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Beyond the Field: The Impact of Farmer Field Schools on Food Security and Poverty Alleviation

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Summary. — We estimate the impact of a Farmer Field School intervention among small-scale farmers in northern Tanzania on two main development objectives: food security and poverty. We employ a series of evaluation methodologies, including a Quasi-Difference-in-Difference setup, to account for potential selection into the project, despite lack of baseline data. We find strong positive effects on food security, but no effect on poverty. Investigating possible mechanisms for this result shows that reallocation of labor resources toward own agricultural production and improved production smoothing may have led to improved food security while poverty remained unaffected.

Key words — impact assessment, Farmer Field Schools, food security, poverty, Tanzania, Africa

1. INTRODUCTION

The majority of poor households in developing countries rely on subsistence agriculture for their own food production and as a source of income. Over the past few decades, various initiatives have been taken aimed at increasing food production by closing the technology gap faced by subsistence farmers. Such initiatives have worked either directly, through the supply of new technologies such as fertilizer, seeds of improved plant varieties, or new animal breeds, or more indirectly, through agricultural extension and advisory services, or both (Anderson & Feder, 2007; Lunduka, Ricker-Gilbert, & Fisher, 2013; Rawlins, Pimkina, Barrett, Pedersen, & Wydick, 2014).

Agricultural extension has long been seen as a key element in improving agricultural development. However, the effectiveness of two dominant approaches to agricultural extension services in particular—Training and Visit (T&V) [1] and Farmer Field Schools (FFS) [2]—has been widely debated. The T&V approach relies on the "top-down" extension of technical information, with specialists and field staff transferring knowledge to "contact farmers" in villages, who in turn are responsible for diffusing knowledge into the local community. As a response to this top-down approach, FFS were developed as a "bottom-up" approach to extension with a focus on participatory, experiential, and reflective learning to improve the problem-solving capacity of farmers through highly trained facilitators working with farmer groups (Anderson & Feder, 2007). In this paper, we assess the impact on food security and poverty of an intervention which seeks to combine both the top-down and bottom-up approaches and which has been implemented among smallholders in northern Tanzania. The intervention, locally known as RIPAT (Rural Initiatives for Participatory Agricultural Transformation), is designed as a modified FFS approach taking its starting point in farmer groups and experiential learning, but with a strong element of traditional technology transfer through the introduction of a "basket" of new technology options. We find that RIPAT has had a large impact on food security, but no detectable impact on poverty.

FFS have been implemented and adopted worldwide (Braun, Jiggins, Röling, Van den Berg, & Snijders, 2006). Nonetheless, the ability of the approach to ensure both sustained technology adoption and increased productivity is still subject to an ongoing debate about appropriate evaluation methodologies, when to evaluate, and choice of outcome measures (Braun & Duveskog, 2011; Feder, Murgai, & Quizon, 2008; Feder, Anderson, Birner, & Deininger, 2010; Davis & Nkonya, 2008; Mancini & Jiggins, 2008; van den Berg & Jiggins, 2008). More recently, a thorough survey of FFS impact studies was provided by Davis, Nkonya, Kato, Mekonnen, Odendo, and Miiro (2012, Table 1), highlighting the fact that the outcomes selected for examination are very mixed, as are the findings. While some papers find positive impacts on adoption, agricultural yields, productivity, and agricultural income, others do not. Most papers studying the impact on various aspects of empowerment find that empowerment increases, which has led to an argument being advanced that FFS is more a model of adult learning than of agricultural extension (Friis-Hansen & Duveskog, 2012; van den Berg & Jiggins, 2007).

The debate in the FFS evaluation literature was initially sparked by Feder, Murgai, and Quizon (2004) criticizing earlier FFS evaluation methodologies for not taking the potential positive bias of non-random program placement and selection of participants into account in their assessments of impact. This led to discussions of evaluation timing and problems of spillover effects. Measuring outcomes using a relatively long time horizon, as Feder *et al.* (2004) do, allows for an assessment of impact sustainability—unless the estimated impact is

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confounded by spillovers from FFS graduates to control farmers living nearby, as suggested by van den Berg and Jiggins (2007, 2008) but proven by Yamazaki and Resosudarmo (2008) not to be the case using the same data as Feder *et al.* (2004).

The best way to obtain an unbiased estimate of impact would be to conduct a randomized controlled trial, but to our knowledge, this has not been done for FFS yet. Given non-random program placement, a few papers, including Godtland *et al.* (2004), Rejesus, Mutuc, Yasar, Lapitan, Palis, and Chi (2012), Davis *et al.* (2012), and Todo and Takahashi (2013), do attempt to take this selection factor into account in a careful manner. However, all four of these studies suffer from having relatively small sample sizes (ranging from 142 to 486 within each country), which may have resulted in no significant impact being found simply due to lack of statistical power, and from operating with a very short time horizon (one to two years since project start). They therefore have to assess the impact on outcomes that are very closely related to project activities, such as knowledge transfer, technology adoption, yields, or agricultural income.[3] Again findings are mixed, though with some indications of improved technology, knowledge transfer, and adoption leading to higher yields and thus to increased agricultural income.

While it is of value to assess the impact of FFS on farmers' knowledge, technology transfer, take-up, and agricultural production, it should be kept in mind that households may simply divert resources away from other activities toward the new project-related activities. It is therefore also important to analyze the impact on broader welfare indicators for the participating households. Although it has become popular to assess empowerment, it is not in itself a welfare measure; rather, it can be a channel through which people may obtain improved welfare. We have not found any studies within the conventional peer-reviewed literature that analyze the impact of FFS on broader welfare factors such as food security or poverty.

This paper is intended to contribute to filling this gap in the literature by presenting a rigorous impact evaluation of RIPAT FFS to examine whether the program improved food security and reduced poverty among participating households. In our evaluation, we have sought to address the main points raised in the FFS evaluation debate summarized above.

We let the original project documentation guide us in the choice of outcome measures. It was explicitly stated that the overall development objectives of the intervention were to increase food security and alleviate poverty among participating households. Any effect on these outcome measures can only be expected to be observable in the medium or long term, as participating households have to first adopt and then implement the new technologies throughout a full agricultural cycle before impacts on food security and poverty can occur. By developing our evaluation strategy and the associated survey instrument accordingly, we have effectively tied the analysis—and our hands—to these outcome measures, and thereby reduced the possibilities of "cherry-picking" convenient results. However, we did not have a full pre-analysis plan laid out, as suggested by Casey, Glennerster, and Miguel (2012).

In explaining our choice of impact assessment methodologies, we discuss the extent to which we can overcome the potential endogeneity issues noted by Feder *et al.* (2004) and Godtland, Sadoulet, Janvry, Murgai, and Ortiz (2004) that stem from non-random program placement and self-selection of participants. To address these issues we collected household data from two different areas: Arumeru district, where RIPAT I was implemented, and Karatu district, where RIPAT II was started two years later. In both areas we collected data from virtually all RIPAT households and from a sample of control households in nearby villages. In addition, we also collected data from non-RIPAT households in RIPAT I villages. We employ four different methodologies to assess the impact of RIPAT I: a simple cross-sectional comparison of RIPAT I and control households in a multivariate setting to control for observable characteristics; an intention-to-treat estimation, in which we include non-RIPAT households within RIPAT I villages, to circumvent the problem of self-selection at the household level; a matching estimation to increase comparability of observable characteristics between RIPAT I and control households and villages[4]; and finally a Quasi Difference-in-Difference estimation exploiting data from the later RIPAT II households and their controls to account for selection. Under the assumption that the household- and village-level selection mechanisms in the two districts were the same, the Quasi Difference-in-Difference takes selection on both observable and unobservable characteristics into account, i.e., we circumvent the endogeneity problems of non-random program placement and self-selection of participants. To the extent that there was already some initial impact among RIPAT II farmers on food security and poverty indicators at the time of the data collection in 2011, which was more than one year after RIPAT I completion and half way through the RIPAT II project period, our impact assessment will be a conservative estimate of the true impact. We thereby avoid the problem of positive selection bias. Throughout the paper, the impact assessment is an assessment of RIPAT I only, unless explicitly stated otherwise.

To address the potential problem of timing and spillover to control farmers diluting the impact of the intervention, as described by van den Berg and Jiggins (2008), we use control farmers living at a sufficient distance from the RIPAT intervention villages. Although there had been spillover within RIPAT I villages at the time of data collection, qualitative findings confirm that we do not have to worry about any potential spillover in food security and poverty from RIPAT I to control villages at the distances used.[5] In addition, by assessing the impact of RIPAT I almost five years after project start and more than one year after completion, we are also able to address issues of sustainability, at least in the medium term.

Our analyses are based on interviews with 2,041 farming households using a highly structured closed-form questionnaire administered in 36 villages, of which 16 were intervention villages. We thus have a large sample size compared to previous FFS impact evaluations.[6]

The vast majority of participants in RIPAT Farmer Field Schools were involved in the project throughout the full project period. We see that half-way through the project period in RIPAT II and one year after project completion in RIPAT I the participating households were more likely to have adopted virtually all the key technologies promoted through the basket of options than farmers in the control villages. This indicates both immediate and sustained adoption of the new technologies. We find that the participating households were more likely to be cultivating improved varieties of banana, to have a larger degree of crop diversification, to be keeping improved breeds of livestock, and to be members of savings groups.

Most importantly, we find that these high levels technology take-up resulted in considerable improvements in food security levels, suggesting an increase in overall household welfare. In the medium term, i.e., five years after project start, we find that RIPAT I households were up to 24 percentage points less likely to experience hunger, that their diet contained more animal proteins, and that their children were more likely to have at least three meals per day. These are substantial impacts, which we believe will be sustainable in the longer term, given the timing of the evaluation.

We do not find any significant impact of RIPAT I on any of our poverty indicators occurring by 2011. This suggests that RIPAT I households might have had a more urgent need to overcome food insecurity than to invest in the more material goods that are typically used as poverty indicators, e.g., good floors or mobile phones. We analyze two possible mechanisms that might have led to our results: reallocation of labor resources toward own agricultural production and production smoothing over the agricultural cycle. We find indications of both.

We have organized the paper as follows. Section 2 describes the RIPAT intervention, and Section 3 presents the data: summary statistics for household and village characteristics, participants, adoption of technologies, and choice of outcome measures. In Section 4 we explain our evaluation strategy, while we turn to the results in Section 5. We analyze the role of labor reallocation and production smoothing in the findings in Section 6. Section 7 concludes.

2. THE INTERVENTION

In this paper we evaluate the agricultural project RIPAT I, but for some elements of the evaluation strategy we also exploit data from a later project, RIPAT II. Both projects were implemented by a local Tanzanian NGO, RECODA. They targeted small- and medium-sized farmers in rural villages with at least one acre and in principle no more than five acres of land. Village leaders were asked to form two groups of 30–35 farmers in each village and to assist the groups in getting access to a joint group field. Membership was voluntary. RECODA explained to village leaders that members should not be rich in terms of the wealth ranking of the village, had to be committed to active participation (attendance records were kept and strict rules enforced), had to be willing to share their knowledge with and demonstrate agricultural techniques to their fellow villagers and should therefore be of good standing in the community, and had to live within the village administrative area. Furthermore, the leaders were told that each group should have an equal number of men and women and only one member per household (Maguzu, Ringo, & Vesterager, 2013). The two RIPAT projects were each implemented in eight villages; these were selected by district officials as being the poorest villages in the given district.

There were thus two sources of endogenous selection into the project. One was endogenous village selection: since program placement was not random and if district officials followed the guidelines given to them, RIPAT villages were less wealthy than the other villages in the district at the outset of the project, i.e., there was a negative selection effect. Secondly, since participation was voluntary, households self-selected into the project (provided they met the targeting criteria) and hence we would expect participating RIPAT households to have been more motivated than other households, resulting in a positive selection effect. The sign of the *net* selection effect thus cannot be assumed *a priori*.

The RIPAT Farmer Field Schools draw on a bottom-up experiential and reflective approach to learning and practical demonstrations of farming techniques, as do most FFS. However, they are described as less participatory and more top-down than other FFS approaches (Aben, Duveskog, & Friis-Hansen, 2013). A key difference is a strong element of traditional technology transfer through training in a predetermined but locally adapted "basket of technology options", rather than in just one technology. These agricultural technology options are chosen by the implementing NGO on the basis of their strong agricultural expertise and in prior consultation with the villages in question. By equipping the farmers with the necessary information, knowledge, and hands-on experience in the use of different relevant and efficient technologies, the program provides each farmer with the means to choose which technologies to adopt in his or her own agricultural production. Each group meets weekly at its demonstration plot or group field. At these meetings, progress is followed and discussed throughout the agricultural cycles. The crops and technologies introduced in the "basket of options" are very diverse and cannot all be fully introduced in a single agricultural cycle as in typical FFS programs; the implementing NGO therefore works with the RIPAT FFS for a three-year period, after which the farmers "graduate". The standard "basket" includes improved varieties of banana with new cultivation techniques, conservation agriculture and crop diversification, improved animal husbandry, fruit and multipurpose trees, soil and water conservation, post-harvesting technologies, and encouragement to participate in savings groups. However, the basket is always adapted to suit local conditions, taking into account, for example, soil, water, and climate. [7]

The two RIPAT FFS projects commenced two years and four months apart in two districts in the Arusha Region. The implementation of RIPAT I in Arumeru District was from May 2006 until the end of 2009, while RIPAT II was implemented in Karatu District from September 2008 until August 2012 (see Figure 1). The implementation strategies for the two projects were the same except for minor adjustments to the content of the basket of options. [8] We exploit this gradual roll-out in one of our empirical strategies below to address the potential problems caused by self-selection of participating farmers and non-random program placement at village level due to unobservable factors.

3. DATA AND SUMMARY STATISTICS

In January 2011 we conducted a large-scale quantitative household survey in both RIPAT and control villages in the two intervention districts. This was one year after completion

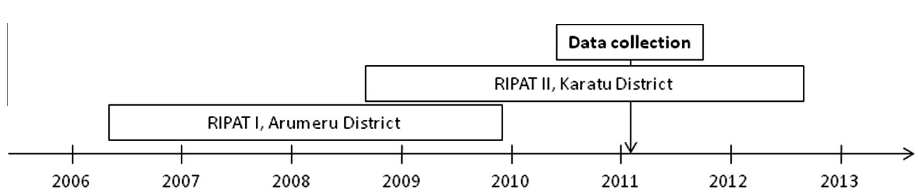

Figure 1. *Timeline of RIPAT projects and data collection.*

Table 1. *Summary statistics for background characteristics*

	All	Arumeru			Karatu	
		RIPAT I	Non-RIPAT	Control	RIPAT II	Control
	(1)	(2)	(3)	(4)	(5)	(6)
Acres 2006	3.04	3.29	2.95	3.02	3.03	2.90
	(1.69)	(1.78)	(1.74)	(1.72)	(1.61)	(1.61)
Head less than seven years educ.	0.32	0.30	0.30	0.32	0.28	0.41
	(0.47)	(0.46)	(0.46)	(0.47)	(0.45)	(0.49)
Head more than seven years educ.	0.05	0.07	0.06	0.06	0.04	0.04
	(0.22)	(0.26)	(0.24)	(0.24)	(0.20)	(0.19)
Age of head	46.82	48.19	44.97	46.32	45.72	48.58
	(14.45)	(13.57)	(16.10)	(16.06)	(11.53)	(15.21)
Head is female	0.15	0.19	0.15	0.19	0.07	0.16
	(0.35)	(0.40)	(0.35)	(0.39)	(0.26)	(0.37)
Number of children of head	1.73	1.49	1.32	1.35	2.37	1.86
	(1.55)	(1.31)	(1.34)	(1.33)	(1.68)	(1.70)
Good at math	0.36	0.41	0.36	0.38	0.36	0.31
	(0.48)	(0.49)	(0.48)	(0.49)	(0.48)	(0.46)
Participation in other projects	0.16	0.27	0.13	0.16	0.15	0.09
	(0.37)	(0.45)	(0.33)	(0.37)	(0.35)	(0.29)
Household rainfall, mm/year	818.59	751.26	749.53	703.84	930.23	905.28
	(106.97)	(53.99)	(54.38)	(41.29)	(50.83)	(61.30)
Village distance to market, km	8.53	9.59	10.09	5.43	8.42	8.98
	(4.83)	(3.68)	(3.71)	(4.86)	(5.96)	(3.93)
Village has secondary school	0.68	0.60	0.65	0.88	0.63	0.67
	(0.47)	(0.49)	(0.48)	(0.33)	(0.48)	(0.47)
Village hosted devel. project	0.51	0.63	0.70	0.39	0.36	0.49
	(0.50)	(0.48)	(0.46)	(0.49)	(0.48)	(0.50)
Observations	2,041	420	335	359	491	436

Notes: Means (and standard deviations) of household and village characteristics for all households in the sample are shown in Column (1) and for subsets of the sample in Columns (2)–(6). The means are unweighted. Since non-RIPAT households are overrepresented in some villages, the village level means differ slightly between Columns (2) and (3).

of RIPAT I and around halfway through implementation of RIPAT II. We used a highly structured closed-form pilot-tested questionnaire to capture the extent to which participating farmers had adopted the technologies introduced through the RIPAT farmer groups and to discover whether this in turn had had an impact on their food security and poverty levels relative to farmers in control villages. A selection of non-RIPAT households were also surveyed in the RIPAT I villages in order to gather data for separate diffusion analyses of the more popular technologies.[9] We interviewed a total of 2,374 households in 36 villages; of these, 2,041 households are included in the analyses in this paper.[10] The bottom row of Table 1 shows how these households are distributed across RIPAT I and II and their respective control households, as well as the stratified random sample of non-RIPAT households surveyed in RIPAT I villages. We aimed at interviewing all the farmers who originally signed up for the RIPAT Farmer Field Schools, including those who later dropped out—provided they had remained in the village. In Arumeru district, 90% of the original RIPAT I farmers were interviewed, and 96% of the RIPAT II farmers were interviewed in Karatu district.

In each household, an interview was conducted with the person mainly responsible for agricultural decisions, often the head of the household. However, in RIPAT households, the person interviewed was always the RIPAT group member, who typically was the head or spouse of the head. The project aimed at achieving gender balance in the RIPAT farmer groups, which resulted in a larger share of female-headed households among the RIPAT farmers than otherwise in the village. The same degree of overrepresentation of female-headed households was sought among the control households. A village-level questionnaire was administered to representatives of each village government as a supplement to the household interviews. We thus have household- and village-level information in the data.

Table 1 lists means (and standard deviations) for the household- and village-level variables. Column (1) presents the averages for all households in the data, while the remaining columns represent subsets of the data used for different analyses. Columns (2) through (4) provide data for Arumeru district and Columns (5) and (6) for Karatu district. Column (2) shows data for households in RIPAT I, Column (3) has non-RIPAT households in RIPAT I villages, and Column (4) shows the averages for the households used as control households for RIPAT I. Columns (5) and (6) present data for RIPAT II households and their control households respectively.

It can be seen from Column (1) that the households included in the analysis generally had around three acres of land, that the majority of household heads had completed seven years of primary school, and that heads were typically middle-aged males with between one and two children living at home. We tested the farmers' math skills with two simple math problems[11]; 36% answered correctly. 16% of the households had participated in other development projects in the past. We also included the average historical rainfall level at 1:1 km resolution based on the household's GPS coordinates from secondary data,[12] since these households mainly rely on rain-fed agriculture. There is a large difference between the two districts, with Karatu receiving almost 200 mm more rainfall than Arumeru.

At the village level, we see that the average distance from each village to its most important market for agricultural output was eight kilometers, that two-thirds of the villages had secondary schools, and that half of the villages had hosted a development project in the past.

In the main part of the analyses below, we will be comparing RIPAT I households to control households from Arumeru district. It is therefore important that these are indeed comparable in terms of observable characteristics. We find that the two groups are well balanced; the only characteristic in Table 1 that differs significantly between RIPAT I and control households is whether the household had participated in another development project in the past, tested at the 5% level with clustered standard errors.

In general, we cluster standard errors at the village level, as this is the most conservative approach when testing against the null hypothesis of no impact. However, this implies that we do not have enough degrees of freedom to include all the household and village characteristics shown. For consistency, we control for the log of acres, education, age, age squared, and gender, and for all village characteristics, in all specifications.[13] In Appendix B, we present regression results corresponding to the analyses below, where all characteristics are included and standard errors are clustered at the sub-village level instead.[14] The inclusion of all household characteristics does not alter the results markedly.

(a) *Who participated?*

We know that the RIPAT project was not randomly allocated and it is interesting to take a closer look at the sources of selection: self-selection of households within villages, and non-random program placement across villages. Table 2 presents estimates from a logit regression of whether or not a household participated in RIPAT I on household and village characteristics. In Column (1) we compare RIPAT I households with non-RIPAT households in RIPAT I villages, which isolates self-selection of households. We can see that RIPAT I household heads were typically older, better at math, and their households more likely to have participated in other projects in the past. The last two points suggest that RIPAT I households were more entrepreneurial than non-RIPAT households, as we expected, which could lead to a positive bias in the impact assessment. In Column (2), we compare the household characteristics of RIPAT I and control households and find that the same differences persist. In addition, RIPAT I households were more likely to be female-headed and received more precipitation than the households in control villages. We include all households ever enrolled in RIPAT I, even if they later dropped out. This is done to ameliorate the issue of

Table 2. *Who participated in RIPAT I?*

Comparison households	Non-RIPAT (1)	Control (2)	Control (3)
Log acres 2006	0.218	0.091	−0.091
	(0.17)	(0.22)	(0.30)
Head less than seven years educ.	−0.238	−0.219	−0.663**
	(0.25)	(0.29)	(0.34)
Head more than seven years educ.	0.216	0.038	0.322
	(0.33)	(0.36)	(0.36)
Age of head	0.163***	0.112***	0.177***
	(0.05)	(0.04)	(0.05)
Age of head, squared/100	−0.138***	−0.100**	−0.162***
	(0.04)	(0.04)	(0.04)
Head is female	0.335	0.335*	0.257
	(0.24)	(0.20)	(0.22)
Number of children of head	0.030	0.107	−0.035
	(0.07)	(0.08)	(0.11)
Good at math	0.267*	0.066	0.135
	(0.15)	(0.21)	(0.27)
Participation in other projects	0.830***	0.494**	0.236
	(0.21)	(0.24)	(0.37)
Household rainfall, mm/year	−0.000	0.024**	0.021***
	(0.00)	(0.01)	(0.01)
Village distance to market			0.335***
			(0.12)
Village has secondary school			−3.191**
			(1.32)
Village hosted devel. project			2.359*
			(1.33)
Constant	−4.450**	−20.185***	−20.706***
	(2.09)	(7.53)	(5.47)
N	755	779	779

Notes: Logit estimates with a RIPAT I household indicator as the outcome variable. The samples consist of households within RIPAT I villages in Column (1), and of RIPAT I and their control households in Columns (2) and (3). Standard errors in parentheses are clustered at the sub-village level.
* Significance level is denoted by 0.1.
** Significance level is denoted by 0.5.
*** Significance level is denoted by 0.01.

household selection, and we consider this to be the most conservative approach.

In the last column of Table 2, we add village characteristics to the regression. We see that RIPAT I villages were further away from their main markets, less likely to have a secondary school and more likely to have hosted a development project in the past. These differences all point to RIPAT I villages being less wealthy than the control villages, confirming the accounts from the original project documentation. This suggests that we might underestimate an impact when comparing the two. With respect to household characteristics, the difference in the likelihood of having participated in a development project is absorbed by the corresponding village-level difference. In addition, when we control for village-level characteristics, we find that heads of RIPAT I households were better educated than heads of control households, supporting the suggestion of positive self-selection of households into RIPAT I.

In Section 4 we present the four evaluation methodologies we employ to address household and village selection on the basis of observable and unobservable characteristics.

Finally, we note that among the RIPAT I participants who initially enrolled in the RIPAT FFS, there was a high level of engagement: the vast majority stayed with the project throughout the three-year project period. In RIPAT I, more than 80% of the participants graduated, and the picture is similar for RIPAT II. It should be noted that participating in RIPAT FFS is rather time-consuming. Attendance rules were strictly enforced, and the need to attend is given as the main reason for dropping out by those who left the RIPAT farmer groups (Lilleør *et al.*, 2013). [15]

(b) *Technology adoption*

The next obvious question is to examine whether or not RIPAT farmers also adopted on their own farms the technologies introduced through the RIPAT farmer groups. Farmers' engagement in project activities and the decision to allocate household resources (labor and land) toward adopting the proposed crops, livestock, and new agricultural practices are in themselves indicators of project implementation success, but they also represent a prior and necessary condition for finding any impact on broader welfare indicators as an outcome of the intervention.

We examine technology adoption among participating farmers in both RIPAT I and RIPAT II, since participants in the latter project had also been exposed to the full set of technologies examined here by the time of the survey in 2011. Because the basket of options entails a myriad of technologies and other elements (Maguzu *et al.*, 2013), we have focused the analysis on six of the main components. We use simple means to indicate whether, relative to their control households, RIPAT I and II households were more likely to have adopted improved banana cultivation, to use more crop diversification, to grow fruit trees, to keep improved breeds of small livestock, to practice zero-grazing in their livestock husbandry, and to participate in savings groups (which was encouraged by RECODA).

In Table 3, we list the means (and standard deviations) for these key adoption measures for RIPAT I and II households and for their respective control households in the two districts, Arumeru and Karatu. Around two-thirds of the RIPAT households were found to be growing an improved banana variety. On average, they were growing around six different types of crop. About half had fruit trees, a quarter of them kept improved poultry breeds, 20–40% kept improved breeds of milking goats, and non-negligible fractions practiced zero grazing and were members of local savings groups.

To see whether there were significant differences between RIPAT and control households in the two districts, we carried out a series of cluster-robust t-tests for the difference being zero. A quick glance at the associated p-values shows that both the RIPAT I graduate households and the RIPAT II households had adopted all the analyzed components of the basket of options to a significantly greater extent than the households surveyed in the control villages. Only zero-grazing restrictions and the use of fruit trees seem not to have caught on in any significant way in RIPAT I compared to the control villages in this simple bivariate setting. [16] It should be noted that improved pig breeds were introduced only in RIPAT II.

This suggests that there was a considerable degree of take-up of the proposed technology options among the RIPAT

Table 3. *Summary statistics for adoption measures*

	Arumeru			Karatu		
	RIPAT I	Control	(p-Value)	RIPAT II	Control	(p-Value)
Improved banana	0.69	0.12	0.00	0.64	0.01	0.00
	(0.46)	(0.33)		(0.48)	(0.08)	
Number of crops in 2010	5.62	4.76	0.02	6.65	4.69	0.00
	(2.30)	(2.22)		(2.71)	(2.12)	
Fruit tree(s)	0.66	0.56	0.46	0.49	0.28	0.02
	(0.48)	(0.50)		(0.50)	(0.45)	
Improved poultry breeds	0.27	0.02	0.00	0.25	0.01	0.00
	(0.44)	(0.14)		(0.44)	(0.10)	
Improved goat breeds	0.40	0.15	0.00	0.19	0.05	0.00
	(0.49)	(0.36)		(0.40)	(0.22)	
Improved pig breeds	0.00	0.00		0.18	0.00	0.00
	(0.00)	(0.00)		(0.38)	(0.05)	
Zero grazing	0.30	0.29	0.93	0.21	0.09	0.02
	(0.46)	(0.45)		(0.41)	(0.29)	
Savings	0.23	0.03	0.00	0.30	0.11	0.01
	(0.42)	(0.18)		(0.46)	(0.31)	
Observations	420	359		491	436	

Notes: The table shows the means (standard deviations) as well as the p-values of a cluster-robust t-test of the differences in means being equal to zero, clustering at the village level.

farmers. It is unlikely that all of these significant differences in take-up could be driven by selection into the project, especially because the improved varieties of crops and breeds of livestock did not exist in the area prior to RIPAT.

Furthermore, these take-up rates indicate both that there was a high level of immediate take-up among RIPAT II farmers, who were only half-way through the project cycle, and high rates of sustained take-up among RIPAT I farmers, who at the point of data collection were more than one year beyond graduation and project closure.

When we analyze the overall degree of take-up, we find that all of the components in the basket of options were adopted by some farmers. No single element was adopted by all farmers, although all farmers were growing or keeping at least one of the promoted crops or animal breeds. This suggests that the element of choice built into the basket of options was indeed used by farmers to pick and choose according to their specific needs and resources.

(c) *Choice of outcome measures*

We evaluate the impact of RIPAT on the basis of the development objectives that it was intended to improve, as stated in the original project documentation: namely, better food security and reduced poverty among the participating households.

(i) *Food security measures*

To assess the food security situation among the respondent households, we employed a household level measure capturing access to food: the "Household Hunger Scale" (HHS).[17] 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".

Due to considerable seasonal variations in the food security status of households, we take three different reference periods into account—the self-assessed best and worst months in terms of food security during the previous year, and the last four weeks prior to the survey.[18] Since this area of Tanzania is not subject to severe and prolonged periods of starvation, we would expect to find most variation in the measure when the period of reference is the self-assessed worst month in terms of food security within the previous year. As it is difficult to interpret the magnitude of an impact on HHS because it is an ordinal measure, we also consider the simple binary variable "No hunger", which is one if the household did not suffer from hunger at any point during the past year according to HHS and zero otherwise. To see whether children benefitted

Table 4. *Summary statistics for development outcomes*

	Arumeru			Karatu		
	RIPAT I	Control	(*p*-Value)	RIPAT II	Control	(*p*-Value)
No hunger	0.40	0.29	0.19	0.39	0.39	0.90
	(0.49)	(0.46)		(0.49)	(0.49)	
HHS worst month	1.43	1.65	0.43	1.23	1.23	0.98
	(1.47)	(1.46)		(1.25)	(1.26)	
HHS best month	0.07	0.04	0.38	0.03	0.03	0.97
	(0.35)	(0.27)		(0.24)	(0.26)	
HHS previous four weeks	0.25	0.32	0.57	0.19	0.32	0.01
	(0.66)	(0.73)		(0.53)	(0.74)	
At least three meals, worst month	0.63	0.62	0.89	0.82	0.82	0.92
	(0.48)	(0.49)		(0.38)	(0.39)	
At least three meals, best month	0.94	0.91	0.39	0.99	0.98	0.42
	(0.24)	(0.29)		(0.11)	(0.13)	
At least three meals, previous four weeks	0.87	0.84	0.43	0.96	0.95	0.62
	(0.34)	(0.37)		(0.19)	(0.21)	
Meat	0.74	0.69	0.48	0.40	0.39	0.70
	(0.44)	(0.46)		(0.49)	(0.49)	
Eggs	0.56	0.36	0.00	0.45	0.38	0.29
	(0.50)	(0.48)		(0.50)	(0.49)	
Dairy products	0.87	0.83	0.44	0.63	0.60	0.62
	(0.34)	(0.38)		(0.48)	(0.49)	
PPI	44.29	44.68	0.89	32.00	33.49	0.56
	(14.81)	(14.04)		(16.41)	(14.84)	
Good quality floor (not earth)	0.26	0.31	0.55	0.13	0.11	0.81
	(0.44)	(0.46)		(0.33)	(0.32)	
Mobile phone	0.68	0.67	0.83	0.61	0.56	0.27
	(0.47)	(0.47)		(0.49)	(0.50)	
Rely on casual labor	0.05	0.15	0.02	0.11	0.20	0.02
	(0.22)	(0.36)		(0.31)	(0.40)	
Hired labor	0.62	0.49	0.03	0.45	0.33	0.05
	(0.49)	(0.50)		(0.50)	(0.47)	
Observations	420	359		491	436	

Notes: The table shows the means (standard deviations) as well as the *p*-values of a cluster robust *t*-test of the differences in means being equal to zero, clustering at the village level.

from RIPAT I, we measured their food consumption by looking at the prevalence of households where children had at least three meals per day during each of the three periods.

Finally, we aim to capture the nutritional quality of the overall household diet by analyzing whether household members had meat, eggs, or dairy products to eat during the previous week.

From the raw averages in Table 4 we see that households in this region did not suffer from food insecurity throughout the year, but that food insecurity was rather pronounced during the worst periods of the year, typically the lean season immediately before harvest. Only 30–40% of households did not experience any hunger during the worst period of the year. Similarly, virtually all children had at least three meals per day during the best part of the year, while on average about a quarter of the households with children served two meals or fewer per day during the lean season. Households in Arumeru seemed to report higher levels of food insecurity than households in Karatu, but in terms of reported nutritional quality, the weekly consumption of meat, eggs, and dairy products was generally lower in the latter district.

A glance at the p-values for the cluster-robust t-tests of whether there were significant differences between RIPAT and control households in the two districts reveals that the raw means of the food security outcome variables are rather similar when we do not control for selection, household, or village characteristics.

(ii) *Poverty measures*

Poverty is a complex outcome to measure. It is a relative measure, and it depends on local circumstances. Tanzania operates with a national poverty line of TZS 492 per adult equivalent per day (or roughly USD 1 per day using Purchasing Power Parity), representing the monetary cost of fulfilling basic needs (Schreiner, 2012).

Household income and consumption levels are notoriously difficult and time-consuming measures to capture, especially if this is to be done using a reasonably short survey instrument (Beegle, Carletto, & Himelein, 2012a, 2012b). We therefore use an asset-based indicator of poverty as a short-cut. 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 Household Budget Survey of 10,466 representative households from all over Tanzania.[19] The PPI score ranges from 0 (most likely to be below a poverty line) to 100 (least likely to be below a poverty line).

We have taken the Progress out of Poverty Index as our key poverty indicator because it is a widely used measure for identifying poverty *levels* and the only one available for Tanzania at the time of data collection. Schreiner (2012) notes that the PPI scorecard also aims to measure *changes* in poverty through time, and therefore in selecting indicators and holding other considerations constant, preference should be given to more sensitive indicators, e.g., ownership of a lantern. However, it places a lot of weight on more static measures, here fertility and female literacy.[20] We have therefore also considered the two best single predictors of poverty, according to Schreiner (2012), in isolation; namely the quality of the floor in the main dwelling and whether or not the household owns a (mobile) phone. In Table 4, the raw averages for the poverty measures show clearly that households in Karatu are on average poorer than households in Arumeru.[21] There are no significant poverty level differences between RIPAT households and their respective control households.

Finally, we also examine the supply of casual labor, as this is often an important source of income for poor households, but also something that is associated with stigma. It is a possible channel for RIPAT households to adjust their allocation of resources, if they can afford to do so; we return to this below. We see that among the control households, 15–20% relied on casual labor as one of the most important sources of income; but also that RIPAT households in both districts relied significantly less on supplying casual labor than the control households, and were also more likely to hire labor to work on their farms.

4. EVALUATION STRATEGY

In order to estimate the impact of RIPAT on participating households, we need a good estimate of the counterfactual situation—of what would have happened to the RIPAT households had they *not* participated in the project. We approach the counterfactual from four different angles, which in different ways and to different degrees take into account the participant self-selection and the non-random project placement at village level.

First, we undertake a *simple cross-sectional impact assessment* comparing outcomes of RIPAT I households to outcomes of households in control villages in a multivariate regression analysis using Ordinary Least Squares (OLS). To the extent that the household and village-level characteristics included in this multivariate setting do not fully account for the endogenous selection at household- and village-level, this simple cross-sectional estimation of the impact may be biased. It will be upward biased if the farmers that chose to participate and thus self-selected into the project were more motivated and entrepreneurial than the average farming household in a control village, *ceteris paribus*. It will be downward biased if the RIPAT I villages were indeed less wealthy than the control villages prior to project implementation, as suggested by the project documentation, and if this difference is not captured by the village characteristics included in the regressions.

Second, to take household self-selection into account, we estimate the impact at village rather than household level. That is, we explore the fact that we have surveyed non-RIPAT households in RIPAT I villages and estimate the *intention-to-treat* (ITT) effect, which pools both RIPAT and non-RIPAT households in RIPAT villages, since they were all intended for treatment. This does not give us an estimate of the average treatment effect among those who initially signed up for the project, but rather an average village-level effect among all those who could have signed up. The ITT estimator is free from self-selection bias and is only biased to the extent that the village-level characteristics included do not fully account for the non-random project placement.

Third, to increase the comparability between RIPAT I households and their control households, we employ a *matching estimator*. This allows us to match more closely each RIPAT I household with a control household that has similar household and village-level characteristics. More specifically, we employ Mahalanobis matching with one nearest neighbor, which implies that a higher weight is given to control observations that are similar to RIPAT observations compared to OLS.[22] In this way, we address the potential bias in the simple cross-sectional comparison due to unbalanced observables, but we still rely on the assumption of no selection on unobservable characteristics.

Finally, we propose a *Quasi-Difference-in-Difference* (QDiD) approach exploiting the gradual roll-out of the project. RIPAT II started more than two years after RIPAT I. RIPAT II participants were at the time of data collection still one and a half years away from graduation. Assuming that the selection mechanisms into RIPAT were the same in the two districts at both household and village levels, we can adjust for this selection in the simple cross-sectional impact assessment of RIPAT I in Arumeru District by subtracting the differences found between RIPAT II and control households in Karatu District. Doing this in a multivariate regression framework results in the QDiD estimator, which does not suffer from selection bias. The central assumption here is that the differences in outcomes due to household and village selection between treated and control households should be the same in the two districts in absence of treatment. Examining the observable characteristics in Table 1 above, we find indications that the RIPAT—control differences in the two districts are very similar. Out of the 12 characteristics listed, only two are significantly different at the 5% level (age and gender of head). This QDiD approach is similar to the evaluation strategy initially employed by Coleman (1999, 2006).

Ideally, for the perfect QDiD estimation, our data collection should have taken place exactly at project start-up of RIPAT II. The fact that the data collection took place two and a half years after project start of RIPAT II may result in QDiD *underestimating* the average treatment effect, since the high level of take-up of the different components in the basket of options could already have resulted in a beginning impact on the broader development outcomes at the time of the survey. There are three reasons why we are not very worried about this. First, it is always better to underestimate than to overestimate, making any significant effect found more credible. Second, during the first year of RIPAT II, a severe drought hit the entire area (both Karatu and Arumeru districts) and caused the virtually complete failure of all agricultural activities in the area. The project was therefore in effect postponed by one year, and project activities were resumed in the following agricultural season. Third, there is a natural time lag in both agricultural production and livestock breeding from the adoption of a new technology until its yields can be harvested, and in any case most households adopt additional new technologies gradually.[23]

5. RESULTS

To assess the food security impact of RIPAT, we consider whether households experienced any hunger, their HHS, whether the children in the households had at least three meals per day, and whether the households had eaten meat, eggs, or dairy products during the previous week.

Table 5 is a compilation of the estimated effects. Each column represents an estimation method, while the rows refer to different outcome measures. Columns (1) and (2) present estimated coefficients for the RIPAT indicator variable from cross-sectional comparison regressions and for the RIPAT village indicator variable from ITT regressions respectively. Column (3) shows the differences between RIPAT and control households from Mahalanobis matching, while Column (4) gives the estimated impact from the QDiD specification.[24] All regressions include village characteristics and the restricted set of household characteristics, and standard errors are clustered at the village level. The same variables are used for the matching procedure. In Appendix C, we show the full set of regressors for the simple cross-sectional comparison and the QDiD regression with the HHS in worst month as the outcome variable.

In the first row of Panel A of Table 5, we show the estimated impact on the *No hunger* indicator. Reading across the columns, we see that RIPAT I increased the probability of being free from hunger by 17–24 percentage points, depending on the evaluation methodology, with the village level ITT impact being the lowest, as expected, but still large and statistically significant.

Having taken self-selection into account in the ITT estimation, the remaining worry is whether the impact is driven by pre-existing village differences, as the project was not randomly placed. In Column (3) we match on village and household characteristics, and thereby aim at a better balance of observables between RIPAT and the control villages and households. The impact on hunger persists in magnitude but is not statistically significant.

However, there might still be remaining *unobserved* differences between villages that we have not fully accounted for, and we therefore employ the QDiD approach using differences between RIPAT II and control households in Karatu to account for potential selection in Arumeru. Assuming that the selection mechanisms were the same in the two districts, this regression provides unbiased estimates of the impact. The result is reconfirmed: RIPAT I households are 24 percentage points less likely than their controls to have suffered from hunger when selection is accounted for. The fact that the QDiD estimate is so close to the estimated impacts from the other specifications suggests that selection on the basis of unobservables did not play a major role.[25]

We also analyze the impact on the HHS for three different reference periods, recalling that higher values on the HHS correspond to more severe hunger. Consistently across all four specifications, we find that RIPAT I significantly reduced hunger in the worst period of the year. We do not see any impact in the best period or the four weeks immediately prior to the time of the interview. From Table 4 we note that there was only a little room for improvement, especially in the best month, as control households in Arumeru had an average HHS value of 0.04.

The reduction in hunger is associated with an increase in the number of meals for the children.[26] We see a consistent impact on the likelihood of having at least three meals in the best period of the year. This is a significant and substantive impact of seven to 10 percentage points of improvement, depending on the specification. For the worst period we estimate an impact almost double that in magnitude for most specifications, but statistically less significant. With respect to the previous four weeks, the picture is more blurred. Taken together, however, these figures suggest that participating in RIPAT not only affected the food security status of households in the lean period as measured by the HHS, but it also improved children's intake of food at other times of the year.

Regarding the nutritional quality of the diet, we find that in general RIPAT I households were significantly more likely than controls to have eaten meat or eggs in the week before the interview, although the ITT results are weak for meat. We do not find a consistent increase in the intake of dairy products.

Based on these findings, we conclude that overall RIPAT I had a clear impact on food security in terms of reducing hunger, increasing the number of meals provided to children, and improving the intake of animal protein.

The next question is then whether RIPAT also succeeded in improving the situation with regard to the other development objective of poverty alleviation. Turning to Panel B of Table 5,

Table 5. *Impact of RIPAT on development outcomes*

	(1) Simple CS	(2) ITT	(3) Matching	(4) QDiD
PANEL A: Food security outcomes				
No hunger	0.208***	0.172**	0.189	0.238***
	(0.052)	(0.062)	(0.204)	(0.063)
HHS, worst month	−0.714***	−0.699**	−0.723***	−0.809***
	(0.193)	(0.277)	(0.204)	(0.226)
HHS, best month	−0.004	0.004	−0.043	−0.023
	(0.031)	(0.037)	(0.052)	(0.034)
HHS, previous four weeks	−0.146	−0.153	−0.146	−0.046
	(0.127)	(0.106)	(0.126)	(0.133)
At least three meals, worst month	0.158*	0.156*	0.083	0.170*
	(0.085)	(0.089)	(0.062)	(0.098)
At least three meals, best month	0.069*	0.100**	0.076**	0.065*
	(0.038)	(0.045)	(0.038)	(0.037)
At least three meals, previous four weeks	0.106**	0.080	0.070	0.101**
	(0.037)	(0.057)	(0.049)	(0.040)
Had meat previous week	0.132*	0.044	0.143***	0.148*
	(0.064)	(0.069)	(0.042)	(0.076)
Had eggs previous week	0.223***	0.145**	0.189***	0.163**
	(0.044)	(0.057)	(0.061)	(0.065)
Had dairy products previous week	0.068	0.005	0.120**	0.050
	(0.072)	(0.083)	(0.047)	(0.092)
PANEL B: Poverty outcomes				
PPI	3.472	0.829	0.077	4.047
	(2.086)	(3.075)	(0.050)	(3.105)
Has good quality floor (not earth)	−0.002	−0.062	1.351	−0.006
	(0.076)	(0.087)	(1.481)	(0.081)
Has (mobile) phone	0.055	−0.063	−0.031	0.064
	(0.033)	(0.037)	(0.060)	(0.047)
Observations	779	1,114	779	1,706

Notes: Each row represents a dependent variable. Columns (1), (2), and (4) show OLS regression coefficients: Column (1) gives the coefficient to the RIPAT I indicator in a simple cross-sectional (CS) comparison using data from Arumeru district only; Column (2) gives the coefficient for the RIPAT I village indicator in ITT regressions including non-RIPAT households from RIPAT I villages, applying inverse sampling probability weights, and using data from Arumeru district only; Column (3) gives the Mahalanobis matching estimates yielded when RIPAT I households are matched to controls in Arumeru district; and Column (4) gives the coefficients for the interaction term between the RIPAT dummy and the Arumeru district dummy in the QDiD specification, i.e., these estimations include both RIPAT I and II households and their respective control households in the two districts. Village characteristics and household characteristics as described in text are controlled for in all specifications. Standard errors in parentheses are clustered at the village level. The numbers of observations are reduced for the "Less than three meals" outcomes to 688, 985, 688, and 1,515, respectively for the four columns.

* Significance level is denoted by 0.1.

** Significance level is denoted by 0.5.

*** Significance level is denoted by 0.01.

the first row shows that we do not find any significant impact of RIPAT on poverty as measured by the PPI. Estimates for the two additional time-variant indicators which have proven to be strong individual predictors of poverty status in Tanzania, quality of the floor and ownership of a mobile phone, are also insignificant. We have also checked for various degrees of heterogeneity in these results, but the conclusion remains the same: RIPAT has not had any significant impact on any of these poverty indicators and thus we believe the overall level of wealth of the participating households to have remained virtually unchanged.

In order to address potential gender differences, we split the results by gender of household head, and a few interesting findings emerge.[27] The female-headed RIPAT I households were more food secure than the female-headed control households during the *best* period of the year (suggesting that there was room for improvement among this subset of households), and they were more likely to have eggs as part of their daily diet. However, they were less likely to consume dairy products, which could be linked to the fact that they were also less likely to have adopted the improved breeds of milking goats.[28]

6. POSSIBLE MECHANISMS

The fact that we find significant improvements in food security among RIPAT households, but no improvement in their poverty status, has led us to wonder why this should be so.

One explanation could be that resources were scarce for RIPAT households at the outset; when they experienced an improvement in their level of resources, they simply prioritized more secure and improved food consumption over higher non-food consumption. We cannot empirically test this any further, but it would explain the above finding.

A second explanation could be that households reallocated their use of labor resources within the household, e.g., shifted from cash income activities toward own agricultural production. This would have meant that the households produced more food, but earned less cash income, which again could have resulted in better food security (from own production) at the expense of lost income. This would make it unlikely that there would be a positive impact on poverty indicators.

Finally, a third explanation could be that the agricultural technologies introduced did not increase the total annual agricultural production, but only smoothed production over the agricultural cycle, thereby increasing food security in what typically would have been the lean period. We analyze the two last explanations empirically below.

(a) *Casual labor*

During qualitative interviews, it became clear that in this local setting casual labor is considered a "last resort", an income source turned to when all other options are exhausted and hence, is greatly stigmatized. However, a reduction in the supply of casual labor could result in a substantial decrease in income, since casual labor can be a remunerative income source for Tanzanian smallholders.[29] We see from Table 4 that casual labor was indeed relatively widespread among the control households in Arumeru and Karatu districts; 15% and 20% respectively of these households supplied casual labor as a primary source of income. However, it was significantly less prevalent among RIPAT I and II farmers; only 5% and 11% respectively in these districts relied primarily on casual labor.[30] This suggests that RIPAT households might have chosen to cut back on casual labor because they had experienced an increase in their agricultural income. Such a cut-back would offset partially or completely any increase in income from agriculture, but still result in a welfare increase, because the household would avoid the stigma of supplying casual labor and at the same time become more food secure.

As to whether households *hired* labor on their own farms during 2010, we see from Table 4 that RIPAT households were 13 and 12 percentage points more likely than the control households to have hired labor on their own farms in Arumeru and Karatu districts respectively.

These impacts may not be causal and could be fully driven by selection, though controlling for household and village characteristics in a simple cross-sectional comparison only increases the differences found and the statistical significance. The ITT estimates are not significantly different from zero.[31] This is not surprising, however, as we would expect labor markets to be local; if RIPAT farmers increased their demand for casual labor, non-RIPAT farmers in RIPAT villages might start to rely more on income from casual labor, which would then even out the village average.

Nevertheless, taken together, the two results are suggestive in providing a possible explanation for why we find a profound impact on food security but no impact from RIPAT on the poverty measures used. The RIPAT households seem to have re-optimized the allocation of labor within their households and begun to invest in their own agricultural production.

(b) *Production smoothing*

The agricultural technologies introduced in RIPAT Farmer Field Schools were selected to enhance production smoothing over the agricultural cycle. Households generally experience large seasonal variation in food security, and they do not seem able to smooth consumption. In the lean period, 70% of the households in Arumeru control villages experienced some kind of hunger, while only 2% experienced any hunger just after harvest. Limited access to proper storage facilities and financial markets inhibit the ability of households to smooth consumption.[32] Several elements in the basket of options introduced by RIPAT are production-smoothing technologies that provide the households with food even in the lean period. Banana plants fruit outside of the main harvest season as long as they receive some water, improved breeds of poultry lay more eggs, and improved breeds of goat produce more milk all year round than their traditional counterparts. It is therefore important to consider whether the impact of RIPAT on food security was mainly driven by the adoption of these three production-smoothing technologies that ease the smoothing of food consumption over the year and thus increase food security in the typical lean period.

The first two columns of Table 6 show that participation in RIPAT I and RIPAT II increased the probability of adopting at least one of the three production-smoothing technologies by 60–65 percentage points, controlling for household and village characteristics. As discussed in Section 3(b) above, participation in RIPAT is significantly correlated with adopting either banana cultivation or the keeping of improved breeds of poultry or goats. The next two columns of Table 6 present regressions of the HHS in the worst month on a smoothing technology dummy that equals one if the household adopted any of the production-smoothing technologies and zero otherwise, for RIPAT and control households respectively. In both groups, households using production-smoothing technologies also experienced significantly less hunger than households that did not use any of the three technologies. This is not necessarily a causal relationship, as the decision to adopt the smoothing technologies was endogenous.

In Column (5) we limit the sample to those households that adopted any of the production-smoothing technologies and run the QDiD regression on this sub-sample in order to analyze whether RIPAT FFS participation brought about any additional degree of food security. The estimates suggest that RIPAT households adopting the production-smoothing technologies achieved the same level of food security as the selected sample of control households that adopted the smoothing technologies. However, 82% and 74% respectively of RIPAT I and RIPAT II households employed such technologies, whereas this was the case for only 25% and 8% of the control households in Arumeru and Karatu respectively. In Column (6) we see the QDiD regression results for the sub-sample of households *not* adopting any of the smoothing technologies. Among these, RIPAT I households experienced less hunger than controls at the 5% significance level after taking the selection into account, suggesting that the impact of RIPAT on food security was not purely driven by the production-smoothing technologies; other elements of the basket of options also improved the food security of households in the lean period.[33]

We can thus conclude that although the use of smoothing technologies is associated with greater food security, the overall impact of RIPAT I on food security was not driven solely by these, as the basket of technology options seems to contain other elements that are also relevant for the food security of households not applying the main smoothing technologies.

7. CONCLUSION

This is, to the best of our knowledge, the first paper which rigorously analyzes the impact of a locally adapted Farmer Field School project on broader welfare indicators and development objectives, namely food security and poverty alleviation, and not just on intermediate and very project-related agricultural outcomes, such as technology knowledge transfer, technology adoption, or agricultural yields from the technologies promoted.

We find that there were strong and sustained positive effects on food security among the participating households more than one year after end of project, in terms of access to food, food consumption, and quality of diet. Participating

Table 6. *Smoothing mechanisms*

Outcome variable	Adoption		HHS, worst month			
	(1)	(2)	(3)	(4)	(5)	(6)
Sample	RIPAT I	RIPAT II	RIPAT	Control	Smooth	Non-smooth
RIPAT	0.601***	0.648***			0.167	0.344***
	(0.07)	(0.04)			(0.20)	(0.12)
Smooth			−0.357*	−0.369***		
			(0.18)	(0.12)		
District			0.654	0.893***	1.199**	0.745**
			(0.56)	(0.29)	(0.55)	(0.27)
RIPAT*District					−0.397	−0.826**
					(0.32)	(0.33)
N	779	927	911	795	828	878

Notes: OLS estimates. The dependent variable in Columns (1) and (2) is an adoption indicator equal to one if the household had adopted any of the smoothing technologies; in Columns (3)–(6) it is HHS in worst month. Village characteristics and household characteristics as described in text are controlled for in all specifications. Standard errors in parentheses are clustered at the village level.
* Significance level is denoted by 0.1.
** Significance level is denoted by 0.5.
*** Significance level is denoted by 0.01.

households experienced less hunger in the lean period, were more likely to have animal protein in their weekly diet, and were more likely to give the children in the household at least three meals per day. We find no impact of the RIPAT project on poverty indicators. There is suggestive evidence that the positive impacts on food security measures, but lack of impact on poverty indicators, could be caused by RIPAT households having prioritized food over non-food consumption, reallocated their labor resources toward improving their own agricultural production, and reduced seasonal peaks and troughs in food production.

Taken together—and when compared with earlier FFS evaluations—these results point to the importance of allowing the passage of time for assessing outcomes. Although the RIPAT II farmers, who had completed the project more recently than the RIPAT I farmers at the time of the survey, were also more likely than their control farmers to have adopted the full range of technologies examined, the impacts on food security can only be expected where the technology adoption has had sufficient time to raise food security levels, which in this case was among RIPAT I farmers. Timing is thus an important factor both when considering the length of the project (a typical RIPAT Farmer Field School runs for at least three years, as opposed to the one agricultural cycle of standard FFS projects) and when considering the timing of the impact evaluation and the outcomes selected for examination, allowing impacts from a change in agricultural systems to materialize. For instance, although we do not find any impact on poverty indicators, it could be that such an impact will materialize in an even longer time horizon, when food security is no longer a concern for RIPAT households. Only time can tell.

A final question which may spring to mind concerns the costs of producing the food security impacts found. The total cost per participating RIPAT household per year was USD 200,[34] which is from three to 20 times as high as the various FFS cost estimates listed in (van den Berg & Jiggins, 2007). However, it should be borne in mind that RIPAT projects differ from the typical FFS in that they offer a full *basket* of technology options, combine top-down teaching with participatory learning methods, have very close follow-up during the phasing-in period for the new technologies, and are implemented over a substantially longer time horizon. Although these key differences clearly increase the cost per farmer, we also believe that they are vital to the impacts found above. None of the existing FFS evaluations have documented improved food security, so potentially the extra money was well spent.

Furthermore, apart from the objectives of improved food security and poverty alleviation among participating households, RIPAT also has the aim of ensuring that the participants are willing to share their knowledge with and demonstrate agricultural techniques to their fellow villagers, thus increasing the probability of diffusion of the improved techniques within RIPAT villages. A study by Gausset (2013) highlights the fact that a reasonably high degree of diffusion of the various RIPAT technologies has taken place. In particular, the improved banana variety has been popular, and by 2011 it had been adopted by one in eight non-RIPAT farmers in RIPAT I villages (Larsen, 2012). With this focus of RIPAT FFS on diffusion as in conventional agricultural extension programs, one can argue that the relevant cost–benefit analysis should be carried out at village rather than household level. The average cost per household is then only USD 30, and this expenditure led to an overall outcome of a 17-percentage-point increase in the probability of households being free from hunger in RIPAT I villages.[35] In comparison, a large nation-wide community-based child nutrition program in Ethiopia resulted in an improvement of only seven percentage points measured on the same household hunger scale (White & Mason, 2012).

NOTES

1. This was primarily promoted by the World Bank in the 1970s and 1980s and developed to tackle some of the inefficiencies present at the time in traditional public extension services.

2. The Farmer Field School concept was originally developed by the FAO to promote integrated pest management among Indonesian rice farmers in the late 1980s, but since then has spread to many countries and

over the years has been so widely adopted and locally adapted that there is no longer a single model for either its technical content or the educational format (van den Berg & Jiggins, 2007).

3. Although Davis *et al.* (2012) state in the title of their paper that they analyze the impact of FFS on agricultural productivity and poverty, they in fact analyze the impact on crop income and livestock income, which they sum as agricultural income.

4. We thank an anonymous referee for suggesting intention to treat and matching estimation.

5. In RIPAT II there were no reports of spillover within intervention villages in 2011, let alone to the control villages.

6. We only know of one study, by Davis *et al.* (2012), with a similar sample size (1,126 households). However, this is spread across 8–10 districts and three countries.

7. For more detailed descriptions, see Maguzu *et al.* (2013) and Vesterager, Ringo, and Maguzu (2013) for shorter and longer accounts, respectively.

8. Savings group participation was encouraged but not facilitated during the RIPAT I project. Furthermore, during RIPAT I it became clear that a more efficient distribution system for the improved breeds of goats would be needed in future projects. Finally, in Karatu there was an additional demand for an improved breed of pigs, which was then also included in the basket of options.

9. The data collection and data entry were 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.

10. We excluded from the dataset all farmers with more than eight acres of land and less than one acre of land in 2006 (for RIPAT, non-RIPAT and control farmers), as these did not comply with the original target criteria for RIPAT participation (174 households). We capped the acres at eight rather than five, as the data show that 17% of the RIPAT farmers did in fact have more than five acres of land, but only 6% had more than eight acres in 2006. Excluding households with more than five acres from the analysis below does not change the overall conclusions. We also excluded all newcomers to the villages (48 households). Finally, we excluded households with missing observations for any of our variables (111 households).

11. The farmer was considered "Good at math" if s/he correctly answered both questions, $29 - 13 = ?$ and $50/10 = ?$

12. We used interpolated data for yearly precipitation measured in mm from the period 1950–2000 available from http://www.worldclim.org/.

13. When including observations from Karatu, we allow village characteristics to have district-specific coefficients.

14. Clustering at the sub-village level leads to 52 clusters in regressions with Arumeru data only, and 130 clusters when all villages are included.

15. In RIPAT I, 77 households dropped out of their farmer groups before the end of implementation, while in RIPAT II, 96 households dropped out. All these drop-outs are included in the analyses throughout the paper and still considered to be RIPAT farmers or RIPAT participants regardless of when they dropped out.

16. When we control for household and village-level characteristics in the comparison of technology adoption between RIPAT I households and their control households, we find that all the listed adoption measures were in fact used to a greater extent among RIPAT I households, with significance levels of $p < 0.01$ or $p < 0.05$.

17. 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 (Ballard, Coates, Swindale, & Deitchler, 2011).

18. Households were interviewed in January, which is neither immediately after harvest nor in the worst hungry period, so we expected the hunger situation in the previous four weeks to have been somewhere in between the best and the worst months.

19. See Figure A1 in Appendix A for the list of questions used in the latest PPI measure for Tanzania. Summing the points gives the overall PPI score.

20. Such measures are often not helpful in analyses of poverty change; for example, we would not expect RIPAT to affect literacy adult females.

21. District means for PPI, good quality floor and mobile phone are all significantly different at the 1% level.

22. We have also employed a propensity score matching estimation and get very similar results. However, we choose to present Mahalanobis matching estimates to obtain valid confidence intervals (Abadie & Imbens, 2006). All observations are within the common support of the propensity score.

23. Most RIPAT participants spend the first agricultural season learning about the new agricultural practices at a demonstration plot before they then in a later agricultural season choose which ones to adopt on their own farms.

24. This corresponds to the regression coefficient for the interaction term between the RIPAT indicator variable and the Arumeru indicator variable in a regression where both indicators are also included separately.

25. To the extent that RIPAT II had already had a (positive) impact on food security or poverty, we underestimate the impact of RIPAT I.

26. Because some households did not have any resident children, we lose 91 observations in Columns (1) and (3), 129 observations in Column (2), and 191 observations in Column (4).

27. The results are not shown, but are available upon request.

28. This is consistent with the qualitative gender research among these women, which highlights the fact that the improved milking goat breeds introduced in the RIPAT groups were zero-grazing goats, which had to be fed. Whereas grazing goats is typically a male task in the local context, collecting fodder and firewood is a female task. Some female RIPAT farmers were therefore against keeping the milking goats, as this would increase the burden of collecting fodder. Later, specific fodder plants, e.g., elephant grass, were introduced to reduce this burden for the women (Mogensen & Pedersen, 2013)

29. For example, weeding one acre of land pays TZS 2,000, which is four times the daily national poverty line.

30. Controlling for household and village characteristics only increases the estimated differences and the statistical significance.

31. Results available upon request.

32. Our results are not driven by access to savings, as they are robust to controlling for membership of a savings group.

33. We reach the same conclusions from a QDiD regression on the full sample where the RIPAT dummy, the district dummy and their interaction term are all interacted with the smoothing dummy.

34. It should be noted that since the RIPAT interventions described here were the first out of a series of such projects, some piloting costs are also included.

35. The potential impact on non-RIPAT households need not only come through increased technology adoption. The analysis above of the demand for hired labor suggests that RIPAT also brought about increased economic activity on the local markets, which could have beneficial effects on the local economy in general.

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APPENDIX A. PROGRESS OUT OF POVERTY INDICATOR (PPI)

The PPI is constructed by Schreiner (2012) based on ten simple questions listed in what he refers to as a scorecard; see the example from Tanzania below.

See Fig. A.1.

Indicator	Value	Points	Score
1. How many household members are 17-years-old or younger?	A. Four or more	0	
	B. Three	8	
	C. Two	15	
	D. One	23	
	E. None	30	
2. Do all children ages 6 to 17 attend school?	A. No	0	
	B. Yes, or no children ages 6 to 17	1	
3. Can the female head/spouse read and write?	A. No	0	
	B. Yes, but not in Kiswahili nor English	0	
	C. No female head/spouse	0	
	D. Yes, only in Kiswahili	5	
	E. Yes, in English (regardless of others)	12	
4. What is the main building material of the floor of the main dwelling?	A. Earth	0	
	B. Concrete, cement, tiles, timber, or other	11	
5. What is the main building material of the roof of the main dwelling?	A. Mud and grass	0	
	B. Grass, leaves, bamboo	8	
	C. Concrete, cement, metal sheets (GCI), asbestos sheets, tiles, or other	11	
6. How many bicycles, mopeds, motorcycles, tractors, or motor vehicles does your household own?	A. None	0	
	B. One	1	
	C. Two or more	11	
7. Does your household own any radios or radio cassettes?	A. No	0	
	B. Yes	5	
8. Does your household own any lanterns?	A. No	0	
	B. Yes	6	
9. Does your household own any irons (charcoal or electric)?	A. No	0	
	B. Yes	6	
10. How many tables does your household own?	A. None	0	
	B. One	2	
	C. Two	4	
	D. Three or more	7	

Microfinance Risk Management, L.L.C., `http://www.microfinance.com` **Total score:**

Figure A.1. *Simple poverty scorecard for Tanzania.*

APPENDIX B. ROBUSTNESS RESULTS

See Tables 7 and 8

Table 7. *Impact of RIPAT on development outcomes*

	(1) Simple CS	(2) ITT	(3) Matching	(4) QDiD
PANEL A: Food security outcomes				
No hunger	0.203***	0.155***	0.224	0.225***
	(0.056)	(0.055)	(0.164)	(0.075)
HHS, worst month	−0.590***	−0.524**	−0.935***	−0.649***
	(0.195)	(0.211)	(0.164)	(0.231)
HHS, best month	0.012	−0.024	−0.013	−0.008
	(0.050)	(0.045)	(0.036)	(0.050)
HHS, previous four weeks	−0.062	−0.082	−0.172**	0.035
	(0.147)	(0.118)	(0.080)	(0.152)
At least three meals, worst month	0.137**	0.143**	0.225***	0.143*
	(0.066)	(0.068)	(0.071)	(0.081)
At least three meals, best month	0.088**	0.133***	0.050	0.085**
	(0.035)	(0.036)	(0.044)	(0.034)
At least three meals, previous four weeks	0.124***	0.093*	0.103*	0.123***
	(0.041)	(0.049)	(0.056)	(0.042)

(*continued on next page*)

Table 7—(*continued*)

	(1) Simple CS	(2) ITT	(3) Matching	(4) QDiD
Had meat previous week	0.173**	0.099	0.156***	0.174**
	(0.066)	(0.082)	(0.059)	(0.077)
Had eggs previous week	0.212***	0.179***	0.221***	0.146**
	(0.057)	(0.063)	(0.051)	(0.071)
Had dairy products previous week	0.066	0.019	0.084**	0.033
	(0.067)	(0.072)	(0.042)	(0.082)
PANEL B: Poverty outcomes				
PPI	0.197	−1.087	0.097	0.562
	(2.473)	(2.767)	(0.064)	(2.918)
Has good quality floor (not earth)	−0.098	−0.131*	2.968*	−0.105
	(0.062)	(0.068)	(1.592)	(0.068)
Has (mobile) phone	0.027	−0.057	−0.036	0.039
	(0.043)	(0.048)	(0.048)	(0.052)
Observations	779	1,114	779	1,706

Notes: Each row represents a dependent variable. Columns (1), (2), and (4) show OLS regression coefficients: Column (1) gives the coefficient for the RIPAT I indicator in a simple cross-sectional (CS) comparison using data from Arumeru district only; Column (2) gives the coefficient for the RIPAT I village indicator in ITT regressions, including non-RIPAT households from RIPAT I villages, applying inverse sampling probability weights, and using data from Arumeru district only; Column (3) gives the Mahalanobis matching estimates yielded when RIPAT I households are matched to controls in Arumeru district; and Column (4) gives the coefficient for the interaction term between the RIPAT dummy and the Arumeru district dummy in the QDiD specification, i.e., this estimation includes both RIPAT I and II households and their respective control households in the two districts. Village characteristics (Distance to market, Has secondary school, Hosted development project in 2006–2010, and in Column (4) all three interacted with Arumeru district dummy) and household characteristics (Log acres in 2006; Household head's gender, education, math skills, age and age squared; Number of children of head; Whether household has participated in other project in the past; Historical rainfall (interacted with the Arumeru district dummy in Column (4))) are controlled for in all specifications. Standard errors in parentheses are clustered at the sub-village level. The numbers of observations are reduced for the "Less than three meals" outcomes to 688, 985, 688, and 1,515, respectively for the four columns.
* Significance level is denoted by 0.1.
** Significance level is denoted by 0.5.
*** Significance level is denoted by 0.01.

Table 8. *Smoothing mechanisms*

Outcome variable	Adoption		HHS, worst month			
	(1)	(2)	(3)	(4)	(5)	(6)
Sample	RIPAT I	RIPAT II	RIPAT	Control	Smooth	Non-smooth
RIPAT	0.567***	0.650***			0.051	0.320***
	(0.09)	(0.04)			(0.27)	(0.10)
Smooth			−0.348*	−0.313**		
			(0.18)	(0.12)		
District			6.403**	9.356*	3.244	5.194*
			(2.45)	(5.09)	(3.35)	(2.90)
RIPAT*District					−0.108	−0.851**
					(0.40)	(0.35)
N	779	927	911	795	828	878

Notes: OLS estimates. The dependent variable in Columns (1) and (2) is an adoption indicator equal to one if the household has adopted any of the smoothing technologies; in Columns (3)–(6) the dependent variable is HHS in worst month. Village characteristics (Distance to market, Has secondary school, Hosted development project in 2006–10, and all three interacted with the Arumeru district dummy) and household characteristics (Log acres in 2006; Household head's gender, education, math skills, age and age squared; Number of children of head; Whether household has participated in other project in the past; Historical rainfall, and the last interacted with the Arumeru district dummy) are controlled for in all specifications. Standard errors in parentheses are clustered at the sub-village level.
* Significance level is denoted by 0.1.
** Significance level is denoted by 0.5.
*** Significance level is denoted by 0.01.

APPENDIX C. HHS IN WORST MONTH, ALL REGRESSION COEFFICIENTS

See Tables 9.

Table 9. *Impact of RIPAT on HHS in worst month; all regression coefficients shown*

	Simple CS			QDiD		
	(1)	(2)	(3)	(4)	(5)	(6)
RIPAT dummy	−0.226	−0.737***	−0.714***	0.003	0.044	0.096
	(0.28)	(0.19)	(0.19)	(0.15)	(0.13)	(0.13)
Arumeru district dummy				0.422*	0.991***	1.054***
				(0.22)	(0.34)	(0.34)
RIPAT*District				−0.228	−0.782***	−0.809***
				(0.31)	(0.23)	(0.23)
Village distance to market		0.044**	0.043**		0.023*	0.023**
		(0.02)	(0.02)		(0.01)	(0.01)
Village has secondary school		−0.741***	−0.726***		0.075	0.059
		(0.23)	(0.23)		(0.12)	(0.11)
Village had devel. project		0.506**	0.521**		0.197*	0.244**
		(0.22)	(0.23)		(0.11)	(0.11)
Village distance to market*District					0.021	0.019
					(0.02)	(0.02)
Village has secondary school*District					−0.816***	−0.784***
					(0.25)	(0.25)
Village had devel. project*District					0.309	0.280
					(0.24)	(0.25)
Log acres 2006			−0.349***			−0.365***
			(0.10)			(0.07)
Head less than seven years educ.			0.123			0.130
			(0.13)			(0.09)
Head more than seven years educ.			−0.529***			−0.594***
			(0.17)			(0.12)
Age of head			0.019			0.022
			(0.03)			(0.01)
Age of head, squared			−0.009			−0.014
			(0.02)			(0.01)
Head is female			−0.115			−0.038
			(0.12)			(0.11)
Constant	1.652***	1.871***	1.543**	1.229***	0.880***	0.472
	(0.20)	(0.31)	(0.70)	(0.10)	(0.15)	(0.39)
N	779	779	779	1,706	1,706	1706

Notes: OLS estimates; Dependent variable is Household Hunger Scale in worst month. Standard errors in parentheses are clustered at the village level. Columns (1)–(3) are based on data from RIPAT I and control households in Arumeru district, while Columns (4)–(6) also include data from Karatu district. '*District' refers to an interaction term with the Arumeru district dummy.

* Significance level is denoted by 0.1.

** Significance level is denoted by 0.5.

*** Significance level is denoted by 0.01.