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Cognitive and non-cognitive costs of daycare 0–2 for girls

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Cognitive and non-cognitive costs of daycare 0–2 for girls*

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Abstract

Exploiting admission thresholds in a Regression Discontinuity Design, we study the causal effects of daycare at age 0–2 on cognitive and non-cognitive outcomes at age 8–14. One additional month in daycare reduces IQ by 0.5% (4.5% of a standard deviation). Effects for conscientiousness are small and imprecisely estimated. Psychologists suggest that children in daycare experience fewer one-to-one interactions with adults, which should be particularly relevant for girls who are more capable than boys of exploiting cognitive stimuli at an early age. In line with this interpretation, losses for girls are larger and more significant, especially in affluent families.

JEL-Code: J13, I20, I28, H75

Keywords: daycare, childcare, child development, cognitive skills, non-cognitive skills.

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1 Introduction

Daycare for infants and toddlers is a convenient solution for parents who need to return to work soon after the birth of a child. Not surprisingly, enrollment rates in center-based daycare are soaring in countries with a developed labor market.[1] Whether daycare at age 0–2 is also beneficial to children is less clear, based on the few existing studies of the consequences of alternative childcare arrangements at this very early age.[2]

We contribute to this literature by studying the causal effects of time spent at age 0–2 in the high-quality public daycare system offered by the city of Bologna, Italy,[3] on cognitive and non-cognitive outcomes at age 8–14. Specifically, we focus on IQ and conscientiousness. In the age range at which we measure outcomes, short-lived effects of daycare 0–2 are likely to have faded away, allowing us to explore more long-term effects.[4] Identification is based on a Regression Discontinuity (RD) design that exploits the institutional rules of the application and admission process to the Bologna Daycare System (BDS). This strategy allows us to compare similar children attending daycare 0–2 for different time spans, including no attendance at all, in a context where daycare crowds out family care.

Applicants to the BDS are assigned to priority groups based on observable family characteristics. Within each priority group, applicants are then ranked (from low to high) based on a household size-adjusted function of family income and wealth, that we label as the Family Affluence Index (FAI). The vacant capacity of a program in a given year determines a FAI

[1] In the 9 largest non-Scandinavian OECD countries for which data are available (Australia, France, Germany, Italy, Japan, Korea, Netherlands, Spain, and UK) the average enrolment rate (weighted by population size at age 0–4) rose from 16.8% in 2002-2003 to 36.6% in 2010-2011. In the US, this rate increased from 27.1% in 2005 to 42% in 2010. In the Scandinavian group (Denmark, Finland, Norway, and Sweden) the enrolment rate of children under 3 in formal childcare is traditionally large, and yet it rose from an average of 36.9% in 2000 to 48.5% in 2010. Daycare 0–2 is also an expensive form of early education that governments provide or subsidize: average public spending across OECD countries was 0.4% of GDP in 2011, or about US $7,700 (at PPP) per enrolled child—the corresponding average enrollment rate was 33%. Source: OECD Family Database, `http://www.oecd.org/els/family/database.htm`. The 2002 EU council in Barcelona set a target of 33% of children in daycare 0-2 by 2010, but this objective was motivated just as a gender policy.

[2] The literature on the effects of childcare at age 3–5 is large, but less than a handful of papers study instead what happens at age 0–2, as summarized in Section 2.

[3] Bologna, 400k inhabitants, is the 7th largest Italian city, as well as the regional capital and largest city of Emilia Romagna, a region in the north of the country. The daycare system that we study is a universal crèche system known as the *asilo nido* which, in this region, is renowned for its high-quality even outside the country (Hewett, 2001).

[4] The available administrative data do not allow us to explore outcomes measured at older ages.

threshold such that applicants whose affluence index is equal or lower than the threshold receive an admission offer, while those whose index is higher do not.

The administrative data we received from the City of Bologna contain the daily attendance record of each child but do not contain information on outcomes. Thus, between May 2013 and July 2015 we interviewed a sample of children who applied for admission to the BDS between 2001 and 2005 and who were between 8 and 14 years of age at the date of the interview. Children were tested by professional psychologists using the "Wechsler Intelligence Scale for Children" (WISC-IV) to measure IQ and the "Big Five Questionnaire for Children" (BFQ-C) to measure conscientiousness.[5] The literature has by now converged on considering these two outcomes as the most relevant cognitive and non-cognitive indicators, because they are mutually uncorrelated while being highly correlated with important outcomes later in life (Elango *et al.*, 2015). The accompanying parent was interviewed by a research assistant, to collect socio-economic information.

We find that an additional month in daycare at age 0–2 reduces IQ, on average, by about 0.5%. This effect corresponds, at the sample mean (116), to 0.6 IQ points and to 4.5% of the IQ standard deviation. The effect on conscientiousness is very small and imprecisely estimated. To interpret these findings we rely on the psychological literature which emphasizes the importance of one-to-one interactions with adults for child development during the early years of life. According to psychologists, these interactions should be particularly relevant for girls who, at this early age, are more mature than boys and thus more capable of benefiting from the cognitive stimuli generated by adult-child contacts. Therefore, if daycare 0–2 is associated with less frequent one-to-one interactions with adults than those offered by family care, then daycare should have more negative effects on girls than on boys. In line with this interpretation, we find that girls attending the BDS, where the adult-to-children ratio is 1:4 at age 0 and 1:6 at age 1 and 2, experience a larger IQ loss of 0.7% for every additional month in daycare 0–2, while the effect for boys is smaller (0.4%) and not statistically significant. Notably, in our sample the most frequent care modes when daycare 0–2 is not available are parents, grandparents, and nannies, all of which imply an adult-to-children ratio of 1 (or

[5] The BFQ-C defines conscientiousness as the tendency to be organized and dependable, show self-discipline, act dutifully, aim for achievement, and prefer planned rather than spontaneous behavior.

somewhat smaller in the presence of siblings).

To further support the mechanism drawn from this interpretation, we explore the heterogeneity of the effect of daycare 0–2 by family background. Arguably, one-to-one interactions with adults at home should be more beneficial for a child's cognitive development when they are associated with richer cultural and economic resources. This is indeed what we find when we isolate the effect of daycare for children in more affluent families: here the IQ loss for girls is as high as 1.6% for one additional month in daycare 0–2, while the loss is statistically and quantitatively insignificant in less favorable family environments. Girls in affluent families appear to suffer also a loss in terms of conscientiousness, but it is imprecisely estimated. Small and insignificant effects are instead estimated for boys independently of family background.

In addition to shedding light on the possible mechanism driving our results, this evidence is interesting because of the information it contains on the consequences of daycare 0–2 for children in more affluent families, who have received little attention in the literature (Elango *et al.*, 2015). This group is the relevant one at the margin of daycare expansions in developed countries, where disadvantaged children are typically already covered by public daycare services. Therefore, it is the group that needs to be studied for an evaluation of the opportunity to increase daycare access for the worldwide growing community of families in which both parents want to work.

After summarizing the economic literature in Section 2, we present in greater detail the institutional setting in Section 3. We then show, in Section 4, how the latter can be used to construct a valid RD design. Section 5 explains how we interviewed a representative subset of children to collect information on outcomes. Section 6 presents the econometric framework and our main results. Section 7 proposes an interpretation based on the suggestions of the psychological literature, and provides support for this interpretation by looking at the heterogeneity of effects by gender and family background. Section 8 concludes.

2 Previous research

This study contributes to the economic literature that investigates how early life experiences shape individual cognitive and non-cognitive skills.[6] Formal daycare at age 0–2 is an experience of this kind, probably the most important extra-familiar one that infants and toddlers can go through during a highly sensitive stage of their life. We do not develop an explicit theoretical framework, given that our empirical investigation can easily be embedded in the workhorse economic model of child development first proposed by Carneiro *et al.* (2003) and Cunha and Heckman (2007). That is, we think of daycare 0–2 as one of the inputs in the production function of specific cognitive and non-cognitive outcomes, which, according to state of the art evidence, exhibits malleability very early in life (but less later on).

The literature distinguishes between daycare 0–2 (e.g., crèches) and childcare 3–5 (e.g., preschool/kindergarten programs). The case of the older between these two groups has been extensively investigated[7], while we know much less about the effects of daycare targeting children in the very first years of their life. Our paper aims at filling this gap.

The few studies in economics that focus on age 0–2 report mixed results. A first group finds, different from us, desirable effects of early daycare attendance for both cognitive and non-cognitive outcomes, concentrated in particular on girls and on children with disadvantaged family background. In this group, Felfe and Lalive (2014) use administrative data from Schleswig-Holstein to study the effect of daycare 0–2 on language ability and motor skills at

[6]See Borghans *et al.* (2008), Almond and Currie (2011), Heckman and Mosso (2014) and Elango *et al.* (2015) for recent surveys.

[7]Duncan and Magnuson (2013) provide a meta analysis of the large literature on childcare 3–5, concluding that these programs improve children "pre-academic skills, although the distribution of impact estimates is extremely wide and gains on achievement tests typically fade over time." (p. 127). Results from the early evaluations of Head Start, the largest randomized study targeting preschoolers, are consistent with these conclusions (Puma *et al.*, 2012). However, a more careful re-examination of the data, with particular reference to the definition of counterfactuals, reveals positive effects for the disadvantaged population that is targetet by this intervention (Elango *et al.*, 2015). In line with this finding, Carneiro and Ginja (2014) find persistent health effects of Head Start, using a RD design based on program eligibility rules. Magnuson *et al.* (2007) use the US Early Childhood Longitudinal Study and suggest that pre-kindergarten daycare attendance improves reading and math skills at school entry, but also increases behavioral problems and reduces self-control. Havnes and Mogstad (2015) evaluated a 1975 Norwegian large subsidised expansion of childcare 3–5, concluding that "the benefits of providing subsidized child care to middle and upper-class children are unlikely to exceed the costs." (p. 101). Felfe *et al.* (2015) reach the same conclusion using data from a similar expansion that took place in Spain during the early 1990s. Dustmann *et al.* (2013) exploit a reform that entitled all German preschoolers to a childcare slot. They find no significant effects for native children and positive effects on school readiness, language and motor skill for children of immigrant parents.

age 5–6, instrumenting the probability of attendance with enrolment/children ratios across school districts and exploiting the variability generated by a daycare expansion enacted in Germany in the early 2000s. They find positive effects which are largest for children whose mothers have attained at most compulsory education as well as for children of immigrant parents. Drange and Havnes (2015) study the effects of age at entry in daycare 0–2 on language and math test scores at age 7, exploiting the randomization of entry offers at the Oslo public daycare facilities. They find that children who entered daycare at 15 months of age have better test scores than those who entered at 19 months, an effect driven by children from lower income families. With specific reference to Italy, Del Boca *et al.* (2015) show that benefits of early daycare for children are larger in areas where the rationing system favors more disadvantaged families. Precursors of these more recent papers are the Carolina Abecedarian Study (Campbell and Ramey, 1994; Anderson, 2008), the Milwaukee Project (Garber, 1988) and Zigler and Butterfield (1968). They all reached similar conclusions.

On the contrary, studies based on the Quebec universal early daycare extension, that heavily subsidized daycare for children in the age range 0–4 beginning in 1997, typically find undesirable effects on all types of cognitive and non-cognitive outcomes, with losses that are concentrated in particular on boys. A seminal paper in this group is Baker *et al.* (2008), who compare Quebec and the rest of Canada in a Difference–in–Difference design, finding that children who benefited from the extension are worse off in terms of behavioral outcomes, social skills and health.[8] More recently, these same authors confirmed the persistence in the long run of these undesirable effects, also showing negligible consequences for cognitive test scores (Baker *et al.*, 2015). Particularly interesting for us, among the studies based on

[8] Three other recent studies, for different countries, provide indirect evidence consistent with this finding for Quebec, exploiting policy changes that alter the amount of maternal care a child receives at 0–2. Carneiro *et al.* (2015) analyze an experiment generated by a 4-month extension of maternity leave enacted in Norway at the end of the 1970s. Looking at very long-run outcomes for treated children — educational attainment and earnings between age 25 and 33 — these authors find positive effects of the extension (i.e., negative effects of less family care at age 0), which are stronger for children of less educated mothers. Bernal and Keane (2011) exploit the 1996 US welfare reform to construct an experiment generating variation in time of maternal care at age 0–2 for children of single mothers. Focusing on children in the 0–5 age range, these authors find a negative effect of less time with mothers on preschool achievement test scores at age 3–6. These effects are larger for children of more educated mothers in this disadvantaged group. Herbst (2013) uses the US Early Childhood Longitudinal Study, Birth cohort (ECLS-B) and estimates negative effects of being in non-parental care at 9 and 24 months on children cognitive scores and motor development. However, outcomes are measured during the treatment and variation of time spent with parents is generated by the comparison between Summer and Winter months.

the Quebec extension are Kottelenberg and Lehrer (2014a) and Kottelenberg and Lehrer (2014b), which focus specifically on the heterogeneity of effects at different ages in the 0–4 range and across genders. They show that the negative effects of this intervention are particularly large among kids who start attendance at an earlier age and among boys.[9]

To the best of our knowledge, however, no study finds the undesirable effects of daycare 0–2 for girls that we uncover.[10] A first important difference between our study and the literature that might explain this divergence of results is that our sample and identification provide estimates for relatively affluent families with both parents working and cohabiting in one of the richest and most highly educated Italian cities. This is precisely a context in which the quality of one-to-one interactions at home is likely to be better than the analogous quality in daycare 0–2, even if Bologna is renowned for the high standard of this service. Moreover, since girls are more capable than boys of making good use of what their families can offer in alternative to daycare, this is the context in which negative effects for girls should emerge more clearly, and in fact they do.

A second possible explanation of the divergence of our results from the literature relates to the characteristics of daycare environments across the different studies. For example, both Felfe and Lalive (2014) and Drange and Havnes (2015) study daycare settings (Germany and Norway, respectively) with an adult-to-child ratio of 1:3. The corresponding ratio at the Bologna daycare facilities during the period that we study was 1:4 at age 0 and 1:6 at ages 1 and 2. From this viewpoint, our study suggests that if daycare 0–2 is a necessity for parents who want to work, it should be designed in a way that ensures a very high teacher to children ratio, if this solution is cost efficient.

Finally, as far as cognitive outcomes are concerned, most other papers typically focus on

[9]Negative, but short run, effects of daycare 0–2 are also found by Noboa-Hidalgo and Urza (2012) in Chile, for children with disadvantaged backgrounds.

[10]In their study of the 4-month extension of maternity leave in Norway at the end of the 1970s (see footnote 8), Carneiro *et al.* (2015) report an increase in the school dropout rate of girls who experience less time with their mother at home after birth (p. 403, Table 14), which is consistent with our results and interpretation. However, they do not elaborate on this finding that is just reported incidentally. Elango *et al.* (2015) re-analyse the original data of four demonstration programs (the Perry Preschool Project, PPP, the Carolina Abecedarian Project, ABC, the Infant Health and Development Program, IHDP, and the Early Training Project, ETP) finding more positive effects for boys than for girls, which lead to a substantial gender gap in benefit-to-cost ratios for at least two of them (ABC and PPP). However, they do not seem to find *negative* effects for girls, possibly because these programs are directed to disadvantged children and are not restricted to the 0–2 age range.

math and language test scores, or indicators of school readiness. Our negative estimates for girls refer instead to IQ measured by professional psychologists. There is a general consensus on the fact that IQ, in addition to being a clinical and standardized indicator, is correlated with a wide set of long term outcomes, including in particular levels of education, types of occupation and incomes (see, for example, Gottfredson, 1997). However, Currie (2001) notes that the literature on the effects of childcare has shifted towards the use of learning test scores or indicators of school readiness as outcomes, probably because "gains in measured IQ scores associated with early intervention are often short-lived" (p. 214).[11] From this viewpoint, a contribution of our study is to show that instead daycare 0–2 may have long term negative effects also on IQ.

3 Institutional setting and administrative data sources

The office in charge of the Bologna Daycare System (BDS) granted us access to the application and admission files for the universe of the 66 daycare facilities operating in the City between 2001 and 2005 (of which 8 are charter). These facilities enroll, every year, approximately 3,000 children of age 0, 1, and 2 in full-time or part-time modules. Henceforth, we refer to these ages as "grades" and we use the term "program" to define a module (full-time or part-time) in a grade (age 0, 1, or 2) of a facility (66 institutions) in a given calendar year (2001 to 2005). There are 941 such programs in our data, and we have information on a total of 9,667 children whose parents applied for admission to one or more programs of the BDS between 2001 and 2005.[12]

The algorithm that matches children to programs boils down to a Deferred Acceptance (DA) market design.[13] Children can apply to as many programs as they wish in the grade-year combination for which they are entitled. We refer to the set of programs a child applies to in a given year as the child's "application set" for that year. Programs in a

[11]The costs of measuring IQ using professional psychologists, compared with the increasing availability of administrative data on school outcomes at almost no cost, probably contribute as well to explain why IQ is used less as an outcome in this literature.

[12]Hereafter, we simplify the exposition by referring to children as the decision makers.

[13]See Gale and Shapley (1962) and the survey in Roth (2007). Abdulkadiroglu *et al.* (2015) have recently proposed empirical strategies that exploit the quasi-experimental variation induced by a DA mechanism. Some specific feature of our case, however, induce us to prefer the strategy that we describe below.

child's application set are ordered according to preferences. Demand for admission to the BDS systematically exceeds supply and there are, on average, about 1,500 vacancies for about 1,900 applicants each year. The rationing mechanism is based on a lexicographic ordering of applicants. At a first level, applicants to each program are assigned to priority groups based on observable family characteristics. First (highest priority), children with disabilities. Second, children in families assisted by social workers. Third, children in single-parent households, including those resulting from divorce or separation. Fourth, children with two cohabiting and employed parents. Fifth (lowest priority) children in households with two cohabiting parents of whom only one is employed. For brevity, we refer to these priority groups as "baskets" 1 to 5. At a second level, within each of these five baskets children are ranked according to a Family Affluence Index (FAI). This is an index of family income and net wealth, adjusted for family size.[14] Families with a lower value of the index (i.e., less affluent families) have higher priority within a basket.

Given the number of vacancies in each program, the BDS makes a first round of admission offers starting from the child with the highest priority in the ranking list of that program. Thus, if a program has n vacancies, the first n children in its ranking list qualify for a first round offer, and the remaining children (whom we refer to as the "reserves") form a waiting list. The FAI of the last child who qualifies for a first-round offer (and who may belong to any of the five baskets) is the "Initial" FAI threshold for that program. Children qualifying in many programs are offered their preferred one only (in the subset of programs in their application set for which they qualify). Moreover, first round offers may be turned down by children who, in the end, decide not to attend daycare. As a result, a set of second-round vacancies may become available for children who do not qualify for any program in their application set at the end of the first round. These second-round vacancies open up in programs that have not filled their capacity at the end of the first round. A new ranking is formed for each of these programs, moving up reserves to fill the new vacancies, and the process is iterated until either all programs have filled their capacity or some or all programs run out of reserves. The "Final" FAI threshold of a program is thus defined as the FAI of the last child who receives an offer from that program and accepts it.

[14] Section 9.1 in the Appendix provides details about how this index is constructed.

In Sections 4 we show how Final FAI thresholds can be used to construct a valid RD design. At this stage it is enough to say that after the final round of the admission process children can be classified in three mutually exclusive and exhaustive ways: the "admitted and attendants", who have received an admission offer and have accepted it; the "reserves", who have not received any offer; the "admitted and waivers" (or "waivers" for brevity), who have received an admission offer and have turned it down. It is important to keep in mind that children who are "reserves" or "waivers" in a given year may re-apply, be offered admission, and attend daycare in later years, as long as they are not older than 2. Therefore, since we construct the RD design on the basis of *the first application of each child*, the possibility to turn down an offer and to re-apply and attend later is one of the reasons of fuzziness in the design.

Two final remarks on the institutional setting are in order before moving forward. First, a child's FAI is relevant not only for admission, but also for the monthly attendance fee that children must pay if they accept an offer, independently of actual days of attendance during the month. This fee is a function of a child's FAI, which is well known to potentially interested families before they decide whether to apply.[15] As such, it should not pose problems in our analysis, which is conditional on children who have already decided to apply, and it is in any case continuous at the thresholds that we will use in our design.

Second, in Section 5 we illustrate how we match the administrative data with information on family characteristics and children outcomes obtained with interviews, whose number (458) was constrained by our budget. To ensure a greater homogeneity of the interview sample, we restrict the entire analysis to children in "basket 4" (i.e., children with both parents employed and cohabiting at the time of the application), which is the largest group of applicants (about 70% of the total): 6,575 *first applications* to 890 programs originate from this basket in the period 2001-2005. Of these programs, 74 end up with no vacancies for basket 4 children (i.e., the Final FAI threshold is in basket 1, or 2, or 3); 271 have sufficient capacity for all basket 4 applicants (i.e., the final FAI threshold is in basket 5), and 545 offer

[15]The fee is an increasing step function of the FAI, such that the entire schedule is approximately linear. For instance, in 2005 the monthly attendance fee (inclusive of meals) ranged between €0 for families with a FAI not greater than €570, and €400 for families with a FAI of €29,400 or greater (values are expressed in 2010 euros).

admission to some but not all the basket 4 applicants (i.e., the Final FAI threshold is in basket 4). The remaining 51 (to reach the total of 941 programs) do not receive applications from basket 4. Some tables and figures below are based on sub-groups of this sample, for the reasons explained in the respective notes.

4 Family Affluence Index thresholds

We begin by showing that families cannot predict Final FAI thresholds and thus cannot manipulate their FAI to secure an admission offer.[16] We then show how we use Final FAI thresholds for the RD design.

4.1 Absence of manipulations of the admission process

If FAI thresholds were persistent across years, it would be easy for families to find out the final thresholds of the programs they wish to apply for. Figure 1 shows that this is unlikely to happen: the two panels plot, for each program, the Final FAI thresholds (left panel) and the basket 4 vacancies (right panel) in year t against the corresponding thresholds and vacancies in year $t-1$. For both these variables, a prediction based on lags would be highly imprecise and, for an accurate guess, families would need a formidable amount of additional information, like for example: the vacant capacity of the programs they wish to apply for, the number of applicants to these programs, the FAI of each applicant, how other applicants rank programs, and how many admitted children in each program turn down the offer they receive.

The ultimate support for the claim that families cannot effectively manipulate their affluence index to secure admission is provided by the continuity of the FAI density and of pre-treatment covariates. We assess this continuity in Figure 2 stacking thresholds and centering them at zero, so that the FAI distance from each threshold is the running variable in the figure. In the top left panel the density of observations is plotted with different colors by

[16] Families can (and in some cases probably do) manipulate their affluence index but, as we will see, they don't know by how much they should reduce the index in order to receive an offer from a specific program, so that manipulations cannot (and indeed do not) generate discontinuities of the density and pre-treatment characteristics at the Final FAI thresholds.

gender, and the McCrary (2008) test rejects in both cases the existence of a discontinuity.[17] Five relevant pre-treatment covariates are considered in the remaining panels (birth day in the year, FAI, average income in the city neighborhood where the program is located, number of siblings at the first application, and number of programs listed in the application set) and again no discontinuity emerges at the thresholds for either boys or girls.[18]

Given the absence of any evidence of manipulation of the admission process at the BDS, it would then seem natural to use observations around each Final FAI threshold for the RD design, but this would be problematic because children applying to many programs would be over-represented in the analysis. Specifically, reserve children would appear as many times as the number of programs they apply for while admitted children and waivers would appear as many times as the number of programs they qualify for. The next section shows how we circumvent this problem in order to associate every child with one threshold only.

4.2 How Final FAI thresholds can be used for the RD design

We are interested in the effects of days of attendance in the BDS *independently of the specific program a child attends*. To see how final thresholds can be used for this purpose and in a way that associates one threshold with each child, consider the example described in Figure 3, which illustrates the hypothetical situation of child i who applies for the first time in a given year to five programs out of a total of C programs she could apply for. Without loss of generality, $c \in \{1, \ldots, 5\}$ denotes these programs, which the child ranks in the following order: $3 \succ 2 \succ 5 \succ 1 \succ 4$.

Let Y_i be the FAI of child i and T_c^Y the Final FAI threshold of program c. In Figure 3 these thresholds are ordered along the horizontal axis from the highest on the left to the lowest on the right. $T_5^Y \equiv T_i^{Ym}$ is the maximum FAI threshold in i's application set. Therefore, if

[17] For males the log discontinuity of the density is -0.078 with a standard error of 0.097. For females, the corresponding estimates are respectively -0.017 and 0.135.

[18] In these panels, a dot represents the average value of the correspondent covariate in bins of €2000 size. Different symbols allow to distinguish the two genders. Solid lines represent estimated conditional mean functions smoothed with LLR using all individual observations separately by gender on the two sides of zero. For the reasons explained in Section 9.2 of the Appendix, observations with exactly zero distance from FAI thresholds are dropped in the construction of these and the following figures where the running variable is a FAI distance, as well as in the related continuity tests. A triangular kernel and optimal bandwidth from Calonico *et al.* (2014b) are used here and in all of the remaining similar figures below.

$Y_i > T_5^Y$ then child i does not receive any offer in the year of her first application because her FAI is too high to qualify in any of the programs in her application set. If, instead, $Y_i \leq T_5^Y$ then with probability 1 the child receives at least one offer at her first application and possibly qualifies for more than one program if Y_i is lower than other thresholds. Thus the probability of qualifying for at least one program when first applying jumps sharply from 0 to 1 at the T_5^Y threshold. This probability is represented by the bold solid line in Figure 3.

The bold dashed line, instead, is the probability of attending daycare following the first application. This probability is highest if $Y_i \leq T_3^Y \equiv T_i^{Yp}$, i.e., if the child receives an offer from the preferred program in her application set — program 3 in this example. This is so because it is reasonable to expect that a child is weakly more likely to attend daycare if she is offered her preferred program. However, even in this case the probability is not equal to 1 because the child may always turn down the offer.[19]

What matters for our purposes is that, for each child, the probability of attendance should jump discontinuously at both her "Preferred" (T_i^{Yp}) and her "Maximum" (T_i^{Ym}) FAI thresholds. We can therefore use both kinds of thresholds in our RD design, and indeed they give qualitatively similar results, but we focus here on Preferred FAI thresholds because we feel more confident in the resulting design.[20] The reason is that children have, potentially, some control on their Maximum FAI threshold given that they can (weakly) increase it by adding more programs to the application set. Note that both Preferred and Maximum FAI thresholds are unique for a child. Therefore, when using them, there are no repeated records and each child is used only once in the analysis.

Figure 4 shows how attendance changes in a discontinuous way around Preferred thresholds in our data. The running variable is the FAI distance from the cutoff, with positive values indicating a FAI lower than the threshold. In the left and middle panels the admis-

[19] The probability of attending daycare does not change if the child has a low enough FAI to qualify in programs 2 and 1, because she would still be offered the strictly preferred program 3. If $Y_{it} > T_3^Y$ (but still $Y_i \leq T_5^Y$) then the probability of attendance declines (relative to the $Y_i \leq T_3^Y$ case) because the child receives offers but not from the preferred program. In this example the probability of attendance is constant for FAI levels between T_3^Y and T_5^Y because in all these cases the child will receive an offer from program 5, which she strictly prefers to program 4. It may appear surprising that the probability of attendance is greater than zero for FAI values above T_5^Y. This fuzziness occurs because a child may not qualify for any program at her *first* application but re-apply, be offered admission, and accept it in later years (if she is not older than 2).

[20] The analysis based on Maximum FAI thresholds is available from the authors.

sion and the attendance rates increase sharply as the FAI crosses the cutoff from higher to lower values, with some fuzziness due to the reasons discussed in the comment to Figure 3. These discontinuities translate into a jump of almost three months (53 working days) of total attendance in the right panel. In Figure 5 we show that the frequency of observations and pre-treatment covariates are all continuous, for both genders, around Preferred FAI thresholds, supporting the validity of a RD design constructed around them.[21] Before describing formally this design, we explain in the next section how we collected information on cognitive and non-cognitive outcomes.

5 The interview sample

The administrative records that we received from the City of Bologna do not contain children outcomes at any stage of their development, nor they contain pre-treatment family characteristics beyond the few ones we have mentioned above. Therefore, we have organized interviews in the field to collect information on outcomes and socioeconomic background for the children included in our final sample.

Between May 2013 and June 2015 we sent invitation letters via certified mail to 1,379 families whose children first applied for admission to a program of the BDS during the period 2001-2005 and who were between 8 and 14 years of age at the time of the invitation. The reason to measure outcomes in this age range is that we are not interested in short-lived effects of daycare 0–2. We would have liked to explore outcomes at an even later age, but the available administrative data on daycare 0–2 admission and attendance did not allow us to go back in time before the 2001-2005 period. In these letters, families were given a brief description of the research project and were invited to contact us (either via e-mail or using a toll-free phone number) to schedule an appointment for an interview. Families were also informed that participants would receive a gift card worth €50 usable at a large grocery store and bookstore chain. After a few weeks from receipt of the letter, families who had not yet responded were sent a reminder via e-mail or phone.

[21] Using the McCrary (2008) test, the log discontinuity of the density is 0.006 with a standard error of 0.11 for males and 0.006 with a standard error of 0.11 for females.

Upon arrival at the interview site (a dedicated space at the University of Bologna), the child was first administered an IQ test by a professional psychologist, while the accompanying parent was interviewed in a separate room by a research assistant to collect socioeconomic information. The test we used is the "Wechsler Intelligence Scale for Children" (WISC-IV) which measures Full Scale IQ. The children that we interviewed have, on average, an IQ of 116.4 on a scale normalized to 100 for the average of the Italian population of children in the same age range that took the WISC-IV. The standard deviation is equal to about 12.4. Girls have a slightly higher IQ than boys (117.2 versus 115.6) but the difference is not statistically significant. After the IQ test, the child was administered (by the same psychologist) the "Big Five Questionnaire for Children" (BFQ-C) to measure personality traits. We focus here on "conscientiousness" because it has a low correlation with IQ (-0.0008 in our data) but like IQ is highly correlated with important long term outcomes (Elango *et al.*, 2015). The average conscientiousness score in our sample is equal to 47.6, on a scale normalized to 50 for the average of the Italian population of children in the same age range that took the BFQ-C. The standard deviation is 10.0. In this case boys score slightly higher than girls (48.3 versus 47.0) but this difference, too, is not statistically significant. Overall, each child and the accompanying parent spent about 3 hours at the interview site.[22]

Our limited budget forced us to stop the interview process when we obtained information on 458 children, corresponding to a response rate of 33.2% of the invited. Of these interviews, only 444 provided a complete set of variables to be used in the econometric analysis for IQ and, of these, 441 have a conscientiousness score.[23]

Next, we discuss the representativeness, with respect to the basket 4 universe, of the 444 interviews that we can use to study the effects on IQ.[24] For this discussion it is important to keep in mind that in order to increase the FAI comparability of treated and control children,

[22] Parents were also asked to fill the "Child Behavior Checklist" (CBCL), and we collected health information by registering the child's height and weight using a precision clinical scale. These outcomes will be analyzed in different papers.

[23] In 7 cases, parents informed us that their children had already been tested recently using the WISC-IV, and this test does not provide reliable information if replicated. In 7 additional cases, parents did not answer all of the socio economic questions, thus generating missing values in some relevant pre-treatment variables. Finally, of the remaining 444 children, 3 did not answer all the questions needed to compute a valid conscientiousness score.

[24] Results, available from the authors, are qualitatively similar for the 441 interviews that we can use for conscientiousness.

the 1,379 families that we contacted were invited starting from those closer to Final FAI thresholds. The top left panel of Figure 6 shows that the interview rate with respect to the B4 universe does not display any relevant discontinuity around Preferred thresholds for both genders. Continuity around thresholds is even more precise for the response rate of the invited (top middle panel) and for the interview rate with respect to the B4 universe (top right panel). However, the invitation rule that we followed resulted in a distribution of invited and interviewed observations that differ in some important dimensions from the one of the non-invited and non-interviewed B4 universe, respectively, as shown in the bottom left and bottom right panels of Figure 6. The bottom middle panel of the same figure shows instead that interviewed observations are located similarly to invited observations with respect to the Preferred FAI thresholds. These patterns are reflected in the descriptive statistics of the universe, the invited and the interviewed samples that we illustrate in Table 1 .

This table compares the means of key administrative variables for the B4 universe, the invited and the interviewed samples. The p-values reported in the last column refer to tests of the equality of means for the B4 universe and the invited (first row), for the invited and the interviewed (second row, in square brackets) and for the B4 universe and the interviewed (third row, in curly brackets). The general pattern suggests, as expected, that there are no significant differences between the interviewed and the invited, while both these groups differ in some dimensions with respect to the B4 universe. However, these differences emerge for good reasons and do not represent a threat to the internal consistency of our identification strategy.

For instance, the invited and the interviewed children have a slightly higher FAI than the universe. This is not surprising given how we invited families and the fact that the evolution of the admission process pushes Final FAI thresholds towards higher FAI values (see Section 3). We also see, in this table, that the offer rate is substantially higher in the universe than in the interview/invited samples. This happens because sampling around Final FAI thresholds implies oversampling reserves. As a result, the attendance rate is somewhat unbalanced too. These are all consequences of the way we selected the invited families, that we traded off to gain homogeneity and comparability at the FAI thresholds. The number of preferences and the number of children in the household at first application are all well balanced across the three samples.

Other variables in Table 1 exhibit significant differences across the groups, because of another unavoidable feature of the sampling design. Since the BFQ-C can be administered only to subjects not older than 14, we could not invite children first applying for entry at grades 1 and 2 in 2001, nor children first applying for entry at grade 2 in 2002. As a result, the invited and the interviewed children are slightly younger than the B4 universe, have first applied for lower grades, and have spent more days in daycare. There are also almost twice as many children turning down offers in the B4 universe as in the invited/interview samples, because we somewhat under-sampled waivers. Once again, these differences are not a threat to the internal validity of our RD design but, as shown and discussed below, they result in a first stage of our Instrumental Variable estimates for the interview sample that is stronger than the one displayed in the right panel of Figure 4 for the B4 universe.

As for external validity, Table 2 compares the means of selected socioeconomic variables, that are available only for the interview sample, with corresponding means for representative samples of the population of families with two employed parents in Northern Italy.[25] The comparison reveals that the interview sample is, by and large, representative of the corresponding Italian population in terms of demographics. However, parents are slightly more educated and less frequently self-employed. The higher educational attainment of the parents in the interview sample is relevant for the interpretation of our results, because it is one of the reasons why, different from other studies, our estimated effects of daycare 0–2 refer to children who, at home, can enjoy a relatively richer cultural environment by Italian standards.

[25] For age and educational characteristics we used the Bank of Italy Survey of Household Income and Wealth (SHIW), a biennial survey that can be weighted to represent the Italian population. From the waves of this survey, we selected observations to mimic the basket 4 universe of the BDS administrative files in 2001-2005. Specifically, we restricted the analysis to households with two employed parents from the 2000–2006 waves, living in cities of Northern Italy with a population of at least 200,000, and who, between 2013 and 2015, had at least one child between 8 and 14 years of age. For parental occupation we used the ISTAT Labor Force Statistics, selecting workers of the 2005 wave, in the 25–44 age range (i.e., the age range of parents in our sample when they first applied for daycare admission 8 to 12 years prior to the interview).

6 A RD design for the effect of daycare 0–2

Let Ω_i be an outcome observed at age 8–14 and denote with D_i the treatment intensity, measured as months (20 working days) spent in daycare over the entire 0-2 age period.[26] The running variable is the FAI, Y_i, at first application and the estimated equation is[27]

$$\Omega_i = \alpha + \beta D_i + f(Y_i) + \gamma A_i + \delta X_i + \epsilon_i, \tag{1}$$

where $f(Y_i)$ is a second order polynomial in the running variable, A_i is a vector of variables that describes the application set of a child (dummies for the city neighborhood of the preferred program and the number of programs included in the application set), and X_{it} is a vector of pre-treatment personal and family variables (parents education, parents year of birth, number of siblings at the first application, whether parents were self-employed — as opposed to employees — during the year preceding the first application, and a dummy for cesarean delivery of the child). As usual in RD designs, the inclusion of pre-treatment observables is not strictly necessary for identification but it may increase efficiency and, most important, similar estimates of the treatment effect β when observables are included or not supports the validity of the identifying assumption that pre-treatment covariates are continuous at the thresholds (Imbens and Lemieux, 2008; Lee and Lemieux, 2010).[28] Finally, ϵ_i captures other unobservable covariates.

Since there is fuzziness in the RD design, we estimate equation (1) by IV using as instrument the dummy P_i which indicates whether a child qualifies for her preferred program at her first application or not,

$$P_i = \mathbb{I}(Y_i \leq T_i^{Yp}). \tag{2}$$

The first row of Table 3 reports estimates of the Intention To Treat (ITT) effect of just

[26] In the administrative data at our disposal we observe the precise daily attendance of children in daycare. For convenience, in the presentation of results we rescale days of attendance in months defined as 20 working days.

[27] In this parametric specification we do not center and stack thresholds, differently than what we do in the continuity figures described in previous sections (see footnote 18), and thus we avoid the problem described in Section 9.2 of the Appendix concerning observations located precisely at a threshold.

[28] Section 9.3 of the Appendix describes results of an explicit continuity test for pre-treatment covariates in the estimation sample.

qualifying for the preferred program in the interview sample.[29] In the left panel the outcome is IQ while it is conscientiousness in the right panel. The specification in the first column of each panel includes a second-order polynomial in the running variable only. The second column adds the application set characteristics, and the third one includes all controls. In both panels the ITT estimates are remarkably similar. Taking the third column in the left panel as the preferred specification, the estimated ITT reveals that crossing the Preferred FAI threshold (i.e., having a FAI barely sufficient to qualify for the preferred program), reduces total IQ by 3.1%. This estimate is statistically different from zero with a p-value of 0.006. The corresponding estimate for conscientiousness is positive (0.6%) but imprecisely estimated.

First stage estimates are reported in the second row of Table 3.[30] Just qualifying for the preferred program increases daycare 0-2 attendance by about six months. The F-test statistic on the excluded instrument is sufficiently large in all specifications and samples, suggesting that weak instruments are not a concern.[31]

Rescaling the ITT effect by the first stage gives the IV estimate of the effect of one month of daycare 0–2 attendance. In our preferred specification for IQ (left panel, third column), and similarly in the others, this is a statistically significant loss of 0.5% (p-value: 0.003) which, at the sample mean (116), corresponds to 0.6 IQ points and to 4.5% of the IQ standard deviation. For conscientiousness, in the right panel, all point estimates are very close to zero but imprecise.

As a check on our parametric assumptions, we follow the methodology suggested in

[29] Specifically, we estimate the following reduced form equation,

$$\Omega_i = \tilde{\alpha} + \tilde{\beta} P_i + \tilde{f}(Y_i) + \tilde{\gamma} A_i + \tilde{\delta} X_i + \tilde{\epsilon}_i; \tag{3}$$

where $\tilde{f}(Y_i)$ is a second order polynomial in the FAI and $\tilde{\beta}$ is the Intention-To-Treat (ITT) effect.

[30] In this case, we estimate the first stage equation,

$$D_i = \bar{\alpha} + \bar{\beta} P_i + \bar{f}(Y_i) + \bar{\gamma} A_i + \bar{\delta} X_i + \bar{\epsilon}_i; \tag{4}$$

where $\bar{f}(Y_i)$ is a second order polynomial in FAI and $\bar{\beta}$ is the first stage estimate.

[31] In the B4 universe described by the right panel of Figure 4, crossing the preferred threshold from higher to lower FAI implies an increase of only about three months of daycare attendance. The fact that in the interview sample the first stage is larger is due to the differences between this sample and the universe discussed in Section 5. Results available from the authors show that the first stage in the invited sample is as large as the first stage in the interview sample, while the first stage in the B4 universe is in line with the one displayed in Figure 4.

Calonico *et al.* (2014b) to obtain non-parametric estimates that can be compared with the ones described in Table 3. These estimates are reported in the first and fourth columns of the Appendix Table B, respectively for IQ and Conscientiousness and are based on a triangular kernel and a local polynomial of degree zero with optimal bandwith selection.[32] Results are in line with those of Table 3 although not statistically significant given the small sample size. For IQ the ITT effect of just qualifying for the preferred program is estimated to be a loss of 2.8% (it is 3.1% in Table 3). The non-parametric first stage estimate is slightly lower than the parametric one as well (4.6 instead of 6.3 months). As for the IV estimate, both the conventional and the bias-corrected non-parametric estimators suggested by Calonico *et al.* (2014b) imply a 0.6% IQ loss induced by one additional month of daycare 0-2 (which is slightly higher than the 0.5% parametric loss reported in Table 3). Non-parametric results for conscientiousness suggest more positive effects but, again, imprecisely estimated.

To summarize, daycare 0-2 has a negative effect on IQ. Parametric and non-parametric methods provide similar estimates, with the former yielding more precise results. No relevant effect is detected for conscientiousness.

7 The "smoking gun" of a plausible explanation

Psychologists have produced persuasive empirical evidence that one-to-one interactions with adults (more than interactions with peers) are a crucial input for cognitive development in the first three years of life of a child. For instance, in an empirical field study of 42 American families, Hart and Risley (1995) have recorded one full hour of words spoken at home every month for two and a half years by parents with their children at age 0–2. They conclude that "the size of the children's recorded vocabularies and their IQ scores where strongly associated with the size of their parents' recorded vocabulary and their parents' scores on a vocabulary pre-test" (p. 176). Along the same lines, Rowe and Goldin-Meadow (2009) and Cartmill *et al.* (2013) show that the quality of parental input in the first three years of life

[32] Estimates based on a higher order local polynomial, which according to Imbens and Lemieux (2008) should be preferable in this context, are unstable and not reliable. Since the parametric analysis is implemented on stacked and centered thresholds, here we drop observations located at zero distance from thresholds for the reasons explained in Section 9.2 in the Appendix. See the note to the table for further details.

(e.g. in terms of parental gesture and talking) improves children's vocabulary before school entry. Similarly, Gunderson *et al.* (2013) finds that parental praise directed to 1-3 years old children predicts their motivation five years later.[33]

What is perhaps even more interesting is why, according to psychologists, these one-to-one interactions with adults early in life are so important. A fascinating theory has been proposed by Csibra and Gergely (2009) and Csibra and Gergely (2011). According to this theory the communication between a trusted adult and a child allows the child to understand more rapidly if an experience has a general value or only a specific value. In the absence of such communication the child has to repeat and confirm the experience many times in order to assess its general or particular validity (very much like in a sort of statistical inference requiring a large sample). An adult, instead, can quickly inform the child on the nature of what he or she is experimenting. If the adult can be trusted, then the child can save time and move on to other experiences, thus gaining an advantage in terms of cognitive development.

The focus on one-to-one interactions in our context is relevant because, as noted by Clarke-Stewart *et al.* (1994), infants and toddlers generally experience less one-to-one attention in daycare than at home because at home they are typically taken care of by a parent, a grandparent, or a nanny. Under these care modes a child receives attention in a 1:1 ratio, possibly somewhat higher if, for example, siblings are present. This is precisely the case for the children in our sample. When we asked their parents which options were available at the time of the first application as an alternative to daycare during the workday, 50.5% of families checked "the mother", 11% checked "the father", 44.8% checked "the grandparents", 4.5% checked "other family members", 18.9% checked "a babysitter or a nanny", and only 12.1% checked "some other daycare center" (multiple answers were possible).

The adult-to-children ratio in daycare 0–2 depends instead on the specific institutional setting. At the BDS, during the period under investigation, this ratio was 1:4 at age 0 and 1:6 at age 1–2. This may be part of the reason why, different from us, both Felfe and Lalive

[33] Related to these results, psychologists, like economists (see footnote 8), have estimated negative effects of increasing parental working time (in particular maternal) on cognitive, non-cognitive and behavioural outcomes of children. See, for example, Brooks-Gunn and Waldfogel (2002), Adi-Japha and Klein (2009), McPherran Lombardi and Levine Coley (2014) and the meta-analysis in Li *et al.* (2013). Differently than in the economic literature, however, most of these studies are observational and do not exploit quasi-experimental identification strategies.

(2014) and Drange and Havnes (2015) find positive effects of daycare 0–2 in Germany and Norway. In their institutional setting, the adult-to-child ratio is about 1:3, much closer to a family environment.

A second claim that psychologists have supported with persuasive empirical evidence is that girls are more mature than boys and, specifically, more capable of absorbing cognitive stimuli at an early age. For example, Fenson *et al.* (1994) study 1,800 toddlers (16-30 months of age) finding that girls perform better in lexical, gestural, and grammatical development. Galsworthy *et al.* (2000) examine about 3,000 2-year-old twin pairs and show that girls score higher on verbal and non-verbal cognitive ability. The longitudinal study of Bornstein *et al.* (2004) on 329 children observed between age 2 and 5 reaches similar conclusions for an age range partially overlapping with ours: they show that "girls consistently outperformed boys in multiple specific and general measures of language" (p. 206).

If girls at this age are more capable of making good use of stimuli that improve their skills, then their development is hurt by an extended exposure to a type of care that implies fewer one-to-one interactions with adults. In the presence of dynamic complementarities in the production of cognitive skills (Cunha and Heckman, 2007), this loss would not be compensated by later access to more stimulating types of care. Therefore given that the adult-to-children ratio in the BDS is lower than at home, attending daycare 0–2 instead of staying at home should have more adverse consequences for girls, because it deprives them of an input that is more valuable for their cognitive development than for boys.

7.1 Gender differences in the effects of daycare 0–2

Figures 7 provides a graphical description of how IQ and conscientiousness change around Preferred FAI thresholds, separately by gender. While in the left panels the LLR fit for boys is continuous when the FAI crosses the Preferred threshold from higher to lower values (left to right), for girls we observe a negative jump in the case of IQ (top right of Figure 7). Girls with a FAI small enough to just qualify for their preferred program have a lower IQ with respect to girls who barely do not.

We can easily dismiss the possibility that this gender heterogeneity in the effects of daycare 0–2 reflects differences in pre-treatment characteristics of boys and girls in our sample: Table 4 as well as previous figures on the continuity of pre-treatment covariates show that these variables and treatment intensity are perfectly balanced across genders. Similar across genders are also the answers given by parents to the question concerning alternative modes of care, which we described above.

We find instead that the evidence for IQ in Figure 7 is confirmed by the parametric estimation of equation (1), separately for boys and girls. Results are reported in the left portion of Table 5, for the same three specifications already considered in the corresponding portion of Table 3. In our preferred specification, which includes all controls in the third column, the ITT effect of just qualifying for the preferred program, is a statistically significant loss of about 4.4% of IQ for girls, while for boys it is half as much and we cannot reject the hypothesis that it is equal to zero. A similar gender difference emerges also in the IV estimates, which indicate that for girls one additional month in daycare 0–2 reduces IQ by 0.7% (p-value = 0.008), while the effect for boys is about half this size and is not statistically different from zero.[34] The middle panel shows that the gender gap in the ITT is not due to differences in the first stage. For both boys and girls, attendance in daycare 0-2 increases by approximately 6.3 months when the FAI crosses the preferred threshold from higher to lower values. A qualitatively similar gender difference in the effects of IQ emerges from the non-parametric estimates reported in columns 2 and 5 of the Appendix Table B where, if anything, the losses for girls appear to be even larger. The estimates for conscientiousness are very imprecise in all specifications, parametric or not.

This cognitive loss for girls induced by daycare attendance is a smoking gun for the relevance of one-to-one interactions with adults as an explanation of our results.[35]

[34] Interestingly, in a longitudinal study of 113 first-born preschool children, 58 girls and 55 boys, Bornstein *et al.* (2006) find, in line with our results, that "Girls who had greater amount of non-maternal care from birth to 1 year scored lower on the Spoken Language Quotient at preschool" (pag. 145).

[35] We have also explored the possibility that the loss suffered by girls depend on sex ratios within each program. Psychologists have observed that in early education "(T)eachers spend more time socializing boys into classroom life, and the result is that girls get less teacher attention. Boys receive what they need ... Girls' needs are more subtle and tend to be overlooked." (Koch, 2003, p. 265). However, we do not find any evidence that sex ratios affect the size of the effects for girls and boys, possibly because the variation in these ratios is quite small for the children in our sample. Moreover, the data do not support another possible hypothesis according to which gender differences in breastfeeding explain the gender gap in the effects of daycare. The duration of breastfeeding has been shown to be positively associated with cognitive outcomes (Anderson *et al.*, 1999; Borra *et al.*, 2012; Fitzsimons and Vera-Hernandez, 2013), and early daycare enrolment or attendance may shorten it. However, we find no effect (and specifically no differential effect by gender) of days in daycare on breastfeeding duration.

7.2 The role of family background

To further support the interpretation suggested in the previous section, we investigate whether the gender differences in the effect of daycare 0–2 become more evident when one-to-one interactions at home are complemented by a richer set of cultural and economic resources offered by a more affluent family background.

We explore the role of resources at home by separating children in two groups according to whether the preferred threshold to which they are associated is above or below the median of all preferred thresholds.[36] Results are reported in Table 6 for girls only and for girls and boys together.[37] Estimates in the left portion of the table are for the less affluent group, and refer to the effect of daycare 0–2 around a preferred threshold of €16.4K on average, while in the right portion the focus is on the more affluent group, around a threshold of about €33.0K. When the outcome is IQ, in the top panel, girls from more affluent families appear to suffer a quite large 1.6% loss for every additional month of daycare 0–2. When all children are considered together the loss drops to 1.1%, suggesting that daycare 0–2 attendance is particularly detrimental for girls with a more favorable home environment. The point estimate for less affluent girls is essentially zero, although with a relatively large standard error. Interestingly, also in the case of conscientiousness we now see a consistently negative effect of time spent in daycare 0-2 for girls in affluent families, although standard errors are again large.

8 Conclusions

To the best of our knowledge, this is the first paper that studies the effects time spent in daycare 0–2 for children from advantaged households, like those with two cohabiting parents in one of the most highly educated and richest Italian cities. For this population as a whole, our results indicate quantitatively and statistically significant losses only for girls. Moreover, these losses are even more pronounced when, within this population, we look at children with more affluent parents. These are typically the relevant marginal subjects to be considered

[36] Cattaneo *et al.* (2015) recommend this practice in the presence of multiple thresholds.

[37] Given the very small sample size defined by the intersection of gender and type of threshold, the first stage for boys is not sufficiently precise.

in an evaluation of daycare expansions for the worldwide increasing community of families in which both parents want to work.

Our results seem relevant not only because of their novelty with respect to the literature, but more importantly because they implicitly support the hypothesis, suggested by psychologists, according to which the sign and size of the effects of daycare 0–2 are mostly driven by three factors: whether this early life experience deprives children of one-to-one interactions with adults at home, by the quality of these interactions and by whether children can make good use of them.

For the girls of affluent families that we have studied, daycare 0–2 has a negative effect on cognitive outcomes precisely because the adults-to-children ratio in the Bologna Daycare System is relatively low with respect to the home environment, because the quality of the interaction at home that the daycare arrangement crowds out is high, and because these girls are developed enough, at this young age, to exploit high quality interactions with adults that for boys are not as valuable.

These results suggest that in the design of daycare expansions for children at very early ages, a careful cost benefit analysis should be performed to evaluate whether the adult-to-child ratios that would be necessary to avoid negative effects are privately and socially cost effective with respect to alternative care modes. Moreover, the gender gap in the effects of daycare 0–2 that we uncover, points to the desirability of some diversification of daycare activities for boys and girls, based on their respective maturity.

9 Appendix

9.1 How the Family Affluence Index is constructed

The Family Affluence Index is the ISEE (*Indicatore della Situazione Economica Equivalente*) which is used by the Italian public administration to determine access priority and fees for a wide range of public services. For the years we consider the index is computed in three steps. First, earnings of all family members living in the household are added to the income from financial activities in a given year. The latter is estimated by applying the average interest rate on 10-year government bonds during the previous year to all financial assets held by family members. If the family pays a rent for its primary dwelling, then an allowance of up to about €5,000 is subtracted from this total income component. Denote with I_{it} this total income component.

Second, the net wealth component is the sum of the values of all non-housing assets (at face value, except for stocks which are priced at their market value at the end of the previous year), and the value of the housing stock (register value), net of the maximum between about €50,000 and the residual value of all mortgage loans for which that stock is a collateral. A further allowance of up to about €15,000 can be subtracted from the value of non-housing assets. The 20% of such measure of net wealth is the net wealth component that we denote by W_{it}.

Finally, the resulting total income and net wealth index is adjusted for family size by dividing the total income and net wealth components by a concave transformation of family size: 1.00 for a single-person household, 1.57 for a two-person household, 2.04 for three members, 2.46 for four members, 2.85 for five members. For households with more than five members, a coefficient of 0.35 is added to the family size factor for each additional member from the sixth onward. The family size factor is further increased by 0.2 if the household has a single-parent with children below 18, 0.2 if the household has two-working-parents , and 0.5 for each family member with a permanent disability. Denoting with S_{it} the family size factor, the FAI index is: $Y_{it} = (I_{it} + W_i)/S_{it}$.

9.2 Stacking thresholds and observations at zero distance

Some specific features of our institutional setting need to be clarified in order to explain why we drop observations at zero distance from thresholds when we center at zero and stack thresholds for descriptive purposes and to estimate treatment effects non-parametrically. These specific features highlight a potential problem of stacking thresholds in RD designs, which may be common to other relevant settings but that seems to have been overlooked in the literature. This potential problem is similar but not identical to the one highlighted by DeChaisemartin and Behaghel (2015), on which we come back below.

Consider a simple example with two programs only: "Poor" and "Rich". The "Poor" program attracts 80% of applications and has a low FAI threshold. The "Rich" program attracts instead only 20% of applications and has a high FAI threshold. When we stack thresholds and consider the distance from each threshold as the running variable, the average of a covariate (e.g. FAI itself) in a neighbourhood of zero distance (but excluding subjects exactly at zero distance) is an average of "Poor" and "Rich" values with weights that are respectively 0.8 and 0.2. At exactly zero distance, instead, there is one observation for each program and therefore the "Poor" and the "Rich" FAI values at exactly zero distance are averaged with equal weights of 0.5. Therefore, while the average of FAI (or of any other pre-treatment covariate) is a continuous function of the distance from the threshold *around zero distance*, it is instead discontinuous *exactly at zero distance*.

Moreover the density around zero distance would be a mixture of the densities around the two separate thresholds and this creates further problems if we modify the above simple example allowing for a larger number of programs (as it is the case in our setting). If densities are continuous at the different thresholds, after centering and stacking them there would be continuity also of the density of the distance *around zero*. However, *exactly at zero* we would observe a probability mass spike because at this value of the distance we would have one observation for each program, corresponding to each threshold. In the real situation analysed in this paper there are 545 programs with rationing in basket 4 and in all of them there is a child with exactly zero distance from the correspondent Final threshold, but the probability of observing any other specific value different than zero is theoretically null and effectively small because the FAI is a continuous variable.

DeChaisemartin and Behaghel (2015) suggest that observations at exactly zero distance from thresholds should be dropped independently of whether the analysis is conducted on thresholds that are centered at zero and stacked. In their settings subjects are characterised by an intrinsic type: being a "accepter" or a "refuser" of a potential offer. While the proportion of accepters is continuous around a threshold, it must be exactly one for subjects located precisely at the threshold. Therefore, independently of stacking, these subjects should be dropped from the analysis. Note, however, that given how we use FAI final thresholds to construct a RD design around Preferred FAI thresholds (see Section 4.2), the problem discussed in DeChaisemartin and Behaghel (2015) does not apply to our parametric analysis.

The problem we highlight may also be reminiscent of the "Donut" problem discussed in Barreca *et al.* (2015), but has a very different origin. In that setting, observations at exactly zero distance are problematic because of heaping and manipulation of the running variable. In our setting, there is no heaping and manipulation and the problem originates from stacking thresholds in a situation in which:

a in each program the threshold is the value of the running variable corresponding to the last child who receives an offer and accepts it;

b the running variable is continuous and not uniformly distributed;

c there is a large number of thresholds.

9.3 Continuity of pre-treatment variables

As a further, explicit test of the continuity of pre-treatment covariates, we follow Abdulkadirolu *et al.* (2014) and estimate a system of 8 equations (one for each covariate $x_{it} \in X_{it}$ of equation (1) via Seemingly Unrelated Regression and then we test the joint significance of the instrument defined by equation (2) across the system equations. The system is represented by:

$$x_{it} = \alpha + \beta P_{it} + \gamma A_{it} + f(Y_{it}) + \epsilon_{it}, \qquad (5)$$

where x_{it} is one of the following: years of father education, years of mother education, father year of birth, mother year of birth, number of siblings at the time of the first application, whether the father was self-employed at the time of the first application, whether the mother was self-employed at the time of the first application, whether Cesarean delivery of the child. Table A reports results and test statistics. Continuity is never rejected.

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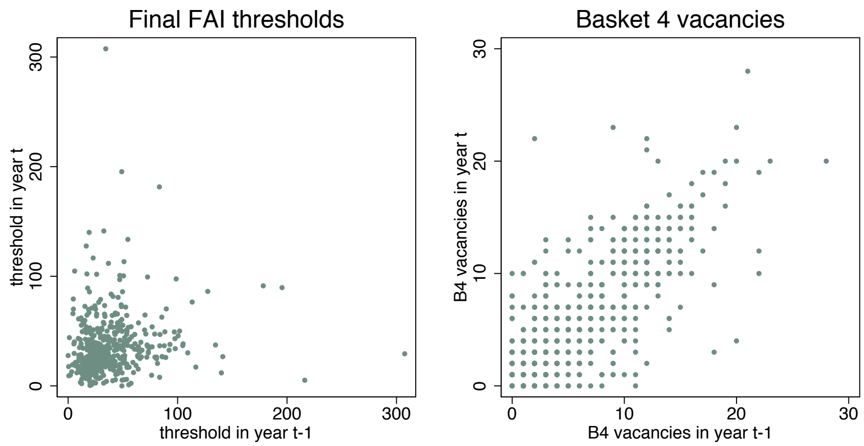
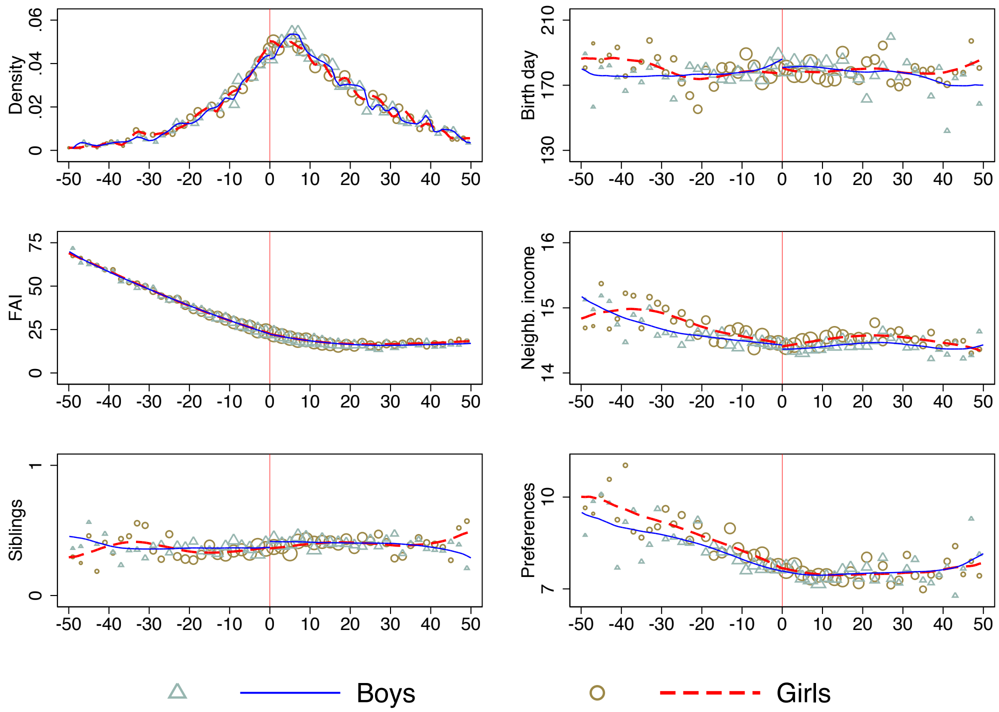
Figures and Tables

Figure 1: Persistence of Final FAI thresholds and Basket 4 vacancies over time



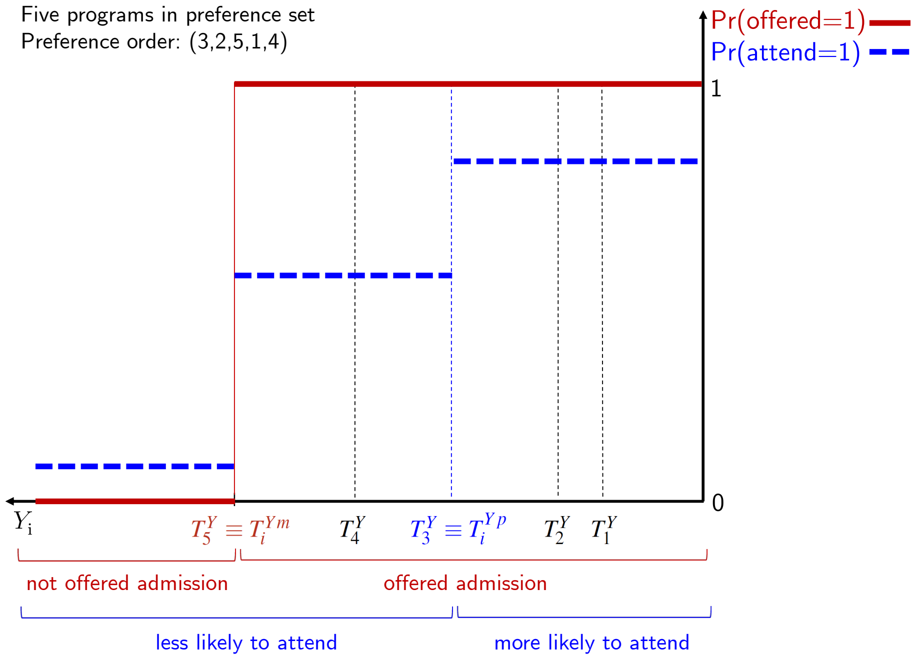
Notes: Each dot represents a program and the coordinates are either the Final FAI thresholds of that program in two consecutive years (left panel), or the vacant capacity for "basket 4" (two working parents) children in two consecutive years (right panel). FAI stands for Family Affluence Index. Sample: 238 programs with rationing for "basket 4" children in two consecutive years. The lack of persistence of Final FAI thresholds is of course even more evident for programs that are not offered every year.

Figure 2: Density of distance and continuity around Final FAI thresholds



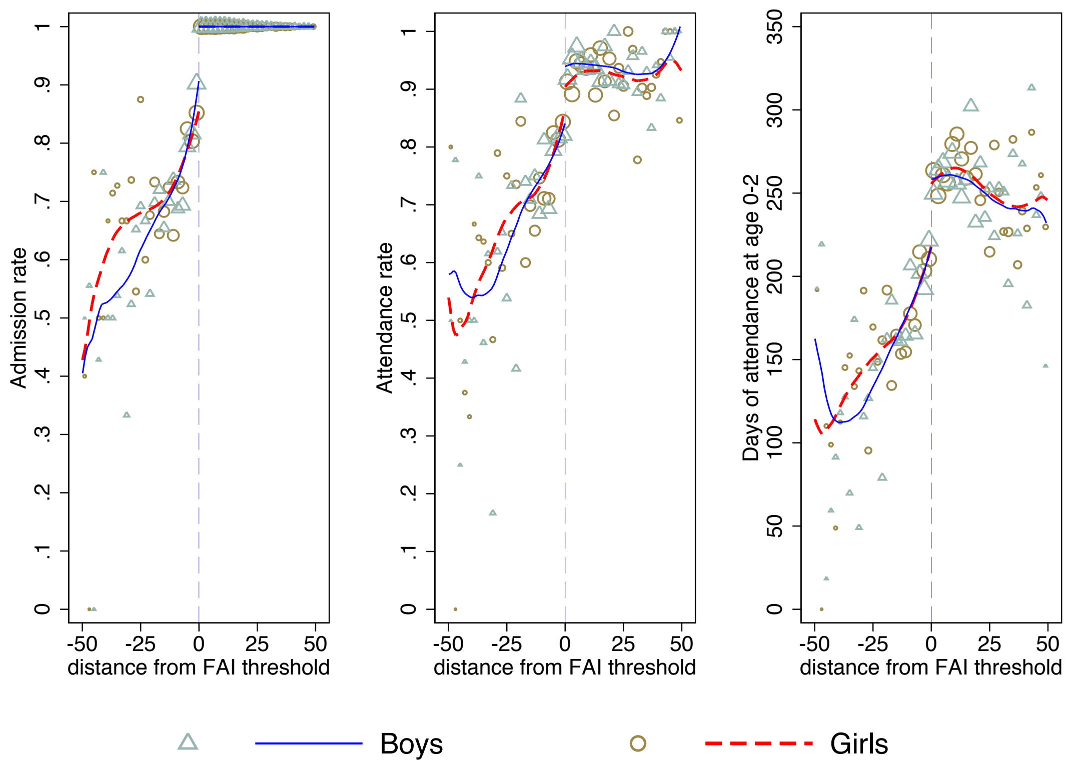
Notes: The dots represent the frequency distribution (top-left panel) and the average of five pre-treatment variables (remaining panels) inside €2000 bins, plotted as a function of the distance (thousands of real €) of a child's FAI from her Final FAI threshold, by gender. The bold lines are LLR on the underlying individual observations, with a triangular kernel and optimal bandwidth selection, from Calonico *et al.* (2014b). FAI stands for Family Affluence Index. Sample: 5,089 children with two working parents, born between 1999 and 2005 who first applied for admission between 2001 and 2005, whose FAI distance from the Final FAI thresholds is at most €50K and is different from zero.

Figure 3: Example: Maximum and Preferred FAI thresholds in the application set



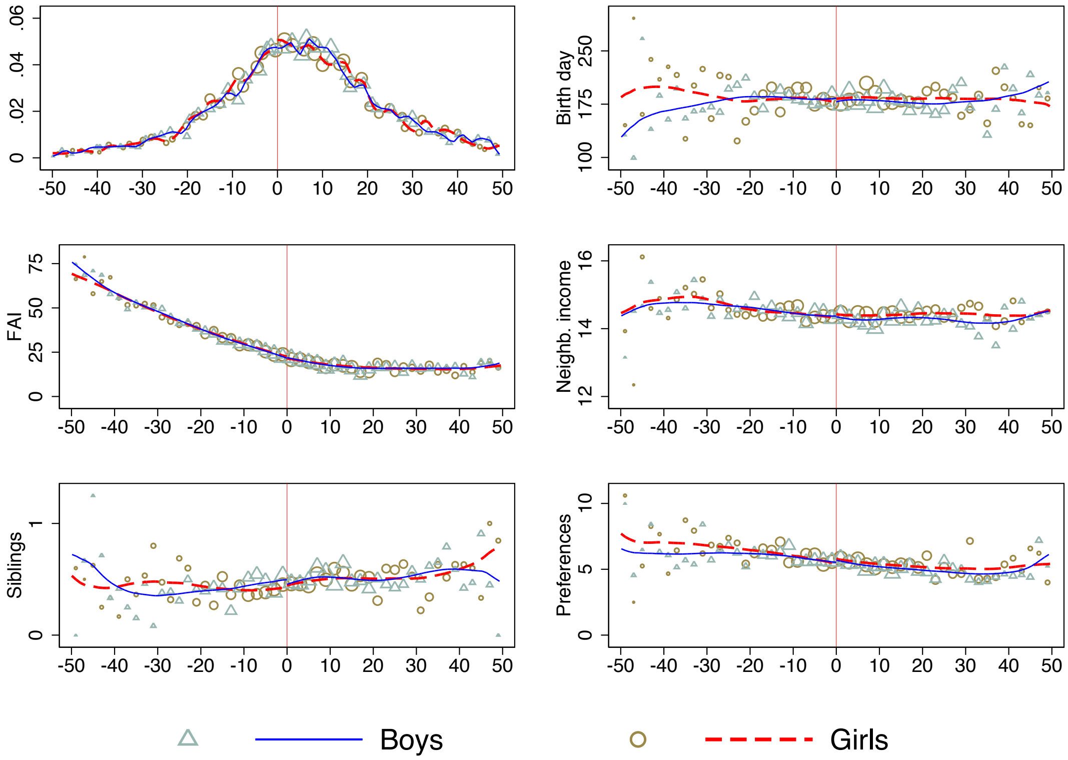
Notes: This figure illustrates the definition of "Maximum" and " Preferred" FAI thresholds for a hypothetical child who first applies for admission in a given year listing five programs in her application set. FAI stands for Family Affluence Index.

Figure 4: Admission offers and attendance around Preferred FAI thresholds



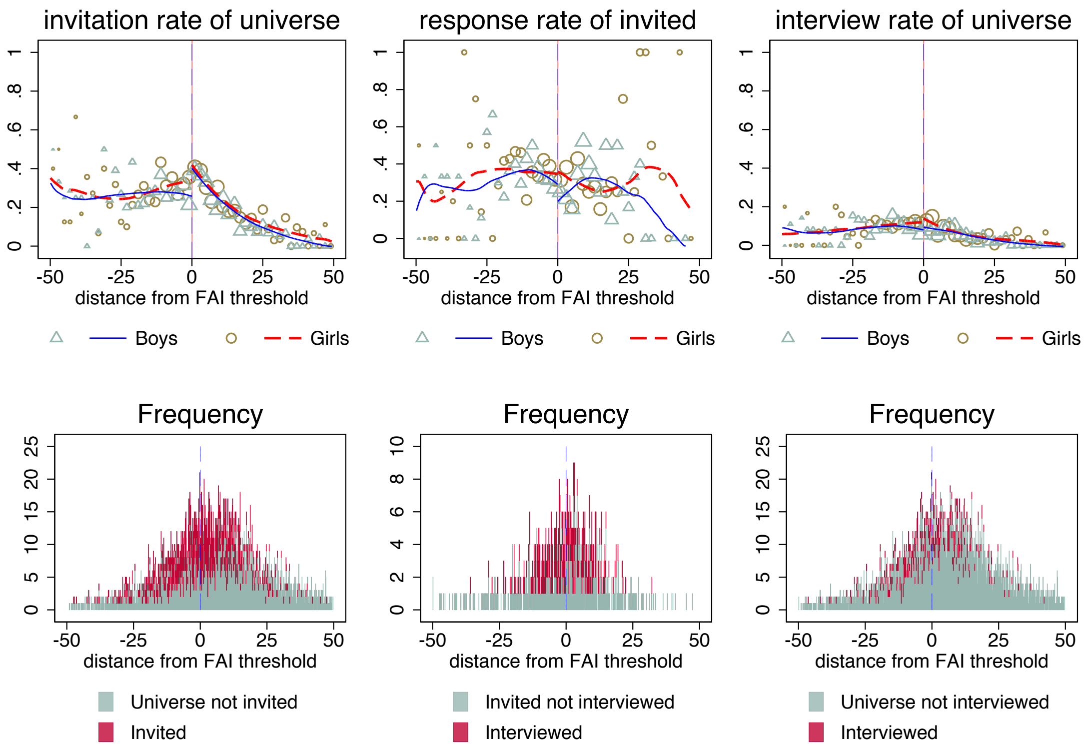
Notes: The dots represent offer rates (left), attendance rates (middle) and average days of attendance at age 0–2 (right) inside €2000 bins, plotted as a function of the distance (thousands of real €) of a child's FAI from her Preferred FAI threshold, by gender. The bold lines are LLR on the underlying individual observations, with a triangular kernel and optimal bandwidth selection, from Calonico *et al.* (2014b). FAI stands for Family Affluence Index. Sample: 5,101 children with two working parents, born between 1999 and 2005 who first applied for admission between 2001 and 2005, whose FAI distance from the Preferred FAI threshold is at most €50K and is different from zero.

Figure 5: Density of distance and continuity of covariates around Preferred FAI thresholds



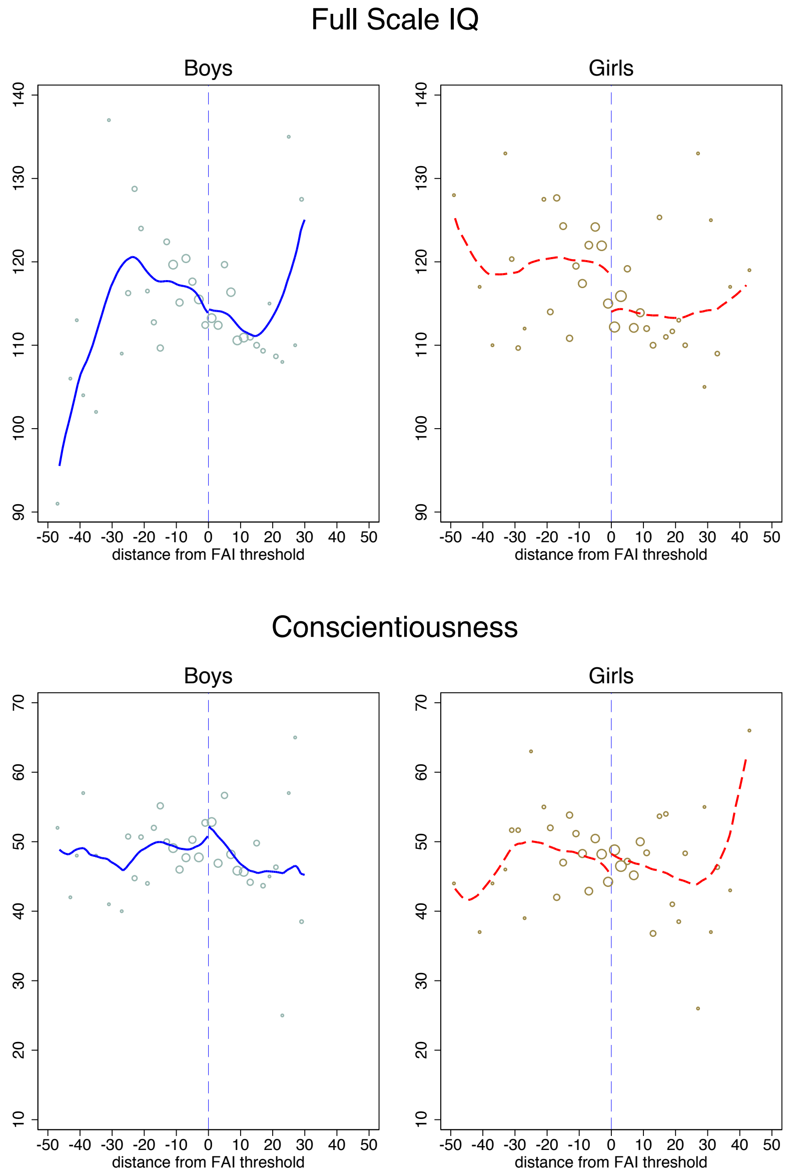
Notes: The dots represent the frequency distribution (top-left panel) and the average of five pre-treatment variables (remaining panels) inside €2000 bins, plotted as a function of the distance (thousands of real €) of a child's FAI from her Preferred FAI threshold, by gender. The bold lines are LLR on the underlying individual observations, with a triangular kernel and optimal bandwidth selection, from Calonico *et al.* (2014b). FAI stands for Family Affluence Index. Sample: 5,101 children with two working parents, born between 1999 and 2005 who first applied for admission between 2001 and 2005, whose FAI distance from the Preferred FAI thresholds is at most €50K and is different from zero.

Figure 6: Interviews and response rates around Preferred FAI thresholds



Notes: The top row shows the continuity of the invitation rate for the universe (left), the response rate of the invited (middle), and the interview rate for the universe (right) at the Preferred FAI threshold, by gender, as a function of the distance from the threshold, meausured in thousands of real €. The bold lines are LLR on the underlying individual observations, with a triangular kernel and optimal bandwidth selection, from Calonico *et al.* (2014b). The bottom row shows the absolute frequency distribution of children in the universe (left and right) and the invited (middle), and, on top of each bar, the corresponding distribution of invited (left) and interviewed children (middle and right) inside €2000 bins, plotted as a function of the distance (thousands of real €) of a child's FAI from her Preferred FAI threshold, by gender. FAI stands for Family Affluence Index. Sample: 5,101 children with two working parents, born between 1999 and 2005 who first applied for admission between 2001 and 2005, whose FAI distance from the Preferred FAI thresholds is at most €50K and is different from zero.

Figure 7: Full Scale IQ and Conscientiousness around Preferred FAI thresholds, by gender



Notes: The dots represent average IQ (top panels) and average Conscientiousness (bottom panels) inside €2000 bins, plotted as a function of the distance (thousands of real €) of a child's FAI from her Preferred FAI threshold, by gender. The bold lines are LLR on the underlying individual observations, with a triangular kernel and optimal bandwidth selection, from Calonico *et al.* (2014b). FAI stands for Family Affluence Index. Sample: 373 (top panels) and 371 (bottom panels) interviewed children with two working parents, born between 1999 and 2005 who first applied for admission between 2001 and 2005, whose FAI distance from the Preferred FAI thresholds is at most €50K and is different from zero.

Table 1: Descriptive statistics for the basket 4 universe, the invited and the interview samples

Variable	Universe B4	Invited	Interview	p-val
FAI at first application	24.87	26.50	27.10	0.007
	(20.50)	(19.70)	(17.55)	[0.547]
				{0.010}
Number of preferences at first application	5.42	5.29	5.59	0.222
	(3.66)	(3.42)	(3.53)	[0.120]
				{0.341}
Siblings at first application	0.48	0.49	0.54	0.755
	(0.66)	(0.65)	(0.70)	[0.151]
				{0.079}
Day of birth in the year	182.8	186.6	180.5	0.222
	(104.1)	(106.)	(111.1)	[0.310]
				{0.673}
Offered admission at first application	0.897	0.777	0.752	0.000
	(0.303)	(0.417)	(0.432)	[0.297]
				{0.000}
Waiver at first application	0.124	0.075	0.068	0.000
	(0.330)	(0.263)	(0.251)	[0.607]
				{0.000}
Year first applied	2003.1	2003.4	2003.5	0.000
	(1.43)	(1.42)	(1.38)	[0.086]
				{0.000}
Year child born	2002.0	2002.5	2002.6	0.000
	(1.58)	(1.63)	(1.62)	[0.086]
				{0.000}
Grade first applied for	0.882	0.568	0.540	0.000
	(0.786)	(0.673)	(0.676)	[0.459]
				{0.000}
Days in	212.2	223.6	230.5	0.010
	(143.3)	(151.4)	(156.3)	[0.417]
				{0.017}
Ever attended (share with days in >0)	0.847	0.784	0.782	0.000
	(0.360)	(0.411)	(0.414)	[0.916]
				{0.001}
N	6,575	1,379	444	

Notes: This table compares the means of variables from the administrative records in the "basket 4" (B4) universe (6,575 children whose parents were both employed at the time of the first application, born between 1999 and 2005 who first applied for admission between 2001 and 2005), in the sample invited for an interview (1,379 children sampled from this universe), and in the interview sample (444 children sampled and interviewed from the universe). The p-values in the last column refer to tests of the equality of means for the B4 universe and the invited (first row), the invited and the interviewed (second row, in square brackets), the B4 universe and the interviewed (third row, in curly brackets). FAI stands for Family Affluence Index.

Table 2: The interviewed sample in comparison to the Northern Italian population

	Interview sample	Northern Italy
Child age	10.7	11.1
	(1.6)	(1.7)
Father age	47.3	47.0
	(4.8)	(4.7)
Mother age	44.9	45.0
	(4.1)	(4.8)
Years education father	14.2	13.1
	(3.8)	(3.0)
Years education mother	15.5	14.4
	(3.2)	(2.5)
Father self-employed	0.236	0.276
	(0.425)	—
Mother self-employed	0.106	0.173
	(0.308)	—
Observations	444	93

Notes: This table compares the means of variables in the interview sample with the corresponding means in the Bank of Italy Survey of Household Income and Wealth (SHIW – age, education) and in the Labor Force Statistics (ISTAT – parental occupation). The SHIW is restricted to households with two employed parents from the 2000, 2002, 2004, and 2006 waves, living in cities of Northern Italy with a population of at least 200,000, and who between 2013 and 2015 have at least one child between 8 and 14 years of age. The Labor Force Statistics refer to year 2005, and to workers between 25 and 44 years of age (i.e., the age range of parents in our sample when they first applied for daycare admission 8 to 12 years prior to the interview.)

Table 3: Effects of daycare attendance on IQ and Conscientiousness for all children

	Log of IQ			Log of Conscientiousness		
ITT effect of qualifying for the preferred program	-0.026* (0.011)	-0.030** (0.011)	-0.031** (0.011)	-0.001 (0.024)	0.005 (0.025)	0.006 (0.024)
First stage: effect of qualifying on months of attendance	6.3** (0.9)	6.4** (0.9)	6.2** (0.9)	6.3** (0.9)	6.4** (0.9)	6.2** (0.9)
IV effect of one month of daycare attendance	-0.004* (0.002)	-0.005** (0.002)	-0.005** (0.002)	-0.000 (0.004)	0.001 (0.004)	0.001 (0.004)
F-stat on excluded instruments	53.8	51.4	46.1	53.1	51.0	46.0
Number of observations	444	444	444	441	441	441
Polynomial in FAI	Yes	Yes	Yes	Yes	Yes	Yes
Appl. set controls		Yes	Yes		Yes	Yes
Pre-treat. controls			Yes			Yes

Notes: Parametric estimates of the effect of one month of daycare 0–2 on the log of IQ and the log of Conscientiousness, with related ITT and first stage. ITT coefficients derive from regressions of the outcome on the instrument (whether the child's qualifies for the preferred program) and controls as in equation (3). First-stage coefficients derive from regressions of months spent in daycare 0–2 on the instrument and controls as in equation (4). IV coefficients derive from regressions of the outcome on months of attendance and controls, as in equation (1), using a dummy for qualification in the preferred program as the instrument. The running variable is the Family Affluence Index (FAI). The polynomial in the running variable is of second order. Sample: interviewed children with two working parents, born between 1999 and 2005, with non missing outcome or covariates and who first applied for admission between 2001 and 2005. Robust standard errors in parentheses, clustered at the facility level. * significant at 5%; ** significant at 1% or better.

Table 4: Characteristics of interviewed boys and girls

	Boys	Girls	p-val		Boys	Girls	p-val
FAI	27.3	26.9	0.82	Father education in years	14.1	14.4	0.49
	(1.3)	(1.1)			(0.26)	(0.24)	
N. of preferences	5.46	5.71	0.46	Mother education in years	15.5	15.4	0.82
	(0.24)	(0.23)			(0.22)	(0.21)	
N. of siblings	1.56	1.53	0.66	Father birthyear	1966.3	1966.2	0.81
	(0.05)	(0.05)			(0.32)	(0.32)	
Offered admission	0.76	0.75	0.78	Mother birth year	1968.5	1968.6	0.85
	(0.03)	(0.03)			(0.27)	(0.28)	
Waiver	0.05	0.08	0.18	Father self-employed	0.24	0.23	0.80
	(0.02)	(0.02)			(0.03)	(0.03)	
Year of first application	2003.4	2003.6	0.18	Mother self-employed	0.11	0.10	0.70
	(0.09)	(0.09)			(0.02)	(0.02)	
Grade at first application	1.57	1.52	0.42	Cesarean delivery	0.30	0.24	0.14
	(0.05)	(0.04)			(0.03)	(0.03)	
Ever attended	0.78	0.78	0.99	Month of breastfeeding	6.45	6.12	0.45
	(0.03)	(0.03)			(0.30)	(0.32)	
Months at entry	15.2	15.0	0.79	Interviewer: psychologist 1	0.433	0.384	0.30
	(0.5)	(0.5)			(0.03)	(0.03)	
Days of attendance	229.8	231.1	0.93	Interviewer: psychologist 2	0.163	0.179	0.65
	(10.5)	(10.5)			(0.03)	(0.03)	
Year born	2002.5	2002.7	0.21	Interviewer: psychologist 3	0.400	0.432	0.49
	(0.11)	(0.11)			(0.03)	(0.03)	
Day born	177.4	183.4	0.57	Year interviewed	2013.7	2013.7	0.43
	(7.5)	(7.4)			(0.04)	(0.04)	
Age at interview	10.7	10.6	0.31	Month interviewed	7.1	7.0	0.69
	(0.11)	(0.10)			(0.2)	(0.2)	

Notes: This table compares the 215 boys and 229 girls of the interview sample (444 children with two working parents). For each variable and gender the table reports the mean, the standard deviation in parenthesis and the p-value of a test that the mean is equal for boys and girls. The source for parental background, type of delivery, and breastfeeding are the interviews. For all the other variables the source is the administrative dataset of the BDS. FAI stands for Family Affluence Index.

Table 5: Gender heterogeneity of the effects of daycare 0–2 on IQ and Conscientiousness

	Log of IQ			Log of Conscientiousness		
ITT effect of qualifying for the preferred program						
Girls	-0.040**	-0.043**	-0.044**	0.006	0.012	0.022
	(0.015)	(0.016)	(0.016)	(0.035)	(0.036)	(0.037)
Boys	-0.013	-0.018	-0.022	-0.006	0.003	0.004
	(0.016)	(0.016)	(0.015)	(0.036)	(0.037)	(0.036)
First stage effect of qualifying on months of attendance						
Girls	6.54**	6.42**	6.36**	6.54**	6.42**	6.36**
	(1.13)	(1.19)	(1.20)	(1.13)	(1.19)	(1.20)
F-stat excl. instr.	33.4	29.3	27.9	33.4	29.3	27.9
Boys	6.11**	6.68**	6.23**	6.11**	6.71**	6.26**
	(0.92)	(0.88)	(0.90)	(0.93)	(0.89)	(0.91)
F-stat excl. instr.	44.3	57.2	47.6	43.1	56.4	47.5
IV effect of one month of daycare attendance						
Girls	-0.006*	-0.007*	-0.007**	0.001	0.002	0.003
	(0.002)	(0.003)	(0.003)	(0.005)	(0.005)	(0.006)
Boys	-0.002	-0.003	-0.004	-0.001	0.000	0.001
	(0.003)	(0.002)	(0.002)	(0.006)	(0.005)	(0.005)
Polynomial in FAI	Yes	Yes	Yes	Yes	Yes	Yes
Appl. set controls		Yes	Yes		Yes	Yes
Pre-treat. controls			Yes			Yes

Notes: Parametric estimates by gender of the effect of one month of daycare 0–2 on the log of IQ and the log of Conscientiousness, with related ITT and first stage. ITT coefficients derive from regressions of the outcome on the instrument (whether the child's qualifies for the preferred program) and controls as in equation (3). First-stage coefficients derive from regressions of months spent in daycare 0–2 on the instrument and controls as in equation (4). IV coefficients derive from regressions of the outcome on months of attendance and controls, as in equation (1), using a dummy for qualification in the preferred program as the instrument. The running variable is the Family Affluence Index (FAI). The polynomial in the running variable is of second order. Sample: interviewed children with two working parents, born between 1999 and 2005, with non missing corresponding outcome or covariates and who first applied for admission between 2001 and 2005. Number of observations: 229 girls for both IQ and Conscientiousness; 215 boys for IQ and 212 for Conscientiousness (see footnote 23). Robust standard errors in parentheses, clustered at the facility level. * significant at 5%; ** significant at 1% or better.

Table 6: Effects of one month of daycare 0–2 by level of the Preferred FAI threshold

	Threshold $\leq$ median (mean treshold: €16.4K)			Threshold > median (mean treshold: €33.0K)		
Log of IQ						
Girls	-0.000	-0.001	-0.001	-0.016**	-0.016**	-0.016*
	(0.003)	(0.003)	(0.003)	(0.006)	(0.005)	(0.007)
Boys & Girls	-0.001	-0.002	-0.001	-0.011*	-0.011*	-0.011*
	(0.004)	(0.003)	(0.003)	(0.005)	(0.005)	(0.005)
Log of Conscientiousness						
Girls	0.011	0.007	0.009	-0.018	-0.019	-0.019
	(0.009)	(0.008)	(0.007)	(0.013)	(0.012)	(0.013)
Boys & Girls	0.004	0.006	0.006	-0.014	-0.013	-0.015
	(0.007)	(0.006)	(0.007)	(0.011)	(0.010)	(0.012)
Poly. FAI	Yes	Yes	Yes	Yes	Yes	Yes
App. set		Yes	Yes		Yes	Yes
Pre-treat.			Yes			Yes

Notes: Parametric IV estimates of the effect of one month of daycare 0–2 on the log of IQ and the log of Conscientiousness, separately for Preferred FAI thresholds below or above the median Fai threshold. Coefficients derive from regressions of the outcome on months of attendance and controls, as in equation (1), using a dummy for qualification in the preferred program as the instrument. The first stage for boys is not sufficiently precise below the median. We therefore present, for each outcome, estimates for girls only and for girls and boys together. The running variable is the Family Affluence Index (FAI). The polynomial in the running variable is of second order. Sample: interviewed children with two working parents, born between 1999 and 2005, with non missing corresponding outcome or covariates and who first applied for admission between 2001 and 2005. Number of observations: 108 girls below the median and 104 girls above in the case of both IQ and Conscientiousness; 204 children below the median and 204 children above in the case of IQ; 203 children below the median and 203 children above in the case of conscientiousness. Robust standard errors in parentheses, clustered at the facility level. * significant at 5%; ** significant at 1% or better.

Figures and Tables for the Appendix

Table A: Continuity of covariates around Preferred FAI thresholds

	1 fedu	2 medu	3 fyob	4 myob	5 siblings	6 fself	7 mself	8 cesarean
All children	-0.077	0.050	-0.043	0.029	0.136	0.048	-0.027	-0.034
	(0.097)	(0.099)	(0.105)	(0.102)	(0.104)	(0.045)	(0.032)	(0.047)
H_0 : coefficient on P_{it} jointly zero in equations 1–8; $\chi^2(8) = 6.40$; p-val = 0.60								
Girls	0.027	0.078	-0.128	-0.101	0.185	0.146*	0.020	-0.077
	(0.138)	(0.140)	(0.152)	(0.149)	(0.147)	(0.063)	(0.045)	(0.063)
H_0 : coefficient on P_{it} jointly zero in equations 1–8; $\chi^2(8) = 9.10$; p-val = 0.33								
Boys	-0.144	0.056	0.031	0.183	0.114	-0.028	-0.075	-0.023
	(0.136)	(0.140)	(0.142)	(0.137)	(0.148)	(0.063)	(0.046)	(0.069)
H_0 : coefficient on P_{it} jointly zero in equations 1–8; $\chi^2(8) = 8.54$; p-val = 0.38								
Polyn. FAI	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
App. set contr	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Seemingly Unrelated Regression estimates of equation (5) of Appendix 9.3 using the pooled sample of boys and girls and separately by gender. The table reports also results of the continuity tests. Each covariate is a dependent variable in the system. The running variable is the Family Affluence Index (FAI). Legend: fedu = father education, years; medu = mother education, years; fyob = father year of birth; myob = mother year of birth siblings = number of siblings at the time of the first application; fself = whether father was self-employed at the time of the first application; mself = whether mother was self-employed at the time of the first application; cesarean = whether Cesarean delivery of child. The regressor of interest is the instrument P_{it}, which indicates whether a child's FAI is below the Preferred FAI threshold or not; second-order polynomials in FAI and application set controls are included on the RHS. Sample: 444 interviewed children with two working parents, born between 1999 and 2005 and who first applied for admission between 2001 and 2005. * significant at 5%; ** significant at 1%.

Table B: Effect of daycare 0–2 on IQ and Conscientiousness: non-parametric estimates.

Gender	All	Girls	Boys	All	Girls	Boys
	Log of IQ			Log of Conscientiousness		
ITT of just qualifying	-0.028	-0.037	-0.011	0.037	0.059	0.009
	(0.020)	(0.026)	(0.041)	(0.048)	(0.072)	(0.043)
First stage	4.6**	3.75*	6.2*	4.1*	2.7	6.2*
	(1.3)	(1.6)	(2.6)	(1.5)	(1.9)	(2.4)
robust p-value	0.045	0.485	0.055	0.083	0.784	0.060
Effect of 1 month (conventional)	-0.006	-0.010	-0.002	0.009	0.022	0.001
	(0.005)	(0.008)	(0.007)	(0.012)	(0.028)	(0.007)
Effect of 1 month (bias-corrected)	-0.006	-0.015^{+}	-0.001	0.015	0.048^{+}	0.002
	(0.005)	(0.008)	(0.007)	(0.012)	(0.028)	(0.007)
Effect of 1 month (robust)	-0.006	-0.015	-0.001	0.015	0.048	0.002
	(0.006)	(0.011)	(0.009)	(0.015)	(0.036)	(0.009)
Bandwith for Loc. Poly (h)	6.544	7.076	5.088	5.023	4.985	6.170
Bandwith for bias (b)	16.351	16.297	10.820	12.815	13.382	14.304
Number of observations	150	91	46	115	70	57

Notes: Non-parametric estimates of the effect of one month of daycare 0–2 on the log of IQ and the log of Conscientiousness, with related ITT and first stage. We follow the methodology suggested in Calonico *et al.* (2014b) and use their software described in Calonico *et al.* (2014a). The grade of local polynomials is zero and the kernel is triangular. We report the optimal bandwiths for the local polynomial (h) and for the bias (b) as well as the p-value of the test for the null that the first stage coefficient is zero, obtained when we use a robust and bias corrected estimator for the first stage equation. The ITT and first stage estimates are obtained using the conventional non-parametric estimator. The effects of one month of daycare 0–2 are obtained using three distinct RD estimators: the local polynomial estimator (conventional), the bias-corrected estimator proposed by Calonico *et al.* (2014b) and the same estimator with robust standard errors. The running variable is the Family Affluence Index (FAI). Sample: interviewed children with two working parents, born between 1999 and 2005, with non missing corresponding outcome or covariate and who first applied for admission between 2001 and 2005. Observation located exactly at the thresholds are excluded (see Section 9.2 in the appendix). $^{+}$ significant at 10%; * significant at 5%; ** significant at 1% or better.

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