

SUPPLY-SIDE RESPONSES TO TARGETED VOUCHERS*

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Abstract

We examine schools' responses to a recent targeted voucher reform in Chile. Private schools' responses to the reform are quicker and larger than public schools' responses; however, schools in both sectors react to the policy along various margins. Private schools enter and exit the market more often. Public and private schools improve educational inputs related to school infrastructure and the teaching staff. Interestingly, private schools' fee responses are such that low income students see an increase in the average fee they face in the market, and higher income students see a decrease in the average fee they face.

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1 Introduction

The education literature that studies markets that include voucher subsidies and school choice often focuses its attention on the demand side of these markets (i.e. students, parents).¹ Studies typically answer important policy questions, such as what are the gains in academic performance and achievement of attending a private school versus a public one, or what are the factors that determine parental school choice. However, in most of the cases, voucher policies involve conditions and changes in regulation that also affect the supply side of the education markets (i.e. schools). Furthermore, effects on student outcomes may, at least partially, be explained by school responses to policies (Neilson, 2020; Dinerstein and Smith, 2021). For instance, a voucher program may induce schools to increase the quality of education they provide, with the consequent effect of improving students’ test scores, even in the absence of an effect on student sorting. Thus, understanding school responses to (voucher) policies is of first order importance for the effective design of policies, and for the complete understanding of the mechanisms driving the policies’ overall effects.

In this paper, we study schools’ responses to a recent targeted voucher reform in Chile that considerably increased government subsidies, and whose new set of rules changed the incentives faced by schools. We examine school responses on both the extensive (i.e. entry, exit) and the intensive (i.e. educational inputs, fees) margins.

Since 1981, public and private schools in Chile receive a flat subsidy for every student that they enroll. We call this subsidy the “universal voucher”. In addition, private schools are allowed to charge fees on top of the flat voucher. In 2008, the Chilean government introduced a new subsidy in the form of a targeted voucher for economically disadvantaged students, that supplemented the existing universal voucher. Schools are invited to participate in the new program, and schools that opt in receive extra funds for every low income student that they enroll, with the requirement of not charging top-up fees to these students. The targeted voucher program represents a considerable budgetary effort for the government, with the size of the new subsidy being about 60% the amount of the flat voucher.

We estimate the impact of the targeted voucher program on schools’ entry and exit, educational inputs, and top-up fee decisions. We identify treatment effect parameters by exploiting the differential incidence of the program across local education markets. We use the share of low income students in the market to define the incidence of the program, or the “intensity of treatment”. The idea is that, in a market where no low income student resides, the targeted voucher program has zero incidence, in terms of adding new funds, because no student is eligible to be a beneficiary of the program. Conversely, a market where all students come from low income families has the

¹See the extensive review in Epple et al. (2017).

highest potential of adding new funds. Hence, we use an event study design that compares outcomes before and after the introduction of the program across markets with different shares of low income students (Card, 1992; Duflo, 2001; Lucas and Mbiti, 2012).² We estimate the effects of the program distinguishing between public and private schools.

We find that both public and (especially) private schools respond to the implementation of the targeted voucher program along various margins. Consistent with the arguments in Pritchett (2013), we find that private schools respond quickly and in a sizable fashion to changes in the regulatory environment. Public schools, on the other hand, take more time to adjust their behavior to the new policy. Nevertheless, public schools' responses become statistically significant by the end of the period of analysis in many of the variables of interest we study (Hoxby, 2003b).

On the extensive margin, we find that private schools enter and exit the market more often than in the absence of the program. Public schools, on the contrary, are unresponsive along this margin.

When it comes to the effects of the reform on schools' educational inputs, we find that both public and private schools significantly adjust their educational inputs allocation due to the reform. More importantly, schools' adjustments are in general in the direction of increasing educational quality, both directly through the improvement of infrastructure items, and indirectly via the improvement of teachers' conditions at work. In particular, public and private schools make costly efforts to improve the subset of infrastructure related inputs we observe (share of multigrade classes, class size). A similar response is observed for schools' inputs that are related to their teaching staff. Specifically, schools employ enough teachers to reduce their pupil-teacher ratio (only in private schools; public schools do the opposite), and provide at least the same job conditions for their teachers than in the absence of the reform (same share of long term contracts, less time lecturing). Finally, the reform induces private schools to first reduce their share of specialized teachers, and then reverse this response, so that they end the period of analysis with their share of specialized teachers being unaffected by the reform.³ Public schools' adjustment of the share of specialized teachers follows a similar trend to that of private schools, but with a lag. As a consequence, we are not able to observe the complete reversion of the effect in the time frame of our analysis.

Possibly the most interesting result we find relates to the effect of the reform on the fees private schools charge to parents on top of the subsidies. Notice that the implementation of the targeted voucher program introduced a kind of third-degree price discrimination scheme for participating schools. To see this, observe that before the reform, all private schools were allowed to charge a unique top-up fee to all students. Once the reform was implemented, non-participating schools

²To avoid endogeneity issues due to endogenous migration, we use the share of low income students in the market one year before the introduction of the program.

³A specialized teacher is a teacher with extra training in one or more subjects.

continued to be allowed to charge a unique fee to families, but schools participating in the program were imposed a constraint that resulted in these schools (potentially) charging differently to low and higher income families. On the one hand, participating schools were mandated to charge zero top-up fees to low income students. On the other hand, they continued to be allowed to charge any (possibly positive) fee to higher income students. Thus, examining the effects of the reform on private schools' top-up fees necessarily implies distinguishing between fees charged to low income students and fees charged to higher income students.

Our results on the effects of the reform on schools' fees show that the program increases the top-up fees low income students face in the market, and reduces the top-up fees higher income students face in the market. The first result is certainly more surprising than the second one, given that the reform mandates participating schools to charge zero fees to low income students. We examine the mathematical conditions for both results to emerge, and note that the first result needs that schools' counterfactual fees in the case they do not participate in the program under the reform be sufficiently higher than schools' fees in the absence of the reform. For the second result to occur, the difference between counterfactual fees in the case schools participate in the program under the reform and fees in the absence of the program needs to be negative and larger in absolute value than the difference between counterfactual fees if schools do not participate in the program under the reform and fees in the absence of the reform. Available variation from the data does not allow us to formally test these conditions; however, we show descriptive statistics suggesting that the stated conditions are met.

This paper contributes to the literature along two main fronts. First, it adds to the existing set of papers that estimate the impact of voucher programs. Epple et al. (2017) review this literature. They organize the evidence around five research questions that motivate the studies, which relate to the effects of vouchers on the students who use them, student sorting across schools, pressure on public schools to improve, overall educational performance in the market, and the political economy underlying the existence of such programs. Since our estimated treatment effects are identified at the market level, we advance our understanding of the mechanisms driving the overall (including equilibrium effects) impact of vouchers on achievement, namely school responses. In that respect, the effect of a voucher program on school behavior is a key determinant of the consequent impact of the program on achievement, as suggested by Abdulkadiroglu et al. (2018), that postulate that the joining of low quality private schools to the Louisiana Scholarship Program is the major driver of this program's negative effects on students' test scores. Additionally, a subset of our results speak to the existing evidence on public schools' productivity, much of which is in the context of the United States (Hoxby, 2003a; Figlio and Rouse, 2006; Chakrabarti, 2008, 2013a,b; Figlio and Hart, 2014).⁴

⁴See, also, Hsieh and Urquiola (2006), that show that public schools' performance worsened in markets with a high

Second, our paper adds to the existing literature on the industrial organization of education markets. Typically, studies in this literature develop and estimate structural models of demand and/or supply to investigate parental preferences for schools, schools' strategies in contexts of imperfect competition, and consequences of actual and counterfactual policies on markets' equilibria.⁵ This paper is motivated by similar competition and regulation questions that motivate those studies, and answers them using a clean reduced form empirical design, that allows us to examine a larger set of outcomes than structural models often do.⁶

The remainder of the paper is organized as follows. Section 2 describes Chile's education system and the targeted voucher program that is the focus of our study. Section 3 describes our data. Section 4 presents the identification strategy and the empirical results. Section 5 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. Today, 38% of students in primary education grades attend public schools, 53% attend private-voucher schools, and 9% attend private-non-voucher schools.

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.⁷ 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,

degree of competition from the private sector after the introduction of the universal voucher in Chile.

⁵See, e.g., Gazmuri (2015), Ferreyra and Kosenok (2018), Allende (2019), Allende et al. (2019), Singleton (2019), Dinerstein et al. (2020), Neilson (2020), Sánchez (2020), and Dinerstein and Smith (2021).

⁶Dinerstein et al. (2020), and Dinerstein and Smith (2021) are other studies that examine schools' responses to policies using reduced form strategies.

⁷The yearly academic calendar in Chile starts in March and ends in December.

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% the universal voucher, depending on the share of eligible low income students enrolled in the school.

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

category	subsidy (US\$)											
	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.⁸ By 2011, about 48% of students in the country were classified to be eligible to receive the targeted voucher, and

⁸See Neilson (2013, 2020) and Aguirre (2020) for more details on the criteria to classify a student to be eligible to receive the targeted voucher.

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.

The evidence on the effects of Chile’s targeted voucher program on student outcomes is encouraging. There is a wide consensus that the program causes test scores to increase (Correa et al., 2014; Sánchez, 2019), and income-based achievement gaps to narrow (Navarro-Palau, 2017; Feigenberg et al., 2019; Neilson, 2020), meaning that it is the most vulnerable students that benefit the most from the program.⁹ Our examination of schools’ responses to the program thus come naturally, as a means to understanding the mechanisms driving the positive effects on students’ achievement.¹⁰

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, schools, and teachers.¹¹ 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

⁹An exception to the above-mentioned findings is Aguirre (2020). She applies an RD design on an index for student eligibility, and finds no significant test scores effects. However, such results do not necessarily contradict the related (difference-in-differences based) evidence finding positive effects on test scores, as she does not estimate the effects of the reform but rather she examines the effects of being eligible to receive the targeted voucher in a context where the targeted voucher program is already in place. Also, her null effects for individuals that are close to the cutoff for eligibility may not necessarily imply that the effects are nonexistent for students in other parts of the eligibility index distribution.

¹⁰Feigenberg et al. (2019) investigate the effects of the reform on class size and a few characteristics of teachers. Their results are mostly in line with the subset of ours that relate to educational inputs.

¹¹See Appendix A for a more detailed description of each of the data sets we use.

families’ preferences for school types in Chile.¹² 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
<i>A. All</i>	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
<i>B. In Program</i>	–	–	–	–	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 Tables 3–5. We keep in the final sample all school-year observations that do not present missing observations in all variables of analysis. Table 3 displays averages and standard deviations (in parentheses) for the number of schools that initiate operations (entry), and the number of schools that finalize operations (exit) per municipality, for the years 2005–2015. We separately describe the data for public (panel A), private-voucher (panel B), and private-non-voucher (panel C) schools. On average, 0.16 public schools enter the market in 2005.¹³ Entries slow down importantly throughout the period, and in 2015 only 0.03 public schools initiate operations, on average. Public school exits are always considerably higher than entries, a pattern that is consistent with the diminishing number of public schools we described in Table 2. In 2005, 0.25 public schools exit the market, on average. In 2015, 0.17 schools finalize operations, on average. Private-voucher schools enter the market more aggressively than public schools, especially early in the period. In 2005, 0.38 private-voucher schools enter the market. The entry frequency slows down over time, and in 2015 we see 0.1

¹²See Neilson (2013, 2020), and Sánchez (2020), among others.

¹³We define a market as being a municipality.

private-voucher schools enter the market, on average. Exits are lower than entries in the years 2005–2012. In 2013, this pattern reverses, and for the rest of the period, we observe a higher number of private-voucher schools exiting than entering the market. The entry and exit behavior of private-non-voucher schools are relatively less dynamic. Early in the period, the frequency of entries is in general lower than that of exits (e.g. 0.03 entries and 0.08 exits per market in 2005). In 2012–2015, private-non-voucher schools enter and exit at the same frequency, 0.02 schools, on average.

Table 3: Summary Statistics - Entry and Exit

		2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015
<i>A. Public</i>												
	entry	0.16 (0.50)	0.04 (0.21)	0.04 (0.23)	0.04 (0.25)	0.06 (0.26)	0.04 (0.20)	0.11 (0.34)	0.10 (0.34)	0.04 (0.22)	0.08 (0.29)	0.03 (0.25)
	exit	0.25 (0.62)	0.41 (0.88)	0.26 (0.66)	0.23 (0.54)	0.18 (0.55)	0.26 (0.75)	0.46 (1.18)	0.27 (0.70)	0.29 (0.71)	0.31 (0.79)	0.17 (0.48)
<i>B. Private-voucher</i>												
	entry	0.38 (0.86)	0.38 (0.87)	0.29 (0.67)	0.34 (0.80)	0.35 (0.77)	0.19 (0.53)	0.19 (0.52)	0.31 (0.69)	0.18 (0.61)	0.16 (0.45)	0.10 (0.38)
	exit	0.21 (0.68)	0.15 (0.47)	0.19 (0.57)	0.18 (0.60)	0.17 (0.47)	0.09 (0.31)	0.12 (0.43)	0.21 (0.52)	0.22 (0.56)	0.17 (0.50)	0.16 (0.50)
<i>C. Private-non-voucher</i>												
	entry	0.03 (0.19)	0.03 (0.23)	0.05 (0.34)	0.04 (0.23)	0.02 (0.19)	0.01 (0.11)	0.01 (0.08)	0.02 (0.13)	0.02 (0.15)	0.02 (0.17)	0.02 (0.14)
	exit	0.08 (0.37)	0.04 (0.23)	0.03 (0.21)	0.04 (0.23)	0.02 (0.15)	0.01 (0.11)	0.03 (0.22)	0.02 (0.18)	0.02 (0.19)	0.02 (0.15)	0.02 (0.15)
	observations	345	345	345	345	345	345	343	341	341	341	341

Notes: Municipality level mean values for entry and exit. Standard deviations are in parentheses.

Table 4 presents averages and standard deviations (in parentheses) for a set of educational inputs that represent school efforts to increase the quality of education, for the years 2004–2015. We distinguish between public (panel A), private-voucher (panel B), and private-non-voucher (panel C) schools. We analyze the share of multigrade classes, class size, pupil-teacher ratio, the share of teachers holding indefinite contracts, the share teachers with specialization, and weekly teaching hours per teacher. There is important heterogeneity in educational inputs across types of school, possibly reflecting the different objective and production functions in each sector, as well as schools’ characteristics.¹⁴ However, regardless of the intrinsic differences between sectors, almost all variables evolve in the direction of increasing education quality, especially in the period

¹⁴For instance, public schools are more likely to be located in rural areas than are private schools (Sánchez, 2020), and schools in rural locations are more likely to offer multigrade classes.

after the introduction of the reform. For instance, the share of multigrade classes reduces to about half in each sector during the period of study. The only exception is the share of teachers holding indefinite contracts, which steadily decreases over time, in all three sectors.

Table 4: Summary Statistics - Educational Inputs

	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015
<i>A. Public</i>												
multigrade classes (share)	0.46 (0.47)	0.45 (0.47)	0.45 (0.47)	0.45 (0.47)	0.44 (0.47)	0.44 (0.47)	0.40 (0.45)	0.31 (0.44)	0.34 (0.45)	0.23 (0.39)	0.24 (0.39)	0.26 (0.41)
class size	21.42 (11.10)	20.90 (11.08)	20.56 (11.07)	20.08 (11.03)	19.62 (10.99)	19.20 (10.97)	18.48 (11.07)	17.85 (11.42)	17.81 (11.14)	16.79 (11.84)	16.92 (11.69)	17.02 (11.49)
pupil-teacher ratio	16.46 (11.09)	15.68 (9.96)	15.52 (9.63)	14.80 (7.75)	14.59 (8.17)	14.32 (19.44)	13.74 (12.41)	12.90 (13.43)	11.45 (8.66)	10.70 (7.20)	10.00 (6.89)	9.56 (6.47)
indefinite contracts (share)	0.81 (0.24)	0.82 (0.23)	0.81 (0.23)	0.81 (0.23)	0.76 (0.25)	0.73 (0.26)	0.72 (0.27)	0.61 (0.29)	0.57 (0.29)	0.47 (0.29)	0.43 (0.29)	0.45 (0.28)
specialized teachers (share)	0.04 (0.10)	0.04 (0.10)	0.04 (0.11)	0.04 (0.10)	0.05 (0.11)	0.06 (0.12)	0.06 (0.12)	0.19 (0.20)	0.19 (0.21)	0.37 (0.28)	0.42 (0.28)	0.46 (0.28)
teaching hours per teacher	24.48 (4.98)	24.43 (5.09)	24.51 (5.17)	24.40 (5.16)	24.55 (5.28)	24.70 (5.39)	24.70 (5.54)	22.66 (5.87)	22.63 (5.78)	21.81 (5.75)	21.48 (5.74)	20.84 (5.72)
observations	5,411	5,375	5,253	5,176	5,100	5,052	4,971	4,758	4,777	4,713	4,638	4,601
<i>B. Private-voucher</i>												
multigrade classes (share)	0.23 (0.40)	0.23 (0.39)	0.22 (0.39)	0.22 (0.39)	0.21 (0.39)	0.20 (0.38)	0.19 (0.37)	0.15 (0.33)	0.14 (0.33)	0.09 (0.27)	0.10 (0.28)	0.11 (0.29)
class size	28.35 (11.07)	27.81 (11.07)	27.74 (11.05)	27.70 (10.87)	27.64 (11.06)	27.59 (10.81)	27.17 (11.01)	26.48 (11.46)	26.51 (11.32)	25.77 (12.34)	25.97 (12.14)	26.24 (11.96)
pupil-teacher ratio	21.37 (12.35)	20.06 (10.20)	20.08 (9.45)	19.83 (9.83)	19.62 (9.29)	19.37 (13.38)	18.74 (7.86)	17.24 (7.86)	17.20 (12.37)	15.99 (7.19)	15.51 (8.06)	15.18 (6.96)
indefinite contracts (share)	0.77 (0.27)	0.76 (0.27)	0.75 (0.27)	0.74 (0.27)	0.73 (0.27)	0.73 (0.27)	0.73 (0.26)	0.68 (0.26)	0.64 (0.27)	0.59 (0.27)	0.56 (0.26)	0.56 (0.26)
specialized teachers (share)	0.15 (0.18)	0.16 (0.18)	0.16 (0.18)	0.16 (0.18)	0.17 (0.18)	0.18 (0.19)	0.19 (0.19)	0.29 (0.22)	0.29 (0.22)	0.46 (0.25)	0.50 (0.25)	0.53 (0.25)
teaching hours per teacher	21.39 (5.66)	21.36 (5.62)	21.34 (5.59)	21.30 (5.61)	21.35 (5.52)	21.30 (5.46)	21.26 (5.46)	20.14 (5.51)	20.22 (5.47)	19.30 (5.39)	19.03 (5.23)	18.40 (5.18)
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
<i>C. Private-non-voucher</i>												
multigrade classes (share)	0.02 (0.11)	0.02 (0.10)	0.02 (0.11)	0.02 (0.10)	0.02 (0.11)	0.01 (0.09)	0.01 (0.08)	0.01 (0.10)	0.01 (0.08)	0.01 (0.06)	0.00 (0.06)	0.01 (0.06)
class size	20.55 (9.00)	20.49 (8.66)	20.75 (8.80)	20.99 (8.49)	21.02 (8.37)	22.49 (10.94)	21.23 (8.24)	21.83 (7.76)	21.00 (8.18)	21.87 (11.72)	21.59 (8.08)	21.74 (8.21)
pupil-teacher ratio	13.21 (11.28)	11.75 (6.49)	11.79 (6.29)	12.09 (6.34)	12.12 (6.17)	12.53 (7.19)	12.46 (6.65)	15.52 (32.73)	11.95 (8.26)	11.59 (5.36)	12.00 (20.40)	25.02 (58.22)
indefinite contracts (share)	0.82 (0.22)	0.84 (0.2)	0.84 (0.2)	0.82 (0.21)	0.82 (0.19)	0.81 (0.21)	0.81 (0.21)	0.84 (0.19)	0.81 (0.22)	0.76 (0.23)	0.73 (0.24)	0.71 (0.24)
specialized teachers (share)	0.36 (0.21)	0.38 (0.20)	0.39 (0.20)	0.38 (0.20)	0.39 (0.21)	0.40 (0.20)	0.41 (0.21)	0.50 (0.19)	0.48 (0.20)	0.58 (0.18)	0.63 (0.18)	0.66 (0.16)
teaching hours per teacher	17.98 (5.26)	17.69 (5.48)	17.68 (5.21)	17.72 (4.97)	17.7 (4.94)	17.67 (5.13)	17.68 (5.08)	17.18 (5.48)	17.06 (5.16)	15.98 (4.67)	15.33 (4.38)	14.63 (4.86)
observations	541	458	443	436	434	421	422	323	415	416	420	413

Notes: School level mean values. Standard deviations are in parentheses.

Table 5 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. Then, the targeted voucher program introduced a version of a third-degree price discrimination scheme for participating schools. Specifically, 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 (possibly positive) top-up fee to all students. Thus, starting in 2008, there exists two vectors of top-up fees, 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 5: Summary Statistics - Private-voucher School Top-up Fees

	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015
low income	16.14 (25.29)	17.36 (26.81)	18.34 (31.25)	17.24 (26.92)	14.46 (27.31)	14.97 (34.07)	15.04 (29.86)	14.40 (30.05)	14.96 (35.24)	15.41 (37.28)	14.95 (33.32)	14.71 (34.31)
higher income	16.14 (25.29)	17.36 (26.81)	18.34 (31.25)	17.24 (26.92)	17.82 (27.47)	19.33 (34.36)	19.29 (30.39)	18.98 (30.42)	20.49 (36.67)	20.87 (37.36)	20.13 (33.51)	20.04 (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. Standard deviations are in parentheses. 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.

4 Empirical Analysis

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.¹⁵ 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.

¹⁵See Topel (1986) and Card (2001) for other studies that use political and administrative boundaries to define local markets.

The particular design of the targeted voucher program implies that some markets are more affected by 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.¹⁶ 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.¹⁷ 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.¹⁸

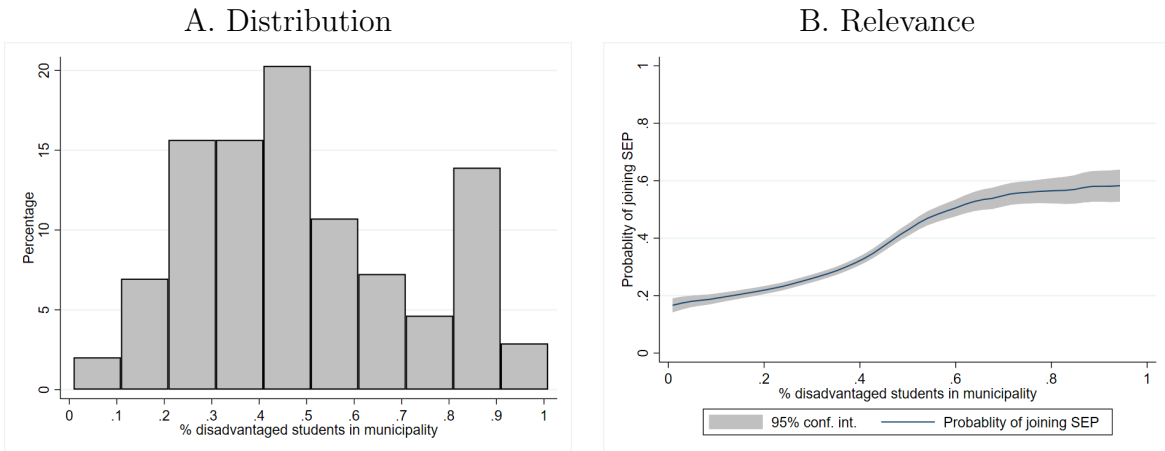
Figure 1 displays the distribution and relevance of the intensity of treatment variable. Panel A shows the distribution of municipalities according to their share of eligible students one year before the targeted voucher reform was implemented. The support of the distribution is complete in the $[0,1]$ range. Also, 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 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 municipality's share of low income students in 2007. The estimated function is monotonically increasing in the full domain $[0,1]$, going from about 0.2 to about 0.6 in the probability. We can, thus, conclude that 1) the distribution of the intensity of treatment provides enough variation to perform our statistical 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' decision to join the program.

¹⁶We 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.

¹⁷As 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).

¹⁸88% 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 Tables 3–5.

To test the effects of the reform on the number of schools entering and exiting the market, we estimate the following equation,

$$y_{st} = \gamma_s + \lambda_t + \sum_{\tau=-3}^7 \delta_\tau (D_{\tau t} \times \text{intensity}_s) + \varepsilon_{st}, \quad (1)$$

where y_{st} is the outcome of interest (entry/exit) for municipality s in period t , γ_s is a municipality fixed effect, λ_t is a year fixed effect, $D_{\tau t}$ is a year dummy variable, with $\tau = 0$ indicating the year of the introduction of the reform, 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_{st} is an error term. 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_τ , as they capture the period effects of the targeted voucher reform.

To test the effects of the reform on schools’ educational inputs and top-up fee decisions, we estimate the following equation,

$$y_{ist} = \gamma_s + \lambda_t + \sum_{\tau=-4}^7 \delta_\tau (D_{\tau t} \times \text{intensity}_s) + \varepsilon_{ist}, \quad (2)$$

which is analogous to equation (1), except that here the data are at the school-municipality-period level. Once again, we cluster standard errors at the municipality level at the moment of performing estimation.

The treatment effect parameters identified by our event study design are the average treatment effects on the treated, where the treatment corresponds to being located in a market where all students are eligible to receive the targeted voucher as opposed to being located in a market where none of the students is eligible to be a targeted voucher beneficiary. Consequently, 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.¹⁹

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

We present and discuss event study results for school entry and exit, educational inputs (multigrade classes, class size, pupil-teacher ratio, teachers under indefinite contracts, specialized teachers, teaching hours per teacher), and top-up fees. For clarity of exposition, we present our results in figures. We report our results separately for public and private-voucher schools, and omit results for private-non-voucher schools. Estimates for private-non-voucher schools are imprecise and in general statistically not distinguishable from zero. Tables B.1–B.8 in Appendix B present the corresponding point estimates and standard errors, including those for private-non-voucher schools.

¹⁹In 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 (2020).

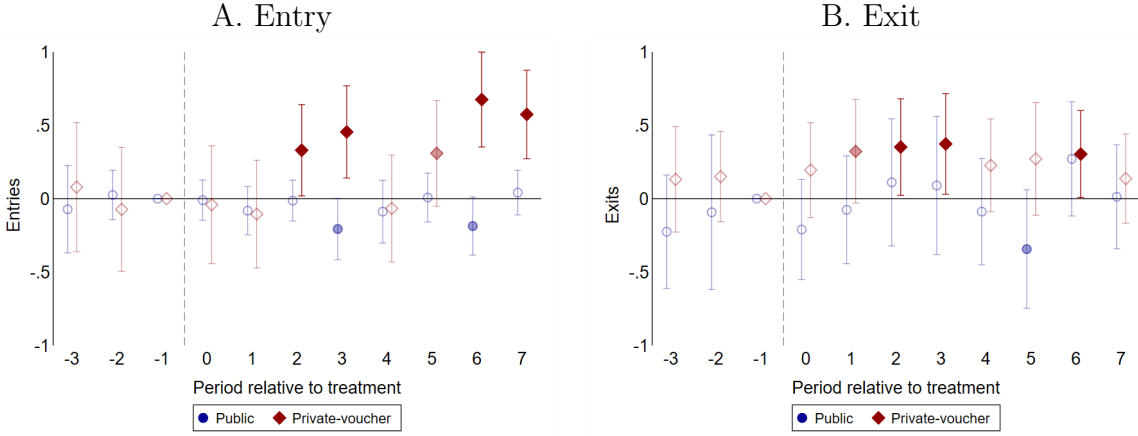
4.3.1 Entry and Exit

Figure 2 presents the effects of the targeted voucher program on school entry (panel A) and exit (panel B) into the market. A school enters the market in period t if it is present in the market in t and it was not present in the market in $t - 1$. Similarly, a school exits the market in period t if it is not present in the market in t and it was present in the market in $t - 1$. We set 2007 as the base year for comparison in the event study, and assign the value -1 to that period. Estimates are presented in blue circles for public schools and in red diamonds for private-voucher schools. Clustered at the municipality level 95% confidence intervals are also included. We do not observe the existence of statistically significant pre-trends before the introduction of the reform, confirming the internal validity of our design.

Panel A in Figure 2 shows that public schools are not too responsive to the program, in terms of school entry. If anything, public schools enter less as a consequence of the reform; however, such effects are only significant at the 10% level in periods 3 and 6, where the reform decreases public schools entry in about 0.2 schools. On the contrary, the effect of the reform on private-voucher schools' entry is important, though not immediate. Only in period 2 the effects start being statistically significant, where about 0.3 new private-voucher schools enter the market. This effect is somewhat larger in period 3. The largest effects are found in periods 6 and 7, where about 0.6 new private-voucher schools enter the market. Once again, these effects are considerable, representing more than 100% the average entry frequency of private-voucher schools one year prior to the reform (Table 3).

Panel B in Figure 2 displays the effects of the reform on school exit. These results are similar to the ones we observed for school entry. Public schools do not respond much to the reform and, if anything, they exit less; however, none of the corresponding estimates is statistically significant at the 5% level. Private-voucher schools, in turn, increase their exit frequency by a somewhat constant 0.3 schools per period, with the estimated effects being statistically significant in periods 1, 2, 3, and 6. These effects are sizable when compared to the average exit frequency of 0.19 private-voucher schools in 2007 (Table 3).

Figure 2: Effects of the Targeted Voucher Program on Public and Private-voucher Schools - Entry and Exit



Notes: A school enters the market in year t if it is observed in period t , but is not in the market in year $t - 1$. A school exits the market in year t if it is observed in year $t - 1$, but not in t . Results come from the estimation of event study regressions that use municipality level data for the period 2004–2015. Point estimates and 95% confidence intervals are presented for public (blue) and private-voucher (red) schools. Hollow circles/diamonds denote statistical insignificance. Light colored circles/diamonds denote statistical significance at the 10% level. Dark colored circles/diamonds denote statistical significance at the 5% level.

In sum, public schools do not modify their entry and exit behavior due to the targeted voucher reform. Private schools, on the other hand, respond relatively quickly, entering and exiting the market in sizable numbers. This evidence is in line with the argument in Pritchett (2013), which blames the lack of flexibility in top-down bureaucracies that are often behind the administration of public schools for the slow reaction observed in this sector. At the same time, Pritchett (2013) posits that it is private schools’ high exposure to market incentives, and the fact that their decisions are made at the institution level, that allows schools in the private sector to respond rapidly to changes in policies and regulation.

4.3.2 Educational Inputs

Figure 3 presents the effects of the reform on public and private-voucher schools’ educational inputs allocation. We examine six inputs that measure infrastructure and teaching staff efforts schools may make in order to increase the quality of education they provide: share of multigrade classes, class size, pupil-teacher ratio, share of teachers holding indefinite contracts, share of specialized teachers, and teaching hours per teacher. Each panel in Figure 3 shows the effects of the reform on a particular educational input.

We first observe that the vast majority of pre-trends are parallel, again confirming the internal validity of our research design.

The evidence presented in panels A and B of Figure 3 shows that in general schools respond to the reform by improving their infrastructure related inputs. Panel A shows that both public and private-voucher schools reduce their share of multigrade classes due to the reform. Responses are not immediate, but once estimates start being statistically significant (period 3), responses are economically important, especially late in the period. For instance, public schools end the period with a decrease in the share of multigrade classes larger than 80% the average value public schools have for this input in 2007 (Table 4). Private-voucher schools, in turn, end the period with a reduction in the share of multigrade classes of about 220% their average value in 2007.

Panel B in Figure 3 shows that the reform induces schools to reduce their class size, especially in the private sector. Public schools' response goes in the direction of improving educational quality, but is not particularly large. Statistically significant effects are only found in periods 5 and 6, where public schools on average reduce their class size by about 1–2 students per class (5–10% relative to 2007). Private-voucher schools, in contrast, show an important response to the reform. They almost immediately reduce their class size in period 1, and estimates are all statistically significant thereafter. The largest effects are found in periods 5 and 6, where private-voucher schools reduce their average class size by about 9 students.

Panels C–F show evidence on the effects of the targeted voucher reform on schools' educational inputs that are related to the staff of teachers they employ. The findings are in general in the direction of improving quality, especially for private-voucher schools.

Panel C in Figure 3 shows schools' responses to the reform in terms of the number of students per teacher they allocate. Interestingly, public schools increase their pupil-teacher ratio due to the reform. The estimated effects are statistically significant only late in the period, and represent 10–25% the average value for this input one year before the reform. In contrast, estimated responses in the private sector show that private-voucher schools immediately reduce their pupil-teacher ratio by about 1 student per teacher in the year of introduction of the program, and end the period with estimated effects of about -3 students per teacher.

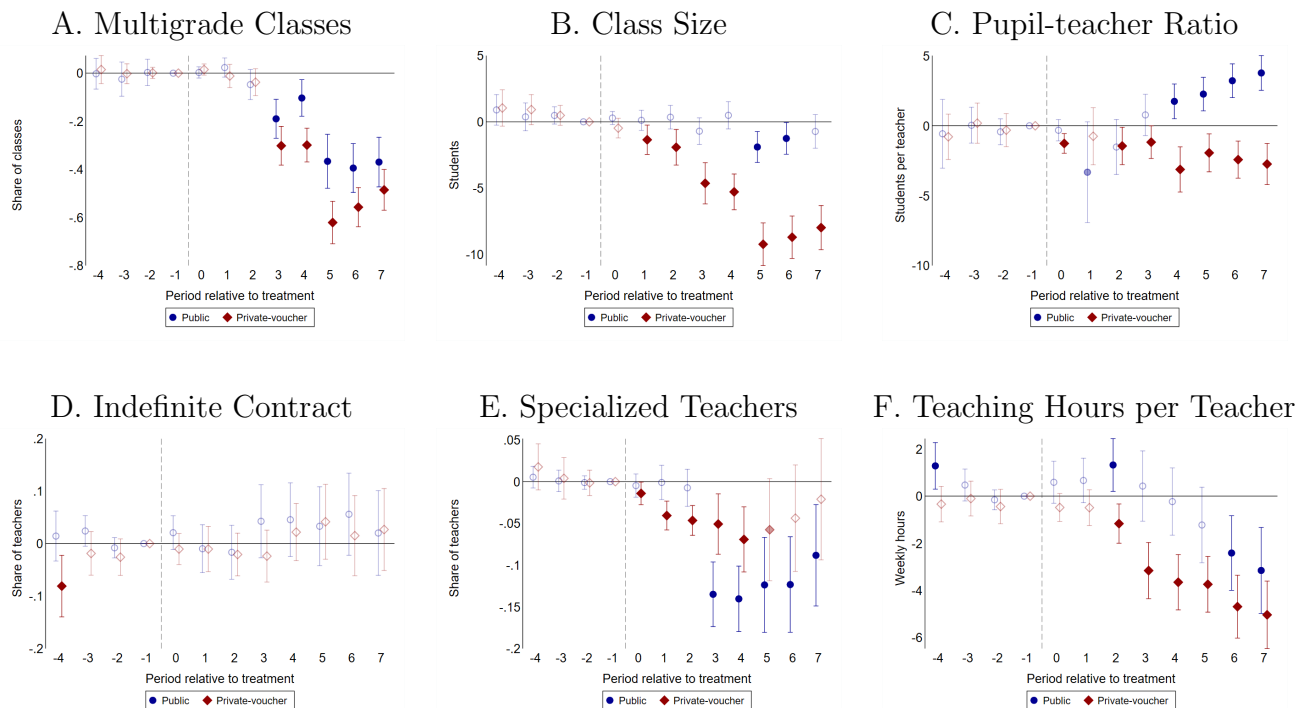
Panel D shows no statistically significant effect of the reform on the share of teachers hired under long-term indefinite contracts, suggesting that teachers did not see affected their work stability in any of the public and private sectors as a consequence of the reform.

Panel E presents the effects of the reform on the share of specialized teachers hired by the schools. A specialized teacher is one with extra training in one or more subjects, and is therefore expected to have higher teaching skills than an otherwise identical non-specialized teacher. Both public and private-voucher schools respond to the reform by reducing their share of teachers with some kind of specialization at some point in time. However, the corresponding reduction in the

public sector is considerably larger, and the reduction in the private sector is reversed by the end of the period. Public schools' statistically significant estimates appear in period 3, and represent 225–350% the average value of this input in the public sector one year prior to the reform. Private-voucher schools, in turn, start the period by reducing their share of specialized teachers, but the estimates are much smaller. The largest effect is found in period 4, where private schools reduce the input in question only by about 5% of its average value in 2007. Furthermore, the statistical significance of the effects for schools in the private sector disappears by the end of the period.

Lastly, panel F shows the effects of the reform on schools' decisions on the number of weekly teaching hours per teacher. Quite plausibly, the more time teachers spend in front of students, the higher the likelihood teachers experience work fatigue, which may harm their performance as lecturers. Results show that both public and private-voucher schools respond to the reform by reducing the average time teachers spend in class. Public schools react to the reform with some delay, and after slightly increasing the time teachers spend teaching in period 2, they reduce it in periods 6 and 7 in about 2–3 hours per week. The estimated effects for private-voucher schools indicate larger responses for schools in this sector relative to those observed for public schools. Estimated effects for private-voucher schools are all negative and statistically significant in periods 2–7, ranging between -5 and -1.2 weekly hours per teacher.

Figure 3: Effects of the Targeted Voucher Program on Public and Private-voucher Schools - Educational Inputs



Notes: Results come from the estimation of event study regressions that use school level data for the period 2004–2015. Each panel shows the effect of the reform on a particular educational input. Point estimates and 95% confidence intervals are presented for public (blue) and private-voucher (red) schools. Hollow circles/diamonds denote statistical insignificance. Light colored circles/diamonds denote statistical significance at the 10% level. Dark colored circles/diamonds denote statistical significance at the 5% level.

In summary, our results show that both public and private-voucher schools react in important manners to the introduction of the targeted voucher program. Schools, in general, improve their infrastructure related inputs, as well as the inputs related to their teaching staff. In line with the arguments in Pritchett (2013), public schools take some time to respond to policy changes relative to private schools; however, public schools end up responding, and their responses are mostly in the direction of increasing quality (Hoxby, 2003b).

4.3.3 Top-up Fees

Before we examine the effects of the targeted voucher reform on private-voucher schools' top-up fee decisions, it is important to note that the reform introduced a kind of a third-degree price discrimination scheme for participating private schools. To see this, notice 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 were imposed a 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, $E[p_j]$ 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 counterfactual top-up fee school j charges to high income students if the school participates in the program.²⁰ 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,

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

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,

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

²⁰Recall that participating schools are mandated to charge \$0 to low income students.

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

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

$$\begin{aligned} E [p_j^L] - E [p_j] &= E [(1 - \tau_j)p_j^0] - E [p_j] \\ &= E \{(1 - \tau_j)E [p_j^0 | \tau] - E [p_j]\}. \end{aligned} \quad (3)$$

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

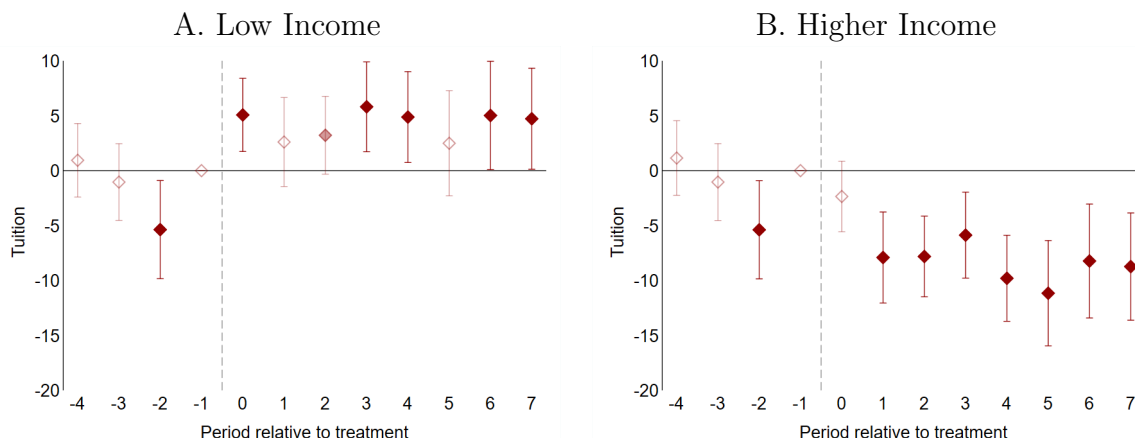
$$\begin{aligned} E [p_j^H] - E [p_j] &= E [\tau_j p_j^1 + (1 - \tau_j)p_j^0] - E [p_j] \\ &= E \{\tau_j E [p_j^1 - p_j | \tau] + (1 - \tau_j)E [p_j^0 - p_j | \tau]\}. \end{aligned} \quad (4)$$

Figure 4 presents the effects of the targeted voucher reform on the average top-up fees schools charge to low income students (panel A) and on the average top-up fees schools charge to higher income students (panel B).²¹ Pre-trends are in general parallel, except in period -2 . Panel A shows that on average private-voucher schools respond to the reform by increasing the top-up fee they charge to low income students. Such response is immediate, statistically significant, and stays somewhat constant over the period of study, at about \$3–6. Given that participating schools are mandated to charge no fee to low income students, this increase in the average fee result is necessarily driven by the counterfactual fees schools charge in the case they do not participate in the program, as is shown in equation (3).

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 estimates show a quick and fairly constant reaction, of about \$8–11 reduction in fees. This response represents 45–65% the average fee private-voucher schools charge in 2007.

²¹Table B.8 in Appendix B presents the corresponding point estimates and standard errors.

Figure 4: Effects of the Targeted Voucher Program on Private-voucher Schools - Top-up Fees



Notes: Fee 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. Results come from the estimation of event study regressions that use school level data for the period 2004–2015. Point estimates and 95% confidence intervals are presented. 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 (3) and (4). Our results imply that equation (3) is greater than zero, and equation (4) is less than zero, simultaneously. Equivalently,

$$E \{ (1 - \tau_j) E [p_j^0 | \tau] \} > E [p_j], \quad (5)$$

$$E \{ \tau_j E [p_j^1 - p_j | \tau] \} < -E \{ (1 - \tau_j) E [p_j^0 - p_j | \tau] \}. \quad (6)$$

That is, in expectation, the counterfactual top-up fees schools charge if they do not join the program under the reform need to be sufficiently higher than the top-up fees schools charge in the absence of the reform. Additionally, the difference between the counterfactual top-up fees schools charge to higher income students if they join the program under the reform and the top-up fees schools charge in the absence of the reform needs to be negative and sufficiently larger in absolute value than the difference between the counterfactual top-up fees schools charge if they do not join the program under the reform and the top-up fees schools charge in the absence of the reform.

We are not able to directly test conditions (5) and (6) with the variation the data provides. Recall that our research design identifies the average response to the reform over all schools in the market, and does not separately identify the average response of participating schools from that of non-participating schools. In other words, we identify $E \{ (1 - \tau_j) E [p_j^0 | \tau] - E [p_j] \}$ in

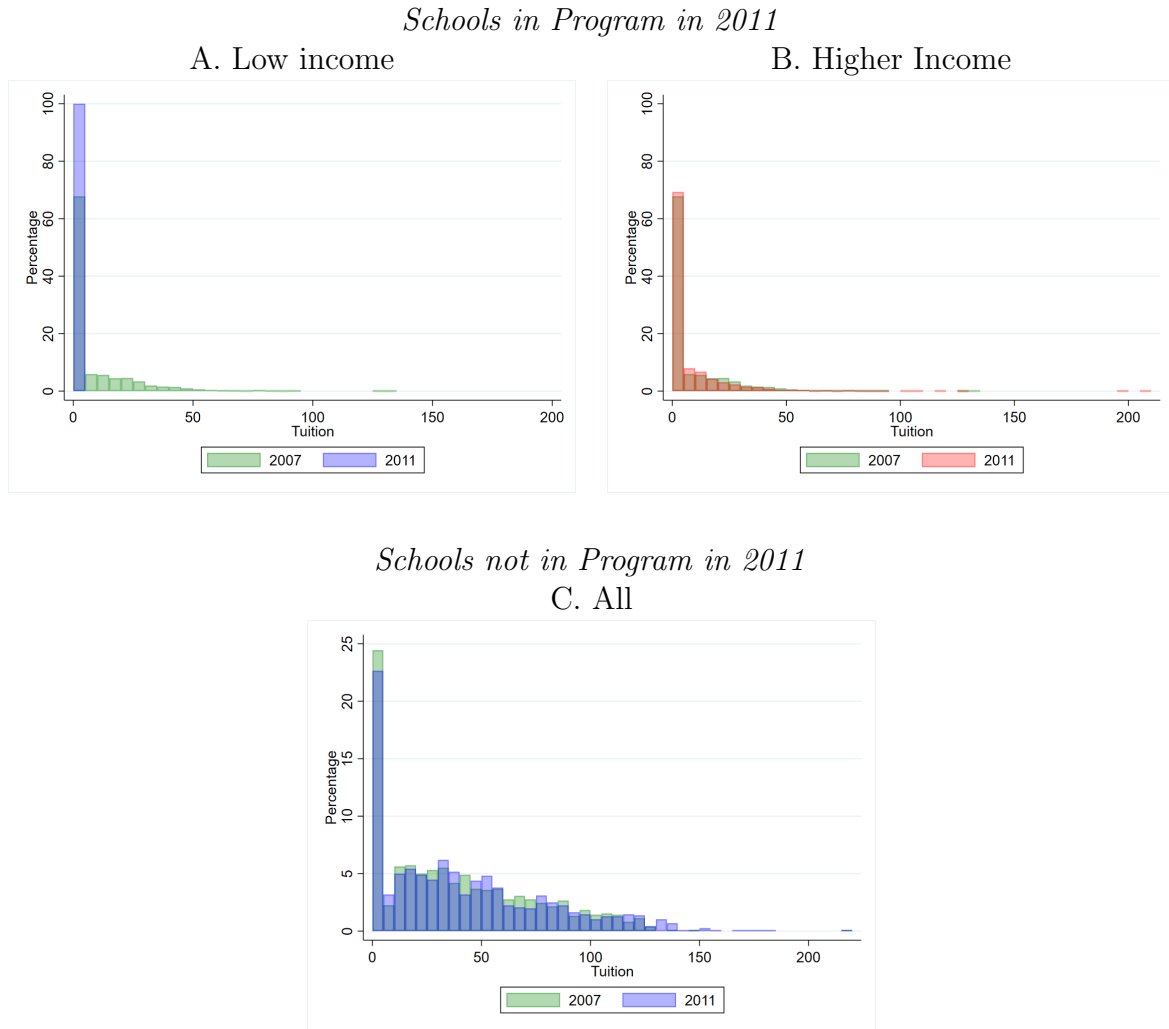
equation (3), but do not separately identify the terms $E\left\{(1 - \tau_j)E\left[p_j^0 \mid \tau\right]\right\}$ and $E[p_j]$ in that same equation. Similarly, we identify $E\left\{\tau_j E\left[p_j^1 - p_j \mid \tau\right] + (1 - \tau_j)E\left[p_j^0 - p_j \mid \tau\right]\right\}$ in equation (4), but do not separately identify the terms $E\left\{\tau_j E\left[p_j^1 - p_j \mid \tau\right]\right\}$ and $E\left\{(1 - \tau_j)E\left[p_j^0 - p_j \mid \tau\right]\right\}$ in that same equation. Nevertheless, we can examine the data descriptively and see if summary statistics are in line with conditions (5) and (6). We do exactly that in Figures 5 and 6.

Figure 5 displays the distributions of top-up fees charged by private-voucher schools to low and higher income students, for the year immediately preceding the introduction of the reform, 2007, and four years after the program was first implemented, 2011. We arbitrarily group schools into those that are part of the program in 2011, and those that are not part of the program in 2011, as a way to distinguish schools that are prone to participate in the program from schools that are less inclined to do so. Panels A and B show fee distributions for the former group of schools. Panel A displays the distribution of top-up fees schools that participate in the program in 2011 charge to low income students in 2007 and 2011. About 70% of schools charge zero fees in 2007, and in general fee levels are low. Also, all participating schools charge zero tuition to low income students in 2011, as is mandated by the program.

Panel B presents the distribution of top-up fees charged to higher income students in 2007 and 2011, for the group of schools that are part of the program in 2011. The distributions resemble each other substantially. More importantly, a visual inspection shows that the fee distribution in 2011 is shifted to the left relative to the 2007 distribution, suggesting that participating private-voucher schools decrease the top-up fees they charge to higher income students due to the reform. This descriptive fact suggests condition (6) is likely to be met.

Panel C displays the top-up fee distributions charged by schools that are not part of the program in 2011, for the years 2007 and 2011. The distributions resemble one another. Notice that a much smaller share of schools in this group charges zero fees (about 23% in 2007, and 25% in 2011) than is observed for schools that participate in the program in 2011 (panels A and B). More importantly, the fee distribution in 2011 is shifted to the right relative to that of 2007, suggesting that non-participating private-voucher schools increase the top-up fee they charge due to the reform, which is in line with condition (5).

Figure 5: Private-voucher Schools' Monthly Top-up Fees Distribution - Before and After the Introduction of the Targeted Voucher Program



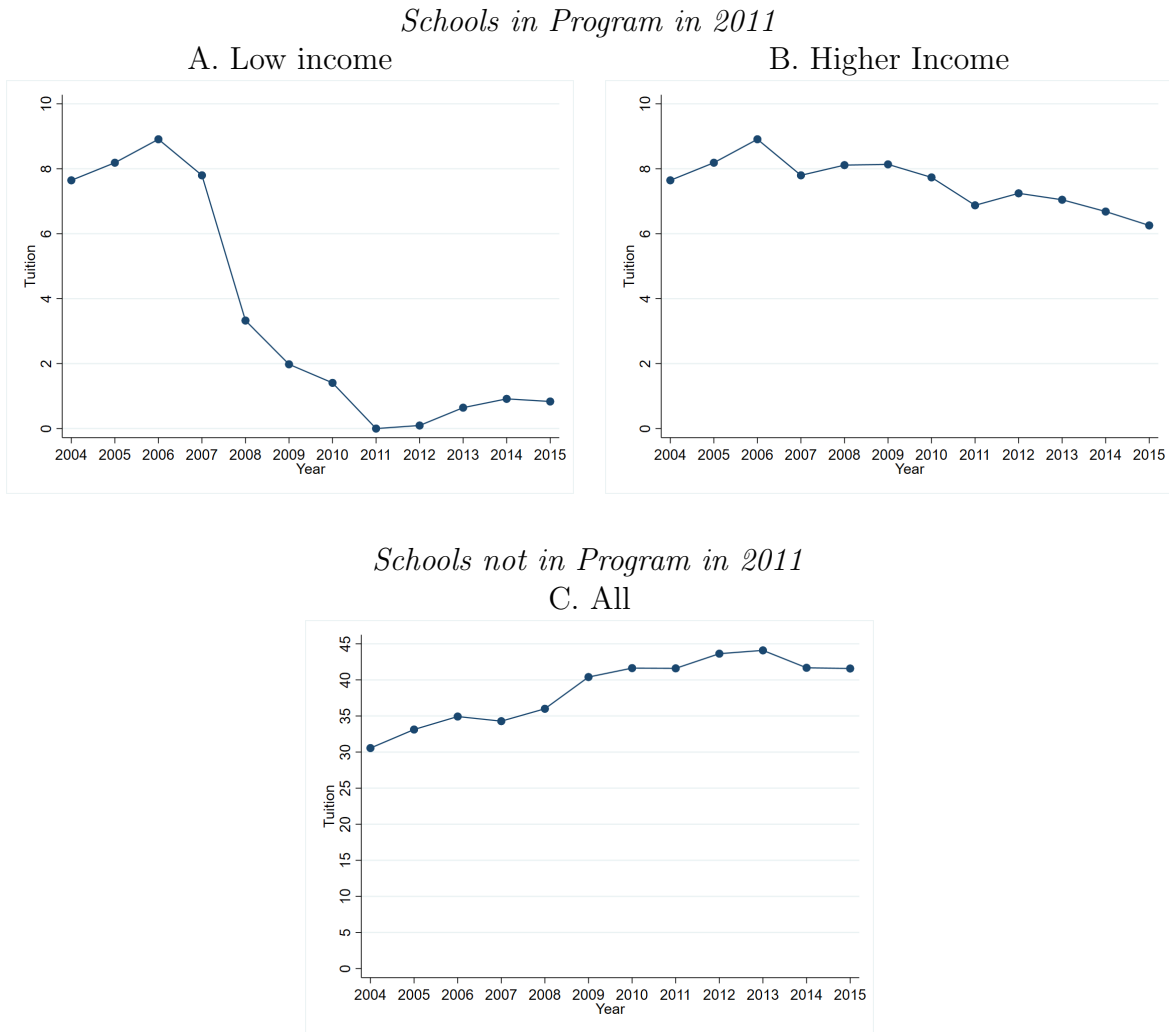
Notes: Fee values are real, and are converted from Chilean pesos to US dollars according to the exchange rate of Ch\$686.52 per US dollar, as of May 16, 2016.

Figure 6 complements the description of the data in Figure 5. It displays the average top-up fees charged by private-voucher schools to low and higher income students over time. Panels A and B show mean fees charged by schools that are part of the program in 2011. Panel A shows that the average top-up fees schools that participate in the program in 2011 charge to low income students dramatically decreases in the period post-implementation of the reform, going from about \$8 in 2007 to near \$0 in 2015. Note that this observation is consistent with the mandate of the

program that participating schools charge zero fees to eligible low income students.²² In panel B we observe that mean fees charged to higher income students steadily decreases in the period after the introduction of the reform, going from about \$8 in 2007 to about \$6 in 2015. Panel C presents mean top-up fees charged by schools that are not part of the program in 2011, over time. The average fee steadily increases over time, both before and after the implementation of the reform. Fees are on average about \$30 in 2004, \$35 in 2007, and \$42 in 2015. All these descriptive facts are in line with conditions (5) and (6).

²²Note that the mean top-up fee is not exactly \$0 for years other than 2011 in the post-reform period. This fact is explained by some schools in this group not joining the program immediately in 2008, as well as some other schools leaving the program after 2011.

Figure 6: Private-voucher Schools' Average Monthly Top-up Fees over Time



Notes: Fee 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.

To sum up, the reform increases the average top-up fee schools charge to eligible low income students. This is so despite the mandate of the program that participating schools charge no fees to eligible students. Additionally, the reform reduces the top-up fees schools charge to higher income students. For both these results to occur, counterfactual fees schools charge if they do not join the program under the reform need to be sufficiently higher than fees schools charge in the absence of the reform, and counterfactual fees schools charge to higher income students if schools participate in the program under the reform need to be sufficiently lower than fees schools charge

in the absence of the reform. Descriptive statistics suggest this is the case.

5 Conclusions

We examine school responses to the implementation of a recent targeted voucher program in Chile. Our results show that public and (especially) subsidized private schools respond to the reform along various margins. Private schools enter and exit more often the market than in the absence of the reform. Schools make successful efforts in improving infrastructure related inputs. When it comes to educational inputs that are related to schools' teaching staff, in general schools' responses are in the direction of improving quality. Lastly, private schools' top-up fee responses are such that low income students see the average fee they face in the market increase, while higher income students see the average fee they face in the market decrease.

Possibly the most interesting result we find is the one on private-voucher schools' fees. Surprisingly, the reform induces schools to increase the top-up fee that is charged to low income students, even with the reform's mandate of participating schools charging zero top-up fees to eligible low income students. We examine the mathematical conditions for our results on fees to occur, and conclude that the counterfactual fees schools charge in the case they do not join the program under the reform need to be sufficiently higher than their fees in the absence of the reform, and that the counterfactual fees schools charge to higher income students in the case they participate in the program under the reform need to be sufficiently lower than their fees in the absence of the reform. We show descriptive statistics supporting these conditions.

Our paper greatly adds to the educational vouchers literature. In particular, it complements existing evidence on the effects of Chile's recent targeted voucher reform, by investigating a piece of the puzzle (supply side) that is rarely the focus of study, but that is central to explaining and producing the observed effects on students' achievement. Furthermore, policymakers wanting to evaluate existing voucher programs or considering implementing voucher policies are in great need of understanding the supply side responses that are triggered by this type of program. Effective designs of voucher programs take into account all of the consequences these programs have on school markets; in particular, supply side responses (Epple et al., 2017).

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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 teachers, 2004–2015.

These data provide information on teachers' gender, date of birth, education degree, subject specialization, institution attended, graduation year, and duration of the degree studied. They also provide information on the characteristics of all schools in which each teacher is hired (municipality, type of management, rural status), and on the teachers' primary and secondary roles (e.g. teacher, principal, supervisor), type of contract, hours contracted, teaching hours, experience, tenure, and teaching subject and level of education.

- 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.

- 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.

B Additional Tables

Table B.1: Effects of the Targeted Voucher Program on School Entry and Exit

	<i>Entry</i>			<i>Exit</i>		
	public	private- voucher	private- non-voucher	public	private- voucher	private- non-voucher
	(1)	(2)	(3)	(4)	(5)	(6)
-3	-0.072 (0.151)	0.079 (0.224)	0.148 (0.126)	-0.225 (0.196)	0.131 (0.182)	-0.291 (0.139)
-2	0.025 (0.086)	-0.073 (0.215)	0.072 (0.086)	-0.093 (0.268)	0.150 (0.156)	-0.034 (0.094)
0	-0.010 (0.069)	-0.041 (0.204)	0.093 (0.158)	-0.210 (0.173)	0.194 (0.165)	-0.005 (0.113)
1	-0.082 (0.083)	-0.105 (0.186)	0.204 (0.146)	-0.076 (0.187)	0.322 (0.179)	0.073 (0.102)
2	-0.013 (0.071)	0.330 (0.158)	0.258 (0.142)	0.111 (0.220)	0.352 (0.167)	0.146 (0.094)
3	-0.206 (0.106)	0.454 (0.159)	0.278 (0.116)	0.090 (0.239)	0.373 (0.174)	0.027 (0.126)
4	-0.088 (0.109)	-0.067 (0.185)	0.225 (0.140)	-0.088 (0.184)	0.227 (0.161)	0.089 (0.080)
5	0.008 (0.085)	0.309 (0.183)	0.203 (0.137)	-0.343 (0.205)	0.271 (0.195)	0.035 (0.070)
6	-0.187 (0.101)	0.675 (0.164)	0.170 (0.127)	0.271 (0.198)	0.304 (0.151)	0.084 (0.106)
7	0.041 (0.077)	0.574 (0.153)	0.226 (0.142)	0.013 (0.180)	0.137 (0.154)	0.096 (0.103)
observations	3,795	3,795	3,795	3,795	3,795	3,795
R^2	0.167	0.412	0.281	0.229	0.372	0.348

Notes: A school enters the market in year t if it is observed in period t , but is not in the market in year $t - 1$. A school exits the market in year t if it is observed in year $t - 1$, but not in t . Results come from the estimation of event study regressions that use school data from three years prior to the introduction of the 2008 targeted voucher program, and from the following eight years. Clustered (at the municipality level) standard errors are in parentheses.

Table B.2: Effects of the Targeted Voucher Program on School Share of Multigrade Classes

	public (1)	private- voucher (2)	private- non-voucher (3)
-4	-0.003 (0.032)	0.015 (0.030)	0.027 (0.054)
-3	-0.025 (0.036)	-0.002 (0.021)	-0.017 (0.037)
-2	0.003 (0.028)	0.001 (0.012)	0.030 (0.059)
0	0.003 (0.012)	0.015 (0.012)	-0.065 (0.034)
1	0.023 (0.020)	-0.012 (0.024)	-0.091 (0.042)
2	-0.047 (0.032)	-0.038 (0.028)	-0.096 (0.047)
3	-0.190 (0.041)	-0.302 (0.041)	-0.106 (0.053)
4	-0.103 (0.039)	-0.299 (0.036)	-0.070 (0.060)
5	-0.367 (0.057)	-0.621 (0.045)	-0.083 (0.046)
6	-0.394 (0.052)	-0.557 (0.041)	-0.073 (0.048)
7	-0.370 (0.052)	-0.485 (0.043)	-0.055 (0.043)
observations	59,566	38,271	5,110
R^2	0.309	0.526	0.147

Notes: Results come from the estimation of event study regressions that use school data from four years prior to the introduction of the 2008 targeted voucher program, and from the following eight years. Clustered (at the municipality level) standard errors are in parentheses.

Table B.3: Effects of the Targeted Voucher Program on School Class Size

	public (1)	private- voucher (2)	private- non-voucher (3)
-4	0.900 (0.586)	1.044 (0.696)	0.652 (2.065)
-3	0.373 (0.535)	0.921 (0.575)	-1.672 (2.190)
-2	0.482 (0.330)	0.488 (0.381)	0.539 (1.945)
0	0.288 (0.248)	-0.477 (0.375)	2.135 (1.356)
1	0.114 (0.387)	-1.355 (0.562)	7.145 (3.553)
2	0.350 (0.452)	-1.922 (0.684)	2.546 (1.975)
3	-0.705 (0.509)	-4.641 (0.790)	1.617 (2.523)
4	0.491 (0.522)	-5.287 (0.685)	0.763 (2.451)
5	-1.901 (0.592)	-9.243 (0.818)	2.473 (2.584)
6	-1.253 (0.609)	-8.711 (0.813)	3.268 (2.417)
7	-0.726 (0.646)	-7.986 (0.847)	4.939 (2.751)
observations	59,566	38,271	5,110
R^2	0.457	0.367	0.173

Notes: Results come from the estimation of event study regressions that use school data from four years prior to the introduction of the 2008 targeted voucher program, and from the following eight years. Clustered (at the municipality level) standard errors are in parentheses.

Table B.4: Effects of the Targeted Voucher Program on School Pupil-teacher Ratio

	public (1)	private- voucher (2)	private- non-voucher (3)
-4	-0.574 (1.249)	-0.788 (0.823)	-0.858 (2.455)
-3	0.040 (0.652)	0.188 (0.729)	-1.239 (1.506)
-2	-0.432 (0.470)	-0.314 (0.598)	0.875 (1.190)
0	-0.319 (0.388)	-1.265 (0.357)	1.400 (1.073)
1	-3.320 (1.831)	-0.750 (1.036)	-0.200 (1.608)
2	-1.521 (0.999)	-1.443 (0.679)	-0.137 (1.482)
3	0.775 (0.750)	-1.173 (0.592)	-10.489 (11.766)
4	1.746 (0.631)	-3.123 (0.821)	5.813 (3.299)
5	2.265 (0.608)	-1.938 (0.690)	4.080 (2.464)
6	3.221 (0.614)	-2.423 (0.674)	13.207 (10.054)
7	3.777 (0.632)	-2.737 (0.748)	-16.837 (17.091)
observations	59,566	38,271	5,110
R^2	0.203	0.191	0.056

Notes: Results come from the estimation of event study regressions that use school data from four years prior to the introduction of the 2008 targeted voucher program, and from the following eight years. Clustered (at the municipality level) standard errors are in parentheses.

Table B.5: Effects of the Targeted Voucher Program on School Share of Indefinite Contracts

	public (1)	private- voucher (2)	private- non-voucher (3)
-4	0.014 (0.024)	-0.081 (0.030)	0.032 (0.101)
-3	0.024 (0.015)	-0.019 (0.021)	0.115 (0.069)
-2	-0.008 (0.010)	-0.026 (0.018)	0.067 (0.060)
0	0.021 (0.016)	-0.010 (0.015)	-0.087 (0.075)
1	-0.010 (0.023)	-0.010 (0.022)	-0.053 (0.084)
2	-0.017 (0.026)	-0.021 (0.021)	-0.103 (0.089)
3	0.043 (0.035)	-0.024 (0.025)	0.022 (0.089)
4	0.046 (0.036)	0.022 (0.028)	-0.060 (0.107)
5	0.033 (0.038)	0.041 (0.036)	-0.064 (0.108)
6	0.056 (0.040)	0.015 (0.039)	-0.106 (0.106)
7	0.020 (0.041)	0.027 (0.040)	-0.011 (0.108)
observations	59,566	38,271	5,110
R^2	0.326	0.158	0.174

Notes: Results come from the estimation of event study regressions that use school data from four years prior to the introduction of the 2008 targeted voucher program, and from the following eight years. Clustered (at the municipality level) standard errors are in parentheses.

Table B.6: Effects of the Targeted Voucher Program on School Share of Specialized Teachers

	public (1)	private- voucher (2)	private- non-voucher (3)
-4	0.005 (0.007)	0.018 (0.014)	-0.199 (0.060)
-3	0.001 (0.007)	0.004 (0.013)	-0.058 (0.054)
-2	-0.001 (0.004)	-0.001 (0.008)	-0.048 (0.026)
0	-0.005 (0.007)	-0.014 (0.007)	-0.005 (0.035)
1	-0.001 (0.010)	-0.041 (0.009)	-0.014 (0.044)
2	-0.007 (0.011)	-0.046 (0.009)	0.012 (0.055)
3	-0.135 (0.020)	-0.051 (0.018)	-0.138 (0.093)
4	-0.140 (0.020)	-0.069 (0.020)	-0.090 (0.065)
5	-0.124 (0.029)	-0.058 (0.031)	-0.080 (0.093)
6	-0.123 (0.029)	-0.044 (0.032)	-0.127 (0.110)
7	-0.088 (0.031)	-0.021 (0.037)	-0.145 (0.110)
observations	59,566	38,271	5,110
R^2	0.506	0.459	0.335

Notes: A specialized teacher is one with extra training in one or more subjects. Results come from the estimation of event study regressions that use school data from four years prior to the introduction of the 2008 targeted voucher program, and from the following eight years. Clustered (at the municipality level) standard errors are in parentheses.

Table B.7: Effects of the Targeted Voucher Program on School Weekly Teaching Hours per Teacher

	public (1)	private- voucher (2)	private- non-voucher (3)
-4	1.285 (0.503)	-0.340 (0.383)	2.257 (1.808)
-3	0.472 (0.345)	-0.103 (0.375)	1.419 (1.294)
-2	-0.158 (0.214)	-0.442 (0.371)	1.365 (0.887)
0	0.589 (0.453)	-0.483 (0.300)	0.074 (0.677)
1	0.665 (0.481)	-0.490 (0.384)	-0.993 (1.168)
2	1.323 (0.569)	-1.167 (0.423)	-1.646 (1.233)
3	0.428 (0.758)	-3.163 (0.606)	-0.066 (2.170)
4	-0.232 (0.727)	-3.657 (0.596)	-0.200 (1.876)
5	-1.226 (0.816)	-3.745 (0.601)	-1.497 (1.965)
6	-2.417 (0.806)	-4.695 (0.678)	-1.687 (1.919)
7	-3.157 (0.929)	-5.042 (0.728)	-0.896 (2.506)
observations	59,566	38,271	5,110
R^2	0.308	0.292	0.163

Notes: Results come from the estimation of event study regressions that use school data from four years prior to the introduction of the 2008 targeted voucher program, and from the following eight years. Clustered (at the municipality level) standard errors are in parentheses.

Table B.8: Effects of the Targeted Voucher Program on Private-voucher School Top-up Fees

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

Notes: Fee 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. Results come from the estimation of event study regressions that use school data from four years prior to the introduction of the 2008 targeted voucher program, and from the following eight years. Column (1) shows the effects of the reform on the top-up fees faced by low income students that are eligible to participate in the program, and column (2) shows the effects of the reform on the top-up fees faced by higher income students. Clustered (at the municipality level) standard errors are in parentheses.