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Can agricultural interventions improve child health? Evidence from Tanzania

Anna Folke Larsen* and Helene Bie Lilleør†

Abstract

Severely reduced height-for-age due to undernutrition is widespread in young African children, with serious implications for their health and later economic productivity. It is primarily caused by growth faltering due to hunger spells in critical periods of early child development. We assess the impact on child health, measured as height-for-age, of an agricultural intervention that improved food security among smallholder farmers by providing these with a basket of new technology options. We find that height-for-age measures among children from participating households increased by about 0.8 standard deviation and the incidence of stunting among them reduced by about 17 percentage points.

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1 Introduction

Undernutrition is a key reason for poor child health in many developing countries. In Sub-Saharan Africa, around 40 per cent of children under the age of five suffer from stunted growth, i.e. severely reduced height-for-age relative to their growth potential (de Onis, Blössner and Borghi, 2011). Stunting is a result of periods of undernutrition in early childhood, and it has been found to have a series of adverse long-term effects in those who survive childhood. It is negatively associated with mental development (Martorell, 1999), with human capital accumulation (Jamison, 1986; Glewwe, Jacoby and King, 2001; Maluccio et al., 2009), adult health (Victora et al., 2008; Adair et al., 2013), and with economic productivity and income levels in adulthood (Hoddinott et al., 2008, 2013).[1]

It is by now well-established that height-for-age can be seen as a "summary indicator" of the health and development of children during the first 1,000 days of their lives, from conception to two years of age (Hoddinott et al., 2013). During this period children have very high growth rates and therefore, when subject to spells of growth faltering, children quickly fall behind the height-for-age growth curves of their peers, with limited chances of catching up subsequently (Victora et al., 2010).[2]

In this paper, we assess the impact on early childhood health, measured as height-for-age, of an agricultural intervention that improved food security in the lean season among smallholder farmers by providing these with a basket of new technology options. The intervention targeted smallholder farmers in Northern

[1] Although Vogl (2014) shows that a fair fraction of higher adult wages may be mediated by ocupational choice, better education and cognitive skills of taller workers.

[2] Although an opportunity window for catch-up may exist in the later puberty period, as recently shown by Hirvonen (2013).

Tanzania by organising farmer groups similar to the widespread Farmer Field School approach. On a common group plot, each group was trained in and experimented with a basket of agricultural and animal husbandry technology options based on locally available resources over a time horizon of three and a half years. Each farmer in turn then adopted his or her preferred technologies according to own needs and resources.

Roughly half of the participating households had children under the age of five years. To identify the impact of the agricultural intervention on early childhood health in terms of height-for-age, we employ a difference in difference comparison of cohorts conceived before and after the phase-in of the project, where only the the latter cohort lived all of their first 1000 days under full project implementation. The height-for-age data for the older cohort allow us to control for systematic differences in nutritional level between children in treatment and comparison households prior to the onset of intervention activities.[3]

Because stunting is widespread in developing countries and has serious long-term implications, its causes and potential prevention strategies have been subject to careful scrutiny. The prevention strategies focus on the nutrition of pregnant women, infants and young children. They include disease prevention strategies, breastfeeding practices, micronutrient supplements, food fortification, and food security strategies (Allen and Gillespie, 2001; Bhutta et al., 2008; Schroeder, 2008). The various authors all note that the evidence of the effectiveness of these strategies in preventing undernutrition is as mixed as the range of strategies itself. Although breastfeeding promotion and providing micronutrient supplements are effective strategies for reducing stunting, they cannot fully prevent stunting in food-insecure environments where the mother is undernourished or there are numerous deficiencies in micronutrients (Schroeder, 2008).

There is a general agreement in the nutritional literature that there is no

[3] This follows closely the identification strategy of Duflo (2003).

magic bullet for solving the undernutrition problem. Rather, it is believed that "to eliminate stunting in the longer term, these [nutritional] interventions should be supplemented by improvements in the underlying determinants of undernutrition" (Bhutta et al., 2008). It is argued that it is necessary to combine nutrition programs with income growth (Alderman, Hoogeveen and Rossi, 2006) and with broader food systems (Miller and Welch, 2013), and to focus more on the overall dietary quality, bringing local needs, cultural conditions and resource constraints into play (Schroeder, 2008) to achieve sustainable solutions to undernutrition. Recently, this has been stressed even more strongly by Ruel and Alderman (2013). They argue that nutrition-sensitive interventions or programs have enormous potential to improve nutrition without being nutrition-specific.[4] They review the nutritional potential of interventions and programs in four different sectors, including agriculture. They conclude that within agriculture in particular, the potential for positive nutritional impacts is great, because agricultural interventions can support livelihoods, increase food production and enhance access to diverse diets. They note, however that the empirical evidence is very scanty, largely due to the poor quality of evaluations.

A recent systematic review by Masset et al. (2012) focuses specifically on whether agricultural interventions, such as home gardens, animal husbandry, and the production of bio-fortified crops, all aimed at improving the nutritional status of children, actually succeeded in doing so. They find that although there is a positive effect on the production and consumption of the agricultural goods promoted, the impact on the overall diet is unclear, and very little positive evidence was found of an effect on the nutritional status of young children. How-

[4]Ruel and Alderman (2013) define nutrition sensitive interventions to be interventions or programs that address the underlying determinants of fetal and child nutrition and development, such as food security, whereas nutrition specific interventions are interventions or programs that address the immediate determinants of fetal and child nutrition and development, such as adequate food and nutrient intake by children, feeding, caregiving and parenting practices, and low burden of infectious diseases.

ever, Masset et al. stress that weak evaluation methodologies and lack of sufficient statistical power cast serious doubt on the validity of an overall and somewhat counterintuitive conclusion that there was a limited impact of the agricultural interventions on nutrition. They therefore call for more rigorous research on the subject in order to be able to answer the question of whether agricultural interventions can reduce undernutrition and therefore should play a more prominent role in the prevention of growth faltering among young children.

We contribute to this literature by providing a careful and rigorous impact assesment on height-for-age and stunting among young children of one such agricultural intervention. To use the terminology of Ruel and Alderman (2013) above, the intervention was nutrition sensitive in that it targeted food security broadly, but it was not nutrition specific. It promoted a more constant level of food security throughout the year by introducing perennial crops and improved breeds of livestock to help increase food availability during the lean season.

Using post-treatment data, we analyse whether the three-and-a-half-year-long intervention led to an improvement in the height-for-age measures among children young enough to have lived all their lives under the intervention. We measure this one year after the completion of the intervention. To identify the impact, we follow the identification strategy in Duflo (2003) and exploit the fact that the height-for-age measure is a strong biological marker of undernutrition in a well-defined age window, from conception to 24 months of life. The intuiton is the following. If the intervention indeed reduced spells of undernutrition or hunger among participating households, children conceived after the phase-in of the project should be taller for their age than their older peers who lived (part of) their first two years of life before the project could have had any impact on food security. However, there may have been a general change in the food security status of children during the project period. To control for this, we employ a cohort difference-in-difference strategy and compare the relative height differential between young children in participating and comparison households to the height differential between their older peers.

We find that young children from participating households on average experience a health improvement in that their standardized height-for-age measures increase by about 0.8 standard deviation. In addition, we do not only find improvements on average, but also in the lower tail of the height-for-age distribution. Looking at the prevalence rates of stunting, which is defined as having a height more than two standard deviations below the mean of a global reference distribution,[5] we find indications that prevalence rates dropped by 17.6 percentage points. Compared to the literature, these are sizable impacts and larger than most nutrition interventions, but comparable to nutritional impacts of cash-transfer programs. We show that improved food security in (severe) hunger periods is a likely mechanism behind this result. Furthermore, we examine our identifying common trend assumption and test our results against various alternative specifications and explanations and find that they are highly robust.

Overall, our findings suggest that agricultural interventions can in fact influence the underlying determinants of undernutrition to such an extent that they translate directly into children coming closer to their full growth potential. Although this is only *one* impact assessment of *one* agricultural intervention, and more rigorous impact assessments are needed to shape policy recommendations, our findings show that in the context studied it is possible to reduce early childhood stunting considerably through a broad nutrition-sensitive agricultural intervention.

[5]We use the international WHO growth standards,(WHO, 2006).

The remainder of the paper is organized as follows. In section 2, we describe the characteristics of the agricultural intervention in more detail, while the data and summary statistics are described in section 3. In section 4, we present our identification strategy, and in section 5, our main results and their robustness. In section 6, we examine our identifying assumption further, and in section 7 we conclude with a discussion of the relative magnitude of the impact and the project costs.

2 The agricultural intervention

The agricultural intervention is called "Rural Initiatives for Participatory Agricultural Transformation", or RIPAT.[6] The intervention we evaluate was the first RIPAT intervention implemented by a local NGO, RECODA, in eight villages in Arumeru District in the Arusha Region of Northern Tanzania between 2006 and 2009, see figure 2.1. Subsequently, another three similar RIPAT interventions have been implemented in nearby districts. The stated overall development goal of RIPAT is to reduce poverty and improve food security among smallholder farmers by facilitating high and sustainable levels of adoption of improved agricultural and livestock technologies disseminated through local farmer groups. The intervention has strong similarities with the Farmer Field School (FFS) approach as outlined in Van den Berg and Jiggins (2007), the main difference being that it implements a variety of technology options as opposed to one technology in FFS, combines both top-down teaching and participatory learning methods, and runs for three years with close follow-up as opposed to one agricultural season in FFS (Aben, Duveskog and Friis-Hansen, 2013).

Participation in RIPAT was not randomly allocated, which makes perfect

[6]See www.ripat.org or Lilleør and Lund-Sørensen (2013) for a thorough description and examination of the intervention.

sense from an implementation perspective, but which poses a challenge to the evaluation of the project. Poor villages with suitable agricultural conditions are selected at the district level. In the chosen villages, interested farmers (typically up to 70 in a village) are organised in farmer groups of 30-35 voluntary participations by the village council. In finding target participants, the village council is asked to select individuals who will be committed to the project (strict attendance records are typically kept), share their new knowledge with fellow villagers, and who are not rich in terms of the internal village wealth ranking. However, to facilitate individual technology adoption, participants must own at least one acre (and no more than five acres) of farm land.

Once the groups have been organised, the facilitators from the implementing NGO meet with the group on a weekly basis in the phase-in period. The first tasks of the group is to agree on a group constitution and elect group leaders. Each farmer group then has to rent an appropriate group field of around one acre of land, which can function as a demonstration plot, typically from a fellow farmer or the village community land. All group meetings are subsequently held at the group field.

The group is offered training in a full basket of technology options, which covers a broad range of local needs. The technology options include new banana cultivation techniques; new improved banana and other perennial and annual crop varieties; conservation agriculture for improved land utilization (such as minimum soil disturbance, cover crops, intercropping, rotation and diversification of crops); post-harvesting technologies; improved animal husbandry; multipurpose trees for fodder, fruit, or firewood; soil and water conservation including rain water harvesting; and savings groups. During a phase-in period of one year, the facilitators from the implementing NGO (typically agronomists) train the group members gradually in each of the technology options according to the agricultural seasons. After this period, the main role of the facilitators is to monitor and provide guidance on a bi-monthly or monthly basis.

The farmer groups are exposed to the full basket of options at the group field's demonstration plot, where the new techniques are implemented and compared with traditional methods with the guidance of the skilled facilitator. This reduces the individual risks involved in trying out or learning new technologies. Each farmer is free to choose which technologies to adopt on his/her own farm according to own needs, constraints and resources. Groups are given an initial set of inputs for free for the training, demonstration and testing of technologies on the group field, including improved breeds of roosters to cross-breed with local chicken. However, individual farmers wanting to adopt the new technologies must purchase inputs from the implementing NGO at cost prices. In the case of improved varieties of banana seedlings and goats, solidarity chains are implemented to promote local diffusion.[7] While some techologies may be more popular than others, the adoption varies considerably from farmer to farmer and often with a time lag.

In the area of study food insecurity is most pronounced during the lean season of the year, during the months leading up to the annual harvest of the main staple crop, maize. The project implementation started in the beginning of the growing season in 2006, and hence we expect the earliest impact on food insecurity to have taken place in the lean season of 2007. Children who are fully exposed to potential benefits of RIPAT are therefore defined to be those conceived in January 2007 or thereafter, see below.

Although the intervention was not nutrition specific, it was nutrition sensitive in its strong focus on achieving food security by promoting agricultural and livestock technologies, which were more drought resistant, more varied, and led to a smoother food production in level throughout the year. In Larsen and Lilleør (2014), we show that the intervention did in fact lead to improved food

[7] After the phase-in period once banana seedlings are available from the group plot the farmers can get free seedlings in exchange for passing on three times the number of the seedlings received to other farmers in or outside the farmer group. The farmer tending an improved variety of a she-goat can keep the goat after passing on the first female offspring to another farmer.

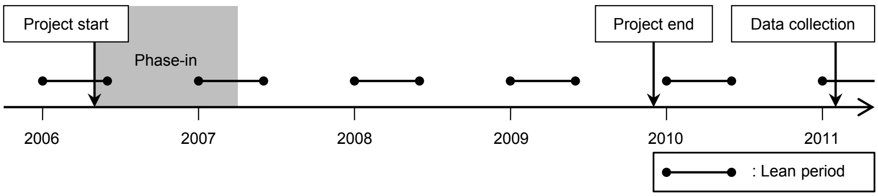


Figure 2.1: Time line

security levels among the full sample of participating households[8] in terms of reduced hunger during the lean season, higher intake of animal protein in terms of meat and eggs and more meals per day.[9] Based on the nutrition literature, we expect children who were exposed to RIPAT in-utero and the first two years of their lives to have benefited from this, as their physical growth is particularly sensitive to insufficient nutrition. In this paper, we therefore speak to the nutrition literature, when we investigate whether the subset of households with young children were in fact able to shield their growth from nutrition related set backs.

3 Data and summary statistics

Our main outcome variable in this paper is the height-for-age z-score for children, which is a very powerful indicator of severe early childhood or in-utero undernutrition, as described above. We construct height-for-age z-scores (HAZ) by subtracting the mean and dividing by the standard deviation of the age- and gender-specific lengths or heights from the reference distribution established in the WHO Multicentre Growth Reference Study based on healthy children from Brazil, Ghana, India, Norway, Oman and USA (de Onis et al., 2004). Though

[8] I.e. that sample also include households without young children as opposed to the sample of this paper.

[9] We also examined whether the intervention succeeded in alleviating poverty. Based on a broad range of single indicators and one composite poverty indicator, we did not detect any impact of RIPAT on poverty levels.

children below 24 months of age are measured recumbent, and hence we measure length rather than height, we henceforth refer to both length and height measures as height.

We also look at the prevalence of stunting using an indicator variable which equals one for those children whose height is less than 2 standard deviations below the age- and gender-specific mean.

3.1 Data

As indicated in the timeline in figure 2.1, we collected household level data more than one year after the project was completed.[10] We interviewed 506 of the 561 original RIPAT households from the eight intervention villages and 395 households from eight comparable non-intervention control villages in the same district.[11] The comparison households were sampled at random among farming households with one to eight acres of land.[12] Out of these 901 households, 469 of them have children aged five years or less, in total 645 children. We are able to construct Height-for-Age Z-scores for 482 children from 382 households. The main reason for attrition is that enumerators were not obliged to measure all children if some children were not present at the time of the interview .[13] The second most important reason for attrition is that not all parents knew the month

[10]In January 2011, we conducted a large scale quantitative household survey using a closed-form highly structured pilot-tested questionnaire to capture the impact of RIPAT on technology adoption, food security and poverty. The data collection and data entry was closely supervised by us in cooperation with a survey management team from the Economic Development Initiative (a Tanzanian survey company). RECODA assisted in the hiring of a team of local interviewers and data entry clerks. The data collection as well as the project implementation was financed by the Rockwool Foundation.

[11]The initial target was 12 comparison villages, but only eight villages in the district were comparable to RIPAT with respect to relevant characteristics, e.g. agriculture is the most important economic activity.

[12]During pilot testing of the survey, we became aware that some RIPAT participant did in fact hold more than five acres of land in 2011. To increase comparability, we therefore allowed households in control villages to to have up to eight acres of land. We control for acres in all the conditional estimations below and impose an acre restriction in the robustness section.

[13]They were required to measure at least one child per household with children below six years of age.

of birth of their child which is required to find the relevant height from the reference distribution to construct the HAZ. We disregard 14 child observations with missing values in the household characteristics and 11 child observations with an absolute HAZ larger than 5 standard deviations to avoid extreme outliers. Furthermore, following the convention in the literature (e.g. Bhutta et al. (2008); de Onis, Blössner and Borghi (2011); Masset et al. (2012)), we focus the analysis on children up to 60 months old, in order to avoid the influence of environmental factors on the heights of the children. This results in a final sample of 335 households with 396 children.

In addition, we interviewed 427 non-participating households in RIPAT villages for a study of diffusion of improved banana cultivation using a stratified random sample (Larsen, 2012).[14] From the households with young children, we have HAZ measures of 195 children which we use in section 6.1 as an alternative comparison group. We apply sampling weights to account for stratification. See table 11 in the appendix for an overview of the sample composition and different reasons for attrition. In section 5.1 below, we address the attrition in different ways. We show that results are robust to the use of a Heckman selection correction model to account for the fact that the probability of being measured may not have been random. Furthermore we show that results are also robust to the inclusion of outliers in HAZ and children aged 61-71 months.

3.2 Summary statistics

In table 1 we list the mean values of key child, parent, households, and village characteristics for the RIPAT households in column (1), and the corresponding values for the comparison households in column (2). In column (3) we present wild cluster bootstrap p-values from two-sided t-tests of whether the means dif-

[14]Non-participating households were therefore oversampled in villages with a larger degree of diffusion, and households growing improved bananas were sampled with a slightly higher probability than other households (see Larsen (2012) for details of the sampling scheme).

fer between RIPAT and comparison households, clustered at the village level.[15]

Looking at the characteristics of children in the sample, we see that the overall HAZ is about one standard deviation below the WHO reference population mean, indicating that they suffer from undernutrition in general. One in four children are stunted and although this appears to be a high prevalence, it is well below the regional stunting prevalence rate of 44 percent as found in the 2010 Demographic and Health Survey (DHS, 2010). This indicates that the children in our sample are somewhat better off than the regional average, possibly reflecting better socio-economic conditions as the area is reasonably fertile and in close proximity to Arusha town.

Slightly more than half of our sample are girls, and most are children of the household head. Their fathers are typically in their late 30s, while their mothers are around 30 years old. Both parents have between six and seven years of schooling, corresponding to having almost completed primary education. However, there is a tendency for the parents in RIPAT households, especially the mothers, to be older and slightly more educated than parents in comparison households.[16]

The children live in households with, on average, five other household members, these being fairly evenly distributed across the four age groups shown. In 2006, prior to the commencement of the RIPAT project, the households owned on average three to four acres of land. The math skills of the farmers interviewed were tested through two simple math questions; less than half answered both of them correctly. We have also included the average historical rainfall level at the household level,[17] since the households mainly rely on rain-fed agri-

[15] We use wild cluster bootstrap p-values for all inference in the paper because we only have 16 clusters (villages), and with few clusters the usual asymptotic theory does not apply (Cameron, Gelbach and Miller, 2008).

[16] When we have not been able to identify the parents, we have imputed the sample mean following Duflo (2003).

[17] We used interpolated data on yearly precipitation on a one-by-one kilometer grid measured in mm from the period 1950-2000 available from http://www.worldclim.org/. The rainfall data is matched to households using GPS coordinates.

culture. In accordance with the village selection criteria of suitable agricultural conditions, RIPAT villages receive more rain than the comparison villages. Both RIPAT households and RIPAT villages are more likely to have participated in a development project in the past than their comparison equivalents. However, these differences are not statiscally significant. The RIPAT villages are situated further away from the main local market and they are less likely to have a secondary school, and although these differences are insignificant they suggest that the program allocation procedure targeted more remote and wet villages.

From table 1 it is thus clear that there are some differences in observables between participating and comparison households, although only few of these are significant at a conventional level. We return to these below. It is, however, still important to account for these characteristics in the analyzes below to increase comparability.

4 The identification strategy

The participation selection process at both village and individual level suggests that more motivated farmers from poorer villages are likely to be project participants. Furthermore, no baseline data were collected prior to the intervention, and therefore we cannot rely on standard difference-in-difference estimates to establish counterfactual outcomes. We want to identify the impact of household participation in RIPAT on the nutritional status of children measured by their height-for-age z-score (HAZ). To find an unbiased estimate of the average treatment effect, we therefore need to account for project placement and self-selection. We do so by employing the identification strategy of Duflo (2003), which exploits the fact that height is a stock variable reflecting accumulated nutrition and infections since conception.

This identification strategy relies on the findings in the medical literature that the in-utero period and the first two years of life are critical periods for childhood development. The length of new-born infants and the height of young children is considered to be more responsive to the nutritional intake than the height of older children (Martorell and Habicht, 1986; Ruel, 2001), and stunting at birth or in early childhood is found to be a strong predictor of later childhood stunting (Adair, 1999; Saleemi et al., 2001). Thus, because stunting is persistent, the HAZ of older children represents reliable recall data, as it is a biological marker of their past nutrition in early childhood (Hoddinott et al., 2013; Victora et al., 2010). We exploit this fact to identify the impact of RIPAT with a difference-in-difference estimator: the HAZ difference between young RIPAT and comparison children conceived after the phase-in of the project, net of the difference for the older children. The difference in height-for-age of the older children captures any systematic differences in nutritional status between RIPAT and comparison children before a potential impact of the project. That is, it captures nutritional-level differences due to the non-random selection and thereby accounts for the selection into the project.

In other words, the idea of the identification strategy is to estimate whether children who were conceived after project phase-in are taller for their age than their older peers who were conceived earlier, relative to a similar cohort difference between younger and older children from comparison households. The young RIPAT children will be fully exposed to potential benefits of the project during the first critical 1,000 days of their lives, while the older RIPAT children will only be partly exposed or not at all. The difference in their HAZ can be assigned to RIPAT after accounting for general time variation in nutrition and infections by deducting the HAZ difference between young and old comparison children. The identifying assumption is that—in absence of treatment—the height-for-age of treated and comparison children would follow a common growth profile.[18] We capture a growth profile curvature by control-

[18] This corresponds to the common trends assumption in a classical difference-in-difference set-up.

ling for age in months quadratically. Our results could be misleading if the growth profiles differ between treated and comparison children in absence of treatment. Below we therefore investigate whether there were any confounding time-varying differences between participating and comparison households, such as changes in fertility patterns or different coping abilities in times of drought, see section 6.

We estimate the average treatment effect of RIPAT with ordinary least squares (OLS) using the specification in equation (1).

$$Y_i = \beta_1 RIPAT_h + \beta_2 young_i + \beta_3 RIPAT_h \cdot young_i + C_i\delta + P_i\phi + X_h\eta + W_v\gamma + \varepsilon_i \quad (1)$$

Y_i is the outcome for child i in household h in village v. The variable $RIPAT_h$ indicates whether household h has ever participated (i.e. including those that dropped out) in a RIPAT farmers' group; $young_i$ indicates whether child i is younger than a certain threshold described below; and $RIPAT_h \cdot young_i$ gives the interaction between the two latter variables. Thus, β_3 will give the estimate of the average treatment effect of RIPAT on the nutritional status of young children, net of selection. We control for child characteristics, denoted as C_i, parent characteristics, P_i, household characteristics, X_h, and village characteristics, W_v, all of which are listed in table 1. Age in months is included quadratically. We take the logarithm of acres of land owned in 2006. Finally, we allow for errors to be correlated within villages, $\varepsilon_{i,v}$.

We have a small subsample of households with measurements of both young and old siblings. This allows us to also provide estimates with household fixed effects instead of parent, household and village characteristics as a simple robustness check.[19]

There is some flexibility in how we define the relevant threshold for the *young* dummy as it depends on when we can expect an impact of RIPAT on

[19]We do not include parent characteristics in the fixed effects regressions, as there is naturally very little variation within households.

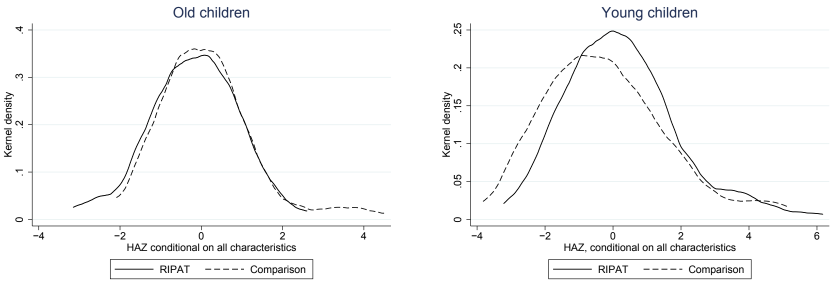


Figure 5.1: Distributions of the HAZ

food security in the households. Food insecurity in this area is highly seasonal and only pronounced in the lean seasons (January to May).[20] This implies that the earliest time we can expect an impact on nutrition of pregnant women and young children is in the first lean season after project start, January-May in 2007. Hence, we define the *young* dummy such that it is equal to one for children conceived in January 2007 or later (henceforth referred to as "young children"). Regardless of the choice of threshold, some children classified as *old* may also be affected by the improved nutrition. If there is any such catch-up growth, it will lead to an underestimation of the impact. We return to the choice of threshold in section 5.1.

5 Results

Before turning to the estimation results we compare the distributions of HAZ presented in figure 5.1 for the old and young children separately. We have conditioned on child, parent, household and village characteristics to reduce noise. We see that the conditional distribution of HAZ for the old RIPAT children is closely aligned to that of old comparison children, suggesting that these children are indeed highly comparable. For the young children the RIPAT distribution is

[20]We define the span of the lean season according to self-assessment of the households in the sample. The majority of households mention the months January-May as part of the "worst period in terms of having enough food for everyone in your household [during 2010]".

clearly shifted to the right of the comparison distribution. Obviously, this graphical inspection does not consitute a formal test, however it does suggest that not only are the young RIPAT children taller for their age than the comparison children *on average*, but it appears that the intervention has affected the entire HAZ distribution of young RIPAT children, in particular the lower tail.

Table 2 shows OLS estimation results for the average treatment effect of RIPAT using the econometric specification given in equation 1. Column (1) to (3) present estimated impacts on the height-for-age z-score (HAZ) of young children in participating households, hence the impact on the mean value of the HAZ distribution. To analyze whether RIPAT affects the lower part of the distribution and decreases the prevalence of severe undernutrition, we also provide estimates for the impact on the likelihood of being stunted in column (4) using the linear probability model. The coefficient to the *RIPAT* and *young* interaction term gives an estimate of the average treatment effect of RIPAT on the HAZ or the probability of being stunted among the younger children who grew up under the influence of RIPAT. In column (1) we show the unconditional estimates, in column (2) we control for child, parent, household, and village characteristics, and in column (3) we allow for household fixed effects.

The unconditional estimate of the impact of RIPAT on HAZ is an average improvement of 0.57 standard deviations (SD) of the WHO reference distribution. When we control for child, parent, household, and village characteristics, the estimate of the impact increases to 0.88 SD. This means that young children in RIPAT households are 0.88 SD taller than their peers in comparison households, controlling for any pre-project differences among the older children. When we include household fixed effects to account for unobserved household characteristics, the point estimate further increases to 1.38 SD. The fact that we still find a positive impact after the introduction of household fixed effects suggest that the results are not driven by unobserved differences in the selection into the project between households with young and old children. However, fixed effect estimation relies on variation in a relatively small subset of the sample, as only 21 RIPAT households and 19 comparison households have both young and old children in the sample and we therefore only include it as a robustness check of the conditional estimates.

Because RIPAT is a village intervention, we cluster standard errors at the village level and the corresponding significance levels are reported with the costumary use of stars. However, since we only have 16 villages and thus 16 clusters, the standard asymptotic theory cannot be applied for inference. We therefore also report p-values in square brackets based on wild cluster bootstrapped t-statistics for the impact coefficients as suggested by Cameron, Gelbach and Miller (2008).

Turning to the impact on stunting in column (4), we see that the average impact on heigh-for-age also translates into an impact in the lower part of the HAZ distribution, as suggested by figure 5.1. Compared to children in control villages, we find that young RIPAT children experience a reduction in the stunting prevalence of 17.6 percentage points, significant at the ten per cent level. We have less statistical power compared to our results for HAZ, since we discard information by reducing the continuous HAZ to a binary variable. However, this does suggest that the nutritional improvements also reach children in the lower parts of the height-for-age distribution who suffer from severe undernutrition.

When we measure the impact of RIPAT on HAZ, we measure the impact on a nutritional stock (height). We expect RIPAT to affect the stock through improvements in the nutritional flows generated from the ongoing agricultural production.[21] This suggests that the effect of RIPAT on height-for-age should increase with the duration of exposure to RIPAT. The longer children are exposed to improved nutrition, the more the impact accumulates in their stock, i.e.

[21] Instead of measuring the impact of RIPAT on the height-for-age of the children, it would be more direct to measure the impact on the nutritional intake of the children in every period. However, it is difficult to collect diary data with precise measurements of calorie and micronutrient intakes, so it is convenient to use the height-for-age as a simple summary measurement of the nutritional status of the child.

their height. On the other hand, we are analyzing the impact of an agricultural project that was gradually phased in, and for which there was a lag from the onset of new project activities to a tangible nutritional outcome from the fields or the livestock. Children born early in the project period therefore receive a weaker nutritional improvement during their first 1000 days, than children born later when new agricultural technologies are potentially adopted, and this factor works in the opposite direction.

In table 3 we present estimates from a model that allow for cohort-specific impacts: instead of a young indicator we include age indicators for the years zero to three, along with the RIPAT indicator and their interaction terms. Four-year-old comparison children then form the reference group. Overall, the impact is driven by the one- and two-year-olds. They both have estimated impacts on the HAZ of 1 SD (see column 2) which suggests that the accumulation of impact we expected to see in the two-year-old children is offset by the gradual impact of the project. We see no significant impact on the youngest children potentially because the differences in nutritional intakes between RIPAT and control children (and their mothers while they are in utero) are not yet detectable in the height measure. The three-year-old children belong to the group of *old* children and as expected, there is no significant difference between the three- and four-year-old RIPAT cohorts relative to the comparison cohorts.[22] The latter result thus supports our common growth profile assumption for treated and comparison groups prior to any impact.

Mechanisms

Although we cannot pin down the exact channel through which RIPAT has influenced the nutritional status of young children, we can examine the most likely chain of events, namely whether our sub-sample of RIPAT households are more

[22] Actually, the young threshold is 39 months corresponding to three years and three months, so 22 of 90 three-year-old children are considered *young* in main analysis.

likely to have adopted the variety of technologies provided through the basket of options and whether they are also more likely to be food secure.

Elsewhere we examined the impact of RIPAT on food security and poverty for the full sample of RIPAT households using a gradual roll-out of the project into a nearby district to account for selection, (Larsen and Lilleør, 2014). We found robust impacts on a variety of food security measures, but no impact on poverty. In this paper, we focus on the sub-sample of households with young children, and assess the impact on their height-for-age with an identification strategy that is only valid for this particular outcome. To confirm that high levels of adoption and food security are also more pronounced for the sub-sample of RIPAT households with young children, we therefore resort to the simple cross-sectional comparison.[23]

In table 4 column (1) and (2), we show the average adoption rates (and standard deviations) for RIPAT and comparison households for six of the central technology options and the number of different crops cultivated in 2010 to capture crop diversification. To test the difference in adoption between RIPAT and comparison household we regress the technologies on a RIPAT indicator and household and village characteristics.[24] Column (3) presents estimates for the RIPAT indicator with cluster standard errors in parentheses and wild cluster bootstrap-t p-values in square brackets. We see that there are high rates of technology adoption. Our RIPAT households are significantly more likely to be growing improved banana varieties, to be raising improved breeds of chickens and goats, to practice zero-grazing among their livestock by keeping these in

[23]In Larsen and Lilleør (2014), we estimate the average treatment effect using both simple cross-sectional comparisons between treatment and control, matching estimators and a difference-in-difference estimator exploiting the gradual roll-out. The findings are reasonably robust across estimation methods, suggesting that selection into the project is not a major driver of results. We are therefore confident that when employing simple cross sectional comparisons on this subsample, it will give a good indication of whether there also is increased adoption and improved food security levels in the sub-sample of RIPAT households with young children.

[24]We include age and education of the household head instead of that of the parents.

smaller enclosures and feeding them, to participate in savings groups,[25] and to have a larger degree of crop diversification. We find no significant difference in the adoption of fruit trees. It is worth noting that both perennial crops (like banana) and improved livestock technologies (poultry providing eggs and meat, and milking-goats providing milk) enhance production smoothing over the agricultural cycle and thereby also help facilitate the smoothing of food consumption over the year.

The fact that RIPAT farmers practice zero-grazing among their livestock by keeping these in smaller enclosures may potentially reduce the disease enviroment of young children, since they will be less exposed to animal excrements. Similarly, in our data we can see that RIPAT households are more likely to have a roof over their pit-latrine as recommended by RIPAT facilitators (along with village and government officials), and this will also reduce the spread of bacteria through flies. A reduced exposure to potential disease environments could therefore be another channel through which the children's growth and thus height-for-age is positively affected (Bhutta et al., 2008; Adair et al., 2013).

The higher levels of adoption of the agricultural and animal husbandry technologies, should in turn lead to higher levels of food security. In table 5 column (1) and (2), we list RIPAT and comparison household means (and standard deviations) for eleven different outcome measures of food security, and in column (3) we show RIPAT regression coefficients from regressions of the food security measures on a RIPAT indicator and household and village characteristics as in table 4.

We find that RIPAT households experience a significantly shorter hunger season than comparison households. When asked about the worst period in terms of having enough food during the last 12 months, RIPAT households report an 11 percent shorter period than comparison households, ceteris paribus.

[25] Although later RIPAT projects (RIPAT 2-4) actively use Village Savings and Loans Associations as a one of the basket options, membership in external savings groups was simply encouraged in RIPAT 1, which we study here.

Similarly, we see that RIPAT households are 16 percentage points less likely to experience any hunger during the past 12 months before the interview. We measure hunger using the Household Hunger Scale (HHS)[26] using three different reference periods: the last four weeks, and the worst and the best month during the last 12 months. We see that hunger is reduced by 0.6 on the HHS (corresponding to 32 percent) during the lean season (worst month), while there is no significant impact on the level of food security in the best period of the year or the four weeks preceeding the interview, where the prevalence of hunger is relatively low. Next, we look at whether the children in the household have at least three meals per day in the best and worst period of the year as well as in the last four months. The coefficients are all positive, and in particular the estimated difference for the worst month is large where we also see the largest room for improvement. However, once the small number of clusters are taken into account, there is not enough power to yield statistically significant results. This is also the case, when analysing whether the households consume meat, eggs or dairy products, all sources of animal protein.[27]

All in all this suggests that the postive impact on the height-for-age of young RIPAT children is likely to run through higher levels of technology adoption promoting higher levels of food security in the lean season of the year. Not being exposed to hunger spells seems to have long lasting consequences on the growth curves of these young children, this may be reinforced by less exposure to animal- and excrement related bacteria. We have also examined whether RIPAT households have lower poverty levels than the comparison households,

[26]The HHS is a modern food security instrument developed by US Aid to ensure cross-cultural comparability. It has been validated in five sub-Saharan African countries. It is based on three questions asking whether, due to lack of resources, anyone in the household 1) went to sleep at night hungry; 2) had no food to eat of any kind in the household; and 3) went a whole day and night without eating. The response codes are 0: never; 1: rarely or sometimes; 2: often. The HHS is simply the sum of the responses to the three questions resulting in an index from zero to six where zero corresponds to "no hunger" and six corresponds to "severe hunger". See Ballard et al. (2011).

[27]In the full sample we find statistically significant impacts on almost all of these measures, (Larsen and Lilleør, 2014).

but find no clear evidence of such differences (see appendix table 12). This suggests that RIPAT did not bring a large income effect with it, but rather that the main impact runs through better smoothing mechanisms shielding households and not least children against hunger in the lean season.

5.1 Robustness of results

In analyses like the one in this paper, one worry whether the results are driven by systematic errors or data decisions. In this section we therefore analyze whether our results are robust to accounting for attrition, to different thresholds of the young indicator and to changing the sample selection with respect to child age, number of acres, outliers and data quality considerations.

5.1.1 Attrition

Not all children living in the surveyed households were measured. If there are systematic differences in which children are measured across RIPAT and comparison households, it could potentially affect our results. We address this issue with a Heckman selection model (Heckman, 1979) where we exploit the variation in enumerator meticulousness as instrument for the probability of being in the sample with an appropriate HAZ measure. The 25 enumerators were instructed to measure at least one child in each household of zero to five years of age, preferably all available children. The instrument is constructed as the share of children the enumerator measures in other households, not including the household in question.[28] In that way, the instrument is unaffected by household specific characteristics that determines whether a child is measured or not and as we can see from panel B in Table 6 it is highly correlated with the probability of being measured. Estimation results are represented in table 6.[29]

[28]This share varies between 0.40 and 0.95.

[29]Alternatively, we can also just include enumerator dummy variables as instruments. In that case, we cannot obtain convergence of the maximum likelihood estimator, but we obtain similar results to the ones presented in table 6 if we apply a two-step estimator instead with

In panel A the estimates for the impact of RIPAT on HAZ are shown when selection in measurement is corrected for. We see that the results are robust and not driven by selection in who is being measured, as we find a large and significant impact of RIPAT just 0.1 standard deviations lower than the corresponding results presented in table 2.

Panel B shows the estimates from the selection equation and here we see no significant differences between RIPAT and comparison households in the likelihood of being measured both for young and for old children. Young children are more likely to be measured than old, probably because they are more likely to be around at the time of the interview.

5.1.2 Threshold for the young indicator

Next, we turn to the choice of threshold for the *young* indicator. We expect some lag from the introduction of new agricultural methods on the common demonstration plot to a change in the agricultural practices of the households and a subsequent improvement in the food security of the household. Following this reasoning, the threshold of the *young* indicator should be later in time than January 2007. On the other hand, with a conception threshold in January 2007, all children born before October 2007 are classified as *old* even though they lived the main part of their first two critical years of life after the implementation of RIPAT. This would speak in favor of an earlier threshold. In table 7 we show results where we move the threshold between May 2006, the start of RIPAT, and January 2008, the second lean season after the start of RIPAT. All estimated impacts are within the confidence bounds of their counterparts in table 2 and apart from column (5) they are all statistically significant at the ten percent level. The latest threshold (January 2008) results in the lowest and least significant impact. This estimate will be downward biased if children born before October 2008 are affected by the project, which may very well be the case. It is reassuring that

these alternative instruments. Results available upon request.

the positive impact found is not specific to a certain choice of threshold for the *young* indicator.

5.1.3 Alternative sample selections

In the main analysis we consider children up to 60 months of age which is common practice in the nutrition literature. As we have height measures for children up to 71 months of age, we investigate whether the results are robust to the inclusion of these older children. The results are shown in table 8 column (1), and we see in panel A that the estimated impact on HAZ is reduced to 0.6 standard deviations and significantly different from zero at the ten percent level. Similarly, the estimated impact on the stunting indicator (shown in panel B) is reduced and statistically insignificant, but still economically significant with an estimate of 11 percentage points reduction in the prevalence of stunting.

Though RIPAT participants are required to have between one and five acres of farm land, our data shows that this requirement has been violated in many cases. Comparison households have been chosen among households with one to eight acres to mirror the actual distribution of farm land holdings among RIPAT households, however 19 RIPAT households and 4 comparison households report to have less than one or more than eight acres of land. In column (2) of table 8 we exclude the 28 children from households with farm land acres of out range. In panel A, this results in a stronger estimated impact on HAZ of 0.9 standard deviations significant at the one percent level. This result indicates that the impact of RIPAT is higher among households targeted for the intervention which is intuitive.[30] The estimated impact on the prevalence of stunting is unaltered.

When we calculate the height-for-age z-scores, a few observations have very extreme values. In the main sample we have disregarded children with a HAZ larger than five in absolute value (11 observations). Column (3) shows that we

[30]Results are very similar if we further restrict the sample to only include the 268 households, which have between 1-5 acres of land.

obtain even stronger results if we include outlier observations. It is particularly noteworthy that the estimated impact on the prevalence of stunting is higher and more significant than the main result. The stunting indicator is not affected by large outliers in HAZ so for this specification it can indeed be argued that the outlier observations should be included.

In the last two columns we consider the data quality of the HAZ. Children below 24 months of age should be measured recumbent while children above 24 months of age should be measured standing. Not all enumerators have followed these guidelines,[31] so in column (4) of table 8 we present regression results excluding children measured in the opposite position for their age. We obtain rather similar results to those in table 2, however the wild bootstrap p-values suggest that the estimated impacts on HAZ and the prevalence of stunting cannot be distinguished from zero.

The calculation of the HAZ is based on the age of the child, and there might be uncertainty about parents' recall of their children's birth dates. For a sub-population of children we have their birth date confirmed by an official clinic card, and regression results in column (4) show that the results are still significant and within one standard error of the estimates in table 2 when we consider this sub-population.

In general, the impact of RIPAT on height-for-age is fairly robust to the selection of the sample with results ranging from 0.6 to 1.2 standard deviations, all but one significant at the ten percent level. Though we have less power when we consider the prevalence of stunting, we also consistently find large impacts of participation in RIPAT.

[31] If a child was measured recumbent though older than 24 months or *vice versa*, we adjust the measurement by 0.7 cm, in accordance with WHO guidelines, (WHO, 2006).

6 Possible alternative explanations

Our identification strategy relies on the standard assumption of treatment and control groups sharing a common trend in the absence of treatment. In our setting this translates into an assumption that children from RIPAT and comparison households would share a common growth profile in the absence of treatment. That is, our difference-in-difference set-up allows for differences in child nutrition *levels* between RIPAT and comparison households, but not for differences in *trends* or time-varying differences not caused by the intervention. If such differences exist, our results could be misleading. We study three potential factors which could lead to differences in the growth profiles, namely time-varying difference at between RIPAT and control villages, differences in fertility patterns between RIPAT and comparison households, and differences in their coping capabilities in times of drought.

6.1 Village differences

In our main analysis above we compare children in RIPAT households with children in comparison households in control villages. If the two groups of villages were differentialy exposed to shocks, e.g. there was a serious drought in 2009 which could have hit the comparison villages harder than the treated villages, or vice versa, our impact estimates may be confounded. We address this issue by comparing the RIPAT children to other children *within* the RIPAT villages who do not live in participating households. The data we have from a stratified random sample of non-RIPAT households within RIPAT villages allows us examine whether the estimated impacts on HAZ and stunting found above are in fact driven by time-varying village level differences. If so, we should expect to see no difference in nutritional levels between children from RIPAT and non-RIPAT households within the RIPAT villages. Nevertheless, we have to keep in mind that there has been considerable diffusion of technologies within the

RIPAT villages (e.g. 13 percent of non-RIPAT farmers in RIPAT villages grow improved banana varieties, see Gausset and Larsen (2013)). A comparison between households within the RIPAT villages may therefore underestimate an impact.

Table 9 corresponds to table 2 above only using children from non-RIPAT households in RIPAT villages as a comparison, rather than children from comparison villages. Again standard errors are clustered at village level (note that now there are only eight villages) and p-values based on wild cluster bootstrapped t-statistics are shown in square brackets. Furthermore, we have added an additional column allowing for village fixed effects, column (4). The estimated impact on HAZ is much in the same order of magnitude as in table 2 and appears slightly more robust across specifications, but the small number of clusters affects the bootstrapped p-values and we have less power. The estimated impact on the stunting indicator increases to a 26.7 percentage point reduction in stunting. It is reassuring that we find the same positive impact regardless of comparison group. This rules out the possibility that the estimates are driven purely by differences in village-level shocks.

6.2 Fertility patterns

Could project participation itself lead to endogenous changes in fertility patterns and thus in cohort composition among the participating relative to comparison households, such that the estimated impact found above is a result hereof?

First of all, if RIPAT induces households to have fewer children, that would imply that the household would have more resources per child, which could potentially lead to an improvement in the nutritional status of the children born. However, since we control for the number of household members between zero and five years of age, this cannot be the mechanism for the impact we find.

Second, if participation in the project changes the timing of fertility, it could potentially affect the group composition of old and young RIPAT children vis a vis the comparison children. Table 1 shows, that the group of RIPAT children are on average slightly older (three months) than the group of comparison children. As the HAZ trends downwards for undernourished children, the difference-in-difference estimate will be upward biased if *young* RIPAT children are on average younger than *young* comparison children or if *old* RIPAT children are older than *old* comparison children. We have tested whether the average age within the old and young group is correlated with RIPAT, and we find no significant correlation. We further test the composition of the age cohorts by regressing age indicators on a RIPAT indicator, while controlling for household and village characteristics. Coefficients and wild cluster bootstrap confidence bounds for the RIPAT indicator are presented in figure 6.1 for each age indicator. None of the age groups are significantly under- or overrepresented among the children in RIPAT households.[32]

Third, if the project affected timing of conception over the year, RIPAT children might have been differently exposed to the lean season relative to the comparison children, which again could affect our results. Hence, we run twelve regressions with month of birth indicators as dependent variables using the same specification as in equation 1.[33] With this difference-in-difference specification we test if potential differences in the seasonal timing of fertility between old RIPAT and comparison children persist among the young children. If the seasonal pattern has changed remarkably, it could be driving the results. As can be seen in figure 6.2, the only significant difference we find is that young RIPAT children are less likely than young comparison children to be born in November relative to any differences among their older peers. For this difference to be driving our results it should be very unfavorable to be born in November as compared to other months of the year.[34] Our results are robust to excluding children born

[32]This also holds with OLS confidence intervals not accounting for clusters.

[33]All children, household and village characteristics are included except the child's age.

[34]It is difficult to hypothesize whether a child is better off being born in November compared

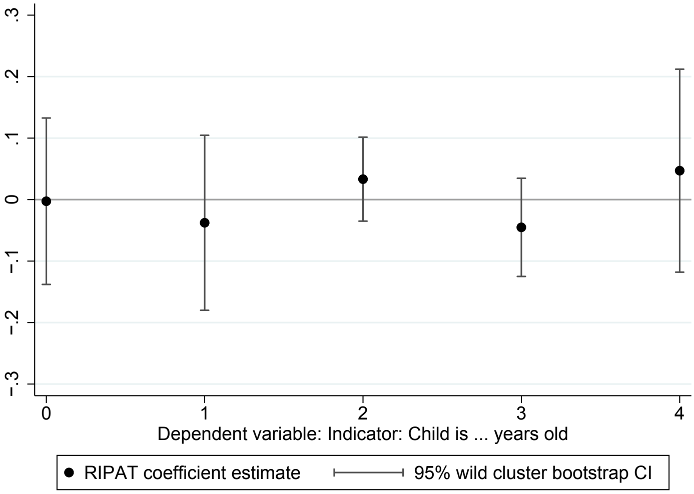


Figure 6.1: Cohort composition

Notes: OLS estimates and wild cluster bootstrap-t confidence intervals for the coefficient to the RIPAT indicator in regressions with age indicators for age 0, 1, 2, 3, and 4 years as dependent variables. The regressions also control for household and village characteristics. The wild bootstrap-t procedure is clustered at the village level following Cameron, Gelbach and Miller (2008) and confidence intervals are constructed by finding the highest possible null hypothesis (from below) that is rejected at the five percent level and imposing symmetry.

in November (results available upon request) and hence, we do not expect this small difference to be driving the large impact that we find.

Taken together, this suggests that the positive impacts found on height-for-age using the cohort difference-in-difference estimator are not driven by changes in fertility patterns or cohort composition during the project period.

6.3 Capabilities for coping with drought

Finally, the common growth profile assumption would also be violated if the RIPAT and comparison households were subject to different shocks or coped with

to June, say. The former is exposed to the lean season during first trimester in utero, while the latter is exposed during second and third trimester. The timing of the weaning period will also be different and this may also play a role.

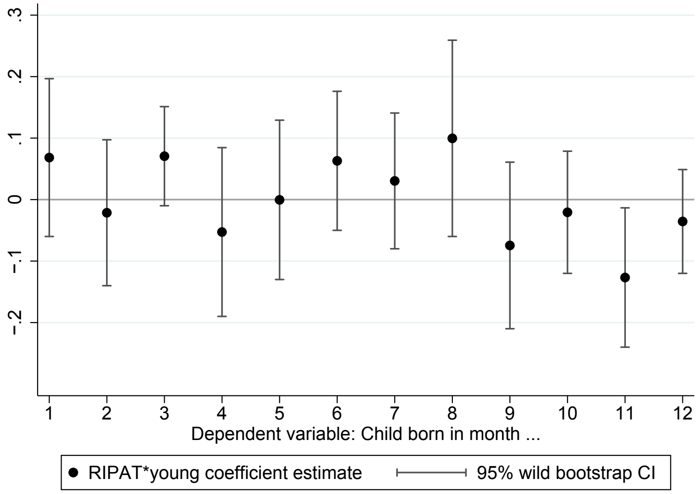


Figure 6.2: Seasonality in fertility

Notes: OLS estimates and wild cluster bootstrap confidence intervals for the coefficients for the RIPAT and young interaction terms in regressions where the dependent variables are indicators for the month when the child was born. 1 corresponds to January, 2 is February, and so forth. The regressions also control for a RIPAT indicator, a young indicator, and child, parent, household, and village characteristics, excluding the age of the child. Wild cluster bootstrap-t confidence intervals are constructed as in figure 6.1. We do not correct for multiple hypothesis testing.

a common shock in different ways, regardless of project participation. Above we showed that we can reject the possibility that our main results are driven by a difference in *village*-level shocks, since we obtain similar results when using comparison children from RIPAT villages as opposed to control villages.

With respect to coping with shocks at the *household* level, we should keep in mind that RIPAT aims at reducing vulnerability to drought shocks by introducing drought-resistant crops and production-smoothing technologies, so we should in fact expect that RIPAT households would have become better at coping with drought shocks. But we need to address the concern that households who select into RIPAT may *initially* have different coping strategies than the comparison households. Coupled with the drought in 2009, this could potentially drive the impacts that we find.

We address this potential selection bias in three ways. First, we investigate whether the impact is driven by any of the observed differences in parent and household characteristics between RIPAT and comparison households in control villages. Table 1 shows that parent characteristics differ significantly between RIPAT and comparison households in terms of father's and mother's age. Furthermore mother's education, which is often a strong predictor of children's health, is also marginally different with children in RIPAT households having more educated mothers. If, say, older or better-educated mothers were better at nourishing their children during the 2009 drought, we would overestimate the impact, since RIPAT mothers are on average better educated.

We demean these key parental variables and interact them with the *young* indicator, the *RIPAT* indicator and their interaction term respectively, to allow for the treatment effect to depend on, for example, mother's age. This results in the following specification where Q_h represents one of the demeaned parent or household characteristics, $Z_{i,h,v}$ comprises all child, parent, household and village characteristics and $\zeta_{i,v}$ is an error term with intra-village correlation:

$$\begin{aligned} Y_i &= \mu_1 RIPAT_h + \mu_2 young_i + \mu_3 RIPAT_h \cdot young_i + \mu_4 RIPAT \cdot Q_h \\ &+ \mu_5 young \cdot Q_h + \mu_6 RIPAT \cdot young \cdot Q_h + Z_{i,h,v}\vartheta + \zeta_{i,v} \qquad (2) \end{aligned}$$

When we allow for the relative difference between young and old children to depend on the age of the mother, the average impact estimate captured by μ_3 is unaffected by any possible influence of RIPAT mothers' age on the nutrition of their young children during the drought spell in 2009.

Table 10 shows estimates of equation (2) with interactions with parental variables in column (1)-(3). The estimate of the mean impact of RIPAT relative to comparison children in control villages is remarkably stable acress these columns confirming that the impact found above is not driven by any of the differences in observed parent characteristics. However, we see that the impact is negatively correlated with father's age indicating that younger households benefit more from RIPAT than older households do.

Second, using the same method, we examine whether three of the selection criteria could be driving the results. Households self-selected into the project, but had to fulfill a land ownership criteria. Villages were partly chosen based on suitable agricultural conditions, including sufficient rainfall. We interact the difference-in-difference variables with three variables capturing this selection: Historical rainfall, log of farm land acres in 2006 and we proxy for self-selection using participation in other projects in the past. From table 10, we see that differences in land ownership (column (4)) or prior participation in other projects do not alter the estimated impact of RIPAT on the HAZ of young children. However, turning to rainfall in column (6), we see that part of the impact of RIPAT on HAZ is driven by a positive interaction with rainfall, reducing the average effect of RIPAT on the HAZ to 0.77 SD. Per extra milimeter of historical rainfall the impact of RIPAT is increased by 0.01 SD of the HAZ. Given that the majority of the technology options also rely on adequate rainfall, especially in the phase-in period, this is not a surprising finding.

Third, selection into the project could still be based on intrinsic *unobserved* differences in strategies for coping with shocks between participating and comparison households. Due to the drought in 2009, such differences could potentially lead to the improvements in height-for-age that we find for the young RIPAT children. However, intrinsic differences in strategies for coping with drought between RIPAT and comparison households should then also be detectable when comparing the HAZ of children exposed to an earlier drought spell. To measure weather shocks, we follow Harari and La Ferrara (2013) and examine monthly Standardized Precipitation and Evapotranspiration Indices (SPEIs) for the geographical area under study, using the average of the four preceding months and considering values of the SPEI below one SD as negative climate shocks. We consider March to June to be the main growing season based on the Food and Agriculture Organization crop calendar.[35]

Figure 6.3 shows SPEIs for the period 2004 to 2011 with three data points per year: the four-month average SPEI for the growing season March-June and similarly for the non-growing seasons July-October and November-February.[36] It can clearly be seen from figure 6.3 that the growing season in 2009 was particularly dry. But we also see that the area was hit by a drought during the growing season in 2006.

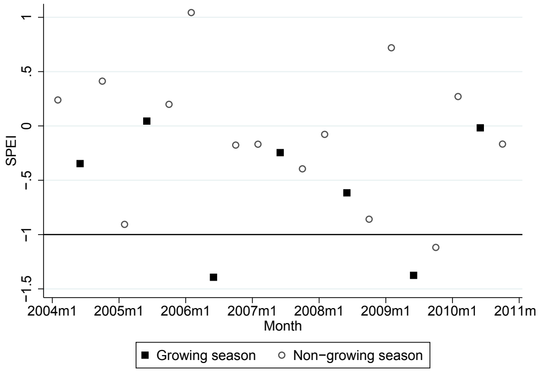
This implies that if RIPAT and comparison households initially had different coping strategies, we should expect to see differences in the HAZ of children conceived just before or during 2006. These are precisely the children we define as old, and where we find no significant difference in their height-for-age between RIPAT and comparison children. Thus, we argue that the improved nutrition among the young RIPAT children cannot be driven by differences in drought coping strategies across treated and comparison households *a priori*. On the contrary, we propose that RIPAT farmers had improved their ability to cope with the 2009 drought through the adoption of drought-resistant crops and production-smoothing technologies. The magnitude of our estimated average treatment effect on HAZ might therefore have been considerably smaller if the area had experienced years of bumper harvest and thus little food insecurity and no hunger spells. In that sense, the 2009 drought has increased the degree of variation in our data, enabling us to identify a larger impact on nutrition.

7 Discussion

In this paper, we have estimated the impact on early childhood nutrition of an holistic agricultural intervention aimed at improving food security and poverty

[35]http://www.fao.org/agriculture/seed/cropcalendar/welcome.do

[36]The graph is from a grid covering half of the villages in our sample; the graph from the neighboring grid covering the remaining villages is very similar and is available from the authors. The global SPEI database can be found at http://sac.csic.es/spei/database.html.



Source: The global SPEI database

Figure 6.3: Standardized Precipitation and Evapotranspiration Index

among smallholder farmers. Given the widespread prevalence of severe undernutrition resulting in stunted growth and the relatively recent acknowledgement of its many long-term adverse implications, combating undernutrition of unborn and infant children has become a very important subject that attracks attention from both researchers and policy-makers, e.g. recent Lancet reviews by Bhutta et al. (2008); Victora et al. (2008); Ruel and Alderman (2013) and the Cost Of Hunger in Africa report by African Union Commission et al. (2014).

The RIPAT intervention studied here is an agricultural intervention. It did not have a direct nutritional aim, but rather an overall aim of improving food security: a nutrition-sensitive intervention in the terminology of Ruel and Alderman (2013). It is a broad intervention with a strong focus on improving drought resilience through a basket of technology options including crop diversification, perennial crops, conservation agriculture, improved animal husbandry, and land use management. This holistic approach may have been key in improving the nutritional status of young children in the participating households, as it is argued elsewhere that these components help improve the nutritional quality of farming output, (Miller and Welch, 2013). We find that the RIPAT intervention had a significant positive impact of about 0.8 SD on the height-for-age z-scores of young children who had been fully exposed to the project in their early life. Similarly, we see a reduction in stunting prevalence among the young RIPAT children of around 17 percentage points.

There are two important points to note concerning these impacts. First, they were measured almost five years after the start of the project, which lasted three and a half years, suggesting that these are sustainable impacts, but not necessarily quick impacts. Second, towards the end of the project implementation period, a serious drought hit the area worsening and lengthening the annual hunger period. This has possibly increased the difference in undernutrition levels found between participating and comparison households, since the intervention was designed to increase the drought resistance of farmers and shield their food production, rather than to boost agricultural output during bumper years.

According to Masset et al. (2012) and Ruel and Alderman (2013), there has been no rigorous empirical investigation showing a significant nutritional impact of an agricultural intervention among young children. Compared to impacts found of more narrow non-agricultural nutritional interventions in Bhutta et al. (2008) and Caulfield, Huffman and Piwoz (1999), the impacts of the RIPAT intervention on HAZ and stunting prevalence are sizable.[37] More recent papers by Linnemayr and Alderman (2011) and Powell-Jackson et al. (2014) find no overall effect of a randomized nutrition program in Senegal or a randomized free health care program in Ghana, although the former do detect a positive impact among the youngest on weight-for-age z-scores of 0.27 SD (and note that their find similar impacts on HAZ).

Based on the limited impacts found in the nutrition literature, one could suspect that height-for-age in itself is a rigid measure hard to influence. However, there is ample evidence from conflict-prone areas in Africa, which show that

[37]Bhutta et al. (2008) report that the provision of food supplements in populations with insufficient food can increase the HAZ by 0.41 SD, while Caulfield, Huffman and Piwoz (1999) review efficacy trials to improve infant dietary intakes and find improvements in HAZ of 0.04-0.46 SD.

this is by no means the case. Two different African conflicts have been shown to have a negative impact on HAZ of about 0.4 SD among young children exposed to the conflict, (Akresh, Lucchetti and Thirumurthy, 2012; Minoiu and Shemyakina, 2014). In addition, Baez (2011) shows that also children who are not directly exposed to conflict can be negatively affected (their HAZ drops by 0.6 SD) by a large and sudden influx of poor refugees into the local communities.

In fact, the magnitude of our results is comparable in size to large-scale cash transfer programs. The nutritional impacts of RIPAT on young children are comparable to those found by (Duflo, 2003) in assessing the impact on young grand-daughters of extending a generous public old-age pension scheme to low-income families in South Africa. Duflo finds that this increased the HAZ of young girls in the household by more than one standard deviation if the recipient was the grandmother. Similarly, when analyzing the impact on childhood nutrition of a large-scale conditional cash transfer program aimed at increasing both health and education among Mexican children, PROGRESA, Behrman and Hoddinott (2005) find that the prevalence of stunting drops to a third of the level among comparison children.

Such an impact on HAZ does of course not come without a cost. Indeed, the cost per household of the RIPAT intervention is also comparable to PROGRESA, which has an annual cost of approximately USD 300[38]. The total cost per household for the 3.5-year RIPAT intervention studied here was USD 700.[39] Although roughly USD 200 per year per family may be a relatively high cost - and considerably higher than the average Farmer Field Schools cost[40] - it must be judged against the benefits found here in terms of improved food security and

[38]The annual budget was USD777 million covering 2.6 million families, (Behrman and Hoddinott, 2005)

[39]This was the first in a series of interventions, which have gradually become more cost-effective. The cost per household is now USD 625 for a full three-year project.

[40]Van den Berg and Jiggins (2007) and Waddington, White and Anderson (2014) report most FFS costs to be between USD20-40 per farmer.

taller children. Based on the findings in the nutrition literature regarding the adverse impact of stunting on health, economic and social outcomes in adulthood, this positive impact is likely to follow the young RIPAT children through life. In addition, we expect the adopted technologies to sustain the improvement in food security into the future, also positively affecting children to come.

All in all, this shows that a broad and highly-sustainable agricultural intervention such as the one studied here building on local resources, needs and constraints and offering a basket of technology options for farmers to choose from, can result not only in the sustainable technology adoption and increased food security among farmers, but also in substantial long-term impacts on the lives of the young children in participating households. Indeed, there are reasons to believe that exactly because of the holistic nature of the intervention and its focus on shielding farmers' food production against adverse impacts of drought, the nutrional and thus growth impacts on young children are sizable and larger than those typically found in more narrow nutrition interventions as reviewed in Bhutta et al. (2008) and Caulfield, Huffman and Piwoz (1999). As hypothesized by both Masset et al. (2012) and Ruel and Alderman (2013), our study confirms that there is scope for agricultural interventions in alleviating undernutrition and that they can indeed be very effective.

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Tables

Table 1: Summary statistics

		RIPAT	Comparison	P-value
Outcome variables	Height-for-Age Z-score	-0.94	-1.05	0.59
		(1.66)	(1.66)	
	Stunting indicator	0.25	0.27	0.65
		(0.44)	(0.45)	
Child characteristics	Young indicator	0.61	0.65	0.26
		(0.49)	(0.48)	
	Age in months	34.11	31.20	0.11
		(15.36)	(15.52)	
	Girl	0.57	0.52	0.19
		(0.50)	(0.50)	
	Child of head	0.83	0.87	0.45
		(0.37)	(0.33)	
Parent characteristics	Father's education	6.78	6.53	0.25
		(1.68)	(1.67)	
	Father's age	39.12	36.99	0.02
		(8.10)	(8.25)	
	Mother's education	6.70	6.08	0.12
		(1.50)	(2.66)	
	Mother's age	31.85	28.67	0.00
		(7.17)	(6.70)	
Household characteristics	Household size	6.20	5.95	0.40
		(2.01)	(1.99)	
	HH members age 0-5	1.58	1.60	0.90
		(0.78)	(0.66)	
	HH members age 6-14	1.61	1.66	0.80
		(1.20)	(1.25)	
	HH members age 15-24	0.98	0.84	0.34
		(1.03)	(1.00)	
	HH members age 25-49	1.63	1.58	0.49
		(0.66)	(0.67)	
	Head is widow(er)	0.06	0.03	0.14
		(0.24)	(0.18)	
	Acres 2006	4.07	3.11	0.19
		(5.32)	(1.79)	
	Good in math	0.41	0.42	0.86
		(0.49)	(0.50)	
	Participation in other projects	0.27	0.16	0.14
		(0.44)	(0.37)	
	Household rain	738.67	706.91	0.21
		(47.86)	(45.64)	
Village characteristics	Village distance to market	9.88	5.76	0.14
		(3.90)	(5.00)	
	Village has secondary school	0.57	0.86	0.29
		(0.50)	(0.35)	
	Village had devel. project	0.60	0.41	0.52
		(0.49)	(0.49)	
Number of children		214	182	
Number of households		182	153	
Number of villages		8	8	

Notes: Variable means in samples of RIPAT and comparison children. Standard deviations in parentheses. Column 3 gives wild cluster bootstrap-t p-values from two-sided t-tests of equal means of the RIPAT and comparison children, calculated as suggested by Cameron, Gelbach and Miller (2008). Clustering is at the village level.

Table 2: Impact of RIPAT on HAZ

	HAZ			Stunting
	(1)	(2)	(3)	(4)
RIPAT and young	0.569*	0.879***	1.377***	-0.176*
	(0.29)	(0.29)	(0.43)	(0.09)
	[0.062]	[0.012]	[0.004]	[0.094]
RIPAT	-0.240	-0.215		0.090
	(0.20)	(0.24)		(0.06)
Young	-0.025	-0.133	-0.302	0.060
	(0.11)	(0.30)	(0.71)	(0.09)
Child characteristics	No	Yes	Yes	Yes
Other characteristics	No	Yes	No	Yes
Household fixed effects	No	No	Yes	No
Clusters (villages)	16	16	16	16
Observations	396	396	396	396

Notes: OLS estimates with HAZ as dependent variable, cluster standard errors in parentheses, and wild cluster bootstrap-t p-values in square brackets. 'Other characteristics' include parent, household, and village characteristics as described in the text. Statistical significance based on standard inference is indicated by ***, **, and * for the 1, 5, and 10 percent levels respectively.

Table 3: Cohort specific impacts on HAZ

	(1)	(2)
RIPAT and age 0	-0.237	0.274
	(0.65)	(0.67)
	[0.666]	[0.700]
RIPAT and age 1	0.666	1.097*
	(0.49)	(0.53)
	[0.198]	[0.064]
RIPAT and age 2	0.473	1.012**
	(0.46)	(0.40)
	[0.282]	[0.018]
RIPAT and age 3	-0.332	0.106
	(0.34)	(0.43)
	[0.388]	[0.786]
RIPAT	-0.042	-0.179
	(0.29)	(0.35)
Age 0	0.681	-0.366
	(0.52)	(1.37)
Age 1	-0.096	-0.751
	(0.30)	(0.88)
Age 2	-0.200	-0.663
	(0.30)	(0.54)
Age 3	0.107	-0.102
	(0.31)	(0.44)
All characteristics	No	Yes
Clusters	16	16
Observations	396	396

Notes: OLS estimates with HAZ as dependent variable, cluster standard errors in parentheses, and wild cluster bootstrap-t p-values in square brackets. 'All characteristics' include child, parent, household, and village characteristics as described in the text. Statistical significance based on standard inference is indicated by ***, **, and * for the 1, 5, and 10 percent levels respectively.

Table 4: Adoption of technologies

	(1) RIPAT	(2) Comparison	(3) Conditional difference
Improved banana cultivation	0.657	0.121	0.523***
	(0.476)	(0.327)	(0.103)
			[0.030]
Fruit tree(s)	0.590	0.497	0.232*
	(0.493)	(0.502)	(0.120)
			[0.322]
Improved breed of poultry	0.309	0.013	0.243***
	(0.463)	(0.115)	(0.055)
			[0.032]
Improved breed of goats	0.354	0.128	0.227***
	(0.480)	(0.335)	(0.044)
			[0.006]
Zerograzing	0.275	0.242	0.230**
	(0.448)	(0.430)	(0.083)
			[0.080]
Savings scheme	0.191	0.040	0.149***
	(0.394)	(0.197)	(0.027)
			[0.024]
Number of crops in 2010	5.551	4.852	0.791*
	(2.538)	(2.126)	(0.390)
			[0.072]
Number of households	178	149	327

Notes: Variable means in samples of RIPAT and comparison children and standard deviations in parentheses in column (1) and (2). Column (3) presents OLS estimates from regressions of the technology on a RIPAT indicator, cluster standard errors are in parentheses, and wild cluster bootstrap-t p-values are in square brackets. Regressions also control for education and age of the household head and household and village characteristics as described in the text. Statistical significance based on standard inference is indicated by ***, **, and * for the 1, 5, and 10 percent levels respectively.

Table 5: Food security

	(1) RIPAT	(2) Comparison	(3) Conditional difference
Number of worst months	3.831	4.150	-0.438***
	(1.338)	(1.445)	(0.135)
			[0.038]
No hunger	0.365	0.265	0.159***
	(0.483)	(0.443)	(0.050)
			[0.036]
HHS, worst month	1.494	1.789	-0.566**
	(1.427)	(1.415)	(0.205)
			[0.070]
HHS, best month	0.062	0.048	-0.034
	(0.304)	(0.270)	(0.034)
			[0.532]
HHS, last four weeks	0.281	0.306	-0.067
	(0.680)	(0.679)	(0.164)
			[0.764]
At least three meals, worst month	0.708	0.667	0.163
	(0.456)	(0.473)	(0.110)
			[0.390]
At least three meals, best month	0.955	0.932	0.061*
	(0.208)	(0.253)	(0.029)
			[0.236]
At least three meals, last four weeks	0.904	0.878	0.068*
	(0.295)	(0.329)	(0.038)
			[0.246]
Meat consumption last week	0.764	0.694	0.183**
	(0.426)	(0.462)	(0.085)
			[0.192]
Egg consumption last week	0.607	0.408	0.152**
	(0.490)	(0.493)	(0.068)
			[0.180]
Dairy consumption last week	0.843	0.810	0.086
	(0.365)	(0.394)	(0.128)
			[0.664]
Number of households	178	147	325

Notes: Variable means in samples of RIPAT and comparison children and standard deviations in parentheses in column (1) and (2). Column (3) presents OLS estimates from regressions of the food security variables on a RIPAT indicator, cluster standard errors are in parentheses, and wild cluster bootstrap-t p-values are in square brackets. Regressions also control for education and age of the household head and household and village characteristics as described in the text. Statistical significance based on standard inference is indicated by ***, **, and * for the 1, 5, and 10 percent levels respectively.

Table 6: Impact on HAZ, Heckman selection model

	(1)	(2)
Panel A: Regression equation		
RIPAT and young	0.492	0.784***
	(0.33)	(0.22)
RIPAT	-0.072	-0.105
	(0.22)	(0.26)
Young	-0.234	-0.354*
	(0.23)	(0.21)
Panel B: Selection equation		
RIPAT and young	-0.191	-0.094
	(0.25)	(0.28)
RIPAT	0.182	0.119
	(0.18)	(0.23)
Young	0.172	0.697**
	(0.17)	(0.32)
Share measured in other households by same enumerator	1.084**	1.663***
	(0.46)	(0.59)
All characteristics	No	Yes
Clusters (villages)	16	16
Observations	535	535

Notes: Maximum Likelihood estimates from a Heckman selection model, and cluster standard errors in parentheses. Panel A gives the estimates from the regression equation with HAZ as dependent variable when controlling for selection in measurement. Panel B gives the estimates from the selection equation. The instrument in the selection equation is the share of children measured in other households by the enumerator. The sample consist of children zero to four years of age out of which 73 percent are measured. When indicated 'All characteristics' are included in both the regression and selection equation. They include child, parent, household, and village characteristics corresponding to the specification in table 2, with two modifications: 1) we include the age and education of the household head instead of parents characteristics as we can only identify parents of measured children; 2) we include age emphin years instead of age in months as we do not have precise age of unmeasured children. For the same reason, the threshold for the young dummy is adjusted to three years of age (36 months) instead of three years and three months (39 months). Statistical significance is indicated by ***, **, and * for the 1, 5, and 10 percent levels respectively.

Table 7: Changing threshold of the *young* indicator

	Young threshold, month of conception				
	(1)	(2)	(3)	(4)	(5)
	May 06	Sep 06	Jan 07	Jul 07	Jan 08
RIPAT and young	0.880**	0.788*	0.879***	0.587**	0.478
	(0.402)	(0.393)	(0.285)	(0.275)	(0.289)
	[0.048]	[0.078]	[0.012]	[0.050]	[0.144]
RIPAT	-0.336	-0.207	-0.215	0.042	0.163
	(0.32)	(0.31)	(0.24)	(0.25)	(0.23)
Young	-0.700	-0.422	-0.133	-0.488	-0.668**
	(0.44)	(0.44)	(0.30)	(0.30)	(0.31)
All characteristics	Yes	Yes	Yes	Yes	Yes
Clusters (villages)	16	16	16	16	16
Observations	396	396	396	396	396

Notes: OLS estimates with HAZ as dependent variable, cluster standard errors in parentheses, and wild cluster bootstrap-t p-values in square brackets. The column headings refer to the threshold of the young dummy where children conceived in or after the month mentioned are coded as young. 'All characteristics' include child, parent, household, and village characteristics as described in the text. Statistical significance based on standard inference is indicated by ***, **, and * for the 1, 5, and 10 percent levels respectively.

Table 8: Alternative sample selections

	(1) Incl. older	(2) Acres	(3) Outliers	(4) Position	(5) Clinic card
Panel A: Outcome variable: HAZ					
RIPAT and young	0.602*	0.942***	1.171***	0.652*	0.604**
	(0.31)	(0.28)	(0.31)	(0.34)	(0.25)
	[0.090]	[0.008]	[0.004]	[0.112]	[0.024]
RIPAT	-0.047	-0.256	-0.103	0.009	-0.241
	(0.23)	(0.21)	(0.28)	(0.28)	(0.28)
Young	-0.150	-0.193	-0.357	0.100	0.135
	(0.29)	(0.35)	(0.31)	(0.37)	(0.29)
Panel B: Outcome variable: Stunting indicator					
RIPAT and young	-0.107	-0.171*	-0.219**	-0.140	-0.155*
	(0.09)	(0.08)	(0.09)	(0.13)	(0.07)
	[0.266]	[0.070]	[0.042]	[0.336]	[0.064]
RIPAT	0.037	0.095*	0.102*	0.069	0.149**
	(0.05)	(0.05)	(0.05)	(0.08)	(0.06)
Young	0.015	0.044	0.096	0.020	-0.027
	(0.08)	(0.10)	(0.10)	(0.13)	(0.13)
All characteristics	Yes	Yes	Yes	Yes	Yes
Clusters	16	16	16	16	16
Observations	457	368	406	328	307

Notes: OLS estimates with HAZ as dependent variable in panel A and the stunting indicator in panel B. In parentheses are cluster standard errors, and in square brackets are wild cluster bootstrap-t p-values. The columns represent different sample selections compared to the main sample: (1) includes children 61-71 months old; (2) excludes children from households with less than one or more than eight acres; (3) includes outliers in HAZ; (4) excludes children measured recumbent when older than 24 months and measured standing when younger than 24 months; and (5) excludes children whose month of birth could not be validated by a clinic card. 'All characteristics' include child, parent, household, and village characteristics as described in the text. Statistical significance based on standard inference is indicated by ***, **, and * for the 1, 5, and 10 percent levels respectively.

Table 9: Impact on HAZ and likelihood of stunting with weighted RIPAT village comparison sample

	HAZ				Stunting
	(1)	(2)	(3)	(4)	(5)
RIPAT and young	0.832*	0.788	0.710*	0.834*	-0.267**
	(0.43)	(0.43)	(0.36)	(0.44)	(0.09)
	[0.104]	[0.138]	[0.078]	[0.474]	[0.056]
RIPAT	-0.129	-0.226		-0.257	0.083*
	(0.31)	(0.28)		(0.31)	(0.04)
Young	-0.287	-0.547	-0.588	-0.550	0.185
	(0.31)	(0.35)	(1.37)	(0.35)	(0.16)
Child characteristics	No	Yes	Yes	Yes	Yes
Household characteristics	No	Yes	No	Yes	Yes
Village characteristics	No	Yes	No	No	Yes
Fixed effects	No	No	Household	Village	No
Clusters (villages)	8	8	8	8	8
Observations	409	409	409	409	409

Notes: OLS estimates using comparison sample within RIPAT villages weighted with inverse sampling probabilities. Column headings refer to the dependent variable. In parentheses are cluster standard errors, and in square brackets are wild cluster bootstrap-t p-values. 'Household characteristics' include parent characteristics. Statistical significance based on standard inference is indicated by ***, **, and * for the 1, 5, and 10 percent levels respectively.

Table 10: Heterogeneous impacts on HAZ

	(1)	(2)	(3)	(4)	(5)	(6)
Q:	Father's age	Mother's education	Mother's age	Log acres 2006	Prior project participation	Historical rainfall
RIPAT and young	0.796**	0.892***	0.781**	0.901***	0.832**	0.773***
	(0.279)	(0.273)	(0.287)	(0.261)	(0.296)	(0.240)
	[0.018]	[0.004]	[0.032]	[0.006]	[0.026]	[0.016]
RIPAT, young and Q	-0.078***	0.015	-0.024	-0.379	-0.749	0.012***
	(0.025)	(0.127)	(0.044)	(0.458)	(0.595)	(0.003)
	[0.024]	[0.902]	[0.544]	[0.394]	[0.286]	[0.014]
RIPAT	-0.196	-0.219	-0.177	-0.232	-0.170	-0.128
	(0.241)	(0.239)	(0.216)	(0.235)	(0.219)	(0.194)
RIPAT and Q	0.024	-0.123	-0.026	0.163	0.958**	-0.012**
	(0.019)	(0.080)	(0.029)	(0.278)	(0.431)	(0.005)
Young	-0.077	-0.173	-0.069	-0.133	-0.066	-0.163
	(0.318)	(0.289)	(0.311)	(0.280)	(0.290)	(0.301)
Young and Q	0.065***	0.026	0.027	-0.004	0.253	-0.002
	(0.014)	(0.049)	(0.035)	(0.420)	(0.312)	(0.002)
Q (not demeaned)	-0.051**	0.004	0.034	0.003	-0.325*	0.003
	(0.019)	(0.057)	(0.027)	(0.225)	(0.179)	(0.003)
All characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Clusters (villages)	16	16	16	16	16	16
Observations	396	396	396	396	396	396

Notes: OLS estimates, cluster standard errors in parentheses, and wild cluster bootstrap-t p-values in square brackets. Q refers to the variable stated in the column heading; the variable is demeaned when it enters an interaction term, but not when included in levels. "All characteristics" include child, parent, household, and village characteristics as described in the text. Statistical significance based on standard inference is indicated by ***, **, and * for the 1, 5, and 10 percent levels respectively.

A Appendix

Table 11: Sample composition

		RIPAT	Comparison	Total	RIPAT village
Number of households	Total interviewed	506	395	901	427
	with children 0-5 years of age	254	215	469	239
	with at least one child with HAZ	208	174	382	195
	In final sample	182	153	335	171
Number of children	Total in interviewed households	344	301	645	329
	No HAZ	76	87	163	91
	Don't know month of birth	19	23	42	13
	Either parent or child refused	13	16	29	14
	Not all children measured in household	29	35	64	35
	Other reasons	15	13	28	29
	with HAZ	268	214	482	238
	with missing values in characteristics	9	5	14	6
	with $\|HAZ\|>5$	7	4	11	10
	older than 60 months	38	23	61	27
	In final sample	214	182	396	195

Notes: The table shows how the final sample of households and children used in the analysis is composed and the different reasons for attrition. The last column gives the numbers for households in RIPAT villages not participating in the RIPAT project (unweighted).

Table 12: Poverty

	(1) RIPAT	(2) Comparison	(3) Conditional difference
PPI	40.611	41.185	5.094*
	(13.733)	(12.710)	(2.733)
			[0.265]
Good quality floor	0.272	0.305	0.042
	(0.446)	(0.462)	(0.099)
			[0.825]
(Mobile) phone	0.728	0.795	0.031
	(0.446)	(0.405)	(0.107)
			[0.855]
Number of households	180	151	331

Notes: Variable means in samples of RIPAT and comparison children and standard deviations in parentheses in column (1) and (2). Column (3) presents OLS estimates from regressions of the poverty variables on a RIPAT indicator, cluster standard errors are in parentheses, and wild cluster bootstrap-t p-values are in square brackets. The "Progress out of Poverty Index" (PPI), as developed by Schreiner (2012), captures the probability that a household falls below the national poverty line. The PPI is country-specific and is based on ten simple questions that together provide a statistically strong and simple predictor of whether a household's consumption level is likely to be below the national poverty line as established in the 2007 Tanzanian Household Budget Survey. The PPI score ranges from 0 (most likely to be below a poverty line) to 100 (least likely to be below a poverty line). The PPI regression also controls for age and gender of household head, log acres 2006 and village characteristics, while the floor quality and phone regressions control for education and age of the household head and household and village characteristics as described in the text. (The household characteristics included in the PPI regression differ because some of the household characteristics enter the PPI calculation and hence they can not be used as covariates). Statistical significance based on standard inference is indicated by ***, **, and * for the 1, 5, and 10 percent levels respectively.