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Parenting Home Visitation versus Cash Transfers for Preschoolers: An RCT in Rural Thailand*

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Abstract

This paper evaluates the impact of a weekly parenting home-visiting program based on the Reach Up curriculum and a cash transfer, using a randomized controlled trial in Thailand. The targeted children were preschoolers, with an average age of 36 months at the start of the parenting program. The intent-to-treat effect of the 10-month parenting program is positive and significant, with an effect size of approximately 0.13-0.16 SD, whereas the cash transfer is positive but insignificant. Treatment-on-the-treated effects reveal that each home visit improves child outcomes by 0.004 SD. The parenting program is more beneficial for younger and disadvantaged children, as measured by having special needs, less educated parents, lower household wealth, and fewer books at home, whereas the cash transfer is more effective for younger children, children with special needs, and boys.

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Keyword: parenting; home visiting; cash transfers; early childhood; parental investment; disadvantaged children
JEL Code: I21; I25; J24

1 Introduction

Human capital is a key driver of economic and social development (e.g., Becker, 1964; Sen, 1983; Lucas, 1988), and early childhood is one of the most sensitive and cost-effective periods for human capital formation (e.g., Knudsen, 2004; Cunha et al., 2006; Heckman, 2008; Cunha et al., 2010; Almond and Currie, 2011). As a result, early childhood interventions have become key developmental tools in recent years. One popular approach is to enrich parents' and caregivers' parenting skills through a parenting home-visiting program (e.g., Attanasio et al., 2014; Gertler et al., 2014; Justino et al., 2023; Zhou et al., 2026). The other is to provide financial resources to the family to alleviate credit constraints and allow parents to purchase market-based inputs for their children (e.g., Fiszbein and Schady, 2009; Carneiro et al., 2021; Duncan et al., 2025). While both approaches have been studied in isolation, a rigorous head-to-head comparison evaluating their relative cost-effectiveness is needed. This absence of comparative evidence leaves a significant gap in our understanding of the household's behavioral response to different types of supports.

This study addresses this gap by presenting evidence from a large-scale randomized controlled trial (RCT) in Thailand. We provide the first direct comparison between a weekly parenting home-visiting program—based on the internationally acclaimed Reach Up curriculum (Grantham-McGregor et al., 1991) — and a (labeled) cash transfer of 4,000 Thai Baht (approximately 132 USD). Ideally, with the cash-transfer and parenting interventions implemented simultaneously, we should have been able to monetize the parenting program's value for this group of children. Unfortunately, the effect of the cash-transfer program is insignificant. As a result, it is not reasonable to use it for this purpose.

Our results demonstrate that the parenting program significantly improves child development, with an intent-to-treat (ITT) effect size of 0.13-0.16 standard deviations (SD) depending on specifications. In contrast, the effect of the cash transfer is positive but insignificant

in all cases. These results indicate that stimulating activities are needed to significantly enhance child development.

As in Attanasio et al. (2020), we examine the impact of each intervention on parental time and material investments, which are key productive inputs for child development. In addition, we consider the potential mechanism through parenting styles. We find that the parenting program does not merely affect the primary caregiver's behavior; it also induces a household-wide shift in engagement. While both treatments increased the primary caregiver's time investment, the parenting program uniquely increased time investment from other adults in the household. However, we find no significant impact on parenting styles.

The sample in this experiment is highly diverse across several dimensions. It was conducted at six sites across all regions of Thailand, with diverse cultural and socio-economic backgrounds. This diversity enables us to perform rich heterogeneous analysis across many dimensions. Our results reveal that the parenting program is more beneficial for disadvantaged children, as measured by having special needs, less educated parents, lower household wealth, and fewer books at home. On the other hand, the cash transfer is more effective for boys relative to girls. In addition, both interventions are more effective for younger children. This finding confirms that early childhood is one of the most sensitive periods for human capital formation.

This paper contributes to the extensive literature on the effectiveness of large-scale parenting home visiting programs (e.g., Attanasio et al., 2014; Grantham-McGregor et al., 2020; Attanasio et al., 2020; Justino et al., 2023; Carneiro et al., 2024; Zhou et al., 2026). First, this study uses the Reach Up parenting curriculum as in Attanasio et al. (2014); Grantham-McGregor et al. (2020); Zhou et al. (2026). Further, as in the Colombian study (Attanasio et al., 2014) and the China Reach (Zhou et al., 2026), our parenting program was implemented with scaling up in mind, using available local personnel as home visitors.

The closest work to ours is Zhou et al. (2026), investigating the impact of the China Reach program, which implemented the same Reach Up curriculum and measured child development using the same DENVER II. The latter common feature allows us to meaningfully compare the impact. Comparing with other studies using different measures may be misleading, since one standard deviation of different measures is generally not comparable.

In addition, our study and the China Reach (Zhou et al., 2026) face a common challenge that is distinct from other studies in that more than 38 percent of our sampled children are left-behind.[1] As a result, home visitors in this experiment have to work with grandmothers mostly. This feature of the samples allows us to test whether the parenting program is sufficiently effective when caregivers are not parents. Our study differs from Zhou et al. (2026) in several respects. First, our study includes a cash-transfer intervention in the experiment. This is a unique feature of our experiment, compared to all existing parenting experiments. Moreover, given operational constraints, we initially implemented the parenting program with samples older than those in the China Reach.

The remainder of the paper is organized as follows. Section 2 describes the experimental design, including key features of the two treatments, sampling, and randomization. Section 3 introduces the baseline and endline surveys, child outcomes, parental investment, and parenting styles. Empirical specifications are presented in Section 4, while empirical results are presented in Section 5. Section 6 compares the results with related studies, and Section 7 concludes the paper.

2 Experimental Design

The experiment was conducted with young children, aged between two and three years old, at the baseline survey. The sampled children attended child development centers in eight provinces across Thailand, namely Chiang Mai, Lopburi, Nakhon Nayok, Khon Kaen, Mahasarakham, Kalasin, Songkhla, and Phatthalung, which were grouped into six areas, with the Mahasarakham-Kalasin and Songkhla-Phatthalung areas the only two with two provinces.[2] In total, it covered 81 subdistricts (called Tambon in Thai). Figure 1 in the Appendix illustrates the experimental areas on a map of Thailand.

[1] Children are left-behind when they are living without one or both parents, who migrated to work in big cities. This issue is prevalent in developing countries, e.g., El Salvador, Guatemala, Mexico, the Philippines, China, and Thailand (Zhang et al., 2014).

[2] Each experimental area covered 12 subdistricts, except for the Lopburi and the Mahasarakham-Kalasin areas, which covered 15 and 18 subdistricts, respectively.

2.1 Two Treatment Arms and Their Implementations

This experiment has two treatment arms: a parenting home-visiting program and a labeled cash transfer.

2.1.1 Parenting Home Visiting Program

This treatment arm is based on the Jamaican home-visiting model, known as the Reach Up program, which has shown short- and long-term positive effects (Grantham-McGregor et al., 1991; Walker et al., 2006; Gertler et al., 2014; Zhou et al., 2026). The program was designed to have home visitors serve as role models for parents or caregivers, encouraging them to engage in a variety of child-developmentally stimulating activities (Grantham-McGregor et al., 1991). The primary goal of the home visits is to enable parents to independently perform developmentally appropriate learning activities. See Figure 2 as an example of a home visitation. Note that, for conciseness, we use the word parent to refer to a primary caregiver, without legal or biological references, unless stated otherwise.

This is an intensive parenting program in which home visitors are expected to visit individual households weekly. The curriculum includes detailed instructions for weekly activities, ready for home visitors to follow. Early childhood experts from the University of the Thai Chamber of Commerce (UTCC) translated it into Thai and adapted it to be culturally appropriate. This thorough and well-structured feature of the Reach Up curriculum enables the program to be implemented by non-professional individuals, as seen in Attanasio et al. (2020) and Zhou et al. (2026), where home visitors were local women with education levels similar to those of the parents.

Home visitors in our intervention primarily comprise early childhood teachers from local child development centers (64%) and village health volunteers (28%). The former has a higher level of education (16 years of schooling) than the average parent, while the latter has a comparable level (12 years of schooling). This feature differs slightly from those in Attanasio et al. (2020) and Zhou et al. (2026), where home visitors and parents have similar levels of education.

The program was implemented in six areas across Thailand, namely Chiang Mai, Khon

Kaen, Lopburi, Mahasarakham-Kalasin, Nakhon Nayok, and Songkhla-Phatthalung. Each area has three supervisors, including the head, who is a local university lecturer, a professional nurse, or an early childhood expert, and two assistants, who hold at least a bachelor's degree. At least one supervisor from each area participated in an intensive 10-day training program led by Reach Up experts from the University of the West Indies. The trained supervisors then trained home visitors in each area for 5 days before home visitation began, and conducted a 2-day refresher training about 3 months later. The primary responsibilities of the supervisors were to recruit, train, supervise, and support home visitors. In addition, the supervisors ensured the quality of home visitation by regularly visiting households together with home visitors. In total, 18 supervisors and 124 home visitors were responsible for 495 children.

For implementation, home visitors are expected to visit households weekly. To make this high-frequency visit possible, an appointment has been made in advance. Most home visitors scheduled a future visit with the parents at the end of the previous visit and then entered the appointment into an online system designed to facilitate home visitation. This online system also helped home visitors organize home visits based on the child's age. The intervention lasted 10 months, from January to October 2023. Our administrative data show that, on average, 82% of households in the treatment group received regular home visits (at least 50% of the expected visits), with an average of 30 home visits per treated child. Home visitors have completed approximately 75% of the scheduled visits. Our home visitation completion rate is comparable to that reported in Attanasio et al. (2020) (about 81%). On the other hand, the China Reach program has an extraordinarily high compliance rate at about 98% (see the Online Appendix of Zhou et al., 2026). Our data also show that an average home visitor has been visited by a local supervisor approximately 6 times.

2.1.2 Cash Transfers with Information Provision

The second treatment arm is a (labeled) cash transfer of 4,000 Thai Baht (THB)[3] and sending a letter to parents or caregivers stating the purpose of the cash transfer (labeling). This cash transfer invention was designed based on the standard conditional cash transfer

[3]As of January 2026, 1 US Dollar can be exchanged for about 30.26 THB.

program of the Equitable Education Fund of Thailand (EEF), which provided 4,000 THB annually to extra-poor students. The main message of the letter reads as follows. "A primary activity of the research project is to give 4,000 THB to children who were randomly chosen by the research team. The main objective of the money is **for the household to spend on learning tools and toys that can help improve child development**." The information provided in the letter to parents was intended to encourage parents or caregivers to purchase child-developmentally appropriate tools (e.g., toys, books). Note that the last part of the message was emphasized using bold letters since it is the primary purpose of the invention. We empirically investigate whether households in this treatment group follow the suggestion, using survey data on parental investment.

The bundling nature of this treatment, which combines cash with information/labeling, prevents us from directly evaluating a pure cash-transfer program, but it should provide a potentially effective way to deliver cash transfers to disadvantaged families. Importantly, with the cash transfer as another treatment arm, we can now compare the developmental impact of the parenting program directly with that of the cash transfer. To our knowledge, this is the first paper to be able to compare the two treatments head-to-head.

There were 418 children in the cash-transfer group. However, parents of 14 children declined the cash transfer, while 8 of them still participated in the data collection for both the baseline and endline surveys. All the transfers were sent to parents' or caregivers' accounts via Internet banking on 13 March 2023, and the money should be available for use within a couple of days.[4] In total, 404 children received a cash transfer of 4,000 THB in March 2023, which was about seven months before the endline survey.

2.2 Targeted Sample and Sample Recruitment

This study recruited young children attending local child development centers in the participating subdistricts. We asked the parents or caregivers of 1,512 eligible children about their willingness to participate in the study, and 1,385 (92%) agreed. The eligible children in those classrooms were supposed to be 2-3 years old. However, 7 of them were younger than

[4]There are only 4 parents who gave incorrect banking information, and had to wait for another week for the transfer. Therefore, there is no need to be concerned regarding the timing effects of the transfers.

2 years old, and 16 were 4 years old or older at the baseline survey. We dropped them from the main sample. We also have to drop 16 more children who were older than 48 months at the start of the parenting program, which began in January 2023 (see the timeline below), since the Reach Up curriculum applies only to children up to 48 months old. This exclusion procedure results in 1,346 eligible children. The sampling process is shown in Figure 3.

2.3 Randomization

This study applied a clustered randomization framework, with the subdistrict as the unit of randomization, to minimize spillover bias. We first ranked all 81 subdistricts by the number of potential samples in each subdistrict. We then formed a matched group of three subdistricts based on the ranking. For each group, we randomly assigned them to the control and the two treatment groups: parenting and cash transfer. The randomization results in 446 children in the parenting group, 404 in the cash transfer group, and the rest (496 children) in the control group.

3 Data Collection and Measurements

3.1 Baseline Survey

The baseline survey included direct assessments of child development and interviews with parents and teachers. It was conducted in September and December 2022, just before the treatment was implemented. We assessed child development using the DENVER II assessment (Frankenburg et al., 1992), and a picture memory test, a subtest of the Wechsler Preschool and Primary Scale of Intelligence, henceforth WPPSI (Wechsler, 2012). The DENVER II combines both direct assessment and interview items (teacher or parent), whereas the picture memory contains only direct assessment items. For the direct assessment items, each child was assessed one-on-one by a trained researcher who held at least a bachelor's degree and had sufficient experience in data collection with young children. The DENVER II interview items were collected from homeroom teachers. We could directly assess 1,061 children at baseline.

The parent questionnaire asked about household characteristics, parental investment, and parenting styles. For the baseline, we asked caregivers to complete the questionnaire themselves and return it to the research team. We received the parent QN for 1,000 of 1,061 children who were directly assessed at baseline. Similarly, the teacher QN collected information on teacher and school characteristics. We received the teacher QN for 1,004 out of 1,061 children.

3.2 Endline Survey

The endline survey included direct assessments of child development and interviews with parents and teachers. It was conducted from November 2023 to January 2024. Children were assessed using not only the same tools as at baseline but also a couple of modules of school-readiness assessment tools (see more details in Kilenthong et al., 2023), namely listening comprehension, producing a set, and mental transformation. The procedure was the same as the baseline survey. This round could directly assess 953 (out of 1,061), which constitutes the main sample of this study. The structure of the parent QN and the teacher QN for the endline was similar to the baseline, except for the parental investment module, which we expanded with more detail and implemented through in-person interviews with caregivers. We could complete the parent QN for 911 of 1,061 children and the teacher QN for 856 of 1,061 children. Note that attrition was significantly correlated with the two treatment dummies in the benchmark model and with the cash-transfer treatment in the model with more controls, as shown in the Online Appendix. As elaborated later, we therefore corrected for attrition bias using inverse probability weighting regression.

3.3 Child Development Outcomes

This paper measured several domains of child development using both direct assessments, including the DENVER II, picture memory test, and school-readiness, and interviews, including parent and teacher QNs. Those direct assessments were grouped into eight domains, four from the DENVER II (language and cognitive, fine motor, gross motor, and social-emotional), one from the picture memory test (working memory), and three from the school

readiness (listening comprehension, producing a set, and mental transformation).

Our primary outcome is the composite index of all available domains, called the child development index (CD). We first applied the standard two-parameter item response theory, henceforth called IRT (Birnbaum, 1968), to estimate the CD index at the baseline using all estimable items from the first five domains for the baseline.[5] We then standardized the index at the baseline to have a mean of zero and a standard deviation of one. The CD index at the endline was derived using the same procedure, based on all estimable items from all eight domains. Importantly, this procedure entails anchoring endline skills to baseline skills by estimating constrained IRT using the baseline parameters, and standardizing the index using the baseline mean and standard deviation. As a result, the estimated impacts are expressed in units of the baseline standard deviation.

Similarly, we estimated a composite index using the same procedure, but with DENVER II items only; this index is henceforth referred to as the DENVER index. Again, we standardized the DENVER index using the baseline mean and standard deviation. In addition, we estimated indices for each of the eight domains separately using the same procedure as above. In particular, we applied a constrained IRT based on parameters estimated for the child development index and standardized them using the baseline parameters when possible, except for listening comprehension, producing a set, and mental transformation, which were available only at the endline.

Figures 4(a) and 4(b) show that both the child development index (CD) and the DENVER index have increased over time, as expected, since the children got older and were more likely to pass more difficult items. More specifically, the endline indices (long-dash red) shift to the right relative to the baseline indices (solid blue). On the other hand, Figures 5(a) and 5(b) indicate that child outcomes at baseline were comparable across the control, parenting,

[5]For the baseline, language, fine motor, gross motor, social-emotional, and working memory skills were derived from 26, 8, 8, 12, 19 items, respectively. As in Zhou et al. (2026), some items were not included because they were too easy or too difficult, resulting in too high or too low levels of raw scores. Adding them would lead to divergence of the likelihood function. For the endline, those were estimated from more items because children have grown older and could pass some of the more difficult items. In particular, they were derived from 26, 11, 11, 16, 19 items, respectively. In addition, if a child did not perform an item, we set the score to 0.

and cash-transfer groups, with the cash-transfer group appearing slightly better. In fact, the balance test presented in the Online Appendix confirms that the difference between the control and cash-transfers groups at baseline is statistically significant.

The child outcomes at the endline, presented in Figures 6(a) and 6(b), indicate a positive but small impact of the parenting treatment on child outcomes, as the distributions of both indices shift noticeably to the right relative to the baseline. The parenting treatment does not lower the proportion of low-skill children as its left tail is not different from the control's. The program seems to affect children at the middle of the distribution, where the parenting group distribution shifts to the right relative to the control group. On the other hand, the distributions of the cash-transfer treatment shift to the right relative to the baseline only slightly. Therefore, we may not find a significant impact of the cash-transfer treatment.

3.4 Parental Investment

This study categorized parental investment into time investment and material investment. Time investment was derived from questions in the parent QN about how much time adults in the household spent with the child on developmentally appropriate activities, including reading, playing, singing/dancing, and writing/coloring. The time investment index was predicted using the Bartlett method in exploratory factor analysis (EFA) and standardized to have a mean of zero and a standard deviation of one. There are two time-investment indices: the primary caregiver's time (CG time index) and others' time (OT time index).

For material investment, the baseline survey asked only for the number of (children) books (storybooks and picture books). As a result, we used the variable to represent the baseline material investment. On the other hand, the endline data covered 10 child-developmentally appropriate tools, including storybooks, picture books, writing and numbering practice books, coloring books, Lego-like toys, jigsaws, wooden blocks, dolls, vehicle toys, and sound toys. We generated two separate indices of material investment using exploratory factor analysis (EFA): the stock index, based on the number of each item available in the household (stock of materials), and the flow index, based on household expenditure (flow of materials). Again, both indices were standardized to have a mean of zero and a standard deviation of one.

3.5 Parenting Styles

This study collected information on parenting styles using the 32-item questionnaire from Robinson et al. (1995), which is based on the Baumrind (1966) typology, at both baseline and endline. See the Online Appendix for the questions. We first calculated the average scores for all 11 subdomains, including warmth and involvement, reasoning/induction, democratic participation, good nurtured/easy-going (capturing authoritative), verbal hostility, corporal punishment, non-reasoning/punitiveness, directiveness (capturing authoritarian), and lack of follow-through, ignoring misbehavior, and self-confidence (capturing permissive). An exploratory factor analysis (EFA) indicated that there are only two latent factors (with eigenvalues larger than one), namely authoritative and authoritarian parenting styles, whose factor scores were obtained using the Bartlett method (Bartlett, 1937). Following Faizi and Kilenthong (2022, 2026), we conceptualize each caregiver's parenting practices as exhibiting both types of parenting, albeit with different intensities.

3.6 Summary Statistics and Balanced Tests

Table 10 in the Appendix presents summary statistics for key variables from the main sample of 953 children, except for variables with missing data or different units. For comparison, the results are categorized into parenting, cash-transfer, control, and entire-sample groups. For brevity, we only comment on a few variables. The sample was gender-balanced, with 51% of participants female. The average age of the sampled students at the baseline was about 3 years. Most children lived in relatively large households (average household size: 5.44), and their home environments were generally poor, with fewer than 4 children's books per family. Approximately 38 percent of children have neither parent at home (left-behind children), and only 64 percent have a parent as their primary caregiver.

More formally, we performed a balanced test using linear regression, regressing each variable on the two treatment dummies and a constant, and clustering standard errors at the subdistrict level (the randomization unit). The results indicate that the randomization is reasonably balanced, as only 4 and 4 out of 17 variables are significantly correlated with the parenting and cash-transfer treatments, respectively. Importantly, child outcomes at

baseline (BL) do not differ significantly between the parenting and control groups, whereas the difference between the cash-transfer and control groups is significant. See details in the Online Appendix.

4 Empirical Specification

We estimate the intent-to-treat (ITT) effect of the parenting and cash-transfer treatments on child outcomes using the following linear regression model:

$$Y_{i1} = \beta_0 + \beta_1 T_{1i} + \beta_2 T_{2i} + \beta_Y Y_{i0} + \boldsymbol{\beta}_X \boldsymbol{X}_i + \varepsilon_i, \tag{1}$$

where Y_{i0} and Y_{i1} denote child i's outcomes at the baseline[6] and the endline, respectively, T_{1i} and T_{2i} are dummy variables indicating whether child i was in the parenting treatment or the cash-transfer treatment, respectively, $\boldsymbol{X}_i$ is a vector of control variables, and ε_i is an error term. Control variables in the benchmark model include child gender (female), child age and its square (at the endline), the number of days between the baseline and endline assessments, the number of potential samples in each subdistrict at baseline, matched-group fixed effects, and province fixed effects.[7] We estimated the model using ordinary least squares (OLS). The key parameters of interest, β_1 and β_2, estimate the intent-to-treat (ITT) effects of the parenting and cash-transfer treatments on the child outcomes, respectively. We also perform all estimations regarding child development with more control variables as a robustness check.[8] All estimations with child development were clustered at the subdistrict level (randomization unit).

In addition, the attrition rate was imbalanced across groups, as shown in the Online

[6] Y_{i0} will be a vector of all five baseline outcomes, including language and cognitive, fine motor, gross motor, and social-emotional, working memory, when estimating the impact on each domain separately.

[7] The province fixed effects are applicable here because there were three groups, whose subdistrict members belong to different provinces in the same area.

[8] The additional control variables include being a special needs child, mother's education, father's education, caregiver's education, having a parent as caregiver, household wealth, household size, number of students in the household, having both parents at home, number of children books at home, using Thai as the main language at home, and student-teacher ratio. Including more control variables increases the estimation precision but reduces the sample size.

Appendix. We therefore assessed the robustness of the benchmark results by correcting for potential bias using inverse probability weighting regression with wild-cluster bootstrapping over 1,000 replications. Similarly, some children have moved from the original child development centers. We therefore also corrected for potential bias arising from movement between child development centers using inverse probability weighting regression.

In the main sample, caregivers of three children in the cash-transfer group declined the cash transfer. In addition, children in the parenting groups received different numbers of home visits. To address those issues, we estimated the following treatment-on-the-treated (TOT) effect using the treatment dummies T_{1i} and T_{2i} as the instruments.

$$Y_{i1} = \alpha_0 + \alpha_1 N_i + \alpha_2 R_i + \alpha_Y Y_{i0} + \boldsymbol{\alpha}_X \boldsymbol{X}_i + \varepsilon_i, \tag{2}$$

where N_i and R_i denote the numbers of home visits that child i received, and the dummy variable indicating that child i's caregiver received the cash transfer, respectively. The key parameters of interest, α_1 and α_2, estimate the treatment-on-the-treated (TOT) effects of receiving a home visit each time (on average) and receiving a cash transfer of 4,000 THB, respectively.

We also investigated whether the treatments affected the children differently across subgroups. There are three main groups: (1) child characteristics including child gender (Xfemale), child age (Xage), prior skills at baseline (Xskills), being a special needs child (Xspecial); (2) parent characteristics including mother's education is lower than college (XMAnoBA), father's education is lower than college (XFAnoBA), caregiver's education is lower than college (XCGnoBA), caregiver is a parent (XparentCG); and (3) household characteristics including household wealth (Xwealth), household size (Xhhsize), having both parents at home or no parental absence (Xnoabsence), number of children books at home (Xbooks). Technically, we estimated the heterogeneous effects by adding interaction terms between the treatments and the variable of interest, H_i, into the benchmark model, one at a time:

$$Y_{i1} = \phi_0 + \phi_1 T_{1i} + \phi_2 T_{2i} + \phi_{1H} T_{1i} \times H_i + \phi_{2H} T_{2i} \times H_i + \phi_Y Y_{i0} + \boldsymbol{\phi}_X \boldsymbol{X}_i + \varepsilon_i, \tag{3}$$

where H_i is the heterogeneous variable of interest, and to preserve the sample, the control $\boldsymbol{X}_i$ includes the benchmark controls and the heterogeneous variable of interest, if not already

included. The key parameters of interest, ϕ_{1H} and ϕ_{1H}, estimate the heterogeneous effects of the parenting and cash-transfer interventions, respectively.

In addition, this paper investigated potential mechanisms by which the intervention could impact child outcomes. We estimated the intent-to-treat (ITT) effect of the treatments on time investment, material investment, and parenting styles using the following linear regression model.

$$I_{1i} = \gamma_0 + \gamma_1 T_{1i} + \gamma_2 T_{2i} + \gamma_I I_{0i} + \boldsymbol{\gamma}_X \boldsymbol{X}_i + \varepsilon_i, \tag{4}$$

where I_{0i} and I_{1i} denote time investment, material investment, or parenting styles, measured at the baseline and the endline, respectively. The control $\boldsymbol{X}_i$ is the benchmark control.

Further, we performed a mediation analysis for child outcomes, using time investment (the index from all adults in the household) and material investment (the stock index), using the following linear specification:

$$Y_{i1} = \beta_0 + \beta_1 T_{1i} + \beta_2 T_{2i} + \beta_3 I^t_{1i} + \beta_4 I^m_{1i} + \beta_Y Y_{i0} + \boldsymbol{\beta}_X \boldsymbol{X}_i + \varepsilon_i, \tag{5}$$

where I^t_{1i} and I^m_{1i} are time investment and material investment indices, respectively. Note that our analysis is similar to Heckman et al. (2013) in that we assume that both time and material investments are exogenous, conditional on the control variables. This differs from Attanasio et al. (2020), which estimated a similar model using an instrumental-variables approach to address potential endogeneity bias. We could not perform the analysis using an IV approach because there are no appropriate IVs in our data.

5 Results

5.1 Main Results

The benchmark results, which represent intent-to-treat effects (ITT), are presented in columns (1) and (2) of Table 1. The result in column (1) indicates that the parenting program significantly raised the child development index (CD) by approximately 0.13 SD (of the baseline sample), while the cash-transfer intervention had an impact size of about 0.11 but was insignificant. The result when using the DENVER index as the outcome, presented in column

(2), confirms that the parenting program positively and significantly impacted child development, while the cash-transfer intervention had a positive but insignificant impact. The impact sizes were slightly larger for parenting (0.14 SD) but slightly smaller for cash transfers (0.10 SD). In addition, the results with more control variables, as shown in columns (3) and (4), confirm that the benchmark results are robust.

Table 1: Intent-to-Treat effects (ITT) on child outcomes using the benchmark specification with basic controls and with more controls.

	With Basic Controls		With More Controls	
	(1)	(2)	(3)	(4)
	CD	DENVER	CD	DENVER
parenting	0.128**	0.139***	0.129**	0.156***
	(0.051)	(0.048)	(0.055)	(0.053)
cash-transfer	0.112	0.102	0.121	0.102
	(0.070)	(0.062)	(0.075)	(0.071)
Observations	953	953	798	798

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors at the sub-district level are in parentheses.

Correcting for potential attrition biases led to similar results as shown in columns (1) and (2) of Table 2. The results were almost identical to those in Table 1. This suggests that the attrition bias should not be a first-order problem in this case. Similarly, we found that the results with child-development-center-change correction were similar to the benchmark results, as shown in columns (3) and (4) of Table 2.

When considering each domain as a separate outcome, we found that the parenting intervention significantly improved child development in the fine motor (FM) and language-cognitive (LG) domains. The significance of these domains is consistent with those in Zhou et al. (2026), where the significant impacts of the China REACH at the endline were found for fine motor (with the effect size of 0.68 SD) and language-cognitive skills (with the effect size of 1.04 SD) as well, and, more specifically, the effect on the language and cognitive skills was largest. In particular, this makes sense since the curriculum for older children, as in our samples, emphasizes language and cognitive skills. However, our impact size is significantly

Table 2: Intent-to-Treat effects (ITT) on child outcomes using the benchmark specification correcting for potential biases due to attrition and childcare change with basic controls.

	Correcting for Attrition		Correcting for Childcare Change	
	(1)	(2)	(3)	(4)
	CD	DENVER	CD	DENVER
parenting	0.123**	0.137***	0.124**	0.136***
	(0.050)	(0.047)	(0.050)	(0.047)
cash-transfer	0.103	0.096	0.105	0.095
	(0.070)	(0.062)	(0.070)	(0.062)
Obs.	953	953	953	953

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors at the sub-district level are in parentheses.

smaller than the one in Zhou et al. (2026). We discuss potential reasons for the smaller impact in Section 6.

Table 3: Intent-to-Treat effects (ITT) on child outcomes using the benchmark specification for each subdomain with basic controls.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	SE	FM	LG	GM	WM	LC	PS	MT
parenting	0.011	0.151*	0.176***	0.081	-0.086	0.034	0.112	0.062
	(0.057)	(0.088)	(0.061)	(0.075)	(0.081)	(0.078)	(0.068)	(0.068)
cash-transfer	-0.003	0.136	0.116	0.119	0.002	0.126	0.060	0.081
	(0.056)	(0.093)	(0.078)	(0.072)	(0.092)	(0.085)	(0.088)	(0.093)
Obs.	953	953	953	953	953	953	953	953

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors at the sub-district level are in parentheses.

Table 4 presents the treatment-on-the-treated (TOT) effects by the number of home visits the child received and a dummy variable indicating whether the child received the cash transfer. The results with basic controls indicate that each home visit increased child development by 0.004 SD (measured at baseline), for both the child development index (CD) and the DENVER index. Given that the average number of home visits was 30, these results imply an impact of about 0.12 SD on the average child, which is very close to the

benchmark result. In addition, this per-visit impact is useful for comparing our results with other interventions that use the same curriculum but have different durations. On the other hand, the results for the cash-transfer intervention were very similar to the benchmark results in Table 1, except that some of them became significant here. This indicates that cash transfer may have a statistically significant but relatively small impact on the treated samples. However, we need to study further to be more certain, given that most of the cash-transfer estimates were insignificant.

Table 4: Treatment-on-the-Treated effects (TOT) on child outcomes using the number of home visits and a receiving cash-transfer indicator as treatment variables.

	With Basic Controls		With More Controls	
	(1)	(2)	(3)	(4)
	CD	DENVER	CD	DENVER
No. of home visits	0.004**	0.004***	0.004**	0.005***
	(0.002)	(0.001)	(0.002)	(0.002)
received cash-transfer	0.112	0.103*	0.122*	0.102
	(0.069)	(0.061)	(0.074)	(0.070)
Observations	953	953	798	798
F-Stat.	453	450	510	505

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors at the sub-district level are in parentheses.

5.2 Potential Mechanisms

Panel A of Table 5 presents the ITT effects on time and material investments. First, both parenting and cash-transfer interventions significantly increased primary caregivers' time investment, but the effect size of the cash-transfer intervention was larger, as shown in column (1). This seems puzzling at first since we already found that the impact of the parenting program was significant, while the cash-transfer intervention was not. Things are more reasonable once we consider it together with the effects of the interventions on time investment from other adults in the household, which indicates that the impact of the parenting was larger and more significant than the cash transfer, as shown in column (2). One

potential explanation for the significance of parenting and the insignificance of cash transfers is that multiple adults engaging in developmentally appropriate activities with children is more effective. Another potential explanation is that our measure of time investment cannot capture its quality, and the parenting program can enhance it, whereas the cash-transfer intervention may affect only the quantity of time, not its quality.

Table 5: Intent-to-Treat effects (ITT) on parental time and material investment indices, and parenting styles.

	Time Investment		Material Investment		Parenting Styles	
	(1)	(2)	(3)	(4)	(5)	(6)
	MC Time	OT Time	Stock	Flow	AT	AR
Panel A: Main Samples						
parenting	0.247***	0.221***	0.306***	0.096	0.082	0.088
	(0.074)	(0.082)	(0.097)	(0.087)	(0.093)	(0.104)
cash-transfer	0.366***	0.168*	0.271***	0.339***	0.116	0.130
	(0.089)	(0.097)	(0.096)	(0.083)	(0.098)	(0.129)
Observations	775	773	703	702	651	653
Panel B: Boys only						
parenting	0.224**	0.320***	0.514***	0.343**	0.122	0.082
	(0.109)	(0.098)	(0.129)	(0.148)	(0.113)	(0.145)
cash-transfer	0.260*	0.275***	0.328***	0.378***	-0.034	0.236
	(0.131)	(0.101)	(0.124)	(0.110)	(0.119)	(0.186)
Observations	376	376	344	334	315	318
Panel C: Girls only						
parenting	0.321***	0.178	0.061	-0.140	0.015	0.114
	(0.104)	(0.115)	(0.114)	(0.124)	(0.138)	(0.150)
cash-transfer	0.427***	0.058	0.127	0.262**	0.236*	0.108
	(0.110)	(0.131)	(0.151)	(0.128)	(0.127)	(0.172)
Observations	399	397	359	368	336	335

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors at the sub-district level are in parentheses. AT and AR are abbreviations for authoritative and authoritarian parenting styles, respectively.

For material investment, we found that both treatments increased the stock index of material investment, as shown in column (3) of Panel A of Table 5. The impact size of

the parenting program was larger than that of the cash-transfer treatment. On the other hand, the impact of parenting on the flow index was not significant, whereas that of the cash-transfer intervention was. As a result, we use only the stock index in the mediation analysis below.

Another potential mechanism is primary caregivers' parenting practices, called parenting styles. The results in columns (5) and (6) of Table 5 show that neither treatment had a significant impact on parenting styles. This makes sense, since neither the parenting nor the cash-transfer interventions explicitly focused on improving parenting styles. The parenting program focused on encouraging caregivers to engage in developmentally appropriate activities with children, which, in principle, could lead to more authoritative and less authoritarian parenting. Unfortunately, that did not happen. On the other hand, it is even less reasonable to expect that a cash-transfer intervention would affect parenting styles.

In addition, we have investigated how a child's gender influences parents' and caregivers' responses to interventions. Boys' caregivers responded to both interventions by spending more time and providing more learning materials, as indicated by the positive and significant estimation coefficients of both types of investments in Panel B of Table 5. On the other hand, the responses of girls' parents differ from those of boys, as most of the estimation coefficients are insignificant. In particular, only the estimation coefficients for primary caregivers' time investment are positive and significant for both interventions, and the flow of material investment and authoritative parenting are significant for the cash-transfer intervention.

5.3 Results from Mediation Analysis

Following Heckman et al. (2013), we conducted a mediation analysis using time investment by main caregivers, time investment by other adults, and material (stock) investment as the mediating factors. The estimation results are presented in Table 6, indicating that the parenting intervention may enhance child development through the direct effect of the parenting program and indirectly through the material (stock) investment. In particular, the material (stock) investment is significant for both cases with basic controls. At the same time, the direct effect is significant for three cases except for the benchmark case with the child development index as the outcome. For the benchmark case with the child development

index as the outcome, the material (stock) investment accounts for about 20 percent of the treatment effect, but only 11 percent in the case of the DENVER index with basic controls.

On the other hand, the estimation coefficient of time investment is insignificant in all cases. These findings are similar to those in Attanasio et al. (2020). One potential explanation for this insignificance is that we can only measure the quantity, not the quality, of time investment, or time investment may be noisier. Alternatively, the home visitors may mistakenly focus on doing activities with the child rather than guiding the parents to perform them. The last explanation aligns with the fact that the parenting program's direct effect is relatively large.

Table 6: The mediation analysis results with basic controls and with more controls.

	With Basic Controls		With More Controls	
	(1)	(2)	(3)	(4)
	CD	DENVER	CD	DENVER
parenting	0.089	0.131**	0.112*	0.168***
	(0.058)	(0.056)	(0.065)	(0.062)
cash-transfer	0.073	0.092	0.095	0.107
	(0.072)	(0.063)	(0.082)	(0.079)
Time (MC)	-0.004	-0.017	-0.014	-0.027
	(0.031)	(0.030)	(0.031)	(0.031)
Time	0.004	0.011	-0.004	0.007
	(0.026)	(0.024)	(0.025)	(0.023)
Material (stock)	0.080***	0.052**	0.038	0.025
	(0.022)	(0.022)	(0.030)	(0.028)
Basic controls	YES	YES	YES	YES
More controls	NO	NO	YES	YES
Observations	824	824	697	697

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors at the sub-district level are in parentheses.

5.4 Heterogeneous Effects: Impact varies with Children, Parent, and Household Characteristics

The parenting home visiting intervention benefits younger children more significantly, as shown in Panel A of Table 7.[9] The estimation coefficients are negative in all cases and are significant for those with the DENVER outcome. This finding is consistent with the literature, which suggests that earlier intervention is generally more effective than later intervention. This result has an important implication when we compare our result with the literature in Section 6.

Our results imply that more disadvantaged children gain more from the parenting program. In particular, we found that the parenting program is more beneficial for children with special needs, as shown in Panel B of Table 7. Similarly, children whose fathers do not complete a bachelor's degree (BA) benefit more than those whose fathers hold a college degree or higher. The estimation results for this case are large and all significant, as shown in Panel A of Table 8. In addition, similar results, though less significant, hold for mothers' and main caregivers' education, as shown in Panels B and C of Table 8. The same conclusion can be drawn from the heterogeneous effects with respect to household wealth and the number of books at home, as presented in Panels A and B of Table 9. These findings suggest that a parenting intervention targeted at more disadvantaged children is more effective.

Even though the intent-to-treat effect of the cash-transfer intervention is insignificant, it shows significant heterogeneity across children's characteristics. Similarly to the parenting program, it benefits the younger children and children with special needs more significantly. Its impact is relatively large and significant in most cases, as shown in Panel A of Table 7.

Interestingly, the cash-transfer benefit is larger for boys than for girls, as the interaction term with being a girl is negative and significant in all cases, as in Panel C of Table 7. This result may seem like a standard gender bias result, where boys received both time and material investment more. However, this result is better explained by the results presented

[9]Recall that the parenting program started in January 2023 and the cash-transfer was delivered at the beginning of April 2023. Therefore, we defined the interaction terms as the product between the parenting dummy and the child's age on January 1, and the product between the cash-transfer dummy and the child's age on April 1.

in Section 5.2 and 5.3. First, the mediation analysis in Section 5.3 suggests that the positive impact may be mediated by material investment (stock). Moreover, the results in Section 5.2 indicate that boys' parents responded to the cash-transfer intervention by having 0.33 SD more stock of learning materials, as shown in Table 5, whereas the effect is much smaller and insignificant for girls, as shown in Table 5. On the other hand, girls received more time investment as a response to the invention. In particular, parents of girls responded to the intervention by spending 0.43 SD more time on activities, whereas the increase was only 0.26 SD for boys. This gender difference in responses to the intervention is consistent with the finding in Dinh and Kilenthong (2021), which shows that parents in rural Thailand tend to believe that the human production function for boys and girls differs, with the productivity of material investment higher for boys and lower for girls.

Note that the parenting program has a homogeneous impact across child gender, prior skills, whether a parent is the primary caregiver, household size, and whether both parents are at home. Similarly, no significant heterogeneous effect for the cash-transfer intervention across fathers', mothers', and caregivers' education, household wealth, number of books, household size, and having both parents at home.

6 Discussion: Comparing with the Literature

There have already been several comparable social experiments in developing countries that have used the Reach Up program in recent years (e.g., Attanasio et al., 2014; Grantham-McGregor et al., 2020; Zhou et al., 2026). First, Attanasio et al. (2014) implemented Reach Up for 18 months, averaging 63 home visits, and found an effect size of 0.260 SD. Using linear interpolation, our effect size would have been $\frac{63}{30} \times 0.128 = 0.269$ SD if we had implemented the program for a more extended period, with an average of 63 visits instead of 30. Similarly, comparing with Grantham-McGregor et al. (2020), which implemented the program for 24 months rather than 10 months as in ours. Again, a linear interpolation implies that our effect size would have been $\frac{24}{10} \times 0.128 = 0.307$ SD if we could have implemented the program for 24 months instead of 10 months. Interestingly, those counterfactual impact sizes are close to those in Attanasio et al. (2014) and Grantham-McGregor et al. (2020), which are 0.260

Table 7: Heterogeneous effects on child outcomes with respect to children's characteristics.

	With Basic Controls		With More Controls	
	(1)	(2)	(3)	(4)
	CD	DENVER	CD	DENVER
Panel A: heterogeneous effect with respect to child age				
parentingXage	-0.208	-0.300**	-0.198	-0.248*
	(0.153)	(0.134)	(0.171)	(0.147)
cash-transferXage	-0.310**	-0.414***	-0.272*	-0.373**
	(0.133)	(0.132)	(0.150)	(0.148)
Observations	953	953	798	798
Panel B: heterogeneous effect with respect to being special-needs				
parentingXspecial	0.279	0.470	1.339*	1.688**
	(0.619)	(0.644)	(0.761)	(0.745)
cash-transferXspecial	0.671	1.003*	1.413*	1.942**
	(0.527)	(0.541)	(0.755)	(0.742)
Observations	946	946	798	798
Panel C: heterogeneous effect with respect to child gender (female)				
parentingXfem	-0.089	-0.030	-0.107	-0.055
	(0.100)	(0.096)	(0.100)	(0.101)
cash-transferXfem	-0.228**	-0.204**	-0.263***	-0.260***
	(0.098)	(0.094)	(0.099)	(0.096)
Observations	953	953	798	798
Panel D: heterogeneous effect with respect to baseline skills				
parentingXskills	-0.043	-0.007	-0.085	-0.010
	(0.068)	(0.063)	(0.066)	(0.060)
cash-transferXskills	-0.111*	-0.093	-0.099	-0.066
	(0.061)	(0.063)	(0.070)	(0.067)
Observations	953	953	798	798

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors at the sub-district level are in parentheses.

Table 8: Heterogeneous effects on child outcomes with respect to parent characteristics.

	With Basic Controls		With More Controls	
	(1)	(2)	(3)	(4)
	CD	DENVER	CD	DENVER
Panel A: heterogeneous effect with respect to father's education				
parentingXFAnoBA	0.496***	0.425***	0.444**	0.404**
	(0.175)	(0.158)	(0.179)	(0.161)
cash-transferXFAnoBA	0.282	0.214	0.190	0.098
	(0.181)	(0.161)	(0.191)	(0.171)
Observations	911	911	798	798
Panel B: heterogeneous effect with respect to mother's education				
parentingXMAnoBA	0.123	0.159	0.187	0.237*
	(0.154)	(0.145)	(0.157)	(0.142)
cash-transferXMAnoBA	0.190	0.218	0.119	0.103
	(0.170)	(0.151)	(0.162)	(0.140)
Observations	935	935	798	798
Panel C: heterogeneous effect with respect to main caregiver's education				
parentingXCGnoBA	0.214	0.169	0.279*	0.202
	(0.161)	(0.151)	(0.161)	(0.153)
cash-transferXCGnoBA	0.095	0.133	0.094	0.058
	(0.168)	(0.161)	(0.174)	(0.163)
Observations	945	945	798	798
Panel D: heterogeneous effect with respect to having a parent as CG				
parentingXparentCG	0.127	0.117	0.047	0.035
	(0.107)	(0.105)	(0.127)	(0.118)
cash-transferXparentCG	0.031	0.010	-0.023	-0.018
	(0.095)	(0.097)	(0.108)	(0.112)
Observations	938	938	798	798

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors at the sub-district level are in parentheses.

Table 9: Heterogeneous effects on child outcomes with respect to household characteristics.

	With Basic Controls		With More Controls	
	(1)	(2)	(3)	(4)
	CD	DENVER	CD	DENVER
Panel A: heterogeneous effect with respect to household wealth				
parentingXwealth	-0.194***	-0.175**	-0.204**	-0.176**
	(0.071)	(0.068)	(0.088)	(0.083)
cash-transferXwealth	-0.072	-0.058	-0.045	-0.008
	(0.070)	(0.069)	(0.087)	(0.084)
Observations	949	949	798	798
Panel B: heterogeneous effect with respect to number of books				
parentingXbooks	-0.013	-0.013	-0.016*	-0.015*
	(0.009)	(0.009)	(0.008)	(0.009)
cash-transferXbooks	0.004	0.014	0.004	0.015
	(0.010)	(0.010)	(0.010)	(0.010)
Observations	856	856	798	798
Panel C: heterogeneous effect with respect to HH size				
parentingXhhsize	0.008	0.023	-0.002	0.017
	(0.034)	(0.033)	(0.037)	(0.035)
cash-transferXhhsize	-0.018	0.011	-0.029	-0.002
	(0.034)	(0.033)	(0.038)	(0.036)
Observations	945	945	798	798
Panel D: heterogeneous effect with respect to having both parents				
parentingXnoabsence	0.006	0.076	-0.060	0.021
	(0.138)	(0.114)	(0.156)	(0.141)
cash-transferXnoabsence	-0.025	-0.016	-0.017	0.010
	(0.119)	(0.104)	(0.131)	(0.117)
Observations	947	947	798	798

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors at the sub-district level are in parentheses.

SD and 0.324 SD, respectively. However, those two studies measured child outcomes using the Bayley Scales (Bayley, 2006), whereas our study used the DENVER II. One standard deviation of the Bayley Scales may be different from that of the DENVER II.

The closest study for comparison is the China Reach program (Zhou et al., 2026), which used DENVER II to measure child outcomes and also faced a similar issue: the placement of left-behind children with grandparents due to parental migration. The China Reach implemented the Reach Up curriculum, averaging 74 visits per child. We use our subdomain estimates to compare with those in Zhou et al. (2026), which reported effect sizes only for subdomains. Recall that the effect sizes of language-cognitive and fine-motor subdomains in Zhou et al. (2026) are 1.04 and 0.68 SD, respectively. A linear interpolation implies that our effect sizes for language-cognitive and fine-motor subdomains would have been $\frac{74}{30} \times 0.176 = 0.434$ and $\frac{74}{30} \times 0.151 = 0.372$ SD, respectively, if we could have implemented the home visit 74 times instead of 30 times. These are significantly less than those reported for the China Reach program (Zhou et al., 2026).

There are several potential explanations for the smaller effect. It could be that our implementation quality was lower than that of the China Reach, or that the Reach Up curriculum may be less effective with children who have already attended child development centers. Alternatively, the program is more effective for younger children, as our treated samples were, on average, 36 months old, whereas China Reach's samples were only 11 months old. Unfortunately, we have no evidence to confirm or contradict the first two reasons. We can discuss the last one more concretely using the heterogeneous effect reported in Section 5.4. First, the difference in child age at the start of our program is about 25 months. Second, the heterogeneous effect by child age implies that children one year younger should gain approximately 0.300 SD more. Therefore, the impact sizes of the parenting program, for language-cognitive and fine-motor subdomains, would have been about $0.434 + \frac{25}{12} \times 0.300 = 1.116$ and $0.372 + \frac{25}{12} \times 0.300 = 1.054$ SD, respectively, if the samples were about 11 months old initially instead of 36 months old. This counterfactual effect size is reasonably close to those reported in Zhou et al. (2026). This has an important practical implication: a parenting home-visiting program is more effective for younger children.

7 Conclusion

This study concurrently evaluates a parenting home-visiting program and a labelled cash-transfer intervention. This randomized experiment was conducted in six areas across Thailand, with a relatively large sample of 953 preschoolers aged 2-4 years at the start of the parenting intervention who already attended local child development centers.

The intent-to-treat (ITT) effect of the parenting program in the benchmark model is significant, with an effect size of approximately 0.13 SD, whereas that of the cash transfer is insignificant. This result is robust across several specifications and corrections. Both treatments significantly increased parental investment, in both time and materials. However, mediation analysis suggests that the significant effect of the parenting program may result mainly from the material investment. Treatment-on-the-treated (TOT) effects indicated that each home visit improved child outcomes by 0.004 SD.

We also found that the effects of both treatments are heterogeneous across children, parents, and household characteristics. In particular, the parenting program was more beneficial for younger and disadvantaged children, as indicated by having special needs, less-educated parents, lower household wealth, and fewer books at home. The cash transfer was more effective for younger children, children with special needs, and boys.

Interestingly, the fact that the parenting program is more effective for younger children helps reconcile the differences in effect sizes between our experiment and the China Reach program, which implemented the same Reach Up home-visiting program with much younger children (Zhou et al., 2026). This suggests we should expect a larger impact when implementing the home-visiting program with younger children. In addition, it confirms that the parenting program can be effectively adapted and implemented in different cultures.

This study has several limitations. First, this experiment was conducted mainly in rural areas. Therefore, it may not be readily extended to urban areas, which have different family and economic contexts. Second, this study assessed child development using the Denver II as the primary measure, enabling direct comparison with the China Reach program. However, there are other relevant measures of child development, such as executive functions. Future research should employ a broader range of measures to deepen our understanding of the

impact of a parenting program on human capital formation. Third, the cash-transfer intervention in this experiment includes an information provision in the form of a letter outlining the purpose of the transfer. Again, the potentially significant impact of the intervention on younger children must be interpreted as the impact of cash transfers with information provision, not as the impact of cash transfers alone. Fourth, this study was conducted with children already enrolled in child development centers, but we did not assess the quality of their teaching. Future research should assess teaching quality to better understand the interaction between the parenting program and teaching quality. Another issue concerns the insignificance of the cash-transfer intervention. One potential explanation is that our cash intervention is "too little, too late". In addition, the impact of the financial resource may be nonlinear, a possibility that cannot be tested in this experiment. It would be interesting to conduct an experiment with multiple levels of cash transfers targeting younger children to test the nonlinearity of the cash-transfer response.

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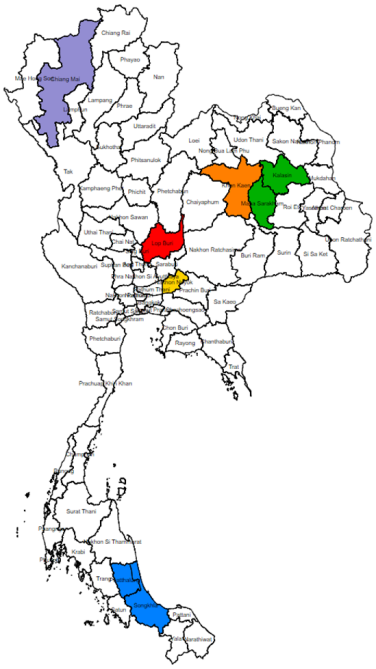


Figure 1: Colored areas are the six experimental areas across Thailand, including Chiang Mai (purple), Lopburi (red), Nakhon Nayok (yellow), Khon Kaen (green), Mahasarakham-Kalasin (green), and Songkhla-Phatthalung (blue).

Figure 2: Parents participated in an activity with the child and a home visitor.

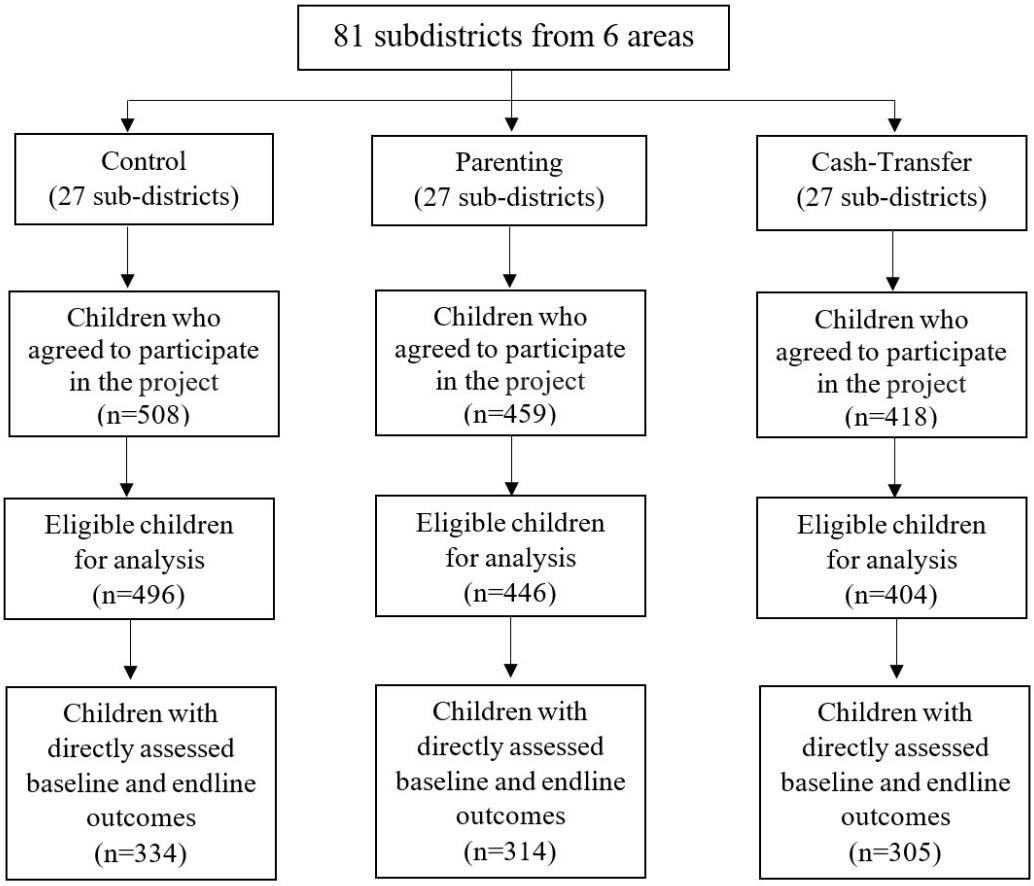
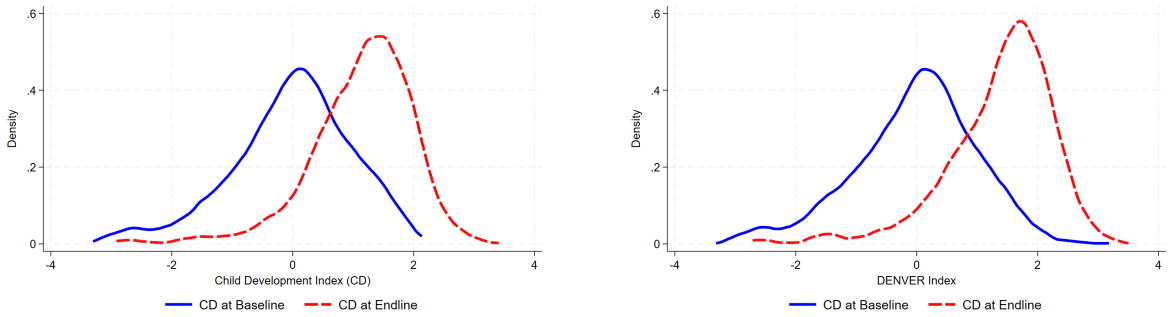


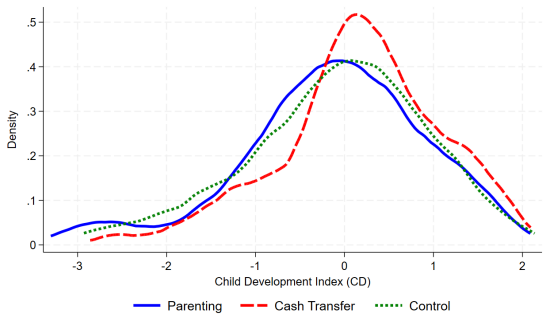
Figure 3: Sampling processes for the main sample of 953 children.



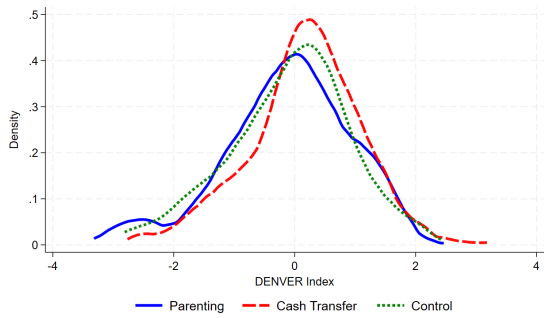
(a) Child Development Index (CD)

(b) DENVER Index

Figure 4: Distributions of child outcomes: (a) the child development index (CD) for the baseline (solid blue) and the endline (long-dash red), (b) the DENVER index for the baseline (solid blue) and the endline (long-dash red).

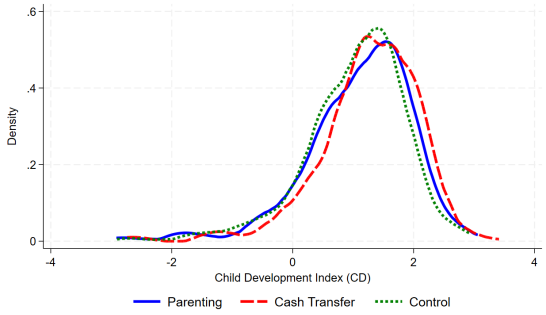


(a) Child Development Index (CD)

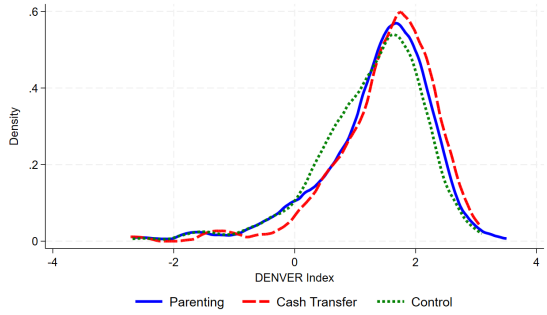


(b) DENVER Index

Figure 5: Distributions of child outcomes at the baseline: (a) the child development index (CD) for parenting (solid blue), cash-transfer (long-dash red) and control (short-dash green), and (b) the DENVER index for parenting (solid blue), cash-transfer (long-dash red) and control (short-dash green).



(a) Child Development Index (CD)



(b) DENVER Index

Figure 6: Distributions of child outcomes at the endline: (a) the child development index (CD) for parenting (solid blue), cash-transfer (long-dash red) and control (short-dash green), and (b) the DENVER index for parenting (solid blue), cash-transfer (long-dash red) and control (short-dash green).

Table 10: Descriptive statistics of the main samples

	Parenting		Cash-tranfer		Control		All sample	
	Mean	N	Mean	N	Mean	N	Mean	N
CD (BL)	-0.113	314	0.158	305	-0.101	334	-0.022	953
DENVER (BL)	-0.093	314	0.147	305	-0.099	334	-0.018	953
SE (BL)	0.020	313	0.067	304	-0.070	334	0.003	951
FM (BL)	-0.129	313	0.193	304	-0.174	334	-0.042	951
LG (BL)	-0.081	313	0.123	304	-0.078	334	-0.015	951
GM (BL)	-0.029	313	0.070	304	-0.049	334	-0.004	951
WM (BL)	-0.067	313	0.109	305	-0.076	334	-0.014	952
Child female	0.513	314	0.528	305	0.500	334	0.513	953
Child age (BL)	2.964	314	3.072	305	2.977	334	3.003	953
Child age (EL)	4.012	314	4.118	305	4.036	334	4.054	953
Age (Jan 2023)	3.131	314	3.237	305	3.142	334	3.169	953
Age (Apr 2023)	3.378	314	3.484	305	3.389	334	3.416	953
Days from BL to EL	383	314	382	305	387	334	384	953
Elig. children/subdist	19.407	27	17.815	27	20.481	27	19.235	81
Special needs child	0.023	311	0.043	301	0.036	334	0.034	946
Wealth	0.835	312	0.834	304	0.648	333	0.769	949
Household sizes	5.471	312	5.559	304	5.307	329	5.442	945
No. child in HH	1.926	312	2.043	304	2.040	329	2.003	945
Both parents at home	0.631	314	0.652	305	0.584	334	0.627	922
Parent caregiver	0.647	309	0.654	301	0.619	328	0.640	938
Thai spoken at home	0.452	292	0.484	287	0.455	314	0.464	893
Caregiver BA	0.183	312	0.132	304	0.109	329	0.141	945
Father BA	0.157	299	0.108	297	0.086	315	0.116	911
Mother BA	0.245	306	0.198	303	0.163	326	0.201	935
No. book at home	4.368	277	3.346	273	3.886	306	3.870	856
Student-teacher ratio	10.521	35	10.215	31	10.443	32	10.399	98

Note: BL = baseline; EL = endline.