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

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March 13, 2020

Abstract

The effectiveness of early childhood interventions depends on how parents respond to the policy and how children are affected in their skills set compared to the many alternative choices families have in the absence of the intervention. In this paper, we assess the multidimensional effects on an early childhood program, taking into account how parents and children change decisions along different fallback choices. We study these in the context of the Early Years Preschool Program (EYPP), in Bangladesh. The EYPP implemented a new skills-based curriculum for four-year olds in existing preschools coupled with parental training on skill development best-practices. Exploiting the random allocation of the program across communities, we find large intent-to-treat effects on children's cognitive and socioemotional development, along with significant impacts on parental investment measures. 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 the intensive—families who would have their children at alternative programs—and extensive margins—families who would have their children at home. The program had larger effects on children who would stay home in the absence of the EYPP offer. Parental investment impacts of the program operate mainly through the extensive margin, partially explaining the lower intensive-margin impacts of EYPP.

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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 (Knudsen et al., 2006; Almond and Currie, 2011) has led governments across the world to implement early childhood interventions aimed at closing gaps for children from different socioeconomic backgrounds (Heckman, 2008; Engle et al., 2011; Elango et al., 2015). However, the effectiveness of these programs depends on various factors. 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 aimed at four-year old children in Bangladesh. We analyze the Early Years Preschool Program (EYPP), which was implemented by Save the Children in existing schools across randomly selected communities in the Meherpur district. EYPP sought to develop children's skills through a play-based curriculum and to improve parenting practices through monthly teacher-parent meetings. We thus examine the impact of the EYPP program on children's multidimensional skill development, and consider the mediating effect explained by changes in parenting practices. We further note that EYPP was implemented in a context with various existing preschool programs. As a result, we consider the impacts of the program for both children drawn from other preschools — intensive margin — with those induced from home care, allowing us to recover extensive margin impacts of preschool attendance.

Receiving the EYPP offer increases the likelihood of program attendance by 50 percentage points. We observe upwards of fifteen measures of children's skills and parental behavior in the follow-up round conducted after program completion, yet each variable may measure underlying skills and parental investment with substantial error (Cunha et al., 2010; Schennach, 2016). We tackle this issue by performing an exploratory factor analysis (EFA) to recover latent factors while accounting for measurement error. 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. To examine heterogeneous impacts across skill sub-domains, we separately assume a dedicated measurement system, and recover literacy, numeracy, executive function, motor development and socioemotional skill measures. We conduct the same analysis for the parental behavior measures and 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, as 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. Noting that 58% and 40% of control and treatment group children attended alternative programs, respectively, we follow Kline and Walters (2016) and show that 37% of compliers would have attended alternative preschool programs in Bangladesh. In fact, intensive-margin compliers come from higher-SES households and have higher skills at baseline relative to the group of extensive-margin compliers.

To evaluate the importance of program substitution, we estimate local average treatment effects (LATE) for children drawn from different choice margins. We use a machine learning algorithm to predict children's counterfactual attendance in absence of the EYPP offer. 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. The effects of EYPP attendance for children who would have remained at home are significant, exceeding one standard deviation in the latent skills factor. While the intensive-margin LATE is smaller in magnitude — equaling 0.49 σ, it remains significant. We find positive intensive- and extensive-margin local average treatment effects across all skill sub-domains, and the estimated LATEs are not different from each other in four of the five measures, thus remarking the importance of the 'quality' component of EYPP. Similar to the ITT results, we fail to find significant impacts on measures of parental styles and quality time spent with the children. On the other hand, the extensive-margin LATE indicates a sizable impact on parents' monetary investment, exceeding 0.8 standard deviations, along with a smaller, yet statistically significant, intensive-margin LATE (0.33 σ).

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 35% of the program's impact can be explained through the effect on parental monetary investments. On the other hand, for children who would have attended alternative programs, the impact on parenting practices explains just 10% of the overall impact on their latent skills development. As such, we remark that the effectiveness of the EYPP program for intensive-margin compliers is likely explained through the implementation of a high-quality curriculum. Nonetheless, this program successfully improved multidimensional skills for children drawn from different choice margins, thus offering a potential pathway for improving early childhood outcomes in Bangladesh.

Our paper makes various contributions to the literature on the effectiveness of early childhood interventions. Our main contribution stems from analyzing the effect of preschool attendance on parental investment along extensive- and intensive-margins. In our setting, we ca directly analyze the mediating role of parental investments and EYPP attendance on child development outcomes for students drawn from alternative forms of care. We thus contribute to a strand of the literature which has examined the impacts of early childhood interventions on parenting behaviors (Campbell and Ramey, 1994; Gertler et al., 2014; Carneiro et al., 2019; Chaparro et al., 2020). At the same time, we fit in with a growing literature analyzing the impacts of early childhood programs in the face of alternative options, including Kline and Walters (2016) in the United States, Dean and Jayachandran (2020) in India and Berkes and Bouguen (2019) in Cambodia. Since we analyze an experimental preschool program in Bangladesh, we contribute to the existing literature on randomized preschool interventions in developing countries (Brinkman et al., 2017; Martinez et al., 2017; Bouguen et al., 2018; Blimpo et al., 2019). Lastly, we contribute to previous work regarding the importance of parental investment on child development outcomes, including Attanasio et al. (2017) and Attanasio et al. (2020), who add to this literature by estimating latent skills production functions and quantifying the mediating role of parental investments in explaining the effects of early childhood interventions. All in all, our methodology allows for assessing substitution bias both in child skills and parental behavior. Finally, we combine all of these elements in a single mediation analysis that distinguishes effects across many dimensions of child development for children who are induced from intensive and extensive margins, separately.

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 3, we present our approach to estimate latent skills free of measurement error. Section 4 presents intent-to-treat estimates of the EYPP intervention on child development and parental investment outcomes. In Section 5, we present an empirical framework to recover the local average treatment effects across fallback choices and show the estimated results. In Section 6, we use the estimated effects across fallback choices to implement a mediation analysis. Lastly, in Section 7, we discuss the results and conclude.

2 Context, Intervention and Summary Statistics

Context and Intervention

Bangladesh has undergone significant economic progress in the past two decades, as GDP per capita growth has averaged 5% over this time period.[1] 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, preschool education could improve achievement indicators by better preparing students for formal schooling. In fact, the government of Bangladesh has prioritized the expansion of pre-primary education through the development of the National Pre-primary Operational Framework in 2008, which called for students to eventually participate in two years of preschool.

Since enrollment in the second year of preprimary education has so far been limited, in 2017 the

[1] Retrieved from https://data.worldbank.org/indicator/NY.GDP.PCAP.KD.ZG.

non-governmental organization Save the Children developed a pilot program for implementing the Early Year Preschool Program (EYPP) targeted at four-year olds.[2] The 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.[3] EYPP offers quality pre-primary education for four year olds in small classes, ranging between 15-20 students. Classes are conducted throughout the calendar year five days a week for two hours each. Moreover, since these take place in existing government preschools, there is no infrastructure cost associated with the program.

The pilot program was carried out in the district of Meherpur, where Save the Children had been active since 2007.[4] Since the EYPP had been piloted in some areas, Save the Children focused the intervention on three upazilas (Gagni, Meherpur Sadar and Mojibnagar) without the program. 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. Within this sample, 50 communities 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. 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.

The intervention was limited to children living within 15 minutes of the local primary school in order to promote program take-up. To determine the set of eligible children, Data International 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. The EYPP program targeted children born in 2013 (four years old at the start of 2018), 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.[5] This restriction resulted in a final sample of 1,903 children, largely equally split by gender.

As noted above, EYPP program is conducted five days a week for two hour sessions. The preschool program created a curriculum designed to develop students' skills through a variety of activities, including signing, rhyming, storytelling and playing, among others. This curriculum is directly aligned with the age-five government preschool program. 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

[2] The program description follows from an implementation report by (for Research, 2018).

[3] The EYPP program 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 and 2016.

[4] 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).

[5] 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.

to incorporate various learning materials in their teaching. Interestingly, 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. In Section 4, we thus consider the impacts of EYPP on parental investment measures. We lastly remark that EYPP involves the communities by creating community-based school management committees in order to set-up the program.

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 twos 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.[6]

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.[7] 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.[8,9] For a number of items, we take advantage of students' binary (correct/incorrect) responses to each question to construct a continuous test score using item response theory. [10]

[6]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.

[7]The IDELA assessment was developed by Save the Children in 2011. The goal of the organization was to develop an assessment which could be comparable across country while covering critical items related to children's cognitive, reading, math and socioemotional skills. IDELA was adapted over a period of three years across eleven developing countries and the final version includes 22 items covering four sub-domains: gross and fine motor development, emergent literacy, emergent numeracy and socioemotional development.

[8]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 measures considers their short-term memory and inhibitory control.

[9]We also consider a measure of children's approach to learning, which is constructed from surveyors' perception of children's attention, confidence, concentration, persistence, motivation and interest.

[10]Across both survey rounds, we observe students' binary responses in the following items: print awareness, phonemic awareness, oral comprehension, measurement and comparison, classification and sorting, shape identification, one-to-one-correspondence, addition and subtraction, short-term memory, inhibitory control, drawing a human shape, self-awareness, emotional awareness and empathy. For the number identification, puzzle solving, number of friends,

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. 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.[11]

Summary Statistics

In Table 1, we present evidence on baseline covariate balance among the 1,815 children who were successfully followed through the midline round. We first note that around ten percent 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 estimate the empirical strategy in Section 4 both including and excluding baseline covariates.

In the last panel of Table 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%).[12] A far smaller share of treated group children remained at home during 2018 (10%), compared to 42% of their control group peers.

vocabulary, letter identification, copying a shape, writing, folding and hopping measures, we observe continuous test scores and thus do not use the item response theory model for these items.

[11]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. It has been in operation since 1997.

[12]There is significant heterogeneity in the types of alternative programs 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).

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	13.26	13.41	13.08	0.33	0.40
Emergent Numeracy	10.20	10.27	10.13	0.14	0.27
Executive Function	3.65	3.73	3.55	0.19	0.99
Approaches to Learning	18.15	18.16	18.15	0.01	0.03
Motor Development	11.72	11.88	11.54	0.35	0.74
Socioemotional Index	5.44	5.58	5.28	0.30	1.63
Preschool Attendance					
SAVE Preschool	0.27	0.50	0.00	0.50	17.43
Public Preschool	0.18	0.16	0.21	-0.04	-1.52
Private Preschool	0.10	0.07	0.13	-0.06	-2.17
BRAC Preschool	0.05	0.03	0.07	-0.04	-2.21
Islamic Preschool	0.15	0.14	0.17	-0.03	-0.86
No Schooling	0.25	0.10	0.42	-0.32	-10.30
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. Preschool attendance is measured in the first follow-up round, conducted in late 2018.

3 Exploratory Factor Analysis

Our aim is to recover the impacts of the EYPP program on children's skill development and 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 address this issue, we follow Heckman et al. (2013a); Andrew et al. (2019); Attanasio et al. (2020), among others, and apply exploratory factor analysis (EFA) to the observed skill (M_T^j) and parental investment measures (M_I^j). EFA seeks to reduce the dimensionality of observed measures by identifying latent factors which load on observed variables which are strongly correlated (Gorsuch, 2003). For observed variables $M_m^j(m \in \{T, I\}, j \in \{1, ..., J\})$, we thus posit the following measurement system:

$$M_m^j = \theta_m^k \alpha_m^k + \varepsilon_m^j \tag{1}$$

where M_m^j is the j^{th} observed measure of type $m \in \{T, I\}$, θ_m^k represents the k^{th} latent factor pertaining to the set of m observed measures, such that $k < J$. α_m^k is the $J \times 1$ vector of factor loadings associated with measure m for latent factor k and ε_m^j represents the error term for measure j which is independent of other measures j' and of the latent factors.

We implement the model laid out in equation (1) 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. We then estimate equation (1) 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. As noted above, we observe item-level responses for fourteen of the twenty-three skill measures available at baseline and follow-up. To measure children's latent ability in each assessment, we use an item response theory model (IRT) which links the difficulty of correctly answering a question with the probability of a correct answer to an underlying measure of latent ability for that assessment.[13] In Table B1, we present the results from the four

[13] As such, IRT models provide a less arbitrary measure of performance vis-a-vis averaging the number of correct answers in each assessment.

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 B2-B3). As a result, 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 B6-B7. 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 (1) across the relevant items and present the rotated loadings in Tables B10-B14.

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 B1). For consistency, we estimate equation (1) to recover three parental investment factors in both survey rounds. Tables B4 snd B5 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 this loadings configuration. As a result, 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. In the next section, we examine whether the EYPP offer had significant impacts across the latent dimensions of children's skills and parental investment identified above.

4 Intent-to-Treat Effects

4.1 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:

$$\theta_{ikc} = \alpha_0 + \alpha_1 Z_c + \phi_d + \varepsilon_{ikc} \tag{2}$$

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_{ic} 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 (2) including the full set of covariates included in Table 1.

4.2 Estimated Effects on Children's Skill Development.

In Figure A1, we first examine whether the distribution of latent skills 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 average difference across the two groups amounts to 0.407 standard deviations. To examine whether these differences are statistically significant, we present the estimated intent-to-treat estimates from equation (2) in Figure 1.

We find that the EYPP offer had significant effects on children's latent skill development, which increased by 0.403 standard deviations relative to the control group. 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 σ. 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).[14]

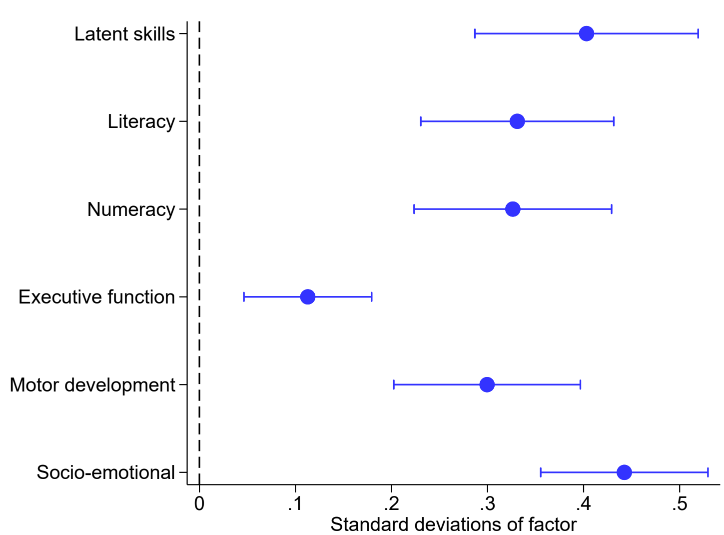
All in all, receiving the offer to participate in the EYPP program resulted in significant improvements in children's skill development as well as in various skill sub-domains. We note that various recent papers have also leveraged randomized interventions to analyze the impacts of preschool in developing 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.

[14] In Figure A2, we examine the robustness of these results to the inclusion of baseline covariates in equation (2). 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.

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 measures children's 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. These results differ significantly from those presented by Dean and Jayachandran (2020) in the Indian context, where a high-quality preschool intervention did not improve socioemotional skills in the short- or medium-run. In the context 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, the EYPP program may improve long-term outcomes through a similar mechanism.

Figure 1: Intent-to-Treat Effects of the EYPP Program on Child Outcomes



Note: Figure 1 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 the first panel of Table A1, 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 A1, where we find strong evidence of dynamic complementarities, as receiving the EYPP offer had significantly larger impacts for higher-skilled students at baseline.

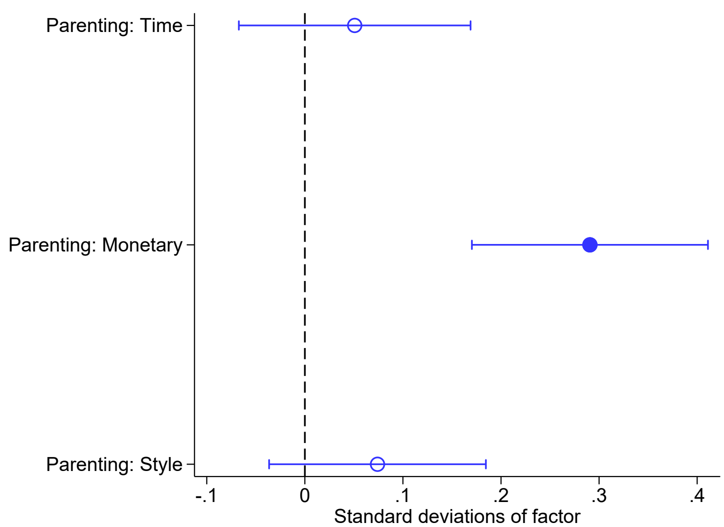
In light of the importance of parental investment in driving children's skill development across various contexts (Cunha et al., 2010; Attanasio et al., 2015, 2017, 2020), we similarly consider heterogeneous impacts across baseline parental investment measures. We present the results in Table A2. We 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 σ, 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.

4.3 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. This component focused on improving parenting practices and provided parents with additional materials for home learning in mathematics and language. In Figure 2, 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. The positive impacts on the monetary investment measure fit in with previous evidence regarding the potential impact for regarding the effects 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); 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 Table A3. 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. In Table A4, we further analyze whether there are larger impacts for parents with high investment levels at baseline. 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, these results suggest the EYPP program encouraged the process of dynamic investment, as we generally find larger effects for high-skilled children with higher-investing parents at baseline. We next analyze the impacts of attending the Early Years Preschool Program in a context with alternative preschool programs.

Figure 2: Intent-to-Treat Effects of the EYPP Program on Parental Investment



Note: Figure 2 presents ITTs effects on parental investment measures. Robust standard errors clustered at the community level.

5 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.[15]

5.1 Local Average Treatment Effects

To fix ideas regarding response types, we follow Kline and Walters (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 (s), and alternative center-based child care (a). Let $D_i(Z_i) \in \{n, s, a\}$ represent the decision as a function of the EYPP offer, yielding 3^2 potential response types. Observed treatment status is thus given by $D_i = D_i(Z_i)$.

We anchor choice behavior in the framework presented by Kline and Walters (2016). In particular, we assume the EYPP offer does not induce switching among home (n) and alternative care (a). Moreover, receiving the EYPP offer does not drive individuals out of EYPP attendance. These restrictions on behavior can be summarized by the following unordered version of monotonicity:

Assumption 1. *Monotonicity.* $D_i(1) \neq D_i(0) > 0 \Rightarrow D_i(1) = s$.

Following the standard framework of Imbens and Angrist (1994), Assumption 1 allows us to rule out the presence of defiers. In particular, the assumption implies that any individuals changing their behavior in response to the EYPP offer do so to attend the EYPP program. As a result, we can stratify the set of allowed response types into five different groups:

$$\begin{aligned}
a - compliers &: D_i(1) = s, D_i(0) = a, \\
n - compliers &: D_i(1) = s, D_i(0) = n, \\
a - never\ takers &: D_i(1) = a, D_i(0) = a, \\
n - never\ takers &: D_i(1) = n, D_i(0) = n, \\
s - always\ takers &: D_i(1) = s, D_i(0) = s
\end{aligned}$$

where a- or n-compliers are induced to participate in EYPP due to the offer, yet they switch out different alternatives depending on their counterfactual choices. Meanwhile, a- or n-never takers take up their alternative option irrespective of the offer and s always-takers attend the EYPP program in all cases.

Let Y_i represent the observed outcome of interest. $Y_i^{d,z}$ captures potential outcomes as a function of $D_i = d$ and $Z_i = z$. In this context, Imbens and Angrist (1994) show that imposing Assumption 1 along with exclusion ($Y_i^{d,z} = Y_i^d$) and independence ($Y_i^d, D_i(z) \perp\!\!\!\perp Z_i$ for all d, z) assumptions,

[15] Throughout this section, we refer to extensive-margin effects as the impacts for children switching from home care into EYPP participation. Meanwhile, intensive-margin effects recover impacts for students switching out of alternative preschool programs into EYPP.

researchers can recover the local average treatment effect (LATE) of program participation under the following instrumental variables (IV) specification:

$$Y_i = \alpha_1 \mathbb{1}\{D_i = s\} + \varepsilon_i \tag{3}$$

$$E[\mathbb{1}\{D_i = s\}] = \beta_1 Z_i. \tag{4}$$

In our context, the local average treatment effect of EYPP participation is thus given by:

$$\alpha_1 = E[Y_i^1 - Y_i^0 \mid D_i(1) = s, D_i(1) \neq s]$$

5.2 Intensive- and Extensive-Margin LATEs

The estimated local average treatment effect recovered by α_1 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):

$$LATE = \omega \times \underbrace{LATE_{s \leftarrow n}}_{\text{extensive margin}} + (1-\omega) \times \underbrace{LATE_{s \leftarrow a}}_{\text{intensive margin}} . \tag{5}$$

where ω represents the share of compliers who would have otherwise remained at home and $LATE_{s \leftarrow k}$ for $k \in \{n, a\}$ measures the impact of EYPP attendance for k-type compliers. We remark the importance of estimating each sub-LATE, as they may offer the valuable information for policymakers. For instance, if the effects of the EYPP program are driven by a large impact on n-compliers ($LATE_{s \leftarrow n}$), 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.

Various approaches have been put forth to identify the sub-LATE parameters described above. 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. Our preferred empirical strategy thus follows Kirkeboen et al. (2016). The authors show that under an assumption of irrelevance of independent alternatives, instrumental variable specifications can recover "sub-LATE' parameters presented above as long as researchers have information on individuals' fallback alternatives. Their approach requires 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. Assuming researchers have access to individuals' fallback alternatives, Kirkeboen et al. (2016) show that by conditioning on the choice in absence of the EYPP offer, we can identify each sub-LATE as follows:

$$LATE_{s \leftarrow a} = \frac{E[Y_i \mid \mathbb{1}\{D_i(1) = s\} = 1, D_i(0) = a] - E[Y_i \mid \mathbb{1}\{D_i(1) = s\} = 0, D_i(0) = a]}{E[\mathbb{1}\{D_i(1) = s\} = 1 \mid D_i(0) = a] - E[\mathbb{1}\{D_i(0) = s\} = 1 \mid D_i(0) = a]},$$

$$LATE_{s \leftarrow n} = \frac{E[Y_i \mid \mathbb{1}\{D_i(1) = s\} = 1, D_i(0) = n] - E[Y_i \mid \mathbb{1}\{D_i(1) = s\} = 0, D_i(0) = n]}{E[\mathbb{1}\{D_i(1) = s\} = 1 \mid D_i(0) = n] - E[\mathbb{1}\{D_i(0) = s\} = 1 \mid D_i(0) = n]},$$

As a result, we can identify $LATE_{s \leftarrow n}$ and $LATE_{s \leftarrow a}$ by estimating equations (3) and (4) and conditioning on $D_i(0) = a$ or $D_i(0) = n$.

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 $\mathbb{1}\{D_i(1) = a\} = 1$ — the likelihood of attending an alternative preschool center — which is 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.[16] 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 group. We select the preferred algorithm by calculating the accuracy rate (which measures the share of correct predictions) in the holdout group. For LASSO, which has the highest accuracy rate — correctly predicting 70.5% of participation decisions in the control group — we select the penalization parameter λ through five-fold cross validation.[17] Using the selected predictors through LASSO, we then predict fallback choices in the treatment group. We can thus condition on individuals' alternative attendance decisions and recover sub-LATEs following Kirkeboen et al. (2016). We examine the robustness of our results to alternative assumptions presented in Hull (2018) in Appendix D.[18]

[16] We describe the procedure in more detail in Appendix C.

[17] 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) criticizng.

[18] 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).

5.3 Results

Estimation Strategy and Response Types. We first exploit Assumption 1 to estimate the impacts of EYPP attendance on those who comply with the EYPP offer. These estimates represent a scaled version of the intent-to-treat effects presented in Section 4 by the first stage parameter (equation (4)), 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 A3, 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 0.81 σ 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.

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 measures. 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

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 using Abadie (2002). The next two columns uses monotonicity 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.

and parental education. Our analysis instead considers differential impacts across extensive- and intensive-margin responses to the preschool offer.

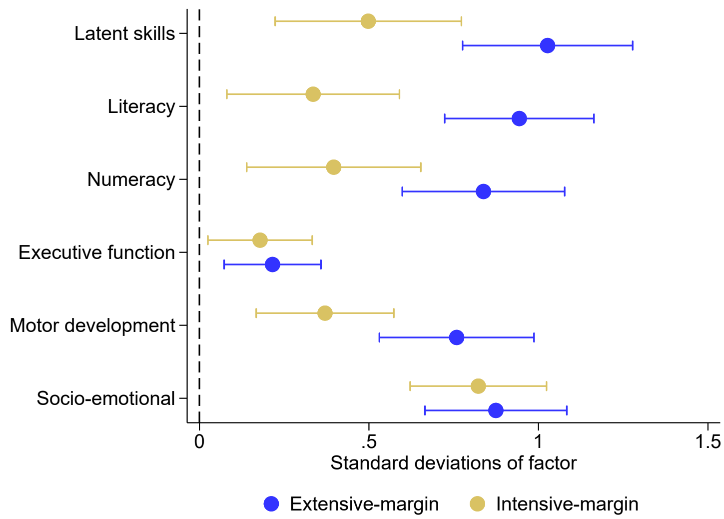
Extensive- and Intensive-Margin LATEs. Given the difference in characteristics across complier types, we consider whether the local average treatment effect of EYPP attendance varies by fallback alternative. As discussed above, to recover the desired sub-LATEs we invoke an additional irrelevance assumption (Kirkeboen et al., 2016), which requires information on counterfactual attendance decisions. We construct children's fallback alternatives by using actual attendance decisions for control group children along with the LASSO-based prediction for those in the treatment group.

We present the estimated sub-LATEs for children's skill outcomes in Figure 3. We find that 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. We also find significant impacts for children who switched preschool programs, as the estimated sub-LATE for a-compliers equals 0.49 σ, highlighting the importance of the 'quality' component associated with the EYPP program. Despite the significant intensive-margin LATE, we note that the two sub-LATEs are statistically different, remarking that the majority of the benefit arising from EYPP participation follows from inducing families to enroll their children in *any* preschool. This result differs somewhat from the existing literature on preschool participation across alternative options, as in the context of Head Start in the U.S., Kline and Walters (2016) only find positive impacts of program attendance 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. We find significant impacts on students' socioemotional skills as well, as both the extensive- and intensive-margin LATEs indicate an impact exceeding 0.8 standard deviations through the follow-up round. The large impact on socioemotional skills for children who switched out of alternative preschool centers further remarks the importance of high-quality early childhood education. We similarly find that the estimated $LATE_{s\leftarrow a}$ is positive across all skill sub-domains — albeit with varying significance, as the estimated coefficient reaches 0.4 standard deviations in numeracy, while remaining below 0.17 σ in executive functioning. Across all skill sub-domains, the estimated $LATE_{s\leftarrow n}$ exceeds the $LATE_{s\leftarrow n}$, yet these parameters are only statistically different in the literacy measure. Nonetheless, the results presented so far strongly indicate the importance of both enrolling children into preschool programs, but also ensuring the quality of these programs, as both dimensions substantially improve children's multidimensional skill development in Bangladesh.[19]

[19]In Table D2, we present extensive- and intensive-margin LATEs under the assumptions laid out by Hull (2018).

Figure 3: Sub-Local Average Treatment Effects of EYPP Attendance on Children's Skill Outcomes

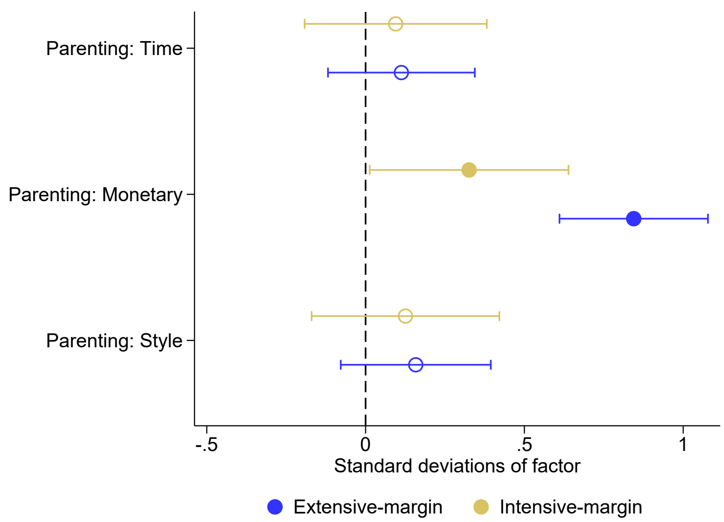


Note: Figure 3 presents the local average treatment effects of EYPP attendance on child development outcomes relative to home care (a-compliers) and alternative preschool attendance (a-compliers).. Robust standard errors are clustered at the community level.

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 4. Similar to the estimated intent-to-treat parameters presented in Section 4, we find positive, yet statistically insignificant impacts of the EYPP program on 'Time Quality' and 'Parenting Style' measures across fallback choices. On the other hand, we find sizable effects on the 'Monetary Investments' measure. For children who would have remained at home, attending the EYPP program improves their parents investment in this dimension by 0.84 σ through the follow-up round. The estimated intensive-margin LATE is smaller in magnitude — reaching 0.33 standard deviations, yet remains significant at the 5% level. On the other hand, the small estimated impacts on parental investments for children switching out of alternative preschool programs implies that the impact on skill measures presented above must may 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 positively affected children's skill development.

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 4: Sub-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 parental investment measures relative to home care (a-compliers) and alternative preschool attendance (a-compliers).. Robust standard errors are clustered at the community level.

6 Mediation Analysis with Multiple Fallbacks

This section explores the mechanisms trough 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 framework combines methods proposed by Heckman et al. (2013b) and Kirkeboen et al. (2016).

Identification of sub-ITTs. 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: $E[Y_i \mid Z_i = 1] - E[Y_i \mid Z_i = 0]$. Define $Y_{i,z}$ 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: $E[Y_i \mid Z_i = 1] - E[Y_i \mid Z_i = 0] = E[Y_{i,1} - Y_{i,0}]$. This parameter can be decomposed in terms of individual drawn from different margins of choice:

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

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 $Y_{i,z}$ in terms of potential outcomes and choices:

$$Y_{i,z} \equiv Y_{i,z}^s + (Y_i^n - Y_i^s)\mathbb{1}\{D_i(z) = n\} + (Y_i^a - Y_i^s)\mathbb{1}\{D_i(z) = a\},$$

which means that ITT at the individual level follows:

$$\begin{aligned} Y_{i,1} - Y_{i,0} = &(Y_i^n - Y_i^s)(\mathbb{1}\{D_i(0) = n\} - \mathbb{1}\{D_i(1) = n\}) \\ &+ (Y_i^a - Y_i^s)(\mathbb{1}\{D_i(0) = a\} - \mathbb{1}\{D_i(1) = c\}). \end{aligned}$$

To fix ideas, suppose we condition on $D_i(0) = n$. Under assumptions1 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:

$$E[Y_i \mid Z_i = 1, D_i(0) = n] = E[Y_{i,1} - Y_{i,0} \mid D_i(0) = n] = E[Y_i^s - Y_i^n](1 - P(D_i(0) = n)).$$

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

Mediation under sequential ignorability. Given identification of extensive- and intensive-margin ITTs, we can proceed with a mediation analysis following Heckman et al. (2013b). A production function for $Y_{i,z}$, given $D_i(0) = k \in \{n, a\}$ is such that:

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

where $\boldsymbol{P}$ is a vector of parental measures. Similar to Heckman et al. (2013b), we are making two assumptions. The first is structural invariance: $\boldsymbol{\beta}_a^k = \boldsymbol{\beta}_n^k$. However, in contrast to Heckman et al. (2013b), we allow for parameters to vary by counterfactual choice k. The second assumption is a form of sequential ignorability: given z and $D_i(0) = k$, $\boldsymbol{P}$ is independent of unobserved factors. Under these two assumptions, we can decompose each sub-ITT in terms of direct and indirect effects:

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

Both terms are identified given sequential ignorability. In our setting, we interpret the direct effect as the effect of attending preschool given a fixed level of parental investment. The indirect effect captures the portion of the sub-ITT explained by changes in parental investment. Both terms can be straightforwardly obtained via OLS of Y_i onto Z_i and $\boldsymbol{P}$, conditioning on $D_i(0) = k$.

Results. Table 3 presents OLS coefficients of the underlying production functions used for mediation. 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 from any fallback choice, the only inputs that show statistically significant coefficients are the ones associated to the EYPP offer and the monetary investment factor. The size of the coefficients larger for children coming from the extensive margin compared to those from the intensive margin.

Table 3: OLS coefficients of production functions

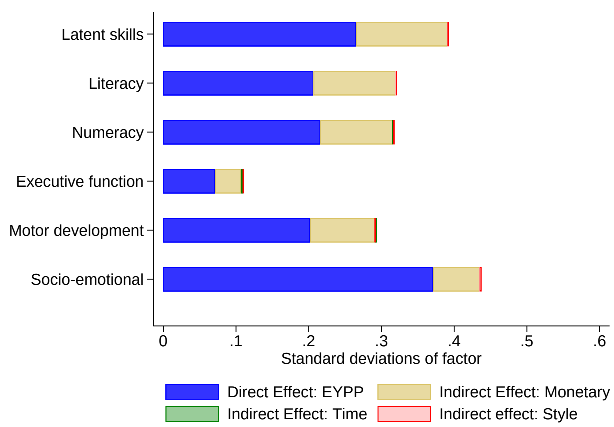
	Latent skills	Literacy	Numeracy	Executive	Motor	Socio-emotional
A. Intensive-margin						
EYPP offer	0.19***	0.13**	0.15***	0.07**	0.14***	0.36***
	(0.06)	(0.05)	(0.05)	(0.03)	(0.04)	(0.05)
Parenting: Time	-0.03	-0.05	-0.02	0.05	-0.03	-0.06
	(0.05)	(0.04)	(0.04)	(0.03)	(0.04)	(0.03)
Parenting: Monetary	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 3 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

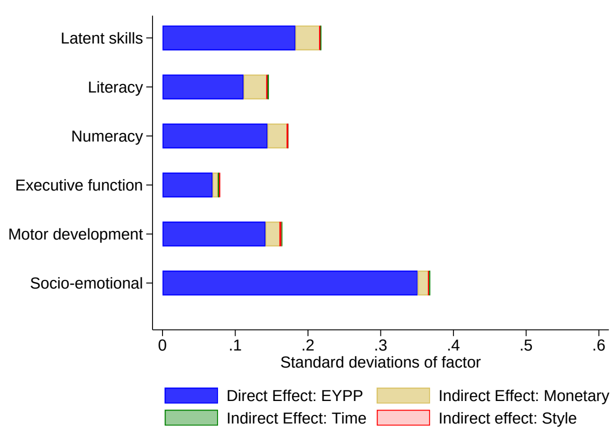
Figure 5 presents the results of the mediation analysis. In the figure, the bar represents the total ITT on each measure. Each bar is divided by the portion explained by EYPP attendance (direct effect) and changes in parental investment (indirect effect). For the overall ITT, over 50% of the EYPP offer effects are explained by preschool attendance. Only parental investment trough monetary resources seem to matter when explaining the effects of the EYPP offer.

Even though the direct effect seems to account for most of the effects, the share that can be accounted by parental investment is non-trivial and might suggest that EYPP was indeed effective in raising parental investment. However, for children coming from home, the effect on parental skills cannot be fully attributed to the EYPP services; a similar raise in investment might be seen even if we induce families to take any other preschool option. To evaluate how effective EYPP was in inducing changes in parental investment we compare intensive- versus extensive-margin effects. First, as discussed, the extensive-margin sub-LATE on parental monetary investment is larger than that of the intensive-margin one. Second, once we include the effects of the latter changes on child outcomes, we find that a minor part of the intensive-margin is actually explained by parental investment (less than 10%). In comparison, nearly 60% of the extensive-margin effects are attributed to parental investment changes.

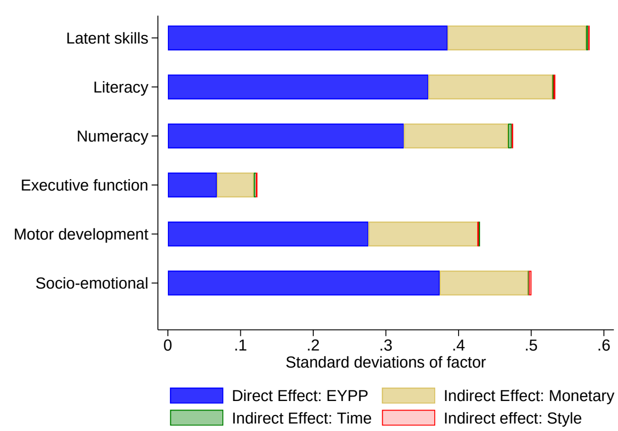
Figure 5: Mediation analysis



(a) Overall



(b) Intensive-margin



(c) Extensive-margin

Note: Figure 5 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.”

7 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 preprimary 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 significant impacts for both groups of students, remarking the importance of the quality component put forth by EYPP. At the same time, in the parental monetary investment measure, we find larger impacts for extensive-margin compliers vis-a-vis their intensive-margin peers. 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 is driven almost entirely by direct program impacts. All in all, these results indicate the EYPP intervention offers promise for improving child development outcomes in Bangladesh and further highlights the importance of high-quality early-life programs.

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Appendix

A Tables and Figures

Table A1: 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 A1 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 A2: 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 A2 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 A3: 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)

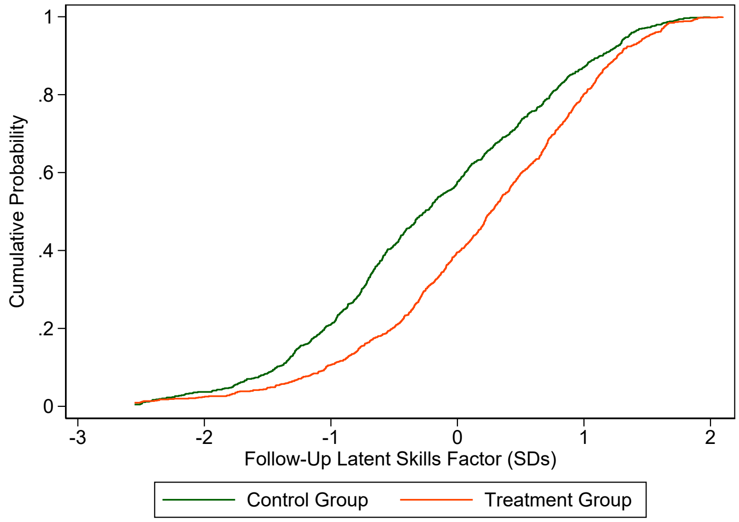
Notes: Table A3 presents the estimated impacts of EYPP program eligibility on parenting investment factors. Robust standard errors are clustered at the community level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A4: 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)

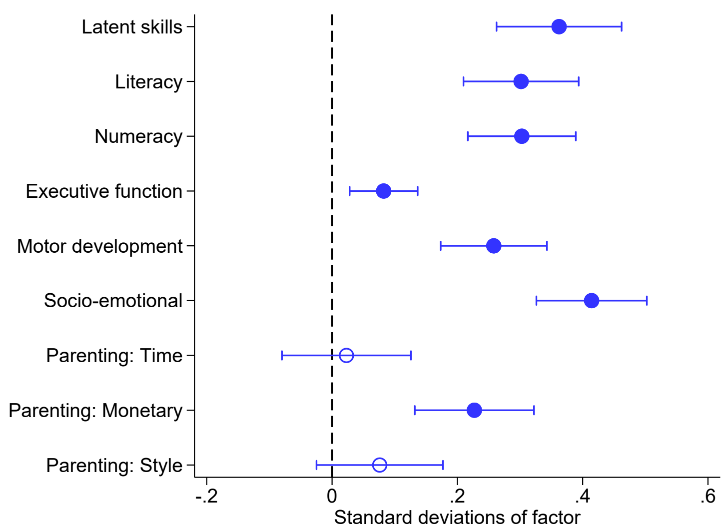
Notes: Table A2 presents the estimated impacts of EYPP program eligibility on parenting investment factors. Robust standard errors are clustered at the community level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Figure A1: Distributional Differences in Follow-Up Latent Skills by Treatment Status



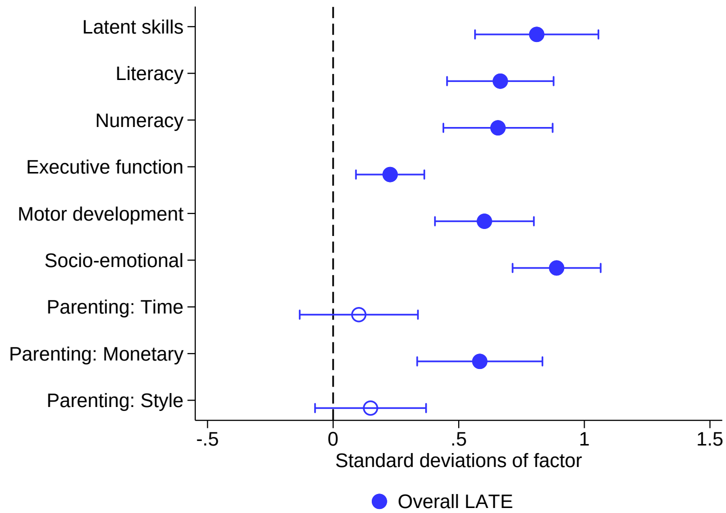
Note: Figure A1 presents the distribution of children's latent skills in the follow-up survey across treatment group status.

Figure A2: Intention-to-Treat Effects of the EYPP Program on Child Outcomes and Parental Investment



Note: Figure A2 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.

Figure A3: Local Average Treatment Effects of EYPP Attendance on Children's Skill and Parental Investment Outcomes



Note: Figure A3 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.

B Exploratory Factor Analysis

Table B1: 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 B1.

Table B2: Factor Loadings: Baseline Test Scores

	e(r_L)	
	Factor1	Factor2
BY: Number ID	.3676334	.7328317
BY: Puzzle Solving	.3417704	.2075325
BY: Number of Friends	.5859509	-.0640456
BY: Vocabulary	.7460427	.0511967
BY: Letter ID	.3747651	.7294451
BY: Copying	.5857036	.2091583
BY: Print Aware	.6366102	.0149474
BY: Phonemic Aware	.3641943	.266117
BY: Oral Comp.	.6552896	.072137
BY: Sizes	.4506522	-.0721692
BY: Sorting	.5504126	.0161815
BY: Shape ID	.5236571	-.0072678
BY: Correspondence	.5623676	.2586409
BY: Add/Subtract	.6332016	.1262608
BY: Memory	.613016	-.0022259
BY: Inh. Control	.5425971	.0885776
BY: Drawing	.5684879	.2357639
BY: Self-Aware	.5170594	-.0261415
BY: Emotional Aware	.5455194	.0081693
BY: Empathy	.4605353	.0473179
BY: Folding	.5536789	.1894898
BY: Hopping	.5886743	-.0342739

Table B2.

Table B3: Factor Loadings: Follow-Up Test Scores

	e(r_L)	
	Factor1	Factor2
F1: Number ID	.6739358	.5486208
F1: Puzzle Solving	.507286	.1306946
F1: Number of Friends	.5701879	-.246784
F1: Vocabulary	.7512461	-.1846917
F1: Letter ID	.6272979	.5566458
F1: Copying	.6455929	.0473392
F1: Print Aware	.6229108	-.0283643
F1: Phonemic Aware	.5633993	.1814663
F1: Oral Comp.	.6525487	-.1389634
F1: Sizes	.3806113	-.1990719
F1: Sorting	.493881	-.0878528
F1: Shape ID	.5773488	-.1536352
F1: Correspondence	.7002989	.2228894
F1: Add/Subtract	.6367503	.1254009
F1: Memory	.5325497	.0417026
F1: Inh. Control	.5171587	.1477349
F1: Drawing	.6458911	.0229112
F1: Self-Aware	.5359269	-.2198839
F1: Emotional Aware	.5650469	-.2497921
F1: Empathy	.3271235	-.2150674
F1: Folding	.5150988	.1481781
F1: Hopping	.552034	-.1578894

Table B3.

Table B4: Factor Loadings: Baseline Parental Investment Measures

	e(r_L) Factor1	Factor2	Factor3
BY: Writing Materials	.0920563	.5472374	-.1569819
BY: Puzzles	.0385743	.3201042	-.0344374
BY: Complex Toys	-.0980269	.379026	-.2772941
BY: Toys for Shapes	.1269751	.5897135	-.0297197
BY: Toys for Counting	.2105529	.4345245	-.0774101
BY: Read Books	.4664701	.1978014	.1795305
BY: Tell Stories	.5263236	.0955983	.1734995
BY: Sing Songs	.505279	.071982	.0481899
BY: Take on Visits	.142804	.0693842	.117621
BY: Play Games	.5180528	.0548186	.1198057
BY: Name Objects	.2806635	.3848516	.0563717
BY: Teach New	.2763353	.2939518	.0034172
BY: Teach Alphabet	.5552053	.1670889	-.0703158
BY: Teach Numbers	.6195529	.0720928	-.0341676
BY: Hug Child	.0971013	.0972024	-.2564681
BY: Hrs. Talking/Walking	.0991351	.1261449	-.0478008
BY: Number of Books	.1604946	.4408997	.0476017
BY: Other Reading Mat.	.1165606	.5682549	-.0759915
BY: Number of Toys	.1204052	.0743089	.000513
BY: (No) Spanking	-.0091122	-.0315958	.6734721
BY: (No) Hitting	.0952214	-.0283973	.7095238
BY: (No) Criticizing	.0403207	-.1245054	.6295135

Table B4.

Table B5: Factor Loadings: Follow-Up Parental Investment Measures

	e(r_L)		
	Factor1	Factor2	Factor3
F1: Writing Materials	.539781	-.181101	-.0808665
F1: Puzzles	.3669191	.0718522	.1284961
F1: Complex Toys	.1780391	.2220941	-.0196995
F1: Toys for Shapes	.5916108	.0811562	.0021609
F1: Toys for Counting	.4659849	.1778618	-.087654
F1: Read Books	.3203813	.373326	.0299284
F1: Tell Stories	.1086898	.5157124	.0377312
F1: Sing Songs	.1256005	.5483504	-.1039223
F1: Take on Visits	.1458763	.187888	-.0079241
F1: Play Games	.1163469	.4932544	.0782603
F1: Name Objects	.5152541	.2486217	-.0382279
F1: Teach New	.4246755	.2089958	-.0899365
F1: Teach Alphabet	.2254296	.3818112	-.0825884
F1: Teach Numbers	.1612329	.519608	.0060191
F1: Hug Child	.0148715	.0084101	-.0299593
F1: Number of Books	.4826117	.0412963	.0689632
F1: Other Reading Mat.	.6438361	.1075572	.0332427
F1: Number of Toys	-.0897395	-.12892	.0376523
F1: (No) Spanking	-.0088566	-.054465	.6837911
F1: (No) Hitting	.0167071	.0265027	.6854834
F1: (No) Criticizing	-.0673992	.021841	.5772237
F1: Hrs. Talking/Walking	-.0124321	.346252	.0308022

Table B5.

Table B6: Estimated Loadings for Latent Skills at Baseline

	e(r_L) Factor1
BY: Number of Friends	.5811473
BY: Vocabulary	.7510332
BY: Copying	.6121996
BY: Oral Comp.	.6548283
BY: Sorting	.5395265
BY: Shape ID	.5040616
BY: Correspondence	.5834952
BY: Add/Subtract	.6368898
BY: Memory	.6063483
BY: Inh. Control	.552363
BY: Drawing	.5982142
BY: Self-Aware	.5013372
BY: Emotional Aware	.5461679
BY: Empathy	.4574727
BY: Folding	.5742685
BY: Hopping	.585508

Table B6.

Table B7: Estimated Loadings for Latent Skills at Follow-Up

	e(r_L) Factor1
F1: Number ID	.7216237
F1: Puzzle Solving	.5195162
F1: Number of Friends	.5471036
F1: Vocabulary	.7314203
F1: Letter ID	.6763296
F1: Copying	.6510185
F1: Print Aware	.6121089
F1: Phonemic Aware	.5813129
F1: Oral Comp.	.632717
F1: Sorting	.4793761
F1: Shape ID	.5510435
F1: Correspondence	.7159006
F1: Add/Subtract	.6424998
F1: Memory	.5315964
F1: Inh. Control	.5333332
F1: Drawing	.6502056
F1: Emotional Aware	.5307067
F1: Folding	.5371596
F1: Hopping	.5397329

Table B7.

Table B8: Estimated Loadings for Parental Investment at Baseline

	by_par Time	Monetary	Style
BY			
Tell Stories	.4926015	.	.
Sing Songs	.5154668	.	.
Play Games	.5512027	.	.
Teach Alphabet	.5182108	.	.
Teach Numbers	.635346	.	.
Writing Materials	.	.5533882	.
Toys for Shapes	.	.6100132	.
Toys for Counting	.	.485413	.
Number of Books	.	.4371736	.
Other Reading Mat.	.	.5746426	.
(No) Spanking	.	.	.6978048
(No) Hitting	.	.	.7173181
(No) Criticizing	.	.	.5906839

Table B8.

Table B9: Estimated Loadings for Parental Investment at Follow-Up

	f1_par Time	Monetary	Style
F1			
Tell Stories	.5292858	.	.
Sing Songs	.581715	.	.
Play Games	.4936077	.	.
Teach Numbers	.4773557	.	.
Writing Materials	.	.4905408	.
Toys for Shapes	.	.6070603	.
Toys for Counting	.	.5001788	.
Name Objects	.	.5520168	.
Teach New	.	.4535439	.
Number of Books	.	.4617976	.
Other Reading Mat.	.	.6265515	.
(No) Spanking	.	.	.6668728
(No) Hitting	.	.	.690611
(No) Criticizing	.	.	.5690012

Table B9.

Table B10: Estimated Loadings for Non-Cognitive Measure at Follow-Up

	e(r_L) Factor1
F1: Self-Aware	.5711398
F1: Emotional Aware	.5961473
F1: Empathy	.4230497
F1: Number of Friends	.5480463

Table B10.

Table B11: Estimated Loadings for Literacy Measure at Follow-Up

	e(r_L) Factor1
F1: Vocabulary	.6737591
F1: Letter ID	.606886
F1: Print Aware	.617848
F1: Phonemic Aware	.6104489
F1: Oral Comp.	.6409345

Table B11.

Table B12: Estimated Loadings for Numeracy Measure at Follow-Up

	e(r_L) Factor1
F1: Number ID	.6711247
F1: Puzzle Solving	.5050885
F1: Sizes	.3625026
F1: Sorting	.4737252
F1: Shape ID	.5555728
F1: Correspondence	.7646159
F1: Add/Subtract	.7002352

Table B12.

Table B13: Estimated Loadings for Executive Function Measure at Follow-Up

	e(r_L) Factor1
F1: Memory	.4631469
F1: Inh. Control	.4631469

Table B13.

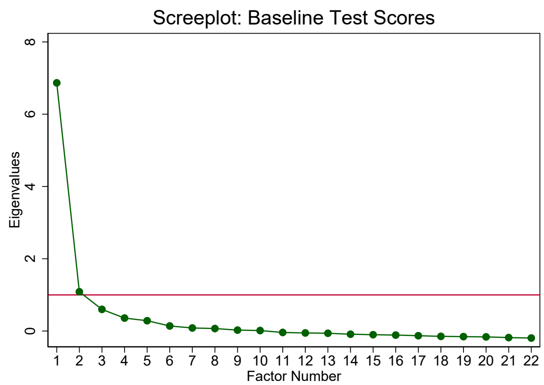
Table B14: Estimated Loadings for Motor Development Measure at Follow-Up

	e(r_L) Factor1
F1: Copying	.7142321
F1: Drawing	.667928
F1: Folding	.552914
F1: Hopping	.5736795

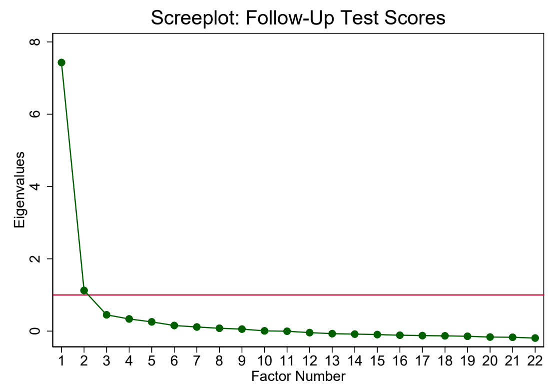
Table B14.

Figure B1: Scree Test for Baseline and Follow-Up Measures

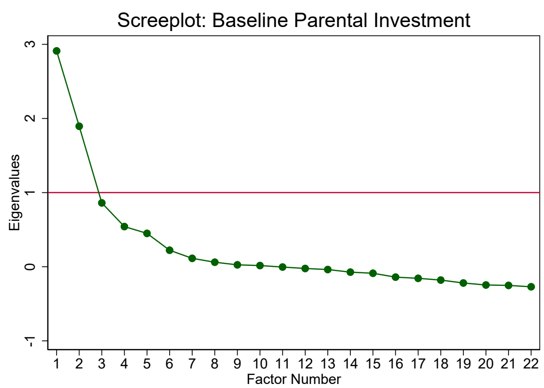
(a) Baseline Test Scores



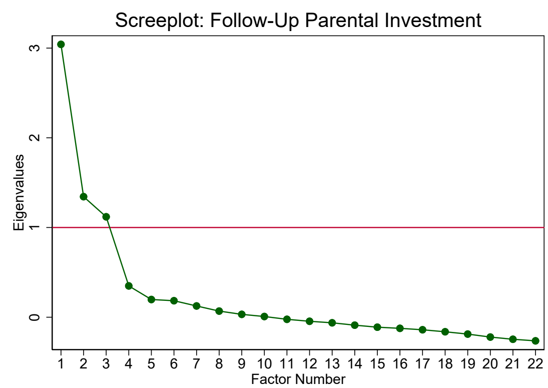
(b) Follow-Up Test Scores



(c) Baseline Parental Investment Measures



(d) Follow-Up Parental Investment Measures



Note: Figure B1.

C Machine Learning Predictions

As discussed in Section 5, 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.[20] 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 70.5%. We thus predict counterfactual attendance choices using the LASSO algorithm detailed above.

[20] 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.

D 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:

$$Y_i = \widetilde{\alpha}_1(1 - \mathbb{1}\{D_i = a\}) + \widetilde{\alpha}_2(1 - \mathbb{1}\{D_i = n\}) + \widetilde{\alpha}_3 f(\boldsymbol{X}_i) + \epsilon \quad (6)$$

$$E[\mathbb{1}\{D_i = s\}] = \widetilde{\beta}_1 Z_i + \widetilde{\beta}_2 Z_i \times f(\boldsymbol{X}_i) + \widetilde{\beta}_3 f(\boldsymbol{X}_i) \quad (7)$$

$$E[\mathbb{1}\{D_i = a\}] = \widetilde{\gamma}_1 Z_i + \widetilde{\gamma}_2 Z_i \times f(\boldsymbol{X}_i) + \widetilde{\gamma}_3 f(\boldsymbol{X}_i) \quad (8)$$

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 (6)-(8) to identify $LATE_{s \leftarrow n}$ and $LATE_{s \leftarrow a}$ 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 3. *(LATE homogeneity) $LATE_{s \leftarrow n}$ and $LATE_{s \leftarrow a}$ are mean-independent of $f(\boldsymbol{X}_i)$.*

Hull (2018) proves that, under Assumption (3), $\widetilde{\alpha}_1 = LATE_{s \leftarrow n}$ and $\widetilde{\alpha}_2 = LATE_{s \leftarrow a}$. 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 3. 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 3, these estimates identify $LATE_{s \leftarrow a}$ and $LATE_{s \leftarrow n}$, respectively. Table D1 shows that both instruments strongly predict both endogenous choices, and that the issue of weak instruments is not a problem in our model.

Table D2 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 5. 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 D1: 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 × 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 D2: Effect of EYPP Program on Skill and Parenting Outcomes by Fallback Options Under Assumption 3

	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)
$LATE_{s \leftarrow a}$	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)
$LATE_{s \leftarrow n}$	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
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$