Price and Market Segmentation Consequences of Targeted Vouchers*

Gabriel Cañedo Riedel   Emiliano Ramírez   Cristián Sánchez

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Abstract

We study how a large targeted voucher reform reshapes tuition-setting incentives and market segmentation in competitive education markets. We focus on Chile's targeted voucher program, introduced in 2008, which increased per-student funding for disadvantaged pupils while prohibiting participating schools from charging top-up fees to eligible students; participation was voluntary. Using administrative data covering the universe of schools and students over 2004–2015, we exploit cross-municipality variation in pre-reform eligibility shares in an event-study design to estimate equilibrium effects on school fees. Throughout, we distinguish between the posted (sticker) top-up fees that schools set and the out-of-pocket fees that particular households face, which under the reform are zero for eligible students at participating schools. We find that the reform segments the price schedule. Posted fees—the fees non-eligible households pay—fall on average in more exposed markets, consistent with participating schools cutting top-up fees for higher-income families. In contrast, the average posted fee attached to the schooling options of eligible students *rises* in more exposed markets: although eligible students pay nothing at participating schools, schools that opt out of the program raise their fees, moving upmarket. We interpret these patterns through models of school competition with peer effects and sorting, and we provide supporting evidence from a pre-reform school choice model with peer composition that families exhibit economically meaningful willingness to pay to avoid low-income peers.

*Gabriel Cañedo Riedel: Department of Economics, University of California San Diego, email, gcanedoriedel@ucsd.edu. Emiliano Ramírez: ITAM, email, emiliano.ramirez.l.14@gmail.com. Cristián Sánchez: School of Business and Economics, Universidad de los Andes, email, cristiansm@gmail.com. We are grateful for comments from Felipe Brugués, Meredith Fowlie, Felix Montag, Mauricio Romero, José Tudón, and seminar participants at the ITAM-UT Austin IO Conference 2025. We thank the Ministry of Education and the *Agencia de Calidad de la Educación* in Chile for providing the data for this paper. All errors are our own.

1 Introduction

Targeted subsidies are a central tool in education policy. By directing additional public resources toward disadvantaged students, governments aim to raise achievement and narrow socioeconomic gaps while preserving the advantages of parental choice and school autonomy. Yet targeted vouchers can also reshape competition among schools because they change schools' effective marginal revenue and the composition of demand they face. When schools can charge top-up fees, a targeted voucher may alter incentives to set prices differently across income groups and to sort students through both prices and program participation decisions. Viewed through the lens of the industrial organization literature on price discrimination, such a policy amounts to policy-induced third-degree price discrimination: schools that join the program are required to price the same seat differently across income groups, while schools that stay out re-optimize a uniform price in response. A long theoretical literature shows that the welfare effects of such differential pricing are in general ambiguous—price discrimination can expand access for low-valuation consumers even as it redistributes surplus (Stole, 2007; Aguirre et al., 2010)—so whether the induced segmentation is harmful is ultimately an empirical question. Understanding these equilibrium responses is essential for evaluating targeted voucher reforms: the distribution of prices that families face and the degree of market segmentation they induce are themselves core outcomes, and they are key mechanisms through which the policy affects access, sorting, quality, and welfare.

This paper studies the effects of Chile's *Ley de Subvención Escolar Preferencial (SEP)*—a large-scale targeted voucher introduced in 2008—on schools' pricing decisions and on market segmentation in primary education. Chile is a natural setting for this question. Since the early 1980s, the country has operated a nationwide voucher system in which public schools and private-voucher schools receive a per-student subsidy, and private-voucher schools may additionally charge a top-up fee.[1] The SEP reform increased per-student funding for economically disadvantaged pupils and introduced a key constraint for participating schools: they could not charge top-up fees to eligible (low-income) students. Participation was voluntary for public and private-voucher schools. As a result, after 2008 the market could feature two price schedules at the same school: a mandated zero fee for eligible students in participating schools and a positive posted fee for non-eligible students.

We ask three related questions. First, how did the targeted voucher reform affect the prices (top-up fees) that schools charged, and did these effects differ across income groups? Second, did the reform lead to market segmentation, in the sense that traditionally low-quality, low-fee schools opted into the program and further reduced their fees, while high-quality, high-fee schools opted out and increased prices? Third, can standard mechanisms in models of school competition—in particular, the interaction between prices and peer composition—help explain the observed pricing patterns?

Our empirical strategy exploits two key features of the policy. The reform was implemented nationwide and unexpectedly, creating a sharp time shock in 2008. At the same time, the reform's potential impact differed across local education markets because municipalities varied substantially in the share of students eligible for the targeted voucher. Following Hsieh and Urquiola (2006) and Sánchez (2019), we define markets

[1] Top-up fees operate under the *financiamiento compartido* (shared financing) regime. Although legally created in 1988 (Law 18,768), the scheme saw little uptake until a 1993 reform (Law 19,247) raised the maximum allowable fee and softened the associated subsidy deduction—which became inversely proportional to the fee charged and capped at 35 percent—after which charging top-up fees became widespread among private-voucher schools. The regime covers private-voucher schools at both primary and secondary levels, and municipal schools at the secondary level only.

at the municipality level and measure each market's *intensity of treatment* as the share of eligible students in the municipality one year before the reform. We then estimate an event-study design that interacts calendar time relative to 2008 with this pre-reform intensity measure. This design captures equilibrium effects operating through both sides of the market: changes in pricing, changes in participation decisions, and changes in student sorting.

We combine administrative data from the Chilean Ministry of Education covering the universe of schools offering primary grades with multiple complementary datasets on students and schools for 2004–2015. We observe school-level top-up fees, enrollment, participation in the targeted voucher program, and school characteristics. These data allow us to distinguish between the fee schedule faced by low-income (eligible) students and that faced by higher-income (non-eligible) students, which is central for interpreting pricing responses under the reform.

Our headline result is that the targeted voucher reform induced a strong segmentation of the fee distribution faced by students. Throughout the paper we are careful to distinguish two objects: the *posted (sticker) top-up fee* that a school sets, and the *out-of-pocket fee* that a given household faces at that school—which, under the reform, is zero for eligible students at participating schools and equals the sticker fee otherwise. For higher-income (non-eligible) students, who pay the sticker fee everywhere, we find that the average fee they face falls in more exposed markets, consistent with participating schools reducing the top-up fees charged to non-eligible students. For low-income (eligible) students, the policy mechanically pushed out-of-pocket fees to zero at participating schools, expanding access to previously fee-charging private-voucher schools; at the same time, the average fee attached to their menu of schools *rises* in more exposed markets, because private-voucher schools that opted out of the program increased their posted top-up fees, moving upmarket. Taken together, these responses imply that the reform did not simply shift the level of fees; it changed the *structure* of prices in a way that is consistent with equilibrium market segmentation.

Our reduced-form findings map naturally into the theoretical predictions of models of school competition with peer effects (Rothschild and White, 1995; Epple and Romano, 1998; Epple et al., 2006). In these frameworks, households value peer composition and schools use tuition and other instruments to sort students, so equilibrium responses to a policy that differentially changes the effective price of enrolling disadvantaged students can take the form of increased differentiation across schools and segmented price schedules. Consistent with this logic, we find that the targeted voucher induces opposing fee movements across income groups and changes the structure of posted fees through voluntary program participation. To provide evidence on a necessary condition of the key mechanism, we estimate a school choice model with peer composition tailored to the Chilean context using pre-reform (2007) data. We find that families exhibit economically meaningful willingness to pay to avoid low-income peers, implying that schools have incentives to adjust prices for non-eligible families to offset peer-composition disutility when participation increases the share of disadvantaged students.

This paper contributes to the literature on vouchers, school competition, and the equilibrium effects of targeted education policies by documenting how a redistributive reform can reshape the *price schedule* faced by different socioeconomic groups. First, we complement work on competition and sorting under generalized school choice systems (Hsieh and Urquiola, 2006) by focusing on a targeted voucher implemented in a context where schools can charge top-up fees and can choose whether to participate. In this environment, the relevant object for distributional analysis is not a single posted tuition, but a menu of prices that depends on program participation and student eligibility. We show that targeted vouchers generate a sharp, policy-induced wedge in feasible pricing—zero fees for eligible students in participating schools alongside positive fees for non-eligible students—and that schools respond endogenously by adjusting the fees charged to non-eligible students and by sorting into (in/out) regimes. These equilibrium price responses yield segmentation patterns that are distinct from, and potentially interact with, the longer-run achievement and inequality impacts of Chile's targeted voucher program studied in Correa et al. (2014), Navarro-Palau (2017), Sánchez (2019), Aguirre (2020), Sánchez (2026), Gazmuri (2024), and Neilson (2025), among others. More broadly, our focus on supply-side price responses to per-student funding connects to work on how school finance shapes the behavior of choice-based suppliers in other settings, including charter school funding and incentives (Ferreyra and Kosenok, 2018; Singleton, 2019) and public school responses to funding and competition (Dinerstein and Smith, 2021).

Second, our results speak to the broader voucher evidence base that has largely emphasized individual-level outcomes and partial-equilibrium effects (see Rouse and Barrow, 2009; Epple et al., 2017, for surveys). In influential randomized voucher settings such as Colombia (Angrist et al., 2002, 2006), vouchers can raise attainment and test scores; evidence on the competitive responses of schools to voucher pressure (Chakrabarti, 2008; Figlio and Hart, 2014; Hoxby, 2003) and on price and quality setting in private schooling markets (Andrabi et al., 2017) has focused mostly on quality margins. Our contribution is to show how, in a nationwide program with voluntary school participation and top-up fees, a targeted subsidy can also induce strategic supply responses that reallocate who faces which prices. By estimating market-level event-study effects that incorporate both demand-side sorting and supply-side participation and pricing, we provide direct evidence on the equilibrium mechanisms that mediate access and stratification when vouchers operate through competitive school markets.

Third, we connect reduced-form evidence on fees to mechanisms emphasized in canonical models of school competition with peer effects (Rothschild and White, 1995; Epple and Romano, 1998; Epple et al., 2006); note that in Epple et al. (2006), tuition discounts to desirable students are precisely an equilibrium form of price discrimination. We interpret the observed fee changes as consistent with strategic complementarities between prices and peer composition (Allende, 2019): after the reform, schools serving a higher share of disadvantaged students may lower fees for non-eligible families to offset peer-related disutility, while schools opting out can raise fees as their demand becomes relatively less price elastic (cream-skimming incentives of this kind are surveyed in Altonji et al., 2015). Our pre-reform school choice estimates provide supporting evidence for this channel by revealing economically meaningful willingness-to-pay to avoid low-income peers. The segmentation consequences we document also complement a growing policy literature on how school-assignment and funding rules shape socioeconomic segregation across schools (Monarrez et al., 2022; Bjerre-Nielsen and Gandil, 2024; Crema, 2024). In our setting, segregation forces operate through a price schedule that the policy itself makes group-specific.

The remainder of the paper proceeds as follows. Section 2 describes Chile's school system and the targeted voucher program. Section 3 presents the administrative data. Section 4 lays out the identification strategy, reports the empirical results on fees, and presents an extensive battery of robustness and heterogeneity analyses. Section 5 develops and estimates the pre-reform school choice model and uses it to interpret the event-study results. Section 6 concludes.

2 The Chilean School System and the Targeted Voucher Program

Schools in Chile can be organized within three main groups according to their management and financing scheme: public schools, private-voucher schools, and private-non-voucher schools. Both public and private-voucher schools are financed by per-student voucher subsidies paid by the government directly to the schools. In addition to the subsidies, private-voucher schools are allowed to charge fees on top of the vouchers they receive. Private-non-voucher schools are entirely financed by fees charged to parents, and serve the country's richest families. In 2015, the last year of our sample, 55% of students in primary education grades attended public schools, 40% attended private-voucher schools, and 5% attended private-non-voucher schools (own calculations from the enrollment registries described in Section 3); the private-voucher share has risen steadily over time, at the expense of the public sector.

Since 1981, every student in Chile is entitled to an individual voucher to be used to (at least partially) cover tuition at any public or private-voucher school of her choice. In 2008, the government introduced a new source of subsidy to complement the existing flat voucher in the form of a targeted voucher to economically disadvantaged students. In February of that year, the *Ley de Subvención Escolar Preferencial (SEP)* law that regulates this new subsidy was enacted and immediately put into practice for the 2008 academic year.[2] The law mandates that each school that participates in the program receives an additional subsidy per every eligible low income student that it enrolls, with the requirement of not charging top-up fees to those students. In addition, each participating school also receives a per-student subsidy that depends on the share of low income students enrolled in the school, called *Subvención por Concentración*, or concentration subsidy. Participation in the program is voluntary on the part of schools, and only public and private-voucher schools are eligible to join. Participating schools are required to set short- and long-term learning goals (i.e. test score achievements), which are evaluated by the government every four years.

Table 1 displays the evolution of the monthly per-student voucher subsidies corresponding to elementary grades 1st–4th, decomposed by its different categories, for the years 2004–2015. The voucher amounts are in US$ as of May 2016, and correspond to schools with full day shifts. The universal voucher, which is set to represent the unit cost of educating a student, starts at $63 in 2004 and steadily increases to reach $93 in 2015. The targeted voucher is set to represent the additional unit cost of educating a low income student, and starts at $37 in 2008, reaching up to $57 in 2015. The concentration subsidy is considerably lower, and stays fairly constant over time at about 4% and 10% of the universal voucher, depending on the share of eligible low-income students enrolled in the school.

[2]The yearly academic calendar in Chile starts in March and ends in December.

Table 1: Monthly Voucher Subsidy Decomposition for Students in 1st–4th Grades

	subsidy (US$)											
category	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015
universal voucher	62.66	63.07	64.92	63.82	62.40	80.86	82.48	82.71	84.77	91.50	90.93	92.62
targeted voucher	–	–	–	–	36.81	42.86	43.71	43.84	44.93	55.93	55.59	56.62
concentration subsidy:												
15–30%	–	–	–	–	2.66	3.00	3.06	3.06	3.14	3.89	3.87	3.94
30–45%	–	–	–	–	4.56	5.14	5.24	5.26	5.39	6.67	6.62	6.75
45–60%	–	–	–	–	6.08	6.85	6.99	7.01	7.18	8.88	8.82	8.99
more than 60%	–	–	–	–	6.84	7.71	7.86	7.89	8.08	9.97	9.91	10.09

Notes: All values are real, and are converted from Chilean pesos to US dollars according to the exchange rate of Ch$686.52 per US dollar, as of May 16, 2016. The universal voucher values correspond to students at schools with full day shifts.

The criteria to classify a student as being eligible to be a beneficiary of the targeted voucher program was set to cover households in the lowest 40% of the income distribution. The eligibility criteria include various components, but the two most important are: (1) the household belongs to the lowest 33% of a socioeconomic index distribution used by the government to guide the delivery of social programs (i.e. *Ficha de Protección Social*), and (2) the household participates in *Chile Solidario*, a social program that serves families in vulnerable conditions.[3] By 2011, about 48% of students in the country were classified to be eligible to receive the targeted voucher, and 88% of them (42% of all students) enrolled in participating schools (either a public school or a private-voucher school that joined the program). In other words, the program impacts almost half of the student population in the country.

In line with its motivating goals, the targeted voucher reform was able to expand access to ex-ante more expensive private schools for some low-income students. To describe this shift, we rank low-income students in a given year by the *pre-reform* (2007) top-up fee of the school they attend, so that the statistic is unaffected by post-reform fee changes. In 2007, more than 75% of low-income students attended schools that charged no fee, so the 75th percentile of this (student-level) distribution was $0. By 2013, the low-income student at the 75th percentile attended a school that had charged $10.3 per month before the reform. Moreover, while 58% and 42% of low-income students attended public and subsidized private schools in 2007, respectively, this enrollment distribution shifted towards the private sector six years after the start of the reform, with 48% and 52% of low-income students enrolling in public and subsidized private schools, respectively.

Pricing behavior also diverged sharply after the reform between private-voucher schools that opted into the program and those that stayed out. Before the reform, schools effectively charged a single posted fee; among eventual participants, this fee averaged $12 per month. Six years after the introduction of the reform, participating schools charged zero top-up fees to low-income students and charged on average $9.9 to high-income students. Schools that stayed out of the program, which already charged far higher fees before the reform ($61.6 on average), posted $74.6 after the reform. Together, these patterns are suggestive of a market segmentation response, with participating schools becoming relatively more affordable to low-income students while non-participating schools moved upmarket.

[3]See Aguirre (2020) and Neilson (2025) for more details on the criteria to classify a student to be eligible to receive the targeted voucher.

3 Data

We combine various administrative data sets of Chilean students and schools for the years 2004–2015 to form a twelve-year (unbalanced) panel sample for schools, and a twelve-year (unbalanced) panel sample for municipalities. The data were obtained from the Ministry of Education, and include the censuses of students and schools.[4] Our analysis sample consists of the universe of Chilean schools that offer any of 1st–8th grades (i.e. primary education).

Table 2 displays the total number of schools offering primary level grades, by type and participation in the targeted voucher program status, for the years 2004–2015. Panel A shows the number of schools, overall and by type. The total number of schools decreases slightly over time, going from 8,908 schools in 2004 to 8,425 in 2015, possibly reflecting the recent changes in demographics and family size preferences in Chile. Public and private-voucher schools represent about 94% of all schools in the country, highlighting the important (ex-ante) potential reach of the targeted voucher program. Public schools are the most numerous, but they importantly decrease their presence over the period studied, going from 5,426 in 2004 to 4,613 in 2015. Private-voucher schools, in contrast, see an increase in their presence in the system, going from 2,928 in 2004 to 3,386 in 2015. This observed pattern for public and private-voucher schools is consistent with estimated models of families' preferences for school types in Chile.[5] Private non-voucher schools represent a small fraction of all schools, and are 554 in 2004, and 426 in 2015. Panel B displays the number of schools that participate in the targeted voucher program, overall and by type. Almost all public schools (97%) immediately join the program in 2008, the year of its introduction. Public schools' participation in the program remains almost universal throughout the period, reaching 99% in 2015. Private-voucher schools' participation is also important in 2008, with 47% of these schools joining the program. Private-voucher schools' participation rapidly increases over time, reaching 75% in 2015. These figures highlight the important ex-post reach of the targeted voucher program in the Chilean system of education.

Table 2: Schools by Type and Participation in the Targeted Voucher Program

	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015
A. All	8,908	8,924	8,871	8,839	8,827	8,848	8,804	8,699	8,674	8,573	8,491	8,425
public	5,426	5,397	5,267	5,192	5,126	5,080	5,004	4,882	4,826	4,740	4,660	4,613
private-voucher	2,928	3,068	3,159	3,202	3,259	3,336	3,365	3,390	3,425	3,414	3,409	3,386
private-non-voucher	554	459	445	445	442	432	435	427	423	419	422	426
B. In Program	–	–	–	–	6,503	6,864	6,912	7,011	7,150	7,133	7,111	7,125
public	–	–	–	–	4,957	4,969	4,898	4,782	4,759	4,665	4,618	4,582
private-voucher	–	–	–	–	1,546	1,895	2,014	2,229	2,391	2,468	2,493	2,543

Notes: Panel A shows the number of schools that offer primary education. Panel B shows the number of schools that offer primary education and that participate in the targeted voucher program.

We describe our analysis sample and outcomes of interest in Table 3. We keep in the final sample all school-year observations with non-missing values in every variable of analysis. The resulting panel of private-voucher schools is unbalanced: the number of observations grows from 2,853 in 2004 to 3,374 in 2015,

[4]See Appendix A for a more detailed description of each of the data sets we use.

[5]See Sánchez (2026), and Neilson (2025), among others.

which mirrors the expansion of the private-voucher sector documented in Table 2 (2,928 to 3,386 schools). Because entry is part of the equilibrium response we study, we keep entrants in the main sample; Section 4.4 shows that the fee results are not driven by compositional change, by re-estimating everything on the set of schools that never enter or exit. Table 3 shows the average and the standard deviation (in parentheses) of top-up fees charged by private-voucher schools, for the years 2004–2015. Fees are in real US$, as of May 2016. Notice that, before the introduction of the targeted voucher program, schools were allowed to charge a unique (possibly positive) fee on top of the subsidies to all students, regardless of the socioeconomic status of the student. With the start of the targeted policy, schools that decide to join the program are mandated to charge zero fees to eligible low-income students, and continue to be allowed to charge any (possibly positive) top-up fee to higher income students. Non-participating schools, in contrast, continue to be allowed to charge a unique top-up fee to all students. Thus, with the targeted reform in place, there exist two vectors of top-up fees in the market, one faced by low-income students that are eligible to be beneficiaries of the targeted voucher program, and another faced by non-eligible higher-income students. Before the reform, the average top-up fee is \$16.14 in 2004, and it slightly increases to \$17.24 in 2007. After the introduction of the targeted voucher reform, top-up fees faced by low income students are considerably reduced to about \$14–15, on average. In contrast, top-up fees charged to higher income students continue to increase slowly, reaching \$20.04 in 2015.

Table 3: Summary Statistics - Private-voucher School Top-up Fees

	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015
low income	16.14	17.36	18.34	17.24	14.46	14.97	15.04	14.40	14.96	15.41	14.95	14.71
	(25.29)	(26.81)	(31.25)	(26.92)	(27.31)	(34.07)	(29.86)	(30.05)	(35.24)	(37.28)	(33.32)	(34.31)
higher income	16.14	17.36	18.34	17.24	17.82	19.33	19.29	18.98	20.49	20.87	20.13	20.04
	(25.29)	(26.81)	(31.25)	(26.92)	(27.47)	(34.36)	(30.39)	(30.42)	(36.67)	(37.36)	(33.51)	(34.67)
observations	2,853	3,013	3,106	3,143	3,201	3,247	3,283	3,341	3,369	3,397	3,395	3,374

Notes: School-level mean values (each school counts once, regardless of enrollment). Standard deviations are in parentheses. The "low income" row reports the average fee that an eligible student would face at each school (zero at participating schools from 2008 onward, the posted fee elsewhere); the "higher income" row reports the posted (sticker) fee, which non-eligible students pay at every school. All values are real, and are converted from Chilean pesos to US dollars according to the exchange rate of Ch\$686.52 per US dollar, as of May 16, 2016. The yearly observation counts sum to 38,722; the event-study regressions in Section 4 use 38,271 of these observations, because 451 school-year observations belong to municipalities for which the 2007 treatment-intensity measure cannot be constructed (mainly municipalities created after 2008).

4 The Effects of Targeted Vouchers on School Prices

4.1 Identification Strategy

We follow Hsieh and Urquiola (2006) and Sánchez (2019), and use the geopolitical boundaries of municipalities to define educational markets in Chile.[6] Enrollment data is consistent with this definition, as about 90% of students attend a school that is located in the same municipality of their residence.

The particular design of the targeted voucher program implies that some markets are more affected by

[6]See Topel (1986) and Card (2001) for other studies that use political and administrative boundaries to define local markets.

the program than others, depending on their share of students that are eligible to be beneficiaries of the new subsidy (i.e. low income students). To make this point clear, take the extreme case of a market in which no eligible low income student resides. The targeted voucher reform has zero incidence in this market, in terms of adding new funds, because no student is eligible to receive the targeted subsidy. Conversely, a market in which all students come from economically disadvantaged families has the maximum potential of receiving additional funds.[7] Thus, it is possible to argue that different markets have different intensities of treatment, and that these treatment intensities depend on the markets' share of eligible students.

To avoid endogeneity issues when conducting our empirical analysis, we use students' municipality of residence one year prior to the introduction of the targeted voucher program.[8] This variable is highly correlated with the current municipality of residence, and is free of endogeneity because the residential decision was made before the program was announced and implemented.[9]

A practical complication is that the administrative registry of eligible (*prioritario*) students begins in 2008, the program's first year, so no direct 2007 eligibility measure exists. We construct the pre-reform intensity measure by linking the 2008 eligibility registry to the 2007 enrollment roster through the national student identifier: for each municipality, the intensity is the share of students enrolled in grades 1–4 in 2007 (by municipality of residence) who appear as eligible in the 2008 registry. Because eligibility is determined by slow-moving household characteristics (income, participation in social programs), the 2008 classification is a good proxy for the same household's 2007 status. One minor limitations follows from this construction: municipalities created after 2008 lack the measure and drop from the estimation sample (451 school-year observations; see the notes to Tables 3 and B.1). Appendix A provides further detail.

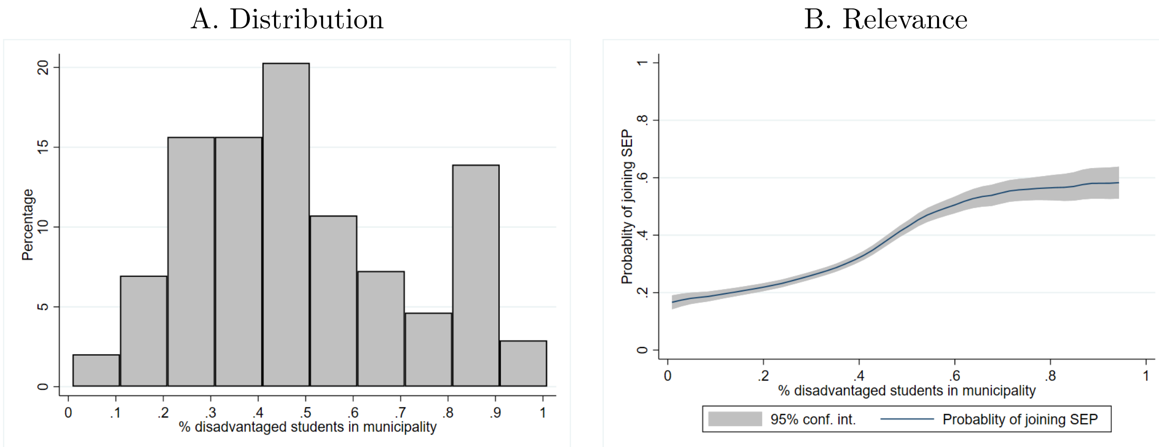
Figure 1 displays the distribution and relevance of the intensity-of-treatment variable. Panel A shows the distribution of municipalities according to their share of eligible students one year before the targeted voucher reform was implemented. The support of the distribution covers the full $[0, 1]$ range. About half of municipalities have between 20% and 50% of eligible low-income students, and only a few have less than 10% or more than 90% of eligible students. Panel B displays a nonparametric estimate of the probability that a private-voucher school joins the targeted voucher program in the first year of implementation (2008) as a function of the municipality's share of low-income students in 2007. The estimated function is monotonically increasing over $[0, 1]$, with the estimated probability rising from about 0.2 to about 0.6. We therefore conclude that (1) the distribution of the intensity-of-treatment variable provides enough variation for our analysis, and (2) the share of low-income students in the municipality (one year before the reform) is a strong predictor of private-voucher schools' participation decisions.

[7] We are careful to say that this increase in the funding is only potential (ex-ante) because it primarily depends on schools deciding to participate in the program. However, as we show below, the share of eligible students in the market highly predicts the likelihood that a school chooses to participate in the targeted voucher program.

[8] As opposed to the municipality of residence prior to the introduction of the program, the current municipality of residence is subject to endogeneity issues via, for example, endogenous migration (Rosenzweig and Wolpin, 1988).

[9] 88% of 4th graders in 2008-2011 live in the same municipality as they did in 2007.

Figure 1: Distribution and Relevance of Intensity of Treatment



Notes: Panel A: distribution of markets according to their share of low income students one year prior to the introduction of the targeted voucher reform (2007). Panel B: nonparametric estimation of the probability that a private-voucher school joins the targeted voucher program in the first year of its implementation (2008), with respect to the share of low income students in the market one year prior to the introduction of the targeted voucher program (2007).

4.2 Empirical Strategy

We exploit the exogenous variation given by the (unanticipated) implementation of the targeted voucher reform, as well as the one given by the intensity of treatment in the form of the share of eligible students in the municipality (one year prior to the reform) to implement an event study design. The implementation of the event study allows us to test whether the reform has an effect on the various variables of interest presented in Table 3.

To test the effects of the reform on schools' top-up fees, we estimate the following equation,

$$y_{ist} = \gamma_s + \lambda_t + \sum_{\substack{\eta=-4 \\ \eta\neq-1}}^{7} \delta_\eta (D_{\eta t} \times \text{intensity}_s) + \varepsilon_{ist}, \tag{1}$$

where y_{ist} is the outcome of interest (fees) for school i in municipality s in period t, γ_s is a municipality fixed effect, λ_t is a year fixed effect, $D_{\eta t}$ is a year dummy variable, with $\eta = 0$ indicating the year of the introduction of the reform (2008), intensity_s is the intensity of treatment (i.e. share of eligible low-income students in the municipality of residence one year before the reform), and ε_{ist} is an error term. The interaction for $\eta = -1$ (the year 2007) is omitted, so that each δ_η measures the difference relative to the year immediately preceding the reform. In the empirical implementation of this regression, we cluster the standard errors at the municipality level (Bertrand et al., 2004; Cameron and Miller, 2015). The coefficients of interest are δ_η, as they capture the period effects of the targeted voucher reform.

The coefficients δ_η measure the effect of the reform *per unit of treatment intensity*: they compare the outcome evolution of markets with different pre-reform eligibility shares, scaled so that the coefficient corresponds to a hypothetical contrast between a market where all students are eligible and one where none is. Because our design is a difference-in-differences with a continuous treatment dose adopted at a common date, this scaling should be read with care: extrapolating δ_η literally to a zero-to-one change in the eligibility share requires the dose–response relationship to be approximately linear (Callaway et al., 2024; de Chaisemartin et al., 2026a). In Section 4.4 we probe this reading directly—estimating the event study by terciles of the intensity, testing the linearity of the dose-response relationship, and re-estimating the effects with the heterogeneity-robust estimator of de Chaisemartin et al. (2026a)—and find that the linear specification is a reasonable summary of the data. With all municipalities untreated before 2008, the identifying assumption is the standard one—that fee trends absent the reform are mean-independent of the 2007 eligibility share—and it imposes no restriction on treatment-effect heterogeneity (de Chaisemartin et al., 2026b). Our estimates incorporate equilibrium effects on the demand (e.g. student sorting among schools) and on the supply (e.g. schools' decision to participate in the program) sides of the market. Moreover, our results include the responses of both participating and non-participating schools.[10]

Our treatment, and therefore its interpretation, is in contrast to that of Correa et al. (2014) and Feigenberg et al. (2019), which corresponds to the effect of a school choosing to participate in the program, and does not necessarily account for equilibrium effects in the market.

4.3 Results

Before we examine the effects of the targeted voucher reform on private-voucher schools' top-up fee decisions, it is important to note that before the introduction of the targeted voucher program, all private-voucher schools were allowed to charge a unique fee to students on top of the per student subsidy they received. The reform, once implemented, did not bring any change to the pricing rules for non-participating schools. However, private-voucher schools that decided to be part of the targeted voucher program faced a new restriction on the fees they charge to low-income students, which resulted in these schools (potentially) charging differently to low- and higher-income students. Specifically, participating schools were mandated to charge zero top-up fees to low income students, and they continued to be allowed to charge any (possibly positive) fee to higher income students. Consequently, as long as at least one private-voucher school joins the program, two different vectors of top-up fees exist in the market: one that is faced by low income students, who are charged zero in participating schools and the sticker top-up fee in non-participating schools; and another that is faced by higher income students, who pay the sticker top-up fee at each school, regardless of the program participation status of the school.

Thus, examining the effects of the reform on private-voucher schools' top-up fee decisions necessarily implies distinguishing between the effects on the top-up fees charged to low-income students and the effects on the top-up fees charged to higher income students.

To make things clear, let p_j be the top-up fee private-voucher school j charges in the absence of the targeted voucher reform. Consequently, $E\left[p_j\right]$ is the expected top-up fee schools charge in the absence of the reform. With the introduction of the targeted voucher program, schools must decide whether they participate in the program or not. Denote by $\tau_j \in \{0,1\}$ school j's decision to participate in the targeted voucher program once implemented (1 indicates participation, 0 non-participation). Also, let p_j^1 denote the

[10] In a game-theoretic and oligopolistic competition setting, where a school's strategies impose externalities on its competitors, the targeted voucher reform impacts the decisions of schools that decide to participate in the program, as well as the strategies of schools that choose not to participate in the program. See, for instance, the model developed in Sánchez (2026).

counterfactual top-up fee school j charges to high income students if the school participates in the program.[11] Similarly, let p_j^0 denote the counterfactual top-up fee school j charges to all students in the case the school does not participate in the program. Thus,

$$\begin{aligned} E\left[p_j^L\right] &= E\left[\tau_j 0 + (1-\tau_j)p_j^0\right] \\ &= E\left[(1-\tau_j)p_j^0\right] \end{aligned}$$

is the expected top-up fee schools charge to low income students under the targeted voucher program, which includes the mandated zero fees for participating schools ($\tau_j = 1$) and the sticker fee for non-participating schools ($\tau_j = 0$). Similarly,

$$E\left[p_j^H\right] = E\left[\tau_j p_j^1 + (1-\tau_j)p_j^0\right]$$

is the expected top-up fee schools charge to higher income students under the targeted voucher program.

Using this notation, the effect of the reform on the average top-up fee schools charge to low income students is

$$E\left[p_j^L\right] - E\left[p_j\right] = E\left[(1-\tau_j)p_j^0\right] - E\left[p_j\right]. \tag{2}$$

Likewise, the effect of the reform on the average top-up fee schools charge to higher income students is

$$E\left[p_j^H\right] - E\left[p_j\right] = E\left[\tau_j p_j^1 + (1-\tau_j)p_j^0\right] - E\left[p_j\right]. \tag{3}$$

Figure 2 presents the effects of the targeted voucher reform on the average top-up fees faced by low-income students (panel A) and on the average top-up fees schools charge to higher-income students (panel B).[12] To be precise about the two outcomes: panel B uses each school's posted (sticker) fee, which non-eligible students pay at every school; panel A uses the fee an eligible student would face at each school, which equals zero at participating schools from 2008 onward and the posted fee at non-participating schools. Both outcomes are school-level averages; enrollment-weighted versions, which answer the distinct question of what the *average student* is exposed to, are presented below.

The pre-period coefficients at $\eta = -4$ and $\eta = -3$ are small and statistically indistinguishable from zero, supporting the parallel-trends assumption. The exception is $\eta = -2$, where both series (which coincide before 2008) show a negative and statistically significant coefficient of -5.4 (s.e. 2.3).[13]

Panel A shows that on average the top-up fee attached to the schools available to low-income students *rises* with treatment intensity after the reform. The response is immediate, statistically significant in most years, and fluctuates around \$3–6 per unit of intensity. Given that participating schools are mandated to charge no fee to low-income students, this increase is necessarily driven by non-participating schools raising their fees, as is shown in equation (2). It bears emphasis that this is a statement about the fee *menu* facing eligible students, not about what they pay: most eligible students attend participating schools and pay zero.

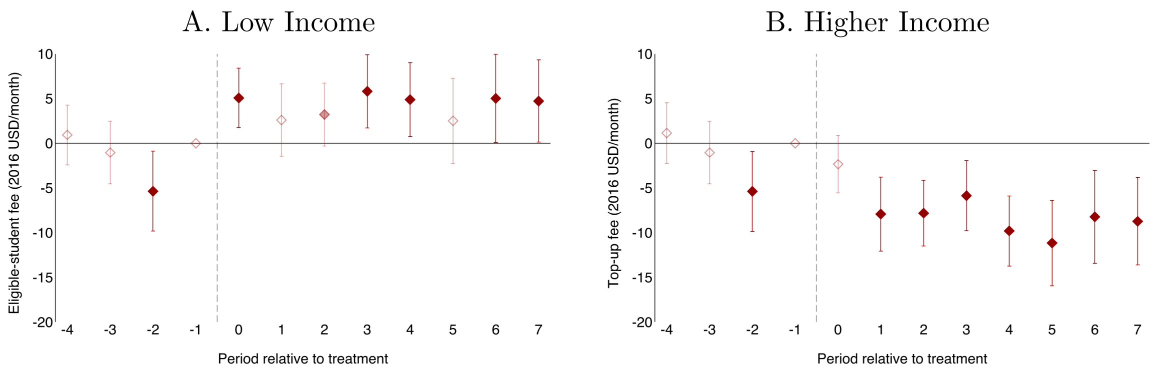
[11] Recall that participating schools are mandated to charge \$0 to low income students.

[12] Table B.1 in Appendix B presents the corresponding point estimates and standard errors.

[13] Section 4.4 reports formal sensitivity analyses in the spirit of Rambachan and Roth (2023) that ask how large post-period violations of parallel trends would have to be, relative to this pre-period deviation, to overturn the conclusions.

Panel B shows that the reform induces private-voucher schools to lower the top-up fees they charge to higher-income students, on average. The response builds quickly: the effect in the adoption year (2008) is a statistically insignificant $-\$2.4$, and from 2009 onward the estimates range from about $-\$6$ to $-\$11$ per unit of intensity. Effects of \$6–11 represent roughly 35–65% of the average fee private-voucher schools charge in 2007.

Figure 2: Effects of the Targeted Voucher Program on Private-voucher Schools - Top-up Fees



Notes: Point estimates of δ_η from equation (1) and 95% confidence intervals, from school-level data for 2004–2015 (N = 38,271). The omitted period is $\eta = -1$ (2007). Panel A's outcome is the fee an eligible (low-income) student would face at each school: zero at participating schools from 2008 onward, the posted fee otherwise. Panel B's outcome is the posted (sticker) top-up fee, which non-eligible (higher-income) students pay at every school. Fees are monthly, in real US dollars, converted from Chilean pesos according to the exchange rate of Ch\$686.52 per US dollar, as of May 16, 2016. Standard errors are clustered at the municipality level. Hollow diamonds denote statistical insignificance; light colored diamonds denote statistical significance at the 10% level; dark colored diamonds denote statistical significance at the 5% level.

To have a better understanding of our findings, we analyze them through the lens of equations (2) and (3). Our results imply that equation (2) is greater than zero, and equation (3) is less than zero, simultaneously. Equivalently,

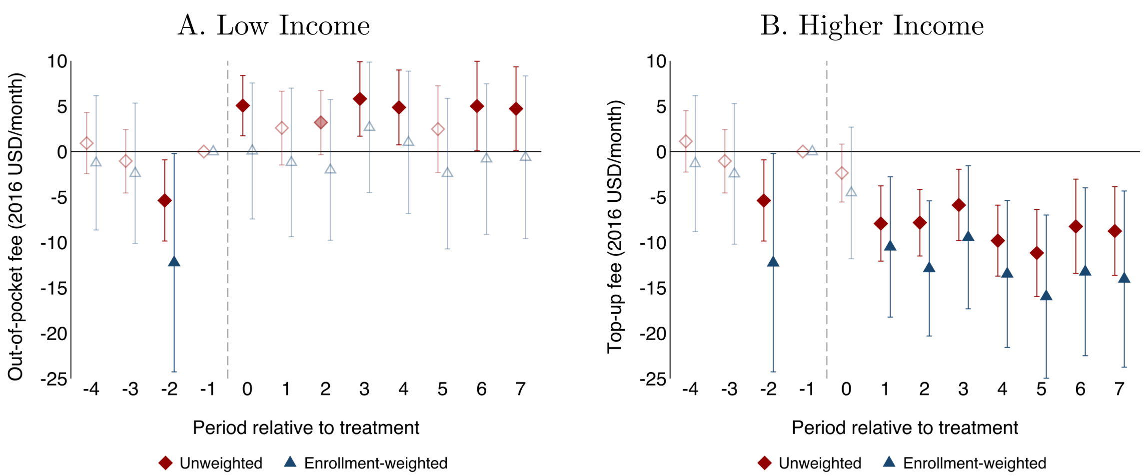
$$E\left[(1-\tau_j)(p_j^0 - p_j)\right] \quad > \quad E\left[\tau_j p_j\right] \geq 0 \tag{4}$$
$$E\left[\tau_j(p_j^1 - p_j)\right] \quad < \quad -E\left[(1-\tau_j)(p_j^0 - p_j)\right] \leq 0, \tag{5}$$

which implies market segmentation in prices; that is, participating schools reduce the fees they charge to higher-income students, while non-participating schools increase their top-up fees. Two caveats on this reading are in order. First, conditions (4) and (5) are *derived* from the signs of the two estimated aggregate effects; they are not separately estimated, because our design identifies market-level averages and participation is an endogenous choice. Second, event studies estimated separately by participation status (Section 4.4) cannot recover the group-specific counterfactuals in (4)–(5) either, precisely because the composition of participants and non-participants itself responds to the treatment intensity. We therefore treat the decomposition as the internally consistent interpretation of the two aggregate effects, and we present the split estimates below as descriptive evidence.

4.4 Robustness and Heterogeneity

Enrollment-weighted estimates. The baseline estimates weight each school equally, which is the right object for studying schools' pricing behavior. To measure what the *average student* is exposed to, we re-estimate equation (1) weighting schools by their 2007 enrollment (fixing weights at their pre-reform values so that post-reform enrollment responses do not contaminate the comparison). Figure 3 overlays the two sets of estimates, and Table B.2 reports the coefficients. For the fee menu faced by eligible students (panel A), the enrollment-weighted increase essentially disappears (e.g., 0.1, s.e. 3.8, in 2008): the rise documented in panel A of Figure 2 is driven by smaller non-participating schools, so the average student's fee menu did not become more expensive even as the average school's posted fee for eligible students rose. For the posted fee (panel B), the enrollment-weighted declines are substantially *larger* than the unweighted ones—between $-\$9$ and $-\$16$ per unit of intensity from 2009 onward—indicating that larger schools cut fees by more. Both facts sharpen the segmentation interpretation: fee reductions were concentrated where students actually are, while fee increases were concentrated in smaller schools moving upmarket.

Figure 3: Effects of the Targeted Voucher Program on Private-voucher Schools - Top-up Fees, Unweighted versus Enrollment-weighted



Notes: Event-study estimates of equation (1) with 95% confidence intervals; the omitted period is $\eta = -1$ (2007). Panels follow the convention of Figure 2: panel A's outcome is the fee an eligible (low-income) student would face at each school, and panel B's outcome is the posted (sticker) top-up fee paid by non-eligible (higher-income) students. The unweighted series weights each school equally (N = 38,271); the enrollment-weighted series weights schools by their 2007 enrollment (analytic weights; N = 35,028, as schools not observed in 2007 are excluded). Standard errors clustered at the municipality level. Fees are monthly, in 2016 US dollars. For each series, hollow markers denote statistical insignificance; light colored markers denote statistical significance at the 10% level; dark colored markers denote statistical significance at the 5% level.

Nonparametric dose-response: terciles of exposure. The baseline specification imposes a linear relationship between the 2007 eligibility share and the outcome evolution. As a transparent check, we split municipalities into terciles of the intensity distribution (cutoffs at eligibility shares of 0.33 and 0.51) and estimate a single event study with separate year effects for the middle and top terciles, the bottom tercile serving as the comparison group. We run the split on both fee schedules, as in Figure 2. Panels A and B of Figure 4 show the results, and Table B.3 reports the coefficients.

For higher-income students, in panel B, effects are broadly increasing in exposure. Top-tercile posted fees fall by \$2.4–5.3 relative to the bottom tercile from 2009 onward, significant at the 1% level in every year, and middle-tercile effects are smaller in magnitude in five of the seven post-reform years. Scaled by the difference in average intensity between the top and bottom terciles, roughly 0.48, these magnitudes are consistent with the per-unit-of-intensity estimates from the linear specification.

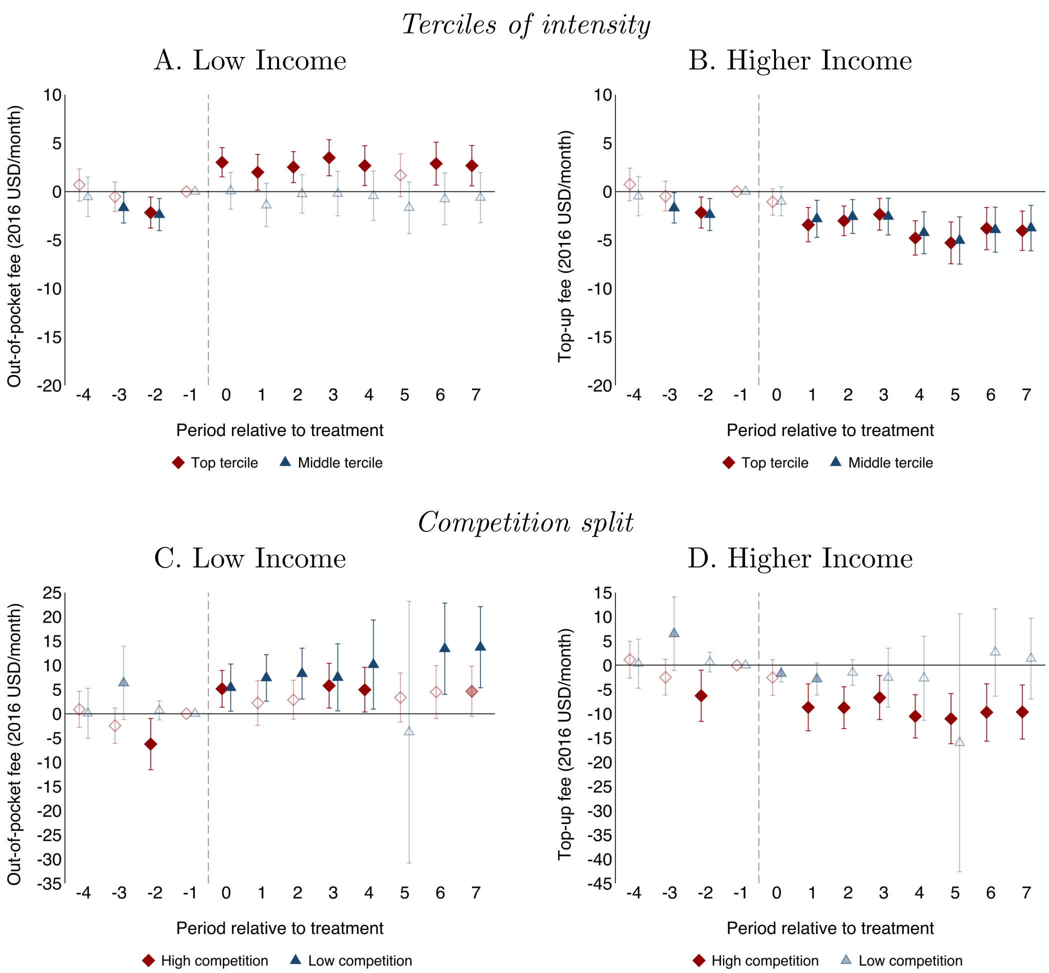
For low-income students, in panel A, the split reproduces the opposite sign. The fee menu facing eligible students in top-tercile municipalities *rises* by \$1.7–3.5 relative to the bottom tercile, significant at the 5% level in every post-reform year except 2013, while middle-tercile effects are small and statistically indistinguishable from zero. The two panels thus recover the segmentation pattern of Figure 2 without imposing linearity in the dose, and they do so with opposite signs in the same municipalities, which is exactly what a segmented price schedule implies.

Heterogeneity by market structure and pre-reform fees. Panels C and D of Figure 4 split the sample by the number of private-voucher schools in the school's municipality in 2007 (above versus below the median among sample municipalities; Table B.4). The posted-fee declines, in panel D, are concentrated in more competitive markets. Schools in above-median-competition municipalities cut fees by \$6.7–11.0 per unit of intensity, significant at the 1% level, while estimates in below-median markets are small and imprecise. This is consistent with the price-competition mechanism: fee responses require competitors to respond to.

The fee menu facing eligible students, in panel C, moves in the opposite direction and with the opposite gradient (Table B.5). Increases are larger in *less* competitive markets, at \$5.4–13.7 per unit of intensity in every post-reform year but 2013, where that subsample of only 4,844 school-year observations yields an imprecise negative estimate. In more competitive markets they are smaller, at \$2.2–5.8, and significant in only about half the post-reform years. Read together, the two panels say that competition disciplines the posted fee where rivals are numerous, while non-participating schools have the most room to move upmarket where rivals are few.

We also split by the school's own pre-reform fee. Figure C.1 in Appendix C contrasts schools that charged a positive fee in 2007 with those that charged nothing. Within the fee-charging group, Table B.4 shows that the posted-fee declines are concentrated in schools whose 2007 fee was *above* the median among fee-charging schools, at $-$\$10.6 to $-$\$23.0 from 2009 onward, against $-$\$3.1 to $-$\$8.8 for below-median ones. These are precisely the schools with room to cut. Schools that charged nothing in 2007 display non-parallel pre-trends, converging toward zero fees already before the reform in more exposed markets, so we do not emphasize that subgroup.

Figure 4: Event Studies by Terciles of Exposure and by Market Competition - Top-up Fees



Notes: Columns follow the convention of Figure 2: the left column's outcome is the fee an eligible (low-income) student would face at each school (zero at participating schools from 2008 onward, the posted fee otherwise), and the right column's outcome is the posted (sticker) top-up fee paid by non-eligible (higher-income) students. Panels A and B: event-study coefficients on year dummies interacted with indicators for the middle and top terciles of the 2007 municipal eligibility share; the bottom tercile is the comparison group; tercile cutoffs are 0.33 and 0.51. Panels C and D: baseline event study estimated separately for schools in municipalities with an above-median versus below-median number of private-voucher schools in 2007 (median = 9 among sample municipalities). Vertical scales are set panel by panel so that confidence intervals are not clipped, and therefore differ across panels. 95% confidence intervals; standard errors clustered at the municipality level. For each series, hollow markers denote statistical insignificance; light colored markers denote statistical significance at the 10% level; dark colored markers denote statistical significance at the 5% level.

Heterogeneity-robust estimation. Our design is a heterogeneous adoption design in the sense of de Chaisemartin et al. (2026a): all municipalities are untreated before 2008, and all receive a heterogeneous but persistent dose at a common date, so no municipality remains untreated. In this setting, two-way fixed-effects estimates identify a convex average of treatment effects only if the dose–response is linear and effects are homogeneous. Otherwise, some of the implicit comparisons receive negative weight (de Chaisemartin and D'Haultfœuille, 2020; Callaway et al., 2024). We therefore implement the diagnostic protocol and the estimator that de Chaisemartin et al. (2026a) propose for exactly this design.

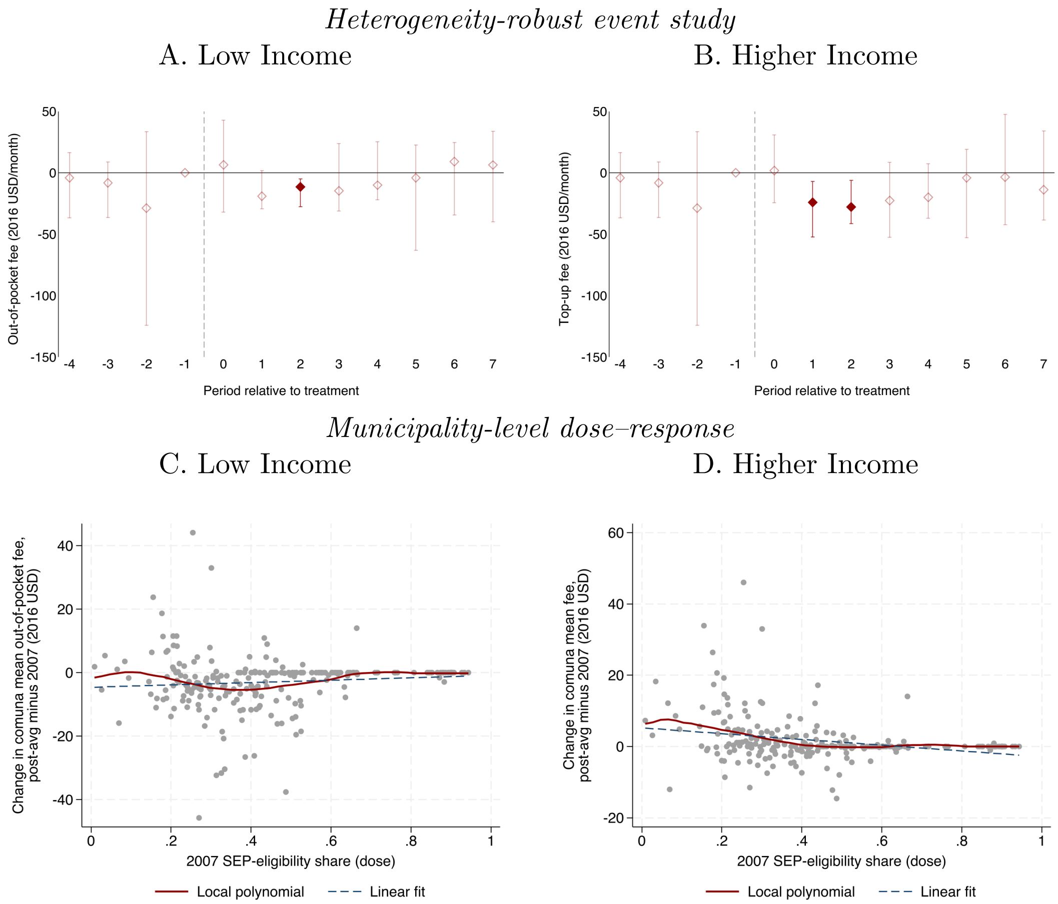
The diagnostics deliver a mixed verdict. First, their support test for a quasi-untreated group does not reject that zero lies in the support of the dose, although the mass of near-zero municipalities is thin, with only three municipalities below 0.05. Second, a Stute (1997) test cannot reject that pre-reform fee changes are mean-independent of the dose, with $p = 0.11$ for 2006–2007, $p = 0.39$ for 2005–2006, and $p = 0.82$ for 2004–2005. Third, linearity of the post-reform dose–response is rejected ($p < 0.01$ for the Stute (1997) test, and $p = 0.055$ for the heteroskedasticity-robust Yatchew (1997) test), and a decomposition following de Chaisemartin and D'Haultfœuille (2020) shows that 876 of the 2,116 implicit comparisons behind the static two-way fixed-effects coefficient receive negative weight, summing to -0.37. It is this last failure that motivates re-estimation.

We re-estimate the event study with their nonparametric estimator, which compares each municipality's fee evolution with the evolution extrapolated to a zero-dose municipality from the least-treated ones, on a balanced panel of 246 municipality-level mean fees. Panel B of Figure 5 reports the posted-fee results. Estimates are negative at almost every horizon and statistically significant in 2009–2010, with point estimates around $-\$20$ per unit of dose, roughly twice the two-way fixed-effects magnitudes. Confidence intervals are wide, reflecting the estimator's slower convergence rate and the modest number of municipalities. A parametric version in the spirit of de Chaisemartin et al. (2024) yields a dose-weighted average slope of $-\$20.8$ for the average post-period change, with a bootstrap 95% confidence interval of $[-30.7, -10.8]$.

The same estimator is far less informative about the fee menu facing eligible students, in panel A. Point estimates are negative at most horizons, but the bias-corrected confidence intervals contain zero everywhere except 2010, where the estimate is -11.5 with an interval of $[-27.7, -4.9]$. All three placebos are insignificant, and the parametric dose-weighted slope is $-\$8.9$, with a bootstrap interval of $[-18.8, 1.8]$. The positive eligible-student effect identified by the two-way fixed-effects specification is thus neither confirmed nor contradicted here: with only 246 municipalities, the estimator cannot resolve effects of the magnitude at issue, roughly \$3–6. We therefore read this exercise as corroborating the posted-fee decline, and as uninformative about the smaller eligible-student increase that the tercile split of Figure 4 does support.

Panels C and D recover the same pattern in raw municipality data, without any regression adjustment. The linear fit of the municipality-level change in mean fees on the dose has a slope of $-\$8.1$ (s.e. 1.8) for the posted fee in panel D, and of $+\$3.7$ (s.e. 2.0) for the eligible-student menu in panel C. Both are close to the corresponding two-way fixed-effects magnitudes. For the posted fee, the relationship is negative throughout and reasonably close to linear over the bulk of the dose distribution, so the rejection of exact linearity is driven by curvature at the extremes. Linearity is likewise rejected for the eligible-student menu ($p < 0.01$ and $p = 0.04$ for the two tests), while its pre-reform changes remain mean-independent of the dose ($p = 0.11$). We conclude that the direction and approximate magnitude of the posted-fee estimates are robust to treatment-effect heterogeneity, and that if anything the linear specification is conservative.

Figure 5: Heterogeneity-robust Estimates and the Dose–Response Relationship - Top-up Fees



Notes: Columns follow the convention of Figure 2: the left column's outcome is the fee an eligible (low-income) student would face at each school, and the right column's outcome is the posted (sticker) top-up fee paid by non-eligible (higher-income) students. Panels A and B: event-study estimates from the nonparametric estimator of de Chaisemartin et al. (2026a) for heterogeneous adoption designs, computed with the `did_had` command of de Chaisemartin et al. (2026a), whose bias-corrected confidence intervals follow Calonico et al. (2019), on a balanced panel of 246 municipality-level mean fees, 2004–2015. The horizontal axis uses the same convention as Figure 2: $\eta = 0$ is 2008 and the reference period is $\eta = -1$ (2007), shown as a hollow marker at zero; estimates at $\eta \geq 0$ are treatment effects and those at $\eta \leq -2$ are placebos. Vertical bars are bias-corrected 95% confidence intervals, which are asymmetric by construction; dark colored diamonds denote intervals that exclude zero, hollow diamonds intervals that do not (the 10% class of the other figures is not available for these bias-corrected intervals). Note that the vertical scale differs from that of Figure 2 and the other event-study figures, : the nonparametric estimator's confidence intervals are an order of magnitude wider, and a common scale would clip them. Panels C and D: change in the municipality mean fee (average of 2008–2015 minus 2007) against the 2007 eligibility share; the solid line is a local polynomial fit, the dashed line a linear fit.

Sensitivity to the pre-trend deviation. The $\eta = -2$ coefficient of -5.4 (s.e. 2.3) is statistically significant and non-negligible relative to the post-period effects, so we quantify how robust the conclusions are to violations of parallel trends using the partial-identification approach of Rambachan and Roth (2023). Their relative-magnitudes bounds allow post-period violations up to $\bar{M}$ times the largest pre-period deviation. For the posted fee, the effect in 2009, the first full post-reform year, remains significantly negative for $\bar{M} = 0.25$, with a robust confidence interval of $[-15.2, -2.0]$, and breaks down between $\bar{M} = 0.25$ and $\bar{M} = 0.5$. The average post-period effect, whose original interval is $[-11.5, -4.0]$, breaks down just below $\bar{M} = 0.25$, because bounds accumulate over the eight post-reform years. In other words, the negative posted-fee effects survive violations of parallel trends up to one-quarter to one-half the size of the observed $\eta = -2$ deviation, depending on the horizon. Table B.7 reports the bounds.

Two considerations weigh against reading the $\eta = -2$ deviation as evidence of a confounding trend. First, it is a single deviation, since the $\eta = -4$ and $\eta = -3$ coefficients are close to zero, and its shape—a dip in 2006 fully reversed by 2007—is not the beginning of a trend that the post-period estimates would extrapolate. Second, the SEP law was publicly debated during 2006–2007 before its enactment in January 2008, so fee-setting in those years may partly reflect anticipation, which would attenuate rather than inflate the post-period estimates.

Composition and school entry. The number of private-voucher schools grows over the sample period, so part of the estimated fee changes could in principle reflect entry of new schools rather than price changes by existing ones—a distinction that matters for interpreting the estimates as schools' *pricing responses*, though not for measuring the fee environment households face. Re-estimating equation (1) on the subset of schools that never enter or exit the sample yields, if anything, slightly larger effects (posted fees: $-\$5.8$ to $-\$10.3$ from 2009 onward; Table B.6 and panel B of Figure C.2), and the eligible-student fee menu results are likewise unchanged (panel A of the same figure). Composition is therefore not driving the price results.

Estimates by participation status. Finally, we estimate the event study separately for schools that ever participate in the program (2,852 schools) and schools that never do (1,038 schools), keeping in mind that these are selected groups: participation is a choice, and its composition varies systematically with the treatment intensity, so these splits do not identify the counterfactual objects in equations (4)–(5). The results (Table B.6) illustrate the difficulty: never-participating schools in more exposed markets show large but imprecise fee declines with non-parallel pre-trends, while ever-participating schools show small effects that turn positive at long horizons. The descriptive evidence in Section 2—participants' posted fees falling from \$12 to \$9.9 while non-participants' rose from \$61.6 to \$74.6—remains, in our view, the most transparent group-level description, and the aggregate event-study effects remain the objects with a causal interpretation.

4.5 Discussion

The market segmentation result we document is in line with Sánchez (2026), who estimates an empirical model of school choice and supply to study sorting under the targeted voucher policy (and counterfactual alternatives) in Chile. The estimated model implies that the policy induces market segmentation: lower-quality schools disproportionately opt into SEP and reduce fees, while higher-quality schools are more likely to opt out and raise fees to screen families that are averse to participation. A key component of the model is a "stigma" effect attached to participating in the program, which is substantially stronger for higher-income students. According to the estimated preferences, high-income families with highly educated mothers are willing to pay over \$18,000 to avoid enrolling in a participating school, whereas low-income families with mothers without a high school diploma are nearly indifferent between participating and non-participating schools, all else equal.

Theoretical models of school competition, such as Rothschild and White (1995), Epple and Romano (1998), and Epple et al. (2006), predict stratified education markets and rationalize segmentation through peer effects.[14] These models assume that learning externalities make students value the ability of their schoolmates, which in equilibrium generates sorting by ability across schools.

In the context of the targeted voucher reform in Chile, the peer effects mechanism would imply that participating schools, which are more likely to be attended by low-income students, would reduce the fees they charge to higher-income students as an effort to counterbalance the disutility experienced by higher-income students with the increase of low-income students in the school after the reform. Non-participating schools, which are more likely to be attended by higher-income students, would increase the fees they charge to all students because they are relatively more attractive to these students after the reform. This strategic complementarity between prices and peers has been previously documented in the education literature (Allende, 2019).

Taken together, the literature and our findings suggest that peer effects are likely to be an important driver of the market segmentation in prices that we document. At the same time, peer effects can be viewed as a microfoundation for the "stigma" effect in Sánchez (2026), because the disutility that higher-income students experience from attending participating schools is plausibly related to expected peer composition.

Section 5 presents a structural model of school choice that allows us to estimate families' preferences for peers and quantify the disutility from low-income peers. The estimated preference parameters can be used to assess the plausibility of the peer effects mechanism being a driver of the market segmentation result that we find.

5 A Structural Model of School Choice Prior to the Reform

The discussion above interprets the estimated fee responses through the interaction between prices and peer composition. This section develops that interpretation formally. We specify and estimate a structural model of demand for schools on pre-reform (2007) data, recover families' willingness to pay for peer composition and other school attributes, and then use the estimates to revisit the event-study results of Section 4.

5.1 The Model

We develop a structural model of demand for schools motivated by Chile's primary education system prior to the introduction of the targeted voucher reform. The model is static. There are several geographically separated education markets, indexed by $m \in \{1, \dots, M\}$. Each market is populated by households that live in different locations within the market and have one primary-school-aged child. Subject to its budget constraint, each household chooses among the schools available in the market.

Students have heterogeneous preferences over schools' fees, quality (i.e. a measure of how much the school increases students' test scores), the share of low-income enrollment, geographic proximity, and a set of schools' observable and unobservable (to the econometrician) characteristics. We capture preference

[14] An alternative mechanism for endogenous stratification is proposed by MacLeod and Urquiola (2015), who model students as valuing school reputation because employers infer productivity from school quality. This mechanism is less directly applicable in our setting because this stage of human capital formation precedes college entry, which in Chile is determined by admission exam scores that primarily reflect accumulated learning rather than a pure reputation signal.

heterogeneity with coefficients that vary with students' observed demographic characteristics. Formally, in each market m, student $i \in \{1, \ldots, I\}$ chooses the school $j \in \{1, \ldots, J\}$ that maximizes her utility. We specify the student's conditional indirect utility as

$$U_{ijm} = \beta_{1i} p_{jm} + \beta_{2i} d_{ijm} + \beta_{3i}' X_{jm} + \beta_{4i}^{\zeta} q_{jm} + \beta_{5i} \hat{z}_{jm}^{y^L} + \xi_{jm}^{\zeta} + \varepsilon_{ijm}, \tag{6}$$

where the superscript $\zeta \in \{L, H\}$ refers to whether the student is low-income or high-income.[15] Thus, p_{jm} is school j's top-up fee, d_{ijm} is the distance from student i's home to school j, X_{jm} is a vector of school j's observable characteristics (full-day shift, secular status, rural status, and public status), q_{jm} is school j's quality measure, $\hat{z}_{jm}^{y^L}$ is the expected share of low-income enrollment at school j, ξ_{jm}^{ζ} is the mean valuation that students of type ζ attach to school j's characteristics that are unobservable to the econometrician, and ε_{ijm} is an i.i.d. preference shock. Note that ξ_{jm}^{ζ} is a school-specific unobservable (a "structural error"), not a parameter to be estimated.

Preferences vary across students in two ways. First, the quality coefficient and the unobservable-valuation term are specific to the student's income type: $\beta_{4i}^{\zeta} = D_i \beta_4^L + (1 - D_i)\beta_4^H$ and $\xi_{jm}^{\zeta} = D_i \xi_{jm}^L + (1 - D_i)\xi_{jm}^H$, where $D_i = \mathbb{1}[i \text{ is low-income}]$. Second, the coefficients on fees, distance, observables, and peer composition vary with income type and with observed demographics: for $k \in \{1, 2, 3, 5\}$, $\beta_{ki} = D_i \beta_{ki}^L + (1 - D_i)\beta_{ki}^H$, where $\beta_{ki}^{\zeta} = \beta_k^{\zeta} + \sum_r z_{ir} \beta_{kr}^{\zeta}$ and z_{ir} denotes student i's demographic characteristic r (in practice, mother's education dummies).

Let $V_{ijm} = \beta_{1i} p_{jm} + \beta_{2i} d_{ijm} + \beta_{3i}' X_{jm} + \beta_{4i}^{\zeta} q_{jm} + \beta_{5i} \hat{z}_{jm}^{y^L} + \xi_{jm}^{\zeta}$, so that $U_{ijm} = V_{ijm} + \varepsilon_{ijm}$. Assuming $\varepsilon_{ijm} \sim$ Type I Extreme Value, the probability that student i chooses school j is logistic:

$$P_{ijm} = \frac{e^{V_{ijm}}}{\sum_k e^{V_{ikm}}}.$$

5.2 Estimation and Identification

We work with non-overlapping geographic markets, closely following Sánchez (2026) and Neilson (2025). For estimation, we proceed in three steps. First, we estimate school popularity fixed effects and the parameters of heterogeneity in students' preferences. Then, given that school quality is not directly observed in the data, we obtain a school quality measure by estimating test scores equations using a selection-on-observables approach, very similar to the work in Allende (2019), Allende et al. (2019), and Neilson (2025). Finally, we link school quality and other characteristics to school popularity estimates to finalize preferences for schools estimation.

Preferences for Schools. We use Maximum Likelihood to estimate preferences for proximity, taste heterogeneity, and mean utilities or school popularity. Heterogeneity in preferences is captured by a set of students' observable demographic characteristics, which in practice are mother's educational level dummies. Mean utilities vary at the student's socioeconomic status (low/high-income), and absorb the remaining preference components from the indirect utility function,

$$\delta_{jm}^{\zeta} = \beta_1^{\zeta} p_{jm} + \beta_3^{\zeta\prime} X_{jm}^{\beta} + \beta_4^{\zeta} q_{jm} + \beta_5^{\zeta} \hat{z}_{jm}^{y^L} + \xi_{jm}^{\zeta}, \tag{7}$$

[15] The low/high-income classification follows the eligibility criteria for the targeted voucher program and is heavily determined by household income.

where $X^{\beta}_{jm} \subseteq X_{jm}$ collects the school observables whose mean valuations are estimated at the income-group level (full-day shift, secular, rural, and public status).

The corresponding log-likelihood function is:

$$LL(\beta) = \sum_i \sum_j e_{ij} \ln \left(\frac{\exp\left((\beta^{\zeta}_{1i} - \beta^{\zeta}_{1}) p_{jm} + \beta_{2i} d_{ijm} + (\beta^{\zeta}_{3i} - \beta^{\zeta}_{3}) X^{\beta}_{jm} + (\beta^{\zeta}_{5i} - \beta^{\zeta}_{5}) \hat{z}^{y^L}_{jm} + \delta^{\zeta}_{jm} \right)}{\sum_k \exp\left((\beta^{\zeta}_{1i} - \beta^{\zeta}_{1}) p_{km} + \beta_{2i} d_{ikm} + (\beta^{\zeta}_{3i} - \beta^{\zeta}_{3}) X^{\beta}_{km} + (\beta^{\zeta}_{5i} - \beta^{\zeta}_{5}) \hat{z}^{y^L}_{km} + \delta^{\zeta}_{km} \right)} \right), \tag{8}$$

where e_{ij} is the choice indicator, i.e. $e_{ij} = 1$ indicates that student i attends school j.

School Quality. To estimate the measure of school quality, we follow Allende (2019), Allende et al. (2019), and Neilson (2025), and estimate the parameters that enter an auxiliary test scores equation using an offline selection-on-observables linear model. Note that school value-added measures, as defined by the value-added literature (Rockoff, 2004; Rivkin et al., 2005; Kane and Staiger, 2008; Chetty et al., 2014a,b; Bau, 2022), are not possible to estimate in this context, because prior test scores are not observed. Instead, our school quality estimates measure the accumulated quality value of a school for grades 1st–4th.

In practice, we estimate by OLS the following regression:

$$Y_{ij} \quad = \quad W_i \gamma + \sum_j q_j e_{ij} + \epsilon_i, \tag{9}$$

where we include a rich set of observables in W_i to capture as much variation as possible and mitigate inconsistency from selection on observables. The covariates include gender, low-income status, mother's education, father's education, household income, having attended pre-K, having attended kindergarten, computer availability at home, internet availability at home, indigenous status, and number of books at home. We use the student-level average of 4th grade national standardized scores in verbal and math as the outcome variable (the SIMCE tests; see Appendix A). This set of observables is in line with Allende (2019), Allende et al. (2019), and Neilson (2025). Moreover, Allende et al. (2019) show that school quality estimates based on this procedure and a similar set of covariates yield results comparable to those obtained when adding prior test scores as controls; they also provide additional experimental evidence supporting the strategy. We estimate the test score equations market by market and obtain $\{\hat{q}_j\}_{j=1}^{J}$ as school quality estimates.

Linking Mean Utilities to School Quality and Other Characteristics. We use the estimated $\hat{\delta}^{\zeta}_{jm}$ terms from the first step, as well as the $\hat{q}_{jm}$ estimated school quality from the test scores regressions to estimate the remaining mean preference parameters in a linear regression of the form:

$$\hat{\delta}^{\zeta}_{jm} \quad = \quad \beta^{\zeta}_1 p_{jm} + \beta^{\zeta\prime}_3 X^{\beta}_{jm} + \beta^{\zeta}_4 \hat{q}_{jm} + \beta^{\zeta}_5 \hat{z}^{y^L}_{jm} + \xi^{\zeta}_{jm}. \tag{10}$$

As is usual in demand models, we assume that X^{β}_{jm} is uncorrelated with ξ^{ζ}_{jm}. However, we allow p_{jm} and $\hat{z}^{y^L}_{jm}$ to be endogenous—i.e. correlated with ξ^{ζ}_{jm}—and estimate equation (10) by 2SLS using the instruments described below.

Identification and Instruments. The term ξ^{ζ}_{jm} represents type-ζ families' preferences for school j's unobserved characteristics, such as principal's charisma or managerial style, educational curriculum, etc., that may be correlated with school's choices for top-up fees and with its student body composition. An artificial example is a school with a mission to serve vulnerable families. Such school may be willing to develop a culture and an educational environment that are particularly welcoming to low-income students, which may intensify the school's propensity to enroll vulnerable students and to set low fees. Thus, consistent estimation of the linear parameters in equation (10) necessitates instruments.

Our choice of instruments for the two endogenous variables in equation (10)—top-up fees and the school's expected share of low-income enrollment—follows related studies (Allende, 2019; Allende et al., 2019; Sánchez, 2026; Neilson, 2025). For prices, we use the average per student universal voucher received by the school, other per student non-voucher subsidies (e.g. teacher incentives, rurality bonus), and the local share of private competitors. For schools' student body composition, we use the local share of students with a highly educated mother. For the local shares of private competitors and students with a highly educated mother, we construct distance-weighted versions of these variables, where the weights are the inverse of the distance to the school. This way, we capture the local market structure and demographic composition that are more relevant for each school.

Note that instruments related to market structure and demographic characteristics are relevant in a context in which students do not travel long distances to attend school. Taking residential sorting and market structure as given, a school located in a neighborhood with a high concentration of high-income families and low competition from other private schools should enroll a higher share of high-income students than a school located in a neighborhood with a lower concentration of high-income families and more private-school competition. The exclusion restrictions require that, conditional on the included school characteristics, subsidy receipts and the local demographic and competitive environment shift fees and expected peer composition without directly entering families' valuations of the school; this is plausible for the subsidy instruments, whose variation reflects administrative formulas, but it is admittedly stronger for the local-composition instruments, which could correlate with unobserved neighborhood amenities. We therefore read the estimates below as evidence on the sign and rough magnitude of preferences rather than as definitive structural parameters, and we report overidentification tests that partially speak to instrument validity.

5.3 Results

5.3.1 School Quality

We estimate school quality under a selection-on-observables assumption (Allende, 2019; Allende et al., 2019; Sánchez, 2026; Neilson, 2025), running test score regressions on a large set of observable characteristics and school indicators, market by market. We use the student-level average of 4th grade national standardized scores in verbal and math. The covariates include gender, low-income status, mother's education, father's education, household income, having attended pre-K, having attended kindergarten, computer availability at home, internet availability at home, indigenous status, and number of books at home. For brevity, we do not present each market's estimates, but they are available upon request.

5.3.2 Preferences

First Stage. As stated in Section 5.2, we estimate the first-stage regressions for the endogenous variables (top-up fees and the share of low-income students in the school) using instrumental variables and school characteristics.

Table 4 presents the first-stage results. Local competition (measured by the pre-reform local share of private competitors) is associated with lower fees (-0.041) and a higher share of low-income students (0.010). The distance-weighted local share of mothers with more than high school education is positively associated with top-up fees (0.265) and negatively associated with the share of low-income students (-0.158), consistent with sorting patterns in which more educated families select into higher-fee schools with fewer low-income peers. The average per-student universal voucher is negatively associated with fees (-0.027) but positively associated with the share of low-income students (0.118), suggesting that schools receiving larger universal voucher subsidies attract more low-income students while keeping fees relatively low. Non-voucher subsidies have virtually no association with fees (coefficient near zero) but a positive association with student composition (0.039), suggesting that schools receiving more subsidies attract more low-income students. Public schools charge significantly lower fees (-0.203) and serve more low-income students (0.073), consistent with their mission. Rural schools charge lower fees (-0.058) and serve more low-income populations (0.116). The quality indicator is positively associated with fees (0.069) and negatively associated with the low-income student share (-0.032), consistent with a quality–price gradient and cream-skimming.

Because equation (10) has two endogenous regressors, equation-by-equation first-stage F statistics (71.44 for fees and 87.80 for the low-income share) do not by themselves establish joint instrument relevance (Sanderson and Windmeijer, 2016). We therefore also report the Kleibergen–Paap rank statistics for the system (Kleibergen and Paap, 2006): the underidentification LM statistic is 109.4 ($p < 0.001$), and the joint weak-identification Wald F statistic is 28.3, above conventional rule-of-thumb thresholds. The instruments are jointly relevant, though the joint statistic is naturally smaller than the equation-by-equation Fs.

Table 4: First Stage Estimates for 2007

	top-up fees		share of low income students	
	coef.	std. err.	coef.	std. err.
local share of private competitors (dist.-weighted)	-0.041	0.014	0.010	0.010
local share of mothers w/ more than high school (dist.-weighted)	0.265	0.016	-0.158	0.012
avg. per student universal voucher	-0.027	0.038	0.118	0.028
avg. per student non-voucher subsidies	-0.000	0.005	0.039	0.003
full day shift	0.066	0.011	-0.019	0.008
secular	0.001	0.007	-0.022	0.005
rural	-0.058	0.009	0.116	0.006
public	-0.203	0.009	0.073	0.007
quality	0.069	0.006	-0.032	0.004
intercept	0.193	0.026	0.108	0.019
F excluded IV (per equation)	71.44		87.80	
Kleibergen–Paap rk Wald F (joint)	28.27			
Kleibergen–Paap rk LM (underid.), p-value	109.40, <0.001			
no. of schools	3,080			

Notes: First-stage regressions of the 2SLS estimation of the mean preference parameters in equation (10). The first two rows are the excluded instruments constructed as distance-weighted local averages (weights equal to the inverse of the distance to the school); the rows labeled "local share of mothers w/ more than high school" and "local share of private competitors" are *neighborhood-level* shares, not the school's own student composition. Unadjusted standard errors. The per-equation F statistics test the excluded instruments in each first stage separately; with two endogenous regressors, joint relevance is assessed with the Kleibergen–Paap rank statistics reported below them (Kleibergen and Paap, 2006; Sanderson and Windmeijer, 2016). We deflated 2007 fee amounts to 2015 values. Fee amounts are in real \$1,000 (2015) and were converted from CLP to USD using the exchange rate as of March 1, 2015 (648.88 CLP/USD).

Preference parameters. We present estimates of families' preferences in Table 5, separately for low- and high-income groups. Within each income group, preferences for most school characteristics vary by mother's education, where the omitted category is "more than high school." Low-income families are substantially more price sensitive than higher-income families (coefficients of -11.195 and -6.668, respectively). There is also meaningful heterogeneity within each income group: families with less educated mothers are more responsive to price changes than families with more educated mothers. Both low- and high-income families exhibit a strong negative preference for schools with higher shares of low-income students (coefficients of -15.524 and -19.406, respectively), with important variation by mother's education. Low-income families display a negative valuation of school quality (coefficient -0.381), whereas high-income families value school quality positively (coefficient 0.610); we discuss this anomalous estimate for the low-income group below. Full-day-shift schools are positively valued by low-income families (coefficient 1.539) but less so by high-income families. Secular and public schools are generally disfavored by both income groups. Both groups prefer schools that are closer to their homes, with stronger distance sensitivity among low-income families (distance coefficients of -0.114 and -0.071, respectively). Overall, the estimates highlight substantial heterogeneity in school choice preferences across socioeconomic groups and mother's education.

The overidentification tests deserve comment. For the high-income equation, the Hansen J statistic is

4.03 ($p = 0.13$), so the overidentifying restrictions are not rejected. For the low-income equation, the J statistic is 16.73 ($p < 0.001$), indicating that at least some instruments are invalid for that equation, or that the linear specification is misspecified for low-income families. This rejection, together with the negative quality coefficient for the low-income group, suggests that the low-income estimates should be read with more caution than the high-income ones. Importantly, the result we rely on most heavily—the negative and economically large valuation of low-income peer shares—is present, with similar relative magnitudes, in *both* equations, including the high-income equation where the overidentifying restrictions are comfortably not rejected.

Table 5: Estimates from Demand Model for 2007

	Low-Income		High-Income	
	Coef.	Std. Err.	Coef.	Std. Err.
Fees	-11.195	1.383	-6.668	1.046
Fees × Mother's education: less than high school	-4.713	0.259	-3.443	0.079
Fees × Mother's education: high school	-2.104	0.223	-1.226	0.047
Fees × Mother's education: missing	-3.573	0.381	-0.990	0.092
Share low income students	-15.524	1.707	-19.406	1.292
Share low income students × Mother's education: less than high school	1.568	0.262	5.102	0.185
Share low income students × Mother's education: high school	-1.284	0.268	0.688	0.167
Share low income students × Mother's education: missing	2.391	0.299	5.920	0.242
Quality	-0.381	0.115	0.610	0.087
Full day shift	1.539	0.174	0.351	0.132
Full day shift × Mother's education: less than high school	-0.541	0.071	0.035	0.035
Full day shift × Mother's education: high school	-0.515	0.072	0.117	0.029
Full day shift × Mother's education: missing	-0.621	0.086	-0.164	0.051
Secular	-0.316	0.110	-0.166	0.084
Secular × Mother's education: less than high school	-0.137	0.054	-0.033	0.028
Secular × Mother's education: high school	-0.096	0.055	-0.056	0.023
Secular × Mother's education: missing	-0.190	0.063	0.061	0.039
Rural	-0.023	0.228	-0.535	0.172
Rural × Mother's education: less than high school	0.313	0.079	0.131	0.054
Rural × Mother's education: high school	0.082	0.082	-0.172	0.051
Rural × Mother's education: missing	0.264	0.089	0.049	0.069
Public	-0.727	0.237	-0.354	0.179
Public × Mother's education: less than high school	-0.009	0.064	-0.025	0.037
Public × Mother's education: high school	-0.044	0.065	0.007	0.033
Public × Mother's education: missing	-0.006	0.075	-0.017	0.050
Distance (interact)	-0.114	0.002	-0.071	0.002
Distance (non-missing)	0.360	0.067	0.263	0.041
No. of schools	3,080			
No. of students	100,714			

Notes: Results from maximum likelihood estimation of distance and preference heterogeneity by mother's education, and from 2SLS estimation of mean preference parameters (equation (10)). Omitted mother's level of education category is "more than high school". The distance from home to school variables are the interaction of a dummy for non-missing and the corresponding distance variables. The non-missing dummy variable is also added in the model. Distance is measured in 100 meters. Standard errors for the mean coefficients are unadjusted 2SLS standard errors; standard errors for the interaction terms come from the maximum-likelihood step. Neither set of standard errors accounts for the estimation error in the two generated regressors (estimated quality $\hat{q}_{jm}$ and expected low-income share $\hat{z}^{y^L}_{jm}$) or in the estimated dependent variable $\hat{\delta}^{\zeta}_{jm}$. We deflated 2007 fee amounts to 2015 values. Fee amounts are in real \$1,000 for the year 2015, and were transformed from CLP to USD according to the exchange rate as of March 1, 2015 (648.88 CLP/USD).

Willingness to pay for school attributes. To complement the demand estimates, we compute families' willingness to pay (WTP) for school attributes by dividing the relevant coefficients by the negative of the price coefficient, for each income group and mother's education level (Berry et al., 1995; Petrin, 2002). Table 6 presents these results for the peer composition of schools. The magnitudes of the estimated parameters imply that families would need to be compensated by about \$877–\$2,910 to attend a school where all students are socioeconomically disadvantaged, relative to a school with no low-income peers. We also observe a clear gradient by family background: families with higher socioeconomic status (proxied by mother's education) require a larger compensation to attend schools with more low-income peers. Even low-income families with mothers without a high school diploma require a substantial compensation (about \$877) to attend a school with exclusively low-income peers.

Table 6: Willingness to Pay for Low-Income Peers

	low-income	high-income
mother's education: less than high school	-877	$-1,415$
	(74)	(100)
mother's education: high school	$-1,264$	$-2,371$
	(95)	(220)
mother's education: more than high school	$-1,387$	$-2,910$
	(114)	(335)
mother's education: missing	-889	$-1,761$
	(82)	(162)

Notes: Willingness-to-pay estimates are calculated by dividing the estimated preference parameter for the share of low-income students by the *negative* of the estimated price coefficient, for each income group and mother's education level, in the school choice model described in Section 5. Because fees enter the model in thousands of dollars, the resulting ratios are expressed here in real 2015 US dollars (CLP converted at the exchange rate of March 1, 2015, 648.88 CLP/USD). Delta-method standard errors are in parentheses. For the "more than high school" row (the omitted education category), the ratio involves only the 2SLS mean coefficients and the standard error uses their full estimated covariance; for the remaining rows, the education-specific taste shifters are estimated in the maximum-likelihood step, and the standard errors treat those estimates as uncorrelated with the 2SLS coefficients and with each other.

5.4 Interpreting the Event-Study Results Through the Model

The demand estimates provide the microfoundation for the market-segmentation reading of Section 4. Three implications follow directly from the estimated preferences.

First, the estimated distaste for low-income peers is exactly the force that makes fee cuts for higher-income families an equilibrium response to program participation. A school that joins the program commits to enrolling eligible students at zero fees, which raises its expected share of low-income enrollment. Given the estimated preferences, this composition change reduces the school's attractiveness to higher-income families—and more so for the most educated among them, whose distaste is largest. To retain those families, a participating school must compensate them through the one instrument it retains: the posted fee it charges non-eligible students. The negative posted-fee effects in panel B of Figure 2, their concentration in competitive markets, and their concentration among ex-ante higher-fee schools (Section 4.4) are all consistent with this compensating-differential logic.

Second, the same preferences rationalize the behavior of schools that opt out. As participating competitors absorb eligible students, a non-participating school's expected peer composition shifts toward higher-income students, which the estimated preferences value positively. Its demand among higher-income families therefore becomes stronger and relatively less price-sensitive (the estimated price coefficients are smallest, in absolute value, for the most educated high-income families), allowing it to raise fees—the pattern that drives the rise in the average fee attached to eligible students' school menus in panel A of Figure 2.

Third, the socioeconomic gradient in the willingness to pay to avoid low-income peers (from $877 for the least-educated low-income families to $2,910 for the most-educated high-income families) implies that the reform's peer-composition shock is priced most heavily at the top of the fee distribution, which is where we find the largest posted-fee declines among fee-charging schools. The gradient also explains why segmentation takes the form of *vertical* differentiation—participating schools moving down-market in fees, non-participants moving up—rather than uniform price adjustments.

Two qualifications are in order. The model is estimated on pre-reform data, so it speaks to the incentives in place when schools made their initial participation and pricing decisions, not to post-reform preference changes. And, as discussed above, the estimates are supporting evidence for a necessary condition of the peer-composition mechanism—families must dislike low-income peer shares for the mechanism to operate—rather than a full equilibrium quantification, which would require modeling schools' joint participation and pricing decisions and is beyond the scope of this paper (see Sánchez, 2026).

6 Conclusions

This paper studies how a large targeted voucher reform affects the equilibrium organization of local education markets. We focus on Chile's *Subvención Escolar Preferencial (SEP)*, which increased per-student funding for economically disadvantaged students while restricting schools' ability to charge top-up fees to eligible students. Using administrative data covering the universe of schools and students over 2004–2015, we document that the reform generated sizable changes in the distribution of prices faced by families and contributed to segmentation of private-voucher markets along socioeconomic lines.

Our empirical analysis shows that private-voucher schools responded to the targeted voucher reform with tuition-setting decisions that differed sharply between participants and non-participants. Participating schools charged zero fees to eligible low-income students, as mandated by the program, while simultaneously lowering the fees faced by non-eligible students. By contrast, schools that opted out of the program increased posted fees. In the aggregate, these responses imply that low-income households experienced an increase in the average fee faced in the market, while higher-income households experienced a decrease in the average fee. The resulting pattern is consistent with the idea that targeted subsidies can, through schools' strategic responses, reshape the effective price schedule faced by different groups of families and lead to re-sorting across schools.

We interpret these findings through the lens of school competition in markets where households value peer composition. When higher-income families derive disutility from attending schools with a larger share of low-income peers, schools have incentives to adjust prices in ways that reinforce differentiation: schools serving (or expecting to serve) more disadvantaged students lower the tuition faced by higher-income students to offset peer-related disutility, while schools specializing in higher-income students can raise fees as their demand becomes relatively less price elastic. The school choice evidence in Section 5 supports a key prerequisite for this mechanism: we estimate a statistically significant disutility from low-income peers across family groups, with willingness-to-pay magnitudes that rise with socioeconomic status.

From a policy perspective, our results underscore that evaluating targeted voucher programs requires accounting for equilibrium supply-side responses. Chile's targeted voucher design expanded resources for disadvantaged students and altered schools' incentives, but it also induced price and composition adjustments that can affect who gains access to which schools and at what cost. These forces are likely to matter for welfare and for the distributional consequences of vouchers, especially when take-up is voluntary and schools can choose whether to segment.

Several limitations point to directions for future work. First, while we provide evidence on tuition responses and estimate preference parameters consistent with peer effects, quantifying the welfare consequences of the reform (including winners and losers across income groups) requires a full equilibrium model that jointly accounts for schools' pricing and participation decisions and families' sorting. Second, understanding longer-run dynamics—entry and exit, investment in quality, and the evolution of schools' reputation—is important in a setting where accountability and monitoring interact with targeted funding. Finally, similar targeted programs are being considered and implemented in other countries (Epple et al., 2017; Abdulkadiroglu et al., 2018); applying the framework developed here to other institutional contexts would help isolate which features of program design are most effective at increasing equity without amplifying market segmentation.

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A Data

We combine various administrative data sets for Chilean students and schools for the years 2004–2015. Specifically, we use:

- Registry of students, 2004–2015.
  These data provide information on students' gender, date of birth, age, municipality of residence, type and level of education, grade, class, grade repetition status, special education status, and various characteristics of the school of attendance, such as municipality, type of administration (public, private-voucher, private-non-voucher), single/double shift schedule, and urban status.
- Registry of students' academic performance, 2004–2015.
  These data provide information on students' gender, date of birth, municipality of residence, type and level of education, grade, class, GPA, average class attendance, and various characteristics of the school of attendance, such as municipality, type of administration, and urban status.
- Registry of schools, 2004–2015.
  These data provide information on schools' municipality, type of administration, urban status, address, and type and level of education offered.
- Registry of schools' summary of enrollment, 2004-2015.
  These data provide information on schools' municipality, type of administration, urban status, male enrollment by education type and level, female enrollment by education type and level, total enrollment by education type and level, total enrollment, number of single-grade classes by education type and level, total number of single-grade classes, number of multigrade classes by education type and level, and total number of multigrade classes.
- Registry of schools that participate in the targeted voucher program, 2008–2015.
  These data provide information on the characteristics of schools that participate in the targeted voucher program. Information on schools' municipality, type of administration, urban status, targeted voucher classification, number of low income students that are eligible to benefit from the targeted voucher subsidy, and number of targeted voucher beneficiary students is also available.
- Registry of students that are eligible to participate in the targeted voucher program, 2008–2015.
  These data provide information on the characteristics of low income students that are eligible to be beneficiaries of the targeted voucher program. They provide information on students' gender, date of birth, targeted voucher participation status, level of education, grade, single/double shift schedule, and on the type of administration, urban status, and targeted voucher category of the school attended by the student. Because this registry begins in 2008, the pre-reform (2007) treatment-intensity measure used in Section 4 is constructed by linking the 2008 registry to the 2007 student-level enrollment registry through the national student identifier: for each municipality, the intensity equals the share of students enrolled in grades 1–4 in 2007, by municipality of residence, who appear as eligible in the 2008 registry. Students who left the school system between 2007 and 2008 are therefore coded as non-eligible, and municipalities created after 2008 lack the measure and are excluded from the estimation sample.
- Private-voucher schools' top-up fees, 2004–2015.
  These databases list all private-voucher schools that charge strictly positive top-up fees and indicate

the amount of such fees. Private-voucher schools that do not appear in a given year are coded as charging zero fees in that year.

- National standardized exams (SIMCE) for 4th graders, student-level, 2005–2015.
  These data provide information on students' test scores for three different subjects: verbal, mathematics, and either social sciences or natural sciences, depending on the year. They also provide information on students' gender and grade. These scores are the outcome variable in the school quality estimation of Section 5.

- 4th grade SIMCE's questionnaire to parents and tutors, 2005–2015.
  These data consist of responses to a survey that parents and tutors answer during the days when the national standardized tests are taken. The survey is voluntary, though more than 90% of parents choose to respond it every year. It provides information on students' household size, house amenities, and time use, total number of books available in the household, household's total monthly income, parents and tutors' time use, education, indigenous identification, occupation, health insurance, participation in social programs, reasons for the choice of the school, beliefs on the student's future educational attainment, satisfaction with the school, knowledge of school's average performance in standardized tests, total monthly expenses related to the student's education other than tuition, and school's admission criteria, tuition, and fees. These variables are the household covariates used in the school quality estimation and demand model of Section 5.

B Additional Tables

Table B.1: Effects of the Targeted Voucher Program on Private-voucher School Top-up Fees

	low income	higher income
	(1)	(2)
-4	0.941	1.148
	(1.700)	(1.724)
-3	-1.040	-1.039
	(1.773)	(1.776)
-2	-5.368**	-5.384**
	(2.269)	(2.270)
0	5.071***	-2.352
	(1.693)	(1.630)
1	2.605	-7.909***
	(2.056)	(2.104)
2	3.216*	-7.815***
	(1.796)	(1.866)
3	5.808***	-5.871***
	(2.077)	(1.987)
4	4.876**	-9.798***
	(2.100)	(1.989)
5	2.493	-11.158***
	(2.429)	(2.434)
6	5.020**	-8.227***
	(2.505)	(2.634)
7	4.722**	-8.734***
	(2.336)	(2.480)
observations	38,271	38,271
R^2	0.218	0.272

Notes: Event-study coefficients δ_η from equation (1); the omitted period is $\eta = -1$ (2007). Column (1)'s outcome is the fee an eligible (low-income) student would face at each school (zero at participating schools from 2008 onward, the posted fee otherwise); column (2)'s outcome is the posted (sticker) top-up fee, which higher-income students pay at every school. Fee values are monthly and real, converted from Chilean pesos to US dollars according to the exchange rate of Ch$686.52 per US dollar, as of May 16, 2016. Standard errors, clustered at the municipality level, are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The estimation sample comprises the 38,722 private-voucher school-year observations of Table 3 minus 451 observations in municipalities without a 2007 intensity measure.

Table B.2: Unweighted and Enrollment-weighted Event Studies

	posted (sticker) fee		fee faced by eligible students	
	unweighted	weighted	unweighted	weighted
	(1)	(2)	(3)	(4)
-4	1.148	-1.306	0.941	-1.229
	(1.724)	(3.794)	(1.700)	(3.764)
-3	-1.039	-2.458	-1.040	-2.385
	(1.776)	(3.932)	(1.773)	(3.924)
-2	-5.384**	-12.235**	-5.368**	-12.216**
	(2.270)	(6.105)	(2.269)	(6.100)
0	-2.352	-4.543	5.071***	0.085
	(1.630)	(3.679)	(1.693)	(3.796)
1	-7.909***	-10.480***	2.605	-1.190
	(2.104)	(3.921)	(2.056)	(4.159)
2	-7.815***	-12.856***	3.216*	-2.015
	(1.866)	(3.788)	(1.796)	(3.938)
3	-5.871***	-9.441**	5.808***	2.667
	(1.987)	(3.998)	(2.077)	(3.644)
4	-9.798***	-13.459***	4.876**	1.030
	(1.989)	(4.115)	(2.100)	(3.971)
5	-11.158***	-15.945***	2.493	-2.404
	(2.434)	(4.564)	(2.429)	(4.210)
6	-8.227***	-13.228***	5.020**	-0.802
	(2.634)	(4.695)	(2.505)	(4.211)
7	-8.734***	-14.012***	4.722**	-0.616
	(2.480)	(4.927)	(2.336)	(4.556)
observations	38,271	35,028	38,271	35,028

Notes: Event-study coefficients δ_η from equation (1); the omitted period is $\eta = -1$ (2007); the row labels denote event time. Columns (2) and (4) weight schools by their 2007 basic-education enrollment (analytic weights; schools not observed in 2007 are excluded). Standard errors, clustered at the municipality level, are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Fees are monthly, in 2016 US dollars.

Table B.3: Event Study by Terciles of Treatment Intensity

	higher income (posted fee)		low income (eligible-student fee)	
	middle tercile	top tercile	middle tercile	top tercile
	(1)	(2)	(3)	(4)
-4	-0.470	0.736	-0.554	0.680
	(1.034)	(0.845)	(1.028)	(0.835)
-3	-1.687**	-0.485	-1.675**	-0.532
	(0.800)	(0.782)	(0.803)	(0.783)
-2	-2.373***	-2.157***	-2.381***	-2.181***
	(0.842)	(0.815)	(0.842)	(0.815)
0	-1.017	-1.070	0.059	3.011***
	(0.755)	(0.694)	(0.956)	(0.769)
1	-2.817***	-3.421***	-1.389	1.982**
	(0.966)	(0.894)	(1.127)	(0.938)
2	-2.584***	-3.031***	-0.247	2.506***
	(0.889)	(0.777)	(1.009)	(0.814)
3	-2.566***	-2.364***	-0.216	3.483***
	(0.963)	(0.835)	(1.154)	(0.942)
4	-4.249***	-4.795***	-0.442	2.663**
	(1.093)	(0.895)	(1.291)	(1.045)
5	-5.038***	-5.291***	-1.664	1.677
	(1.238)	(1.094)	(1.356)	(1.119)
6	-3.935***	-3.811***	-0.763	2.872**
	(1.187)	(1.103)	(1.356)	(1.116)
7	-3.759***	-4.045***	-0.636	2.667**
	(1.188)	(1.033)	(1.315)	(1.050)
observations	38,271			

Notes: Coefficients on year dummies interacted with tercile-of-intensity indicators, from a single regression with municipality and year fixed effects; the bottom tercile is the comparison group and $\eta = -1$ (2007) is the omitted period. Columns (1)–(2) correspond to panel B of Figure 4 and columns (3)–(4) to panel A. Tercile cutoffs of the 2007 municipal eligibility share, computed over municipalities: 0.33 and 0.51. Municipalities without a 2007 eligibility measure cannot be assigned a tercile and are excluded, so the sample matches the baseline event study. Fees are monthly, in 2016 US dollars. Standard errors, clustered at the municipality level, are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.4: Heterogeneity: Competition and Pre-reform Fee Splits

	competition (# pv schools, 2007)		own 2007 fee	
	below median	above median	zero	positive
	(1)	(2)	(3)	(4)
-4	0.335	1.144	-10.079***	-5.637
	(2.560)	(1.911)	(2.874)	(4.381)
-3	6.488*	-2.469	-9.790***	-1.164
	(3.833)	(1.860)	(3.164)	(3.426)
-2	0.662	-6.281**	-13.882***	-5.300
	(1.032)	(2.670)	(4.651)	(4.149)
0	-1.708*	-2.533	-9.809***	1.006
	(0.899)	(1.859)	(3.062)	(2.609)
1	-2.837*	-8.694***	-12.105***	-7.070***
	(1.669)	(2.444)	(3.420)	(2.547)
2	-1.498	-8.728***	-11.767***	-13.024**
	(1.323)	(2.175)	(3.488)	(5.038)
3	-2.544	-6.671***	-11.469***	-9.756**
	(3.084)	(2.294)	(3.306)	(4.350)
4	-2.670	-10.504***	-11.388***	-9.954*
	(4.373)	(2.256)	(3.527)	(5.477)
5	-15.975	-11.015***	-12.094***	-13.072**
	(13.459)	(2.596)	(3.570)	(5.086)
6	2.659	-9.715***	-11.958***	-11.324*
	(4.546)	(2.988)	(3.351)	(6.312)
7	1.385	-9.639***	-11.803***	-14.041**
	(4.222)	(2.834)	(3.563)	(6.545)
observations	4,844	33,427	17,762	17,266

Notes: Baseline event study (posted fee) estimated on the indicated subsamples. Columns (1)–(2): schools in municipalities with a below/above-median number of private-voucher schools in 2007 (median = 9 among sample municipalities). Columns (3)–(4): schools whose own 2007 fee was zero versus positive; because unlisted schools are coded as charging zero, column (3) mixes free and unlisted schools, and it displays non-parallel pre-trends—see the discussion in Section 4.4. Among the positive-fee schools of column (4), splitting further at the median fee yields substantially larger declines for above-median-fee schools ($-\$10.6$ to $-\$23.0$ from 2009 onward) than for below-median ones. Standard errors, clustered at the municipality level, are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.5: Heterogeneity, Fee Faced by Eligible Students: Competition and Pre-reform Fee Splits

	competition (# pv schools, 2007)		own 2007 fee	
	below median	above median	zero	positive
	(1)	(2)	(3)	(4)
-4	0.080	0.910	-10.003***	-5.530
	(2.620)	(1.881)	(2.850)	(4.378)
-3	6.355*	-2.465	-9.656***	-1.128
	(3.836)	(1.858)	(3.144)	(3.428)
-2	0.679	-6.259**	-13.867***	-5.325
	(1.005)	(2.669)	(4.649)	(4.133)
0	5.402**	5.158***	-7.611***	-13.015***
	(2.455)	(1.917)	(2.355)	(4.429)
1	7.412***	2.225	-9.105***	-17.189***
	(2.420)	(2.335)	(2.544)	(4.320)
2	8.286***	2.866	-8.233***	-16.942***
	(2.649)	(2.030)	(2.410)	(4.777)
3	7.493**	5.794**	-7.027***	-13.756**
	(3.509)	(2.342)	(2.133)	(5.474)
4	10.145**	4.948**	-6.982***	-11.240*
	(4.642)	(2.332)	(2.302)	(6.521)
5	-3.799	3.345	-6.807***	-20.509***
	(13.646)	(2.550)	(2.192)	(6.154)
6	13.407***	4.485	-6.890***	-15.627**
	(4.763)	(2.765)	(2.051)	(6.256)
7	13.712***	4.625*	-7.137***	-17.408**
	(4.230)	(2.619)	(2.378)	(7.160)
observations	4,844	33,427	17,762	17,266

Notes: As in Table B.4, but the outcome is the fee an eligible (low-income) student would face at each school: zero at participating schools from 2008 onward, the posted fee otherwise. Columns (1)–(2) correspond to panel C of Figure 4; columns (3)–(4) to panel A of Figure C.1. Subsamples and fixed effects are identical to those of Table B.4, so the two tables are directly comparable. Standard errors, clustered at the municipality level, are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.6: Composition Robustness and Estimates by Participation Status

	never-entering/exiting schools		by participation status	
	sticker	eligible-student fee	ever participant	never participant
	(1)	(2)	(3)	(4)
-4	1.184	0.936	-0.364	0.846
	(1.789)	(1.758)	(1.484)	(4.777)
-3	-0.516	-0.515	-0.869	-6.365
	(1.857)	(1.849)	(1.510)	(5.051)
-2	-6.583***	-6.569***	-5.688**	-7.097
	(2.501)	(2.499)	(2.563)	(4.915)
0	-2.144	5.753***	-1.238	-5.150
	(1.727)	(1.875)	(1.395)	(4.476)
1	-5.904***	5.370***	-2.114	-21.408**
	(1.716)	(1.852)	(1.403)	(9.036)
2	-7.828***	4.300**	-1.769	-16.813***
	(2.050)	(2.013)	(1.614)	(5.600)
3	-5.758***	7.224***	1.818	-13.711
	(1.992)	(2.129)	(1.535)	(10.163)
4	-10.280***	5.710**	-0.500	-12.744
	(2.080)	(2.237)	(1.779)	(9.672)
5	-9.800***	5.351**	2.036	-22.872**
	(2.178)	(2.174)	(1.522)	(11.111)
6	-7.823***	6.622***	4.393**	-9.939
	(2.417)	(2.175)	(1.878)	(13.411)
7	-7.509***	6.786***	6.615***	-20.403*
	(2.467)	(2.315)	(1.779)	(11.874)
observations	32,144	32,144	29,896	8,375

Notes: Columns (1)–(2): baseline event studies restricted to schools that neither enter nor exit the sample. Columns (3)–(4): posted-fee event studies estimated separately for schools that participate in the program in at least one year 2008–2015 (2,852 schools) and schools that never participate (1,038 schools); these splits are descriptive—participation is endogenous and its composition varies with the treatment intensity—and do not identify the counterfactual objects in equations (4)–(5). Standard errors, clustered at the municipality level, are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

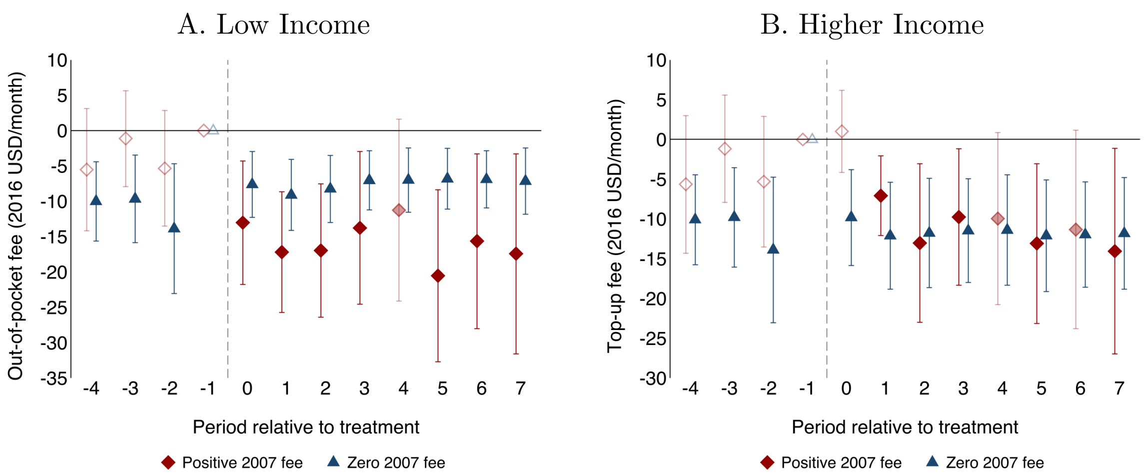
Table B.7: Robustness to Violations of Parallel Trends (Relative-Magnitudes Bounds)

	posted (sticker) fee		fee faced by eligible students	
$\bar{M}$	$\eta = 1$	avg. post	$\eta = 1$	avg. post
(original CI)	$[-12.0, -3.8]$	$[-11.5, -4.0]$	$[-1.4, 6.6]$	$[0.5, 8.0]$
0	$[-12.0, -3.8]$	$[-11.4, -4.0]$	$[-1.4, 6.6]$	$[0.5, 7.9]$
0.25	$[-15.2, -2.0]$	$[-20.2, 2.0]$	$[-4.5, 8.3]$	$[-8.1, 14.1]$
0.5	$[-19.5, 1.1]$	$[-30.4, 11.7]$	$[-8.7, 11.7]$	$[-18.1, 23.7]$
1	$[-28.4, 9.3]$	$[-50.6, 31.8]$	$[-17.7, 20.0]$	$[-38.3, 43.8]$

Notes: 95% robust confidence sets for the indicated event-study estimands under the relative-magnitudes restriction of Rambachan and Roth (2023), which allows post-period violations of parallel trends up to $\bar{M}$ times the largest pre-period deviation (here, the $\eta = -2$ coefficient of -5.4). "$\eta = 1$" is the 2009 effect; "avg. post" is the average of the 2008–2015 effects. Computed with the `honestdid` implementation of Rambachan and Roth (2023) from the baseline estimates of Table B.1.

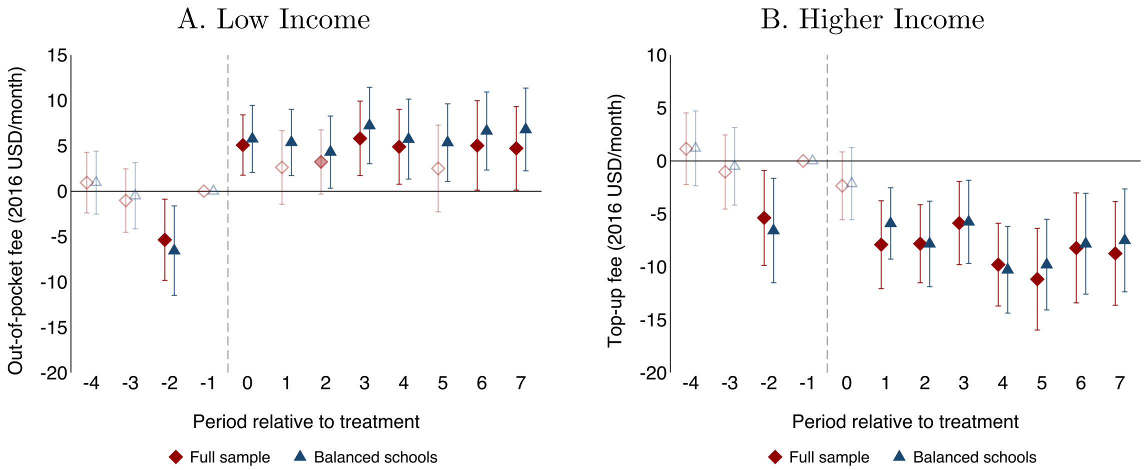
C Additional Figures

Figure C.1: Event Studies by the School's Own Pre-reform Fee - Top-up Fees



Notes: Event study estimated separately for schools with a positive 2007 fee and schools with a zero 2007 fee. Panels follow the convention of Figure 2: panel A's outcome is the fee an eligible (low-income) student would face at each school, and panel B's outcome is the posted (sticker) top-up fee paid by non-eligible (higher-income) students. Zero-fee schools include unlisted schools and display non-parallel pre-trends in both panels; see Section 4.4. 95% confidence intervals; standard errors clustered at the municipality level. For each series, hollow markers denote statistical insignificance; light colored markers denote statistical significance at the 10% level; dark colored markers denote statistical significance at the 5% level.

Figure C.2: Composition Robustness: Full versus Balanced Samples - Top-up Fees



Notes: Baseline event study and the same specification restricted to schools that never enter or exit the sample. Panels follow the convention of Figure 2: panel A's outcome is the fee an eligible (low-income) student would face at each school, and panel B's outcome is the posted (sticker) top-up fee paid by non-eligible (higher-income) students. 95% confidence intervals; standard errors clustered at the municipality level. For each series, hollow markers denote statistical insignificance; light colored markers denote statistical significance at the 10% level; dark colored markers denote statistical significance at the 5% level.