Preschool and Parental Response in a Second Best World:

Evidence from a School Construction Experiment[1]

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Abstract

Interventions targeting early childhood development hold promise for increasing human capital and reducing the intergenerational transmission of poverty. This paper presents results from a randomized evaluation of a preschool construction program in Cambodia, and suggests caution. The overall impact of the program on early childhood outcomes was small and statistically insignificant. For the cohort with highest program exposure, the impact on cognitive indicators was negative; with the largest negative effects among children of poorer and less educated parents. The results are consistent with frequent underage enrollment in primary school in the absence of preschools, stricter enforcement of the minimum age for primary school entry after the intervention, substitution between primary and preschool following intervention, and difference in demand responses to the new preschools between more and less educated parents. The results show that context, program specifics, and behavioral responses, can potentially lead to perverse effects of well-intentioned interventions.

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1. INTRODUCTION

At least 200 million children in low- and lower-middle income countries fail to achieve their potential in terms of cognitive and overall development (Grantham-McGregor, et al. 2007). Cognitive development in early childhood is important in its own right and, in addition, low levels of cognitive development are often associated with inadequate school readiness, which possibly contributes to poor school performance. Low levels of cognitive development, amplified with poor school performance can undermine children's potential future economic success (Heckman 2008). Studies from low-, middle-, and high-income countries that track individuals from early childhood into adulthood show that children brought up in a more favorable early environment are healthier and taller, have higher cognitive ability and educational attainment, and earn significantly higher wages (Paxson and Schady 2010; Stith, Gorman and Choudhury 2003; Liddell and Rae 2001, Walker, et al. 2005, Gertler, et al. 2014, Havnes and Mogstad 2011). Neuro-scientific evidence confirms that early childhood is a critical step in human development: it is the period in which the development of the synapse, the connections between neurons and the child's ability to absorb new sounds and languages occurs (Shonkoff and Philips 2000). Consequently, policymakers in many countries are increasingly seeing early childhood as a particularly promising period to target when trying to address socio-economic gaps in human capital development.

Yet little is known about parental responses to the introduction of new early childhood programs in low-income countries. Whether and how the availability of new programs, such as preschools, translates into better cognitive and socio-emotional outcomes for young children will depend on parental behavior responses. Parents may be unwilling to leave the child with an unknown adult at early ages, not appreciate the value of preschool for early childhood development, or believe that socio-emotional development is better carried out in

the home.[2] If such beliefs are positively correlated with lower levels and quality of parental investment, children who need pre-school the most may not be the ones that are sent to preschool (Blau and Currie 2006).

This paper studies parental response to a preschool construction program in Cambodia, as well as its impact on a wide set of early childhood outcomes. We rely on an experimental design to evaluate the impacts of a relatively large-scale intervention. The program, implemented by the government of Cambodia (partly funded by a grant from the Education Fast Track Initiative administered by the World Bank), involved the construction of preschool classrooms within the primary schools of poor rural villages. This was accompanied by training, deployment, and supervision of new preschool teachers and the provision of materials. The new preschools were integrated within the regular Cambodian public educational system. We therefore study the short-term impacts of increased access to government preschools on both cognitive and non-cognitive outcomes of 4 to 6 year old children. Importantly, in Cambodia, the counterfactual to preschool was not only parental care at home, but also informal underage enrollment in primary school. Such underage enrollment in primary school is not unique to Cambodia, but a phenomenon widely observed in the developing world (O'Gara, 2013). This paper hence estimates the effect of a pre-school program in a context where the intervention might trigger reallocation between preschool, primary school and parental care at home.

The results suggest limited overall impacts on child development. Poor implementation led to limited exposure time and to poor service quality. Parental response led to substitution between underage enrollment in primary school and preschool enrollment for some children, and for others it led to withdrawal from any formal education. We find negative impacts on

[2] There is a related literature on the effects of "redshirting" (delayed entry into formal schooling) in developed countries (Black, Devereux and Salvanes 2011).

cognitive development for five year olds, and these negative effects were the largest for children from less educated and poorer parents. These results indicate that the design of preschool interventions should start with a good understanding of parental and teacher decision-making. More generally, they show how implementation and behavioral responses might not only limit positive impacts, but could even lead to perverse impacts of such interventions.

These results resonate with some of the literature on preschools in high-income countries. Baker, Gruber and Milligan (2008) find negative impacts of subsidized childcare on children's non-cognitive outcomes and interpret this in light of the quality of parenting at home. And Cornelissen et al. (2016) show that for a vast preschool extension program in Germany families most likely to send their children to preschool are from higher socio-economic background whose children have the lowest return to preschool.

The rest of the paper is organized as follows: Section 2 further reviews the literature; section 3 describes the intervention, the experimental design and the data; section 4 discusses implementation and take-up of the program; section 5 presents the empirical strategy and the main impact evaluation results, including robustness checks; Section 6 interprets and discusses the findings; Section 7 concludes.

2. LITERATURE REVIEW

In the United States, most of the observable cognitive gap between wealthier and poorer children is already present before children enter school, and early cognitive and non-cognitive traits are strong predictors of success in term of subsequent school attainment, economic status (Chetty, et al. 2011), criminality (Currie, 2001) and social behavior (Heckman, Stixrud and Urzua 2006). Similarly, in many developing countries, there are steep

socioeconomic gradients in early childhood cognitive development—children from poorer households show significantly worse outcomes that often grow with age (Halpern, et al. 1996, Ghuman, et al. 2005, Grantham-McGregor, et al. 2007, Fernald, et al. 2011, Naudeau, et al. 2011, Schady, et al. 2015). The accumulating evidence further suggests that early stimulation, even after an initial period of fade-out, may trigger large long-term effects. Long-term effects of education-related interventions during early childhood have been found, for instance, for large class-size reduction programs (Chetty, et al. 2011), formalized preschool intervention (Schweinhart, et al. 2005) or early social skill training (Algan, et al. 2014). Such findings motivate dynamic skill formation models (Cunha, Heckman and Schennach 2010) that also point to early investment as the most effective in reducing gaps in cognitive attainment. Based on these findings, policies targeting early childhood development are often believed to be cost-effective and inequality-reducing interventions. Promoting cognitive and overall development among disadvantaged children from early on is expected to provide a better base for learning in primary school and in later stages of life and, as such, to help break the intergenerational transmission of poverty.

Preschool interventions for children in the 3 to 5 age group are often assumed to hold considerable promise to achieve those goals. Compared to parental care at home, preschools are thought to better prepare children for a more structured primary school environment, and interactions with professional teachers and with peers are often thought to increase both cognitive and non-cognitive outcomes—especially for disadvantaged children whose low-educated parents might not be able to provide similar stimulation at home. Duncan and Magnuson (2013) review the evidence for the US and conclude that impacts are mostly in line with these expectations. They also note, however, that the results from programs implemented for large and representative populations are generally much smaller than those

found for small-scale pilot programs.[3] Leak et al. (2010), in a meta-analysis covering 117 preschool programs in the US, find that a quarter of them have negative effect sizes, and that there are no significant differences in impacts between programs lasting less than 6 months, when compared to longer durations (Leak et al. 2010).[4]

Evidence regarding preschool interventions in low- and middle-income countries is generally positive, though mostly based on non-experimental designs. In Uruguay, the expansion of the provision of preschool education led to significant and positive effect of pre-primary education on school attainment via a reduction in drop-outs (Berlinski, Galiani and Manacorda, 2008). In Argentina, Berlinski, Galiani and Gertler (2009) found significant effects on school competencies three years after children participated in a pre-primary school class. In Bolivia, Behrman, Cheng and Todd (2004) show cognitive and psychosocial effects of a preschool program (with health and psychological components) on children aged between 6 to 59 months, with no effects found before seven months of treatment exposure. Non-experimental evidence from Cambodia also suggests positive impacts of preschool programs on child development (Rao et. al. 2012)—a result that contrasts with the findings we present here (we return to the reasons for this difference in section 5). To the best of our knowledge, Martinez, Naudeau, and Pereira (2012) is the only large-scale experimental evaluation of preschool versus no preschool availability in a low-income country. They show positive impacts on the cognition, subsequent school participation, and socio-emotional development of children participating in an NGO-implemented program combining preschool

[3] Some of the best known experimental evidence such as Abecedarian, Perry Preschool, or the "Milwaukee Project" that show large impacts (Currie 2001) were pilot programs with small number of participants (Perry preschool impacts were evaluated on 123 observations, Abecedarian on 111 observations).

[4] Duncan and Magnuson (2013, online appendix), show that even short summer programs can have positive impacts, suggesting that the short duration does not automatically translate in lack of impacts.

and a parenting intervention in rural Mozambique.

Evidence from other types of early childhood programs in low- and middle-income countries suggests that there are potentially large impacts of improving parental investments early in life. Most notably, evidence from a randomized control trial in Jamaica points to strong and lasting impacts of early childhood stimulation, both in the short-term (Grantham-McGregor, et al. 1991) as well as the long-term—impacts of the early psychosocial stimulation were still detectable on risky behaviors, criminality, IQ tests and labor market outcomes when recipients were 22 years old (Grantham-McGregor, et al. 2007, Gertler, et al. 2014). Positive results on cognitive and non-cognitive outcomes were also found for similar stimulation programs aimed at changing parental caregiving practices at home in Colombia (Attanasio, et al. 2014), Nicaragua (Macours, et al. 2012), Bangladesh (Nahar, et al. 2009, Aboud and Akhter 2011), India (Bentley, et al. 2010), Chile (Lozoff, et al. 2010), and Pakistan (Gowani, et al. 2014). Most of these interventions were implemented at small scale and combined early stimulation with nutrition-specific interventions. While the stimulation intervention consistently benefited child development, little evidence was found of synergistic interaction between nutrition and stimulation on child development outcomes (Grantham-McGregor, et al. 2014). Finally cash transfer programs—often large scale and with conditions targeting parental decisions on health practices and nutrition—have led to significant improvements in health and nutrition outcomes in Mexico (Gertler 2004) and the Philippines (Kandpal, et al. 2016) and cognitive development in Ecuador (Paxson and Schady 2010) and Nicaragua (Macours, Schady and Vakis, 2012; Barham, Macours and Maluccio 2013).

In sum, the existing evidence confirms the "proof of concept" that early childhood interventions, including preschools, can have positive impacts. However, much of this evidence comes from small-scale programs with committed implementing partners and with non-representative samples, which potentially affects their external validity. Recent evidence

from other (non-ECD) education programs shows that impacts during scale-up by government agencies can be quite different from those obtained in NGO projects (Bold, et al. 2013). So far, there is very little evidence regarding larger-scale educational system interventions specifically targeting early childhood cognitive development and school readiness from low-income countries.

3. THE PRESCHOOL PROGRAM, EXPERIMENTAL DESIGN AND DATA

Cambodia's Ministry of Education Youth and Sports started a large-scale effort to increase preschool availability in 2009. The goal was to increase access to preschools through the construction of a preschool classroom within newly renovated primary schools in 138 villages situated in disadvantaged rural areas. Before the intervention, preschool attendance at the national level was only 12% (Rao and Pearson 2007). The scale of the intervention and the fact that it was implemented by the government (as opposed to a dedicated NGO) makes this an interesting setting for an evaluation with potentially high external validity.

The newly built preschool classroom was open to children between 3 and 5 years old. In practice, 5 year olds were prioritized for enrollment, reflecting the program's goal to increase subsequent enrollment in primary school and children's adjustment to the formal school system (the official age for entry into the first grade of primary school is 6 years old). In addition to construction and the provision of teaching materials (books, tables, etc…) the program included provisions for preschool teacher recruitment, training, salaries and supervision. Parents were expected to be responsible for the purchase of additional learning materials such as a pen, a pencil, chalk, a slate board and a notebook.[5] The preschools' activities included singing, drawing (mixing colors, reproducing signs/geographic figures on

[5] Children are expected to wear a uniform but this rule is rarely enforced.

a board or with small sticks), physical activities (such as gymnastic and games), some vocabulary (listing words), and counting. The curriculum was designed for the 3 to 5 age group and did not explicitly aim to teach writing or reading. Teachers often organized social games in which children had to recall the name of other students, and add or subtract them from a group of pupils.

The preschool program was integrated into a primary school rehabilitation effort and, therefore, targeted villages with a primary school that needed upgrading at the start of the intervention. This upgrading typically involved building additional classrooms so that the school would be able to cover from preschool through to grade 6 of primary school; sometimes it involved construction of an entirely new school building. The fact that the new preschool classrooms were established in conjunction with other construction may have had effects on primary school outcomes (for example through class size effects or through access to schooling for older siblings). As these effects are potentially most relevant for the oldest cohort in our sample we return to this in point in the interpretation of the findings.

Among villages eligible for preschool construction, 26 villages were randomly selected to receive a preschool in the first school year of implementation (2009/10), while 19 villages were randomly selected as control.[6] The 45 villages were selected in three large provinces, making it unlikely that the treatment had any impact on children in the control villages.[7] Baseline data were collected between December 2008 and February 2009 on a sample of

[6] All treatment and control villages were selected from a list of "eligible" villages. In villages selected as control, school construction and rehabilitation would commence 2 years after it had been carried out in treatment villages.

[7] The program was implemented in 5 provinces, but the impact evaluation focused on the three provinces with the largest number of villages. While the provinces were not explicitly selected to be representative, the selected provinces are not atypical of Cambodian provinces, and baseline education and preschool indicators are similar as in all Cambodian provinces combined.

children, ranging from 24 to 59 months old, sampled for their eligibility for preschool exposure during the planned program implementation period (Figure 1). Follow-up data on the same sample were collected between June and August 2011. The quantitative data were complemented by qualitative data collected after program implementation (May 2012) to increase understanding of the preliminary results of the evaluation, focusing on issues of program implementation, potential reasons for low program take-up, as well as the content of the intervention itself.

At baseline, up to 40 households with at least one child aged between 24 and 59 months old were sampled in each village.[8] In total, 1399 households, and 1731 children, were surveyed. Information about the household and the children was collected from caregivers, and a series of child development tests was administered to all children between 36 and 59 months at baseline. As the tests were not age-appropriate for the youngest target cohort (children ages 24-35 months) at baseline, this cohort was only administered tests at follow-up. An adapted version of the Ages and Stages Questionnaire (ASQ)[9] was translated into Khmer and administered (with four different age-specific versions of the instrument, i.e., one for every 6-month age interval) to capture child development outcomes across various domains, including fine motor, gross motor, cognitive development (communication and problem solving), and social competencies. Results from this instrument are based partly on responses given or behaviors demonstrated by the child (i.e. for questions where a specific child

[8] When more than 40 target households were present in the village, a random sample was drawn based on a complete list of households with children in the targeted age group obtained from the village leader. With the conventional power level (80%) and significance level (5%), and the intra-cluster correlation of 0.043 for the Woodcock-Johnson test at baseline, this would have given a MDE of 0.18 standard deviation with full compliance and using a set of control variables.

[9] Ages & Stages Questionnaires® (ASQ), Second Edition: A Parent-Completed, Child-Monitoring System, by Diane Bricker and Jane Squires. Copyright © 1999 by Paul H. Brookes Publishing Co., Inc. www.agesandstages.com. Used with permission of the publisher.

response or behavior could be elicited in the context of the household visit), partly by those given by the caregiver. In addition, a translated Khmer version of the TVIP (*Test de Vocabulario en Imagenes Peabody)* was administered. The TVIP is a version of the Peabody Picture Vocabulary Test (PPVT) adapted and normalized for populations in low-income settings. It measures early receptive vocabulary acquisition and is often considered a good indicator of early cognitive development.[10] Finally a translated version of the Woodcock Johnson (WJ) associative (short-term) memory test was also administered. In the follow-up, all instruments were re-administered to children, with the exception of the social competencies of the ASQ. Instead, the Strength and Difficulty test (SDQ), a test of a socio-emotional competencies based on parental response, was added at follow-up. The SDQ provides a measure of children's potential problematic behavior (emotional, hyperactivity, conduct, peer relationships) and of their pro-social skills. [11]

Both rounds of data also include one cognitive test for the caregivers, the Raven Progressive Matrices test (a test of non-verbal reasoning ability), and a parental involvement score, based on parents' responses to eight questions regarding engagement in education and cognitive development of their child.[12] The household survey further includes questions regarding the

[10] While the original version of the TVIP was standardized for low-income populations in Mexico and Puerto Rico, the version used in the Cambodia context was translated into Khmer and was piloted and validated prior to baseline data collection with the support of key informants. Only raw TVIP scores (interpreted as the number of words correctly recognized by a child until a test is suspended) are discussed in this paper instead of externally standardized TVIP scores, as the later would explicitly benchmark the scores of children in the sample to the score obtained by children obtained in the reference sample to norm the test in Mexico and Puerto Rico, which would not necessarily be appropriate.

[11] In addition, the ASQ was administered to the younger siblings (age 36 to 59 months) of the target children at follow up. For all children in primary school, the EDI (Early Development Instrument) was also collected through observations in each primary school. The latter is not analyzed in this paper as data cannot be merged with the survey data.

[12] Parents were asked how often they read a book to the child, tell a story, sing songs, talk to, play games with numbers, play games with words, play active games, and teach to become self-sufficient. Possible answers are often, sometimes or rarely. The average score is

households' economic situation, medical care, education background, and parental behavior. Separately, data on schools and villages were collected through interviews with the school director and the village leader.

Column 2 of Table 1 reports baseline characteristics of the sample. The children in our study are substantially disadvantaged: 54% of the children are stunted (height-for-age less than 2 standard deviations below the WHO standard) and 17% are severely stunted (lower than 3 standard deviations). In Cambodia as a whole, 45% of children were stunted and 16% were severely stunted (National Institute of Statistics, Directorate General for Health, and ICF Macro, 2011). Sample children live in relatively large families (5.7 members on average) whose income mostly comes from subsistence farming (average income from paid work is small). Less than half of the caregivers are literate. At baseline, 6.4% of the targeted children were attending any formal school, with 2.3% in preschool and 4.1% in primary school.

Columns 3 and 4 of Table 1 report the test result of the balance between treatment and control villages. In the full sample only one of the baseline characteristics (out of 29) is statistically significantly different between treatment and control groups, consistent with pure chance and therefore confirming that randomization produced comparable groups. For the "5-year old cohort" only 2 out of 29 variables (gender and mothers' height) are significantly different at the 10% level. As these three variables could influence the outcomes of interest, they will be controlled for throughout the analysis. A similar conclusion is reached when looking at other age cohorts (not displayed).

The attrition rate across survey rounds is modest, and is not significantly different between

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calculated by assigning a score of 1, 2 or 3 to those possible answers. Results are similar when using alternative aggregation methods.

treatment and control groups (10.8% for treatment and 10.4% for controls).[13] As attrition could lead to selection concerns, we follow (Fitzgerald, Gottschalk and Moffit, 1998), and first analyze the correlates of attrition by estimating:

$$A_{i1} = \gamma T_i + \beta y_{i0} + \delta(y_{i0} * T_i) + \varepsilon_i \qquad (1)$$

where y_{i0} is a baseline indicator (e.g. a baseline test score, or a child or household characteristic), T_i the treatment assignment of the village of child i, A_{i1} is a dummy variable indicating whether the child i was missing at follow-up, and $y_{i0} * T_i$ is the interaction between the baseline indicator and treatment. The coefficient β gives the baseline characteristics' association with attrition in the control group while δ captures the attrition differential between treatment and control groups. As discussed below we are particularly concerned with the effects of the program on the "5 year-old" cohort which was most affected by the program, and therefore present this analysis for the sample as a whole as well as for just this subsample. Table 2 presents resulting coefficient estimates. In the full sample, only one coefficient (mother's height) points to differential attrition, while on the 5 year old sample, both mother's height and child height-for-age, as well as gender and income suggests differential attrition that could induce selection bias in the ITT estimates. All other estimates of interest do not appear to have been affected by attrition. The estimates suggest that attrited children in treatment villages had slightly lower height-for-age, and have mothers with lower height, than those in the control. Given that height-for-age at baseline is a proxy for the child's development at baseline, we add it to the standard set of controls, reducing any potential bias resulting from this differential attrition.

3.1. PRESCHOOL CONSTRUCTION COMPLIANCE

[13] In a regression predicting attrition on the basis of treatment status, the coefficient on treatment is -0.004 with a standard error 0.026. The corresponding estimate for the 5 year old cohort is -0.012, standard error with a standard error of 0.027.

Administrative records of school construction show that compliance with the experimental design was imperfect: school upgrading occurred in two out of the 19 control villages (Table 3). Moreover, while construction in treatment villages should have started in early 2009, the first preschools only opened in January 2010, and most of them were only open to students in October 2010—the beginning of the 2010/11 school year. By June 2011, seven preschools in the treatment arm of the study were not finished. Information gathered from school directors and village chiefs paint a similar picture: directors in 5 out of 26 treatment villages report not having a functioning preschool by June 2011.

As most schools opened for school entry in 2010/11, the follow-up data capture the impact of the program after only one school year (7 months; see Figure 1). The delays also meant that preschool exposure of the oldest cohort (48-59 months at baseline) was limited, since all children 70 months or older in October 2010 were expected to enroll in primary school for that school year.

The delays and, in some villages, the incomplete preschool construction have implications for the analysis of impacts on tests scores, as the limited exposure duration reduces the likelihood of measuring impacts (Behrman and King 2009).[14] That said, they also point to an important first lesson of this study, namely the implementation difficulties that can hamper the effectiveness of a program at scale such as this one (at least over the relatively short period discussed in this paper).[15]

[14] The policy literature typically suggests 7 to 9 months as the minimum exposure needed to yield positive results (Naudeau et al, 2010). These recommendations are supported by findings from Loeb et al. (2007) in the US and Behrman, Cheng and Todd (2004) in Bolivia. Human capital production function models such as Cunha, Heckman and Schennach (2007) also predict that early skills help accumulate future skills, and longer programs hence could be expected to yield larger positive and significant results.

[15] In parallel to this RCT, the government also implemented two types of new informal preschools (home based program and community based preschool) in other regions of the

3.2. PRESCHOOL AND PRIMARY SCHOOL ATTENDANCE

Table 4 presents several measures of preschool and primary school attendance at follow-up as reported by children's caregivers. Children in the treatment group are significantly more likely to have participated in preschool, with a treatment-control difference of 25 percentage points. Consistent with the priority given to 5 year olds, the impacts in preschool attendance are highest for this group (32 percentage points). The relatively small differences between treatment and control are largely driven by low program take-up and, to a lesser extent, by some reported preschool enrollment in the control (10.6%).[16] Qualitative fieldwork indicated that low take-up was not driven by capacity constraints. There was no enforced maximum limit of children per preschool and school directors never reported refusing a child because of capacity constraints. This is further confirmed by quantitative data: asked for the reasons their child was not going to preschool, none of the parents mentioned their application being turned down.[17] Monitoring data from the preschools indicate that class size varies from 20 to 51 children, with an average of 29 children. There are no threshold or spikes in the distribution that would suggest a maximum class size rule.[18]

Take-up was also not driven by the availability of alternative early childhood programs.[19] There are no significant differences across treatment and control villages in participation in

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country. A separate RCT was set up to evaluate their impact, but implementation issues were even larger than for the formal program, leading to even lower level of compliance and take up, and no results on early childhood outcomes (Bouguen, et al. 2013).

[16] The later is in part related to the construction that took place in control villages (2% of children), and mostly to children reported as attending a preschool despite there not being one in the village (8% of children)

[17] Only a handful of them declared that they were unable to register because they missed the deadline and 4 parents mentioned that they were too poor.

[18] Ideally, data on vacancies in the preschools could help to better understand take-up, but unfortunately such data is not available.

[19] As mentioned above, Home-based program and Community-Based Preschool were being set up at the same time by the government, but were targeted to other villages.

alternative early childhood programs, and overall participation in other programs is low (6% of children in treatment villages had attended community-based preschools; 15% had home-visits or community meetings).

The relative low take-up of preschool in the treatment villages suggests that there are constraints to preschool attendance that the school construction program did not address. Low parental demand for preschool could be explained by a lack of information, disinterest in preschool education or low perceived returns to preschool. Most parents not sending their child to school mentioned that their preschool aged child was too young to go to school (67% of answer). Other constraints suggested by parents in follow-up qualitative interviews included low quality of preschool (essentially low teacher quality or "overly disciplinarian" teachers) and parental time constraints preventing bringing or fetching children at preschool.

While the experimental design of this evaluation does not allow disentangling the importance of the various mechanisms, Table 5 shows the correlates of preschool attendance (in villages with a preschool) that are consistent with some of these reported constraints. The results (reported as odds-ratios based on logit estimations) suggest that higher socioeconomic status is associated with higher levels of preschool participation: household income (calculated as the average sum of salary earned in a week per household adult member), parents' educational attainment, mother's score on the Raven's test and scores on the parental involvement questions are significantly associated with higher attendance. Living in a dwelling with a thatch roof and the number of children per household are associated with lower preschool attendance. Overall, these findings point to inequalities in preschool enrollment that are related to households' socio-economic background.

The low exposure duration and low program take-up in treatment villages, along with non-compliance in program implementation, result in a very small difference in average exposure to preschool treatment between children in the treatment and control villages. Overall, and

considering all cohorts together, the difference in average exposure is about two months (Table 4). This will be important for the interpretation of the program impacts on cognitive and non-cognitive outcomes.

4. EMPIRICAL SPECIFICATION AND RESULTS

4.1. EMPIRICAL SPECIFICATION

To analyze the effect of the treatment on children's school attendance and cognitive and non-cognitive development we estimate a basic reduced form empirical model:

$$Y_i = \beta_0 + \beta_1 T_i + \beta_2 \boldsymbol{X}_i + \epsilon_i \quad (2)$$

with T_i indicating whether the village child i lives in was assigned to treatment, and $\boldsymbol{X}$ a vector of control variables—a full set of monthly baseline age dummies, a gender dummy and interactions, province fixed effects, and three additional baseline control variables (the number of children under 6 in the household, baseline height-for-age, and mother's height).[20] The main focus of our analysis is β_1, the intent-to-treat estimate, which reflects in part parental responses to the intervention, including the low take-up. This is the policy relevant variable as we study the overall impact of preschool construction, and argue that the behavioral responses are important to understand this overall impact.

For impacts on cognitive and non-cognitive test scores, we estimate the impact on each test separately, as well as the average effect across all tests following Kling and Liebman (2004). First, all scores are standardized using the standard deviation of the control group.[21] Then,

[20] In the main specification, no controls are added for the baseline test scores, as tests were not administered to the youngest age cohort at baseline. We introduce baseline test scores as extra controls in a robustness tests for the five-year old cohort.

[21] When necessary SDQ are reversed so that a positive result can be interpreted as a "better" outcome.

each score is regressed individually on the treatment variable using a Seemingly Unrelated Regression (SUR) model. Average impacts on all, or subsets, of the outcomes variables and standard errors of those averages (accounting for potential correlation between standard errors of individual estimates) can then be computed. We calculate the overall average, as well as subgroup averages by development domain: a "Motor development index" (gross motor and fine motor), the "Anthropometrics index" (height-for age and weight-for-age), the "Cognitive development index" (the TVIP, the Woodcock Johnson memory test and the cognitive competences of the ASQ, that is "problem solving and communication") and "Non cognitive development index" (combining the different sub-domains of the SDQ questionnaire).

Impacts are analyzed over the entire sample of children and also estimated separately for three age cohorts. We do so to reflect on the timing of the (delayed) intervention and the priority given to preschool enrollment of 5 year olds. The three cohorts are defined as follows: children who were at baseline between 24 and 35 months, between 36 months and 47 months, and between 48 months and 59 months. Since baseline data were collected between December 2008 and February 2009, children in the first cohort were between 46 and 57 months old at the beginning of the 2010/11 school year (the effective start date of the intervention). We will therefore refer to this group as the "four year-old" cohort. Children in the second cohort were between 58 and 69 months old at the start of the 2010/11 school year. We refer to them as the "five year-old" cohort. Finally, children in the third cohort were between 70 and 81 months at the start of the 2010/11 school year. We refer to them as the "six year-old" cohort.

We focus much of our analysis on the five year-old cohort: this is the cohort with the largest differential take-up, and therefore for whom we have most statistical power to detect impacts. It is also the group for whom underage enrollment in primary school is a particularly

important part of the counterfactual. Children from the six-year-old cohort were older than 70 months when the 2010/11 school year started—above the official primary school enrollment age. As a result, very few of them were supposed to be attending preschool. Nevertheless, this group includes a small proportion of students who were old enough to have benefited from one year of pre-school and who have since spent some time in primary school. Results for this group may in part reflect the longer-term impact of the program, though this of course is bundled with the direct impact of the primary school rehabilitation. Finally, while the difference in preschool exposure between treatment and control is smaller for the four year-old cohort, results for this group are illustrative, as underage enrollment in primary school is more limited for them.

4.2. RESULTS ON DELAYING ENTRY INTO PRIMARY SCHOOL

Table 4 shows that the program also had (unanticipated) effects on primary school participation: the percentage of children who were attending primary school during the year of the follow-up survey is lower by a statistically significant 10 percentage points in treatment villages. This decline is consistent with a corresponding increase in participation in preschool (25 percentage points). There is no significant effect of the program on ever attending any formal school" (that is, preschool or primary school) suggesting, on average, a substitution between preschool and primary school attendance.[22] Compliance by age group is illustrated in Figure 2 where the average attendance rates by age at follow-up for each school type and by treatment status is presented. The significant positive difference in preschool participation induced by the intervention (Panel 2) is offset by a significant negative

[22] In table 4 primary school attendance is measured "currently" (i.e. in the year of the follow-up survey) while any school and pre-school enrollment are also measured retrospectively ("ever attended"). To compare attendance, Figure 2 contrasts the probability of "ever attending" primary school, preschool and any school. In Figure 4 (see infra) we provide a similar comparison for enrollment during the year of the follow-up survey.

differential in primary school participation (Panel 1). Overall participation in any school is similar in treatment and control group (Panel 3).

Disaggregating by age suggests that the negative effect on primary school enrollment is largely driven by the five year-old cohort for whom participation in preschool is the highest: for this cohort the increased attendance to preschool (32 percentage points) is in large part compensated by a decrease in (current) primary school enrollment (by 21 percentage points). While this cohort was not technically eligible for primary school in October 2010 (for school year 2010/11), the share of control group children enrolled in the first grade of primary school is nevertheless very high: almost 60%. Indeed, many children attend primary school before reaching the official minimum age, and informal registration of underage children in grade 1 is common practice.[23] Figure 3 shows the density of the age of first enrollment in primary school by actual treatment status, which clearly shows a shift to the right (older ages) for the treatment group.[24] The mean age of entry increases from 68 months in villages without a preschool to 71 months old in villages with a preschool. We discuss the possible interpretations of this effect in section 5.

4.3. IMPACT OF PROGRAM ON AVERAGE TEST SCORES

The average substitution from primary school to preschool could *a priori* have a positive or negative effect on children's cognitive and non-cognitive development depending on the quality of teaching and learning in each environment, the age-appropriateness of the

[23] Qualitative interviews indicated that minimum age for formal registrations were relatively well enforced: parents have to hand in an official birth certificate or the family book containing birth dates. Nonetheless, a large share of pupils who attended classes were simply not registered, and this group indeed appeared to be younger.

[24] The graph relies on data regarding date of birth and age obtained from caregivers not from the school. When possible, the date of birth was verified using the birth certificates. Nonetheless, parents had no incentive to misreport the date of birth as data collection was unrelated to the enrollment process.

approaches used in each setting, and the socio-economic backgrounds of children going to preschool versus primary school. Table 6 shows the impact of the preschool program on the cognitive, motor and non-cognitive test scores for the full sample and suggests that, overall, there is very little evidence of any positive impacts. Out of the 13 impacts measured, two are statistically significant (at the 10% level, and only when additional covariates are included), and one of these is negative.[25] The impact is positive for the ASQ gross motor scale; the impact is negative and statistically significant—albeit small—for the ASQ problem solving subscale. Table 7 reports results derived when considering the outcomes grouped by domain. All point estimates for the family outcomes are negative but not statistically significantly different from zero, except maybe for the negative effect on the "Cognitive development index" which is close to the 10 % acceptance level.

Table 8 reports the corresponding results disaggregated by age cohort. The overall impact of the intervention on tests scores for the five year-old cohort is negative—a result driven by the impact on cognitive development, which is large (-0.19 standard deviations) and statistically significant. Impacts on the four year-old cohort, which can be interpreted as the pure effect of pre-school (since few of these children ever registered in primary school) are marginally significant on motor skills, suggesting a positive impact on this dimension for the youngest cohort.

Finally, impacts for the six year-old cohort are small and statistically insignificant. The vast majority of this age cohort is in primary school by the time of the follow survey. In the treatment villages, they were therefore exposed to the primary school rehabilitation and may have been exposed to better premises in first grade, more homogeneous grade 1 classrooms or reduced class-size. In addition, a subset of them had stronger exposure to preschool in the

[25] As expected because of the randomization, point estimates are similar with or without covariates but standard errors decrease when controls are added.

previous year. While the design does not allow disentangling the effects of these different exposures, the results show that there was no overall net short-term effect on any of the indicators for this cohort.[26]

4.4. ROBUSTNESS

Column 1 of the table in Annex B shows intent-to-treat results on the five year-old cohort for the main outcomes without any covariates, the following columns (2 to 4) progressively include additional covariates. In specification (5) we control for the corresponding baseline test score in addition to other covariates. Point estimates remain very similar and significant across all specifications, and as expected, precision increases as more covariates are added. In the last column (6), we include all variables listed in Table 2: while we lose some observations due to missing observations for caregiver and household head' education at baseline, point estimates remain unaffected.

Next, we assess the importance of outliers by estimating impacts after removing observations with extreme values of the outcome variable. The three panels of Annex C exclude observations with values above and below three, two, and one standard deviation, respectively, around the mean. Estimates remain qualitatively similar across the specifications, as well as in comparison to the full sample (compare to column 4 of the table in Annex C).

5. INTERPRETATION AND DISCUSSION

5.1. SUBSTITUTION EFFECT BETWEEN PRIMARY SCHOOL AND PRESCHOOL

[26] The primary school rehabilitation may also have impacted the older siblings of the sample children. We find notably a small but significant increase in the number of older siblings registered in grade 2-6 of primary school (+ 0.16 siblings). Unfortunately the data do not allow assessing the impact of such primary school participation increase on their performance. And there is no significant heterogeneity in the treatment effects for the main cohorts of interests depending on the presence of older primary-school age siblings in the household.

Interpreting the negative impacts in the five year-old cohort hinges on a good understanding of the counterfactual. Children from the five year-old cohort in the control group are either at home (where they might get early education from their parents) or attending primary school through informal underage enrollment. Introducing preschool into this context potentially crowds out those other forms of education. If those induced to attend preschool by the intervention are those who would have been in primary school (as underage enrollees) in the absence of the intervention, then our negative estimates on the five years old would capture the substitution effect between preschool and primary school (as an underage enrollee). And if those induced to attend preschool would have been at home in the absence of the program, then the estimates would capture the substitution of preschool for home.

At first glance the results in Table 4 may suggest that the former is more likely, as the increase in preschool enrollment is concomitant with a decrease in primary school enrollment. Figure 4 (left-hand panel) further illustrates that for the five year-old cohort as a whole, the preschool construction program is associated with a gain of around 20 percentage points in (current) preschool attendance (from 10 percent to 30 percent), and an equivalent decline in (current) primary school attendance—while the share of children not going to school remained constant (around 30% in both groups).

Yet, this interpretation depends on whether all the children enrolling in preschool are indeed the same children that would have enrolled in primary school in the absence of the program. In that scenario, the children not going to any school (preschool or primary school) should be fully unaffected by the preschool intervention. In the following section, we present evidence that this is not the full story and that the treatment has modified the composition of the schooled children.

5.2. COMPOSITION EFFECT

The composition effect is illustrated in the middle and right-hand panels of Figure 4 by showing school status at follow up (by group) for children with a literate mother (middle panel) and for those with an illiterate mother (right-hand panel). While overall enrollment in any type of school remains unchanged at around 30 percent (left-hand panel), for children with a literate mother (middle panel), the probability of being at any school increases substantially (almost 9 points): for that sub-group, the preschool "compliers" (those induced to school participation as a result of the intervention) appear therefore taken not only from the primary school students but also from the not schooled children. For the children with an illiterate mother preschool "compliers" seem to be coming from the group of primary school children, while the net effect of the intervention was a decrease in children who were in any formal school environment. The overall effect is hence a change of the composition of the schooled children, which is not consistent with a pure substitution effect between primary school and preschool.

To see whether these patterns hold in a more systematic way, we tested whether the impact of the treatment on enrollment in *any* school at follow-up (primary school or preschool) is significantly different depending on baseline socio-economic characteristics, for both the full sample, and in the 5 year-old cohort. The results confirm that children in any school at follow-up are disproportionally more from higher socio-economic backgrounds in the treatment than in the control.[27] While the data do not allow us to identify the exact reason for this sorting, it likely results in part from a stricter enforcement of the minimum-age rules for primary school enrollment in treatment villages. In the treatment group, the average primary school registration age is closer to the official age (70 months, see Figure 3). It would appear, therefore, that poorer families who can no longer register their children as underage enrollees

[27] Estimated with same controls as in equation (2). Results available upon request. See also the related unconditional results in Table 9.

in primary school in treatment villages are opting out of any formal school environment: They may lack information about how to register for preschool, they may have low demand for the newly established preschools, or they may not be able to overcome other registration requirements (despite the fact that preschool registration is free).

5.3. Interpreting the results in the Angrist, Imbens and Rubin (1996) framework

The findings that overall school take-up is similar in treatment and control but that the social and economic background of school attendees differs between treatment and control can be interpreted in the Angrist, Imbens and Rubin (1996) framework (AIR). Adopting their notation, let D be the observed treatment status, and Z be the binary indicator of random assignment.

$$Y_i = \beta_o + \beta_1 D_i + \varepsilon_i$$

$$D_i^* = \alpha_o + \alpha_1 Z_i + u_i \text{ with } D_i = 1 \text{ if } D_i^* > 0 \text{ and } D_i = 0 \text{ if } D_i^* \leq 0$$

$D_i(Z)$ is an indicator for whether child i would go to school given random assignment and we know that $D_i(Z) \neq Z_i$ (imperfect compliance). $Y_i(Z_i, D_i)$ is the potential outcome for child i given (Z_i, D_i).

As AIR point out it is useful to consider four potential cases. D_i (1) – D_i (0) = 0 for the never takers and the always takers, D_i (1) – D_i (0)= 1 for those that are induced to take the treatment because of the assignment, i.e. the compliers. And D_i (1) – D_i (0) = -1 for those that are induced by the treatment not to take the assignment, i.e. the defiers. Angrist (2004) further illustrates how the differences in unconditional estimates of D on Z between samples defined by covariates can characterize the distribution of covariates among compliers. We focus in particular on covariates that characterize the caregivers' socio-economic and educational background: whether the caregiver is literate, the family does not live under a

thatch roof, has paper and pen in house, the caregiver's Raven score, and the parental involvement score. We also construct an aggregate SES index averaging 13 proxy characteristics for socio-economic background.[28] For the scores and the index, we split the sample between those in the bottom quartile and the rest, to illustrate the differences in take-up between disadvantaged groups and others.

In our study, the intended treatment assignment was preschool participation. Following Angrist (2004) we estimate the unconditional potential first stage estimates of the relationship between random assignment and preschool participation. The first column of table 9 shows that preschool compliers are more likely to come from higher SES backgrounds, with parents that are significantly more likely to be literate, and less likely to live under a thatched roof. We can however not use these potential first stages to estimate the (preschool) LATE, as the exclusion restriction is likely violated, i.e. the random assignment affects children's outcome not just through preschool participation, it also induces some children to switch between primary school and homecare.

An alternative treatment assignment to consider would therefore be participation in any school (whether preschool or primary school). Table 9 therefore also shows the potential first stage estimates of the relationship between random assignment and any school participation for different subgroup with distinct baseline characteristics. This shows that on average, children are not more likely to go to school in treatment than in control (first row table 9 second column). But there is important heterogeneity, with children of literate caregivers more likely to go to school, and more children from low SES households less likely to go to

[28] Results are similar when using the first principal component instead (following Filmer and Pritchett 2001). The 13 characteristics include the five proxies included also separately in the table, as well as income per capita, household head's years of education, caregiver's years of education, whether the household head is literate, whether the household head has attended formal school, whether the caregiver has attended school, and the number of children below 6 in the household.

school in treatment than in control. These results suggest that there are indeed school-defiers in the sample, and that they are more likely to come from disadvantaged backgrounds.

5.4. HETEROGENEITY EFFECT ON PRESCHOOL RETURN AND INEQUALITY

We hence cannot estimate the LATE of either preschool or school participation due to likely violations of the key assumptions – exclusion restriction for preschool and monotonicity for school participation. But we can estimate the ITT for different subgroups and show how impacts differ depending on households' socio-economic background. We estimate the following model that interacts treatment with parent characteristics:

$$Y_{i1} = \beta_1 T_i + \beta_2 P_0 + \beta_3 T_i * P_0 + \beta_4 \boldsymbol{X}_i + \varepsilon_i \quad (4)$$

Where Y_{i1} are cognitive and non-cognitive follow-up test score indexes and P_0 is a baseline parental characteristic. The coefficient β_3 captures the differential effect of the program for the subgroup with the specific parental characteristic (over and above the effect of the program, and the relationship of the characteristic to the outcome).

Results from estimating equations (4) presented in Table 10 show important heterogeneous effect of the preschool program. Caregiver's literacy strongly interacts with treatment in determining child cognitive outcomes and similar results are found for having paper and pen in the house, and high baseline parenting score (column 2). In the last rows of the table, we interact the treatment with the average index of parental SES (as explained above), which confirms that the negative treatment effects on cognition are driven by children from low SES backgrounds. While heterogeneity of impacts on motor development and non-cognitive outcomes are possibly less consistent (column 3 and 4), the results for the overall development index (column 1) confirm that the treatment leads to negative outcomes for children from disadvantaged backgrounds, but not for the others.

For most variables, the negative effect estimated for the lower socio-economic group (β_1) is

approximately of the same magnitude than the point estimates for the higher socio-economic group and a test of $(\beta_1 + \beta_3)$ suggests that these are never significantly different from zero. Our interpretation of this finding is that children from educated and wealthier backgrounds who took-up the preschool program did not benefit from the amount of exposure they received (in terms of the child development outcomes measured), while the poorer children, who may have benefited from a school program, were less likely to be in a formal school environment and had hence lower outcomes.

All in all, while ECD interventions are often motivated based on their potential ability to reduce the gap in school readiness between children from poorer and wealthier backgrounds, we show that this did not hold for this particular program. The net effects of the various effects at play tended to both reduce the overall treatment effect size and increase the gap in early cognitive development.

5.5. OTHER POSSIBLE LIMITATIONS OF THE PRESCHOOL INTERVENTION

Beyond the substitution and composition effects discussed above, it is perhaps reasonable to question whether the quality of the preschool services offered may explain the lack of impact on outcomes. Indeed, we do not find that cognitive outcomes are improved as a result of the intervention among the four year-old cohort, for whom underage enrollment in primary school was less widespread. While we have limited quantitative information on quality, there are a number of elements worth considering.

The two school environments share a variety of features. First, by design, the preschools we evaluate have the same physical infrastructure as the comparison primary schools (since they are co-located). Second, preschool teachers were at least in part recruited from the same pool of applicants as primary school teachers, and their training and supervision were coordinated by the same institutions. Third, both preschool and primary school classes have a similar 3-

hour per day session.

But there are important differences as well. First, teacher quality may have been lower in preschool than in grade 1. Preschool teachers were newly recruited: They had less experience, and perhaps lower motivation or skills than their primary school counterparts. Qualitative interviews suggested that preschool teachers were indeed typically younger and had lower wages (possibly because they had less experience) than primary school teachers. Their training, by design, was also different as it was focused on the preschool curriculum for mixed age groups.

Another potentially important factor that differentiates the two settings is the curriculum itself. The curriculum in primary school focuses mostly on schooling competences (writing, reading, calculus). On the other hand, the preschool curriculum is play-based, and activities such as counting and vocabulary are integrated with physical activities, singing, games, or other age-appropriate activities that cater to a mixed age group of children ages 3 to 5 years. While the preschool curriculum may be developmentally appropriate, it is possible that the curriculum provided in grade 1 was more conducive to cognitive and fine motor skill gains among five year-old children.[29]

5.6. Difference with other findings from Cambodia

As indicated in the introduction, our findings differ from the non-experimental results reported in Rao et al. (2012) who study seemingly similar preschools in the same Cambodian context. In their analysis they conclude that "something is better than nothing" as their findings point to an effect size of 1.68 (on the Cambodian Development Assessment Test) of

[29] One could hypothesize that gains from stimulation in preschool might have been offset by increases in other early childhood risk factors (such as for instance health setbacks due to frequent contacts with other sick children in preschool.) We therefore analyzed impacts on a wide set of intermediary outcomes related to health, nutrition and stimulation and found no results supporting this hypothesis (results available from authors).

State preschools versus a control group. However the approach used to reach this conclusion differs substantially from ours in at least three significant ways: First, Rao et al. (2012) sample students who have attended preschool programs that were established prior to 2000—i.e. that have been in operation for a long time. These are programs that have therefore had time to mature and potentially offer better quality, and to whom the community has become accustomed. Second, the "treatment" and "control" villages in Rao et al. (2012) are not randomly assigned. As indicated in the paper, treatment villages have self-selected, or were selected by officials, to have a preschool, while control villages were, by construction in the analysis, villages that have not chosen, or were not chosen by officials, to receive a preschool. The villages are likely, therefore, to differ along a number of observed and unobserved dimensions.[30] Third, Rao et al. (2012) compare children who chose to attend a preschool in the preschool villages versus children who chose to not attend any school in the control villages. As our analysis points out, not everyone with access to a preschool actually attends one, and when there is no preschool in a village some children enroll in primary school as underage enrollees.

6. CONCLUSION

Given the importance of early childhood development for outcomes later in life, early childhood interventions are often considered promising interventions with long-term pay-offs. In the context of a developing country, they may also compensate for existing socio-economic gradients in cognitive development, and hence potentially address one of the root

[30] While the authors control for the differences in some key variables, such as maternal education and type of livelihood, simply doing so is unlikely to account for all existing important differences. Differences in relevant observed variables are large, suggesting that those for unobserved variables are likely to be large as well.

causes of existing inequalities. Due to their potential scalability, preschools are often seen as particularly promising for reaching many disadvantaged children at once. However, relatively little is known on the impact of preschool interventions in low-income settings, and about parental responses to such interventions, particularly when these are implemented at large scale.

In the context of this study, we find minimal impacts of a preschool construction program on the overall development of targeted children, which can in part be attributed to low take up rates and parental responses to the intervention. The evaluation also reveals a surprising negative impact of preschool participation on the cognitive development of the cohort with the highest exposure to the program (the five year-old cohort). This is consistent with two phenomena. First, many five-year old children—mostly from better socioeconomic backgrounds—who attend the newly established preschools would have attended primary school as underage enrollees in their absence. Second, many five-year old children—mostly from worse socioeconomic backgrounds—who would have been enrolled in primary school as underage enrollees in the absence of the program leave the formal school system entirely when the official age of primary school enrollment is enforced. The negative impacts on cognitive development are the largest for these children with less educated and poorer parents, thus resulting in increased inequality.

Due to delays in program implementation, these impacts were all measured shortly after program exposure, and length of exposure was limited. These constitute important caveats to the results. It is possible that a longer exposure time might allow for positive impacts to materialize for the overall group of beneficiary children and/or mitigate some of the negative effects we observe among five-year-olds. That said, we don't find evidence of such a positive long-term effect among the six year olds, for whom at least a fraction benefited from both preschool and primary school. As preschools become more established, demand among the

poor might increase. As preschool teachers gain more experience, it is possible that they could improve the age-appropriateness and effectiveness of their teaching.

Nevertheless, our findings suggest that, at least in the short-term, underage primary school enrollment for children from wealthier and poorer families alike led to more equitable outcomes than the enrollment patterns following the implementation of the preschool program. While higher exposure to preschool and a reduction of early enrollment in primary school might be expected to result in positive impacts, our findings suggest this was not the case in Cambodia, in part because it led to decreased school participation among the most disadvantaged. And while it is possible that the newly constructed preschools might become more effective over time, the cohorts that were exposed in the early years, studied in this paper, suffered early childhood set-backs that might be hard to catch up from. This suggests that the impact of preschool programs can be highly context-specific and determined in large part by the—sometimes unexpected—behavioral responses to an intervention. It also suggests that a better understanding of the behavioral underpinnings, and the more general determinants of the counterfactual, could potentially inform better design of preschool program in Cambodia as well as in other similar contexts.

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Figure 1 : Project timeline

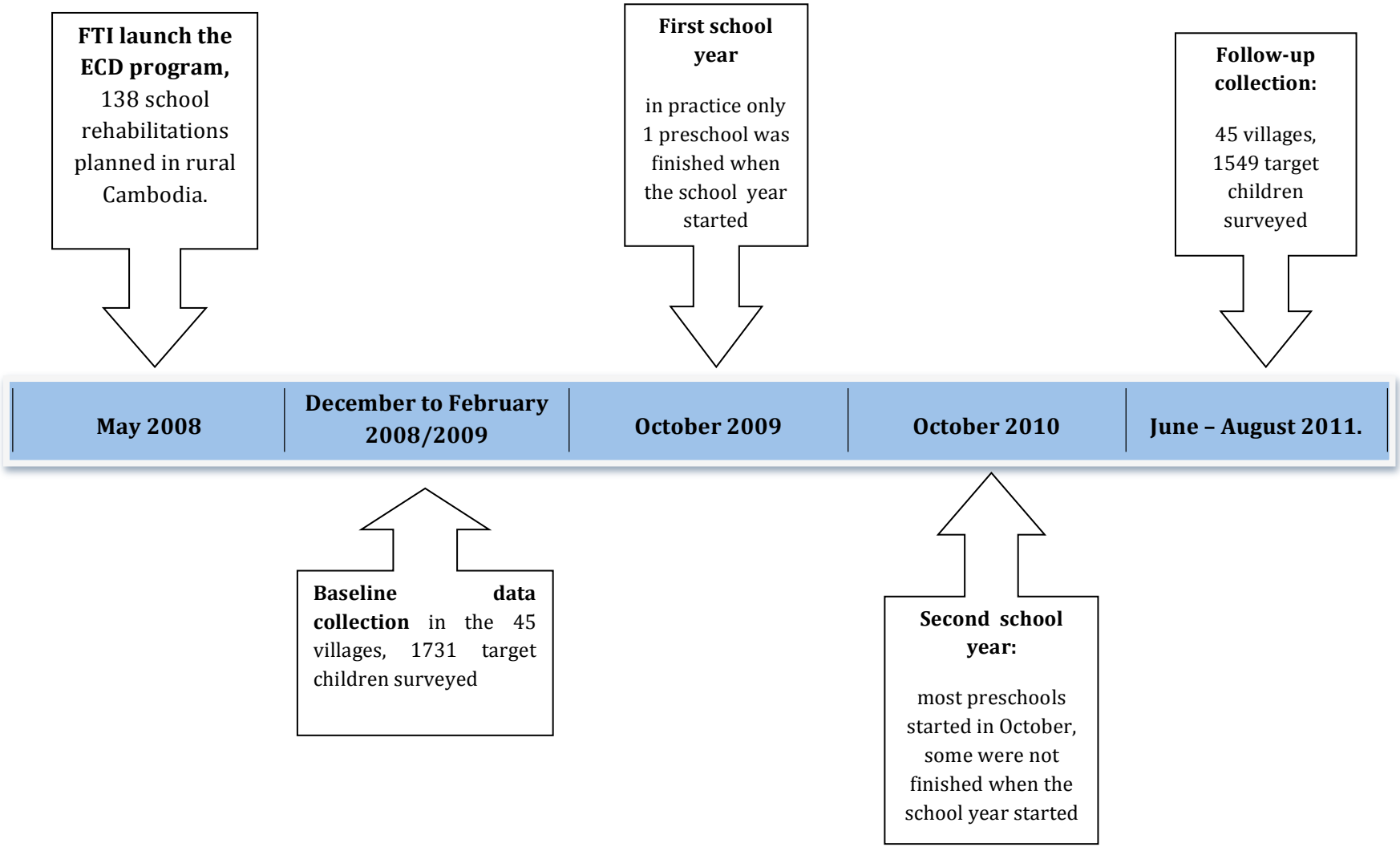

Figure 2: Participation in primary school, preschool or any school, by age

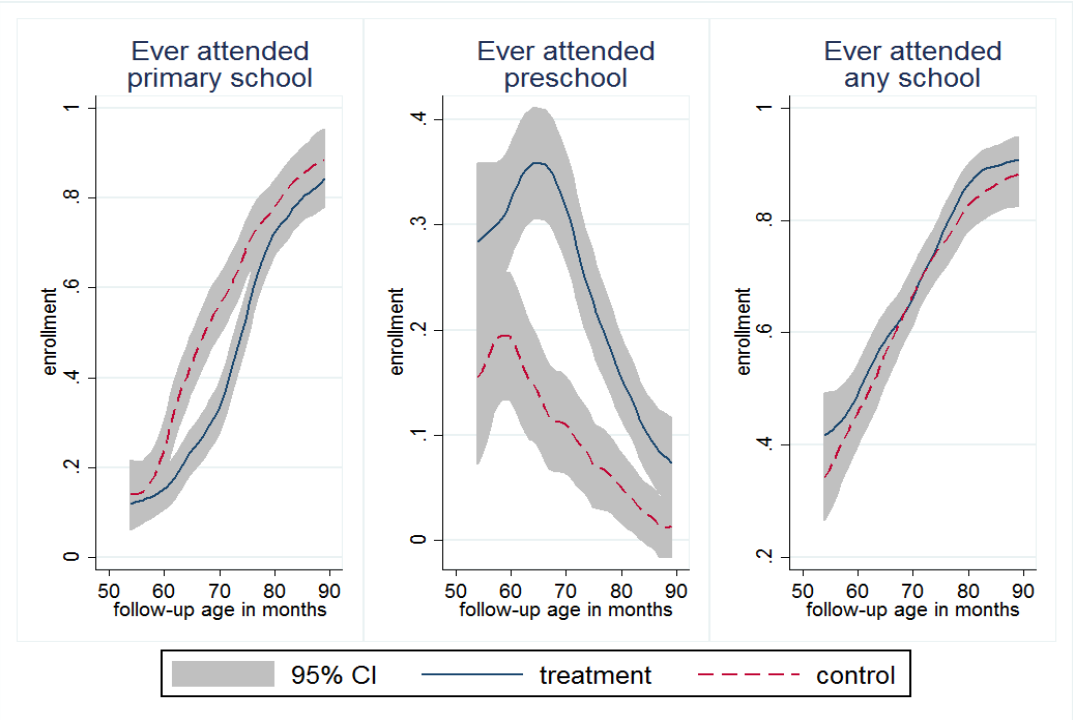

Figure 3 : Primary school enrollment age density by actual village treatment status

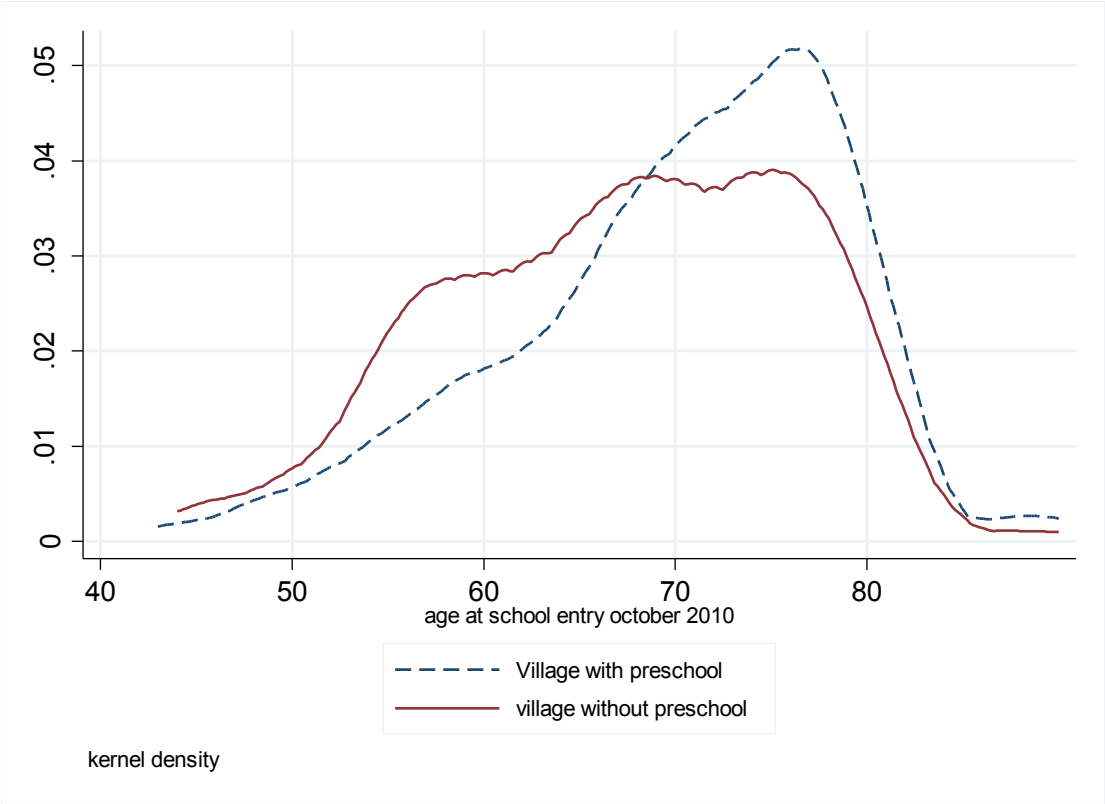

Figure 4: School attendance of 5 year old cohort: composition effect

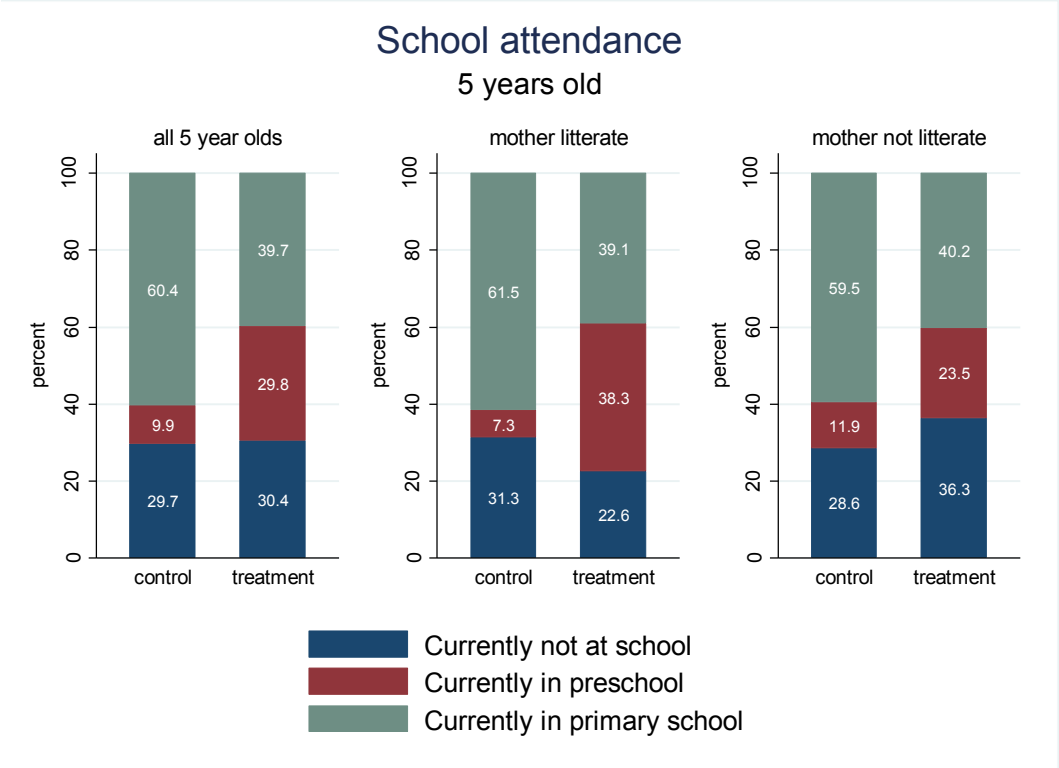

TABLE 1
DESCRIPTIVE STATISTICS AND BALANCE CHECK FOR BASELINE SAMPLE

	Full Sample					5 year old cohort		
	obs.	*Mean*	*Control*	*Treat-control*	*(s.e.)*	*obs.*	*Treat-control*	*(s.e.)*
Children								
Age in months	1731	41.469	41.259	0.345	(0.576)	599	-0.16	(0.339)
Male	1731	0.525	0.52	0.009	(0.023)	599	0.073*	(0.038)
Attendance at school	1731	0.064	0.063	0.001	(0.025)	599	0.013	(0.020)
Attendance at preschool	1731	0.023	0.031	-0.014	(0.018)	599	-0.003	(0.015)
PPVT score	1176	5.501	5.484	0.028	(0.086)	599	-0.073	(0.096)
ASQ communication	1157	3.101	3.106	-0.009	(0.102)	585	0.079	(0.090)
ASQ gross motor	1159	4.887	4.881	0.01	(0.067)	586	0.048	(0.084)
ASQ fin motor	1157	2.272	2.29	-0.03	(0.068)	584	0.036	(0.089)
ASQ problem solving	1156	2.398	2.422	-0.039	(0.072)	584	0.023	(0.084)
Woodcock-Johnson raw score	1154	1.102	1.06	0.07	(0.084)	582	-0.054	(0.085)
Height-for-age z score	1731	-2.11	-2.093	-0.029	(0.078)	598	-0.087	(0.080)
Household composition								
Number of members in household	1731	5.71	5.623	0.143	(0.153)	599	0.194	(0.160)
Number of children below 6	1731	1.566	1.477	0.146**	(0.064)	599	0.069	(0.074)
Number of adults	1731	2.658	2.704	-0.076	(0.081)	599	-0.112	(0.083)
Economy								
Paid hours (per hh memb)	1731	6.745	6.882	-0.226	(1.450)	599	0.395	(1.541)
Income (per hh memb)	1730	3.928	3.276	1.073	(1.419)	599	0.923	(1.652)
Income from paid hours father	1730	6.144	4.47	2.755	(2.431)	599	2.211	(3.167)
Income from paid hours mother	1597	1.965	1.406	0.923	(0.846)	545	2.152	(1.457)
Live under a thatch roof	1731	0.354	0.384	-0.051	(0.069)	599	-0.041	(0.072)
Competences/involvement of parents								
Household head years of education	1563	3.055	2.901	0.252	(0.392)	552	-0.029	(0.391)
Caregiver years of education	1621	2.066	1.974	0.153	(0.288)	563	0.327	(0.333)
Household head is literate	1728	0.634	0.623	0.018	(0.051)	599	-0.011	(0.068)
Caregiver is literate	1731	0.417	0.445	-0.046	(0.058)	599	-0.004	(0.067)
Household head attended school	1701	0.747	0.735	0.019	(0.046)	589	0.016	(0.054)
Caregiver attended school	1731	0.653	0.642	0.019	(0.052)	599	0.052	(0.068)
Mother's Raven score	1730	2.346	2.344	0.005	(0.089)	599	-0.006	(0.117)
Parental involvement score	1731	4.616	4.654	-0.062	(0.096)	599	0.003	(0.109)
Have paper & pen at home	1731	0.784	0.775	0.015	(0.032)	599	0.004	(0.037)
Mother's height	1731	153.12	153.23	-0.179	(0.514)	599	-1.07*	(0.637)

Note: Columns *Treat-Control* report the results of the difference between treatment and control. Test scores are standardized. Standard errors are robust and account for intra-village correlation. * 10% significant level ** 5% significant level *** 1% significant level

TABLE 2
COEFFICIENT ON TREATMENT, ATTRITION AND INTERACTION AT BASELINE

	Full sample				5 year olds			
	obs.	γ	β	δ	*Obs.*	γ	β	δ
Children								
Age	1731	0.062	-0.001	-0.002	599	0.232	0.005	-0.005
		(0.068)	(0.001)	(0.001)		(0.256)	(0.005)	(0.006)
Male	1731	-0.02	-0.017	0.03	599	-0.031	-0.064*	0.086*
		(0.031)	(0.021)	(0.029)		(0.039)	(0.037)	(0.049)
Summary index Motor	1159	0.014	-0.018	-0.006	586	0.109	-0.025	-0.026
		(0.099)	(0.018)	(0.026)		(0.121)	(0.020)	(0.033)
Summary index Cognition	1176	0.067	0.005	-0.021	599	0.048	-0.012	-0.01
		(0.069)	(0.012)	(0.016)		(0.106)	(0.020)	(0.027)
Height for age z score	1731	-0.068	0.009	-0.03	599	-0.137**	0.034*	-0.072***
		(0.044)	(0.015)	(0.019)		(0.052)	(0.020)	(0.024)
Household composition								
# household members	1731	-0.018	-0.004	0.003	599	0.081	0.008	-0.012
		(0.066)	(0.008)	(0.010)		(0.083)	(0.010)	(0.013)
# children below 6	1731	0.001	0.02	-0.005	599	-0.065	-0.041	0.053
		(0.053)	(0.029)	(0.034)		(0.071)	(0.031)	(0.039)
Economy								
Household income	1730	-0.012	0.003*	0.001	599	-0.01	0.001	0.006**
		(0.025)	(0.002)	(0.003)		(0.025)	(0.002)	(0.003)
Live under a thatch roof	1731	-0.009	-0.007	0.014	599	-0.004	-0.024	0.045
		(0.030)	(0.029)	(0.041)		(0.032)	(0.025)	(0.048)
Parents								
Hh head education (year)	1563	-0.004	0.007	0	552	0.019	0.011	-0.005
		(0.033)	(0.005)	(0.006)		(0.038)	(0.010)	(0.012)
Caregiver education (year)	1621	-0.021	-0.003	0.009	563	0.026	0.009	-0.008
		(0.028)	(0.006)	(0.008)		(0.035)	(0.013)	(0.014)
Household head literate	1728	0.001	0.047**	-0.009	599	0.002	0.026	0.018
		(0.025)	(0.023)	(0.033)		(0.035)	(0.042)	(0.057)
Caregiver read literate	1731	-0.02	-0.027	0.036	599	0.009	0.003	0.007
		(0.030)	(0.027)	(0.034)		(0.034)	(0.038)	(0.048)
Raven test score	1730	-0.018	0.033**	0.006	599	0.021	0.033	-0.003
		(0.038)	(0.012)	(0.017)		(0.064)	(0.022)	(0.029)
Have paper & pen at home	1731	-0.023	-0.072**	0.026	599	0.049	0.027	-0.045
		(0.049)	(0.034)	(0.045)		(0.052)	(0.042)	(0.061)
Mothers height	1731	1.223***	0.004*	-0.008***	599	2.367***	0.009**	-0.015***
		(0.404)	(0.002)	(0.003)		(0.749)	(0.004)	(0.005)

The above table presents the results from regression (1): listed in the first column are the baseline characteristics. Column Obs. gives the number of observation, column γ the coefficient of the treatment variable, column β the coefficient of the baseline characteristics on the attrition level, and δ the interaction between the baseline characteristics and the treatment variable (the attrition bias). Standard errors are robust and clustered at the village level but do not control for any additional baseline variables. * 10% significant level ** 5% significant level *** 1% significant level.

TABLE 2
COEFFICIENT ON TREATMENT, ATTRITION AND INTERACTION AT BASELINE

	Full sample				5 year olds			
	obs.	*T*	*β*	*δ*	*Obs.*	*T*	*β*	*δ*
Children								
Age	1731	0.062	-0.001	-0.002	599	0.232	0.005	-0.005
		(0.068)	(0.001)	(0.001)		(0.256)	(0.005)	(0.006)
Male	1731	-0.02	-0.017	0.03	599	-0.031	-0.064*	0.086*
		(0.031)	(0.021)	(0.029)		(0.039)	(0.037)	(0.049)
Summary index Motor	1159	0.014	-0.018	-0.006	586	0.109	-0.025	-0.026
		(0.099)	(0.018)	(0.026)		(0.121)	(0.020)	(0.033)
Summary index Cognition	1176	0.067	0.005	-0.021	599	0.048	-0.012	-0.01
		(0.069)	(0.012)	(0.016)		(0.106)	(0.020)	(0.027)
Height for age z score	1731	-0.068	0.009	-0.03	599	-0.137**	0.034*	-0.072***
		(0.044)	(0.015)	(0.019)		(0.052)	(0.020)	(0.024)
Household composition								
# household members	1731	-0.018	-0.004	0.003	599	0.081	0.008	-0.012
		(0.066)	(0.008)	(0.010)		(0.083)	(0.010)	(0.013)
# children below 6	1731	0.001	0.02	-0.005	599	-0.065	-0.041	0.053
		(0.053)	(0.029)	(0.034)		(0.071)	(0.031)	(0.039)
Economy								
Household income	1730	-0.012	0.003*	0.001	599	-0.01	0.001	0.006**
		(0.025)	(0.002)	(0.003)		(0.025)	(0.002)	(0.003)
Live under a thatch roof	1731	-0.009	-0.007	0.014	599	-0.004	-0.024	0.045
		(0.030)	(0.029)	(0.041)		(0.032)	(0.025)	(0.048)
Parents								
Hh head education (year)	1563	-0.004	0.007	0	552	0.019	0.011	-0.005
		(0.033)	(0.005)	(0.006)		(0.038)	(0.010)	(0.012)
Caregiver education (year)	1621	-0.021	-0.003	0.009	563	0.026	0.009	-0.008
		(0.028)	(0.006)	(0.008)		(0.035)	(0.013)	(0.014)
Household head literate	1728	0.001	0.047**	-0.009	599	0.002	0.026	0.018
		(0.025)	(0.023)	(0.033)		(0.035)	(0.042)	(0.057)
Caregiver read literate	1731	-0.02	-0.027	0.036	599	0.009	0.003	0.007
		(0.030)	(0.027)	(0.034)		(0.034)	(0.038)	(0.048)
Raven test score	1730	-0.018	0.033**	0.006	599	0.021	0.033	-0.003
		(0.038)	(0.012)	(0.017)		(0.064)	(0.022)	(0.029)
Have paper & pen at home	1731	-0.023	-0.072**	0.026	599	0.049	0.027	-0.045
		(0.049)	(0.034)	(0.045)		(0.052)	(0.042)	(0.061)
Mothers height	1731	1.223***	0.004*	-0.008***	599	2.367***	0.009**	-0.015***
		(0.404)	(0.002)	(0.003)		(0.749)	(0.004)	(0.005)

The above table presents the results from regression (1): listed in the first column are the baseline characteristics. Column Obs. gives the number of observation, column β the effect of the baseline characteristics on the attrition level, and δ the interaction between the baseline characteristics and the treatment variable (the attrition bias). Standard errors are robust and clustered at the village level but not control for any additional baseline variables. * 10% significant level ** 5% significant level *** 1% significant level.

TABLE 3:
PRESCHOOLS AT FOLLOW-UP: VILLAGE LEVEL STATISTICS

	Total	*Control*	*Treatment*
Number of villages	45	19	26
Number of villages with a preschool at follow-up (*admin data*)	21	2	19
Number of villages with a preschool at follow-up (*school survey*)	24	3	21

Notes: The table presents the number of villages in the control and treatment group for different subsample and from different sources of information (administrative data and surveys with school directors and village chiefs).

TABLE 4
PARTICIPATION RATE IN TREATMENT AND EARLY CHILD CARE PROGRAMS

		Obs.	*Control*	*Treat.-Control*	*(S.E.)*
1	**Ever attend formal *preschool* program**				
a	… in full sample	1548	0.106	0.252***	(0.053)
b	… on 4 year olds	489	0.119	0.249***	(0.073)
c	… on 5 year olds	534	0.09	0.32***	(0.055)
d	… on 6 year olds	525	0.11	0.189***	(0.062)
e	… in villages with a functioning preschool	861	0.405	-0.014	(0.140)
2	**Currently in *primary* school**				
a	… in full sample	1547	0.561	-0.102**	(0.048)
b	… on 4 year olds	489	0.244	-0.088	(0.070)
c	… on 5 year olds	534	0.604	-0.206***	(0.071)
d	… on 6 year olds	524	0.832	-0.046	(0.039)
3	**Ever attend formal *school* system**				
a	… in full sample	1549	0.66	0.038	(0.046)
b	… on 4 year olds	489	0.409	0.067	(0.075)
c	… on 5 year olds	534	0.703	-0.007	(0.064)
d	… on 6 year olds	526	0.864	0.032	(0.032)
4	**Preschool duration (months)**				
a	… in full sample	1548	0.649	1.993***	(0.477)
b	… in villages with a functioning preschool	401	6.141	1.242	(0.921)
5	**Ever attend Community Center-based Program**	1548	0.129	-0.074	(0.063)
6	**Ever receive a home visit or community meetings**	1548	0.208	-0.06	(0.043)

Note: column *Control.* gives the average of the dependent variable in the control group, column *Treat-Control* gives the ITT estimates of the dependent variables. Standard errors in column *(S.E)* are robust and account for intra-village correlation.* 10% significant level ** 5% significant level *** 1% significant level

TABLE 5
PRESCHOOL PARTICIPATION FACTORS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Gender (1=male)	0.850*	0.846*	0.826*	0.878	0.850*	0.842*	0.853
	(0.081)	(0.079)	(0.082)	(0.095)	(0.076)	(0.080)	(0.101)
Age at endline	0.984*	0.983**	0.982**	0.983**	0.982**	0.983**	0.983**
	(0.009)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.009)
Number of target children (>2 & <6) in household		0.663***	0.700***	0.645***	0.665***	0.678***	0.678***
		(0.078)	(0.081)	(0.072)	(0.079)	(0.083)	(0.079)
Number of older siblings (>6 & <18) in household		0.923*	0.938	0.92	0.931	0.915*	0.926
		(0.043)	(0.043)	(0.049)	(0.043)	(0.043)	(0.047)
Number of adult (>18)		1.046	1.021	1.022	1.05	1.034	0.998
		(0.070)	(0.065)	(0.075)	(0.071)	(0.070)	(0.067)
Thatch roof			0.580**				0.603**
			(0.138)				(0.134)
Income household head			1.010*				1.007
			(0.006)				(0.006)
Parents years of education				2.331***			2.147***
				(0.534)			(0.458)
Raven score mother					1.188*		1.063
					(0.107)		(0.089)
Parental involvement score						1.161*	1.047
						(0.105)	(0.111)
Observation	861	861	860	793	860	861	791

Note: Each column presents the results of a logit model and gives the odd ratio for each explanatory variable. Regressions are restricted to the villages with a preschool. Each regression model controls for the treatment assignment. Standard errors are robust and account for intra-village correlation. * 10% significant level ** 5% significant level *** 1% significant level

TABLE 6
INTENT-TO-TREAT ESTIMATES: FULL SAMPLE

		(1)	(2)
	Obs.	*Coef.*	*Coef.*
Receptive Vocabulary (PPVT)	1542	-0.026	-0.041
		(0.085)	(0.069)
Ages and Stages Questionnaire			
Communication	1532	-0.105	-0.109
		(0.093)	(0.086)
Gross motor	1530	0.09	0.097*
		(0.056)	(0.051)
Fine motor	1531	-0.046	-0.073
		(0.088)	(0.069)
Problem solving	1530	-0.101	-0.133*
		(0.091)	(0.072)
Woodcock Johnson	1533	0.032	-0.014
		(0.083)	(0.066)
Strength and Difficulties Questionnaire			
Emotion	1545	0.04	0.063
		(0.062)	(0.061)
Conduct	1545	-0.064	-0.042
		(0.083)	(0.083)
Hyperactivity	1545	-0.009	-0.015
		(0.080)	(0.075)
Peer	1545	-0.01	-0.002
		(0.072)	(0.069)
Prosocial	1545	-0.086	-0.083
		(0.074)	(0.063)
Anthropometrics			
Height for age z score	1524	-0.02	-0.007
		(0.078)	(0.043)
Weight for age z score	1529	-0.014	-0.01
		(0.055)	(0.044)
Covariates		No	Yes

Note: The table presents intent-to-treat estimates of the impact of the program on follow-up cognitive, non-cognitive, motor and anthropometrics measures with different set of covariates. Column 1 shows results without any covariate, column 2's results control for baseline age, gender, age gender interaction dummies, number of children in household, height for age at baseline, mother's height, and province fixed effects. Standard errors are robust and account for intra-village correlation (45 clusters). All scores are standardized using the standard deviation of the control group.

* 10% significant level ** 5% significant level *** 1% significant level

TABLE 7
IMPACT ON FAMILY OF OUTCOMES: FULL SAMPLE

		(1)		(2)	
	Obs.	*Coef.*	*(S.E.)*	*Coef.*	*(S.E.)*
Overall development index	1549	-0.029	(0.048)	-0.032	(0.035)
Cognitive development index	1542	-0.064	(0.075)	-0.087	(0.057)
Motor development index	1532	0.022	(0.060)	0.012	(0.048)
Anthropometrics index	1541	-0.017	(0.063)	-0.009	(0.035)
Non cognitive index	1545	-0.026	(0.051)	-0.016	(0.044)

Note: Results from the Seemingly Unrelated Regression model (SUR). Column 1 shows results from regressions without any covariates, while column 2 shows results with the more complete set of covariates (baseline age, gender, age gender interaction dummies, number of children in household, height for age, mother's height and province fixed effect). Overall development index accounts for all tests scores; cognitive development index is an index of all cognitive tests (vocabulary, memory, problem solving and communication of the ASQ); Motor development index is composed of gross motor and fine motor; Anthropometrics index includes weight-for-age and height-for-age; and the non-cognitive index corresponds to the index of the subscales of the Strength and Difficulties Questionnaire. Standard error (s.e.) are robust and account for intra-village correlation.

* 10% significant level ** 5% significant level *** 1% significant level

TABLE 8
SUR REGRESSIONS BY COHORT

	4 year old cohort			5 year old cohort			6 year old cohort		
	Obs.	*Coef.*	*(S.E)*	*Obs.*	*Coef.*	*(S.E)*	*Obs.*	*Coef.*	*(S.E)*
Overall development index	489	0.008	(0.045)	534	-0.063*	(0.034)	526	-0.021	(0.051)
Cognitive development index	486	-0.037	(0.072)	531	-0.189***	(0.065)	525	0.002	(0.091)
Motor development index	483	0.16*	(0.095)	528	-0.089	(0.060)	521	-0.013	(0.043)
Anthropometrics index	487	-0.021	(0.054)	534	-0.023	(0.051)	520	0.033	(0.051)
Non cognitive index	489	-0.005	(0.056)	530	0.032	(0.055)	526	-0.064	(0.071)

Note: results from the Seemingly Unrelated Regression model (SUR). All estimates control for age, gender, age gender interaction dummies, number of children in household, height for age, mother's height, and province fixed effect. Overall development index accounts for all tests scores; cognitive development index is an index of all cognitive tests (vocabulary, memory, problem solving and communication of the ASQ); Motor development index is composed of gross motor and fine motor; Anthropometrics index includes weight-for-age and height-for-age; and the non-cognitive index corresponds to the subscales of the Strength and Difficulties Questionnaire. Standard errors (s.e.) are robust and account for intra-village correlation. *10% significant level **5% significant level *** 1% significant level

TABLE 9:
BASELINE CHARACTERISTICS OF THE PROGRAMS' TAKERS

		PRESCHOOL TAKE-UP			ANY SCHOOL TAKE-UP			
		Coef	*(S.E.)*	*P-value diff*	*Coef*	*(S.E.)*	*P-value diff*	*obs*
Full sample		0.32***	(0.055)		-0.007	(0.064)		534
Caregiver literate	yes	0.418***	(0.062)	**0.015**	0.087	(0.088)	**0.065**	229
	no	0.248***	(0.060)		-0.077	(0.063)		305
No thatch roof	yes	0.377***	(0.061)	**0.032**	0.017	(0.065)	0.287	346
	no	0.207***	(0.070)		-0.061	(0.082)		188
Paper and pen in house	yes	0.339***	(0.055)	0.266	0.032	(0.064)	**0.052**	427
	no	0.241**	(0.092)		-0.165	(0.103)		107
Raven score caregiver	top 75	0.321***	(0.058)	0.892	0.023	(0.069)	0.302	408
	bottom 25	0.312***	(0.076)		-0.108	(0.117)		126
Parental involvement score	top 75	0.336***	(0.061)	0.516	-0.005	(0.063)	0.888	399
	bottom 25	0.273***	(0.087)		-0.019	(0.106)		135
Parental SES index	top 75	0.353***	(0.059)	0.141	0.043	(0.063)	**0.030**	399
	bottom 25	0.229***	(0.079)		-0.131	(0.087)		135

Note: the table reports the OLS estimates of the regression of the preschool or school participation on the treatment variable for different baseline covariates subsamples. For Raven, parental involvement and average SES score we compare parents in lowest quartile with the rest of the sample. SES score is an unweighted average of 13 standardized variables measuring socio-economic status. Robust standard errors, clustered at village level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1

TABLE 10
HETEROGENEOUS TREATMENT EFFECT: 5 YEAR OLD COHORT

	Overall development index		Cognitive development index		Motor development index		Non cognitive development index	
	Coef.	*(S.E)*	*Coef.*	*(S.E)*	*Coef.*	*(S.E)*	*Coef.*	*(S.E)*
Full sample	-0.063*	(0.034)	-0.189***	(0.065)	-0.089	(0.060)	0.032	(0.055)
Characteristic 1								
Caregiver literate	-0.031	(0.113)	-0.07	(0.115)	-0.031	(0.110)	0.063	(0.084)
Treatment	-0.164**	(0.068)	-0.314***	(0.087)	-0.176**	(0.081)	0.065	(0.084)
Caregiver literate * Treatment	0.261*	(0.133)	0.353**	(0.144)	0.232*	(0.135)	-0.009	(0.110)
Characteristic 2								
No thatch roof	0.101	(0.105)	-0.035	(0.090)	-0.084	(0.089)	0.044	(0.102)
Treatment	-0.032	(0.119)	-0.228*	(0.120)	-0.298***	(0.093)	0.008	(0.104)
No thatch roof * Treatment	-0.038	(0.128)	0.146	(0.147)	0.342***	(0.112)	0.033	(0.129)
Characteristic 3								
Raven score	-0.032	(0.080)	0.08	(0.059)	0.062*	(0.032)	0.042	(0.029)
Treatment	-0.305*	(0.153)	-0.317*	(0.176)	-0.108	(0.142)	0.012	(0.100)
Raven score * Treatment	0.109	(0.081)	0.08	(0.068)	0.015	(0.053)	0.008	(0.038)
Characteristic 4								
Parental involvement	-0.059	(0.072)	-0.018	(0.033)	0.052	(0.044)	-0.013	(0.025)
Treatment	-0.659**	(0.326)	-0.492**	(0.238)	0.16	(0.293)	-0.404**	(0.159)
Involvement * Treatment	0.132*	(0.075)	0.078*	(0.046)	-0.05	(0.064)	0.095***	(0.033)
Characteristic 5								
Paper & pen	-0.074	(0.056)	-0.21**	(0.093)	-0.01	(0.124)	-0.014	(0.074)
Treatment	-0.253***	(0.063)	-0.479***	(0.134)	-0.127	(0.128)	-0.143	(0.094)
Paper & pen * Treatment	0.252***	(0.081)	0.438***	(0.133)	0.068	(0.147)	0.218**	(0.103)
Characteristic 6								
Parental SES index	0.006	(0.052)	0.012	(0.067)	-0.005	(0.087)	0.058	(0.040)
Treatment	-0.044	(0.058)	-0.121*	(0.065)	-0.065	(0.067)	0.033	(0.056)
Parental SES index * Treatment	0.129**	(0.064)	0.151*	(0.078)	0.124	(0.086)	0.018	(0.048)

The table gives the results of the interaction term of regression equation (4) for five different parental baseline characteristics: caregiver is literate, household revenue, raven score, an index of parental involvement and whether the household has paper and pen. The Index from characteristic 6 is the same as the one used for table 9.* 10% significant level ** 5% significant level *** 1% significant level

Annex A
VILLAGE PARTICIPATION

	obs.	*C*	*T-C*	*(S.E)*
Village has a primary school according to survey	1731	1	0	0
Village has a formal preschool according to survey	1731	0.131	0.686***	(0.125)
Village has a formal preschool according to admin	1731	0.128	0.605***	(0.121)
Preschool classes were given according to survey	1731	0.09	0.683***	(0.105)
Village has a informal preschool according to survey	1731	0.163	-0.12	(0.1)
Village has a home based program according to survey	1731	0.131	0.064	(0.119)

The table presents the individual participation of children in schools. Column "obs." gives the number of children concerned, column "av." presents the average participation, "c" the participation in the control group, "t" the one in the treatment group and "t-c" the results from the regression of the dependent variable on the treatment group variable. Standard errors are clustered at village level and are robust.

* 10% significant level ** 5% significant level *** 1% significant level*

ANNEX B

ROBUSTNESS OF TREATMENT EFFECT FOR DIFFERENT SPECIFICATIONS: 5 YEAR OLD COHORT

	(1)		(2)		(3)		(4)		(5)		(6)	
	Obs.	*Coef. (S.E)*	*Obs.*	*Coef. (S.E)*	*Obs.*	*Coef. (S.E)*	*Obs.*	*Coef. (S.E)*	*Obs.*	*Coef. (S.E)*	*Obs.*	*Coef. (S.E)*
Overall development index	534	-0.074	534	-0.072	534	-0.066	534	-0.063*	534	-0.075*	468	-0.071*
		(0.047)		(0.046)		(0.041)		(0.034)		(0.041)		(0.039)
Cognitive development index	531	-0.187**	531	-0.203**	531	-0.199**	531	-0.189***	531	-0.186**	465	-0.157**
		(0.086)		(0.084)		(0.079)		(0.065)		(0.078)		(0.067)
Motor development index	528	-0.107	528	-0.099	528	-0.096	528	-0.089	528	-0.109*	463	-0.111*
		(0.068)		(0.066)		(0.062)		(0.060)		(0.059)		(0.057)
Anthropometrics index	534	-0.045	534	-0.042	534	-0.026	534	-0.023	534	-0.05	468	-0.044
		(0.075)		(0.075)		(0.052)		(0.051)		(0.071)		(0.079)
Non cognitive index	530	0.018	530	0.032	530	0.036	530	0.032	530	0.016	464	0.002
		(0.054)		(0.054)		(0.056)		(0.055)		(0.053)		(0.054)
Controls:												
Age, gender and interactions				✓		✓		✓		✓		✓
Child & caregiver anthropo.						✓		✓		✓		✓
Province FE & # children <= 6								✓		✓		✓
Baseline test scores										✓		✓
All baseline variable[◊]												✓

Note: results from the Seemingly Unrelated Regression model (SUR) on five year old for different set of covariates. Overall development index accounts for all tests scores; cognitive development index is an index of all cognitive tests (vocabulary, memory, problem solving and communication of the ASQ); Motor development index is composed of gross motor and fine motor; Anthropometrics index includes weight-for-age and height-for-age; and the non-cognitive index corresponds to the subscales of the Strength and Difficulties Questionnaire. Standard errors (s.e.) are below in parenthesis. They are robust and account for intra-village correlation.

◊ See table 2 for list of variables.

* 10% significant level ** 5% significant level *** 1% significant level

Annex C

INTENT TO TREAT RESULTS EXCLUDING OUTLIERS: 5 YEAR OLD COHORT

	Truncation above +/- 3 sd			Truncation above +/- 2 sd			Truncation above +/- 1 sd		
	Obs.	*Coef.*	*(S.E)*	*Obs.*	*Coef.*	*(S.E)*	*Obs.*	*Coef.*	*(S.E)*
Overall development index	530	-0.062*	-0.034	527	-0.069**	-0.032	435	-0.022	-0.027
Cognitive development index	526	-0.186***	-0.064	519	-0.182***	-0.064	411	-0.124***	-0.045
Motor development index	524	-0.103*	-0.057	517	-0.1*	-0.056	416	-0.032	-0.048
Anthropometrics index	531	-0.027	-0.05	514	-0.033	-0.047	381	0.028	-0.036
Non cognitive index	528	0.031	-0.053	506	0.021	-0.047	374	0.003	-0.032

Note: Table presents the ITT SURE results for the 5 year old cohort after excluding selected outliers. Regressions include usual controls and can be compared to results in column 4 of Table 9. * 10% significant level ** 5% significant level *** 1% significant level