The Impact of Social Safety Nets: Evidence from a Randomized Evaluation of the Role of Cash, Information, and Home Visits*

Richard Akresh
University of Illinois at Urbana-Champaign

Damien de Walque
The World Bank

Harounan Kazianga
Oklahoma State University

Abigail Stocker
University of Illinois at Urbana-Champaign

July 15, 2024

PRELIMINARY. PLEASE DO NOT CITE.

Abstract

Cash transfers have become a critical part of developing country governments' social safety nets. Recently, home visits programs are being implemented to help households overcome barriers to improving children's development. Working with the Burkina Faso government, we conduct a randomized experiment in 225 rural villages to assess the impact of an integrated social safety net over the child's life cycle. Villages were randomized to a control group or one of the following treatments: cash transfers only; cash transfers plus government-run information meetings focused on children's health and psycho-social development; or cash transfers, information, and home visits reinforcing the information meetings. Households receiving the full intervention (cash, information, and home visits) have fewer pregnancies, more medically assisted childbirths, enhanced health behaviors, improved child anthropometrics, and better educational outcomes. Additionally, home visits are critical for improving early childhood development, while cash transfers, with or without information meetings, do not improve these outcomes.

JEL Codes: J13, I38, O12, I12
Keywords: Early Childhood Development, Home Visits, Cash Transfers, Africa

---

* The evaluation was funded by the World Bank Strategic Impact Evaluation Fund trust fund (SIEF). The research protocol received IRB clearance (IRB Protocol #14156) from the Institutional Review Board at Innovations for Poverty Action (IPA) and from the Burkina Faso National IRB ("Comité d'Ethique"). The project was registered with the AEA RCT Registry (RCT ID: AEARCTR-0002897). The findings, interpretations, and conclusions expressed in this paper are entirely those of the authors. They do not necessarily represent the views of the World Bank, its Executive Directors, or the countries they represent.

Contact Information: Richard Akresh, University of Illinois at Urbana-Champaign, Department of Economics. Email: akresh@illinois.edu; Damien de Walque, The World Bank, Development Research Group. Email: ddewalque@worldbank.org; Harounan Kazianga, Oklahoma State University, Department of Economics. Email: harounan.kazianga@okstate.edu; Abigail Stocker, University of Illinois at Urbana-Champaign, Department of Economics. Email: stocker3@illinois.edu

1. Introduction

There are more than 240 million children under-five in developing countries that do not reach their early childhood development potential (Grantham-McGregor et al., 2007). These children are less likely to become healthy, productive, and socio-economically successful adults. A growing body of evidence from developed and developing countries shows that cognitive, non-cognitive, and physical health developments of young children are critical factors for health and socioeconomic well-being in adulthood (Heckman, 2006, Almond and Currie, 2011). These returns are both to individuals—who benefit from increased schooling, labor force participation, wages, and material and socio-emotional well-being—as well as to societies that benefit from higher productivity leading to economic growth, lower inequality, and a more participatory citizenry (Heckman and Masterov, 2007; Walker et al., 2007; Alderman, 2011; Walker et al., 2011; Gertler, Heckman et al., 2014).

The purpose of this paper is to evaluate the impact of an integrated early childhood development social safety net intervention in rural Burkina Faso that combines cash transfers, village-level information meetings focused on children's health and psycho-social development, and home visits reinforcing the information meetings. We conduct a randomized control trial in 225 rural villages in Burkina Faso where villages were randomly assigned to a control group or the following treatments: cash transfers only; cash transfers plus government-run information meetings focused on children's health and psycho-social development; or cash transfers, information, and home visits reinforcing the information meetings. Burkina Faso is a low-income environment with relatively weak administrative capacity, but where rigorous impact evaluations of social protection programs, such as cash transfers or food transfers, have already been conducted and have shown impact (Akresh, de Walque, and Kazianga, 2013, 2024; Kazianga, de

Walque, and Alderman, 2014). Households in the treatment arms receive a quarterly cash transfer of approximately $60 USD. Monthly village-level information meetings were organized by the government for groups of around 25 mothers, and the project facilitators did regularly scheduled home visits to monitor progress and follow-up on the physical and cognitive development of the children. The cash intervention lasted for 30 months, while the information and home visits lasted for 18 months.[1]

For over 25 years, governments in developing countries have used cash transfers delivered to poor households to help children reach their full potential (see Fiszbien and Schady, 2009 and Bouguen, Huang, Kremer, and Miguel, 2019 for reviews of this literature).[2] While lack of income is often cited by these governments as a key constraint facing poor households, lack of information may also be critical (see Bhutta et al., 2008; Bhutta et al., 2013; and Ruel et al., 2013 for a review of information campaigns related to maternal and child nutrition).[3] Despite both

[1] Cash transfers started immediately after the baseline survey was completed, while the information meetings and home visits were delayed for approximately 12 months while the Burkina Faso government trained project facilitators to implement those components.

[2] Cash transfers are often given to poor households conditional on the household taking measures to increase their children's human capital (e.g., enrolling their children in school and maintaining their attendance, taking them for regular preventive health care visits). Alternatively, social safety net programs can provide unconditional cash transfers where cash is given to poor households without any conditions. There is credible evidence that both types of transfer schemes (conditional and unconditional) substantially improve child education and health outcomes. For evidence of the impact of conditional cash transfers on education: in Mexico see Schultz 2004; Behrman, Sengupta and Todd 2005; de Janvry et al. 2006; Attanasio, Meghir and Santiago 2011; in Colombia see Attanasio et al. 2010; in Nicaragua see Maluccio and Flores 2005; Macours, Schady and Vakis 2008; in Honduras see Glewwe and Olinto 2004; in Brazil see Glewwe and Kassouf 2012; in Cambodia see Filmer and Schady 2011; in Indonesia see Cahyadi et al. 2020; in Burkina Faso see Akresh, de Walque, and Kazianga, 2013; for evidence of the impact of unconditional cash transfers on education: in Ecuador see Schady and Araujo 2008; in South Africa see Case, Hosegood, and Lund 2005; Edmonds 2006; in Burkina Faso see Akresh, de Walque, and Kazianga, 2024; and in the United States for the long-term impacts see Aizer et al. 2016. For evidence of the impact of conditional cash transfers on health: in Colombia see Attanasio et al. 2005; in Ecuador see Paxson and Schady 2008; in Nicaragua see Maluccio and Flores 2005; Macours, Schady, and Vakis 2008; for evidence of the impact of unconditional cash transfers on health: in South Africa see Case and Deaton 1998; Case, Hosegood, and Lund 2005; and Duflo 2003.

[3] Recent research in Nepal evaluates a randomized control trial that provides information on best practices regarding childcare and cash transfers, finding significant impacts of the information plus cash treatment arm (Levere, Acharya, and Bharadwaj, 2024). Another project in northern Nigeria randomly provides cash transfers to mothers beginning during pregnancy and information related to pregnancy and infant feeding (Carneiro et al., 2021). The cash transfers continue until the child turns 24 months old. They find large and significant improvements in anthropometrics and health outcomes, and both information and cash are critical.

income and information being potentially important for child development and many countries implementing these types of interventions, millions of children remain at risk of not accumulating sufficient human capital.

More recently, governments have begun to also focus on the quality of parenting behaviors as these are thought to be critical for early childhood development (Cunha and Heckman, 2008).[4] Home visits focused on changing parental behaviors have attracted significant interest for improving children's development as these visits can overcome household or individual barriers to change.[5] Seminal work in Jamaica led to large and sustained impacts in adult outcomes decades after the home visits took place (Grantham-McGregor et al., 1991; Gertler et al., 2014). However, most evaluations of home visit interventions find impacts that are greatly reduced (Yousafzai et al., 2016) or completely fade out once the treatments stop (Andrew et al., 2018).[6] Despite the efforts to produce evidence from rigorous evaluations, there is one area where the evidence remains unclear: how to deliver integrated early childhood development that removes both cash and knowledge constraints and provides a mechanism to overcome household barriers to change.

This paper makes three main contributions to the literature. First, this is the first paper to compare in the same setting, interventions that provide only cash, cash plus information, or cash plus information plus home visits. Existing evidence shows that each component (cash, information, and home visits) could lead to improved early childhood development, but it is not

[4] In developed country settings, this early childhood development focus has typically been on programs that work directly with children in daycare centers and does not involve changing parent's behaviors such as the HighScope Perry Preschool Program (Heckman et al., 2010).

[5] Group-based parenting interventions are an alternative approach to improving early childhood development by providing information to parents but without the individualized attention from a one-on-one home visit. Evidence from Rwanda (Justino et al., 2023) and Chile (Carneiro et al., 2024) have found significant improvements in children's development.

[6] Recent home visit interventions building on the Jamaica Study yield smaller, but still positive, short-term impacts at endline (Attanasio et al., 2014; Grantham-McGregor, 2020; Araujo et al., 2021).

clear where the behavioral constraints lie and whether removing all of these barriers leads to larger impacts. Given that many governments in developing countries are considering adopting home visits as part of their social safety nets, it is also important to understand why home visits work and whether they are necessary or if simply providing cash or knowledge is sufficient.

Second, due to insecurity in parts of Burkina Faso, the endline survey was delayed by approximately 15 months after the treatment intervention ended. Therefore, all of our program impacts should be interpreted as the medium-term effects once the program ended. There are a limited number of evaluations that can look at impacts beyond the program ending and given that the previously discussed literature found early childhood development impacts disappeared or faded out, it is therefore promising that we still are able to observe positive impacts of the full treatment intervention 15 months after the program ended.

Third, while it was not the intention of the Burkina Faso government (or us as researchers) to be evaluating this social safety net in an environment with insecurity and conflict, those factors did present themselves and allow us to contribute to a very limited literature looking at the role of social safety nets in conflict settings. Our setting also allows us to examine whether the interventions, or specific components, had different effects on more vulnerable households.

Households receiving the full intervention (cash, information, and home visits) show improvements across a wide range of child outcomes.[7] We find that mothers that receive the full intervention have fewer pregnancies, and they are older at each birth. Importantly, their most recent birth is also more likely to have been a medically assisted childbirth. We find evidence

[7] To measure the impact of the intervention, we conduct a baseline survey prior to the rollout of the treatment, and we were scheduled to conduct an endline survey at the conclusion of the intervention. However, security and conflict issues in Burkina Faso forced us to delay the endline survey until approximately 15 months after the treatment ended, so all results should be interpreted as medium-term impacts after the intervention had ended.

that mothers adopt healthier behaviors, including being more likely to wash their hands after using the toilet, and children have improved anthropometrics, in particular the medium-term measure of arm circumference. Older children in households receiving the full intervention have improved educational outcomes, including being more likely to be enrolled in school, completing more years of schooling, ever attending school, and being educationally on track for their age.

We also find suggestive evidence that the cumulative addition of home visits to the treatment program may increase early childhood development outcomes. We conduct three distinct tests of early childhood development: the Denver Prescreening Developmental Questionnaire, which has subsections in Language, Gross Motor, Fine Motor, and Personal Social; the Head, Toes, Knees, Shoulders (HTKS) assessment, which measures cognitive self-regulation; and the Strengths and Difficulties test, which comprises 25 items across five different scales (emotional problems scale, conduct problems scale, hyperactivity scale, peer problems scale, and prosocial scale). Cash transfers by themselves or cash transfers plus information meetings do not seem to improve children's outcomes on these three tests, suggesting that the home visits are a critical component of the intervention package.

Additionally, we examine the heterogeneity of impacts across poor and non-poor households (as defined by expenditures) and by level of exposure to conflict and insecurity (as defined by being close to conflict events), and we find suggestive evidence that more vulnerable households may have benefitted more from the full intervention. This suggests that targeting package interventions with cash, information, and home visits to more vulnerable households may result in higher impacts, leading the interventions to be more cost effective.

The remainder of the paper is organized as follows. Section 2 describes the context of our experiment and the design of the cash transfer, information, and home visit program. In Section

3, we discuss our empirical identification strategy. Section 4 presents the main results for child welfare outcomes. Section 5 concludes.

2. Context and Experimental Design

2.1 Context

Burkina Faso offers an important setting for exploring the effects of cash transfers and early childhood development programs on children's education, health outcomes, and rural fertility. Even by African standards, child education and health in Burkina Faso are poor. The United Nations Human Development Index ranks Burkina Faso 185 out of 193 countries (United Nations Development Program, 2024). In 2010, the net attendance ratio for primary school in rural Burkina Faso was 44.4 (45.5 for boys and 43.1 for girls) and the gross attendance ratio was 64.9 (66.2 for boys and 63.5 for girls) (Institut National de la Statistique et de la Démographie and ICF International, 2012).[8]

Formal education in Burkina Faso consists of six years of primary school, four years of lower secondary school, three years of upper secondary school, then tertiary education. A national law officially makes school mandatory until age 16. However, this law is not enforced, because the government lacks the resources to provide education to all school-aged children. In principle, children can attend public primary schools free of charge. However, parents still must pay for various required school fees and school supplies. These expenses must be paid in cash and in a timely manner, with most being paid at the start of the academic year, thus presenting a significant constraint for cash-strapped households. Cash transfers could reduce this constraint in addition to compensating for the opportunity cost of child time.

[8] The primary school net attendance ratio is the percentage of children attending primary school who are of the official school age. The primary school gross attendance ratio is the number of primary school students, irrespective of age, as a percentage of the official primary school age population. If there is a significant number of underage or overage students in primary school, the gross attendance ratio will be higher than the net attendance ratio.

Health outcomes among children in Burkina Faso are similarly poor. In 2020, approximately 35 percent of children in Burkina Faso under age 5 were considered stunted and 18 percent were considered underweight (Ministère de la Santé, 2020).[9] In 2021, only 65 percent of children had received all the recommended vaccinations for their age group, up from 50 percent in 2019-2020 (Cooper et al., 2023).

Fertility rates in Burkina Faso are high and are coupled with poor pregnancy-related outcomes. The average fertility rate in Burkina Faso is 5 children per woman. While 94% of women in rural areas have their birth assisted by a medical professional, very few women make at least 4 antenatal care visits, only 31% in rural areas overall and 24% for the poorest households (United Nations Children's Fund, 2018). Given the status of education, health, and fertility in Burkina Faso, there is scope for interventions to improve each of these outcomes.

2.2 Experimental Design: Burkina Faso Social Safety Net Pilot Project Overview

The cash transfer, information, and home visit interventions that we study were conducted in 225 villages in the East and Center-East regions in Burkina Faso, located 250-350 kilometers from the capital, Ouagadougou.[10] Households in these regions are predominantly subsistence farmers growing millet, sorghum, and peanuts. The 225 villages in the East and Center-East regions were randomly allocated into five groups, stratified at the commune level. These groups are shown in Figure 1, and the groups consisted of: (i) households receiving cash transfers; (ii) households receiving cash transfers and information meetings with 25-30 mothers; (iii) households receiving cash transfers, information meetings with 25-30 mothers, and home visits focused on nutrition

[9] Stunting is defined as children being below minus two standard deviations from the median height-for-age of a reference population. Underweight is defined as children being below minus two standard deviations the median weight-for-age of a reference population.

[10] We restricted the village sampling frame to villages that had at least 90 households to ensure that there would be at least 21 beneficiary households to survey in each village and to villages that had less than 400 households to avoid situations where the households were spread over areas that were too large to survey.

and health; (iv) households receiving cash transfers, information meetings with 25-30 mothers, and home visits focused on both health issues as well as improving parenting and early childhood development; and (v) a control group.[11] [12] Randomization took place at the village level, there were 45 villages in each treatment arm at the baseline, and only households with children age 5 and under or women who were pregnant at the time of the baseline were eligible to receive the intervention.[13]

In each sampled village, an initial census was conducted to enumerate all households eligible for the program.[14] In this census, the Proxy Means Test (PMT) targeting method was used and a PMT questionnaire was administered to all households of the village. The questionnaire included questions about household composition, consumption, and assets. The data was used to calculate a PMT score and to rank households in terms of poverty, with the poorest households (PMT score < -0.60) eligible for the program.[15] In each village, 21 households and seven replacement households were randomly selected from the eligible population.

All households in the treatment group received monthly cash transfers of $20 delivered each quarter. Each of these households had at least one child aged five or under or a pregnant

[11] For most of our analysis, we combine treatment arms (iii) and (iv).
[12] When the intervention was carried out in practice, the government modified the treatment protocol and provided home visits to extremely vulnerable households who were in treatment arm (ii) receiving cash and information meetings.
[13] However, it is important to note that some of these households do not have children under age 5 at the endline, as their children are now older, and they may not have had additional children.
[14] In treatment villages, the census was conducted by Filets Sociaux, the implementing partner, with support from Innovations for Poverty Action (IPA). In the control group, IPA conducted the same census to determine households eligible for the program.
[15] In the intervention villages, the PMT method was completed by a community validation method: a village meeting was organized, and the assembly could amend the list of eligible households. They had the possibility to report that a household ranked as poor was not poor enough to be in the program. They could also say that a household ranked as not poor was poor enough to be in the program. This method ensures acceptability of the selection among villagers but might undermine the comparability between household finally selected for the intervention and households in the control group, as the latter only had the PMT survey and not the community validation.

woman.[16] The cash transfers were delivered electronically, so households were also provided with a cell phone which they could use to receive the cash transfers. The treatment arms that provided information to households, treatment arms (ii), (iii), and (iv), did so in the form of meetings with groups of 25-30 mothers. These meetings were designed to occur on a monthly basis. Households in treatment arms (iii) and (iv) also received home visits, with households in treatment arm (iii) receiving home visits to reinforce the health information and treatment arm (iv) receiving home visits to reinforce both the health information and parenting behaviors. A list of the topics covered in the information sessions and home visits can be found in Appendix Table A1. The health topics broadly cover food and nutrition for the youngest children, diet during pregnancy, prevention of malaria and anemia, management of child illnesses, personal hygiene, and family planning. The child development topics include giving affection to children, protecting them from dangerous objects, providing a recreational environment to them, and teaching them to share with others.

The baseline survey was conducted between January and May 2018, the cash intervention started in June 2018, and the information and home visits interventions began in June 2019. All of the interventions ended in December 2020. The COVID-19 pandemic, which occurred during the middle of the intervention, impacted the implementation of information sessions and home visits. The endline survey was conducted in early 2022.

Between baseline and endline data collection, the security situation in Burkina Faso deteriorated, particularly in the East region. Conflict spread into the regions targeted by the program, and this impacted the implementation of the endline survey, since many of the villages

[16] To minimize child fostering in response to the program introduction and reduce any potential sample attrition (see Akresh (2009) for evidence on the relationship between income shocks and child fostering), eligibility for cash transfers was based only on the children who were present in the household at the time of the baseline survey.

that were included in the program were deemed unsafe for surveyors to travel to. As a result, we were unable to conduct the endline survey for most of the villages in the East region and some of the villages in the Center-East region. Out of 225 villages at baseline, we were able to return to 74 villages at endline.

Panel A of Table 1 shows baseline summary statistics for the 74 villages in our endline sample. In the 74 villages in our analysis, there are 2566 individuals living in each village on average. Ninety-one percent of villages have a primary school, while only 20 percent of villages have a secondary school. The average distance to the nearest health center is 5 km, and the average distance to the nearest paved road is 22 km. On average, roads are impassable for 3.5 months out of the year.

Panel B of Table 1 shows the baseline summary statistics for the 1513 households in the 74 villages in our endline sample. Eighty percent of the household heads in our sample are male with an average age of nearly 46, and only 9 percent of household heads ever attended school. On average, there are 9.25 members in each household, of whom five are biological children of the household head under age 15.

3. Empirical Strategy

3.1 Empirical Identification Strategy

The key question we address is whether cash, information, and home visits improve children's human capital and development in the recipient households. The randomized experimental design provides a strong identification strategy that allows us to attribute any differences in outcomes between the treatment and control groups to the impact of the program. To evaluate the intervention, we use data from the endline survey and compare across the treatment and control arms, relying on our randomization into treatment to produce a causal estimate. We have a total

of 74 villages and 1513 households in our endline sample, and we pool treatment arms (iii) and (iv), which both receive home visits as well as information and cash. We estimate the following regression:

$$Y_{ihvc} = \alpha + \beta_1 T1_{hvc} + \beta_2 T2_{hvc} + \beta_3 T3T4_{hvc} + \gamma_c + \delta \boldsymbol{X}_{ihvc} + \epsilon_{ihvc} \quad (1)$$

where $T1_{hvc}$ is an indicator for being randomized into treatment arm (i), $T2_{hvc}$ is an indicator for being in treatment arm (ii), $T3T4_{hvc}$ is an indicator for being in treatment arm (iii) or (iv), γ_c is a strata fixed effect (at the commune level), $\boldsymbol{X_{ihv}}$ is a vector of household, village, and individual-level controls, including household head age, child age and gender, and other LASSO-selected controls (specific to each regression and described in more detail below). Robust standard errors are used in all regressions and are clustered at the unit of randomization, which is the village. In this specification, treatment arms (iii) and (iv) are pooled because we do not have a way to verify whether the information provided in the two types of home visits was substantially different.

Regressions for some of the sets of outcomes are run at the individual level (for child-specific outcomes), and others are run at the household level (for outcomes that likely affect all individuals in the household). For the individual-level regressions, we include child age and gender as control variables. For the household-level regressions of child outcomes, we include average child age and gender ratio in the household as control variables. For all regressions, we control for the age of the household head. We use a LASSO procedure to select covariates (at the household and village level) for our main specification, which allows us to control for potential differences in baseline values of these covariates across treatment arms. The LASSO selects the covariates that are important for improving precision in each regression. For the LASSO, we include a set of 81 potential covariates at the household or village level. λ is selected using cross-

validation, and the LASSO procedure provides a separate set of optimal controls for each outcome. For robustness, we also run the regressions without controls.

Because many of the survey questions are only asked for children within a limited age range, we do not control for baseline values of the outcome variable in our main specification. Controlling for baseline values would require us to drop a large fraction of the children in our sample, since many of the children surveyed at endline were not yet born at the time of the baseline survey, and many of the children in the baseline survey had aged out of the respective survey sections by the time of the endline survey. For robustness in the sections where the baseline and endline samples have sufficient overlap, we use an ANCOVA specification and control for baseline values of the outcome variables.

For results related to early childhood development, we also analyze the cumulative effects of the stacked interventions, also using only the endline data. This allows us to see the additional effect of information on top of receiving cash and the additional effect of home visits on top of receiving both information and cash. This requires the assumption that the effects of the various components of the intervention are additive. The regression equation for this specification is as follows:

$$Y_{ihvc} = \alpha + \beta_1 Cash_{hvc} + \beta_2 Info_{hvc} + \beta_3 HomeVisits_{hvc} + \gamma_c + \delta \boldsymbol{X}_{ihvc} + \epsilon_{ihvc} \qquad (2)$$

where $Cash_{hvc}$ is an indicator for receiving cash (this occurs in all four treatment arms), $Info_{hvc}$ is an indicator for receiving information (in treatment arms (ii), (iii), and (iv)), $HomeVisits_{hvc}$ is an indicator for receiving home visits (in treatment arms (iii) and (iv)), γ_c is a strata fixed effect (at the commune level), and $\boldsymbol{X_{ihv}}$ is a vector of household, village, and individual-level controls, including household head age, child age and gender, and other LASSO-selected controls (specific to each regression, as described above). Robust standard errors are clustered at the

village level. Note that, because all treatment arms received cash, for example, we cannot identify the effects of information alone or home visits alone. Thus, β_2 should be interpreted as the additional effect of receiving information, compared to and conditional on receiving cash, and β_3 should be interpreted as the additional effect of home visits, compared to and conditional on receiving both cash and information.

3.2. Sample and Baseline Balance

The baseline household survey was conducted in early 2018, and the endline household survey was conducted in early 2022.[17] Both baseline and endline surveys measure household demographics and characteristics, child outcomes related to early childhood development and health, parenting behaviors, and fertility-related outcomes.

For mothers with a child under five years old in the household, we have information on birth histories and total number of pregnancies. These women are asked their age at the time of each of their births (up to ten births) as well as more detailed information about their most recent birth, including whether a medical professional assisted delivery.

Mothers of children ages 0 to 48 months are also asked about prenatal and child health outcomes. For the pregnancies associated with each of those children, mothers are asked about the number of prenatal visits, whether they took vitamin A and iron, and whether the child received any doses of vitamin A in the past 12 months. Additionally, mothers are asked which vaccines their children have received, allowing us to determine whether each child is fully vaccinated based on the recommendations for their age group. Finally, mothers of children below

[17] While the endline survey was originally scheduled to be conducted earlier, it was delayed due to the outbreak of COVID-19 and conflict in the study area.

age 5 are also asked about their handwashing habits, specifically whether they wash their hands after using the toilet.

For children below age 5, we have anthropometrics measures for height, weight, and arm circumference. We use these three measures to calculate z-scores for each of the three outcomes, relative to the WHO reference population.

For older children ages 6-15, we have information on whether they are enrolled in school, how many years of schooling they have completed, what grade they are in, and whether they are able to read and write. We also calculate grade progression as actual grade divided by expected grade, where expected grade is calculated based on the child's age and based on the assumption that they start school at age seven.

We have several child-specific development outcomes, including Denver test scores for children ages 0 up to 72 months, HTKS test scores for children ages 36 up to 72 months, and Strengths and Difficulties test scores for children ages 24 up to 72 months.[18] The Denver test consists of a series of items related to childhood development, and the total possible score is given by the number of items that 90 percent of children of a given age group should be able to complete. The child's actual score is a percentage, calculated by dividing the actual number of items that the child completes by the expected number of items for their age group. This percentage score allows for comparisons across age groups, since each age group is expected to complete a different number of items.

The HTKS (Head, Toes, Knees, Shoulders) test is one in which children play a game that involves touching their heads, toes, knees, and shoulders in various sequences following the

[18] Note that these ECD modules were pretested by IPA to ensure that they were culturally appropriate and correctly translated for the local environment.

instructions of the enumerator. Children received points for each time they followed the correct sequence.

The Strengths and Difficulties test consists of a list of statements that may be either positive if true about the child (strengths) or negative if true about the child (difficulties). These lists of statements several categories, and parents answer whether or not each statement is true for the child. For most sections, a lower score is more desirable, so that answering "true" to the difficulties statements or "false" to the strengths statements adds to the child's total points. The exception is the prosocial section, where a higher score is more desirable.[19]

In Table 2, we use baseline data to confirm that child and household characteristics are balanced across treatment and control groups for all 225 villages before the beginning of the intervention.[20] The variable means for each of the three treatment groups and the control group measured at baseline are shown separately in columns 1, 2, 4, and 6, with p-values for tests of equality between the control group and each treatment group in columns 3, 5, and 7. Note that, for the reasons described above and to be consistent with our analysis in the following section, we combine the households in treatment arms (iii) and (iv). Column 8 shows the p-value of a joint F-test of equality across all four groups, in most cases indicating that we cannot reject equality across all arms. There does seem to be some imbalance in the age of the household head, the household size, and the number of non-related members in the household.[21] In the joint test across all four study groups, we find only three significant outcomes (at the 10 percent level) out of 43 tests, showing that the study groups seem to be balanced at baseline.

[19] as a result, the prosocial score cannot be aggregated with the total score for the other sections, since the two move in opposite directions

[20] To see the baseline balance across the 74 villages used in our analysis, see Appendix Table A2. The patterns are generally similar across the two tables.

[21] To account for these differences, we include household head age in all regressions and include household size and the number of non-related household members in the options for the LASSO-selected controls.

3.3. Attrition

At baseline, we have 225 villages, distributed evenly across the four treatment arms and the control group. As described above, one of the challenges associated with the project is the fact that many of the baseline villages could not be re-surveyed at endline due to conflict in the area. As a result, we lost a large number of villages, and the endline survey was only able to capture 74 villages. Because of the large number of villages lost as a result of the conflict and since this could be a concern for identification if the villages lost were not balanced across treatment arms, we perform several tests for differential attrition.

First, in Table A3 we show the number of villages (in Panel A) and the number of households (in Panel B) by treatment arm that were surveyed at endline, out of the 225 villages and 4730 households at baseline. The first row in each panel shows the percentage of baseline villages or households in each arm surveyed at endline, and the second row shows the p-value associated with an F-test comparing the percentage of villages and households returned to in each treatment arm with the percentage of households and villages returned to in the control group. In all cases, we cannot reject that the attrition at the household and village level by treatment arm was the same. The remaining rows in Panel A show the number of baseline and endline villages surveyed, and those in Panel B show the number of households surveyed at baseline, the number of households surveyed at baseline in the villages we returned to at endline, and the actual number of endline households surveyed. These numbers are similar across arms. It is also important to note that, in the 74 villages that we returned to at endline, we only lost 1.8 percent of households, which is a relatively low attrition rate.

Table A4 shows the attrition estimates in regression form. The first column shows that, across the treatment arms (with treatment arm (iii) and treatment arm (iv) combined to match our

main specification), there is no difference in the probability that the village is present at endline. Column 2 shows the same for households, suggesting that, while conflict led to large overall levels of attrition due to the fact that we were unable to return to some of the villages for security reasons, this attrition does not seem to be correlated with the treatment.

Finally, in Table A5, we show the baseline household characteristics and child outcomes across the attritor and non-attritor households by treatment arm. Within a treatment arm, there are differences in the characteristics and baseline values of the outcomes across the attritors and non-attritors.[22] Column 9 tests whether the differences across the attritors and non-attritors within each treatment arm are the same across arms. Out of the 43 variables that we test in column 9, the only one with significant differences across the within-arm comparisons is the probability that the household head is male. Based on this table, while attrition due to conflict does not appear to be random, there does not seem to be differential attrition across treatment arms.

3.4. Strategies to Address the Large Number of Outcomes

To address the large number of outcomes and avoid overemphasizing any single significant result, we adopt two strategies. First, we create indices for each family of outcomes following Kling, Liebman, and Katz (2007). To construct the indices, we define each outcome so that higher values correspond with better outcomes, standardize each outcome into a Z-score by subtracting the mean and dividing by the standard deviation of the control group in the baseline period, and average all the Z-scores. We then estimate the effect of each treatment arm on these standardized outcome indices.

[22] P-values for the differences within each treatment arm are available upon request.

Second, we use the randomization inference-based tests proposed by Young (2019) to address the potential issues arising from multiple hypothesis testing. In the main regression tables, in addition to showing the coefficients and clustered standard errors, we also report randomization inference p-values that are based on exact p-values for the sharp null hypothesis of no treatment effect for each treatment arm. Differences between exact randomization p-values for individual significance tests and the estimates from the clustered standard errors are very small, and the same conclusions (and levels of significance) are generally obtained.

4. Empirical Results

4.1. Program Impacts

In the subsequent tables, we report the results of the impact evaluation. Each table shows the results separately for each treatment arm, following equation (1). The coefficient on the variable "cash" can be interpreted as the impact of the cash only arm (T1), the coefficient on "cash + info" is the impact of the cash and information arm (T2), and the coefficient on "cash + info + home visits" is the impact of the cash, information, and home visits arms (T3 & T4).

Tables showing the results of regressions estimating Equations (1) and (2) with the separate and cumulative treatment specifications include two panels. Panel A shows the results separately for each treatment arm, following equation (1). Panel B focuses on the cumulative effects of cash, information, and home visits. In those panels, the coefficient on the variable "cash" can be interpreted as the impact of cash transfers, the coefficient on the variable "+ info" can be interpreted as the additional effect of information (conditional on receiving cash), and the coefficient on the variable "+ home visits" can be interpreted as the additional effect of home visits (conditional on receiving cash and information).The p-value for "cash + info + HV"

indicates whether the total effect of all three components of the intervention together is statistically different from zero.

4.2. Impacts on Fertility and Pregnancy

We first look at the effects of the intervention on pregnancy, birth spacing, and delivery at the household level. Table 3 shows that there are no effects of cash alone or cash + information. However, the cash + information + home visits arms (treatment arms (iii) and (iv)) cause a reduction in the number of times a mother has been pregnant by 6 percent of the control group mean. This aligns with the results in columns 2-6, showing a later age at each pregnancy, ranging from a statistically insignificant increase of 0.305 years for the first pregnancy to a statistically significant increase of 1.052 years for the fifth pregnancy.[23]

Additionally, women in households in the cash + info + home visits arm are on average 3.5 percentage points more likely to have had their last delivery assisted by a medical professional. Given that 96 percent of women in the control group report having their last delivery assisted by a medical professional, this increase of 3.5 percentage points brings women in the cash + information + home visits arms up to nearly 100% coverage for assisted childbirth. The Kling et al. (2007) index across the pregnancy outcomes is also significant and positive for the households receiving cash + information + home visits, showing improved overall outcomes related to pregnancy, birth spacing, and delivery. There are no effects in the cash only or cash + info arms, indicating that the full package of the intervention is the only one that improves pregnancy-related outcomes.

4.3. Impacts on Household Health Behaviors

[23] While individuals were also asked about later pregnancies, the sample size decreases so much for pregnancies six and beyond that we do not include these results in the regression. The average number of pregnancies in our sample is 4.6, and the fertility rate in Burkina Faso is five.

Next, we investigate the impacts on outcomes related to child health in Table 4. As in Table 3, the outcomes are calculated as averages at the household level across all children in the relevant age range in the household. Again, we see that the cash only and cash + information treatment arms do not lead to an improvement in prenatal and child health outcomes.

However, the cash + information + home visits treatment arms do result in an increase of 13 percent in the probability that all children in the household receive vitamin A relative to the control group mean, as well as an increase of 3.5 percentage points in the probability of mothers of young children reporting washing their hands after using the toilet. Once again, the index across all outcomes also shows statistically significant increases in outcomes for the cash + info + home visits treatment arm. We do not see effects on the number of prenatal visits, the probability that a mother receives vitamin A or iron during her pregnancy, or the probability that a child is fully vaccinated.

4.4. Impacts on Child Welfare (Anthropometrics and Education)

Table 5 shows the impacts of the intervention on child anthropometrics. All outcomes are z-scores, calculated using the WHO reference population for the relevant age and gender. The regressions are run at the level of the individual child, and children under 60 months of age at the endline survey are included in the regressions. In column 2, individuals in the cash arm have a 0.27 standard deviation increase in arm circumference for age and a 0.16 standard deviation increase in weight for age in column 3. However, there is no statistically significant effect on height for age; in fact, and the coefficient for the cash arm in column 1 is negative. Receiving cash only does not have a statistically significant impact on the anthropometrics index.

While cash alone improves arm circumference-for-age and weight-for age, cash + information does not have an effect. Across the three individual outcomes and the index, the

coefficients are negative and are not statistically significant. In the cash + info + home visits arm, there is an increase of 0.15 standard deviations in the arm circumference for age in column 2. However, there are no statistically significant improvements in height-for-age or weight-for-age. Additionally, the index in column 4 shows no overall changes in anthropometrics across the board.

The anthropometrics results above appear to be driven by changes in children's food consumption. In Table A6, we show the impacts on children's food consumption. The outcome in each column is the number of times that a child ages 0-71 months consumed food items in the respective categories over the past 7 days, averaged at the household level. We see mostly positive effects across the board, though the only statistically significant effects are an increase of 51 percent for milk in the cash + info arm in column 2 and an increase for eggs of 99 percent in the cash arm and 76 percent in the cash + info + home visits arms in column 4. The index in column 6 shows an overall increase in the number of times food groups are consumed for the cash and cash + info + home visits arms, which aligns with the effects on anthropometrics found in Table 5.

We also examine the effects on education for older children, ages 6-15, in Table 6. These regressions are run at the individual level. Surprisingly, cash only has very little impact on education outcomes, with the only statistically significant increase coming from an 8.7 percentage point increase in the probability of completing any schooling. Cash + information does not have an effect on schooling, either, with the point estimates across all outcomes being quite low and statistically insignificant.

However, cash + information + home visits, on the other hand, has strong positive effects on most education outcomes. The probability of being enrolled in school increases by 7.8

percentage points, children complete an addition 0.3 years of school, the likelihood of completing any schooling increases by 8.5 percentage points, and grade progression (calculated as completed grade divided by expected grade and capped at 1) increases by 0.065. There are no improvements in reading and writing in French. The index, once again, shows a positive and statistically significant increase in education outcomes overall.

4.5. Impacts on Early Childhood Development

While these improvements in health, anthropometrics, and education are promising, we also want to evaluate the impacts on early childhood development, as improvements in this area were one of the goals of the project. Table 7 shows the impacts on Denver test scores at the individual level for children ages 0-72 months at the time of the survey. The first column shows the impacts on the total score, and columns 2-5 break out the individual components of cash, information, and home visits. Panel A shows the estimates with the main specification in equation (1), and we can see that the intervention did not have any positive effects on Denver scores. If anything, cash + information reduced the language and gross motor scores of the children.

In Panel B, on the other hand, we show the estimates for the cumulative specification from equation (2). Again, the impact of cash and the additional impact of information are generally negative and statistically insignificant. However, this specification shows that the additional effect of receiving home visits, conditional on receiving both cash and information, is positive for most components of the Denver score. For example, the additional effect of home visits is 12 percent of the control mean for the total score in column 1, 12 percent for the language score in column 2, 13 percent for the gross motor score in column 4, and 11 percent for the personal social score in column 5. While we cannot separately identify the effects of home visits on the Denver score, since everyone who received home visits also received cash and

information, these results suggest that home visits may be important for improving early childhood development outcomes.

In Table 8, we show additional early childhood development outcomes from the Head-Toes-Knees-Shoulders (HTKS) test. Similar to the Denver test, when looking at the main specification in Panel A, the only statistically significant coefficients are negative effects on cash + information. However, the story looks slightly different when we estimate the cumulative specification in Panel B. We can see that, while cash and information have negative (though not always statistically significant) effects on HTKS scores, the additional effect of home visits, conditional on receiving cash and information, is positive and statistically significant. The additional impact of home visits leads to a 12 percent increase in the official score in column 1, a 12 percent increase in the score on the first three sections of the test in column 2, and an 11 percent increase in the probability of scoring higher than zero in column 3. The HTKS index in column 4 also experiences a statistically significant increase of 0.201.

As our final measure of ECD outcomes, we investigate the impacts on Strengths and Difficulties scores in Table 9. In column 3 of Panel A, cash improves the likelihood of being in the average range for the prosocial score by 4 percentage points, and the impact on the index in column 5 is also positive and statistically significant. There is no effect of cash + information. Cash + information + home visits improve the likelihood of being in the average range for the prosocial score by 5.9 percentage points (column 3) and the likelihood of having a z-score for the prosocial component of over 0.5 (column 4). Additionally, the effect of cash + information + home visits is positive and statistically significant for the index in column 5. None of the treatment arms show any statistically significant effect on the total difficulties score.

Panel B shows the estimates of the cumulative specification from equation (2). Information does not have an effect on outcomes, but the additional effect of home visits on prosocial scores, conditional on receiving cash and information, is positive and statistically significant, at 3.6 percentage point in column 3 and 5.6 percentage points in column 4. However, the effect on the index in column 5 is no longer statistically different from zero.

4.6. Household Perceptions

We also find evidence that the intervention impacted household perceptions. Household heads were asked several questions about perceptions: (i) "Are you very satisfied, satisfied, dissatisfied or dissatisfied with the diet of your household members in the last 12 months?" (ii) "Are you very satisfied, satisfied, dissatisfied or dissatisfied with the state of health of the members of your household in the last 12 months?" (iii) "Are you very satisfied, satisfied, dissatisfied or dissatisfied with the conditions of school education of the members of your household in the last 12 months?" and (iv) "Are you very satisfied, satisfied, dissatisfied or dissatisfied with your household income level in the last 12 months?"

Table A7 shows the impacts on household head's perceptions of their household's status, with the outcome for each column being an indicator for the household reporting to be very satisfied with the household's status for each of the respective categories.[24] Column 1 shows that the cash + information and cash + information + home visits arms improved the household head's perception of the household's health status (2 and 2.2 percentage points more likely to answer that they were very satisfied, respectively), column 2 shows that cash + information improved the household head's perception of the household's health status (3.9 percentage points

[24] Level of satisfaction was elicited on a 4-point scale, and "very satisfied" was the highest level of satisfaction on the scale.

more likely to respond that they were very satisfied), and column 3 shows that there was no effect on perceptions of education. Household heads in all treatment arms were more likely to reporting being satisfied with the household's income, as seen in column 4, and the index in column 5 shows that cash + info and cash + info + home visits significantly increased overall satisfaction.

4.7. Intervention Fidelity

To understand the implementation and take-up of the intervention, we also ask households at endline to report their receipt of cash transfers, attendance at information sessions, and participation in home visits. Table 10 shows the impacts on receipt of cash transfers. Column 1 shows that, across all treatment arms, households were 94-96 percentage points more likely than the control group to receive cash transfers. Additionally, the fraction of households receiving cash transfers in the control group is very low, at around one percent. The likelihood of receiving a mobile phone, which was used to deliver the electronic cash transfer payments, increases by a similar amount. The amounts of the last transfer and the total amount received, shown in columns 3 and 4, also appear to be similar across the three treatment arms.

Table 11 shows the impacts on attendance at information sessions and participation in home visits. The results in this table are not as clean as those in Table 10. In column 1 of Panel A, we see that the likelihood of at least one household member attending any nutrition information sessions increases in all three treatment arms. Additionally, the probability of attending a nutrition information session is 20 percent in the control group. Column 1 of Panel B shows that, while cash increases the probability of attending any nutrition information sessions, the additional effect of being randomized into an information treatment is also positive and statistically significant. A similar pattern is true for behavior information sessions in column 2.

The total number of information sessions attended also increases across all arms in column 3 of Panel A, but there is no significant additional impact of information in Panel B.

Column 4 shows that the probability of receiving any nutrition home visits increases for all treatment arms in Panel A, though 22 percent of the control group is also receiving nutrition home visits. In Panel B, being randomized into the cash and home visits arms is the main driver of this increase. Column 5 shows that all treatment arms are more likely to receive behavioral home visits in Panel A, while being randomized into the home visits treatment does not make households any more likely to receive home visits in Panel B. The impact on the total number of home visits is shown in column 6, and the pattern is similar to that in column 4. Being randomized into the home visits treatment does increase the number of home visits received in Panel B.

One caveat to keep in mind with all of the results presented in this section is that we were not able to conduct an immediate endline survey due to delays resulting from the COVID-19 pandemic and insecurity in the region. While the interventions ended in December 2021, it was not until spring of 2022 that the endline survey was administered, leading to a 15-month gap between the end of the intervention and the endline responses. As a result, this should be interpreted as more of a medium-term effect of the intervention instead of an immediate, short-term effect.

4.8. Heterogeneity

We also investigate heterogeneity in our main results. In Table A8, we show heterogeneity across girls and boys for the child-level outcomes. In nearly all cases, the point estimates for boys and girls are quite similar, indicating that there does not seem to be a large difference in the effects across gender.

Table A9 shows heterogeneity by exposure to conflict based on whether villages were above or below the median distance from the nearest conflict incident. For all outcomes except for the anthropometrics index, point estimates are large and most statistically significant for households below the median distance from the conflict. This suggests that households that are more vulnerable, measured as being closer to the conflict, are more likely to benefit from receiving the package of cash, information, and home visits. Additionally, in the case of health, education, and the Denver score, cash results in improvements in villages that are nearer conflict.

A similar pattern seems to hold in Table A10, which shows heterogeneity by expenditures, based on whether the households had above median or below median expenditures at baseline. Again, the point estimates for the effects in households with below median expenditure are generally larger and more statistically significant than the point estimates in the households with above median expenditure. This suggests that more vulnerable households are more responsive to receipt of cash, information, and home visits.

4.9. Robustness

To show that our results are robust to various specifications and inclusion of controls, for our five main tables we run alternate specifications for the indices or total outcomes for each table. These results are shown in Table A11. Panel A repeats the main estimates of the index for pregnancy outcomes, household health behaviors, anthropometrics, and education as well as the total Denver test score. Panel B shows the results for the same five outcomes without the LASSO-selected controls, but still including controls for age and gender, when relevant. Overall, the estimates are similar in magnitude and significance to those in Panel A. While the effect of "cash + info + home visits" on education index is no longer statistically significant, the point estimate in Panel B is very similar to that in Panel A.

Panel C shows the estimates from an ANCOVA specification that includes controls for baseline values of the outcome variables, when available. The sample sizes in Panel C are smaller than in Panel A because not all individuals who were surveyed at endline were also surveyed at baseline, especially for the anthropometrics outcomes. Fertility-related questions were not asked in the baseline survey, so we cannot run this specification for the pregnancy index. However, the remaining outcomes in columns 2-4 show similar effects to the main specification in Panel A, indicating that our results are not driven by our choice of specification.

5. Conclusion

In this paper, we evaluate a randomized control trial that examines the impacts of an integrated social safety nets program including cash transfers, information meetings, and home visits, on children's human capital and development outcomes. We find a decrease in reported numbers of pregnancies, an increase in age at time of birth, and an increase in medically assisted deliveries among mothers of children under age 5 who received the cash transfers, information meetings, and home visits. Additionally, we find improvements in household health behaviors, education outcomes, and child anthropometrics. Finally, we find suggestive evidence that home visits, on top of cash and information, may increase early childhood development outcomes, as measured through the Denver, HTKS, and Strengths and Difficulties tests. Cash alone or cash + information does not seem to improve children's outcomes, suggesting that the home visits are a critical component of the intervention package.

Additionally, the heterogeneity across high- and low-expenditure households and by level of conflict exposure provide suggestive evidence that more vulnerable households may have benefitted more from the full intervention. This suggests that targeting package interventions

with cash, information, and home visits to more vulnerable households may result in higher impacts, leading the interventions to be more cost effective.

Finally, this RCT is unique in that it is a government-run intervention with cash transfers that are provided at scale across the entire country. Issues experienced with implementation, therefore, are representative of the problems that come from a "real-life" policy intervention where the research team is not in full control of the implementation. On the other hand, the results that we find are also "real-life" impacts from a realistic social safety nets policy, meaning that this experiment provides an accurate picture of the impacts that can be expected from this type of intervention in the real world.

References

Adhvaryu, Achyuta and Anant Nyshadham. 2014. "Endowments at Birth and Parents' Investments in Children." *Economic Journal,* 126 (593): 781-820.

Agüero, Jorge M., Michael R. Carter, and Ingrid Woolard. 2007. "The Impact of Unconditional Cash Transfers on Nutrition: The South African Child Support Grant." International Poverty Center Working Paper 39.

Ahmed, Akhter, John F. Hoddinott, and Shalini Roy. 2019. "Food Transfers, Cash Transfers, Behavior Change Communication and Child Nutrition: Evidence from Bangladesh." International Food Policy Research Institute Discussion Paper 01868.

Aizer, Anna, Shari Eli, Joseph Ferrie, and Adriana Lleras-Muney. 2016. "The Long-Run Impact of Cash Transfers to Poor Families." *American Economic Review*, 106(4): 935-971.

Akresh, Richard, Damien de Walque, and Harounan Kazianga. 2013. "Cash Transfers and Child Schooling: Evidence From a Randomized Evaluation of the Role of Conditionality." World Bank Policy Research Working Paper 6340.

Akresh, Richard, Damien de Walque, and Harounan Kazianga. 2016. "Alternative Cash Transfer Delivery Mechanisms: Impacts on Routine Preventative Health Clinic Visits in Burkina Faso." *NBER Africa Successes: Human Capital*, Volume 2. Editors: Sebastian Edwards, Simon Johnson, and David Weil. University of Chicago Press

Akresh, Richard, Damien de Walque, and Harounan Kazianga. 2024. "Cash Transfers and Child Welfare: Evidence from a Randomized Evaluation of the Role of Conditionality and Transfer Recipient." Unpublished manuscript.

Akresh, Richard, Emilie Bagby, Damien de Walque, and Harounan Kazianga. 2012a. "Child Ability and Household Human Capital Investment Decisions in Burkina Faso." *Economic Development and Cultural Change*, 61(1): 157-186.

Akresh, Richard, Emilie Bagby, Damien de Walque, and Harounan Kazianga. 2012b. "Child Labor, Schooling and Child Ability." World Bank Policy Research Working Paper 5965.

Akresh, Richard. 2009. "Flexibility of Household Structure: Child Fostering Decisions in Burkina Faso." *Journal of Human Resources*, 44(4), 976-997.

Alderman, Harold, Jere R. Behrman, and Chloe Puett. 2017. "Big Numbers about Small Children: Estimating the Economic Benefits of Addressing Undernutrition." *World Bank Research Observer* 32 (1): 107–25.

Almås, Ingvild, Alex Armand, Orazio Attanasio, and Pedro Carneiro. 2018. "Measuring and Changing Control: Women's Empowerment and Targeted Transfers." *Economic Journal*, 128(612): 609-639.

Almås, Ingvild, Johannes Haushofer, and Jeremy P. Shapiro. 2019. "The Income Elasticity for Nutrition: Evidence from Unconditional Cash Transfers in Kenya." NBER Working Paper 25711.

Almond, Douglas and Janet Currie. 2011. "Human Capital Development Before Age 5." In *Handbook of Labor Economics,* Volume 4b. Editors Orley Ashenfelter and David Card. Elsevier, Amsterdam, North Holland

Araujo, M Caridad, Marta Dormal, Sally Grantham-McGregor, Fabiola Lazarte, Marta Rubio-Codina, and Norbert Schady. 2021. "Home Visiting at Scale and Child Development." *Journal of Public Economics Plus*, 2:100003.

Armand, Alex, Orazio Attanasio, Pedro Carneiro, and Valerie Lechene. 2020. "The Effect of Gender-targeted Conditional Cash Transfers on Household Expenditures: Evidence from a Randomized Experiment." *Economic Journal*, 130(631): 1875-1897.

Armand, Alex. 2015. "Who Wears the Trousers in the Family? Intrahousehold Resource Control, Subjective Expectations, and Human Capital Investment: Evidence from a Randomized Experiment." NCID Working Papers 03/2015, Navarra Center for International Development, University of Navarra.

Attanasio, Orazio, Camila Fernandez, Emla Fitzsimons, Sally Grantham-McGregor, Costas Meghir, and Marta Rubio-Codina. 2014. "Using the Infrastructure of a Conditional Cash Transfer Program to Deliver a Scalable Integrated Early Child Development Program in Colombia: Cluster Randomized Controlled Trial." *BMJ*, 349: g5785.

Attanasio, Orazio, Costas Meghir and Ana Santiago. 2012. "Education Choices in Mexico: Using a Structural Model and a Randomized Experiment to Evaluate Progresa." *Review of Economic Studies,* 79 (1): 37-66.

Attanasio, Orazio, Emla Fitzsimons, Ana Gomez, Martha Gutierrez, Costas Meghir, and Alice Mesnard. 2010. "Children's Schooling and Work in the Presence of a Conditional Cash Transfer Program in Rural Colombia." *Economic Development and Cultural Change,* 58(2): 181-210.

Attanasio, Orazio, Sarah Cattan, Emla Fitzsimons, Costas Meghir, and Marta Rubio-Codina. 2020. "Estimating the Production Function for Human Capital: Results from a Randomized Controlled Trial in Colombia." *American Economic Review* 110 (1): 48–85.

Attanasio, Orazio, Veruska Oppedisano, and Marcos Vera-Hernández. 2015. "Should Cash Transfers Be Conditional? Conditionality, Preventive Care, and Health Outcomes." *American Economic Journal: Applied Economics*, 7(2): 35–52.

Baird, Sarah, Craig McIntosh, and Berk Özler. 2011. "Cash or Condition? Evidence from a Cash Transfer Experiment." *Quarterly Journal of Economics*, 126(4): 1709-1753.

Baird, Sarah, Craig McIntosh, and Berk Özler. 2019. "When the Money Runs Out: Do Cash Transfers Have Sustained Effects on Human Capital Accumulation?" *Journal of Development Economics* 140: 169–85.

Baird, Sarah, Jacobus de Hoop, and Berk Özler. 2013. "Income Shocks and Adolescent Mental Health." *Journal of Human Resources*, 48(2): 370-403.

Baird, Sarah, Joan Hamory Hicks, Michael Kremer, and Edward Miguel. 2016. "Worms at Work: Long-Run Impacts of a Child Health Investment." *Quarterly Journal of Economics* 131 (4): 1637–80.

Baird, Sarah, Richard Garfein, Craig McIntosh, and Berk Özler. 2012. "Impact of a Cash Transfer Program for Schooling on Prevalence of HIV and HSV-2 in Malawi: A Cluster Randomized Trial." *The Lancet,* 379(9823): 1320-1329.

Bandiera, Oriana, Robin Burgess, Narayan Das, Selim Gulesci, Imran Rasul, and Munshi Sulaiman. 2017. "Labor Markets and Poverty in Village Economies." *Quarterly Journal of Economics* 132 (2): 811–70.

Banerjee, Abhijit, Dean Karlan, Hannah Trachtman, and Christopher R. Udry. 2020. "Does Poverty Change Labor Supply? Evidence from Multiple Income Effects and 115,579 Bags." Unpublished.

Banerjee, Abhijit, Esther Duflo, Nathanael Goldberg, Dean Karlan, Robert Osei, William Parienté, Jeremy Shapiro, Bram Thuysbaert, Christopher Udry. 2015. "A Multi-faceted Program Causes Lasting Progress for the Very Poor: Evidence from Six Countries." *Science* 348 (6236): 1260799.

Banerjee, Abhijit, Paul Niehaus, and Tavneet Suri. 2019. "Universal Basic Income in the Developing World." *Annual Review of Economics*. 11: 959-983.

Banerjee, Abhijit, Rema Hanna, Gabriel Kreindler, and Benjamin Olken. 2017. "Debunking the Stereotype of the Lazy Welfare Recipient: Evidence from Cash Transfer Programs." *World Bank Research Observer*, *32*(2): 155-184.

Barham, Tania, Karen Macours, and John A. Maluccio. 2013. "Boys' Cognitive Skill Formation and Physical Growth: Long-Term Experimental Evidence on Critical Ages for Early Childhood Interventions." *American Economic Review* 103 (3): 467–71.

Bastagli, Francesca, Jessica Hagen-Zanker, Luke Harman, Valentina Barca, Georgina Sturge, and Tanja Schmidt. 2019. "The Impact of Cash Transfers: A Review of the Evidence from Low- and Middle-income Countries." *Journal of Social Policy*, 48(3): 569-594.

Behrman, Jere R., Mark R. Rosenzweig, and Paul Taubman. 1994. “Endowments and the Allocation of Schooling in the Family and in the Marriage Market: The Twins Experiment.” *Journal of Political Economy*, 102 (6): 1131-74.

Behrman, Jere, Piyali Sengupta, and Petra Todd. 2005. “Progressing Through PROGRESA: An Impact Assessment of a School Subsidy in Mexico.” *Economic Development and Cultural Change,* 54(1): 237-75.

Benhassine, Najy, Florencia Devoto, Esther Duflo, Pascaline Dupas, and Victor Pouliquen. 2015. “Turning a Shove into a Nudge: A “Labeled Cash Transfer” for Education.” *American Economic Journal: Economic Policy*, 7(3): 86-125.

Bergman, Peter, Raj Chetty, Stefanie DeLuca, Nathaniel Hendren, Lawrence F. Katz, and Christopher Palmer. 2019. “Creating Moves to Opportunity: Experimental Evidence on Barriers to Neighborhood Choice.” NBER Working Paper 26164.

Bharadwaj, Prashant, Katrine Loken, and Christopher Neilson. 2013. "Early Life Health Interventions and Academic Achievement." *American Economic Review*, 103(5): 1862-1891.

Bhutta, Zulfiqar A., Jai K. Das, Arjumand Rizvi, Michelle F. Gaffey, Neff Walker, Susan Horton, Patrick Webb, Anna Lartey, and Robert E. Black. 2013. “Evidence-Based Interventions for Improvement of Maternal and Child Nutrition: What Can Be Done and at What Cost?” *Lancet* 382 (9890): 452–77.

Bhutta, Zulfiqar A., Tahmeed Ahmed, Robert E. Black, Simon Cousens, Kathryn Dewey, Elsa Giugliani, Batool A. Haider, et al. 2008. “What Works? Interventions for Maternal and Child Undernutrition and Survival.” *Lancet* 371 (9610): 417–40.

Blattman, Christopher, Nathan Fiala, and Sebastian Martinez. 2014. “Generating Skilled Self-Employment in Developing Countries: Experimental Evidence from Uganda.” *Quarterly Journal of Economics*, 129(2): 697-752.

Borghans, Lex, Angela Lee Duckworth, James Heckman, and Bas ter Weel. 2008. “The Economics and Psychology of Personality Traits.” *Journal of Human Resources*, 43(4): 972-1059.

Bouguen, Adrien, Yue Huang, Michael Kremer, and Edward Miguel. 2019. “Using Randomized Controlled Trials to Estimate Long-Run Impacts in Development Economics.” *Annual Review of Economics* 11: 523–61.

Braido, L.H., Olinto, P. and Perrone, H., 2012. “Gender bias in intrahousehold allocation: Evidence from an unintentional experiment.” *Review of Economics and Statistics*, *94*(2): 552-565.

Caeyers, Bet, Sonya Krutikova, and Orazio Attanasio. 2016. *Parent Support in the Income Sector: A Review of the Evidence*. IFS Report.

Cahyadi, N., Hanna, R., Olken, B.A., Prima, R.A., Satriawan, E. and Syamsulhakim, E., 2020. "Cumulative impacts of conditional cash transfer programs: Experimental evidence from Indonesia." *American Economic Journal: Economic Policy*, *12*(4), pp.88-110.

Caldes, Natalia, David Coady, and John Maluccio. 2006. "The Cost of Poverty Alleviation Transfer Programs: A Comparative Analysis of Three Programs in Latin America." *World Development*, 34(5): 818-837.

Campbell, Frances, Gabriella Conti, James J. Heckman, Seong Hyeok Moon, Rodrigo Pinto, Elizabeth Pungello, and Yi Pan. 2014. "Early Childhood Investments Substantially Boost Adult Health." *Science* 343 (6178): 1478–85.

Carneiro, Pedro, Emanuela Galasso, Italo Lopez Garcia, Paula Bedregal, and Miguel Cordero. 2024. "Impacts of a Large-Scale Parenting Program: Experimental Evidence from Chile." *Journal of Political Economy*, 132(4): 1113-1161.

Carneiro, Pedro, Lucy Kraftman, Giacomo Mason, Lucie Moore, Imran Rasul, and Molly Scott. 2021. "The Impacts of a Multifaceted Prenatal Intervention on Human Capital Accumulation in Early Life." *American Economic Review*, 111(8): 2506-2549.

Case, Anne, Victoria Hosegood, and Frances Lund. 2005. "The Reach and Impact of Child Support Grants: Evidence from KwaZulu-Natal." *Development Southern Africa*, 22(4): 467-482.

Chattopadhyay, R. and Duflo, E., 2004. Women as policy makers: Evidence from a randomized policy experiment in India. *Econometrica*, 72(5), pp.1409-1443.

Conti, Gabriella, James J. Heckman, and Rodrigo Pinto. 2016. "The Effects of Two Influential Early Childhood Interventions on Health and Healthy Behavior." *Economic Journal* 126 (596): F28–65.

Cooper, Sarah, Frank Bicaba, Cheick Oumar Tiendrebeogo, Alice Bila, Abel Bicaba, Thomas Druetz. 2023. "Vaccination Coverage in Rural Burkina Faso Under the Effects of COVID-19: Evidence from a Panel Study in Eight Districts." *BMC Health Services Research*, 23(1): 1016.

Cunha, Jesse M. 2011. "Testing Paternalism: Cash vs. In-kind Transfers in Rural Mexico." Working paper, Naval Postgraduate School.

Currie, Janet, and Maya Rossin-Slater. 2013. "Weathering the Storm: Hurricanes and Birth Outcomes." *Journal of Health Economics* 32 (3): 487–503.

Das, Jishnu, Quy-Toan Do, and Berk Özler. 2005. "Reassessing Conditional Cash Transfers Programs." *World Bank Research Observer*, 20(1): 57-80.

de Janvry, Alain and Elisabeth Sadoulet. 2005. "Conditional Cash Transfer Programs for Child Human Capital Development: Lessons Derived From Experience in Mexico and Brazil." University of California, Berkeley, manuscript.

de Janvry, Alain, Frederico Finan, Elisabeth Sadoulet, and Renos Vakis. 2006. "Can Conditional Cash Transfer Programs Serve as Safety Nets in Keeping Children at School and From Working When Exposed to Shocks?" *Journal of Development Economics*, 79(2): 349-373.

de Mel, Suresh, David McKenzie, and Christopher Woodruff. 2009. "Are Women More Credit Constrained? Experimental Evidence on Gender and Microenterprise Returns." *American Economic Journal: Applied Economics*, 1(3): 1-32.

de Walque, Damien and Christine Valente. 2018. Incentivizing School Attendance in the Presence of Parent-Child Information Frictions" World Bank Policy Research Working Paper #8476.

Dewey, Kathryn G., and Seth Adu-Afarwuah. "Systematic Review of the Efficacy and Effectiveness of Complementary Feeding Interventions in Developing Countries." *Maternal and Child Nutrition* 4: 24–85.

Doyle, Orla. 2019. "The First 2,000 Days and Children's Skills." *Journal of Political Economy* 128 (6): 2067–122.

Duflo, Esther and Christopher Udry. 2004. "Intrahousehold Resource Allocation in Côte d'Ivoire: Social Norms, Separate Accounts and Consumption Choices." National Bureau of Economic Research Working Paper No. 10498.

Duflo, Esther. 2003. "Grandmothers and Granddaughters: Old-Age Pensions and Intrahousehold Allocation in South Africa." *World Bank Economic Review*, 17(1): 1-25.

Duflo, Esther. 2012. "Women Empowerment and Economic Development." *Journal of Economic Literature*, 50(4): 1051-1079.

Edmonds, Eric. 2008. "Child Labor." In *Handbook of Development Economics*, Volume 4. Editors, T. Paul Schultz and John Strauss. Elsevier, Amsterdam, North Holland.

Egger, D., Haushofer, J., Miguel, E., Niehaus, P. and Walker, M., 2022. "General equilibrium effects of cash transfers: experimental evidence from Kenya." *Econometrica*, *90*(6): 2603-2643.

Engle, Patrice L., Maureen M. Black, Jere R. Behrman, Meena Cabral De Mello, Paul J. Gertler, Lydia Kapiriri, Reynaldo Martorell, Mary Eming Young, and International Child

Development Steering Group. 2007. “Strategies to Avoid the Loss of Developmental Potential in More than 200 Million Children in the Developing World.” *Lancet* 369(9557): 229–42.

Evans, David, Michael Kremer, and Muthoni Ngatia. 2009. “The Impact of Distributing School Uniforms on Children’s Education in Kenya.” World Bank Working Paper.

Fafchamps, Marcel, David McKenzie, Simon Quinn, and Christopher Woodruff. 2014. “Microenterprise Growth and the Fly-paper Effect: Evidence from a Randomized Experiment in Ghana.” *Journal of Development Economics*, 106: 211-226.

Fernald, Lia C. H., Rose M. C. Kagawa, Heather A. Knauer, Lourdes Schnaas, Armando Garcia Guerra, and Lynnette M. Neufeld. 2017. “Promoting Child Development Through Group-Based Parent Support Within a Cash Transfer Program: Experimental Effects on Children’s Outcomes.” *Development Psychology* 53 (2): 222–36.

Filmer, Deon and Norbert Schady. 2011. “Does More Cash in Conditional Cash Transfer Programs Always Lead to Larger Impacts on School Attendance?” *Journal of Development Economics*, 96(1): 150–157.

Fink, Günther, Evan Peet, Goodarz Danaei, Kathryn Andrews, Dana Charles McCoy, Christopher R. Sudfeld, Mary C. Smith Fawzi, Majid Ezzati, and Wafaie W. Fawzi. 2016. “Schooling and Wage Income Losses Due to Early-Childhood Growth Faltering in Developing Countries: National, Regional, and Global Estimates.” *American Journal of Clinical Nutrition* 104 (1): 104–12.

Fiszbein, Ariel and Norbert Schady 2009. *Conditional Cash Transfers: Reducing Present and Future Poverty*. World Bank Policy Research Report. World Bank: Washington, DC.

Food and Agriculture Organization of the United Nations. 2014. FAOSTAT database, available at http://faostat3.fao.org/home/E (accessed on 10/30/2014).

Frankenburg, William K. and Josiah B. Dodds. 1967. “The Denver Developmental Screening Test”. *The Journal of Pediatrics*, 71 (2): 181-91

Gelbach, Jonah and Lant Pritchett. 2002. “Is More for the Poor Less for the Poor? The Politics of Means-Tested Targeting.” *B.E. Journal of Economic Analysis and Policy*, 2(1): article 6.

Gertler, Paul J., Sebastian W. Martinez, and Marta Rubio-Codina. 2012. “Investing Cash Transfers to Raise Long-Term Living Standards.” *American Economic Journal: Applied Economics* 4 (1): 164–92.

Gertler, Paul, James Heckman, Rodrigo Pinto, Arianna Zanolini, Christel Vermeersch, Susan Walker, Susan M. Chang, and Sally Grantham-McGregor. 2014. “Labor Market Returns to an Early Childhood Stimulation Intervention in Jamaica.” *Science* 344 (6187): 998–1001.

Glewwe, Paul and Ana Lucia Kassouf. 2012. "The Impact of the Bolsa Escola/Familia Conditional Cash Transfer Program on Enrollment, Dropout Rates and Grade Promotion in Brazil." *Journal of Development Economics*, 97(2): 505-517.

Glewwe, Paul and Pedro Olinto. 2004. "Evaluating the Impact of Conditional Cash Transfers on Schooling: An Experimental Analysis of Honduras. PRAF Program." Unpublished manuscript, University of Minnesota.

Grantham-McGregor, Sally, Akanksha Adya, Orazio Attanasio, Britta Augsburg, Jere Behrman, Bet Caeyers, Monimalika Day, Pamela Jervis, Reema Kochar, and Prerna Makkar. 2020. "Group sessions or home visits for early childhood development in India: a cluster RCT." *Pediatrics*, 146(6).

Grantham-McGregor, Sally, Christine Powell, Susan Walker, and John Himes. 1991. "Nutritional Supplementation, Psychosocial Stimulation, and Mental Development of Stunted Children: The Jamaican Study." *The Lancet*, 338(8758): 1-5.

Grantham-McGregor, Sally, Yin Bun Cheung, Santiago Cueto, Paul Glewwe, Linda Richter, and Barbara Strupp. 2007. "Development Potential in the First 5 Years for Children in Developing Countries." *Lancet* 369 (9555): 60–70.

Haddad, Lawrence, John Hoddinott, and Harold Alderman. 1997. *Intrahousehold Resource Allocation in Developing Countries: Methods, Models, and Policy*. Johns Hopkins University Press: Baltimore, MD.

Handa, Sudhanshu, David Seidenfeld, Benjamin Davis, Gelson Tembo, and the Zambia Cash Transfer Evaluation Team. 2016. "The Social and Productive Impacts of Zambia's Child Grant." *Journal of Policy Analysis and Management* 35 (2): 357–87.

Hanna, R. and Olken, B.A., 2018. "Universal basic incomes versus targeted transfers: Anti-poverty programs in developing countries." *Journal of Economic Perspectives*, *32*(4): 201-26.

Haushofer, Johannes and Jeremy Shapiro. 2016. "The Short-Term Impact of Unconditional Cash Transfers to the Poor: Experimental Evidence from Kenya." *Quarterly Journal of Economics*, 131(4): 1973-2042.

Heckman, James J., and Ganesh Karapakula. 2019. "Intergenerational and Intragenerational Externalities of the Perry Preschool Project." NBER Working Paper 25889.

Heckman, James J., Seong Hyeok Moon, Rodrigo Pinto, Peter A. Savelyev, and Adam Yavitz. 2010. "The Rate of Return to the HighScope Perry Preschool Program." *Journal of Public Economics* 94 (1–2): 114–28.

Hoddinott, John, Jere R. Behrman, John A. Maluccio, Paul Melgar, Agnes R. Quisumbing, Manuel Ramirez-Zea, Aryeh D. Stein, Kathryn M. Yount, and Reynaldo Martorell. 2013. "Adult Consequences of Growth Failure in Early Childhood." *American Journal of Clinical Nutrition* 98 (5): 1170–78.

Hoddinott, John, John A. Maluccio, Jere R. Behrman, Rafael Flores, and Reynaldo Martorell. 2008. "Effect of a Nutrition Intervention During Early Childhood on Economic Productivity in Guatemalan Adults." *Lancet* 371 (9610): 411–16.

Institut National de la Statistique et de la Démographie (INSD) and ICF International, 2012. *Enquête Démographique et de Santé et à Indicateurs Multiples du Burkina Faso 2010*. Calverton, Maryland, USA.

Jakiela, Pamela, and Owen Ozier. 2016. "Does Africa Need a Rotten Kin Theorem? Experimental Evidence from Village Economies." *Review of Economic Studies* 83 (1): 231–68.

Jensen, Robert. 2010. "The (Perceived) Returns to Education and the Demand for Schooling." *Quarterly Journal of Economics*, 125(2): 515-548.

Justino, Patricia, Marinella Leone, Pierfrancesco Rolla, Monique Abimpaye, Caroline Dusabe, Marie Uwamahoro, Richard Germond. 2023. Improving Parenting Practices for Early Child Development: Experimental Evidence from Rwanda." *Journal of the European Economic Association*, 21(4): 1510-1550.

Kandpal, Eeshani, Harold Alderman, Jed Friedman, Deon Filmer, Junko Onishi, and Jorge Avalos. 2016. "A Conditional Cash Transfer Program in the Philippines Reduces Severe Stunting." *Journal of Nutrition* 146 (9): 1793-800.

Kazianga, Harounan and Zaki Wahhaj. 2013. "Gender, Social Norms and Household Production in Burkina Faso". *Economic Development and Cultural Change*. 61(3): 539-576.

Kazianga, Harounan, Damien de Walque, and Harold Alderman. 2012. "Educational and Child Labor Impacts of Two Food for Education Schemes: Evidence from a Randomized Trial in Rural Burkina Faso." *Journal of African Economies*, 21(5): 723-760.

Kazianga, Harounan, Damien de Walque, and Harold Alderman. 2014. "School Feeding Programs, Intrahousehold Allocation and the Nutrition of Siblings: Evidence from a Randomized Trial in Rural Burkina Faso." *Journal of Development Economics*, 106: 15-34.

Kling, Jeffrey R., Jeffrey B. Liebman, and Lawrence F. Katz. 2007. "Experimental Analysis of Neighborhood Effects." Econometrica, 75(1): 83-119.

Kremer, Michael, and Rachel Glennerster. 2012. "Improving Health in Developing Countries: Evidence from Randomized Evaluations." In *Handbook of Health Economics*. Vol. 2,

edited by Mark V. Pauly, Thomas G. McGuire, and Pedro Pita Barros, 201–316. New York: Elsevier.

Lagarde, Mylene, Andy Haines, and Natasha Palmer. 2007. "Conditional Cash Transfers for Improving Uptake of Health Interventions in Low- and Middle-Income Countries: A Systematic Review." *JAMA*, 298(16): 1900–1910.

Levere, Michael, Gayatri Acharya, and Prashant Bharadwaj. 2024. "The Role of Information and Cash Transfers on Early Childhood Development: Short- and Long-Run Evidence from Nepal." *Economic Development and Cultural Change*, 72(3): 1267-1293.

Lundberg, Shelly, Robert Pollak, and Terence Wales. 1997. "Do Husbands and Wives Pool Their Resources? Evidence from the United Kingdom Child Benefit." *Journal of Human Resources*, 32(3): 463-480.

Macours, Karen, Norbert Schady, and Reno Vakis. 2012. "Cash Transfers, Behavioral Changes, and the Cognitive Development of Young Children: Evidence from a Randomized Experiment." *American Economic Journal: Applied Economics*, 4(2): 247-73.

Maluccio, John and Rafael Flores. 2005. "Impact Evaluation of the Pilot Phase of the Nicaraguan *Red de Proteción Social*." International Food and Policy Research Institute, Food Consumption and Nutrition Division Discussion Paper 141.

Manley, James, Seth Gitter, and Vanya Slavchevska. 2013. "How Effective Are Cash Transfers at Improving Nutritional Status?" *World Development* 48: 133–55.

Martinelli, Cesar and Susan Parker. 2003. "Should Transfers to Poor Families be Conditional on School Attendance? A Household Bargaining Perspective" *International Economic Review*, 44(2): 523-544.

McIntosh, Craig, and Andrew Zeitlin. 2018. "Benchmarking a Child Nutrition Program against Cash: Experimental Evidence from Rwanda." Unpublished.

Miguel, Edward and Michael Kremer. 2004. "Worms: Identifying Impacts on Education and Health in the Presence of Treatment Externalities." *Econometrica*, 72(1): 159-217.

Ministère de la Santé. 2020. Enquête Nutritionnelle Nationale 2019, Burkina Faso. Burkina Faso: Ministère de la Santé.

Nguyen, Trang. 2008. "Information, Role Models and Perceived Returns to Education: Experimental Evidence from Madagascar." Unpublished manuscript.

Okeke, Edward N., and Isa S. Abubakar. 2020. "Healthcare at the Beginning of Life and Child Survival: Evidence from a Cash Transfer Experiment in Nigeria." *Journal of Development Economics* 143: 102426.

Olken, Benjamin, Junko Onishi, and Susan Wong. 2014. "Should Aid Reward Performance? Evidence from a Field Experiment on Health and Education in Indonesia." *American Economic Journal: Applied Economics*, 6(4): 1-34.

Paxson, Christina, and Norbert Schady. 2010. "Does Money Matter? The Effects of Cash Transfers on Child Development in Rural Ecuador." *Economic Development and Cultural Change* 59 (1): 187–229.

Raven, John E., John C. Raven, and John H. Court. 1998. *Manual for Raven's Progressive Matrices and Vocabulary Scales: Section 1 General Overview*. Oxford: Oxford Psychologists Press.

Richter, Linda M., Bernadette Daelmans, Joan Lombardi, Jody Heymann, Florencia Lopez Boo, Jere R. Behrman, Chunling Lu, et al. 2017. "Investing in the Foundation of Sustainable Development: Pathways to Scale Up for Early Childhood Development." *Lancet* 389 (100064): 103–18.

Robertson, Laura, Phyllis Mushati, Jeffrey Eaton, Lovemore Dumba, Gideon Mavise, Jeremiah Makoni, Christina Schumacher, Tom Crea, Roeland Monasch, Lorraine Sherr, Geoffrey Garnett, Constance Nyamukapa, and Simon Gregson. 2013. "Effects of Unconditional and Conditional Cash Transfers on Child Health and Development in Zimbabwe: A Cluster-Randomized Trial." *The Lancet*, 381(9874): 1283-1292.

Ruel, Marie T., Harold Alderman, and Maternal and Child Nutrition Study Group. 2013. "Nutrition-Sensitive Interventions and Programmes: How Can They Help to Accelerate Progress in Improving Maternal and Child Nutrition?" *Lancet,* 382(9891): 536–51.

Samson, Michael. 2006. "Are Conditionalities Necessary for Human Development." Presentation at the Third International Conference on Conditional Cash Transfers, Istanbul, Turkey, June 26-30.

Schady, Norbert and Maria Caridad Araujo. 2008. "Cash Transfers, Conditions, and School Enrollment in Ecuador." *Economía*,8: 43-70.

Schubert, Bernd and Rachel Slater. 2006. "Social Cash Transfers in Low-Income African Countries: Conditional or Unconditional?" *Development Policy Review*, 24(5): 571-578.

Schultz, T. Paul. 2004. "School Subsidies for the Poor: Evaluating the Mexican Progresa Poverty Program." *Journal of Development Economics* 74(1): 199-250.

Sridhar, Devi, and Arabella Duffield. 2006. *A Review of the Impact of Cash Transfer Programmes on Child Nutritional Status and Some Implications for Save the Children UK Programmes*. London: Save the Children UK.

Strauss, John and Duncan Thomas. 1995. “Human Resources: Empirical Modeling of Household and Family Decisions.” In *Handbook of Development Economics*, T.N. Srinivasan and J. Behrman, editors. North Holland: Amsterdam.

Strauss, John, Germano Mwabu, and Kathleen Beegle. 2000. “Intrahousehold Allocations: A Review of Theories and Empirical Evidence.” *Journal of African Economies*, 9(0), Supplement 1, 83-143.

Svedberg, Peter, 1990. “Undernutrition in sub-Saharan Africa: Is There a Gender Bias?” *Journal of Development Studies*, 26(3): 469–486.

Thomas, Duncan. 1990. “Intrahousehold Resource Allocation: An Inferential Approach.” *Journal of Human Resources*, 25(4): 635–664.

United Nations Children’s Fund (UNICEF). 2018. *Maternal and Newborn Health Disparities Burkina Faso.*

United Nations Development Program. (2024). *Country insights*. Human Development Reports. https://hdr.undp.org/data-center/country-insights#/ranks

Wamani, Henry, Anne Nordrehaug Astrom, Stefan Peterson, James Tumwine, Thorkild Tylleskar. 2007. “Boys Are More Stunted than Girls in sub-Saharan Africa: A Meta-Analysis of 16 Health and Demographic Surveys.” *BMC Pediatrics*, 7(17).

Yoong, Joanne, Lila Rabinovich, and Stephanie Diepeveen. 2012. “The Impact of Economic Resource Transfers to Women Versus Men: A Systematic Review.” Technical Report. EPPI-Center, Social Science Research Unit, Institute of Education, University of London.

Young, Alwyn. 2019. “Channeling Fisher: Randomization Tests and the Statistical Insignificance of Seemingly Significant Experimental Results.” *Quarterly Journal of Economics*, 134(2): 557-598.

Yousafzai, Aisha, Jelena Obradović, Muneera Rasheed, Arjumand Rizvi, Ximena Portilla, Nicole Tirado-Strayer, Saima Siyal, and Uzma Memon. 2016. “Effects of Responsive Stimulation and Nutrition Interventions on Children's Development and Growth at Age 4 Years in a Disadvantaged Population in Pakistan: A Longitudinal Follow-up of a Cluster-Randomized Factorial Effectiveness Trial.” *The Lancet Global Health*, 4(8): e548-e558.

Zeba Augustin, Hélène F. Delisle, Clémentine Rossier, Genevieve Renier. 2013. “Association of High-Sensitivity C-Reactive Protein with Cardiometabolic Risk Factors and Micronutrient Deficiencies in Adults of Ouagadougou, Burkina Faso.” *British Journal of Nutrition*, 109(7): 1266-1275.

Figure 1: Summary of Treatment and Control Group Randomization Plan

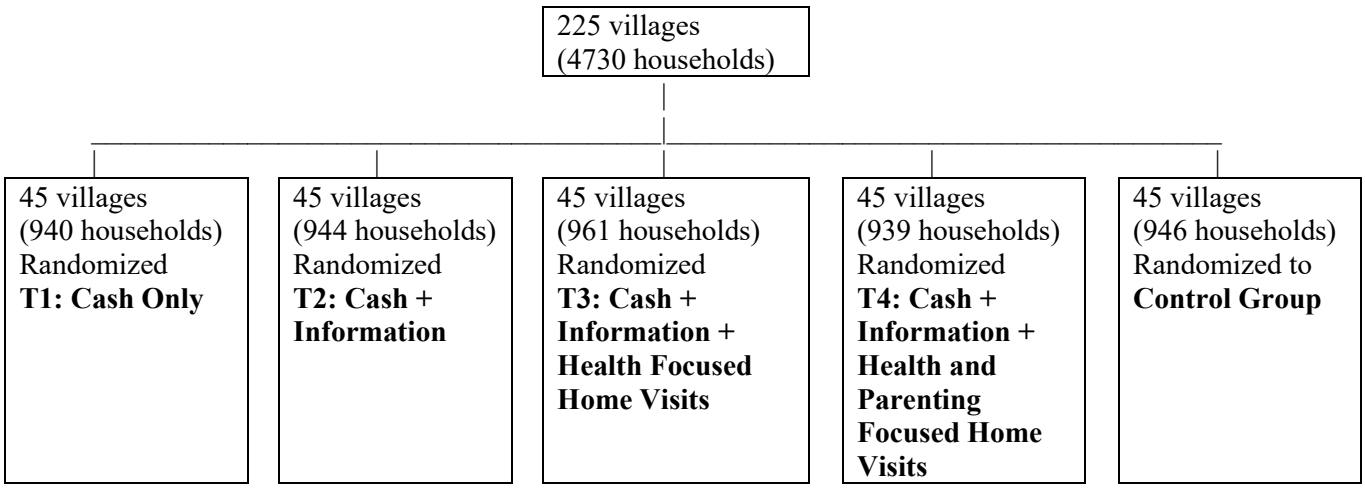

Table 1. Baseline Village and Household Characteristics

Panel A. Village Characteristics		
	Mean	**Standard deviation**
Village population	2566	2189
Health center in village	0.15	0.36
Distance to nearest health center (in km)	5.11	4.27
Distance to nearest market (in km)	4.72	4.98
Primary school in village	0.91	0.29
Secondary school in village	0.20	0.4
Distance to nearest secondary school (in km)	6.53	5.6
Distance to nearest paved road (in km)	21.53	15.96
Number of months roads to the village are impassable	3.46	1.91
Number of villages	74	
Panel B. Household Characteristics		
	Mean	**Standard deviation**
Household head male	0.80	0.40
Household head age	45.96	12.68
Household head any education	0.09	0.29
Household size	9.25	4.09
# spouses	1.22	0.82
# children under 15	5.00	2.58
# other household members	2.03	3.29
Household head married	0.92	0.27
Household head monogamous	0.46	0.50
Household head polygamous	0.46	0.50
Household head Gourmanche	0.10	0.29
Household head Mossi	0.43	0.50
Household Head Peulh	0.06	0.24
Household head Yana	0.21	0.41
Household head Catholic or Protestant	0.29	0.45
Household head Muslim	0.70	0.46
Household head animist or no religion	0.02	0.13
Number of households	1513	

Notes: The sample includes the 74 villages and 1513 households surveyed at endline. Panel A shows the village characteristics. Panel B shows household characteristics. # of children under 15 includes only biological children of the household head. # other household members includes members who are not either the spouse or a biological child of the household head (of any age).

Table 2. Baseline Balance, All 225 Villages

	Control	T1	T1=control	T2	T2=control	T3/T4	T3/T4=control	All equal
Household Characteristics	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Male	0.88	0.84	0.07	0.85	0.10	0.84	0.03	0.11
Age	45.92	43.82	0.01	43.94	0.04	44.38	0.05	0.07
Any education	0.06	0.06	0.60	0.08	0.14	0.08	0.05	0.18
Household size	10.07	8.87	0.00	8.94	0.00	8.90	0.00	0.00
# spouses	1.36	1.29	0.17	1.26	0.06	1.29	0.09	0.21
# children	5.40	5.22	0.31	5.17	0.21	5.23	0.32	0.65
# other members	2.30	1.35	0.00	1.51	0.01	1.38	0.00	0.01
Married	0.93	0.94	0.24	0.93	0.50	0.95	0.02	0.13
Monogamous	0.45	0.45	0.77	0.49	0.24	0.47	0.55	0.48
Polygamous	0.47	0.49	0.43	0.45	0.37	0.48	0.70	0.46
Gourmanche ethnicity	0.35	0.35	0.98	0.36	0.92	0.35	0.99	1.00
Mossi ethnicity	0.15	0.20	0.43	0.20	0.49	0.21	0.31	0.76
Peulh ethnicity	0.14	0.09	0.16	0.08	0.04	0.11	0.31	0.21
Yana ethnicity	0.25	0.27	0.72	0.29	0.56	0.26	0.80	0.95
Catholic or Protestant	0.27	0.28	0.99	0.33	0.21	0.31	0.39	0.53
Muslim	0.57	0.57	0.92	0.52	0.54	0.55	0.85	0.89
Animist or no religion	0.16	0.15	0.82	0.14	0.73	0.14	0.55	0.94
Prenatal/Health								
# prenatal visits	4.29	4.38	0.50	4.35	0.68	4.31	0.85	0.91
Mother receiving vitamin A	0.77	0.77	0.84	0.81	0.29	0.78	0.76	0.65
Mother receiving iron	0.89	0.90	0.55	0.89	0.69	0.88	0.76	0.77
Child receiving vitamin A	0.50	0.47	0.36	0.52	0.64	0.51	0.80	0.54
Child fully vaccinated	0.39	0.38	0.84	0.36	0.55	0.40	0.59	0.53
Mother washes hands after toilet	0.82	0.80	0.53	0.80	0.51	0.81	0.50	0.90
Anthropometrics								
Height-for-age z	-1.39	-1.28	0.22	-1.32	0.39	-1.32	0.41	0.66
Arm circumference-for-age z	-1.02	-1.08	0.33	-1.11	0.16	-1.13	0.04	0.23
Weight-for-age z	-1.25	-1.19	0.37	-1.27	0.80	-1.26	0.86	0.66
Education								
Enrollment	0.44	0.43	0.88	0.45	0.82	0.43	0.70	0.94
Years of school completed	1.67	1.71	0.77	1.72	0.78	1.58	0.56	0.77
Any school completed	0.48	0.49	0.84	0.48	0.94	0.47	0.86	0.98
Progression	0.44	0.45	0.89	0.45	0.87	0.43	0.66	0.92
Read/write in French	0.28	0.29	0.76	0.29	0.59	0.27	0.79	0.85

Denver								
Total score	0.42	0.42	0.91	0.41	0.95	0.41	0.70	0.96
Language	0.39	0.39	0.66	0.39	0.92	0.38	0.59	0.77
Fine motor	0.40	0.40	0.97	0.40	1.00	0.41	0.91	1.00
Gross motor	0.47	0.47	0.81	0.47	0.89	0.47	0.87	0.98
Personal-social	0.43	0.42	0.79	0.42	0.78	0.42	0.67	0.98
HTKS								
Official score	8.63	9.14	0.54	8.57	0.94	9.16	0.45	0.72
Score T1, P1, E1	6.36	6.71	0.50	6.45	0.87	6.80	0.34	0.70
Total over 0	0.75	0.78	0.39	0.76	0.69	0.79	0.19	0.57
Strengths & Difficulties								
Total difficulties score in avg range	0.28	0.34	0.05	0.31	0.38	0.32	0.10	0.18
Total z-score under -0.5	0.29	0.34	0.09	0.31	0.56	0.32	0.15	0.30
Prosocial in avg range	0.22	0.16	0.08	0.17	0.13	0.17	0.06	0.24
Prosocial z-score over 0.5	0.33	0.27	0.10	0.28	0.11	0.27	0.05	0.22

Notes: Calculations in this table are based on baseline data from all 225 villages. Standard errors are clustered at the village level. The treatment arms are abbreviated as "T1" (cash only), "T2" (cash + information), "T3/T4" (cash + information + home visits). Column 1 presents the baseline mean in the control group; columns 2, 4, and 6 present the baseline means for each of the treatment arms; columns 3, 5, and 7 present p-values from a test of mean equality between the control arm and each of the respective treatment arms; and column 8 shows p-values for an F-test for equality across all four groups. Outcomes from the Prenatal/Health section are reported at the household level: "# of prenatal visits" is calculated as the average number of prenatal visits in for children ages 0-48 months in the household. "Mother vitamin A" is the percentage of pregnancies during which mothers of children ages 0-48 received vitamin A. "Mother iron" is the percentage of pregnancies during which mothers of children ages 0-48 months received iron. "Child vitamin A" is the percentage of children ages 0-48 months in the household who received any vitamin A in the past 12 months. "Child fully vaccinated" is the percentage of children in the household who have received all 10 recommended vaccines. "Mother washes hands after toilet" is the percentage of women in the household who report washing their hands after using the toilet. Anthropometrics outcomes are reported at the individual level for children less than 60 months, and z-scores are calculated using the WHO reference population. Outcomes from the Education section are reported at the individual level, for children who are at least 6 years old and less than 15 years old. "Enrollment" is an indicator for the child being enrolled in school, "years of school" is the number of years of school that the child has completed, "any schooling" is an indicator for completing any years of schooling, "progression" is the actual grade divided by the expected grade (where children are expected to begin school at age 7) and capped at 1, and "read/write French" is an indicator for the individual being able to read and write in French. Outcomes in the Denver section are reported at the individual level for children ages 0-71 months old. "Total Score" is the percentage of questions that a child answered correctly, with a reference population of children in the same age group. The denominator is based on the number of questions that 90% of children in that age group are able to answer correctly. Language, Fine Motor, Gross Motor, and Personal-Social are also percentages for the questions answered correctly in each of the respective categories. HTKS outcomes are reported at the individual level for children 36-71 months old. "Official Score" is the total score for all sections, with full credit given if children self-correct an incorrect response. "Score T1, P1, E1" is the score on the first three sections, and "Total Over 0" is an indicator for having an official score greater than 0 in the first three sections. In the Strengths & Difficulties section, which is reported at the individual level for children ages 24-71 months old, "Total difficulties score in avg range" is an indicator for the total strengths & difficulties score being in the normal range, "Total z-score under -0.5" is an indicator for the total score being at least 0.5 SD below the mean (where a higher score is worse), "Prosocial in avg range" is an indicator for the prosocial score being in the normal range, and "Prosocial z-score over 0.5" is an indicator for the prosocial score being at least 0.5 SD above the mean (where a higher score is better).

Table 3. Impacts on Fertility and Pregnancy

	# times pregnant	Age at first pregnancy	Age at 2nd pregnancy	Age at 3rd pregnancy	Age at 4th pregnancy	Age at 5th pregnancy	Indicator for last birth assisted by medical professional	Pregnancy index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Cash Arm	0.022	-0.012	-0.012	0.059	0.337	0.379	0.008	0.047
	(0.127)	(0.219)	(0.227)	(0.221)	(0.277)	(0.406)	(0.021)	(0.067)
	[0.865]	[0.941]	[0.967]	[0.817]	[0.228]	[0.329]	[0.705]	[0.488]
Cash + Info Arm	0.035	-0.017	-0.117	-0.134	0.145	0.532	-0.003	-0.019
	(0.119)	(0.191)	(0.206)	(0.215)	(0.279)	(0.422)	(0.020)	(0.055)
	[0.773]	[0.933]	[0.590]	[0.572]	[0.609]	[0.233]	[0.896]	[0.746]
Cash + Info + Home Visits Arm	-0.294**	0.305	0.380*	0.498**	0.943***	1.052**	0.035*	0.199***
	(0.117)	(0.202)	(0.209)	(0.233)	(0.292)	(0.461)	(0.018)	(0.055)
	[0.016]	[0.146]	[0.082]	[0.037]	[0.005]	[0.033]	[0.059]	[0.002]
Observations	1,054	1,054	991	921	818	694	1,036	1,054
Control Group Mean	4.63	18.69	21.55	24.13	26.48	28.92	0.96	0.00

Notes: These outcomes are aggregated and regressions are run at the household level. Mothers with children under age 5 at the time of the endline are included in the regression. "Cash arm" is an indicator for the household being in a village that is assigned to treatment arm 1, "cash + info arm" is an indicator for the household being in a village that is assigned to treatment arm 2, and "cash + info + home visits arm" is an indicator for the household being in a village that is assigned to either treatment arm 3 or treatment arm 4. The pregnancy index in column 8 is a standardized index across the outcomes in columns 1-7. Standard errors are shown in parenthesis, and randomization inference p-values are shown in brackets below each coefficient estimate. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

Table 4. Impacts on Household Health Behaviors

	# prenatal visits	Mother receiving vitamin A during pregnancy	Mother receiving iron during pregnancy	Child receiving vitamin A	Child fully vaccinated	Mother washes hands after using toilet	Health behavior index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Cash Arm	0.248	-0.028	-0.019	0.008	-0.021	-0.002	0.013
	(0.164)	(0.029)	(0.016)	(0.042)	(0.052)	(0.021)	(0.028)
	[0.135]	[0.334]	[0.241]	[0.844]	[0.684]	[0.895]	[0.621]
Cash + Info Arm	0.164	-0.031	-0.040*	-0.004	-0.080	-0.004	-0.015
	(0.148)	(0.030)	(0.021)	(0.037)	(0.059)	(0.020)	(0.042)
	[0.295]	[0.339]	[0.062]	[0.922]	[0.173]	[0.831]	[0.707]
Cash + Info + Home Visits Arm	0.200	-0.003	-0.010	0.070*	-0.009	0.035**	0.097***
	(0.147)	(0.026)	(0.018)	(0.035)	(0.054)	(0.015)	(0.028)
	[0.184]	[0.932]	[0.594]	[0.064]	[0.866]	[0.025]	[0.003]
Observations	996	1,019	1,019	959	1,020	1,147	1,188
Control Group Mean	4.64	0.85	0.94	0.55	0.51	0.94	0.14

Notes: These outcomes are aggregated and regressions are run at the household level. The outcomes in columns 1-5 are measured for each child ages 0-47 months in the households and aggregated to the household level. The outcome in column 6 is asked to all mothers of children under age 5 at the time of the endline and are averaged to the household level. "Cash arm" is an indicator for the household being in a village that is assigned to treatment arm 1, "cash + info arm" is an indicator for the household being in a village that is assigned to treatment arm 2, and "cash + info + home visits arm" is an indicator for the household being in a village that is assigned to either treatment arm 3 or treatment arm 4. For variable definitions, see Table 2. The health behavior index in column 7 is a standardized index across the outcomes in columns 1-6. Standard errors are shown in parenthesis, and randomization inference p-values are shown in brackets below each coefficient estimate. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

Table 5. Impacts on Anthropometrics

	Height-for-age z-score	Arm circumference-for-age z-score	Weight-for-age z-score	Anthropometrics index
	(1)	(2)	(3)	(4)
Cash Arm	-0.125	0.267***	0.163*	0.101
	(0.086)	(0.094)	(0.092)	(0.063)
	[0.155]	[0.004]	[0.080]	[0.123]
Cash + Info Arm	-0.133	-0.000	-0.014	-0.030
	(0.089)	(0.089)	-0.100	(0.068)
	[0.138]	[0.996]	[0.888]	[0.657]
Cash + Info + Home Visits Arm	-0.001	0.149*	0.085	0.042
	(0.084)	(0.080)	(0.084)	(0.059)
	[0.991]	[0.067]	[0.325]	[0.478]
Observations	1,944	1,870	1,965	1,967
Control Group Mean	-1.14	-1.11	-1.21	-0.03

Notes: These regressions are run at the individual level for children ages 0-59 months. "Cash arm" is an indicator for the household being in a village that is assigned to treatment arm 1, "cash + info arm" is an indicator for the household being in a village that is assigned to treatment arm 2, and "cash + info + home visits arm" is an indicator for the household being in a village that is assigned to either treatment arm 3 or treatment arm 4. Z-scores are calculated using the WHO reference population. The anthropometrics index in column 4 is a standardized index across the outcomes in columns 1-3. Standard errors are shown in parenthesis, and randomization inference p-values are shown in brackets below each coefficient estimate. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

Table 6. Impacts on Education Outcomes

	Enrolled in school	Years of school completed	Any school completed	Progression (completed/expected)	Read/write in French	Education index
	(1)	(2)	(3)	(4)	(5)	(6)
Cash Arm	0.042	0.160	0.087**	0.037	-0.012	0.035
	(0.032)	(0.158)	(0.037)	(0.036)	(0.036)	(0.075)
	[0.211]	[0.324]	[0.027]	[0.320]	[0.751]	[0.636]
Cash + Info Arm	0.018	0.084	0.032	0.011	-0.037	-0.018
	(0.032)	(0.135)	(0.032)	(0.033)	(0.029)	(0.062)
	[0.591]	[0.515]	[0.297]	[0.724]	[0.195]	[0.755]
Cash + Info + Home Visits Arm	0.078***	0.313***	0.085***	0.065**	0.010	0.102*
	(0.024)	(0.117)	(0.026)	(0.026)	(0.026)	(0.053)
	[0.002]	[0.011]	[0.004]	[0.019]	[0.702]	[0.067]
Observations	5,185	5,171	5,165	4,627	5,763	5,763
Control Group Mean	0.46	2.17	0.57	0.52	0.32	0.05

Notes: These regressions are run at the individual level for children at least 6 years old and less than 15 years old. "Cash arm" is an indicator for the household being in a village that is assigned to treatment arm 1, "cash + info arm" is an indicator for the household being in a village that is assigned to treatment arm 2, and "cash + info + home visits arm" is an indicator for the household being in a village that is assigned to either treatment arm 3 or treatment arm 4. For variable definitions, see Table 2. The education index in column 6 is a standardized index across the outcomes in columns 1-5. Standard errors are shown in parenthesis, and randomization inference p-values are shown in brackets below each coefficient estimate. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

Table 7. Impacts on Denver Early Childhood Development Assessment

Panel A: Separate Treatment Specification

	Total score	Language	Fine motor	Gross motor	Personal social
	(1)	(2)	(3)	(4)	(5)
Cash Arm	-0.008	-0.004	-0.018	-0.018	-0.027
	(0.030)	(0.026)	(0.036)	(0.033)	(0.030)
	[0.799]	[0.870]	[0.653]	[0.617]	[0.381]
Cash + Info Arm	-0.038	-0.039*	-0.032	-0.053*	-0.023
	(0.026)	(0.022)	(0.030)	(0.030)	(0.029)
	[0.148]	[0.087]	[0.274]	[0.089]	[0.434]
Cash + Info + Home Visits Arm	0.016	0.014	0.006	0.014	0.029
	(0.025)	(0.022)	(0.029)	(0.028)	(0.027)
	[0.518]	[0.525]	[0.824]	[0.633]	[0.303]
Observations	2,456	2,456	2,456	2,456	2,456
Control Group Mean	0.46	0.44	0.48	0.52	0.48

Panel B: Cumulative Treatment Specification

	Total score	Language	Fine motor	Gross motor	Personal social
Cash	-0.008	-0.004	-0.018	-0.018	-0.027
	(0.030)	(0.026)	(0.036)	(0.033)	(0.030)
	[0.799]	[0.870]	[0.653]	[0.617]	[0.381]
+ Info	-0.03	-0.034	-0.015	-0.035	0.004
	(0.028)	(0.023)	(0.033)	(0.029)	(0.030)
	[0.292]	[0.139]	[0.646]	[0.250]	[0.881]
+ Home Visits	0.054**	0.0528***	0.039	0.067***	0.052*
	(0.022)	(0.0180)	(0.025)	(0.024)	(0.028)
	[0.021]	[0.004]	[0.124]	[0.004]	[0.063]
Observations	2,456	2,456	2,456	2,456	2,456
Pvalue for Cash+Info+HV	0.52	0.52	0.83	0.61	0.29
Control Group Mean	0.46	0.44	0.48	0.52	0.48

Notes: These regressions are run at the individual level for children ages 0-71 months. Panel A shows the main specification with the estimates for the separate treatment arms. "Cash arm" is an indicator for the household being in a village that is assigned to treatment arm 1, "cash + info arm" is an indicator for the household being in a village that is assigned to treatment arm 2, and "cash + info + home visits arm" is an indicator for the household being in a village that is assigned to either treatment arm 3 or treatment arm 4. Panel B shows the estimates from the cumulative specification. "Cash" is an indicator for being assigned to receive cash (T1, T2, T3, T4), "+ Info" is an indicator for being assigned to receive information (T2, T3, T4), and "+ Home Visits" is an indicator for being assigned to receive home visits (T3, T4). For variable definitions, see Table 2. Standard errors are shown in parenthesis, and randomization inference p-values are shown in brackets below each coefficient estimate. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

Table 8. Impacts on Head, Toes, Knees, and Shoulders (HTKS) Assessment

Panel A: Separate Treatment Specification

	Official Score	Score on training 1, practice 1, exam 1	Scored over 0 on T1, P1, E1	HTKS index
	(1)	(2)	(3)	(4)
Cash Arm	-1.421*	-0.900*	-0.0111	-0.120
	(0.776)	(0.464)	(0.0307)	(0.0737)
	[0.077]	[0.062]	[0.736]	[0.109]
Cash + Info Arm	-1.865*	-1.126*	-0.071**	-0.199**
	(0.959)	(0.574)	(0.035)	(0.094)
	[0.046]	[0.049]	[0.047]	[0.033]
Cash + Info + Home Visits Arm	-0.275	-0.061	0.022	0.002
	(0.747)	(0.474)	(0.026)	(0.074)
	[0.708]	[0.887]	[0.418]	[0.977]
Observations	1,261	1,261	1,261	1,261
Control Group Mean	13.25	9.00	0.81	0.35

Panel B: Cumulative Treatment Specification

	Official Score	Score on training 1, practice 1, exam 1	Scored over 0 on T1, P1, E1	HTKS index
Cash	-1.421*	-0.900*	-0.011	-0.120
	(0.776)	(0.464)	(0.031)	(0.074)
	[0.077]	[0.062]	[0.736]	[0.109]
+ Info	-0.445	-0.226	-0.060*	-0.079
	(0.962)	(0.526)	(0.034)	(0.088)
	[0.652]	[0.673]	[0.082]	[0.370]
+ Home Visits	1.590*	1.065**	0.093***	0.201**
	(0.909)	(0.511)	(0.029)	(0.084)
	[0.083]	[0.039]	[0.001]	[0.018]
Observations	1,261	1,261	1,261	1,261
Pvalue for Cash+Info+HV	0.71	0.90	0.41	0.97
Control Group Mean	13.25	9.00	0.81	0.35

Notes: These regressions are run at the individual level for children ages 36-71 months. Panel A shows the main specification with the estimates for the separate treatment arms. "Cash arm" is an indicator for the household being in a village that is assigned to treatment arm 1, "cash + info arm" is an indicator for the household being in a village that is assigned to treatment arm 2, and "cash + info + home visits arm" is an indicator for the household being in a village that is assigned to either treatment arm 3 or treatment arm 4. Panel B shows the estimates from the cumulative specification. "Cash" is an indicator for being assigned to receive cash (T1, T2, T3, T4), "+ Info" is an indicator for being assigned to receive information (T2, T3, T4), and "+ Home Visits" is an indicator for being assigned to receive home visits (T3, T4). For variable definitions, see Table 2. The HTKS index in column 4 is a standardized index across columns 1-3. Standard errors are shown in parenthesis, and randomization inference p-values are shown in brackets below each coefficient estimate. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

Table 9. Impacts on Strengths and Difficulties Assessment

Panel A: Separate Treatment Specification

	Total difficulties in average range	Total z-score under -0.5	Prosocial in average range	Prosocial z-score over 0.5	Strengths & difficulties index
	(1)	(2)	(3)	(4)	(5)
Cash Arm	0.079	0.081	0.040*	0.032	0.131**
	(0.052)	(0.051)	(0.023)	(0.035)	(0.064)
	[0.142]	[0.128]	[0.106]	[0.368]	[0.061]
Cash + Info Arm	0.023	0.035	0.023	0.002	0.049
	(0.052)	(0.054)	(0.019)	(0.031)	(0.066)
	[0.667]	[0.536]	[0.245]	[0.949]	[0.491]
Cash + Info +	0.026	0.038	0.059***	0.058*	0.100*
Home Visits Arm	(0.045)	(0.048)	(0.015)	(0.031)	(0.053)
	[0.558]	[0.404]	[0.000]	[0.048]	[0.069]
Observations	1,655	1,655	1,655	1,655	1,655
Control Group Mean	0.28	0.28	0.06	0.17	-0.18

Panel B: Cumulative Treatment Specification

	Total difficulties in average range	Total z-score under -0.5	Prosocial in average range	Prosocial z-score over 0.5	Strengths & difficulties index
Cash	0.079	0.081	0.040*	0.032	0.131**
	(0.052)	(0.051)	(0.023)	(0.035)	(0.064)
	[0.142]	[0.128]	[0.106]	[0.368]	[0.061]
+ Info	-0.056	-0.046	-0.017	-0.03	-0.082
	(0.054)	(0.054)	(0.024)	(0.032)	(0.075)
	[0.306]	[0.401]	[0.488]	[0.357]	[0.293]
+ Home Visits	0.004	0.003	0.036*	0.056*	0.052
	(0.047)	(0.048)	(0.019)	(0.028)	(0.061)
	[0.947]	[0.948]	[0.044]	[0.040]	[0.400]
Observations	1,655	1,655	1,655	1,655	1,655
Pvalue, Cash+Info+HV	0.56	0.42	0.00	0.06	0.06
Control Group Mean	0.28	0.28	0.06	0.17	-0.18

Notes: These regressions are run at the individual level for children ages 24-71 months. Panel A shows the main specification with the estimates for the separate treatment arms. "Cash arm" is an indicator for the household being in a village that is assigned to treatment arm 1, "cash + info arm" is an indicator for the household being in a village that is assigned to treatment arm 2, and "cash + info + home visits arm" is an indicator for the household being in a village that is assigned to either treatment arm 3 or treatment arm 4. Panel B shows the estimates from the cumulative specification. "Cash" is an indicator for being assigned to receive cash (T1, T2, T3, T4), "+ Info" is an indicator for being assigned to receive information (T2, T3, T4), and "+ Home Visits" is an indicator for being assigned to receive home visits (T3, T4). For variable definitions, see Table 2. The strengths and difficulties index in column 5 is a standardized index across columns 1-4. Standard errors are shown in parenthesis, and randomization inference p-values are shown in brackets below each coefficient estimate. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

Table 10. Impact on Receipt of Cash Transfers

	Any transfers	Any mobile phones	Amount of last transfer	Total amount received
	(1)	(2)	(3)	(4)
Cash Arm	0.943***	0.947***	38,037***	522,752***
	(0.014)	(0.014)	(1,520)	(16,208)
	[0.000]	[0.000]	[0.000]	[0.000]
Cash + Info Arm	0.944***	0.945***	39,990***	558,144***
	(0.015)	(0.016)	(1,319)	(19,192)
	[0.001]	[0.001]	[0.001]	[0.001]
Cash + Info + Home Visits Arm	0.964***	0.968***	40,308***	563,538***
	(0.009)	(0.009)	(1,260)	(15,733)
	[0.000]	[0.000]	[0.000]	[0.000]
Observations	1,512	1,513	1,509	1,513
Control Group Mean	0.01	0.00	405	3366

Notes: These outcomes are reported at the household level. "Cash arm" is an indicator for the household being in a village that is assigned to treatment arm 1, "cash + info arm" is an indicator for the household being in a village that is assigned to treatment arm 2, and "cash + info + home visits arm" is an indicator for the household being in a village that is assigned to either treatment arm 3 or treatment arm 4. Standard errors are shown in parenthesis, and randomization inference p-values are shown in brackets below each coefficient estimate. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

Table 11. Impacts on Information Sessions and Home Visits

Panel A: Separate Treatment Specification

	Any nutrition info sessions	Any behavioral info sessions	Number of info sessions	Any nutrition home visits	Any behavioral home visits	Number of home visits	Information session and home visit index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Cash Arm	0.346***	0.521***	3.414***	0.281***	0.469***	1.313***	1.349***
	(0.053)	(0.036)	(0.685)	(0.046)	(0.048)	(0.469)	(0.168)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.005]	[0.000]
Cash + Info Arm	0.455***	0.661***	4.168***	0.315***	0.625***	1.610***	1.675***
	(0.052)	(0.033)	(1.126)	(0.037)	(0.042)	(0.415)	(0.182)
	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
Cash + Info + Home Visits Arm	0.474***	0.659***	3.574***	0.387***	0.681***	3.327***	1.887***
	(0.042)	(0.033)	(0.558)	(0.033)	(0.037)	(0.483)	(0.132)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Observations	1,513	1,506	1,513	1,508	1,513	1,513	1,513
Control Group Mean	0.20	0.09	0.55	0.22	0.13	0.59	0.00

Panel B: Cumulative Treatment Specification

	Any nutrition info sessions	Any behavioral info sessions	Number of info sessions	Any nutrition home visits	Any behavioral home visits	Number of home visits	Information session and home visit index
Cash	0.346***	0.521***	3.414***	0.281***	0.469***	1.313***	1.349***
	(0.053)	(0.036)	(0.685)	(0.046)	(0.048)	(0.469)	(0.168)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.005]	[0.000]
+ Info	0.109*	0.140***	0.754	0.034	0.156***	0.297	0.326
	(0.062)	(0.043)	(1.087)	(0.053)	(0.052)	(0.570)	(0.224)
	[0.081]	[0.004]	[0.527]	[0.525]	[0.004]	[0.628]	[0.173]
+ Home Visits	0.019	-0.002	-0.594	0.072*	0.056	1.717***	0.211
	(0.051)	(0.036)	(1.009)	(0.037)	(0.037)	(0.535)	(0.186)
	[0.705]	[0.943]	[0.584]	[0.046]	[0.119]	[0.001]	[0.271]
Observations	1,513	1,506	1,513	1,508	1,513	1,513	1,513
Pvalue for Cash+Info+HV	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Control Group Mean	0.20	0.09	0.55	0.22	0.13	0.59	0.00

Notes: These outcomes are reported at the household level. "Cash arm" is an indicator for the household being in a village that is assigned to treatment arm 1, "cash + info arm" is an indicator for the household being in a village that is assigned to treatment arm 2, and "cash + info + home visits arm" is an indicator for the household being in a village that is assigned to either treatment arm 3 or treatment arm 4. Panel B shows the estimates from the cumulative specification. "Cash" is an indicator for being assigned to receive cash (T1, T2, T3, T4), "+ Info" is an indicator for being assigned to receive information (T2, T3, T4), and "+ Home Visits" is an indicator for being assigned to receive home visits (T3, T4).The information session and home visit index in column 7 is a standardized index across the outcomes in columns 1-6. Standard errors are shown in parenthesis, and randomization inference p-values are shown in brackets below each coefficient estimate. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

Table A1. Topics for Information and Home Visits iIntervention

Panel A: Health Topics
Compliance with 7 prenatal visits
Exclusive breastfeeding 0-6 months
Feeding the child 6-23 months
Consumption of foods rich in micronutrients (Vitamin A, Iron and Iodine)
Early breastfeeding and breastfeeding technique
Food groups
Diet of the pregnant woman
Prevention of anaemia, malaria in pregnant women
Feeding of breastfeeding women
Iron supplementation in vit. A and prevention of malaria in breastfeeding women
Feeding the sick child
Personal and clothing hygiene and hygiene of the living environment
Family planning
Management of diarrhea at home
Common signs of severe malnutrition
Water and food hygiene and hand hygiene
Panel B: Child Development Topics
Registering the birth and issuing birth certificates of children
Always giving affection to children
Providing a recreational environment for children
Protecting children from dangerous objects
Protecting children from dangerous objects and saving children first in emergencies
Teaching children to share what they have with others

Notes: The list of topics for the monthly information meetings and home visits comes from the 5-year intervention report.

Table A2. Baseline Balance, 74 Villages in Endline Sample

	Control	T1	T1=control	T2	T2=control	T3/T4	T3/T4=control	All equal
Household Characteristics	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Male	0.87	0.79	0.03	0.83	0.25	0.75	0.00	0.00
Age	47.87	44.39	0.02	45.71	0.21	45.87	0.17	0.11
Any education	0.07	0.08	0.87	0.11	0.15	0.11	0.15	0.32
Household size	10.24	8.92	0.01	9.14	0.03	8.86	0.01	0.04
# spouses	1.33	1.24	0.30	1.23	0.19	1.12	0.01	0.06
# children	4.93	5.13	0.35	5.08	0.50	4.91	0.91	0.63
# other members in household	2.99	1.55	0.01	1.83	0.04	1.82	0.03	0.08
Married	0.90	0.92	0.56	0.93	0.20	0.93	0.25	0.54
Monogamous	0.45	0.42	0.55	0.49	0.42	0.48	0.53	0.45
Polygamous	0.45	0.49	0.42	0.44	0.84	0.45	0.91	0.81
Gourmanche ethnicity	0.08	0.08	0.98	0.13	0.52	0.10	0.73	0.91
Mossi ethnicity	0.28	0.42	0.24	0.43	0.29	0.55	0.03	0.17
Peulh ethnicity	0.12	0.05	0.07	0.05	0.07	0.05	0.08	0.29
Yana ethnicity	0.24	0.28	0.79	0.21	0.79	0.15	0.36	0.60
Catholic or Protestant	0.25	0.25	0.96	0.26	0.94	0.35	0.18	0.51
Muslim	0.72	0.74	0.84	0.73	0.89	0.64	0.31	0.54
Animist or no religion	0.03	0.01	0.12	0.01	0.28	0.02	0.38	0.39
Prenatal/Feeding								
# prenatal visits	4.61	4.55	0.69	4.67	0.77	4.55	0.70	0.91
Mother vitamin A	0.74	0.76	0.62	0.78	0.43	0.78	0.37	0.79
Mother iron	0.89	0.92	0.29	0.90	0.82	0.89	0.83	0.55
Child vitamin A	0.51	0.44	0.25	0.45	0.29	0.52	0.81	0.38
Child fully vaccinated	0.45	0.44	0.83	0.38	0.25	0.50	0.37	0.08
Mother washes hands after toilet	0.86	0.86	0.89	0.84	0.71	0.87	0.81	0.92
Anthropometrics								
Height-for-age z-score	-1.31	-1.29	0.84	-1.24	0.63	-1.10	0.05	0.14
Arm circumference-for-age z-score	-0.94	-0.96	0.79	-0.94	0.96	-0.93	0.99	0.99
Weight-for-age z-score	-1.22	-1.17	0.64	-1.19	0.80	-1.18	0.65	0.96

Education								
Enrollment	0.54	0.53	0.84	0.49	0.38	0.57	0.54	0.65
Years of school	2.16	2.09	0.76	1.97	0.48	2.28	0.62	0.75
Any schooling	0.59	0.58	0.90	0.53	0.28	0.64	0.30	0.38
Progression	0.55	0.53	0.81	0.49	0.35	0.58	0.51	0.58
Read/write in French	0.36	0.36	0.93	0.34	0.56	0.39	0.49	0.65
Denver								
Total score	0.43	0.43	0.80	0.44	0.53	0.44	0.65	0.92
Language	0.39	0.41	0.36	0.40	0.66	0.41	0.41	0.76
Fine motor	0.42	0.41	0.80	0.43	0.63	0.45	0.18	0.33
Gross motor	0.48	0.48	0.99	0.50	0.48	0.50	0.50	0.78
Personal-social	0.43	0.43	0.97	0.45	0.48	0.44	0.71	0.82
HTKS								
Official score	7.07	7.69	0.64	7.47	0.73	8.39	0.27	0.70
Score T1, P1, E1	5.33	5.82	0.58	5.73	0.61	6.28	0.23	0.66
Total over 0	0.70	0.74	0.48	0.72	0.71	0.76	0.23	0.66
Strengths & Difficulties								
Total difficulties score in avg range	0.32	0.42	0.07	0.34	0.63	0.43	0.02	0.06
Total z-score under -0.5	0.33	0.42	0.11	0.36	0.62	0.44	0.02	0.08
Prosocial in avg range	0.28	0.22	0.34	0.24	0.50	0.25	0.59	0.80
Prosocial z-score over 0.5	0.37	0.29	0.21	0.33	0.56	0.31	0.29	0.61

Notes: Calculations in this table are based on baseline data from all 225 villages. Standard errors are clustered at the village level. The treatment arms are abbreviated as "T1" (cash only), "T2" (cash + information), "T3/T4" (cash + information + home visits). Column 1 presents the baseline mean in the control group; columns 2, 4, and 6 present the baseline means for each of the treatment arms; columns 3, 5, and 7 present p-values from a test of mean equality between the control arm and each of the respective treatment arms; and column 8 shows p-values for an F-test for equality across all four groups. For variable definitions, see Table 2.

Table A3. Attrition by Treatment Arm

	Control	T1	T2	T3	T4	All groups
Panel A: Village-level attrition, Est & Centre-Est	(1)	(2)	(3)	(4)	(5)	(6)
Percentage of Baseline Villages Surveyed at Endline	37.8	37.8	33.3	28.9	26.7	32.9
P-value for F-test Against Control	-	1	0.66	0.38	0.26	0.717
Number of Baseline Villages	45	45	45	45	45	225
Number of Endline Villages	17	17	15	13	12	74
Panel B: Household-level attrition, Est & Centre-Est	**Control**	**T1**	**T2**	**T3**	**T4**	**All groups**
Percentage of Baseline Households Surveyed at Endline	37.2	36.8	32.6	27.3	26.1	32.0
P-value for F-test Against Control	-	0.97	0.65	0.31	0.25	0.675
Number of Baseline Households	946	940	944	961	939	4730
Number of Baseline Households in Villages Surveyed at Endline	357	356	315	267	246	1541
Number of Endline Households	352	346	308	262	245	1513

Notes: Calculations in this table are based on baseline data from all 225 villages. The first row in each panel shows the percentage of villages or households that were surveyed at endline, and the second row contains the p-value for a test of equality between each of the treatment arms and the control group. Standard errors are clustered at the village level. The treatment arms are abbreviated as "T1" (cash only), "T2" (cash + information), "T3" (cash + information + nutrition home visits), "T4" (cash + information + nutrition & behavioral home visits). The remaining rows show the raw numbers of villages and households.

Table A4. Attrition by Treatment Arm

	Village present at endline	Household present at endline
	(1)	(2)
Cash Arm	0.000	-0.004
	(0.103)	(0.100)
Cash + Info Arm	-0.044	-0.046
	(0.102)	(0.099)
Cash + Info + Home Visits Arm	-0.1	-0.105
	(0.087)	(0.085)
Observations	225	4,730
Control Group Mean	0.38	0.37

Notes: "Cash arm" is an indicator for the household being in a village that is assigned to treatment arm 1, "cash + info arm" is an indicator for the household being in a village that is assigned to treatment arm 2, and "cash + info + home visits arm" is an indicator for the household being in a village that is assigned to either treatment arm 3 or treatment arm 4. Standard errors are shown in parenthesis and are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

Table A5. Attrition by Arm, All 225 Villages

	Control non-attritted village	Control attritted village	T1 non-attritted village	T1 attritted village	T2 non-attritted	T2 attritted village	T3/T4 non-attritted village	T3/T4 attritted village	Differences equal across arms
Household Characteristics	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Male	0.87	0.89	0.78	0.88	0.82	0.86	0.74	0.88	0.04
Age	47.71	44.84	44.14	43.62	45.74	43.05	45.66	43.91	0.45
Any education	0.07	0.05	0.08	0.05	0.10	0.07	0.12	0.07	0.87
Household size	10.25	9.96	8.86	8.87	9.08	8.87	8.81	8.93	0.87
# spouses	1.32	1.38	1.22	1.33	1.21	1.29	1.12	1.35	0.23
# children	4.94	5.68	5.11	5.29	5.04	5.24	4.88	5.36	0.25
# other members in household	2.98	1.89	1.53	1.24	1.83	1.34	1.81	1.22	0.61
Married	0.90	0.94	0.92	0.95	0.93	0.94	0.93	0.96	0.75
Monogamous	0.45	0.46	0.43	0.46	0.49	0.49	0.48	0.46	0.83
Polygamous	0.46	0.48	0.49	0.50	0.44	0.45	0.45	0.49	0.91
Gourmanche ethnicity	0.08	0.51	0.08	0.52	0.13	0.47	0.10	0.44	0.75
Mossi ethnicity	0.27	0.08	0.43	0.06	0.42	0.08	0.55	0.08	0.18
Peulh ethnicity	0.12	0.15	0.05	0.12	0.05	0.10	0.05	0.13	0.74
Yana ethnicity	0.25	0.24	0.27	0.27	0.21	0.33	0.15	0.30	0.54
Catholic or Protestant	0.25	0.29	0.25	0.29	0.25	0.37	0.35	0.30	0.33
Muslim	0.72	0.47	0.74	0.47	0.73	0.42	0.64	0.52	0.23
Animist or no religion	0.03	0.23	0.01	0.23	0.01	0.21	0.02	0.18	0.64
Prenatal/Feeding									
# prenatal visits	4.61	4.11	4.55	4.28	4.67	4.18	4.55	4.23	0.67
Mother vitamin A	0.74	0.79	0.76	0.77	0.78	0.82	0.78	0.78	0.76
Mother iron	0.89	0.88	0.92	0.89	0.90	0.89	0.89	0.88	0.96
Child vitamin A	0.51	0.50	0.44	0.48	0.45	0.56	0.52	0.51	0.36
Child fully vaccinated	0.45	0.35	0.44	0.34	0.38	0.35	0.50	0.37	0.43
Mother washes hands after toilet	0.86	0.80	0.86	0.77	0.84	0.79	0.87	0.78	0.83
Anthropometrics									
Height-for-age z-score	-1.31	-1.44	-1.29	-1.27	-1.24	-1.35	-1.10	-1.40	0.19
Arm circumference-for-age z-sc	-0.94	-1.06	-0.96	-1.15	-0.94	-1.19	-0.93	-1.19	0.69
Weight-for-age z-score	-1.22	-1.27	-1.17	-1.20	-1.19	-1.30	-1.18	-1.29	0.91

Education									
Enrollment	0.54	0.38	0.53	0.37	0.49	0.42	0.57	0.37	0.37
Years of school	2.16	1.35	2.09	1.47	1.97	1.58	2.28	1.33	0.37
Any schooling	0.59	0.41	0.58	0.43	0.53	0.46	0.64	0.41	0.22
Progression	0.55	0.37	0.53	0.39	0.49	0.43	0.58	0.37	0.29
Read/write in French	0.36	0.22	0.36	0.24	0.34	0.27	0.39	0.23	0.32
Denver									
Total score	0.43	0.41	0.43	0.41	0.44	0.40	0.44	0.40	0.89
Language	0.39	0.38	0.41	0.38	0.40	0.38	0.41	0.37	0.71
Fine motor	0.42	0.40	0.41	0.40	0.43	0.39	0.45	0.39	0.42
Gross motor	0.48	0.46	0.48	0.47	0.50	0.45	0.50	0.46	0.72
Personal-social	0.43	0.42	0.43	0.42	0.45	0.41	0.44	0.41	0.76
HTKS									
Official score	7.07	9.49	7.69	9.90	7.47	9.02	8.39	9.42	0.75
Score T1, P1, E1	5.33	6.93	5.82	7.18	5.73	6.74	6.28	6.98	0.77
Total over 0	0.70	0.78	0.74	0.80	0.72	0.78	0.76	0.80	0.87
Strengths & Difficulties									
Total difficulties score in avg ran	0.32	0.27	0.42	0.30	0.34	0.29	0.43	0.29	0.28
Total z-score under -0.5	0.33	0.27	0.41	0.30	0.34	0.29	0.43	0.29	0.30
Prosocial in avg range	0.28	0.19	0.22	0.14	0.24	0.15	0.25	0.15	0.99
Prosocial z-score over 0.5	0.39	0.29	0.30	0.25	0.35	0.24	0.32	0.25	0.83

Notes: Calculations in this table are based on baseline data from all 225 villages. Standard errors are clustered at the village level. The treatment arms are abbreviated as "T1" (cash only), "T2" (cash + information), "T3/T4" (cash + information + home visits). Columns 2, 4, 6, and 8 present the baseline means for the group of villages that we did not return to at endline, by treatment arm. Columns 1, 3, 5, and 7 present the baseline means for the group of villages that we did return to at endline, by treatment arm. Column 9 presents p-values for an F-test of equality across the differences within the pairs of columns (e.g. comparing the differences between the means in columns 1 and 2 with the differences between the means in columns 3 and 4, columns 5 and 6, and columns 7 and 8. For variable definitions, see Table 2.

Table A6. Impacts on Children's Food Consumption

	Fruits	Milk	Legumes	Eggs	Meat	Food consumption index
	(1)	(2)	(3)	(4)	(5)	(6)
Cash Arm	0.208	0.020	0.645	0.149**	0.034	0.145**
	(0.320)	(0.155)	(0.463)	(0.073)	(0.137)	(0.072)
	[0.516]	[0.896]	[0.158]	[0.049]	[0.810]	[0.053]
Cash + Info Arm	0.033	0.336***	-0.646	0.011	0.057	0.069
	(0.273)	(0.124)	(0.538)	(0.080)	(0.123)	(0.082)
	[0.912]	[0.013]	[0.246]	[0.909]	[0.663]	[0.441]
Cash + Info + Home Visits Arm	0.288	0.193	0.347	0.114*	0.213	0.207***
	(0.282)	(0.135)	(0.416)	(0.058)	(0.132)	(0.067)
	[0.310]	[0.166]	[0.408]	[0.049]	[0.117]	[0.001]
Observations	1,013	1,019	1,019	1,017	1,019	1,017
Control Group Mean	2.15	0.66	7.93	0.15	0.69	0.31

Notes: These outcomes are aggregated and regressions are run at the household level. Children ages 0-71 months are included in these regressions. "Cash arm" is an indicator for the household being in a village that is assigned to treatment arm 1, "cash + info arm" is an indicator for the household being in a village that is assigned to treatment arm 2, and "cash + info + home visits arm" is an indicator for the household being in a village that is assigned to either treatment arm 3 or treatment arm 4. For columns 1-5, the outcome is the number of times in the past 7 days that a child consumed foods in that food group, averaged at the household level. The food consumption index in column 6 is a standardized index across the outcomes in columns 1-5. Standard errors are shown in parenthesis, and randomization inference p-values are shown in brackets below each coefficient estimate. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

Table A7. Impacts on Household Perceptions

	Perception of food	Perception of health	Perception of education	Perception of income	Perceptions index
	(1)	(2)	(3)	(4)	(5)
Cash Arm	0.017	0.011	0.008	0.016**	0.078
	(0.012)	(0.015)	(0.015)	(0.007)	(0.056)
	[0.151]	[0.464]	[0.635]	[0.011]	[0.163]
Cash + Info Arm	0.020*	0.039**	0.026	0.019*	0.165**
	(0.012)	(0.017)	(0.017)	(0.010)	(0.067)
	[0.092]	[0.017]	[0.126]	[0.061]	[0.012]
Cash + Info + Home Visits Arm	0.022*	0.02	0.005	0.015*	0.104*
	(0.012)	(0.016)	(0.016)	(0.008)	(0.060)
	[0.084]	[0.214]	[0.759]	[0.067]	[0.100]
Observations	1,511	1,512	1,486	1,503	1,512
Control Group Mean	0.01	0.03	0.03	0.01	-0.02

Notes: "Cash arm" is an indicator for the household being in a village that is assigned to treatment arm 1, "cash + info arm" is an indicator for the household being in a village that is assigned to treatment arm 2, and "cash + info + home visits arm" is an indicator for the household being in a village that is assigned to either treatment arm 3 or treatment arm 4. Household heads are asked how satisfied they are with the food, health, education, and income of their households, and the outcomes in these regressions are indicators for the household head reporting that they are "very satisfied" with that category (the highest possible level of satifaction on a 4-point scale. The perceptions index in column 5 is a standardized index across the outcomes in columns 1-4. Standard errors are shown in parenthesis, and randomization inference p-values are shown in brackets below each coefficient estimate. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

Table A8. Heterogeneity, by Gender

	Anthropometrics index		Education index		Denver total score	
	Boy	**Girl**	**Boys**	**Girls**	**Boys**	**Girls**
	(1)	(2)	(3)	(4)	(5)	(6)
Cash Arm	0.155*	0.073	0.024	0.043	0.017	-0.037
	(0.081)	(0.088)	(0.101)	(0.077)	(0.040)	(0.031)
	[0.063]	[0.419]	[0.843]	[0.606]	[0.691]	[0.243]
Cash + Info Arm	-0.083	0.020	0.015	-0.032	-0.041	-0.036
	(0.080)	(0.089)	(0.081)	(0.071)	(0.032)	(0.029)
	[0.296]	[0.856]	[0.852]	[0.661]	[0.191]	[0.233]
Cash + Info + Home Visits Arm	0.023	0.103	0.104	0.111*	0.012	0.017
	(0.064)	(0.088)	(0.085)	(0.059)	(0.034)	(0.028)
	[0.715]	[0.246]	[0.214]	[0.066]	[0.722]	[0.541]
Observations	993	974	2,915	2,835	1,241	1,215
Control Group Mean	-0.08	0.03	0.08	0.03	0.46	0.46

Notes: "Cash arm" is an indicator for the household being in a village that is assigned to treatment arm 1, "cash + info arm" is an indicator for the household being in a village that is assigned to treatment arm 2, and "cash + info + home visits arm" is an indicator for the household being in a village that is assigned to either treatment arm 3 or treatment arm 4. This table contains the KLK indices (or in the case of columns 5 and 6, the total score) from Tables 5 through 7, separately for boys and girls. Standard errors are shown in parenthesis, and randomization inference p-values are shown in brackets below each coefficient estimate. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

Table A9. Heterogeneity, by Distance from Conflict

	Pregnancy index		Health behaviors index		Anthropometrics index		Education index		Denver total score	
	Below median distance from conflict	Above median distance from conflict	Below median distance from conflict	Above median distance from conflict	Below median distance from conflict	Above median distance from conflict	Below median distance from conflict	Above median distance from conflict	Below median distance from conflict	Above median distance from conflict
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Cash Arm	-0.036	0.043	0.073*	-0.034	-0.010	0.317***	0.173**	0.077	0.053*	-0.047
	(0.078)	(0.070)	-0.041	(0.082)	(0.069)	(0.087)	(0.075)	(0.118)	(0.028)	(0.039)
	[0.650]	[0.543]	[0.081]	[0.679]	[0.877]	[0.000]	[0.023]	[0.524]	[0.070]	[0.239]
Cash + Info Arm	-0.020	-0.093	-0.029	-0.086	-0.081	0.082	0.105	-0.207**	-0.010	-0.044
	(0.078)	(0.056)	(0.050)	(0.094)	(0.078)	(0.129)	(0.071)	(0.088)	(0.030)	(0.029)
	[0.819]	[0.111]	[0.569]	[0.395]	[0.289]	[0.582]	[0.137]	[0.033]	[0.759]	[0.155]
Cash + Info +	0.287***	0.076	0.113***	0.028	0.059	0.137	0.248***	0.158**	0.069**	0.009
Home Visits Arm	(0.073)	(0.065)	(0.041)	(0.062)	(0.083)	(0.101)	(0.076)	(0.077)	(0.028)	(0.029)
	[0.002]	[0.251]	[0.011]	[0.690]	[0.495]	[0.227]	[0.004]	[0.061]	[0.020]	[0.714]
Observations	541	517	620	568	1,026	941	2,973	2,788	1,273	1,183
Control Group Mean	-0.02	0.02	0.13	0.17	0.01	-0.08	0.09	0.00	0.46	0.47

Notes: "Cash arm" is an indicator for the household being in a village that is assigned to treatment arm 1, "cash + info arm" is an indicator for the household being in a village that is assigned to treatment arm 2, and "cash + info + home visits arm" is an indicator for the household being in a village that is assigned to either treatment arm 3 or treatment arm 4. This table contains the KLK indices (or in the case of column 5, the total score) from Tables 3 through 7, separately based on whether the village was above or below the median distance from the nearest conflict. Standard errors are shown in parenthesis, and randomization inference p-values are shown in brackets below each coefficient estimate. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

Table A10. Heterogeneity, by Baseline Expenditures

	Pregnancy index		Health behaviorsiIndex		Anthropometrics index		Education index		Denver total score	
	Below median expend-iture	Above median expend-iture	Below median expend-iture	Above median expend-iture	Below median expend-iture	Above median expend-iture	Below median expend-iture	Above median expend-iture	Below median expend-iture	Above median expend-iture
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Cash Arm	0.009	0.010	0.039	0.035	0.150	0.03	0.075	0.014	-0.025	-0.003
	(0.070)	(0.106)	(0.054)	(0.049)	(0.103)	-0.073	(0.071)	(0.079)	(0.033)	(0.038)
	[0.899]	[0.928]	[0.448]	[0.466]	[0.139]	[0.687]	[0.310]	[0.848]	[0.487]	[0.946]
Cash + Info Arm	-0.095	-0.006	-0.045	-0.017	0.024	-0.094	0.020	-0.045	-0.044	-0.017
	(0.070)	(0.090)	(0.053)	(0.066)	(0.126)	(0.060)	(0.066)	(0.083)	(0.035)	(0.028)
	[0.173]	[0.945]	[0.416]	[0.811]	[0.838]	[0.107]	[0.774]	[0.630]	[0.224]	[0.538]
Cash + Info +	0.236***	0.143*	0.035	0.107**	0.098	0.025	0.167***	0.063	0.052*	0.009
Home Visits Arm	(0.071)	(0.081)	(0.049)	(0.044)	(0.077)	(0.063)	(0.063)	(0.070)	(0.028)	(0.026)
	[0.002]	[0.087]	[0.472]	[0.030]	[0.199]	[0.699]	[0.007]	[0.395]	[0.076]	[0.726]
Observations	509	549	572	616	846	1,119	2,628	3,133	1,078	1,378
Control Group Mean	-0.05	0.02	0.17	0.13	-0.13	0.03	0.01	0.08	0.46	0.47

Notes: "Cash arm" is an indicator for the household being in a village that is assigned to treatment arm 1, "cash + info arm" is an indicator for the household being in a village that is assigned to treatment arm 2, and "cash + info + home visits arm" is an indicator for the household being in a village that is assigned to either treatment arm 3 or treatment arm 4. This table contains the KLK indices (or in the case of column 5, the total score) from Tables 3 through 7, separately based on whether the village was above or below the median level of expenditures at baseline. Standard errors are shown in parenthesis, and randomization inference p-values are shown in brackets below each coefficient estimate. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

Table A11. Impacts on Main Indices, Alternative Specifications

Panel A: Indices for Main Tables

	Pregnancy index	Health behaviors index	Anthropometrics index	Education index	Denver total score
	(1)	(2)	(3)	(4)	(5)
Cash Arm	0.047	0.013	0.101	0.035	-0.008
	(0.067)	(0.028)	(0.063)	(0.075)	(0.030)
	[0.488]	[0.621]	[0.123]	[0.636]	[0.799]
Cash + Info Arm	-0.019	-0.015	-0.030	-0.018	-0.038
	(0.055)	(0.042)	(0.068)	(0.062)	(0.026)
	[0.746]	[0.707]	[0.657]	[0.755]	[0.148]
Cash + Info + Home Visits Arm	0.199***	0.097***	0.042	0.102*	0.016
	(0.055)	(0.028)	(0.059)	(0.0530)	(0.025)
	[0.002]	[0.003]	[0.478]	[0.067]	[0.518]
Observations	1,054	1,188	1,967	5,763	2,456
Control Group Mean	0.00	0.14	-0.03	0.05	0.46

Panel B: Indices for Main Tables - No Controls

	Pregnancy index	Health behaviors index	Anthropometrics index	Education index	Denver total score
Cash Arm	-0.005	0.039	0.082	0.025	-0.008
	(0.060)	(0.038)	(0.062)	(0.099)	(0.030)
	[0.937]	[0.328]	[0.192]	[0.810]	[0.799]
Cash + Info Arm	-0.048	-0.033	-0.048	-0.015	-0.038
	(0.0502)	(0.043)	(0.070)	(0.088)	(0.026)
	[0.344]	[0.445]	[0.491]	[0.866]	[0.148]
Cash + Info + Home Visits Arm	0.171***	0.079***	0.050	0.113	0.016
	(0.057)	(0.030)	(0.060)	(0.083)	(0.025)
	[0.002]	[0.011]	[0.400]	[0.185]	[0.518]
Observations	1,054	1,188	1,967	5,763	2,456
Control Group Mean	0.00	0.14	-0.03	0.05	0.46

Panel C: Indices for Main Tables - ANCOVA Specification

	Pregnancy index	Health behaviors index	Anthropometrics index	Education index	Denver total score
Cash Arm	-	0.001	0.246**	0.047	0.028
	-	(0.030)	(0.100)	(0.060)	(0.044)
	-	[0.986]	[0.024]	[0.440]	[0.535]
Cash + Info Arm	-	-0.009	-0.010	0.005	-0.021
	-	(0.039)	(0.073)	(0.051)	(0.040)
	-	[0.800]	[0.904]	[0.922]	[0.597]
Cash + Info + Home Visits Arm	-	0.091***	0.095	0.099**	0.058
	-	-0.028	(0.076)	-0.046	(0.047)
	-	[0.003]	[0.232]	[0.040]	[0.235]
Observations	-	1,151	342	5,367	733
Control Group Mean	-	0.14	-0.03	0.05	0.46

Notes: "Cash arm" is an indicator for the household being in a village that is assigned to treatment arm 1, "cash + info arm" is an indicator for the household being in a village that is assigned to treatment arm 2, and "cash + info + home visits arm" is an indicator for the household being in a village that is assigned to either treatment arm 3 or treatment arm 4. Each panel contains the KLK indices (or in the case of column 5, the total score) from Tables 3 through 7. Panel A repeats the results with the main specification, Panel B shows the results without lasso-selected covariates, and Panel C shows the results from an ANCOVA specification that controls for baseline values of the outcome (lasso-selected controls are also included in this specification). Standard errors are shown in parenthesis, and randomization inference p-values are shown in brackets below each coefficient estimate. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.