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The Role of Information and Cash Transfers on Early Childhood Development

Evidence from Nepal

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Abstract

Although substantial progress has been made in combating malnutrition at the global level, chronic maternal and child malnutrition remains a serious problem in many parts of the developing world. This paper, using a randomized control trial design in Nepal, evaluates a program that provided information on best practices in providing child care and cash to families in extremely poor areas with pregnant mothers and/or children below the age of 2. The analysis finds significant and sizable impacts of the information plus cash intervention on maternal knowledge, behavior, child development, and nutrition. The sizes of these impacts along some measures of knowledge and development are significantly different from the information-only intervention group, suggesting a potential role for providing a short-term cash safety net along with information to tackle the problem of malnutrition.

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The Role of Information and Cash Transfers on Early Childhood Development: Evidence from Nepal

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1 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 remains a serious problem in parts of the developing world. Even within developing countries, the rural poor disproportionately bear the burden of child malnutrition (World Development Report 2008). A key task for researchers and policy makers alike is to examine the reasons behind malnutrition in rural areas.

Two important causes of malnutrition are a lack of information and a lack of income. Unlike other childhood diseases, malnutrition is a "silent killer" - symptoms are not obvious until it is often too late to reverse stunting or its other deleterious consequences (UNICEF 2011). Therefore, providing parents and/or pregnant mothers information on best practices regarding nutrition and maternal health issues can lead to significant reductions in malnutrition. The second relevant cause is a lack of income. Even if parents know the right ways to care for a child, they might lack the resources to engage in these best practices. For example, young children need diets that are higher in nutritional content than adult diets (Dewey, 2013). However, income constraints might prevent the parents from engaging in these activities. In this paper, we evaluate a program that provided information on best practices regarding child care 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 Living Standard Survey 2010/11, 46.7 percent of children under the age of 5 are stunted, 15.1 percent are wasted, and 36.3 percent are underweight. Moreover, pregnant mothers have sub-optimal 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, Frongillo Jr, Schroeder, and Habicht, 1995; Strupp and Levitsky, 1995; Alderman, Hoddinott, and Kinsey, 2006). The economic costs of malnutrition are very high – an estimated 2-3 percent of GDP (US$250 million

to US$375 million) is lost every year in Nepal due to vitamin and mineral deficiencies alone (USAID 2014). 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 on the importance of balanced diet, proper sanitation and hygiene, breastfeeding, and other health matters have only 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 unclear whether it is the lack of money or the behavior associated with cultural and social practices that have helped perpetuate malnutrition for so long in Nepal.

Using a randomized control trial design in rural areas in Nepal, we evaluate the effects of two different treatments on child development outcomes. One treatment arm was provided information on best practices regarding nutrition and child care for children below the age of two, and a second treatment arm received the same information and in addition, a conditional cash transfer. To receive the cash transfer, a woman simply had to attend the regular group meeting. A transfer of NPR 700 (USD 7) per month, equivalent to 8-20% of median monthly household income, was given over a period of five months. 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 nine months, and were identical across the treatment arms with and without a cash transfer. Importantly, our work utilized existing health and financial infrastructures (such as community health volunteers and group 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 best practices of childcare in our treatment groups. Our information plus cash treatment group, for example, saw increases of nearly 1 standard deviation in maternal knowledge relative to the control group; knowledge in the information only group also significantly increased relative to the control, but by significantly less than the information plus cash group (about half the size).

Given improvements in knowledge, we then look to see if women are changing behaviors and incorporating the new knowledge into their daily lives. Households in the information and cash treatment group consumed approximately 100 more calories per person per day. They also improved various maternal behaviors such as breast feeding, vitamin A take up, prenatal check ups, etc. The effects on

behavioral changes observed in the information only group were not significantly different even though their knowledge gains were half as large. This is surprising since one might have expected that cash would be more important in affecting behavior, such as increased regular feeding for young children, than in building new knowledge.

Given that women did implement behavioral changes, we then study child outcomes to see if the improvements in behaviors are passed on to children. In the information plus cash group we find that child cognitive development, as measured by the Ages and Stages Questionnaire, improves by 0.1 SD. This is statistically significant and different from the essentially zero improvement in 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 between 25-36 months at baseline) in the information plus cash intervention group. Hence, the intervention, and especially the provision of cash as a short term safety net, seems to have resulted in a marked improvement in maternal knowledge about best practices, maternal behaviors, child development, and nutrition.

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 (see Bhutta et al. (2013) for a review on interventions related to maternal undernutrition and Lagarde, Haines, and Palmer (2007) and Fiszbein, Schady, and Ferreira (2009) for a review on conditional cash transfers and take up of health interventions). 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 (see Almond and Currie (2011) for a review). 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. Our study is also notable for its focus on maternal knowledge improvements as a result of the information campaigns. 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 post conflict setting such as Nepal is an important policy goal and this paper provides crucial evidence towards this.

Our paper is most closely related to the recent work by Macours, Schady, and Vakis (2012), who study the impacts of cash transfers to households in Nicaragua under the *Atencion 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. This suggests that transfers can lead to long lasting changes in behavior. 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 *Atencion 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 child care; 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.

2 Experimental Design

2.1 Randomization

The intervention was implemented in four food insecure districts in Nepal (4 of 75 districts nationwide), through a Community Challenge Fund (CCF) administered by the Ministry of Federal Affairs and Local Development.[1] 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), that bring 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 agricultural based areas to more mountainous regions.[2]

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

[2] 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.

The intervention consists of two treatment groups – one which receives information only, and one which receives information plus cash – and a control group. We implement a stratified randomized cluster design; within each district, we randomly assign each Village Development Committee[3] (VDC) to one of the three treatments. Henceforth, we will refer to VDC's as counties and COs as villages for ease of interpretation.[4] Within a county, we randomly select up to four villages (out of about 30 villages within a county) to be in our sample. Every village in a county receives 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 participate in the intervention. Figure 1 shows the administrative levels and where randomization occurred.

The county is 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 be able to attend or even know of the information treatment occurring in a village of a neighboring county. Second, Social Mobilizers (SMs), who led the information sessions, are responsible for all villages within a county, including non-experimental 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.[5]

Due to differential attrition in the first endline survey wave, discussed in greater detail in section 3, our primary analysis will only focus on the 139 counties that were part of the second endline survey wave, 45 of which are control counties, 48 of which are information only counties, and 46 of which are information plus cash counties. Table 1 shows the baseline means for all families, with the number in

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

[4]VDC's 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.

[5]Indeed, in discussions with local SM's, 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 4 that took place in the evaluation. It additionally shows the importance of randomizing at the county level to minimize spillovers.

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.[6] 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 breastmilk within three days of birth, and there are exceptionally high levels of malnutrition as evidenced by high rates of underweight, stunting, and wasting.

2.2 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 pre-existing 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

[6] Appendix table A1 shows the analogous baseline balance table, but for the remaining 45 counties that were interviewed in the first endline survey wave.

focus on nutrition for mothers in utero and best practices during pregancy, 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.

The cash transfer was set at NPR 700 per month (approximately $7 USD), which represents about 8-20% of median monthly income in our four districts. This figure is approximately in line with standard CCT's (Fiszbein, Schady, and Ferreira, 2009). The transfer was distributed 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" as 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 conditional cash transfer programs, and that the transfer was only distributed for five 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.

3 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 two years old. Baseline data collection occurred in August-October 2013, prior to the start of the intervention. 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 Ozler, 2016), we conducted a midline and endline survey. One-fourth of counties were randomly assigned to be surveyed at midline in August-September 2014, with the remaining three-fourths surveyed at endline in November-December 2014.

A total of 4,228 women and 3,695 children under two 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 sample consists of 2,338 women and 1,953 children.

The baseline, midline, 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 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 cognitive development as measured by the Ages and Stages Questionnaire (ASQ).

The ASQ is a screening mechanism that asks a mother if 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 1-60 months old.[7] 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 total population has a mean of 0 and a standard deviation of 1 on each module.

In the first survey wave, there was statistically significant differential attrition across treatment groups, with only 8% of women interviewed at baseline in the info plus cash group not found at endline, and 15% of the control group not found at endline. These data were collected during the final two months of the intervention to capture the contemporaneous effects of the cash transfer on household expenditures, given that these effects might quickly fade after the cash transfer stopped. However, 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. Ap-

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

pendix Table A2 shows that these individuals who remain have significantly lower monthly expenditures at baseline.

The primary analysis therefore focuses on the endline survey, conducted entirely after the intervention ended in November-December 2014. Table 2 shows there was no statistically significant difference in attrition, nor any selective attrition by particular characteristics. The raw levels of attrition, similar to the control group from the midline survey, indicate that the difference observed in the first wave 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. The 2,855 households interviewed at baseline that were interviewed again at endline will make up our main sample.

At endline, we also interviewed new households with a newly pregnant woman or infant born since baseline. This was done primarily to measure the spillovers associated with the information intervention, given that these women would not have been eligible for the cash intervention,[8] but may have elected to participate in the information sessions.

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 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 in general targets especially poor areas with little economic development, and this likely explains 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. Only about half of women indicated that a newborn infant should be fed breast milk exclusively for exactly six months, and about half answered that a pregnant woman should eat more food compared to 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

[8]Newly eligible women were not eligible for the cash transfer given that this may have led to the perverse incentive to increase fertility.

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.

4 Empirical Strategy

Because we randomly assign counties to each of the two treatment arms and 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:

$$y_i = \alpha + \beta_1 \text{INFO}_i + \beta_2 \text{CASH}_i + \gamma \mathbf{X}_i + \varepsilon_i$$

y_i refers to an outcome for person i, where the individual 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.

We should not need to include any controls in $\mathbf{X}_i$ because the groups are comparable at baseline. However, in order to improve precision of our estimates, we include basic control variables that depend on if the outcome variable is for the household, mother, or child, such as household composition, age, mother's schooling, and child's baseline height and weight. 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.

In estimating the effects on a child's cognitive development, we add an enumerator fixed effect. The answer to these screening questions reflects the mother's subjective assessment of if 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 woman. Therefore, we use an enu-

merator fixed effect so that any effects we find are entirely identified by variation in treatment status for each individual enumerator.[9]

We also use item response theory to estimate an "ability" parameter for each of the five cognitive screening modules as well as an overall ability parameter for cognitive 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 indices 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 in Anderson (2008) and used in Attanasio, Oppedisano, and Vera-Hernández (2015).[10] First, we pick variables that are closely related. 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 then normalize each of the variables to have a mean of zero and a standard deviation of one, redefine all variables to have a positive interpretation,[11] and then take a weighted average of the normalized outcomes. 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.

[9]Given potential measurement issues with small infants, we also tested if including a similar enumerator fixed effect changed our anthropometric results. However, it did not. We only report the results on anthropometrics not including the enumerator fixed effect.

[10]We also construct a knowledge index using the same ten 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 upon request.

[11]For example, one question we ask is "In the first three days after delivery, was your child given anything to drink other than breastmilk?". We change this outcome to be counted as if a child is fed only breastmilk within 3 days of delivery.

5 Results

Table 3 presents results on the effectiveness of the information intervention. Column 5 shows an index of knowledge, which is the sum of the number of correct answers to 10 questions regarding maternal health and nutritional issues. The index is our preferred measure of knowledge because it deals with the fact that we study many outcomes, and without any type of correction we will be likely to find some statistically significant improvements 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. Women in both the information only and information plus cash group do significantly better at endline, suggesting that the information sessions led to improved knowledge. Women in the information plus cash intervention on average answer 1 additional question correctly, which is an improvement of 17% above the control group mean, and is also significantly higher than women in the information only group. Columns 1-3 in table 3 present three of the individual items which make up the index, and column 4 shows the number of beneficial food items that a mother lists when given an open ended question on what a child should eat. All 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 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 4 of table 4 shows that women in the information session are significantly more likely to have attended a village meeting in the last month than women in both the control group and the information only group. Note that this is only suggestive evidence as the previous month the question refers to takes place after the intervention.[12] Column 1 of table 4 shows that expenditures in the past month were not significantly higher in the information plus cash group, though in column 3 we

[12]Additionally, 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.

see that there is higher caloric intake in the past 24 hours among the information plus cash group. However, it is unclear if this is a reallocation of income to have increased expenditure on food and less expenditure 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 acts as an incentive to take up the information side of the intervention, which is important in interpreting the meaning of the results.

Table 5 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. Our preferred outcome is the behavioral index in column 8, which aggregates the first six outcomes in the table into one summary variable. There is a statistically significant increase in "good" behavioral practices for women in the information plus cash group, despite the sample size being much smaller. The women thus seem to act upon the increased knowledge and actually change behaviors. Children in treatment groups are also fed more diverse diets in the treatment groups as is shown in column 7 – among all children aged six months and older, children ate from more food groups[13], though the difference is only significant from the control group in the information only group. 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.

Given the increases in knowledge and changes in behavior, it is important to see if these improvements are passed down to children in better overall outcomes. Table 6 shows cognitive outcomes and table 7 shows anthropometric outcomes for children who were aged two years or younger at baseline. There is a statistically significant improvement in the cognitive development of children in the information plus cash group, relative both to the control group and the information only group, even after controlling for enumerator fixed effects. The increase of about 0.1 standard deviations in the cognitive index, shown in column 6, is in line with the gains found in Macours, Schady, and Vakis (2012). These gains are mostly driven by improvements in gross motor and fine motor skills (in columns 2 and 3), which are skills likely to be particularly hindered by malnutrition (Engle, Black, Behrman, De Mello, Gertler, Kapiriri, Martorell, Young, Group, et al., 2007). However, table 7 shows that there are no significant improvements in anthropometrics. This is somewhat surprising given the results of prior interventions, which

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

have tended to find the strongest gains to alleviating malnutrition in reduced levels of stunting (e.g., Fernald, Gertler, and Neufeld (2009); Rivera, Sotres-Alvarez, Habicht, Shamah, and Villalpando (2004); Maluccio and Flores (2005)).

Tables 8 and 9 show the same outcomes as tables 6 and 7, but only include new children born to women in households where the woman was interviewed at baseline. These are presumably the children most likely to be influenced by the changes in behavior shown in table 5, which only focused on the mothers with new children. There are no indications of any improvements in cognitive or anthropometric measures for these youngest children.

If complying with suggested best practices, such as exclusively breastfeeding a child for six months, one particular outcome that should improve among the youngest children is a reduction in rates of diarrhea. This would occur both because of increased nutrients from drinking breastmilk and from reduced likelihood of contamination from dirty water. For new children born since the baseline in both of the treatment groups, there is a substantial, albeit insiginficant reduction, in the likelihood of having diarrhea, as shown in column 8 of table 9. Though the reduction is only between 1 and 2 percentage points, this is a 15-25% lowering of the likelihood of having diarrhea over the 6.9 percent of children in the control group who had diarrhea in the past thirty days. This suggests that the treatments may have improved child outcomes, and perhaps some of the broader insignificant results are due to the short time window in which we observe the children. Since the intervention only took place over nine months, it may not have been enough time to sufficiently capture the gains, but by following up in the future we may be able to more accurately measure improvements in child outcomes if they have indeed improved.

This analysis has focused only on participants in the endline survey. The midline survey results are left to an appendix because of issues with attrition discussed in section 3. 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 A3 estimates the upper and lower bounds on the treatment effects for the indices reported in the main tables. Estimates are reported separately for the information only versus control group and the information plus cash versus control group. The bounds for the most part cannot rule out the findings from the endline, which implies that the two survey waves can be consistent with each other.[14]

[14]Specific tables that mimic the exact main results for the midline survey waves are available on request, but are left out to save space.

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. Table 10 shows the anthropometric outcomes of older siblings. These are the siblings of children in tables 6 and 7 who were aged 25-36 months at baseline, and are thus not technically eligible for the intervention. Despite having a very small sample, we see that siblings in information plus cash households have much better overall anthropometric outcomes, as seen by the anthropometric index in column 8.[15] These improvements are driven by a lower likelihood of being underweight, and a lower likelihood of being sick in the past 30 days.

Finally, table 11 considers heterogeneous treatment effects by a child's gender. The outcomes presented are the cognitive and anthropometric indices for the eligible child and the anthropometric index for older siblings, initially shown in the final columns of tables 6, 7, and 10, respectively. Cognitive improvements in the overall population are predominantly driven by boys. Boys cognitive scores on the ASQ increase of 0.13 standard deviations, which is significant at the 5% level, while girls scores only increase 0.04 standard deviations, which is insignificant. Anthropometric estimates for the eligible children remain imprecise, as in the main results. Anthropometric gains for the siblings of eligible children are also driven by male children, as shown in column 3. Taken together, these results suggest differential levels of investment in male and female children following the intervention. Nothing in the intervention implied such investments would be relatively more productive, so these results suggest a gender bias with a preference for greater investment in male children.

6 Mechanisms

The overall theory of change presented so far has demonstrated that the information sessions improved knowledge, particularly in the cash group; gains in knowl-

[15] There are some small differences at baseline between siblings in each of the three treatment groups. For example, the mothers of siblings in the control group are significantly more likely to have attended school than the mothers of the siblings in both treatment groups; this difference was insignificant though in the same direction for the full sample. However, these differences likely work against finding a positive difference in outcomes at endline since on average a mother having attended school is correlated with better child outcomes. The balance table for this subsample of older siblings is available on request. Additionally, we look at the importance of birth order of these older siblings, but the general balance across treatment types means that controlling for birth order makes little difference.

edge lead to changes in behavioral practices; and behavioral improvements in turn spur 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.[16] 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 if knowledge of the issue corresponds with actually implementing it.

Table 12 presents the results of running 7 such simple regressions, each taking a measurable action, and regressing if knowledge of the item was associated with having done it. 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 5, 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 may have especially been acted upon.

Column 3 indicates one of the strongest correlations between knowledge and behavior. Among women who know that the first milk should be given to the baby, there is a 27 percentage point increase in first breastfeeding the baby within one hour of birth. This is a 110% increase over the 24 percent of women who first breastfeed their baby within one hour despite not knowing 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.

The correct timing on the practices seems to be especially important. Column 5 shows that there is a 30 percentage point (or 177%) increase in taking iron tablets for the suggested amount of time if a woman knows the correct recommendation. If we rather assess the correlation between this knowledge and taking iron tablets at all, the increase 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

[16] A knowledge item that does not directly correspond to an action is if 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 of a question without using definitive numbers. This would more likely produce inaccurate responses so we did not ask such a question.

(or 18%) increase in the likelihood of ever taking iron tablets during pregnancy. This provides further suggestive evidence that knowledge directly leads to action.

Behavior changes should then lead to improvements in child outcomes; these practices are WHO suggestions precisely because they are scientifically shown to improve outcomes. However, our results on this front are somewhat mixed. We do see cognitive gains among children aged between 0 and 2 years old at baseline. There are no corresponding changes in anthropometrics, and no gains whatsoever among children newly born since baseline.

There are several potential explanations for this phenomenon. Time has been too short to accurately measure any differences. The information sessions lasted for nine months, and the cash transfer was only provided for five months. Additionally, by measuring children immediately following the completion of the intervention, there may not have been enough time for the behavioral changes to be fully reflected in the child's outcomes.

Another potential explanation is that the general level of poverty may be so high that even making behavioral changes might not be able to overcome the barrier associated with achieving proper development. For example, if households cannot access clean drinking water or mothers experience severe micronutrient deficiencies, the adverse effects of these prevailing conditions may be such that even improvements to the targeted practices might not do anything. As suggestive evidence, we find a clear correlation between having a toilet and access to good drinking water on general anthropometric outcomes. Analyzing the results by heterogeneity along these variables yields little difference in the impact of the treatments on child outcomes.[17]

7 Conclusion

In this paper, we evaluate the effects of providing information on best practices regarding nutrition and child care for children below the age of two, and the same information component with a conditional cash transfer on early childhood development outcomes using a randomized control 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

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

twice as large in the group that also received the cash transfer. Both groups experienced similar improvements in maternal health and early childhood best practices, indicating that the intervention was successful at not just increasing knowledge but also creating behavioral change. However, only children in the information plus cash group experienced improved childhood development, with cumulative child development as measured by the Ages and Stages Questionnaire improving by 0.1 standard deviations. Hence, the intervention, and especially the provision of cash seems to have resulted in a marked improvement in maternal knowledge about best practices, maternal behaviors, child development, and nutrition.

It is of note that we see significant improvements in child outcomes in the information plus cash group despite the temporary nature of the short term safety net provided by the cash transfer; this suggests that short-run interventions can have longer-term outcomes. The design of the intervention also builds on existing investments in training and community participation and should be another factor of interest for policy makers. This is especially important when policy makers consider the types of practical, cost-effective tools at their disposal for fighting malnutrition.

One puzzle presented by our results is why cognitive and anthropometric outcomes would differ for children in the information plus cash group relative to children in the information only group given that changes in behaviors were statistically indistinguishable. Future studies should more carefully identify the precise mechanism and type of behavioral change that affects child outcomes. For example, how a mother divides her time between work, caring, and playing with the child might be important. Another factor to consider is the physical and mental health of parents. Effects also likely differ by a child's age, but we do not have enough power to precisely estimate effect sizes in small age bins.

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. 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 thirty 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 this, and make future decisions about where to best focus resources based on this. We leave such a study to future work.

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FIGURE 1: Randomization and Sample Selection Protocol

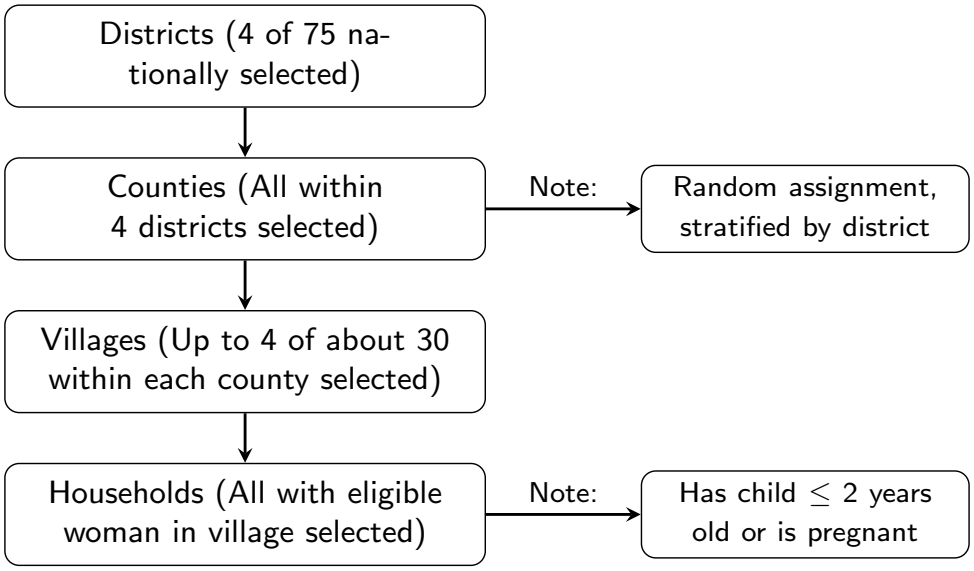

TABLE 1: Baseline Balance

	Control	Info Only	Info + Cash
Mother Age	25.68	26.07	26.05
		(0.42)	(0.48)
Mother Attended School	0.32	0.27	0.25
		(0.26)	(0.12)
Knowledge Index	5.16	5.23	4.89
		(0.81)	(0.29)
Fed non-breastmilk w-in 3 days	0.48	0.42	0.43
		(0.49)	(0.55)
Child Age	1.07	1.04	1.04
		(0.32)	(0.33)
Child Underweight	0.34	0.32	0.34
		(0.53)	(0.98)
Child Stunted	0.40	0.41	0.39
		(0.79)	(0.73)
Child Wasted	0.24	0.20	0.25
		(0.25)	(0.96)
Number Household Members	8.03	7.87	7.69
		(0.46)	(0.11)
Household Head Male	0.76	0.77	0.75
		(0.79)	(0.78)
Has Electricity	0.69	0.63	0.68
		(0.41)	(0.88)
Stone Roofing Material	0.66	0.66	0.66
		(0.90)	(0.90)
Annual Income	118377	108236	121841
		(0.26)	(0.70)
Monthly Expenditures	7226	7273	6474
		(0.96)	(0.33)

NOTE: Each row presents the mean within each treatment group. The values in parentheses in columns (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 VDC level. The sample sizes are 973 eligible women/859 eligible children in 903 eligible households in the control group; 1,032 eligible women/909 eligible children in 982 eligible households in the info only group; and 1,022 eligible women/900 eligible children in 970 eligible households in the info plus cash group.

TABLE 2: Attrition

	Control	Info Only	Info + Cash
Level of Attrition			
Women Attrition	0.168	0.172	0.155
		(0.83)	(0.50)
Child Attrition	0.163	0.176	0.151
		(0.53)	(0.55)
Characteristics of Non-Attriters			
Mother Age	25.86	26.50	26.35
		(0.26)	(0.40)
Mother Attended School	0.31	0.26	0.23
		(0.28)	(0.09)
Knowledge Index	5.14	5.24	4.87
		(0.70)	(0.31)
Fed non-breastmilk w-in 3 days	0.49	0.43	0.45
		(0.47)	(0.60)
Child Age	1.08	1.04	1.05
		(0.20)	(0.31)
Child Underweight	0.36	0.32	0.35
		(0.32)	(0.84)
Child Stunted	0.41	0.41	0.39
		(0.93)	(0.62)
Child Wasted	0.25	0.21	0.25
		(0.32)	(0.90)
Number Household Members	8.21	8.00	7.85
		(0.41)	(0.14)
Household Head Male	0.76	0.77	0.75
		(0.84)	(0.84)
Has Electricity	0.70	0.63	0.69
		(0.39)	(0.95)
Stone Roofing Material	0.67	0.66	0.69
		(0.98)	(0.73)
Annual Income	118398	105922	121175
		(0.19)	(0.77)
Monthly Expenditures	7245	7091	6553
		(0.87)	(0.38)

NOTE: The top two rows present the share of women and children that were interviewed at baseline not found at endline. In the second half of the table, each row presents the mean within each treatment group for those who did not attrit. The values in parentheses in columns (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 VDC level.

TABLE 3: Woman Knowledge

	(1) Breastfeed For Excl. 6 Months	(2) Eat More During Pregnancy	(3) Breastfeed More Diarrhea	(4) Number of Food Items Mentioned for Kids	(5) Knowledge Index
Info Only	0.044	0.066	0.152***	0.350*	0.526**
	(0.043)	(0.050)	(0.050)	(0.180)	(0.207)
Info + Cash	0.131***	0.109**	0.089*	0.459***	0.924***
	(0.040)	(0.050)	(0.048)	(0.174)	(0.216)
Difference	0.087**	0.043	-0.063	0.109	0.398*
p-Value	0.042	0.407	0.253	0.538	0.055
Control Mean	0.571	0.608	0.437	4.159	5.330
Observations	2,338	2,338	2,338	2,338	2,338
R^2	0.028	0.021	0.042	0.038	0.085

NOTE: The dependent variable in columns (1)-(3) is an indicator variable signifying if 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 breastmilk?", with options of less than, more than, or exactly equal to 6 months. The dependent variable in column (1) is the share of respondents answering exactly 6 months. The knowledge index in column (5) is the number of questions answered correctly out of 10 general knowledge questions, including those in columns (1)-(3). Basic controls includes 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).

TABLE 4: Household Characteristics

	(1) Monthly Expenditures	(2) Annual Income	(3) Calories Per Person	(4) Attend CO Meeting	(5) Has/Expects New Child
Info Only	-0.032	-0.058	50.725	0.024	-0.046
	(0.095)	(0.067)	(53.415)	(0.049)	(0.030)
Info + Cash	-0.045	0.044	98.796*	0.111**	-0.015
	(0.107)	(0.069)	(53.092)	(0.049)	(0.036)
Difference	-0.014	0.102	48.071	0.087*	0.031
p-Value	0.896	0.158	0.353	0.068	0.312
Control Mean	8.407	11.434	2208.527	0.358	0.441
Observations	2,249	2,249	2,249	2,338	2,338
R^2	0.102	0.092	0.025	0.108	0.062

NOTE: Income and expenditures are in logs. Basic controls includes the number of household members for columns (1)-(3) and dummies for age of the female respondent for columns (4)-(5), household level controls (age and gender of household head, number of household members) in all columns, as well as asset indicators for electricity, a stone roof, and a separate kitchen for columns (1)-(3), and the female respondent's weight, height, and if she ever attended school in columns (4)-(5).

TABLE 5: Health Practices with New Child

	(1) Number of Antenatal Visits	(2) Vitamin A Within 42 Days	(3) Fed Non-Breastmilk Within 3 Days	(4) Iron Tablets For 5-6 Months	(5) First Breastfed Within 1 Hour	(6) Deworming Pills	(7) Food Groups	(8) Behavior Index
Info Only	-0.136	0.085	-0.136**	0.032	0.009	0.027	0.239*	0.038
	(0.249)	(0.064)	(0.059)	(0.052)	(0.069)	(0.055)	(0.133)	(0.028)
Info + Cash	0.441*	0.184***	-0.133**	0.133**	0.071	0.065	0.136	0.079***
	(0.238)	(0.063)	(0.054)	(0.055)	(0.068)	(0.052)	(0.121)	(0.024)
Difference	0.577**	0.099	0.003	0.100*	0.062	0.038	-0.103	0.041
p-Value	0.039	0.165	0.950	0.068	0.360	0.510	0.341	0.166
Control Mean	3.102	0.458	0.343	0.292	0.462	0.725	2.399	0.520
Observations	678	678	678	678	678	678	1,156	678
R^2	0.057	0.043	0.044	0.036	0.021	0.030	0.044	0.051

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. Food groups 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. No controls includes only dummies age cohorts of the female respondent. The behavior index is created by taking a weighted sum of the first six demeaned variables by using the method described in Anderson (2008). Basic controls includes dummies for age cohorts of the female respondent, as well as individual level controls (if the woman ever went to school, baseline weight and height) and household level controls (age and gender of household head, and number of household members).

TABLE 6: Cognitive Scores

	(1) Communication	(2) Gross Motor	(3) Fine Motor	(4) Personal Social	(5) Problem Solving	(6) Cognitive Index
Info Only	0.046	-0.025	0.049	-0.082	-0.056	-0.006
	(0.054)	(0.060)	(0.061)	(0.056)	(0.050)	(0.042)
Info + Cash	0.051	0.120*	0.134**	0.080	-0.031	0.086*
	(0.054)	(0.063)	(0.062)	(0.068)	(0.052)	(0.044)
Difference	0.005	0.145**	0.086	0.162**	0.024	0.091**
p-Value	0.926	0.033	0.183	0.012	0.664	0.040
Control Mean	-0.044	-0.052	-0.066	-0.041	0.019	-0.047
Observations	1,646	1,646	1,646	1,646	1,646	1,646
R^2	0.265	0.302	0.362	0.326	0.454	0.415

NOTE: All outcomes are expressed in standardized Z-scores for each 6-question module. The cognitive index is created by taking a weighted sum of the five demeaned scores by using the method described in Anderson (2008). Basic controls includes dummies for gender and age cohorts taking each separate ASQ module, as well as individual level controls (if mother ever went to school and baseline weight-for-age and height-for-age Z scores) 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.

TABLE 7: Anthropometric Measures

	(1) Underweight	(2) Stunted	(3) Wasted	(4) Weight-for-Age	(5) Height-for-Age	(6) Weight-for-Length	(7) Sick Past 30 Days	(8) Anthropometric Index
Info Only	0.030	0.002	-0.011	-0.018	-0.019	-0.019	0.003	0.003
	(0.026)	(0.031)	(0.021)	(0.069)	(0.098)	(0.100)	(0.037)	(0.033)
Info + Cash	-0.010	0.016	-0.019	0.025	-0.176	0.171*	-0.041	0.034
	(0.026)	(0.032)	(0.021)	(0.063)	(0.107)	(0.101)	(0.032)	(0.032)
Difference	-0.040*	0.014	-0.008	0.044	-0.157	0.189*	-0.044	0.031
p-Value	0.089	0.660	0.729	0.513	0.151	0.070	0.244	0.356
Control Mean	0.324	0.557	0.133	-1.570	-2.167	-0.558	0.397	0.004
Observations	1,953	1,953	1,953	1,953	1,953	1,953	1,953	1,953
R^2	0.151	0.132	0.043	0.250	0.178	0.067	0.024	0.127

NOTE: The anthropometric index creates an index of the four health indicator variables for if a child is underweight, stunted, wasted, or has been sick in the past 30 days by using the method described in Anderson (2008). Basic controls includes dummies for gender and age cohorts taking each separate ASQ module, as well as individual level controls (if mother ever went to school and baseline weight-for-age and height-for-age Z scores) and household level controls (age and gender of household head, and number of household members).

TABLE 8: Cognitive Scores (New Children In Baseline HH's)

	(1) Communication	(2) Gross Motor	(3) Fine Motor	(4) Personal Social	(5) Problem Solving	(6) Cognitive Index
Info Only	-0.251***	-0.129*	-0.108	-0.144*	0.010	-0.132**
	(0.077)	(0.077)	(0.071)	(0.078)	(0.083)	(0.058)
Info + Cash	-0.067	-0.105	-0.114*	-0.106	0.055	-0.063
	(0.072)	(0.075)	(0.065)	(0.069)	(0.077)	(0.052)
Difference	0.185**	0.024	-0.006	0.038	0.045	0.069
p-Value	0.019	0.770	0.923	0.643	0.556	0.218
Control Mean	-0.035	-0.049	-0.022	0.031	-0.129	-0.045
Observations	782	782	782	782	782	782
R^2	0.369	0.277	0.412	0.405	0.422	0.512

NOTE: All outcomes are expressed in standardized Z-scores for each 6-question module. The cognitive index is created by taking a weighted sum of the five demeaned scores by using the method described in Anderson (2008). Includes only new children born since the baseline in households eligible for treatment. Basic controls includes dummies for gender and age cohorts taking each separate ASQ module, as well as individual level controls (if mother ever went to school and baseline weight-for-age and height-for-age Z scores) 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.

TABLE 9: Anthropometric Measures (New Children In Baseline HH's)

	(1) Underweight	(2) Stunted	(3) Wasted	(4) Weight-for-Age	(5) Height-for-Age	(6) Weight-for-Length	(7) Sick Past 30 Days	(8) Diarrhea Past 30 Days	(9) Anthropometric Index
Info Only	-0.002	0.029	-0.067**	-0.051	-0.241	0.073	-0.076	-0.010	0.083*
	(0.042)	(0.046)	(0.033)	(0.160)	(0.190)	(0.217)	(0.052)	(0.024)	(0.050)
Info + Cash	-0.008	0.056	-0.033	-0.052	-0.178	0.034	0.006	-0.018	-0.009
	(0.041)	(0.042)	(0.038)	(0.149)	(0.193)	(0.220)	(0.047)	(0.020)	(0.050)
Difference	-0.006	0.027	0.033	-0.001	0.063	-0.038	0.081	-0.008	-0.092*
p-Value	0.885	0.511	0.236	0.994	0.713	0.856	0.105	0.717	0.068
Control Mean	0.255	0.398	0.185	-0.981	-1.448	-0.003	0.529	0.069	-0.018
Observations	782	782	782	782	782	782	782	782	782
R^2	0.056	0.049	0.034	0.080	0.059	0.053	0.029	0.022	0.060

NOTE: The anthropometric index creates an index of the four health indicator variables for if a child is underweight, stunted, wasted, or has been sick in the past 30 days by using the method described in Anderson (2008).Includes only new children born since the baseline in households eligible for treatment. Basic controls includes dummies for gender and age cohorts taking each separate ASQ module, as well as individual level controls (if mother ever went to school and baseline weight-for-age and height-for-age Z scores) and household level controls (age and gender of household head, and number of household members).

TABLE 10: Anthropometric Measures (Siblings of Eligible Children)

	(1) Underweight	(2) Stunted	(3) Wasted	(4) Weight-for-Age	(5) Height-for-Age	(6) Weight-for-Length	(7) Sick Past 30 Days	(8) Anthropometric Index
Info Only	-0.154**	-0.054	-0.044	0.191	0.024	0.256	0.205***	-0.024
	(0.070)	(0.063)	(0.042)	(0.122)	(0.157)	(0.181)	(0.071)	(0.087)
Info + Cash	-0.149**	-0.029	-0.037	0.302*	0.060	0.395*	-0.085*	0.158*
	(0.071)	(0.068)	(0.047)	(0.158)	(0.161)	(0.227)	(0.049)	(0.081)
Difference	0.006	0.024	0.007	0.111	0.036	0.139	-0.290***	0.182**
p-Value	0.835	0.759	0.818	0.441	0.625	0.600	0.000	0.024
Control Mean	0.346	0.577	0.090	-1.551	-2.261	-0.304	0.192	0.002
Observations	245	245	245	245	245	245	245	245
R^2	0.246	0.253	0.069	0.356	0.292	0.159	0.129	0.196

NOTE: Sample includes siblings of children in eligible households (those included in tables 6 and 7) who were aged between 25 and 36 months at baseline. The anthropometric index creates an index of the four health indicator variables for if a child is underweight, stunted, wasted, or has been sick in the past 30 days by using the method described in Anderson (2008). Basic controls includes dummies for gender and age cohorts taking each separate ASQ module, as well as individual level controls (if mother ever went to school and baseline weight-for-age and height-for-age Z scores) and household level controls (age and gender of household head, and number of household members).

TABLE 11: Child Outcome Indices, by Gender

	(1) Cognitive Index (Eligible Child)	(2) Anthropometric Index (Eligible Child)	(3) Anthropometric Index (Older Siblings)
	Child Measured is Male		
Info Only	-0.017	-0.009	-0.060
	(0.054)	(0.038)	(0.127)
Info + Cash	0.128**	0.018	0.188*
	(0.064)	(0.084)	(0.106)
Difference	0.145***	0.026	0.248*
p-Value	0.009	0.537	0.057
Control Mean	-0.065	-0.009	0.030
Observations	866	1,029	112
	Child Measured is Female		
Info Only	-0.015	0.001	-0.006
	(0.060)	(0.047)	(0.120)
Info + Cash	0.041	0.048	0.127
	(0.053)	(0.049)	(0.113)
Difference	0.056	0.047	0.133
p-Value	0.366	0.289	0.195
Control Mean	-0.022	0.019	-0.030
Observations	780	924	128

NOTE: All outcomes are indices, created by taking a weighted sum of raw item scores by using the method described in Anderson (2008). Basic controls includes dummies for gender and age cohorts taking each separate ASQ module, as well as individual level controls (if mother ever went to school and baseline weight-for-age and height-for-age Z scores) and household level controls (age and gender of household head, and number of household members). Column (1) also includes a fixed effect for which enumerator conducted the endline survey.

TABLE 12: Connection Between Knowledge and Practice

	(1) Antenatal Visits	(2) Exclusive Breastfeed Six Months	(3) First Breastfed w/in 1 hour	(4) Only Breastmilk w/in 3 days	(5) Took Iron Pills For 5-6 months	(6) Took De-Worming Pills	(7) Vitamin A W/in 45 days
Knowledge	0.656***	0.094**	0.271***	0.230***	0.303***	0.228***	0.411***
	(0.163)	(0.039)	(0.064)	(0.056)	(0.035)	(0.034)	(0.035)
Constant	2.911***	0.531***	0.246***	0.538***	0.171***	0.600***	0.332***
	(0.110)	(0.031)	(0.061)	(0.053)	(0.027)	(0.028)	(0.025)
Observations	678	678	678	678	678	678	678
R^2	0.023	0.009	0.025	0.024	0.099	0.061	0.170

NOTE: Runs a simple OLS regression between if a woman correctly answers a question in the knowledge portion of the survey and her reported practice with her youngest newborn child. For example, column (1) regresses if a woman knows the appropriate number of antenatal care visits is 4 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 5.

TABLE A1: Baseline Balance – Midline

	Control	Info Only	Info + Cash
Mother Age	26.01	25.28	25.94
		(0.39)	(0.95)
Mother Attended School	0.26	0.25	0.30
		(0.90)	(0.68)
Knowledge Index	5.23	5.27	4.75
		(0.92)	(0.39)
Fed non-breastmilk w-in 3 days	0.39	0.54	0.40
		(0.26)	(0.94)
Child Age	1.04	1.02	1.02
		(0.68)	(0.72)
Child Underweight	0.32	0.38	0.31
		(0.36)	(0.84)
Child Stunted	0.40	0.43	0.35
		(0.60)	(0.43)
Child Wasted	0.23	0.23	0.28
		(0.92)	(0.41)
Number Household Members	7.42	7.84	7.69
		(0.21)	(0.45)
Household Head Male	0.80	0.71	0.83
		(0.20)	(0.50)
Has Electricity	0.49	0.65	0.65
		(0.26)	(0.29)
Stone Roofing Material	0.61	0.65	0.67
		(0.61)	(0.43)
Annual Income	105969	107273	106943
		(0.92)	(0.94)
Monthly Expenditures	7090	7081	5163
		(0.99)	(0.16)

NOTE: Each row presents the mean within each treatment group. The values in parentheses in columns (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 VDC level. The sample sizes are 368 eligible women/324 eligible children in 350 eligible households in the control group; 346 eligible women/279 eligible children in 324 eligible households in the info only group; and 320 eligible women/274 eligible children in 302 eligible households in the info plus cash group.

TABLE A2: Attrition – Midline

	Control	Info Only	Info + Cash
Level of Attrition			
Women Attrition	0.152	0.139	0.081
		(0.52)	(0.00)
Child Attrition	0.160	0.129	0.099
		(0.24)	(0.03)
Characteristics of Non-Attriters			
Mother Age	26.13	25.32	26.14
		(0.35)	(0.99)
Mother Attended School	0.25	0.26	0.30
		(0.91)	(0.58)
Knowledge Index	5.23	5.28	4.78
		(0.90)	(0.41)
Fed non-breastmilk w-in 3 days	0.38	0.53	0.41
		(0.22)	(0.81)
Child Age	1.03	1.01	1.03
		(0.74)	(0.99)
Child Underweight	0.31	0.38	0.31
		(0.20)	(0.94)
Child Stunted	0.40	0.42	0.36
		(0.69)	(0.48)
Child Wasted	0.21	0.24	0.28
		(0.56)	(0.26)
Number Household Members	7.48	8.18	7.91
		(0.07)	(0.32)
Household Head Male	0.80	0.71	0.83
		(0.19)	(0.60)
Has Electricity	0.51	0.64	0.68
		(0.34)	(0.24)
Stone Roofing Material	0.60	0.68	0.66
		(0.36)	(0.43)
Annual Income	103959	113241	108026
		(0.52)	(0.77)
Monthly Expenditures	7018	6972	4897
		(0.97)	(0.08)

NOTE: The top two rows present the share of women and children that were interviewed at baseline not found at endline. In the second half of the table, each row presents the mean within each treatment group for those who did not attrit. The values in parentheses in columns (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 VDC level.

TABLE A3: Midline Outcomes, Lee Bounds

	(1) Knowledge Index	(2) Behavior Index	(3) Cognitive Index	(4) Anthropometric Index
		Information Only		
Lower Bound	-0.129	-0.011	0.004	-0.065
	(0.205)	(0.042)	(0.078)	(0.063)
Upper Bound	0.048	0.046	0.174**	-0.039
	(0.220)	(0.042)	(0.087)	(0.065)
(Endline Estimate)	0.526**	0.038	-0.006	0.003
Observations	714	201	603	603
		Information Plus Cash		
Lower Bound	0.096	0.100***	-0.102	-0.068
	(0.178)	(0.037)	(0.069)	(0.065)
Upper Bound	0.694***	0.153***	0.104	0.033
	(0.183)	(0.038)	(0.071)	(0.067)
(Endline Estimate)	0.924***	0.079***	0.086*	0.034
Observations	688	187	598	598

NOTE: All outcomes are indices, created by taking a weighted sum of raw item scores by using the method described in Anderson (2008). The upper and lower bounds from estimating Lee bounds are reported, using data from the midline survey and correcting for attrition. No control variables are used in estimating Lee bounds. The endline estimate can be found in the corresponding table from the main text.