The Role of Information and Cash Transfers on Early-Childhood Development: Short- and Long-Run Evidence from Nepal

MICHAEL LEVERE
Colgate University and Mathematica

GAYATRI ACHARYA
World Bank Group

PRASHANT BHARADWAJ
University of California, San Diego, and National Bureau of Economic Research

I. Introduction

Health and human capital are important drivers of economic growth, and research from multiple disciplines has shown that health during infancy is critical in determining how children develop throughout their lives. Malnutrition is a key obstacle to achieving appropriate levels of early-childhood development. While substantial progress has been made in combating malnutrition at a global level, chronic maternal and child malnutrition remain a serious problem in parts of the developing world. Even within developing countries, the rural poor disproportionately bear the burden of child malnutrition (World Bank 2007), two important causes of which are a lack of information and a lack of income. In this paper, we evaluate a program that provided information on best practices regarding infant health and cash to families in extremely poor areas with pregnant mothers and/or children below the age of 2.

The context of our study is Nepal, where maternal and child malnutrition remains a serious problem. Nepal has one of the highest malnutrition, stunting, and wasting rates for children under the age of 5. According to the Nepal

Thanks to Karthik Muralidharan, Jasmine Rajbhandary, Tara Shrestha, Venkatesh Sundararaman, Kanchan Tamang, Bishnu Thapa, and Karishma Wasti. This project would not have been possible without support from the Government of Nepal's Poverty Alleviation Fund and the Ministry of Local Development and Federal Affairs. We are grateful for funding from the Strategic Impact Evaluation Fund (SIEF). Data are provided through Dataverse (https://doi.org/10.7910/DVN/WB1OC6). Contact the corresponding author, Prashant Bharadwaj, at prbharadwaj@ucsd.edu.

This article was peer-reviewed and accepted for publication prior to Prashant Bharadwaj's appointment as editor in chief of Economic Development and Cultural Change.

Electronically published February 29, 2024

Economic Development and Cultural Change, volume 72, number 3, April 2024.

https://doi.org/10.1086/723203

Living Standard Survey 2010/11, 46.7% of children under the age of 5 are stunted, 15.1% are wasted, and 36.3% are underweight. Moreover, pregnant mothers have suboptimal weight gain during pregnancy. The consequences are significant and long-term, ranging from increased neonatal mortality and morbidity to irreversible adverse physical and cognitive outcomes that harm health, productivity, and economic growth (Pelletier et al. 1995; Strupp and Levitsky 1995; Alderman, Hoddinott, and Kinsey 2006). The economic costs of malnutrition are very high—an estimated 2%–3% of gross domestic product (US$250 million to US$375 million) is lost annually in Nepal due solely to vitamin and mineral deficiencies (World Bank 2012). While Nepal has made considerable progress in reducing maternal and child mortality, it has a long way to go in tackling malnutrition. Promotional campaigns aimed to raise awareness about the importance of balanced diet, proper sanitation and hygiene, breastfeeding, and other health matters have produced mixed results, especially in the context of food-insecure populations (Bhutta et al. 2008). While the lack of income may be a reason for households' inability to address malnutrition, it is also possible that noneconomic factors have helped perpetuate malnutrition for so long in Nepal.

Using a randomized controlled trial design in rural areas in Nepal, we evaluate the effects of two different treatments on maternal health practices and child development outcomes in the short and long runs. One treatment arm was provided information on best practices regarding nutrition and health for children below the age of 2, and a second treatment arm received the same information plus a conditional cash transfer. To receive the cash transfer, a woman simply had to attend the regular group meeting. A transfer of NPR 700 (US$7) per month, equivalent to 8%–20% of median monthly household income, was given over a period of 5 months.[1] The cash transfer could thus affect outcomes through two channels: (1) as an incentive to attend the informational meeting, it could lead to even larger improvements in knowledge; and (2) as a source of income, it can allow mothers to spend more money to improve the health outcomes of their children, such as through increased food consumption. Given the short time frame of the cash treatment and the simple conditionality, this cash transfer can be viewed as a short-term safety net. Information sessions started earlier and took place for 9 months and were identical across the treatment arms with and without a cash transfer. Importantly, our work utilized existing health and financial infrastructures (e.g., community health volunteers and group

[1] As discussed below, the monthly cash transfer is comparable in size (relative to household income) to others implemented in the rest of the world (see Fiszbein and Schady 2009). However, because the payments lasted for only 5 months, this represents a smaller total infusion of cash than other programs.

meetings organized by the Nepal Poverty Alleviation Fund), allowing for overall lower costs, easier replication, and potential scale-up.

We find significant and sizable impacts on maternal knowledge regarding infant health best practices in our treatment groups in the short and long runs. Our information plus cash treatment group, for example, answered about one more question correctly (out of 10 asked) relative to the control group in the short run, and about half answered an additional question 2 years after the intervention ends; knowledge in the information-only group also significantly increased relative to the control in the short and long runs, but the short-run impact was only about half the size of the improvement in the information plus cash group.

Given improvements in knowledge, we then look to see whether women are changing behaviors and incorporating the new knowledge into their daily lives. Households in the information and cash treatment group improved various maternal behaviors such as breastfeeding, vitamin A take-up, prenatal checkups, and so on. Similar to the gains in knowledge, maternal behavioral practices also demonstrated a significant improvement relative to those in the information-only group. In the information-only group, the improvements in knowledge did not translate into changes in behaviors. Together, this suggests the importance of cash in affecting behavior, such as increased regular feeding for young children. In the 2-year follow-up, we find that some of these improved behavioral practices persist for the information plus cash group, though the effects are not significantly different from the information-only group in the long run.

Given that women implemented behavioral changes, we then study child outcomes to see whether the improvements in behaviors are passed on to children. We find suggestive evidence of improvements in child development in the short run, as measured by the Ages and Stages Questionnaire (ASQ), relative to the information-only group. While we find no increases in child anthropometrics in the treatment groups, we find significant anthropometric improvements among the older siblings of the treated children (these siblings were 25–36 months at baseline) in the information plus cash intervention group. However, in the long run, we do not find sustained significant improvements in child development or anthropometric outcomes in either the information plus cash or the information-only group.

This paper is related to existing research in epidemiology and economics focusing on the role of information campaigns and cash transfers to improve health outcomes (for a review on interventions related to maternal undernutrition, see Bhutta et al. 2013; for a review on conditional cash transfers and take-up of health interventions, see Lagarde, Haines, and Palmer 2007; Fiszbein

and Schady 2009). Our results are a robust contribution to the literature, specifically focusing on the connection between social safety nets, nutrition, and early-childhood development, which has shown inconclusive links (Ruel, Alderman, and Maternal and Child Nutrition Study Group 2013). We also link our findings with the vast literature in medicine and economics, which has identified the in utero phase and the first 2 years of life as the most critical in terms of determining future outcomes related to human capital (for a review, see Almond and Currie 2011). Therefore, measures aimed at tackling nutritional deficiencies in children must necessarily focus on this "critical window of opportunity" (popularly referred to as the first 1,000 days of life). Our paper builds on this literature by focusing on the extent to which a shorter-term intervention can significantly impact outcomes in the short and long runs. Our study is also notable for its focus on maternal knowledge improvements as a result of the information campaigns. Information campaigns are often a critical element of conditional cash transfer programs but also are often used to promote health and behavior change in developing countries more broadly, such as addressing HIV risk (Dupas and Miguel 2017). Finally, the population we study in Nepal is an extremely poor and marginalized subset of the overall population. Improving early-childhood health among the poorest of the poor in a postconflict setting such as Nepal is an important policy goal, and this paper provides crucial evidence toward this.

Our paper is most closely related to work by Macours, Schady, and Vakis (2012) and Carneiro et al. (2021). Macours and colleagues study the impacts of cash transfers to households in Nicaragua under the Atención a Crisis program on child development. They find that cash transfers improve overall child development and that the positive effects of the program last long after the transfers stop. Carneiro et al. (2021) study the Child Development Grant Programme (CDGP) in Nigeria, which offered cash transfers and information on maternal best practices to newly pregnant women. They find significant improvements in child health outcomes, which persist through when the child turns 4. Importantly, the cash transfers in Nigeria represent 85% of monthly household income—a substantially larger share than in our study. The authors also show that the accompanying information sessions (together with the cash transfer) led to improvements in both knowledge and in best practices. Both studies suggest that cash transfers can lead to long-lasting improvements in child well-being. Our paper can directly test the value added of cash over and above information; we add an explicit information-based intervention to a basic cash component, while the Atención a Crisis program seems to have included a large number of programs as part of its treatment (including informational sessions). Importantly, we explicitly measure maternal knowledge

about best practices regarding infant health; hence, a key contribution here is whether maternal knowledge improves as a result of the intervention and whether knowledge is better put into practice when cash is additionally given.

II. Experimental Design

A. Randomization

The intervention was implemented in four food-insecure districts in Nepal (four of 75 districts nationwide), through a Community Challenge Fund (CCF) administered by the Ministry of Federal Affairs and Local Development.[2] The CCF specifically targeted high-risk communities within community organizations (COs) supported by the Nepal Poverty Alleviation Fund (PAF). PAF is a program created by the Government of Nepal that seeks to improve outcomes in poor, marginalized communities by community-driven development. COs supported by PAF are designed to hold monthly meetings, facilitated by a local social mobilizer (SM) who brings together people from the community. PAF supports community infrastructure and income-generating activities for poor and socially vulnerable households. These four districts supported by the CCF cover Nepal's diverse geography, from flat-lying agriculture-based areas to more mountainous regions.[3]

The intervention consisted of two treatment groups—one that received information only and one that received information plus cash—and a control group. We implemented a stratified randomized cluster design; within each district, we randomly assigned each Village Development Committee (VDC) to one of the three groups.[4] Henceforth, we will refer to VDCs as counties and COs as villages for ease of interpretation.[5] Within a county, we randomly selected up to four villages (out of about 30 villages within a county) to be in our sample. Every village in a county received the same treatment status. The total sample contains 184 counties across the four districts, with a total of 591 villages. Within a village, every household where a woman was either pregnant or had a child aged 2 years or under at baseline was surveyed and invited to

[2] Under a Social Safety Nets Project financed by the World Bank.

[3] The four districts are Sarlahi, Rautahat, Sindhuli, and Ramechaap. Sarlahi and Rautahat are in the *terai*, a low-lying region that consists of marshy grasslands at the foot of the Himalayas. Sindhuli and Ramechaap are in the hills.

[4] The VDC is an administrative unit below the district and is similar to a municipality or county.

[5] VDCs are quite similar to counties in that both are administrative regions at a larger geographic entity than a village but a smaller geographic entity than a state. The CO and village comparison is slightly less apt. A community organization is created by PAF and is the specific group within a village that meets once a month. It is not itself an administrative unit. However, for the purposes of interpretation, this distinction is not important.

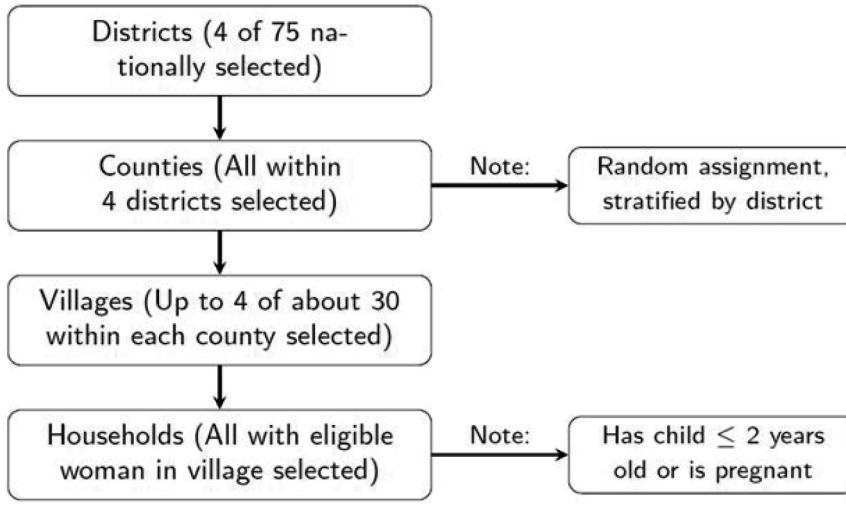

Figure 1. Randomization and sample selection protocol.

participate in the intervention. Figure 1 shows the administrative levels and where randomization occurred.

The county was chosen as the unit of randomization for two primary reasons. First, randomizing at the village level would have led to potential for spillovers based on geographic proximity; counties are large-enough units geographically that it is unlikely that a member of a village in a control county would have been able to attend or even know of the information treatment that occurred in a village of a neighboring county. Second, SMs who led the information sessions are responsible for all villages within a county, including nonexperimental villages. Asking an SM who had undergone training for the information arm of the intervention to withhold that information in some of her meetings would have both been unethical and led to a higher likelihood of contamination.[6]

Our primary analysis focuses only on the 139 counties surveyed after the completion of the intervention in 2014, 45 of which are control counties, 48 of which are information-only counties, and 46 of which are information

[6] Indeed, in discussions with local SMs, some noted that they found the information so helpful and valuable that they planned to implement it in all of the villages that they worked in. This reflects the potential benefits from scaling the intervention, in that there are economies of scale in having an SM implement the information in all 30 villages she is responsible for, as opposed to the maximum of four that took place in the evaluation. It additionally shows the importance of randomizing at the county level to minimize spillovers.

TABLE 1
BASELINE BALANCE

	Control	Information Only	Information + Cash
Mother age	25.91	26.44	26.48
		(.35)	(.33)
Mother attended school	.30	.25	.22
		(.34)	(.08)
Knowledge index	5.13	5.23	4.84
		(.73)	(.27)
Fed non-breast milk within 3 days	.48	.44	.45
		(.54)	(.65)
Child age	1.09	1.04	1.05
		(.12)	(.28)
Child underweight	.36	.32	.35
		(.44)	(.94)
Child stunted	.41	.41	.39
		(.90)	(.60)
Child wasted	.25	.21	.25
		(.33)	(.96)
Number household members	7.92	7.78	7.60
		(.55)	(.16)
Household head male	.76	.77	.78
		(.79)	(.65)
Has electricity	.69	.62	.67
		(.29)	(.78)
Stone roofing material	.68	.67	.67
		(.96)	(.93)
Annual income	116,697	106,019	120,215
		(.26)	(.69)
Monthly expenditures	7,319	7,094	6,257
		(.82)	(.20)

Note. Each row presents the mean within each treatment group. The values in parentheses in cols. 2 and 3 represent the *p*-value for a test of equality of means between that group and the control group, clustering standard errors at the Village Development Committee level. The sample sizes are 757 eligible women/635 eligible children in 724 eligible households in the control group; 802 eligible women/684 eligible children in 775 eligible households in the information-only group; and 802 eligible women/674 eligible children in 775 eligible households in the information plus cash group.

plus cash counties.[7] Table 1 shows the baseline means for all families that are part of this primary analysis sample, with the number in parentheses indicating the *p*-value for a test of equality of means between that experimental treatment arm and the control group. The groups are statistically indistinguishable across all variables. The table also indicates the relative lack of economic development among our sample—about one-third of women never attended school, almost half of newborn infants were fed something other than breast milk within 3 days of birth, and there are exceptionally high levels of malnutrition as evidenced by high rates of underweight, stunting, and wasting.

[7] As discussed below, we chose to survey about one-fourth of counties in a midline survey shortly before the intervention was completed. These counties are excluded from our primary analysis.

Finally, we also conducted a follow-up survey in the fall of 2016. This survey was mostly similar to the baseline and endline surveys conducted in 2013 and 2014, respectively. We removed several items to make the survey shorter and added questions about the severity of the April 2015 earthquake in Nepal and home practices with children. The survey was conducted in all households that were part of the intervention.

B. The Intervention

We explore the importance of two primary barriers to achieving full nutrition—lack of information and lack of money. In order to address the lack of information, we added a module to the regular monthly PAF meeting focusing on maternal health and infant nutrition issues, led by local SMs and female community health volunteers (FCHVs), henceforth, referred to collectively as local health workers.

To ensure that local health workers had appropriate levels of knowledge themselves to lead the information sessions, each local health worker underwent a weeklong training session that taught participants both the technical health issues and effective methods to engage local women and encourage behavioral change. The training sessions both taught the material to health workers and provided the opportunity for them to practice how they would lead their own meeting. All materials, such as cards and handouts to be used in the actual meetings, were provided and used during the training sessions.

Crucially, as part of the regular PAF meetings and basic health issues, local women were already familiar with their local health workers who led the intervention. Using the preexisting social capital developed through local health workers builds on the premise that existing capacity and institutional structures should be used to deliver impacts efficiently without needing to create new pathways to deliver the information and cash incentive treatments. Women were also presumably more likely to internalize and act upon new knowledge acquired as part of the intervention due to prior trusting relationships with local health workers.

The information session was added on to the end of the standard monthly meeting that already took place as part of PAF. The content of the information sessions promoted infant health and appropriate levels of development. This entailed some focus on nutrition for mothers with offspring in utero and best practices during pregnancy, as well as practices with infants regarding breastfeeding, care when sick, and supplemental feeding when older. In the meeting, the health worker used cards portraying the issue at hand to lead a group-wide discussion. For example, one card was a drawing of a woman breastfeeding, where the discussion might then revolve around issues related to frequency

depending on age of the child, proper attachment, and ways to try to solve issues such as not creating enough milk. The overall curriculum was developed by Helen Keller International, based on World Health Organization standards. There was no psychosocial component of the intervention.

The cash transfer was set at NPR 700 per month (approximately US$7), which represents about 8%–20% of median monthly income in our four districts. This figure is approximately in line with standard conditional cash transfers (CCTs; Fiszbein and Schady 2009). The transfer was distributed to the eligible mother at the regular village meeting, meaning that there was a conditionality on the cash transfer that can additionally be viewed as an incentive to take up the information treatment. The transfer was "labeled" for use on the child. Given that the conditionality is on simply going to collect the money, not on any type of behavior, as is standard in CCT programs, and that the transfer was only distributed for 5 months, this should not be viewed as a standard CCT program. Rather, the cash provided a short-term safety net, spurring the critical question of whether a shorter, cost-effective intervention can have similar improvements in child outcomes as would a more elaborate, prolonged CCT program.

III. Data

To estimate the impacts of the intervention, we gathered detailed data on eligible households in each experimental village. A household was deemed eligible if there was a woman present who was either pregnant or had a child younger than 2 years old. Baseline data collection occurred in August–October 2013, prior to the start of the intervention (see fig. A1 [fig. A1 and tables A1–A4 are available online] for a project time line of data collection and the intervention). Endline data collection occurred in November–December 2014 with a random sample of three-quarters of counties.[8] Two years after the end of the intervention, we conducted a long-run follow-up of all counties in November–December 2016.

[8] In order to better measure effects of how the cash was used and because effects of cash have been shown to fade very quickly after the cash is no longer being distributed (Baird, McIntosh, and Özler 2019), we also conducted a midline survey in August–September 2014 with the other one-quarter of counties. However, there was statistically significant differential attrition across treatment groups, with only 8% of women interviewed at baseline in the information plus cash group not found at midline and 15% of the control group not found at midline. Conditionality of the cash transfer (needing to attend the meeting) may have led some individuals from the cash group to be found who would not have been in the absence of the cash transfer, which may bias results. Individuals who remain have significantly lower monthly expenditures at baseline (results not shown). We therefore focus primarily on results from the endline and follow-up surveys.

A total of 4,228 women and 3,695 children under 2 years old were surveyed at baseline. Of these, 3,152 women and 2,783 children were in the counties surveyed at endline, which will be primarily used in the analysis. After attrition and trimming outliers, the primary women sample consists of 2,335 women. For children, attrition and trimming outliers leaves 1,972 children who were interviewed at both baseline and endline. The final child sample also includes 985 children surveyed at endline who were in households that were interviewed at baseline, including some newly born infants and some youth who had missing data at baseline. The final child sample thus consists of 2,957 children.

The baseline and endline surveys were nearly identical, and each included three separate modules to measure information on the household, the eligible mother, and the eligible infant. The household component of the survey was intended to be answered by either the household head or the eligible mother and measured a household's composition, assets, annual income, monthly expenditures, and daily food intake. The mother then answered questions about herself and her children, which measured her knowledge of maternal health and nutritional best practices and her actual behaviors with her youngest child while pregnant and breastfeeding. The survey concluded with anthropometric measurements of the child and a measure of child development as measured by the ASQ.

The ASQ is a screening mechanism that asks a mother whether her child can perform a specific task in one of five skill categories—communication, gross motor, fine motor, problem solving, and personal social. These questions are age specific and can be asked of children ranging from 1 to 60 months old.[9] Each of the five modules consists of six yes or no questions. For example, one of the gross motor questions for children ages 11–12 months old is, "When you hold one hand just to balance your baby, does she take several steps forward?" The raw score on each module is simply the number of "yes" answers. For ease of interpretation, within each age cohort, we standardize an individual's score so that the control group has a mean of 0 and a standard deviation of 1 on each module.

Though the ASQ captures child development, there are questions as to its reliability as an instrument in this context. First, it is designed to screen whether the child is at risk of developmental delays, not necessarily to represent a continuous measure of child development. Second, because it relies on mother's self-reports, it can be prone to biases. These biases may be exacerbated in light

[9] The age intervals of the questions are 2 months for children under 2 years old, 3 months for children from 2 to 3 years old, and then 6 months for children over 3 years old.

of social desirability bias and that parents in the treatment group in particular may want their children to appear to have stronger development. Together, recent research suggests the ASQ has significant drawbacks that can introduce measurement error, particularly for especially young children (Yue et al. 2019).

The primary analysis focuses on the endline survey, conducted entirely after the intervention ended in November–December 2014 (see fig. A1 for a project time line). Attrition did not differ by assigned group, nor was attrition selective by particular characteristics.[10] The 2,795 households interviewed at baseline that were interviewed again at endline make up our main sample.

The baseline data indicate that the women and households in our sample are particularly disadvantaged. More than 70% of women interviewed never attended any type of formal school, and about the same number are illiterate. This is somewhat different from data from the 2011 Nepal Demographic and Health Surveys (DHS), where only 44% of women in rural areas who would be considered eligible (by nature of being pregnant or having a child under 24 months old) never attended any school, and only 38% are illiterate. PAF targets poor areas with little economic development, likely driving the difference between women in our sample and women in rural areas from the nationally representative DHS survey.

The level of knowledge among eligible women at baseline indicates that there is substantial room for improvement from the information intervention. The average score on the knowledge index is only around five questions right (out of 10). Only about half of women indicated that a newborn infant should be fed breast milk exclusively for exactly 6 months, and about half answered that a pregnant woman should eat more food compared with before getting pregnant. Out of the total respondents, 40% responded that a baby should be breastfed more than usual during an episode of diarrhea, and 40% knew that a recently delivered woman should begin vitamin A supplementation within 45 days of delivery. This relative lack of knowledge on several crucial health issues means that the information component of the intervention has the potential for large gains, which could hopefully lead to improved developmental outcomes.

For the 2-year follow-up, there were 4,550 women and 5,036 children surveyed. Many of these women and children come from households that were not interviewed in previous survey waves—when we restrict the sample

[10] The raw levels of attrition, similar to the control group from the midline survey, indicate that the differential in the midline survey was driven by particularly low attrition in the cash group. This is consistent with the conditionality of the cash transfer driving the low level of attrition in the midline survey. See n. 8 for more detail.

to those who are in households that were interviewed at baseline, there are 2,762 women and 3,788 children.[11]

IV. Empirical Strategy

Because we randomly assign counties to each of the two treatment arms and the control group, women and children should have comparable outcomes at baseline and should be expected to continue to have comparable outcomes in the absence of any intervention. Table 1 showed the three groups were all statistically indistinguishable at baseline. Thus, any differences after the intervention can be attributed to be the causal effect of the intervention itself. Such an estimating equation is given by

$$y_i = \alpha + \beta_1 \text{INFO}_i + \beta_2 \text{CASH}_i + \gamma \boldsymbol{X_i} + \varepsilon_i, \quad (1)$$

where y_i refers to an outcome for person i, who could be the mother or her child. The coefficient β_1 captures the causal impact of the information-only treatment, and β_2 captures the causal impact of the information plus cash treatment. We also use an F-test to test for a statistical difference between the information-only and information plus cash groups. Our empirical strategy is similar both for the short-run impacts immediately following the intervention and the longer-term impacts 2 years after the intervention ended.

We should not need to include any controls in $\boldsymbol{X_i}$ because the groups are comparable at baseline. However, in order to improve precision of our estimates, we include basic control variables that depend on whether the outcome variable is for the household, mother, or child, such as household composition; age; mother's schooling; whether the child was stunted, overweight, or wasted at baseline; and whether the child was surveyed at baseline.[12] We estimate specifications both using no controls and using basic controls, and the results are similar. Due to the increase in precision, we report the specifications that include basic controls. Results using no controls are available on request.

In estimating the effects on child development (through the ASQ), we add an enumerator fixed effect. The answer to these screening questions reflects the mother's subjective assessment of whether a child can or cannot do something. The mother may be more or less willing to answer these questions honestly depending on the rapport established between the enumerator and the

[11] All of these numbers exclude those interviewed as part of the midline survey to keep the samples comparable between endline and follow-up.

[12] Because the sample includes some children who were not measured at baseline, both because they were not born yet or because they were not present when the household was surveyed, the omitted group is those who were not present at baseline.

woman. Therefore, we use an enumerator fixed effect so that any effects we find are entirely identified by variation in treatment status for each individual enumerator.[13]

We also use item response theory to estimate an "ability" parameter for each of the five ASQ screening modules as well as an overall ability parameter for child development. Some of the questions reveal more information about a child's true ability, and so item response theory is a way of giving additional weight to questions that are particularly informative. However, the results using these methods and using the standardized score on each module yield very similar results, so for simplicity we report only the latter method.

Because we analyze many outcomes, it is likely that some will be statistically significant purely by chance. In order to deal with multiple hypotheses, we construct indexes to aggregate many outcomes into a single index measure. For women's knowledge, we score a mother's answers to 10 separate questions as right or wrong and then sum up the total score on this 10-question "test." For all other areas, we follow the methods of Anderson (2008) and used by Attanasio, Oppedisano, and Vera-Hernández (2015).[14] For example, we construct a behavior index that aggregates all of the measures of a woman's behavior during pregnancy and with a newborn infant into one measure. We first pick the variables in our survey that are closely related. We then normalize each of the variables to have a mean of zero and a standard deviation of one in the control group, redefine all variables to have a positive interpretation, and then take a weighted average of the normalized outcomes.[15] The weights are taken from the variance covariance matrix of all the outcomes considered, with higher weight placed on items that contain unique information and lower weight placed on those that are highly correlated with other variables in the index.

V. Results—Short Run

Table 2 shows the intermediate impacts of the information intervention on women's knowledge. Women in both the information-only and information

[13] Though there could be potential measurement issues with small infants, including a similar enumerator fixed effect does not change our anthropometric results. We report only the results on anthropometrics not including the enumerator fixed effect.

[14] We also construct a knowledge index using the same 10 variables using the Anderson (2008) method that is used for other key outcomes. The results are similar. For ease of interpretation, we report the simple sum of the knowledge index in the text and in tables, though the results with the alternative index are available on request.

[15] For example, one question we ask is, "In the first 3 days after delivery, was your child given anything to drink other than breast milk?" We invert this outcome so it is an indicator for whether a child is fed only breast milk within 3 days of delivery.

TABLE 2
WOMEN'S KNOWLEDGE

	Breastfeed Exclusively for 6 Months (1)	Eat More during Pregnancy (2)	Breastfeed More When Baby Has Diarrhea (3)	Number of Food Items Mentioned for Kids (4)	Knowledge Index (5)
Information only	.047	.065	.156***	.388**	.513**
	(.045)	(.050)	(.050)	(.180)	(.210)
Information + cash	.125***	.108**	.084*	.442**	.908***
	(.040)	(.049)	(.048)	(.173)	(.219)
Difference	.079*	.043	−.073	.054	.396*
	(.043)	(.053)	(.054)	(.178)	(.206)
Control mean	.567	.608	.433	4.131	5.313
Observations	2,335	2,335	2,335	2,335	2,335
R^2	.029	.020	.041	.033	.083

Note. The dependent variable in cols. 1–3 is an indicator variable signifying whether the woman responded affirmatively that the item was true. For example, a question asked, "In your opinion, for how long should a newborn infant be given nothing but breast milk?" with options of less than, more than, or exactly equal to 6 months. The dependent variable in col. 1 is the share of respondents answering exactly 6 months. The knowledge index in col. 5 is the number of questions answered correctly out of 10 general knowledge questions, including those in cols. 1–3. Regression controls include dummies for age cohorts of the female respondent, as well as individual-level controls (if the woman ever went to school and baseline weight and height) and household-level controls (age and gender of household head and number of household members).
* $p < .10$.
** $p < .05$.
*** $p < .01$.

plus cash group do significantly better at endline, suggesting that the information sessions led to improved knowledge. The knowledge index (col. 5), which counts the number of correct answers to 10 questions regarding maternal health and nutritional issues, indicates that women in the information plus cash intervention on average answer 0.9 more questions correctly than women in the control group. Relative to the control group mean, this is an improvement of 17%. The impact is also significantly higher than that of women in the information-only group. The index is our preferred measure of knowledge because it aggregates across many related outcomes. Without any type of correction, we would likely find statistically significant improvements on some outcomes purely by statistical chance. Levels of knowledge in the control group are still low at endline, as there has been essentially no change from the baseline level of the knowledge index. The impacts on the other individual outcomes considered in the table are consistent with the information intervention improving knowledge, particularly so for women in the information plus cash group.

It is somewhat surprising that women who participate in the same session on information experience different gains in knowledge. It is possible that women who also receive cash are more invested in the sessions as they believe

TABLE 3
HOUSEHOLD CHARACTERISTICS

	Monthly Expenditures (1)	Annual Income (2)	Calories per Person (3)	Food Groups for Children Ages 7–24 Months (4)	Attend CO Meeting (5)	Has/Expects New Child (6)
Information only	−.076	−.104	32.201	.236*	.013	−.046
	(.099)	(.070)	(56.925)	(.131)	(.049)	(.030)
Information + cash	−.089	.029	84.093	.137	.117**	−.020
	(.125)	(.072)	(55.842)	(.117)	(.049)	(.036)
Difference	−.013	.132*	51.892	−.098	.104**	.026
	(.117)	(.077)	(56.281)	(.109)	(.047)	(.031)
Control mean	8.421	11.433	2204.884	2.396	.353	.424
Observations	2,169	2,169	2,169	1,158	2,335	2,335
R^2	.057	.066	.014	.048	.114	.061

Note. Expenditures and income are in logs. Food groups (col. 4) are the number of key food groups (dairy, grains, vitamin A–rich vegetables, other vegetables, eggs, meat, and nuts) that the child ate from in the past 24 hours, as reported by the mother. All regressions control for the number of household members, age and gender of household head. In cols. 1–3, which are measures at the household level, regression controls also include asset indicators for electricity, a stone roof, and a separate kitchen. In cols. 4–6, which are measures at the female level, regression controls also include the female respondent's weight, height, and whether she ever attended school.

* $p < .10$.

** $p < .05$.

cash is important to making some of the behavioral changes, such as appropriate supplemental feeding for young children. Alternatively, it is possible that women in the information plus cash group simply participate more in the information sessions, especially since receiving the cash transfer was conditional on attending the meeting. Column 5 of table 3 shows that women in the information plus cash group are significantly more likely to have attended a village meeting in the past month than women in both the control group and the information-only group, though the reference window for the question is after the intervention ended.[16] However, this finding is consistent with women who also received cash more regularly attending meetings, while the intervention was ongoing and then continuing to do so afterward.

Column 1 of table 3 shows that expenditures in the past month were statistically indistinguishable across all groups. In column 3, we see that there is marginally higher caloric intake in the past 24 hours among the information plus cash group (p-value = .134). It is unclear whether this is a reallocation of income to spend more on food and less elsewhere or an increase in particularly high-caloric foods given the knowledge gained from the information aspect of the intervention. Either way, we cannot rule out that the cash transfer simply

[16] At the midline survey, when the question refers to a month during which the intervention is taking place, there is no statistical difference in attendance across treatment groups. However, issues of attrition during the midline survey mean that this finding is not completely reliable.

acts as an incentive to take up the information side of the intervention, which is important in interpreting the meaning of the results. Children in treatment groups are also fed more diverse diets, as is shown in column 4—among all children aged 7 months and older, children ate from more food groups, though the difference is only significant from the control group in the information-only group.[17] A similar pattern emerges when considering an increased likelihood of eating protein, which is the food group most important for better health and growth in young children.

Table 4 shows the impacts of the intervention on behavioral practices, only including women who had a new child since the baseline survey, including first-time mothers. Women in the information plus cash group exhibit more "good" behavioral practices than those in either the control group or the information-only group, as shown by the behavioral index in column 7. This index aggregates the first six outcomes in the table into one summary variable. The women thus seem to act upon the increased knowledge found in table 2 and adjust behaviors, particularly so for those women in the information plus cash group who had the biggest gains in knowledge.

Though women increase knowledge and improve their behavior, particularly for the information plus cash group, we do not find evidence that the improvements are passed down to children in better overall outcomes. Table 5 shows development outcomes, and table 6 shows anthropometric outcomes for children who were aged 2 years or younger at baseline, including those who were not yet born. By pooling together newborn children and infants who were already alive, we are able to fully capture effects on children most likely to be affected by the intervention. Though children in the information plus cash group have similar child development scores as the control group, they perform better than children in the information-only group (p-value $= .104$). The increase of nearly 0.1 standard deviations in the development index (relative to the information-only group), shown in column 6, is in line with the gains found in Macours et al. (2012). These gains relative to the information-only group are partially driven by improvements in gross motor skills (col. 2), which are likely to be particularly hindered by malnutrition (Engle et al. 2007). Improvements in personal social skills also play an important role. Anthropometric outcomes are statistically similar across all groups, indicating the intervention had no impact on these measures. Prior interventions have tended to find the strongest gains to alleviating malnutrition in reduced levels of stunting (e.g., Rivera et al. 2004; Maluccio and Flores 2005; Fernald, Gertler, and Neufeld 2009). We

[17] The food groups considered are dairy, grains, vitamin A–rich vegetables, other vegetables, eggs, proteins, and nuts.

TABLE 4
HEALTH PRACTICES WITH NEW CHILD

	Number of Antenatal Visits (1)	Vitamin A within 42 Days (2)	Fed Non-Breast Milk within 3 Days (3)	Iron Tables for 5–6 Months (4)	First Breastfed within 1 Hour (5)	Deworming Pills (6)	Behavior Index (7)
Information only	−.111	.068	−.132**	.041	.010	−.038	.010
	(.251)	(.062)	(.060)	(.053)	(.070)	(.055)	(.039)
Information + cash	.477**	.189***	−.139***	.138**	.067	−.064	.116***
	(.234)	(.062)	(.053)	(.054)	(.067)	(.051)	(.041)
Difference	.588**	.122*	−.007	.097*	.057	−.026	.106**
	(.275)	(.069)	(.053)	(.056)	(.069)	(.057)	(.042)
Control mean	3.095	.461	.344	.290	.465	1.274	.000
Observations	679	679	679	679	679	679	679
R^2	.060	.059	.050	.043	.024	.032	.036

Note. The sample now consists only of women who had a new child between baseline and endline, including first-time mothers. The dependent variable refers to her practices both during and after pregnancy. The behavior index is created by taking a weighted sum of the first six demeaned variables by using the method described in Anderson (2008). Regression controls include dummies for age cohorts of the female respondent, as well as individual-level controls (if the woman ever went to school, and baseline weight and height) and household-level controls (age and gender of household head, and number of household members).
* $p < .10$.
** $p < .05$.
*** $p < .01$.

TABLE 5
DEVELOPMENT MEASURES

	Communication (1)	Gross Motor (2)	Fine Motor (3)	Personal Social (4)	Problem Solving (5)	Development Index (6)
Information only	−.005	−.036	.030	−.063	−.009	−.019
	(.046)	(.053)	(.046)	(.052)	(.050)	(.037)
Information + cash	.022	.064	.068	.039	.000	.041
	(.046)	(.057)	(.046)	(.058)	(.048)	(.040)
Difference	.027	.101*	.037	.102**	.010	.060
	(.048)	(.054)	(.048)	(.051)	(.052)	(.037)
Control mean	−.003	−.002	−.002	−.002	−.002	−.002
Observations	2,614	2,614	2,614	2,614	2,614	2,614
R^2	.269	.254	.339	.309	.418	.418

Note. All outcomes are expressed in standardized *Z*-scores for each six-question module. The development index is created by taking a weighted sum of the five demeaned scores by using the method described in Anderson (2008). Regression controls include dummies for gender and age cohorts taking each separate ASQ module, as well as individual-level controls (whether mother ever went to school and indicators for whether the child was stunted, underweight or wasted at baseline or did not have baseline data) and household-level controls (age and gender of household head and number of household members). Also includes a fixed effect for which enumerator conducted the endline survey.
* $p < .10$.
** $p < .05$.

TABLE 6
ANTHROPOMETRIC MEASURES

	Underweight (1)	Stunted (2)	Wasted (3)	Weight-for-Age (4)	Height-for-Age (5)	Weight-for-Length (6)	Sick Past 30 Days (7)	Anthropometric Index (8)	Anthropometric Index (Older Siblings) (9)
Information only	.011	.028	−.028	−.019	−.118	.045	−.031	.025	−.025
	(.024)	(.026)	(.019)	(.079)	(.094)	(.105)	(.034)	(.032)	(.073)
Information + cash	−.008	.035	−.019	−.009	−.193*	.129	−.039	.020	.108
	(.025)	(.026)	(.020)	(.074)	(.103)	(.114)	(.028)	(.030)	(.074)
Difference	−.019	.007	.009	.010	−.075	.084	−.008	−.005	.132**
	(.023)	(.027)	(.021)	(.070)	(.100)	(.103)	(.033)	(.031)	(.064)
Control mean	.313	.515	.151	−1.428	−1.970	−.433	.444	−.013	.161
Observations	2,956	2,956	2,956	2,956	2,956	2,956	2,956	2,956	264
R^2	.137	.117	.033	.187	.131	.069	.038	.097	.220

Note. The anthropometric index creates an index of the four health indicator variables for whether a child is underweight, stunted, wasted, or has been sick in the past 30 days by using the method described in Anderson (2008). Regression controls include dummies for gender and age cohorts taking each separate ASQ module, as well as individual-level controls (if mother ever went to school and indicators for whether the child was stunted, underweight or wasted at baseline or did not have baseline data) and household-level controls (age and gender of household head and number of household members). Column 9 includes siblings of children in eligible households (same as the rest of the table) who were aged between 25 and 36 months at baseline.

* $p < .10$.

** $p < .05$.

also find no differential impacts on anthropometric and development outcomes by gender (not shown).[18]

The cash transfer is labeled as being targeted at the youngest child, but a parent could allocate the additional cash to another child in the household. Though much of the information is specific to infants, issues like nutritional diversity apply to all children. The final column of table 6 shows that anthropometric outcomes of older siblings are significantly higher for the information plus cash group than the information-only group. These are the siblings of children in tables 5 and 6 who were aged 25–36 months at baseline and are thus not technically eligible for the intervention. These improvements are driven by a lower likelihood of being underweight and a lower likelihood of being sick in the past 30 days.

VI. Mechanisms

The overall theory of change presented so far has demonstrated that the information sessions improved knowledge, particularly in the cash group; these gains in knowledge in the cash group lead to changes in behavioral practices; and behavioral improvements, in turn, may spur some child development. In this section, we further probe each aspect of this chain to demonstrate the link between knowledge and behavior and to posit why there may be a lack of results on anthropometrics.

Many of the questions about knowledge translate directly into practices that we surveyed the women about.[19] For example, one knowledge item asks a mother how long she should exclusively breastfeed her newborn infant. The associated practice assesses how long she actually exclusively breastfed her newborn infant. We can assess whether knowledge of the issue corresponds with actually implementing it.

Table 7 shows that having knowledge regarding a behavior significantly increases the likelihood of implementing that behavior. Each column shows the

[18] This analysis has focused only on participants in the endline survey. The midline survey results are not shown because of issues with attrition discussed in n. 8. However, we follow the procedure in Lee (2009) to estimate upper and lower bounds to correct for the sample selection issues stemming from attrition. Table A1 estimates the upper and lower bounds on the treatment effects for the indexes reported in the main tables. Estimates are reported separately for the information-only group vs. control group and the information plus cash group vs. control group. The bounds often cannot rule out the findings from the endline, implying that the two survey waves can be roughly consistent with each other.

[19] A knowledge item that does not directly correspond to an action is whether a pregnant woman should eat more, less, or the same as before she became pregnant. Asking a woman about her eating practices while pregnant would be too broad a question without using definitive numbers. This would more likely produce inaccurate responses, so we did not ask such a question.

TABLE 7
CONNECTION BETWEEN KNOWLEDGE AND PRACTICE

	Antenatal Visits (1)	Exclusive Breastfeed 6 Months (2)	First Breast-fed within 1 Hour (3)	Only Breast Milk within 3 Days (4)	Took Iron Pills for 5–6 Months (5)	Took Deworming Pills (6)	Vitamin a within 45 Days (7)
Knowledge	.656***	.102***	.273***	.246***	.307***	−.234***	.412***
	(.163)	(.039)	(.064)	(.056)	(.035)	(.034)	(.035)
Constant	2.905***	.527***	.246***	.523***	.169***	1.404***	.326***
	(.110)	(.031)	(.061)	(.053)	(.026)	(.028)	(.025)
Observations	679	679	679	679	679	679	679
R^2	.023	.010	.026	.028	.102	.065	.171

Note. Runs a simple ordinary least squares regression between whether a woman correctly answers a question in the knowledge portion of the survey and her reported practice with her youngest newborn child. For example, col. 1 regresses if a woman knows the appropriate number of antenatal care visits is four on the number of reported antenatal care visits. No control variables are used, and the treatment status is not included in any fashion. The sample consists of women who had a new baby since the baseline survey, as in table 4.
*** $p < .01$.

results of regressing having knowledge of an item on actually having done it. With the exception of the first column, the coefficient on "knowledge" can be interpreted as the additional probability of taking an action if you know it—for example, women who know a newborn infant should be exclusively breastfed for 6 months are 10.2 percentage points more likely to exclusively breastfeed their child for 6 months. This regression does not incorporate treatment status in any way but rather is suggestive by providing the correlation between knowing something and the likelihood of practicing that behavior. As in table 4, we focus on the subset of women who had a new child after the baseline survey so that the behavioral practice questions correspond to when new knowledge could have been acted on.

Women who know that the first milk should be given to the baby are 27 percentage points more likely to first breastfeed the baby within 1 hour of birth. This is a 111% increase over the 25% of women who first breastfeed their baby within 1 hour despite not correctly answering that the first milk should be given to the baby. Though not causal, there is a clear correlation between knowledge and action. The other columns indicate similar patterns.

Knowing the correct timing for implementing practices is particularly predictive of compliance with the recommended best practice. Column 5 shows that there is a 31 percentage point (or 182%) increase in taking iron tablets for the suggested amount of time if a woman knows the correct recommendation. The correlation between this knowledge and taking iron tablets at all is still significant, but the magnitude is much smaller; knowledge of the correct timing to take iron tablets is associated with a 14 percentage point (or 18%) increase in

the likelihood of ever taking iron tablets during pregnancy. This provides further suggestive evidence that knowledge directly leads to action.

Yet in the information-only group, changes in knowledge did not lead to changes in behavior. Improvements in both knowledge and behavior were significantly larger in the information and cash group relative to the information-only group. But in comparison to the control group, the information-only group experienced an improvement in knowledge but not behavior. One possibility is that of a threshold effect—that some minimal change in knowledge is required before behavior also improves, which the information-only group did not exceed. Another possibility is that our measures of behavior were in the right tail of behavioral actions—so that the smaller improvement in knowledge in the information-only group may have led to small behavioral changes, but our measures did not capture this change. With our data, we are unable to distinguish these stories. We also assessed whether there were differential impacts by income and age of the youngest child (tables A3 and A4) but did not find a significant impact on behavior for either subgroup in the information-only group.[20]

Behavior changes should likely lead to improvements in child outcomes; these practices are World Health Organization suggestions precisely because they are scientifically shown to improve outcomes. However, our results on this front are somewhat mixed. We do see gains in child development among children aged between 0 and 2 years old at baseline, though only relative to the information-only group. However, there are no corresponding changes in anthropometrics.

Several potential factors might explain this phenomenon. First, time has been too short to accurately measure any differences. The information sessions lasted for 9 months, and the cash transfer was provided for only 5 months. Additionally, by measuring children immediately following the completion of the intervention (we collected endline data 2–3 months after the completion of the intervention, as shown in fig. A1), there may not have been enough time for the behavioral changes to be fully reflected in the child's outcomes. However, as discussed in the next section on long-run impacts, we similarly find no development or anthropometric gains 2 years after the end of the intervention.

Another potential explanation is that the general level of poverty may be so high that even behavioral changes might not be sufficient to overcome the barrier associated with achieving proper development. For example, if households

[20] We found a differentially larger change in behavior for the information-only group for low-income women relative to higher-income women. Yet because neither of the impact estimates for low- or high-income women were statistically different from zero, we do not place much weight on this differential pattern across subgroups.

cannot access clean drinking water or mothers experience severe micronutrient deficiencies, the adverse effects of these prevailing conditions may be such that improving targeted practices might not help children. As suggestive evidence, we find a clear correlation between having a toilet and access to good drinking water on general anthropometric outcomes. Impacts on anthropometric and child development outcomes do not differ significantly for those with and without a toilet or access to good drinking water.[21] We also divide the population into those with baseline income above and below the median within our sample but find no impacts on anthropometric and child development outcomes for either subgroup (table A3).[22]

VII. Results—Long-Run Follow-Up

Table 8 shows the primary effects on indexes measuring maternal knowledge of best practices, behavior of mothers with new children, and child development and anthropometric outcomes 2 years after the intervention (surveys conducted in November and December of 2016). The samples analyzed include mothers and children surveyed at the follow-up who were in families who were also surveyed at baseline and who lived in counties that were part of the endline sample; people can be included even if they were not present at the endline survey, either due to nonresponse or because a child was not yet born.[23] We find that mothers in the information plus cash group answer an additional half-question correctly out of 10 questions about maternal health and infant nutrition best practices. This is statistically significant, though not significantly different from the 0.4 increase in the information-only group. Immediately following the intervention, the estimated gain in knowledge was higher (almost one question more answered correctly) and statistically significantly higher than the information-only group. Some of this change may be that the control group mean increased from 5.3 to 5.8.

The results on behavior in column 2 display a somewhat similar pattern; there is still a nearly significant estimated impact of the intervention on behavioral practices with newborn children among women in the information plus

[21] These two sets of regressions are not reported but are available on request from the authors.

[22] In addition, we assessed whether the intervention had differentially larger impacts for children who were younger than 1 or older than 1 at baseline (table A4). We did not find significant effects of information plus cash regardless of the age of the children. Information only reduced development in the information-only group for children ages 13 to 24 months old relative to both the control and information plus cash groups, though did not affect children 12 months and under.

[23] If we include the full sample of counties, including those not surveyed at endline, we would find similar results. To make the endline and follow-up results most comparable, we focus on only those counties that make up the endline sample.

TABLE 8
LONG-RUN FOLLOW-UP RESULTS

	Knowledge Index (1)	Behavior Index (2)	Development Index (3)	Anthropometric Index (4)
Information only	.422*	.045	.021	−.030
	(.223)	(.038)	(.045)	(.041)
Information + cash	.487**	.054	−.017	−.045
	(.229)	(.035)	(.044)	(.035)
Difference	.065	.009	−.038	−.014
	(.226)	(.039)	(.052)	(.041)
Control mean	5.754	.010	−.017	.003
Observations	2,340	1,100	3,213	3,373
R^2	.081	.043	.364	.008

Note. The knowledge index is created by summing the score of correct questions out of 10 general knowledge questions. All other indexes are created by taking a weighted sum of several relevant demeaned scores by using the method described in Anderson (2008). All regressions include basic controls, and the development index in col. 3 also includes a fixed effect for which the enumerator conducted the follow-up survey.
* $p < .10$.
** $p < .05$.

cash group (p-value = .120), but the estimated impact is somewhat smaller than what was found at endline.

Though the gains we found in knowledge and behavior persist 2 years later, we do not find similar long-term gains in child anthropometric or development outcomes. Columns 3 and 4 show that there are no statistically significant differences between groups on the development or anthropometric index. In addition to being statistically insignificant, the estimated coefficients are also small in magnitude and often negative.

As in the analysis of outcomes focusing only on those at endline, the analytic sample included in the follow-up analysis displays few and small differences at baseline (table A2). This table is measured using only those individuals that are part of the analysis in table 8. Since the groups were similar at baseline, and the only difference that affects particular households is their treatment assignment, we believe that the estimates from table 8 are the long-run causal impact of the intervention.

In the results immediately following completion of the intervention, we found positive impacts on child development in the information plus cash group relative to the information-only group. There was little understanding as to the precise mechanism that drove this finding. In order to better understand the reasons for improvement in child development, we added several questions to the survey about practices with children. These include things like whether a child has at least three books or at least one toy and whether parents often talk to their child or eat meals together as a family. Table 9 shows the results of estimating the impact of the intervention on these child practice

TABLE 9
CHILD PRACTICES (IMPACT REGRESSIONS)

	Has at Least Three Books (1)	Has at Least One Toy (2)	Believe Parents Should Teach Kids (3)	Eat Meals with Family (4)	Often Talk to Child (5)
Information only	−.014	−.017	−.017	−.098**	.005
	(.015)	(.066)	(.015)	(.046)	(.016)
Information + cash	.007	.087	−.019	−.034	.011
	(.018)	(.063)	(.017)	(.048)	(.017)
Difference	.025	.096	−.001	.052	.004
	(.015)	(.062)	(.017)	(.045)	(.013)
Control mean	.042	.535	.984	.768	.968
Observations	845	845	845	845	845
R^2	.024	.029	.010	.013	.004

Note. The dependent variable in all columns is an indicator variable. All questions are newly asked in the follow-up survey. The sample only includes households where the youngest child is under 2 years old.
** $p < .05$.

outcomes. The regressions are estimated for similar households as the primary follow-up results and include the same basic controls, though only include households where the youngest child is under 2 years old. All measures are structured so that positive coefficients imply better practices. The intervention did not have a statistically significant positive impact on any of these measures and may even have slightly reduced the number of children in the information-only group who eat meals with their family.

VIII. Conclusion

In this paper, we evaluate the effects on early-childhood development of providing information on best practices regarding nutrition and health for children below the age of 2 and the same information component with a cash transfer, using a randomized controlled trial in Nepal. We find that there were significant increases in knowledge for women in both the information and information plus cash groups, though the increase in knowledge was approximately twice as large in the group that also received the cash transfer. Women in the information plus cash group, who had the biggest gains in knowledge, also improved in maternal health and early-childhood best practices, indicating that the intervention was successful at not just increasing knowledge but also creating behavioral change. We also found suggestive evidence that child development improves in the short run, albeit only for those in the information plus cash group relative to the information-only group. Yet challenges with the measure of child development indicate these results should be considered cautiously.

We also conducted a follow-up survey after the intervention ended to assess long-run impacts. We find that 2 years after the intervention ends there is still

a higher level of knowledge and better behavioral practices in areas that received information and cash. However, these improvements did not manifest in higher levels of child development and anthropometric outcomes in children.

Moving forward, it would also be useful to study the role of the individual health workers and how their effectiveness as leaders and prior relationships with community members impacted the information intervention. One thing that makes the information intervention unique is its use of local social mobilizers and female community health volunteers, who were already familiar to the community. Using these existing systems likely improved the effectiveness of the intervention given that women would be more likely to implement behavioral changes when taught by someone they trusted in a cultural context they understood, rather than someone unfamiliar simply teaching something as the "best" way of doing it. Leveraging existing systems also makes the intervention more cost-effective and thus of greater interest to policymakers. The local health workers are particularly important when considering scaling up this experiment given that they were only asked to add the information to at most four of the approximately 30 villages in which they operate. Thus, if factors like the communication skills, knowledge, gender, or any observable characteristic of the local health worker play an important role in the effectiveness of the information component of the intervention, any positive effects would be even more pronounced when scaled up. It would be imperative to understand the role that the contribution that especially effective health workers play and make future decisions about where to best focus resources based on findings. We leave such a study to future work.

References

Alderman, Harold, John Hoddinott, and Bill Kinsey. 2006. "Long Term Consequences of Early Childhood Malnutrition." *Oxford Economic Papers* 58, no. 3:450–74.

Almond, Douglas, and Janet Currie. 2011. "Killing Me Softly: The Fetal Origins Hypothesis." *Journal of Economic Perspectives* 25, no. 3:153–72.

Anderson, Michael L. 2008. "Multiple Inference and Gender Differences in the Effects of Early Intervention: A Reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects." *Journal of the American Statistical Association* 103, no. 484:1481–95.

Attanasio, Orazio P., Veruska Oppedisano, and Marcos Vera-Hernández. 2015. "Should Cash Transfers Be Conditional? Conditionality, Preventive Care, and Health Outcomes." *American Economic Journal: Applied Economics* 7, no. 2:35–52.

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

Bhutta, Zulfiqar A., Tahmeed Ahmed, Robert E. Black, Simon Cousens, Kathryn Dewey, Elsa Giugliani, Batool A. Haider, et al. 2008. "What Works? Interventions for Maternal and Child Undernutrition and Survival." *Lancet* 371, no. 9610:417–40.

Bhutta, Zulfiqar A., Jai K. Das, Arjumand Rizvi, Michelle F. Gaffey, Neff Walker, Susan Horton, Patrick Webb, et al. 2013. "Evidence-Based Interventions for Improvement of Maternal and Child Nutrition: What Can Be Done and at What Cost?" *Lancet* 382, no. 9890:452–77.

Carneiro, Pedro, Lucy Kraftman, Giacomo Mason, Lucie Moore, Imran Rasul, and Molly Scott. 2021. "The Impacts of a Multifaceted Pre-natal Intervention on Human Capital Accumulation in Early Life." *American Economic Review* 111, no. 8:2506–49.

Dupas, Pascaline, and Edward Miguel. 2017. "Impacts and Determinants of Health Levels in Low-Income Countries." In *Handbook of Economic Field Experiments*, vol. 2, 3–93. Amsterdam: North-Holland.

Engle, Patrice L., Maureen M. Black, Jere R. Behrman, Meena Cabral De Mello, Paul J. Gertler, Lydia Kapiriri, Reynaldo Martorell, Mary Eming Young, and International Child Development Steering Group. 2007. "Strategies to Avoid the Loss of Developmental Potential in More than 200 Million Children in the Developing World." *Lancet* 369, no. 9557:229–42.

Fernald, Lia C. H., Paul J. Gertler, and Lynnette M. Neufeld. 2009. "10-Year Effect of Oportunidades, Mexico's Conditional Cash Transfer Programme, on Child Growth, Cognition, Language, and Behaviour: A Longitudinal Follow-Up Study." *Lancet* 374, no. 9706:1997–2005.

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

Lagarde, Mylene, Andy Haines, and Natasha Palmer. 2007. "Conditional Cash Transfers for Improving Uptake of Health Interventions in Low- and Middle-Income Countries: A Systematic Review." *JAMA* 298, no. 16:1900–1910.

Lee, David S. 2009. "Training, Wages, and Sample Selection: Estimating Sharp Bounds on Treatment Effects." *Review of Economic Studies* 76, no. 3:1071–102.

Macours, Karen, Norbert Schady, and Renos Vakis. 2012. "Cash Transfers, Behavioral Changes, and Cognitive Development in Early Childhood: Evidence from a Randomized Experiment." *American Economic Journal: Applied Economics* 4, no. 2:247–73.

Maluccio, John, and Rafael Flores. 2005. *Impact Evaluation of a Conditional Cash Transfer Program: The Nicaraguan Red de Protección Social*. Washington, DC: International Food Policy Research Institute.

Pelletier, David L., Edward A. Frongillo Jr., Dirk G. Schroeder, and Jean-Pierre Habicht. 1995. "The Effects of Malnutrition on Child Mortality in Developing Countries." *Bulletin of the World Health Organization* 73, no. 4:443.

Rivera, Juan A., Daniela Sotres-Alvarez, Jean-Pierre Habicht, Teresa Shamah, and Salvador Villalpando. 2004. "Impact of the Mexican Program for Education, Health, and Nutrition (Progresa) on Rates of Growth and Anemia in Infants and Young Children: A Randomized Effectiveness Study." *JAMA* 291, no. 21:2563–70.

Ruel, Marie T., Harold Alderman, and Maternal and Child Nutrition Study Group. 2013. "Nutrition-Sensitive Interventions and Programmes: How Can They Help

to Accelerate Progress in Improving Maternal and Child Nutrition?" *Lancet* 382, no. 9891:536–51.

Strupp, Barbara J., and David A. Levitsky. 1995. "Enduring Cognitive Effects of Early Malnutrition: A Theoretical Reappraisal." *Journal of Nutrition* 125, no. 8:2221S–2232S.

World Bank. 2007. *World Development Report 2008: Agriculture for Development*. Washington, DC: World Bank.

———. 2012. *Nutrition in Nepal: A National Development Priority*. Washington, DC: World Bank.

Yue, Ai, Qi Jiang, Biaoyue Wang, Cody Abbey, Alexis Medina, Yaojiang Shi, and Scott Rozelle. 2019. "Concurrent Validity of the Ages and Stages Questionnaire and the Bayley Scales of Infant Development III in China." *PLoS ONE* 14(9):e0221675.