Early childhood interventions and their impact on poor children and their mothers: Evidence from Ecuador*

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Abstract

This paper estimates the effects of two early childhood interventions – home visits and child care centers – on poor children and their mothers in Ecuador, exploiting a discontinuity in the programs' funding scheme in a regression discontinuity design. The forcing variable is a proposal-level score that takes only 34 distinct values. We therefore complement conventional cluster-robust estimates with three inference methods designed for discrete running variables: a wild cluster bootstrap, bias-aware honest confidence intervals, and finite-sample randomization inference. Our most robust finding is that home visits raise children's overall development. The point estimates further suggest that home visits improve specific cognitive and motor skills and mothers' psychological wellbeing while reducing mothers' labor supply, and that child care centers are associated with higher maternal working hours and income but with mixed and less precise effects on children. This is consistent with a trade-off between child development and maternal labor supply. We are explicit about which effects survive the most conservative inference. *JEL*-codes: J13, I28, H40, O12

*This version: June 2026. An earlier version of this paper circulated in 2011 as a Tinbergen Institute discussion paper but was never published. At the time we were reluctant to publish the results because we were uncertain whether the inference was valid: the forcing variable is a proposal-level funding score that takes only 34 distinct values, and it was not clear that the conventional standard errors we reported adequately reflected this discreteness and the small number of underlying clusters. This version resolves that concern. It re-estimates the design with modern local-polynomial regression-discontinuity methods and, crucially, assesses the robustness of every conclusion using three inference procedures developed for discrete running variables and few clusters (a wild cluster bootstrap, bias-aware honest confidence intervals, and finite-sample randomization inference), and reports explicitly which effects survive the most conservative of these. We acknowledge the financial support from the Ecuadorian government and the Inter-American Development Bank. The usual disclaimer applies. Rosero is the Director of the Statistics Division and Chief Statistician in FAO based in Rome-Italy. Oosterbeek is affiliated with the Vrije Universiteit Amsterdam and Tinbergen Institute.

1 Introduction

This paper examines the impact of two early childhood interventions – home visits and child care centers – on cognitive, motor and health outcomes of young children from poor families in Ecuador, and on the parenting styles, psychological wellbeing and labor market outcomes of their mothers.

At the time of our study, the largest funding organization of early childhood programs in Ecuador ranked proposals of prospective providers of such programs on the basis of a score which was a mixture of perceived quality of the providers and the social background of the community served by the program. The organization then allocated its available budget to the proposals from high to low scores until its funds were depleted. This funding route has since been replaced, but both interventions remain in operation today (Section 2).

Our sample consists of children (and their mothers) who were listed in submitted proposals. Roughly half of the children in our sample were listed in proposals with a score that just fell short of the funding threshold. These form the control groups of the two interventions. The other half of the children in our sample were listed in proposals with a score that just exceeded the funding threshold. These form the treatment groups.

A large literature documents the effects of early childhood interventions. Reviews of the developed-country evidence are provided by Heckman (2006), Currie (2001) and Almond and Currie (2011). In developing countries, home-visit programs across six countries consistently find positive effects on child development (Engle et al., 2007; Walker et al., 2011; Grantham-McGregor et al., 2007), and these gains can extend well beyond the program years: the twenty-year follow-up of the Jamaican stimulation experiment finds large returns in adult earnings (Gertler et al., 2014), and a randomized trial in Colombia shows that home stimulation improves child development by raising parental investments (Attanasio et al., 2020). Programs taken to national scale tend to find more modest effects (Andrew et al., 2018; Carneiro et al., 2024). Center-based programs covering eleven developing countries similarly find positive child-development effects (Engle et al., 2007; Leroy et al., 2012), and center-based care reliably raises mothers' labor-market participation (Halim et al., 2023). Broader syntheses are provided by Black et al. (2017) across the life course and by Attanasio et al. (2022) from an economics perspective. Despite this extensive evidence, direct comparisons of home visits and center-based care within the same design and population remain scarce; Sylvia et al. (2022) find that home-based delivery outperforms a center-based version of the same curriculum in rural China, but comparable evidence for large-scale national programs is limited.

Our study contributes in several ways to the existing literature. First, we consider a broad range of outcomes: children's cognitive and motor development, children's health, parenting styles, mothers' labor supply and income, and mothers' psychological wellbeing. Looking at this range of outcomes gives a more complete picture of the effects of early childhood interventions. Second, we evaluate the impacts of two different early childhood interventions using the same sampling design, the same tests and questionnaires and the same estimation method. Third, in comparison to most other studies that evaluate home visit programs, we analyze a large scale national program instead of a small, tailor-made intervention and we collected data from a relatively large sample.

The main results are as follows. (i) Home visits raise children's overall development. This is the most robust finding: the honest confidence interval excludes zero and the effect is significant under local-randomization inference. (ii) Home visits also improve specific cognitive and fine-motor outcomes and mothers' psychological wellbeing while reducing mothers' labor-market participation. These effects are significant under conventional inference and local randomization, but not under the wild cluster bootstrap or honest confidence intervals, so we treat them as suggestive. (iii) For child care centers the estimated effects on children are mixed in sign and are not robust to the inference methods for the discrete running variable. (iv) The point estimates for child care centers show higher maternal labor supply and income, but, apart from the household head's earnings, these are not statistically significant even under conventional standard errors in the local-linear specification, and none survives the wild cluster bootstrap. The point estimates are thus consistent with a trade-off between child development and maternal labor supply, but the evidence for the individual components of this trade-off varies in strength.

We proceed as follows. The next section provides further details about the early childhood development programs in Ecuador that we study, and about the context. Section 3 discusses the empirical approach used to identify the programs' impacts. Section 4 describes the sampling design and the scales on which various outcomes are measured. Section 5 provides descriptive statistics of the main variables used in the analysis. It also present evidence supporting the assumption that the groups above and below the funding threshold are not systematically different. Section 6 presents and discusses the main results. Section 7 summarizes and concludes.

2 Context and interventions

2.1 Context

Ecuador is an upper-middle income country, characterized by high poverty and high inequality. Of its total population of about 18 million people (2025), some 7.6 percent are children under five years old (2022 census), on the order of 1.4 million. National income poverty stood at 25.5 percent in 2024, and it is considerably higher among young children: more than half of children under five are poor on the unsatisfied-basic-needs measure. At the time of our study (data collected in 2008), the population was around 13 million. According to Grantham-McGregor et al. (2007), in 2004 Ecuador was together with Peru, Bolivia and Paraguay among the four countries in South-America where 20 to 40 percent of children under 5 years are disadvantaged. In the rest of South-America the percentage of disadvantaged children below age 5 is less than 20 percent. Paxson and Schady (2007) document a strong association in Ecuador between socio-economic status and children's early cognitive development. Schady (2011) shows that this association persists into primary school age and that early scores are strong predictors of later scores.

2.2 *The interventions*

The two programs that we examine in this paper are CIBV (for Centros Infantiles del Buen Vivir) and CNH (for Creciendo con Nuestros Hijos). The first is a program for child care centers, the second is a program for home visits. Both were publicly funded by the Child Development Fund (FODI), whose role and the way it selected providers we describe in Section 2.3. Both programs figure prominently in the report of Harris-Van Keuren and Rodríguez Gómez (2013), in which the authors present a comparative analysis of the guidelines of 19 programs for infants and toddlers under the age of 3 years in Latin America and the Caribbean.

One third of the children served by these programs attend a child care center, the other two thirds are exposed to home visits. Both programs have as their main objective to improve the early development of young children from poor families. The programs are provided at the community or neighborhood level. There is never more than one program per community/neighborhood. Children who are not served by one of the programs – the control groups – are normally looked after by their mother or another family member (grandparents or older sibling) or a neighbor.

Child care centers. The child care centers provide daycare, nutrition (breakfast and lunch) and educational services. It is full time: 52 weeks per year, 5 days per week, and 8 hours per day. Trained teachers work with groups of 8 to 10 children. There is no involvement of other adults. One center serves at most 45 children in the age range of 0 to 72 months.[1] The annual cost of the program amounts to US$ 488 per child. To put this amount into perspective, the average monthly income in our sample is US$ 300 per family or US$ 55 per capita.[2] FODI contributes at most 80% of the total costs of

[1]Primary education in Ecuador starts when children are between 6 and 7 years old. Enrollment in primary school is almost universal, but drops sharply at the secondary school level (see Oosterbeek et al., 2008).

[2]In 2000, Ecuador adopted the US dollar as its official currency.

Table 1. Breakdown of the costs of the programs

Financial Structure	Child care centers	Home Visits
Wages of administrative staff	13.4%	33.2%
Initial costs	9.8%	0.5%
Wages of pedagogical staff	31.5%	45.4%
Training	1.5%	3.0%
Food	37.5%	Not provided
Monitoring and evaluation	0.7%	2.2%
Pedagogical material	1.0%	4.5%
Others	4.6%	11.2%

Source: FODI.

child care centers; 20% has to come from other sources. The first column of Table 1 shows the breakdown of the total costs of child care centers into various components; the main components are wages of the pedagogical and administrative staff and food for the children.

The child care centers work with a curriculum for preschool education which is designed by the Ecuadorian government for children between 0 to 72 months old. This curriculum includes competencies and activities for the four classical domains of early childhood development: i) physical/motor development (including gross and fine motor skills); ii) socio/emotional development (including the notion of self-concept, self-confidence and the ability to interact with the environment); iii) language development (including verbal and nonverbal communication and the ability to initiate a conversation), and; iv) cognitive development (including abilities such as recognition, memory, imitation, problem solving and counting). For each domain, the curriculum lists competencies expected of children in a specific age range. Each competency is linked to indicators which are used to monitor children's progress. For each class (or meeting) an activity agenda is planned according to the curriculum.

The child care centers are targeted to communities where mothers are more likely to work. By making it easier for mothers to work (more) and to earn (more) income, the provision of child care centers may affect the outcomes of children and their mothers through various indirect channels. On the one hand, the extra income can be used to buy books, toys or more nutritious food, which may improve children's cognitive and motor skills and their health outcomes. On the other hand, when mothers work more hours, they will spend less time with their children. This may in turn have adverse effects on the outcomes of children and on the parenting styles of the mother. Moreover, combining the care of children with work, may deter mothers' psychological wellbeing.

Home visits. Through home visits, FODI attempts to stimulate children and to improve parents' attitudes, knowledge and behavior towards the development of their children. An important aim of the home visits is to teach mothers how to engage with their child in enriching activities, how to interact with their child in a nonaggressive way, how to create a responsive environment and how to prepare nutritional meals for their children. During a home visit, advice is communicated directly by explaining what, how, when and why an activity is performed. Activities are based on guidelines set by FODI. These guidelines are based on the same curriculum as the child care center intervention.

Children and their mother are treated individually when the child is younger than 3, and in groups of on average 22 children when the child is above 3. Home visits last 1 hour per week. The annual cost of this intervention is US$ 109 per child. FODI's contribution to home visits was up to 95% of this; only 5% has to come from other sources. The second column of Table 1 shows a breakdown of the costs of home visits into various components. Wages of teachers and administrative staff make up around 80% of the costs of this intervention.

Although there is no administrative record of the actual profile of the advisors working in the evaluated programs, the guidelines for the competitive process made no discrimination of gender among advisors. On the other hand, although it was not compulsory it was suggested that the advisors knew the locality as they were required to get to the community on a weekly basis. Moreover, they were also supposed to be in permanent contact with the community authorities in the rural area and keep them informed about their progress. FODI's application rules demand that instructors have at least a high school diploma and that they have at least one year of prior experience in the field of early childhood development. Instructors of awarded proposals received specific training about home visits during a period of three months.

The home visits are targeted to communities where mothers are more likely to stay at home. By teaching mothers how to interact with their children in an enriching, nonaggressive and responsive way, home visits may improve the cognitive and motor outcomes of children. They may also reduce mothers' willingness to work (by increasing the opportunity cost). Not having to combine the care of children with work together with the improved interaction with their children, may increase mothers' psychological wellbeing. Teaching mothers how to prepare healthy meals may have a positive impact on children's health outcomes.

Table 2 summarizes the main characteristics of the two programs.

Quality and implementation. Harris-Van Keuren and Rodríguez Gómez (2013) give both interventions a score of 0.5 on a scale from 0 to 1. The meaning of this score is

Table 2. Main characteristics of interventions

Program characteristic	Child care centers (CDI)	Home Visits (CNH)
Child Care services	Center based intervention consisting of nursery and pedagogical stimulation	No child care provided
Parental support	No parental support provided	Weekly session with parents and children at home for children 0 to 36 months and at a community center for older children
Nutritional services	Breakfast and lunch	Nutritional education for parents
Age group served	0 to 72 months	0 to 72 months
Operating schedule	52 weeks per year, 5 days per week, 8 hours per day	12 months per year, 1 day per week, 1 hour per visit
Residential Area coverage	Urban and rural	Urban and rural
Target population	Low income children in poor areas with working or likely to work mothers	Low income children in poor areas with non-working mothers
Children/instructor ratio	8 to 10 children. Maximum 45 children per center	Individual intervention at home 22 children per educator for children >3
Level of education of instructor	High school degree	High school degree; bachelor degree
Annual Cost per child (2006)	US$487.84	US$109.26

that the curricula of the two interventions meet the ideal indicators for 50%. The two programs can therefore not be regarded as showcase interventions. A score of 0.5 is, however, equal to the mean score of the 19 programs in Latin America and the Caribbean that the authors have assessed. This suggests that the programs considered in our evaluation are representative for this type of intervention in the region.

In the child care program, the curriculum concentrates 34% on physical/motor skills (versus 43% for the average of similar programs in the region), 24% on cognitive development (versus 17%), 25% on language (versus 23%), and 17% on social/emotional skills. Compared to the regional average, the Ecuadorian child care program therefore puts somewhat more emphasis on cognition and language and somewhat less on motor skills.

For the home visit program, the curriculum concentrates 34% on physical/motor development, 25% on cognitive development, 24% on language, and 17% on social and emotional development. The regional averages for comparable home visit programs are 20%, 21%, 26% and 33% respectively. Relative to the regional average, the Ecuadorian home visit program puts more emphasis on motor, cognitive and language development and less on social and emotional development.

FODI's application rules require that the home visit interventions started as soon as the contract was signed while the child care interventions were supposed to start at most 30 days after the contract had been signed. The latter is also shown by the data that we collected on length of treatment. Treated children in our sample had an average exposure to treatment of 21 months and this exposure is the same for treated children in either home visits or child care centers.

2.3 Selection process

At the time of our study, early childhood development programs in Ecuador were offered by both private and public providers. Private provision was small and mainly targeted towards middle and high income families, while public provision primarily served low income families, targeting children in rural and poor urban areas. There were three main public providers; the largest was FODI ("Fondo de Desarrollo Infantil").[3] FODI started in 2005 and, at the time of our study, served around 300,000 children from poor families all around Ecuador.

FODI does not run its own centers but subsidizes non-profit suppliers of early childhood development centers. FODI allocates its budget through a contest. It organized such

[3] The other two large public providers were INNFA (for "Instituto del Niño y la Familia") and ORI (for "Operación Rescate Infantil"). In July 2008, all three were merged into the Instituto de la Niñez y la Familia (INFA) by Executive Decree No. 1170. INFA was itself absorbed into the Ministry of Economic and Social Inclusion (MIES) in 2012 (Executive Decree No. 1356).

contests in 2005, in 2006 and in 2008. In this paper we use data from applicants (winners and losers) to the contest of 2006. This contest was organized to expand services to poor unserved neighborhoods or communities. The available budget for this contest was US$ 12 million. 240 organizations submitted a proposal, 95 of which were awarded for a total coverage of 60,000 children. Awarded proposals initially receive funding for a two year period, which is normally renewed afterward.

One proposal could – and typically did – involve the opening of centers in more than one community. A given community could, however, not be included in more than one proposal. All centers in a given proposal offer the same type of program (either home visits or child care centers) and pertain to only one province.

In the 2006-contest, a proposal was only considered for funding if: (i) it included a list with the names of the children it would serve if awarded, (ii) the area was not yet served by a public provider, and (iii) the proposal fulfilled certain standards regarding infrastructure and educators' skills. Proposals meeting these requirements were given a score based on five criteria (behind each criterion, the maximum number of points that can be earned for it): socioeconomic characteristics of the neighborhood (180); coherence of the proposal (130); share of outside funding (30); quality of personnel (150); financial aspects (110).

Adding the maximum scores per element gives an overall maximum score of 600. FODI then allocated its available budget for the second contest by funding proposals from high to low until the budget was exhausted. In practice FODI spent the last dollar of the available budget on a proposal that received an overall score of 425.[4] This last proposal offers home visiting programs, but the same cutoff score then applies to proposals for child care centers. All proposals for child care centers with a score above 425 received funding, while no proposal for child care centers with a score below 425 received funding. We will exploit the funding threshold in a regression discontinuity design. The threshold could not be anticipated by the applicants or the organizers, reducing the likelihood of manipulation.[5] The discontinuity applies at the level of proposals and not at the level of children listed in a proposal. There is no ranking of individual children within or across proposals.

The two interventions are still in operation today, now under Ecuador's "Misión Ternura" early childhood strategy and administered by what is currently the Ministry of Human Development. What changed is therefore not the programs but the way they are funded: the FODI proposal-scoring mechanism that we exploit was discontinued through

[4]Beforehand, a score of 400 was set as the minimum required quality standard. Proposals with a score below 400 would not qualify for funding even if the available budget was not exhausted.

[5]In Section 5 we show a density plot of the number of proposals by score. There is indeed no sign of manipulation and McCrary's density test fails to reject the null of no difference in density estimates just to the left and just to the right.

the mergers described above. This makes the regression discontinuity we study a historical episode, while the policy question it speaks to remains current.

3 Empirical approach

We are interested in the causal impact of exposure to child care centers and home visits on outcomes of children and their mothers. Naive OLS estimates are likely to be biased by selection: programs target disadvantaged communities, so without intervention treated children would probably have worse outcomes than the untreated. Conversely, parents may enroll children they perceive as particularly able.

The way in which FODI allocated its budget provides a regression discontinuity (RD) design (cf. Lee and Lemieux, 2010; Cattaneo and Titiunik, 2022). For each program type, proposals with a score at or above the funding threshold received support while those below did not. We instrument actual program exposure with the indicator $Z_i = \mathbf{1}[s_i \geq s_0]$, where s_i is the proposal score and $s_0 = 425$ is the cutoff. Compliance on the funded side is 97–99%, so the design is nearly sharp. The causal impact of home visits is identified from:

$$Y_i = \alpha_{HV} + \delta_{HV} HV_i + f_{HV}(s_i) + X_i\beta_{HV} + \varepsilon_{HVi} \tag{1}$$

where Y_i is an outcome, $HV_i = 1$ if the child was exposed to home visits and $HV_i = 0$ for children in the home-visit comparison group, and $f_{HV}(s)$ controls flexibly for the forcing variable. Analogously, the causal impact of child care centers is identified from:

$$Y_i = \alpha_{CC} + \delta_{CC} CC_i + f_{CC}(s_i) + X_i\beta_{CC} + \varepsilon_{CCi} \tag{2}$$

where $CC_i = 1$ if the child was enrolled in a child care center and $CC_i = 0$ for the child care comparison group. HV_i and CC_i are instrumented by Z_i. The parameters of interest are δ_{HV} and δ_{CC}, estimated on entirely separate samples: δ_{HV} uses children listed in home-visit proposals and δ_{CC} uses children listed in child care center proposals, so the same instrument is never used to identify two parameters simultaneously.

We estimate both equations by local-linear (p = 1) 2SLS with a triangular kernel and an MSE-optimal data-driven bandwidth chosen separately for each outcome and program using `rdrobust` (Calonico et al., 2014). The function $f(s)$ is approximated by a linear polynomial on each side of the cutoff within the bandwidth window; no global functional form is imposed. We report three specifications: (1) local linear without covariates; (2) local linear with covariate adjustment (Calonico et al., 2019) using MSE-optimal bandwidth; and (3) local linear with covariate adjustment using a coverage-error-rate (CER) optimal bandwidth, which is smaller than the MSE-optimal bandwidth and is chosen to minimize the error in confidence interval coverage rather than the bias–variance trade-off of the point estimate. Covariates include child sex, household size, urban status, welfare receipt, mother's age, schooling of mother and household head, mother's vocabulary score, parenting-practice dummies, ethnicity, household composition, and mother's literacy. Standard errors are clustered at the level of project $\times$ parish (98 clusters). All point estimates are bias-corrected and confidence intervals are bias-robust.

A feature of our design that complicates inference is that the funding score takes only 34 distinct values (one per proposal) because the score is constant within a proposal and there are 34 proposals in the vicinity of the cutoff. Standard local-polynomial confidence intervals may therefore overstate precision (Kolesár and Rothe, 2018). We complement the main estimates with three inference methods suited to this setting: wild-cluster-bootstrap p-values computed over the 34 proposals (Cameron et al., 2008); bias-aware honest confidence intervals for a discrete running variable (Kolesár and Rothe, 2018); and finite-sample randomization inference in a narrow window around the cutoff (Cattaneo et al., 2015). These are collected in a robustness table; when interpreting individual results we note which findings survive the most conservative of the three.

Comparing δ_{HV} with δ_{CC} requires care. Because the two estimates come from different samples, a direct comparison is valid only if the populations served are comparable. Even within the bandwidth window, however, HV and CC applicants differ on observable characteristics: home-visit proposals were more likely to be urban and served non-indigenous populations at higher rates (Table 8). A naive comparison of unadjusted estimates would therefore confound program effects with population differences. We address this by comparing the covariate-adjusted estimates (specification 2), which absorb the observable differences. The standard error of the difference $\hat{\delta}_{HV} - \hat{\delta}_{CC}$ is $\sqrt{SE_{HV}^2 + SE_{CC}^2}$, which is valid because the two samples are drawn from independent communities. The formal comparison is presented in Section 6.4.

4 Data

A total of 113 submitted proposals passed the minimum quality requirement of 400 points. Because data collection is expensive and available resources for this project limited, we focused data collection on proposals with a score relatively close to the funding threshold. For the appreciation of the results reported in this paper, it is important to consider that we thus start with a sample that can be regarded as a discontinuity sample (e.g. Angrist and Lavy, 1999). We subsequently selected a random sample of the centers that were included in these proposals. A proposal can include multiple centers, but only for the same type of child care program. The sampling was stratified by: assigned treatment status, type of

Table 3. Number of children by program, eligibility and age (number of centers in parentheses)

Program	Age	Non-eligible	Eligible	Total
Home visits	all	830 (28)	988 (33)	1818 (61)
	age >36 months	739 (27)	794 (33)	1533 (60)
	age <60 months	465 (27)	704 (33)	1169 (60)
Child care centers	all	411 (12)	478 (26)	889 (38)
	age >36 months	371 (11)	421 (26)	792 (37)
	age <60 months	194 (12)	318 (26)	512 (38)

program (home visits or child care centers), urban or rural area, center size and province (a proposal only serves communities in the same province).

Within each center, we selected a random sample from the children whose names were on the list attached to the proposal. Notice that the lists with names of the children that would be served are vital for our design. Without those lists it is impossible to know which children would have been treated by the providers that did not receive funding.

The final sample consists of 2,707 children in 99 centers; 38 providing child care centers and 61 home visits. Table 3 shows the numbers of observations (children and centers) in our final sample by type of program, age group and treatment eligibility.[6] We present a breakdown by age because some cognitive and motor tests are only validated for children older than 36 months while others are only validated for children younger than 60 months.

Teams of data collectors visited the homes of all children included in the final sample. They collected data through interviews with the mothers and through tests and measurements. Data were collected between September and December of 2008. At the moment of data collection, treated children in our sample had an average exposure to treatment of 21 months. This is the same for child care centers and home visits.

We used standard and validated test instruments to measure the cognitive, motor and social-emotional development of children. Some tests are specific for children older than 36 months. These are the Spanish versions of the Peabody Picture Vocabulary test which measures receptive vocabulary (language), the Woodcock-Johnson-Munoz test which measures long term memory, and the Pegboard test which measures fine motor skills. For all children from 0 to 60 months old, we use the Nelson-Ortiz test which measures four dimensions of child development: language skill, gross motor skill, fine motor skill and social behavior.

Test scores are standardized by age. We normalized the scores on the Peabody, Wood-

[6] As we will show in Section 5 treatment eligibility and actual exposure to treatment are very highly correlated.

cock and Pegboard tests to mean zero and standard deviation one. Impact estimates are thus expressed in standard deviation units. Scores on the Nelson Ortiz tests are binary: above or below the mean of the age group.

To measure children's health we use height for age and weight for age. Height for age is an indicator for long-term health outcomes, while weight for age more reflects short-term health conditions. We also took blood samples to measure the hemoglobin levels of the children to detect iron deficiency anemia. For the mothers we measure the following outcomes: the Center for Epidemiological Studies Depression (CES-D) scale to measure depression and psychological stress, the Home Observation for Measurement of the Environment (HOME) scale to measure responsiveness to children, and variables related to the labor market such as participation, working hours and income. The CES-D covers the main symptoms of depression and is derived from five validated depression scales. The score on the HOME test is based on the interviewer's evaluation of the mother's attitudes and behavior towards the child during the interview. We converted respondents' scores on the CES-D test and the HOME test to mean zero and standard deviation one, so that again impact estimates are measured in standard deviation units.

5 First stage and identifying assumptions

In this section we will first show that whether a proposal is above or below the funding threshold almost perfectly determines whether a child that is on the list of a proposal, is exposed to treatment. We then discuss the identifying assumption and provide evidence in support of it.

All proposals with a score above the threshold received funding from FODI and all proposed programs had been implemented at the moment of data collection. Likewise, none of the proposals with a score below the threshold received funding from FODI and none of these proposed programs have been implemented. At the proposal or program level, the allocation of FODI's budget thus represents a sharp regression discontinuity design; the score assigned to the proposal perfectly determines whether the proposal receives funding and whether the proposed program is implemented.

The sharp design at the proposal or program level translates into an almost sharp design at the level of children. Just a few children included in a proposal that received funding did not participate in the program, and also just a few children included in a proposal that did not receive funding participated in an(other) early childhood program.[7] The almost perfect compliance with the assigned treatment status results in first stage esti-

[7] Two children did not participate in a home visit program while they should, 9 children did not participate in a child care center program while they should, 7 children participated in a home visit program while they should not, and 5 children participated in a child care center while they should not.

Table 4. First stage regressions

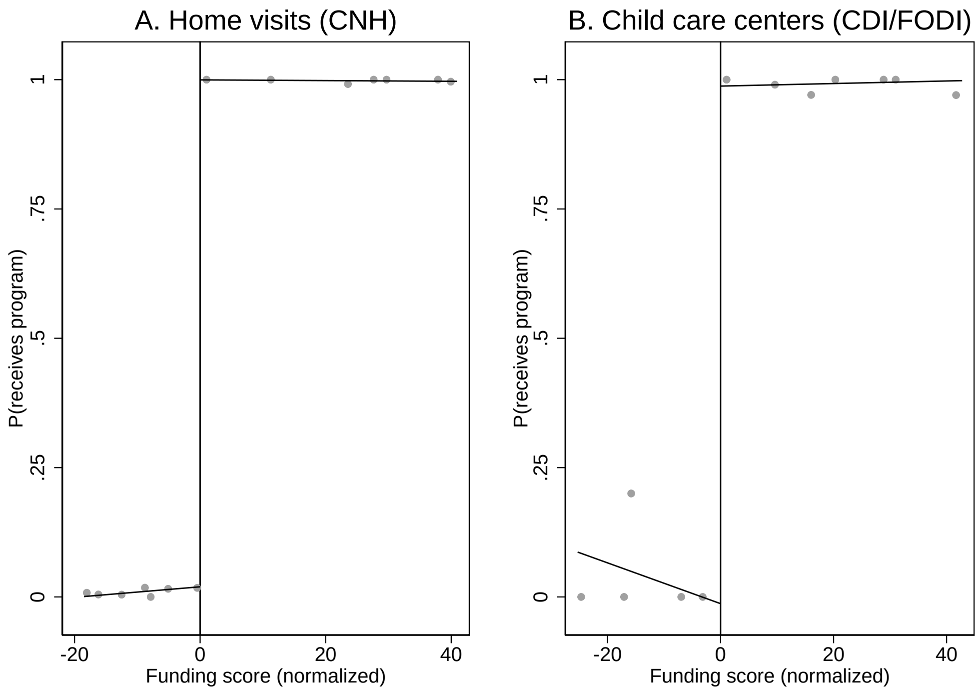
	(1) LL, MSE	(2) LL, MSE+X	(3) LL, CER+X
A. Home visits			
Above threshold (Z)	0.981***	0.976***	0.976***
	(0.011)	(0.007)	(0.007)
Effective N	1174	1147	1029
Bandwidth h	24.7	23.8	19.4
B. Child care centers			
Above threshold (Z)	1.010***	1.089***	1.011***
	(0.030)	(0.031)	(0.035)
Effective N	308	418	305
Bandwidth h	14.3	15.9	13.2

Notes: Each cell reports the bias-corrected RD estimate of the effect of being above the funding threshold on actual program take-up, with robust standard error in parentheses, obtained with `rdrobust` (Calonico et al., 2014). Specifications match those of Tables 9–12: (1) local linear (p=1), MSE-optimal bandwidth, no covariates; (2) local linear, MSE-optimal, with covariate adjustment (Calonico et al., 2019); (3) local linear, CER-optimal bandwidth, with covariates. Standard errors clustered on project × parish. $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

mates close to one. Figure 1 plots the near-perfect jump in program take-up at the funding cutoff for both programs; Table 4 quantifies this with the same local-linear specifications as the main results tables.

As mentioned before, proposals with a score above 425 (on a scale from 0 to 600) received funding and proposals with a lower score did not. Table 5 shows average values of the scores for proposed programs above and below the threshold in our sample, separately for home visits and for child care centers. The table also shows the average scores the proposals received on each of the five components of the total score. Because the score is assigned at the proposal level, we compute these averages over the 34 proposals rather than over centers; with so few proposals the tests have limited power. For home visits, the only component that differs significantly between funded and unfunded proposals is the quality of staff (column (3)); the differences in the other components, including the socio-economic background of the communities served, are not significant. For child care centers, the quality of staff and the coherence of the proposal differ between funded and unfunded proposals, though only marginally given the small number of proposals (column (6)); the remaining components do not differ significantly. The final column compares home-visit and child care center proposals. The point estimates suggest that home visits are proposed for somewhat better-off communities (they collect fewer points for SES), but with only 34 proposals this difference is not statistically significant; the components that do differ across programs are co-funding and the community share of

Figure 1. First stage: program take-up by funding score



Notes: Binned means of treatment take-up against the normalized funding score, estimated by `rdplot` (Calonico et al., 2014). Left panel: home visits (CNH); right panel: child care centers (CDI/FODI). Take-up jumps from near zero to near one at the cutoff in both programs, confirming a near-sharp design. Compliance rates are 97–99% on the funded side; the quantitative first-stage estimates are in Table 4.

child labor. Total scores do not differ significantly between the two programs, so by this grading system the two interventions are of comparable overall quality. The more salient differences between the populations served, in urbanicity and ethnicity, are documented when we compare the two programs (Table 8), and motivate the covariate adjustment used there.

The identifying assumption in the regression discontinuity design is that conditional on a smooth function of the underlying score and observables included in the analysis, there are no systematic unobserved differences between observations just below the threshold and observations just above the threshold. While this assumption cannot be tested, we can test whether observations above and below the threshold are not systematically different in terms of their observable characteristics.

Tables 6 and 7 show the mean values of observable characteristics. The first for variables measured at the community level, the second for variables measured at the level of children and their mothers. The tables show the average values for community and background variables separately for observations below and above the funding threshold and separately for the two programs. They also report p-values for the difference in means

Table 5. Components of score

Variable	Home visits (HV)			Child care centers (CC)			HV vs CC
	$s < 425$	$s \geqslant 425$	p	$s < 425$	$s \geqslant 425$	p	p
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Total score	415.69	452.96	0.000	412.10	445.26	0.000	0.383
	(6.24)	(14.39)		(8.88)	(12.96)		
SES	174.29	173.46	0.800	178.00	173.89	0.110	0.441
	(6.73)	(6.89)		(2.74)	(4.86)		
Co-funding	25.33	26.43	0.739	20.40	22.67	0.214	0.041
	(7.98)	(6.43)		(0.89)	(3.74)		
Quality of staff	22.65	48.67	0.000	23.71	45.39	0.054	0.772
	(12.83)	(11.97)		(17.53)	(18.55)		
Coherence	105.97	110.56	0.368	102.43	113.79	0.094	0.843
	(11.35)	(10.20)		(15.07)	(8.67)		
Financial aspects	87.46	93.84	0.213	87.56	89.52	0.750	0.457
	(8.76)	(11.32)		(11.10)	(10.68)		
N	7	13		5	9		34

Notes: Average values of the total funding score and its five sub-components for proposals below ($s <$ 425) and above ($s \geqslant 425$) the funding threshold, separately for home visit (HV) and child care center (CC) proposals. The unit of observation is the proposal, which is the level at which the score is assigned and treatment is determined: there are 34 proposals (20 HV, 14 CC), each with a single score. Standard deviations in parentheses. Columns (3) and (6) report p-values from a t-test for the difference in means between below- and above-threshold proposals; column (7) the p-value for the difference in means between HV and CC proposals (pooling below and above threshold). Given the small number of proposals these tests have limited power. The funding score ranges from 0 to 600 and is composed of five criteria: socioeconomic status of the target community (SES), co-funding commitment, quality of proposed staff, coherence of the project plan, and financial soundness.

between below- and above-threshold groups. For the community-level variables in Table 6 we test at the proposal level, the level at which the score is assigned and treatment is determined (34 proposals), using a simple difference in means; for the child- and mother-level variables in Table 7 we test at the individual level, conditioning on a second-order polynomial of the forcing variable. The p-values in columns (3) and (6) of Table 6 indicate that, for both programs, funded and unfunded communities are not significantly different on the community characteristics. The only difference significant at conventional levels is the school supply per 1,000 inhabitants for home visits ($p = 0.05$), and even there the point difference is modest. (The apparent imbalances in level of schooling and access to water that arise when the test is run at the center level do not survive once the proposal is used as the unit of analysis.)

The p-values in columns (3) and (6) of Table 7 indicate that the characteristics of children eligible for treatment are in most cases not significantly different from the characteristics of children not eligible for treatment. There is only a systematic difference in age.[8] In the samples of both programs eligible children are significantly younger than non-eligible children; the age gap is about 6 months. Part of this difference can be attributed to eligible children being interviewed earlier than non-eligible children. But even after we correct for that, a significant difference remains. We have no explanation for this.

To assess whether the difference in age between treatment and control groups can drive our results, we regressed each outcome variable on age using data from the control groups only. We did that for the two control groups together and separately. For most outcomes there is no significant relation with the age of the child. There are three exceptions: fine motor skills for children older than 36 months, anemia and non-responsiveness of the mother. In all three cases, the signs of these relationships are, however, the opposite of what would explain our findings. For instance, the relation between age and fine motor skills is positive. In the absence of a treatment effect, this would imply that eligible children do worse than non-eligible children. This implies that estimated treatment effects that are not, or insufficiently, corrected for the age pattern, are biased downwards. Similar reasonings apply to the effects of the treatments on the other two outcomes. We will report results from specifications i) that do not control for age (other than that some outcome variables are standardized by age); ii) that only control for age (and the running variable); iii) that control for age along with other covariates.[9]

[8]The differences between eligible and non-eligible children are also statistically significant for gender in the home visits program and for receipt of the cash transfer in the child care center program. The means of the these variables are, however, almost identical for eligible and non-eligible children.

[9]We also redid all analyses including higher order terms of age. This does not change any of the results.

Table 6. Differences in community characteristics, by eligibility status

Variable	Home visits (HV)			Child care centers (CC)			HV vs CC
	$s < 425$	$s \geqslant 425$	p	$s < 425$	$s \geqslant 425$	p	p
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Poverty (share)	0.60	0.65	0.293	0.69	0.58	0.336	0.760
	(0.14)	(0.08)		(0.19)	(0.18)		
School enrollment (share)	0.93	0.92	0.495	0.89	0.92	0.175	0.348
	(0.03)	(0.03)		(0.04)	(0.03)		
High school enrollment (share)	0.68	0.65	0.480	0.64	0.67	0.695	0.923
	(0.07)	(0.10)		(0.12)	(0.12)		
Level schooling (years)	8.47	7.32	0.120	6.26	7.78	0.287	0.493
	(0.67)	(1.78)		(1.71)	(2.75)		
Child labor (share)	0.02	0.03	0.520	0.06	0.03	0.097	0.022
	(0.01)	(0.01)		(0.04)	(0.02)		
Access water (share)	0.39	0.29	0.218	0.18	0.38	0.169	0.776
	(0.15)	(0.18)		(0.16)	(0.27)		
Access sanitation (share)	0.77	0.80	0.512	0.70	0.74	0.572	0.051
	(0.07)	(0.09)		(0.11)	(0.13)		
Outpatient health centers (per 1000)	2.63	3.19	0.445	2.77	3.17	0.471	0.941
	(1.17)	(1.67)		(1.00)	(0.96)		
Inpatient health centers (per 1000)	0.34	0.27	0.504	0.52	0.45	0.616	0.027
	(0.21)	(0.23)		(0.34)	(0.18)		
School supply (per 1000)	1.62	2.70	0.046	2.08	1.98	0.881	0.457
	(0.74)	(1.21)		(1.11)	(1.30)		
High school supply (per 1000)	0.05	0.05	0.754	0.05	0.04	0.793	0.744
	(0.02)	(0.03)		(0.02)	(0.02)		
N	7	13		5	9		34

Notes: Mean values of community-level characteristics for proposals below and above the 425-point funding threshold, separately for home visits (HV) and child care centers (CC). The unit of observation is the proposal (34 in total: 20 HV, 14 CC); community-level variables are averaged to the proposal. Standard deviations in parentheses. p-values in columns (3) and (6) are from t-tests for equality of means between below- and above-threshold proposals within each program; column (7) reports the p-value for the difference in means between all HV and all CC proposals. With so few proposals these tests have limited power.

Table 7. Differences in characteristics of children and mothers, by eligibility status

Variable	Home visits (HV)			Child care centers (CC)			HV vs CC
	$s < 425$	$s \geqslant 425$	p	$s < 425$	$s \geqslant 425$	p	p
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Boy (dummy)	0.52	0.51	0.050	0.50	0.52	0.274	0.743
	(0.50)	(0.50)		(0.50)	(0.50)		
Age (months)	55.8	49.6	0.000	58.3	52.0	0.002	0.055
	(14.4)	(12.9)		(14.3)	(12.4)		
Household size (persons)	4.87	4.85	0.437	5.70	5.25	0.245	0.026
	(1.71)	(1.71)		(1.97)	(2.00)		
Urban (dummy)	0.70	0.66	0.934	0.49	0.40	0.802	0.088
	(0.46)	(0.48)		(0.50)	(0.49)		
Cash transfer (dummy)	0.58	0.55	0.388	0.61	0.58	0.036	0.460
	(0.50)	(0.50)		(0.49)	(0.49)		
Mother's age (years)	30.7	30.5	0.847	31.6	30.6	0.109	0.283
	(8.56)	(8.12)		(8.46)	(7.96)		
Schooling mother (years)	7.24	7.61	0.421	5.38	5.96	0.375	0.000
	(3.66)	(4.13)		(3.55)	(3.56)		
Schooling head (years)	6.66	6.72	0.968	5.51	5.74	0.854	0.006
	(3.87)	(5.36)		(3.50)	(3.37)		
Language score mother	72.1	70.5	0.994	59.78	62.2	0.398	0.016
	(23.9)	(26.2)		(26.0)	(25.1)		
Father present (dummy)	0.72	0.81	0.436	0.83	0.77	0.154	0.324
	(0.45)	(0.39)		(0.38)	(0.42)		
Mother present (dummy)	0.95	0.96	0.721	0.95	0.96	0.940	0.734
	(0.22)	(0.21)		(0.23)	(0.19)		
N	830	988		411	478		2707

Notes: Mean values of child and mother characteristics for children below and above the 425-point funding threshold, separately for home visits (HV) and child care centers (CC). Observations are at the child level. Standard deviations in parentheses. p-values in columns (3) and (6) are for the below-vs-above difference within each program, conditioning on a second-order polynomial of the forcing variable, with standard errors clustered on project $\times$ parish. Column (7) reports the p-value for the difference in means between all HV and all CC children.

The last column reports p-values for differences in characteristics between children whose name appeared on the list of a proposal for home visits and children whose name appeared on the list of a proposal for a child care center. These cross-program comparisons are relevant for interpreting the RD estimates side by side. The results show that children listed for child care centers are significantly older than children listed for home visits. There also is a difference in social background between children listed for home visits and children listed for child care centers. This difference shows up in the schooling levels of the mother and of the household head, mother's language score, and household size. As we already saw in Table 5, home visits serve children from relatively better-off families.

Because of the differences between the groups targeted by the two interventions we check whether the populations of HV and CC applicants are comparable *within* the MSE bandwidth window. Table 8 reports covariate means for HV and CC applicants whose proposal score falls within ± 15 normalized score units of the cutoff, along with the difference and its p-value. Even within this narrow window some differences remain: HV applicants are more likely to be urban (75 % vs. 36 %) and non-indigenous (67 % vs. 34 %), and have somewhat more educated mothers and household heads. The joint F-test rejects balance at the 5 % level. These differences reflect the program designs: home visits can operate in urban settings and require only a visiting worker, while child care centers require a physical facility and are more often located in rural or indigenous communities.

Table 8. Covariate balance: home visits vs. child care centers within ± 15 score units of the cutoff

	HV	CC	Difference	p-value
Child age (months)	53.9	56.2	-2.3	0.354
Male	0.526	0.485	0.041	0.227
Children < 5 in household	1.7	2.0	-0.3**	0.032
Urban	0.753	0.361	0.392	0.118
Welfare transfer	0.556	0.590	-0.034	0.660
Mother age (years)	30.2	31.1	-0.9	0.225
Mother schooling (years)	7.4	5.6	1.7*	0.073
Head schooling (years)	6.6	5.7	0.9**	0.044
Mother vocab. score	71.7	58.4	13.3*	0.093
Parenting index 1	0.780	0.816	-0.036	0.402
Parenting index 2	0.964	0.944	0.020	0.206
Indigenous	0.328	0.656	-0.328***	0.004
Adolescents in household	1.1	1.7	-0.5	0.172
Adults in household	1.9	2.0	-0.1	0.323
Elderly in household	0.0	0.0	-0.0	0.350
Mother illiterate	0.072	0.167	-0.095	0.244
Joint F-test (16 covariates)			$F = 2.24$**	0.022

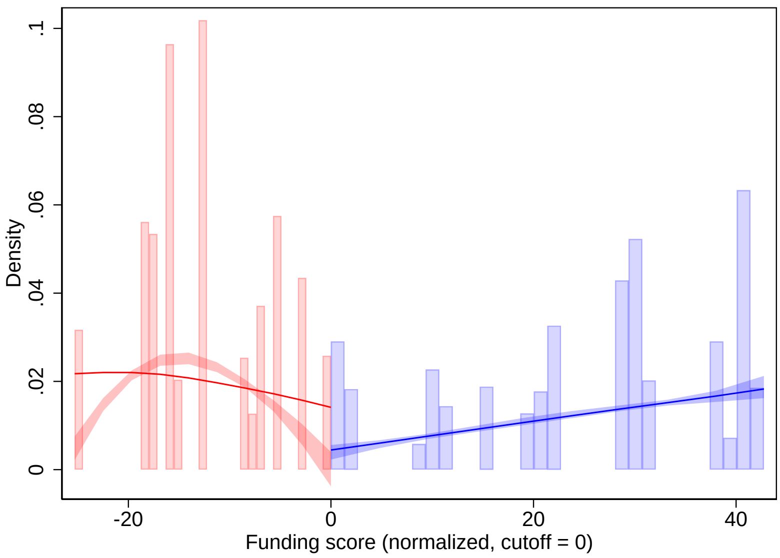
Notes: Sample restricted to children whose proposal score falls within ± 15 normalized score units of the funding cutoff. Means are child-level; differences estimated by OLS with standard errors clustered on project $\times$ parish. Stars on the difference and the joint F-test denote significance: $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Because observable differences persist near the cutoff, a direct comparison of the two reduced-form RD estimates would confound program effects with differences in the populations served. We therefore compare the two programs using the covariate-adjusted specifications (column 2 of Tables 9–12), which absorb the observable differences between HV and CC applicants. The difference table is presented in Section 6.4.

As a formal test for manipulation of the running variable, we apply the local-polynomial density test of Cattaneo et al. (2020) at the proposal level; there should be no systematic bunching of proposals just above or just below the cutoff. Figure 2 shows the estimated densities on each side and reports the test result. The test yields $p = 0.075$, failing to reject the null of no discontinuity in density at the conventional 5% level. With only 34 distinct proposal-level scores the test has limited power, but the institutional design also makes strategic score manipulation implausible: the funding score was computed externally by government evaluators, not by the applicants themselves.

Smoothness of the proposal density does not imply smoothness in the number of sampled observations across the score distribution, for two reasons. First, proposals differ in size. Second, we randomly sampled centers stratified by treatment status, program type, urban/rural area, center size, and province to achieve balance across treated and untreated groups. Without stratification, we might have over-sampled the two geographically extreme proposals closest to the threshold, biasing estimates for spatially heterogeneous outcomes.

Figure 2. Test for manipulation in the running variable



Notes: Local-polynomial density estimates on each side of the funding-score cutoff (normalized to zero), with 95% confidence intervals, estimated by `rddensity` (Cattaneo et al., 2020). A robust test for a discontinuity in the density yields $p = 0.075$. The near-uniform spread of proposals on both sides of the threshold is consistent with the external scoring process; strategic manipulation of the score is institutionally implausible. The running variable takes only 34 distinct values, which reduces power of the test.

6 Results

This section presents in three subsections results for children's cognitive outcomes, for children's health outcomes, and for mothers' outcomes. We begin the first subsection with a graphical presentation of the cognitive and motor outcomes, and then present and discuss estimates of the impact of the two interventions on the various outcomes.

6.1 Children's cognitive and motor outcomes

Figure 3 plots binned sample means of three standardized cognitive and motor outcomes against the normalized proposal funding score, separately for home visits (left column) and child care centers (right column), estimated with `rdplot` (Calonico et al., 2014). Each bin represents the mean outcome of children whose proposal score falls within an evenly-spaced interval; the curves are local-linear fits on each side of the cutoff.

The slopes within each side of the cutoff are shallow and mostly negative, reflecting that the proposal score rewards poorer communities, so children from higher-scoring proposals tend to have lower baseline outcomes. For home visits the local-linear fits display a clear upward jump at the cutoff for all three outcomes. For child care centers the discontinuity is smaller and less consistent across outcomes.

We further investigate this in Tables 9 and 10, which report local-linear RD estimates of the impact of home visits and child care centers on all cognitive, motor and health outcomes. The top panel of each table groups the tests administered to children older than 36 months, the second panel the Nelson-Ortiz dimensions for children younger than 60 months, and the bottom panel the health outcomes. Table 9 reports estimates for home visits and Table 10 for child care centers, based on equations (1) and (2), respectively. Each table reports three specifications: local linear with the MSE-optimal bandwidth and no covariates (column 1), the same with covariate adjustment (column 2), and local linear with the CER-optimal bandwidth and covariates (column 3).[10]

The results for home visits in the top panel reveal the same pattern as Figure 3. Home visits have a positive point estimate on most cognitive and motor outcomes in both specifications, and adding covariates produces very similar but more precise estimates. Focusing on the specification with covariates, all three tests for children older than 36 months, together with fine motor skills and the overall development index for children under 60 months, are significantly positive at conventional levels. The point estimates are sizeable: the standardized cognitive and motor tests improve by between 0.6 and 0.9 of a standard deviation, and the overall Nelson-Ortiz index, measured as the probability of scoring above the age-group mean, rises by about 30 percentage points relative to a control-group base of 17 percent. These conventional-inference results should be read with caution: as Table 14 shows, only the effect on the overall development index survives the inference methods designed for the discrete running variable, while the individual test results are

[10] The data also contain a variable indicating households' poverty. This variable cannot be included as a control variable because it has been measured after the intervention and might therefore have been affected by it.

not robust to the wild cluster bootstrap or the honest confidence intervals. We therefore treat the individual cognitive-test effects as suggestive.

For child care centers the estimates are mixed in sign and, with the exception of fine motor skills for children over 36 months, mostly insignificant. That one large positive estimate is not credible as a stable effect: it is highly sensitive to the bandwidth (it more than halves when the bandwidth is doubled) and does not survive the wild cluster bootstrap or the honest confidence interval (Table 14). Across the cognitive and motor outcomes there is thus no consistent or robust pattern, and we find no reliable evidence that child care centers improve children's cognitive development.[11]

6.2 Children's health outcomes

We next look at the impact of the programs on children's health outcomes; the estimates are in the bottom panels of Tables 9 and 10. For home visits, the covariate-adjusted estimates point to a reduction in the incidence of anemia of about 8 percentage points, relative to a control-group rate of 0.46 (a reduction of roughly one-sixth). The point estimates for underweight and below height for age are positive (that is, in the adverse direction) and significant under the clustered analytic standard errors. None of these health effects is robust, however. Once we apply the inference methods for the discrete running variable, the anemia effect loses significance (Table 14), and the same holds for the underweight and below-height estimates: their wild-cluster-bootstrap p-values are 0.14 and 0.40 and their honest confidence intervals comfortably include zero. We therefore do not interpret these as evidence that home visits affect children's physical health in either direction. For child care centers the point estimates also indicate higher rates of underweight and below height, but these likewise do not survive inference designed for the discrete running variable (wild-bootstrap p-values of 0.48 and 0.24, honest confidence intervals spanning zero). Overall, we find no robust effect of either program on children's measured health.

6.3 Mothers' outcomes

Early childhood interventions potentially also have an impact on the way mothers interact with their children and on mothers' psychological well-being and labor market outcomes. Tables 11 and 12 present results for maternal wellbeing and labor market outcomes.

The parenting and wellbeing results for home visits are in the top panel of Table 11. Focusing on the covariate-adjusted specification, the point estimate indicates that home visits make mothers more responsive to their children (a reduction of about half a stan-

[11] We also inquired whether impacts are different for boys than for girls, and for children above and below 24 months old. Neither for home visits nor for child care centers, do we find any evidence of heterogeneity of impacts across these groups.

dard deviation in the non-responsiveness index), though this effect is significant only in the specification without covariates and, at the ten-percent level, under the CER bandwidth. Home visits have no significant effect on whether the mother reads to her child or on the presence of learning materials (books, clay, toys) at home. The clearest maternal result is for psychological wellbeing: home visits reduce stress and depression by about 0.7 of a standard deviation, an effect that is significant across all three specifications. For child care centers (Table 12) the corresponding point estimates for responsiveness and for maternal stress are close to zero and not significant in the covariate-adjusted specification, so the favourable wellbeing pattern is specific to home visits. As with the child outcomes, the home-visit effect on maternal stress is significant under conventional and local-randomization inference but not under the wild cluster bootstrap or the honest confidence interval (Table 14), and we treat it as suggestive.

The direct comparison of the two programs, reported in Section 6.4 (Table 13), points in the same direction: relative to child care centers, home visits are associated with more responsive parenting and lower maternal stress.

The maternal labor-market results appear in the bottom panels of Tables 11 and 12. The covariate-adjusted point estimates have the expected opposite signs. Child care centers raise mothers' labor supply (about 10 percentage points more likely to work and roughly 5 additional hours per month), while home visits lower it, by about 26 percentage points and roughly 9 hours per month. For home visits these labor-supply estimates are significant under the clustered analytic standard errors; for child care centers they are not even conventionally significant in the local-linear specification. On earnings, home visits are associated with a significant reduction in the mother's own earnings and an insignificant effect on the household head's earnings, whereas child care centers show a positive and significant effect on the household head's earnings but an insignificant effect on the mother's own earnings.[12] These labor-market effects must again be read against the discrete-running-variable inference (Table 14): none survives the wild cluster bootstrap, and the home-visit effects on labor supply, while significant under conventional and local-randomization inference, we treat as suggestive. The direct program comparison is reported in Section 6.4.

6.4 Comparing home visits and child care centers

Table 13 formalises the comparison between the two programs. For each outcome we report the HV$-$CC difference in the covariate-adjusted RD estimates (the underlying es-

[12]The larger effect on the household head's earnings than on the mother's own earnings most likely reflects that many poor households in Ecuador earn their income in the informal sector through activities in which household members work together.

timates are in column (2) of Tables 9–12) and the p-value for the null of equal effects. The difference is identified under the assumption that, conditional on the included covariates, the observable differences between HV and CC applicants documented in Table 8 fully account for selection into program type.

The estimates point to a broadly consistent pattern. On children's development, the HV$-$CC difference is positive and significant for Woodcock memory and for the overall Nelson-Ortiz development index, favouring home visits. For the pegboard fine-motor test the difference is large and negative, but this is driven by the implausibly large and non-robust child-care fine-motor estimate discussed above rather than by a home-visit shortfall; the difference in language is not significant. On health, the differences favour home visits (lower anemia, underweight and below height). For maternal outcomes the pattern reverses: child care centers are associated with greater maternal labor supply and earnings, while home visits are associated with lower maternal stress and labor supply, with the differences in stress, labor supply and earnings all sizeable. Taken together, and bearing in mind that these differences rest on a selection-on-observables assumption and inherit the fragility documented in Table 14, the estimates are consistent with a trade-off: home visits invest more in child development while constraining mothers' labor supply, whereas child care centers free up mothers to work with weaker and less consistent child-development gains.

6.5 Robustness of inference to the discrete running variable

The forcing variable takes only 34 distinct values. Table 14 reports our main covariate-adjusted local-linear estimates (identical to column (2) of the main tables) alongside three inference methods designed for this setting. The point estimates are unchanged, but the wild cluster bootstrap p-values and the honest confidence intervals are substantially less precise than the clustered analytic standard errors. The wild cluster bootstrap, the most demanding of the methods, leaves no individual outcome significant at conventional levels. The one home-visit effect that survives throughout is the effect on the Nelson-Ortiz overall development score: its honest confidence interval excludes zero and it is significant under local-randomization inference. The home-visit effects on memory, fine motor skills, maternal stress and maternal labor supply remain significant under conventional and local-randomization inference but not under the wild cluster bootstrap or the honest confidence intervals, so we treat them as suggestive. For child care centers, the apparent effects on mothers' labor supply and stress are not significant under the local-linear specification even with clustered analytic standard errors (Panel B); the one child-care effect that is significant across conventional and local-randomization inference is on children's fine motor skills. We therefore interpret the individual test results with caution.

Figure 4 examines sensitivity to the choice of bandwidth. Each panel traces the bias-corrected local-linear estimate and its 95% robust confidence interval as the bandwidth varies from 40% to 250% of the MSE-optimal value (marked by the vertical red line). The estimates are stable across the full range for the home-visit outcomes (memory, maternal stress, and maternal employment). For child care centers the estimates are noisier, especially at the smallest bandwidths, reflecting the much smaller sample; but they show no systematic trend that would overturn the estimates at the MSE-optimal bandwidth. This supports the local-linear specification as a reasonable summary of the data near the cutoff.

7 Conclusions

The evidence on the effects of early childhood interventions in the context of developing countries is thin. In this paper we provide evidence on the impact of child care centers and home visits on a broad range of outcomes: children's cognitive and motor development, children's health, parenting styles, mothers' labor supply and income and mothers' psychological well-being. Home visits and child care centers are both evaluated against no intervention through a regression discontinuity design, and are compared directly through the difference in their covariate-adjusted RD estimates.

The point estimates indicate that home visits improve children's development and reduce mothers' labor supply and stress, while child care centers raise mothers' labor supply with smaller and less precise effects on children. This is consistent with a trade-off between child development and maternal labor-market activity. Because the forcing variable takes only 34 distinct proposal-level values, we also apply inference methods designed for a discrete running variable. The effect of home visits on children's overall (Nelson-Ortiz) development is robust to all methods: the honest confidence interval excludes zero and the effect is significant under local-randomization inference. The effects of home visits on memory, fine motor skills, maternal stress and maternal labor supply are significant under conventional inference and local randomization, but not under the wild cluster bootstrap or honest confidence intervals, so we treat them as suggestive. The effects of child care centers on children vary in sign and do not survive the inference methods for the discrete running variable, and the apparent effects of child care centers on mothers' labor supply and stress are not significant under the local-linear specification. We therefore treat the effect of home visits on overall child development as the main finding. That individual cognitive-test effects are fragile is consistent with the scale-up literature: Andrew et al. (2018) find that gains from a scalable home-stimulation program in Colombia fade out within two years, and Carneiro et al. (2024) find positive but modest effects of a national parenting program in Chile. The cost of a child care center per child is also almost five times as high as the cost of a home visit.

The pattern of point estimates is consistent with a framework in which children's outcomes are determined by the quantity and the quality of time spent with them by the mother and by the early childhood program, and in which the psychological well-being of the mother is determined by her working hours and the outcomes of the child. In this framework, the (suggestive) association of child care centers with less responsive parenting and longer maternal working hours would work against child outcomes and maternal wellbeing, whereas home visits, which are associated with more responsive parenting and shorter working hours, would not. We stress that the child-care side of this interpretation rests on estimates that do not survive our most conservative inference.

This discussion makes explicit that our findings are conditional on the quality of the early childhood interventions included in our design. We emphasize that our analysis looks at the effects of programs that were above the minimum quality standards required to receive funding. Moreover, the home visit programs and child care centers that we evaluate are of the same quality level (measured by the scores that the proposals received in the contest for quality indicators). At the same time, lower quality of child care centers in Ecuador than those operated elsewhere might explain why we do not find the positive effects of child care centers on children's development that studies for Norway and Germany report (Havnes and Mogstad, 2011; Felfe and Lalive, 2012). The absence of clear child-development gains from child care centers is, however, in line with the results reported for Canada (Baker et al., 2008).

The trade-off that we identified poses a difficult choice to policy makers and funding agencies. Children and women in poor families in developing countries are both vulnerable groups. Home visits appear to benefit children, whereas child care centers are associated with greater maternal labor-market participation and income (and, in the point estimates, with more maternal stress), even though these child-care effects do not survive our most conservative inference. We trust that information about these potentially conflicting interests is helpful in formulating and implementing efficient policies.

Declaration of generative AI and AI-assisted technologies in the writing process

During the preparation of this work the authors used Claude (Anthropic) to assist with editing the text, developing the replication code, and checking the consistency of code, tables, and text. The authors reviewed and verified all output and take full responsibility for the content.

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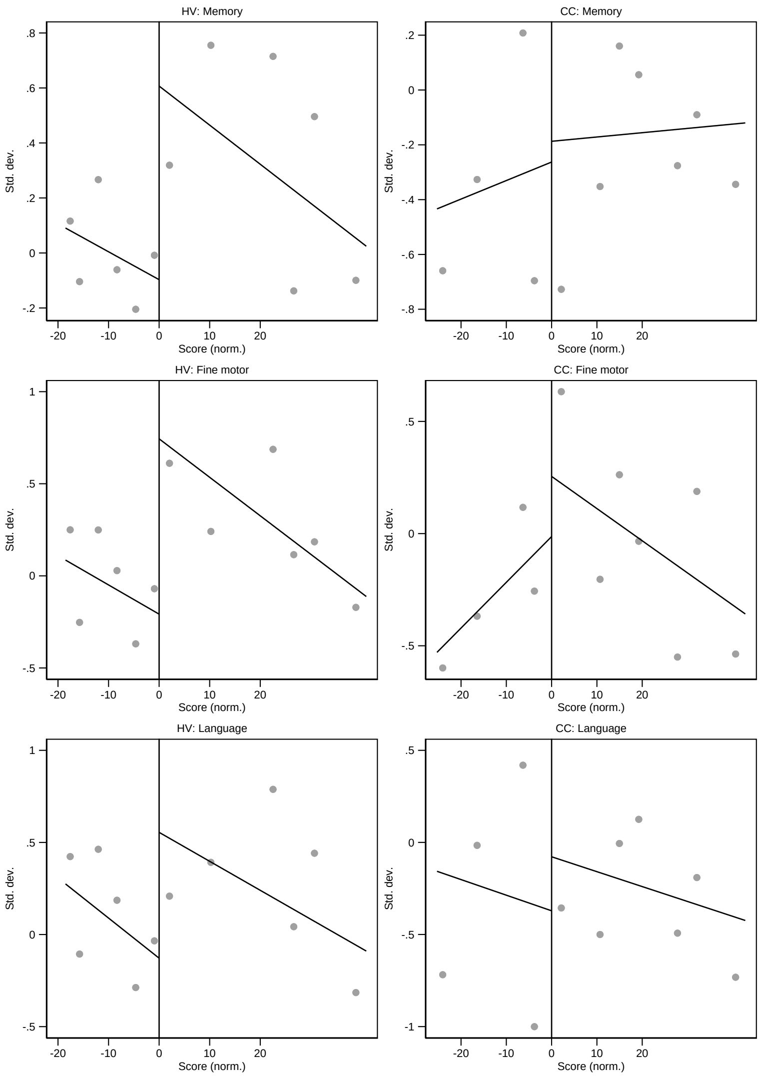
Figure 3. RD plots: cognitive and motor outcomes by type of intervention



Notes: Binned sample means (circles) and local-linear polynomial fits on each side of the cutoff (solid lines), estimated with `rdplot` (Calonico et al., 2014). Running variable normalized to zero at the funding cutoff. Left column: home visits (CNH). Right column: child care centers (CDI/FODI). All outcomes standardized to mean zero, unit standard deviation (children >36 months): Woodcock-Johnson memory test (top), pegboard fine-motor test (middle), TVIP vocabulary test (bottom). Displayed window: ± 20 normalized score units around the cutoff; local-linear fit uses the full data range.

Table 9. Effects of home visits on cognitive, motor and health outcomes

	(1) LL, MSE	(2) LL, MSE+X	(3) LL, CER+X
A. Cognitive (children >36 months)			
Memory	0.682***	0.673***	0.688***
	(0.191)	(0.194)	(0.189)
Language	0.745**	0.631***	0.588***
	(0.354)	(0.189)	(0.168)
Fine motor	0.838***	0.898***	0.929***
	(0.152)	(0.136)	(0.125)
B. Development (children ≤60 months)			
Gross motor	0.151	0.189	0.164
	(0.177)	(0.146)	(0.146)
Fine motor	0.238***	0.116***	0.164***
	(0.024)	(0.028)	(0.026)
Language	0.043	-0.024	0.035
	(0.079)	(0.042)	(0.032)
Social/personal	0.231	0.222*	0.218*
	(0.199)	(0.133)	(0.127)
Total	0.436***	0.300***	0.400***
	(0.088)	(0.086)	(0.077)
C. Health			
Anemia	-0.096	-0.078*	-0.101***
	(0.076)	(0.042)	(0.036)
Underweight (WAZ<-2)	0.072**	0.065**	0.065**
	(0.029)	(0.027)	(0.027)
Below height (HAZ<-2)	0.130	0.140**	0.146***
	(0.108)	(0.057)	(0.055)

Notes: Each cell reports the bias-corrected RD point estimate (top) with the robust standard error in parentheses (bottom), obtained with `rdrobust` (Calonico et al., 2014). (1) local linear (p=1), MSE-optimal bandwidth, no covariates; (2) local linear, MSE-optimal, with covariate adjustment (Calonico et al., 2019); (3) local linear, CER-optimal bandwidth, with covariates. Local quadratic specifications are not feasible because bandwidth selection fails with the discrete running variable (34 mass points); see Kolesár and Rothe (2018). All specifications use a triangular kernel and standard errors clustered on project × parish (98 clusters). Covariates: child sex, number of children under five, urban, welfare benefit, mother's age quadratic, mother's and household head's schooling, mother's vocabulary score, two parenting-practice dummies, ethnicity, household composition, and mother's literacy. Standardized outcomes are in standard-deviation units. Bandwidths (h) and effective sample sizes (N) vary by outcome and specification. $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 10. Effects of child care centers on cognitive, motor and health outcomes

	(1) LL, MSE	(2) LL, MSE+X	(3) LL, CER+X
A. Cognitive (children >36 months)			
Memory	0.577***	-0.176	0.717***
	(0.210)	(0.173)	(0.093)
Language	1.531***	0.321	1.568***
	(0.403)	(0.278)	(0.224)
Fine motor	1.352***	2.257***	2.266***
	(0.135)	(0.159)	(0.151)
B. Development (children ≤60 months)			
Gross motor	-0.163***	-0.074**	-0.011
	(0.027)	(0.035)	(0.039)
Fine motor	0.179*	-0.003	-0.094**
	(0.104)	(0.046)	(0.039)
Language	0.151**	0.061	0.103*
	(0.068)	(0.062)	(0.062)
Social/personal	0.223*	0.073	0.122
	(0.133)	(0.061)	(0.080)
Total	0.283***	-0.019	0.169***
	(0.088)	(0.054)	(0.049)
C. Health			
Anemia	0.068	0.124**	0.110**
	(0.078)	(0.056)	(0.046)
Underweight (WAZ<-2)	0.225***	0.262***	0.290***
	(0.043)	(0.029)	(0.026)
Below height (HAZ<-2)	-0.071	0.384***	0.384***
	(0.159)	(0.070)	(0.092)

Notes: Each cell reports the bias-corrected RD point estimate (top) with the robust standard error in parentheses (bottom), obtained with `rdrobust` (Calonico et al., 2014). (1) local linear (p=1), MSE-optimal bandwidth, no covariates; (2) local linear, MSE-optimal, with covariate adjustment (Calonico et al., 2019); (3) local linear, CER-optimal bandwidth, with covariates. Local quadratic specifications are not feasible because bandwidth selection fails with the discrete running variable (34 mass points); see Kolesár and Rothe (2018). All specifications use a triangular kernel and standard errors clustered on project × parish (98 clusters). Covariates: child sex, number of children under five, urban, welfare benefit, mother's age quadratic, mother's and household head's schooling, mother's vocabulary score, two parenting-practice dummies, ethnicity, household composition, and mother's literacy. Standardized outcomes are in standard-deviation units. Bandwidths (h) and effective sample sizes (N) vary by outcome and specification. $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 11. Effects of home visits on parenting and maternal labor market outcomes

	(1) LL, MSE	(2) LL, MSE+X	(3) LL, CER+X
A. Parenting			
Non-responsive (HOME, std.)	-0.625**	-0.502	-0.531*
	(0.319)	(0.310)	(0.291)
Mother reads to child	-0.031	-0.009	-0.003
	(0.093)	(0.039)	(0.042)
Learning materials at home	-0.122*	-0.076	-0.103
	(0.063)	(0.057)	(0.077)
B. Maternal wellbeing and labor market			
Stress/depression (CES-D, std.)	-0.742***	-0.689***	-0.763***
	(0.143)	(0.116)	(0.130)
Mother works	-0.285***	-0.257***	-0.250***
	(0.107)	(0.061)	(0.059)
Hours worked per month	-10.788***	-8.918***	-8.608***
	(3.398)	(1.817)	(1.712)
Mother's earnings	-11.600	-37.278***	-38.504***
	(15.336)	(8.897)	(8.255)
Household head's earnings	24.723	-6.705	-9.947
	(30.275)	(15.213)	(14.784)

Notes: Each cell reports the bias-corrected RD point estimate (top) with the robust standard error in parentheses (bottom), obtained with `rdrobust` (Calonico et al., 2014). (1) local linear (p=1), MSE-optimal bandwidth, no covariates; (2) local linear, MSE-optimal, with covariate adjustment (Calonico et al., 2019); (3) local linear, CER-optimal bandwidth, with covariates. Local quadratic specifications are not feasible because bandwidth selection fails with the discrete running variable (34 mass points); see Kolesár and Rothe (2018). All specifications use a triangular kernel and standard errors clustered on project $\times$ parish (98 clusters). Covariates: child sex, number of children under five, urban, welfare benefit, mother's age quadratic, mother's and household head's schooling, mother's vocabulary score, two parenting-practice dummies, ethnicity, household composition, and mother's literacy. Standardized outcomes are in standard-deviation units. Bandwidths (h) and effective sample sizes (N) vary by outcome and specification. $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 12. Effects of child care centers on parenting and maternal labor market outcomes

	(1) LL, MSE	(2) LL, MSE+X	(3) LL, CER+X
A. Parenting			
Non-responsive (HOME, std.)	-0.232**	0.062	-0.051
	(0.093)	(0.080)	(0.082)
Mother reads to child	-0.174***	-0.053***	-0.061***
	(0.031)	(0.016)	(0.014)
Learning materials at home	0.415***	0.602***	0.572***
	(0.072)	(0.066)	(0.055)
B. Maternal wellbeing and labor market			
Stress/depression (CES-D, std.)	0.228	0.120	0.050
	(0.144)	(0.081)	(0.040)
Mother works	-0.031	0.102	0.081
	(0.081)	(0.069)	(0.074)
Hours worked per month	5.000*	4.823	5.982
	(2.782)	(3.138)	(3.663)
Mother's earnings	30.330***	10.652	12.858*
	(3.963)	(6.891)	(7.636)
Household head's earnings	168.041***	108.277***	114.855***
	(6.446)	(7.736)	(7.072)

Notes: Each cell reports the bias-corrected RD point estimate (top) with the robust standard error in parentheses (bottom), obtained with `rdrobust` (Calonico et al., 2014). (1) local linear (p=1), MSE-optimal bandwidth, no covariates; (2) local linear, MSE-optimal, with covariate adjustment (Calonico et al., 2019); (3) local linear, CER-optimal bandwidth, with covariates. Local quadratic specifications are not feasible because bandwidth selection fails with the discrete running variable (34 mass points); see Kolesár and Rothe (2018). All specifications use a triangular kernel and standard errors clustered on project $\times$ parish (98 clusters). Covariates: child sex, number of children under five, urban, welfare benefit, mother's age quadratic, mother's and household head's schooling, mother's vocabulary score, two parenting-practice dummies, ethnicity, household composition, and mother's literacy. Standardized outcomes are in standard-deviation units. Bandwidths (h) and effective sample sizes (N) vary by outcome and specification. $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 13. Home visits minus child care centers: covariate-adjusted RD estimates

	HV−CC	p(diff= 0)
A. Cognitive (children >36 months)		
Memory (std.)	0.849***	0.001
	(0.260)	
Language (std.)	0.311	0.356
	(0.336)	
Fine motor (std.)	-1.359***	0.000
	(0.209)	
B. Development (children ≤60 months)		
Gross motor (std.)	0.263*	0.079
	(0.150)	
Fine motor (std.)	0.119**	0.027
	(0.054)	
Language (std.)	-0.085	0.254
	(0.075)	
Social/personal (std.)	0.150	0.307
	(0.146)	
Total (std.)	0.319***	0.002
	(0.101)	
C. Health		
Anemia	-0.202***	0.004
	(0.070)	
Underweight (WAZ<−2)	-0.197***	0.000
	(0.039)	
Below height (HAZ<−2)	-0.245***	0.007
	(0.090)	
D. Parenting		
Non-responsive (HOME, std.)	-0.564*	0.078
	(0.321)	
Mother reads to child	0.045	0.292
	(0.042)	
Learning materials at home	-0.678***	0.000
	(0.088)	
E. Maternal wellbeing and labor market		
Stress/depression (CES-D, std.)	-0.809***	0.000
	(0.141)	
Mother works	-0.360***	0.000
	(0.092)	
Hours worked per month	-13.741***	0.000
	(3.626)	
Mother's earnings	-47.930***	0.000
	(11.254)	
Household head's earnings	-114.981***	0.000
	(17.067)	

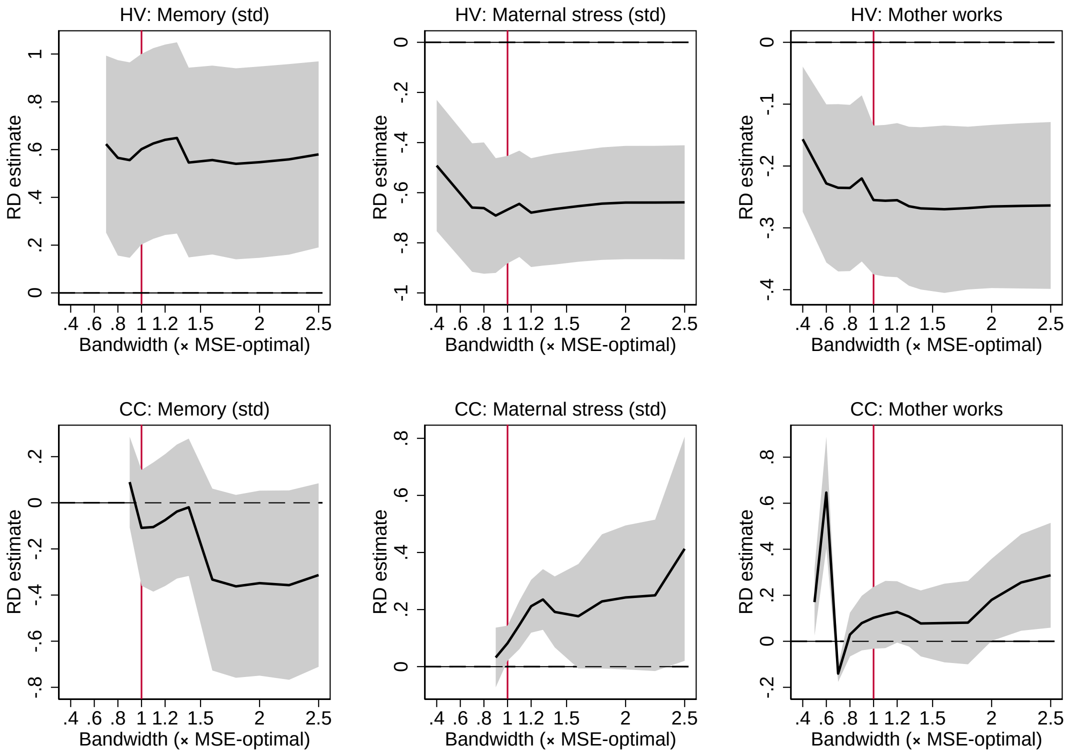
Notes: Difference in covariate-adjusted RD estimates (HV minus CC), specification (2) of Tables 9–12. Standard error of the difference in parentheses, assuming independence of the two samples. $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 14. Robustness of inference to the discrete running variable

Outcome	RD est. (LL, $+X$)	p (robust, by cluster)	p (wild bootstrap)	Honest CI (Armstrong-Kolesár)	Local-rand. p
Panel A. Home visits					
Woodcock memory	0.67	0.001	0.149	[-0.50, 1.86]	0.029
Fine motor (pegboard)	0.90	0.000	0.131	[-1.08, 2.46]	0.000
TVIP (vocabulary)	0.63	0.001	0.179	[-1.16, 2.64]	0.119
Nelson-Ortiz total dev.	0.30	0.000	0.358	[0.16, 0.68]	0.002
Anemia	-0.08	0.066	0.194	[-0.44, 0.29]	0.569
Mother stress (CES-D)	-0.69	0.000	0.065	[-2.10, 0.81]	0.000
Mother works	-0.26	0.000	0.231	[-0.35, 0.17]	0.000
Mother hours worked	-8.92	0.000	0.220	[-21.24, 6.18]	0.000
Panel B. Child care					
Woodcock memory	-0.18	0.310	0.557	[-4.00, 5.00]	0.909
Fine motor (pegboard)	2.26	0.000	0.720	[-2.15, 4.60]	0.000
TVIP (vocabulary)	0.32	0.249	0.384	[-9.98, 12.92]	0.000
Nelson-Ortiz total dev.	-0.02	0.728	0.452	[–, –]	0.825
Anemia	0.12	0.027	0.528	[–, –]	1.000
Mother stress (CES-D)	0.12	0.139	0.238	[-1.68, 2.08]	0.753
Mother works	0.10	0.138	0.110	[-3.06, 2.95]	1.000
Mother hours worked	4.82	0.124	0.134	[-102.17, 105.83]	0.397

Notes: Each row is the local effect of the program (relative to no program) at the funding cutoff; continuous outcomes in comparison-group SD units. Column 2 reproduces the main covariate-adjusted local-linear estimate from column (2) of Tables 9–12 (`rdrobust`, MSE-optimal bandwidth, triangular kernel); column 3 its robust bias-corrected p-value, clustered on project $\times$ parish. Column 4 is the wild-cluster-bootstrap p-value for the reduced-form discontinuity over the 34 proposals (Cameron et al., 2008); column 5 the bias-aware honest 95% CI for a discrete running variable (Kolesár and Rothe, 2018); column 6 the finite-sample randomization-inference p-value in a ± 5 window (Cattaneo et al., 2015).

Figure 4. Bandwidth sensitivity of main estimates



Notes: Each panel plots the bias-corrected RD estimate (solid line) with 95% robust confidence interval (shaded band) as the bandwidth varies from 0.4× to 2.5× the MSE-optimal value. The vertical red line marks the MSE-optimal bandwidth; the dashed horizontal line marks zero. Estimates use local linear with covariate adjustment (`rdrobust`, triangular kernel, clustered on project × parish). Rows: home visits (top) and child care centers (bottom). Columns: memory test (left), maternal stress (center), mother works (right).