Empowering Mothers and Enhancing Early Childhood Investment: Effect on Adults Outcomes and Children Cognitive and Non-Cognitive Skills*

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Abstract

Empowering women and enhancing children's early development are two important goals that are often pursued via independent policy initiatives in developing countries. In this paper we study a unique approach that pursues both goals at the same time: empowering mothers through tools that also advance their children's development. A program operated by AVSI, an Italian NGO, in a poor neighborhood of Quito, Ecuador, targets parents of children from birth to age 5. It provides family advisor-guided parent training sessions once every two weeks for groups of six to eight mothers and their children. We find that the program empowered women in various dimensions, including higher labor force participation and employment, higher likelihood of a full-time job in the formal-sector and higher wages. Treated mothers are also more likely to continue their education, make independent decisions regarding their own finances, have greater role in intra-household decisions, especially on issues involving children's education and discipline and increase parental inputs into their children's development. We find that treated children improve their cognitive and non-cognitive skills, for example, they are less likely to repeat a grade or temporarily drop-out from schooling, are less absent from and have improved behaviors in school, have better attitudes towards learning, and achieve higher scores on cognitive tests. All results hold when we estimate aggregate treatment impacts, use summary indices instead of individual outcomes in order to account for multiple inference, when we use entropy balancing to adjust for differences in pre-treatment covariates, when we perform matching exercises and when we use other robustness checks.

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1. Introduction

Empowering women and enhancing early childhood development are important policy goals which are often pursued as separate, discrete initiatives in developing countries. We study an approach that exploits potential complementarity by pursuing both goals through one measure, based on empowering mothers and teaching parenting skills that advance their children's development.

The accumulation of evidence that education programs are both more effective and less costly when delivered to younger children has sharpened the focus on early childhood research and policy (Heckman, Pinto, and Savelyev 2010, Heckman et al. 2013, Gertler et al. 2014). However, questions remain about which policy tools are most effective. Similarly, empowering women has been the focus of research and policy because engaging women as equal participants in the community and economy enhances development outcomes (Klugman et al. 2014, Wong 2012). Doing both - empowering women in a way that also supports early childhood development - has not been widely studied, however, it could be a means of achieving both goals. This approach has the potential to deliver improvements in women's empowerment and status at home and in the community, as well as improving children's education. Improving mothers' empowerment in different dimensions can have positive effect on early childhood (Barber and Gertler 2009, Carneiro et al. 2013, Kiernan and Huerta 2008[1]). Evidence of spillover effects from early child development programs on the empowerment of their mothers is scarce and needed (Baker-Henningham and Lopez-Boo 2010).

In this paper we study the consequences of a home-preschool program designed to enhance both women's empowerment and children's early childhood development. The *PelCa* (*preescolar en la casa* – home pre-schooling) program started in Pisullì, one of the poorest neighborhoods of Quito, Ecuador, in 2005. It is run by the Association of Volunteers in International Service (AVSI), an international non-governmental organization founded in Italy that focuses on human development. The program is open to mothers with children one to three years old, and currently involves hundreds of mothers and children.

In the program, a qualified family advisor trained groups of six to eight mothers. Children accompanied their mothers in these group sessions, which were held every two weeks in the NGO offices. There were three parts to each group session. In the first part, mothers received structured training focused on building their self-confidence and self-awareness, while also strengthening their role in the family. Mothers participated in discussions on the importance of self-care, personal growth,

[1] Carneiro et al. (2013) find that maternal education improves the home environments, particularly for low-ability mothers. Moreover, more educated mothers invest more quality time with their children (breastfeeding, reading books to children and engaging children in more outdoor recreational activities). The authors suggest that these child investments lead to better children's cognitive outcomes and fewer behavioral problems. Barber and Gertler (2009), argue that encouraging women to be more informed and active health consumers reduces child mortality, morbidity, anemia, and stunting. Kiernan and Huerta (2008) show that economic deprivation and maternal depression negatively affect children's cognitive and emotional outcomes, partly because of less nurturing and parental engagement.

gaining awareness of their own talents and acting upon them, while also learning parenting techniques that emphasized their children's early development. At the same time, children socialized using educational games and didactic materials. In the second part, family advisors taught mothers and children educational activities that could be reproduced at home, to improve the quality of maternal-child interactions and enhance mothers' participation in child development activities. In the third part, advisors monitored and assessed the home assignments from the previous meeting.

The program was implemented non-experimentally, but since its initiation in 2005, new families have joined every year. Assuming that new applicants resemble those who joined the program earlier, we selected a control group from the applicants in 2012. Following our suggestion, the NGO made a special effort to reach as many eligible families as possible in 2012. This provided us with a large pool of applicants from which we selected our control group, which consisted of families that had an older child in any grade in primary school and a younger child who would enroll in the program jointly with his or her mother. This can be viewed as a quasi-natural experiment, and we will demonstrate that it yields well-balanced treatment and control groups.

The treatment could empower women through several channels, including discussions and reflection on their own value, the importance of self-care, investment in own human capital, and engagement in income-generating activities. Acquiring new parental skills can improve self-confidence and control. Relying on a new network of women that support each other can instill security. We evaluate the effect on women's empowerment after two to seven years of participation in the program by focusing on the following outcomes: self-esteem, inputs into child education, labor-market participation and earnings, allocation of decision-making within the household, and economic and social independence. We also examine the impact on children's educational outcomes, such as how likely children were to repeat a grade or drop out of school, and how they fared in cognitive tests.

Our evidence shows that the program empowered women in various dimensions: participants are more likely to be employed, have a full-time job, in particular in the formal-sector job, earn higher wages, manage their own money and make spending decisions more independently, and often return to school as an adult. Gains are also evident in women's greater role in intra-household decisions, particularly in matters related to children's education and discipline. Mothers increase their child investment, for example by spending more time practicing cognitive and social skills with their children.

The program had positive effects on children: it significantly reduced the drop-out rate and likelihood of temporarily withdrawing from school, strengthened non-cognitive skills, and improved scores in cognitive tests (though it is precisely measured only for some sub-groups of the overall sample).

Allowing for heterogeneity of treatment effects by mother's pre-program empowerment and by child gender reveals meaningful differences, which we use to interpret our findings. All of the above results hold when we estimate aggregate treatment impacts, using summary indices instead of individual

outcomes, in order to account for multiple inferences, when we use entropy balancing to adjust for differences in pre-treatment covariates, and when we use other robustness checks.

The remainder of our paper is structured as follows. In Section 2 we present an overview of the literature on women's empowerment and on early childhood development. Section 3 outlines the background and design of the quasi-natural experiment. In Section 4 we describe the data. In Section 5 we discuss the empirical analysis, results and we explore potential mechanisms through which results are achieved. In Section 6 we discuss robustness checks and the conclusion is in Section 7.

2. Related Literature

The present work is related to two different literatures: studies on women's empowerment and studies on early childhood development. The literature on women's empowerment is more extensive. Women's empowerment is often defined as the "ability to make choices" in ways that change power relations and affect women's education, employment, and political participation (Kabeer 2005), or as increased access and utilization of social, political and economic opportunities (Duflo 2012). The role of women in decision-making within the household is also used as an indicator of the distribution of power within the household (Alkire 2007, Narayan et al. 2005). We will follow this approach and explore intra-household decisions, capturing women's power relations within the household and their access to the constituents of development (e.g. whether they are allowed to work).

Different channels for empowering women have been explored. Education is sometimes proposed as one of the main drivers of empowerment (Oyitso and Olomukoro 2012), but the evidence is mixed. There is substantial evidence that education can improve cognitive skills, raise aspirations, allow access to information, raise awareness to real conditions, and help coping with dis-equilibrium (Kabeer 2005, LeVine 2001). Educated women are also more intolerant and/or experience less domestic violence (Mocan and Cannonier 2012, Kabeer 2005, Sen 1999). On the other hand, Andrabi et al. (2012) do not find an effect of higher maternal education on intra-household decision-making.

Women are also empowered by accumulating wealth but there is little evidence that suggests ways to enhance this mechanism. Microfinance programs can facilitate the accumulation of economic assets but evidence for a causal effect of such programs on women's empowerment is mixed. Kabeer (2001, 2005) suggests that women's access to credit improves women's self-perception, reduces domestic violence and increases women's power in the household decision-making. In households where the loan recipient was male, the role of women in decision-making regarding loan use, enterprise management and allocation of profits was much lower than in households where the loan recipient was a female. However, Banerji et al. (2013) find no short- or long-run effects of micro-credit in India on women's empowerment.

Our paper complements studies on women's empowerment by examining an intervention based on group parenting sessions, which could empower mothers through the channels outlined in the

introduction: increased self-awareness of their value, support from a network of women, encouragement to invest in their own human capital and work, acquisition of new parenting skills.

Moreover, the intervention that we evaluate aims at improving mothers' parenting skills needed for enhancing children's development from an early age. Therefore, our study is the first to focus on long-term exposure to group parenting sessions and on its impact on both mothers and their children, contributing to the flourishing literature on early age interventions. Among others, we note the Abecedarian project, High Scope Perry Preschool Program, Chicago Child-Parent Centers and the Head Start Program. Evidence suggests that these programs led to improved schooling attainment and better outcomes in adulthood (higher employment rate and earnings, lower crime rates).[2] We note, however, that these interventions target the most-disadvantaged groups and that such programs may be unfeasible in most developing countries because they are expensive. Most related evidence in developing countries is often based on very short interventions and small samples (see Baker-Henningham and Lopez Boo 2010 and Nores and Barnett 2010 for a literature review). Few studies focus on longer treatment and long-term child outcomes. Exceptions are Watanabe et al. (2005) and Kagitcibasi et al. (2009), both providing evidence of positive effects on cognitive outcomes, while Kagitcibasi et al. (2009) find positive effects on other socio-economic outcomes.[3]

Another related study is Rosero and Oosterbeek (2011), which evaluates the effect of home visits and child care centers on mothers and children in Ecuador. Child care centers provide day care for the whole day throughout the entire year, and groups of eight to ten children are supervised by a trained teacher. Weekly home visits, each lasting about an hour, teach mothers how to stimulate and nourish their children, in individual sessions if children are younger than three, and in groups if children are older. Rosero and Oosterbeek (2011) find that child care centers increase mothers' labor force participation but have detrimental effects on children's cognitive and health outcomes, while they observe the opposite effects for home visits. The early childhood development program that we study is more similar to the home visits than to the child centers described above.[4] However, while Rosero and Oosterbeek (2011) can analyze short term treatment effects only[5], we are able to explore long-term

[2] For example, the pre-school Abecedarian project improved children's reading and mathematics achievements, lowered grade retention and increased completed education at adulthood (Campbell et al. 2002, Temple and Reynolds 2007). The High Scope Perry preschool program affected the schooling outcomes of girls only: by age 19 treated females had a higher school GPA and completed a higher grade (Heckman et al. 2013); by age 27 treated females were 30 percent less likely to drop-out from high-school (Nores et al. 2005).

[3] For example, children exposed to an early treatment entered the workforce later (due to longer schooling) and found jobs of a higher status as young adults.

[4] The main difference between the *PelCa* program and the home visits analyzed by Rosero and Oosterbeek (2011) is that in the latter mothers with their children are visited in their homes once a week, and individually if the child is younger than 3, while in the *PelCa* program mothers and children go to the NGO office twice a month and are taught in groups from the beginning. Moreover, the topics discussed in the *PelCa* program go beyond children's education and tackle women's empowerment directly.

[5] Data was collected after the children in the program were exposed to treatment during 21 months (Rosero and Oosterbeek 2011).

effects on the mother and children. Attanasio et al. (2014) also evaluate the effects of a weekly home visit program in Colombia that targeted children 12-24 months and lasted for 18 months. They find that enhancing mothers' engagement with their children positively affects their cognitive and socio-emotional domains through an increase in parental investments and find no effects on mothers' depression (Attanasio et al. 2014, 2015).

3. Background and Research Design

The school system in Ecuador---- The constitution and education acts provide the legal framework for the Ecuadorian school system. As of 2011, compulsory primary education targeted children aged 6. The education had a nominal duration of 6 years and was divided into three 2-year cycles. After completion of primary school, two 3-year cycles followed*: ciclo básico* and *bachillerato*.

In 2011, the *Ley Orgánica de Educación Intercultulral* made primary and secondary education compulsory from age 5 to 17, extending the age range for compulsory education by one year. *Educación general básica (EGB)* lasts 10 years and is aimed at children 5-14 years old. The *bachillerato general unificado (BGU)* has a nominal duration of 3 years and is intended for students aged 15-17 (Ministry of Education, 2013).

Children in the treatment and control groups under analysis entered school when 6 years old for the most part, with the only exception of children turning 5 in 2011 and 2012, who entered primary school under the new law.[6] All the children in the analysis are enrolled in the *educación general básica,* which we refer to as primary education for the rest of the paper. The school year differs depending on the region where the student attends school. The academic year in Quito, where the program takes place, operates under the Andean scheme and goes from September to June.

The NGO---- AVSI, the Association of Volunteers in International Service, is an international not-for-profit, non-governmental organization (NGO) based in Milan, Italy. Founded in 1972, it operates in 30 countries in Eastern Europe, Africa, Latin America, and the Middle East and it runs more than 100 long-term projects. It started its activities in Ecuador in 2001, focusing on infant and child development and education. In 2005 an AVSI branch was opened in Pisullí, a disadvantaged, urban neighborhood to the northwest of Quito. In collaboration with Fundación Sembrar, a local non-profit organization, and the local parish, AVSI funded a community development center where it implements a modified version of *PelCa*. The program expanded rapidly, providing after-school programs and other services to more than 700 children, youth and their families in 2013, with more than 50 members on the local staff.

[6] In a robustness check we perform the analysis excluding children who entered school at age 5, results are invariant.

3.1 The Intervention

PelCa is a preschool program targeted at parents of children age 5 and under, based on group-parenting sessions. The program mainly aims at building self-confidence and self-awareness of women, with a particular focus on how to translate this into the family context and in the relationship with spouse and child. Fortnightly meetings are held in the NGO for small groups (usually six-eight mothers – with their children), under the guidance of a family advisor. The meetings are 2 hours long: from 8:30am to 10:30am on either Tuesday, Wednesday or Thursday. In the first part of the meeting, children socialize with each other, playing games using didactic materials, while parents read and discuss material about personal growth and family education. Subjects discussed include taking care of your own self, the importance of education (women are encouraged not to drop out of school if they are still in school, or to go back and finish their studies if they have dropped out), reaching awareness of your own capabilities and possibilities, the importance of acting on them and take initiative (by going back to study, start working, etc.), the importance of savings, the importance of cultivating relationships in the local community (with other parents met in the sessions, but also with neighbors and the local community), among others. Parents are encouraged by educators to actively engage in the discussions, to share difficulties but also lessons learned, and improvements made between sessions.

In the second part of the meeting, parents and children work together: they learn songs, educational games and various development activities that parents can reproduce with their children at home (e.g., reading books, playing with puppets, playing building games, etc.). The family advisor gives every child a notebook of age-appropriate activities that focus on different areas of development, and parents and children are expected to undertake these activities in the two weeks between the program sessions. In the last part of the meeting, the family advisors monitor the progress made by each child and verify whether they have completed home assignments with the parent (e.g., by having children show drawings, or having children answer questions based on a story that was to be read to them by the parent). The family advisor gives each parent and child reinforcement activities to perform at home in the next two weeks. These activities are geared towards mothers and children achieving specific targets and goals.[7] These NGO-set goals are the basis for the outcomes that we evaluate in the paper.

Goals for the mother: increase self-confidence and self-awareness and improve the ability to relate to their environment and peer group; build a sense of value in oneself and in material assets.

At the personal level: awaken interest in life; assume responsibility through personal commitment; enhance self-perception of own abilities and the capacity to take initiative; and to give appropriate value to assets and saving.

At the level of relationships with the family and community: engage in dialogue, patience, and reflection;

[7] The goals are taken from the NGO handbook in Spanish.

strengthen the level of involvement of each member of the family in the light of existing dynamics; share between the couple the responsibility for the children's education; develop an attitude that favors the autonomy of children and teenagers; develop relationships of intimacy and solidarity with the group in the meetings, and with neighbors in the neighborhood.

Goals for the children: facilitate children's integral growth in different areas of development (psychometric, language, cognitive, socio-affective).

Families usually find out about the program through word of mouth or a poster placed outside the NGO building. If they express interest, AVSI employees visit the family to collect information about its circumstances, observe conditions at home, assess the need for support, and identify family weaknesses and strengths. Children up to three years old are eligible to enter the program (so that they can participate in the program for at least two years). The mother commits to participate in fortnightly meetings and to perform the assigned tasks at home. The selection process also takes into account the family's financial standing and the proximity of the home to the NGO sites where sessions are held. Parents and children can remain in the program until the child is 12 years old but once the child reaches five years of age they move to the NGO *PelCa* school program, which has the same format of the *PelCa* pre-school program, but targets school-aged children. The application process for the *PelCa* pre-school program starts at the end of April and lasts for two weeks. Approximately 50 families (the number can vary depending on funding for that year) are selected to start the program in September.

The home school program has expanded and it allowed an expansion of a network of family day care centers which enabled neighborhood mothers to care for children of mothers who work or go to school. However, *PelCa* and the nursery are two strictly separate programs. Working mothers can apply for family day care center for their child, but they are not allowed to participate simultaneously in *PelCa* and in family day care center.

3.2 Design: choosing a comparison group

As depicted in Figure 1, mothers can enroll in the *PelCa* preschool program with their children aged 0-3. Our treatment group is group A (blue oval), made up of mothers who enrolled in the *PelCa* program when children were 0-3 years old and whose children are now in primary school. In this study, we observe treated families when children enter primary school.

Mothers who do not enroll in the intervention are in group B (orange oval). Using B as a comparison group might lead however to a bias in treatment effect estimates since it includes mothers who did not self-select to the program, and therefore might differ in observable and unobservable characteristics from group A. Our control group is instead group C (white oval) which includes mothers who applied to the *PelCa* preschool program in 2012. In the summer of 2012, we mimicked the program's selection process, but on a larger scale. The NGO advertised the *PelCa* program in schools and through posters, as had been done previously in the program. However, it extended the application

period to approximately two months to reach as many families as possible, and as a result, it attracted a much larger pool of applicants compared to other years. We selected applicant families with a preschool-age child who had also, similar to our treated mothers, at least one child enrolled in primary school. The identifying assumption is that the sample of mothers with children of primary school age who did not participate before in *PelCa* but chose to do so now with a younger child (group C), represents a sound counterfactual for *PelCa* mothers and their primary-school-age children (group A).

The families in the control and treatment groups were invited to an interview in June-July 2012. The mother participated in a structured interview, while her primary school children were tested for cognitive and non-cognitive skills. The mother was asked to bring to the interview her children's vaccination certificate and birth certificate, which includes information on the child's birth height, weight and head circumference and the older child's school report cards for the previous and current years. We then selected from among the applicants, those who had an older child at a primary school age. We also held a follow-up interview with these control and treatment groups a year later in the summer of 2013.[8]

In addition, mothers from the control group were asked as to why they did not enroll in the program before. As a robustness check, we exploit this information and also re-estimate all models in Section 6 by limiting the control sample to those who did not participate in the program due to different reasons. We note that the capacity of the program did not increase, and most of the families that form our control group could not join the program.[9]

4. Data

We conducted face-to-face interviews with mothers and children, using a questionnaire we developed specifically for this study.[10] The questionnaire seeks information on family members (mother, partner, and children), demographic characteristics, labor-market activities (type of job, full-time/part-time, formal/informal sector, wage, etc.), intra-household decision-making, and parents' inputs into child rearing. All questions related to the current time or prior to enrollment in the program.[11] The interview lasted approximately 45 minutes. The mother then took the Big Five Personality Test[12]

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[8] We collected additional data in 2013 for two reasons. First, to check whether effects continued in a subsequent period. Second, to increase the number of observations and hence precision of estimated effects.

[9] In 2012, when there was an over subscription to the program at baseline, mothers were selected based on the age of the children: families with the youngest children were selected in order to permit a longer treatment period. This criterion that was not applied previously.

[10] We piloted the survey questionnaire in January 2012, interviewing 23 treated mothers: 12 of them had a primary-school-age child who participated in the *PelCa* preschool program and 11 of them had a primary-school-age child who did not participate in the *PelCa* program. We revised the questionnaire following this pilot test.

[11] We will provide details about the questionnaire upon request.

[12] The Big Five Personality Test is based on decades of research. It consistently evaluates five broad traits of personality through a series of questions: openness to experience, conscientiousness, extraversion, agreeableness, and neuroticism. In 1981 these factors became known as the "Big Five" to indicate the broad dimensions to which

and the Rosenberg self-esteem scale.[13]

Each child took cognitive tests in Spanish and mathematics.[14] Information on weight, height and head circumference at the birth of the school-age children were collected from the vaccination and birth certificates though for some children, this information was incomplete or not available.

The 2012 control group included 164 children and 115 mothers, while the treatment group included 219 children and 166 mothers, a total of 383 children and 281 mothers. We interviewed some grandmothers who participated in the program on behalf of the mothers, but we excluded these families from the analysis because we do not have grandmothers in the comparison group.

In summer 2013, we conducted follow-up interviews. Ten female interviewers from the area conducted home visits with all of the mothers in the sample. To obtain comparable information in the two rounds of data collection we used the same questionnaire, but with slight modifications. Some questions were added in order to clarify issues we encountered in the 2012 data. However, where we introduced new questions, we also sought retrospective information. Mothers were asked to bring the vaccination and birth certificates again (because for some families these documents were missing in the previous year). Eventually, we collected data on height at birth for 44 percent of the children, weight at birth for 41 percent, and head circumference at birth for 38 percent. We think that the extent of missing values for these variables preclude a meaningful analysis of these variables. Mothers were also asked to bring children's school report cards for 2010-11 and 2011-12. The children were tested again in Spanish and mathematics, using tests appropriate for the student's school grade. The follow-up sample included 136 control children (83 percent) and 98 control mothers (85 percent), and from the treatment group, 197 children (90 percent) and 150 mothers (90 percent).

4.1 Treatment-Control Comparisons: Balancing Tests

We examine in this section whether pre-treatment covariates are balanced between treatment and control groups. The evidence suggests that mothers and children in both groups are very similar on observed and predetermined characteristics, supporting our view of the empirical setup as a quasi-natural experiment. The first two columns of Tables 1-2 display the means for the treatment and control groups, while the last two columns present the difference in means between the two groups and its standard error. The tables contain the full list of all the pre-program characteristics we collected.[15]

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they refer. It has since been used intensely.

[13] The Rosenberg test, developed in 1965 by Dr. Morris Rosenberg, includes 10 Likert-type questions and it is used to evaluate the self-esteem of an individual and is widely used today by psychologists, sociologists, and social scientists. It has been translated into various languages (e.g., French, Norwegian, Spanish, Portuguese, Chinese, and Italian).

[14] Grade specific language and mathematics tests were constructed based on national tests from Ecuador, known as "pruebeas SER (Sistema de Evaluación y Rendición de la Educación)" and on Peruvian national tests for grades for which Ecuadorian tests were not available.

[15] Pre-program characteristics were collected in 2012 asking retrospective questions.

Mothers' characteristics (Table 1) are balanced in most of the dimensions, except that control mothers were more likely to be employed before joining the program: 47.0 percent of treated mothers were working versus 60.9 percent of control mothers. Control mothers were also more likely to be working full-time. However, among working mothers, the self-employment rate and the rate of mothers working in the formal sector are not significantly different, and the reasons for not working among non-working mothers are not significantly different either. Moreover, mothers' characteristics are well balanced in important dimensions, including age, education, and power in intra-household decision making. There is a very small and non-significant difference in the primary school completion rate, which is the most common highest level achieved (39.2 percent of treated mothers completed primary school, and 37.4 control mothers did); the rate of mothers taking joint decisions with the spouse on different topics are also not significantly different by group. Pre-treatment paternal characteristics and most of the household characteristics are well balanced,[16] with a few exceptions: whether the family owned a house, the number of rooms and the availability of drinkable water in the house. Overall, 3 of the 32 pre-treatment maternal characteristics differences are significantly different at 10 percent level of significance. This is less than 10 percent of all characteristics and pre-program outcomes. The F-test on the bottom of Table 1 shows that all of the pre-treatment maternal characteristics[17] are not significantly different between groups, suggesting that the imbalance in the pre-program employment status of the mothers is an exception. We will however include in the regressions pre-treatment controls to capture these differences between the treatment and control groups. It will be shown that the estimates are not sensitive to adding these controls.

With respect to the child characteristics (Table 2), control group children are half a year older on average. This is probably due to the fact that we selected children from 1st to 7th grade and that, as we will see, control children are more likely to repeat a grade and therefore be older at the survey date. The F-test on the significance of all the characteristics together suggests that overall children's characteristics are not linearly correlated with treatment status.

4.2 Entropy Balancing

An alternative way to control for the differences in some of the pre-treatment characteristics is to use entropy balancing (Hainmueller 2011). Entropy balancing is a data-preprocessing method to achieve covariate balance. It computes the means (or higher moments of covariate distributions) of the covariates in the treatment group and looks for a set of entropy reweights so that the means in the reweighted control group match the means in the treatment group.[18] We implement entropy balancing

[16] Details are provided in the Table A1 and A2 in the Appendix.
[17] Pre-treatment characteristics related to intra-household decisions before treatment are included.
[18] Entropy balancing creates balanced samples for observational studies with binary treatments. It generates the

for the means of the covariates that we will include as control variables in our analysis (child and household pre-treatment economic characteristics).[19] Entropy balancing makes the treatment-control covariate balance almost perfect: differences in means are not significantly different from zero for all covariates (Table A3).[20] This approach is preferred over a propensity score matching because the former eliminates all treatment-control imbalances. In addition, the propensity score matching requires treated and controlled units to be comparable within the common support. As a consequence, the individuals who do not lie in the common support (5 out of 281 mothers in 2012 and 22 out of 496 mothers in the pooled data) are dropped from the sample.

4.3 Matching

To check robustness, we also re-estimate the effects of the program through various matching methods. Three popular matching methods are adopted: the bias-corrected nearest neighbor matching via the Mahalanobis distance metric by Abadie and Imbens (2006, 2011), and kernel matching and the nearest neighbor matching via the estimated logit propensity score (Rosenbaum and Rubin 1983, Becker and Ichino 2002). We report Abadie and Imbens' (2006, 2011) robust standard errors for their suggested bias-corrected matching method. For the propensity score matching, 50 bootstrap replications are performed to compute the standard errors. When estimating the effect using the matching method, the summary index estimator is given by $\widehat{SI} = \frac{1}{K}\sum_{k=1}^{K} \hat{\beta}_k / \sigma_{k,c}$ where $k = 1, \dots, K$ indices K outcomes, $\hat{\beta}_k$ is the estimated average treatment effect on outcome k using the matching approach, and $\sigma_{k,c}$ is the control group standard deviation of the k^{th} outcome. Taking the variance operator, we obtain the variance estimator of the summary index by $\widehat{Var(SI)} = \frac{1}{K^2}\sum_{k=1}^{K} Var\left(\hat{\beta}_k\right) / \sigma_{k,c}^2$.

In addition to performing matching using the full sample, we also implement a tighter matching where we only consider treated and control mothers who have both preschool and school children, to address concerns about treated mothers having young children at home and therefore being more likely to work. First, we keep only the sample of mothers who have both children in preschool and children in primary school. Second, based on this sample, we match mothers on their age as well as on the ages of their children. Third, we match these mothers not only on mother's and children's age, but also on their employment status (whether they worked full time).

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weights for the dataset by minimizing a proposed entropy distance metric subject to a set of specified moment conditions such that the reweighted sample is strictly balanced. For the detailed discussion of theoretical properties and numerical implementation, please refer to Hainmueller (2011). A more recent paper by Zhao and Percival (2017) also discussed the double robustness and the semiparametric efficiency property of the average treatment effect estimator reweighted via the entropy balancing scheme. The Stata package ebalance developed by Hainmueller and Xu (2013) implements the entropy balancing.

[19] We also include an indicator of whether mothers were working full-time before treatment.

[20] We also obtain balanced samples through entropy balancing when we consider the children's samples or when we pool the two years of data together.

5. Empirical Strategy and Results

In this paper, we employ a quasi-experiment setting that mimics a randomized trial. Let us consider a random sample of n individuals are drawn from a population of N individuals. Each individual i is exposed to a binary treatment $Treatment_i$. $Treatment_i = 1$ if unit i receives treatment and 0 otherwise. X is a matrix of exogenous pre-treatment characteristics. $Y_i\,(Treatment_i)$ are the potential outcomes that individual i attains if it receives treatment or not. Observed outcomes for each i are:

$$Y_i = Y_i(1)\ Treatment_i + (1 - Treatment_i)\ Y_i(0)$$

That is, we never observe both potential outcomes for the same individual. The treatment effect is defined as $\tau_i = Y_i(1) - Y_i(0)$ and the population average treatment effect on the treated is given by $\text{PATT} = \text{E}[Y(1) - Y(0)|Treatment = 1] = \text{E}[Y(1)|Treatment = 1] - \text{E}[Y(0)|Treatment = 1]$. The second expectation is unobserved and in experimental studies, where treatment is by design independent of the potential outcomes, $Y(1), Y(0) \perp Treatment$, $\text{E}[Y(0)|Treatment = 0]$ is used as our estimate of $\text{E}[Y(0)|Treatment = 1]$. That is, in a true randomization, these two quantities are the same: there is no selection bias and simply taking the difference in outcomes between the treated and the controlled captures the treatment effect. Although the randomized experiment is the golden rule, in the absence of a real experiment, we adopted a quasi-experiment setting that mimics a randomized, as detailed in Section 3.2. The conventional solution in a quasi-experimental setting to the difference between the two terms is to assume ignorable treatment assignment and overlap (Rosenbaum and Rubin, 1983), that is, assume that $Y(1), Y(0) \perp Treatment|X$, and that $\Pr(Treatment = 1|X = x) < 1$ for all x in the support. In sum, conditional on all confounding factors X, the potential outcomes are stochastically independent of *Treatment* and PATT can be estimated.

Hence, controlling for covariates is important in this setting, and covariates need to be balanced. Matching and entropy balancing will help us reduce imbalances in the covariate distributions to decrease the error in the estimation of the treatment effect.

We estimate the effect of participating in the program on the outcomes of interest using the following regression model:

$$\begin{aligned} y_{it} &= \beta_0 + \beta_1 Treatment_i + \beta_2 ChildCharacteristics_i + \beta_3 HouseholdDemographics_i \\ &+ \beta_4 HouseholdEconomics_i + \beta_5 TimeFE_t + \beta_6 SchoolFE_i \\ &+ \varepsilon_{it} \end{aligned} \quad (1)$$

where i is the individual and t is time. y_{it} is a vector of the maternal and the child's outcomes.

Since we face a multiple outcomes problem, we will also compute summary indices[21,22] for domains of outcomes. $Treatment_i$ is a dummy equal to 1 when mother and child participate in the *PelCa* program in 2005-2012 and 0 otherwise.[23] To shed light on heterogeneous treatment effects by the number of years of participation in the program, we will also use the specification outlined above with a linear effect of the number of years a mother/child participated in the program. Exposure to the program varies from two years and four months to seven years and eleven months.

$ChildCharacteristics_i$ includes year of birth, birth order, number of siblings as of 2005, i.e. before the program started, and gender. $HouseholdDemographics_i$ are pre-treatment household demographic characteristics: mother's and father's age, their civil status (married, lived together, mother was divorced, separated, widow, single) at the time of the birth of the first child, and the parents' level of education before the birth of the first child, a dummy equal to 1 if the mother was born in Quito, a dummy equal to 1 if the parents came from the same city, and the number of children the mother had in 2005. $HouseholdEconomics_i$ are pre-treatment household economic characteristics: whether the mother worked before treatment, whether the father worked, the mean firm size of mother's and father's employer, average monthly family income before treatment. $TimeFE_t$ is a dummy equal to 1 when the observation corresponds to 2013, 0 if 2012; ε_{it} is the error term, clustered at the mother level when we run regressions pooling the observations in the two years together or when we analyze outcomes for children. $SchoolFE_i$ are school fixed effects.[24] They are included when we analyze outcomes for children. A more detailed description of the control variables is provided in the appendix.

5.1 Results based on Summary Indices

As we note in Section 3.1, the primary purposes of the *PelCa* program are to empower mothers, harmonize intra-family relations, and increase investment in education in early childhood. The breadth

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[21] We follow Kling et al. (2007) to construct each summary index as an "equally weighted average of z-scores of its components, with the sign of each measure oriented [...] so that more beneficial outcomes have higher scores. The z-scores are calculated by subtracting the control group mean and dividing by the control group standard deviation."

[22] We developed a Stata package "mseffect" to calculate the mean effect size on the summary index with the advantage that we account for different weights, reversibility of outcome sign, and different types of robust standard errors. The Stata package "mseffect" is issued as a part of the online appendix to this paper, which is available at the Statistical Software Components Archive: https://ideas.repec.org/c/boc/bocode/s458290.html

[23] Not all the control mothers will become part of the program at the end of 2012, since we allowed for oversubscription of mothers into the program without the NGO increasing its admission capacity. The control mothers who will enter the program at the end of 2012 are still assigned a treatment dummy equal to zero, to indicate that they are part of the control group despite one year of participation in the program. If anything, this would bias downwards the estimates. Therefore, the causal effect we are evaluating is the lower bound of the causal effect of receiving treatment for mothers. Given the proportion of control mothers who become treated is extremely small and that they are treated for one year only and given that those mothers are only observed in 2013 when treated, we are inclined to believe that the lower bound we estimate is very close to the true causal treatment effect.

[24] 55 are the number of schools that children attend in 2012.

of the goals implies that the consequences of the program can be measured in multiple domains. We decide to measure the following domains, each of which contains multiple outcomes for mothers and children. For mothers, the domains (and specific outcomes) are: labor-market outcomes (whether working, working full-time, working with a contract and average family monthly income), mothers' economic and social independence (whether she has control over her own money, participates in voluntary activities, is currently studying, and whether she has a role in her own employment decisions), mothers' intra-household decision-making (role in decisions regarding child's education, health, and discipline, expenditures in general and on food, having children, use of contraceptives, own health), mothers' child investment (own time inputs with child, aspirations/expectations for child's education), mothers' self-esteem (Rosenberg scale) and Big Five personality traits (agreeableness, conscientiousness, extraversion, neuroticism and openness to experience). Summary statistics on the domains indices and individual outcomes are presented in Table 3, in panel A for mother's labor market outcomes, in panel B for mother's economic and social independence outcomes, in panel C for mother's role in intra-household decision making, in panel D for mother's child's investment, in panel E for mother's non-cognitive skills and fertility choices, and in panel F for the father's labor market outcomes.[25]

For children, the domains (and outcomes) are: test scores (language and mathematics test score), school dropout and grade repetition, non-cognitive I: attitudes towards schooling (whether child likes school[26] from both their own and their mothers' perspectives), and non-cognitive II: absences[27] and behavior in school.

Before presenting the detailed estimates of the effects on each specific outcome, we analyze each domain by creating domain-specific summary indices. This allows us to control for the potential problem of over-rejection of the null hypothesis due to multiple inferences. Because different outcomes have different data scales, simply averaging the estimators for the treatment effect is not likely to produce a meaningful statistic. To address this concern, we follow the summary-index approach as in Kling et al. (2007). The summary index of multiple outcomes is the average of z-scores of each outcome variable. Z-scores are calculated by subtracting the control mean from the outcome and dividing by the

[25] The summary indices are based on the exhaustive list of outcomes we collected through the interviews, none of the outcomes have been left out of the analysis.

[26] The question being asked is: "Do you like to go to school?". The answer is captured by the categorical variable "Likes school", which takes values: "No", "A bit", "Quite", and "Very much".

[27] Note that we reverse the signs of the absence variable since the school absence is a negative measure of the non-cognitive skill.

control standard deviation.[28] This summary index is a special case of the z-score[29] and it is identical to the mean effect size of treatment if there is no missing value.[30] In general, the sign of the summary index reveals information on the direction of the aggregate impact of a class of outcomes, and the more the summary index deviates from zero, the stronger is the implied aggregate effect.

Estimated effects on summary indices of mothers based on the 2012 data (panel A) and on the pooled 2012-13 data (panel B) are presented in Table 4. We report estimates from three different specifications, with only the treatment indicator as a covariate (column 1), with child and household demographic characteristics as controls (column 2) and with the addition of household economic characteristics as additional controls. Based on the fully specified regression (column 3), we conclude that the program enhanced mother's participation in the labor market; the treatment effect on the corresponding summary index based on the 2012 data is 0.482 (se=0.098) and based on the pooled 2012-13 data it is 0.503 (se=0.098). These positive estimates are measured precisely and are practically unchanged after controlling for child and household demographics (column 2) and for economic covariates (column 3) as well. The entropy balancing estimates in columns 4-6 are very similar to the unweighted estimates presented in columns 1-3.

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[28] Consider we have K outcome variables, each of them is denoted by the vector Y_k for $k = 1, \dots, K$. For outcome variables Y_k, we build its z-score Z_k by subtracting the control group mean μ_{k_C} and dividing by the control group standard deviation σ_{k_C}. Stacking all $Z_k, k = 1, \dots, K$ vertically into a single vector creates the summary index denoted by $= (Z_1', Z_2', \dots, Z_K')'$. By doing this, the mean of the index of the treatment group is reflected in columns (1) and (5), and the control group mean has been normalized to zero as displayed in columns (2) and (6) of Table 3. Let T be the treatment indicator(s), and X be the matrix consist of controlled variables and the constant. The average treatment effect on the index τ_1, and its standard error can be acquired through a linear regression:

$$\mathbf{Z} = \mathbf{T}\tau_1 + \mathbf{X}b + \boldsymbol{\epsilon}$$

where $\mathbf{T}$ is the treatment indicator stacking treatment dummies K times, and X is vertically stacked of X. The estimates in columns (4) and (8) for the summary index reflects $\hat{\tau}_1$ with no covariates adjustment.

[29] Here we replace the minuend and the divisor in the z-score by the control group mean and standard deviation respectively. In other words, we do require some dispersion in the controlled outcomes to guarantee the validity of standardization.

[30] In the regression specification this approach yields standardized estimators as follows: the treatment effects for K outcomes are aggregated and reflected in a single standard normal statistic:

$$\tau_2 = \frac{1}{K}\sum_k \frac{\beta_k}{\sigma_{k_C}}, \qquad k = 1, \dots, K$$

where β_k indicates the average treatment effect for outcome k and σ_{k_C} denotes the standard deviation of the k^{th} control outcome. Note that the point estimator τ_1 described in footnote 16 is identical to τ_2 in the absence of missing value. Having included the covariates, the K average treatment effects (β_k) and its variances can be easily acquired through a linear regression. By doing so, the above equation can be thought of as a point estimator representing a collection of standardized treatment effects. Moreover, this paper also takes account of the covariance of effects and therefore adapt a seemingly uncorrelated regression (O'Brien 1984, Kling et al. 2007):

$$\mathbf{Y} = I_K \otimes (T \quad X)\boldsymbol{\beta} + \boldsymbol{\upsilon}$$

where $\mathbf{Y} = (Y_1', Y_2', \dots, Y_K')'$, $\boldsymbol{\beta} = (\beta_1', \beta_2', \dots, \beta_K')'$, T is the treatment indicator(s), and X consists of controlled regressors as well as a constant term.

The corresponding treatment estimates on mothers' economic and social independence are also positive and large, 0.366 (se=0.076) when using the 2012 data and 0.276 (se=0.056) when using the pooled 2012-2013 data. A similar positive treatment effect is evident for the household decision-making outcomes, albeit the estimated coefficient for this summary index is smaller, 0.126 based on the 2012 data and 0.093 based on the 2012-13 pooled sample. The program encouraged an increase in parental investment in early child development. The estimated effect on the overall index capturing mothers' investment is positive and significant, 0.207 (se=0.065). This positive effect is seen in improvements in several measures of children's outcomes (test scores, drop-out rates, repetition). It is possible that the better education outcomes for children were the result of other improvements generated by the program, such as the increase in family income, and the direct training in cognitive and non-cognitive skills that the children received in the biweekly meetings with the family adviser. We will discuss this further when we present evidence of the effect of the *PelCa* program on children.

We note again that the estimates for these summary indices are robust to the inclusion of control variables and also to a re-weighting with entropy balancing. Taken together, these results suggest that we can gain further insights by examining in detail the effect on the mothers' outcomes. However, we do not find a strong average treatment effect on mothers' self-esteem and on personality traits (openness to experience, conscientiousness, extraversion, agreeableness, neuroticism): the estimated effect on the summary index of these aspects is small (0.045) and not precisely measured[31]. This contrasts with Kabeer (2001), who finds that access to credit improves women's self-esteem. However, our findings do not necessarily imply the absence of a treatment effect on self-esteem or personality traits, as it could also be that the instruments used to measure these outcomes were not the most appropriate.

Heterogeneous Treatment Effects

Development interventions usually seek to target the most vulnerable, which in this setting are less empowered mothers. Hence, it is key to understand whether the program successfully empowers this group of women. We identify the most vulnerable mothers by looking at pre-treatment characteristics such as education, employment status, and power in intra-household decision making, which we consider proxies for empowerment.

Lower educated mothers are generally less empowered, and they naturally constitute a targeted population for such interventions. Moreover, parental education has been found in many studies to be an important factor that can positively affect children's educational outcomes. It is harder to improve one's level of education later in life, but this program could be a good substitute for the education of mothers at this stage. Status of employment and power in intra-household decision making are also

[31] When we evaluate the effects on a summary index for fertility choices separately we find no effects.

related to mothers' empowering status before the program.

Therefore, we allow heterogeneous treatment effects by mother's working condition, education, and role in intra-household decisions. Table 5 presents the aggregate-estimated effect on the summary indices with control for all covariates and for subsamples of mothers based on the 2012 data. We first examine the subsamples of mothers by pre-treatment working conditions: this evidence is presented in columns 1-2 of Table 5. Among the group of treated mothers, 148 reported that they were working when their interviewed child was born, and 133 reported they were not working at that time. Our estimates indicate that the program had a larger effect on labor market outcomes of mothers who were not working before joining *PelCa*, 0.794 (se=0.191) versus 0.340 (se=0.126), the F-test strongly rejecting that they are equal (at 5 percent significance level).

The same pattern emerges when dividing the sample by mother's role in intra-household decisions. Although the F-statistic is less significant, the estimated effect for mothers who were not working before *PelCa* is greater, 0.213 versus 0.086, respectively. The program has the same effect on the two groups' summary measure of economic and social independence outcomes (0.351 and 0.484, respectively). Interestingly, the average effect on child investment comes mainly from the effect among mothers who worked at baseline (0.373) with practically no effect among mothers who did not work before enrolling in the program. The mothers with a higher pre-program employment rate might have had a more enriched upbringing, which complemented the knowledge about child rearing acquired during the program.

In columns 3-4, we present estimates by sub-samples stratified by mother's pre-program educational attainment (up to/more than primary school education). These are almost equal samples, 144 and 136, respectively. The treatment effect on the labor market index for the higher education mothers is 0.611 (se=0.163), versus 0.462 (se=0.130) in the lower education sample, both estimates being statistically significant. A similar heterogeneous pattern is seen in the effect on mother's role in family decision-making, larger for the more educated mothers, 0.205 (se=0.087) versus 0.098 (se=0.076), though the difference is not statistically significant. We find a similar effect size on the summary index of all mothers' economic and social independence outcomes for both groups (0.420 vs. 0.386). The effect on child investment is strikingly different however for the two groups, 0.320 (se=0.105) for less-educated mothers and 0.070 (se=0.100), for more educated mothers, and the difference is statistically significant. One possible explanation is that mothers with lower education levels are less skilled in child rearing and hence have higher marginal benefits from their participation in the *PelCa* program.

Next, we explored heterogeneous treatment effects by mothers' pre-treatment role in decision-making. We expect that mothers who initially (before 2005) were less involved in family decision-

making would benefit more from the program.[32] The estimates in Table 5, columns 5-6, suggest that this group had larger gains in the labor market and in household decision-making. With respect to the latter effect, the difference is striking, 0.197 (se=0.073) versus -0.017 (se=0.083).

We next examine heterogeneity in the treatment effects by child gender. The results are presented in Table 6. Based on 2012 data, the treatment effect on labor market outcomes does not vary by the child gender, 0.534 for mothers of boys vs. 0.500 for mothers of girls. The effect on child investment is larger for boys (0.268 vs. 0.193) but the difference is not statistically significant. Interestingly, mothers of boys have also a larger and significant treatment effect on the score in the Rosenberg self-esteem scale test and the Big Five Personality Traits test (0.177 vs. -0.021). However, mothers of girls have larger treatment effects on intra-household decision-making (0.161, significant at 5 percent level, versus 0.079 and not significant) and on economic and social independence outcomes (0.471, significant at 1 percent level, versus 0.323, significant at 1 percent level).

In the rest of the paper, we will study which specific mother's and child's outcomes drive the response of our aggregate measures. First, we will present and discuss estimates based on the full sample, with and without the pre-treatment covariates. Second, we will check if our results hold when the sample is reweighted through entropy balancing. Finally, we will perform other robustness checks.

5.2 Estimated Effects on Mothers' Specific Outcomes

Labor-Market Outcomes

The estimates based on the 2012 data are presented in panel A of Table 7 and those based on the pooled 2012-13 data are presented in panel A of Table 8. Labor-market outcomes reflect empowerment and emancipation of women. As we pointed out in the previous section, the effect on the summary index in Table 4 suggests an overall significant improvement in mothers' employability and family income, and the evidence in Tables 7-8 strengthens this conclusion. Treated mothers are 17.6 percent more likely to be working, 20.7 percent more likely to be working full-time and 20.4 percent more likely to be working in the formal sector. These estimated effects relative to the untreated mothers are an increase of 36 percent, 130 percent and 230 percent, respectively, indicating a large increase in mothers' employability.[33] Results based on the pooled 2012-13 data are very similar. All these estimated coefficients are significantly different from zero at the 1 percent significance level, and they are not affected by adding any of the control variables in the regression. Moreover, we find that treated mothers have more stable employment: 69 percent of the working mothers from the program were

[32] We made use of pre-treatment variables of mothers' intra-household decisions. After calculating the number of total household decisions that mothers made in 2005, we divide the full sample into two by the mean of total decisions. In the 2012 sample of mothers there are 179 and 93 mothers who made more decisions and fewer decisions respectively.

[33] The types of jobs that treated mothers hold are typically low-skilled jobs: mainly domestic cleaners, but also seamstresses and shopkeepers. More details on the job categories are available upon request.

working in both 2012 and 2013, whereas only 49 percent of the working mothers in the control group were working in both years.

Another important result in Table 7 shows that the 2012 average monthly income of treated families is $44.48 higher than the families in the control group – a finding that we attribute largely to increased wages for mothers. The median wage of workers in the neighborhood is the minimum wage ($292 per month in 2012) and, therefore, the income gain from the program is large. In 2013 we collected the data on mothers' wages. Using this information, we estimate that family income is up mainly because of an increase in mothers' wages: treated mothers earn $13.33 (se=4.87) more per week than control mothers, i.e. more than $57 extra per month (Table 8, panel A), while fathers' wages are not significantly different in the two groups. Our result of a significant impact on mothers' wages without any similar effect on the spouse is in contrast to the findings reported in Rosero and Oosterbeek (2011) who study the effect of child care centers on labor market outcomes of mothers. They report an estimated increase in the likelihood that a mother is working by 22 percent[34] and of household income by $80[35], but unlike in the *PelCa* program the latter is not driven by a rise in mothers' earnings, but by the partners' income. Actually, Rosero and Oosterbeek (2011) even find that home visits reduce the proportion of mothers working by 17 percent, while leaving mothers' income unaffected; however, we note that our study is based on a longer term follow-up and that the long term effect of the home visits program might be different than the short term effect reported in Rosero and Oosterbeek (2011).

Economic and Social Independence

As seen in Table 4, the effect on mothers' independence from an economic and social perspective is very large. The evidence presented in panel B of Tables 7 and 8 shows that treated mothers in 2012 are 21.3 percent (se=0.056) more likely to manage their own money. Relative to the control group mean (44.7 percent), this is a 47.7 percent increase. Again, these effects are unchanged when we add to the regression each set of control variables and when we expand the sample to include also the 2013 data.

Another sign of the program's effect on women's empowerment is a larger proportion (8.1 percent more, se=0.036) of treated mothers who are studying at the survey date, which is about 155 percent higher than the rate of mothers in the control group. This estimate is significant at the 5 percent level and is unchanged even after adding all the control variables. When using the pooled 2012-13 data, the treatment effect on this outcome is 8.5 percent (se=0.031). A concern may be that entering the job market may lower the incentives and time for studying, and mothers who participated in the program

[34] The child care centres' effect on mothers' employability is stronger than the *PelCa* program effect (22 percent vs. 16.4 percent), but it goes in the same direction.

[35] Please note that household income in Rosero and Oosterbeek (2011) is measured in 2007, while in the current analysis it is measured in 2013, making the two incomes not perfectly comparable.

were more likely to be working. To address this issue, we check whether mothers who quit studying in 2013 also found a job in 2013; we find little evidence of such correlation.

More evidence of the program's effect on women's empowerment is based on the program effect on mothers' role in the decision whether she can work. In the 2012 survey, we asked the mother who decides whether she can work. Treated mothers were 13.2 percent (se=0.044) more likely to report that this decision is taken by themselves or jointly with their partner. The estimated impact relative to the control group is 16.4 percentage points higher. This holds true when we pool data from the 2012 and 2013 surveys: treated mothers are 10.4 percent (se=0.031) more likely to have a role in this decision. Both estimates are statistically significant at the 1 percent level. Here as well, adding controls and using a weighted regression does not move the point estimates at all.

Household Decision-Making

Treated mothers are more likely to make decisions alone or with their partners about issues related to children's education (9.9 percent effect, significant at the 1 percent level) and on children's discipline (8.7 percent effect, significant at the 5 percent level), which are 11.6 and 10.4 percent greater, respectively, than the outcome means of control mothers. These estimated effects remain unchanged when controls are added. In the 2012 and 2013 pooled data, we estimated similar effects. However, we find no effect on the other domains (intra-household decisions on spending, on having children, on contraceptives and on what to do when children are ill). In the follow-up survey in 2013, we also collected intra-household decision outcomes on who decides on issues related to mothers' health, on purchasing important items and on whether mothers can visit friends and relatives. Effects on these intra-household decisions are non-conclusive. When stratifying the sample by the length of participation in the program (Tables A10-A11), we find that mothers who have been in the program for a longer period of time are more likely to be involved in household decision-making.

Together with the results based on the single-treatment dummy, it seems safe to conclude that mothers participating in the *PelCa* program assume greater intra-household responsibilities and participate more fully in intra-household decision-making.

Access to credit was also found to have a positive effect on women's power in household decision-making, for decisions related to the loan (Kabeer 2001, 2005). The effect of access to credit seems higher than what we find here. However, this might be due to the lower initial power of women who received the loan in the analysis by Kabeer (2001, 2005), where 20 percent of women in the comparison group had some sort of role in decision-making, compared to the women in our study, where even before the treatment, 70/80 percent of women had some power within the household.

Mothers' Child Investments

The program is intended to improve children's outcomes by enhancing investments in children's cognitive and non-cognitive skills. We therefore asked mothers in the 2012 survey how much

time per week they spend interacting with their children in a variety of activities. Table 11 presents evidence on forms of child investment that we aggregated in the overall respective index. The *PelCa* program enhanced three of the four types of maternal investment that we considered. The estimates show that treated mothers are more likely to have conversations with children (4 percent effect, significant at 5 percent level) and also more likely to listen to and talk to their children about the books or other material they read (10 percent effect, significant at 5 percent). Treated mothers also invest more time in playing educational leisure games (for example dancing) with children (6.6 percent effect, se=0.025). We find no similar effects on mothers going to the library regularly with children (2 percent effect, se=0.040).

Taken together, the results suggest that participation in the *PelCa* program has increased mothers' investment in their children, in the form of increased mother-child educational interaction and recreational activities. In the next section we will report positive effects of the *PelCa* program on children's schooling and non-cognitive outcomes and we view the above evidence on maternal investment as indication of a possible mechanism through which the program might have improved child development. These reduced form effects are similar to findings about the effect of a home visiting program in Colombia that increased varieties of play materials and play activities in the home and also improved children's cognitive, language and socio-emotional development (Attanasio et al. 2014). Using a structural model, Attanasio et al. (2015) provide evidence that directly links these parental investments in children to the children's improved cognitive outcomes.

Treatment Effect by Number of Years in the Program

We estimated the treatment effect where exposure is measured by number of years in the program. This specification imposes a linear effect of years in the program. We prefer this specification over estimating separate regressions using stratified samples by number of years in the program because of sample size considerations. The number of years that mothers in our sample participated in the program ranges from two to eight and the mean is 5.5 years (Figure A1 of online appendix). We note that when using a linear effect specification of years in the program as a treatment measure, the summary index cannot be calculated because the treatment variable is no longer binary. In Tables A13 and A14 we report the linear effect estimate for each of the mothers' outcomes. Most of the estimates are positive and significantly different from zero. The pattern of positive or no effects in these two tables is consistent with the estimates we present in earlier tables. For example, the intensity of treatment measured as number of years in the program has no effect on mothers' role in decision making regarding food expenditure, having children and use of contraceptives (Table A13), which is exactly in line with the evidence presented in Tables 8-9, where we use a simple indicator of program participation as a measure of treatment.

Next, we re-estimated the effect on mothers' and children's outcomes by two subsamples --

whether the treated enrolled in the program before 2007 or from 2007. Enrollment in early years is positively correlated with the length of time mothers were in the program, and therefore it is an alternative approach to assess the effect of treatment intensity. The resulting estimates are reported in panels A of Tables A15-A16. Overall, the estimated effects on labor market outcomes, mothers' economic and social independence and mothers' care of children are larger for early participants than participants who joined later. For example, the effect on probability of making intra-household decisions among mothers who joined the program early is 0.195, while for later participants (from 2007) this effect is small and non-significant. The results based on the pooled sample of 2012 and 2013 present a similar pattern. The results presented in Tables A15-A16 suggest that the effect on children's overall summary indices for mothers who started the program before 2007 is positive and significant (0.138, se=0.063), whereas it is smaller and not significant for mothers who enrolled in the program later (0.109, se=0.094). However, we cannot reject the possibility that the two estimates are not statistically different from each other.

5.3 Effect on Children's Outcomes

The estimated effect of the program on children's outcomes is presented in Table 12. In the first row we present the program impact on the summary index of children's outcomes and in rows 2-4 we present the estimates on the specific outcomes that make this index.[36] The effect on the summary index is based on fewer observations than the sample size of each of the detailed outcomes because there are missing values for some of these outcomes, mostly for test scores, because not all the children were tested in both subjects. The estimated effect on the summary index of children's outcomes is positive and significant, 0.211 (se=0.063), suggesting that children are positively affected by participating in the *PelCa* program. We note again that this estimate is not sensitive at all to adding child, household demographics and household economics controls, nor to the entropy balancing.

The estimated effect on the average of the test scores in Spanish language and mathematics is 0.115 (se=0.103) when including all the controls. This implies that treated children have marginally higher school performance. This effect becomes clearer when we divide the children's sample by gender. Table 13 reports the heterogeneous effects on children's outcomes by gender and mothers' education. In columns 1 and 2, the estimated summary indices are based on 191 girls and 192 boys, separately. The *PelCa* program increased girls' average test score by 0.378 (se=0.144) and boys' by 0.012 (se=0.147). The effect on girls is larger and precisely measured and the difference between the two

[36] The effect on the summary index is estimated through a seemingly uncorrelated regression which considers only outcome observations that are all observable. However, when looking at data on children's report cards, 193 out of 383 report cards do not contain information about non-cognitive II index (i.e. the school absences and the behavior of the child) and, therefore, the non-cognitive II outcomes are excluded in the overall summary index. Specifically, the overall summary index is made up of outcomes of tests scores, schooling dropout and grade repetition, and non-cognitive I: attitudes towards schooling.

groups is statistically significant. The F-test statistic for this difference is 3.146, significant at 10 percent level. These results are different from Rosero and Oosterbeek (2011), who do not find significant gender differences in the (positive) treatment effects of home visits on children's cognitive outcomes. The High Scope Perry Preschool program however had stronger effects on female students (Heckman et al. 2013).

We also estimated the program effect on this cognitive outcome when we stratified the child sample by the mother's educational levels. The results are presented in Table 13, columns 3-4. We divide the full sample of children into two groups according to whether their mothers had completed primary school; our data show that 207 mothers finished up to primary school, and 176 mothers have more than primary education. The estimates in columns 3-4 suggest that the effect of the program on this cognitive index is higher for children of less educated mothers (0.341, se=0.117). The respective estimated effect on children with more educated mothers is actually negative though imprecisely measured (-0.193, se=0.154). F-statistics for group differences in the treatment effects are reported in square brackets and show that the estimated impacts for the two groups are significantly different. The negative effect on children of more educated mothers could be related to the higher employment rate of these mothers before and after they joined the program and also to the lower increase in child investment during the program. We have noted above the lower gain in early childhood investment in children of educated mothers that could be explained by the small impact the program had on time inputs of these mothers into child rearing. The estimates presented in Table 13 columns 5-6 provide additional support for this explanation. In these columns we present program effect on stratified samples by mother's pre-program employment status and find that the effect of the program on test scores was much higher for the sample of children whose mother did not work at baseline, 0.302 (se=0.180) versus -0.092 (se=0.122).

The effect size on children's dropout rates and grade repetition presented in Table 12 is -0.192 (se=0.082) in a specification without covariates; it practically remained unchanged, -0.182 (se=0.073), when adding all the relevant controls, clearly indicating that treated children generally have improved educational attainment due to the program. The estimates presented in Table 13 suggest that impacts on girls' dropout rate or grade repetition (-0.256, se=0.089) are larger than on boys' (-0.087, se=0.116), and the difference is marginally significant. The negative effect on these outcomes is in line with evidence from other pre-school early childhood programs such as the Abecedarian project, the High Scope Perry Preschool program and the Chicago Child-Parent Centers, where grade retention is significantly lowered for treated students (Nores et al. 2005, Temple and Reynolds 2007).

The estimated effect on children's attitudes towards schooling (non-cognitive I) in Table 12 is positive and significant (0.163, se=0.086), and this is true for both female and male students (Table 13).

Among 383 children, 190 have information on their report cards about school absence and

behavior in school (non-cognitive II).[37] The estimated mean effect size of the program is 0.286 and is statistically significant at 5 percent level. In particular, in Table 13 we present a virtually large improvement in the school attendance and behavior for children whose mothers have poorer educational background (0.483, se=0.131). On the other hand, it seems that children who have more-educated mothers, after participating in the program, did not make a progress in this dimension, compared to controlled children with better educated mothers.

6. Robustness Checks

Evidence on Entropy Balancing: In this section we examine whether treatment effects are robust to reweighting the sample with entropy balancing. The results after imposing entropy balancing are reported in columns 4-6 of Tables 4, Tables 7-12, Table 14 and online appendix Tables A3-A6. With respect to mothers' outcomes, we also calculate summary indices for the reweighted sample. The estimated effects on the indices of mothers' labor-market outcomes, independence and household decision-making exhibit similar patterns to those previously reported. The estimated impacts on summary indices tend to suggest a slightly larger overall effect on mothers' employment in 2012 (0.577 vs. 0.482 in Table 7). However, the 95 percent confidence interval with re-weighting (0.388, 0.766) overlaps with the confidence interval without re-weighting. The pooled data in Table 8 tell the same story. When it comes to disaggregated labor-market outcomes of mothers, the estimated parameters remain roughly the same: in 2012 treated mothers are 21.6 percent more likely to be working (the estimated coefficient in the reweighted sample is slightly higher than in the sample without reweighting, where we estimated a 17.6 percent increase), 23.4 percent more likely to be working full-time (20.7 percent without reweighting), and 22.0 percent more likely to be working in the formal sector (20.4 percent without reweighting). These estimates are not statistically different from the estimates in the original sample. The same pattern is observed regarding the estimated effects on average monthly family income in 2012, and mothers' wage in 2013.

The estimated effect on mother's independence does not change much either when we use the entropy re-weighting. Treated mothers are 20.5 percent more likely to control their own money, consistent with the 22.2 percent likelihood we found earlier. The effects of the program on other outcomes are also similar to previous results: treated mothers in the reweighted sample are approximately 9.5 percent more likely to be studying in 2012 or 2013 or both, and they are about 11.6 percent more likely to participate in making the decision on their job status. We find small effects on mothers' engagement in social voluntary activities. Again, the confidence interval at 1 percent for

[37] In appendix A1 we show that this information is likely to be missing at random, as it is not correlated with treatment status nor with most of observable characteristics of children and mothers.

economic and social independence outcomes based on the reweighted sample overlaps greatly with the estimates using the original sample.[38]

The estimated effects on intra-household decision-making also exhibit the same pattern with entropy re-weighting: positive effect on mother's power in decisions on children's education and children's discipline (columns 4-6 in Tables 9-10) and no significant effect on other decision-making outcomes. The estimated aggregate effect on intra-household decision-making in 2012 is 0.124 (statistically significant at the 1 percent level), which is, again, almost identical to the previous result without re-weighting (0.126). In the pooled data, the estimated effects on the indices using the weighted data are smaller and less significant, which is consistent with our finding using the unweighted data, partly because the effects on the additional outcomes in the panel data are negligible.

The estimated effects on mothers' child investment are similar after we re-weighted the sample. The magnitudes of coefficients on the summary index and the detailed outcomes, albeit being statistically weaker, are still positive and remain within a reasonable distance of the unweighted estimates.

We present the estimated effects on the children sample adjusted by entropy balancing in Table 12 (columns 4-6). We find the very same pattern that we found in the unadjusted data. Estimated effects on the non-cognitive I: attitude towards schooling become slightly larger and more statistically significant based on the reweighted sample. The estimates on test scores and educational attainment are also consistent with those found in the original sample.

We also check whether improved women's status hinders men's status in the family. Reassuringly, we find no evidence of a change in the economic status of fathers with entropy-balancing either.

Matching Exercises: We now focus on matching exercises. We start by adopting kernel matching estimation and nearest neighbor matching to focus on a treated family at a time. For each treated family, these methods assign matching weights across all control families that are used to evaluate causal effects. Eventual variability on family structure at the time of the outcome measurement is controlled non-parametrically.

Table 15 shows the effects on mothers' outcomes re-estimated through kernel matching via propensity score (column 1), nearest-neighbor matching through propensity score (column 2) and bias-corrected nearest neighbor matching via the Mahalanobis distance metric (column 3). It is reassuring to see that, irrespective of the matching method, results all go in the same direction. Moreover, they confirm the results found in Table 4: the treatment has positive significant effects on mother's labor market outcomes, economic and social independence, intra-household decision making and child

[38] This is true both for 2012 data and the pooled data.

investments.

In Table 16 we reassess heterogeneity in treatment effects through kernel matching and we find them qualitatively comparable to those reported in Table 5.[39]

Results shown in Tables 8 and 10 are also robust to the different matching methods and can be found in the online appendix (Tables A7-A8). Finally, positive treatment effects on children's outcomes are also confirmed by the matching methods (Table 17).

To address lingering concerns about treated mothers having fewer young children at home and therefore being more likely to work, we perform a much tighter matching on mothers who have both preschool and school children and other characteristics. First, we now keep only the sample of mothers who have children in preschool and children in primary school. Second, we match treated and control mothers from this reduced sample based on mother's and children's age. Third, we match this reduced sample not only on mother's and children's age, but also on mother's employment status. Most of pre-treatment variables and the family composition at the time of the survey are balanced (see Tables A9-A10 in the online appendix). Table 18 shows the results of bias-corrected matching (columns 1-2) and nearest-neighbor matching via propensity score (columns 3-4) for this sample.[40] Irrespective of the matching method, treated mothers in this reduced sample still exhibit significant positive effects on labor market outcomes, economic and social independence, and child investment.[41] The estimated effects on children outcomes are also confirmed by these tighter matchings (Table 19).

Attrition Analysis: A potential concern in our paper is attrition bias. Since mothers can enroll in *PelCa* soon after their child's first birthday and up to age 5, and then enroll in the *PelCa* school program, participation in the program can be up to twelve years. Mothers who remained in the program for longer may be different from mothers who decided to leave earlier. Therefore, the sample of treated mothers might be different from the mothers in the control group. To examine this potential threat to our identification strategy, we take advantage of a sample of mothers and children who enrolled in the *PelCa* program between 2004 and 2013 and left the program between 2005 and 2015. For this sample we have information on mothers' characteristics such as age, living status, working status and highest educational level, and age of their child who was enrolled in the program. This sample includes 311 mothers and 430 children.

We first restricted this "attrite" sample to 172 mothers and 258 children who joined the *PelCa*

[39] For brevity we only report heterogeneity in treatment effects re-estimated through kernel matching. Results through other types of matching go in the same direction and are available upon request.

[40] In columns 1 and 3 mothers and children are matched on their age, in columns 2 and 4 they are also matched on mothers' employment status.

[41] Table A11 in the online appendix reports disaggregated treatment effects on labor market outcomes and mother's economic and social independence and shows that indeed, even in this sample, treated mothers are significantly more likely to be working and to be working full-time.

program during the same period that the treated mothers joined the program, namely between 2005 and 2009. We find that the two groups are very well balanced in terms of their pre-program characteristics (column 4 of Table A12 in the online appendix). This result is unchanged even if we restrict further the attrite sample to mothers and children who joined the program between 2005 and 2009 and left it after 4 years (column 5 of Table A12).

One important difference between the two samples is that all treated children have reached primary school age by 2012 and moved to the school-*PelCa* program while 67 out of 258 attrite children (25.97 percent) were still in the pre-school *PelCa* program. We therefore limit further the attrite sample to mothers whose children were all in the school-*PelCa* program when they left the program. The means of the pre-program characteristics of the attrite group, the treatment group and the control group are presented in columns 1-3 of Table 20. T-tests on pre-program differences of mothers' pre-characteristics (mother's age, number of children in 2005, living status, working status and highest educational level) and in child's age before entering the program are well balanced at 10 percent level of significance. The results of this comparison between the attrite and treatment groups are presented in column 4 and the attrite and the control group in column 5. We think this evidence suggests that the decision to leave the program is not correlated with observed characteristics of mothers and children, which raises the likelihood that they are also uncorrelated with their unobserved characteristics.

Additional Robustness Checks: Our analysis relies on the similarity between treatment and control groups. However, we note that 10 percent of mother or child characteristics were not balanced at the 10 percent significance level. We interpreted this difference in pre-treatment observables characteristics as random, as might happened even in a randomly assigned treatment and control groups. We think that the close similarity between the control and treatment groups results from the similar self-selection of mothers into the two groups, as both have shown their desire to participate in the program. In support of this identification approach we show below that mothers who decided to participate in the program in 2012 did not participate before for reasons that do not affect the outcomes we study. For example, we found some imbalance in mothers working status before joining or showing interest in joining the program. We therefore re-do the analysis by subsamples: first we analyze the treatment effects in the group of treated and control mothers who were not working at baseline, and secondly in a sub-sample of treated and control mothers who were working at baseline (2005). The estimated treatment effects obtained from these two sub-samples are very similar and they are also very similar to the estimates we obtained from the full sample. This evidence rules out the possibility that our results are driven by non-comparable treatment and control groups in terms of employment rate at baseline.

It is still important, however, to understand why control group mothers did not enroll in the program previously. As Figure 2 shows, almost half of the mothers (44.55 percent) had not previously applied because they did not know about the program, a sign that information is not flowing smoothly,

which does not necessarily imply that mothers who applied later are different from mothers who applied before. Almost an additional 10% showed interest in similar programs by being affiliated with another NGO, which leads us to think that these mothers are not so different from treated mothers either. Another fourth of the control group used to live far away and that was an obstacle to enroll in the program, whereas now they are probably living closer. Hence, at least for approximately 4/5 of mothers in the control group, there is no reason to assume them to be different in any way from mothers who were in the program since the beginning. Moreover, these responses exclude the possibility that mothers chose not to enroll in the program because they questioned its effectiveness. We also note here that none of these explanations are correlated with the previous working condition or with working full-time before.

As a further robustness check, we re-estimate all models by limiting the sample to the 45 out of 115 control mothers who did not enroll in the program because they were not aware of it. Table A21 in the appendix presents the estimated average effect sizes on mothers' outcomes using both 2012 data and the pooled data of 2012 and 2013. Comparing these results with treatment effects in the original sample in Table 4, we see that the estimated effects in the 45 mothers' sub-sample are slightly higher, though they are generally very similar to those from the full sample.

Moreover, we re-estimate all models by restricting the sample to the control mothers who did not enroll in the program either because they were already affiliated with another NGO, or because there was a problem with their application forms. The first group of control mothers, if anything, should be more attentive than treated mothers. The second group can be interpreted as randomly assigned to the control group. Unfortunately, the sample here is very small (21 mothers): the estimated effects we obtain from this sample are qualitatively similar to those obtained from the full sample, but they are much less precisely estimated.

Finally, to mitigate concerns regarding the possibility that mothers whose children are doing badly might be enrolling their younger children as a reaction, we perform an exercise in which we only consider children who are not the first born. This way, we are left with 134 (61.9 percent) out of 219 treated children. As we can see from Table 21, estimated treated effects on these children are perfectly in line with results from the full sample.

7. Conclusion

In this paper we analyze an innovative method to empower women and increase their early childhood investment in their children. *PelCa*, a home-preschool program, placed the mother at the center of her children's education, and provided guidance and training for achieving its goals. Relying on a 'designed' quasi-natural experiment, we are able to identify and measure the causal effects of this program on both mothers and children after several years of program participation.

First, we find that the intervention empowers women across different domains. It facilitates their entry into the labor market: treated mothers are much more likely to be working, more likely to be working full-time and more likely to be working in the formal sector. All of these estimates are precisely measured and are robust to the inclusion of covariates. Moreover, treated mothers become more financially independent and more likely to manage and make spending decisions about their own money; to be studying; and to decide whether they can work outside of the home. The results suggest that after joining the *PelCa* program, mothers are also willing to spend more time interacting with their children, on cognitive or social activities. The treatment further modifies the allocation of power in the house: mothers become more likely to take part in decisions about children's education and discipline. However, we find no effect on mother's role in decisions about what to do when the child is ill, about various types of expenditures, about having children, or the use of contraceptives. Treatment intensity plays a role here: the longer the mother stays in the program, the more empowered she becomes within the household. Moreover, mothers who were less empowered at baseline are the ones who gain the most in terms of advancing their role in decisions about the household and the allocation of resources.

All of the above results hold when we estimate aggregate treatment impacts, use summary indices instead of individual outcomes in order to account for multiple inferences, when we use entropy balancing to adjust for differences in pre-treatment covariates, and when we use other robustness checks.

We also evaluate the program's impact on children. We firstly examine children's cognitive tests. The estimated impact on the summary index of cognitive achievement of treated children is positive but only marginally significant. But the average treatment effect disguises meaningful heterogeneity. Girls in the program appear to make large positive progress in test scores but there is no corresponding effect on boys; children whose mothers have a lower educational attainment benefit much more from the program in terms of improved cognitive tests than children of more educated mothers. Our findings also suggest that there are differential treatment effects by mothers' pre-treatment working status and that children with less-educated mothers at baseline (before enrolling in the program) show more improvements in their cognitive testing.

Moreover, consistent with findings from other pre-school programs, students in the *PelCa* program are much less likely to drop out of school or to repeat a grade. These effects are marginally larger for female students, but the difference between genders is not significant. We also find significant treatment effects on children's non-cognitive skills. After participating in the *PelCa* program, children exhibit less school absence and better behavior at school, and the male students appear to have better attitudes towards schooling. Overall, there is evidence that the home-preschool program that we study helped mothers raise their children in a more learning-conducive environment, which led to positive effects on children, as well as on empowering mothers at home and in the community.

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Table 1: Mothers' Characteristics and Pre-program Outcomes (2012)

	Treatment mean	Control mean	Difference in means	Std. error
	(1)	(2)	(3)	(4)
A: Mothers' characteristics before Treatment				
Age	31.988	31.183	0.805	(0.729)
From Quito	0.560	0.496	0.065	(0.061)
Parents from same city	0.510	0.456	0.053	(0.064)
Live together with partner	0.801	0.817	-0.016	(0.048)
Divorced/separated/widow	0.018	0.009	0.009	(0.014)
Single	0.181	0.174	0.007	(0.047)
Number of children in 2005	1.849	1.632	0.218	(0.180)
Did not complete primary	0.114	0.148	-0.033	(0.041)
Completed primary	0.392	0.374	0.018	(0.059)
Did not complete secondary	0.295	0.304	-0.009	(0.056)
Completed secondary	0.169	0.165	0.003	(0.045)
Started university	0.024	0.009	0.015	(0.016)
Not religious	0.096	0.070	0.027	(0.034)
Christian	0.831	0.861	-0.030	(0.044)
B: Mothers' pre-program outcomes				
Manage own money	0.582	0.526	0.056	(0.061)
Worked	0.470	0.609	-0.139**	(0.060)
Worked full-time†	0.551	0.729	-0.177**	(0.078)
Self-employed†	0.808	0.786	0.022	(0.067)
Worked in the formal sector†	0.256	0.300	-0.044	(0.074)
Mean firm size	10.182	12.739	-2.557	(5.855)
Reason Not Working				
Because of children	0.333	0.252	0.081	(0.056)
Because there was no job	0.073	0.078	-0.006	(0.032)
Because partner does not want	0.073	0.052	0.021	(0.030)
Other reasons	0.048	0.009	0.040*	(0.021)
Joint Decision with Spouse				
Child's education	0.899	0.875	0.024	(0.066)
Own health	0.963	0.968	-0.005	(0.040)
Discipline	0.875	0.903	-0.028	(0.069)
Expenditures	0.764	0.693	0.071	(0.054)
Food expenditures	0.758	0.789	-0.032	(0.051)
Own labor force participation	0.800	0.770	0.030	(0.050)
Having children	0.878	0.858	0.020	(0.041)
Contraceptives	0.896	0.856	0.040	(0.040)
F(23, 36) =1.2918 Prob > F = 0.2402				
Observations	166	115	281	

Notes: Statistics are based on the 2012 survey of mothers. Standard errors are presented in parentheses in the column (4); * p < 0.1, ** p < 0.05, *** p < 0.01. An F-test on the overall significance of the pre-treatment variables is shown at the end of the table.

† Conditional on working.

Table 2: Children's Characteristics before Treatment (2012)

	Treatment mean	Control mean	Difference in means	Std. error
	(1)	(2)	(3)	(4)
Female	0.521	0.470	0.051	(0.052)
Age	8.344	8.835	-0.491**	(0.198)
Mean birth order	2.023	1.878	0.145	(0.136)
1 younger sibling in 2005	0.201	0.201	-0.000	(0.041)
2 younger siblings in 2005	0.027	0.049	-0.021	(0.019)
3 younger siblings in 2005	0.000	0.006	-0.006	(0.005)
Height at birth (cm)	48.258	48.790	-0.531	(0.462)
Weight at birth (gram)	3046.155	3029.197	16.957	(74.818)
Head circumference at birth (cm)	33.723	33.572	0.151	(0.283)
Dummy grade ½	0.320	0.305	0.015	(0.048)
Dummy grade ¾	0.365	0.348	0.018	(0.050)
Dummy grade 5/6	0.242	0.268	-0.026	(0.045)
Dummy grade 7	0.073	0.079	-0.006	(0.027)
F(11, 103) = 1.3029 Prob > F = 0.2334				
Observations	219	164	383	

Notes: Statistics are based on the 2012 survey of children. Standard errors are presented in parentheses in the column (4); * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. An F-test on the overall significance of the pre-treatment variables is shown at the end of the table.

Table 3: Treatment-Control Mean Comparisons of Outcomes and Summary Indices

	2012 sample				Pooled 2012 and 2013			
	Treatment Mean	Control Mean	Difference (1)-(2)	Std. Error	Treatment Mean	Control Mean	Difference (5)-(6)	Std. Error
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Labor market outcomes (mothers)								
Summary index	**0.497**	**0.000**	**0.497*** **	**(0.071)**	**0.559**	**0.000**	**0.559*** **	**(0.062)**
Works	0.693	0.487	0.206***	(0.058)	0.767	0.546	0.221***	(0.042)
Working full-time	0.378	0.157	0.222***	(0.054)	0.416	0.199	0.217***	(0.042)
Working with contract	0.279	0.087	0.192***	(0.047)	0.272	0.082	0.190***	(0.036)
Average family monthly income	344.848	300	44.848**	(20.526)	--	--	--	--
Weekly wage†	--	--	--	--	46.727	28.559	18.168***	(4.460)
B: Economic and social independence (mothers)								
Summary index	**0.319**	**0.000**	**0.319*** **	**(0.064)**	**0.251**	**0.000**	**0.251*** **	**(0.047)**
Manage own money	0.661	0.447	0.213***	(0.059)	0.681	0.487	0.194***	(0.044)
Participates in voluntary activities	0.639	0.562	0.077	(0.066)	0.584	0.550	0.034	(0.048)
Currently studying	0.133	0.052	0.081**	(0.036)	0.114	0.051	0.063**	(0.026)
Own or joint decision on own work status	0.927	0.805	0.122***	(0.039)	0.956	0.871	0.085***	(0.024)
C: Intra-household decision-making (mothers)								
Summary index	**0.093**	**0.000**	**0.093** **	**(0.043)**	**0.085**	**0.000**	**0.085*** **	**(0.028)**
Own/joint decision on child's education	0.952	0.852	0.099***	(0.034)	0.973	0.897	0.076***	(0.021)
Own/joint decision on own health	0.945	0.93	0.016	(0.029)	0.953	0.928	0.025	(0.021)
Own/joint decision on child's discipline	0.921	0.833	0.087**	(0.039)	0.939	0.856	0.083***	(0.027)

Own/joint decision on expenditure	0.805	0.754	0.05	(0.050)	0.819	0.732	0.087**	(0.038)
Own/joint decision on food expenditure	0.806	0.789	0.017	(0.049)	0.827	0.794	0.033	(0.036)
Own/joint decision on having children	0.963	0.956	0.007	(0.024)	0.959	0.958	0.001	(0.018)
Own/joint decision on contraceptives	0.920	0.946	-0.026	(0.031)	0.918	0.957	-0.039*	(0.023)
Own/joint decision on own health	--	--	--	--	0.906	0.918	-0.011	(0.026)
Own/joint decision on if mothers can visit	--	--	--	--	0.878	0.857	0.020	(0.031)
Own/joint decision on important matters	--	--	--	--	0.899	0.837	0.062**	(0.030)
D: Child's investment (mothers)								
Summary index	**0.166**	**0.000**	**0.166*** **	**(0.053)**	**--**	**--**	**--**	**--**
Talk to child (weekly)	0.988	0.965	0.022	(0.018)	--	--	--	--
Listen and talk to child about child's readings (weekly)	0.891	0.809	0.082*	(0.042)	--	--	--	--
Visit library with child (weekly)	0.115	0.087	0.028	(0.037)	--	--	--	--
Plays or dances with child (weekly)	0.988	0.928	0.060***	(0.023)	--	--	--	--
E: Self-esteem, Big Five Personality Traits and Fertility Choices (mothers)								
Summary index††	**0.030**	**0.000**	**0.030**	**(0.041)**	**--**	**--**	**--**	**--**
Rosenberg scale	3.141	3.107	0.034	(0.056)	--	--	--	--
Agreeableness	3.399	3.414	-0.015	(0.067)	--	--	--	--
Conscientiousness	3.724	3.714	0.010	(0.078)	--	--	--	--
Extraversion	3.086	3.166	-0.080	(0.066)	--	--	--	--
Neuroticism	3.044	2.964	0.080	(0.070)	--	--	--	--
Openness to Experience	3.570	3.507	0.063	(0.075)	--	--	--	--
Pregnant	0.018	0.035	-0.017	(0.019)	--	--	--	--
More children (including pregnant women)?	0.217	0.149	0.068	(0.048)	--	--	--	--
F: Labor market outcomes (fathers)								
Summary index	**-0.120**	**0.000**	**-0.120.**	**(0.076)**	**0.018**	**0.000**	**0.018.**	**(0.052)**
Working	0.953	0.970	-0.017	(0.025)	0.891	0.885	0.006	(0.030)

Working full-time	0.860	0.890	-0.03	(0.043)	0.817	0.785	0.032	(0.038)
Working with contract	0.480	0.550	-0.07	(0.065)	0.428	0.451	-0.023	(0.047)
G: Children's outcomes								
Overall summary index†††	**0.182**	**0.000**	**0.182*** **	**(0.034)**	--	--	--	--
Test scores summary index	**0.117**	**0.000**	**0.117***	**(0.069)**	--	--	--	--
Language test	0.076	-0.058	0.134	(0.110)	--	--	--	--
Maths test	0.049	-0.039	0.088	(0.113)	--	--	--	--
Schooling dropout and grade repetition summary index	**-0.192**	**0.000**	**-0.192*** **	**(0.056)**	--	--	--	--
Repeats at least once	0.023	0.085	-0.062***	(0.022)	--	--	--	--
Child temporarily leaves	0.014	0.049	-0.035**	(0.017)	--	--	--	--
Non-Cognitive I: Attitude towards schooling	**0.191**	**0.000**	**0.191** **	**(0.070)**	--	--	--	--
Child likes school	0.010	-0.040	0.050	(0.104)	--	--	--	--
Child likes school (mother's perspective)	0.111	-0.237	0.348***	(0.103)	--	--	--	--
Non-Cognitive II: Absences and behavior in school††††	**0.227**	**0.000**	**0.227*** **	**(0.075)**	--	--	--	--
School absence in report card	-3.878	-4.506	0.628	(0.653)	--	--	--	--
Behavior of child in report cards	18.514	18.254	0.260	(0.165)	--	--	--	--
Observations of mothers/fathers	166	115	281		300	196	496	
Observations of children	219	164	383					

Notes: In each panel, statistics for summary index of corresponding outcomes are reported in shading rows. Each summary index of multiple outcomes is calculated by the z-score (See section 5.1 and Kling et al., 2007 for details). Standard errors are presented in parentheses in columns (4) and (8) * p < 0.1, ** p < 0.05, *** p < 0.01.

† Estimated results for mother's weekly wage is based on the data of 2013;

†† This summary index is constructed by only outcomes of Rosenberg self-esteem scale and Big Five Personality Traits.

††† Signs of outcomes of schooling dropout and grade repetition are reversed when calculating the overall summary index of children's outcomes.

†††† Signs of the outcome of school absence in report card are reversed in this summary index.

Table 4: Estimated Effects on Mothers' Outcomes: Summary Indices

	Not weighted			Weighted		
	(1)	(2)	(3)	(4)	(5)	(6)
A: Sample based on 2012 survey						
Labor market outcomes	0.494***	0.467***	0.482***	0.581***	0.577***	0.577***
	(0.100)	(0.099)	(0.098)	(0.114)	(0.101)	(0.096)
Economic and social independence	0.295***	0.336***	0.366***	0.249***	0.275***	0.302***
	(0.074)	(0.075)	(0.076)	(0.089)	(0.081)	(0.070)
Intra-household decision-making	0.119**	0.118**	0.126**	0.126*	0.125**	0.124**
	(0.055)	(0.056)	(0.057)	(0.072)	(0.061)	(0.055)
Child investment	0.175***	0.209***	0.207***	0.213**	0.219**	0.212**
	(0.061)	(0.063)	(0.065)	(0.095)	(0.091)	(0.085)
Self-esteem and Big Five Personality Traits	0.038	0.033	0.045	0.060	0.060	0.063
	(0.065)	(0.067)	(0.068)	(0.083)	(0.090)	(0.083)
Observations	281	281	281	281	281	281
B: Pooled sample of 2012 and 2013						
Labor market outcomes	0.554***	0.502***	0.503***	0.660***	0.664***	0.665***
	(0.099)	(0.103)	(0.098)	(0.116)	(0.105)	(0.102)
Economic and social independence	0.231***	0.259***	0.276***	0.263***	0.285***	0.288***
	(0.054)	(0.055)	(0.056)	(0.072)	(0.061)	(0.062)
Intra-household Decision-making	0.106**	0.101**	0.093*	0.076	0.068	0.070
	(0.053)	(0.051)	(0.051)	(0.068)	(0.056)	(0.052)
Observations	496	496	496	496	496	496
Child Controls	No	Yes	Yes	No	Yes	Yes
Household Demographics	No	Yes	Yes	No	Yes	Yes
Household Economics	No	No	Yes	No	No	Yes

Notes: Each cell reports the estimated mean effect from a separate regression. Columns (1)-(3) present results using the original sample without entropy balancing. Columns (4)-(6) stem from the weighted sample adjusted by entropy balancing. Standard errors are presented in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Estimated Effects on Mothers' Outcomes by Pre-Treatment Outcomes Heterogeneity (2012)

	Mothers' pre-treatment employment status		Mothers' education		Mothers' pre-treatment role in decision-making	
	Not worked before	Worked before	Up to primary schooling	More than primary schooling	Below average	Above average
	(1)	(2)	(3)	(4)	(5)	(6)
Labor market outcomes Index	0.794***	0.340***	0.462***	0.611***	0.729***	0.394***
	(0.191)	(0.126)	(0.130)	(0.163)	(0.145)	(0.131)
	[4.120]**		[0.514]		[2.650]*	
Economic and social independence Index	0.351***	0.484***	0.420***	0.386***	0.482***	0.415***
	(0.120)	(0.095)	(0.105)	(0.113)	(0.118)	(0.108)
	[0.776]		[0.049]		[0.154]	
Household decisions-making index	0.213**	0.086	0.098	0.205**	0.197***	–0.017
	(0.083)	(0.079)	(0.076)	(0.087)	(0.073)	(0.083)
	[1.191]		[0.861]		[2.987]*	
Child investment Index	0.059	0.373***	0.320***	0.070	0.219***	0.186**
	(0.096)	(0.092)	(0.105)	(0.100)	(0.103)	(0.086)
	[5.586]**		[2.890]*		[0.060]	
Observations	133	148	144	136	93	179

Notes: Each cell reports the estimated aggregate effect on a summary index from a separate regression based on the 2012 survey. Covariates of child characteristics, household demographics and household economics are included in regressions. In columns (1) – (6), F-test (Chow-test) statistic for subgroup difference in treatment effects are presented in square brackets. Estimated results are based on the original sample without entropy balancing. Standard errors are presented in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Estimated Effects on Mothers' Outcomes by Gender of Child in the Program: Summary Indices (2012)

	Boy			Girl		
	(1)	(2)	(3)	(4)	(5)	(6)
Labor market outcomes	0.582***	0.493***	0.534***	0.466***	0.479***	0.500***
	(0.147)	(0.137)	(0.135)	(0.125)	(0.123)	(0.122)
Economic and social independence	0.272***	0.308***	0.323***	0.379***	0.447***	0.471***
	(0.096)	(0.094)	(0.094)	(0.107)	(0.102)	(0.105)
Intra-household decision-making	0.059	0.084	0.079	0.147**	0.128*	0.161**
	(0.078)	(0.080)	(0.082)	(0.067)	(0.068)	(0.066)
Child Investment	0.226***	0.265***	0.268***	0.141*	0.176**	0.193**
	(0.085)	(0.087)	(0.090)	(0.076)	(0.081)	(0.081)
Self-esteem and Big Five Personality Traits	0.120	0.172*	0.177**	-0.026	-0.052	-0.021
	(0.087)	(0.088)	(0.089)	(0.083)	(0.085)	(0.083)
Child Controls	No	Yes	Yes	No	Yes	Yes
Household Demographics	No	Yes	Yes	No	Yes	Yes
Household Economics	No	No	Yes	No	No	Yes
Observations	159	159	159	165	165	165

Notes: Each cell reports the estimated effect obtained in a separate regression without entropy balancing. Columns 1-3 present results using the sample of mother with a boy in the program and columns 4-6 present estimates for the sample of mothers to girls in the program; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Estimated Effects on Labor Market Outcomes and Mothers' Economic and Social Independence (2012)

	Not weighted			Weighted		
	(1)	(2)	(3)	(4)	(5)	(6)
A: Labor market outcomes						
Summary index	0.494***	0.467***	0.482***	0.581***	0.577***	0.577***
	(0.100)	(0.099)	(0.098)	(0.114)	(0.101)	(0.096)
Works	0.206***	0.172***	0.176***	0.213***	0.216***	0.216***
	(0.058)	(0.061)	(0.062)	(0.081)	(0.069)	(0.066)
Working full-time	0.222***	0.205***	0.207***	0.233***	0.238***	0.234***
	(0.054)	(0.057)	(0.059)	(0.061)	(0.052)	(0.051)
Working with contract	0.192***	0.202***	0.204***	0.221***	0.223***	0.220***
	(0.047)	(0.048)	(0.049)	(0.044)	(0.041)	(0.041)
Average family monthly income	44.848**	44.538**	44.479**	44.507*	43.193*	43.242**
	(20.526)	(21.189)	(20.462)	(24.849)	(22.267)	(20.884)
B: Mothers' economic and social independence						
Summary index	0.295***	0.336***	0.366***	0.249***	0.275***	0.302***
	(0.074)	(0.075)	(0.076)	(0.089)	(0.081)	(0.070)
Manage own money	0.213***	0.233***	0.222***	0.202**	0.204***	0.205***
	(0.059)	(0.062)	(0.064)	(0.082)	(0.067)	(0.065)
Participates in voluntary activities	0.077	0.105	0.142*	0.069	0.087	0.105
	(0.066)	(0.071)	(0.072)	(0.092)	(0.076)	(0.074)
Currently studying	0.081**	0.111***	0.123***	0.087**	0.086**	0.095***
	(0.036)	(0.039)	(0.040)	(0.037)	(0.036)	(0.034)
Own or joint decision on own work status	0.122***	0.115***	0.132***	0.112*	0.108**	0.116**
	(0.039)	(0.042)	(0.044)	(0.061)	(0.054)	(0.052)
Child Controls	No	Yes	Yes	No	Yes	Yes
Household Demographics	No	Yes	Yes	No	Yes	Yes
Household Economics	No	No	Yes	No	No	Yes
Observations	281	281	281	281	281	281

Notes: Each cell reports the estimated treatment effect from a separate regression based on the 2012 survey. Estimated summary indices of corresponding outcomes are reported in shading rows. Columns 1-3 present results using the original sample without entropy balancing. Columns 4-6 stem from the weighted sample adjusted by entropy balancing. Standard errors are presented in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Estimated Effects on Labor Market Outcomes and Mothers' Economic and Social Independence (Pooled 2012 and 2013)

	Not weighted			Weighted		
	(1)	(2)	(3)	(4)	(5)	(6)
A: Labor market outcomes						
Summary index	0.554***	0.502***	0.503***	0.660***	0.664***	0.665***
	(0.099)	(0.103)	(0.098)	(0.116)	(0.105)	(0.102)
Works	0.221***	0.167***	0.164***	0.204**	0.204***	0.204***
	(0.056)	(0.061)	(0.060)	(0.081)	(0.068)	(0.067)
Working full-time	0.217***	0.192***	0.194***	0.240***	0.241***	0.241***
	(0.049)	(0.053)	(0.052)	(0.055)	(0.050)	(0.050)
Working with contract	0.190***	0.193***	0.193***	0.218***	0.218***	0.218***
	(0.038)	(0.038)	(0.037)	(0. 036)	(0.035)	(0.034)
Weekly wage†	18.168***	14.848***	13.331***	18.504***	17.886***	17.081***
	(4.460)	(4.852)	(4.874)	(5.713)	(4.999)	(4.966)
B: Mothers' economic and social independence						
Summary indices	0.231***	0.259***	0.276***	0.263***	0.285***	0.288***
	(0.054)	(0.055)	(0.056)	(0.072)	(0.061)	(0.062)
Manage own money	0.194***	0.187***	0.178***	0.146**	0.146***	0.146***
	(0.050)	(0.052)	(0.051)	(0.068)	(0.056)	(0.053)
Participates in voluntary activities	0.034	0.051	0.067	0.017	0.019	0.019
	(0.050)	(0.053)	(0.054)	(0.073)	(0.059)	(0.055)
Currently in school	0.063**	0.077**	0.085***	0.087***	0.088***	0.088***
	(0.028)	(0.031)	(0.031)	(0.028)	(0.028)	(0.027)
Own or joint decision on own work status	0.085***	0.093***	0.104***	0.120**	0.123***	0.125***
	(0.029)	(0.030)	(0.031)	(0.048)	(0.045)	(0.041)
Child Controls	No	Yes	Yes	No	Yes	Yes
Household Demographics	No	Yes	Yes	No	Yes	Yes
Household Economics	No	No	Yes	No	No	Yes
Observations	496	496	496	496	496	496

Notes: Each cell reports the estimated treatment effect from a separate regression based on 2012 and 2013 surveys. Estimated summary indices of corresponding outcomes are reported in shading rows. Columns 1-3 present results using the original sample without entropy balancing. Columns 4-6 stem from the weighted sample adjusted by entropy balancing. Standard errors are presented in parentheses and are clustered at the maternal level. † Estimated results for mothers weekly wage is based on the data of 2013.

Table 9: Estimated Effects on Mothers' Intra-Household Decision-making (2012)

	Not weighted			Weighted		
	(1)	(2)	(3)	(4)	(5)	(6)
Summary index	0.119**	0.118**	0.126**	0.126*	0.125**	0.124**
	(0.055)	(0.056)	(0.057)	(0.072)	(0.061)	(0.055)
Own/joint decision on child's education	0.099***	0.099***	0.102***	0.143**	0.145**	0.143**
	(0.034)	(0.038)	(0.039)	(0.062)	(0.059)	(0.056)
Own/joint decision on child's health	0.016	0.003	0.006	–0.019	–0.018	–0.018
	(0.029)	(0.032)	(0.032)	(0.025)	(0.025)	(0.026)
Own/joint decision on child's discipline	0.087**	0.081*	0.087**	0.117*	0.116*	0.115*
	(0.039)	(0.041)	(0.042)	(0.061)	(0.060)	(0.059)
Own/joint decision on expenditure	0.050	0.052	0.069	0.106	0.106	0.103
	(0.050)	(0.054)	(0.055)	(0.077)	(0.067)	(0.063)
Own/joint decision on food expenditure	0.017	-0.001	0.017	0.030	0.031	0.029
	(0.049)	(0.051)	(0.053)	(0.078)	(0.067)	(0.062)
Own/joint decision on having children	0.007	0.007	0.005	–0.004	–0.005	–0.006
	(0.024)	(0.026)	(0.027)	(0.024)	(0.023)	(0.023)
Own/joint decision on contraceptives	–0.026	–0.008	–0.019	–0.027	–0.030	–0.030
	(0.031)	(0.033)	(0.034)	(0.040)	(0.030)	(0.030)
Child Controls	No	Yes	Yes	No	Yes	Yes
Household Demographics	No	Yes	Yes	No	Yes	Yes
Household Economics	No	No	Yes	No	No	Yes
Observations	281	281	281	281	281	281

Notes: Each cell reports the estimated treatment effect from a separate regression based on the 2012 survey. Estimated summary indices of corresponding outcomes are reported in shading rows. Columns 1-3 present results using the original sample without entropy balancing. Columns 4-6 estimates are based on the weighted sample adjusted by entropy balancing. Standard errors are presented in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Estimated Effects on Mothers' Intra-Household Decision-making (Pooled 2012 and 2013)

	Not weighted			Weighted		
	(1)	(2)	(3)	(4)	(5)	(6)
Summary index	0.106**	0.101**	0.093*	0.076	0.068	0.070
	(0.053)	(0.051)	(0.051)	(0.068)	(0.056)	(0.052)
Own/joint decision on child's education	0.076***	0.063***	0.066***	0.095**	0.095***	0.095***
	(0.025)	(0.026)	(0.026)	(0.035)	(0.031)	(0.030)
Own/joint decision on child's health	0.025	0.018	0.021	0.007	0.007	0.007
	(0.032)	(0.034)	(0.033)	(0.032)	(0.031)	(0.031)
Own/joint decision on child's discipline	0.083**	0.088**	0.086**	0.119**	0.119**	0.119**
	(0.033)	(0.036)	(0.037)	(0.056)	(0.054)	(0.053)
Own/joint decision on expenditure	0.087	0.083	0.090	0.104	0.103	0.104*
	(0.055)	(0.057)	(0.057)	(0.078)	(0.064)	(0.060)
Own/joint decision on food expenditure	0.033	0.011	0.031	0.031	0.031	0.031
	(0.052)	(0.052)	(0.051)	(0.076)	(0.065)	(0.060)
Own/joint decision on important matters	0.062	0.058	0.048	0.016	0.016	0.015
	(0.045)	(0.051)	(0.052)	(0.052)	(0.047)	(0.046)
Own/joint decision on having children	0.001	0.001	–0.003	–0.018	–0.018	–0.019
	(0.026)	(0.026)	(0.027)	(0.022)	(0.021)	(0.021)
Own/joint decision on contraceptives	–0.039	–0.018	–0.024	–0.024	–0.026	–0.025
	(0.031)	(0.033)	(0.036)	(0.043)	(0.033)	(0.032)
Own/joint decision on own health	–0.011	–0.006	–0.021	–0.053*	–0.052*	–0.054*
	(0.037)	(0.040)	(0.040)	(0.031)	(0.031)	(0.030)
Own/joint decision on if mothers can visit	0.020	0.022	0.013	0.040	0.043	0.043
	(0.045)	(0.049)	(0.049)	(0.062)	(0.058)	(0.058)
Child Controls	No	Yes	Yes	No	Yes	Yes
Household Demographics	No	Yes	Yes	No	Yes	Yes
Household Economics	No	No	Yes	No	No	Yes
Observations	496	496	496	496	496	496

Notes: Each cell reports the estimated treatment effect from a separate regression based on 2012 and 2013 surveys. Estimated summary indices of corresponding outcomes are reported in shading rows. Columns 1-3 present results using the original sample without entropy balancing. Columns 4-6 stem from the weighted sample adjusted by entropy balancing. Standard errors are presented in parentheses and are clustered at the maternal level; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 11: Estimated Effects on Mothers' Child Investment (2012)

	Not weighted			Weighted		
	(1)	(2)	(3)	(4)	(5)	(6)
Summary index	0.175***	0.209***	0.207***	0.213**	0.219**	0.212**
	(0.061)	(0.063)	(0.065)	(0.095)	(0.091)	(0.085)
Talk to child (weekly)	0.022	0.038**	0.040**	0.040	0.045	0.045
	(0.018)	(0.018)	(0.019)	(0.039)	(0.034)	(0.034)
Listen and talk to child about child's readings (weekly)	0.082*	0.089*	0.100**	0.101	0.105*	0.108*
	(0.042)	(0.045)	(0.047)	(0.066)	(0.057)	(0.056)
Visit library with child (weekly)	0.028	0.025	0.020	0.054	0.048	0.047
	(0.037)	(0.039)	(0.040)	(0.040)	(0.039)	(0.038)
Plays or dances with child (weekly)	0.060***	0.068***	0.066***	0.026	0.034*	0.032*
	(0.023)	(0.024)	(0.025)	(0.018)	(0.019)	(0.018)
Child Controls	No	Yes	Yes	No	Yes	Yes
Household Demographics	No	Yes	Yes	No	Yes	Yes
Household Economics	No	No	Yes	No	No	Yes
Observations	281	281	281	281	281	281

Notes: Each cell reports the estimated treatment effect from a separate regression based on 2012 surveys. Estimated summary indices of corresponding outcomes are reported in shading rows. Columns 1-3 present results using the original sample without entropy balancing. Columns 4-6 stem from the weighted sample adjusted by entropy balancing. Standard errors are presented in parentheses and are clustered at the maternal level; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 12: Estimated Effects on Children's Outcomes: Summary Indices (2012)

	Not weighted			Weighted		
	(1)	(2)	(3)	(4)	(5)	(6)
Overall summary index†,††	0.212***	0.197***	0.211***	0.240***	0.235***	0.212***
(290 observations used)	(0.066)	(0.064)	(0.063)	(0.076)	(0.077)	(0.066)
Tests scores	0.122	0.124	0.115	0.068	0.131	0.120
(322 observations used)	(0.104)	(0.099)	(0.103)	(0.155)	(0.107)	(0.106)
Schooling dropout and grade repetition	–0.192**	–0.170**	–0.182**	–0.112	–0.174*	–0.181*
(381 observations used)	(0.082)	(0.073)	(0.073)	(0.089)	(0.096)	(0.098)
Attitude towards schooling	0.210**	0.149*	0.163*	0.267***	0.243***	0.210**
(347 observations used)	(0.085)	(0.089)	(0.086)	(0.087)	(0.081)	(0.085)
Absences and behavior in school†††	0.210*	0.253**	0.286**	0.152	0.240**	0.254**
(190 observations used)	(0.121)	(0.124)	(0.131)	(0.189)	(0.110)	(0.112)
Child Controls	No	Yes	Yes	No	Yes	Yes
Household Demographics	No	Yes	Yes	No	Yes	Yes
Household Economics	No	No	Yes	No	No	Yes
Observations	383	383	383	383	383	383

Notes: Each cell reports the estimated effect on a summary index from a separate regression based on the 2012 survey. Columns 1-3 present results using the original sample without entropy balancing. Columns 4-6 stem from the weighted sample adjusted by entropy balancing. Standard errors are presented in parentheses and are clustered at the maternal level; * p < 0.1, ** p < 0.05, *** p < 0.01.

† Signs of outcomes of schooling dropout and grade repetition are reversed when calculating the overall summary index.

†† Outcomes of Non-Cognitive II (absences and behavior in school) contains 193 missing values and therefore are not included in the overall summary index.

††† Signs of the outcome of school absence in report card are reversed in this summary index.

Table 13. Estimated Effects on Children's Outcomes by Gender, by Maternal Education and by Mothers' Employment Status (2012)

	Gender		Mothers' education		Mothers' pre-treatment employment status	
	Male	Female	Up to primary schooling	More than primary schooling	Not worked before	Worked before
	(1)	(2)	(3)	(4)	(5)	(6)
Tests scores summary index	0.012	0.378***	0.341***	–0.193	0.302**	-0.092
	(0.147)	(0.144)	(0.117)	(0.154)	(0.180)	(0.122)
	[3.146]*		[7.610]***		[3.173]*	
Schooling dropout and grade repetition summary index	–0.087	–0.256***	–0.073	-0.133*	-0.076	-0.225***
	(0.116)	(0.089)	(0.101)	(0.079)	(0.104)	(0.089)
	[1.332]		[0.219]		[1.177]	
Attitude towards schooling summary index	0.266***	0.263**	0.179	0.097	0.159	0.152
	(0.103)	(0.123)	(0.133)	(0.167)	(0.130)	(0.116)
	[0.000]		[0.150]		[0.002]	
Absences and behavior in school summary index†	0.178	0.201	0.483***	–0.505***	0.214	0.308
	(0.232)	(0.239)	(0.131)	(0.167)	(0.168)	(0.201)
	[0.005]		[21.710]***		[0.131]	
Observations	192	191	207	176	185	198

Notes: Each cell reports the estimated aggregate effect on the summary index from a separate regression based on the 2012 survey. F-test (Chow-test) statistic for subgroup difference in treatment effects is presented in square brackets. Estimated results are based on the original sample without entropy balancing. Covariates of child characteristics, household demographics and household economics are included in regressions. Standard errors are presented in parentheses and are clustered at the maternal level; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

† Signs of the outcome of school absence in report card are reversed in this summary index.

Table 14: Correlations between Working Condition Before Treatment and Reasons Why Control Mothers Did Not Apply

	Worked before	Working now	Worked full-time before	Working full-time now
Worked before	1			
Working now	0.2108*	1		
Did not know about the program	-0.0440	-0.0952	0.1217	-0.0616
Did not have children	-0.1049	-0.0497	0.0803	0.0838
Distance	0.0833	-0.0292	-0.1444	-0.0694
Problems with the application	-0.0306	0.1407	-0.1118	0.0921
Mother was working	0.0777	0.1051	0.0803	0.2305*
Affiliation with another NGO	0.0277	-0.0193	0.0855	-0.0405
Did not know the age limits	0.0777	-0.0952	-0.2008	-0.0434
The child was over age	-0.0519	0.0101	0.1145	0.0509

Notes: Each cell reports the correlation coefficient based on the 2012 survey; * $p < 0.05$.

Table 15: Estimated Effects on Mothers' Outcomes: Summary Indices: Matching Estimates for the Full Sample (Exercise 3)

	Kernel matching via propensity score	NN matching via propensity score	Bias-corrected matching
	(1)	(2)	(3)
B: Sample based on 2012 survey			
Labor market outcomes	0.383*	0.498*	0.511*
	(0.076)	(0.086)	(0.073)
Economic and social independence	0.299*	0.352*	0.334*
	(0.080)	(0.085)	(0.077)
Intra-household decision-making	0.089*	0.108*	0.113*
	(0.053)	(0.054)	(0.054)
Child investment	0.131*	0.233*	0.203*
	(0.065)	(0.070)	(0.078)
Self-esteem and Big Five Personality Traits	0.066	0.043	0.1
	(0.057)	(0.064)	(0.065)
Observations	281	281	281
C: Pooled sample of 2012 and 2013			
Labor market outcomes	0.423*	0.516*	0.555*
	(0.062)	(0.074)	(0.068)
Economic and social independence	0.198*	0.311*	0.283*
	(0.056)	(0.060)	(0.057)
Intra-household Decision-making	0.082*	0.113*	0.109*
	(0.032)	(0.033)	(0.033)
Observations	496	496	496
Child Controls	Yes	Yes	Yes
Household Demographics	Yes	Yes	Yes
Household Economics	Yes	Yes	Yes

Notes: Each cell reports the estimated mean effect from a separate regression. Columns (1)-(3) are based on the matching on child controls, household demographics, and household economics. Standard errors are presented in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 16: Estimated Effects on Mothers' Outcomes by Pre-Treatment Outcomes Heterogeneity (2012) – *Kernel Matching using full sample (Exercise 3)*

	Mothers' pre-treatment employment status		Mothers' education		Mothers' pre-treatment role in decision-making	
	Not worked before	Worked before	Up to primary schooling	More than primary schooling	Below average	Above average
	(1)	(2)	(3)	(4)	(5)	(6)
Labor market outcomes Index	0.651*	0.344*	0.444*	0.844*	0.704*	0.450*
	(0.135)	(0.113)	(0.111)	(0.152)	(0.120)	(0.126)
Economic and social independence Index	0.351***	0.484***	0.430*	0.412*	0.477*	0.264*
	(0.120)	(0.095)	(0.091)	(0.197)	(0.158)	(0.107)
Household decisions-making index	0.265*	0.006	0.082	0.1	0.137	-0.053
	(0.102)	(0.073)	(0.075)	(0.157)	(0.096)	(0.096)
Child investment Index	0.189	0.295*	0.325*	-0.029	0.227*	0.099
	(0.185)	(0.079)	(0.109)	(0.109)	(0.112)	(0.120)
Observations	133	148	144	136	93	179

Notes: Each cell reports the estimated aggregate effect on a summary index from a separate regression based on the 2012 survey. For all matching estimators, we perform matching on child controls, household demographics, and household economics. Estimated results are based on the original sample without entropy balancing. Standard errors are presented in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 17: Estimated Effects on Children's Outcomes: Summary Indices (2012)

	Kernel matching via propensity score	NN matching via propensity score	Bias-corrected matching
	(1)	(2)	(3)
Overall summary index†,††	0.168*	0.173*	0.178*
	(0.046)	(0.053)	(0.052)
Tests scores	0.091	0.107	0.142
	(0.090)	(0.099)	(0.098)
Schooling dropout and grade repetition	-0.187*	-0.208*	-0.136*
	(0.064)	(0.069)	(0.063)
Attitude towards schooling	0.226*	0.203*	0.255*
	(0.084)	(0.104)	(0.087)
Absences and behavior in school†††	0.041	0.139	0.154
	(0.113)	(0.142)	(0.122)
Child Controls	Yes	Yes	Yes
Household Demographics	Yes	Yes	Yes
Household Economics	Yes	Yes	Yes
Observations	383	383	383

Notes: Each cell reports the estimated effect on a summary index from a separate regression based on the 2012 survey. For all matching estimators, we perform matching on child controls, household demographics, and household economics. Standard errors are presented in parentheses and are clustered at the maternal level; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

† Signs of outcomes of schooling dropout and grade repetition are reversed when calculating the overall summary index.

†† Outcomes of Non-Cognitive II contains 193 missing values and therefore are not included in the overall summary index.

††† Signs of the outcome of school absence in report card are reversed in this summary index.

Table 18: Estimated Effects on Mothers' Outcomes: Summary Indices: using the sample of families which have both primary school children and preschool children

	Bias-corrected matching		NN matching via propensity score	
	(1)	(2)	(3)	(4)
B: Sample based on 2012 survey				
Labor market outcomes	0.387*	0.404*	0.349*	0.265*
	(0.092)	(0.091)	(0.098)	(0.092)
Economic and social independence	0.324*	0.313*	0.321*	0.273*
	(0.080)	(0.078)	(0.076)	(0.079)
Intra-household decision-making	0.003	0.06	0.027	0.031
	(0.056)	(0.060)	(0.059)	(0.056)
Child investment	0.161*	0.153*	0.145*	0.152*
	(0.072)	(0.070)	(0.074)	(0.075)
Self-esteem and Big Five Personality Traits	0.025	-0.025	0.049	-0.036
	(0.063)	(0.063)	(0.064)	(0.066)
Observations	200	200	200	200
C: Pooled sample of 2012 and 2013				
Labor market outcomes	0.417*	0.486*	0.335*	0.241*
	(0.074)	(0.076)	(0.078)	(0.082)
Economic and social independence	0.240*	0.271*	0.211*	0.177*
	(0.061)	(0.056)	(0.054)	(0.054)
Intra-household Decision-making	-0.059	-0.009	-0.024	0.024
	(0.037)	(0.037)	(0.027)	(0.026)
Observations	346	346	346	346
Mother age	Yes	Yes	Yes	Yes
Child age	Yes	Yes	Yes	Yes
Mother work full time	No	Yes	No	Yes

Notes: Each cell reports the estimated mean effect from a separate regression. Columns (1) and (3) present results based on the matching on mother age, and child age. Columns (2) and (4) are based on the matching on mother age, child age, and whether mother work full time. For the bias-corrected matching perform exact matching on whether mother work full time, and nearest neighbor matching for mother age, and child age. Standard errors are presented in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 19: Estimated Effects on Children's Outcomes: Summary Indices (2012)

	Bias-corrected matching		NN matching via propensity score	
	(1)	(2)	(3)	(4)
Overall summary index†,††	0.161*	0.164*	0.163*	0.146*
	(0.054)	(0.053)	(0.055)	(0.052)
Tests scores	0.092	0.017	0.088	0.087
	(0.109)	(0.108)	(0.111)	(0.103)
Schooling dropout and grade repetition	-0.179*	-0.211*	-0.234*	-0.136*
	(0.073)	(0.067)	(0.067)	(0.068)
Attitude towards schooling	0.211*	0.264*	0.168*	0.214*
	(0.095)	(0.095)	(0.100)	(0.097)
Absences and behavior in school†††	0.274*	0.270*	0.270*	0.228
	(0.120)	(0.126)	(0.122)	(0.139)
Mother age	Yes	Yes	Yes	Yes
Child age	Yes	Yes	Yes	Yes
Mother work full time	No	Yes	No	Yes
Observations	272	272	272	272

Notes: Each cell reports the estimated effect on a summary index from a separate regression based on the 2012 survey. Columns (1) and (3) present results based on the matching on mother age, and child age. Columns (2) and (4) are based on the matching on mother age, child age, and whether mother work full time. For the bias-corrected matching perform exact matching on whether mother work full time, and nearest neighbor matching for mother age, and child age. Standard errors are presented in parentheses and are clustered at the maternal level; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

† Signs of outcomes of schooling dropout and grade repetition are reversed when calculating the overall summary index.

†† Outcomes of Non-Cognitive II contains 193 missing values and therefore are not included in the overall summary index.

††† Signs of the outcome of school absence in report card are reversed in this summary index.

Table 20. Characteristics and Pre-program Outcomes of the Usable Sample and the Sample of Mothers/Children who Enrolled between 2005 and 2009 and All Children were in the School-*PelCa* when They Left

	Attrition mean	Treatment mean	Control means	Difference in means (1)-(2)	Difference in means (1)-(3)
	(1)	(2)	(3)	(4)	(5)
Mother age when enrolled	26.321	26.185	---	0.136	---
				(1.291)	
Number of children in 2005	1.974	1.849	1.632	0.125	0.342
				(0.211)	(0.237)
Mother lived together with partner	0.781	0.801	0.817	-0.020	-0.036
				(0.078)	(0.079)
Mother worked	0.493	0.470	0.609	0.023	-0.116
				(0.071)	(0.075)
Highest educational level: primary school	0.676	0.687	0.678	-0.010	-0.002
				(0.067)	(0.072)
Highest educational level: secondary school	0.250	0.169	0.165	0.081	0.085
				(0.057)	(0.061)
Highest educational level: started university	0.015	0.024	0.009	-0.009	0.006
				(0.021)	(0.016)
Family lived in Pisulli	0.579	0.675	0.583	-0.096	-0.004
				(0.066)	(0.073)
Child age when enrolled	2.903	2.696	---	0.207	---
				(0.189)	
Observations of mothers	77	162	115		
Observations of children	108	219	164		

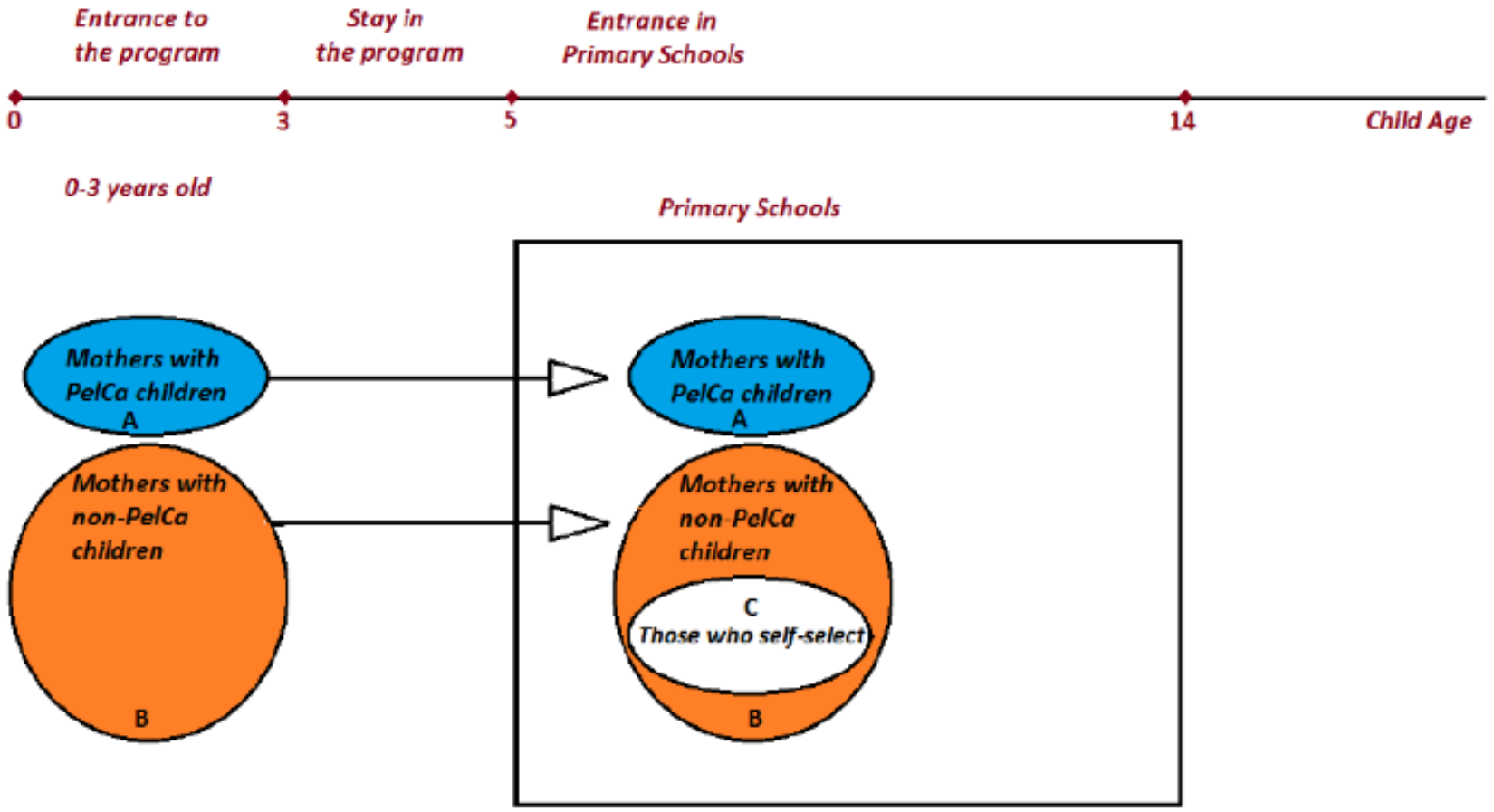
Notes: The column (1) is based on the sample of mothers/children who enrolled between 2005 and 2009 and all children were in the School-*PelCa* program when they left. Columns (2) and (3) are based on the 2012 survey. Standard errors are presented in parentheses in the column (4); * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 21. Effects on Children's Outcomes: Summary Indices (2012) – based on the treated children who are not the first-born child

	Not weighted			Weighted		
	(1)	(2)	(3)	(4)	(5)	(6)
Overall summary index†,††	0.170**	0.187**	0.198***	0.133	0.274***	0.284***
	(0.074)	(0.077)	(0.075)	(0.098)	(0.094)	(0.100)
Tests scores	0.064	0.126	0.121	0.018	0.144	0.14
	(0.113)	(0.110)	(0.115)	(0.160)	(0.116)	(0.117)
Schooling dropout and grade repetition	-0.172*	-0.136*	-0.139*	-0.085	-0.200*	-0.204*
	(0.088)	(0.078)	(0.077)	(0.098)	(0.107)	(0.113)
Attitude towards schooling	0.171*	0.081	0.094	0.215**	0.194*	0.190**
	(0.097)	(0.109)	(0.101)	(0.102)	(0.099)	(0.093)
Absences and behavior in school†††	0.208	0.329**	0.404***	0.154	0.312***	0.383***
	(0.135)	(0.128)	(0.128)	(0.196)	(0.109)	(0.108)
Child Controls	No	Yes	Yes	No	Yes	Yes
Household Demographics	No	Yes	Yes	No	Yes	Yes
Household Economics	No	No	Yes	No	No	Yes
Observations	298	298	298	298	298	298

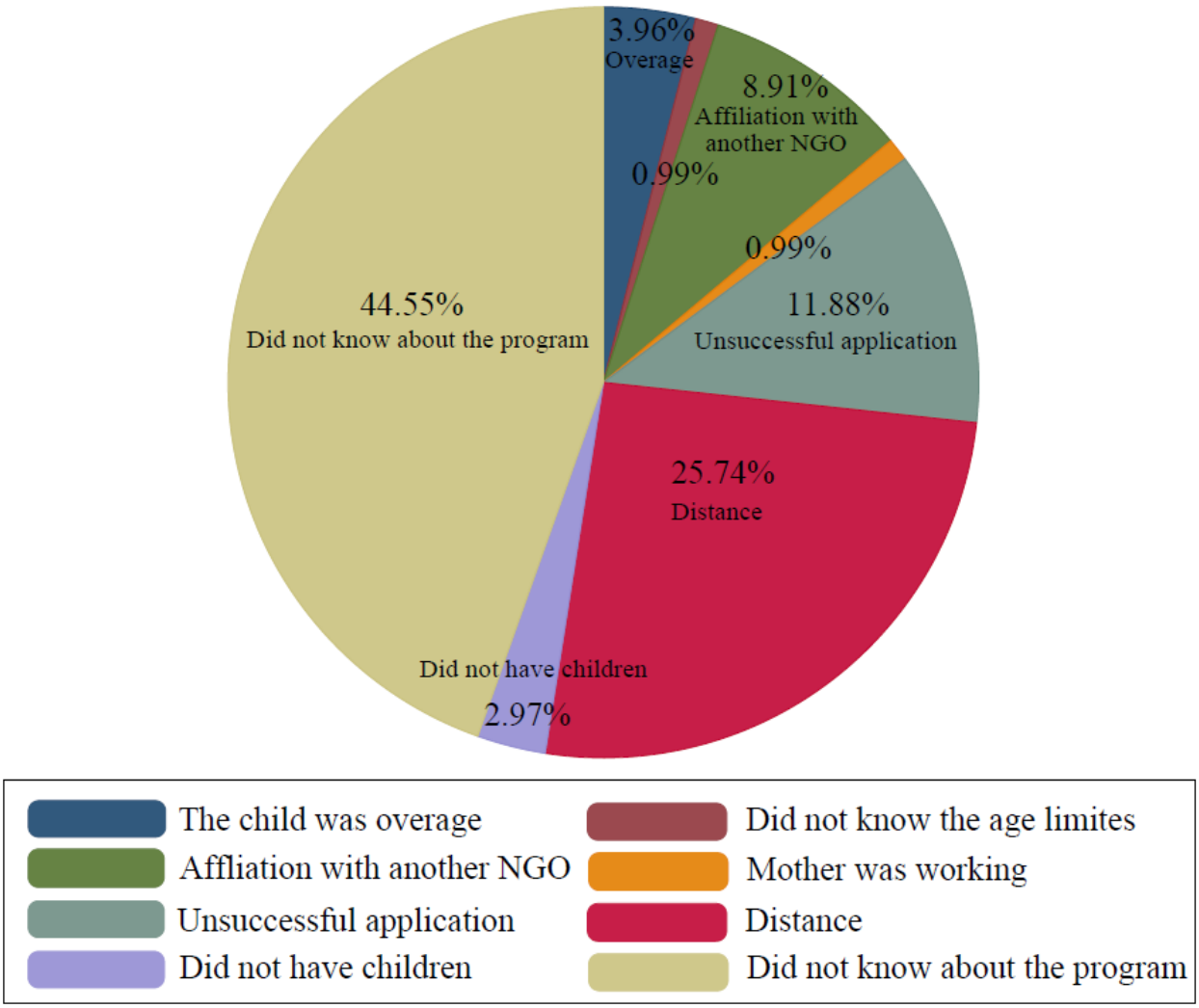
Notes: Each cell reports the estimated effect on a summary index from a separate regression based on the 2012 survey. Columns 1-3 present results using the original sample without entropy balancing. Columns 4-6 stem from the weighted sample adjusted by entropy balancing. Standard errors are presented in parentheses and are clustered at the maternal level; * p < 0.1, ** p < 0.05, *** p < 0.01.
† Signs of outcomes of schooling dropout and grade repetition are reversed when calculating the overall summary index.
†† Outcomes of Non-Cognitive II contains 193 missing values and therefore are not included in the overall summary index.
††† Signs of the outcome of school absence in report card are reversed in this summary index.

Figure 1: Identification Strategy



Notes: The figure above depicts the identification strategy. Group A in the blue ovals represents the treatment group: mothers who enter the *PelCa* program with their children when 0-3 years old and stay in the *PelCa* preschool program until their children are 5. Once children turn 5, they enter primary schools and at this point both mothers and children will be compared to the control group C. Group B in the orange ovals represent mothers who do not enter the *PelCa* program with their children 0-3 years old. However, once these children turn 5 and enter primary school, some of these mothers self-select themselves and approach the NGO to apply to the *PelCa* program with another younger child. The primary school children and mothers of this latter group, group C, become part of the control group and will be compared to group A.

Figure 2: Why control mothers did not enroll before in the program



Appendix: Construction of Control Variables

Household demographics controls

- *Mothers' and fathers' education* (separately): indicator variables (0/1) for whether primary schooling not completed, primary schooling completed, secondary schooling not completed, secondary schooling completed, and university schooling completed.
- *Mother age* and *Father age*.
- *Mother personal status*: indicator variables (0/1) for whether the mother was married, cohabitated, or single.
- *Mother origin*: the indicator variable (0/1) for whether the mother is from Quito.
- *Parents from same city*: the indicator variable (0/1).
- *Number of children in 2005*.

Household economics controls

- *Mother working before child enrolled in PelCa*: the indicator variable (0/1).
- *Father working before child enrolled in PelCa*: the indicator variable (0/1).
- *Mother's firm size* if employed: (0, 1, 3.5, 8, 15.5, 35.5, 75.5, 300.5).
- *Father's firm size* if employed: (0, 1, 3.5, 8, 15.5, 35.5, 75.5, 300.5).
- *Family average income*: before child enrollment in *PelCa* (0, 50, 200, 350, 450, 600, 800).

Child controls

- *Birth order* (1,…,10).
- *Child age* (5,…,14).
- *Number of young siblings in 2005* (0,…,3).
- *School fixed effects*