Are Behavioral Change Interventions Needed to Make Cash Transfer Programs Work for Children? Experimental Evidence from Myanmar *

Erica Field† Elisa M. Maffioli‡

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Abstract

We experimentally evaluate the impact on child malnutrition of a maternal cash transfer program in Myanmar that was supplemented with Social Behavior Change Communication (SBCC) in a subset of villages. The combination of interventions significantly reduced the proportion of children stunted, while cash alone had no impact on stunting. SBCC appears to have worked in conjunction with cash to reduce stunting by encouraging mothers to increase children's diet diversity, in particular proteins consumed. The findings underscore the importance of adding SBCC to cash transfer programs.

Keywords: Nutrition, Cash Transfers, Social and Behavior Change Communication

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† Department of Economics, Duke University, Durham, NC 27710 (e-mail: field.erica@duke.edu)

‡ Health Management and Policy, University of Michigan School of Public Health, Ann Arbor, MI 48109 (e-mail: elisamaf@umich.edu)

1 Introduction

Despite widescale improvements in economic wellbeing over the past few decades, child malnutrition remains a global health concern, affecting more than 150 million children annually (World Bank, 2017). When families cannot afford to provide children with sufficient nutrient-rich food during critical windows of growth, malnutrition can lead to irreversible decreases in health and cognitive human capital that are visible in permanent differences in stature. In Myanmar, where this study takes place, an estimated 29 percent of the children under age five are stunted, a rate that can reach as high as 50 percent in poor rural areas (Demographic Health Surveys, 2015; United Nation Standing Committee on Nutrition, 2010).

The provision of adequate nutrition in early life is believed to be crucial to realizing an individual's full physical and cognitive potential throughout the life course. Evidence from both medical and social science research has contributed to the consensus that the in-utero phase and the first two years of life constitute the most critical period of development (Almond and Currie, 2011a). Inadequate nutrition during the first 1000 days of life is believed to produce higher susceptibility to illness, and impaired physical and cognitive ability later in life (Victora et al., 2010; Almond and Currie, 2011b; Doyle, 2019). These early life health insults have the potential to lower human capital accumulation, productivity and earnings in adult life, thereby contributing to the inter-generational transmission of poverty (Engle et al., 2007; Hoddinott et al., 2013; Richter et al., 2017).

Concern over malnutrition in utero and during the first two years of life has motivated a number of governments, NGOs, and international agencies to direct cash transfers programs to households with young children, which to date have reached 1.3 billion people globally (Bastagli et al., 2016; Hoddinott et al., 2017; Gentilini, 2022). There are two principal ways in which cash transfers may directly reduce early life malnutrition. First, households at risk of malnourishment could spend a large fraction of each additional dollar of income on child nutrition (both quantity and quality of food) simply because investing in children who are malnourished during critical windows of physical and cognitive development is one of the highest return investments a family can make. Likewise, cash transfers could also facilitate similarly high-return investments in child health production such as health care expenditures. Second, these programs are frequently directed to mothers ("maternal cash transfer programs"), and increasing women's control over income is believed to promote investment in children in and of itself.[1]

However, it is not obvious that simply giving cash to families at risk is sufficient to make a dent in early life malnourishment. In fact, there is no consensus from evaluations of existing programs that cash transfers generate significant reductions in stunting. While cash transfer programs have been associated with a wide range of positive household outcomes, including increases in schooling and business income of vulnerable populations (Bastagli et al., 2016), the evidence on positive effects on child nutrition is limited (Baird et al., 2019; Ahmed et al., 2019). As described in evidence reviews (Ritcher, 2010; Soares et al., 2010; Cecchini and Madariaga, 2011; Manley et al., 2013; Bastagli et al., 2016; Tirivayia et al., 2016; Biscaye et al., 2017; Millan et al., 2019; Manley et al., 2020, 2022), while some program evaluations have shown positive impacts, several others reported mixed or null results.

Maternal cash transfer programs may fail to reduce malnutrition for two reasons. First, households may lack sufficient information on child health production to understand the value of early life nutrition, including the importance of a more diverse diet, reducing the marginal impact of income on child health. Second, households may lack financial resources and/or fail to direct consumption towards child health if those who control income do not fully internalize welfare and hence prefer an alternative consumption bundle. [2]

To increase the impact on child malnutrition, policymakers increasingly enhance maternal cash transfer programs with complementary features designed to address one or both constraints. In many settings, programs include conditionalities such as mandatory health visits in order to incentivize households who may otherwise prefer to put cash elsewhere to invest in child health. While a number of studies show positive effects of conditional cash transfers (CCTs) on stunting, there is ongoing concern that

[1]There is a substantial body of literature linking female income share to increases in children's expenditure. Bobonis (2009) finds that climate shocks that contribute to female bargaining power, increased household expenditures on children's goods (education, health). Similarly, Duflo and Udry (2004) find that positive rainfall shocks to women-controlled crops in Cote D' Ivoire increased shares of education expenditures for children. Lépine and Strobl (2013) find that positive rainfall shocks to women-controlled crops in Ghana increased children's nutritional status. Duflo (2003) find that providing pensions to South African grandmothers increased height-for-age of grandchildren. There are several theories explaining this bias including gender differences in old age security, preferences, and altruism.

[2]Maternal cash transfer programs may also fail to reduce malnutrition if there are important constraints to local food availability, such as occurs during famines or in food deserts. This does not apply in the context of our evaluation, as our sample villages lie within two hours of a major city and no severe economic shocks happened during the study period.

imposing conditionalities excludes the most vulnerable households from receiving program benefits (Cahyadi et al., 2020; Kandpal et al., 2016). In-kind transfer programs can mitigate both constraints, but are costly to implement and may be difficult to tailor to households' idiosyncratic dietary needs.

An alternative strategy for promoting child health that is frequently implemented in conjunction with maternal cash transfers are information programs delivered via Social and Behavior Change Communication (SBCC). SBCC sessions attached to maternal cash transfer programs are designed to tackle the information constraint by supplementing cash transfers with information on child health production, and their curricula typically focus on infant and child health and feeding practices. Participation in SBCC programs is voluntary (not tied to the receipt of cash benefits), and hence immune to concerns over systematic exclusion. However, it is unclear whether information frictions impose a binding constraint on the marginal propensity to invest in child health out of cash transfer income, and also whether relevant information on child health production can be effectively delivered at scale.

In empirical work to date, the efficacy of supplementing maternal cash transfers with SBCC in bolstering child health remains uncertain.[3] A number of studies have evaluated child health impacts of maternal SBCC programs (Luo et al., 2012; Zulfiqar A Bhutta et al., 2013), but evidence is often inconclusive and limited to behavior change outcomes. For instance, several evaluations show that cash transfer programs can lead to improved infant and child feeding practices only if they are combined with SBCC (Fiszbein et al., 2009; Avula et al., 2013; Hoddinott et al., 2017), but fail to measure impacts on child health outcomes. Among those, the bulk of existing evidence on child outcomes suggests that, even when SBCC programs are successful in promoting behavior change among transfer recipients, those changes are insufficient to reduce malnutrition in children. For example, a recent randomized experiment in Nepal found meaningful effects of supplementing cash transfers with SBCC on health knowledge and behavior, but null effects on child malnutrition (Levere et al., 2016). One exception is an experimental evaluation of a child nutrition SBCC program without cash transfers in Malawi, which was associated with gains in HAZ (0.27 SD) (Fitzsimons et al., 2016). A contemporaneous study in Nigeria also finds meaningful impacts on child stunting of the combination of SBCC plus cash transfers, but is unable to isolate the marginal

[3] There is huge evidence on conditional and unconditional cash transfers aimed at improving health, but our paper is primarily related and contributes specifically to the literature on unconditional cash transfers for nutrition.

contribution of SBCC because the research does not evaluate the impact of cash alone (Carneiro et al., 2021).

In this study we seek evidence on the potential value of supplementing cash transfers with SBCC for child malnutrition. We evaluate a combination of interventions designed to reduce chronic malnutrition during the first 1000 days of life by providing cash transfers with and without SBCC to women who are pregnant or have children under age two. The program, sponsored by the Government of Myanmar and implemented by Save the Children International (SCI), was run for 30 months in 416 rural villages as a pilot for the government's national maternal transfer program.[4] The program targeted all pregnant women in intervention villages, who were provided monthly cash transfers from enrollment until their child reached age two (i.e. for 24-30 months). In a randomly chosen subset of treatment villages, program recipients also received monthly SBCC group sessions for the duration of the program that covered a range of topics relevant to child health and nutrition.

We assess the impact after 30 months of implementation of transfers alone and in combination with SBCC on child height-for-age Z-scores (HAZ scores) and stunting, a well-validated biometric measure of chronic malnutrition in children (Leroy and Frongillo, 2019). We restrict our analysis to women found to be pregnant immediately prior to program announcement, which allows us to gauge the impact of receiving the full duration of program transfers while also circumventing concerns over selective fertility or migration into treatment villages.

Our results indicate that the combination of cash transfers and SBCC leads to a 4.6 percentage point (13.5%) statistically significant reduction in the proportion of children who are stunted. The program appears to be effective for children at risk of moderate but not severe stunting, which indicates that more heavy-handed approaches or higher levels of transfers might be required to address malnutrition among the most vulnerable children. Meanwhile, cash alone has no detectable impact on child anthropometrics relative to the control group.

Survey data on health behaviors collected at endline indicate that the cash transfers, when combined with SBCC, reduced stunting through some combination of improvements in total food consumption, dietary diversity, breastfeeding and hand-washing practices, all of which are reported to be significantly higher among those treated with the combined interventions relative to both control and cash only groups. Most notably,

[4]The name of the program was LEGACY, which stands for "Learning, Evidence Generation, and Advocacy for Catalyzing Policy".

relative to the control group, food consumption in the combined treatment group rises by 15%, accompanied by a significant improvement in a standardized index of child dietary diversity. Both amount and type of food like protein can directly reduce chronic malnutrition by increasing energy availability, while higher rates of breastfeeding and hand-washing lower stunting by reducing nutrient-depleting episodes of diarrheal disease. In this sense, disease environment plays an important role in determining baseline levels of nutrient absorption and therefore stunting.

We evaluate these competing mechanisms by examining survey data on child illness episodes, health care expenditures and food diary reports of specific foods consumed, all of which point towards dietary diversity being the critical behavior change. First, we find no decrease in child illness episodes or health care spending among children in the cash plus SBCC group, which indicates limited roles of hand-washing and breastfeeding behaviors in reducing child stunting. Second, dietary reports reveal that transfer recipients who were also exposed to the SBCC curriculum incorporate significantly more protein-rich foods into children's diets, including meat, pulses, dairy, and eggs. While fruits and vegetables can improve child nutrition, animal proteins in particular have been shown to have a significant impact on child stunting in multiple settings.[5] In sum, the weight of evidence indicates that most of the reductions in stunting observed among children whose mothers received cash transfers alongside the SBCC program arise from improvements in dietary diversity. Our pattern of results is corroborated by contemporaneous evidence from a similar RCT that was conducted in Bangladesh at the same time as our study, which finds similar evidence in preliminary reports that cash transfers plus SBCC reduces stunting in children relative to both control and cash alone (Ahmed et al., 2019).

Meanwhile, cash transfers both with and without SBCC improved reported take-up of prenatal care and lead to higher levels of food consumption. However, the similar rates of stunting between the cash only and control groups indicate that these behavior modifications were insufficient to influence chronic malnutrition. While prenatal care is unlikely to have a significant effect on stunting in any setting, the absence of a stunting effect on the cash only arm is more surprising. However, not only is the increase in food consumption among the cash only group relative to control significantly lower than that observed in SBCC villages (7%, $p < 0.01$), but there is no significant change

[5]See Laplante and Sabatini (2012) and Semba et al. (2016a,b) for meat and fish; Molgaard et al. (2011); Iannotti et al. (2013); Dyer et al. (2016) for dairy products; Semba et al. (2016c); Bekdash (2016) for eggs.

in child dietary diversity, both of which could account for a null result on stunting.

These findings provide novel evidence on the policy importance of combining maternal cash transfers with behavioral change interventions in order to generate meaningful improvements in child nutrition. By targeting both financial and information constraints, this study offer a fundamental lesson for the design and implementation of maternal cash transfer programs in low-income countries. First and foremost, in settings such as rural Myanmar where child malnutrition remains a significant problem, policies that increase household income still have large potential to improve child malnutrition as long as mothers are also provided adequate knowledge to purchase the appropriate quantity and quality of foods. Moreover, given that the improvements in stunting appear to be driven largely by promoting changes in dietary diversity, lessons on infant and child feeding practices should be heavily emphasized in SBCC curriculum and prioritized whenever programs are streamlined for scale-up. While we cannot rule out the possibility that information alone would have had comparable impacts on malnutrition, our results indicate that the SBCC curriculum reduced malnutrition by convincing households to increase food expenditures rather than by convincing them to change health practices such as infant feeding and hand-washing, which implies that liquidity constraints potentially bind the behavioral responses to the program. Hence, it is unlikely that SBCC delivered *without* cash transfers would have achieved as large an impact on child stunting.

Our findings also offer a key lesson for the evaluation of maternal health programs. In particular, both cash transfers and SBCC have the potential to generate meaningful changes in parental health behaviors *without* significant reductions in child malnourishment. As a result, tracking child health outcomes and not only behavioral outcomes is critical for comprehensive evaluation of program effects on children.

2 Methods

2.1 Setting

In Myanmar, close to 1 out of 3 children are chronically malnourished. To address concerns over child malnutrition, in 2014 the government of Myanmar committed to rolling out a national maternal cash transfer program, which is projected to reach 2.25 million beneficiaries and 0.32% of GDP by 2024 (The Republic of The Union of Myanmar, 2014). SCI was chosen to implement a pilot version of the program as a randomized controlled trial (RCT) for 30 months prior to national scale-up in order to test the delivery model, including the inclusion of a maternal behavior change component, and measure impacts on malnutrition.[6]

The pilot was implemented between 2016 and 2019 in 416 villages in three townships of Myanmar's Dry Zone – Pakkoku, Yesagyo, and Mahlaing. All villages within two hours of an urban center were eligible for the study. Overall, study villages have reasonable access to food markets (96% have a food market located in their community), but relatively poor access to medical care: only 18% have a village health facility, and only 22% have a midwife that visits regularly. The majority of households in this area earn income from agriculture (89%) and livestock (27%), and casual labor (77%) (Appendix Table 1 presents these statistics by treatment and control groups).

Malnutrition in this area is representative of the country as a whole. Baseline data collected prior to the intervention reveal that 28.7% of children under 5 in the study villages were stunted, almost identical to the national rate of under 5 stunting of 29% in 2015 (Demographic Health Surveys, 2015). In terms of weight-for-height, 18% were wasted and 31% were underweight.[7] Meanwhile, malnutrition among mothers is relatively low. Only 6.7% of pregnant or lactating mothers were found to be malnourished at baseline, as measured by mid-and-upper arm conference (MUAC), which is considerably lower than rates found in other Asian countries.[8]

In terms of parental health behaviors that contribute to child malnourishment and hence are generally included in SBCC curricula, baseline data reveal that households in this setting performed very well on some measures and relatively poorly on others. Breastfeeding is nearly universal in the study area, as is early initiation of breastfeeding: 98% of children 0-23 months were ever breastfed and 94% received colostrum. Consistent with this, at baseline, 99% of mothers were aware of the best time to initiate breastfeeding. Hence, there is little scope for program participation to influence nursing practices in this setting.

Likewise, households do fairly well at baseline in terms of WASH behaviors. Almost every woman reported using soap when washing their hands (99%), and the vast majority reported doing so consistently after going to the toilet (77%), the most critical

[6]Since 2018 the program has been extended at scale in several states and (in 2020-2021) to further support vulnerable households during the COVID-19 pandemic (Livelihoods and Food Security Fund, 2020).

[7]The rate at baseline is somewhat higher among children 22-35 months old, the age group used for our endline analysis. In the age group, 30% were stunted, 16% were wasted and 33% under-weight.

[8]See for example Vasundhara et al. (2020).

routine WASH behavior for disease control. However, there is room for improvement in terms of hand-washing practices in all other situations of heightened contamination risk. Only half of the sample reported using soap before or after eating (51% and 47%) and fewer than half reported using soap before cooking (37%), after disposing baby feces (31%), after cleaning their baby's bottom (17%), before feeding children (16%) and before or after handling children (4%).

In contrast, nutritional intake of children over 6 months is poor in this setting. Only 37% of children 6-23 months have a minimum acceptable diet in terms of food diversity. Moreover, there appears to be room for information interventions to have an impact on complementary feeding practices, which are a major focus of SBCC. In particular, only 85% of mothers are aware of the best age to introduce complementary feeding. In addition, program participation has scope to influence health-seeking behavior. Only 78% of mothers attended 4 or more antenatal care visits as recommended by WHO.

In addition to addressing information constraints on child feeding and health care practices, the cash transfer alone has the potential to lead to improvements in child diet and take-up of health care services by relaxing household budget constraints. 97.4% of women reported to have used the cash for food. However, it is relevant to note that consumption data gathered from our analysis sample indicate that few households in this setting are living hand-to-mouth. At baseline, households with stunted children reported spending on average only 54% of their budget on food, and reported spending their remaining income on a number of "non-essentials" including an average of 7% on gifts and donations. Given this, it is not obvious that households in the program with infants at risk of malnourishment will provide different amount and type of food to their children from the infusion of cash alone, unless it is the case that directing disposable income to mothers has a large effect on child consumption shares.

2.2 Program Design

The program comprised two interventions: 1) monthly cash transfers to mothers beginning in pregnancy until their children turned two; and 2) monthly cash transfers supplemented with monthly SBCC that covered a range of topics relevant to child health and nutrition.[9]

[9] Although this was not a labeled cash transfer, per se, cash and SBCC interventions were likely linked in the sense that they were delivered at the same time by support staff from the same institution (SCI).

Both interventions were randomized across 102 sub-rural health care center catchment areas (the geographic unit of randomization) located within two hours of an urban center. To minimize differences across experimental arms, prior to random assignment, catchment areas were first grouped into 34 triplets (strata) based on geographic clustering. Within each stratum, individual catchment areas were randomly assigned to one of three experimental arms: (1) Treatment 1 ($Cash + SBCC$), in which cash transfers and SBCC activities were provided jointly (N = 34 catchment areas encompassing 142 villages); (2) Treatment 2 ($CashOnly$), in which only cash transfers were provided (N= 34 catchment areas encompassing 146 villages); and (3) Control, in which neither cash transfers nor SBCC were offered (N = 34 catchment areas encompassing 149 villages) (Appendix Fig 1).

Within both T1 and T2 catchment areas, all pregnant women were assigned to receive monthly cash transfers worth 10,000 MMK (about 6.5 USD) beginning in their second trimester of pregnancy until their child reached age two.[10] As a reference, the legal minimum wage in Myanmar at that time was 3,600 MMK per day, so the cash transfer amount represented about 3-4 days of work at the minimum wage. In addition to monthly cash transfers, beneficiaries in T1 were targeted with SBCC in the form of monthly information sessions on four main topics: infant and young child feeding (IYCF) practices, health-seeking behavior, hygiene practices, and household expenditures.

The program was implemented by SCI in collaboration with the Myanmar Nurse and Midwives Association (MNMA), a national non-governmental organization that provides prevention and community-based care, and Pact Global Microfinance (PGMF), a nonprofit international development organization that delivers microfinance in rural areas. PGMF managed monthly cash disbursements by creating an ad-hoc bank account for each program beneficiary into which transfers were deposited on a monthly basis and delivered through PGMF's network of rural loan agents. MNMA was responsible for coordinating the sensitization and enrollment of eligible women in each treatment village and organizing SBCC activities in villages assigned to the T1 group.[11]

[10]In October 2017 the implementer (SCI) increased the amount to 15,000 MMK (about 10 USD) to stay in line with similar initiatives in other parts of the country. The value was estimated to cover enough nutritious food for a mother and a child for a month.

[11]One aspect of delivery the government was interested in testing in this pilot program was utilizing a NGO for distribution of cash payments versus government workers. Hence, in 40 villages, payments were delivered by government workers instead of PGMF. A description of the difference in delivery agents and findings from that evaluation are the subject of a companion paper (Field and Maffioli, 2021).

SBCC activities were implemented in two stages. First, between May 2016 and January 2017, MNMA delivered basic SBCC programming within each village aimed at mobilizing communities to address poor nutrition. Basic programming included mother-to-mother support groups (including 12-15 pregnant women or mothers of under 5 years old children) in which mothers were brought together monthly to disseminate information and share experiences with feeding practices during pregnancy, lactation, and early childhood; and a handful of participatory community-level sessions (13-15 community members) that explored perceptions and current practices around diet and nutrition, health care, and household and food expenditures.

Based on the information gathered through the basic SBCC activities, SCI then designed a series of intensive SBCC sessions that focused on key behaviors and messaging across four topics: IYCF (including dietary diversity and breastfeeding), health-seeking behavior, hygiene practices, and household expenditures. These sessions were delivered both to the maternal support groups, and also through separate sessions targeted to fathers and elderly household members. The last cohort of mothers was enrolled in May 2018. The last monthly cash transfer and the last SBCC interventions were completed in November 2018 (allowing for at least 6 months of cash transfers for the last cohort enrolled) and May 2019, respectively. Appendix Figure 2 describes the timeline of survey and program activities.[12]

SBCC participation was voluntary. While all mothers in SBCC villages were encouraged to attend the sessions, they still received the full transfer if they were unable or refused to attend. Nevertheless, administrative data indicate high participation in SBCC sessions: administrative data from SCI found that in *Cash+SBCC* villages, 99% of enrolled mothers attended at least one SBCC session and 81% attended five times or more.[13] Unsurprisingly, take-up of the cash transfer was also high and relatively "clean" in terms of eligibility criteria: monitoring activities conducted independently by the research team 30 months into implementation in one of three townships revealed low exclusion and inclusion errors to the cash transfer programs (6.8% and 9.8%, respectively), and all inclusion errors were women in treatment villages who received

[12]Note that the analysis sample for this evaluation only includes women found to be pregnant at the time of program announcement, i.e., the first enrolled cohort covered by the program for about 30 months.

[13]Survey data also ask about attendance at SBCC sessions and report lower participation. However, we trust administrative data from SCI more than survey reports, given that self-reported attendance data tend to be underreported due to recall bias whereas administrative data were collected by direct observation.

transfers despite not meeting the eligibility criteria rather than non-compliers from outside villages, reducing concern over contamination of the control group.[14]

2.3 Analysis Sample

We evaluate the program's impact on child nutrition among women who were pregnant at the time of enrollment. Restricting the sample to this group mitigates concern over selective fertility and migration into study areas that could confound a comparison between babies that were conceived in treatment versus control groups after program announcement. Moreover, women who were pregnant at enrollment are the only program beneficiaries to receive the full 30 months of coverage as part of the pilot.[15]

Women in this group were identified by conducting a full listing of individuals (Appendix Figure 2) in treatment and control study villages two months prior to the start of the program (February 2016) in which community health workers recorded every woman's age and pregnancy status. All 2,338 pregnant women identified in the listing were enrolled in the study. After 30 months we successfully tracked and administered an endline survey to 91.3% of women, resulting in an analysis sample of 2,134 women. Although attrition was slightly higher in the control group (10.8%) relative to treatment groups (7.9%), as is common in program evaluations due to the greater ease of tracking individuals where administrative data is collected regularly, attrition has no measurable impact on the balance of observable characteristics of

[14] Monitoring data were collected only in one township because of limited funding availability and the implementing partner's (SCI) preference for the implementation of a related project (Field and Maffioli, 2021).

[15] The trial was registered on the AEA RCT registry (AEARCTR-0004189) in 2019, prior to the completion of endline data reconciliation. Although we did not register a separate Pre-Analysis Plan, the design, primary outcomes of interest and analysis sample are described in the trial registration document. More relevant, the analysis plan, primary outcome of interest (stunting) and power calculations are detailed in the 2016 Research Protocol that was delivered to our government partner for study approval prior to the onset of the RCT (https://elisamaffioli.files.wordpress.com/2021/12/160404_legacy_rct_research-protocol_final.pdf). The only change to the protocol that was made in 2019 prior to endline analysis (and described in the trial registry) was the decision to restrict the study to women who were pregnant at the time of program launch in order to mitigate concerns over endogenous selection into program benefits among those who become pregnant after the program starts. Our experiment was powered on the primary outcome of stunting. Assuming an ICC of 0.03 and standard deviation =1.14, it was determined that 20 women per cluster (34 clusters) were necessary to detect a change in mean HAZ score from -1.42 to -1.12, at 90% power and alpha=0.01. Similarly, assuming an ICC of 0.03, it was determined that a similar number of observations (21 women per cluster) were necessary to detect a reduction of 8 percentage points in the proportion of children stunted, at 80% power and alpha=0.05. Our analysis sample includes approximately 21 women in each of the 102 clusters.

respondents across treatment arms (see Appendix Table 2). The fact that observables are almost identical pre- and post-attrition waylays concern over differential attrition that could bias our estimates (see Appendix Table 2 vs Table 3).

The endline survey gathered data on household and individual characteristics, including weight and height data of all children under age 5, socio-economic status including income and assets, food consumption including dietary diversity, health-seeking behaviors emphasized in the SBCC sessions, credit and saving, decision-making, desired and realized fertility, and program participation. Our analysis estimates the effects of the interventions on the 2,154 children born to these women during the study, i.e, those covered by the LEGACY program for their first 1,000 days of life. At endline, the children that benefited from the full duration of treatment are between 22 and 35 months old.[16]

2.4 Empirical Strategy

The random assignment of interventions across villages allows us to identify the causal effect of cash transfers and the relative importance of pairing cash transfers with SBCC by comparing endline outcomes across study arms. We estimate program effects with the following ordinary least squares (OLS) model:

$$Y_{iv} = \alpha + \beta * [Cash + SBCC]_v + \gamma * [CashOnly]_v + \delta X_{iv} + t + \epsilon_{iv}$$

where Y is the primary health outcome of interest for child or mother i living in village v. To capture nutritional impacts on children, we use child height and age data from endline to construct height-for-age z-scores (HAZ), a well-validated anthropometric measure of chronic malnutrition using the WHO child growth standards (World Health Organization, 2006). A HAZ value of -1 indicates that, given sex and age, a child's height is one standard deviation below the median child in her age/sex reference group. In addition, we construct an indicator of stunting that equals one if

[16]Several additional women, who were not classified as pregnant at the time of the initial listing, either because they were unaware of, or reluctant to report pregnancy status early on, or because they were not found in the village at the time of the initial listing, were reclassified as eligible midway through the program and received program benefits thereafter. However, significantly more of such women were found in treatment relative to control villages (unsurprisingly, given their greater incentive to reveal themselves in order to receive benefits once they became aware of the program), which could bias our estimates of program effects were we to include them in the analysis. Hence, we restrict our evaluation of program impacts to women identified as pregnant at the onset of the study, prior to the announcement of the program.

$HAZ < -2$; an indicator of severe stunting that equals one if $HAZ < -3$; and an indicator of moderate stunting that equals one if $-3 <= HAZ < -2$.[17]

To better understand potential pathways of influence, we examine a number of behavioral outcomes available at endline that capture economic and health determinants of malnutrition, focusing on knowledge and behaviors emphasized in the SBCC curriculum. These include infant feeding practices (dietary diversity and breastfeeding), total expenditures on food and healthcare, illness episodes and visits to skilled health personnel, and hand-washing behavior.

$Cash + SBCC$ is an indicator of whether the respondent's village was assigned to T1, and $CashOnly$ is an indicator of whether the village was assigned to T2. The excluded group is the control group (CG). The model also controls for a number of predetermined observables, X, which include (i) individual demographic controls, including mother's age and education, household head's age and education, and child's sex and age (child-level analysis only); and (ii) village-level controls, including distance to large and small markets, indicators for main source of livelihood (agriculture, livestock, or casual labor), availability of government provided electricity, and participation in a concurrent water, sanitation and hygiene (WASH) intervention. Village-level controls were collected prior to the start of program activity. In accordance with our randomization design, the model includes fixed effects for geographic strata (one variable for each triplet of sub-rural health catchment areas, t), which control for unobserved characteristics that may vary across clusters and influence program delivery. Standard errors are clustered at the village cluster level, the unit of randomization.

To ensure that comparability across study arms was achieved by random assignment, we test for observable differences across experimental arms based on time-invariant individual characteristics (Appendix Table 3) and village-level data collected prior to the program launch (Appendix Table 1). Overall, the randomization was successful with only 3 out of 99 comparisons unbalanced across any of the treatment arms at a 95% confidence interval at the individual level, and 1 out of 99 unbalanced at a 90% confidence interval at the village level. Moreover, the mean differences that are significant are small in magnitude and work against our ability to attribute differences in stunting to treatment: for example, the head of the household's education is lower

[17] As a quality check, Appendix Figure 3 verifies no irregularity in the relationship between HAZ score and age of children in months. Note that, to measure anthropometrics in the field, enumerators were trained to follow SMART technology, the gold standard measurement protocols for nutrition-related anthropometrics (World Health Organization, 2006).

in the $Cash + SBCC$ arm, so could potentially bias downward a comparison of differences in stature at endline. We control for these unbalanced covariates in the empirical model.

It is important to note that our study design precludes non-random imbalances across treatment arms driven by selection into treatment. Although maternal cash transfers have the potential to incentivize women to become pregnant earlier than they would have otherwise or might encourage migration into treatment villages, our analysis sample is limited to women who were living in the village and pregnant *prior* to learning about the program, so is not subject to concerns over endogenous selection into the sample. However, because we run a handful of regressions on the sample of new mothers – the only group among which we are able to measure detailed infant feeding practices –, we also test directly whether the program led to fertility responses that could bias a comparison of infant feeding outcomes across experimental arms. Relative to the control group, women in $Cash + SBCC$ and $CashOnly$ are no more likely to be currently pregnant and do not report a greater number of pregnancies since the start of the program, indicating that there is no increase in fertility in response to the program (Appendix Table 4).

3 Results

Our primary outcome of interest - as specified in the trial registration - is stunting, which provides a cumulative measure of nutritional benefits in the first 1000 days of life to children born to mothers in the treatment groups. For comparison purposes, we also investigate flow measures of nutritional intake, including whether a child is underweight at endline, in order to gauge whether program beneficiaries continue to experience improved nutrition after program completion.[18]

3.1 Program Effects on Chronic Malnutrition

Table 1 reveals that, in control villages, a full 34% of children in this cohort are stunted, including 7% that are severely stunted. Table 1 shows that children born to mothers who received both cash and SBCC ($Cash+SBCC$, T1) from pregnancy until the child reached 24 months are an estimated 4.6 percentage points (a 13.5 percent reduction,

[18]Note that weight-based measures largely reflect recent nutritional intake and health status, whereas stunting is thought to reflect cumulative early childhood conditions (Hoddinott et al., 2013).

p<0.05) less likely to be stunted at 22-35 months of age compared to children living in control group households. Meanwhile, children in $CashOnly$ (T2) villages are no less stunted than children in the control group, and we can firmly reject the equality of $Cash + SBCC$ and $CashOnly$ treatment effects on stunting (p-value=0.02). This pattern supports the hypothesis that cash transfers – even those directed to mothers – are only able to effectively combat chronic malnutrition in children when paired with an intervention that encourages behavior change.[19] We do not find differential effects by gender of the child: results are statistically significant for both male (6.7 percentage points) and female (5.2 percentage points) children (Appendix Table 5).

Column 4 of Table 1 presents the treatment indicators regressed on a continuous measure of height for age, i.e., HAZ score. Although the $Cash + SBCC$ intervention arm does not have a statistically significant effect on the continuous measure of height, the point estimate is large (0.074) and close to significance at the 10% level.[20]

Moreover, a more nuanced test of the distribution of HAZ scores reveals program effects that are consistent with the stunting results.[21] In particular, Figure 1 reveals a rightward shift in the distribution of HAZ scores among $Cash + SBCC$ beneficiaries compared to the control group. We test whether the distributions are the same across interventions, using a Kolmogorov-Smirnov non-parametric test. We reject the null hypothesis of equal distributions of HAZ scores in the $Cash + SBCC$ and $CashOnly$ arms (p=0.048), and in the $Cash + SBCC$ arm and the control group (p=0.098). We also reject the null hypothesis of equal distributions of HAZ scores in the $CashOnly$ arm and control group (p=0.071).

Consistent with these distributional patterns, columns 2 and 3 of Table 1 show that the reduction in malnutrition achieved by the program corresponds to a decrease in

[19] We followed Newson and Team 2003 and Newson 2011 and report frequentist q-values (or adjusted p- values) for multiple-test procedures, by defining the discovery set to control either the familywise error rate (FWER) or the false discovery rate (FDR). For each input p-value, the corresponding q–value is the lowest input uncorrected critical p-value (FWER or FDR) which would cause the input p-value to be included in the discovery set, if the specified multiple-test procedure was applied to the full vector of P–values. To be conservative, we use a step-up method where the q-value for each p-value is equal to the cumulative minimum of all the r-values corresponding to p-values of rank equal to or greater than that p-value.

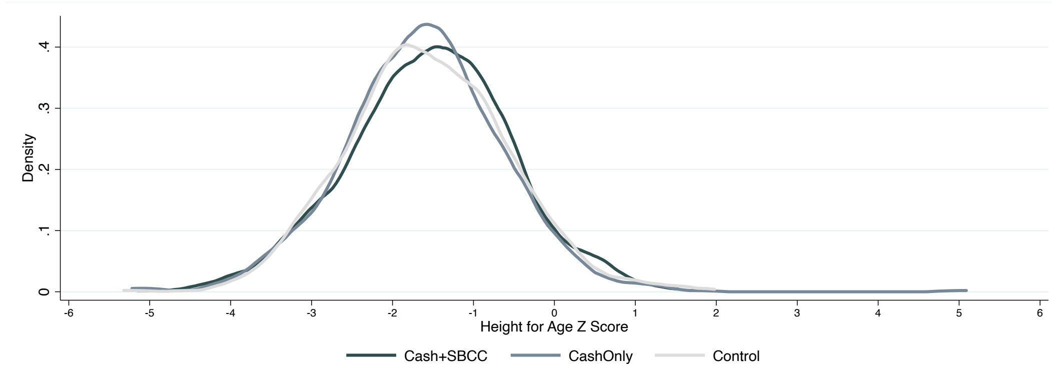
[20] As a point of comparison, the point estimate is similar in magnitude to a deworming intervention in Kenya that increased the mean HAZ by 0.09 SD, which was significant in a larger sample (Miguel and Kremer, 2004). Our estimates are also in line with the non-experimental estimates of HAZ impacts from other conditional or unconditional cash transfer programs (Biscaye et al., 2017; IEG World Bank, 2011) or early stimulation and nutrition interventions (Attanasio et al., 2018).

[21] Note that the distribution of HAZ is centered at zero only if the population follows the WHO standard growth curve, which is not the case in our context given the high prevalence of malnutrition.

Table 1: Child stunting

	(1)	(2)	(3)	(4)
	Child stunted	Child moder-ately stunted	Child severely stunted	HAZ score (WHO)
Cash+SBCC	-0.046**	-0.053***	0.007	0.074
	(0.021)	(0.018)	(0.011)	(0.047)
	[0.033]	[0.004]	[0.513]	[0.122]
CashOnly	-0.004	-0.008	0.004	-0.017
	(0.021)	(0.020)	(0.011)	(0.041)
	[0.846]	[0.687]	[0.728]	[0.681]
Observations	2151	2151	2151	2151
Mean Control	0.34	0.27	0.07	-1.57
Clusters	102	102	102	102
Cash+SBCC=CashOnly	0.02	0.01	0.75	0.02

Notes: The table presents OLS estimates of the effects of the maternal cash transfer program interventions on measures of stunting for children whose mothers were pregnant at enrollment, following WHO classification."Cash+SBCC" indicates T1 villages, where cash transfers and SBCC activities were provided jointly; "CashOnly" indicates T2 villages, where only cash transfers were provided; the reference group are villages in the control group where neither cash transfers nor SBCC took place. Outcomes include the proportion of children stunted as children with Height for Age Z score (HAZ) < -2 (1); the proportion of children moderately stunted as children with HAZ < -2 and >= -3 (2); the proportion of children severely stunted as children with HAZ < -3 (3); and, HAZ (4). Controls include (i) individual demographic controls, including child's sex and age, mother's age and education, and household head's age and education; (ii) village-level controls, including distance to large and small markets, main source of livelihood (agriculture, livestock, or casual labor), availability of government provided electricity, and participation in a concurrent WASH intervention. Fixed effects per geographic strata (34) are included. Standard errors are clustered at the village cluster level and reported in parenthesis. Adjusted pvalues are calculated to account for multiple hypothesis testing following Newson and Team (2003) and Newson (2011) and reported in squared brackets. P-values from t-test from the difference in means are reported. ***p<0.01, ** p<0.05, * p<0.1.



Notes: This figure describes the distribution of Height for Age Z score (HAZ) for children whose mothers were pregnant at enrollment, by treatment status. "Cash+SBCC" indicates T1 villages, where cash transfers and SBCC activities were provided jointly; "CashOnly" indicates T2 villages, where cash transfers only were provided; "Control" indicates villages in the control group where neither cash transfers nor SBCC took place.

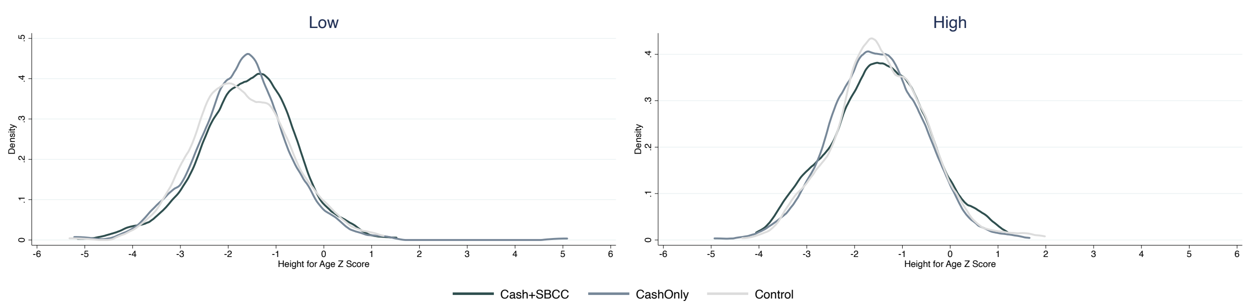
Figure 1: Child HAZ distribution, by treatment

the proportion of children moderately stunted (5.3 percentage points, a 19.6 percent reduction, $p<0.01$) but no change in the proportion severely stunted. This implies that, while chronic malnutrition in early childhood fell for many children at risk, the SBCC intervention did not succeed in combating malnutrition among the most vulnerable households.

Although severe stunting is relatively low in this setting (7% in the control group), the absence of a program effect on this tail of the HAZ distribution is somewhat counterintuitive given that severe stunting is likely to be concentrated among the poorest households, and one might anticipate that the same amount of cash makes a bigger difference for households in more dire circumstances. One explanation that we consider is lower SBCC participation rates of extremely poor households. However, self-reported data from endline do not indicate significantly lower participation rates among households below median income relative to those above median income. Alternatively, the SBCC curriculum or mode of delivery may be inappropriately designed to meet the needs of very poor participants. For instance, households at risk of severe stunting may lack sufficient human capital to translate information into behavior change, or might face additional financial barriers to implementing changes such as diet diversification, even with the additional liquidity provided through a cash transfer. Finally, households at risk of severe stunting may be concentrated in villages with poor infrastructure to support the adoption of certain health practices such as access to clean water or food products.

To further evaluate whether the absence of a program effect on severe stunting is related to differences in socio-economic status (SES), we examine patterns of treatment effects across villages according to village-level SES. In the absence of baseline data on wealth or income, we proxy for village SES with the average number of years of education attained by women in the sample.

It is first worth noting that rates of severe stunting are similar in magnitude across villages with low versus high average levels of parental education. This pattern alone suggests that some fraction of the population may face a poverty trap such as chronic reinfection that keeps them in a state of persistent malnutrition even when village resource levels rise. Interestingly, results from the subsample analysis indicate that the program effects are concentrated in low SES villages. In particular, we observe that the distribution of the HAZ scores is strongly shifted to the right in the $Cash+SBCC$ intervention arm compared to the $CashOnly$ arm or the control group only in low SES villages (Figure 2), and the difference is statistically significant. Meanwhile, in the above-median villages, HAZ score distributions are similar across experimental arms. Appendix Table 6 shows a similar heterogeneous pattern in a regression framework: the results indicate that relatively low-SES villages gain the most in terms of reductions in rates of stunting from maternal cash transfers combined with SBCC.



Notes: This figure describes the distribution of Height-for-age Z-score (HAZ) for children whose mothers were pregnant at enrollment, by treatment status, and by low vs high socio-economic status. As a proxy, we use the average number of years of education attained by women in the sample. "Cash+SBCC" indicates T1 villages, where cash transfers and SBCC activities were provided jointly; "CashOnly" indicates T2 villages, where cash transfers only were provided; "Control" indicates villages in the control group where neither cash transfers nor SBCC took place.

Figure 2: Child HAZ distribution by women village-level average education

Moreover, households below median income *within* the low-SES village benefit disproportionately in terms of reductions in moderate stunting relative to those above the village median (Appendix Table 7). That is, if we divide the sample into four groups according to both village-level SES and median household income at endline, the pattern of results indicates that reductions in moderate stunting due to exposure to cash plus SBCC are fully concentrated among the quartile of households in the lower half of the income distribution *within* the lowest SES half of villages. Still, even among this subsample, severe stunting does not improve with either version of the program, consistent with non-convexities at extremely low levels of income.

Figure 2 also reveals that, within low-SES villages, the $CashOnly$ and the $Cash + SBCC$ treatments appear to operate on the same distribution of children who are on the left-hand side of the stunting distribution, but the $Cash + SBCC$ treatment appears to push them relatively further rightward in terms of HAZ scores relative to the $CashOnly$ treatment. That is, the patterns of HAZ score distributions indicates that the $CashOnly$ treatment is effective for the same number of marginal responders as the $Cash + SBCC$ treatment just to a lesser extent, rather than being equally effective for fewer kids.

We also present estimates of program participation on a flow measure of malnutrition, a binary indicator of a child being underweight (Table 2). It is important to note that, by the time of endline, the majority of children in our sample were 22-35 months old and thus they had not received cash supplements for several months. For this reason we are less likely to observe differences in underweight at endline relative to what we might have observed during program implementation since flow measures of malnutrition are more likely to change during the program rather than after the program ended. This is in contrast to program effects on stunting, which are likely to be observed indefinitely. Differences in flow measures of nutritional intake at endline would indicate that the program benefits on nutrition continue even when the cash transfer is taken away from mothers, potentially because of habit formation in feeding behavior, knowledge gained directly through program participation, or indirect learning-by-doing about the value of dietary diversity. The evidence in Table 2 suggests no persistence in nutritional benefits (as measured by a child being underweight) of cash transfers once the transfer is terminated.

Table 2: Child underweight

	(1)	(2)	(3)	(4)
	Child underweight	Child moderately underweight	Child severely underweight	WAH score (WHO)
Cash+SBCC	-0.012	-0.027	0.016	-0.004
	(0.025)	(0.023)	(0.012)	(0.050)
	[0.641]	[0.227]	[0.197]	[0.934]
CashOnly	-0.006	-0.026	0.021	-0.036
	(0.024)	(0.022)	(0.012)	(0.049)
	[0.812]	[0.237]	[0.103]	[0.474]
Observations	2145	2145	2145	2145
Mean Control	0.28	0.24	0.04	-1.43
Clusters	102	102	102	102
Cash+SBCC=CashOnly	0.83	0.96	0.72	0.49

Notes: The table presents OLS estimates of the effects of the maternal cash transfer program interventions on measures of stunting for children whose mothers were pregnant at enrollment, following WHO classification."Cash+SBCC" indicates T1 villages, where cash transfers and SBCC activities were provided jointly; "CashOnly" indicates T2 villages, where only cash transfers were provided; the reference group are villages in the control group where neither cash transfers nor SBCC took place. Outcomes include the proportion of children underweight as children with Weight for Age Z score (WAH) < -2 (1); the proportion of children moderately underweight as children with WAH < -2 and >= -3 (2); the proportion of children severely underweight as children with WAH < -3 (3); and, WAH (4). Controls include (i) individual demographic controls, including child's sex and age, mother's age and education, and household head's age and education; (ii) village-level controls, including distance to large and small markets, main source of livelihood (agriculture, livestock, or casual labor), availability of government provided electricity, and participation in a concurrent WASH intervention. Fixed effects per geographic strata (34) are included. Standard errors are clustered at the village cluster level and reported in parenthesis. Adjusted pvalues are calculated to account for multiple hypothesis testing following Newson and Team (2003) and Newson (2011) and reported in squared brackets. P-values from t-test from the difference in means are reported. ***p<0.01, ** p<0.05, * p<0.1.

3.2 Mechanisms

To better understand the channels through which a combination of $Cash + SBCC$ generates positive effects on child health, in this section we explore program effects on maternal health behaviors, including the amount and type of food consumed, as measured by total food consumption and child food diversity, and health-care utilization as measured by total health expenditures. We also investigate whether treatment is associated with increases in maternal knowledge about child health production. Finally, to disentangle whether stunting impacts are driven by reductions in nutrition-depleting illnesses versus increases in the intake of nutritious food, we examine whether treatment is associated with reductions in reported episodes of child illness.

3.2.1 Program Effects on Maternal Health Behaviors

Table 3 describe the program impacts on behaviors that were emphasized in the SBCC curriculum. Specifically, we focus on the following key topics covered by the education sessions: food consumption, dietary diversity, breastfeeding, hand washing practices, and health-seeking behavior. To capture dietary diversity, we take the standard approach in the literature (based on WHO guidelines) of constructing a dietary diversity score (DDS) measured as the number of food groups consumed by the child in the previous day out of the following seven: (1) cereals, roots and tubers; (2) legumes and nuts; (3) milk and milk derivatives; (4) meat products (meat, poultry, offal, and fish); (5) eggs; (6) vitamin A-rich fruits and vegetables (leafy green vegetables, yellow fruits and vegetables); and (7) other fruits and vegetables. A DDS of four is considered the minimum DDS for a healthy diet. As children in our sample are at least 22 months old, the DDS is measured excluding milk, following WHO guidelines.

Because stunting is associated with low levels of protein-rich foods in particular, we also look specifically at how treatment assignment influences food consumption in categories 2-5 aggregated. Not only are higher levels of protein-rich food consumption most likely to translate into reductions in child stunting, but – in addition to emphasizing the general importance of food diversity – SBCC health messaging focused specifically on the importance of feeding young children with protein-rich food groups.

Hand-washing practices are measured as a cumulative score of regularly adopted practices, where each practice is counted as 1 when the respondent reports washing hands with soap in that specific situation and 0 otherwise: after cleaning a baby's bottom, after using the toilet, before preparing and eating food, before feeding children, after disposing of baby feces, before and after handling children, and on other occasions. Total food consumption is measured as recalled household consumption in the past 7 days and is winsorized at the 99th percentile level.[22]

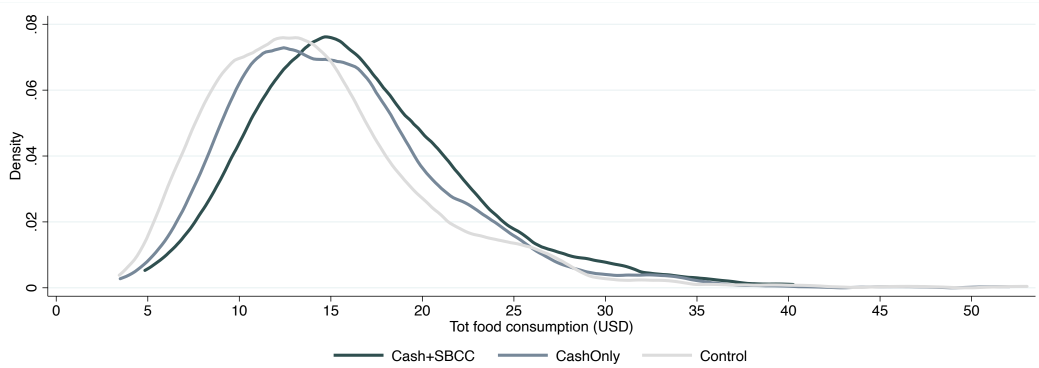
Table 3: Maternal health behaviors

	(1)	(2)	(3)	(4)	(5)	(6)
	Tot. food consumption (USD)	Child dietary diversity score (non-milk, 24 hrs recall)	Child ever breastfed	Child received colostrum	Index of hand-washing behavior (0-9)	Mother with at least 4 ANC visits to skilled health personnel
Cash+SBCC	2.168***	0.655***	0.007*	0.021**	0.651***	0.161***
	(0.373)	(0.063)	(0.003)	(0.008)	(0.128)	(0.024)
CashOnly	1.097***	0.096	0.003	0.001	0.151	0.117***
	(0.365)	(0.070)	(0.004)	(0.010)	(0.118)	(0.024)
Observations	2134	2154	2154	2151	2134	2134
Mean Control	14.33	3.39	0.99	0.96	2.60	0.67
Clusters	102	102	102	102	102	102
Cash+SBCC=CashOnly	0.00	0.00	0.26	0.06	0.00	0.05

Notes: The table presents OLS estimates of the effects of the maternal cash transfer program interventions on measures of behavior related to four topics covered by the education sessions in SBCC activities (1) IYCF -including diet diversity (column 1) and breastfeeding (columns 2-3), hand-washing practices (column 4), health-seeking behavior (column 5) and food expenditures (column 6). "Cash+SBCC" indicates T1 villages, where cash transfers and SBCC activities were provided jointly; "CashOnly" indicates T2 villages, where cash transfers only were provided; the reference group are villages in the control group where neither cash transfers nor SBCC took place. Outcomes include: total food consumption, winsorized at the 99th percentile level (in last 7 days, in USD, exchange rate at 31 December 2018, 1); child diversity score constructed following WHO standards, for children at least 22 months old (2); the proportion of children ever breastfed (3); the proportion of children who received colostrum (4); an index of hand washing practices combining whether mothers report always washing hands after cleaning a baby's bottom, after using the toilet, before preparing and eating food, before feeding children, after disposing of baby feces, before and after handling children, and on other occasions (5); the proportion of mothers receiving at least 4 Antenatal Care visits with skilled health personnel, as defined by WHO standards (6); Controls include (i) individual demographic controls, including child's sex and age, mother's age and education, and household head's age and education for child-level analysis; mother's age and education, and household head's age and education for mother-level analysis; (ii) village-level controls, including distance to large and small markets, main source of livelihood (agriculture, livestock, or casual labor), availability of government provided electricity, and participation in a concurrent WASH intervention. Fixed effects per geographic strata (34) are included. Standard errors are clustered at the village cluster level and reported in parenthesis. ***p<0.01, ** p<0.05, * p<0.1.

Consistent with the stunting results, women assigned to the $Cash + SBCC$ intervention spend significantly more money on food relative to the control group (increase of 2.2 USD, column 1). Women assigned to the $CashOnly$ intervention also exhibit a positive change, but the increase in spending is significantly less stark: weekly food consumption in the $CashOnly$ arm is 7.65% higher than the control group compared with a 15.13% difference among the $Cash + SBCC$ arm (column 1). The changes in the weekly food consumption represents about 17% (for $CashOnly$) and 33% (for

[22] Results are robust to using raw consumption data.



This figure describes the distribution of the total food consumption in the last 7 days by treatment status. "Cash+SBCC" indicates T1 villages, where cash transfers and SBCC activities were provided jointly; "CashOnly" indicates T2 villages, where cash transfers only were provided; "Control" indicates villages in the control group where neither cash transfers nor SBCC took place.

Figure 3: Food consumption distribution by treatment status

$Cash + SBCC$) of the cash amount (10,000MMK), suggesting a meaningful change in household food consumption that could account for large changes in stunting. This increase in food consumption is also reflected in a statistically significant rightward shift in the distribution (Figure 3) for $Cash + SBCC$ compared to $CashOnly$ and to the control group (p-value=0.00), when using a Kolmogorov-Smirnov non-parametric test for equality of distributions. Instead there is no statistically significant difference in the food consumption distribution between $CashOnly$ and the control group (p-value=0.81).

We also find positive changes in behaviors related to child food diversity and breastfeeding practices (Table 3), and these results are all significantly larger for the $Cash+SBCC$ arm. In particular, we find a 0.655 unit increase in the child food diversity score (column 2) in the $Cash + SBCC$ arm that is significantly different from the $CashOnly$ and the control group. We also find a change in the proportion of children ever breastfed (0.7 percentage points, column 3) and in the proportion of children who received colostrum (2.1 percentage points, column 4) in the $Cash + SBCC$ intervention arm only. Although these treatment effects are statistically significant, it is worth noting that they are small in magnitude on account of the near universality of these practices prior to the intervention, as exhibited also by the high control group means. Essentially, SBCC participation shifts the fraction of children receiving colostrum from 96% to 98%, relative to both $CashOnly$ and the control group. Finally, we find a 0.651

unit increase in the index of hand-washing behavior (column 5) in the $Cash + SBCC$ group relative to both $CashOnly$ and the control group, as well as a significant increase in the proportion of mothers attending at least 4 antenatal care visits (column 6) that is observed in both treatment arms but is significantly higher for the $Cash + SBCC$ arm (16.1 percentage points for $Cash + SBCC$ and 11.7 percentage points for $CashOnly$).

The absence of a program effect from cash alone on WASH and breastfeeding behaviors is unsurprising given that income alone should not be expected to increase rates of early initiation, so this can be readily interpreted as an impact of information on maternal health behavior. In contrast, ANC visits among the $Cash + SBCC$ arm have the potential to be influenced by both an income effect of receiving cash transfers as well as an information effect of SBCC participation. However, the *difference* in health-seeking behavior between $CashOnly$ and $Cash + SBCC$ can be interpreted as the impact of information on health care utilization whereas the treatment effect women in the $CashOnly$ group picks up the income effect on health-seeking behavior.

Table 4: Household budget shares of food consumption

	(1)	(2)	(3)	(4)	(5)
	Animal proteins	Vegan proteins	Vegetables and fruits	Staples	Other
Cash+SBCC	0.058***	0.015***	0.008**	-0.040***	-0.039***
	(0.006)	(0.002)	(0.004)	(0.007)	(0.003)
CashOnly	0.038***	0.005*	-0.001	-0.015**	-0.027***
	(0.007)	(0.002)	(0.004)	(0.007)	(0.004)
Observations	2134	2134	2134	2134	2134
Mean Control	0.28	0.05	0.15	0.34	0.19
Clusters	102	102	102	102	102
Cash+SBCC=CashOnly	0.00	0.00	0.00	0.00	0.00

Notes: The table presents OLS estimates of the effects of the maternal cash transfer program interventions on household budget shares of food consumption. Outcomes include the share of total household food consumption spent on animal proteins (dairy, meat and fish, eggs, 1); vegan proteins (pulses and nuts, 2); vegetables and fruits (3); staples (4) and other food, including oil and other condiments (5). "Cash+SBCC" indicates T1 villages, where cash transfers and SBCC activities were provided jointly; "CashOnly" indicates T2 villages, where cash transfers only were provided; the reference group are villages in the control group where neither cash transfers nor SBCC took place. Controls include (i) individual demographic controls, including child's sex and age, mother's age and education, and household head's age and education for child-level analysis; mother's age and education, and household head's age and education for mother-level analysis; (ii) village-level controls, including distance to large and small markets, main source of livelihood (agriculture, livestock, or casual labor), availability of government provided electricity, and participation in a concurrent WASH intervention. Fixed effects per geographic strata (34) are included. Standard errors are clustered at the village cluster level and reported in parenthesis. P-values from t-test from the difference in means are reported. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

The results in Table 3 column 1 highlight how cash transfers with and without SBCC increase total household food consumption. Table 4 additionally explores the shares of the household food budget (in the last 7 days) spent on specific categories of foods: (animal or vegan) protein-rich foods, fruits and vegetables, staple carbohydrates including rice, wheat, maize, soybeans, potatoes, and other food, including oil and condiments. We find that cash transfers with or without SBCC increased household consumption of animal proteins (meat, fish, eggs and dairy), while decreasing consumption of less nutritious food such as staples and other food. However, the increase is statistically significantly higher for $Cash + SBCC$ relative to $CashOnly$ (5.8 percentage points vs 3.8 percentage points, respectively, column 1). In addition, those households exposed to the SBCC curriculum spent more of their budget on vegan proteins (pulses, 1.5 percentage points) and slightly more on vegetables and fruits (0.5 percentage points).[23]

Reassuringly, these patterns are also reflected in measures of child dietary intake reported in food diaries. In particular, Table 5 investigates food intake from various food groups among the sample of children in the endline analysis sample. Data on child diets isolate changes in child feeding practices among mothers exposed to the SBCC curriculum rather than just household-level changes in food consumption, which may not be directed towards children. Table 5 shows clearly that children's (reported) food intake improved systematically in the $Cash + SBCC$ arm. Mothers in villages where cash transfers were supplemented with SBCC report that their children were 13.6 percentage points more likely to consume animal proteins than those in the control group. Households in the $CashOnly$ arm are also 7.7 percentage points more likely to eat animal proteins than households in the control group, but the difference is significantly lower that observed among children in the $Cash+SBCC$ arm (p-value=0). In addition, children in households exposed to SBCC are also 21.7 percentage points and 9.3 percentage points more likely to consume vegan proteins and vegetables and fruits, respectively, compared to children in the control group. Instead, we do not find any statistically significant changes in children's diets for the $CashOnly$ arm, and more generally we do not observe shifts towards less nutritious food (columns 4 and 5).

[23] As shown in related research (Carneiro et al., 2021), if the program changed behavior and the effects were sustained, it is unsurprising that we find impacts on budget shares of food consumption in the previous seven days, even if the program have stopped some months before endline for the older children in the sample. We are currently exploring whether the effects of the interventions were sustained up to 2 years after the end of the program, and whether beneficiary households were resilient to the economic shock of the current COVID-10 pandemic in a companion paper.

Table 5: Inclusion of protein-rich food groups in children's diet

	(1)	(2)	(3)	(4)	(5)
	Animal proteins	Vegan proteins	Vegetables and fruits	Staples	Other
Cash+SBCC	0.136***	0.217***	0.093***	0.005	0.005
	(0.015)	(0.025)	(0.022)	(0.006)	(0.006)
CashOnly	0.077***	0.025	-0.012	-0.001	0.001
	(0.015)	(0.024)	(0.023)	(0.007)	(0.006)
Observations	2154	2154	2154	2154	2154
Mean Control	0.80	0.31	0.76	0.99	0.99
Clusters	102	102	102	102	102
Cash+SBCC=CashOnly	0.00	0.00	0.00	0.26	0.34

Notes: The table presents OLS estimates of the effects of the maternal cash transfer program interventions on children's diet. Outcomes include the proportion of children eating: animal proteins (dairy, meat and fish, eggs, 1); vegan proteins (pulses and nuts, 2); vitamin-rich vegetables and fruits (3); staples (4); other, including oil and other condiments (5)."Cash+SBCC" indicates T1 villages, where cash transfers and SBCC activities were provided jointly; "CashOnly" indicates T2 villages, where cash transfers only were provided; the reference group are villages in the control group where neither cash transfers nor SBCC took place. Controls include (i) individual demographic controls, including child's sex and age, mother's age and education, and household head's age and education for child-level analysis; mother's age and education, and household head's age and education for mother-level analysis; (ii) village-level controls, including distance to large and small markets, main source of livelihood (agriculture, livestock, or casual labor), availability of government provided electricity, and participation in a concurrent WASH intervention. Fixed effects per geographic strata (34) are included. Standard errors are clustered at the village cluster level and reported in parenthesis. P-values from t-test from the difference in means are reported. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

These patterns suggest that one source of explanation for why cash transfers alone increase food expenditure but do not reduce stunting may be that the additional food is not going towards children's diets. This implies that one key mechanism through which SBCC might enhance the malnutrition impact of cash transfers is by convincing parents to direct increases in food consumption towards young children. For instance, they may be feeding school-going children without realizing the importance of critical windows of growth.[24]

This pattern of results on program effects on specific food groups is consistent with the observed findings on child stunting, as protein-rich foods are generally needed to generate medium-run changes in chronic malnutrition through diet alone. In particular, a large literature in medicine and nutrition posits that nutrients from proteins primarily support child growth. Animal proteins from meat and fish are a source of amino acids (Laplante and Sabatini, 2012; Semba et al., 2016a,b), dairy products – and specifically cow milk – are an important source of amino acids and micronutrients (calcium, vitamin A, zinc) (Molgaard et al., 2011; Iannotti et al., 2013; Dyer et al., 2016), and eggs are an excellent source of choline (Semba et al., 2016c; Bekdash, 2016). Similarly, vegan proteins contain essential amino-acids, although in smaller doses. Meanwhile, the key nutritional value of vegetables and fruits is the vitamins (including vitamin A) and minerals (Gilbert, 2013) they contain. While vitamin deficiencies can increase risk of infections, their impact on stunting is likely to be of second-order importance compared to the role of protein-rich foods.

Compared to animal proteins, households are less likely to increase consumption of non-animal proteins and vitamin-rich foods without outside information and encouragement since they are less likely to be informed about the nutritional value of these food groups. The fact that SBCC is successful in promoting child consumption of vegan proteins in addition to greater consumption of animal proteins is particularly valuable given that vegan proteins are likely to be significantly more cost-effective means of increasing child protein intake.

Consistent with the changes in behavior, we find treatment effects on maternal knowledge (Appendix Table 8). Overall, we observe that maternal knowledge is quite high, with more than 90% of mothers in the control group reporting knowing that child food diversity is important (91%), the correct meaning of exclusive breastfeeding

[24]Unfortunately, our data do not allow us to test this directly by looking at program effects on weight-for-height among older children and adults in the sample as biometric data were only collected for young children.

(94%), the best time to initiate breastfeeding (98%), and the best time to introduce complementary feeding (81%). While these high means suggest little scope for the SBCC intervention to improve maternal health knowledge, we find a small but statistically significant difference between $Cash + SBCC$ (0.143 SD) and $CashOnly$ (0.063 SD) when knowledge is indexed across all survey measures (pvalue=0.03). The absence of stronger treatment effects on individual indicators of maternal knowledge may reflect the fact that the survey instrument poorly captured key changes in knowledge gleaned through the program. Alternatively, SBCC may have operated not by changing specific knowledge of child health production, per se, but rather by way of cultural or social capital created during a variety of SBCC activities among mothers and other members of the community.

3.2.2 Program Effects on Child Illness

The evidence on maternal behavior indicates that reductions in child stunting were achieved in the $Cash + SBCC$ treatment by encouraging mothers to increase total food consumption and shift children's diets towards a broader array of protein-rich foods. In addition, combining cash transfers with SBCC appears to have encouraged a higher rate of early initiation of breastfeeding, better hand-washing practices, and greater use of prenatal care. While these specific behaviors are unlikely to account for the large reduction in stunting observed in Table 1, it is possible that they correlate with a broader range of changes in infant feeding practices (e.g. longer duration of exclusive breastfeeding) and health care utilization (e.g. use of Oral Rehydration Therapy to curtail episodes of diarrheal disease) and disease control measures that could have more direct effects on stunting but are unobserved in our data. That is, some of the impact on stunting may have resulted from changes in health behaviors other than food intake if they led to significant reductions in diarrheal disease, which in and of itself can produce chronic undernourishment in children.

To ascertain whether this is a source of stunting impacts, it is useful to note that hand-washing, breastfeeding exclusivity and health-seeking behavior can only impact child stunting via a reduction in nutrition-depleting illness. Hence, if these behavior changes contributed to reductions in child stunting, we should see corresponding reductions in diarrheal disease in the $Cash + SBCC$ arm. Likewise, although antenatal care is unlikely to impact stunting itself, it could be correlated with an increase in expenditures on other health care services like oral rehydration therapy (ORT) that could have directly reduced nutrition-depleting illnesses.

Hence, to provide further evidence on the mechanisms through which the program reduces stunting, we examine survey data on child illness episodes and health care expenditures as a proxy for severity of illness. As shown in Table 6, we do not find any evidence that the interventions led to changes in whether the child was brought in for treatment (column 4-5), or on total annual health expenditures on children under five (column 6). We find weak evidence that children were less likely to experience diarrhea in the last two weeks, but the effects are not statistically significantly different between the $CashOnly$ and $Cash + SBCC$ arms (p-value=0.39), suggesting that SBCC, along with cash, did not contribute more to reducing the risk of infections. There are also no significant effects on the likelihood that children had pneumonia or fever in the past two weeks.[25]

Finally, we confirm that the effects on child health are not driven by disproportionate changes in women's decision-making power in the $Cash + SBCC$ arm. It is first worth noting that the pattern of results does not indicate that changes in child health are driven by an increase in female financial empowerment given that women in both treatment groups received the same amount of cash but only those in the $Cash + SBCC$ arm exhibit improvements in child health. However, it is theoretically possible that female decision-making power improved disproportionately in the $Cash + SBCC$ arm due to an interaction effect on female agency of cash provision in conjunction with increased knowledge from participation in SBCC sessions. To rule out this mechanism, we use endline data on spousal decision-making over various spending categories. Appendix Table 9 shows no differential effects on female decision-making of either $CashOnly$ or $Cash + SBCC$. Endline data indicate that mothers in both $CashOnly$ and $Cash + SBCC$ are no more likely to decide on expenditures from their own or spouse's earnings, health care, major household purchases, or visiting relatives, than those in the control group.

[25] We observe a reduction in diarrhea, but no effects on health seeking behavior. One one hand, it is possible that the interventions reduced illnesses, thus reducing demand for health care. On the other hand, cash transfers could increase demand for health care, alleviating financial constraints. This would potentially result in a net null effect. Yet, conditioning on experiencing a illness, we do not find impacts on health seeking behavior (not shown), suggesting that this is not the case.

Table 6: Child Illness, seeking behavior and health expenditures

	(1)	(2)	(3)	(4)	(5)	(6)
	Diarrhea	Pneumonia	Fever	Seek treatment	Pay treatment	Health expenditures (children U5)
Cash+SBCC	-0.014*	0.003	0.008	-0.021	-0.022	0.417
	(0.008)	(0.027)	(0.026)	(0.020)	(0.021)	(2.516)
CashOnly	-0.020**	0.002	-0.017	-0.008	-0.005	-0.033
	(0.008)	(0.026)	(0.025)	(0.023)	(0.025)	(2.143)
Observations	2154	2154	2154	2154	2153	2134
Mean Control	0.03	0.20	0.72	0.89	0.86	27.75
Clusters	102	102	102	102	102	102
Cash+SBCC=CashOnly	0.39	0.93	0.31	0.56	0.46	0.85

Notes: The table presents OLS estimates of the effects of the maternal cash transfer program interventions on children outcomes. Outcomes include: the proportion of children with diarrhea in the past two weeks (1); the proportion of children with pneumonia in the past two weeks (2); the proportion of children with fever in the past two weeks (3); the proportion of children who sought treatment for that illness (4); the proportion of children who payed for the treatment (5); total health expenditures for children under 5 years old in the last year (in USD, 6). "Cash+SBCC" indicates T1 villages, where cash transfers and SBCC activities were provided jointly; "CashOnly" indicates T2 villages, where cash transfers only were provided; the reference group are villages in the control group where neither cash transfers nor SBCC took place. Controls include (i) individual demographic controls, including child's sex and age, mother's age and education, and household head's age and education for child-level analysis; mother's age and education, and household head's age and education for mother-level analysis; (ii) village-level controls, including distance to large and small markets, main source of livelihood (agriculture, livestock, or casual labor), availability of government provided electricity, and participation in a concurrent WASH intervention. Fixed effects per geographic strata (34) are included. Standard errors are clustered at the village cluster level and reported in parenthesis. P-values from t-test from the difference in means are reported. *** p<0.01, ** p<0.05, * p<0.1.

3.3 Robustness Checks

Although administrative program data show no documented cases of cash being delivered through the program to individuals residing in control villages, two forms of contamination in the SBCC intervention may compromise the validity of some of our estimates. First, according to SCI's administrative data on the SBCC rollout, 18 villages assigned to the *CashOnly* treatment received SBCC activities for 20 rather than 30 months because of an error in program implementation. Second, SBCC activities were expanded to all *CashOnly* villages beginning on January 1, 2019, although part of the endline data were collected after December 31, 2018 at which point those respondents had already received at least one month of SBCC activities.

To address these two issues, we re-run the analysis excluding those 18 *CashOnly* villages and all mothers interviewed after December 2018, for a total of 138 mothers or 6.5 percent of the 2,134 women in the analysis sample. As reported in Appendix Table 10, the results are robust to these exclusions.

In addition, the main results are robust to considering the full endline sample (Appendix Tables 11) identified in the 2017 listing of pregnant mothers, which includes women who may have migrated into the village or become pregnant after announcement of the program. Results are also robust to the clustering of standard errors at the level of the program delivery (village) rather than the unit of randomization (health center catchment area), as shown in Appendix Table 12. Finally, the main results on moderate stunting are robust in a specification that includes only unbalanced covariates or excludes control variables (Appendix Table 13).

4 Conclusion

Our findings provide novel evidence from anthropometric data that (unconditional) maternal cash transfer programs delivered for the first 1000 days of life lead to statistically significant reductions in the proportion of children (moderately) stunted, but only when they are combined with intensive Social Behavior Change Communication (SBCC). The significant effects on stunting are concentrated among below-median-income households in low-SES villages, consistent with the notion that nutrition programs matter most where vulnerability to malnutrition is highest, yet the combination of interventions improved outcomes only among those at risk of moderate but not severe stunting. These patterns provide insight into which sub populations are most easily reached by such interventions, and indicate that greater efforts are needed to combat severe malnutrition. Tailoring SBCC programming to households with low levels of resources or facing chronic infections may be needed to address the most severe cases of malnutrition.

The program was successful in changing a number of maternal health behaviors, including total food consumption, dietary diversity, breastfeeding, hand-washing practices, and utilization of prenatal care. However, given that we do not find evidence that the combined interventions reduced child illness episodes, our pattern of results suggests that SBCC succeeded in reducing child malnutrition primarily through improvements in children's diet, including a more diverse diet, and in particular protein-rich foods. In contrast, while cash alone increased child food intake and consumption of animal proteins, the changes were significantly smaller than those observed in the $Cash + SBCC$ arm, and there was no increase in consumption of the more affordable vegan proteins emphasized in the SBCC curriculum. Moreover, the changes in child diet achieved through cash alone were insufficient to improve anthropometric indicators of malnutrition in young children who are in a critical window of growth.

Together, these findings underscore the importance of adding information components to social safety net programs involving cash disbursement in order to successfully change investment in human capital and thereby disrupt the inter-generational cycle of poverty. In our setting, the interventions show that (unconditional) cash transfers alone are insufficient to reduce chronic malnutrition. Instead, providing mothers with knowledge on how to use the additional disposable income can be transformative in reducing stunting. More generally, our analysis reveals that the combination of cash and SBCC tackled both the financial and information constrains that households faced in our context, and SBCC was fundamental in changing mothers' feeding practices, thus reducing chronic malnutrition.

Further research is needed to better understand which particular curricular components are key to maximizing the child health gains of maternal cash transfer programs. In addition, more research is needed to establish whether information alone would be similarly effective in improving child health outcomes.

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Appendix: Additional Tables and Figures

Table 1: Balance on village-level characteristics

	T1	T2	CG	PV (T1-CG)	PV (T2-CG)	PV (T1-T2)	N(T1/T2/CG)
Tot. population (No. HH)	182.17	175.54	160.92	0.263	0.418	0.747	133 / 135 / 139
	(128.98)	(135.99)	(106.39)				
Tot. literacy rate	85.19	85.50	83.58	0.384	0.247	0.866	133 / 135 / 139
	(13.45)	(12.50)	(12.87)				
Main livelihood: Agriculture	0.92	0.85	0.91	0.654	0.312	0.145	133 / 135 / 139
	(0.26)	(0.36)	(0.29)				
Main livelihood: Livestock	0.29	0.30	0.24	0.583	0.500	0.887	133 / 135 / 139
	(0.45)	(0.46)	(0.43)				
Main livelihood: Casual Labor	0.77	0.79	0.76	0.779	0.714	0.885	133 / 135 / 139
	(0.42)	(0.41)	(0.43)				
Type land-dry land farming	0.53	0.64	0.55	0.837	0.370	0.297	133 / 135 / 139
	(0.50)	(0.48)	(0.50)				
Type land-flood plains or irrigated	0.47	0.35	0.44	0.780	0.322	0.229	133 / 135 / 139
	(0.50)	(0.48)	(0.50)				
Accessible by car/truck in all weather	0.79	0.79	0.75	0.614	0.677	0.955	133 / 135 / 139
	(0.41)	(0.41)	(0.44)				
Village has Gov electricity	0.24	0.22	0.22	0.839	0.992	0.837	133 / 135 / 139
	(0.43)	(0.42)	(0.42)				
Village has primary school	0.63	0.58	0.59	0.544	0.832	0.411	133 / 135 / 139
	(0.48)	(0.50)	(0.49)				
Village has small markets	0.02	0.05	0.03	0.735	0.352	0.231	133 / 135 / 139
	(0.15)	(0.22)	(0.17)				
Village has home markets	0.97	0.96	0.96	0.773	0.729	0.551	133 / 135 / 139
	(0.17)	(0.21)	(0.19)				
Distance to large market	34.76	32.96	40.24	0.310	0.150	0.687	133 / 135 / 139
	(24.83)	(20.05)	(26.39)				
Distance to small markets	24.77	20.46	28.07	0.487	0.084*	0.214	133 / 135 / 139
	(18.62)	(15.49)	(23.55)				
Village has health facility	0.16	0.19	0.19	0.269	0.803	0.472	133 / 135 / 139
	(0.37)	(0.39)	(0.40)				
Village has midwife	0.21	0.21	0.24	0.358	0.269	0.937	133 / 135 / 139
	(0.41)	(0.41)	(0.43)				
Water shortage past year	0.42	0.46	0.36	0.441	0.189	0.632	133 / 135 / 139
	(0.50)	(0.50)	(0.48)				

Notes: This table presents the balance check on village characteristics by treatment arm for the sample of villages included in the analysis. T1 ("Cash+SBCC") refer to villages where cash transfers and SBCC activities were provided jointly; T2 ("CashOnly") refer to villages where cash transfers only were provided; CG (control group) refer to villages where neither cash transfers nor SBCC took place. Fixed effects per geographic strata (34) are included. Standard errors are clustered at the village cluster level and reported in parenthesis. P-values from t-test from the difference in means are reported. *** p<0.01, ** p<0.05, * p<0.1.

Table 2: Balance on woman-level characteristics

	T1	T2	CG	PV (T1-CG)	PV (T2-CG)	PV (T1-T2)	N(T1/T2/CG)
Resp married	0.96	0.97	0.96	0.541	0.199	0.386	840 / 802 / 695
	(0.19)	(0.17)	(0.20)				
Resp age	32.16	31.42	31.26	0.013**	0.683	0.049**	840 / 802 / 696
	(6.37)	(6.52)	(6.17)				
Resp educ years	5.70	5.86	6.08	0.132	0.370	0.499	840 / 802 / 696
	(3.19)	(3.38)	(3.36)				
HH size	4.95	4.99	4.81	0.224	0.103	0.719	840 / 802 / 696
	(1.71)	(1.77)	(1.64)				
Children U5	1.12	1.11	1.13	0.750	0.370	0.511	840 / 802 / 696
	(0.37)	(0.34)	(0.38)				
HH head female	0.08	0.09	0.07	0.593	0.421	0.725	840 / 802 / 696
	(0.28)	(0.28)	(0.26)				
HH head tot yrs educ	5.49	5.79	6.06	0.030**	0.292	0.177	840 / 802 / 696
	(3.19)	(3.35)	(3.35)				
HH head worked past 3m	0.89	0.83	0.86	0.650	0.386	0.225	840 / 802 / 696
	(0.66)	(0.39)	(0.58)				
HH head income	349964.00	330294.12	332720.14	0.627	0.949	0.588	839 / 799 / 695
	(478142.69)	(486734.66)	(466277.61)				
Any electricity	0.42	0.38	0.47	0.515	0.198	0.615	840 / 802 / 696
	(0.49)	(0.48)	(0.50)				
Always electricity	0.38	0.33	0.40	0.775	0.350	0.561	833 / 790 / 688
	(0.48)	(0.47)	(0.49)				
Cooking fuel electricity	0.29	0.24	0.29	0.953	0.360	0.420	840 / 802 / 696
	(0.46)	(0.43)	(0.45)				
Tot no. rooms in house	1.15	1.18	1.14	0.825	0.474	0.606	838 / 796 / 693
	(0.81)	(0.80)	(0.75)				
Improved roof material	0.87	0.85	0.87	0.991	0.681	0.645	840 / 802 / 696
	(0.34)	(0.35)	(0.34)				
Improved wall material	0.23	0.22	0.21	0.390	0.704	0.663	840 / 802 / 696
	(0.42)	(0.41)	(0.41)				
Improved floor material	0.33	0.32	0.31	0.695	0.898	0.809	840 / 802 / 696
	(0.47)	(0.47)	(0.46)				

Notes: This table presents the balance check on women characteristics by treatment arm for the sample of all mothers who were pregnant at enrollment (2,338). T1 ("Cash+SBCC") refer to villages where cash transfers and SBCC activities were provided jointly; T2 ("CashOnly") refer to villages where cash transfers only were provided; CG (control group) refer to villages where neither cash transfers nor SBCC took place. Fixed effects per geographic strata (34) are included. Standard errors are clustered at the village cluster level and reported in parenthesis. P-values from t-test from the difference in means are reported. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Balance on woman-level characteristics for women sample followed-up at endline

	T1	T2	CG	PV (T1-CG)	PV (T2-CG)	PV (T1-T2)	N(T1/T2/CG)
Resp married	0.96	0.97	0.96	0.581	0.189	0.311	769 / 744 / 620
	(0.19)	(0.17)	(0.20)				
Resp age	32.07	31.41	31.39	0.082*	0.964	0.099*	769 / 744 / 621
	(6.38)	(6.53)	(6.18)				
Resp educ years	5.71	5.84	6.09	0.134	0.339	0.581	769 / 744 / 621
	(3.17)	(3.32)	(3.36)				
HH size	4.92	4.99	4.83	0.418	0.164	0.565	769 / 744 / 621
	(1.69)	(1.80)	(1.63)				
Children U5	1.14	1.12	1.14	0.975	0.326	0.279	769 / 744 / 621
	(0.36)	(0.33)	(0.36)				
HH head female	0.08	0.08	0.07	0.585	0.699	0.869	769 / 744 / 621
	(0.28)	(0.27)	(0.26)				
HH head tot yrs educ	5.47	5.81	6.04	0.032**	0.371	0.142	769 / 744 / 621
	(3.14)	(3.33)	(3.34)				
HH head worked past 3m	0.89	0.83	0.86	0.642	0.426	0.204	769 / 744 / 621
	(0.65)	(0.38)	(0.60)				
HH head income	350743.23	335991.91	334404.03	0.654	0.967	0.704	768 / 742 / 620
	(466361.87)	(500633.62)	(483130.36)				
Any electricity	0.42	0.38	0.48	0.503	0.177	0.585	769 / 744 / 621
	(0.49)	(0.49)	(0.50)				
Always electricity	0.38	0.33	0.41	0.718	0.309	0.564	762 / 733 / 614
	(0.49)	(0.47)	(0.49)				
Cooking fuel electricity	0.29	0.24	0.30	0.881	0.253	0.428	769 / 744 / 621
	(0.46)	(0.43)	(0.46)				
Tot no. rooms in house	1.14	1.17	1.13	0.890	0.536	0.621	767 / 739 / 619
	(0.80)	(0.79)	(0.75)				
Improved roof material	0.86	0.85	0.87	0.866	0.698	0.779	769 / 744 / 621
	(0.34)	(0.35)	(0.34)				
Improved wall material	0.23	0.22	0.21	0.436	0.750	0.678	769 / 744 / 621
	(0.42)	(0.41)	(0.41)				
Improved floor material	0.33	0.31	0.31	0.642	0.989	0.657	769 / 744 / 621
	(0.47)	(0.46)	(0.46)				

Notes: This table presents the balance check on individual characteristics by treatment arm for the sample of mothers who were pregnant at enrollment included in the analysis as followed-up at endline (2,134). T1 ("Cash+SBCC") refer to villages where cash transfers and SBCC activities were provided jointly; T2 ("CashOnly") refer to villages where cash transfers only were provided; CG (control group) refer to villages where neither cash transfers nor SBCC took place. Fixed effects per geographic strata (34) are included. Standard errors are clustered at the village cluster level and reported in parenthesis. P-values from t-test from the difference in means are reported. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Fertility

	(1)	(2)
	Pregnant	Tot no. pregnancies since start of program
Cash+SBCC	0.010	0.002
	(0.009)	(0.018)
CashOnly	0.001	-0.028*
	(0.009)	(0.017)
Observations	2134	2134
Mean Control	.03	1.14
Clusters	102	102

Notes: The table presents OLS estimates of the effects of the maternal cash transfer program interventions on measures of fertility. "Cash+SBCC" indicates T1 villages, where cash transfers and SBCC activities were provided jointly; "CashOnly" indicates T2 villages, where cash transfers only were provided; the reference group are villages in the control group (CG) where neither cash transfers nor SBCC took place. Outcomes include whether the mother is currently pregnant at endline (1), and her total number of pregnancies between June 2016 and endline, calculated from the household roster as the sum of biological living children under 5 years old. Controls include (i) individual demographic controls, including mother's age and education, and household head's age and education; (ii) village-level controls, including distance to large and small markets, main source of livelihood (agriculture, livestock, or casual labor), availability of government provided electricity, and participation in a concurrent WASH intervention. Fixed effects per geographic strata (34) are included. Standard errors are clustered at the village cluster level and reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Child stunting, by gender

	(1)	(2)	(3)	(4)
	Child stunted	Child moder-ately stunted	Child severely stunted	HAZ score (WHO)
Female children				
Cash+SBCC	-0.052*	-0.058*	0.007	0.087
	(0.030)	(0.030)	(0.017)	(0.068)
CashOnly	0.000	-0.009	0.009	-0.020
	(0.033)	(0.033)	(0.015)	(0.061)
Observations	1044	1044	1044	1043
Mean Control	0.30	0.24	0.05	-1.48
Clusters	101	101	101	101
Male children				
Cash+SBCC	-0.067**	-0.074***	0.007	0.082
	(0.030)	(0.028)	(0.017)	(0.060)
CashOnly	-0.026	-0.022	-0.004	-0.005
	(0.031)	(0.029)	(0.017)	(0.061)
Observations	1107	1107	1107	1107
Mean Control	0.38	0.30	0.08	-1.65
Clusters	102	102	102	102

Notes: The table presents OLS estimates of the effects of the maternal cash transfer program interventions on measures of stunting for children whose mothers were pregnant at enrollment, following WHO classification, by gender. "Cash+SBCC" indicates T1 villages, where cash transfers and SBCC activities were provided jointly; "CashOnly" indicates T2 villages, where only cash transfers were provided; the reference group are villages in the control group where neither cash transfers nor SBCC took place. Outcomes include the proportion of children stunted as children with Height for Age Z score (HAZ) < -2 (1); the proportion of children moderately stunted as children with HAZ < -2 and >= -3 (2); the proportion of children severely stunted as children with HAZ < -3 (3); and, HAZ (4). Controls include (i) individual demographic controls, including child's sex and age, mother's age and education, and household head's age and education; (ii) village-level controls, including distance to large and small markets, main source of livelihood (agriculture, livestock, or casual labor), availability of government provided electricity, and participation in a concurrent WASH intervention. Fixed effects per geographic strata (34) are included. Standard errors are clustered at the village cluster level and reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Children stunting - by village socio-economic status (SES)

	(1)	(2)	(3)	(4)
	Child stunted	Child moder-ately stunted	Child severely stunted	HAZ score (WHO)
Panel A: Low SES Villages				
Cash+SBCC	-0.093**	-0.093***	0.001	0.158**
	(0.037)	(0.032)	(0.020)	(0.071)
CashOnly	-0.041	-0.055	0.014	-0.026
	(0.038)	(0.035)	(0.020)	(0.070)
Observations	1051	1051	1051	1051
Mean Control	0.34	0.27	0.07	-1.57
Clusters	92	92	92	92
Panel B: High SES Villages				
Cash+SBCC	0.044	0.020	0.024	-0.036
	(0.031)	(0.030)	(0.016)	(0.078)
CashOnly	0.057*	0.048	0.009	-0.069
	(0.033)	(0.031)	(0.014)	(0.080)
Observations	961	961	961	961
Mean Control	0.34	0.27	0.07	-1.57
Clusters	86	86	86	86

Notes: The table presents OLS estimates of the effects of the maternal cash transfer program interventions on measures of stunting for children whose mothers were pregnant at enrollment, following WHO classification."Cash+SBCC" indicates T1 villages, where cash transfers and SBCC activities were provided jointly; "CashOnly" indicates T2 villages, where only cash transfers were provided; the reference group are villages in the control group where neither cash transfers nor SBCC took place. Outcomes include the proportion of children stunted as children with Height for Age Z score (HAZ) < -2 (1); the proportion of children moderately stunted as children with HAZ < -2 and >= -3 (2); the proportion of children severely stunted as children with HAZ < -3 (3); and, HAZ (4). Controls include (i) individual demographic controls, including child's sex and age, mother's age and education, and household head's age and education; (ii) village-level controls, including distance to large and small markets, main source of livelihood (agriculture, livestock, or casual labor), availability of government provided electricity, and participation in a concurrent WASH intervention. The analysis excludes the sample contaminated by the imperfect implementation (139 children). Low or high SES is proxied by the average number of years of education attained by women in the sample below or above the median.

Table 7: Children stunting - by village socio-economic status (SES) and household income

	(1)	(2)	(3)	(4)
	Child stunted	Child moderately stunted	Child severely stunted	HAZ score (WHO)
Panel A: Low SES, Low income				
Cash+SBCC	-0.140***	-0.122**	-0.017	0.210**
	(0.050)	(0.047)	(0.029)	(0.099)
CashOnly	-0.053	-0.034	-0.019	-0.010
	(0.056)	(0.052)	(0.030)	(0.104)
Observations	544	544	544	544
Mean Control	0.43	0.35	0.08	-1.72
Clusters	89	89	89	89
Panel B: Low SES, High income				
Cash+SBCC	-0.028	-0.059	0.031	0.029
	(0.047)	(0.050)	(0.030)	(0.087)
CashOnly	-0.012	-0.065	0.053*	-0.089
	(0.043)	(0.046)	(0.030)	(0.079)
Observations	507	507	507	507
Mean Control	0.42	0.35	0.07	-1.69
Clusters	86	86	86	86
Panel C: High SES, Low Income				
Cash+SBCC	0.039	0.027	0.012	0.035
	(0.045)	(0.039)	(0.024)	(0.105)
CashOnly	0.054	0.059	-0.005	-0.015
	(0.047)	(0.040)	(0.021)	(0.097)
Observations	454	454	454	454
Mean Control	0.26	0.18	0.07	-1.50
Clusters	81	81	81	81
Panel D: High SES, High Income				
Cash+SBCC	0.029	0.003	0.025	-0.084
	(0.045)	(0.044)	(0.024)	(0.111)
CashOnly	0.078	0.044	0.034	-0.178
	(0.053)	(0.053)	(0.025)	(0.132)
Observations	507	507	507	507
Mean Control	0.27	0.22	0.05	-1.39
Clusters	80	80	80	80

Notes: The table presents OLS estimates of the effects of the maternal cash transfer program interventions on measures of stunting for children whose mothers were pregnant at enrollment, following WHO classification."Cash+SBCC" indicates T1 villages, where cash transfers and SBCC activities were provided jointly; "CashOnly" indicates T2 villages, where only cash transfers were provided; the reference group are villages in the control group where neither cash transfers nor SBCC took place. Outcomes include the proportion of children stunted as children with Height for Age Z score (HAZ) < -2 (1); the proportion of children moderately stunted as children with HAZ < -2 and $>= -3$ (2); the proportion of children severely stunted as children with HAZ < -3 (3); and, HAZ (4). Controls include (i) individual demographic controls, including child's sex and age, mother's age and education, and household head's age and education; (ii) village-level controls, including distance to large and small markets, main source of livelihood (agriculture, livestock, or casual labor), availability of government provided electricity, and participation in a concurrent WASH intervention. The analysis excludes the sample contaminated by the imperfect implementation (139 children). Low or high SES is proxied by the average number of years of education attained by women in the sample below or above the median. Low or high income is defined as below or above the household median income. Fixed effects per geographic strata (34) are included. Standard errors are clustered at the village cluster level and reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Maternal health knowledge

	(1)	(2)	(3)	(4)	(5)
	Mother knows child food diversity is important	Mother knows the meaning of exclusive breastfeeding	Mother knows the best time to initiate breastfeeding	Mother knows the best time to introduce complementary feeding	Knowledge index
Cash+SBCC	0.033**	0.031*	0.013***	0.089***	0.143***
	(0.015)	(0.016)	(0.005)	(0.022)	(0.033)
CashOnly	0.030**	-0.000	0.009*	0.033	0.063*
	(0.014)	(0.019)	(0.005)	(0.025)	(0.034)
Observations	2134	2134	2134	2134	2134
Mean Control	0.91	0.94	0.98	0.81	0
Clusters	102	102	102	102	102
Cash+SBCC=CashOnly	0.87	0.11	0.39	0.01	0.03

Notes: The table presents OLS estimates of the effects of the maternal cash transfer program interventions on measures of knowledge covered by the education sessions in SBCC activities. Outcomes include: the proportion of mothers who know the importance of food diversity in their children diet (1); the proportion of mothers who know the meaning of exclusive breastfeeding (2); the proportion of mothers who know the best time to initiate breastfeeding (3); the proportion of mothers who know the best time to introduce complementary feeding (4); an index of knowledge constructed following Kling et al. 2007. “Cash+SBCC” indicates T1 villages, where cash transfers and SBCC activities were provided jointly; “CashOnly” indicates T2 villages, where cash transfers only were provided; the reference group are villages in the control group where neither cash transfers nor SBCC took place. Controls include (i) individual demographic controls, including child’s sex and age, mother’s age and education, and household head’s age and education for child-level analysis; mother’s age and education, and household head’s age and education for mother-level analysis; (ii) village-level controls, including distance to large and small markets, main source of livelihood (agriculture, livestock, or casual labor), availability of government provided electricity, and participation in a concurrent WASH intervention. Fixed effects per geographic strata (34) are included. Standard errors are clustered at the village cluster level and reported in parenthesis. P-values from t-test from the difference in means are reported. *** p<0.01, ** p<0.05, * p<0.1.

Table 9: Women's decision-making

	(1)	(2)	(3)	(4)	(5)
	Mother decides on own earnings	Mother decides on spouse earnings	Mother decides on health	Mother decides on major purchases	Mother decides on visit relatives
Cash+SBCC	0.027	-0.004	0.047	0.043*	0.022
	(0.031)	(0.024)	(0.031)	(0.024)	(0.027)
CashOnly	0.045*	0.017	0.029	0.012	-0.014
	(0.027)	(0.024)	(0.028)	(0.025)	(0.029)
Observations	1958	2088	2130	2130	2132
Mean Control	0.34	0.23	0.24	0.18	0.23
Clusters	102	102	102	102	102
Cash+SBCC=CashOnly	0.51	0.40	0.59	0.17	0.19

Notes: The table presents OLS estimates of the effects of the maternal cash transfer program interventions on measures of decision-making. Outcomes include: the proportion of mothers who decide on own (1) or spouse's earnings (2); on health (3); or on major household purchases (4) or visiting relatives (5). "Cash+SBCC" indicates T1 villages, where cash transfers and SBCC activities were provided jointly; "CashOnly" indicates T2 villages, where cash transfers only were provided; the reference group are villages in the control group where neither cash transfers nor SBCC took place. Controls include (i) individual demographic controls, including child's sex and age, mother's age and education, and household head's age and education for child-level analysis; mother's age and education, and household head's age and education for mother-level analysis; (ii) village-level controls, including distance to large and small markets, main source of livelihood (agriculture, livestock, or casual labor), availability of government provided electricity, and participation in a concurrent WASH intervention. Fixed effects per geographic strata (34) are included. Standard errors are clustered at the village cluster level and reported in parenthesis. P-values from t-test from the difference in means are reported. *** p<0.01, ** p<0.05, * p<0.1.

Table 10: Children stunting - addressing contamination

	(1)	(2)	(3)	(4)
	Child stunted	Child moder- ately stunted	Child severely stunted	HAZ score (WHO)
Cash+SBCC	-0.050**	-0.057***	0.007	0.074
	(0.022)	(0.018)	(0.011)	(0.047)
CashOnly	-0.002	-0.006	0.004	-0.031
	(0.025)	(0.022)	(0.012)	(0.044)
Observations	2012	2012	2012	2012
Mean Control	0.34	0.27	0.07	-1.57
Clusters	102	102	102	102

Notes: The table presents OLS estimates of the effects of the maternal cash transfer program interventions on measures of stunting for children whose mothers were pregnant at enrollment, following WHO classification.'Cash+SBCC" indicates T1 villages, where cash transfers and SBCC activities were provided jointly; "CashOnly" indicates T2 villages, where only cash transfers were provided; the reference group are villages in the control group where neither cash transfers nor SBCC took place. Outcomes include the proportion of children stunted as children with Height for Age Z score (HAZ) < -2 (1); the proportion of children moderately stunted as children with HAZ < -2 and >= -3 (2); the proportion of children severely stunted as children with HAZ < -3 (3); and, HAZ (4). Controls include (i) individual demographic controls, including child's sex and age, mother's age and education, and household head's age and education; (ii) village-level controls, including distance to large and small markets, main source of livelihood (agriculture, livestock, or casual labor), availability of government provided electricity, and participation in a concurrent WASH intervention. The analysis excludes the sample contaminated by the imperfect implementation (139 children). Fixed effects per geographic strata (34) are included. Standard errors are clustered at the village cluster level and reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table 11: Children stunting - endline full sample

	(1)	(2)	(3)	(4)
	Child stunted	Child moder-ately stunted	Child severely stunted	HAZ score (WHO)
Cash+SBCC	-0.038**	-0.042***	0.004	0.060*
	(0.016)	(0.015)	(0.009)	(0.035)
CashOnly	-0.006	-0.009	0.003	-0.014
	(0.017)	(0.016)	(0.009)	(0.035)
Observations	3176	3176	3176	3176
Mean Control	0.35	0.28	0.06	-1.58
Clusters	102	102	102	102

Notes: The table presents OLS estimates of the effects of the maternal cash transfer program interventions on measures of stunting for children of the full endline sample, following WHO classification."Cash+SBCC" indicates T1 villages, where cash transfers and SBCC activities were provided jointly; "CashOnly" indicates T2 villages, where only cash transfers were provided; the reference group are villages in the control group where neither cash transfers nor SBCC took place. Outcomes include the proportion of children stunted as children with Height for Age Z score (HAZ) < -2 (1); the proportion of children moderately stunted as children with HAZ < -2 and >= -3 (2); the proportion of children severely stunted as children with HAZ < -3 (3); and, HAZ (4). Controls include (i) individual demographic controls, including child's sex and age, mother's age and education, and household head's age and education; (ii) village-level controls, including distance to large and small markets, main source of livelihood (agriculture, livestock, or casual labor), availability of government provided electricity, and participation in a concurrent WASH intervention. Fixed effects per geographic strata (34) are included. Standard errors are clustered at the village cluster level and reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table 12: Children stunting - standard errors clustered at village level

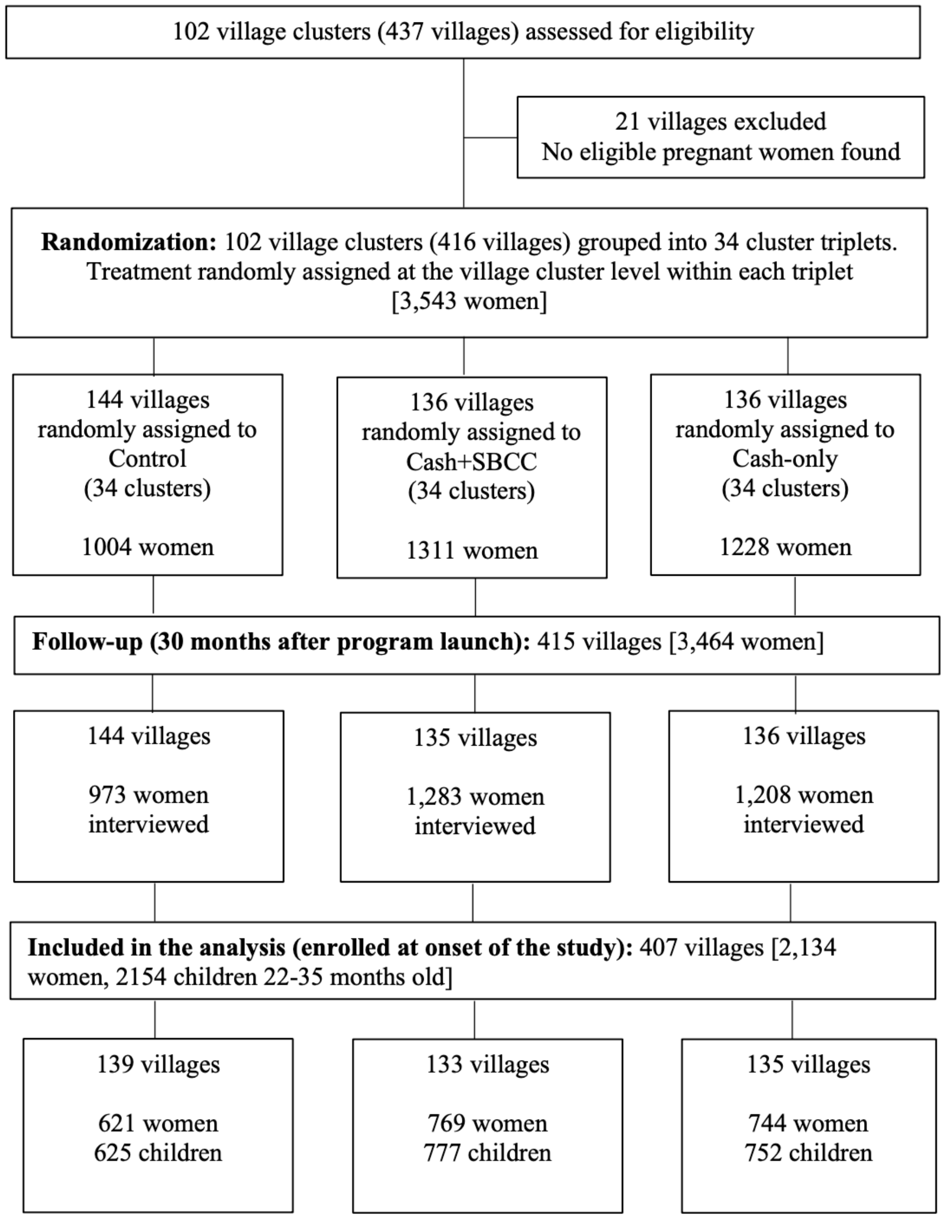
	(1)	(2)	(3)	(4)
	Child stunted	Child moderately stunted	Child severely stunted	HAZ score (WHO)
Cash+SBCC	-0.046*	-0.053**	0.007	0.074
	(0.025)	(0.023)	(0.014)	(0.054)
CashOnly	-0.004	-0.008	0.004	-0.017
	(0.026)	(0.024)	(0.014)	(0.051)
Observations	2151	2151	2151	2151
Mean Control	0.34	0.27	0.07	-1.57
Clusters	407	407	407	407

Notes: The table presents OLS estimates of the effects of the maternal cash transfer program interventions on measures of stunting for children whose mothers were pregnant at enrollment, following WHO classification.'Cash+SBCC" indicates T1 villages, where cash transfers and SBCC activities were provided jointly; "CashOnly" indicates T2 villages, where only cash transfers were provided; the reference group are villages in the control group where neither cash transfers nor SBCC took place. Outcomes include the proportion of children stunted as children with Height for Age Z score (HAZ) < -2 (1); the proportion of children moderately stunted as children with HAZ < -2 and >= -3 (2); the proportion of children severely stunted as children with HAZ < -3 (3); and, HAZ (4). Controls include (i) individual demographic controls, including child's sex and age, mother's age and education, and household head's age and education; (ii) village-level controls, including distance to large and small markets, main source of livelihood (agriculture, livestock, or casual labor), availability of government provided electricity, and participation in a concurrent WASH intervention. Fixed effects per geographic strata (34) are included. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

Table 13: Child stunting (alternative controls)

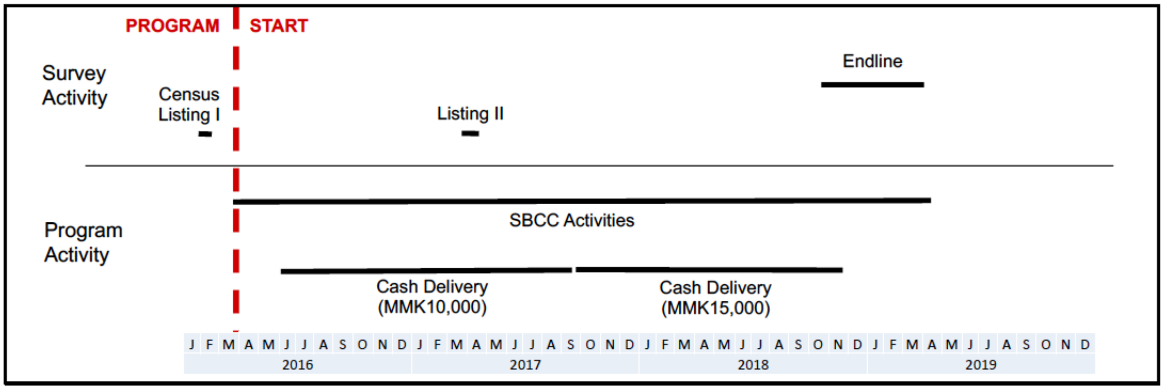
	(1)	(2)	(3)	(4)
	Child stunted	Child moderately stunted	Child severely stunted	HAZ score (WHO)
Panel A: Basic controls				
CASH+SBCC	-0.040	-0.051**	0.011	0.051
	(0.025)	(0.020)	(0.011)	(0.058)
CASH	-0.008	-0.012	0.004	-0.025
	(0.026)	(0.022)	(0.011)	(0.054)
Observations	2151	2151	2151	2151
Mean Control	0.34	0.27	0.07	-1.57
Clusters	102	102	102	102
Panel B: No controls				
Cash+SBCC	-0.033	-0.046**	0.013	0.038
	(0.026)	(0.021)	(0.011)	(0.059)
CashOnly	-0.006	-0.010	0.004	-0.030
	(0.027)	(0.022)	(0.012)	(0.056)
Observations	2151	2151	2151	2151
Mean Control	0.34	0.27	0.07	-1.57
Clusters	102	102	102	102

Notes: The table presents OLS estimates of the effects of the maternal cash transfer program interventions on measures of stunting for children whose mothers were pregnant at enrollment, following WHO classification."Cash+SBCC" indicates T1 villages, where cash transfers and SBCC activities were provided jointly; "CashOnly" indicates T2 villages, where only cash transfers were provided; the reference group are villages in the control group where neither cash transfers nor SBCC took place. Outcomes include the proportion of children stunted as children with Height for Age Z score (HAZ) < -2 (1); the proportion of children moderately stunted as children with HAZ < -2 and >= -3 (2); the proportion of children severely stunted as children with HAZ < -3 (3); and, HAZ (4). Panel A includes only unbalanced covariates (respondent's age and head of the household's years of education, Appendix Table 2); Panel B does not include any covariate. Fixed effects per geographic strata (34) are included. Standard errors are clustered at the village cluster level and reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.



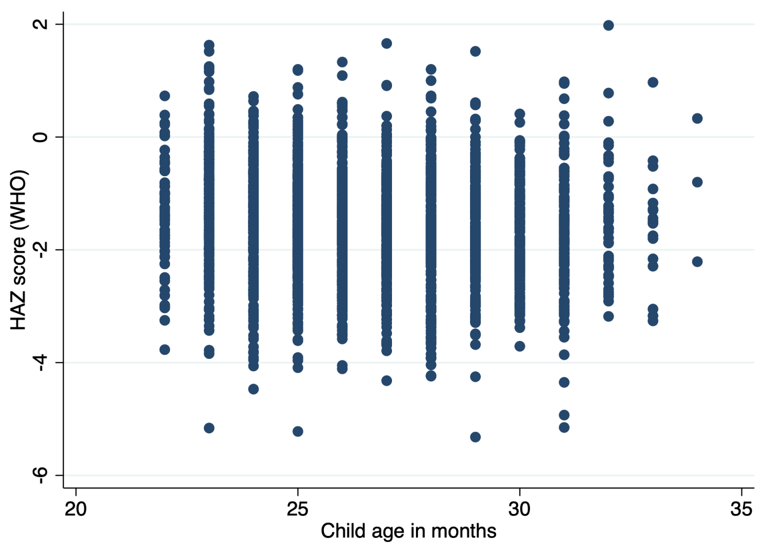
Notes: This figure presents the profile of the randomized controlled trial.

Figure 1: Profile of the Randomized Controlled Trial



Notes: This figure presents the timeline of the data collection rounds (survey activity) and the maternal cash transfer program rollout (program activity).

Figure 2: Timeline



Notes: This figure plots the relationship between HAZ score and age of the children (in months) in the sample.

Figure 3: Relationship between HAZ and age of children