

## ONLINE APPENDIX

O-1. *Provision of Preschool Services in Chile*

Chile's preschool sector combines public and private providers, with enrollment rates that have risen substantially but remain below OECD averages.<sup>27</sup> Two public networks, Junji (approximately 3,000 centers) and Fundación Integra (approximately 1,000 centers), provide tuition-free services and are explicitly tasked with reaching children from low-income families nationwide. In 2016, 42% of enrolled 3–4-year-olds attended Junji centers and 18% attended Integra centers; the remaining children attended private providers, some publicly funded and others charging tuition. Admission to both public and private centers is decentralized, with public providers giving priority to lower-income families (Aguirre, 2011). Several targeted public programs also support families in sending children to preschool. Until 2016, Junji also carried out light monitoring of private providers, primarily related to inputs; an independent quality-assurance agency has since assumed that role. Expenditure per student was \$6,408 in 2015, of which 15% came from private sources (OECD, 2016).

O-1.1. *Fundación Integra*

Fundación Integra (hereafter Integra) is the second-largest public preschool network in Chile, serving more than 72,000 children aged three months to four years in tuition-free centers concentrated in low-income neighborhoods. Integra does not offer primary education, so its oldest cohort must choose a primary school each year. This feature makes Integra an ideal partner for studying school choice: we can reach families precisely when they are deciding where to enroll, using the network's existing infrastructure for data collection and intervention delivery, without the selection concerns that would arise from recruiting at primary schools themselves.

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<sup>27</sup>This subsection draws on OECD (2016). As of 2015, enrollment of 0–2 year-olds in formal care was 18% (vs. 33% across the OECD), rising to 54% for 3-year-olds (vs. 71%), nearly double the 2005 rate but still below the OECD average of 71%.

### O-2. *Sample Selection*

We worked in Chile's three largest regions (Valparaíso, Biobío, and Santiago), selecting urban Integra preschools located in areas with meaningful school competition. Specifically, all preschools in municipality  $i$  were eligible if there were at least 10 primary schools within a 2 km radius and the ratio  $(\text{primary schools}_i / \text{preschools}_i) \geq 2$ . We further restricted attention to schools in the three lowest SES categories (as classified by the Ministry of Education), which constitute nearly all schools in the effective choice set of families in the first three income quintiles.

These criteria yielded 143 eligible preschools. We randomly assigned them to treatment and control groups, stratifying by region, grade level, and the number of primary schools within 2 km. We then contacted each preschool to schedule a visit during an existing parent meeting between August and December 2010.

Of the 143 preschools, 10 could not be visited, owing to principal refusal, scheduling conflicts, or low parent attendance. Table O-1 compares observable characteristics of preschools with and without meetings. The 10 excluded centers have a somewhat higher share of mothers with tertiary education, a higher share of families in the second income quintile, and a lower share of indigent families. Because no parents at these centers signed the informed consent, the effective sample consists of 133 preschools.

In the 133 participating preschools, 1,832 parents signed informed consent and completed a baseline survey before any information was distributed. The survey asked whether the family planned to enroll the child in primary school in 2011, whether a school had already been chosen, whether enrollment was complete, and whether older siblings already attended primary school. Because visits were not announced in advance, meeting attendance was plausibly independent of treatment status. Table O-5 confirms balance on observables, and Table O-2 shows that attendance at parent meetings was similar across treatment and control preschools, with nearly all parents present signing the informed consent (only 27 of 1,859 declined, or 1.4%).

TABLE O-1  
DIFFERENCES BETWEEN PRESCHOOLS WITH AND WITHOUT MEETINGS

	With Meeting	Without Meeting	Difference	t-stat
Enrollment	40.21	46.50	-6.29	-1.02
Mean Attendance	27.99	32.40	-4.41	-0.77
Mother w. Complete Tertiary (%)	9.07	12.81	-3.74	-1.76*
Mother w. Complete Secondary (%)	47.74	45.36	2.38	0.70
Mother w. Incomplete Secondary (%)	34.72	34.89	-0.17	-0.05
Mother w. Complete Primary (%)	7.81	6.94	0.87	0.65
Q1 of Income (%)	58.35	53.89	4.47	1.22
Q2 of Income (%)	31.56	38.42	-6.86	-2.02**
Q3 of Income (%)	8.00	5.95	2.05	1.27
Indigent (%)	15.37	9.68	5.69	2.73***
Poor non-indigent (%)	40.67	42.10	-1.42	-0.41
Non-poor (%)	43.96	48.23	-4.27	-1.33
Number of Preschools	133	10	143	

Notes: Columns 1–2 report averages for preschools with and without meetings. Column 3 reports the difference; Column 4 reports the *t*-statistic for the null of equal means, with standard errors clustered at the preschool level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

TABLE O-2  
ATTENDANCE TO PRESCHOOL AND PARENT MEETING

	Control	Treatment	Total
Enrollment (year)	41.47	39.57	40.21
Mean attendance (year)	28.69	27.64	27.99
Attendance to meeting	13.24	14.35	13.98
Number of preschools	45	88	133

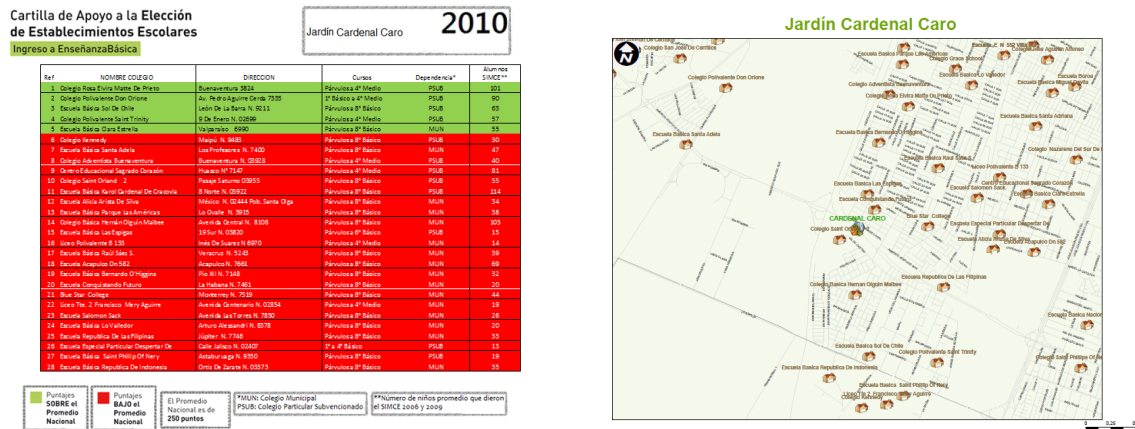
Notes: Columns 1–2 report averages for control and treatment preschools; column 3 reports the overall average. Mean attendance to the meeting represents roughly 35% of enrolled students and about half of typical daily attendance. Visits were not announced in advance.

### O-3. Treatment Design and Implementation

The intervention had two components: a personalized report card and a short video. This subsection describes each in detail.

*Report card.* Each treated family received a report card listing characteristics of nearby primary schools.<sup>28</sup> The card reported: (i) test scores, averaged over Math and Reading for 2006–2009 to reduce noise from any single year; (ii) the trend in test scores across years, since a school at the median that has substantially improved may be a better match for some parents than one with the same level score that has worsened; (iii) the official tuition charged to parents, from Ministry of Education records;<sup>29</sup> (iv) school type (public or private); and (v) school location. A map showing all listed schools accompanied each card. To signal relative quality, schools above the national mean test score (roughly 250 points) were colored green and those below were colored red. This design feature reflected policymaker preferences for the planned intervention and was intended to address the potential asymmetry of information parents face regarding school quality. Figure O-1 shows an example.

FIGURE O-1.—Report Card and School Map



*Video.* The video featured three role models from low-income backgrounds. The first was a mother who had decided to transfer her second-grade son to a school with higher test scores in order to give him a better education. The second was a college student completing

<sup>28</sup>The report card included up to 30 schools in the lowest three SES categories within 2 km of the preschool. Higher-SES schools were excluded because they are generally not in the effective choice set of families in this context. When more than 30 schools fell within 2 km, we randomly dropped schools away from the extremes of the quality distribution to avoid presenting a selected subset of information.

<sup>29</sup>Official tuition does not capture all costs families face (e.g., materials fees, parent-association dues) or discounts that individual schools may offer. We used it because it is an objective, comparable measure available for all schools.

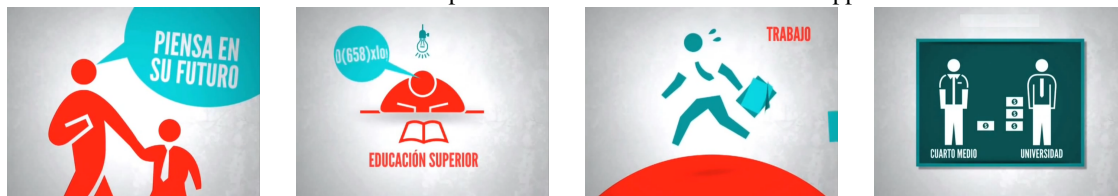
a degree in computer engineering who had attended a strong high school in a low-income neighborhood. The third was a young woman from a similarly disadvantaged background who, partly because of her high school, had completed vocational training and now held a stable job at a bank. All three were chosen to convey that good schools and higher education are accessible to low-income families, consistent with the evidence in Nguyen (2008) on the effectiveness of role models from similar backgrounds. The video also presented descriptive evidence on the returns to tertiary education in Chile (for example, that college graduates earn roughly three times the average earnings of those with only a high school diploma) and highlighted the observed correlation between primary school quality and later enrollment in higher education, without claiming a causal relationship (Jensen, 2010).

FIGURE O-2.—Video: Role models from low-income neighborhoods



*Notes:* Screenshots from the video shown to treated families. Panel (a): a mother who transferred her son to a higher-performing school. Panel (b): a computer engineering student from a low-income neighborhood. Panel (c): a young woman who works as a commercial assistant. All three role models come from low-income backgrounds.

FIGURE O-3.—Video: Importance of school choice for future opportunities



*Notes:* These panels summarize the part of the intervention that emphasized the long-run returns to school choice. The video encouraged parents to think about their child's future education and employment opportunities and linked school quality to preparation for higher education and better jobs.

The video thus complemented the report card's factual content with motivational framing: even families who know which schools score well may underweight quality in their decisions if they do not perceive the returns to be large or attainable.

O-3.1. *Direct Effects of Report Card Design*

TABLE O-3  
DIRECT EFFECTS OF REPORT CARD DESIGN

	On Report Card		Green		Top 5	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Full Sample</i>						
Treatment	-0.118*** ( 0.029)	-0.112*** ( 0.030)	0.039 ( 0.046)	0.028 ( 0.044)	0.075* ( 0.041)	0.064* ( 0.039)
N obs.	1775	1775	1136	1136	1136	1136
<i>Panel B: Enrolled sample</i>						
Treatment	-0.007 ( 0.054)	0.004 ( 0.053)	0.069 ( 0.076)	0.038 ( 0.072)	0.048 ( 0.069)	0.025 ( 0.069)
N obs.	596	596	389	389	389	389
<i>Panel C: Not enrolled sample</i>						
Treatment	-0.172*** ( 0.032)	-0.159*** ( 0.033)	0.019 ( 0.059)	0.036 ( 0.060)	0.076* ( 0.041)	0.090** ( 0.041)
N obs.	975	975	639	639	639	639
Randomization controls	×		×		×	
Expanded controls		×		×		×

Note: Randomization controls include market characteristics of schools (number and test scores mean, standard deviation and percentiles 25, 50 and 75.). Expanded controls include Mother's education, household information (size, durable goods, owned house), baseline school choice information.

Notes: ITT estimates from regressions of report-card-specific outcomes on treatment assignment. Standard errors in parentheses are clustered at the preschool-center level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table O-3 asks whether specific design features of the report card affected school choice beyond the information content of the intervention. Columns 1–2 test whether treated families became more likely to choose a school that was listed on the card. Columns 3–6 are estimated among families who chose a school that appeared on the report card, since the green and top-five classifications are only defined for listed schools. Columns 3–4 test whether, conditional on choosing a listed school, treated families were more likely to choose one coded in green (above the national mean test score). Columns 5–6 test whether they were more likely to choose one of the top-five schools highlighted on the card. Within each pair, the first column includes the randomization controls and the second adds the expanded family controls.

The results provide little evidence that these design features mechanically drove choices. If anything, treated families are less likely to choose a school that appears on the card, especially in the not-yet-enrolled sample, which is consistent with the main-text finding that treated families travel farther in order to attend better schools. The estimates on choosing a green school are small and statistically insignificant. By contrast, the estimates in columns 5–6 suggest that treated families are somewhat more likely to choose a school ranked among the top five in the card, particularly among those who had not yet enrolled. We interpret this pattern as consistent with the intervention making quality differences more salient, rather than with a purely mechanical response to the visual design of the card.

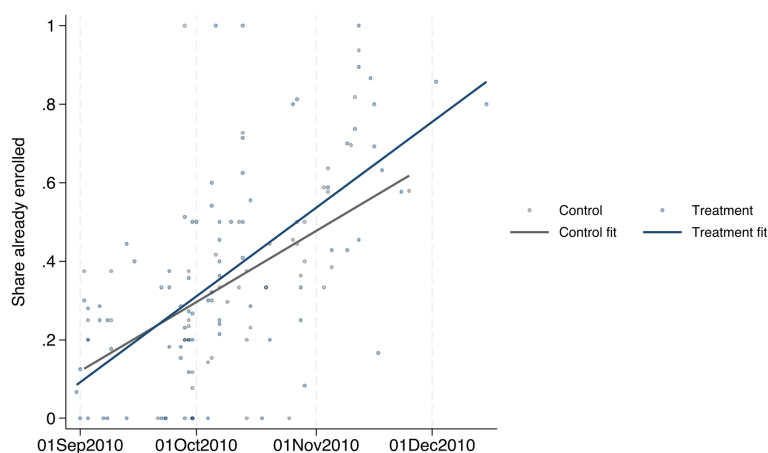
#### *O-4. Selection on Enrollment at Baseline*

A substantial share of families in our sample had already enrolled their child in a primary school by the time of the preschool meeting. Because the reduced-form analysis distinguishes between already-enrolled and not-yet-enrolled families, and because the structural estimation uses only the latter, it is important to verify that enrollment status at baseline reflects meeting timing rather than systematic differences between the two groups.

The meetings were conducted over a 16-week window between August and December 2010. Figure O-4 shows that the share of families reporting prior enrollment rises sharply with the calendar date of the meeting: roughly 20% in September and 65% by November. The pattern is similar across treatment and control groups, consistent with enrollment status being driven by when each preschool's meeting was scheduled rather than by family characteristics.

Table O-5 compares observables across enrolled and not-yet-enrolled families. The two groups are balanced on mother's education, poverty status, household size, and birth outcomes. Enrolled families report somewhat more durable goods and are marginally more likely to have had a hospital birth, but the differences are small.

FIGURE O-4.—Timing and Probability of Matriculation



## O-5. Balance Tables and Summary Statistics

TABLE O-4  
TREATMENT BALANCE AT THE SCHOOL LEVEL

	Difference T-C		Control Group Mean	
	(1)		(2)	
Enrollment	-1.898	(3.248)	41.467	(2.530)
Mean attendance	-1.053	(2.430)	28.689	(1.903)
Mother HE	-0.643	(1.552)	9.495	(1.320)
Mother HS	-0.915	(2.195)	48.347	(1.652)
Mother NHS	0.760	(1.010)	7.309	(0.697)
Q1 Income	0.577	(2.996)	57.970	(2.348)
Q2 Income	0.288	(2.142)	31.365	(1.587)
Q3 Income	-1.136	(1.216)	8.752	(0.930)
Very Poor	0.637	(1.865)	14.947	(1.406)
Poor	0.083	(2.233)	40.619	(1.816)

Notes: Column 1 presents the coefficient and standard error for the difference between the treatment and control groups in a regression of each variable on treatment status. Column 2 presents the coefficient and standard error for the control group mean. \* p-value<10% \*\* p-value<5% \*\*\* p-value<1%.

TABLE O-5  
BALANCE FOR BEING ENROLLED AT BASELINE

	Difference Enrolled - Non-Enrolled		Non-Enrolled Group Mean	
	(1)		(2)	
<i>Panel A: SES characteristics</i>				
Household size	-0.035	(0.108)	4.927	(0.081)
Durable goods	0.382***	(0.116)	4.461	(0.080)
Owns Dwelling	0.047	(0.031)	0.343	(0.019)
Mother head of hh	0.001	(0.030)	0.832	(0.014)
Mother NHS	-0.010	(0.022)	0.192	(0.016)
Mother HS	-0.039	(0.025)	0.391	(0.019)
Mother HE	0.007	(0.019)	0.836	(0.016)
Poor	-0.013	(0.016)	0.895	(0.011)
Another child in primary	0.010	(0.029)	0.405	(0.018)
<i>Panel B: Birth characteristics</i>				
Gestation Weeks	-0.019	(0.094)	38.751	(0.056)
Birth Weight	-3.982	(25.338)	3,342.137	(15.176)
Mother's Age	0.329	(0.364)	25.332	(0.232)
Father's Age	-1.646	(1.217)	36.472	(0.933)
Marital Status	-0.021	(0.023)	1.735	(0.014)
Doctor	-0.011	(0.024)	0.333	(0.015)
Hospital	0.014*	(0.008)	0.959	(0.006)
Number of Children	0.102	(0.087)	1.871	(0.035)

Notes: Column 1 presents the coefficient for the difference between households enrolled and non-enrolled at baseline in a regression of each variable on an indicator for being enrolled at baseline. Column 2 presents the coefficient and standard errors for the non-enrolled group mean. Regressions include the observations for which there is data on baseline enrollment (N=1,612). \* p-value<10% \*\* p-value<5% \*\*\* p-value<1%.

TABLE O-6  
TREATMENT BALANCE AT THE FAMILY LEVEL

	Full Sample (N=1,832)		Enrolled (N=606)		Non-Enrolled (N=1,006)	
	Difference T-C (1)	Control Mean (2)	Difference T-C (3)	Control Mean (4)	Difference T-C (5)	Control Mean (6)
<i>Panel A: SES characteristics</i>						
Household size	0.114 (0.126)	4.837 (0.101)	0.060 (0.172)	4.852 (0.144)	0.146 (0.158)	4.829 (0.118)
Durable goods	0.069 (0.179)	4.558 (0.160)	0.221 (0.259)	4.694 (0.231)	-0.023 (0.182)	4.477 (0.159)
Owens Dwelling	0.022 (0.039)	0.345 (0.032)	0.116** (0.058)	0.311 (0.046)	-0.035 (0.042)	0.366 (0.036)
Mother head of hh	0.010 (0.027)	0.826 (0.021)	-0.035 (0.047)	0.857 (0.029)	0.037 (0.031)	0.807 (0.025)
Mother $\leq$ Basic Inc.	-0.022 (0.031)	0.203 (0.026)	-0.017 (0.046)	0.194 (0.038)	-0.025 (0.034)	0.209 (0.029)
Mother $\leq$ Basic	-0.051 (0.039)	0.411 (0.032)	-0.038 (0.057)	0.378 (0.046)	-0.059 (0.040)	0.431 (0.033)
Mother $\leq$ HS	0.029 (0.028)	0.820 (0.022)	0.041 (0.039)	0.816 (0.032)	0.022 (0.032)	0.822 (0.025)
<i>Panel B: Baseline school choice</i>						
Already enrolled	0.002 (0.048)	0.375 (0.038)	-	-	-	-
Another child	-0.012 (0.032)	0.416 (0.024)	-0.008 (0.051)	0.420 (0.039)	-0.015 (0.037)	0.414 (0.029)
<i>Panel C: School choice 2011</i>						
Will apply on 2011	-0.015 (0.041)	0.759 (0.033)	0.061 (0.069)	0.776 (0.062)	-0.069 (0.045)	0.743 (0.033)

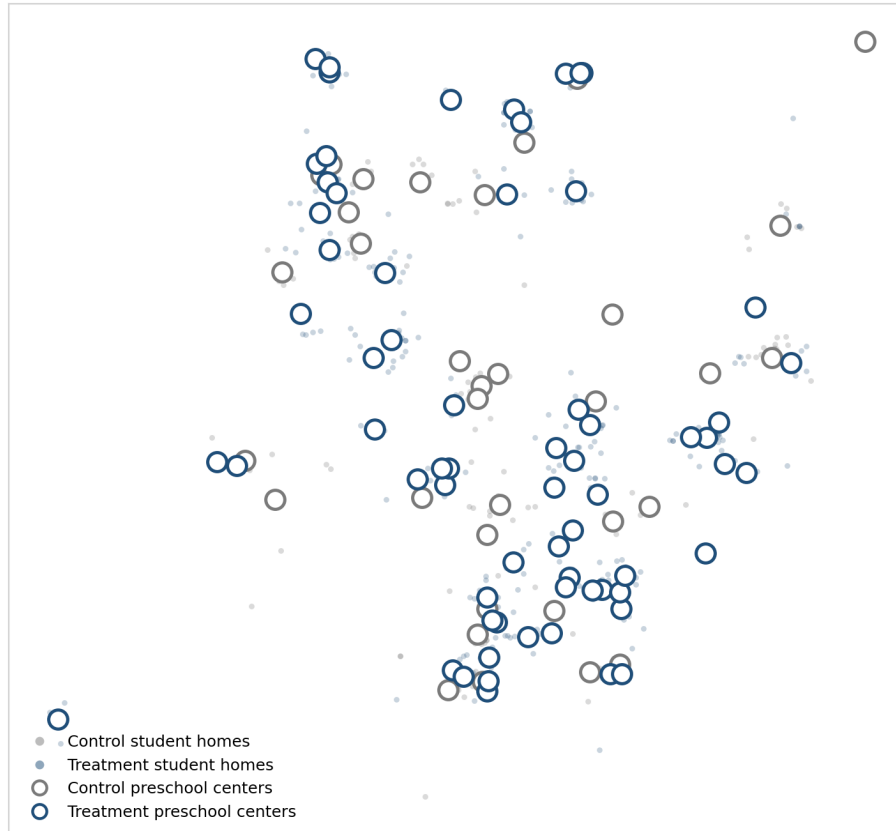
*Notes:* Odd columns report the treatment–control difference and its standard error from a regression of each variable on treatment status. Even columns report the control group mean and its standard error. Panels split by enrollment status at the time of the intervention; 1,612 observations have non-missing enrollment status. Mother’s education indicators are cumulative: “ $\leq$  Basic Inc.” = at most incomplete basic education; “ $\leq$  Basic” = at most completed basic education; “ $\leq$  HS” = at most completed high school. These are nested indicators, not mutually exclusive categories. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

TABLE O-7  
SUMMARY STATISTICS FOR ESTIMATION DATA

	Mean	Std. Dev.	10th pctl.	90th pctl.
<i>Panel A: School characteristics</i>				
Quality (value-added, s.d.)	0.097	0.581	-0.653	0.869
First-grade enrollment	46.3	35.4	13	88
Monthly copayment (CLP)	779	1,766	0	3,182
Share priority students	0.441	0.249	0.062	0.750
Public school	0.345			
Private voucher school	0.549			
Private unsubsidized school	0.106			
SEP eligible	0.335			
<i>Panel B: Market structure</i>				
School-year observations			14,378	
Unique schools			3,842	
Markets			53	
Years		4 (2006, 2007, 2011, 2012)		
Residential nodes			3,392	
Schools per market-year	67.8	208.1	9	94

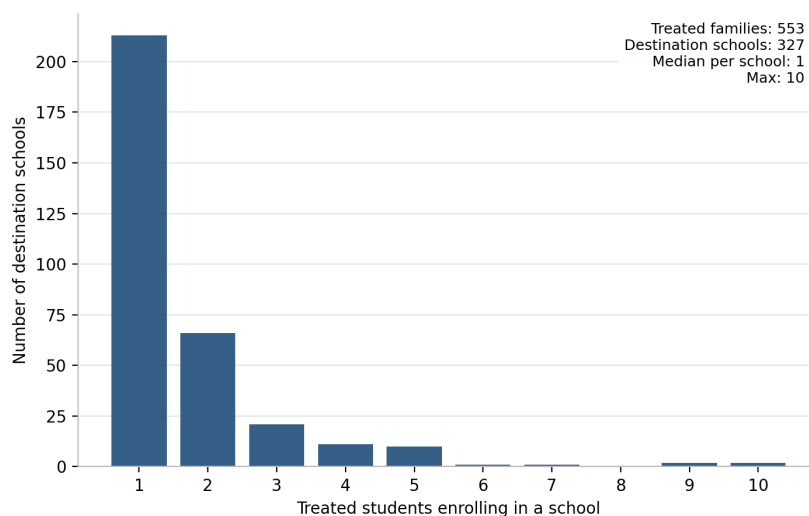
*Notes:* Panel A reports school-level characteristics for the baseline demand estimation sample (2006, 2007, 2011, 2012). Quality is the school-level value-added measure described in Section 3. Monthly copayment is the out-of-pocket tuition charged by schools; public and most voucher schools charge zero. Panel B summarizes market structure. The supply estimation uses a 10-year panel (2006–2015) with the same 53 markets. Family-level characteristics for the experimental sample are reported in the balance tables above.

FIGURE O-5.—Treatment and Control Preschool Centers and Student Homes in Santiago



*Notes:* Small dots denote geocoded student homes in the experimental sample. Larger hollow circles denote preschool-center centroids, computed from the homes of children attending each center; colors distinguish treatment and control status. In the 2011 market data, Santiago contains 75,396 first-grade students. The experimental sample in this market includes 670 students, of whom 448 are treated.

FIGURE O-6.—Treated Students per Destination School



### O-6. *Reduced-Form Results Without Family Controls*

Tables O-8 and O-9 replicate the main treatment-effect regressions (Tables 1 and 2) without the mother-education family controls used in the preferred specification. The point estimates are very similar, confirming that the randomization is well balanced and the main conclusions do not depend on the inclusion of family controls. The only notable difference is that the already-enrolled subgroup (Panel B) shows larger and statistically significant PAES college entrance exam effects without controls, consistent with the interpretation in the main text that some of the Panel B association reflects pre-existing compositional differences rather than a causal treatment effect.

TABLE O-8  
EFFECTS ON CHARACTERISTICS OF SCHOOLS CHOSEN (WITHOUT FAMILY CONTROLS)

	Distance (kms) (1)	Positive Price (2)	Lang 2nd (3)	Lang 4th (4)	Math 4th (5)	V. Added <sup>a</sup> (6)
<b>Panel A: Full Sample</b>						
Treatment	0.088 (0.061)	0.049 (0.036)	0.004 (0.023)	0.006 (0.028)	0.014 (0.029)	0.027 (0.032)
Control mean	1.081	0.494	-0.119	-0.095	-0.082	-0.024
N obs.	1,427	1,701	1,693	1,692	1,692	1,701
<b>Panel B: Already enrolled at the time of the PreK visit</b>						
Treatment	-0.147 (0.132)	-0.001 (0.055)	-0.028 (0.044)	-0.023 (0.051)	-0.044 (0.061)	-0.031 (0.062)
Control mean	1.192	0.589	-0.031	-0.007	0.046	0.067
N obs.	512	578	576	577	577	578
<b>Panel C: Not enrolled at the time of the PreK visit</b>						
Treatment	0.205*** (0.069)	0.125*** (0.041)	0.051* (0.028)	0.039 (0.031)	0.078** (0.034)	0.094** (0.038)
Control mean	1.010	0.434	-0.179	-0.137	-0.146	-0.067
N obs.	800	934	928	926	926	934

*Notes:* Same specification as Table I but without family controls. Standard errors in parentheses are clustered at the preschool-center level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

TABLE O-9  
EFFECTS ON LONG-TERM STUDENT OUTCOMES (WITHOUT FAMILY CONTROLS)

	SIMCE Test Scores				Grad. HS (5)	College Entrance Exam (PAES)		
	4th Grade		8th Grade			Took PAES (6)	(Cond. on taking PAES)	
	Lang (1)	Math (2)	Lang (3)	Math (4)			Lang (7)	Math (8)
<b>Panel A: Full Sample</b>								
Treatment	0.080 (0.057)	0.108** (0.054)	-0.003 (0.081)	0.208** (0.102)	-0.008 (0.035)	0.024 (0.035)	0.176** (0.072)	0.145*** (0.056)
Control mean	-0.222	-0.145	-0.059	-0.103	0.728	0.592	-0.367	-0.225
N obs.	1,583	1,584	610	615	1,775	1,775	1,083	1,060
<b>Panel B: Already enrolled at the time of the PreK visit</b>								
Treatment	-0.078 (0.117)	-0.085 (0.097)	-0.190 (0.228)	0.364** (0.171)	-0.043 (0.054)	0.008 (0.060)	0.242** (0.110)	0.296*** (0.094)
Control mean	-0.039	0.058	0.169	0.044	0.770	0.613	-0.287	-0.145
N obs.	543	533	241	243	596	596	373	364
<b>Panel C: Not enrolled at the time of the PreK visit</b>								
Treatment	0.218*** (0.079)	0.202*** (0.066)	0.090 (0.121)	0.207* (0.112)	0.003 (0.042)	0.027 (0.037)	0.195** (0.083)	0.150** (0.068)
Control mean	-0.339	-0.252	-0.188	-0.162	0.706	0.579	-0.447	-0.284
N obs.	857	865	301	301	975	975	581	568

Notes: Same specification as Table II but without family controls. Standard errors in parentheses are clustered at the preschool-center level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

### O-7. Estimating School Value Added

Our main measure of school quality is a school-level value-added estimate that captures the school's contribution to student achievement after conditioning on observable student characteristics. Following Neilson (2026), we estimate

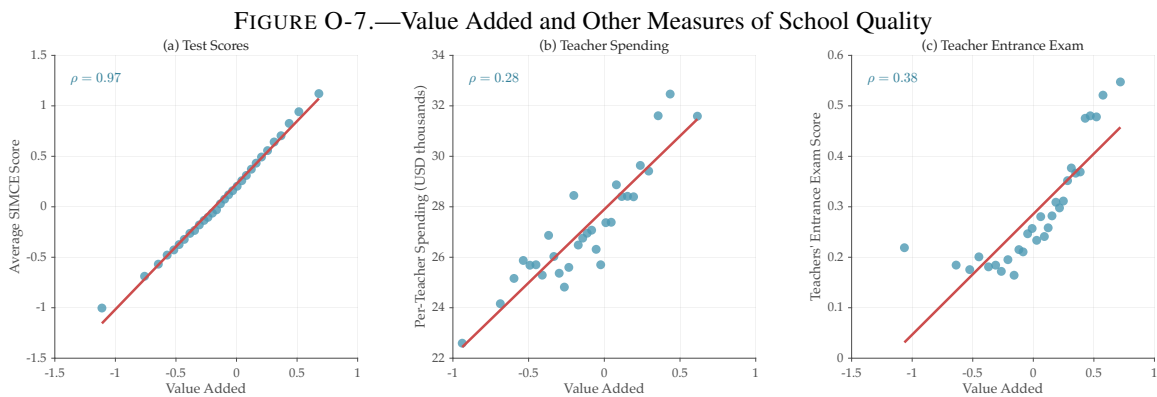
$$Y_{ist} = X'_{ist}\beta + \delta_{jt} + \varepsilon_{ist},$$

where  $Y_{ist}$  is the standardized test score of student  $i$  at school  $j$  in year  $t$ ,  $X_{ist}$  is a vector of student characteristics (including gender, age, SEP eligibility, and birth outcomes from administrative health records), and  $\delta_{jt}$  is a school-by-year fixed effect. The value-added

measure  $q_j$  is the average of the estimated  $\hat{\delta}_{jt}$  across years, normalized to have mean zero and unit standard deviation across schools. We average over multiple years to reduce noise and obtain a more stable measure of persistent school quality. The controls in  $X_{ist}$  do not include lagged test scores, since many students in our sample are entering first grade and have no prior score. Neilson (2026) shows that value-added rankings are very similar whether or not prior scores are included, conditional on the rich set of student background characteristics available in Chilean administrative data.

### O-7.1. Correlations between value added and other measures of school quality

Figure O-7 provides descriptive evidence on these correlations. Panel (a) plots school-level value-added against raw SIMCE test scores—the quality measure displayed in the report cards given to treated families. The two measures are strongly positively correlated, so the information parents received through the intervention is closely aligned with the value-added measure used in the structural model. Panel (b) shows that schools with higher value-added spend more per teacher on salaries. Panel (c) links value-added to teachers’ scores on Chile’s national university entrance exam (PSU, now called PAES), providing external validation by connecting a measure of school output to a measure of teacher quality that is entirely independent of student test scores.



*Notes:* Each dot is a bin average (30 bins) computed from school-level means pooled across years. The horizontal axis in all panels plots school-level value-added estimated from the model described in Section O-7. Panel (a): raw SIMCE test scores (average of Math and Reading), the quality measure displayed in the report cards given to treated families. Panel (b): per-teacher expenditure on salaries (USD thousands, trimmed at the 99th percentile). Panel (c): average score on Chile’s national university entrance exam (PSU, now PAES) of the school’s teachers. Lines show linear fits;  $\rho$  denotes the school-level Pearson correlation computed on the underlying (non-binned) data.

O-8. *Additional Supply-Side Results*TABLE O-10  
SUPPLY MODEL ESTIMATES

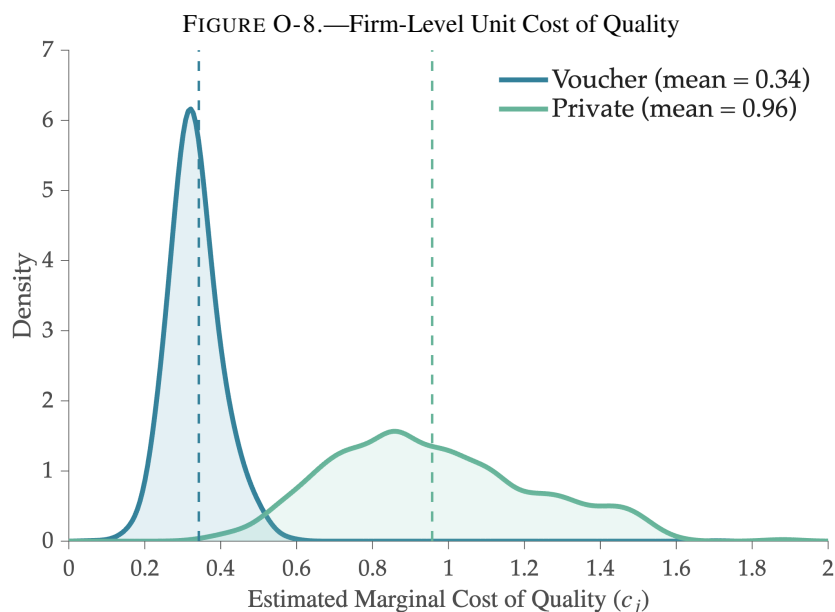
	Estimate	SE	<i>t</i> -stat
$\gamma_{\text{SEP}}$ — SEP cost shifter	0.212	0.032	6.58
<i>Unit cost of quality (<math>c_j</math>)</i>			
Voucher schools (mean)	0.349		
Private schools (mean)	1.103		
<i>Type-specific time trends (<math>\delta_{\text{type}} \cdot t</math>)</i>			
$\delta_{\text{voucher}}$	0.006	0.004	1.82
$\delta_{\text{private}}$	-0.046	0.028	-1.63
<i>Capacity constraint regime</i>			
Constrained obs. (of 22,882)	2,414 (10.5%)		
$\Omega$ std (unconstrained)	1.200		
$\Omega$ std (constrained)	0.167		

*Notes:* Two-regime GMM estimates of the quality first-order conditions. Sample: voucher and private non-voucher schools, 2006–2015, 53 markets (22,882 school-year observations, 2,389 schools). Market-year-type fixed effects absorb common cost shocks within each school type and local market.  $\gamma_{\text{SEP}}$ : coefficient on SEP program participation. School-specific costs  $c_j$  are concentrated out analytically; means reported by school type. Type-specific time trends allow secular changes in cost structures across voucher and private schools. Instruments: demand-side exclusion restrictions interacted with school type, plus a Nearby Teacher Quality Index (Table O-11). Constrained observations: school-years with enrollment-to-capacity ratio  $\geq 0.95$  and SEP differential revenue (post-2008).  $\Omega$ : structural residual.

TABLE O-11  
FIRST-STAGE REGRESSION FOR THE COMPETITIVE QUALITY RESIDUAL

	Quality
<i>Exogenous School Characteristics</i>	
Private Non-Voucher	1.435 (0.974)
For Profit	1.174* (0.635)
Religious	0.083 (0.057)
SEP School	-0.018 (0.017)
Religious $\times$ Voucher	-0.778 (0.505)
<i>Demand-Side Instruments</i>	
Transfer changes, SEP exposure, nearby school characteristics	Yes
<i>Cost-Side Instrument</i>	
Nearby Teacher Quality Index	-0.720*** (0.124)
Market-Year FE	Yes
$R^2$ (within)	0.234
F-stat, excluded instruments	56.8
F-stat, Nearby Teacher Quality Index (partial)	32.6
N Obs	22,882
N Market-Years	530

*Notes:* First-stage regression of the competitive quality residual implied by the unconstrained supply FOC,  $\text{CompQ}_{jt} = q_{jt} + \text{MD}_{jt}$ , on instruments. The Nearby Teacher Quality Index is the inverse-distance-weighted average of public school teacher PSU scores within each market. Demand-side instruments include transfer changes, SEP exposure, and nearby school characteristics interacted with school type. Standard errors clustered at the school level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .



Notes: Distribution of the school-specific unit cost of quality  $c_j = \gamma_q + \omega_j$  for voucher and private non-voucher schools. The  $x$ -axis is truncated at  $c_j = 2$ , which covers 99.96% of schools. See Table O-10 for full parameter estimates.

### O-9. Unrestricted Demand Estimation

Table O-12 reports treatment-effect estimates from the unrestricted specification that also estimates the price treatment effect for SEP families ( $\psi_{\text{SEP}}^{\text{op}}$ ). As discussed in Table IV, SEP-eligible families face zero out-of-pocket prices at SEP schools, so this parameter is poorly identified from the experimental variation. The unrestricted estimate is  $\hat{\psi}_{\text{SEP}}^{\text{op}} = 0.369$  (SE = 0.653), far from significant. All other treatment parameters are qualitatively unchanged: the quality treatment effect for SEP families is 0.476 (SE = 0.242) versus 0.500 (SE = 0.230) in the restricted specification, and the kappa parameters are similar (1.521 vs. 1.410 for SEP). The GMM objective changes minimally ( $Q = 2.637$  unrestricted vs.  $Q = 2.922$  restricted), confirming that the restriction is empirically innocuous.

TABLE O-12  
UNRESTRICTED DEMAND ESTIMATES (TREATMENT EFFECTS)

	Quality ( $q$ )		Price (op)		Distance ( $d$ )	
	Coef.	(SE)	Coef.	(SE)	Coef.	(SE)
$\psi_{\text{SEP}}^c$	0.476**	(0.242)	0.369	(0.653)	0.500*	(0.258)
$\psi_{\text{nonSEP}}^c$	0.331	(0.329)	-0.147**	(0.072)	0.100	(0.290)
Scale parameter ( $H_0: \kappa = 1$ )						
$\kappa_{\text{SEP}}$			1.521	(0.379)		
$\kappa_{\text{nonSEP}}$			1.361	(0.564)		
GMM Objective: $Q = 2.637$						

*Notes:* This table reports treatment-effect parameters from the unrestricted specification that freely estimates  $\psi_{\text{SEP}}^{\text{op}}$ . Baseline demand parameters (Panel A of Table IV) are unchanged. Standard errors clustered at the preschool-center level. \*\*\*, \*\*, \*: significant at 1%, 5%, 10%.

### O-10. Counterfactual Robustness

Table O-13 reports mean expected quality under alternative counterfactual scenarios. Panel A repeats the baseline from Table VI for reference.

Panels B and C restrict the information treatment to SEP-eligible families only (types 1, 3) or non-SEP families only (types 2, 4), holding the other group untreated. These scenarios are informative about targeted implementations of the policy. Under free choice (demand only), the untreated group is mechanically unaffected: non-SEP families remain at 0.512 in Panel B, and SEP families remain at 0.298 in Panel C. Under DA, the untreated group experiences a small *negative* displacement effect as treated families crowd into higher-quality schools (non-SEP families fall to 0.487 in Panel B; SEP families fall to 0.272 in Panel C). Crucially, the supply response more than reverses this displacement: in Panel B, untreated non-SEP families gain +0.043 SD relative to baseline under DA + Supply, and in Panel C, untreated SEP families gain +0.031 SD. These spillovers arise because the quality improvements that schools make in response to the treated group's demand shift are non-excludable—they benefit all students attending the school, not just those who received the information.

Panel D relaxes the assumption that public schools do not respond to competitive incentives, allowing all schools—including public—to adjust quality via the supply FOC.

This amplifies the equilibrium treatment effect substantially (from +0.158 to +0.208 for all families), reflecting the large share of public schools in the market.

Panels E and F explore partial take-up ( $\tau = 0.75$  and  $\tau = 0.50$ ), where only a fraction of eligible families within each type receive the treatment. At  $\tau = 0.50$ , the demand-only free-choice effect roughly halves relative to Panel A (e.g., +0.064 vs. +0.122 for all families), as expected. However, the DA + Supply effect retains approximately 60% of the full-treatment magnitude rather than 50%: +0.095 vs. +0.158 for all families. The non-linearity arises from the same spillover mechanism as in Panels B–C: schools respond to the  $\tau$ -weighted demand shift by improving quality, and this improvement benefits *all* families—including the untreated fraction—creating a public-good-like externality. This interpretation is confirmed by the consistency between Panels B–C and Panel F: the average of the “All” treatment effects in Panels B (+0.099) and C (+0.085) under DA + Supply is +0.092, closely matching Panel F’s +0.095. Even at 50% take-up, the supply-inclusive equilibrium effect (+0.095) exceeds the demand-only capacity-constrained effect at full take-up (+0.061), indicating that supply-side responses are quantitatively meaningful even under incomplete program adoption.

TABLE O-13  
ROBUSTNESS OF COUNTERFACTUAL SIMULATIONS

	No Treatment	Treatment (Free Choice)	Treatment with Constraints (DA)	
			Capacity	Capacity + Supply
<i>Panel A: Baseline (all types 1–4 treated, <math>\tau = 1</math>)</i>				
All (types 1–6)	0.558	0.680	0.619	0.716
SEP (types 1, 3)	0.298	0.483	0.407	0.522
Non-SEP (types 2, 4)	0.512	0.656	0.588	0.692
<i>Panel B: SEP-Only Treatment (types 1, 3 treated)</i>				
All (types 1–6)	0.558	0.621	0.595	0.657
SEP (types 1, 3)	0.298	0.483	0.442	0.517
Non-SEP (types 2, 4)	0.512	0.512	0.487	0.555
<i>Panel C: Non-SEP-Only Treatment (types 2, 4 treated)</i>				
All (types 1–6)	0.558	0.616	0.588	0.643
SEP (types 1, 3)	0.298	0.298	0.272	0.329
Non-SEP (types 2, 4)	0.512	0.656	0.618	0.678
<i>Panel D: Public Schools Also Respond</i>				
All (types 1–6)	0.558	0.680	0.619	0.766
SEP (types 1, 3)	0.298	0.483	0.407	0.586
Non-SEP (types 2, 4)	0.512	0.656	0.588	0.731
<i>Panel E: Partial Take-Up (<math>\tau = 0.75</math>)</i>				
All (types 1–6)	0.558	0.651	0.607	0.689
SEP (types 1, 3)	0.298	0.440	0.385	0.480
Non-SEP (types 2, 4)	0.512	0.622	0.573	0.657
<i>Panel F: Partial Take-Up (<math>\tau = 0.50</math>)</i>				
All (types 1–6)	0.558	0.622	0.594	0.653
SEP (types 1, 3)	0.298	0.396	0.361	0.431
Non-SEP (types 2, 4)	0.512	0.588	0.556	0.617

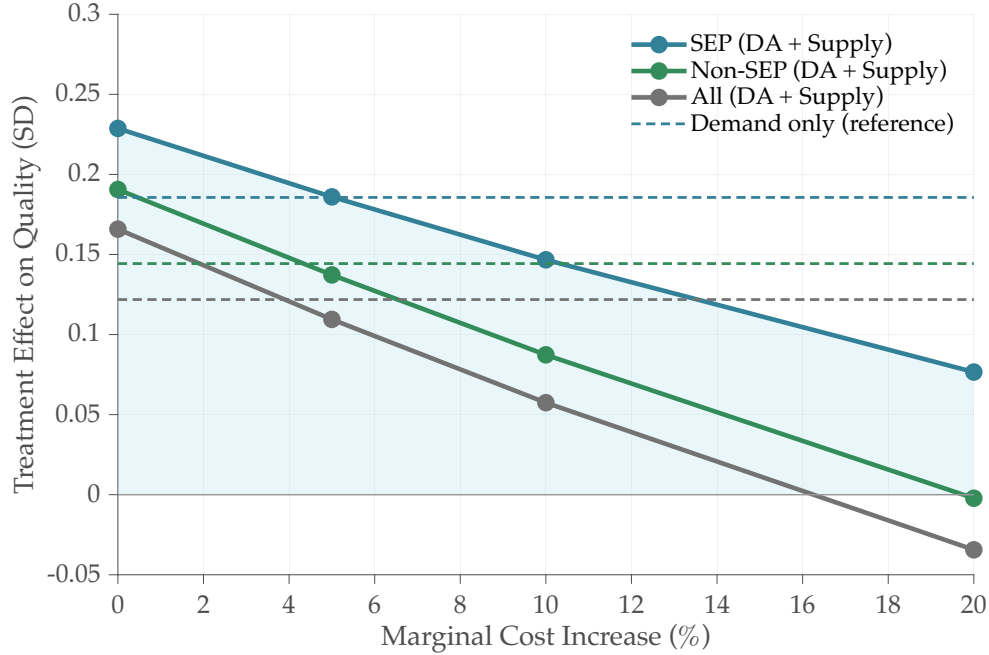
*Notes:* Each panel reports mean expected school quality (in SD of normalized value added) under the four scenarios in columns. Panel A: baseline with all types 1–4 treated at full take-up ( $\tau = 1$ ). Panels B–C: only SEP or only non-SEP families treated. Panel D: public schools also adjust quality via the supply FOC. Panels E–F: partial take-up ( $\tau = 0.75$  and  $\tau = 0.50$ ). Capacities and the DA mechanism are identical across panels.

*O-11. Sensitivity to Input Cost Increases*

The equilibrium counterfactuals in the main text hold the unit cost of quality  $c_j$  fixed at estimated values. If the information policy were implemented at scale and many schools simultaneously tried to improve quality, increased demand for inputs (e.g., skilled teachers) could put upward pressure on costs (Tincani, 2020). Figure O-9 explores this possibility by re-solving the DA equilibrium with supply response for cost increases of 0%, 5%, 10%, and 20%.

Figure O-9 plots the equilibrium treatment effect under DA with capacity constraints and supply response—the same object reported in Table VI—as a function of the assumed cost increase. The dashed horizontal lines show the demand-only treatment effect (no supply response) for reference. At a 5% cost increase, the equilibrium treatment effect for SEP families remains well above the demand-only benchmark. At 10%, the equilibrium effect is attenuated but still positive and economically meaningful. At 20%, the effect for all families turns slightly negative, though the SEP-specific effect remains positive. These results suggest that the qualitative conclusions of the equilibrium analysis are robust to empirically plausible magnitudes of input cost pressure.

FIGURE O-9.—Equilibrium Treatment Effect under Rising Input Costs



Notes: Solid lines with markers show the equilibrium treatment effect on expected school quality under DA with capacity constraints and supply response, as the unit cost of quality  $c_j$  is uniformly scaled upward. This matches the preferred specification in Table VI. Dashed lines show the demand-only treatment effect (no supply response) for each group. Cost scales: 0%, 5%, 10%, 20%. Public schools held fixed in all scenarios.

### O-12. Scale-Up RCT Design Details

This section supplements Section 7 with the full design-space search and sensitivity to take-up and buffer width. All tables use the school-level regression on the equilibrium quality change  $\Delta Q_j$  for voucher and private schools, with standard errors clustered at the hexagon level and random hex grid origins, matching the main-text specification.

#### O-12.1. Full Design Space

Table O-14 reports RMSE and power across the full  $(r, I)$  grid at  $\tau = 0.25$  take-up with no buffer ( $B = 0$ ), corresponding to the main-text figure. RMSE decreases with hex radius  $r$  (less boundary contamination) and is relatively flat in core radius  $I$ , while power increases strongly with  $I$  (more core schools in the regression).

TABLE O-14  
 RMSE AND POWER: EQUILIBRIUM QUALITY ( $\tau = 25\%$ ,  $B = 0$ )

<i>RMSE: Phase 2 Supply (<math>B = 0.0</math> km, Santiago, 50% take-up)</i>						
$r \setminus I$	0.3	0.5	0.7	1.0	1.5	2.0
0.8	0.0175	0.0130	0.0123	–	–	–
1.0	0.0173	0.0134	0.0125	0.0122	–	–
1.5	0.0316	0.0142	0.0133	0.0126	0.0123	–
2.0	0.0470	0.0190	0.0166	0.0154	0.0139	0.0132
2.5	0.0336	0.0312	0.0269	0.0223	0.0214	0.0172
3.0	0.0490	0.0309	0.0269	0.0215	0.0214	0.0214
<i>RMSE: Phase 2 Supply (<math>B = 0.3</math> km, Santiago, 50% take-up)</i>						
$r \setminus I$	0.3	0.5	0.7	1.0	1.5	2.0
0.8	0.0209	–	–	–	–	–
1.0	0.0193	0.0178	0.0164	–	–	–
1.5	0.0284	0.0140	0.0128	0.0147	–	–
2.0	0.0448	0.0169	0.0155	0.0143	0.0132	–
2.5	0.0327	0.0299	0.0256	0.0212	0.0197	0.0159
3.0	0.0447	0.0294	0.0261	0.0212	0.0208	0.0202
<i>RMSE: Phase 2 Supply (<math>B = 0.5</math> km, Santiago, 50% take-up)</i>						
$r \setminus I$	0.3	0.5	0.7	1.0	1.5	2.0
0.8	–	–	–	–	–	–
1.0	0.0221	0.0227	–	–	–	–
1.5	0.0243	0.0162	0.0153	0.0179	–	–
2.0	0.0401	0.0163	0.0139	0.0137	0.0132	–
2.5	0.0304	0.0262	0.0221	0.0180	0.0170	0.0141
3.0	0.0453	0.0272	0.0241	0.0188	0.0175	0.0168

Notes: Simulation-based RMSE and power for the school-level equilibrium quality regression.  $\beta^* = 0.054$  s.d., Santiago,  $B = 0$ . Each cell averages over 20 parameter draws  $\times$  50 hex-assignment draws with random grid origins. Cells marked “–” are infeasible ( $I > r$ ).

Table O-15 shows how a buffer strip of  $B = 0.3$  km affects the results. The buffer modestly reduces RMSE at some configurations but also reduces power by excluding schools near hex boundaries from the regression.

TABLE O-15  
 RMSE AND POWER: EQUILIBRIUM QUALITY ( $\tau = 25\%$ ,  $B = 0.3$  KM)

<i>RMSE: Supply equilibrium, school-level (<math>B = 0.3</math>, <math>\tau = 25\%</math>, <math>\beta^* = 0.054</math>)</i>						
$r \setminus I$	0.3	0.5	0.7	1.0	1.5	2.0
0.8	0.047	–	–	–	–	–
1.0	0.043	0.043	0.044	–	–	–
1.5	0.032	0.033	0.033	0.036	–	–
2.0	0.030	0.026	0.026	0.027	0.031	–
2.5	0.032	0.022	0.022	0.022	0.024	0.028
3.0	0.031	0.020	0.020	0.019	0.021	0.023
<i>Power (%)</i>						
$r \setminus I$	0.3	0.5	0.7	1.0	1.5	2.0
0.8	45	–	–	–	–	–
1.0	59	77	88	–	–	–
1.5	80	85	92	95	–	–
2.0	76	85	92	95	99	–
2.5	73	87	91	95	99	100
3.0	71	91	92	94	99	100

*Notes:* Same specification as Table O-14 but with buffer strip  $B = 0.3$  km.

### O-12.2. Sensitivity to Take-Up Rate

The main text uses  $\tau = 0.25$  take-up. Table O-16 reports results at  $\tau = 0.50$ , which doubles the estimand ( $\beta^* = 0.104$  s.d.). Power is uniformly higher, reaching 95–100% for most configurations with  $I \geq 0.7$ , and RMSE is correspondingly larger in absolute terms. The bias–power trade-off is less binding at higher take-up because the larger signal makes detection easier across the grid.

TABLE O-16  
 RMSE AND POWER: EQUILIBRIUM QUALITY ( $\tau = 50\%$ ,  $B = 0$ )

<i>RMSE: Supply equilibrium, school-level (<math>B = 0</math>, <math>\tau = 50\%</math>, <math>\beta^* = 0.104</math>)</i>						
$r \setminus I$	0.3	0.5	0.7	1.0	1.5	2.0
0.8	0.083	0.084	0.086	–	–	–
1.0	0.071	0.075	0.077	0.080	–	–
1.5	0.055	0.058	0.060	0.064	0.070	–
2.0	0.048	0.047	0.049	0.051	0.057	0.063
2.5	0.052	0.039	0.040	0.042	0.046	0.053
3.0	0.055	0.036	0.035	0.036	0.039	0.043
<i>Power (%)</i>						
$r \setminus I$	0.3	0.5	0.7	1.0	1.5	2.0
0.8	79	96	98	–	–	–
1.0	84	98	99	100	–	–
1.5	87	97	99	100	100	–
2.0	84	95	99	100	100	100
2.5	74	96	98	100	100	100
3.0	72	95	97	99	100	100

*Notes:* Same specification as Table O-14 but with  $\tau = 0.50$  take-up.  $\beta^* = 0.104$  s.d.

### O-12.3. Demand-Only Outcome

For comparison, Table O-17 reports RMSE and power for the demand-only outcome (population-weighted average expected school quality, hex-level regression, no supply adjustment). Because the demand-only estimand is larger ( $\beta^* = 0.051$  s.d. at  $\tau = 0.50$ ), power exceeds 95% for nearly all configurations and the bias–power trade-off is not binding. This confirms that the equilibrium quality outcome, which requires detecting the supply response, is the more demanding design target.

TABLE O-17

RMSE AND POWER: DEMAND ONLY (HEX-LEVEL,  $\tau = 50\%$ ,  $B = 0$ )

<i>RMSE: Phase 1 Demand (<math>E[Q]</math>, <math>B = 0</math>, Santiago, 50% take-up)</i>						
$r \setminus I$	0.3	0.5	0.7	1.0	1.5	2.0
0.8	0.0328	0.0283	0.0269	–	–	–
1.0	0.0322	0.0272	0.0268	0.0255	–	–
1.5	0.0439	0.0285	0.0282	0.0239	0.0225	–
2.0	0.0492	0.0289	0.0276	0.0253	0.0252	0.0243
2.5	0.0324	0.0359	0.0294	0.0268	0.0270	0.0246
3.0	0.0494	0.0323	0.0292	0.0244	0.0247	0.0273

Notes: Hex-level regression on population-weighted average expected school quality.  $\beta^* = 0.051$  s.d.,  $B = 0$ ,  $\tau = 0.50$ . HC1 standard errors.

### O-13. Data Construction

#### O-13.1. Data Usage Across Empirical Exercises

Because the paper combines an experiment with pre-existing structural estimates and a separate school panel, different empirical objects use different samples and years.

- **Reduced-form experiment.** The information intervention was implemented in the second half of 2010. We measure its effect on school choice using the experimental cohort observed enrolling in first grade in 2011.
- **Baseline demand.** The first-step demand parameters are inherited from Neilson (2026), who estimates the baseline choice model using administrative first-grade choice data from 2006, 2007, 2011, and 2012 across 53 urban markets.
- **Treatment-effect estimation.** The second-step structural estimation uses the subset of experimental families that had not yet enrolled at the time of treatment and that can be matched to the six overlap markets represented in the baseline demand model.
- **Supply estimation.** The supply-side moments are constructed from the 53-market urban school-year panel for 2006–2015 used in the structural estimation.
- **Counterfactuals.** The main policy simulations are evaluated in the 2011 market environment, the cohort directly exposed to the intervention.

### O-13.2. *School Capacity Imputation*

The capacity-constrained counterfactual in Section 6.1 requires school-level enrollment capacities. Since these are not directly observed in our experimental data, we impute them using multiple administrative sources in the following priority order.

1. **SAE 2020 enrollment slots (public and voucher schools).** We match schools to Chile's 2020 Sistema de Admisión Escolar data by RBD code and use the reported total enrollment slots (*cupos totales*). Because SAE capacities reflect the 2020 physical plant, which may differ from the 2006–2015 estimation period, we adjust for changes in the number of classrooms. Specifically, we infer 2020 classrooms as  $\lceil \text{cupos totales} / 45 \rceil$  (45 being the legal class-size ceiling) and obtain estimation-period classrooms from administrative enrollment records (distinct first-grade sections per school per year). For schools where these differ, we scale the SAE capacity by the ratio of estimation-period to 2020 classrooms. This adjustment affects roughly 21% of SAE-matched observations. As an additional data-quality floor, observations where enrollment exceeds the adjusted capacity are set to enrollment. This yields matches for 94.5% of public schools and 89.0% of voucher schools in our sample.
2. **Administrative capacity records (private schools and unmatched).** For private schools and those not matched in step 1, we use an alternative capacity variable from a separate administrative file, matched by RBD. Private schools without a match are assigned a default capacity of 90.
3. **Maximum observed enrollment (remaining unmatched).** Schools still without a capacity estimate are assigned the maximum enrollment observed across years in the baseline data.
4. **Market median (last resort).** Any remaining schools receive the median capacity in their market.

A minimum capacity of 5 students is imposed to avoid degenerate markets. The resulting enrollment-to-capacity ratio across all schools has a mean of 0.73 and a median of 0.72, indicating that the imputed capacities produce meaningful but not unrealistic slack. The 10th and 90th percentiles are 0.66 and 0.80, respectively.