Reducing Child Malnutrition through Community Intervention Programs: Evidence from a Large Scale Randomized Trial in Rural Senegal

Harold Alderman[‡]
The World Bank

Sebastian Linnemayr[♦]
Harvard University

Preliminary version 5 March 2008

Abstract

This article investigates the impact of the *Programme de Renforcement de la Nutrition* – Nutrition Enhancement Program (PRN) that aims to improve child nutrition in Senegal based on randomized community intervention. Despite substantial deviation from the original assignment status of villages between the two data waves in 2004 and 2006, we find a significant impact of the PRN on medical inputs and child care measures taken by mothers in villages originally assigned treatment status. We do not detect a strong overall program impact on the outcome measure of weight for age, possibly because the program has not been in place long enough to show a tangible effect. Children with longest exposure to the program, in particular the ones whose mothers benefited from the program during their pregnancy, show a significant improvement in their nutritional status, lending support to this hypothesis. We compare the results of this prospective analysis based on the planned randomization status with those from an ex-post difference-in-difference approach, finding that this treatment-on-the-treated approach leads to larger estimates of the program impact. The findings represent a step into a more complete understanding of the effectiveness of large-scale nutrition interventions.

[‡] Harold Alderman, World Bank, Washington DC, USA; email: halderman@worldbank.org.

[♦] Sebastian Linnemayr, Harvard School of Public Health, Harvard University, Cambridge, USA; email: slinnema@hsph.harvard.edu.

Acknowledgement of Data Organization and Collection; General Acknowledgement

The data for this nutrition intervention program have been organized and collected by the Research Centre for Human Development under the direction of its director Salif Ndiaye, whom we owe thanks. We also would like to thank Claudia Rokx as well as Biram Ndiaye, Coordinator of Cellule de Lutte contre la Malnutrition – Primature (PRN), Dakar, for collaboration on the design and implementation of the research. We would also like to thank the regional coordinators of the PRN and the regional organisers of the Forecasting and Statistics Directorate for their help. We gladly acknowledge the competence, enthusiasm, and availability of the members of the technical teams. The personnel on the ground, in particular the chauffeurs, interviewers, and team leaders have contributed significantly to the success of the survey. Last but not least we would like to thank the people who were interviewed for generously offering their time, energy, and commitment.

Sebastian Linnemayr would like to thank the Service Enseignement Supérieur-Technologie-Recherche Région Provence-Alpes-Côte d’Azur for a doctoral scholarship, the participants at the Lunch Seminar Series at PSE and UMR 379 for comments, and Holger Stichnoth for helpful advice.

1. Introduction

Malnutrition is a persistent problem in developing countries; its close link to poverty lead to its inclusion as the first goal in the Millennium Development Goals[2]. WHO estimates the fraction of malnutritioned children in developing countries at 33% as measured by the percentage of children stunted, i.e. children that fall below -2 standard deviations of the United States National Center for Health Statistics / WHO international reference median value (de Onis, Frongillo, and Blössner 2000). This has far-reaching consequences: malnutrition *in utero* or in infancy can have a long-lasting negative impact on the ability of these children to acquire knowledge, on their performance on test-scores, and on their capacity to subsequently achieve sufficient income to provide for their own children when adults (Alderman, Hoddinott, and Kinsey 2006; Victora et al., 2008).

There is substantial agreement on the efficacy of a number of nutrition interventions. There is little doubt, for example, the breastfeeding promotion saves infant lives, or that vitamin A prophylaxis reduced child mortality. However, these is little consensus that community based programs can reduce stunting or underweight (Bhutta et al. 2008, Alderman, 2007). This study assesses the impact of a package of health inputs on anthropometric status of children in Senegal as well as the treatment effect on variables related to health behavior, as well as health inputs. The programs ability to influence these intermediary inputs not only helps explain the final outcomes but also indicates whether other health measures can be improved in the course of a program with a primary objective framed in terms of nutritional status. While the study is designed to

[2] The Millennium Development Goals can be accessed at http://www.un.org/millenniumgoals/.

inform on the impact of a large scale intervention, a secondary objective of this paper is to illustrate some means to address a common pitfall of evaluation of programs, imperfect randomization.

The outline of the paper is as follows: in section 2, we present the regions under consideration and the type of program they receive. We then briefly summarize the identification strategies for the estimators in section 3 that we will use in the subsequent analyses. The sections 5.1 and 5.2 present the results for nutritional status expressed as height for age, weight for age, and weight for height, as well as looking at measures of behavioral change and the availability of health inputs. Section 5.3 presents the results when using a retrospective approach to the data analysis. Section 6 concludes and discusses the differences in outcome of the two approaches.

2. Characteristics of the Intervention

This study looks at a package of interventions designed to address malnutrition in Senegal. Senegal designed, a strategy in 2002 to fight malnutrition that is scheduled to reach 50% of children under five years of age by the year 2011, the *Programme de Renforcement de la Nutrition* – Nutrition Enhancement Program (PRN). The PRN program is based on three pillars that set it apart from previous programs: the first one is a traditional nutrition supplementation approach in combination with growth promotion and integrated disease control. Second, the program is based on the principle of multisectoral interventions, involving several ministries for implementation of the program components. Third, it aims at the reinforcement of institutional capacity of the relevant agencies with the goal of future sustainability of the program.

The first phase of the Nutrition Enhancement Program targeted 20% of children under the age of five with growth promotion and the integrated service of child diseases at the community level. In the three regions under consideration, most women give birth at home: the percentage is highest in Kolda (68.9%) and lowest in Fatick (60.6%). The percentage of children who are exclusively breastfed in the first four months of their lives is very low: it varies between 1.1% in Kaolack and 2.6% in Kolda, even though the WHO recommends exclusive breastfeeding up to at least six months of age. Children in Senegal are also suffering from the lack of micronutrients that remains widespread despite interventions that have been taken place in the past. 84% of children under 5 years of age suffer from anemia, as do 61% of women. Vitamin A deficiency is a public health problem, with about 61% of children under the age of six years suffering from this deficiency. Summary statistics for the sample population are presented in Table 1. The z-scores of height for age, weight for age, and weight for height indicate the poor nutritional standard of the children in the sample: on average, their nutritional situation is about one standard deviation lower than the mean for the US reference group. For example, a two year old boy would on average weigh about 12.2 kg in the US; in Senegal, his average weight could be expected to be about 1.5 kg lower at about 10.8 kg (WHO Child Growth Standards). The z-score measures did not develop uniformly between the two data waves: whereas height for age deteriorated slightly between 2004 and 2006, weight for age improved by about one tenth of a standard deviation, and weight for height by about twice that.

Over the course of the first wave of the PRN, monthly growth promotion was directed at 200 000 mothers and their children with NGO agencies contracted to provide these services. One of the main pillars of the approach were monthly discussion rounds with mothers related to nutrition that were organized at the community (i.e. village or village-neighborhood) level. Some of the more novel features of the nutrition intervention concern its strategies of implementation: great care was being put into involving communities and key figures within these communities such as village elders, the marabou (a religious leader), or grandmothers, who traditionally play a big role in influencing feeding and child care practices. The goal of targeting these people in the implementation strategy is on the one hand to involve the agents actually influencing behavior of mothers, and on the other hand to educate these people who will then pass on the knowledge, thereby supporting the sustainability of the project. By educating the grandmothers about signs of severe malaria, for example, one addresses the key decision maker directly. Other relatively novel strategies used in the program are the organization of meetings of pregnant women in order to generate a forum where these women can exchange ideas and experiences concerning pregnancy and child-rearing. These meetings also help these women to escape the social isolation that they are often subjected to. Another strategy encouraged is the principle of 'positive deviation': individuals who show behavior different form that of the other villagers and who avoid certain health problems are invited to share their experience and to teach the other women these novel strategies.

The following activities constituted program components that were carried out in all three regions:

- Growth promotion: during these sessions, the health worker weighs the child and discusses its progress with the mother by comparing it to a growth chart distributed
- Vitamin A supplementation: in the course of the weighing sessions, vitamin A is distributed to children 6 – 59 months and mothers in the 42 days after having given birth
- Iron supplementation: in discussion rounds, pregnant women are encouraged to take iron supplements that are distributed by health centers
- Bednets were distributed for a fee (including a subsidy) and the mothers were shown how to impregnate them with insecticide.
- Deworming was offered to all children aged 6 – 59 months.

- Exclusive breastfeeding without supplementation for at least the first six months of the child's life was one of the key behavioral changes targeted in the discussion groups
- Appropriate nutrition: as part of the community activities, cooking workshops were organized to demonstrate the preparation of nutritious foods for the mothers as well as supplements to breastfeeding after six month of age

The first phase of the PRN included a randomized evaluation. The implementing NGOs were asked to provide a list of villages in which they had the means and intention to intervene. From the total list of about 1000 villages, 212 villages were randomly chosen in the three regions. Based on these villages, 220 clusters were built (as some villages are large enough to have 2 clusters, and one village had three clusters), and in each cluster up to 20 households were randomly drawn based on the list of households in the village. The nutrition intervention was randomly assigned to half of these villages; the NGOs were asked to schedule services to the other half in a later wave of implementation. They were free to include other villages not among the 212 in the intervention at any time.

A baseline survey was conducted in April 2004 in all 212 villages, collecting data about the health status of the children, socioeconomic variables of the households these children are residing in, and extensive information about the nutrition and child care practices of the mother. The survey teams administered three questionnaires: a village questionnaire, a household questionnaire, and an individual questionnaire for the mother of the child. If there was no child under three years of age in the household, the household was dropped in the first round without replacement. In the second round, these households were replaced with other randomly drawn households from the same village list until the fixed number of households was interviewed. This change in the survey design explains the significantly larger number of children measured in the second round. In June 2006 the same information was collected in these villages.

3. Theory underlying the empirical analysis

When evaluating the effectiveness of a treatment *T*, we would like to compare the difference *D* in the outcome variable of interest *Y* for the same individual *i* once he receives the treatment and once when he does not[3]:

$$(1)\ D = Y_i^T - Y_i^C ,$$

where the superscript *T* denotes an individual receiving the treatment, and *C* stands for the outcome without the treatment. Evidently, this is impossible as we cannot observe the same individual or unit in two states of the world at the same time, and we face the so-called problem of the missing counterfactual. However, it may be possible to discern the average effect of a certain intervention on a group of individuals:

$$(2)\ E\left[Y_i^T - Y_i^C\right].$$

[3] Key references on this topic include Duflo, Glennerster, and Kremer (2006), Angrist and Krueger (1999) and see Chapter 25 in Cameron and Trivedi (2005). For the original reference for the Rubin causal model see Rubin (1974).

When subtracting and adding the unobserved but typically well-defined term $E[Y_i^C \mid T]$, i.e. the outcome of the treatment group in the absence of treatment, we can state the evaluation problem as the situation in which the total change in the outcome consists of the treatment effect and the selection bias that confounds causal identification:

(3) $D = E\left[Y_i^T \middle| T\right] - E\left[Y_i^C \middle| T\right] + E\left[Y_i^C \middle| T\right] - E\left[Y_i^C \middle| C\right].$

Much of empirical work is concerned with finding ways to control for selection bias, the difference in the non-treatment outcome between treatment and control individuals. The challenge is to establish a close estimate of the missing observation of the non-treatment outcome of the treatment group. Randomization achieves this since it guarantees that the control and treatment groups in the absence of the intervention have on average the same outcome:

(4) $E\left[Y_i^C \middle| T\right] - E\left[Y_i^C \middle| C\right] = 0,$

In such a situation, a simple comparison of the sample post-intervention means suffices to measure the average treatment effect of the intervention. In terms of regression analysis one can regress the outcome on covariates and a dummy variable for inclusion in the treatment group:

(5) $Y_{it} = X_{it}\beta + T_t\delta + e_{it}$

Where e is an error term composed of individual, family and community unobserved fixed characteristics as well as a stochastic disturbance term, μ_{it}

(6) $e_{it} = \nu_i + \eta_i + \varepsilon_i + \mu_{it}$

The estimate of δ provides an unbiased estimate of the program impact if the placement of the program is independent from e_{it}, that is, $[E(e_{it} \mid T_t)=0]$. By design random assignment ensures this.

Random assignment is not without its pitfalls, however. For example, individuals selected for the treatment may not take it up, so that the intention to treat does not provide an accurate assessment of the impact of the treatment on the treated. Or individuals assigned to the control group obtain the service from an alternative source, say a private provider. This is often called a crossover effect. Angrist et al. (2002) provide an illustration in which some individuals who received a randomly assigned school voucher did not utilize it and other in the control group received a scholarship from private groups.

As is well know, at times it may be administratively not possible to use random assignment or the assignment may be imperfect, especially in large scale interventions. One approach to the problem is to use a difference in difference method in the context of panel data. By construction, the fixed effects remove the corresponding component of the error term and thus any correlation between it and the treatment variable *T*. This simple problem can be implemented in a regression set-up with two data-waves such as:

(7) $Y_{it} = \alpha + \beta \cdot 1(i \in T) + \gamma \cdot 1(t = 2) + \delta \cdot 1(t = 2) * 1(i \in T) + \varepsilon_{it}$,

where we control with the second term on the right hand side for initial differences between the control and the treatment group, the third term controls for a time-trend common to both groups, and the fourth term indicates the treatment effect. The estimate of interest is *(δ-β)* which measures the treatment effect purged of initial differences. The assumptions this specification is based on is that in the absence of treatment both groups would experience a similar trend in the outcome variable, ruling out selection into treatment based on responsiveness to the program. The approach then controls for differences in levels but not in time trends. Like for the case of randomization, identification breaks down in the presence of substantial spill-overs between treatment and control group.

However, in cases in which the assignment is based on observed values of the outcome desired – where the treatment is prioritized to groups with low test scores or nutritional status, for example – then it is likely that $[E(\mu_{it} \mid T) \neq 0]$ since measurement error partially determines the assignment. Chay et al. (2005) present an example of such an assignment to treatment based on baseline performance where difference in difference results are biased due to regression in the mean.

Several studies have used both approaches in order to evaluate the degree of selection bias that observational studies may be subjected to. This tradition started with Lalonde (1986) who found that there are often significant differences between prospective and retrospective empirical approaches for the evaluation of programs. There has been a growing interest in this subject in the development economics literature since then, for an overview see Duflo, Glennerster, and Kremer (2006). The current paper then aims to contribute to this discussion by employing both approaches on the same dataset, while keeping the focus on the prospective evaluation of the PRN program described above.

4. Empirical Analysis

4.1. Measures of program success used

The main focus of the evaluation of the Senegal nutrition intervention is to assess the impact of the set of services offered on nutritional status of children .When preparing the intervention, the outcome measure of weight for age z-score was determined as the original indicator to determine the sample size of the program as well as the indicator for tracking success of the program on the part of the organizations financing the PRN[4].

The outcome of interest reflects a package of services which are valued not only for their impact on weight but also as indicators of the functioning of a community health program in general. For this reason, we begin by examining the availability of health care measures before and after birth such as micronutrient supplementation or malaria bednets. The analysis of these measures can provide evidence of program success (in the sense that the health inputs reached the villages/households) that are less prone to measurement error, rely on faster-moving measures, and indicate potential future success of the program if it takes time to transform these input measures into measurable change

[4] Apart from weight-for-age, we also investigated the impact of the program on height-for-age, another frequently used anthropometric measure that captures more long-term impacts (Alderman 2000). The results obtained for this measure are virtually identical to the ones for weight-for-age and are therefore not presented to reduce the number of output tables. These omitted tables are available from the authors.

in the outcome z-scores. This aspect adds an important dimension to the analysis as measurement error has been cited to be a common problem for the analysis of household surveys involving health measures (Deaton 1997).

We also investigate other dimensions of the services that might influence child nutritional status. Typically, it is assumed that there are three pillars to the health of a child (UNICEF 1990): health and sanitation services, nutrition inputs including micronutrients, and knowledge about proper childcare practice. The PRN program may have an impact on one or several of these inputs. Thus, we will investigate whether there were changes in child care practices and utilization of health services in the treatment communities relative to the control. A finding of a significant positive improvement of these components is an indication that the intervention was effective in delivering the services it intended (as discussed above) and that it led to a behavioral change on the part of the mothers, although it would not be proof that these inputs are sufficient for an improvement in the z-score outcome measures in the time period under consideration. This first step of the analysis then will allow for a more nuanced evaluation of the nutrition intervention.

The unit of randomization in the current study is the village and not the individual; all households in a village belong either to the control or the treatment group. As a result, actual take-up of the program by individuals is not observed, although the village health workers tried to encourage all mothers in the village to participate in the program. Therefore, it is the impact of the availability of the program in a village rather than its actual take-up that is evaluated. As actual take-up is a choice variable, investigation the effects of mere availability of the program may be less prone to selection bias (Strauss 1990).

Spillovers of program impacts from treatment to control villages may occur when women from treatment villages communicate information concerning child care practices to women in control villages, for example when women meet at weekly markets that are important social occasions in Senegal (Perry 2000). Certain program components are more prone to benefit control units than others: information is a non-rival good and hence freely shared; the same is not true for bednets or other program components requiring physical inputs. While indicators of behavioral change are those that can be expected to move quickest in response to information transmitted in the process of the nutrition intervention, it is also the component most easily shared. In such a situation, we would expect the indicator to increase for both groups, but more so for the treatment units given their longer and more intense exposure to the program.

In addition to investigating health inputs as well as health outcomes, we also try to take potential heterogeneity of the program impact into account. In particular, we test whether younger children have a different response than the older cohort. While all communities in the treatment had the program for the same period, given that the program was available for less than two years in the villages, we test the possibility that the children that were exposed to the intervention already *in utero* have greater benefits. This would be in keeping with evidence on that malnutrition occurs at very early ages and is fairly unresponsive by 18 months (Shrimpton et al. 2002). We also test for a different form of treatment heterogeneity that is related to the socioeconomic status of women. Nutrition and other health-related services may either represent substitutes or compliments to the inputs given to the children by their mothers. Discussion rounds, one of the main pillars of transmission of health-related behavior, are likely to give access to information to women who in their household hold low status, for example because they are young wives of household heads, or if they are uneducated (given that they have the chance of participating). Other program components may benefit educated women more. *A priori*, it is not clear which type of woman will benefit relatively more from the services offered.

4.2. Control of the success of the randomization

The randomization of interventions among participants has been used in a large number of studies in order to take account of selection bias when evaluating interventions in the development economics literature[5]. The current dataset was collected from a pilot study with the goal of measuring the impact of the nutrition enhancement program of the government of Senegal in three rural regions in Senegal before the extension of the intervention to the rest of the rural communities in the country. In a first step we investigate whether the villages appearing on the treatment and control lists appeared there randomly, i.e. that the villages were not pre-selected onto the lists. As this step of the randomization was implemented by the authors there is little reason to suspect such a possibility. If the treatment status was assigned to the villages in a random fashion, we would expect the outcome variables as well as the conditioning variables to not differ significantly between the treatment and the control group in the observation period before

[5] For an overview of a number of studies and the lessons learned from them, see Duflo (2006).

the intervention is administered (Behrman and Todd 1999). The comparison of villages along their planned treatment status in Table 2 shows that the two types of villages show largely similar outcome and control variables.

As discussed above, the assignment of treatment was planned to be executed in a purely random fashion. However, the NGOs implementing the program may not have understood the importance of the randomization procedure prescribed or deviated from the planned treatment status of villages for other reasons. In Table 3, we see that there was considerable deviation of actual treatment status from the status initially assigned. Of the 111 villages that were initially assigned treatment status, 80 ended up receiving treatment, while 31 villages (28%) did not receive the intervention. Of the 100 initial control villages, for one village no second round data were available, and of the 99 remaining villages, 8% received the intervention despite their control status. In the analysis to follow, we therefore have to address the problem of partial compliance.

Basing the analysis on the actual treatment status would lead to the potential introduction of a selection bias as villages may have purposely been selected into and out of the treatment group by the implementing NGOs. While selection on fixed community characteristics can be address using difference-in-differences analysis that is employed, as mentioned, this step would not necessarily be the case if the selection criteria included time varying factors.

When we compare the most important group of villages that deviated from the original design, the 31 villages that initially were assigned treatment status but that did subsequently not receive intervention along some key dimensions with the remaining 80 villages from the group initially assigned treatment status, there is some evidence that the deviation did not happen randomly, as reported in Table 4. The villages from the 2004 planned treatment list that subsequently did not receive the nutrition intervention were initially somewhat better off in terms of their nutritional status, and had less children that were mildly underweight (below -2 standard deviations from the mean of the US reference population). They more often had a market, and showed a lower presence of NGOs and healthposts than the villages that retained their treatment status. These statistics indicate the possibility that the NGOs purposely selected villages by focusing on the worst-off villages in the treatment sample and on those villages in which there were already other NGOs present (potentially the intervening NGO itself).

Basing the analysis on actual rather than planned treatment-status would lead to a systematic underestimation of the treatment status if the worst-off villages were less able to profit to the same degree from the nutrition intervention program compared to their better-off peers. On the other hand, if the improvement of these villages were partly due to a reversion to the mean as discussed in section 3, we would tend to overestimate the positive effect of the intervention. In section 5.3 we will present some key regressions when using actual rather than planned treatment status of the villages, and discuss the difference to the results arrived at using the original treatment status of a village.

4.3. Empirical Specification

As discussed above, we will first examine the program impact on the z-score measures height-for-age and weight-for-age. We can implement this comparison of the means of the control and the treatment group in a regression framework, and control for the clustering of standard errors at the level of the village, the unit of randomization[6]:

(8) $y_{ij} = \alpha + \beta Z_j + \varphi X_{ij} + v_j + \varepsilon_{ij}$,

where *y* stands for the nutritional status of individual *i* in village *j*, *Z* stands for the planned treatment status, *v* is a village-specific effect, while ε is a random error term. In a situation of less than perfect compliance with the assigned treatment status, this measure of effectiveness is called the "intention to treat" estimator, and can be of interest when the effectiveness of a program "as is" is of interest. For the current situation, where the success of a pilot study that is intended to be scaled up to the national level is evaluated, the intention-to-treat estimator likely is a realistic predictor of the potential future success of the program.

This specification controls for a source of imprecision of estimation as a robustness check: in particular for small sample sizes, there is the possibility that villages differ in their characteristics influencing nutritional status. Therefore, we include socioeconomic variables *X* at the individual and household level that in previous studies have been

[6] As there is sometimes more than one child per mother and/or more than one mother in the same households, in future analysis these data characteristics will be taken into account, but are left aside in the current analysis.

shown to influence nutritional status (see for example Behrman and Skoufias 2004), and that for the households in the current study might not be equal on average. Note that the introduction of control variables at the individual and household level should not change the estimate of β unless Z and X are correlated (Angrist and Krueger 1999).

In addition, we use the first wave data to control for village-level invariant observables as well as unobservables by including a dummy variable for each village in the sample. This specification essentially transforms the regression into one based on a within-village estimator. Using both data waves allows investigating a potential common time trend in the outcome measure that is experienced by both groups.

5. Results

5.1. Estimation based on the original randomization classification

Tables 5 and 6 present the results for weight for age using the original randomization classification in 2004, irrespective of whether the villages actually received the program. In column (1), the results for the intention to treat estimator including control variables using both data waves are presented. In column (2) we test whether the program is more effective at reducing the probability of children to show a mild form of undernutrition, i.e. falling under minus two standard deviations of the respective z-score[7], as a strategy of targeting the worst-off children may have been pursued by the implementing agencies in

[7] As the results for mild and severe malnutrition in almost all cases go in the same direction, we omit in the result tables below the regressions for severe malnutrition and focus on mild malnutrition only. The omitted output tables are available from the authors.

the face of budget, time and human resources constraints. In all specifications, we allow for the clustering of standard errors at the village level.

Table 5 presents the results for weight for age, a measure capturing both short- and long-term influences on child nutritional status. The value of the coefficient of the constant gives the mean value of height for age for the control group, while the coefficient for the variable *Planned status 2006* indicates the difference of the mean value for the villages that were initially assigned treatment status irrespective of whether they received the intervention subsequently. For neither of the specifications in column (1) or (2) do we find a statistically significant positive impact in the program villages in comparison to the control villages. We include age dummies of the children in six months age groups, with the children between 30 months and 3 years of age representing the omitted group. The age dummies reflect the common finding of a deterioration of the nutritional situation for the children with increasing age when compared to the children in the US reference group. The other control variables mirror the findings in previous studies on the determinants of child nutrition: parent's education and sanitary facilities in the household improve the nutritional status, while the status of being a twin reduces it significantly. The gender dummy is insignificant, as discrimination by gender is typically not observed in Africa (Svedberg, 1990). As indicated by the indicator variable '*second round',* the villages in the sample experience an overall increase in the weight for age indicator of about one tenth of a standard deviation for both types of villages that is statistically significant at the 1% level. This finding confirms the general trend observed in the summary statistics in Table 1.

5.2. Heterogeneity of Program Impact

As briefly discussed in the introduction, there are several reasons as to why the program may have exerted a more pronounced effect on certain subgroups of the study population. For example, the impact of the treatment may differ by the age at which the children were exposed to treatment (Alderman, 2007). Children who at the time of the baseline survey in April 2004 were six months old, for example, were included in the second wave in 2006 although these children likely were weaned by the time the intervention began. In contrast, a child born after April 2004 would have had the additional benefit for their mothers participating in the discussion groups and micronutrient provision for pregnant women, two important program components. There is increasing evidence that the experiences *in utero* can have long-lasting effects (Behrman and Rosenzweig 2004; Strauss 2000).

Additionally, take-up and/or potential benefit of the program may be influenced by the characteristics of the mother. The services may substitute for lack of resources available to the mother, in which case we would expect a relatively larger impact of the program for women lacking the particular resource. On the other hand, the services offered might complement the human resource of the mothers, in which case we would expect a positive coefficient on the interaction term of the education of the woman and the indicator variable taking value one if the village was scheduled to receive the intervention. Similarly, we include an interaction for teenage mothers who are likely to have less experience and less status; the treatment may substitute or complement either of these characteristics.

In Table 5 in the last two columns we present the results when we interact the treatment status with child age groups. We find that for both weight for age as a continuous variable in column (3) as well as for the logit regression in column (4) we find the planned treatment status to have a statistically significant and sizeable beneficial impact. As discussed above, it appears that only the youngest children, i.e. those whose mothers benefited from the program when they were pregnant, benefitted from the intervention.

When investigating whether mother characteristics influence the degree to which children benefit from the intervention as measured by the planned treatment status assigned to the village, we see in Table 6 that there is only very weak evidence for such an effect. When we distinguish between mothers with no primary education and mothers with at least such basic education levels in column (1), we do not find evidence that either type of mother benefits more than the other as indicated by a statistically insignificant coefficient on the interaction between having no education and planned treatment status of the village. However, we do find, as expected, that children of uneducated women have a lower nutritional status than their peers of mothers with some education. In Column (2), we interact the status of being the child of a teenage mother with planned treatment status and find a positive coefficient estimates that is however just borderline significant at the 10% level. These results then indicate that the beneficial impact of the program on children is not caused by a beneficial impact on mothers with these characteristics only but seems to be more wide-spread among different types of mothers.

5.3 Estimation based on the actual treatment status

Although the randomized design is only guaranteed to be free of selection bias when using the original treatment status assigned to the village irrespective of actual receipt of the program, the variable used in the analysis is an imperfect indicator of services actually delivered. As such, using the planned assignment status avoids a correlation with unobserved factors at the possible expense of errors in variable from mismeasurement from which an attenuation bias is expected. The intended treatment classification contains a significant fraction of villages that did not receive the intervention even though they listed in the treatment-group and, conversely, a few control communities did receive the treatment. Thus, this approach dilutes the estimate compared to the result we would obtain had all villages conformed to their planned treatment status.

To address this issue, we repeat the above estimations using actual instead of planned treatment status in a difference in difference framework as discussed in the methodology section above. Implicitly, we assume that in the absence of treatment, the villages in both groups would experience a similar trend in malnutrition rates and, further, that selection was not based on the observed level of the outcome at time the baseline was implemented. This could introduce a bias that differs from that using the assigned treatment, but the direction is not known. We can, however, control for time-invariant unobservables at the village level by implementing the estimation in a regression set-up with village fixed effects.

The results, presented in Tables 7 and 8 shows a statistically significant and positive treatment effect for the z-score measure of weight for age, in contrast to the results in table 5 when not stratifying by child age groups. The finding of a significant impact in the differences-in-differences specification compared to an insignificant result for the estimation based on randomized assignment mirrors that in Glewwe et al. (2000) that found in a study based on school inputs in Kenya that ex-post evaluations tend to overestimate treatment when compared to analysis based on randomization, even when using a difference-in-differences set-up. Moreover, this approach still finds that children whose mothers benefited from the program availability during their pregnancy show a weight-for-age score that is significantly higher than that of children of the same age in villages that did not receive the intervention. Children up to six months of age are also less likely to fall below less than two deviations from the mean of the US reference population, although the coefficient is only borderline at the 10% level. Table 8, as in the case before when analysis was based on intent-to-treat, shows little evidence for heterogeneity of treatment impact for mothers of different characteristics. Whereas children of uneducated mothers show a significantly lower weight-for-age score, they do not profit relatively more from the intervention than other children.

5.4 Behavioral indicators

As discussed in a previous section, the PRN program targets different pillars of the child health production function, in particular behavioral change through discussion groups with mothers, grandmothers, and other key decision makers, the provision of micronutrients, the provision of drugs against common diseases in the areas (malaria, hookworm), and the provision of preventive measures such as impregnated bednets.

As progress on improving these behaviors and providing the medicines is an indicator of the effectiveness of service delivery per se, we first investigate a set of health seeking measures. In Table 10, the results for a number of behavioral changes as well as the provision of health inputs and disease incidence are presented. Most of the coefficients measuring the impact of being in a village assigned treatment status in 2004 are significant. The top rows in Table 10 present the regression output for variables relating to behavioral change that may have happened in response to information transfer through discussion groups that formed part of the intervention. In the following rows the results for health and nutrition input variables such as usage of drugs against common diseases are presented. A significant difference in the proportion of women in the treatment group receiving such physical inputs indicates that they have reached the planned target group. The results for a third category of variables entering the child nutrition function are presented in the bottom rows of Table 10, where we compare the disease occurrence in the two types of villages. We would expect treatment villages to have less sick children in response to the provision of health care services as part of the program. These indicators are likely to be correlated with the z-scores presented below, as children suffering from diarrhea, malaria, or pneumonia typically experience disease-related weight loss.

The early introduction of liquids other than breast milk is a practice that may be influenced by the information campaign incorporated in the intervention through discussion groups with key decision makers. The giving of sanctified water, sugar water, or tea is a prevalent behavior in rural regions in Senegal with potentially serious health consequences for the infant. The results in column 1 indicate that this practice is significantly less prevalent in planned intervention villages than in initial control villages in 2006, despite there not having been a significant difference in the prevalence between the groups in 2004. When expressing the coefficient in terms of marginal probabilities, we find that there was a reduction in the probability of giving liquids other than breast milk in the first three days following birth of 11% in the treatment group as compared to the control group. We also investigate another variable indicating behavioral change: we look at whether the mother followed the recommended practice of giving the colostrum to the baby after birth and find that this practice is more prevalent for the planned treatment than for the control group. The marginal effect of being in the treatment group translates into an increase of 9% of the probability of giving the colostrum.

The next regression results reported in Table 10 investigate variables gauging the availability of health care measures that require the provision of physical inputs: availability of malaria pills, worm drugs, and bednets against infection with malaria. For two out of three measures we observe a statistically significant impact of planned treatment status on the availability of these health measures. For the provision of bednets, the coefficient is not significant at the 10% level. Similar results are found for the provision of micronutrients: for vitamin A for infants and iron supplements for pregnant mothers, there is a statistically significant impact of being in a planned treatment village for vitamin A, and a borderline significant effect for iron supplementation. For vitamin A, the marginal effect of being in the treatment group leads to an increase of 6% in the probability of receiving this micronutrient.

The last two rows in Table 10 show that disease prevalence is not affected by planned treatment status. There is no statistically significant difference between having had diarrhea or a cough in the two weeks preceding the survey for the two types of villages. This finding is not surprising as disease prevalence is likely to be correlated with the outcome measure of weight-for-age, for which we also found no significant impact between the planned treatment and control villages.

6. Conclusion

The aim of the current study is to evaluate the success of a pilot program forming part of the *Programme de Renforcement de la Nutrition*, a nutrition intervention program targeted at young children in Senegal that introduces the program components to three poor rural regions. Identification of the treatment effects is based on the random assignment of the treatment status among 212 villages in April 2004 before receiving the intervention and being re-surveyed in June 2006.

We find significant changes in health care practices in the villages assigned to the treatment status. But using this assignment as an indicator of treatment, we do not find an average overall impact on children. We do, however, observe that those children whose mothers benefit from the intervention during their pregnancy display a significantly improved nutritional status than their older peers who were likely weaned before the program began. These observations can guide the allocation of resources in similar programs.

However, while these results give an indication of the project's success, the magnitude of the impact is biased downwards due to cross over effects. Thus, we also report results using differences in differences based on the actual treatment status as of June 2006 instead of the planned one from 2004. The difference in difference approach to evaluating the study tends to results in larger estimates of the treatment effects compared to the results based on prospective analysis above, confirming the results of previous studies.

References

Alderman, Harold. 2007. Improving Nutrition through Community Growth Promotion: Longitudinal Study of the Nutrition and Early Child Development Program in Uganda. World Development. 35(8): 1376-1389.

Alderman, Harold, John Hoddinott, and Bill Kinsey. 2006. "Long term consequences of early childhood malnutrition." *Oxford Economic Papers* 58, pp. 450-74.

Alderman, Harold. 2000. "Anthropometry." in *Designing Household Survey Questionnaires for Developing Countries: Lessons from 15 years of the Living Standards Measurement Study*, eds. Grosh, Margeret and Paul Glewwe. World Bank Publications, Washington, DC.

Allen, Lindsay, and Stuart Gillespie. 2001. "What Works? A Review of the Efficacy and Effectiveness of Nutrition Interventions", ACC/SCN Nutrition Policy Paper No. 19, United Nations Administrative Committee on Coordination Sub-Committee on Nutrition, September 2001.

Angrist**,** Joshua; Bettinger, Eric; Bloom, Erik; King**,** Elizabeth; Kremer, Michael. 2002. Vouchers for Private Schooling in Colombia: Evidence from a Randomized Natural Experiment**..** American Economic Review**,** Vol. 92 Issue 5, p1535-1558

Angrist, Joshua, and Alan Krueger (2001): "Instrumental variables and the search for identification: from supply and demand to natural experiments.", *Journal of Economic Perspectives* 15 (4), pp. 69-85.

-. 1999. "Empirical Strategies in Labor Economics.", Chapter in *Handbook of Labor Economics*, Vol. 3A, eds. Orley Ashenfelter and David Card, Amsterdam North Holland, pp. 1277-1366.

Behrman, Jere and Mark Rosenzweig. 2004. The Returns to Birth Weight. Review of Economics and Statistics 86, 586-601.

Behrman, Jere, and Emmanuel Skoufias. 2004. "Correlates and determinants of child anthropometrics in Latin America: background and overview of the symposium." *Economics and Human Biology* 2 (3), pp. 335-51.

Behrman, Jere, and John Hoddinott. 2000. "An Evaluation of the Impact of PROGRESA on Pre-school Child Height.", International Food Policy Research Institute Working Paper, July 28, 2000.

Behrman, Jere, and Petra Todd. 1999. "Randomness in the Experimental Samples of PROGRESA (Education, Health, and Nutrition Program)". International Food Policy Research Institute Research Report, March 26, 1999.

Bound, John, David Jaeger, and Regina Baker. 1995. "Problems with instrumental variables estimation when the correlation between the instruments and the endogenous explanatory variables is weak." *Journal of the American Statistical Association* 90 (430), pp. 443-50.

Bhutta, ZA T Ahmad, RE Black et al. 2008. What works. Interventions for maternal and child undernutrition and survival, Lancet Volume 371: p 417-440.

Cameron, A. Colin, and Pravin Trivedi. 2005. *Microeconometrics: Methods and Applications*. Cambridge University Press.

Chay Kenneth Y.; McEwan, Patrick J.; Urquiola, Miguel.. 2005. The Central Role of Noise in Evaluating Interventions that use Test Scores to Rank Schools. American Economic Review. Vol. 95 Issue 4, p1237-1258,

De Onis, Mercedes, Edward Frongillo, and Monika Blössner. 2000. "Is malnutrition declining? An analysis of changes in levels of child malnutrition since 1980." *Bulletin of the World Health Organization* 78, pp. 1222-33.

Deaton, Angus. 1997. *The Analysis of Household Surveys*. World Bank Publications, Washington DC.

Demographic Health Survey. 1999. *Enquête Sénégalaise sur les Indicateurs de Santé*. Ministère de la Santé, SERDHA and Macro International Inc. 1999.

Duflo, Esther. 2006. "Field Experiments in Development Economics." Paper prepared for the World Congress of the Econometric Society, January 2006.

Duflo, Esther, Rachel Glennerster, and Michael Kremer. 2006. "Using Randomization in Development Economics Research: A Toolkit.", Centre for Economic Policy Research Discussion Paper No. 6059.

Gertler. Paul. 2004. "Do Conditional Cash Transfers Improve Child Health? Evidence from PROGRESA's Control Randomized Experiment." *AEA Papers and Proceedings* 94 (2), pp. 336 – 41.

Glewwe, Paul, Michael Kremer, Sylvie Moulin, and Eric Zitzewitz. 2000. "Retrospective vs. Prospective Analyses of School Inputs: the Case of Flip Charts in Kenya." NBER Working Paper 8018, November 2000.

Honoré, Bo. 2001. "Nonlinead Models with Panel Data." Center for Microdata Methods and Practice Working Paper CWP 13/02.

Lalonde, Robert. 1986. "Evaluating the Econometric Evaluations of Training Programs with Experimental Data." *American Economic Review* 76 (4), pp. 604-20.

Perry, Donna. 2000. “Rural weekly markets and the dynamics of time, space and community in Senegal.” *The Journal of Modern African Studies* 38 (3), pp. 461-86.

Rubin, Donald. 1974. “Estimating Causal Effects of Treatments in Randomized and Non-randomized Studies.” *Journal of Educational Psychology* 66, pp. 688-701.

Shrimpton R., Victora C., de Onis M., Costa Lima R., Blössner M., and G. Clugston. (2001). Worldwide Timing of Growth Faltering: Implications for Nutritional Interventions *Pediatrics*, 107(5): 75-81.

Strauss, John. 1990. “Households, communities and preschool children’s nutrition outcomes: Evidence from rural Cote d’Ivoire.” *Economic Development and Cultural Change* 36 (2), pp. 231-62.

Strauss, Richard S., 2000, “Adult Functional Outcome of Those Born Small for Gestational Age,” *Journal of the American Medical Association* 283:5 (February), 625-632.

Svedberg, P. (1990), Undernutrition in Sub-Saharan Africa: is there a gender bias? *Journal of Development Studies*, 26, 469-86.

UNICEF. 1990. *Strategy for Improved Nutrition of Children and Women in Developing Countries*. UNICEF Policy Review presented to the UNICEF Executive Board in April 1990.

Victora, CG, L Adair, C Fall *et al.* 2008. Maternal and child undernutrition: consequences for adult health and human capital, *Lancet* 371: p340-357.

World Development Report. 2004. *Making Services Work for the People*. World Bank Publications, Washington DC.

Table 1: Summary Statistics of socioeconomic variables in 2004 and 2006

	2004		**2006**	
Continuous variables	Mean	Standard Deviation	Mean	Standard Deviation
Height for age	-.998	1.483	-1.048	1.486
Weight for age	-1.317	1.417	-1.210	1.415
Categorical variables				
Male Dummy	.511	.500	.511	.500
Age 0-5 months	.195	.395	.178	.383
Age 6-11 months	.170	.376	.172	.378
Age 12-17 months	.190	.392	.196	.397
Age 18-23 months	.145	.352	.148	.355
Age 24-29 months	.187	.390	.153	.360
Age 30-35 months	.113	.317	.121	.326
Mother primary schooling	.143	.351	.173	.378
Mother secondary schooling	.023	.149	.033	.180
Husband primary schooling	.121	.326	.132	.339
Husband secondary schooling	.072	.259	.063	.243
Household size	14.889	8.483	14.294	7.195
Access to tap water	.372	.483	.215	.411
Water Closet	.121	.326	.064	.244
NGO in village	.673	.470	.810	.394
Healthpost in village	.313	.465	.286	.453
# of observations	4296		6144	

Table 2: Comparison of control and treatment villages along key dimensions in 2004

Village status	Planned Treatment Group	Planned Control Group	p-value
# of villages (number of children in sample)	111 (2321)	100 (1975)	
Height for Age in 2004	-1.043	-.945	.158
Weight for Age in 2004	-1.352	-1.276	.265
Took iron supplements	.845	.846	.971
Took malaria medication	.828	.830	.931
Early introduction of liquids	.782	.791	.772
Took vitamin A during pregnancy	.617	.593	.423
Child had diarrhea in last two weeks	.333	.337	.849
Child received oral rehydration solution	.056	.042	.090
Child received deworming medicine	.073	.073	.990
Early introduction of solid foods	.162	.167	.807
Household has bednets	.390	.406	.693

Table 3: Planned versus actual treatment status of villages

		Realised Status 0	Realised Status 1	Total
Planned Status	0	91 (92%)	8 (8%)	99
	1	31 (28%)	80 (72%)	111
		122	88	

Table 4: Comparison of planned treatment villages along actual intervention status in 2006

Village status	Planned Treatment, treatment received	Planned Treatment, no treatment received	p-value
# of villages (number of children in sample)	80 (1733)	31 (588)	
Weight for age in 2004	-1.397	-1 .220	.102
Height for Age in 2004	-1.102	-.868	.066
% of children under -2SD wfa	.330	.267	.038
% villages with a market	.175	.323	.092
Road impassable	.263	.290	.770
NGO active in 2004	.788	.581	.028
Health post in 2004	.363	.290	.477

Table 5: Prospective analysis - Weight for age

	(1) Weight-for-age	(2) Mild malnutrition	(3) Weight-for-age	(4) Mild malnutrition
Second round	0.126***	-0.171**	0.120***	-0.166**
	(0.046)	(0.072)	(0.046)	(0.072)
Second round * planned intervention	-0.020	0.074	-0.107	0.123
	(0.061)	(0.096)	(0.089)	(0.156)
Age group 0-5 months	1.117***	-1.525***	1.011***	-1.374***
	(0.050)	(0.108)	(0.063)	(0.127)
Age group 6-11 months	0.286***	-0.335***	0.254***	-0.291***
	(0.047)	(0.089)	(0.058)	(0.109)
Age group 12-17 months	-0.452***	0.782***	-0.454***	0.769***
	(0.042)	(0.081)	(0.053)	(0.100)
Age group 18-23 months	-0.509***	0.798***	-0.517***	0.795***
	(0.052)	(0.085)	(0.061)	(0.105)
Age group 24-29 months	-0.306***	0.515***	-0.334***	0.533***
	(0.048)	(0.084)	(0.059)	(0.103)
Treatment * 0-5 months group	-	-	0.345***	-0.589**
			(0.099)	(0.252)
Treatment * 6-11 months group	-	-	0.087	-0.132
			(0.092)	(0.189)
Treatment * 12-17 months group	-	-	-0.007	0.050
			(0.091)	(0.169)
Treatment * 18-23 months group	-	-	0.009	0.019
			(0.101)	(0.180)
Treatment * 24-29 months group	-	-	0.074	-0.046
			(0.097)	(0.178)
Male Child	-0.019	0.002	-0.019	0.002
	(0.028)	(0.047)	(0.027)	(0.047)
Twin	-0.724***	1.126***	-0.720***	1.123***
	(0.106)	(0.143)	(0.105)	(0.143)
Primary edu female	0.078*	-0.239***	0.081**	-0.242***
	(0.040)	(0.073)	(0.040)	(0.073)
Secondary edu female	0.068	-0.114	0.074	-0.117
	(0.091)	(0.159)	(0.091)	(0.159)
Primary edu male	0.011	-0.049	0.011	-0.050
	(0.042)	(0.080)	(0.042)	(0.080)
Secondary edu male	0.159***	-0.213*	0.157***	-0.210*
	(0.057)	(0.110)	(0.058)	(0.110)
Husband edu missing	0.018	-0.054	0.014	-0.051
	(0.042)	(0.080)	(0.041)	(0.080)
Tapwater	0.027	-0.018	0.028	-0.019
	(0.038)	(0.069)	(0.037)	(0.069)
Watercloset	0.047	-0.017	0.043	-0.014
	(0.052)	(0.098)	(0.052)	(0.098)
Wealth index	0.011	-0.006	0.011	-0.006
	(0.012)	(0.019)	(0.012)	(0.019)
Constant	-1.385***		-1.350***	
	(0.046)		(0.052)	

Observations	10127	10127	10127	10127
Number of villages	211	211	211	211
R^2	0.19		0.19	

Notes: Absolute value of standard errors below the coefficient estimates in parentheses. * indicates significance at 10% level; ** at 5% level and *** significant at 1% level of confidence. The omitted age group are children between 30 and 36 months of age. Standard errors corrected for clustering at the village level.

Table 6: Prospective analysis - Treatment impact for young and uneducated mothers on weight for age

	(1)	(2)
Second round	0.091*	0.095*
	(0.049)	(0.049)
Secondround * Planned treatment	-0.039	-0.045
	(0.090)	(0.067)
No edu female	-0.102**	
	(0.047)	
No edu * planned treatment	0.023	
	(0.085)	
Male child	-0.003	-0.003
	(0.030)	(0.030)
Twin	-0.756***	-0.746***
	(0.096)	(0.097)
Primary edu male	0.038	0.033
	(0.046)	(0.046)
Secondary edu male	0.173***	0.166***
	(0.061)	(0.062)
Husband edu missing	0.049	0.034
	(0.047)	(0.047)
Tapwater	0.036	0.036
	(0.042)	(0.042)
Watercloset	0.037	0.039
	(0.053)	(0.053)
Wealth index	0.008	0.007
	(0.013)	(0.013)
Teen mother		0.056
		(0.045)
Teenmother * planned treatment		0.140*
		(0.085)
Primary edu female		0.086**
		(0.042)
Secondary edu female		0.126
		(0.091)
Constant	-1.233***	-1.342***
	(0.053)	(0.033)
Observations	10127	10127
Number of villages	211	211
R^2	0.01	0.01

Notes: Absolute value of standard errors below the coefficient estimates in parentheses. * indicates significance at 10% level; ** at 5% level and *** significant at 1% level of confidence. The omitted age group are children between 30 and 36 months of age. Standard errors corrected for clustering at the village level.

Table 7: Retrospective analysis - Weight for age

	(1) Weight-for-age	(2) Mild malnutrition	(3) Weight-for-age	(4) Mild malnutrition
Second round	0.066	-0.039	0.062	-0.036
	(0.040)	(0.066)	(0.040)	(0.066)
Second round * planned intervention	0.112*	-0.205**	0.012	-0.113
	(0.062)	(0.097)	(0.093)	(0.163)
Age group 0-5 months	1.114***	-1.523***	1.040***	-1.428***
	(0.050)	(0.108)	(0.058)	(0.123)
Age group 6-11 months	0.283***	-0.329***	0.238***	-0.266***
	(0.047)	(0.089)	(0.053)	(0.103)
Age group 12-17 months	-0.455***	0.786***	-0.453***	0.769***
	(0.042)	(0.081)	(0.046)	(0.095)
Age group 18-23 months	-0.511***	0.801***	-0.529***	0.836***
	(0.052)	(0.085)	(0.057)	(0.099)
Age group 24-29 months	-0.309***	0.522***	-0.330***	0.552***
	(0.047)	(0.084)	(0.054)	(0.098)
Treatment * 0-5 months group			0.297***	-0.427 *
			(0.100)	(0.265)
Treatment * 6-11 months group	-	-	0.171*	-0.255
			(0.101)	(0.205)
Treatment * 12-17 months group	-	-	-0.016	0.073
			(0.099)	(0.181)
Treatment * 18-23 months group	-	-	0.060	-0.130
			(0.110)	(0.193)
Treatment * 24-29 months group	-	-	0.072	-0.109
			(0.101)	(0.190)
Male Child	-.019	.003	-0.019	0.002
	(.028)	(.047)	(0.028)	(0.047)
Twin	-0.726***	1.128***	-0.724***	1.127***
	(0.106)	(0.144)	(0.106)	(0.144)
Primary edu female	0.079**	-0.240***	0.082**	-0.242***
	(0.040)	(0.073)	(0.040)	(0.073)
Secondary edu female	0.066	-0.111	0.069	-0.111
	(0.092)	(0.159)	(0.091)	(0.159)
Primary edu male	0.011	-0.047	0.010	-0.046
	(0.042)	(0.080)	(0.042)	(0.080)
Secondary edu male	0.161***	-0.218**	0.160***	-0.217**
	(0.057)	(0.110)	(0.058)	(0.110)
Husband edu missing	0.018	-0.054	0.015	-0.051
	(0.042)	(0.080)	(0.041)	(0.080)
Tapwater	0.028	-0.018	0.028	-0.018
	(0.038)	(0.069)	(0.038)	(0.069)
Watercloset	0.046	-0.018	0.043	-0.017
	(0.052)	(0.098)	(0.052)	(0.098)
Wealth index	0.010	-0.005	0.010	-0.006
	(0.012)	(0.019)	(0.012)	(0.019)
Constant	-1.382***		-1.352***	

	(0.045)		(0.048)	
Observations	10127	10127	10127	10127
Number of villages	211	211	211	211
R^2	0.19		0.19	

Notes: Absolute value of standard errors below the coefficient estimates in parentheses. * indicates significance at 10% level; ** at 5% level and *** significant at 1% level of confidence. Age dummies are binary variables for six-months age groups of the child. Standard errors corrected for clustering at the village level.

Table 8: Retrospective analysis - Treatment impact for young and uneducated mothers on weight for age

	(1)	(2)
Second round	0.066	0.065
	(0.040)	(0.041)
Secondround * Planned treatment	0.075	0.104
	(0.087)	(0.064)
No edu female	-0.089**	
	(0.045)	
No edu * planned treatment	0.045	
	(0.083)	
Male child	-0.019	-0.019
	(0.028)	(0.028)
Twin	-0.725***	-0.727***
	(0.106)	(0.107)
Primary edu male	0.011	0.011
	(0.042)	(0.042)
Secondary edu male	0.160***	0.161***
	(0.056)	(0.057)
Husband edu missing	0.018	0.019
	(0.042)	(0.042)
Tapwater	0.028	0.029
	(0.038)	(0.038)
Watercloset	0.045	0.045
	(0.052)	(0.052)
Wealth index	0.010	0.010
	(0.012)	(0.012)
Teen mother		-0.024
		(0.044)
Teenmother * planned treatment		0.046
		(0.081)
Primary edu female		0.080**
		(0.040)
Secondary edu female		0.066
		(0.092)
Constant	-1.294***	-1.378***
	(0.062)	(0.046)
Observations	10127	10127
Number of villages	211	211
R^2	0.19	0.19

Notes: Absolute value of standard errors below the coefficient estimates in parentheses. * indicates significance at 10% level; ** at 5% level and *** significant at 1% level of confidence. The omitted age group are children between 30 and 36 months of age. Standard errors corrected for clustering at the village level.

Table 9: Variable definitions for regressions in Table 10

Variable name in regression	**Question from survey instrument**
Early liquid introduction	In the first three days after the birth of your child, did (s)he receive any other liquids than your breastmilk?
Colostrum	Do you think that one should give the baby the yellow liquid coming out of the breast before the normal milk arrives?
Worm drugs	Has your child (*name*) received drugs against worms in the last six months?
Bednets	Do you have malaria bednets in your household?
Malaria pills	During your pregnancy, have you taken any medication against malaria?
Vitamin A	Has your child in the last six months received a dose of vitamin A such as this one (*show the container*)?
Took iron during pregnancy	During your pregnancy, have you been given iron capsules or syrup containing iron?
Diarrhea	Has your child had diarrhea in the last two weeks?
Cough	Has your child suffered from a cough, at any moment, over the last two weeks?

Source: Translation of the survey instruments by the author.

Table 10: Prospective analysis - Behavioral Variables and Health Inputs

		Coefficient	S.E.
Behavioral Change			
Early introd. of liquids	Second round	-.859 ***	(.083)
	Planned Treatment	-.412 ***	(.108)
	# of obs.	10318	
	p-value	.000	
Should give colostrum	Second round	.548 ***	(.068)
	Planned Treatment	.508 ***	(.094)
	# of obs.	10283	
	p-value	.000	
Physical Health Inputs			
Worm drugs	Second round	.858 ***	(.108)
	Planned Treatment	.804 ***	(.143)
	# of obs.	9987	
	p-value	.000	
Bednets	Second round	1.310 ***	(.074)
	Planned Treatment	.190 *	(.099)
	# of obs.	10297	
	p-value	.000	
Malaria pills	Second round	-.109	(.084)
	Planned Treatment	.369 ***	(.115)
	# of obs.	10098	
	p-value	.000	
Vitamin A	Second round	-1.114 ***	(.066)
	Planned Treatment	.182 **	(.088)
	# of obs.	10328	
	p-value	.000	
Iron supplement	Second round	.323 ***	(.094)
	Planned Treatment	.310 **	(.127)
	# of obs.	9958	
	p-value	.000	
Disease Incidence			
Diarrhea	Second round	-.159 **	(.067)
	Planned Treatment	-.122	(.090)
	# of obs.	10328	
	p-value	.000	
Cough	Second round	-.266 ***	(.063)
	Planned Treatment	-.104	(.085)
	# of obs.	10328	
	p-value	.000	

Note: The results were derived using the same control variables as in Table 5 that are not presented for space reasons.