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Nutrition, information and household behavior: Experimental evidence from Malawi

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ABSTRACT

Incorrect knowledge of the health production function may lead to inefficient household choices and thereby to the production of suboptimal levels of health. This paper studies the effects of a randomized intervention in rural Malawi that, over a six-month period, provided mothers of young infants with information on child nutrition without supplying any monetary or in-kind resources. A simple model first investigates theoretically how nutrition and other household choices including labor supply may change in response to the improved nutrition knowledge observed in the intervention areas. We then show empirically that the intervention improved child nutrition, household food consumption and consequently health. We find evidence that labor supply increased, which might have contributed to partially fund the increase in food consumption. This paper is the first to study whether non-health choices, particularly parental labor supply, might be affected by parents' knowledge of the child health production function.

1. Introduction

Since Becker's (1965) seminal contribution, economists have recognized that many goods are not directly bought in the market, but are produced at home using a combination of market and non-market goods. The home production framework has been particularly fruitful in studying the production of health, in particular child health (Gronau, 1986, 1997; Grossman, 1972; Rosenzweig and Schultz, 1983). An important implication of such models is that households make choices given their knowledge of the (child) health production function. Consequently, deficiencies in knowledge lead to suboptimal household choices and thereby distorted levels of child health. Establishing empirically the consequences of deficiencies in knowledge on household behavior has, however, been challenging because knowledge is endogenous and is usually either unobserved or proxied by education, which also affects child health through other channels including earnings.

In this paper, we overcome this challenge by exploiting an intervention, implemented through a cluster randomized trial, aiming to improve mothers' knowledge of the child health production function in rural Malawi. The intervention solely provided information on child nutrition to mothers, thus yielding a clean source of identification. Our contribution is twofold. First, we assess whether the intervention improved child nutrition and consequently health. Second, drawing on a simple theoretical model, we investigate how other household choices change to accommodate the improved knowledge of the production function. In so doing, we assess whether non-health choices, particularly parental labor supply, might be affected by parents' knowledge of the child health production function.

In the context we study, rural Malawi, mothers have many misconceptions about child nutrition. To take some examples, it is common practice to give porridge diluted with unsterilized water to infants as young as one week; the high nutritional value of groundnuts, widely available in the area, is not well known; and widespread misplaced beliefs include that eggs are harmful for infants as old as 9 months and that the broth of a soup contains more nutrients than the meat or vegetables therein. This evidence suggests that important changes can be expected if these misconceptions are corrected.

The intervention we study delivered information in an intense manner: trained local women visited mothers in their homes once before the birth of their child and four times afterwards and provided information on early child nutrition on a one-to-one basis. Moreover, the fact that the intervention had been running for at least three years when outcome data were collected allows a sufficient time frame for practices to change. This lapse also allows us to measure medium-term impacts, which is important since interventions often perform much better in the short rather than medium term (Banerjee et al., 2008; Hanna et al., 2016).

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Consistent with gains in knowledge, we find evidence of improvements in infants' diets and household food consumption, particularly an increase of protein-rich foods and of fruit and vegetables. We also find that household food consumption increases and there is suggestive evidence that this might have been partially financed through increased labor supply. Overall, the findings are consistent with households learning that some relatively costly foods are more nutritious than they previously believed and adjusting their labor supply so as to facilitate increases in their children's intake of them. Indeed, we show that households adjust their behavior on several margins including child diet inputs and labor supply, making their response more complex than simply changing the composition of consumption while keeping total consumption constant.

We find that the intervention improved children's physical growth, particularly height, a widely used indicator of long-term nutritional status. This finding is particularly important for policy: child malnutrition is a severe and prevalent problem in developing countries (de Onis et al., 2000) and leads to poor health and excess child mortality (Bhutta et al., 2008; Pelletier et al., 1994) and is also linked to poor human capital outcomes later on in life.[1]

The paper deals carefully with the increasingly important issue of inference in cluster randomized trials when the number of clusters is small. It is well known that in this situation, standard statistical formulae for clustered standard errors based on asymptotic theory (cluster-correlated Huber–White estimator) provide downward-biased standard error estimates (Bertrand et al., 2004; Cameron et al., 2008; Donald and Lang, 2007; Wooldridge, 2004). We use two leading methods for inference in this case – randomization inference (Fisher, 1935; Rosenbaum, 2002) and wild-cluster bootstrap-t (Cameron et al., 2008). Furthermore, we assess their performance in our data using Monte Carlo experiments and find that both methods perform relatively well. Presenting the performance of these two methods side-by-side is of interest for many empirical applications, given the increasing trend in randomized trials with a small number of clusters.

Lewycka et al. (2013) study the effect of the intervention we study on exclusive breastfeeding and infant mortality. Our paper addresses a different question: whether improving knowledge of the health production function affected consumption, labor supply, nutritional practices and child nutrition to the age of around 5 years. We also use a different dataset; they interview mothers until their child is 6 months old, while we rely on a representative sample of women of reproductive age and their households. More details about the design of the intervention can be found in Lewycka et al. (2010).

Our work contributes to a number of strands of literature. First, it adds to the discussion on the effects of health information on behavior (Dupas, 2011a).[2] The evidence is mixed: Dupas (2011b); Jalan and Somanathan (2008) and Madajewicz et al. (2007) find that providing information on, respectively, the risks of contracting HIV and the arsenic and fecal concentration of water improves associated practices, while Kamali et al. (2003); Kremer and Miguel (2007) and Luo et al. (2012) find that health behaviors relating to, respectively, HIV, deworming and anemia do not respond to health education. This paper departs from these studies by not only considering a multifaceted information intervention, but also by studying household responses on a wider range of margins than those directly targeted by the intervention. In doing so, this is one of the first papers to investigate how behaviors not directly related to the topic of an information campaign adjust to it.

Second, this paper contributes to the literature evaluating the effects of nutrition information interventions on nutrition practices and child health. Haider et al. (2000) and Morrow et al. (1999) find increased rates of exclusive breastfeeding within small-scale randomized control trials in Bangladesh and Mexico respectively, while Alderman (2007); Galasso and Umapathi (2009) and Linnemayr and Alderman (2011) find improvements in child weight-for-age, an indicator for medium-term health status, using non-experimental methods. Our paper builds on these by studying the effects on a range of measures of child health, health practices and other margins of household behavior, all identified through a randomized control trial.

Finally, it relates to the literature investigating the causal effects of parental education on child health. In developed countries, Currie and Moretti (2003) and McCrary and Royer (2011) find, respectively, decreased incidence of low birth weight and modest effects on child health of increased maternal schooling in the US, while Lindeboom et al. (2009) find little evidence that parental schooling improves child health in the UK. For developing countries, Breierova and Duflo (2004) and Chou et al. (2010) find that parental schooling decreases infant mortality in Indonesia and Taiwan respectively. However, it is difficult to disentangle whether the effect of education is working through changes in knowledge of the child health production function, or through increased income and hence access to more and better-quality care. Related to this, Glewwe (1999) and Thomas et al. (1991) find that almost all of the impact of maternal education on child's height in Morocco and Brazil can be explained by indicators of access to information and health knowledge.

The rest of the paper is structured as follows. Section 2 provides background information on rural Malawi and describes the experimental design and data, Section 3 describes the theoretical framework and Section 4 sets out the empirical model. Our main results are presented in Section 5. Section 6 rules out alternative potential explanations behind our findings, while Section 7 concludes.

2. Background and intervention

2.1. Background

Malnutrition in the early years (0–5) is one of the major public health and development challenges facing Malawi, one of the poorest countries in sub-Saharan Africa. The 2004 Malawi Demographic and Health Survey (DHS) Report indicates an under-5 mortality rate of 133 per 1000, and under-nutrition is an important factor driving this: Pelletier et al. (1994) estimate that 34% of all deaths before age 5 in Malawi are related to malnutrition (moderate or severe). Moreover, 48% of Malawian children aged under 5 suffer from chronic malnutrition, a rate that is the second highest in sub-Saharan Africa.

Poor feeding practices are at least partly responsible for these extreme malnutrition indicators. Over half of all infants aged under 6 months are given food and/or unsterilized water (2004 DHS Report), which can lead to gastrointestinal infections and growth faltering (Haider et al., 2000; Kalanda et al., 2006) and is contrary to the World Health Organization (WHO) recommendation of exclusive breastfeeding for the first 6 months of an infant's life. Furthermore, porridge diluted with unsterilized water is often given in large quantities to infants as young as 1 week (Bezner-Kerr et al., 2007). In terms of nutrition for infants aged over 6 months, their diets – rich in staples such as maize flour – frequently lack the necessary diversity of foods to provide sufficient amounts of energy, proteins, iron, calcium, zinc, vitamins and folate: in our sample, 25% of children aged 6–60 months did not consume any proteins over the three days prior to the survey, with a further 30% consuming just one source of protein. Poor nutritional practices are likely to be related to a lack of knowledge: for instance, only 15% of mothers in our sample knew how to best cook fish combined with the local staple so as to maximize nutritional value.

It is against this background that, in 2002, a research and development project called MaiMwana (Chichewa for 'Mother and Child') was

[1] See, among others, Alderman et al. (2001); Almond and Currie (2011); Banerjee et al. (2010); Barham (2012); Behrman (1996); Behrman and Rosenzweig (2004); van den Berg et al. (2006, 2009, 2010), Bhalotra et al. (2015); Currie (2009); Currie et al. (2010); Glewwe et al. (2001); Hoddinott et al. (2008); Lindeboom et al. (2010); Maccini and Yang (2009); Maluccio et al. (2009); Schultz (2005) and Strauss and Thomas (1998).

[2] For the case of education, see for instance Jensen (2010).

set up in Mchinji District, in the Central region of Malawi.[3] Its aim was to design, implement and evaluate effective, sustainable and scalable interventions to improve the health of mothers and infants. Mchinji is a primarily rural district, with subsistence agriculture being the main economic activity. The most commonly cultivated crops are maize, groundnuts and tobacco. The dominant ethnic group in the district is the Chewa (over 90% in our data). According to the 2008 Malawi census, socio-economic conditions are comparable to or poorer than the average for Malawi (in parentheses in what follows), with literacy rates of just over 60% (64%), piped water access for 10% (20%) of households and electricity access for just 2% (7%) of households.

2.2. The intervention

In 2005, MaiMwana established an infant feeding counseling intervention in Mchinji District (ongoing at time of follow-up), to impart information and advice on infant feeding to mothers of babies aged under 6 months.[4] The intervention thus targets the very first years of life, a critical period for growth and development, during which nutritional interventions are likely to be most beneficial (Schroeder et al., 1995; Shrimpton et al., 2001; Victora et al., 2010). The information is provided by trained female volunteers ('peer counselors' hereafter) nominated by local leaders. In practice, peer counselors are literate local women aged 23–50 with breastfeeding experience.[5]

Each peer counselor covers an average population of 1000 individuals, identifying all pregnant women within this population and visiting them five times in their homes: once before giving birth (third trimester of pregnancy) and four times afterwards (baby's age 1 week, 1 month, 3 months, 5 months). Although all pregnant women are eligible for the intervention and participation is free, in practice around 60% of them are visited by the peer counselors. Our data show that women who were visited by the peer counselor tend to be poorer: in particular, they were 4.8 percentage points (7.5 percentage points) less likely to have a floor (roof) built with good materials.

Exclusive breastfeeding is strongly encouraged in all visits. Information on weaning is provided from when the baby is 1 month old (visits 3–5) and includes suggestions of suitable locally available nutritious foods, the importance of a varied diet (particularly the inclusion of protein and micronutrient-rich foods, including eggs) and instructions on how to prepare foods so as to conserve nutrients and ease digestion (e.g. to mash vegetables rather than liquidize them and to pound fish before cooking it). Peer counselors were provided with a manual to remind them of the content relevant for each visit and with simple picture books to aid in explaining concepts.

2.2.1. Experimental design

The evaluation is based on a cluster randomized control trial designed as follows (see Lewycka (2011) and Lewycka et al. (2010, 2013)). Mchinji District was divided into 48 clusters by combining enumeration areas of the 1998 Malawi Population and Housing Census (National Statistical Office, Malawi, 2008).[6] This was done in a systematic way, based on the contiguity of enumeration areas and respecting boundaries of Village Development Committees (VDCs), such that each cluster contained approximately 8000 individuals. Within each cluster, the 3000 individuals (equating to 14 villages on average) living closest to the *geographical* center of the cluster were chosen to be included in the study.[7] The study population therefore comprises of individuals living closest to the geographical center of the clusters and was selected in this way in order to limit contamination between neighboring clusters by creating a natural buffer area. Twelve clusters were randomly selected to receive the infant feeding counseling intervention, with an average of three peer counselors per cluster. A further 12 serve as controls.[8]

2.2.2. Evaluation sample description

A census of women of reproductive age was conducted by MaiMwana in all clusters in 2004 ('baseline census' hereafter), before the intervention started in July 2005 (see Fig. 1).[9] Approximately 3.5 years into the intervention, which was still in place, we drew a random sample from the baseline census in order to conduct the first follow-up survey.[10] Specifically, in 2008 we drew a random sample of 104 women of reproductive age (17–43), regardless of their childbearing status,[11] from each of the 24 clusters, leaving us with a target sample of 2496 women. The baseline census contains some socio-economic and demographic characteristics of these women and their households, as shown in the left panel of Table 1. Women are on average 24.5 years old, just over 61% of them are married, over 70% have some primary schooling but just 7% have some secondary schooling. Households are predominantly agricultural and poverty is high, as indicated by the housing materials and assets. The table also shows that the randomization worked well, with the sample well balanced across intervention and control clusters at baseline given that only 1 out of 24 variables turns out to be unbalanced.[12]

We assess the impact of the intervention over 3.5 years after it began. While this has the benefit of allowing us to assess the effect of the intervention in the medium rather than short term, it also increases the risk of attrition. We succeeded in interviewing around two-thirds of the sample drawn for the first follow-up survey: 65% in intervention clusters and 68% in control clusters. Apart from the time lapse between baseline and the first follow-up, two additional factors contributed to the attrition. First, the district of Mchinji is particularly challenging for the collection of panel data because respondents are known to report 'ghost members' – fictitious household members – with the intention of increasing future official aid/transfers that may depend positively

[3] MaiMwana is a Malawian trust established as a collaboration between the Department of Pediatrics, Kamuzu Central Hospital, the Mchinji District Hospital and the UCL Centre for International Health and Development. See http://www.maimwana.malawi.net/MaiMwana/Home.html.

[4] Though the intervention is predominantly focused on nutrition, it also touches on other issues such as birth preparedness, HIV testing and counseling, vaccinations and family planning. Section 6 discusses how these aspects relate to our results.

[5] Peer counselors receive an initial five-day and annual refresher training, and attend monthly meetings. They are not paid, but receive a bicycle, meeting allowances, registers, calendars and supervision forms. They are supervised by 24 government health surveillance assistants and three MaiMwana officers.

[6] The District Administrative Center was excluded because it is relatively more urbanized and less comparable to the rest of the District.

[7] The geographic center was chosen to be the most central village in the cluster as shown on a cartographic map from the National Statistical Office, Malawi. See Lewycka (2011, p. 122) for more details.

[8] Another 24 clusters were randomly assigned to receive a participatory women's group intervention, whereby women of reproductive age were encouraged to form groups to meet regularly to resolve issues relating to pregnancy, childbirth and neonatal health. Child nutrition was not a primary focus and so we exclude these clusters from this analysis (see, instead, Rosato et al. (2006, 2009) and Lewycka et al. (2013)). MaiMwana Project also improved health facilities across the District, which equally benefitted intervention and control clusters.

[9] Further details on this baseline census can be found in Lewycka et al. (2010). We take the intervention start date to be July 2005, the date by which the first six-month cycle had been fully completed, in line with Lewycka et al. (2013).

[10] Data collection was carried out by MaiMwana in collaboration with the authors. Data were collected in Nov-2008–Mar-2009 (Oct-2009–Jan-2010) at first (second) follow-up using PDAs. To ensure that results were not driven by seasonality, field teams collected data in intervention and control clusters at the same time. The data are available for download at https://discover.ukdataservice.ac.uk/catalogue/?sn=6996&type=Data%20catalogue.

[11] This was done to avoid any potential bias arising from endogenous fertility decisions in response to the intervention. This turns out not to be an important concern, as we show in Section 6.

[12] Other welfare programs were operating in the District at the same time as this intervention. The potentially most important is the Mchinji Social Cash Transfer, providing cash transfers to the poorest 10% of households in the District. At follow-up, the intervention was in the pilot stage and only 2.5% of households in our sample (distributed evenly between intervention and control clusters) report having received it.

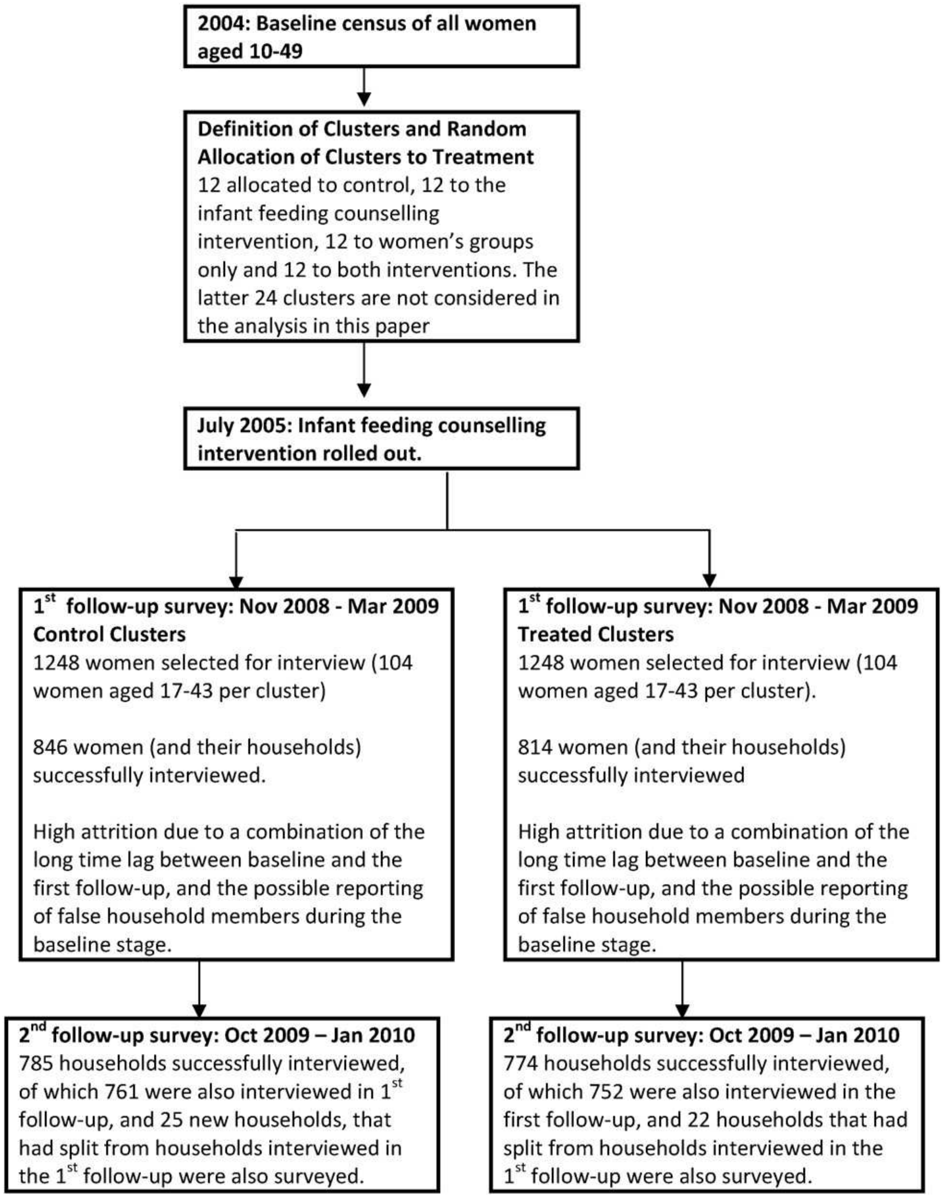

Fig. 1. Flowchart.

on household size (see Miller and Tsoka (2012) for more on 'ghost members' and Giné et al. (2012) for problems relating to personal identification in Malawi). Hence, it is possible that some women listed in the baseline census were in fact 'ghost members' and so could not be found by the field team in 2008. Second, an unexpected sharp drop of the British Pound against the Malawi Kwacha resulted in fewer resources to track women who had moved.

The middle panel of Table 1 shows that the balance on baseline characteristics is maintained in the sample of women who were found ('interviewed sample'). A small imbalance is detected on just 1 variable at the 10% level, suggesting that attrition between baseline and first follow-up was not significantly different between intervention and control clusters. While this is reassuring, it could nonetheless be the case that there is differential attrition in terms of unobserved variables. We dispel these concerns in Appendix A. We conducted a second follow-up survey of these women one year later, in 2009–10, successfully interviewing around 94% of the women interviewed at first follow-up: 95% and 93% in intervention and control areas respectively. The balance on baseline characteristics for this sample, displayed in the right panel of Table 1, is very similar to that for the first follow-up.

The surveys contain detailed information on household consumption; consumption of liquids and solids for each child in the household (≤6 years); breastfeeding practices (≤2 years); health for all individuals in the household, reported by main respondent; weights and heights of children (≤6 years); labor supply (≥6 years); and the main respondent's knowledge about child nutrition.

3. Conceptual framework

In order to understand how information of the type provided by the intervention might affect household decisions, we present a simple theoretical model in which households care about adult consumption and leisure and about their child's health, which is a function of the child's consumption of a combination of nutrition inputs. For simplicity, we assume that this is a bundle of two inputs, C_1 and C_2. We also assume that households have one adult and one child. The adult chooses simultaneously the amounts to spend on each child consumption

Table 1
Baseline sample balance.

	Full sample			Interviewed sample — wave 1			Interviewed sample — wave 2		
	Control group	Difference: treatment–control	p-Value	Control group	Difference: treatment–control	p-Value	Control group	Difference: treatment–control	p-Value
Woman's characteristics									
Married (dummy variable = 1)	0.615	−0.021	0.386	0.661	−0.034	0.184	0.654	−0.024	0.340
Some primary schooling or higher	0.707	0.033	0.402	0.682	0.040	0.340	0.68	0.037	0.438
Some secondary schooling or higher	0.066	0.010	0.535	0.060	−0.007	0.545	0.059	−0.006	0.607
Age (years)	24.571	−0.180	0.637	25.492	−0.429	0.376	25.397	−0.217	0.621
Chewa	0.948	−0.044	0.330	0.957	−0.050	0.246	0.959	−0.054	0.268
Christian	0.977	0.006	0.476	0.979	0.008	0.336	0.981	0.005	0.454
Farmer	0.661	−0.075	0.108	0.688	−0.060	0.128	0.678	−0.055	0.220
Student	0.236	0.015	0.438	0.204	0.022	0.274	0.208	0.017	0.410
Small business/rural artisan	0.036	0.030	0.129	0.037	0.024	0.220	0.039	0.025	0.264
Household characteristics									
Agricultural household	0.995	−0.005	0.471	0.995	0.002	0.591	0.995	0.003	0.500
Main flooring material: dirt, sand or dung	0.913	−0.041	0.232	0.916	−0.027	0.474	0.916	−0.028	0.422
Main roofing material: natural material	0.853	−0.018	0.697	0.857	−0.004	0.891	0.86	−0.008	0.861
HH members work on own agricultural land	0.942	−0.057	0.124	0.950	−0.056	0.120	0.95	−0.06	0.140
Piped water	0.011	0.040	0.314	0.009	0.032	0.340	0.01	0.034	0.440
Traditional pit toilet (dummy variable = 1)	0.772	0.054	0.218	0.791	0.054	0.182	0.796	0.044	0.324
# of HH members	5.771	0.066	0.817	5.848	0.132	0.863	5.903	0.096	0.833
# of sleeping rooms	2.116	0.199	0.038*	2.152	0.166	0.128	2.174	0.155	0.136
HH has electricity	0.002	0.007	0.166	0.002	0.004	0.338	0.003	0.004	0.394
HH has radio	0.630	0.030	0.408	0.641	0.015	0.709	0.645	0.014	0.655
HH has bicycle	0.509	0.015	0.643	0.512	0.008	0.843	0.512	0.01	0.769
HH has motorcycle	0.008	0.001	0.925	0.007	0.002	0.779	0.008	0.003	0.685
HH has car	0.006	−0.002	0.612	0.007	−0.003	0.298	0.008	−0.004	0.302
HH has paraffin lamp	0.925	0.032	0.262	0.926	0.036	0.178	0.935	0.026	0.360
HH has oxcart	0.058	−0.015	0.204	0.059	−0.022	0.090+	0.06	−0.022	0.072+
N	1248	1248		846	814		785	774	

Notes to table: p-values are computed using the wild-cluster bootstrap-t procedure as in Cameron et al. (2008), explained in Section 4.1. 'Full sample' includes all women (and their households) originally drawn to be part of the 2008–09 survey. 'Analysis sample — wave 1' includes women (and their households) who were interviewed in 2008–09 (wave 1), while 'Analysis sample — wave 2' includes women (and their households) who were interviewed in 2009–10 (wave 2).

* $p < 0.05$.
** $p < 0.01$.
+ $p < 0.1$.

input, C_1 and C_2, adult consumption, A, and leisure, L (or labor supply, $T-L$, where T is total time endowment of the adult). The household's optimization problem is therefore:

$$\begin{aligned} &\max_{\{C_1,C_2,A,L\}} U(A,L,H) \\ st: \quad & A + p_1C_1 + p_2C_2 \le w(T-L) \\ & H = F(C_1,C_2) \end{aligned}$$

where $U(.,.,.)$ captures the utility from adult consumption, leisure and child health, p_1 and p_2 are the prices of child nutrition inputs relative to adult consumption, and w is the wage per unit of time.[13] The function $F(.,.)$ represents the health production function, which is increasing in both C_1 and C_2, and concave. Following Cunha et al. (2013) and Del Boca et al. (2014), we assume that both the utility function and the production function are Cobb–Douglas, i.e. $U(A,L,H) = A^{\alpha}L^{\beta}H^{\gamma}$ and $H = C_1^{\delta}C_2^{\theta}$, with $\alpha,\beta,\gamma,\delta,\theta > 0$ and $\delta+\theta < 1$. We can therefore rewrite the optimization problem as:

$$\begin{aligned} &\max_{\{C_1,C_2,A,L\}} A^{\alpha}L^{\beta}{C_1}^{\gamma_1}{C_2}^{\gamma_2} \\ st: \quad & A + p_1C_1 + p_2C_2 \le w(T-L) \end{aligned}$$

where $\gamma_1 = \gamma\delta$ and $\gamma_2 = \gamma\theta$.[14]

Households make their consumption and labor decisions under their own perception of the child health production function, $C_1^{\delta}C_2^{\theta}$, which might differ from the true one (see Cunha et al. (2013)). This perceived production function depends on δ and θ, two parameters that measure the household's perception of the returns to child nutrition inputs. Changes in these parameters will change γ_1 and γ_2.

To study the effect of the intervention, we differentiate the first-order conditions with respect to γ_1 (see Appendix B) and find that $\frac{dC_1}{d\gamma_1} > 0$, but that $\frac{dC_2}{d\gamma_1} < 0$, $\frac{dA}{d\gamma_1} < 0$ and $\frac{dL}{d\gamma_1} < 0$. This allows us to establish the following proposition:

Proposition 1. If γ_1 increases, then C_1 and total household consumption increase, but C_2, A and L decrease. Similarly, if γ_2 increases, then C_2 and total household consumption increase, but C_1, A and L decrease.

The intuition is as follows. If the perceived productivity of C_1, γ_1, increases, then more will be consumed of this input. Given the concavity of the utility function, this increase is better accommodated by a small decrease in all other arguments of the utility function (C_2, A and L) than by a large decrease in only one of them. Note that the increase in C_1 is not fully offset by the decrease in C_2 and A, because L also decreases, which implies that labor supply increases. As there is no borrowing or saving, the increase in labor supply implies an increase in overall household consumption.[15]

The intervention promotes the consumption of protein-rich foods, fruit and vegetables relative to others such as staples. If C_1 summarizes

[13] We use a static, unitary model to draw out the key behavioral responses to the intervention in the simplest way. See Blundell et al. (2005) and Chiappori (1997), among others, for work that incorporates labor supply, household production and/or children within a collective framework. See Grossman (1972) for dynamic considerations of a health production function.

[14] We assume that the household cannot borrow, which is consistent with well-known credit constraints in developing countries, as discussed for instance in Dupas (2011a).

[15] Our simple model abstracts from differential labor supply responses of mother and father. In a two-parent model, one could imagine that additional time devoted to the acquisition and preparation of more nutritious foods might be to the detriment of the mother's labor supply and/or leisure.

the goods that the intervention promotes and C_2 summarizes the consumption of staples, then the effect of the intervention can be summarized in terms of increasing γ_1 but decreasing γ_2. Following Proposition 1, we expect an important composition effect (an increase in C_1 and a decrease in C_2) but the predictions on labor supply and adult and total consumption are in principle ambiguous because these will depend on whether the γ_1 or the γ_2 effect dominates. This is ultimately an empirical issue, which we study below.

4. Empirical framework

4.1. Estimation and inference

The randomized experiment provides a clean and credible source of identification to test the proposition emerging from the theoretical framework above. To do so, we estimate OLS regressions of the form

$$Y_{ict} = \alpha + \beta_1 T_c + X_{ict}\beta_2 + Z_{c0}\beta_3 + \mu_t + u_{ict}, \quad t = 1, 2 \tag{1}$$

where Y_{ict} includes outcomes for unit i (household or individual, depending on the outcome of interest) living in cluster c at time t (=1, 2 for first and second follow-ups, 2008–09 and 2009–10, respectively).[16] In line with the model, the dimensions of household behavior likely to be affected include household and child consumption, labor supply and child health;[17] T_c is a dummy variable that equals 1 if the main respondent of our survey was, at the time of the baseline in 2004, living in a cluster that later received the intervention; X_{ict} is a vector of household/individual-level variables measured at time t including a quadratic polynomial in age and gender; Z_{c0} is a vector of cluster-level variables measured at baseline such as proportions of women with Chewa ethnicity and proportions with primary or secondary schooling; μ_t is a vector of month–year dummies indicating the month of the interview; and u_{ict} is an error term, which is uncorrelated with the error term of others living in other clusters ($E[u_{ict}u_{jwq}] = 0$ for $i \neq j, c \neq w$) but which may be correlated in an unrestricted way with that of others living in the same cluster, independently of the time period ($E[u_{ict}u_{jcq}] \neq 0$). Note that this correlation structure allows for the error term for individuals/households in the same cluster to be correlated over time, and also for the presence of spillovers within but not across clusters, which is reasonable for our case given the presence of large buffer areas in place between study areas in adjacent clusters, as discussed in Section 2.2.1.

The treatment indicator, T_c, takes the value 1 if the respondent was living in a treatment cluster at the time of the 2004 baseline census and 0 if living in a control cluster at that time. Therefore, we identify an intention-to-treat parameter. Moreover, defining T_c on the basis of baseline rather than current residence circumvents any bias that might arise from selective migration from control to treatment clusters.

In terms of inference, standard statistical formulae for clustered standard errors based on asymptotic theory (cluster-correlated Huber–White estimator) provide downward-biased standard error estimates if the number of clusters is small, thus over-rejecting the null of no effect (Bertrand et al., 2004; Cameron et al., 2008; Donald and Lang, 2007; Wooldridge, 2004). This is a potential issue, as there are just 24 clusters. We use two approaches proposed to obtain valid inference: wild cluster bootstrap-t (Cameron et al., 2008) and randomization inference (Fisher, 1935, Rosenbaum, 2002).

To implement randomization inference, we follow Small et al. (2008) to account for covariates by regressing the outcome variable on all covariates, except for T_c, and applying the randomization inference procedure to the residuals from this regression. The test statistic is as follows:

$$\sum_{c:T_c=1} \frac{\hat{v}_{ict}}{N_1} - \sum_{c:T_c=0} \frac{\hat{v}_{ict}}{N_0}$$

where $\hat{v}_{ict}$ is the residual of the first-stage regression for household i in cluster c at time t and N_1 and N_0 are the number of observations in treated and control clusters respectively. Randomization inference constructs the distribution for the test statistic for every possible permutation of the randomization across clusters.[18] In practice, given the large number of possible permutations (2,704,156 in our case), it is not possible to compute the test statistic for every possible permutation of the random allocation. We instead use 100,000 randomly selected permutations to construct the distribution. The p-value is then constructed based on the proportion of test statistic values that are greater than the actual test statistic value.

In each of the estimation tables, we report clustered standard errors computed using the cluster correlated Huber–White estimator, as well as the p-values of tests of the null that the coefficient is 0 computed using both the wild-bootstrap cluster-t procedure and randomization inference. Moreover, in Appendix C, we perform a Monte Carlo exercise where we compare the test size for these two approaches with the nominal test size, within data-generating processes that incorporate the main features of our data (number of clusters, number of observations and intra-cluster correlation). The simulations indicate that both inference methods perform relatively well.

4.2. Outcomes

In line with the theoretical model, our outcomes of interest span six domains: nutrition knowledge, child's consumption at under and over six months of age, household consumption, adult labor supply, and child's physical growth and morbidity. For child health and morbidity, which were the main focus of the intervention, we focus on children aged over 6 months, for whom the intervention would have completed. We pool data from the 2008–09 and 2009–10 follow-up surveys for the analysis. Details on the various measures within each domain are provided in Appendix D. However, two points are worth highlighting here. First, child consumption is measured from maternal reports of the foods consumed by each child. Second, special care was taken to measure household consumption rather than household expenditures. This is important in this context, since a large proportion of consumption is self-produced rather than purchased from a market.

Within each domain, we have several outcome measures, meaning that we end up with over 30 outcome variables. To limit the problem caused by multiple inference (the probability of rejecting a test is increasing in the number of tests carried out), we aggregate the multiple outcome measures within a domain into a summary index, following Anderson (2008).[19] The index is a weighted mean of the standardized values of the outcome variables (with outcome variables re-defined so that higher values imply a better/more desirable outcome), with the weights calculated to maximize the amount of information captured in the index by giving less weight to outcomes that are highly correlated with each other. Another benefit of averaging across outcomes is that power is increased by reducing measurement error. Table E1 in

[16] For binary outcomes, results using probit models are very similar and are not reported.

[17] Adult consumption also may be affected but, unfortunately, no good measure of adult-specific goods is available in our data.

[18] Randomization inference is non-parametric and exploits the randomization, rather than asymptotic results, for inference. A disadvantage, however, is that inference is conducted on a sharp null hypothesis of no effect for any unit in the data, rather than the more interesting hypothesis of null average effect.

[19] While this helps to limit the problem of multiple inference, it does not address it fully because we still use eight indices. Indeed, if the data on the eight indices were independent, the familywise error rate would be at 40%. Adjusting for multiple inference within domains but not across domains is the most commonly used option (see for instance Finkelstein et al. (2012)).

Table 2
Effects on summary indices.

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
	Main respondent's knowledge on nutrition	Child food consumption		Household food consumption	Labor supply		Child physical growth	Child morbidity (reversed)
		< 6 months	> 6 months		Adult males	Adult Females	> 6 months	> 6 months
T_c	0.167+	0.250*	0.143+	0.218*	0.262+	0.018	0.102*	−0.013
Standard error	[0.085]	[0.098]	[0.074]	[0.082]	[0.131]	[0.165]	[0.036]	[0.102]
Wild-cluster bootstrap-t p-value	{0.070}	{0.016}	{0.076}	{0.020}	{0.074}	{0.915}	{0.022}	{0.861}
Randomization inference p-value	{0.065}	{0.028}	{0.099}	{0.030}	{0.062}	{0.903}	{0.035}	{0.920}
Observations	1512	151	1280	3200	3642	4138	2175	2356
R-squared	0.098	0.214	0.099	0.063	0.183	0.136	0.026	0.053
Intra-cluster correlation	0.169	0.041	0.085	0.087	0.146	0.140	0.021	0.150
Mean control areas	−0.040	−0.109	−0.054	−0.099	−0.135	−0.050	0.266	0.022

Notes to table: Standard errors computed using the cluster-correlated Huber–White estimator are reported in square brackets, with clustering at the level of the cluster (at which treatment was assigned); wild-cluster bootstrap-t p-values and randomization inference p-values in curly brackets. All regressions include controls for cluster-level average education and Chewa ethnicity, both measured in 2004, and dummies for month of interview. All regressions other than in column 4 include controls for age and age-squared. Regressions in columns 2, 3, 7 and 8 also control for gender, while those in columns 5 and 6 control for education. Outcome variables are summary indices of variables relating to that domain of outcomes. They are constructed as described in Section 4.2. Higher values of the index in columns 7 and 8 indicate lower morbidity. The component variables for each index are outlined in Table E1 in Appendix E. Sample of children includes all those born after the intervention began in July 2005, and were therefore aged 0–53 months at time of interview. Specific samples are as follows. Column 1: all households present in waves 1 and 2 with a female main respondent aged 15 years or more; column 2: all children at wave 2 aged <6 months (some components of food consumption for this group not measured at wave 1); column 3: all children at wave 2 aged 6–53 months (food consumption for this group not measured at wave 1); column 4: all households at waves 1 or 2; columns 5 (6): all adult males (females) aged 15–65 years at waves 1 or 2; columns 7 and 8: all children at wave 1 or wave 2 aged 6–53 months. Note small discrepancies in samples between columns 7 and 8 due to missing values of outcome indicators.

* $p < 0.05$.
** $p < 0.01$.
+ $p < 0.1$.

Appendix E reports the outcomes used to compute the index associated with each domain.

By using a summary index, our results provide a statistical test for whether the intervention has a 'general effect' on each of the six main domains being tested that is robust to concerns about multiple inference (Kling et al., 2007; Liebman et al., 2004). However, because it is not possible to assess the magnitude of the effect from the results using the index, we also report the results on individual outcome variables.

Descriptive statistics pertaining to the outcomes and the indices for households and individuals in the control clusters are provided in Table E2. The table indicates that maternal knowledge on infant nutrition is mixed: questions related to weaning and nutritious value of foods were mostly correctly answered, while those related to food preparation and feeding when the child or its mother is unwell were often incorrectly answered. The food intake information indicates poor feeding practices: almost half of infants aged under 6 months were given water, while each of the protein-rich foods was consumed by fewer than half of children aged over 6 months. Low consumption of protein-rich foods is also apparent from the data on household consumption. Labor supply rates are similar for males and females: over 80% have at least one paid job, while around 9% have an additional job; both men and women work on average around 25 hours weekly. Finally, child health in this setting is very poor: the average child has a height-for-age z-score that is below −2 standard deviations of the WHO benchmark (and thus is considered to be stunted) and the incidence of illness is relatively high.

5. Results

We first show the impacts on all six composite indices, pooled across waves in Table 2 and separated by wave in Table 3. The subsequent tables (Tables 4–9) display the impacts on the sub-components of each index for those indices that show an overall statistically significant effect.[20] Note that, for ease of reading, each of Tables 4–9 reproduces, in its first column, the summary index from Table 2.

[20] Tables E3 and E4 display results for the sub-components of indices that do not show a statistically significant intervention impact.

5.1. Overall findings

Table 2 displays intervention impacts on all six composite indices, as described in Section 4.2. For child-level outcomes, we estimate the impacts on children born after the intervention began in July 2005, as these are the ones whose mothers were eligible to be visited by the peer counselor. This means that we consider impacts for children aged up to 4.5 years at the time of the second follow-up survey. Furthermore, since the intervention was ongoing at follow-up, we estimate impacts separately for children aged under 6 months (whose mothers were potentially being visited by the counselors at the time) and those aged over 6 months, and report impacts on health outcomes for the latter group only. For household and adult outcomes, we consider impacts on our entire sample, regardless of whether the household was directly exposed to the intervention and of the household's fertility choices.

The key rationale underlying the intervention is that households are inefficient producers of child health because they do not have the correct knowledge. In other words, the child health production function that households optimize over is 'distorted'. In line with this, column 1 of Table 2 reports that the intervention improved main respondent's (mostly mothers) knowledge of child nutrition.[21] The effect is only significant at the 10% level, possibly due to the high intra-cluster correlation in this variable. These improvements in knowledge translated into improved food consumption for both children aged under 6 months and children aged over 6 months (columns 2 and 3 in Table 2).[22,23] The positive impact on the latter group imply that benefits of the intervention were retained even once the peer counselor stopped visiting the household.

Though the intervention provides no monetary or in-kind resources, household consumption could increase (see Section 3). In line with this, column 4 of Table 2 shows that the intervention increases total

[21] The knowledge index was constructed from questions designed in consultation with program staff and tailored to the content of the intervention. Though the questions were piloted, no formal validation exercise was conducted.

[22] Note that child-specific consumption for children over 6 months is measured at second follow-up only.

[23] That the intervention improved both knowledge and child nutrition suggests that improving knowledge of the child health production function improves nutrition choices. One might want to test this mechanism directly using the intervention as an instrument for knowledge. Unfortunately, the intervention impact on knowledge is not sufficiently strong to allow us to do this without encountering a weak instrument problem.

Table 3
Effects on summary indices by wave.

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
	Top panel: wave 1 results							
	Main respondent's knowledge on nutrition	Child food consumption		Household food consumption	Labor supply		Child physical growth	Child morbidity (reversed)
		< 6 months	> 6 months		Adult males	Adult females	> 6 months	> 6 months
T_c	0.195			0.156	0.183	−0.016	0.093	0.027
Standard error	[0.136]			[0.113]	[0.135]	[0.163]	[0.045]	[0.103]
Wild-cluster bootstrap-t p-value	{0.228}			{0.212}	{0.216}	{0.985}	{0.108}	{0.769}
Randomization inference p-value	{0.143}			{0.206}	{0.244}	{0.920}	{0.107}	{0.853}
Observations	1512			1644	1790	2080	932	1061
R-squared	0.079			0.069	0.177	0.157	0.032	0.040
Intra-cluster correlation	0.156			0.141	0.140	0.183	0.026	0.175
Mean control areas	−0.054			−0.075	−0.119	−0.033	0.286	0.001
	Bottom panel: wave 2 results							
	Main respondent's knowledge on nutrition	Child food consumption		Household food consumption	Labor supply		Child physical growth	Child morbidity (reversed)
		< 6 months	> 6 months		Adult Males	Adult Females	> 6 months	> 6 months
T_c	0.152	0.250*	0.143+	0.305**	0.323*	0.050	0.112*	−0.051
Standard error	[0.119]	[0.098]	[0.074]	[0.092]	[0.148]	[0.193]	[0.040]	[0.124]
Wild-cluster bootstrap-t p-value	{0.273}	{0.016}	{0.076}	{0.002}	{0.036}	{0.877}	{0.022}	{0.743}
Randomization inference p-value	{0.248}	{0.028}	{0.099}	{0.014}	{0.036}	{0.768}	{0.032}	{0.746}
Observations	1512	151	1280	1556	1852	2058	1243	1295
R-squared	0.043	0.214	0.099	0.050	0.184	0.125	0.028	0.043
Intra-cluster correlation	0.190	0.041	0.085	0.085	0.192	0.150	0.017	0.197
Mean control areas	−0.035	−0.109	−0.0541	−0.132	−0.148	−0.073	0.238	0.045

Notes to table: Standard errors computed using the cluster-correlated Huber–White estimator are reported in square brackets, with clustering at the level of the cluster (at which treatment was assigned); wild-cluster bootstrap-t p-values and randomization inference p-values in curly brackets. All regressions include controls for cluster-level average education and Chewa ethnicity, both measured in 2004, and dummies for month of interview. All regressions other than in column 4 include controls for age and age-squared. Those in columns 5 and 6 also control for individual education. Finally, those in columns 2, 3, 7 and 8 also control for gender. Outcome variables are summary indices of variables relating to that domain of outcomes. They are constructed as described in Section 4.2. Higher values of the index in column 8 indicate lower morbidity. The component variables for each index are outlined in Table E1 in Appendix E. Sample of children includes all those born after the intervention began in July 2005, and were therefore aged 0–53 months at time of interview. Specific samples are as follows. Column 1, both panels: all households present in both waves 1 and 2 with a female main respondent aged 15 years or more; column 2, bottom panel: all children at wave 2 aged under 6 months; column 3, bottom panel: all children at wave 2 aged 6–53 months (food consumption for this group not measured at wave 1); column 4, top (bottom) panel: all households at wave 1 (2); column 5, top (bottom) panel: all adult males aged 15–65 years at wave 1 (2); column 6, top (bottom) panel: all adult females aged 15–65 years at wave 1 (2); columns 7 and 8, top (bottom) panel: all children at wave 1 (2) aged 6–44 months (6–53 months). Note small discrepancies in samples between columns 7 and 8 due to missing values of outcome indicators. Knowledge index in wave 1 constructed with three questions asked in wave 1; and that in wave 2 with four questions asked in wave 2 only.

* $p < 0.05$.
** $p < 0.01$.
\+ $p < 0.01$.

household food consumption, measured using the composite index, at 5% significance. The increase in household consumption might have been partially funded by improvements in adult labor supply, particularly of males (column 5); female labor supply is unchanged by the intervention (column 6). Although our model of Section 3 already indicated that labor supply could increase, other factors may also explain increased consumption, including borrowing and/or drawing down savings. Increases in labor supply could also be due to a reduction in time devoted to caring for sick children.

A key policy question is whether the observed adjustments on various margins of household behavior (increased consumption and labor supply) improved child health. Column 7 shows that these changes in behavior translate into improved child physical growth for children aged over 6 months. No significant effect is found on child morbidity.[24] Note though that given the substantial infant mortality reductions found by Lewycka et al. (2013), and under the assumption that weaker children are the ones more likely to survive as a result of the intervention (Bozzoli et al., 2009; Deaton, 2007), the reported effects likely *underestimate* the true effect of the intervention on child health.

Table 3 shows the results by follow-up survey round ('wave'), which are of interest in order to see whether the effects are sustained over time. In general, the table shows that the point estimates share the same signs across both waves and are not significantly different from each other. Notably, the point estimates of household food consumption, male labor supply and child physical growth all show a tendency to be larger in wave 2 than in wave 1, and they are statistically significant in wave 2 only, although they are not significantly different from the wave 1 estimates.[25] The tendency for larger treatment effects on consumption and male labor supply in wave 2 may be due to some heterogeneity of treatment effect according to the time when the surveys

[24] We also considered the intervention impacts on child anthropometrics and morbidity for children under 6 months who were undergoing the intervention at the time of the survey and for whom these would be intermediate-stage data. We find a positive but statistically insignificant effect on both outcomes. Interestingly, we find that the prevalence of diarrhea decreases for children under 6 months, consistent with the reduced intake of water and non-maternal milk for this group.

[25] Note that there are more children aged over 6 months who would have been eligible for the intervention in wave 2 than in wave 1 since the former includes children born between July 2005 and July 2009 while the latter includes children born between July 2005 and October 2008.

Table 4
Effects on components of the knowledge index.

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
	Summary index	Breastfeeding when infant has diarrhea	Are biscuits or groundnuts/soya more nutritious for children aged 6 months to 3 years?	From what age should solid foods be given to infants?	How should an HIV-positive woman feed her baby?	Is nsima or porridge more nutritious for an infant aged >6 months?	What is the best way to cook fish with porridge for an infant aged >6 months?	Should eggs be given to an infant aged >9 months?
T_c	0.167+	0.253+	−0.052	0.037	0.138	−0.101	0.067**	0.104
Standard error	[0.085]	[0.115]	[0.041]	[0.026]	[0.150]	[0.078]	[0.019]	[0.069]
Wild-cluster bootstrap-t p-value	{0.070}	{0.084}	{0.290}	{0.166}	{0.444}	{0.210}	{0.002}	{0.186}
Randomization inference p-value	{0.065}	{0.028}	{0.222}	{0.292}	{0.399}	{0.179}	{0.008}	{0.192}
Observations	1512	1512	1512	1512	1512	1512	1512	1512
R-squared	0.098	0.104	0.052	0.043	0.045	0.072	0.038	0.020
Intra-cluster correlation	0.169	0.277	0.082	0.049	0.408	0.183	0.057	0.107
Mean control areas	−0.040	0.217	0.938	0.88	0.393	0.857	0.026	0.719

Notes to table: All regressions include controls for age, age-squared, average cluster-level education and Chewa ethnicity, both measured in 2004, and dummies for month of interview. Standard errors computed using the cluster-correlated Huber–White estimator are reported in square brackets, with clustering at the level of the cluster (at which treatment was assigned); wild-cluster bootstrap-t and randomization inference p-values in curly brackets. Sample includes all households with a female main respondent aged 15 years and over. 'Summary Index' aggregates the measures in columns 2–8 using the method described in Section 4.2. The variables in columns 2–8 are dummy variables equal to 1 if the respondent answered correctly. Questions in columns 2–6 and column 8 were multiple-choice questions where respondents chose one correct answer from three–five options. Question in column 7 was an open-ended question, with interviewers marking correctly answered options.

* $p < 0.05$.
** $p < 0.01$
\+ $p < 0.1$.

were conducted. Wave 1 data were collected between mid November and the end of March, while wave 2 data were collected between October and early January. The levels of the consumption and male labor supply indices are the lowest in the October to mid November period, which is when the treatment effect is the highest.

While the composite indices allow us to assess the general impact of the intervention on each domain, their magnitudes cannot be interpreted, as the weighting used to build the index distorts the scale. To shed more light on the magnitude of the effects, we next report and discuss findings for individual outcomes for the composite indices for which there is a statistically significant effect of the intervention. We note that the results on the index components must be considered exploratory and interpreted carefully since the familywise error rate is not being controlled for. Appendix F reports results on individual outcomes by wave.

Table 5
Effects on intake of liquids by children under 6 months.

	[1]	[2]	[3]
	Summary index	Water	Milk other than maternal
T_c	0.250*	−0.107	−0.082*
Standard error	[0.098]	[0.069]	[0.034]
Wild-cluster bootstrap-t p-value	{0.016}	{0.122}	{0.012}
Randomization inference p-value	{0.028}	{0.212}	{0.115}
Observations	151	151	151
R-squared	0.214	0.362	0.087
Intra-cluster correlation	0.041	0.000	0.060
Mean control areas	−0.109	0.474	0.101

Notes to table: All regressions include controls for age, age-squared, gender, average cluster-level education and Chewa ethnicity, both measured in 2004, and dummies for month of interview. Standard errors computed using the cluster-correlated Huber–White estimator are reported in square brackets, with clustering at the level of the cluster (at which treatment was assigned); wild-cluster bootstrap-t and randomization inference p-values in curly brackets. Sample includes children at wave 2 aged under 6 months. 'Summary index' aggregates the measures in columns 2–3 using the method described in Section 4.2. 'Water' is an indicator for whether the child had any water in the three days prior to the survey. 'Milk other than maternal' is an indicator (measured in second follow-up only) for whether the child had milk other than breastmilk in the three days prior to the survey.

* $p < 0.05$.
** $p < 0.01$.
\+ $p < 0.1$.

5.2. Nutritional knowledge, consumption and labor supply

The intervention resulted in improvements in the main respondent's knowledge of child nutrition. The index aggregates together the correct responses to seven questions (reproduced in Appendix G). Columns 2–8 of Table 4 report the impact of the intervention in terms of the proportion of respondents who correctly answered each of the seven questions. The results show that the knowledge improvements are concentrated on breastfeeding practices when infants are ill and on knowledge of food preparation practices. We note that the intra-cluster correlation coefficient is very high for most components of the index, which makes it particularly difficult to detect statistically significant differences.[26]

Improvements in food consumption were detected for children under and over 6 months. For the former group, we see from Table 5 that the improvement comes from a reduction in non-maternal milk. There is also a reduction (though not statistically significant) in the consumption of water. Table 6 shows that improvements for the latter group are driven by substantially higher consumption of protein-rich beans in the three days prior to the interview. The intakes of meat and eggs (also protein rich) are also positive, although not statistically significant, most likely due to the reduced sample size (child food intake was collected at second follow-up only). Overall, these results indicate that the intervention significantly affected the composition of child nutritional intake.

[26] Note that the number of observations is lower than for other household-level variables. This is because we combine wave 1 and wave 2 questions into a single index, to maximize its informational content, and drop households without a female main respondent aged 15 years or above. Note that the three questions in wave 1 are a subset of the seven questions asked in wave 2. We construct the index to include responses from wave 1 to the three common questions and responses to the four questions unique to wave 2. This is because there was evidence of households having learnt or found out answers to the three questions carried over from wave 1 to wave 2.

Table 6
Effects on food consumption by children aged 6–53 months.

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
	Summary index	Any beans	Any meat	Any fish	Any eggs	Any Vegetables	Any fruit	Any nsima	Any porridge
T_c	0.143^{+}	0.225**	0.089	0.006	0.025	−0.010	−0.009	0.025	0.096
Standard error	[0.074]	[0.056]	[0.095]	[0.099]	[0.052]	[0.020]	[0.058]	[0.015]	[0.064]
Wild-cluster bootstrap-t p-value	{0.076}	{0.002}	{0.474}	{0.925}	{0.655}	{0.723}	{0.941}	{0.144}	{0.208}
Randomization inference p-value	{0.099}	{0.007}	{0.289}	{0.954}	{0.632}	{0.634}	{0.895}	{0.140}	{0.251}
Observations	1280	1280	1280	1280	1280	1280	1280	1280	1280
R-squared	0.099	0.067	0.021	0.012	0.010	0.142	0.153	0.144	0.035
Intra-cluster correlation	0.085	0.116	0.084	0.112	0.048	0.018	0.093	0.000	0.136
Mean control areas	−0.054	0.258	0.290	0.462	0.163	0.959	0.699	0.929	0.800

Notes to table: All regressions include controls for age, age-squared, gender, average cluster-level Chewa ethnicity and education, both measured in 2004, and dummies for month of interview. Standard errors computed using the cluster-correlated Huber–White estimator are reported in square brackets, with clustering at the level of the cluster (at which treatment was assigned); wild-cluster bootstrap-t and randomization inference p-values in curly brackets. Sample contains all children at wave 2 aged 6–53 months (data on child solid intake collected at wave 2 only). 'Summary index' aggregates the measures in columns 2–9 using the method described in Section 4.2. The variables in columns 2–9 are dummy variables equal to 1 if the corresponding food was consumed by the child in the three days prior to the survey.

* $p < 0.05$.
** $p < 0.01$.
$^{+}$ $p < 0.1$.

Table 7
Effects on household food consumption.

	[1]	[2]	[3]	[4]	[5]
		Per-capita monthly food consumption for:			
	Summary index	Cereals	Proteins	Fruit and vegetables	Other foods
T_c	0.218*	−9.768	129.150^{+}	269.987*	60.701
Standard error	[0.082]	[52.432]	[54.802]	[108.591]	[33.552]
Wild-cluster bootstrap-t p-value	{0.020}	{0.863}	{0.066}	{0.044}	{0.126}
Randomization inference p-value	{0.030}	{0.865}	{0.025}	{0.033}	{0.069}
Observations	3200	3200	3200	3200	3200
R-squared	0.063	0.117	0.020	0.195	0.025
Intra-cluster correlation	0.087	0.074	0.042	0.173	0.053
Mean control areas	−0.099	605.80	349.10	679.80	149.50

Notes to table: Standard errors computed using the cluster-correlated Huber–White estimator are reported in square brackets, with clustering at the level of the cluster (at which treatment was assigned); wild-cluster bootstrap-t and randomization inference p-values in curly brackets. Sample includes all households at waves 1 or 2. All regressions include controls for age, age-squared, average cluster-level education and Chewa ethnicity, both measured in 2004, and dummies for month of interview. Coefficients in columns 2–5 are in terms of Malawi Kwacha. (The average exchange rate to the US Dollar was approximately 140MK = 1 US$ at the time of the surveys.) 'Summary index' is an index of the food items in columns 2–5, constructed as described in Section 4.2. 'Cereals' includes consumption of rice, maize flour and bread. 'Proteins' includes consumption of milk, eggs, meat, fish and pulses. 'Fruit and vegetables' includes consumption of green maize, cassava, green leaves, tomatoes, onions, pumpkins, potatoes, bananas, masuku, mango, ground nuts and other fruits and vegetables. 'Other foods' includes consumption of cooking oil, sugar, salt, alcohol and other foods.

* $p < 0.05$.
** $p < 0.01$.
$^{+}$ $p < 0.1$.

We saw from Table 2 that the intervention resulted in improvements in overall household food consumption. Columns 2–5 of Table 7 show that the improvement is due to an increase in the consumption of proteins and of fruit and vegetables. The effects are relatively large. Focusing on proteins, which are particularly important for child growth as shown by, for example, Puentes et al. (2014), we decompose the effect on the extensive (i.e. moving from consuming no proteins to some proteins) and intensive margin (i.e. moving from consuming some proteins to more proteins).[27] Around 26% of households in control clusters report consuming no protein-rich foods in the seven days prior to interview; hence there is clear potential for improvement in the extensive margin. Indeed, the extensive margin accounts for one-third of the consumption increase.[28] The increase in the intensive margin corresponds to 210 g of meat/poultry extra and 640 g of beans extra per child per month. To put these quantities in perspective, a toddler will usually consume 50 g of beans in one portion, together with some vegetables and carbohydrates.

A number of factors are likely to explain this substantial increase in food consumption: first, the time span of the intervention is sufficiently long (it had already been up and running for over 3.5 years by the time consumption was first measured); second, the intervention was intensive, involving up to five one-to-one home visits; third, as seen from the labor supply results in Table 2, there was scope for labor supply to increase and thereby fund at least some of the increased consumption.

Table 2 also showed that the male labor supply index increased as a result of the intervention. Looking at the sub-components of the index – work participation, likelihood of having at least two jobs and hours worked – Table 8 reports positive effects of the intervention on all three, though only statistically significant for the probability of having at least two jobs. However, the intra-cluster correlations are much higher for work participation and for the number of hours worked than for the probability of having at least two jobs (0.213 and 0.100 vs. 0.033), which greatly reduces the power to detect a significant effect of the intervention on the former.

[27] Calculations available upon request.
[28] The consumption increase coming from the extensive margin is calculated under the assumption that the households in the treated clusters induced to consume protein-rich foods as a result of the intervention all consume proteins equivalent to the average consumed by control cluster households with non-zero protein consumption. The increase on the intensive margin – corresponding to the rest of the consumption increase – is further decomposed into food quantities (beans and meat/poultry) under the assumptions that the entire amount is consumed by children aged under 12 years only (who are, in control clusters, 2.4 per household on average) and that households pay prices equivalent to the average cluster-level median unit values.

Table 8
Effects on male labor supply.

	[1] Summary index	[2] Works	[3] Has at least 2 jobs	[4] Weekly hours worked
T_c	0.262^{+}	0.106	0.080**	4.310
Standard error	[0.131]	[0.080]	[0.025]	[2.918]
Wild-cluster bootstrap-t p-value	{0.074}	{0.272}	{0.010}	{0.240}
Randomization inference p-value	{0.062}	{0.220}	{0.011}	{0.202}
Observations	3642	3642	3642	3642
R-squared	0.183	0.183	0.057	0.163
Intra-cluster correlation	0.146	0.213	0.033	0.100
Mean control areas	−0.135	0.819	0.094	25.740

Notes to table: All regressions include controls for age, age-squared, average cluster-level education and Chewa ethnicity, both measured in 2004, individual education and dummies for month of interview. Standard errors computed using the cluster-correlated Huber–White estimator are reported in square brackets, with clustering at the level of the cluster (at which treatment was assigned); wild-cluster bootstrap-t and randomization inference p-values in curly brackets. Sample includes all males aged 15–65 years at wave 1 or 2. 'Summary index' contains the variables in columns 2–4 and is computed using the method described in Section 4.2. 'Works' is an indicator of whether individual had an income-generating activity at the time of the survey. 'Has at least 2 jobs' is an indicator of whether individual has at least two income-generating activities. 'Weekly hours worked' gives the total hours worked in the week prior to the survey on both income-generating activities.
* $p < 0.05$.
** $p < 0.01$.
+ $p < 0.1$.

Table 9
Effects on physical growth of children aged 6–53 months.

	[1] Summary index	[2] Height for age	[3] Healthy weight for age	[4] Healthy weight for height
T_c	0.102*	0.274*	0.028	0.042^{+}
Standard error	[0.036]	[0.100]	[0.017]	[0.024]
Wild-cluster bootstrap-t p-value	{0.022}	{0.022}	{0.120}	{0.132}
Randomization inference p-value	{0.035}	{0.055}	{0.308}	{0.147}
Observations	2175	2175	2175	2175
R-squared	0.026	0.048	0.020	0.029
Intra-cluster correlation	0.021	0.023	0.018	0.014
Mean control areas	0.266	−2.326	0.829	0.852

Notes to Table: Standard errors computed using the cluster-correlated Huber–White estimator are reported in square brackets, with clustering at the level of the cluster (at which treatment was assigned); wild-cluster bootstrap-t and randomization inference p-values in curly brackets. All regressions include controls for age, age-squared, gender, dummies for month of interview and average cluster-level education and Chewa ethnicity, both measured in 2004. Sample includes children aged 6–53 months at wave 1 or 2. 'Summary index' contains the variables in columns 2–4 and is computed using the method described in Section 4.2. 'Height for age' is a standardized z-score relative to the WHO reference population. 'Healthy weight for age' is a dummy variable = 1 if child's weight-for-age z-score is not more than 2 standard deviations above or below the WHO reference population. 'Healthy weight for height' is a dummy variable = 1 if child's weight-for-height z-score is within 2 standard deviations of the WHO reference population.
* $p < 0.05$.
** $p < 0.01$.
+ $p < 0.1$.

The finding that the intervention increases male labor supply is consistent with it being a margin with considerable scope for increase. Indeed, previous research in Malawi has shown that labor supply is upward sloping rather than fixed (Dimova et al., 2010; Goldberg, 2016). In our data, only 9% of males in control clusters have a second job, most of them in non-agricultural self-employment activities.[29] Moreover, there is considerable entry into and exit from secondary jobs: among those with (without) a secondary job at first follow-up, 33% (7%) have one at second follow-up, a year later. While an extensive literature has documented increases in labor supply in response to increases in uncertainty and income shocks in developing countries (Ito and Takashi, 2009; Kochar, 1999; Lamb, 2003; Rose, 2001; Saha, 1994), this is the first paper to document labor supply responses to changes in the perceived child health production function.

Beyond the mechanism for the increase in labor supply indicated in Section 3, important cultural features of Malawian society are likely to contribute to the increase in male, rather than female, labor supply. In particular, the main ethnic group in Mchinji – the Chewa – is a matrilocal and matrilineal group, where men usually move to their wives' villages on marriage, and wealth (predominantly land) is held by women and passed on through the matriline (Phiri, 1983; Sear, 2008). As a consequence, women have more power and authority than in patrilineal societies common across most of Africa and South Asia (Reniers, 2003). Indicative of this empowerment, all three measures of labor supply are strikingly similar for males and females (last row of Table 8 and Table E3).[30] Finally, mothers are generally the main caregivers of children. So the finding that male labor supply increases in response to the mother receiving information on child nutrition is in line with the cultural background, where females are relatively empowered.

[29] Over half of these second jobs involve employment in own/family business, a quarter involve work on the family farm and the rest involve work as an employee in public/private sector (~20%) or on someone else's farm (<5%).

[30] This has been documented by others for the Malawian context, including Goldberg (2016) and the 2004 Malawi DHS Report (National Statistical Office, Malawi and ORC Macro, 2005, pp. 34–36). In the matrilineal Khasi society (India), women and men also have similar labor supply profiles (Gneezy et al., 2009).

5.3. Child health

Table 2 documented improvements in child physical growth for children over 6 months. Looking at the sub-components of the physical growth index in Table 9, we see that the improvements are due to an increase in the average height-for-age z-score by 0.27 of a standard deviation of the WHO norm.[31] This is an important increase, and corresponds in magnitude to 65% of the average effect size obtained with the direct provision of food in food-insecure populations (Bhutta et al., 2008). Interestingly, further analysis, documented in Table E5, indicates that the effects on physical growth are much stronger for children aged 6–24 months.[32]

Clearly, we cannot disentangle whether the improvement in physical growth is due to the reduction in intake of liquids other than breast milk when the child was under 6 months old or to the improvement in child food intake after age 6 months, or a combination of both. Our key message is that households responded to the intervention by increasing consumption and working more, which is the first such finding in this literature.[33]

6. Alternative explanations

We have argued, using the model of Section 3, that consumption and labor supply will increase because the perceived productivity of child consumption (in terms of child health) increased as a result of the intervention. Here we consider four alternative explanations. First, we consider and rule out that the increases in adult labor supply are driven

[31] As is common with anthropometric data from developing countries, the SD of the height-for-age z-score in our sample is larger than that in the WHO reference population (1.5 instead of 1), and so this increase corresponds to an increase of 0.18 of a SD using the SD for our sample.

[32] These patterns are consistent with two non-competing explanations: that the intervention did not work very well at the beginning and/or children in control clusters experienced catch-up growth at slightly older ages.

[33] We have also examined the heterogeneity of the effect of the intervention on the anthropometric and morbidity indices according to whether the mother has had more than one child since the intervention started. The interaction terms were far from statistically significant (p-value of 0.45 or larger).

by improvements in adult health somehow generated by the intervention (Table E6). Second, parental investment in child nutrition could have increased as a result of decreased fertility caused by the intervention, potentially yielding an increase in child quality (Becker and Tomes, 1976). The intervention could have reduced fertility by reducing infant mortality and consequently inducing households to demand fewer children, or through the family planning component of the intervention. Analysis of the intervention effects on family planning behavior and births to women in our sample (as reported in the MaiMwana Health Surveillance System[34]) reveals very small and statistically insignificant effects, ruling out this channel (Table E7).[35]

Third, the reduction in infant mortality and improvement in child health could have affected parental labor supply, through changing the demand for childcare. It is plausible that if infant mortality declines and there are more surviving children, mothers in treated clusters may increase their time devoted to childcare, therefore working less, leading to fathers working more to compensate for this. However, as we showed in Section 5.1, the intervention does not appear to have reduced female labor supply, suggesting that this mechanism is not at play in our context. Another potential channel through which labor supply may change as a result of improvements in children's health is through reducing the need for fathers to be at home to help take care of children, thus facilitating an increase in their labor supply.

Finally, effects could also be driven by information provided by the intervention on issues other than infant feeding practices, e.g. vaccination of infants, promotion of HIV testing and hygiene practices. Though these could have improved child health, it is unlikely that they would improve household consumption and labor supply. Available evidence suggests that these other components would have had very modest or no effects. Lewycka et al. (2013) find mixed intervention effects on vaccination rates (BCG vaccination rates increased, while polio vaccination rates decreased). Moreover, vaccination rates in control clusters were high, leading to small intervention effects. The authors also find that the intervention was not effective in improving antenatal HIV counseling and treatment. This is not surprising, since the intervention simply encouraged women to get tested for HIV, without any efforts to alleviate cost constraints or stigma effects related to being tested (Derksen et al., 2014; Ngatia, 2011; Thornton, 2008). Finally, our finding that the intervention did not reduce the prevalence of diarrhea for children aged between 6 and 53 months and adults (Tables E4 and E6) suggests that the component on hygiene information probably had limited success.

7. Conclusion

In this paper, we use exogenous variation in mothers' knowledge of the child health production function induced by a cluster randomized intervention in Malawi, to study empirically whether improving knowledge of the child health production function influences a broad range of household behaviors.

We first document that the intervention improved mothers' knowledge of nutrition. Using a simple theoretical model, we show that households should react to this improved knowledge by changing the composition of child food intake in favor of protein-rich foods, fruit and vegetables. The intervention could also increase household food consumption and adult labor supply, although the theoretical predictions are ultimately ambiguous. Our empirical results show that, indeed, both child's food intake and child nutritional status improved and that ultimately both labor supply and household food consumption increased.

We hypothesize that two issues might have contributed to the success of the intervention. First, the provision of information was not merely a one-off event in the intervention areas, but a sustained activity, still in place, that serves to spread information and to remind households of the importance of child nutrition on an ongoing basis. This may also explain why households adjusted on non-health margins to adhere to advice provided by this nutrition intervention and may shed light on why some health information campaigns have been successful while others have failed. Second, the main ethnic group in rural Malawi, the Chewa, is a matrilineal one, in which women are likely to have more bargaining power and authority within the household than women in patrilineal societies common in much of the rest of Africa and South Asia. This higher female empowerment might indicate that women are in a good position to implement the recommendations given by the counselors as well as to encourage fathers to work more. It is not clear whether such responses may emerge in other settings and we see this as an area worthy of further investigation.

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Appendix A. Supplementary Appendices and Data

Supplementary data to this article can be found online at http://dx.doi.org/10.1016/j.jdeveco.2016.05.002.

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[34] The MaiMwana Health Surveillance System interviews the mothers of all children born in the 24 clusters since 2005 at 1 month and 7 months of age, and thus provides a more complete picture of births in the study areas than cross-sectional surveys.

[35] Because the intervention decreased infant mortality, an alternative explanation for our findings is that (a) the children who survive tend to have worse health and (b) parents compensate for the worse health by providing them with more resources. Based on the results of Lewycka et al. (2013), we estimate that the marginal surviving children would be approximately 2.3% of the intervention sample, which is too small to explain the magnitude of the treatment effects if they were to be driven entirely by these marginal children.

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