1,048,371
Repeats next year	0.007**	0.020*	-0.001
	(0.003)	(0.010)	(0.003)
per 10 points	0.0007	0.0020	-0.0001
mean of outcome	0.029	0.045	0.012
observations	2,598,054	1,319,582	1,278,472

Notes: Each cell is the coefficient on the migrant share (0–1): the number of Venezuelan pupils in year t in the pupil's grade at the school she attended in $t-1$, divided by the size of that grade at that school in $t-1$; from a regression with baseline-school-by-year and grade fixed effects and controls for age, sex and the standardized math mark in $t-1$. Non-Venezuelan primary pupils in grades 2–6, 2017–2019 (2017–2018 for permanent exit). Standard errors clustered by baseline school and grade; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Per 10 points* is the coefficient divided by ten, the effect of a ten-percentage-point higher migrant share. Sample: pupils whose baseline school is in the ten markets.

Table 35: How families respond, without the baseline mark (grade 1 included)

	All	Public baseline school	Private baseline school
Switched school	0.378***	0.117***	0.446***
	(0.013)	(0.014)	(0.014)
per 10 points	0.0378	0.0117	0.0446
mean of outcome	0.123	0.092	0.194
observations	6,867,454	4,762,066	2,105,388
Math mark (SD)	-0.217***	-0.331***	-0.002
	(0.021)	(0.052)	(0.019)
per 10 points	-0.0217	-0.0331	-0.0002
mean of outcome	0.031	-0.028	0.161
observations	8,208,421	5,658,391	2,550,030
Language mark (SD)	-0.250***	-0.478***	-0.010
	(0.020)	(0.052)	(0.019)
per 10 points	-0.0250	-0.0478	-0.0010
mean of outcome	0.039	-0.024	0.179
observations	8,206,067	5,656,517	2,549,550
Failed the year	-0.001	-0.011*	0.002
	(0.002)	(0.006)	(0.002)
per 10 points	-0.0001	-0.0011	0.0002
mean of outcome	0.023	0.030	0.007
observations	8,281,736	5,704,709	2,577,027
Left the register (2017–18)	-0.006***	0.004	-0.006***
	(0.002)	(0.007)	(0.002)
per 10 points	-0.0006	0.0004	-0.0006
mean of outcome	0.008	0.008	0.007
observations	5,484,652	3,770,294	1,714,358
Repeats next year	-0.006**	-0.007	0.000
	(0.003)	(0.007)	(0.002)
per 10 points	-0.0006	-0.0007	0.0000
mean of outcome	0.030	0.040	0.010
observations	6,927,527	4,756,025	2,171,502

Notes: Each cell is the coefficient on the migrant share (0–1): the number of Venezuelan pupils in year t in the pupil's grade at the school she attended in $t-1$, divided by the size of that grade at that school in $t-1$; from a regression with baseline-school-by-year and grade fixed effects and controls for age, sex and the standardized math mark in $t-1$. Non-Venezuelan primary pupils in grades 2–6, 2017–2019 (2017–2018 for permanent exit). Standard errors clustered by baseline school and grade; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Per 10 points* is the coefficient divided by ten, the effect of a ten-percentage-point higher migrant share. This version drops the baseline mark from the controls, which brings first graders (whose baseline school is their own school) into the sample.

Table 36: Where the switchers go, all baseline schools

	Coefficient on the migrant share			Mean of outcome		
	All	Public	Private	All	Public	Private
to a school with fewer migrants	0.028***	0.014***	0.042***	0.020	0.016	0.030
	(0.004)	(0.002)	(0.007)			
to a school with more migrants	0.066***	0.045***	0.093***	0.022	0.012	0.047
	(0.003)	(0.003)	(0.005)			
to a school with smaller sections	0.058***	0.046***	0.075***	0.053	0.047	0.069
	(0.005)	(0.003)	(0.009)			
to a school with higher marks	0.304***	0.393***	0.219***	0.057	0.046	0.086
	(0.006)	(0.006)	(0.011)			
to a public school	0.413***	0.672***	0.152***	0.070	0.067	0.079
	(0.006)	(0.005)	(0.007)			
to a private school	0.162***	0.032***	0.306***	0.044	0.018	0.116
	(0.006)	(0.002)	(0.011)			
public to public	0.343***	0.672***	0.001***	0.050	0.067	0.000
	(0.005)	(0.005)	(0.000)			
public to private	0.017***	0.032***	0.000	0.013	0.018	0.000
	(0.001)	(0.002)	(0.000)			
private to public	0.072***	0.000***	0.151***	0.021	0.000	0.078
	(0.003)	(0.000)	(0.007)			
private to private	0.145***	0.000***	0.307***	0.031	0.000	0.116
	(0.006)	(0.000)	(0.011)			
to a private school with a higher fee	0.273***		0.273***	0.060		0.059
	(0.012)		(0.012)			
to a private school with a lower fee	0.165***		0.167***	0.059		0.059
	(0.009)		(0.009)			
Observations	8,779,390	6,454,345	2,272,584			

Notes: Each cell is the coefficient on the migrant share (0–1): the number of Venezuelan pupils in year t in the pupil's grade at the school she attended in $t-1$, divided by the size of that grade at that school in $t-1$; from a regression with baseline-school-by-year and grade fixed effects and controls for age, sex and the standardized math mark in $t-1$. Non-Venezuelan primary pupils in grades 2–6, 2017–2019 (2017–2018 for permanent exit). Standard errors clustered by baseline school and grade; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The effect of a ten-percentage-point higher share is the coefficient divided by ten. See Table 9. Observations are those of the first outcome. The two fee outcomes are defined for pupils whose baseline school reports a fee (2,058,868 pupils, nearly all in private schools); cells with fewer than 1,000 observations are left blank.

Table 37: Family responses by quintile of exposure, urban fully staffed baseline schools

	Q1	Q2	Q3	Q4	Q5
public baseline schools					
Mean migrant share in bin	0.007	0.014	0.024	0.044	0.126
Switched school	0.0007	0.0038***	0.0036***	0.0059***	0.0130***
	(0.0008)	(0.0009)	(0.0010)	(0.0012)	(0.0017)
Math mark (SD)	-0.0021	0.0019	0.0052	-0.0055	-0.0111
	(0.0049)	(0.0047)	(0.0049)	(0.0057)	(0.0077)
Language mark (SD)	-0.0025	0.0014	0.0036	-0.0029	-0.0107
	(0.0048)	(0.0047)	(0.0048)	(0.0056)	(0.0075)
Failed the year	0.0004	-0.0003	-0.0017**	-0.0003	0.0004
	(0.0007)	(0.0007)	(0.0007)	(0.0008)	(0.0011)
Left the register (2017–18)	0.0003	0.0002	0.0003	-0.0001	-0.0008
	(0.0003)	(0.0003)	(0.0004)	(0.0005)	(0.0009)
private baseline schools					
Mean migrant share in bin	0.008	0.014	0.024	0.045	0.339
Switched school	0.0079***	0.0036*	0.0026	0.0029	0.0510***
	(0.0028)	(0.0020)	(0.0020)	(0.0021)	(0.0039)
Math mark (SD)	-0.0050	0.0013	-0.0084	-0.0001	-0.0010
	(0.0217)	(0.0117)	(0.0089)	(0.0064)	(0.0069)
Language mark (SD)	0.0208	-0.0159	-0.0083	0.0011	-0.0125*
	(0.0174)	(0.0118)	(0.0094)	(0.0065)	(0.0068)
Failed the year	0.0002	0.0001	0.0004	0.0005	0.0000
	(0.0010)	(0.0005)	(0.0005)	(0.0005)	(0.0006)
Left the register (2017–18)	0.0000	0.0001	0.0003	-0.0009	-0.0014*
	(0.0008)	(0.0006)	(0.0006)	(0.0006)	(0.0009)

Notes: Coefficients on indicators for the quintile of the migrant share among pupils with a positive share, relative to pupils with a zero share, from the specification of Table 8. Standard errors clustered by baseline school and grade.

Table 38: Does the response depend on whether the school was full? all baseline schools

	Switched	Math (SD)	Language (SD)	Failed	Left the register
public baseline schools					
Migrant share	0.711***	-0.181***	-0.190***	0.039***	-0.008***
	(0.005)	(0.015)	(0.014)	(0.003)	(0.002)
Migrant share × full school	-0.508***	0.205	0.361**	-0.099***	0.099***
	(0.031)	(0.170)	(0.164)	(0.022)	(0.022)
Mean of outcome	0.087	-0.046	-0.048	0.044	0.012
Observations	6,464,341	6,416,751	6,412,995	6,464,342	4,289,127
private baseline schools					
Migrant share	0.450***	-0.045***	-0.043***	0.004**	-0.001
	(0.009)	(0.015)	(0.015)	(0.002)	(0.002)
Migrant share × full school	0.266***	0.003	-0.073	-0.015	0.005
	(0.096)	(0.128)	(0.120)	(0.013)	(0.011)
Mean of outcome	0.202	0.139	0.158	0.009	0.007
Observations	2,295,858	2,271,045	2,270,416	2,295,859	1,521,821

Notes: The specification of Table 8 with the migrant share interacted with an indicator that the baseline school was full in 2016 (utilization, enrollment over thirty times the number of sections, at or above 0.95). The main effect of the indicator is absorbed by the baseline-school-by-year fixed effects. Sample: all baseline schools. Standard errors clustered by baseline school and grade; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 39: Does the response depend on whether the school was full? the ten markets

	Switched	Math (SD)	Language (SD)	Failed	Left the register
public baseline schools					
Migrant share	0.111***	-0.150**	-0.219***	0.022**	-0.029***
	(0.017)	(0.059)	(0.054)	(0.009)	(0.010)
Migrant share × full school	0.051	0.081	0.279	-0.051**	0.040**
	(0.035)	(0.188)	(0.179)	(0.024)	(0.019)
Mean of outcome	0.106	-0.052	-0.036	0.035	0.008
Observations	1,652,177	1,640,481	1,639,805	1,652,177	1,086,847
private baseline schools					
Migrant share	0.395***	-0.043**	-0.055***	0.001	-0.002
	(0.012)	(0.018)	(0.018)	(0.002)	(0.002)
Migrant share × full school	0.328***	0.027	-0.018	-0.014	0.006
	(0.096)	(0.127)	(0.115)	(0.012)	(0.011)
Mean of outcome	0.200	0.138	0.157	0.009	0.007
Observations	1,577,490	1,560,389	1,559,935	1,577,490	1,048,371

Notes: The specification of Table 8 with the migrant share interacted with an indicator that the baseline school was full in 2016 (utilization, enrollment over thirty times the number of sections, at or above 0.95). The main effect of the indicator is absorbed by the baseline-school-by-year fixed effects. Sample: the ten markets. Standard errors clustered by baseline school and grade; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 40: Does the response depend on whether the school was full? urban fully staffed baseline schools in the ten markets

	Switched	Math (SD)	Language (SD)	Failed	Left the register
public baseline schools					
Migrant share	0.083***	-0.098	-0.189***	0.018*	-0.019*
	(0.017)	(0.065)	(0.060)	(0.010)	(0.010)
Migrant share × full school	0.082**	0.026	0.246	-0.046*	0.030
	(0.035)	(0.190)	(0.181)	(0.025)	(0.019)
Mean of outcome	0.105	-0.052	-0.035	0.034	0.008
Observations	1,634,290	1,622,852	1,622,187	1,634,290	1,075,506
private baseline schools					
Migrant share	0.383***	-0.056**	-0.070***	0.000	-0.003
	(0.017)	(0.023)	(0.023)	(0.003)	(0.002)
Migrant share × full school	0.339***	0.034	-0.010	-0.014	0.007
	(0.097)	(0.127)	(0.116)	(0.013)	(0.011)
Mean of outcome	0.191	0.143	0.162	0.008	0.007
Observations	1,477,098	1,462,648	1,462,244	1,477,098	982,640

Notes: The specification of Table 8 with the migrant share interacted with an indicator that the baseline school was full in 2016 (utilization, enrollment over thirty times the number of sections, at or above 0.95). The main effect of the indicator is absorbed by the baseline-school-by-year fixed effects. Sample: urban fully staffed baseline schools in the ten markets. Standard errors clustered by baseline school and grade; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

directivo", the specialist positions and "otro docente" over the education-level rows; the share with a pedagogical degree is the row "estudios pedagógicos concluidos con título" over the total. Classrooms come from the building module, with the year-specific items documented in the code. Fees come from the Ministry's cost survey of private schools ($COSTO\_IE$), 2016–2019, deflated to 2019 soles with the Lima consumer price index.

Sections. A section is a distinct classroom label within a school, grade and year with at least one enrolled pupil. Labels are upper-cased, stripped of accents, spaces and punctuation, and the single-section synonyms ("ÚNICA", "UNICO", "U", "SECCIÓN ÚNICA") and blanks are mapped to one label. The count is at the school code by campus.

Exposure. Schools are located with the roster coordinates (2019, then 2018, then 2016) and, for the few schools with no roster coordinate, a project file of geocoded addresses; 145 roster coordinates outside Peru's bounding box are treated as missing. For every pair of primary schools within ten statute miles we compute the great-circle distance. Exposure at radius R is the sum of Venezuelan primary pupils over the other schools within R miles divided by the number of those schools. The shift-share is the same sum in 2014 divided by the national count in 2014, times the national count in the year.

Table 41: Alternative measures of fullness: the interaction term, urban fully staffed baseline schools

	Switched	Math (SD)	Language (SD)	Failed	Left the register
public baseline schools					
Utilization $\geq$ 1.00	0.054	-0.175	0.121	-0.033	0.038
	(0.041)	(0.226)	(0.225)	(0.031)	(0.025)
Utilization, continuous	-0.034	0.293	0.545**	-0.075*	0.092**
	(0.102)	(0.270)	(0.278)	(0.041)	(0.039)
Full at a 25-pupil norm	0.037	0.113	0.206**	-0.013	0.021
	(0.028)	(0.107)	(0.101)	(0.016)	(0.016)
Share of sections above 30 in 2016	0.085	0.227	0.624**	-0.066	0.093***
	(0.064)	(0.300)	(0.296)	(0.041)	(0.035)
Own section above 30 in $t-1$	-0.058***	0.131*	0.173**	-0.030**	-0.013
	(0.016)	(0.072)	(0.070)	(0.013)	(0.010)
private baseline schools					
Utilization $\geq$ 1.00	-0.630***	-0.618	-0.671	-0.027	0.067
	(0.140)	(0.496)	(0.530)	(0.034)	(0.049)
Utilization, continuous	0.095	0.183**	0.036	-0.012	0.013
	(0.076)	(0.086)	(0.093)	(0.009)	(0.009)
Full at a 25-pupil norm	0.086	0.036	-0.023	-0.010	0.014**
	(0.069)	(0.063)	(0.082)	(0.007)	(0.006)
Share of sections above 30 in 2016	0.103	-0.218	-0.505*	-0.046	0.026
	(0.274)	(0.257)	(0.287)	(0.028)	(0.026)
Own section above 30 in $t-1$	-0.044	0.084	0.192**	-0.005	0.006*
	(0.028)	(0.089)	(0.084)	(0.006)	(0.004)

Notes: Coefficient on the interaction of the migrant share with the stated measure of the baseline school's fullness, in the specification of Table 15. Standard errors clustered by baseline school and grade.

Table 42: Own-school and district fullness, public baseline schools

	All public			Urban fully staffed		
	Left the register	Switched	Failed	Left the register	Switched	Failed
Migrant share	-0.012***	0.754***	0.038***	-0.004	0.169***	-0.013
	(0.003)	(0.005)	(0.004)	(0.018)	(0.045)	(0.020)
× own school full	0.188***	-0.355***	-0.167**	0.093*	0.215**	-0.093
	(0.064)	(0.090)	(0.068)	(0.049)	(0.099)	(0.071)
× district share of full schools	0.028*	-0.383***	0.000	-0.021	-0.247	0.116
	(0.015)	(0.024)	(0.016)	(0.067)	(0.153)	(0.073)
× own full × district share	-0.255	-0.120	0.176	-0.109	-0.250	0.047
	(0.173)	(0.203)	(0.171)	(0.146)	(0.250)	(0.185)
Migrant share (district interaction only)	-0.011***	0.751***	0.037***	-0.005	0.166***	-0.006
	(0.003)	(0.005)	(0.004)	(0.016)	(0.039)	(0.019)
× district share of full schools	0.031**	-0.424***	-0.008	0.009	-0.177	0.054
	(0.015)	(0.024)	(0.016)	(0.056)	(0.119)	(0.064)

Notes: The specification of Table 15 with the migrant share also interacted with the enrollment-weighted share of urban fully staffed primary schools (both sectors) in the baseline school's district that were full in 2016. Public baseline schools, 2017–2018 for exit. Standard errors clustered by baseline school and grade.

Table 43: Are the exits changes of student identifier? Match rates and corrected exit regressions

A. Share of exiters matched to a pupil first appearing the next year with the same sex and birth date, in grade g or $g+1$

	Anywhere in Peru		Same district		Same school	
	Actual	Placebo	Actual	Placebo	Actual	Placebo
All exiters (N = 60,592)	40.61	40.71	1.18	0.86	0.27	0.03
grade 1	84.20	84.37	6.20	4.58	1.02	0.19
grade 2	59.01	59.76	2.79	2.29	0.35	0.04
grade 3	41.93	42.50	1.84	1.51	0.27	0.02
grade 4	34.02	34.04	1.62	0.86	0.62	0.05
grade 5	29.05	28.93	1.42	0.86	0.62	0.04
grade 6	37.22	37.21	0.12	0.14	0.01	0.00
school not full, migrant share $\leq 2\%$						
school not full, migrant share $> 2\%$						
school full, migrant share $\leq 2\%$						
school full, migrant share $> 2\%$						

B. Exit regressions with matched exits reclassified as non-exits (urban fully staffed public baseline schools)

	Migrant share	$\times$ full school
Left the register, as measured	-0.010	0.050***
	(0.009)	(0.019)
dropping same-school matches	-0.009	0.049***
	(0.009)	(0.018)
dropping same-district matches	-0.010	0.040**
	(0.009)	(0.018)
dropping any match (not a valid correction)	-0.008	0.007
	(0.006)	(0.013)
Matched in the same district (incidence)	0.001	0.010**
	(0.001)	(0.005)
Matched in the same school (incidence)	-0.000	0.001
	(0.000)	(0.002)

Notes: Exiters are non-Venezuelan public-school pupils in grades 2–6 whose last year in the register is 2017 or 2018. A match is a pupil whose first appearance in SIAGIE (any level) is the following year, with the same sex and birth date and in the exiter's grade or the next; the placebo shifts the birth date by seven days. Panel B re-estimates the specification of Table 15 after setting exit to zero for matched exiters.

Table 44: The denominator matters: three definitions of exposure, urban fully staffed baseline schools

	Switched	Math (SD)	Failed	Left the register
public baseline schools				
Share, current cell size in the denominator	0.291***	-0.165**	0.009	-0.005
	(0.016)	(0.071)	(0.011)	(0.010)
Number of Venezuelan pupils in the cell (tens)	0.020***	-0.018**	0.003**	0.001
	(0.002)	(0.009)	(0.001)	(0.001)
Share, cell size in $t-1$ in the denominator	0.067***	-0.089**	0.003	-0.001
	(0.010)	(0.037)	(0.005)	(0.003)
private baseline schools				
Share, current cell size in the denominator	0.261***	-0.042	0.005	-0.004
	(0.022)	(0.057)	(0.005)	(0.009)
Number of Venezuelan pupils in the cell (tens)	0.007	0.016	0.001	-0.003
	(0.008)	(0.035)	(0.002)	(0.003)
Share, cell size in $t-1$ in the denominator	0.312***	0.002	0.000	0.000
	(0.023)	(0.011)	(0.001)	(0.001)

Notes: The specification of Table 8 with three definitions of the treatment: the Venezuelan share of the baseline cell with its current size in the denominator (the definition used in earlier work on this episode); the number of Venezuelan pupils in the cell, in tens; and the share with the cell's size in $t-1$ in the denominator, capped at one (the definition used in the paper). Standard errors clustered by baseline school and grade.

Family outcomes. Switching, permanent exit, repetition, failing and the marks are defined in Section 3.4. The baseline school is the school attended in $t-1$ when the pupil was in primary in $t-1$; for first graders and pupils first observed in t it is the own school, and those pupils enter only the specifications without the lagged mark. The treatment cell is the school, campus, grade and year; its Venezuelan share is computed over all pupils in the cell.

Reconciliation with the earlier pipeline. Previous drafts of this project used a student panel and a school panel built by a different pipeline from the same raw files. Because every dataset here was rebuilt from the raw files, we checked the two against each other. National primary enrollment agrees within 0.15 percent in every year (3,694,306 pupils in 2019 here against 3,689,189 there) and the number of Venezuelan primary pupils within 0.1 percent (46,066 against 46,022 in 2019). The permanent-exit rate is identical to three decimals in every year (1.04 percent in 2017, 1.06 in 2018). Two definitions differ by construction. Our switching rate (12.2 percent in 2017) is higher than the earlier one (10.3 percent) because the earlier variable set switching to zero for pupils first observed in the year and for first graders, whereas we leave it undefined for them. Our failure rate (3.1 percent of primary pupils in 2017; 4.4 percent in public schools, grades one to five) is seven times the earlier one (0.4 percent) because the earlier variable counted only pupils who failed the recovery

Table 45: Instrumenting the migrant share of the grade with neighborhood exposure and the national grade profile of the inflow

	Switched	Math (SD)	Language (SD)	Failed	Left the register
public baseline schools					
OLS	0.114***	-0.110**	-0.162***	0.009	-0.005
	(0.014)	(0.053)	(0.051)	(0.008)	(0.008)
Reduced form (instrument)	0.095***	-0.008	-0.127***	0.015***	0.018***
	(0.004)	(0.023)	(0.022)	(0.004)	(0.004)
IV: exposure × grade share	3.256***	-0.269	-4.347***	0.513***	0.451***
	(0.342)	(0.776)	(0.846)	(0.132)	(0.109)
	[F = 96.5]	[F = 96.4]	[F = 96.5]	[F = 96.5]	[F = 73.4]
IV: shift-share × grade share	2.566***	-0.691	-3.912***	0.503***	0.244***
	(0.282)	(0.681)	(0.748)	(0.112)	(0.092)
	[F = 93.8]	[F = 94.2]	[F = 94.3]	[F = 93.8]	[F = 70.4]
OLS, with full-school interaction: migrant share	0.101***	-0.127**	-0.217***	0.017**	-0.012
	(0.016)	(0.056)	(0.053)	(0.009)	(0.009)
× full school	0.088***	0.119	0.379**	-0.060**	0.048***
	(0.033)	(0.178)	(0.171)	(0.024)	(0.018)
IV, with full-school interaction: migrant share	2.863***	-1.130	-4.027***	0.938***	-0.073
	(0.327)	(0.727)	(0.802)	(0.150)	(0.100)
× full school	1.908*	4.157*	-1.536	-2.066***	2.028***
	(0.998)	(2.272)	(2.335)	(0.410)	(0.386)
private baseline schools					
OLS	0.449***	-0.028	-0.038*	0.002	-0.004**
	(0.014)	(0.020)	(0.020)	(0.002)	(0.002)
Reduced form (instrument)	-0.015	0.161***	0.078***	0.005***	0.009***
	(0.015)	(0.030)	(0.029)	(0.002)	(0.003)
IV: exposure × grade share	2.370**	19.925***	9.873**	0.505**	0.805*
	(1.071)	(6.870)	(4.545)	(0.242)	(0.487)
	[F = 12.5]	[F = 11.7]	[F = 11.7]	[F = 12.5]	[F = 4.2]
IV: shift-share × grade share	-0.738	12.558***	3.391	0.355*	0.646**
	(0.853)	(4.734)	(3.447)	(0.205)	(0.304)
	[F = 15.2]	[F = 15.2]	[F = 15.3]	[F = 15.2]	[F = 8.9]
OLS, with full-school interaction: migrant share	0.443***	-0.027	-0.037*	0.002	-0.005**
	(0.014)	(0.020)	(0.020)	(0.002)	(0.002)
× full school	0.275***	-0.019	-0.080	-0.013	0.009
	(0.096)	(0.129)	(0.120)	(0.013)	(0.011)
IV, with full-school interaction: migrant share	5.262**	18.412**	6.065	0.757**	0.565
	(2.380)	(7.181)	(4.708)	(0.355)	(0.534)
× full school	41.084	-24.255	-61.883	3.585	-2.528
	(30.938)	(27.647)	(43.767)	(3.005)	(2.559)

Notes: The specification of Table 8, urban fully staffed baseline schools. The instrument is the baseline school's neighborhood exposure in year t (Venezuelan pupils per other primary school within three miles) multiplied by the national share of Venezuelan primary pupils who are in the pupil's grade in year t; the second instrument replaces exposure with the 2014-base shift-share prediction. With baseline-school-by-year and grade fixed effects, identification comes from exposed schools in the grades where the national inflow concentrated. Kleibergen–Paap F in brackets. In the interaction rows the share and its interaction with the full-school indicator are instrumented by the instrument and its interaction. Standard errors clustered by baseline school and grade.

period, setting to zero pupils whose end-of-year result was "failed" and who did not sit the recovery examination. The definition used here is the one that corresponds to repetition: 99.9 percent of the pupils we classify as failing are in the same grade the following year, against 0.65 percent of those we classify as passing.

E Legal sources for class-size and admission rules

This appendix reproduces the passages of the Ministry of Education norms on which Section 2.3 relies. All documents were retrieved from the Ministry's legal repository (`gob.pe`) or the official gazette and checked against the page images.

RSG N° 1825-2014-MINEDU (16 October 2014), *Normas para el proceso de racionalización de plazas de personal docente, directivo y jerárquico en las instituciones educativas públicas de Educación Básica y Técnico Productiva.* In force until 28 December 2018; RM N° 627-2016-MINEDU (numeral d.1.2) defers to it for the 2017 school year. Numeral 6.5.2 d), "Número de alumnos por aula o sección," table "Número referencial Alumnos por sección – Urbana / Rural": EBR Primaria unidocente – / 20; polidocente multigrado 25 / 20; polidocente completo (**) 30 / 25. Note (**): "El número referencial de estudiantes pueden variar en más o menos 5, dependiendo el tamaño de las aulas y razones debidamente justificadas por la Comisión de la Institución Educativa." Numeral 6.5.2 a), Primaria: "1 docente por cada sección." Numeral 6.5.1: "La reubicación de plazas se determina en función al número de estudiantes por aula, o sección, o módulo."

RM N° 721-2018-MINEDU (28 December 2018), same title; derogates RSG 1825-2014 and governs the 2019 process. Numeral 10.19, Cuadro 07, "Número de estudiantes por sección – Urbana / Rural": EBR Primaria unidocente – / 20; polidocente multigrado 25 / 20; polidocente completo (**) 30 / 25. Note (**): "El número total de estudiantes por sección puede variar en más o menos 5, dependiendo el tamaño de las aulas y razones debidamente justificadas por la Comisión de la Institución Educativa." Numeral 10.1, Cuadro 02, Primaria: "1 docente por cada sección. Por cada 10 secciones, podrá considerarse 1 plaza para educación física." Numeral 10.21: "La acreditación del número de estudiantes por aula o sección se realiza a través del SIAGIE." Glossary d): "Metas de atención: Número de estudiantes de las instituciones educativas y programas educativos, a quienes se brinda el servicio educativo."

RVM N° 307-2019-MINEDU (3 December 2019), *Disposiciones para el proceso de racionalización en el marco de la Ley N° 29944*; derogates RM 721-2018 and governs 2020 onward. Numeral 10.3 b), Cuadro 6: EBR Primaria unidocente – / 15; polidocente multigrado 25 / 20; polidocente completo (**) 30 / 25, with the same note (**) and note (****): "Corresponde un docente por cada sección para los niveles de inicial y primaria en EBR."

RM N° 665-2018-MINEDU (4 December 2018), enrollment norm. Numeral 5.7.4: "Las vacantes totales disponibles por aula, en el caso de II.EE. públicas o Programas se calculan según lo regulado en las normas específicas correspondientes; y en el caso de II.EE. privadas según sus metas de atención autorizadas." Numeral 5.7.2 b) requires schools to publish the "Número de vacantes por aula para el período lectivo" before enrollment. RM 627-2016 (numeral a.6) and RM 657-2017 (numeral 7.3) reserve at least two places per classroom for pupils with special educational needs.

Absence of a section-splitting rule. None of the three rationalization norms contains a provision that requires or authorizes the opening of an additional section when enrollment exceeds the reference (searches for *nueva sección*, *sección adicional*, *desdoblamiento* and *exceda* return nothing). The only splitting rule in the subsequent chain is RVM N° 071-2024-MINEDU, Cuadro 10, note (*****), which allows single-teacher schools with 15 pupils to split into two sections when infrastructure, furniture and a funded post are available; it postdates our window and is conditional. Section counts are therefore a school and UGEL decision constrained by funded posts, not a mechanical function of enrollment.