Preschool Attendance, Parental Investment, and Child Development: Experimental Evidence from Bangladesh

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Abstract

In this paper, we examine the effects of an experimental preschool intervention in Bangladesh on children's skill development and parental investment decisions. We consider the Early Years Preschool Program (EYPP) in Bangladesh, which implemented a play-based curriculum along with monthly teacher-parent meetings to improve parenting practices. Since the program was implemented in the presence of alternative preschool programs, we examine heterogeneous impacts across fallback options. We exploit the random allocation of the program across communities, and find large ITT effects on children's cognitive and socioemotional development, along with positive impacts on parents' monetary investments in their children. Assuming that EYPP availability does not make alternative programs more attractive, we use machine learning techniques to predict fallback choices and recover local-average treatment effects along intensive and extensive margins. EYPP attendance had sizable impacts on children who have stayed at home in absence of the program offer. For children switching out of other preschool programs, we find positive impacts on their socioemotional skills. We present a mediation analysis and find that changes in parents' monetary investment account for one-third of the extensive margin effect.

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Keywords: Early Childhood Development, Preschool Attendance, Heterogeneous Effects.

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1 Introduction

The importance of early-life circumstances in determining outcomes in adulthood has led governments across the world to implement early childhood interventions aimed at closing gaps for children from different socioeconomic backgrounds (Heckman, 2008; Almond and Currie, 2011; Engle et al., 2011; Elango et al., 2015). However, the effectiveness of these programs is not guaranteed. First, early childhood programs are usually implemented in contexts where families have access to alternative options, and program substitution might bias the overall evaluation of program effectiveness (Heckman et al., 2000). Furthermore, these programs may directly, or indirectly, change parenting behaviors within the family. Given the importance of parental investment in driving children's skill development (Cunha and Heckman, 2007; Cunha et al., 2010), understanding the impacts of these interventions on parental behavior may strongly predict overall program success.

This paper sheds light on these margins by examining the impact of an experimental preschool intervention in Bangladesh aimed at four-year old children. We analyze the Early Years Preschool Program (EYPP). The goal of EYPP was to develop children's skills through a play-based curriculum while improving parenting practices through monthly teacher-parent meetings. The program was implemented in existing schools in randomly selected communities in the Meherpur district and children attended five days a week for two hours. In this context, we study the impact of the EYPP program on children's multidimensional skill development and examine the mediating effect explained by changes in parenting practices. Furthermore, we consider the impacts of the program for both children drawn from other preschools—intensive margin—and home care—extensive margin. By studying the quantitative role of program substitution and parental responses, we provide a comprehensive picture of the impacts of the EYPP intervention. Our study illustrates the importance of considering the intensive and extensive margin effects as well as parental responses across these margins when understanding the effects of early childhood interventions.

EYPP databases contain rich information on child skills. We take advantage of data from the baseline and follow-up survey rounds, which was conducted at the end of the program implementation. We observe upwards of twenty measures of children's skills and fifteen measures of parental behavior. As each variable may measure underlying skills and parental investment with substantial error (Cunha et al., 2010; Schennach, 2016), we perform an exploratory factor analysis (EFA) to recover latent factors of children's skills and parenting practices. While the observed skill measures can be seemingly divided into cognitive and socioemotional constructs, our EFA-based results indicate the existence of a single measure of children's multidimensional skills, both at baseline and follow-up. Moreover, we identify three dimensions of parenting practices, covering monetary investments, quality time spent with children and parenting styles.

We first estimate intent-to-treat effects and find sizable effects on children's skill development. We show that treated group children experienced a 0.4 standard deviation (σ) improvement in the latent skills factor relative to control group students. While we fail to find significant impacts on the quality time or parenting style variables, we document sizable effects on parents' monetary investment, reaching 0.29 σ through the follow-up round. While these results offer suggestive evidence on the effectiveness of EYPP, this hypothesis may not hold true in light of alternative preschool programs. As such, if EYPP does not change the production function of child outcomes and parental investment vis-a-vis existing programs, expanding the program may not be a worthwhile investment. We note that receiving the EYPP offer successfully increased the likelihood of attendance by 50 percentage points. However, 37% of compliers would have attended alternative preschool programs in Bangladesh. Furthermore, intensive-margin compliers come from higher-SES households and have higher skills at baseline relative to the group of extensive-margin compliers. Both phenomena suggest that ITT effects might be influenced by program substitution.

To evaluate the importance of program substitution and the role played by parental investments in this regard, we estimate local average treatment effects (LATE) for children drawn from different choice margins. We follow Kirkeboen et al. (2016) and invoke an irrelevance assumption—the EYPP offer does not make alternative programs more attractive—to estimate intensive- and extensive-margin LATEs, along with the standard exclusion, independence and monotonicity assumptions in instrumental variable designs (Imbens and Angrist, 1994; Kline and Walters, 2016). We use a machine learning algorithm to predict children's counterfactual attendance in absence of the EYPP offer. Since we do not perfectly predict children's attendance decisions in the control group, we follow the literature on misclassified variables to recover the parameters of intrest.

We find substantial heterogeneity in the effects of EYPP attendance across intensive and extensive margins. On the one hand, the effects of EYPP attendance for children who would have remained at home are significant, exceeding one standard deviation in the latent skills factor. On the other hand, the intensive-margin LATE is not statistically significant. However, we find positive impacts of EYPP attendance on intensive-margin compliers' socioemotional development. At the same time, the extensive-margin LATE shows sizable impacts on parental monetary investments, along with an insignificant impact on their intensive-margin complier counterparts. In light of these results, we examine the mechanisms through which the EYPP program affected child development outcomes heterogeneously across fallback choices. We perform a mediation analysis which considers the importance of changes in parental investment and preschool attendance. For extensive-margin compliers, we find that upwards of 30% of the program's impact can be explained through the effect on parental monetary investments. In contrast, for children who would have attended alternative programs, we find that the positive impact on their socioemotional development is explained through the direct impact of EYPP attendance rather than through changed parental investment choices. Our analysis thus remarks the importance of considering heterogeneous impacts of program attendance in light of existing alternatives while also documenting evidence on the mechanisms behind impacts on children's skill development.a

Our paper advances the literature on early childhood interventions in various ways. To the best of our knowledge, this is the first paper to examine heterogeneous effects of preschool participation on children's skill development across fallback alternatives while providing evidence on the role of parental investment in driving the effects. We build on recent work in exploiting experimental variation to estimate the effects of early interventions in developing countries; that is, in contexts of potentially high ex-ante returns to early childhood education with low participation rates (Brinkman et al., 2017; Martinez et al., 2017; Bouguen et al., 2018; Andrew et al., 2019; Blimpo et al., 2019; Carneiro et al., 2019). Yet we go further by noting that the identified effect of an early intervention is influenced by the existence of close substitutes and/or the reactions of parents when faced with this policy shock. In this regard, the early childhood literature considers at most one of these two potential phenomena. First, Feller et al. (2016) and Kline and Walters (2016) in the United States, Dean and Jayachandran (2020) in India, and Berkes and Bouguen (2019) in Cambodia study the importance of alternative programs. However, they do not ponder the role of parents as potential mediators. Second, a growing literature examines the impacts of early childhood interventions on parenting behavior (Campbell and Ramey, 1994; Gertler et al., 2014; Carneiro et al., 2019; Chaparro et al., 2020). More similar to our setting, Attanasio et al. (2017) and Attanasio et al. (2020) estimate latent skills production functions and quantify the mediating role of parental investments in explaining the effects of early childhood interventions. We combine these two strands of literature, by studying the role of program substitution along with parental behavior.

The rest of the paper proceeds as follows. In Section 2, we discuss the context and the preschool intervention. We present summary statistics and examine covariate balance. In Section **??**, we present our approach to estimate latent skills free of measurement error. Section 3 presents intent-to-treat estimates of the EYPP intervention on child development and parental investment outcomes. In Section 4, we present an empirical framework to recover the local average treatment effects across fallback choices and show the estimated results. In Section 4.3, we use the estimated effects across fallback choices to implement a mediation analysis. Lastly, in Section 5, we discuss the results and conclude.

2 Context, Intervention and Summary Statistics

2.1 Context and Intervention

Bangladesh has recently undergone significant economic progress, halving the poverty rate between 2000 and 2016 and reaching a 6% annual GDP growth rate in the past decade. While this growth has been accompanied by increased primary school enrollment rates, reaching 90% in 2011 (Dang et al., 2011), achievement indicators have lagged behind, as a sizable share of primary school students fail to solve basic math problems (Asadullah and Chaudhury, 2013). In this context, the government has recognized the potential of preschool education for improving educational outcomes by issuing the National Pre-primary Operational Framework in 2008, which called for students to eventually participate in two years of preschool.

However, preschool enrollment in Bangladesh has been limited, as only 45% of five-year olds attended preschool in 2013, along with just 21% of their four-year old peers. Moreover, existing preschool programs do not conform to curriculum standards, as just 25% of classrooms had supplementary teaching-learning and play materials (Bhatta et al., 2020). In this context, the non-governmental organization Save the Children developed a pilot program for implementing the Early Year Preschool Program (EYPP) targeted at four-year olds.[1] EYPP was randomly offered across communities in the Meherpur district of Bangladesh with the goal of developing evidence for the implementation of pre-primary education for four-year olds across the country.[2] EYPP offers quality pre-primary education for four year olds in small classes, ranging between 15-20 students. Moreover, since these take place in existing government primary schools, there is no infrastructure cost associated with the program.

EYPP classes are conducted throughout the calendar year five days a week for two hours each. The program's curriculum is directly aligned with the age-five government preschool program, and it includes a wide variety of activities, such as signing, rhyming, storytelling, and free play. In fact, since EYPP includes a no-cost material development workshop for teachers, children are able to play with blocks, shape cards, puzzles, picture cards, charts, color pencils and storybooks. EYPP teachers also work as teachers in the government pre-primary school, yet they are directly trained by program staff for five days to learn child development techniques, child behavior management and how to incorporate various learning materials in their teaching. The program includes a monthly parents meeting facilitated by EYPP teachers, which focus on parenting techniques aimed at furthering their children's skill development, through improved talking and listening with their children, reading, counting and sorting activities, among others. EYPP also involves local communities by creating community-based school management committees in order to set-up the program.

In this paper, we focus on the implementation of EYPP in 2018 across three upazilas (Gagni, Meherpur Sadar and Mojibnagar) in the district of Meherpur.[3,4] 100 communities were included in the randomization sample, and half were offered the EYPP program while the rest constituted the control group. Randomization took place at the union level, representing a stratified design, in which each of the 18 unions in the sample had at least one treated and one control community.[5] To promote program take-up, the intervention was limited to children living within 15 minutes of the local primary school.[6] The EYPP program targeted children born in 2013, resulting in a sample of 1,986 children. Since in fourteen treated and in six control communities there were more than 25 eligible children, 25 students were randomly selected in these communities.[7] This restriction

[1]The program description follows from an implementation report by AIR (2018).

[2]EYPP was first developed in 2013, jointly with government officials and experts in early education, and has since been improved through small pilot implementations in 2013 2016.

[3]Districts in Bangladesh are divided into upazilas (sub-districts) — there are 492 total in the country — and further sub-divided into upwards of 4,000 union councils (unions).

[4]Within these upazilas, 238 communities were potentially eligible for the intervention. After dropping 90 communities with existing community-based schools, as well as those with multiple schools, the final sample yielded 105 eligible communities for intervention. Funding restrictions limited the analysis to 100 communities, and five communities were randomly dropped.

[5]While the EYPP program was first implemented in 2017 in 44 out of the 50 treated communities, this paper focuses on the cohort of four-year olds who first enrolled in 2018.

[6]To determine the set of eligible children, the research team conducted a census of all households within a 15-minute walk to the school, yielding a total of 36,806 households across the 100 sample communities.

[7]While the ideal EYPP class size is between 18-20 students, EYPP centers may enroll up to 25 children. In the eighty communities with fewer than 25 children, all children were included in the randomization.

resulted in a final sample of 1,903 children.

2.2 Data Sources

The baseline survey was conducted between December 20, 2017 and January 12, 2018, prior to the implementation of the EYPP program. 1,856 of the 1,903 selected households were successfully interviewed. The baseline survey included detailed individual and household information on demographic and socioeconomic characteristics, including educational attainment, family composition, and household size, among others. Critical to the analysis of parental investment, the baseline survey included questions related to parents monetary investment in their children, including whether they have writing materials for the child, puzzles, complex toys requiring hand-eye coordination, toys to teach their child about colors and/or counting. Moreover, it also covered parents' responses to questions regarding their time investment in their children.[8] In the baseline survey, target children also completed the International Development and Early Learning Assessment (IDELA), which includes a number of measures aimed at assessing children's physical, cognitive and socio-emotional development.[9] In particular, the assessment covers children's development in five dimensions — motor development, emergent literacy, emergent numeracy, executive function and socioemotional development — by testing them in 23 different items.[10]

The first follow-up survey was conducted in December, 2018, after the completion of the EYPP program. Attrition was low, as only 41 children were not successfully tracked.[11] This survey similarly included information on child and household characteristics, as well as students' performance on the IDELA assessment — covering the same items as the baseline survey, thus allowing for a direct achievement comparison. The follow-up survey also included measures of parental monetary and time investments, thus allowing us to examine the impact of EYPP on parental investments. This survey also collected detailed information on children's preschool participation in 2018, including whether they had in fact attended the EYPP program, or any program among the available alternatives in the Meherpur district, which include public, private, Islamic and BRAC preschools.[12]

Our aim is to recover the impacts of the EYPP program on children's skill development and

[8] In particular, the questions measured whether parents read books with their child, tell them stories, sing songs with them, take them outside the home, play simple games with them, name objects to them, teach them new things (such as new words), teach them the alphabet, play counting games, hug their child, the amount of time spent with them, as well whether they hit, spank or criticize them.

[9] The IDELA assessment was developed by Save the Children in 2011, seeking to develop comparable cross-country measures in children's cognitive, reading, math and socioemotional skills.

[10] The emergent literacy index measures children's vocabulary, print awareness, letter identification, phonemic awareness, writing level and listening comprehension. Emergent numeracy considers their performance in 'measurement and comparison', classification and sorting, shape identification, number identification, one-to-one-correspondence, addition and subtraction and puzzle solving. Motor development measures their performance on hopping, copying a shape, drawing a human shape and folding paper. Socioemotional development measures responses on children's self-awareness, peer relationships and empathy. Lastly, the executive function considers their short-term memory and inhibitory control.

[11] We additionally drop 18 children who did not provide answers to the follow-up assessments or whose parents did not provide information their investment choices.

[12] Students attending 'public preschools' could have either attended the EYPP program or enrolled in the age-five preschool program a year early. BRAC is the largest provider of pre-primary education in Bangladesh.

on parental investment decisions. While we observe multiple measures of children's test scores and parenting behavior both at baseline and follow-up, previous work has documented the extent to which each observed variable measures underlying constructs with substantial error (Cunha et al., 2010; Schennach, 2016). A potential solution is to average across all variables pertaining to a particular construct (i.e. parental monetary investments), yet this approach involves arbitrarily assigning observed measures to such constructs (Heckman et al., 2013a) . To overcome this arbitrariness, we implement an exploratory factor analysis (EFA) to obtain measures of skills free of measure of measurement error (Heckman et al., 2013a; Andrew et al., 2019; Attanasio et al., 2020). EFA seeks to reduce the dimensionality of observed measures by identifying latent factors which load on observed variables which are strongly correlated (Gorsuch, 2003). Appendix A describes this procedure. Our EFA assumes a dedicated measurement system of measures, meaning that each observed measure is associated to at most one underlying factor. This allows for an direct interpretation of what such factor represents. The EFA results for skills development measures point to one underlying factor driving common correlation of the twenty three available measures.[13] We refer to this factor as "latent skill" throughout the paper and—given the set of IDELA measures—we interpret it as representing a combination of the different cognitive and noncognitive skills. However, to provide robustness to our results, we also compute latent factors from blocks of measures as pre-defined by the IDELA assessment; these latent factors are socioemotional development, motor development, emergent literacy, emergent numeracy, and executive function. Finally, EFA applied to parental investment measures yields three underlying factors. Given the loading estimates, we refer these three factors as Monetary Investments, Quality Time, and Parenting Style.[14]

2.3 Sample Characteristics

In Table 1, we present evidence on baseline covariate balance for the 1,855 children who were present in the baseline survey round. Only 10% of mothers and fathers in the sample had completed tertiary education. Moreover, a sizable share of fathers were illiterate at baseline. The parental investment measures indicate that fewer than 10% of parents owned puzzles for their children and around 20-25% owned toys to teach them shapes and counting. On the other hand, two-thirds of parents reported reading to their children, singing songs to them and taking them on vistings. In terms of covariate balance across the treatment and control groups, the covariates of interest are largely balanced across the two groups. Nonetheless, the parents of control group children were more likely to have taken their children on visits and performed slightly worse on the baseline socioemotional index. Following Imbens and Rubin (2015), we fail to reject any significant differences across groups in a joint test of equality across all variables. Nonetheless, to check for the robustness of results we

[13] We estimate an IRT model for each measure of skills development before implementing our EFA. IRT identifies underlying ability to perform in each specific assessment.

[14] Examples of dedicated measures to Monetary Investments are "number of books and other reading materials" and "number of toys." Furthermore, we obtain that "tell stories," "sing songs," "play games," and "teach numbers" are dedicated to Quality Time. Finally, "parents spank," "parents hit" and "parents criticize" are loaded into the Parenting Style Factor.

estimate the empirical strategy in Section 3 both including and excluding baseline covariates.

Table 1: Baseline Characteristics and Covariate Balance

	Full Sample (1)	Treatment (2)	Control (3)	Difference (4)	T-Stat (5)
Household Characteristics					
Household Size	4.73	4.76	4.68	0.09	0.92
Number of Siblings	0.62	0.62	0.62	0.00	0.02
Mom Read	0.84	0.84	0.83	0.01	0.47
Mom Write	0.84	0.85	0.83	0.02	0.79
Dad Read	0.65	0.65	0.64	0.01	0.21
Dad Write	0.66	0.66	0.65	0.01	0.43
Mom Ed: Primary	0.23	0.24	0.23	0.01	0.54
Mom Ed: Secondary	0.56	0.56	0.57	-0.01	-0.27
Mom Ed: Tertiary	0.09	0.09	0.08	0.01	0.42
Dad Ed: Primary	0.25	0.24	0.26	-0.02	-0.66
Dad Ed: Secondary	0.32	0.33	0.31	0.02	0.67
Dad Ed: Tertiary	0.11	0.11	0.11	0.00	0.02
Child Characteristics					
Male	0.51	0.52	0.50	0.02	0.90
Age	4.44	4.42	4.46	-0.04	-2.73
Parental Investments					
Writing Materials	0.39	0.41	0.36	0.06	1.42
Puzzles	0.07	0.08	0.06	0.02	1.59
Complex Toys	0.49	0.49	0.48	0.02	0.39
Toys: Shapes	0.19	0.20	0.17	0.03	1.31
Toys: Counting	0.23	0.25	0.21	0.04	1.54
Read Books	0.69	0.69	0.68	0.01	0.17
Tell Stories	0.68	0.69	0.66	0.02	0.53
Sing Songs	0.64	0.64	0.65	-0.01	-0.28
Take Child on Visits	0.73	0.70	0.77	-0.07	-2.61
Play Games	0.52	0.53	0.49	0.04	0.86
Name Objects	0.23	0.24	0.22	0.02	0.77
Teach New Things	0.56	0.55	0.58	-0.03	-0.70
Teach Alphabet	0.79	0.80	0.79	0.01	0.32
Teach Numbers	0.52	0.55	0.49	0.06	1.34
Hug Child	0.94	0.95	0.94	0.00	0.05
Child Skill Measures					
Emergent Literacy	0.00	0.02	-0.02	0.03	0.40
Emergent Numeracy	0.00	0.01	-0.01	0.02	0.27
Executive Function	0.00	0.03	-0.04	0.07	0.99
Motor Development	0.00	0.02	-0.03	0.05	0.74
Socioemotional Index	0.00	0.05	-0.06	0.11	1.63
Observations	1,855	991	864		

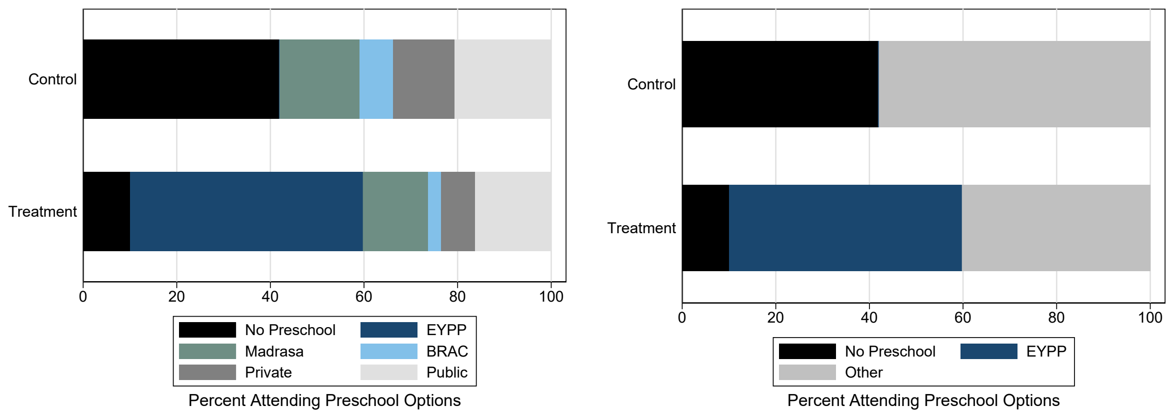
Table 1 presents summary statistics for the full sample and for children/households in treatment/control communities separately. The variables used in this table are from the baseline data collection in late 2017. The t-statistic corresponds to a test of equality between the control and treatment means. Baseline skill measures follow the IDELA test construction, for each measure, we add the relevant items and then standardize each measure in the full sample at baseline.

In Figure 1, we present evidence on preschool attendance by treatment group status. 50% of eligible children participated in the EYPP program, whereas only one control group child successfully enrolled in the program. A sizable share of treatment and control group children attended alternative programs, yet participation in these programs was significantly higher for control group (58%) children vis-a-vis their treated counterparts (40%).[15] A far smaller share of treated group

[15] As shown in the first panel of Figure 1, there is significant heterogeneity in the types of alternative programs

children remained at home during 2018 (10%), compared to 42% of their control group peers.

Figure 1: Preschool Attendance Choices by Treatment and Control Groups



(a) By Specific Alternatives **(b)** By Program Category

Note: Figure 1 presents the preschool programs attended by children in treatment and control communities. The first panel presents attendance across various alternative programs. The second panel groups alternative preschool programs into one category, as in the sub-LATE analysis presented in Section 4.

3 Intent-to-Treat Effects

3.1 Empirical Strategy

To examine the impact of the EYPP program on test score and parental investment outcomes, we take advantage of the experimental nature of the program. We first estimate the intent-to-treat (ITT) effects of the EYPP offer in the following regression:

$$\theta_{ikc} = \alpha_0 + \alpha_1 Z_c + \phi_d + \varepsilon_{ikc} \tag{1}$$

where θ_{ikc} represents the k^{th} latent skill or parental investment factor for child i residing in community c measured in the follow-up survey. Z_c is an indicator variable which equals one in treated communities, ϕ_d is a vector of union (stratum) fixed effects and ε_{ikc} is the error term. Standard errors are clustered at the community level following the randomization design. As noted above, since the treated and control groups are largely balanced, the main specification does not include covariates. However, to examine the robustness of the results, we also present estimates of equation (1) including household and child characteristics along with baseline skill and parental investment factor.

attended, yet in our empirical analysis we consider these categories as a unique alternative due to power issues, as in Kline and Walters (2016) and Dean and Jayachandran (2020).

3.2 Estimated Effects on Children's Skill Development.

We present the effects of the EYPP offer on child outcomes in Figure 2. The EYPP intervention had significant effects on children's latent skill development. The left panel of Figure 2 examines whether the distribution of the overall latent skill factor differs across treatment and control groups in the follow-up survey round. We find significant differences, as the latent factor for control group children is first-order stochastically dominated by that of their peers in the treatment group. The right panel presents ITT estimates from equation (1). We find that receiving the EYPP offer has a positive effect of 0.4 standard deviations in the latent skill factor. We further analyze whether these effects are present across specific skill domains. Receiving the EYPP offer similarly improves offered students' literacy skills through the follow-up round, which increase by upwards of 0.33 standard deviations. We find similar impacts in the numeracy domain, as the estimated intent-to-treat parameter equals 0.32 standard deviations. We remark the magnitude of the estimated impacts across these two domains, as Hanushek et al. (2015) have shown that numeracy and literacy skills strongly predict labor market outcomes across countries. Additionally, we show that children in the treatment group had higher scores in the executive functioning and motor development measures vis-a-vis their peers in the control group, as the estimated ITT parameters reach 0.11 and 0.30 standard deviations, respectively. The estimated impact on executive functioning, which measures children's short-term memory and inhibitory behavior, may lead to economically significant effects, as this measure has been shown to strongly predict schooling achievement (Blair, 2016) as well as drug-use and criminal convictions in adulthood (Moffitt et al., 2011).[16]

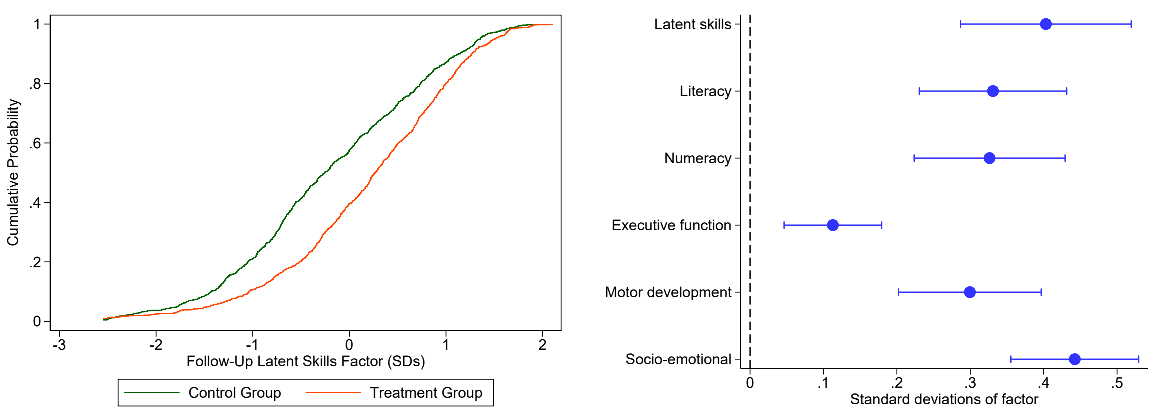
All in all, the EYPP intervention resulted in significant improvements in children's skill development as well as in various skill sub-domains. We note that various recent papers have also leveraged randomized interventions to analyze the impacts of preschool in developing countries. For instance, Dean and Jayachandran (2020) evaluate the impacts of a preschool scholarship in India. In the midline survey conducted twelve months following the intervention, they find the scholarship offer increases children's cognitive skills by 0.39 standard deviations, after one year, with the estimated impact falling to 0.2 σ after two years. Martinez et al. (2017) similarly find that a preschool construction program in rural Mozambique increased treated students' cognitive skills by 0.19 standard deviations two years following the intervention. On the other hand, Andrew et al. (2019) show that the effects of preschool attendance in Colombia vary significantly across program quality. Berkes and Bouguen (2019) study a preschool construction program in Cambodia and find ITT impacts on cognitive skill measures in the range of 0.05 standard deviations, whereas Blimpo et al. (2019) document null-to-negative impacts from community-based preschool centers in The Gambia. As a result, the ITT effects presented so far fit in with the largest estimated impacts relative to other interventions in developing countries.

While the exploratory factor analysis indicates that a single latent factor captures children's

[16]In Appendix B, we examine the robustness of these results to the inclusion of baseline covariates in equation (1). We find that the effect of the EYPP offer on children's latent skills remains large and significant, as well as across the various skill sub-domains discussed above.

multidimensional skills, we separately consider the effects of the program on socioemotional skills, given the importance of this dimension on later-life outcomes (Heckman et al., 2006). Receiving the EYPP offer increases treated children's socioemotional skills by 0.44 σ relative to their control group peers, largely fitting in with the results presented for the other skill sub-domains. We remark these results in light of Heckman et al. (2013a)'s finding that the Perry Preschool program led to positive long-term outcomes partly through its impact on non-cognitive skills.

Figure 2: Effects of the EYPP Program on Child Outcomes



(a) Distributional Effects

(b) Estimated ITTs

Note: Figure 2 presents effects of the EYPP offer on child outcomes. The left panel shows the distribution of children's latent skills in the follow-up survey across treatment group status. The right panel presents ITTs effects on child outcomes. Robust CIs clustered at the community level.

Heterogeneous Treatment Effects. While various early-life interventions in developing countries are geared towards reducing gender disparities, Asadullah and Chaudhury (2009) document a reverse gender gap in schooling attainment in Bangladesh. In Appendix C (first panel of Table C1), we examine whether the intervention had differential effects by gender. While the ITT estimate for boys is positive and significant in the latent skills factor as well as across each skill sub-domain, we find larger — and statistically different — impacts for girls in Bangladesh across all skill measures. As such, girls who had access to the EYPP program outperformed their control-group peers by 0.507 standard deviations in the latent skills factor.

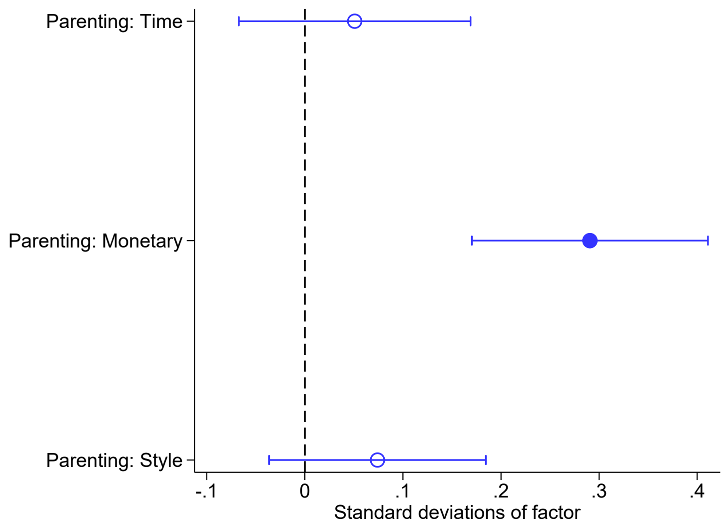
Given the existing evidence on the importance of dynamic complementarities — where a higher stock of initial skills raises the productivity of subsequent investments (Cunha and Heckman, 2007; Cunha et al., 2010) — we also examine heterogeneous impacts of the EYPP offer across students' baseline latent skills. We present the results in the last two columns of Table C1, where we find strong evidence of dynamic complementarities, as receiving the EYPP offer had significantly larger impacts for higher-skilled students at baseline. We also consider heterogeneous impacts across baseline parental investment measures (Table C2). We find larger effects for children from households with higher levels of baseline "Quality Time' and "Monetary' investments, as a one standard deviation in these measures resulted in a larger ITT effect on latent skills by 0.208 and 0.171 standard deviations, respectively. We thus remark that the EYPP program had far larger impacts for high-skilled children coming from households with higher levels of baseline investments, as well.

3.3 Estimated Effects on Parental Investment Measures

As discussed in Section 2, the Early Years Preschool Program included a parental engagement component designed to improve parenting practices. In Figure 3, we thus present the estimated intent-to-treat effects of the program on the parental investment measures identified through exploratory factor analysis. We fail to find significant impacts on the 'Quality Time' parental investment variable, which measures whether parents spend time with their children on various enrichment activities. Similarly, the estimated impacts on the 'Parenting Style' measures are positive (0.075 σ), yet statistically insignificant. On the other hand, we find large effects on the 'Monetary Investments' measure, indicating that parents whose children received the EYPP offer increased their monetary investments in their children by 0.29 standard deviations. These results fit in with recent work on the impact of early-life interventions on parental behavior. For instance, Carneiro et al. (2019) find that an early childhood intervention in Chile significantly improved parenting practices and parents' self-beliefs regarding their role in the child development process. Attanasio et al. (2020) similarly show that an early-life intervention in Colombia led to significant improvements in parents' material and time investments in their children. The positive impacts on monetary investments may partly explain the effects of the EYPP program on children's skills, as Del Boca et al. (2013) and Attanasio et al. (2020) have shown that parental resource investments are critical for children's skill development.

We extend the analysis to examine heterogeneous impacts on parental investment measures across background characteristics. We present the results in Appendix C (Tables C3 and C4). Unlike the estimated effects on children's latent skills, we fail to find significant evidence of heterogeneous effects across the time and parenting style measures. On the other hand, we find larger impacts on parental monetary investments for children with higher baseline skills. Finally, across the "Quality Time' and "Monetary Investment' measures, we find evidence that parents with higher time and monetary investment levels at baseline underwent larger improvements from receiving the EYPP offer relative to their lower-investment counterparts. All in all, our results suggest the EYPP program encouraged the process of dynamic investment, given larger impacts for higher-skilled children and higher-investment parents at baseline. We next consider the estimated effects of EYPP attendance for children drawn from different margins of care.

Figure 3: Intent-to-Treat Effects of the EYPP Program on Parental Investment



Note: Figure 3 presents ITTs effects on parental investment measures. Robust standard errors clustered at the community level.

4 Effects of EYPP Relative to Alternative Options

The results presented so far indicate that the intervention successfully improved children's multidimensional skill outcomes, along with improved parental monetary investments. However, these results are not necessarily informative about the effectiveness of the EYPP program. First, the EYPP offer was not always accepted, leading to imperfect compliance. Moreover, offer compliers would have followed different modes of care in absence of the offer, as some children may have attended other preschools whereas some of their peers would have remained at home. As a result, identifying heterogeneous impacts across groups with different fallback alternatives can recover important information regarding the extensive- and intensive-margin impacts of preschool participation.

4.1 Identification Assumptions

Local Average Treatment Effects. To fix ideas regarding the impacts of EYPP attendance, we follow Kline and Walters (2016) and Kirkeboen et al. (2016). Let Z_i equal 1 if person i receives the EYPP offer and 0 otherwise. The individual can choose among three potential types of education: home (n), EYPP (e), and alternative center-based child care (a). Let $D_i(Z_i) \in \{n, e, a\}$ represent the decision as a function of the EYPP offer, yielding 3^2 potential response types. Observed treatment status is thus given by $D_i = D_i(Z_i)$.

Let θ_i represent the observed outcome of interest. $Y_i^{d,z}$ captures potential outcomes as a function of $D_i = d$ and $Z_i = z$. As in Imbens and Angrist (1994), we impose the exclusion ($\theta_i^{d,z} = Y_i^d$), independence ($\theta_i^d, D_i(z) \perp\!\!\!\perp Z_i$ for all d, z) and the following monotonicity assumption:

Assumption 1. *Monotonicity.* $\mathbb{1}\{D_i(1) = e\} \geq \mathbb{1}\{D_i(0) = e\}$.

Assumption 1 states that receiving the EYPP offer does not make it less likely that an individual attends EYPP. Imbens and Angrist (1994) show that these assumption allows researchers to recover the local average treatment effect (LATE): $E[\theta_i^e - \theta_i^{d \neq e} \mid D_i(1) = e, D_i(0) \neq e]$.

Intensive- and Extensive-Margin LATEs. The estimated local average treatment effect presented above represents a weighted average of the effect of EYPP for students who both moved into *any* preschool attendance as well as for those switching across preschool programs. Specifically, the standard LATE estimator of Imbens and Angrist (1994) measures a weighted average of the intensive- versus extensive-margin effects of EYPP attendance (Kirkeboen et al., 2016; Hull, 2018; Mountjoy, 2018):

$$LATE = \omega \times \underbrace{LATE_{e \leftarrow n}}_{\text{extensive margin}} + (1-\omega) \times \underbrace{LATE_{e \leftarrow a}}_{\text{intensive margin}} . \tag{2}$$

where ω represents the share of compliers who would have otherwise remained at home and $LATE_{e \leftarrow k}$ for $k \in \{n, a\}$ measures the impact of EYPP attendance for k-type compliers. Kline and Walters (2016) and Mountjoy (2018) show that ω is identified from Assumption 1, by comparing the share of children in home care across treatment and control communities. However, the sub-LATEs are not directly identified, yet convey important information regarding the effectiveness of the EYPP program. For instance, if the effects of the EYPP program are driven by a large impact on n-compliers ($LATE_{e \leftarrow n}$), then policies focused on enrolling children into existing preschool programs may suffice to improve outcomes, rather than seeking to expand alternative offerings such as the EYPP.

To recover the intensive- and extensive-margin sub-LATEs, we directly follow Kirkeboen et al. (2016), who show that that these parameters can be identified under the following additional assumption:

Assumption 2. *Irrelevance.* $\mathbb{1}\{D_i(1) = n\} = \mathbb{1}\{D_i(0) = n\} = 0 \Rightarrow \mathbb{1}\{D_i(1) = a\} = \mathbb{1}\{D_i(0) = a\}$.

Assumption 2 implies that if receiving the EYPP offer does not lead an individual to change her participation decision from home care to EYPP attendance, it does not lead her to attend an alternative program either.[17] Assuming researchers have access to individuals' fallback alternatives,

[17] Other approaches have been proposed to recover sub-LATEs. Kline and Walters (2016) propose a control function estimator which exploits heterogeneous responses to Head Start offers across observable characteristics. Meanwhile, Hull (2018) proposes an estimator which interacts the instrument across stratifying controls, while assuming homogeneous sub-LATEs across observed characteristics. We remark that our goal is to understand whether the EYPP program boosted children's skills through the effect on parental investments across fallback choices. As a result, to perform the desired mediation analysis, we require individual-level information on fallback alternatives.

Kirkeboen et al. (2016) show that each sub-LATE is identified by conditioning on the choice in absence of the EYPP offer:

$$LATE_{e\leftarrow k} = E[\theta_i^e - \theta_i^k \mid D_i(1) = e, D_i(0) = k]$$

where $k \in \{a, n\}$ is child i's counterfactual preschool attendance in absence of the EYPP offer. As a result, we can recover $LATE_{e\leftarrow n}$ and $LATE_{e\leftarrow a}$ by estimating a two stage least squares specification separately for those who would have remained at home ($D_i(0) = n$) or attended alternative programs ($D_i(0) = a$).

Identifying Fallback Choices. While this framework provides a clear identification result which allows us to examine the mechanisms through which EYPP affects children's skill development, it necessitates information on individuals' fallback options. We do not have direct information on these options. As a result, we approximate them through a prediction of the the likelihood of attending an alternative preschool center in absence of the EYPP offer, as a function of observed characteristics $f(\boldsymbol{X}_i)$. To this end, we use machine learning techniques to predict participation on alternative preschools in the control group sample based on observed characteristics.[18] We follow Mullainathan and Spiess (2017), McKenzie and Sansone (2019) and consider three different machine learning (ML) approaches, including LASSO, Support Vector Machines and Boosted Regression. For each ML approach, we split the control group into a training sample (90% of individuals) and a holdout sample. We select the preferred algorithm by calculating the accuracy rate (which measures the share of correct predictions) in the hold-out sample.

LASSO has the highest accuracy rate, correctly predicting 70.2% of participation decisions in the hold-out sample.[19] Using the selected predictors through LASSO, we then predict fallback choices in the treatment group ($E[Y_i \mid D_i(0) = j]$ or $\widehat{D}_i(0) = a$). While we could thus invoke Assumptions 1 and 2 to recover $LATE_{e\leftarrow n}$ and $LATE_{e\leftarrow a}$, our machine learning procedure does not perfectly predict fallback alternatives in the control group. As such, this approach would allow us to recover biased LATE parameters.

To assess the extent of the misclassification issue, consider the following measurement error model applied to our setting. Let $Y_i \in \{\theta, D\}$, where θ represents latent skills or parental investment factors at follow-up and D_i is a binary variable which equals one if child i attended EYPP. Our objects of interests are $E[Y_i \mid D_i(0) = j]$, for $j = \{a, n\}$. These are used to compute the desired extensive- and intensive-LATEs. Define misclasification probabilities as $1 - Pr(\widehat{D}_i(0) = j \mid D_i(0) = j) = 1 - p_j$, for $j \in \{a, n\}$. In a setting similar to Horowitz and Manski (1995) and Molinari (2008),

[18] We describe the procedure in more detail in Appendix D.

[19] The penalization parameter λ, which equals 0.1, is selected through five-fold cross validation. The set of selected covariates under LASSO is as follows: full set of union fixed effects, children's age (in months); baseline test scores: phonemic awareness, number identification, vocabulary, letter identification, copying, folding, hopping, print awareness, oral comprehension, sorting, shape identification, one-to-one correspondence, short-run memory, inhibitory control, drawing, self-awareness, emotional awareness empathy; along with parental behavior measures: writing materials for child, number of puzzles, toys to teach shapes, play games with child, name objects with child, teach child new things, (no) spanking, (no) hitting, (no) criticizing.

we can relate true and miss-measured conditional expectations as follows:

$$\begin{aligned}
E[Y_i \mid D_i(0)=a] &= E[Y_i \mid D_i(0)=a, \widehat{D}_i(0)=a]p_a + E[Y_i \mid D_i(0)=a, \widehat{D}_i(0)=n](1-p_a),\\
E[Y_i \mid D_i(0)=n] &= E[Y_i \mid D_i(0)=n, \widehat{D}_i(0)=n]p_n + E[Y_i \mid D_i(0)=n, \widehat{D}_i(0)=a](1-p_n),\\
E[Y_i \mid \widehat{D}_i(0)=a] &= E[Y_i \mid D_i(0)=a, \widehat{D}_i(0)=a]p_a + E[Y_i \mid D_i(0)=n, \widehat{D}_i(0)=a](1-p_n),\\
E[Y_i \mid \widehat{D}_i(0)=n] &= E[Y_i \mid D_i(0)=n, \widehat{D}_i(0)=n]p_n + E[Y_i \mid D_i(0)=a, \widehat{D}_i(0)=n](1-p_a).
\end{aligned} \tag{3}$$

An advantage of our machine learning procedure is that we can provide information on the extent of misclassification. For the holdout sample, we directly observe individuals' latent choices in absence of the offer — whether they are in alternative preschools or home care — as well as their predicted fallback choice through LASSO. We can thus calculate various misclassification probabilities, such as the probability a child attended an alternative preschool given our machine learning algorithm classified her to have remained at home. However, having estimates of these misclassification probabilities is not enough to identify the unknowns of the system of equations (3). To recover the parameters of interest, we state the following assumption.

Assumption 3. *Random Misclassification. Y_i is mean independent from $\widehat{D}_i(0)$ conditional on $D_i(0)$:*

$$E[Y_i \mid D_i(0)=j, \widehat{D}_i(0)=a] = E[Y_i \mid D_i(0)=j, \widehat{D}_i(0)=n] = E[Y_i \mid D_i(0)=j],$$

for $j \in \{a, n\}$ and $Y_i \in \{\theta, D\}$.

The assumption above states that misclassification occurs randomly in our sample. Assumption 3 reduces the system given by (3) to:

$$E[Y_i \mid \widehat{D}_i(0)=a] = E[Y_i \mid D_i(0)=a]p_a + E[Y_i \mid D_i(0)=n](1-p_n), \tag{4}$$

$$E[Y_i \mid \widehat{D}_i(0)=n] = E[Y_i \mid D_i(0)=n]p_n + E[Y_i \mid D_i(0)=a](1-p_a). \tag{5}$$

which gives two equations for two unknowns, $E[Y_i \mid D_i(0)=a]$ and $E[Y_i \mid D_i(0)=n]$. Once we compute these conditional expectations, we can identify reduced-form and first stage conditional on the correct fallback j by computing $E(Y_i \mid Z_i=1, D_i(0)=j) - E(Y_i \mid Z_i=0, D_i(0)=j)$, for $Y_i \in \{\theta_i, D_i\}$. Given those, we point identify the required sub-LATEs.[20]

Comparison with alternative methods.

Our methods connects to a recent literature that studies the identification of heterogeneous effects across fallback choices. The methods vary in the extent of the available data and behavioral assumptions. Heckman and Pinto (2018) and Mountjoy (2018) explore different versions of

[20] We examine the robustness of our results to alternative assumptions presented in Hull (2018) in Appendix E.In particular, we take advantage of the estimated propensity score ($f(\boldsymbol{X}_i)$) of alternative preschool attendance using the LASSO-selected covariates. We interact the EYPP offer instrument with the propensity score to recover the sub-LATE under the LATE-homogeneity assumption laid out in Hull (2018).

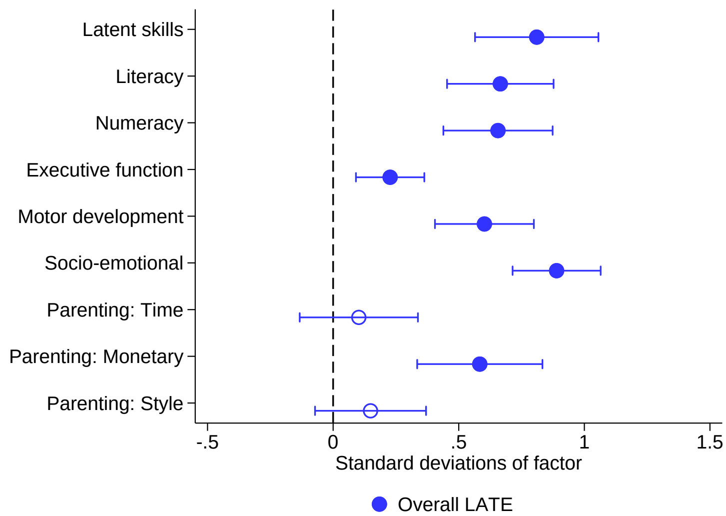
monotonicity to identify local average treatment effects conditional for different groups defined by fallback choices. Both papers require using multiple instruments, ideally with large support, to deal with multiple endogenous variables. Alternatively, Hull (2018) solves the need for multiple instruments by interacting only one with the available covariates $\mathbf{X}$— thereby generating a second instrument. However, identifying subLATEs in this IV model with multiple endogeneous variables requires the strong assumption that subLATEs are homogeneous within stratum defined in the support of $\mathbf{X}$. This is the approach followed by Dean and Jayachandran (2020) in a similar context than ours. As discussed, we instead build our method based on Kirkeboen et al. (2016), who show that standard IV suffices to identify effects relative to second-best choices provided that the econometrician has access to real information on those choices. While Hull (2018) and Dean and Jayachandran (2020) assume lack of heterogeneity depending on the information contained in $\mathbf{X}$, we restrict this heterogeneity largely depending on the capacity of our ML algorithm in predicting different fallback choices for different types of individuals. Therefore, in contexts where ML have good predictive power, the method will rely less on the random misclassification assumption, which can be viewed as another form of subLATE homogeneity (Hull, 2018).

4.2 Main Results

Response Types. We first exploit Assumption 1 to estimate the impacts of EYPP attendance on compliers. These estimates represent a scaled version of the intent-to-treat effects presented in Section 3 by the first stage parameter, which indicates that receiving the offer increased the likelihood of EYPP attendance by 0.495 percentage points. In the first column of Table 2, we present evidence on the characteristics of EYPP-offer compliers, which do not show significant differences with the full sample.

In Figure 4, we present the estimated local average treatment effect of EYPP attendance on child development and parental investment measures. The estimated LATE indicates that EYPP attendance significantly improved students' skill development, leading to a 0.81σ increase in the latent skills measure. We further find a significant impact on the non-cognitive skills measure, reaching 0.89 standard deviations through the follow-up round. Lastly, the local average treatment effect indicates a sizable impact on parental monetary investments, reaching 0.58 standard deviations, with insignificant impacts along the 'Quality Time' and 'Parenting Style' measures.

Figure 4: Local Average Treatment Effects of EYPP Attendance on Children's Skill and Parental Investment Outcomes



Note: Figure 4 presents the local average treatment effects of EYPP attendance on child development and parental investment outcomes. Robust standard errors are clustered at the community level.

Nonetheless, the nature of the choice set faced by families in the context of the randomization implies the estimated LATE is a weighted average of the impacts of EYPP on children coming from alternative forms of care. The share of children pertaining to the five response groups identified above can be non-parametrically identified under Assumption 1, as shown by Abadie (2002). Since the EYPP offer reduces the share of children in other centers from 58.0% to 40.2%, a-compliers represent 17.8% of the sample. At the same time, treated group children have a far lower likelihood of staying at home, falling from 41.9% to 10%, implying that n-compliers account for 30.9% of the sample. We thus remark that 36.6% of compliers would have attended alternative preschool programs in Bangladesh. Additionally, 40.2% of households decline EYPP offers in favor of other preschools (the share of a-never-takers), 10% of households offered EYPP decline it for no preschools (n-never-takers) and 0.1% of households attend EYPP without an offer (s-always-takers).

Building on results by Abadie (2002), Kline and Walters (2016) show how to non-parametrically identify the mean characteristics of different complier groups. We present the results in the last two columns of Table 2. The set of children who switch from alternative preschool programs into EYPP in light of the offer (a-compliers) exhibit similar characteristics relative to the full population sample, yet this is not the case for extensive-margin participants (n-compliers). First, these children come from households in which the mother is less likely to read and to have completed a secondary school degree. Importantly, they exhibit far lower skills at baseline vis-a-vis the full sample, for instance trailing their peers in the a-complier group by 0.42 σ in the baseline latent skills factor. On the other hand, we do not find evidence of significant differences across baseline investment mea-

Table 2: Compliers baseline characteristics

	Compliers	a - Compliers	n - Compliers
A. Baseline characteristics			
Household size	4.78	4.51	4.69
	(0.07)	(0.33)	(0.14)
Number of siblings	0.80	0.78	0.80
	(0.02)	(0.06)	(0.03)
Mom read	0.55	0.60	0.52
	(0.02)	(0.10)	(0.04)
Mom Ed: Secondary	0.50	0.66	0.49
	(0.02)	(0.09)	(0.04)
B. Baseline child skills			
Latent skills	-0.08	0.00	-0.42
	(0.08)	(0.21)	(0.08)
Literacy	-0.10	0.11	-0.39
	(0.07)	(0.20)	(0.06)
Numeracy	-0.07	-0.06	-0.25
	(0.08)	(0.20)	(0.09)
Executive function	-0.03	0.04	-0.19
	(0.04)	(0.11)	(0.05)
Motor development	-0.10	0.04	-0.37
	(0.04)	(0.18)	(0.06)
Socio-emotional	-0.04	-0.11	-0.23
	(0.05)	(0.16)	(0.06)
C. Baseline parenting investment			
Time	0.04	-0.08	0.04
	(0.06)	(0.23)	(0.10)
Monetary	0.02	-0.19	-0.25
	(0.04)	(0.18)	(0.07)
Style	-0.01	-0.11	0.14
	(0.05)	(0.21)	(0.09)
Share (%)	0.50	0.37	0.63

Notes: Table 2 presents baseline mean characteristics of compliers by subgroup. The first column computes mean characteristics following Abadie (2002). The next two invokes the independence assumption and results by Kline and Walters (2016) to calculate means of the two types of compliers. Robust standard errors in parenthesis, are clustered at the community level.

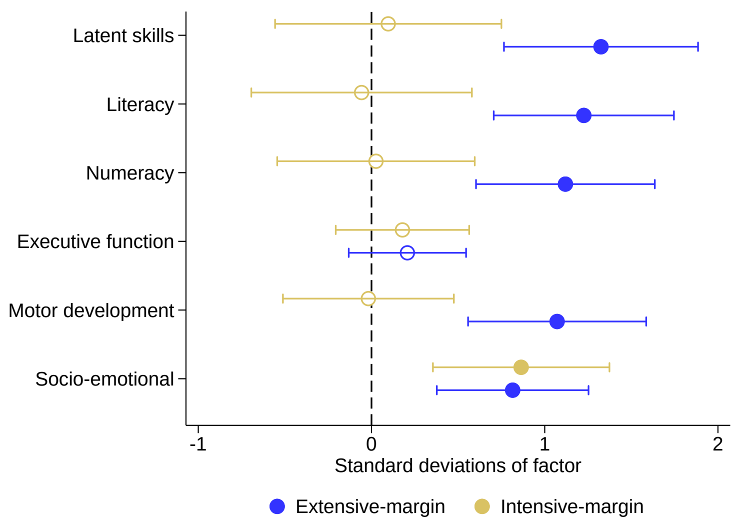
sures. Dean and Jayachandran (2020) also examine differences in complier characteristics across fallback groups. They consider compliers from public child care centers and private preschools, and find that the latter group of compliers had higher baseline test scores and parental education. Our analysis instead considers differential impacts across extensive- and intensive-margin responses to the preschool offer.

Extensive- and Intensive-Margin LATEs. Given the difference in characteristics across complier types, we consider whether the local average treatment effect of EYPP attendance varies by fallback alternative. We estimate the empirical strategy outlined above and recover the LATEs for intensive- and extensive-margin compliers. We present the results for children's skill outcomes in Figure 5. For children who would have otherwise remained at home, EYPP attendance had a sizable impact on their skill development, as the estimated sub-LATE on the latent skills factor exceeds one standard deviation. However, we fail to find significant impacts for children who switched preschool programs, as the estimated sub-LATE for a-compliers equals 0.09 σ, and it is not statistically significant. As a result, our analysis indicates that the majority of the benefit arising from EYPP participation follows from inducing families to enroll their children in *any* preschool. Our results fits in with existing evidence across various contexts, Kline and Walters (2016) only find positive impacts of Head Start attendance in the United States for children who switched out of home care. Using a bounding approach, Berkes and Bouguen (2019) similarly find positive impacts of preschool attendance in Cambodia for children who would have otherwise stayed at home. While Dean and Jayachandran (2020) consider heterogeneous LATEs across intensive-margin options and fail to find significant differences through the medium-term across fallback choices.

Additionally, we examine heterogeneous impacts of EYPP attendance for specific skill domains across heterogeneous complier types. For extensive-margin compliers, we find significant impacts on their literacy and numeracy skills, exceeding one σ through the follow-up round. While we fail to find significant impacts on their executive function, we find positive effects on extensive margin compliers' motor development. For their intensive-margin counterparts, we fail to find significant impacts across these four skill dimensions, fitting in with the estimated effects on the latent skills factor. However, for both groups of compliers, we find positive and significant effects on their socioemotional development, exceeding 0.8 standard deviations. While the positive intensive-margin impacts on children's socioemotional skills stand in contrast to the effects on the other skill measures, this effect may be explained by the program design, which encouraged a variety play-based activites, possibly helping children socialize.[21]

[21] In Table E2, we present extensive- and intensive-margin LATEs under the assumptions laid out by Hull (2018). We find positive local average treatment effects on the latent skills dimension across both complier types, which largely fit in with the results presented in this Section.

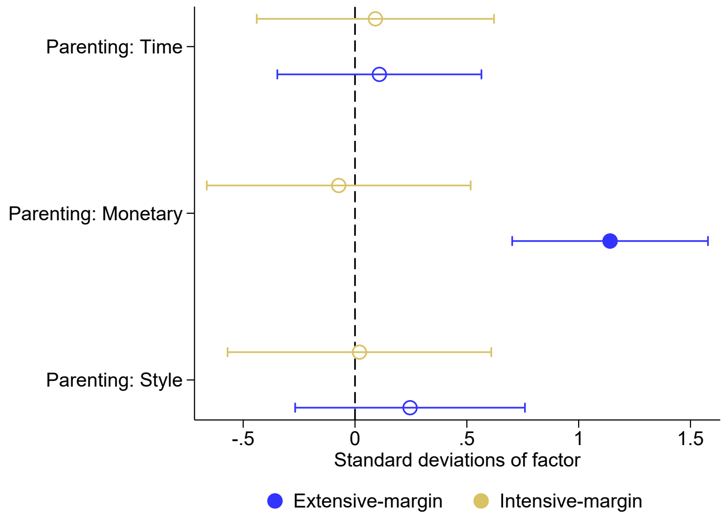
Figure 5: Sub-Local Average Treatment Effects of EYPP Attendance on Children's Skill Outcomes



Note: Figure 5 presents the local average treatment effects of EYPP attendance on child development outcomes relative to home care (a-compliers) and alternative preschool attendance (a-compliers).. Robust standard errors are clustered at the community level.

At the same time, the program may have successfully changed parental investment decisions. Given the explicit parental engagement component included in the EYPP program, we further examine whether attending the program significantly impacted parenting practices across fallback choices. We present the estimated sub-LATEs in Figure 6. Similar to the estimated intent-to-treat parameters presented in Section 3, we find positive, yet statistically insignificant impacts of the EYPP program on 'Time Quality' and 'Parenting Style' measures across fallback choices. For the 'Monetary Investments' measure, we instead find heterogeneous impacts across complier types. For children who would have remained at home, attending the EYPP program improves their parents investment in this dimension by 1.37 σ through the follow-up round. The estimated intensive-margin LATE is not statistically significant. As such, the small estimated impacts on parental investments for children switching out of alternative preschool programs implies that the positive impact on children's socioemotional development must be driven by other mechanisms. Nonetheless, given the extensive showing the importance of parental investment in the skill development process (Cunha et al., 2010; Attanasio et al., 2015, 2017, 2020), we next present a framework which allows us to precisely quantify the mechanisms through which the EYPP program affected children's skill development.

Figure 6: Sub-Local Average Treatment Effects of EYPP Attendance on Children's Skill and Parental Investment Outcomes



Note: Figure 6 presents the local average treatment effects of EYPP attendance on parental investment measures relative to home care (a-compliers) and alternative preschool attendance (a-compliers).. Robust standard errors are clustered at the community level.

4.3 Mediation Analysis with Multiple Fallbacks

This section explores the mechanisms through which EYPP affected child outcomes for different groups. We implement a mediation analysis that evaluates the importance of changes in parental investment and preschool attendance. Furthermore, we propose a mediation analysis that takes into account multiple unordered treatment alternatives. Our method works under the strong assumption of sequential ignorability (Heckman et al., 2013b). Therefore, the results from this section should be assessed through the lens of this relatively restrictive assumption.

Consider a production function of skills that depends on parental investments and EYPP attendance. Let $\theta_{i,z}$ for $Z_i = z \in \{0, 1\}$ be the potential outcome when individual i receives the EYPP offer z. We assume that the production of skills under $Z_i = z$ for families with fallback choice $D_i(0) = k$ is given by:

$$E[\theta_{i,z} \mid D_i(0) = k] = \tau_z^k + E[\boldsymbol{P}_{i,z} \mid D_i(0) = k]\boldsymbol{\beta}_z^k$$

where $\boldsymbol{P}$ is a vector of parental measures.

Following Heckman et al. (2013b), we make two assumptions to identify the necessary ingredients for identification of the channels through which the EYPP offer affects child skills. First, we assume structural invariance. This condition states that having access to the EYPP offer does not change the underlying production function ($\boldsymbol{\beta}_1^k = \boldsymbol{\beta}_0^k$).[22] Under this assumption, we can decompose

[22] In contrast to Heckman et al. (2013b), we allow for parameters to vary by counterfactual choice k.

each sub-ITT in terms of direct and indirect effects:

$$E[\theta_{i,1} - \theta_{i,0} \mid D_i(0) = k] = \underbrace{\tau_1^k - \tau_0^k}_{\text{direct effect}} + \underbrace{E[\boldsymbol{P}_{i,1} - \boldsymbol{P}_{i,0} \mid D_i(0) = k]\boldsymbol{\beta}^k}_{\text{indirect effect}}. \tag{6}$$

where the direct effect is the effect of attending preschool given a fixed level of parental investment and the indirect effect captures the portion of the causal effect of the EYPP offer explained by changes in parental investment. To identify these two terms we need a further assumption, which is a form of "sequential ignorability" (Heckman et al., 2013b): given z and $D_i(0) = k$, $\boldsymbol{P}$ is independent of unobserved factors. This last assumption can be restrictive in our setting. For example, sequential ignorability implies that families do not act on information about the quality of EYPP to assess what preschool type is better for their children. If this assumption is met, then direct and indirect effects are identified; conditional on identifying the left-hand side of the equation, both terms can be straightforwardly obtained via OLS of θ_i onto Z_i and $\boldsymbol{P}$, conditioning on $D_i(0) = k$.

As noted above, to implement the mediation analysis of equation (6) we must first identify intensive- and extensive-margin ITTs. As in the LATE analysis from previous sections, the overall ITT is a linear combination of intensive- and extensive-ITTs. Having access to fallback choices, one could directly identify the sub-ITTs using standard OLS conditioning on this information under Assumption 2.[23] Furthermore, misclassification of our ML prediction algorithm can be accounted for by exploiting Assumption 3.

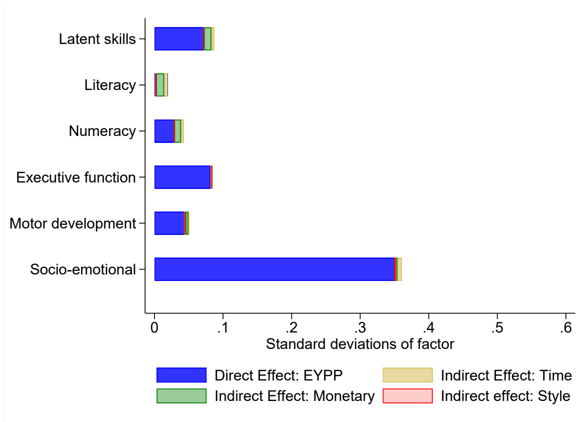
To carry out the analysis above, we run OLS regressions on parental investment and a dummy for EYPP attendance, conditioning on the predicted fallback choice. We estimate OLS equations including our three factors of parental investment: monetary, time, and style investments. The dependent variables are factor capturing latent skills and the sub-set of skills obtained via the dedicated measurement system. For children in both fallback options, the only inputs that show statistically significant coefficients are the ones associated to the EYPP offer and the monetary investment factor. Finally, we adjust conditional expectations using the same ML-based procedure we explained in Section 4. Appendix F (Table F1) presents OLS coefficients of the underlying production functions used for mediation.

Figure 7 presents the results of the mediation analysis. In the figure, the bar represents the total intent-to-treat estimate on each outcome variable. Each bar is divided by the portion explained by EYPP attendance (direct effect) and changes in parental investment (indirect effect). We find that the positive sub-ITT on children's socioemotional skills is explained through the direct impact of the intervention. On the other hand, for children coming from home, the effect of EYPP attendance cannot be solely explained through program participation. We find that the impacts of the program on parents' monetary investment decisions account for upwards of 30% of the estimated ITT across all skill dimensions. We thus remark that any preschool participation may lead to improved outcomes for children, partly through engaging their parents. As such, any positive impacts of the

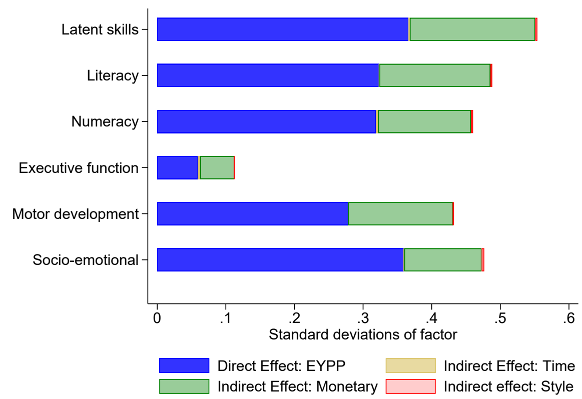
[23] Appendix F works out this result.

EYPP program on children who switched preschool programs are not driven by changes in parental behavior.

Figure 7: Mediation Analysis



(a) Intensive Margin



(b) Extensive Margin

Note: Figure 7 presents a mediation analysis. Panel (a) shows mediation analysis for the whole sample. Panels (b) and (c) show results for the sample with fallback choices "other preschools" or "home."

5 Conclusion

The sizable growth in pre-primary enrollment across the world has brought increased attention to the quality of preschool programs in which children are enrolling. In recent years, various preschool interventions in developing countries have followed experimental designs (Brinkman et al., 2017; Martinez et al., 2017; Bouguen et al., 2018; Berkes and Bouguen, 2019; Dean and Jayachandran, 2020; Blimpo et al., 2019), allowing researchers to recover credible estimates of the effects of prepri-mary education. However, these programs are often implemented in the presence of alternative options, which implies that recovering the effects of program participation on child development outcomes is not a straight-forward endeavor. As a result, the design of improved preschool programs necessitates a better understanding of whether experimentally-designed interventions deliver positive impacts relative to the existing programs.

In this context, we have examined the short-term effects of the Early Years Preschool Program in Bangladesh, which was implemented in a setting with extended availability of alternative preschool arrangements. On the other hand, the EYPP program includes various "gold-standard" components aimed at delivering quality pre-primary education, by engaging with teachers, parents and the community. The intent-to-treat estimates indicate that EYPP eligibility successfully increased offered children's multidimensional skill development, while also yielding positive impacts on parents' monetary investments in their children.

Since EYPP participants are drawn both drawn from home care and from other preschools, we consider an empirical framework which allows us recover the impact of EYPP participation for both extensive- and intensive-margin compliers. Across various skill development measures, we find larger impacts for children drawn from home care. However, for children switching across preschool programs, we find that EYPP increased their socioemotional development, remarking the importance of the quality component put forth by EYPP. At the same time, in the parental monetary investment measure, we find positive impacts for extensive-margin compliers. To uncover the mechanisms driving the impacts on child development outcomes, we perform a mediation analysis for each group of compliers. For extensive margin participants, we find that changes in parents' monetary investments account for one-third of the effect on children's multidimensional skills. On the other hand, the intensive-margin impact on children's socioemotional skills is driven almost entirely by direct program impacts. These results further highlight the importance of exploring heterogeneous impacts of program participation.

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Appendix

A Exploratory Factor Analysis

We posit the following measurement system separately for observed measures $M^j_{m,t}(m \in \{T, I\}, t \in \{1, 2\})$

$$M^j_{m,t} = \theta^k_{m,t}\alpha^k_{m,t} + \varepsilon^j_{m,t} \tag{7}$$

where $M^j_{m,t}$ is the j^{th} observed child skill ($m = T$) and parental investment ($m = I$) measure in period t, $\theta^k_{m,t}$ represents the k^{th} latent factor pertaining to the set of m observed measures, such that $k < J$. $\alpha^k_{m,t}$ is the $J \times 1$ vector of factor loadings associated with measure m for latent factor k and $\varepsilon^j_{m,t}$ represents the error term for measure j in period t which is independent of other measures j' and of the latent factors.

We implement the model laid out in equation (7) by assuming a dedicated measurement system in which each observed measure j loads on at most one factor k. Dedicated factor structures identify blocks of observed measures which are strongly correlated within factors but weakly correlated across blocks (Heckman et al., 2013a). Moreover, this approach allows for a clear interpretation of the latent factors. We first determine the number of latent factors at baseline and follow-up by following standard methods in the psychometric literature, including Kaiser's eigenvalue rule, the scree test, Horn's parallel analysis and Velicer's minimum average partial correlation rule (Andrew et al., 2019). We then estimate equation (7) using quartimin rotation to identify dedicated measures for each factor. Specifically, we drop measures which are weakly associated with latent factors (loadings below 0.4) as well as those loading on multiple factors (with at least two loadings greater than 0.4).

Skill Development Measures. We observe item-level responses for fourteen of the twenty-three skill measures available at baseline and follow-up. To measure children's performance in each assessment, we use an item response theory model (IRT) which posits a link between item-specific characteristics, such as difficulty and discrimination, and an underlying measure of latent ability for that assessment. As a result, IRT models overstep arbitrary aggregation decisions, such as the share of correct responses, which equally weighs all items in an assessment. In Table A1, we present the results from the four methods employed to determine the number of latent factors capturing children's skills. We find that two factors should be extracted from the observed test scores both at baseline and follow-up. However, none of the rotated loadings in the second factor exceed 0.4 (Tables A2-A3). We thus examine the effects of the EYPP program on a single measure of children's skills, which measures both cognitive and non-cognitive components, as shown by the rotated factor loadings presented in Tables A6-A7. We refer to this factor as a "latent skills' measure throughout the rest of the paper. Exploratory factor analysis thus indicates that cognitive and socioemotional skills do not belong to separate constructs at such an early age in Bangladesh. Nonetheless, we also examine the impacts of the program on the pre-defined skill categories in the IDELA assessment. For each of these measures — socioemotional development, motor development, emergent literacy, emergent numeracy and executive function — we estimate equation (7) across the relevant items and present the rotated loadings in Tables A10-A14.

Parental Investment Measures. In both survey rounds, we observe twenty-two measures regarding parenting behavior. While the number of estimated parental investment factors varies at baseline across the estimated methods, in the follow-up survey we identify three factors (Table A1). For consistency, we estimate equation (7) to recover three parental investment factors in both survey rounds. Tables A4 and A5 report rotated loadings for each parental investment measure at baseline and follow-up, respectively, which indicate three clear groupings. In the follow-up round, the following measures load heavily on the first factor: number of books and other reading materials owned by the households, the number and type of toys along with two measures of time investment, including the time spent teaching their children new things and naming new objects. Given the loadings configuration, we label this factor as a 'Monetary Investment' measure. We further find that variables measuring whether parents tell stories, sing songs, play games and teach numbers to their children load heavily on the second factor, which we label as a 'Quality Time' measure. Lastly, the three variables measuring whether parents spank, hit or criticize their children load on a third factor which we term a 'Parenting Style' factor.

Table A1: Number of Selected Factors Under Different Approaches

	Number of Selected Factors			
	Kaiser	Cattell	Velicer's MAP	Horn's parallel
Baseline Test Scores	2	2	1	3
Follow-Up Test Scores	2	2	2	2
Baseline Parental Investment	2	3	4	5
Follow-Up Parental Investment	3	3	3	4

Table A1 shows the number of factors selected from estimating equation (7) for baseline and follow-up test scores and parental investment measures. We present the number of factors selected under four standard approaches in the psychometric literature.

Table A2: Factor Loadings: Baseline Test Scores

	Factor1	Factor2
Number ID	.368	.733
Puzzle Solving	.342	.208
Number of Friends	.586	-.064
Vocabulary	.746	.051
Letter ID	.375	.729
Copying	.586	.209
Print Aware	.637	.015
Phonemic Aware	.364	.266
Oral Comp.	.655	.072
Sizes	.451	-.072
Sorting	.550	.016
Shape ID	.524	-.007
Correspondence	.562	.259
Add/Subtract	.633	.126
Memory	.613	-.002
Inh. Control	.543	.089
Drawing	.568	.236
Self-Aware	.517	-.026
Emotional Aware	.545	.008
Empathy	.461	.047
Folding	.554	.190
Hopping	.589	-.034

Table A2 presents the estimated factor loadings from equation (7) for the baseline test scores in the two latent factor specification as indicated by the number of factors selected in Table A1.

Table A3: Factor Loadings: Follow-Up Test Scores

	Factor1	Factor2
Number ID	.674	.549
Puzzle Solving	.507	.131
Number of Friends	.570	-.247
Vocabulary	.751	-.185
Letter ID	.627	.557
Copying	.645	.047
Print Aware	.623	-.028
Phonemic Aware	.563	.181
Oral Comp.	.653	-.139
Sizes	.381	-.199
Sorting	.493	-.088
Shape ID	.577	-.154
Correspondence	.700	.223
Add/Subtract	.637	.125
Memory	.533	.042
Inh. Control	.517	.148
Drawing	.646	.023
Self-Aware	.536	-.220
Emotional Aware	.565	-.250
Empathy	.327	-.215
Folding	.515	.148
Hopping	.552	-.158

Table A3 presents the estimated factor loadings from equation (7) for the follow-up test scores in the two latent factor specification as indicated by the number of factors selected in Table A1.

Table A4: Factor Loadings: Baseline Parental Investment Measures

	Factor1	Factor2	Factor3
Writing Materials	.092	.547	-.157
Puzzles	.039	.320	-.034
Complex Toys	-.098	.379	-.277
Toys for Shapes	.127	.590	-.030
Toys for Counting	.211	.435	-.077
Read Books	.466	.198	.180
Tell Stories	.526	.096	.173
Sing Songs	.505	.072	.048
Take on Visits	.143	.069	.118
Play Games	.518	.055	.120
Name Objects	.281	.385	.056
Teach New	.276	.294	.003
Teach Alphabet	.555	.167	-.070
Teach Numbers	.620	.072	-.034
Hug Child	.097	.097	-.256
Hrs. Talking/Walking	.099	.126	-.048
Number of Books	.160	.441	.048
Other Reading Mat.	.117	.568	-.076
Number of Toys	.120	.074	.0005
(No) Spanking	-.009	-.031	.673
(No) Hitting	.095	-.028	.710
(No) Criticizing	.040	-.125	.630

Table A4 presents the estimated factor loadings from equation (7) for the baseline parental investment measures in the three factor specification as indicated by the number of factors selected in Table A1.

Table A5: Factor Loadings: Follow-Up Parental Investment Measures

	Factor1	Factor2	Factor3
Writing Materials	.540	-.181	-.081
Puzzles	.367	.072	.128
Complex Toys	.178	.222	-.020
Toys for Shapes	.591	.081	.002
Toys for Counting	.466	.178	-.088
Read Books	.320	.373	.030
Tell Stories	.109	.516	.038
Sing Songs	.126	.548	-.104
Take on Visits	.146	.188	-.008
Play Games	.116	.493	.078
Name Objects	.515	.249	-.038
Teach New	.425	.209	-.0899
Teach Alphabet	.225	.381	-.083
Teach Numbers	.161	.520	.006
Hug Child	.015	.008	-.029
Number of Books	.483	.041	.069
Other Reading Mat.	.644	.108	.033
Number of Toys	-.090	-.129	.038
(No) Spanking	-.009	-.054	.683
(No) Hitting	.017	.027	.685
(No) Criticizing	-.067	.022	.577
Hrs. Talking/Walking	-.012	.346	.031

Table A5 presents the estimated factor loadings from equation (7) for the follow-up parental investment measures in the three factor specification as indicated by the number of factors selected in Table A1.

Table A6: Estimated Loadings for Latent Skills at Baseline

	Factor1
Number of Friends	.581
Vocabulary	.751
Copying	.612
Oral Comp.	.655
Sorting	.534
Shape ID	.504
Correspondence	.583
Add/Subtract	.637
Memory	.606
Inh. Control	.552
Drawing	.598
Self-Aware	.501
Emotional Aware	.546
Empathy	.4574
Folding	.574
Hopping	.586

Table A6 presents the estimated factor loadings from equation (7) for baseline test scores in the one factor model.

Table A7: Estimated Loadings for Latent Skills at Follow-Up

	Factor1
Number ID	.722
Puzzle Solving	.520
Number of Friends	.547
Vocabulary	.731
Letter ID	.676
Copying	.651
Print Aware	.612
Phonemic Aware	.581
Oral Comp.	.633
Sorting	.479
Shape ID	.551
Correspondence	.716
Add/Subtract	.642
Memory	.532
Inh. Control	.533
Drawing	.650
Emotional Aware	.531
Folding	.537
Hopping	.540

Table A7 presents the estimated factor loadings from equation (7) for follow-up test scores in the one factor model.

Table A8: Estimated Loadings for Parental Investment at Baseline

	Time	Monetary	Style
Tell Stories	.493	.	.
Sing Songs	.515	.	.
Play Games	.551	.	.
Teach Alphabet	.518	.	.
Teach Numbers	.635	.	.
Writing Materials	.	.553	.
Toys for Shapes	.	.610	.
Toys for Counting	.	.485	.
Number of Books	.	.437	.
Other Reading Mat.	.	.575	.
(No) Spanking	.	.	.698
(No) Hitting	.	.	.717
(No) Criticizing	.	.	.591

Table A8 presents the estimated factor loadings from equation (7) for baseline parental investment measures in the three factor model.

Table A9: Estimated Loadings for Parental Investment at Follow-Up

	Time	Monetary	Style
Tell Stories	.529	.	.
Sing Songs	.581	.	.
Play Games	.494	.	.
Teach Numbers	.477	.	.
Writing Materials	.	.491	.
Toys for Shapes	.	.607	.
Toys for Counting	.	.500	.
Name Objects	.	.552	.
Teach New	.	.454	.
Number of Books	.	.462	.
Other Reading Mat.	.	.627	.
(No) Spanking	.	.	.667
(No) Hitting	.	.	.691
(No) Criticizing	.	.	.569

Table A9 presents the estimated factor loadings from equation (7) for follow-up parental investment measures in the three factor model.

Table A10: Estimated Loadings for Non-Cognitive Measure at Follow-Up

	Factor1
Self-Aware	.571
Emotional Aware	.596
Empathy	.423
Number of Friends	.548

Table A10 presents the estimated factor loadings from the dedicated measurement system for the follow-up non-cognitive skill measures.

Table A11: Estimated Loadings for Literacy Measure at Follow-Up

	Factor1
Vocabulary	.674
Letter ID	.607
Print Aware	.617
Phonemic Aware	.610
Oral Comp.	.641

Table A11 presents the estimated factor loadings from the dedicated measurement system for the follow-up literacy skill measures.

Table A12: Estimated Loadings for Numeracy Measure at Follow-Up

	Factor1
Number ID	.671
Puzzle Solving	.505
Sizes	.363
Sorting	.474
Shape ID	.556
Correspondence	.765
Add/Subtract	.700

Table A12 presents the estimated factor loadings from the dedicated measurement system for the follow-up numeracy skill measures.

Table A13: Estimated Loadings for Executive Function Measure at Follow-Up

	Factor1
Memory	.463
Inh. Control	.463

Table A13 presents the estimated factor loadings from the dedicated measurement system for the follow-up executive function measures.

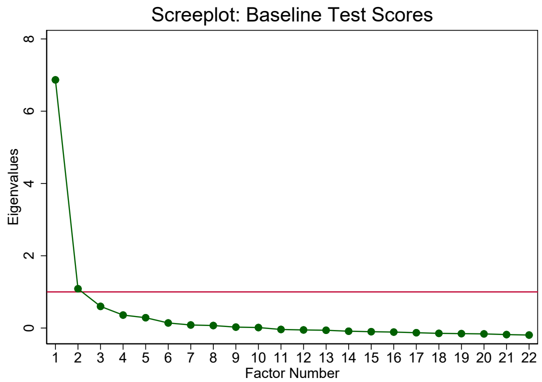
Table A14: Estimated Loadings for Motor Development Measure at Follow-Up

	Factor1
Copying	.714
Drawing	.668
Folding	.553
Hopping	.574

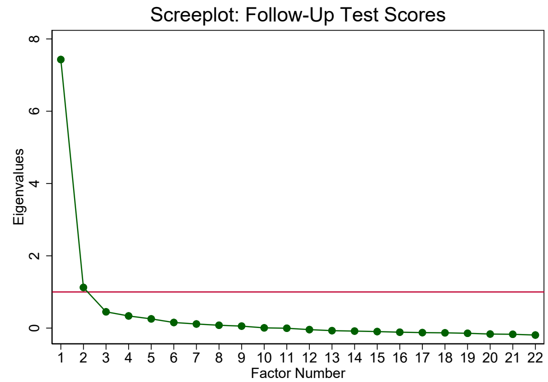
Table A14 presents the estimated factor loadings from the dedicated measurement system for the follow-up motor development measures.

Figure A1: Scree Test for Baseline and Follow-Up Measures

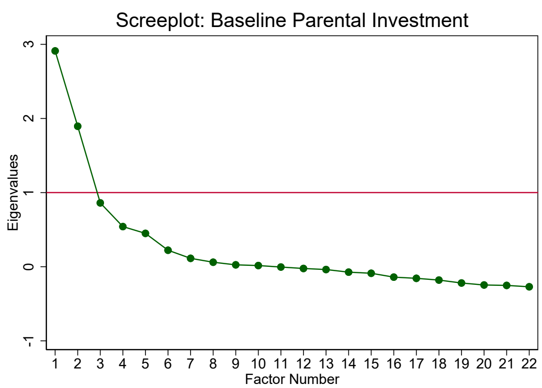
(a) Baseline Test Scores



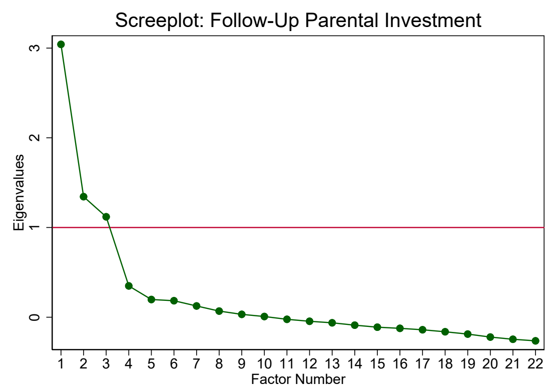
(b) Follow-Up Test Scores



(c) Baseline Parental Investment Measures



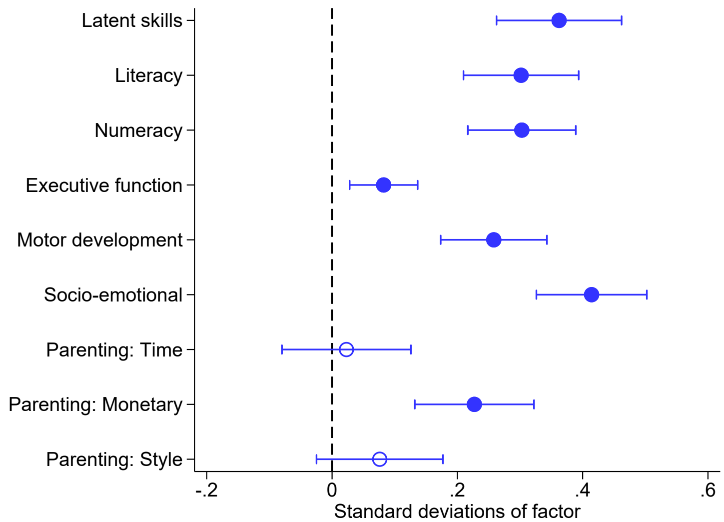
(d) Follow-Up Parental Investment Measures



Note: Figure A1 presents the estimated eigenvalues with each factor for the baseline and follow-up test score and parental investment measures.

B Intent-to-Treat Estimates: Robustness to Control Variables

Figure B1: Intention-to-Treat Effects of the EYPP Program on Child Outcomes and Parental Investment



Note: Figure B1 presents ITTs effects on child outcomes and parental investment factors including baseline family background, children's test scores and parental investment measures as control variables. Robust CIs clustered at the community level.

C Heterogeneous Skills Effects and Preschool Choices of the EYPP Offer

Table C1: Heterogeneous Effects of EYPP Program on Child Outcomes

	EYPP offer	Gender		Baseline skills	
		Offer	Offer × Girl	Offer	Offer × Skills
A. One factor					
Latent skills	0.403***	0.311***	0.196***	0.377***	0.482***
	(0.059)	(0.068)	(0.056)	(0.066)	(0.042)
B. Dedicated measures					
Literacy	0.331***	0.240***	0.193***	0.310***	0.385***
	(0.051)	(0.059)	(0.053)	(0.056)	(0.036)
Numeracy	0.326***	0.282***	0.095*	0.305***	0.402***
	(0.052)	(0.061)	(0.050)	(0.057)	(0.033)
Executive function	0.113***	0.087**	0.054*	0.100***	0.233***
	(0.034)	(0.036)	(0.031)	(0.036)	(0.024)
Motor development	0.299***	0.207***	0.195***	0.282***	0.329***
	(0.050)	(0.056)	(0.045)	(0.055)	(0.035)
Socio-emotional	0.442***	0.381***	0.131***	0.429***	0.248***
	(0.044)	(0.052)	(0.049)	(0.047)	(0.035)

Notes: Table C1 presents the estimated impacts of EYPP program eligibility on child outcomes. Panel A presents effects on latent skills obtained assuming a single factor and a measurement system that includes all available developmental measures. Panel B estimates a single factor for each pre-determined domain. Robust standard errors are clustered at the community level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table C2: Effect of EYPP Program on Child Outcomes by Parenting Measures at Baseline

	EYPP Offer	Offer × Time	Offer × Monetary	Offer × Style
A. One factor				
Latent skills	0.386***	0.208***	0.171***	0.059
	(0.059)	(0.046)	(0.034)	(0.037)
B. Dedicated measures				
Literacy	0.317***	0.146***	0.157***	0.039
	(0.051)	(0.039)	(0.034)	(0.033)
Numeracy	0.313***	0.184***	0.127***	0.078**
	(0.051)	(0.042)	(0.030)	(0.030)
Executive function	0.104***	0.094***	0.096***	0.026
	(0.034)	(0.029)	(0.020)	(0.026)
Motor development	0.288***	0.147***	0.118***	0.028
	(0.050)	(0.041)	(0.033)	(0.033)
Socio-emotional	0.435***	0.107***	0.070**	-0.008
	(0.045)	(0.034)	(0.032)	(0.034)

Notes: Table C2 presents the estimated impacts of EYPP program eligibility on child outcomes. Panel A presents effects on latent skills obtained assuming a single factor and a measurement system that includes all available developmental measures. Panel B estimates a single factor for each pre-determined domain. Robust standard errors are clustered at the community level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table C3: Heterogeneous Effects of EYPP Program on Parental Investment

	EYPP offer	Gender		Baseline skills	
		Offer	Offer × Girl	Offer	Offer × Skills
Time	0.051***	0.054	-0.006	0.048	0.050
	(0.060)	(0.062)	(0.041)	(0.060)	(0.035)
Monetary	0.291***	0.249***	0.088	0.282***	0.161***
	(0.061)	(0.071)	(0.061)	(0.061)	(0.030)
Style	0.074***	0.038	0.076	0.074	-0.005
	(0.056)	(0.063)	(0.052)	(0.056)	(0.031)

Notes: Table C3 presents the estimated impacts of EYPP program eligibility on parenting investment factors. Robust standard errors are clustered at the community level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table C4: Effect of EYPP Program on Parenting Investment by Parenting Measures at Baseline

	EYPP Offer	Offer × Time	Offer × Money	Offer × Style
Time	0.040	0.181***	0.088**	0.022
	(0.059)	(0.036)	(0.034)	(0.033)
Monetary	0.269***	0.115***	0.303***	0.026
	(0.060)	(0.042)	(0.035)	(0.041)
Style	0.073	0.022	-0.001	0.163***
	(0.054)	(0.043)	(0.033)	(0.036)

Notes: Table C4 presents the estimated impacts of EYPP program eligibility on parenting investment factors. Robust standard errors are clustered at the community level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

D Machine Learning Predictions

As discussed in Section 4, we consider different machine learning approaches to predict the likelihood of attending an alternative preschool center among the control group, which includes 842 individuals. The set of potential predictors includes the full set of union fixed effects, parental characteristics, baseline test scores (including both average and IRT-based measures) and baseline parental behavior responses. Moreover, we include interactions of baseline test scores and background characteristics as well as the squared term of baseline test scores and parental behavior measures. We thus consider a set of 735 potential covariates. We split the control group into a training sample, comprised of 90% of individuals, and a hold-out group. We briefly discuss the three machine learning algorithms we consider for prediction below (Mullainathan and Spiess, 2017; McKenzie and Sansone, 2019).

LASSO (Least Absolute Shrinkage and Selection Operator) corresponds a least squares objective function with a penalty parameter which shrinks the magnitude of the coefficients towards zero. The penalty parameter λ, or regularizer, reduces the number of parameters with coefficients larger than zero, thus selecting the covariates with the highest predictive power. We additionally consider Support Vector Machine (SVM), which is a supervised machine learning algorithm which can be used for classificiation. SVM classifies the dependent variable into categories based on a hyperplane which reduces classification errors (Athey and Imbens, 2019). We lastly use a Boosting algorithm, which is consiered as an 'ensemble' mehtod, as it combines predictions of individual classifiers. In particular, we use the gradient boosting algorithm, which fits an iterative sequence of regression trees through the residuals of each observation relative to an initial prediction using just a constant.

Across these three machine learning approaches, we follow the five-fold cross-validation approach pursued by McKenzie and Sansone (2019) to select important model parameters.[24] Specifically, we first divide the training sample into five folds. For instance, in the LASSO algorithm, we select one of the 50 potential values of λ and train the algorithm in four folds, and predicting the participation decision in the remaining fold. We repeat this procedure across the five folds and compute the mean squared error for each potential parameter value. We then select the λ with the lowest mean squared error. After selecting this parameter through cross-fold validation, we estimate each machine learning algorithm in the training sample and predict participation decisions in the 10% holdout sample. We compute confidence intervals using bootstrapping and select the algorithm with the highest accuracy rate. As a result, while SVM and boosting correctly predict 63.5% and 66.7% of participation decisions in the holdout sample, respectively, we find that the accuracy rate for LASSO is 70.2%. We thus predict counterfactual attendance choices using the LASSO algorithm detailed above.

[24]For LASSO, we use cross-validation to select the penalization term λ. We consider 50 different values for λ between zero and one. For SVM, we select the penalization term and the kernel smoothing parameter. Lastly, for the boosting algorithm, we select the number of trees and interactions.

E LATE Homogeneity (Hull, 2018) Assumption

We alternatively consider the identification of heterogenous local average treatment effects across complier types using the framework introduced by Hull (2018). Let $\boldsymbol{X}_i$ be a vector of K individual covariates, and suppose we construct an new instrument based on $Z_i f(\boldsymbol{X_i})$, where $f(.)$ is a real-valued function. With this new instrument, consider the following 2SLS model:

$$Y_i = \widetilde{\alpha}_1(1 - \mathbb{1}\{D_i = a\}) + \widetilde{\alpha}_2(1 - \mathbb{1}\{D_i = n\}) + \widetilde{\alpha}_3 f(\boldsymbol{X}_i) + \epsilon \tag{8}$$

$$E[\mathbb{1}\{D_i = s\}] = \widetilde{\beta}_1 Z_i + \widetilde{\beta}_2 Z_i \times f(\boldsymbol{X}_i) + \widetilde{\beta}_3 f(\boldsymbol{X}_i) \tag{9}$$

$$E[\mathbb{1}\{D_i = a\}] = \widetilde{\gamma}_1 Z_i + \widetilde{\gamma}_2 Z_i \times f(\boldsymbol{X}_i) + \widetilde{\gamma}_3 f(\boldsymbol{X}_i) \tag{10}$$

In general, without further assumptions regarding individual behavior, it is not possible to identify each component of LATE (Kirkeboen et al., 2016; Mountjoy, 2018). Hull (2018) proposes estimating (8)-(10) to identify $LATE_{e\leftarrow n}$ and $LATE_{e\leftarrow a}$ under the assumption of homogeneity of subLATEs across the $\boldsymbol{X}_i$. Formally, we for this procedure to work, we need to assume the following.

Assumption 4. *(LATE homogeneity) $LATE_{e\leftarrow n}$ and $LATE_{e\leftarrow a}$ are mean-independent of $f(\boldsymbol{X}_i)$.*

Hull (2018) proves that, under Assumption (4), $\widetilde{\alpha}_1 = LATE_{e\leftarrow n}$ and $\widetilde{\alpha}_2 = LATE_{e\leftarrow a}$. Intuitively, $\boldsymbol{X}_i$ stratifies the sample in a way that fallback alternatives change but LATEs do not; in this way, differences in the reduced-form effects are attributed solely to differences in complying behavior, thereby identifying subLATEs.

Estimates under Assumption 4. Following Hull (2018), we use Z_i and $Z_i \times f(\boldsymbol{X}_i)$ as instruments for $(1 - \mathbb{1}\{D_i = a\})$ and $(1 - \mathbb{1}\{D_i = n\})$. Under Assumption 4, these estimates identify $LATE_{e\leftarrow a}$ and $LATE_{e\leftarrow n}$, respectively. Table E1 shows that both instruments strongly predict both endogenous choices, and that the issue of weak instruments is not a problem in our model.

Table E2 shows the estimated subLATEs following this approach. We find that EYPP attendance has positive impacts relative to both staying at home as well as attending other programs. Relative to staying at home, EYPP attendance increases latent skills by 0.85 σ, whereas for intensive-margin compliers, the estimated impact exceeds 0.6 standard deviations. We find similar impacts across the five skill sub-domains, and the estimated magnitudes largely resemble the results presented in Section 4. For parental investment measures, we fail to find significant impacts on the quality time or parenting styles outcomes, yet there are sizable effects on the monetary investment measure for both complier types. In fact, we find larger estimated effects for children who would have otherwise remained at home, showing that across two different sets of assumptions regarding response behavior, the EYPP program successfully boosted children's skill development through different channels across extensive- and intensive-margin participants.

Table E1: First Stage of two-way 2SLS Model of EYPP and Other Preschool Attendance

	(1)	(2)
	(1-$\mathbb{1}\{\text{Other} = 1\}$)	(1-$\mathbb{1}\{\text{No Preschool} = 1\}$)
EYPP Offer	-0.284***	0.949***
	(0.050)	(0.045)
EYPP Offer $\times$ Prop. Score	0.828***	-1.117***
	(0.074)	(0.063)
Sanderson and Windmeijer Statistic	173.022	
Sanderson and Windmeijer p-value	0.000	
Kleibergen-Paap F-Statistic	97.662	
Observations	1,797	

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E2: Effect of EYPP Program on Skill and Parenting Outcomes by Fallback Options Under Assumption 4

	Latent Ability	Literacy	Numeracy	Executive Function	Motor Development	Non-Cognitive Skills	Quality Time	Monetary Investment	Parenting Style
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$LATE_{e \leftarrow a}$	0.601***	0.412***	0.502***	0.147	0.463***	0.835***	-0.051	0.367**	0.244
	(0.169)	(0.151)	(0.164)	(0.097)	(0.125)	(0.137)	(0.168)	(0.180)	(0.173)
$LATE_{e \leftarrow n}$	0.853***	0.751***	0.679***	0.237***	0.628***	0.879***	0.184	0.676***	0.093
	(0.116)	(0.098)	(0.108)	(0.067)	(0.113)	(0.102)	(0.127)	(0.114)	(0.121)
Observations	1,797								

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

F Mediation Analysis

The mediation literature has focused on studying the mechanisms through which randomized treatments affect outcomes. In our context, this objective translates into decomposing the intent-to-treat effect: $E[\theta \mid Z_i = 1] - E[\theta \mid Z_i = 0]$. Define $\theta_{i,z}$ for $Z_i = z \in \{0, 1\}$ the potential outcome when individual i receives the EYPP offer z. Because of random assignment, the causal effect of Z_i in Y_i is identified: $E[\theta \mid Z_i = 1] - E[\theta \mid Z_i = 0] = E[\theta_{i,1} - \theta_{i,0}]$. This parameter can be decomposed in terms of individual drawn from different margins of choice:

$$E[\theta_{i,1} - \theta_{i,0}] = \underbrace{E[\theta_{i,1} - \theta_{i,0} \mid D_i(0) = a]P(D_i(0) = a)}_{\text{intensive-margin ITT}} + \underbrace{E[\theta_{i,1} - \theta_{i,0} \mid D_i(0) = n]P(D_i(0) = n)}_{\text{extensive-margin ITT}}.$$

Our goal is to perform mediation analysis in each term on the right-hand side—nonetheless, without further assumptions these terms are not identified. To exploit information on fallback options, let us express $\theta_{i,z}$ in terms of potential outcomes and choices:

$$\theta_{i,z} \equiv \theta^e_{i,z} + (\theta^n_i - \theta^e_i)\mathbb{1}\{D_i(z) = n\} + (\theta^a_i - \theta^e_i)\mathbb{1}\{D_i(z) = a\},$$

which means that ITT at the individual level follows:

$$\begin{aligned}\theta_{i,1} - \theta_{i,0} = &(\theta^n_i - \theta^e_i)(\mathbb{1}\{D_i(0) = n\} - \mathbb{1}\{D_i(1) = n\}) \\ &+ (\theta^a_i - \theta^e_i)(\mathbb{1}\{D_i(0) = a\} - \mathbb{1}\{D_i(1) = a\}).\end{aligned}$$

To fix ideas, suppose we condition on $D_i(0) = n$. Under Assumptions 1 and 2 the second term on the right-hand side is canceled and we can identify the causal effect of the EYPP offer, for those who $D_i(0) = n$, by exploiting the random assignment of Z_i and access to information on fallback choices:

$$E[\theta_i \mid Z_i = 1, D_i(0) = n] = E[\theta_{i,1} - \theta_{i,0} \mid D_i(0) = n] = E[\theta^s_i - \theta^n_i](1 - P(D_i(0) = n)).$$

By the same argument, we can identify $E[\theta_{i,1} - \theta_{i,0} \mid D_i(0) = a]$ with the irrelevance assumption and information on those who $D_i(0) = a$.

Table F1: OLS coefficients of production functions

	Latent skills	Literacy	Numeracy	Executive	Motor	Socio-emotional
A. Intensive-margin						
EYPP offer	0.19***	0.13**	0.15***	0.07**	0.14***	0.36***
	(0.06)	(0.05)	(0.05)	(0.03)	(0.04)	(0.05)
Parenting: Time	-0.03	-0.05	-0.02	0.05	-0.03	-0.06
	(0.05)	(0.04)	(0.04)	(0.03)	(0.04)	(0.03)
Parenting: Monetary	0.29***	0.29***	0.24***	0.06**	0.19***	0.14***
	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
Parenting: Style	-0.02	-0.01	0.00	0.01	-0.04	0.01
	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
B. Extensive-margin						
EYPP offer	0.37***	0.32***	0.32***	0.06	0.28***	0.36***
	(0.07)	(0.06)	(0.06)	(0.05)	(0.06)	(0.06)
Parenting: Time	0.04	0.02	0.06	0.07	-0.04	0.01
	(0.05)	(0.05)	(0.04)	(0.03)	(0.05)	(0.05)
Parenting: Monetary	0.43***	0.38***	0.32***	0.12***	0.36***	0.27***
	(0.04)	(0.04)	(0.04)	(0.03)	(0.05)	(0.04)
Parenting: Style	0.03	0.02	0.03	-0.01	0.00	0.03
	(0.04)	(0.04)	(0.03)	(0.02)	(0.04)	(0.03)

Notes: Table F1 presents OLS coefficients of production functions. Panel A shows estimated coefficients for children who are predicted to be attending other preschool centers when not having the EYPP offer. Panel B presents coefficients for children choosing home when not having the offer. The dependent variables are the factor capturing latent skills and the sub-set of skills obtained via the dedicated measurement system (Literacy, Numeracy, Executive function, Motor development and Socio-emotional). Each regression includes the EYPP offer dummy and three factors of parental investment: monetary, time, and style investments. Robust standard errors in parenthesis clustered at the community level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.