Childcare Before Age Three and Long-Term Academic Achievement: Evidence from a Lottery in Barcelona*

Laurenz Baertsch[†]
Antoni-Italo De Moragas[‡]
Gabriel Facchini[§]
Libertad González [¶]

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Abstract

We examine the long-term effects of early childcare using administrative data from Barcelona. In oversubscribed public childcare centers, seats are allocated by lottery among applicants with identical priority scores, generating random variation in access. We exploit this design to estimate the causal effect of admission to childcare before age three on standardized test scores measured at age 12. We find that admission reduces scores by 0.08 standard deviations. Since not all admitted children enroll, this is an intention-to-treat estimate. These negative impacts vary by subject and age at entry: language penalties are largest for children admitted at the youngest age, while mathematics effects emerge at older ages. Effects are negative for both high- and low-education families, and in areas with both high and low private childcare availability. The fact that effects persist even in areas where the counterfactual is predominantly home-based parental care rather than private centers suggests that, for marginal applicants, early childcare displaced developmentally richer home environments without providing equivalent stimulation.

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[†]Organization for Economic Cooperation and Development (OECD).
[‡]Department of Economics, CUNEF Universidad. antoni.demoragas@cunef.edu.
[§]Department of Economics at Royal Holloway, University of London.
[¶]Department of Economics and Business, Universitat Pompeu Fabra and Barcelona School of Economics.

1 Introduction

Governments worldwide are rapidly expanding public childcare for children under age three. The European Union's "Barcelona targets" set coverage benchmarks of 33% by 2010 and 45% by 2030; the EU average reached 39% by 2024, and many countries are still scaling up.[1] These expansions are motivated by influential research showing that early childhood is a critical period for skill formation and that investments during the first years of life yield high returns, particularly for disadvantaged children (Carneiro and Heckman, 2003; Cunha, Heckman and Schennach, 2010; Currie and Almond, 2011).

Yet the evidence base behind these expansions is thinner than it appears. Most credible causal studies focus on center-based care for children aged three and older (e.g., Berlinski, Galiani and Manacorda 2008; Havnes and Mogstad 2011), and the few studies examining care before age three produce mixed results that depend critically on context (Felfe, Nollenberger and Rodríguez-Planas, 2015; Fort, Ichino and Zanella, 2020; Drange and Havnes, 2019). In settings where the counterfactual is high-quality parental or private care, the developmental effects of public childcare may be small, null, or even negative. Whether expanding access to childcare for the youngest children improves their long-run outcomes is therefore an open empirical question—one with direct implications for the design of universal systems.

In this paper, we estimate the long-term causal effect of public childcare before age three on academic achievement at age 12, using administrative data from Barcelona. We exploit a setting with widespread demand for public early education but limited capacity, leading to oversubscribed municipal centers. Admission to these centers is determined by a centralized system with tie-breaking lotteries among applicants with identical priority scores, providing quasi-random variation in access to childcare.

An important condition for the viability of this approach—both in terms of ensuring statistical power and enhancing external validity—is that these lotteries are sufficiently widespread. In fact, 95% of applications list an oversubscribed center as their first choice. Moreover, the structure of the priority scoring system further reinforces the randomness of the assignment: 76% of applicants receive exactly 30 points (the score corresponding to applying to a nearby center with no additional priority, see Table 16), meaning that a large share of placements is effectively determined by lottery. Given this institutional framework, our identification strategy balances internal and external validity by studying lotteries that affect a large share of marginal applicants. Our estimates identify a local average treatment effect for marginal applicants with 30 points and may not extrapolate to disadvantaged children with additional priority points, such as those from very low-income families, single-parent households, or those with special needs. From a policy perspective, this is precisely the group most affected by incremental capacity expansions:

[1] See Figure 6 in the Appendix.

when additional seats are added, the first beneficiaries are the 30-point applicants currently facing lottery risk.

We find that access reduces later academic performance by approximately 0.08 standard deviations. The effects vary by subject and age at entry, with language penalties largest at younger ages and mathematics effects more pronounced at older ages. These findings persist across core subjects, are robust to controls for subsequent school type and peer environment, and are not driven by sorting into lower-quality primary schools. Importantly, we show that the negative effects are present in areas with both high and low private childcare availability, with no statistically significant difference between the two groups. This is inconsistent with a simple "public versus private childcare" interpretation and instead points to home care as a relevant counterfactual being displaced. Our results suggest that, for the marginal applicants in our sample, early childcare replaced developmentally rich home environments without delivering equivalent stimulation.

Our findings contribute to a growing literature emphasizing that the developmental effects of early childcare depend crucially on quality and on the counterfactual environment it replaces. In contexts where public systems operate under resource constraints and the marginal applicant already has access to relatively rich home or private care, expanding access without quality improvements can have unintended costs.

Conceptually, our study highlights the interaction between timing, quality, and family background in determining long-run returns to early childcare. The results are consistent with the notion that institutional care before age one may be too early if staff–child ratios are high and pedagogical content is limited, whereas later entry mitigates these risks as cognitive and socio-emotional skills become more resilient. We view the results not as evidence against early education per se, but as evidence of quality-sensitive returns to early childcare.

2 Related Literature

We contribute to a growing literature on the long-term effects of early childhood education and care (ECEC). A robust conclusion emerging from this literature is that high-quality early childcare generates larger benefits for children from disadvantaged backgrounds, while effects for more advantaged families are often modest, null, or even negative (Carta and Rizzica, 2018; Almond, Currie and Rossin-Slater, 2018; Felfe, Nollenberger and Rodríguez-Planas, 2015). This heterogeneity reflects, in large part, differences in counterfactual home environments: formal care often substitutes more enriching family settings for higher-income children.

The majority of existing empirical studies focus on education for children aged three and older, reflecting the design of public childcare and preschool systems in most countries and the relative ease of accessing administrative data at those ages. Notable examples

include universal programs in Norway and Argentina, which show substantial average effects on human capital and maternal labor supply (Havnes and Mogstad, 2011; Berlinski, Galiani and Manacorda, 2008). In contrast, credible causal evidence on the effects of childcare before age three remains scarce, despite increasing recognition from developmental neuroscience and economics that the earliest stages of life are especially formative for long-run outcomes (Heckman and Kautz, 2014; Black et al., 2016).

The few studies that examine effects on cognitive outcomes for early childcare at ages 0-2 produce mixed results that depend critically on context. Fort, Ichino and Zanella (2020) find adverse effects on IQ at ages 8–14 in Bologna for children from affluent families (average household income nearly twice the national average), while Drange and Havnes (2019) report positive effects on test scores at ages 6–7 in Oslo, concentrated among low-income children. Felfe and Lalive (2018) find modest positive effects on school readiness in Germany using an instrumental variables approach. This variation likely reflects differences in program quality and counterfactual care arrangements across contexts. Quality benchmarks from successful interventions—such as Perry Preschool and Abecedarian—maintained child-teacher ratios of 3:1 to 6:1 for children under three (Schweinhart et al., 2005; Campbell et al., 2012), substantially lower than the ratios common in universal systems. Research consistently shows that ratios exceeding 8:1 for infants are associated with lower quality interactions (Burchinal et al., 2000).

A related literature examines adult outcomes of early childcare, including labor market participation, earnings, crime, and health. Baker, Gruber and Milligan (2019) document negative long-run impacts of Quebec's universal childcare expansion on adult crime and health, while Black et al. (2014) find positive effects on adult earnings for disadvantaged children in Norway. These studies complement the cognitive outcome literature but measure different margins of impact and are not directly comparable to test score effects. Our focus on cognitive outcomes at age 12 allows a more direct comparison with the test score literature, summarized in Table 1.

An important question regarding early education programs is whether effects persist over time or fade out as children age. A substantial body of research documents that many programs show initial cognitive gains that diminish by elementary school. Currie and Thomas (1995) find that Head Start gains largely fade out by age 10-12 for most children, and Cascio and Staiger (2012) and Lipsey, Farran and Durkin (2018) document similar patterns for state pre-K programs. The mechanisms underlying fadeout remain debated, but may include dynamic complementarity in skill formation (Cunha and Heckman, 2007)—early gains may require continued high-quality inputs to be sustained.

Table 1 summarizes the key studies on early childcare effects on cognitive and educational outcomes, distinguishing between programs serving children under age three (Panel A) and preschool programs for ages three and older (Panel B). The comparison highlights two patterns. First, effects on test scores for exposure at ages 0–2 are mixed and

context-dependent: negative in settings with affluent samples (Bologna, Barcelona), but positive in more representative or disadvantaged samples (Norway, Germany). Second, preschool programs for ages 3+ tend to show consistently positive effects on cognitive outcomes across diverse contexts, including test scores (Berlinski, Galiani and Manacorda, 2008; Cascio and Schanzenbach, 2013) and years of education (Havnes and Mogstad, 2015). Our study contributes to the first set of studies with long-run estimates (age 12 test scores) from a lottery design in a universal system—filling a gap between studies of affluent families (Fort, Ichino and Zanella, 2020) and targeted interventions for disadvantaged children.

Table 1: Comparison with existing literature on early childcare effects

Study	Country	Ages	Outcome	Sample	Effect (SD)	Design
Panel A: Childcare for ages 0–2						
This paper	Spain	0–3	Test scores, age 12	Near avg. income	−0.08**	Lottery
Fort et al. (2020)	Italy	0–2	IQ, age 8–14	2× avg. income	−0.10**	RD
Drange & Havnes (2019)	Norway	1–2	Test scores, age 7	All incomes	+0.06*	Lottery
Felfe & Lalive (2018)	Germany	0–3	Developmental skills, age 6	All incomes	+0.05*	IV
Panel B: Preschool for ages 3+						
Berlinski et al. (2008)	Argentina	3–5	Test scores, age 8–15	All incomes	+0.08**	DiD
Felfe et al. (2015)	Germany	3–6	School readiness, age 6–10	All incomes	+0.05*	RD
Havnes & Mogstad (2015)	Norway	3–6	Years of education	All incomes	+0.06**	DiD
Cascio & Schanzenbach (2013)	USA	4–5	Test scores, age 9–11	Disadvantaged	+0.15**	DiD

Notes: Effect sizes are expressed in standard deviations. All studies report cognitive or educational outcomes (test scores, school readiness, or years of education). RD = regression discontinuity; DiD = difference-in-differences; IV = instrumental variables. Panel A includes studies of childcare programs for children under age three; Panel B includes preschool programs for children aged three and older. * p<0.10, ** p<0.05.

Our contribution is threefold. First, we provide rare long-run causal evidence on care before age three by measuring outcomes at age 12—substantially longer follow-up than most 0-3 studies, which typically observe children through preschool or early elementary school. Second, our lottery-based sample represents marginal applicants in a universal system (76% of applicants receive exactly 30 priority points), a population distinct from both the highly disadvantaged children studied in targeted interventions and the affluent families (mean income double the national average) analyzed by Fort, Ichino and Zanella (2020). This group is policy-relevant because they constitute the marginal beneficiaries of capacity expansions in universal systems. Third, we contribute evidence from a Southern European context where public centers operate at higher child-teacher ratios (8:1 for ages 0-1, 13:1 for ages 1-2, 20:1 for ages 2-3) than the benchmarks associated with positive effects in experimental programs, and where counterfactual care likely includes a mix of private childcare, parental care by relatively educated mothers, and grandparent care—

common in Spain (Ahn and Mira, 2001; Nollenberger and Rodríguez-Planas, 2016).

3 Institutional background

Early Childhood Education (*Educación Infantil*) in Spain is divided into two cycles: a non-compulsory first cycle for children aged 0 to 3, and a second cycle for ages 3 to 6, which is free, universal, and integrated into the public school system. This paper focuses on the first cycle, which is not guaranteed and depends on local availability.

In Barcelona, the first cycle is offered through publicly funded early childhood education centers (*escoles bressol municipals*), and includes three levels —level i0, i1, and i2— corresponding to children who turn 0, 1, and 2 years old within the calendar year of enrollment, respectively.[2] These levels correspond to ISCED 01 (Early Childhood Educational Development) in the UNESCO International Standard Classification of Education (UNESCO Institute for Statistics, 2012). Families may apply to any of these levels depending on the child's age. The admission process is identical across levels, and children admitted in i0 are automatically guaranteed progression to i1 and i2 within the same center.

Admission to public childcare centers in Barcelona follows a centralized, point-based allocation system designed to promote fairness and transparency. Because demand frequently exceeds supply, applications are scored according to a set of socioeconomic and family-related criteria. When more applicants than seats share the same score, a lottery is used to break ties.

The admission process unfolds in several stages. Parents submit applications during a designated period, after which applications are scored and ranked. Points are assigned based on six official criteria, grouped into priority criteria (*criterios prioritarios*) and complementary criteria (*criterios complementarios*). Priority criteria carry the most weight in determining admission, while complementary criteria help differentiate among applicants with equal scores. The ranking structure is as follows:

Priority Criteria. The priority criteria for school admission assign points as follows: 40 points if siblings or parents are already enrolled at the school; up to 30 points for the proximity of residence or workplace—30 points for residence within the catchment area, 15 for residence within the district, 10 for residence within the municipality, or 20 points if the applicant's workplace is in the catchment area. Regarding family income, applicants who are beneficiaries of the minimum insertion income receive 10 points. In cases of disability, 10 points are awarded if the applicant, a parent, or a sibling has a disability of 33% or more.

[2]Ajuntament de Barcelona. "Preinscripció a les escoles bressol municipals." Available at: https://ajuntament.barcelona.cat/escolesbressol/es/preinscripcio.

Complementary Criteria. Additional points may be awarded under complementary criteria: 15 points for large families and 10 points for chronic illnesses, including digestive, endocrine, or metabolic conditions, such as celiac disease.

Approximately 76% of applicants receive exactly 30 points, indicating that their score is solely based on residing within the catchment area of the childcare center to which they apply. Notably, and in contrast to other admission systems, the income-based criterion applies only to families with very low income. In fact, fewer than 2% of applicants receive points for low income, reflecting the narrow scope of this eligibility condition. Table 16 in the Appendix presents the distribution of applicants according to the priority and complementary admission criteria.

Within each center–year–level, applicants with points above 30 are admitted first. When capacity is reached within the 30-point group, remaining seats are allocated by a tie-breaking lottery among those applicants, while applicants with fewer than 30 points are not admitted. This is the source of quasi-random variation we use in the analysis.

Once the applications are evaluated and scored, the provisional ranking is published. Families are given an opportunity to review their assigned points and submit appeals. After this review process, a final ranking list is established. When multiple applicants receive the same score, a lottery mechanism determines the final seat allocation. This is the exogenous variation that we exploit.

The lottery operates by assigning a random numerical identifier to each application, which is generated through a publicly conducted draw. A sequence of digits is extracted to determine a reference number, after which applications are ranked sequentially based on their assigned numbers. This procedure ensures that, conditional on having the same number of points, each applicant has an equal probability of securing a seat.

Upon publication of the final list of admitted students, families must formally enroll their child within the designated period. The overall structure of the system aims to promote fairness, transparency, and equal access to early childhood education, while simultaneously recognizing and accommodating household constraints that may impact children's educational trajectories. Although the priority score is rule-based, families can strategically choose which school to apply to. In practice this margin operates through school choice: the policy is designed to encourage applications to the nearest center (which yields the catchment-area points), and some families may also avoid high-demand centers to increase admission chances.

4 Data and empirical strategy

4.1 Data

4.1.1 Linked administrative data (IDESCAT)

The core individual-level dataset consists of administrative records linked by the Catalan Statistical Institute (Institut d'Estadística de Catalunya, IDESCAT) using secure individual identifiers. The universe of individuals in this linked dataset is defined by the IMEB childcare application records: all other data sources—academic outcomes, household characteristics—are linked exclusively for children who appear in the IMEB applicant pool. This linked dataset integrates four administrative sources covering childcare applications, children's later academic outcomes, and household characteristics.

Childcare applications (IMEB): Data on childcare applications come from the Municipal Institute of Education of Barcelona (Institut Municipal d'Educació de Barcelona, IMEB). These data contain each family's preference list over childcare centers, their priority points, as well as each child's lottery outcome (i.e., the assigned center or non-admission), and the lottery number for the academic years 2005–06 to 2012–13. These variables are crucial for our identification strategy. Although we observe each child's assigned childcare center, information on the enrollment decisions, namely the center and length of actual enrollment, is not available; according to IMEB sources, take-up among admitted applicants is around 90%. Additionally, the data contain information collected by IMEB when parents apply for a childcare seat for their child, which allows us to confirm that characteristics of admitted and non-admitted children are indeed balanced. Specifically, we observe parental characteristics, namely their age, education level, and nationality, as well as the families' location of residence (census tracts) and the children's birth date. Moreover, we can identify twins and siblings through a unique household identifier in the application data. The full IMEB applicant pool spans academic years 2005–06 to 2012–13. We restrict our analysis to application cohorts 2005–06 to 2009–10 and to children born between 2003 and 2007, as these are the birth cohorts for whom standardized test scores at age 12 (6th grade) are available in the linked data (academic years 2014/15–2018/19). The resulting full applicant pool includes 23,817 applications.

Academic outcomes (Departament d'Educació and Consell Superior d'Avaluació): We obtain information on academic outcomes from two sources within the Catalan education system. Standardized test scores in languages (Catalan, Spanish, and English) and mathematics for academic years 2014/15–2018/19 come from the Consell Superior

d'Avaluació del Sistema Educatiu.[3] These tests, taken in the final year of primary school (6th grade), serve as our main measure of cognitive ability. From the Departament d'Educació, we also observe students' enrollment by grade in pre-primary, primary, and secondary school, annual average grades (incomplete for 2011/12–2013/14), and school type. To protect confidentiality, school characteristics are provided at aggregated geographic levels (census groupings, and in Barcelona at the neighborhood level).

Linkage to test scores is high but not complete. Table 2 summarizes the sample-selection steps and the corresponding reductions in the number of observations as we impose the outcome availability and lottery-based restrictions.

Household characteristics (Padró Municipal): Household characteristics come from the Municipal Population Register of Barcelona (Padró Municipal d'Habitants). We observe household size, co-residence indicators with each tutor (as a proxy for single-parent households), and basic demographics (sex, month/year of birth, nationality in broad regions). Residential locations are provided at the census-grouping level (*agrupació censal*) to preserve confidentiality.

4.1.2 Supplementary data sources

We complement the IDESCAT-linked individual-level data with additional sources that provide neighborhood-level characteristics, aggregate childcare market statistics, and survey evidence on counterfactual care arrangements.

Neighborhood characteristics (IDESCAT): Because individual income is not available in the linked administrative data, we supplement the individual-level variables with neighborhood characteristics at the *agrupació censal* level from IDESCAT. Barcelona is divided into 190 *agrupacions censals*, which are intermediate geographic units between census tracts (*seccions censals*) and districts.[4] These indicators include the share of foreign-born residents from low-income countries, the share of the population without post-secondary education, the share of young adults without post-secondary education, the employment rate, average household income, and the share of workers in low-qualified occupations.

District-level childcare enrollment (Generalitat de Catalunya): To characterize the aggregate childcare market in Barcelona, we use publicly available education statistics

[3]The dataset also includes test scores in Sciences, French, and Aranese; however, we exclude them from our analysis because they are only available for a limited subset of the sample.

[4]These neighborhood indicators are measured in 2018, which postdates our application cohorts (2005–2012). To the extent that relative neighborhood characteristics are persistent over time, these variables remain informative proxies for the socioeconomic environment at the time of application.

from the Generalitat de Catalunya's Departament d'Educació.[5] These data provide annual counts of educational centers, classes, enrolled children, and educators, disaggregated by district, school sector (public or private), and educational level—with a particular focus on Early Childhood Education 1st cycle (ages 0–2). We use data for academic years 2005–06 to 2009–10, which correspond to our main analysis sample. Barcelona is divided into 10 districts, each comprising several *agrupacions censals*. We combine these enrollment statistics with district-level data on newborns from the Municipal Population Register (Padró Municipal d'Habitants), which allows us to compute the number of resident children aged 0–2 in each district and year. Together, these data allow us to measure private childcare availability at the district level and to compute enrollment shares by sector (public, private, or no formal childcare).

Survey on childcare arrangements (IGOP): To characterize the care arrangements of families who apply but are not admitted to public childcare, we use data from an independent survey of families with young children in Barcelona, conducted in 2020 by the Institut de Govern i Polítiques Públiques (IGOP) at the Universitat Autònoma de Barcelona (González, 2021). The survey collected information from 1,219 families on childcare arrangements at each age between 0 and 3, including the frequency and daily hours with each caregiver type (mother, father, grandparents, public childcare, private childcare, babysitter, and others). Crucially, the survey asked whether families had applied to public childcare and, if so, whether they were admitted. This allows us to identify the care arrangements of families who applied but were not admitted—the relevant counterfactual for our lottery-based estimates. We use this survey in Section 6.1.1 to document that the modal counterfactual to public childcare is maternal care, followed by private childcare and grandparents.

Class size ratios (Generalitat de Catalunya): To explore potential mechanisms related to childcare quality, we use data on average class sizes in early childhood education from the Departament d'Educació of the Generalitat de Catalunya.[6] These data report the average number of children per educator in Catalonia, disaggregated by school sector (public or private) and age level, for academic years 2006–07 to 2021–22.

4.2 Sample Restrictions

The IDESCAT-linked individual-level data constitute our main analysis dataset. We restrict this sample to include only applications that we can confidently classify as having

[5] Available at https://educacio.gencat.cat/ca/departament/estadistiques/estadistiques-ensenyament/cursos-anteriors/.

[6] Available at https://educacio.gencat.cat/web/.content/home/departament/estadistiques/indicadors/sistema-educatiu/escolaritzacio/a0207.xlsx.

entered the lottery. To achieve this, we apply the following exclusion criteria:

1. We restrict our sample to individuals for whom we have information about both parents.

2. We keep only applications for which we have data on educational outcomes. Given that we want a unique treatment for each individual in our sample to merge it with the different outcomes of interest, we keep only the first application of an individual.[7]

3. We exclude invalid applications. These include duplicate submissions, applications submitted after the deadline, or applications where the child's age did not match the education level. These applications do not enter the lottery and face a lower probability of admission than valid ones.[8]

4. We exclude applications for children with special needs. Each school reserves a quota for these children, which means their applications are not subject to the general lottery and enjoy a higher likelihood of admission.[9]

5. We exclude applications involving twins. During the study period, twins did not receive additional priority points; their applications entered the lottery like any other. However, schools guaranteed that if one twin was admitted, the other would also be automatically admitted. As a result, twins had a higher probability of admission, and we exclude them to maintain randomness in our sample.

6. We restrict the sample to applications with a point total that ensures entry into the lottery. In principle, the threshold for entering the lottery could vary across childcare center–year–level combinations. However, in practice, almost all lotteries occur at the 30-point threshold, which corresponds to applicants meeting only the proximity criterion. Therefore, we retain only those with 30 points.

7. We keep only those center–year–level combinations in which at least one applicant with 30 points was admitted and at least one with the same score was not. Given our prior sample restrictions, this ensures that the assignment among these applicants was random. While this final restriction does not affect our estimates—since we always include center–year–level fixed effects—it helps clarify the number of effective observations in our estimation sample.

[7] Appendix B presents results including re-applicants (Tables 26 and 27), which are similar to the main findings.

[8] The only exception are applications to incorrect education levels that exhibit higher probability of admission, but this is driven by the abnormal share of children with special needs among these applications.

[9] Unfilled special needs slots are subsequently made available to regular applicants.

After applying these restrictions, we are left with 7,980 observations for children participating in the childcare admission process of the academic years 2005–06 to 2009–10. Table 2 reports the share of observations that are dropped in each of these restrictions.

External validity. The lottery sample is positively selected relative to the full applicant pool. Table 14 in the Appendix compares the two groups and shows that the lottery sample has somewhat higher parental education (70% vs. 67% of mothers with a post-secondary education), higher Spanish nationality rates (83% vs. 73%), and fewer prior siblings in the system (0.06 vs. 0.12). These differences arise because applicants with priority points (from low income, disability, or having siblings in the school) are admitted outside the lottery, leaving a relatively more advantaged group competing for the marginal seats.

This positive selection has implications for interpreting our estimates. Our LATE applies to 30-point marginal applicants—families who meet only the proximity criterion and have no other priority. These are precisely the families who would gain access if public childcare capacity were expanded, making our estimates directly policy-relevant for expansion decisions. However, they are not the most disadvantaged families, who receive priority and are admitted without entering the lottery. Importantly, while our sample is positively selected, the selection is considerably less pronounced than in some comparable studies. For instance, Fort, Ichino and Zanella (2020) study a sample with median income approximately 2.5 times the Italian median; our applicant pool is far closer to the population average. We return to the implications of sample composition when examining heterogeneity by parental education in Section 5.5.

Table 2: Sample Selection

Restriction	**% dropped**	**Obs. remaining**
Full sample	—	23,817
(1) No missing pre-treatment variables	12.6%	20,818
(2) No missing outcome variables	17.9%	16,557
(3) Valid application	1.2%	16,260
(4) No special educational needs	0.2%	16,217
(5) No simultaneous sibling applications	5.8%	14,830
(6) Priority score = 30 (catchment area only)	14.3%	11,419
(7) First-choice school held lottery	14.4%	7,980

Notes: For each of the restrictions, *% dropped* reports the percentage of the full sample that does not fulfill the additional restriction and *Obs. remaining* the number of observations that remain after applying all the previous restrictions.

4.3 Empirical strategy

To estimate the causal effect of access to public childcare on student academic achievement in primary school, we exploit the random assignment of seats in childcare centers in Barcelona.

Our empirical strategy follows a lottery-based design similar to that used by Drange and Havnes (2019) for childcare in Oslo and Abdulkadiroglu et al. (2011) for charter schools in Boston. We compare outcomes of children admitted to those of children who applied for a seat in the same level, year and center but were not admitted. We define intention-to-treat (ITT) effects using the following baseline specification:

$$y_{it} = \gamma \text{Admitted}_{it} + \text{D}'_{it}\alpha + \text{X}'_{it}\beta + \epsilon_{it}, \tag{1}$$

where i denotes the individual and t denotes the cohort. Admitted_{it} is a dummy equal to 1 if the child was admitted to a public childcare place generated in the assignment lottery. Admitted_{it} refers to admission to the first-choice center; in the application data, only about 3% of children are admitted to a second or lower choice. X_{it} are a set of sociodemographic characteristics of the child and parents and characteristics of the census grouping of their residence at the time of applying for childcare. We present estimates both with and without these covariates. While the inclusion of covariates should not affect our results if the treatment variable is truly random, it can enhance the precision of our estimates. Throughout the analysis, we cluster the standard errors (ϵ_{it}) at the level of the first-choice institution.

What is crucial for identification of γ is the inclusion of the risk sets D_{it}. These are a full set of dummy variables for each center by year and level, so that for each center, year and level there is a dummy equal to 1 if child i applied to that level, in that center, in that year and 0 otherwise.

4.4 Balancing

First, we show that applicants who were admitted at their first-choice center and those whose first choice was rejected are balanced in their pre-treatment characteristics (Table 3). Columns (1) and (2) show the unconditional mean of pre-treatment characteristics of students admitted to their first-choice school and those who were not admitted, respectively. Overall, the two groups exhibit similar characteristics, with differences primarily in variables such as parental education and the average income of their residential area. These differences could be attributed to the broader availability of public childcare in less affluent neighborhoods. It is important to note that families applying to different schools, grade levels, or years may have had varying probabilities of success in the lottery.

Column (3) shows the estimated coefficient from equation (1) with each predetermined

characteristic as the outcome variable. In column (4) we report the corresponding p-values. We can observe that after we control for the lottery fixed effects, there are no statistically significant differences in pre-treatment characteristics between admitted and rejected students. A joint F-test for the null hypothesis that all covariates are simultaneously unrelated to treatment status also fails to reject (reported at the bottom of Table 3). Additionally, in Appendix A.2 we show that admitted and rejected children have similar attrition rates from the test score sample, ruling out differential selection into our estimation sample.

Table 3: Lottery assignment and predetermined characteristics

Variable	Mean Admitted	Mean Rejected	Difference	p-value
Panel A: Individual and family characteristics				
# Preferences expressed	2.45	2.36	-0.01	0.67
Female child	0.48	0.48	-0.02	0.23
Age child at application	1.29	1.18	-0.01	0.41
# siblings who applied before	0.06	0.06	0.00	0.62
Age Mother	33.60	33.81	0.14	0.24
Age Father	35.60	35.82	0.09	0.54
Spanish nationality Mother	0.83	0.83	0.00	0.87
Spanish nationality Father	0.83	0.82	-0.00	0.68
Mother has Bachelor Degree	0.66	0.72	0.00	0.72
Father has Bachelor Degree	0.59	0.65	-0.01	0.33
Both parents with a Bachelor Degree	0.48	0.55	0.00	0.85
Resides with Mother	0.92	0.91	0.00	0.86
Resides with Father	0.85	0.86	-0.01	0.13
Resides with both parents	0.81	0.81	-0.01	0.18
Panel B: Neighborhood characteristics (agrupació censal)				
Foreigners from Low-income countries (%)	13.14	12.17	0.00	0.99
Population w/o post-secondary(%)	16.47	14.86	-0.08	0.28
Young w/o post-secondary (%)	23.96	20.42	-0.06	0.71
Employed population(%)	64.42	65.26	0.05	0.51
Average income (€)	13,988.66	14,986.18	45.10	0.38
Low qualified workers (%)	10.55	8.85	-0.06	0.47
Observations	2,704	5,276		

Notes: *Mean Admitted*: mean of the reported variable for applicants who were admitted in their first choice. *Mean Rejected*: mean of the reported variable for applicants who were not admitted in their first choice. *Difference*: t-test of the difference in the reported variable for applicants who were admitted in their first choice and those who did not with lottery fixed effects. *p-value*: p-value of the test. Standard errors are clustered at the level of the lottery fixed effects.*** $p<0.01$, ** $p<0.05$, * $p<0.1$. A joint F-test of the null that all covariates are simultaneously unrelated to treatment status yields $F = 0.82$ ($p = 0.68$).

5 Results

5.1 Main results: test scores (average)

We begin with the overall effect on the weighted average test score across subjects at age 12. Table 4 reports intent-to-treat (ITT) estimates of the effect of admission to the applicant's first-choice public childcare center under alternative specifications that progressively account for the lottery-based assignment mechanism.

Columns (1) and (2) provide descriptive benchmarks. Column (1) pools all applicants and yields a large negative estimate, while column (2) restricts the sample to applicants participating in oversubscribed lotteries. These specifications do not fully account for differences in admission probabilities across applicants and therefore combine variation in admission status with variation in risk sets. As a result, the estimates in columns (1) and (2) should not be interpreted as causal.

The remaining columns focus on the lottery sample and explicitly address this concern. Column (3) controls for the predicted admission probability, ensuring that admitted and non-admitted applicants are compared within comparable risk sets. Column (4) instead includes lottery fixed effects, which absorb all between-lottery variation in admission probabilities and applicant composition. Both approaches deliver very similar estimates, around -0.08 standard deviations, indicating that the negative effect is not driven by differential admission probabilities across lotteries.

Our preferred specification is column (5), which includes lottery fixed effects along with socioeconomic controls. Lottery fixed effects provide a transparent way to condition on the full risk set faced by each applicant and require fewer functional form assumptions than probability controls. In this specification, admission to first-choice childcare reduces average test scores at age 12 by -0.082 standard deviations, significant at the 1% level. We therefore take this estimate as the central result of the paper.

To contextualize this magnitude, we follow Kraft (2020), who proposes benchmarks for interpreting effect sizes in education research based on the distribution of effects from nearly 750 randomized controlled trials. By his classification, effects below 0.05 SD are considered small (typical of most scaled-up interventions), 0.05–0.20 SD are medium (comparable to class size reductions or tutoring programs), and above 0.20 SD are large (achieved mainly by intensive early childhood programs like Perry Preschool). Our estimate of -0.08 SD falls squarely in the medium range. Notably, Kraft emphasizes that traditional Cohen benchmarks (0.2, 0.5, 0.8) substantially overstate what is achievable in field-based education interventions: 36% of RCT effect sizes in his sample are below 0.05 SD. By this standard, our medium-sized ITT effect represents a meaningful impact on child development. Since not all admitted children enroll and a small share of non-admitted children receive lower-preference assignments, the per-child effect of attendance

is likely somewhat larger.

For additional context, our ITT estimate is similar in magnitude to the test score gap between children born in January versus December of the same school cohort (the "relative age" effect), which is typically around 0.10 SD. It is also consistent with Fort, Ichino and Zanella (2020), who report an ITT effect of −0.10 SD in Bologna using a regression discontinuity design, and with several other studies of early childcare that find effects in the −0.05 to −0.15 SD range for children from relatively advantaged backgrounds.

Across specifications, the estimated effect is stable. The point estimates are similar when we move from admission-probability controls to lottery fixed effects and when we add socioeconomic covariates. Later in the paper we also show that the results are robust to including primary school fixed effects (Table 12), suggesting that the findings are not driven by differential sorting into primary schools. Results are also robust to defining treatment as admission to *any* public childcare center rather than just the first choice (Appendix A.7).

Importantly, these estimates are local to applicants at the admission margin within each lottery. Non-admitted applicants do not remain untreated but instead rely on a mix of alternative care arrangements, including parental care, grandparents, and private childcare. The estimated ITT effect thus reflects both the quality of the public childcare option and the displacement of these counterfactual forms of care.

Intention-to-treat versus treatment-on-the-treated. Our estimates are intention-to-treat (ITT) effects—the impact of being *offered* a seat at the first-choice public childcare center. The relationship between ITT and the effect of actually attending public childcare is complicated by several factors. First, not all admitted applicants enroll: according to IMEB administrative records, approximately 90% of those offered a seat take it up. Second, some applicants not admitted to their first choice are assigned to a lower-preference center—about 3% of our sample (Table 18).

These compliance patterns have offsetting implications. Non-take-up among the treated (10%) attenuates the ITT relative to the effect of attendance. But partial treatment of the control group through lower-preference assignments also attenuates the ITT by reducing the contrast between treatment and control. We cannot quantify these channels precisely without individual enrollment data. However, our estimates should be interpreted as the effect of winning the lottery for a first-choice seat, which remains the policy-relevant parameter for capacity expansion decisions: it captures what happens when additional seats are offered to marginal applicants at oversubscribed centers.

Table 4: Effect of childcare admission on average test score at age 12

	(1) Average Test Score	(2) Average Test Score	(3) Average Test Score	(4) Average Test Score	(5) Average Test Score
Admitted	-0.157***	-0.136***	-0.085***	-0.085***	-0.082***
	(0.018)	(0.027)	(0.028)	(0.028)	(0.026)
Observations	16557	7980	7980	7980	7980
Mean	-0.00	-0.00	-0.00	-0.00	-0.00
FE Probability			Yes		
FE Lottery				Yes	Yes
Controls					Yes
Sample	All applicants	Lotteries	Lotteries	Lotteries	Lotteries

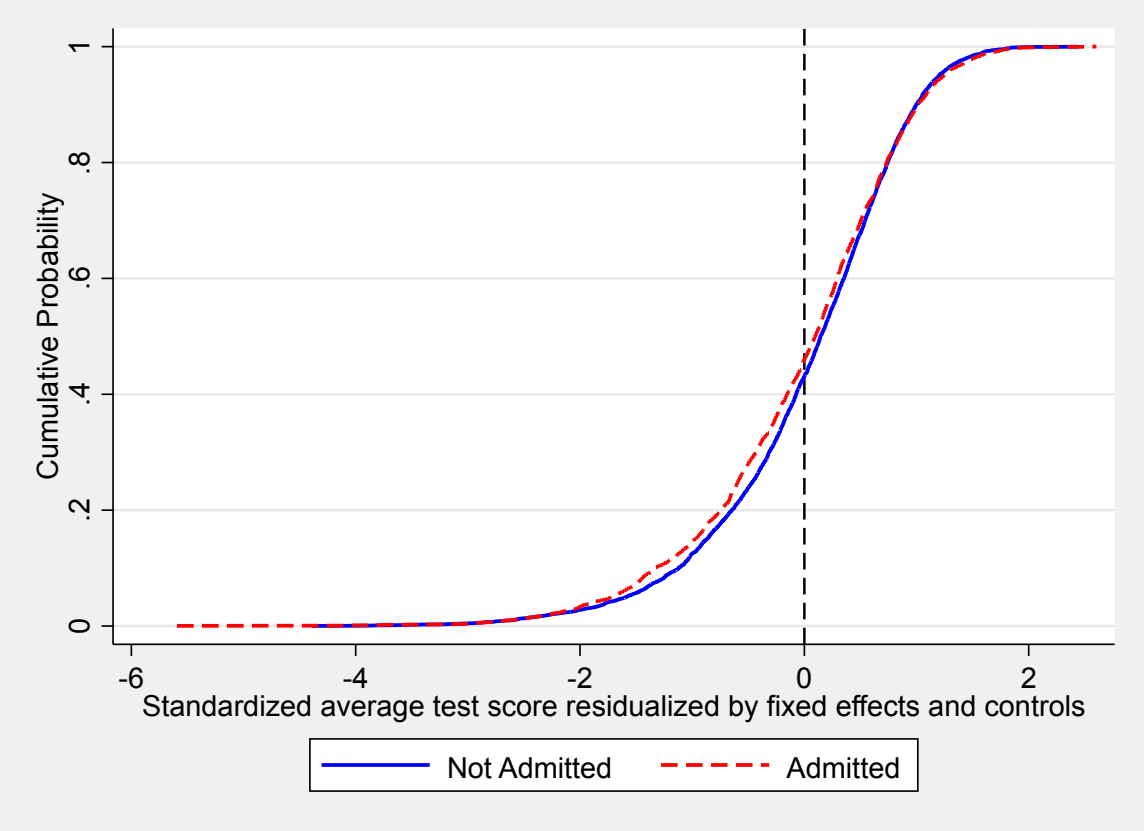
Notes: Outcome is the weighted average of the four subject tests. Models include geographical unit fixed effects and local time trends. Standard errors are clustered at the lottery level. *** p<0.01, ** p<0.05, * p<0.1.

5.2 Results: test score distribution

Figure 1 provides complementary evidence by comparing the cumulative distribution of residualized test scores for admitted and non-admitted applicants. The CDF for admitted applicants lies uniformly to the left of the CDF for non-admitted applicants across most of the distribution. This pattern indicates first-order stochastic dominance of the non-admitted distribution over the admitted distribution: at any given test score threshold, a larger fraction of admitted children falls below that threshold compared to non-admitted children.

This distributional pattern is consistent with mechanisms that operate for a wide range of applicants at the margin, such as sustained differences in learning environments or instructional quality, rather than effects limited to a small subgroup. The visual separation between the two CDFs confirms that the negative treatment effect is not driven by changes at specific quantiles but reflects a systematic shift across the entire distribution.

Figure 1: Cumulative distribution of residualized test scores by admission status



Notes: Cumulative distribution functions of standardized average test scores after partialling out lottery fixed effects and controls. Residuals are computed from a regression of test scores on lottery fixed effects and the full set of control variables. The dashed vertical line indicates zero. The CDF for admitted applicants lies to the left of the CDF for non-admitted applicants, consistent with first-order stochastic dominance of the non-admitted distribution.

5.3 Results: test scores by subject

Table 5 decomposes the average effect by subject. Admission to first-choice childcare leads to negative and statistically significant effects in Catalan (-0.059, significant at the 5% level), Spanish (-0.086, significant at the 1% level), and Mathematics (-0.098, significant at the 1% level). In contrast, the estimate for English is small and statistically indistinguishable from zero (-0.017). The null effect for English is consistent with this interpretation: if the counterfactual for non-admitted children is home-based parental care, we would expect no effect on English because families in Barcelona do not typically speak English with young children at home. Neither childcare nor parental care provides meaningful English exposure before age three; English proficiency at age 12 is determined primarily by formal schooling, which begins later and is common to both admitted and

non-admitted children.[10]

These results indicate that the overall decline in average test scores is driven by outcomes plausibly shaped during early childhood—local languages and early numeracy—rather than by skills acquired later through formal instruction. The pattern is consistent with the interpretation that home-based care provides richer exposure to Catalan and Spanish interactions, or that differences in adult–child ratios matter more for foundational language and numeracy development.

Table 5: Effect of childcare admission on subject test scores at age 12

	(1) Average Test Score	(2) Test Score Catalan	(3) Test Score Spanish	(4) Test Score English	(5) Test Score Maths
Admitted	-0.082***	-0.059**	-0.086***	-0.017	-0.098***
	(0.026)	(0.026)	(0.027)	(0.027)	(0.027)
Observations	7980	7980	7980	7980	7980
Mean	0.00	0.00	0.00	0.00	0.00

Notes: Models include lottery fixed effects and socioeconomic controls. Standard errors are clustered at the lottery level. *** p<0.01, ** p<0.05, * p<0.1.

5.4 Results: by childcare level (age)

Table 6 examines heterogeneity by childcare application level, corresponding to the child's age at entry. Each panel reports estimates of equation (1) separately for applicants at ages 0–1, 1–2, and 2–3.

Two broad patterns emerge. First, language penalties are larger at younger ages. In Panel A (Age 0–1), admission yields relatively large negative effects in language-related outcomes, with point estimates ranging from −0.125 to −0.175, although estimates are imprecise due to the smaller sample size. In Panel B (Age 1–2), language effects remain negative but are smaller in magnitude, around −0.076 to −0.081. In Panel C (Age 2–3), language estimates are mixed, with some close to zero and others remaining negative.

Second, mathematics penalties emerge more clearly at later ages. The mathematics estimate is close to zero in Panels A and B but becomes larger and statistically significant in Panel C (−0.135, significant at the 1% level). This pattern suggests that the domains affected by admission may vary systematically with the age at which children

[10]Neither public nor private childcare for ages 0–2 in Barcelona included English instruction during our sample period (2005–2010), and 90% of Barcelona residents reported Catalan, Spanish, or both as their first language (Generalitat de Catalunya, 2008). See Appendix D for details on the language of instruction in early childhood education centers.

enter childcare.

These age-specific results should be interpreted with caution. Differences across levels may reflect genuine heterogeneity in treatment effects by age at entry, but they may also capture differences in applicant composition, in the quality of counterfactual care across age groups, and in treatment duration—since children admitted at level i0 are guaranteed progression to i1 and i2, effectively receiving up to three years of public childcare compared to two years for i1 admits and one year for i2 admits. Nevertheless, the pattern of larger early language penalties and later-emerging mathematics penalties is consistent with a view in which early childcare environments are particularly salient for language development, while later experiences may matter more for mathematical skills. As in the main results, these estimates reflect the combined effect of public childcare quality and displacement from alternative care arrangements available to non-admitted applicants. Tables 21–23 in Appendix A confirm that admitted and rejected applicants are well balanced within each age level.

Table 6: Effect of childcare admission on test scores by application level

	(1) Average Test Score	(2) Test Score Catalan	(3) Test Score Spanish	(4) Test Score English	(5) Test Score Maths
Panel A: Age 0–1 (entry at 4–12 months)					
Admitted	-0.125*	-0.161**	-0.175**	-0.021	-0.041
	(0.073)	(0.076)	(0.087)	(0.077)	(0.079)
Observations	792	792	792	792	792
Mean	0.21	0.11	0.09	0.19	0.25
Panel B: Age 1–2 (entry at 12–24 months)					
Admitted	-0.074*	-0.076**	-0.081*	-0.001	-0.078*
	(0.040)	(0.039)	(0.041)	(0.038)	(0.040)
Observations	4336	4336	4336	4336	4336
Mean	0.02	0.03	0.00	0.03	0.00
Panel C: Age 2–3 (entry at 24–36 months)					
Admitted	-0.087**	-0.018	-0.074*	-0.044	-0.135***
	(0.041)	(0.040)	(0.040)	(0.045)	(0.043)
Observations	2852	2852	2852	2852	2852
Mean	-0.09	-0.08	-0.03	-0.10	-0.07

Notes: Each panel reports estimates of equation (1) for applicants to each childcare level. Standard errors are clustered at the lottery level. *** p<0.01, ** p<0.05, * p<0.1.

5.5 Results: heterogeneity by parental education

As noted in Section 4.2, the lottery sample is positively selected: families competing for marginal seats at 30 points have somewhat higher education and are more likely to be Spanish nationals than the full applicant pool. A natural question is whether the negative effects we document are concentrated among these higher-SES families, or whether they extend to children from less advantaged backgrounds.

Table 7 reports estimates separately by parental education, splitting the sample into families where both parents have post-secondary education ("High Education") versus families where at least one parent does not ("Low Education"). The results show negative effects in both groups. Among high-education families, the estimated effect is -0.090 SD (significant at the 1% level). Among low-education families, the effect is -0.074 SD (significant at the 10% level). The difference between the two coefficients is small (0.016 SD) and not statistically significant.

This pattern has two important implications. First, it suggests that the negative effects are not driven solely by high-SES children being displaced from exceptionally high-quality home environments. Children from less-educated families—who are less likely to have access to enriched home care—also experience negative effects from public childcare admission. Second, it underscores that even if one were to reweight our estimates toward a less advantaged population, the qualitative conclusions would be unlikely to change: the point estimates remain negative across the education distribution.

The finding of negative effects among low-education families is worth noting in light of the broader literature. Most studies documenting negative or null effects of childcare have focused on relatively advantaged populations—for example, Fort, Ichino and Zanella (2020) study a sample with median income 2.5 times the Italian average, and Baker, Gruber and Milligan (2019) examine a universal program in Quebec where take-up was highest among higher-income families. By contrast, studies finding positive effects have typically focused on disadvantaged populations, such as the Perry Preschool and Abecedarian programs in the United States. Our finding that negative effects extend to children from less-educated families suggests that the quality of institutional care—not just the quality of displaced home care—matters for child outcomes.

The similarity of effects across education groups is somewhat surprising. One might expect that high-education families have better outside options (either higher-quality home care or better access to private alternatives), which would generate larger negative effects for this group. The absence of strong heterogeneity suggests that the mechanisms underlying the negative effects—whether related to child-teacher ratios, curriculum, or peer composition—operate broadly across the SES distribution, at least among the population of families who apply to public childcare. Appendix B presents additional heterogeneity analyses by gender (Table 24) and month of birth (Table 25), which show similar

patterns of negative effects across these dimensions.

Table 7: Effect of childcare admission by parental education

	(1) Average Test Score	(2) Average Test Score	(3) Average Test Score	(4) Average Test Score
Admitted x High Edu	-0.090***	-0.189**	-0.075	-0.084
	(0.033)	(0.073)	(0.048)	(0.057)
Admitted x Low Edu	-0.074*	-0.014	-0.072	-0.091
	(0.038)	(0.123)	(0.059)	(0.056)
Observations	7980	792	4336	2852
Mean	-0.00	0.21	0.02	-0.09
Sample	All levels	Age 0–1	Age 1–2	Age 2–3
p-value (H0: equal)	0.75	0.17	0.96	0.93

Notes: "High Education" families have both parents with post-secondary education (CNES $\geq$ 40); "Low Education" families have at least one parent without post-secondary education. All specifications include lottery fixed effects and controls. Standard errors are clustered at the lottery level. *** p<0.01, ** p<0.05, * p<0.1.

6 Discussion

6.1 Counterfactual care

A key element in interpreting the estimates is the quality of the counterfactual care for non-admitted applicants. In Barcelona, rejected applicants can remain at home or enroll in private childcare, and these alternatives are not random: they correlate with parental resources and preferences. This makes it plausible that the marginal admitted child is displaced from relatively high-quality care, which can generate negative average effects even when public childcare expands access.

To probe this channel, we examine heterogeneity by the local availability of private childcare. We construct a district-level measure of private childcare availability as the ratio of children enrolled in private childcare centers to children enrolled in public childcare centers within each district.[11]

The point estimates are negative in both groups: -0.088 (significant at the 5% level)

[11]The data come from Barcelona's annual education yearbooks ("Anuaris d'Escolarització"), which report enrollment counts by district, age, and sector (public, subsidized private, and non-subsidized private). Table 8 splits applicants by whether their district has above-median private availability (median ratio is 2.20, meaning the median district has 2.2 children in private childcare for each child in public childcare).

in areas with low private childcare availability and −0.066 (significant at the 10% level) in areas with high availability. While the estimate is somewhat larger in low-availability areas, the difference between the two coefficients is small (0.022 SD) and not statistically significant. The pattern is similar across age groups, with no statistically distinguishable differences between high- and low-availability areas.

These results are informative for interpretation. If the negative effects were driven primarily by public childcare being inferior to private childcare, we might expect larger penalties in high-availability areas, where rejected applicants are more likely to enroll in private centers. The fact that effects are at least as large in low-availability areas—where non-admitted children are more likely to remain in parental care—suggests that home-based care is a relevant counterfactual. Qualitative evidence from Barcelona further undermines the "inferior public quality" hypothesis: interviews with mothers reveal that public nurseries are widely perceived as offering higher quality than private alternatives, with better facilities, more qualified staff, and more innovative pedagogical approaches (Saurí and González Motos, 2022). This perception reflects institutional reality: Barcelona's public nurseries operate under strict quality regulations (established by regional law in 2004) governing facilities, pedagogy, and teacher-child ratios, whereas the private sector is considerably more heterogeneous in quality. This suggests that the relevant counterfactual for marginal applicants in our sample is not private institutional care, but rather home-based care—likely provided by parents or extended family members. Given that applicants in our lottery sample tend to have relatively educated parents (as shown in Table 3), home care may provide a developmentally rich environment that public childcare, operating under resource constraints, does not fully replicate. The relatively high education level of applicants is consistent with broader evidence of a "Matthew effect" in childcare access: more advantaged families are systematically more likely to apply for and use formal childcare services, while children from more vulnerable backgrounds—who might benefit most from early education—are less likely to enroll (Saurí and González Motos, 2022). We document this pattern directly in Appendix C, where we compare applicants to non-applicants in the same survey data: applicants have significantly higher education, income, and are more likely to be native to Barcelona (Table 31).

6.1.1 Survey evidence on counterfactual care

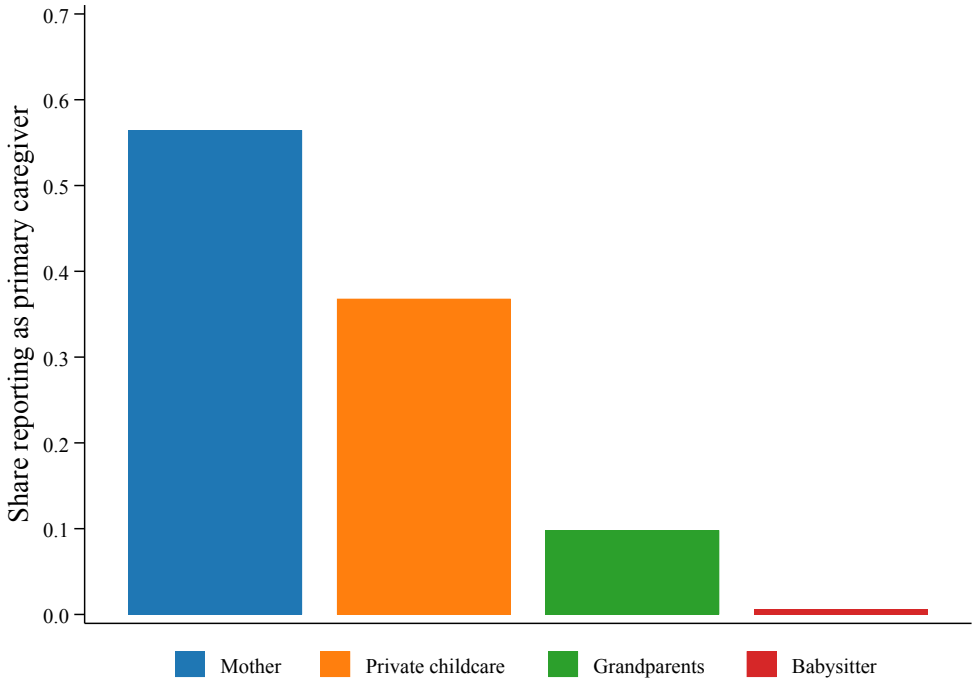
Direct evidence on the childcare arrangements of families who apply but are not admitted to public childcare supports this interpretation. Figure 2 presents descriptive statistics from an independent survey of families in Catalonia who applied to public childcare centers.[12] The figure shows the share of families reporting each type of care arrangement

[12]The data come from a survey conducted by Raquel Gallego and Sheila González Motos as part of the project "Models d'educació de 0 a 3 anys i participació en el mercat laboral: estudi de la innovació social

as the primary daily caregiver among non-admitted applicants.

The patterns reveal that maternal care is the dominant counterfactual: pooling across ages 0–3, 56% of non-admitted families report the mother as the primary caregiver, while 37% use private childcare and 10% rely on grandparents. These figures indicate that for the majority of families who apply but are rejected from public childcare, the alternative is not institutional care but home-based care provided by family members.

Figure 2: Childcare arrangements for families not admitted to public childcare



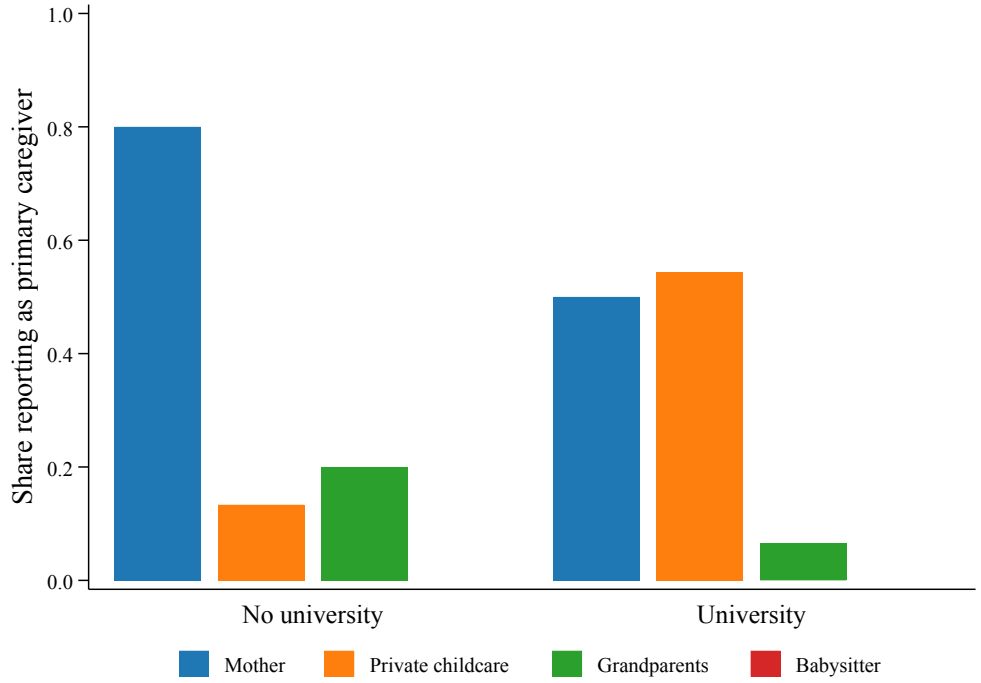
Notes: Data from an independent survey of families in Catalonia who applied to public childcare. Sample restricted to families who were not admitted to their preferred center, pooling all ages 0–3 (N=163 family-year observations: 71 at age 0–1, 61 at age 1–2, 31 at age 2–3). Each bar shows the share of families reporting that caregiver type as the primary daily caregiver. Source: Independent survey.

These counterfactual arrangements vary substantially by family background. Figure 3 shows that among low-education mothers not admitted to public childcare, 80% report the mother as the primary caregiver, compared to only 50% among high-education mothers. Conversely, private childcare is the primary arrangement for just 13% of low-education

a la ciutat de Barcelona," funded by Recercaixa 2017 and Agència Catalana d'Universitats Públiques (ACUP). The survey collected information on childcare arrangements for children aged 0–3 in families who applied to public childcare between 2018 and 2021—a later period than our estimation sample (2005–2010). We restrict the sample to families who were not admitted to their preferred center, pooling across all ages 0–3 (N=163 family-year observations). While the timing difference warrants caution, the institutional structure of Barcelona's childcare lottery system has remained stable over this period, making the survey evidence informative about counterfactual arrangements for applicants. See González-Motos and Saurí (2023) and Saurí and González Motos (2022) for related qualitative analyses from the same research project.

families but 54% of high-education families. Grandparents serve as the primary caregiver for 20% of low-education families and only 7% of high-education families. These patterns confirm that private childcare substitution is concentrated among high-education families, while low-education families—who face binding financial constraints—predominantly rely on maternal care when denied a public slot.

Figure 3: Counterfactual childcare arrangements by mother's education



Notes: Data from an independent survey of families in Catalonia who applied to public childcare. Sample restricted to families who were not admitted to their preferred center, for children aged 1–2. "No university" refers to mothers without a university degree ($N = 15$); "University" refers to mothers with a university degree or higher ($N = 46$). Each bar shows the share of families reporting that caregiver type as the primary daily caregiver. The small subgroup sample sizes warrant caution in interpretation. Source: Independent survey.

This evidence is consistent with our main interpretation. In areas with low private childcare availability, the counterfactual for non-admitted families is predominantly maternal care—and the negative effects we estimate are at least as large in these areas as in areas with high private availability. For marginal applicants from educated families, early childcare appears to displace developmentally rich home environments provided by parents who have the time and resources to invest in their children's early development.

Table 8: Heterogeneity by private childcare availability

	(1) Average Test Score	(2) Average Test Score	(3) Average Test Score	(4) Average Test Score
Admitted × High Private Availability	-0.066*	-0.086	-0.096	-0.070
	(0.037)	(0.082)	(0.058)	(0.049)
Admitted × Low Private Availability	-0.088**	-0.204	-0.051	-0.118**
	(0.037)	(0.130)	(0.051)	(0.049)
Observations	7772	782	4433	4115
Mean	0.00	0.20	0.03	-0.06
Sample	All levels	Age 0–1	Age 1–2	Age 2–3
p-value (H0: equal)	0.66	0.43	0.55	0.49

Notes: Each column reports estimates from a separate regression of average test score on interactions between admission and private childcare availability. High Private Availability indicates districts with above-median private slots per public/concertada slot (median = 2.20). Column (1) pools all age levels; columns (2)–(4) report estimates separately by application level. All specifications include lottery fixed effects and controls. Standard errors are clustered at the lottery level. *** p<0.01, ** p<0.05, * p<0.1.

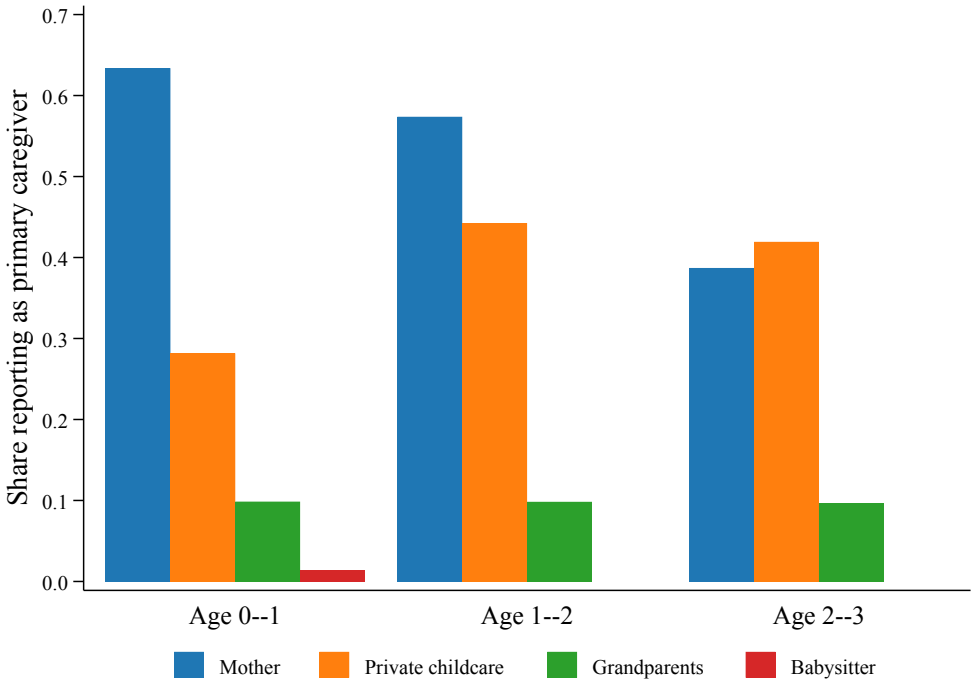
The distributional evidence in Figure 1 is consistent with this interpretation: the CDF for admitted applicants lies to the left of that for non-admitted applicants, indicating approximate first-order stochastic dominance of the non-admitted distribution. This shows that the effect is not driven only by a small subgroup at the tails but reflects a systematic shift across the entire distribution. This pattern fits a displacement story in which marginal applicants face a broadly lower-quality care environment after admission, rather than a mechanism that affects only the most fragile or the most advantaged children.

The subject-specific results reinforce this point. The negative effects are concentrated in Catalan, Spanish, and Mathematics, while English is close to zero, indicating that the impact is not uniform across skill domains. One plausible interpretation is that the counterfactual care for marginal applicants provides richer exposure to local-language interactions or numeracy-relevant inputs than the public alternative. The null effect for English is consistent with this interpretation: English proficiency at age 12 in Barcelona is predominantly determined by formal schooling rather than early care environments, since neither public childcare nor typical home or private care for children under three would involve substantial English exposure. The evidence does not isolate these channels directly, but the pattern suggests that early care quality matters primarily for skills that are actively developed during the 0-3 age window.

Finally, the age-gradient in Table 6 suggests that timing matters: language penalties are larger at the youngest entry level, whereas math penalties are more pronounced at later levels. This pattern aligns with the idea that early care environments are particularly salient for language development, while later entry may affect math-related skills more strongly.

Importantly, counterfactual care arrangements also vary systematically with child age. Figure 4 shows that among non-admitted families, maternal care declines substantially as children grow older: 63% of families rely primarily on the mother at age 0–1, compared to 57% at age 1–2 and only 39% at age 2–3. Conversely, private childcare use increases from 28% at age 0–1 to 44% at age 1–2, remaining stable at 42% for age 2–3. This pattern suggests that the counterfactual against which public childcare is compared shifts from predominantly home-based maternal care for younger children to a mix of maternal and private institutional care for older children. These differences in the counterfactual environment may contribute to the heterogeneous treatment effects observed across entry ages, though disentangling age-specific treatment effects from differences in displaced care remains an important challenge.

Figure 4: Counterfactual childcare arrangements by child age



Notes: Data from an independent survey of families in Catalonia who applied to public childcare. Sample restricted to families who were not admitted to their preferred center at each age level: age 0–1 (N=71), age 1–2 (N=61), age 2–3 (N=31). Each bar shows the share of families reporting that caregiver type as the primary daily caregiver. Source: Independent survey.

6.1.2 Administrative data on childcare enrollment

Administrative data on childcare enrollment in Barcelona provide population-level context for interpreting the survey evidence. Table 9 shows the share of children enrolled in public childcare, private childcare, or no formal childcare by age group, averaged over the years 2005–2010 (the cohorts in our estimation sample). The table reveals that at the youngest ages (0–1), nearly 90% of children are not enrolled in any formal childcare, with only 3% in public and 8% in private centers. Enrollment rises substantially with age: by ages 2–3, about 17% attend public childcare, 46% attend private childcare, and 37% remain outside formal care.

A comparison between the survey and administrative data is informative. Private childcare use is notably higher among non-admitted applicants in the survey (37% pooling across ages) than in the general population at younger ages (8% at ages 0–1, 30% at ages 1–2). This difference reflects selection: families who apply to public childcare have revealed a preference for formal care, making them more likely to enroll in private centers when denied a public slot. The general population, by contrast, includes many families who never sought formal childcare and rely exclusively on parental or informal care. This selection pattern implies that our survey-based estimates of the counterfactual—while directly relevant for the applicant population—overstate private childcare use relative to the population average.

One might argue that selection could operate in the opposite direction: families with a strong preference for private childcare may not apply to public centers in the first place, which would imply that public childcare applicants are negatively selected on private childcare use. However, several features of the Barcelona context make this unlikely. First, private childcare is substantially more expensive than public childcare, so most families—regardless of preferences—have a financial incentive to apply for a public slot. Second, the application process is low-cost and non-binding: families can apply to public centers while simultaneously exploring private options, and admission does not require enrollment. Third, given the limited availability of public slots, many families likely apply to public childcare as a hedge against the uncertainty of securing affordable care. These institutional features suggest that the applicant pool includes families across the spectrum of preferences for formal care, rather than being restricted to those with a strong aversion to private options.

Conversely, the administrative data show that even among the general population, home-based care dominates at the youngest ages: 89% of children aged 0–1 are not enrolled in any formal childcare. This population-level pattern reinforces the interpretation that, for the youngest applicants in our sample, the counterfactual to public childcare is predominantly parental care rather than private institutional care. The administrative data also highlight the limited reach of public childcare: even at ages 2–3, only about

one in six children in Barcelona attends a public center.

Together, the two data sources paint a consistent picture. The survey evidence, which targets the relevant population of applicants, shows that maternal care is the dominant counterfactual arrangement for non-admitted families. The administrative data confirm that formal childcare coverage is low at young ages across the entire population, implying that home-based care is the norm rather than the exception. Both sources support the interpretation that marginal applicants to oversubscribed public centers face a counterfactual environment where home-based care—rather than private institutional care—is the most common alternative.

Table 9: Childcare enrollment shares by age group (administrative data)

	Public	Private	No childcare
0–1	3.0%	7.8%	89.2%
1–2	9.5%	29.7%	60.8%
2–3	16.9%	45.7%	37.3%

Notes: Shares of children enrolled in public childcare, private childcare, or no formal childcare in Barcelona, averaged over academic years 2005–06 to 2009–10. "No childcare" is the residual (1 minus total enrollment rate). Source: District enrollment administrative data.

6.2 Mechanisms: later school selection

Attending a public childcare center may influence parental decisions regarding subsequent school enrollment, which in turn could affect later academic outcomes. In this section, we investigate whether our results are driven by this mechanism.

Table 10 shows that admission to a public childcare center significantly increases the likelihood of later attending a public school, both at the pre-primary and primary levels. Several factors could explain this pattern, including peer effects during the childcare years or simple inertia within the public education system. Alternatively, children who are not admitted to public childcare may be more likely to attend private centers, thereby generating opposite inertia effects.

Table 10: Effect of childcare admission on later enrollment in public or private Schools

	(1) Public 3-5	(2) Public Primary	(3) Public Secondary
Admitted	0.047**	0.031**	0.019
	(0.020)	(0.013)	(0.013)
Observations	3717	7980	7926
Mean	0.58	0.59	0.51

Notes: This table reports estimates of equation (1). *Panel A* only includes controls for the individual risk set by including a full set of dummy variables for each level by center and year (see Section 4.3). *Panel B* adds controls for individual and census grouping characteristics. Standard errors clustered at the lottery level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

If public pre-primary and primary schools differ in quality from private alternatives, our estimates might reflect these differences rather than the direct impact of early childcare attendance. While we lack direct measures of school quality, we approximate it using the average test scores of peers in the same primary school.[13] Table 11 presents this analysis. Column (1) shows that admission to public childcare does not significantly alter the average test scores of peers in primary school. The only exception is a small, marginally significant reduction in the Spanish test scores of peers, which, in any case, is an order of magnitude smaller than the main effect on individual scores.

These findings suggest that differences in subsequent school quality or selectivity are unlikely to drive our results. Instead, the observed test score gaps appear to stem directly from early exposure to childcare.

[13]We compute average peer test scores excluding the individual's own score to avoid mechanically capturing the treatment effect. However, note that some residual concern remains, as peers are also more likely to have attended public childcare.

Table 11: Effect of childcare admission on average test score of peers in primary school

	(1) Average Test Score Peers	(2) Test Score Catalan Peers	(3) Test Score Spanish Peers	(4) Test Score English Peers	(5) Test Score Maths Peers
Admitted	-0.011	-0.002	-0.015*	-0.007	-0.008
	(0.009)	(0.008)	(0.008)	(0.008)	(0.009)
Observations	7801	7805	7804	7804	7807
Mean	0.02	0.01	0.01	0.02	0.02

Notes: This table reports estimates of equation (1). *Panel A* only includes controls for the individual risk set by including a full set of dummy variables for each level by center and year (see Section 4.3). *Panel B* adds controls for individual and census grouping characteristics. Standard errors clustered at the lottery level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Lastly, in Table 12, we demonstrate that our main results are robust to the inclusion of primary school fixed effects. Panel A reports estimates with the risk set fixed effects. Panel B augments the specification by including also primary school fixed effects, which are potentially endogenous. The similarity of the estimates across both panels further supports the interpretation that our findings are not driven by differences in subsequent school environments. Instead, they reflect the causal impact of early childcare enrollment relative to the relevant counterfactual—namely, staying at home.

Table 12: Effect of childcare admission on standardized test scores at age 12. Robustness to primary school fixed effects.

	(1) Average Test Score	(2) Test Score Catalan	(3) Test Score Spanish	(4) Test Score English	(5) Test Score Maths
Admitted	-0.082***	-0.062**	-0.080***	-0.019	-0.099***
	(0.029)	(0.028)	(0.029)	(0.030)	(0.029)
Observations	7980	7980	7980	7980	7980
Mean	0.00	0.00	0.00	0.00	0.00

Notes: This table reports estimates of equation (1). *Panel A* includes controls for the individual risk set by including a full set of dummy variables for each level by center and year (see Section 4.3). *Panel B* controls additionally for the primary school of the individual. Standard errors clustered at the lottery level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

6.3 Mechanisms: quality of childcare

Our identification strategy focuses on children who applied to childcare centers where demand exceeded supply. As a result, children who were admitted joined classrooms operating at or near full capacity. The maximum child–teacher ratios for the first cycle of early childhood education (ages 0–3) in Barcelona are regulated by Decree 282/2006, dated July 4.[14] This regulation applies to both public and authorized private centers and establishes the following maximum group sizes per educator: 8 for level 1 (ages 0–1), 13 for level 2 (ages 1–2), and 20 for level 3 (ages 2–3). Note that the regulation uses a different numbering convention (levels 1, 2, 3) than Barcelona's application system (i0, i1, i2), though both cover the same age ranges.

In Table 13, we report the average number of children per teacher in public and private childcare centers in Catalonia for academic years 2006–2007 to 2012–2013.[15] While average ratios across public and private centers appear broadly similar—and even slightly higher in private centers for level 1—it is important to note that our analytical sample is limited to oversubscribed public centers. In such settings, maximum ratios are more likely to be binding.

Consequently, the effective child–teacher ratios in public centers within our sample are expected to exceed the reported averages. Based on regulatory limits, we estimate that children admitted to oversubscribed public centers likely faced child–teacher ratios that were approximately 1.5 students higher at Age 0–1, 0.7 students higher at Age 1–2, and 3 students higher at Age 2–3, relative to their counterparts in private childcare. These differences suggest that children not admitted to public childcare but enrolled in private centers may have experienced significantly more favorable staff-to-child ratios. However, it is worth noting that these figures are based on regional averages and may not fully capture conditions specific to Barcelona.

Table 13: Average Class Size by Level and Childcare Type
(Averages for school years 2006–2007 to 2012–2013)

	Public Childcare	**Private Childcare**	**Difference**
Age 0–1	7.4	6.5	0.9
Age 1–2	12.1	12.3	−0.2
Age 2–3	18.0	17.0	0.9

Notes: Class size reports the average number of children per caregiver. Difference is public minus private. Averages are computed over school years 2006–2007 to 2012–2013.

[14] See: https://portaljuridic.gencat.cat/ca/document-del-pjur/?documentId=404093

[15] Data source: educacio.gencat.cat/.../a0207.xlsx. Appendix D provides a detailed description of the regulatory framework governing childcare quality in Catalonia, including qualification requirements and physical infrastructure standards.

These child–teacher ratios are substantially higher than those observed in many other developed countries, and particularly higher than those featured in prior studies examining the effects of early childcare. For example, Felfe, Nollenberger and Rodríguez-Planas (2015) and Drange and Havnes (2019) analyze early education settings with adult-to-child ratios as low as 1:3, which allow for much more individualized attention and interaction. Similarly, Fort, Ichino and Zanella (2020) report adult-to-child ratios of 1:4 for infants (age 0) and 1:6 for toddlers (ages 1–2) in their study of daycare impacts in Bologna, Italy.

Compared to these benchmarks, the regulated maximum ratios in Barcelona—8:1 for infants, 13:1 for toddlers, and 20:1 for children aged 2–3—represent considerably less intensive staffing. In oversubscribed public centers, these ratios are likely to be at the upper bound, further exacerbating the gap in adult availability. This discrepancy in adult–child interaction may help explain the negative effects we observe: settings with more children per caregiver may struggle to deliver the appropriate stimulation that is critical in the earliest years of life.

This also highlights an important interpretive point: our findings should not be read as a critique of early childcare per se, but rather as evidence that quality—particularly in terms of staffing ratios—matters profoundly when evaluating the long-term impacts of such programs.

7 Conclusions

This paper offers new evidence on the long-term effects of early childcare for children under age three. Leveraging randomized admission via lotteries in oversubscribed municipal centers in Barcelona, we find that access to early childcare leads to a persistent reduction in standardized test scores at age 12. These effects vary by subject and age at entry: language penalties are particularly pronounced for children admitted at the youngest ages, while mathematics effects are more evident at older ages. Effects are negative for children from both high- and low-education households, with no statistically significant difference between the two groups.

Importantly, our results do not suggest that early childcare is inherently harmful. Rather, they highlight that in systems with limited resources, expanding access without commensurate investment in quality can displace more enriching alternatives and lead to unintended negative effects. Notably, the negative effects are present in areas with both high and low private childcare availability, and the estimates are not statistically distinguishable across these groups. This pattern is inconsistent with a simple interpretation in which public childcare is merely inferior to private childcare; instead, it suggests that the relevant counterfactual for marginal applicants includes high-quality home care. The average child in our sample—applying to a municipal center but not receiving income-based priority—was likely to face a favorable home environment with

educated parents. From a policy perspective, this is precisely the marginal group that would receive the first additional seats in an expansion toward universal coverage, so our ITT estimates—representing the effect of offering additional seats to 30-point applicants—are directly relevant for capacity decisions. Since 76% of applicants fall into this group, the findings are informative for a broad segment of the applicant population. Crucially, the negative effects are not confined to high-education families: children from households without post-secondary education also experience statistically significant reductions in test scores, indicating that the costs of low-quality early childcare are not limited to families with exceptionally rich home environments. Whether effects differ for children at the very bottom of the socioeconomic distribution—those with income-based or disability priority who are admitted outside the lottery—remains an open question, as their counterfactual care environments are likely less enriching, which could shift the balance in favor of public childcare for this group. In such contexts, the developmental returns to early childcare may be negative unless quality is high and staff–child ratios are appropriate to the age and needs of the child.

Our findings contribute to a growing body of evidence that the benefits of early childhood education are not universal, but context-dependent. They point to the need for early care policies to be designed with careful attention to who is being served, what the alternative care arrangements are, and whether the public system has the capacity to deliver high-quality developmental environments at scale.

Future work should explore the interaction between childcare quality, family background, and developmental timing more directly. In particular, understanding how institutional features—such as staff qualifications, curricula, and pedagogical practices—mediate the long-run effects of early care could provide crucial guidance for designing equitable and effective childcare systems.

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A Robustness Checks and Threats to Identification

A.1 External Validity: Lottery Sample vs. All Applicants

Table 14: Comparison of Lottery Sample and Full Applicant Pool

Variable	Mean Lottery	Mean Applicants	Difference	p-value
Panel A: Individual and family characteristics				
# Preferences expressed	2.39	2.03	0.30***	0.00
Female child	0.48	0.48	0.01	0.31
Age child at application	1.21	1.20	0.01	0.25
# siblings who applied before	0.06	0.12	-0.08***	0.00
Age Mother	33.74	33.73	0.20**	0.02
Age Father	35.74	36.00	-0.08	0.41
Spanish nationality Mother	0.83	0.73	0.14***	0.00
Spanish nationality Father	0.82	0.72	0.15***	0.00
Mother has Bachelor Degree	0.70	0.67	0.07***	0.00
Father has Bachelor Degree	0.63	0.63	0.04***	0.00
Both parents with a Bachelor Degree	0.53	0.52	0.05***	0.00
Resides with Mother	0.91	0.92	-0.02***	0.00
Resides with Father	0.85	0.85	-0.01	0.35
Resides with both parents	0.81	0.82	-0.02**	0.01
Panel B: Neighborhood characteristics (agrupació censal)				
Foreigners from Low-income countries (%)	12.50	12.77	-0.13	0.13
Population w/o post-secondary(%)	15.40	14.41	-0.06	0.24
Young w/o post-secondary (%)	21.62	20.88	-0.66***	0.00
Employed population(%)	64.98	64.78	0.06	0.27
Average income (€)	14,648.17	15,212.49	141.74***	0.00
Low qualified workers (%)	9.43	9.06	-0.31***	0.00
Observations	7,980	20,818		

Notes: *Mean Lottery* and *Mean Applicants* are sample means for the lottery sample and the full applicant pool in 2005–06 to 2009–10. *Difference* reports the coefficient from regressing each covariate on *lottery_sample* with lottery fixed effects; p-values are two-sided and standard errors are clustered at the lottery level. The Observations row reports the lottery sample size used in the paper and the total number of applicants in 2005–06 to 2009–10. *** p<0.01, ** p<0.05, * p<0.1.

Table 14 compares the lottery estimation sample with the full pool of applicants in the same years and shows meaningful, but interpretable, differences. The largest gap is in prior siblings (0.06 vs 0.12), which is expected because having a sibling already in the

school yields priority points and places applicants outside the lottery. Other differences are concentrated in priority-related traits: Spanish nationality and cohabitation are more prevalent in the lottery sample, and parental education is modestly higher, consistent with the working sample being somewhat more advantaged. The working sample also lists about 0.30 more preferences on average. There are two main reasons applicants fall outside the working sample: they do not have 30 points, or the school-level-year they applied to did not run a lottery (either demand did not exceed supply, or the marginal applicants had more or fewer than 30 points). Overall, the imbalances suggest the lottery sample is somewhat more educated and family-stable than the full pool, but the gaps are not extreme. For context, Fort, Ichino and Zanella (2020) study a working sample with a median income about 2.5 times the Italian median; our applicant pool is far closer to the population average.

A.2 Attrition from the Test Score Sample

Our main analysis restricts the sample to children for whom we observe test scores at age 12. This raises the concern that attrition from the test score sample may differ between admitted and rejected applicants, potentially biasing our estimates. To address this, we test whether lottery assignment predicts attrition.

Table 15 reports attrition rates by admission status. The attrition rate—defined as the share of lottery-eligible applicants without test scores at age 12—is 18% among admitted children and 20% among rejected children. Once we control for the lottery fixed effects, the difference is not statistically significant ($p = 0.47$), indicating no differential attrition by treatment status. This result holds across all age levels: at each level, admitted children have slightly lower attrition than rejected children, but none of the differences are statistically significant (p-values range from 0.56 to 0.79). These results support the validity of our estimates: the sample restriction to children with test scores does not introduce systematic selection bias.

Table 15: Attrition Check: Does Admission Predict Missing Test Scores?

Variable	**Mean Admitted**	**Mean Rejected**	**Difference**	**p-value**
Attrition (all levels)	0.18	0.20	-0.01	0.47
Age 0–1	0.17	0.18	-0.01	0.63
Age 1–2	0.17	0.20	-0.01	0.56
Age 2–3	0.19	0.22	-0.00	0.79
Observations	3307	6622		

Notes: Attrition equals 1 if a lottery-eligible child does not have test scores at age 12. *Mean Admitted*: mean attrition rate for applicants admitted to their first choice. *Mean Rejected*: mean attrition rate for applicants not admitted to their first choice. *Difference*: coefficient from regressing attrition on admission status with lottery fixed effects; standard errors clustered at the lottery level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

A.3 Priority Score Admissions

Table 16: Priority Score Admissions

	%
Home in Catchement area	92.93
Home in Municipality	2.24
Workplace in Catchement area	1.97
Siblings in Childcare center	5.00
Guaranteed Minimum Income Beneficiary	1.63
Disability	2.77
Large/single-parent families	10.77
Chronic Illness	1.10
Exactly 30 points (catchment area only)	75.60
Observations	60,648

Notes: Entries report means and standard deviations of priority-score components in the applicant data. Variables are binary indicators, so means can be interpreted as shares. *Exactly 30 points* identifies applicants whose only priority comes from residing in the catchment area (no siblings, income support, disability, or other criteria). The Observations row reports the sample size when available.

A.4 Grade Retention

Table 17 examines effects on grade retention. The point estimates are essentially zero and statistically insignificant. However, grade retention is a rare event in our sample (2%), which limits statistical power: detecting an effect comparable to our test score findings would require approximately five times our current sample size.

Table 17: Grade Retention

	(1) Grade Retainer	(2) Grade Retainer Primary	(3) Grade Retainer Secondary
Admitted	0.000	-0.002	0.002
	(0.004)	(0.003)	(0.002)
Observations	7980	7980	7926
Mean	0.02	0.01	0.01

Notes: Coefficients are from equation (1) with lottery fixed effects and controls. Outcomes are grade retention indicators. Standard errors are clustered at the lottery level. The Mean row reports the sample mean. *** p<0.01, ** p<0.05, * p<0.1.

A.5 Admission in Lower Preferences

Table 18: Admission in Lower Preferences

Assignment outcome	**N**	**%**
Assigned to 1st preference	2704	33.9
Assigned to lower preference		
2nd preference	181	2.3
3rd preference	44	0.6
4th preference	12	0.2
5th preference	4	0.1
Not assigned to any school	5035	63.1
Total	7980	100.0

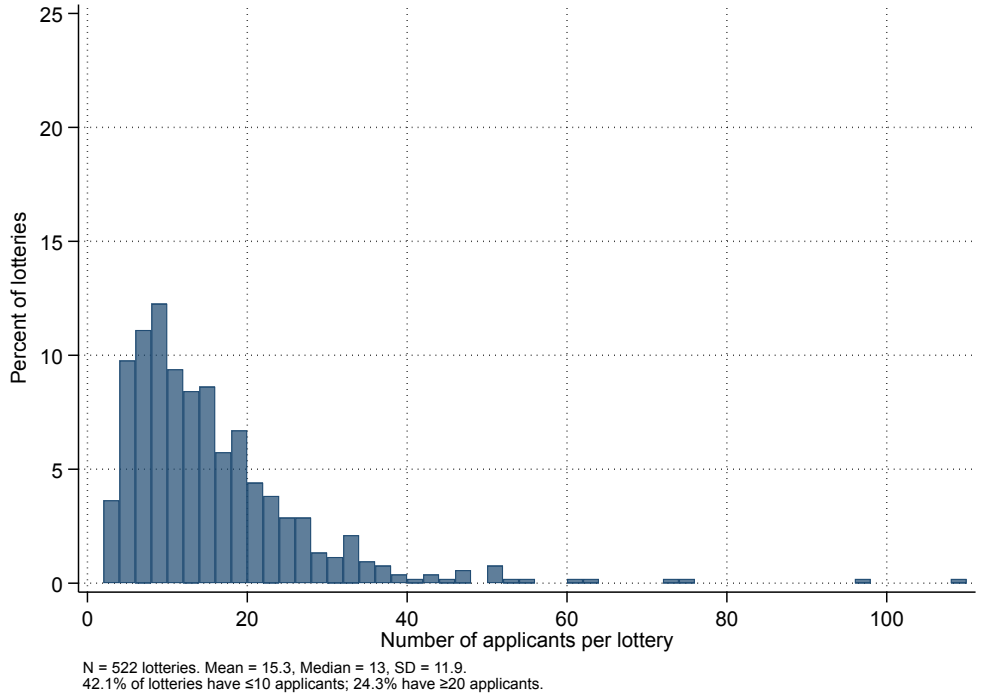
Notes: Distribution of applicants by assignment outcome. Only 3% of applicants are assigned to a lower preference (2nd–5th choice), while the majority either receive their first choice or remain unassigned.

A.6 Lottery Size Distribution

Our identification strategy relies on comparing applicants who competed in the same lottery—that is, applicants who applied to the same first-choice school, in the same year, and for the same age level. We refer to these groups as "risk sets" or lotteries. Figure 5 shows the distribution of lottery sizes in our sample.

Our sample includes 522 lotteries with a median of 13 applicants per lottery. The large number of lotteries ensures that our estimates are not driven by any particular lottery. Moreover, most lotteries have a sufficient number of applicants to generate meaningful variation in treatment status.

Figure 5: Distribution of Lottery Size



Notes: Distribution of the number of applicants per lottery. A lottery (risk set) is defined as applicants competing for the same first-choice school in the same year and age level. The sample includes 522 lotteries with a total of 7,980 applicants.

A.7 Robustness: Admission to Any School

Our main analysis compares children admitted to their first-choice school with those not admitted to their first choice. Here we present results using an alternative treatment definition: T_{all}, which equals one if the child was assigned to *any* public childcare center (first choice or otherwise), and zero otherwise.

Table 19 presents a balance check for the alternative treatment variable T_{all}. The table shows that admitted and non-admitted applicants are well balanced on most observable characteristics. The only statistically significant difference is in the number of preferences expressed: admitted applicants listed 0.12 more preferences on average. This difference is mechanical—applicants who list more preferences have more opportunities to be assigned—and does not reflect a failure of random assignment. All other differences are small and statistically insignificant.

Table 19: Balance Check: Admission to Any School (T_{all})

Variable	Mean Admitted	Mean Rejected	Difference	p-value
Panel A: Individual and family characteristics				
# Preferences expressed	2.52	2.32	0.12***	0.00
Female child	0.48	0.48	-0.02	0.13
Age child at application	1.29	1.17	-0.01	0.26
# siblings who applied before	0.06	0.06	0.00	0.53
Age Mother	33.58	33.84	0.12	0.33
Age Father	35.53	35.86	0.02	0.86
Spanish nationality Mother	0.83	0.83	-0.00	0.96
Spanish nationality Father	0.83	0.82	-0.01	0.56
Mother has Bachelor Degree	0.67	0.72	0.01	0.32
Father has Bachelor Degree	0.59	0.66	-0.01	0.39
Both parents with a Bachelor Degree	0.48	0.55	0.01	0.46
Resides with Mother	0.92	0.91	0.00	0.80
Resides with Father	0.85	0.86	-0.01	0.17
Resides with both parents	0.81	0.81	-0.01	0.28
Panel B: Neighborhood characteristics (agrupació censal)				
Foreigners from Low-income countries (%)	13.21	12.08	-0.04	0.73
Population w/o post-secondary(%)	16.49	14.77	-0.10	0.16
Young w/o post-secondary (%)	23.95	20.26	-0.17	0.32
Employed population(%)	64.40	65.32	0.07	0.30
Average income (€)	13,965.37	15,047.54	57.68	0.24
Low qualified workers (%)	10.59	8.75	-0.08	0.33
Observations	2,945	5,035		

Notes: *Mean Admitted* and *Mean Rejected* are sample means for applicants assigned and not assigned to any public childcare center. *Difference* reports the coefficient from regressing each covariate on T_{all} with lottery fixed effects; p-values are two-sided and standard errors are clustered at the lottery level. *** p<0.01, ** p<0.05, * p<0.1. Joint F-test: F = 1.76 (p = 0.02).

Table 20 reports the effect of being assigned to any public childcare center on test scores at age 12. The specification controls for the number of preferences expressed, which addresses the imbalance documented in Table 19. The results are similar to our main findings: admission reduces average test scores by 0.068 standard deviations ($p < 0.01$). Effects are negative and significant for Catalan , Spanish, and Mathematics , but not for English. These patterns closely mirror the main results, confirming that our findings are robust to this alternative definition of treatment. One might expect the estimates of Table 20 results for T_all to be greater than those for T, because we are removing

from the control group observations that were essentially treated. However, this is not necessarily the case if people admitted into lower preference schools are less likely to be enrolled than those admitted in the first preference.

Table 20: Effect of Admission to Any School on Test Scores (T_{all})

	(1) Average Test Score	(2) Test Score Catalan	(3) Test Score Spanish	(4) Test Score English	(5) Test Score Maths
Admitted	-0.068***	-0.048*	-0.074***	-0.016	-0.078***
	(0.026)	(0.026)	(0.027)	(0.027)	(0.028)
Observations	7980	7980	7980	7980	7980
Mean	0.00	0.00	0.00	0.00	0.00

Notes: Coefficients are from equation (1) with lottery fixed effects and controls, using T_{all} (assigned to any school) as the treatment variable. Standard errors are clustered at the lottery level. The Mean row reports the sample mean. *** p<0.01, ** p<0.05, * p<0.1.

A.8 Balance by Age Level

Tables 21–23 present balance checks separately for each age level. Overall, admitted and rejected applicants are well balanced within each level. A few differences emerge at the 10% level in neighborhood characteristics, but these are consistent with sampling variation given the number of comparisons.

Table 21: Balance Check: Age 0–1

Variable	Mean Admitted	Mean Rejected	Difference	p-value
Panel A: Individual and family characteristics				
# Preferences expressed	2.55	2.36	-0.08	0.47
Female child	0.51	0.48	0.01	0.77
Age child at application	0.30	0.30	-0.01	0.34
# siblings who applied before	0.02	0.03	-0.00	0.81
Age Mother	33.45	33.61	0.22	0.55
Age Father	35.07	35.29	-0.11	0.78
Spanish nationality Mother	0.85	0.88	-0.02	0.54
Spanish nationality Father	0.83	0.87	-0.03	0.32
Mother has Bachelor Degree	0.80	0.82	0.02	0.60
Father has Bachelor Degree	0.69	0.74	-0.00	0.90
Both parents with a Bachelor Degree	0.60	0.67	0.00	0.97
Resides with Mother	0.87	0.88	-0.01	0.77
Resides with Father	0.83	0.82	0.01	0.81
Resides with both parents	0.77	0.76	0.00	0.93
Panel B: Neighborhood characteristics (agrupació censal)				
Foreigners from Low-income countries (%)	12.89	11.35	0.38	0.12
Population w/o post-secondary(%)	15.70	13.45	0.27	0.18
Young w/o post-secondary (%)	21.56	17.28	0.77*	0.06
Employed population(%)	64.57	65.90	-0.23	0.19
Average income (€)	14,438.80	15,691.29	-187.35	0.15
Low qualified workers (%)	9.43	7.30	0.17	0.37
Observations	265	528		

Notes: Sample restricted to applicants for childcare at age 0–1. *Mean Admitted* and *Mean Rejected* are sample means. *Difference* reports the coefficient from regressing each covariate on admission status with lottery fixed effects; p-values are two-sided and standard errors are clustered at the lottery level. *** p<0.01, ** p<0.05, * p<0.1. Joint F-test: F = 0.73 (p = 0.79).

Table 22: Balance Check: Age 1–2

Variable	Mean Admitted	Mean Rejected	Difference	p-value
Panel A: Individual and family characteristics				
# Preferences expressed	2.59	2.46	0.05	0.33
Female child	0.47	0.47	-0.02	0.32
Age child at application	0.93	0.93	-0.00	0.82
# siblings who applied before	0.04	0.05	0.02*	0.06
Age Mother	33.37	33.75	0.08	0.64
Age Father	35.46	35.74	0.19	0.31
Spanish nationality Mother	0.87	0.84	0.00	0.74
Spanish nationality Father	0.86	0.83	0.01	0.65
Mother has Bachelor Degree	0.71	0.73	0.03	0.13
Father has Bachelor Degree	0.61	0.67	-0.01	0.69
Both parents with a Bachelor Degree	0.51	0.56	0.01	0.69
Resides with Mother	0.92	0.91	0.01	0.36
Resides with Father	0.86	0.86	-0.01	0.45
Resides with both parents	0.82	0.82	-0.01	0.64
Panel B: Neighborhood characteristics (agrupació censal)				
Foreigners from Low-income countries (%)	13.35	12.43	0.04	0.83
Population w/o post-secondary(%)	16.60	15.13	-0.19*	0.09
Young w/o post-secondary (%)	24.70	20.89	-0.19	0.48
Employed population(%)	64.12	65.04	0.07	0.49
Average income (€)	13,791.89	14,849.20	47.67	0.49
Low qualified workers (%)	10.83	9.09	-0.08	0.50
Observations	1,257	3,083		

Notes: Sample restricted to applicants for childcare at age 1–2. *Mean Admitted* and *Mean Rejected* are sample means. *Difference* reports the coefficient from regressing each covariate on admission status with lottery fixed effects; p-values are two-sided and standard errors are clustered at the lottery level. *** p<0.01, ** p<0.05, * p<0.1. Joint F-test: F = 1.21 (p = 0.25).

Table 23: Balance Check: Age 2–3

Variable	Mean Admitted	Mean Rejected	Difference	p-value
Panel A: Individual and family characteristics				
# Preferences expressed	2.29	2.18	-0.08	0.10
Female child	0.49	0.51	-0.02	0.37
Age child at application	1.89	1.90	-0.01	0.40
# siblings who applied before	0.08	0.10	-0.01	0.46
Age Mother	33.88	34.01	0.19	0.32
Age Father	35.86	36.13	0.02	0.94
Spanish nationality Mother	0.80	0.78	0.00	0.86
Spanish nationality Father	0.80	0.78	-0.01	0.60
Mother has Bachelor Degree	0.59	0.66	-0.02	0.23
Father has Bachelor Degree	0.54	0.60	-0.02	0.31
Both parents with a Bachelor Degree	0.42	0.48	-0.00	0.88
Resides with Mother	0.92	0.93	-0.01	0.54
Resides with Father	0.84	0.86	-0.03*	0.08
Resides with both parents	0.81	0.82	-0.03*	0.09
Panel B: Neighborhood characteristics (agrupació censal)				
Foreigners from Low-income countries (%)	12.98	11.93	-0.14	0.41
Population w/o post-secondary(%)	16.50	14.78	-0.04	0.71
Young w/o post-secondary (%)	23.72	20.54	-0.13	0.62
Employed population(%)	64.71	65.49	0.08	0.42
Average income (€)	14,096.99	15,020.61	101.41	0.24
Low qualified workers (%)	10.51	8.88	-0.09	0.48
Observations	1,182	1,670		

Notes: Sample restricted to applicants for childcare at age 2–3. *Mean Admitted* and *Mean Rejected* are sample means. *Difference* reports the coefficient from regressing each covariate on admission status with lottery fixed effects; p-values are two-sided and standard errors are clustered at the lottery level. *** p<0.01, ** p<0.05, * p<0.1. Joint F-test: F = 1.47 (p = 0.09).

B Additional Heterogeneous Effects

B.1 Gender

Table 24: Heterogeneity by gender

	(1) Average Test Score	(2) Average Test Score	(3) Average Test Score	(4) Average Test Score
Admitted x Female	-0.034	-0.159*	-0.024	-0.045
	(0.034)	(0.092)	(0.049)	(0.047)
Admitted x Male	-0.126***	-0.093	-0.112**	-0.149***
	(0.036)	(0.105)	(0.052)	(0.044)
Observations	7980	792	4532	4248
Mean	-0.00	0.20	0.03	-0.06
Sample	All levels	Age 0–1	Age 1–2	Age 2–3
p-value (H0: equal)	0.04	0.61	0.17	0.08

Notes: Coefficients are interactions between admission and gender. All specifications include lottery fixed effects and controls. Standard errors are clustered at the lottery level. *** p<0.01, ** p<0.05, * p<0.1.

B.2 Month of Birth

Table 25: Heterogeneity by month of birth

	(1) Average Test Score	(2) Average Test Score	(3) Average Test Score	(4) Average Test Score
Admitted x Jan-June	-0.114***	-0.126*	-0.071	-0.177***
	(0.036)	(0.074)	(0.054)	(0.050)
Admitted x July-Dec	-0.052		-0.074	-0.044
	(0.034)		(0.046)	(0.046)
Observations	7980	792	4532	4248
Mean	-0.00	0.20	0.03	-0.06
Sample	All levels	Age 0–1	Age 1–2	Age 2–3
p-value (H0: equal)	0.18	0.09	0.96	0.05

Notes: Coefficients are interactions between admission and month-of-birth group. All specifications include lottery fixed effects and controls. Standard errors are clustered at the lottery level. *** p<0.01, ** p<0.05, * p<0.1.

B.3 Results Including Re-Applicants

The main analysis excludes children who applied to public childcare more than once across different years. This subsection presents results including these re-applicants for completeness. The sample sizes are larger, but the estimates are similar to the main results.

Table 26: Effect of childcare admission on test scores by application level (including re-applicants)

	(1) Average Test Score	(2) Test Score Catalan	(3) Test Score Spanish	(4) Test Score English	(5) Test Score Maths
Panel A: Age 0–1 (entry at 4–12 months)					
Admitted	-0.126*	-0.161**	-0.175**	-0.021	-0.041
	(0.074)	(0.076)	(0.087)	(0.077)	(0.079)
Observations	792	792	792	792	792
Mean	0.20	0.10	0.08	0.18	0.24
Panel B: Age 1–2 (entry at 12–24 months)					
Admitted	-0.072*	-0.080**	-0.080**	0.005	-0.076*
	(0.039)	(0.038)	(0.040)	(0.037)	(0.039)
Observations	4532	4532	4532	4532	4532
Mean	0.03	0.04	0.01	0.03	0.00
Panel C: Age 2–3 (entry at 24–36 months)					
Admitted	-0.098***	-0.037	-0.074**	-0.076**	-0.110***
	(0.034)	(0.035)	(0.033)	(0.037)	(0.037)
Observations	4248	4248	4248	4248	4248
Mean	-0.06	-0.06	-0.02	-0.07	-0.05

Notes: Each panel reports estimates of equation (1) for applicants to each childcare level. This sample includes children who applied more than once across different years. Standard errors are clustered at the lottery level. *** p<0.01, ** p<0.05, * p<0.1.

Table 27: Heterogeneity by parents' education (including re-applicants)

	(1) Average Test Score	(2) Average Test Score	(3) Average Test Score	(4) Average Test Score
Admitted x High Edu	-0.090***	-0.191**	-0.076	-0.088*
	(0.030)	(0.074)	(0.046)	(0.045)
Admitted x Low Edu	-0.083**	-0.014	-0.069	-0.107**
	(0.036)	(0.124)	(0.059)	(0.049)
Observations	9572	792	4532	4248
Mean	0.00	0.20	0.03	-0.06
Sample	All levels	Age 0–1	Age 1–2	Age 2–3
p-value (H0: equal)	0.87	0.17	0.92	0.76

Notes: "High Education" families have both parents with a post-secondary education; "Low Education" families have at least one parent without a post-secondary education. This sample includes children who applied more than once across different years. All specifications include lottery fixed effects and controls. Standard errors are clustered at the lottery level. *** p<0.01, ** p<0.05, * p<0.1.

Tables 28–30 present balance checks by age level for the sample including re-applicants.

Table 28: Balance Check: Age 0–1 (including re-applicants)

Variable	**Mean Admitted**	**Mean Rejected**	**Difference**	**p-value**
Panel A: Individual and family characteristics				
# Preferences expressed	2.55	2.36	-0.08	0.47
Female child	0.51	0.48	0.01	0.77
Age child at application	0.30	0.30	-0.01	0.34
# siblings who applied before	0.02	0.03	-0.00	0.81
Age Mother	33.45	33.61	0.22	0.55
Age Father	35.07	35.29	-0.11	0.78
Spanish nationality Mother	0.85	0.88	-0.02	0.54
Spanish nationality Father	0.83	0.87	-0.03	0.32
Mother has Bachelor Degree	0.80	0.82	0.02	0.60
Father has Bachelor Degree	0.69	0.74	-0.00	0.90
Both parents with a Bachelor Degree	0.60	0.67	0.00	0.97
Resides with Mother	0.87	0.88	-0.01	0.77
Resides with Father	0.83	0.82	0.01	0.81
Resides with both parents	0.77	0.76	0.00	0.93
Panel B: Neighborhood characteristics (agrupació censal)				
Foreigners from Low-income countries (%)	12.89	11.37	0.38	0.12
Population w/o post-secondary(%)	15.70	13.45	0.27	0.18
Young w/o post-secondary (%)	21.56	17.28	0.77*	0.06
Employed population(%)	64.57	65.88	-0.23	0.19
Average income (€)	14,438.80	15,689.44	-187.35	0.15
Low qualified workers (%)	9.43	7.31	0.17	0.37
Observations	265	527		

Notes: Sample restricted to applicants for childcare at age 0–1, including re-applicants. *Mean Admitted* and *Mean Rejected* are sample means. *Difference* reports the coefficient from regressing each covariate on admission status with lottery fixed effects; p-values are two-sided and standard errors are clustered at the lottery level. *** p<0.01, ** p<0.05, * p<0.1. Joint F-test: F = 0.73 (p = 0.79).

Table 29: Balance Check: Age 1–2 (including re-applicants)

Variable	**Mean Admitted**	**Mean Rejected**	**Difference**	**p-value**
Panel A: Individual and family characteristics				
# Preferences expressed	2.58	2.45	0.05	0.31
Female child	0.46	0.47	-0.02	0.33
Age child at application	0.95	0.95	-0.00	0.76
# siblings who applied before	0.05	0.05	0.02*	0.08
Age Mother	33.44	33.79	0.10	0.54
Age Father	35.48	35.77	0.17	0.33
Spanish nationality Mother	0.87	0.84	0.01	0.70
Spanish nationality Father	0.85	0.83	0.00	0.80
Mother has Bachelor Degree	0.71	0.74	0.03	0.11
Father has Bachelor Degree	0.61	0.67	-0.01	0.62
Both parents with a Bachelor Degree	0.52	0.57	0.01	0.69
Resides with Mother	0.92	0.91	0.01	0.25
Resides with Father	0.86	0.86	-0.01	0.67
Resides with both parents	0.83	0.82	-0.00	0.90
Panel B: Neighborhood characteristics (agrupació censal)				
Foreigners from Low-income countries (%)	13.39	12.42	0.10	0.53
Population w/o post-secondary(%)	16.59	15.10	-0.16	0.13
Young w/o post-secondary (%)	24.65	20.81	-0.13	0.62
Employed population(%)	64.10	65.05	0.07	0.50
Average income (€)	13,780.94	14,857.84	34.01	0.61
Low qualified workers (%)	10.82	9.05	-0.05	0.71
Observations	1,303	3,229		

Notes: Sample restricted to applicants for childcare at age 1–2, including re-applicants. *Mean Admitted* and *Mean Rejected* are sample means. *Difference* reports the coefficient from regressing each covariate on admission status with lottery fixed effects; p-values are two-sided and standard errors are clustered at the lottery level. *** p<0.01, ** p<0.05, * p<0.1. Joint F-test: F = 1.27 (p = 0.20).

Table 30: Balance Check: Age 2–3 (including re-applicants)

Variable	Mean Admitted	Mean Rejected	Difference	p-value
Panel A: Individual and family characteristics				
# Preferences expressed	2.32	2.15	-0.01	0.81
Female child	0.49	0.50	-0.00	0.79
Age child at application	1.90	1.92	-0.02**	0.03
# siblings who applied before	0.10	0.13	-0.02	0.17
Age Mother	34.07	34.47	0.04	0.81
Age Father	36.01	36.42	-0.06	0.77
Spanish nationality Mother	0.80	0.79	0.01	0.59
Spanish nationality Father	0.81	0.80	-0.01	0.56
Mother has Bachelor Degree	0.63	0.70	-0.01	0.45
Father has Bachelor Degree	0.56	0.64	-0.02	0.12
Both parents with a Bachelor Degree	0.46	0.53	-0.01	0.63
Resides with Mother	0.92	0.93	-0.00	0.95
Resides with Father	0.85	0.86	-0.02*	0.06
Resides with both parents	0.82	0.83	-0.02	0.15
Panel B: Neighborhood characteristics (agrupació censal)				
Foreigners from Low-income countries (%)	12.96	11.77	-0.12	0.36
Population w/o post-secondary(%)	16.26	14.37	-0.03	0.72
Young w/o post-secondary (%)	23.17	19.66	-0.03	0.88
Employed population(%)	64.69	65.51	0.05	0.55
Average income (€)	14,268.90	15,313.31	85.10	0.21
Low qualified workers (%)	10.26	8.47	-0.02	0.83
Observations	1,648	2,600		

Notes: Sample restricted to applicants for childcare at age 2–3, including re-applicants. *Mean Admitted* and *Mean Rejected* are sample means. *Difference* reports the coefficient from regressing each covariate on admission status with lottery fixed effects; p-values are two-sided and standard errors are clustered at the lottery level. *** p<0.01, ** p<0.05, * p<0.1. Joint F-test: F = 1.44 (p = 0.11).

C Survey Evidence: Applicants vs. Non-Applicants

This appendix presents descriptive evidence on the characteristics of families who apply to public childcare compared to those who do not. We use data from the same independent survey described in Section 6.1.1, conducted by Raquel Gallego and Sheila González Motos as part of the project "Models d'educació de 0 a 3 anys i participació en el mercat

laboral," funded by Recercaixa 2017 and ACUP.[16] The survey collected information from mothers of children born between 2016 and 2019 in Barcelona on family demographics, labor market participation, and childcare arrangements during the first three years of the child's life.

Table 31 compares applicants to public childcare (*escola bressol municipal*) with non-applicants across several dimensions. Applicants are defined as mothers who either enrolled their child in public childcare at any age between 0 and 3, or who applied but were not admitted. Non-applicants are mothers who never applied to public childcare.

The available variables from the survey include:

- **Mother's education**: Whether the mother completed university education (post-secondary education or higher).
- **Mother's origin**: Whether the mother was born abroad or in Barcelona.
- **Labor market status**: Whether the mother worked before the child's birth, her employment type (employee vs. self-employed), and contract type (permanent vs. temporary).
- **Household income**: Total household income before the child's birth, categorized into brackets. We define "high income" as households earning more than 25,000 EUR annually.
- **Income share**: Whether the mother was the main earner in the household (providing more than half of household income).
- **Household composition**: Number of children and partner's education level.

The results show that applicants to public childcare are positively selected relative to non-applicants. Applicants are significantly more likely to have a university degree (51.3% vs. 45.1%), to be born in Barcelona (45.6% vs. 39.2%), and to have higher household income (55.4% vs. 47.6% with income above 25,000 EUR). Partners of applicants are also more educated (46.3% vs. 38.7% with university degrees). These patterns are consistent with the observation that public childcare is oversubscribed and that families with higher socioeconomic status may be better positioned to navigate the application process.

The child's birth year also differs significantly between groups: applicants' children were born earlier in the sample period (mean 2017.0 vs. 2017.4). This may reflect that the survey was conducted in 2020, so children born in later years (2018–2019) had fewer opportunities to apply before the survey date.

These descriptive patterns complement our main analysis in two ways. First, they confirm that the population applying to public childcare is not representative of all families—applicants are positively selected on education and income. Second, they provide context for interpreting our estimates: the negative effects we document in the main text apply to this relatively advantaged population of applicants, not to the general population.

[16] See González-Motos and Saurí (2023) and Saurí and González Motos (2022) for related qualitative analyses from this research project.

Table 31: Comparison of Applicants vs. Non-Applicants to Public Childcare (Survey Data)

	Mean Applicants	**Mean Non-applicants**	**Difference**	**p-value**
Panel A: Mother characteristics				
Mother has university degree	0.513	0.451	0.062**	0.030
Mother born abroad	0.120	0.149	-0.028	0.146
Mother born in Barcelona	0.456	0.392	0.064**	0.024
Panel B: Labor and income characteristics				
Mother worked before birth	0.929	0.910	0.019	0.232
Mother was employee	0.463	0.353	0.110	0.290
Had permanent contract	0.526	0.444	0.082	0.630
Income > 25,000 EUR	0.554	0.476	0.078***	0.008
Mother is main earner	0.542	0.587	-0.045	0.122
Panel C: Household and partner characteristics				
Number of children (0-18)	1.63	1.56	0.07*	0.092
Children born since 2016	1.18	1.14	0.04*	0.089
Partner has university degree	0.463	0.387	0.077***	0.008
Child's birth year	2017.01	2017.37	-0.36***	0.000
Observations	616	599		

Notes: Data from the Gallego and González Motos survey (2020) of mothers of children born 2016–2019 in Barcelona. Applicants are families who applied to public childcare (*escola bressol municipal*) at any age between 0–3, whether admitted or not. Non-applicants are families who never applied. "University degree" indicates completion of a post-secondary education or higher. "High income" indicates household income above 25,000 EUR before the child's birth. "Main earner" indicates the mother provided more than half of household income. Differences are estimated via OLS regression of each variable on applicant status with robust standard errors. * p<0.10, ** p<0.05, *** p<0.01.

D Institutional Background: Quality of Childcare in Catalonia

This appendix describes the quality standards governing early childhood education centers (ages 0–3) in Catalonia during our study period. The primary regulatory framework is Decree 282/2006, dated July 4, issued by the Generalitat de Catalunya, which establishes minimum requirements for all authorized centers—both public and private.[17]

[17]Decree 282/2006, of July 4, on the requirements for first-cycle early childhood education centers (*Decret 282/2006, de 4 de juliol, dels requisits dels centres del primer cicle de l'educació infantil*). Available

D.1 Child–Teacher Ratios

Article 12 of Decree 282/2006 establishes the maximum number of children per group:

- Children under 1 year (level i0): **8 children** per group
- Children aged 1–2 years (level i1): **13 children** per group
- Children aged 2–3 years (level i2): **20 children** per group

These ratios are substantially higher than those observed in settings where early childcare has been shown to have positive effects on child development. Table 32 compares the Catalan regulatory limits with ratios from other studies in the literature.

Table 32: Child–Teacher Ratios: Catalonia vs. Other Settings

Setting	**Study**	**Age 0–1**	**Age 1–2**	**Age 2–3**
Catalonia (regulatory max.)	This paper	8:1	13:1	20:1
Bologna, Italy	Fort, Ichino and Zanella (2020)	4:1	6:1	8:1
Oslo, Norway	Drange and Havnes (2019)	3:1	3:1	6:1
Perry Preschool (US)	Schweinhart et al. (2005)		3:1 to 6:1	
Abecedarian (US)	Campbell et al. (2012)		3:1 to 6:1	

Notes: Ratios express maximum children per educator. Catalan ratios are regulatory maximums from Decree 282/2006. Other ratios are from the cited studies. Abecedarian enrolled children from infancy (around 4 months) through age 5. Perry Preschool enrolled children at ages 3–4; ratios shown reflect program standards rather than infant care.

The comparison reveals that Catalan centers operate with ratios roughly 2–4 times higher than those in settings with documented positive effects. This difference is particularly pronounced for infants: the 8:1 ratio for children under one year in Catalonia is double the 4:1 ratio in Bologna and nearly triple the 3:1 ratio in Oslo. Research consistently shows that ratios exceeding 8:1 for infants are associated with lower quality interactions (Burchinal et al., 2000).

D.2 Professional Qualifications

Article 11 of the decree specifies qualification requirements for educators:

- All educators must hold either: (a) a university degree in early childhood education (*maestro especialista en educación infantil*), or (b) a higher technical certificate in early childhood education (*técnico superior en educación infantil*).
- The minimum number of professionals present simultaneously must equal the number of groups in operation plus one, with an additional professional required for

at: https://portaljuridic.gencat.cat/ca/document-del-pjur/?documentId=404093. The decree was enacted following the Spanish Organic Law 2/2006 on Education (LOE) and the Catalan Law 5/2004 on the creation of quality nursery schools.

every three groups.

- At least one professional per three groups must hold a university-level teaching degree.

These qualification requirements ensure a baseline level of professional training but do not address the fundamental constraint imposed by high child–teacher ratios.

D.3 Physical Infrastructure

Article 13 establishes minimum space requirements:

- **Classrooms**: Minimum of 2 square meters per child, with each room at least 30 square meters.
- **Differentiated areas**: Rooms for children under 2 years must have separate areas for rest and hygiene.
- **Outdoor space**: A playground with minimum 2 square meters per child and at least 75 square meters total.
- **Support spaces**: A multipurpose room (minimum 30 m^2), administrative space (minimum 10 m^2), and a food preparation area or kitchen.

D.4 Authorization and Oversight

The decree establishes a formal authorization system for childcare centers:

- All centers providing regular care to children aged 0–3 must obtain authorization from the educational administration (Articles 1 and 9).
- Authorized centers must use the official denomination "llar d'infants" (nursery school) or "escola bressol" (cradle school), followed by "pública" or "privada" according to ownership (Article 6).
- Unauthorized establishments are prohibited from using these denominations, helping families distinguish between regulated and unregulated care.
- Local municipalities may assume delegated competencies for authorization and oversight of private centers (Article 20).

D.5 Implications for Interpreting Our Results

The regulatory framework has important implications for understanding our findings:

Binding constraints in oversubscribed centers. Our analysis focuses on oversubscribed public centers where demand exceeds supply. In these settings, classrooms operate at or near the maximum permitted ratios. This means admitted children likely experienced child–teacher ratios at the regulatory ceiling—8:1, 13:1, or 20:1 depending on age

level—which are considerably less favorable than those documented in studies finding positive effects of early childcare.

Uniform standards across sectors. The same regulations apply to both public and authorized private centers. Therefore, differences in observed ratios between sectors (Table 13) reflect differences in capacity utilization rather than differential regulatory standards. Private centers, facing less excess demand, are more likely to operate below the regulatory maximum.

D.6 Language of Instruction

Public nurseries (*escoles bressol*) used Catalan as the sole language of instruction, following the 1983 Linguistic Normalization Act. Private nurseries operated primarily in Catalan and Spanish; bilingual English programs were extremely rare and catered almost exclusively to international/expat families—a negligible share of the lottery sample used in this study. Content and Language Integrated Learning (CLIL) programs in Catalonia during this period were limited to primary and secondary schools, not early childhood education. Regarding home language exposure, the *Enquesta d'Usos Lingüístics de la Població* (Generalitat de Catalunya, 2008) reports that 90% of Barcelona residents identified Catalan, Spanish, or both as their *llengua inicial* (first language). These facts imply that neither public nor private childcare for ages 0–2 provided meaningful English exposure during our sample period (2005–2010).

D.7 EU Childcare Coverage

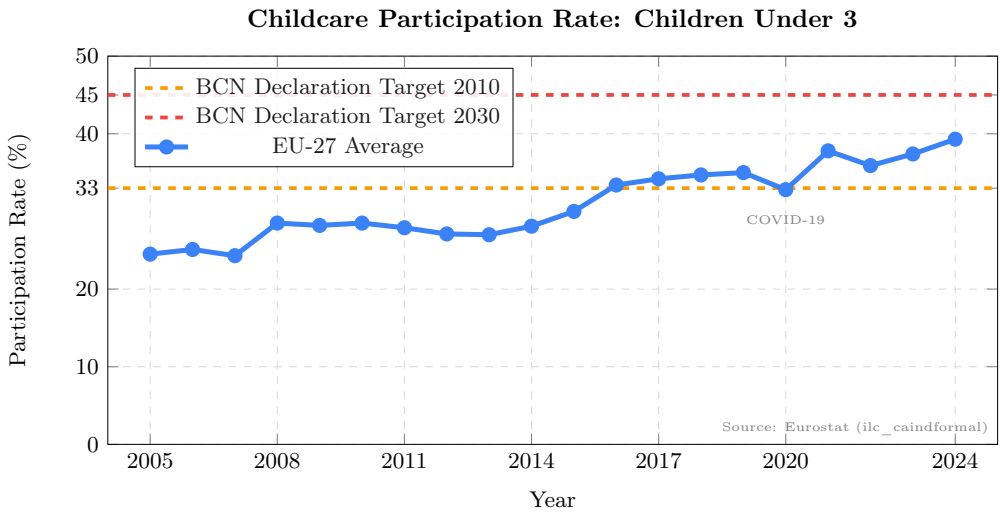


Figure 6: Childcare participation rate for children under 3 years: EU average
Notes: Participation rates in formal childcare for children under age 3 in the EU. The 2010 Barcelona target (33%) and 2030 target (45%) are shown as dashed lines. Source: Eurostat (ilc_caindformal).

E Data Appendix

This appendix provides detailed documentation of the data sources used in this study. We first describe the core individual-level dataset linked by IDESCAT, then the supplementary data sources used for neighborhood characteristics, aggregate market statistics, and survey evidence.

E.1 IDESCAT-Linked Administrative Data

The core dataset for this study consists of individual-level administrative records linked by the Catalan Statistical Institute (Institut d'Estadística de Catalunya, IDESCAT) using secure individual identifiers. The linked dataset integrates information from four administrative sources:

A. Childcare applications (IMEB). The Institut Municipal d'Educació de Barcelona (IMEB) provided administrative records on all applications to public childcare centers (*escoles bressol municipals*) in Barcelona for academic years 2005–06 to 2012–13.

Table 33: IMEB Childcare Application Data: Key Variables

Variable category	**Description**
Application identifiers	Child ID, household ID, application ID, academic year
Preferences	Ranked list of preferred childcare centers (up to 10)
Priority points	Total points; breakdown by criterion (proximity, siblings, income support, disability, single parent, etc.)
Lottery outcome	Assigned center (if admitted) or non-admission; lottery number
Child characteristics	Birth date (month/year), sex
Parental characteristics	Age, education level, nationality, employment status (at time of application)
Household characteristics	Census tract of residence, household identifier (to link siblings/twins)

Notes: Total observations: 60,648 applications (2005–06 to 2012–13).

B. Academic outcomes (Departament d'Educació). The Departament d'Educació of the Generalitat de Catalunya provided student enrollment records for pre-primary, primary, and secondary education.

Table 34: Educational Enrollment Data: Key Variables

Variable category	**Description**
Enrollment records	Grade level, academic year, school identifier
School characteristics	School type (public, private subsidized, private), geographic aggregation (census grouping, neighborhood)
Academic performance	Annual average grades (partially available for 2011/12–2013/14)

Notes: Total observations: 366,270 enrollment records.

C. Standardized test scores (Consell Superior d'Avaluació del Sistema Educatiu). The Consell Superior d'Avaluació del Sistema Educatiu provided standardized test scores for students completing 6th grade (final year of primary school) in academic years 2014/15–2018/19.

Table 35: Standardized Test Score Data: Key Variables

Variable category	**Description**
Test scores	Catalan, Spanish, English, Mathematics
Additional subjects	Sciences, French, Aranese (limited availability, excluded from analysis)
Test administration	Academic year, school identifier

Notes: Total observations: 19,092 test records. Test scores are standardized within cohort to have mean zero and standard deviation one.

D. Household characteristics (Padró Municipal d'Habitants). The Municipal Population Register of Barcelona provided household-level information linked to the child-care applicants.

Table 36: Municipal Population Register Data: Key Variables

Variable category	**Description**
Household composition	Household size, number of members
Co-residence	Indicators for co-residence with each tutor (proxy for single-parent households)
Demographics	Sex, month/year of birth, nationality (broad regions)
Location	Census grouping (*agrupació censal*) of residence

E.2 Supplementary Data Sources

The following data sources are not part of the IDESCAT-linked individual-level dataset. They provide aggregate statistics and survey evidence used to complement our main analysis.

E.2.1 Neighborhood Characteristics (IDESCAT)

We supplement the individual-level administrative data with neighborhood-level socioeconomic indicators from IDESCAT, measured at the *agrupació censal* level. Barcelona contains 190 *agrupacions censals*, which are intermediate geographic units between census tracts (*seccions censals*) and districts.

Table 37: Neighborhood Characteristics: Variables and Definitions

Variable	Definition
Foreign-born share (low-income countries)	Share of residents born in low-income countries
Population without post-secondary education	Share of adult population without post-secondary education
Young adults without post-secondary education	Share of population aged 25–34 without post-secondary education
Employment rate	Share of working-age population employed
Average household income	Mean household income in the *agrupació censal*
Low-qualified occupations	Share of workers in low-qualified occupations

Notes: All indicators measured in 2018.

E.2.2 District-Level Childcare Enrollment (Generalitat de Catalunya)

To characterize the aggregate childcare market in Barcelona, we use publicly available education statistics from the Generalitat de Catalunya's Departament d'Educació, combined with population data on newborns.

Education statistics. Annual data on childcare centers, classes, enrollment, and educators are available from the Departament d'Educació's statistical portal.[18]

Table 38: District-Level Education Statistics: Variables

Variable category	Description
Centers	Number of educational centers by district, sector (public/private), and level
Classes	Number of classes (units)
Enrollment	Number of children enrolled
Educators	Number of educators/teachers

Notes: Data disaggregated by Barcelona's 10 districts, school sector (public or private), and educational level (Early Childhood Education 1st cycle, ages 0–2). Academic years 2005–06 to 2009–10 used in main analysis.

Newborn data. To compute enrollment shares, we use district-level counts of newborns from the Municipal Population Register (Padró Municipal d'Habitants), which allows us to estimate the number of resident children aged 0–2 in each district and year.

[18] Available at https://educacio.gencat.cat/ca/departament/estadistiques/estadistiques-ensenyament/cursos-anteriors/.

Table 39: Newborn Data: Variables

Variable	Description
Newborns by district	Annual count of births registered in each of Barcelona's 10 districts
Children aged 0–2	Computed as sum of newborns in current year plus two preceding years

Constructed variables. Combining enrollment and population data, we compute:

- Share enrolled in public childcare = (Public enrollment) / (Children aged 0–2)
- Share enrolled in private childcare = (Private enrollment) / (Children aged 0–2)
- Share with no formal childcare = 1 − (Total enrollment) / (Children aged 0–2)

E.3 Construction of Heterogeneity Variables

This section documents the construction of the key variables used in the heterogeneity analyses reported in Section 5.5 (parental education) and the private childcare availability analysis in Section 6.1.1.

E.3.1 Parental Education

Parental education is recorded in the IMEB application data using the Catalan classification of educational attainment (CNES codes). The CNES system assigns numerical codes to educational levels, with higher values indicating higher attainment. We define a parent as having post-secondary education if their CNES code is 40 or higher, which corresponds to upper secondary education (*batxillerat*), vocational training (*formació professional de grau superior*), or university degrees.

Table 40: Education Variable Construction

Variable		Definition
Mother (`ba_t1`)	post-secondary	= 1 if mother's CNES code $\geq$ 40; = 0 otherwise
Father (`ba_t2`)	post-secondary	= 1 if father's CNES code $\geq$ 40; = 0 otherwise
At least one parent (`ba`)		= 1 if `ba_t1` = 1 or `ba_t2` = 1
Both parents (`both_ba`)		= 1 if `ba_t1` = 1 and `ba_t2` = 1

Notes: CNES codes 40+ include: upper secondary (*batxillerat*, code 42), vocational training (*formació professional de grau superior*, codes 40–41), and university degrees (*diplomatura* code 44, *llicenciatura/grau* code 46, master's/doctorate code 48). We use a threshold of 40 rather than 44 (university only) to ensure adequate sample sizes in both groups. Missing education values (CNES = 99 or missing) are coded as missing.

Table 41 provides the full CNES classification for reference.

Table 41: CNES Education Classification (Catalan System)

Code	Description	Category
10	Cannot read or write	Low (<40)
11	Cannot read or write	
20	Below primary education level	
21	No formal education	
22	Incomplete primary, 5 years EGB or equivalent	
30	Primary complete or equivalent	
31	Lower secondary (EGB, ESO) or vocational certificate	
32	Vocational training (first level)	
40	Upper vocational training (second level)	High ($\geq$40)
41	Upper vocational, *mestria industrial*	
42	Upper secondary (*batxillerat*, BUP)	
43	Other intermediate qualifications	
44	*Diplomatura* (3-year university degree)	
45	Technical architect/engineer	
46	*Llicenciatura/Grau* (full university degree)	
47	Non-university higher education	
48	Doctorate, Master's, postgraduate	
99	Unknown	Missing

Notes: CNES = Classificació Nacional d'Educació de Catalunya. Our "High Education" indicator equals 1 if CNES $\geq$ 40 for both parents.

In the main heterogeneity analysis (Table 7), we use `both_ba` (both parents with post-secondary education) as the splitting variable. In our lottery sample, 66% of mothers and 59% of fathers have post-secondary education, and 48% of families have both parents with post-secondary education. These shares are substantially higher than the general population, reflecting positive selection into the applicant pool (see Section 4.2).

E.3.2 Private Childcare Availability

To examine whether treatment effects vary with the local availability of private childcare alternatives, we construct a district-level measure of private childcare market thickness:

$$\text{Private Availability}_d = \frac{\text{Children in private childcare}_d}{\text{Children in public/subsidized childcare}_d} \tag{2}$$

where d indexes Barcelona's 10 districts. The numerator counts children aged 0–2 enrolled in non-subsidized private childcare centers (*escoles bressol privades*); the denominator counts children enrolled in public municipal centers (*escoles bressol municipals*) plus subsidized private centers (*escoles bressol concertades*).

Table 42: Private Childcare Availability by District

District	**Availability Ratio**	**Classification**
Sarrià-Sant Gervasi	4.85	High
Les Corts	3.42	High
Eixample	2.98	High
Gràcia	2.35	High
Sant Martí	2.21	High
Median	*2.20*	—
Horta-Guinardó	1.89	Low
Sants-Montjuïc	1.76	Low
Sant Andreu	1.54	Low
Ciutat Vella	1.32	Low
Nou Barris	0.98	Low

Notes: Ratios computed using average enrollment counts over the sample period (2005–2010). Districts with ratio $>$ 2.20 (sample median) are classified as "High" private availability; districts with ratio $\leq$ 2.20 are classified as "Low."

We split families into "High Private Availability" and "Low Private Availability" groups based on whether their district of residence has a ratio above or below the sample median of 2.20. This median split assigns roughly half of the sample to each group. As shown

in Table 42, there is substantial variation across districts, with Sarrià-Sant Gervasi (the most affluent district) having nearly five private slots per public/subsidized slot, while Nou Barris (one of the least affluent) has fewer than one.

E.3.3 Survey on Childcare Arrangements (IGOP)

To characterize the counterfactual care arrangements of families who apply but are not admitted to public childcare, we use data from an independent survey conducted by the Institut de Govern i Polítiques Públiques (IGOP) at the Universitat Autònoma de Barcelona (González, 2021).

Table 43: IGOP Survey: Overview

Characteristic	**Description**
Sample	1,219 families with children born 2016–2019 in Barcelona
Survey year	2020
Conducted by	Institut de Govern i Polítiques Públiques (IGOP), Universitat Autònoma de Barcelona
Funding	Projecte Recercaixa "Edu 0-3"

Table 44: IGOP Survey: Key Variables

Variable category	**Description**
Childcare arrangements	Primary caregiver at each age (0–1, 1–2, 2–3): mother, father, grandparents, public childcare, private childcare, babysitter, other
Hours of care	Daily hours with each caregiver type
Application to public childcare	Whether family applied to *escola bressol municipal*; if so, whether admitted
Maternal characteristics	Education, employment status, income, main earner status
Child characteristics	Birth year, birth order

E.3.4 Class Size Ratios (Generalitat de Catalunya)

To explore mechanisms related to childcare quality, we use data on average class sizes in early childhood education from the Departament d'Educació of the Generalitat de Catalunya.

Table 45: Class Size Data: Overview

Characteristic	**Description**
Source	Departament d'Educació, Generalitat de Catalunya
Coverage	Catalonia (regional aggregates)
Years	Academic years 2006–07 to 2021–22
Disaggregation	By school sector (public, private) and age level (0–1, 1–2, 2–3)