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Program Participation Intensity and Children's Nutritional Status: Evidence from a Randomized Control Trial in Mozambique

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Program Participation Intensity and Children's Nutritional Status: Evidence from a Randomized Control Trial in Mozambique

Alan de Brauw[1], Patrick Eozenou[2] and Mourad Moursi[3]

ABSTRACT

Agricultural interventions are thought to have the potential to improve nutrition, but very little rigorous evidence is available about programs that link the two. In this paper, we study the nutritional impacts of Reaching End Users, a biofortification project in Mozambique that had integrated agricultural and nutritional components. We provide evidence on the dietary impacts of the program, but more importantly we study the impacts of the program by participation intensity. Using ordinary least squares and instrumental variables techniques, we find that more intense participation in both the agricultural and the nutritional components led to larger impacts. Therefore, the results could have important implications for refining the design of future projects that attempt to link agricultural and nutritional interventions.

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I. INTRODUCTION

Agricultural interventions are thought to have the potential to improve nutrition, but very little rigorous evidence is available about programs that link the two. A recent review of the existing scientific literature indicated that two main factors contributed to the continued uncertainty about the strength of the link between agriculture and nutrition. First, agricultural interventions are rarely designed with the explicit goal of improving nutrition. And second, even when improving nutrition is an explicit goal of an agricultural intervention, the evaluation of the impact on nutritional indicators is often flawed. Such evaluations are rarely randomized, they tend to lack controls for the effects of confounding factors, or they lack statistical power to detect expected effects (Masset et al. 2012).[1]

The recent Reaching End Users (REU) project in Mozambique and Uganda aimed to promote the adoption and consumption of vitamin A biofortified orange-fleshed sweet potato (OSP). Implemented in Mozambique between 2006 and 2009, the REU integrated agricultural, nutritional, and market-level components. The impacts of the REU were assessed with a prospective, cluster randomized control study addressing many of the methodological weaknesses found in previous similar studies. The study found that vitamin A intakes increased and vitamin A deficiency levels decreased among target populations in both Mozambique and Uganda, and serum retinol levels increased among moderately deficient children in Uganda (Hotz et al. 2012a, 2012b). Previous research on the REU in both Mozambique and Uganda demonstrates that the project had impacts on average vitamin A intakes (Hotz et al. 2012a, 2012b), and that average impacts on adoption and vitamin A intakes appear to have largely resulted from the agricultural component of the intervention (de Brauw et al. 2013). However, as with many interventions, we know the average effects, but we do not know much about how much the intensity of participation mattered for impacts to occur. As with many interventions, in the REU farmers (or, more properly, members of farm households) could choose their level of participation in the project, which might have made those households more or less successful at growing OSP and feeding OSP to target populations.

In this paper, we estimate the average impacts of the REU and several outcomes related to children's nutritional status in Mozambique, going beyond the impacts estimated in Hotz et al. (2012a). The outcomes we primarily study can be categorized as measures of both narrow and broad changes in dietary quality. We consider narrow changes to be changes that only occur as a result of changes in OSP consumption, while broad changes in dietary quality would include increased consumption of OSP and of other nutritious foods. Since OSP are a new source of vitamin A, and reduced vitamin A deficiency can lead to reduced morbidity, we descriptively show how weight-for-height Z (WHZ) scores are affected.

The paper goes beyond just providing estimates of the average treatment effects on outcomes such as those described above, and attempts also to measure how the intensity of participation affects outcomes. Though the REU model was integrated, farmers could choose their level of participation in the intervention once it was offered at the farmer group level along any of the dimensions in the project. Specifically, they could choose their level of involvement in the agricultural component, the nutritional component, and the marketing component.[2] As the level of participation can also affect outcomes, in this paper we explore whether participation intensity in either the agricultural or the nutritional components appears to affect outcomes. To define intensity, from project data and the endline survey data we measure whether farm households participated in specific activities associated with the agricultural and nutritional components, where specific activities clearly suggest more intense participation. We use the data from Mozambique for this exercise, as there is more variation in participation, due to a difference in implementation described in this paper.

A clear challenge in defining intensity is that the choice of participation intensity is endogenous. Consequently, our primary interpretations of coefficients are as correlations. That said, to attempt to control for positive selection into higher participation intensity, we also present instrumental variables (IV) estimates of composite intensity variables, generated through correspondence analysis on the discrete measures of participation intensity. We use variables as instruments that arguably affect the intensity of participation, but do not independently affect nutritional outcomes at the endline. The IV estimates can be interpreted as causal and, as expected, they are all lower in magnitude than the ordinary least squares (OLS) estimates, suggesting some positive participation bias in the latter estimates.

[1] In a related review, Dangour et al. (2013) searched for evidence that agricultural price policies have affected nutritional outcomes, and found a paucity of evidence in general.

[2] We do not study the marketing component of the REU here for several reasons. First, the marketing component of the intervention was somewhat focused on what were termed “medium-scale producers,” who were a relatively small number of farmers attempting to produce larger amounts of OSP for market. Second, the remaining portion of the marketing component took a long while to materialize and was not uniformly implemented, even within the farmer groups selected for the intervention. As a consequence, the impact evaluation report found no large impacts on variables associated with the marketing component in either Mozambique or Uganda (de Brauw et al. 2010). We therefore focus on the agricultural and nutritional components here.

The paper proceeds as follows. First, we describe the intervention in more detail and describe how participation intensity varied for the agricultural and nutritional components. Second, we describe the data and how we measure nutritional outcomes using the available data. Third, we provide results on both average treatment effects and estimates of participation intensity. Finally we discuss the results and our conclusions.

2. THE REU IN MOZAMBIQUE

The goal of the REU project was to reduce vitamin A deficiency among two target populations—women of child-bearing age and children under 5 years old—using an integrated approach. The project integrated three different components: an agricultural component, a nutritional component, and a marketing component. These three components were designed to ensure that farm households could produce OSP, understood the rationale for consuming OSP and other sources of vitamin A, and ideally knew how to select OSP for sale on local markets. The theory of change behind the project was that to reduce vitamin A deficiency through OSP, farm households must either produce OSP for home consumption or understand the value of purchasing OSP from local markets. The demand creation component stressed the value of OSP consumption among target members of the household (women of child-bearing age and young children). The marketing component was deemed important to promote local markets for OSP. De Brauw et al. (2013) studied the causal mechanisms in detail, and found that in Mozambique nearly the entire average increase in vitamin A intakes among children can be attributed to OSP production and consumption of home produce. As discussed in the introduction, in this paper we focus on the production (agriculture) and consumption (nutrition) components of the REU.[3]

In each village chosen for the project and not randomly selected into the control group, a group of interested farmers was assembled prior to the first year; the project goal was for this group to include 100 households in each village. Since previously formed community groups are rare in Mozambique, the project typically worked with church groups to attempt to reach the target of 100 farm households in each group. As with many development projects, within farmer groups farmers could choose how extensively to participate in the project. Below, we describe the agricultural and nutritional components in more detail, as well as the participation decisions that farmers could make.

2.1 Participation in the Agricultural Component

To stimulate the production of OSP, the agricultural component of the REU distributed multiple OSP varieties of vines to project farmers, and taught or reinforced growing techniques to farmers. Prior to the growing season, the REU grew large quantities of OSP vines for distribution, and then it conducted annual vine distributions to participating farmers, with vine distribution policies that varied by year.[4] During each annual vine distribution, farmers in participating village groups were offered vines at no charge, and additional vines were made available for purchase at approximately the average production cost to the project. In each distribution, multiple OSP varieties were available, and farmers were trained about the agronomic, taste, and health characteristics of the different varieties. At the final vine delivery in 2009 and throughout the extension meetings that took place that year, farmers were told vines would not be delivered in the following year.

After receiving vines, farmers in participating groups also had the opportunity to participate in extension meetings in which the REU disseminated messages about growing OSP. These meetings specifically taught farmers how to plant OSP vines, grow OSP while avoiding pests, maintain planting material through the dry season, and properly store OSP roots to consume after the primary season.[5] One volunteer promoter per farmer group was chosen from among the village group to help promote and reinforce these messages throughout the project. These promoters may have also visited farmers' fields to ensure that they were growing OSP or to help farmers with farming practices.

Based on this description, participation in the project could have taken several forms. First and most basically, farmers could have received vines from the project but little else. For simplicity, here we define receiving vines as receiving vines in any of the three years. Farmers could have also participated in the additional trainings that the project offered. Conditional on participation, in 2009 farmers in our endline survey reported participating in about three meetings on average over the past three years; in the course of the past year, farmers reported having attended approximately one meeting, on average, which would likely be the meeting at which they obtained vines. Finally, about 60 percent of households reported that promoters visited

3 The REU was implemented in both Mozambique and Uganda using two intervention models in each country; the two models varied in intensity to measure variation in cost-effectiveness. However, as average impacts did not differ by intervention model, here we treat farmers in all treated villages as the "treatment" group, and the remainder as the control group.

4 A primary reason that the REU conducted annual vine distributions is that there is only one primary agricultural season in Zambezia province. The remainder of the year is dry with very little rain.

5 In the more intensive intervention model, these trainings were largely repeated in each of the three years, whereas in the less intensive model, only the planting training occurred every year in conjunction with the vine distribution, whereas the other trainings took place in the first year.

their OSP fields over the course of the project, with many visits occurring in the past year.

For the purposes of this paper, we consider three categories of participation that relate to farmer effort. Though they were nominally group members, as defined by having shown an interest prior to the project, farmers could have not participated at all levels. Farmers could have also just obtained vines, or they could have obtained vines and participated in at least some extension meetings. We leave out other forms of participation, such as promoter visits, because they do not relate to farmer effort.[6]

2.2 Participation in the Demand Creation Component

The demand creation component used multiple strategies to train and inform people about the nutritional benefits of consuming OSP and other sources of vitamin A. Information was conveyed through a variety of sources, including group trainings with farmer group members, community theatre sessions related to the health benefits of OSP, radio spots, billboards, and other advertising. With regard to extension, the demand creation component had a structure similar to that of the agricultural component. The nutrition extensionist worked with several nutritional promoters per village, as it was deemed important prior to the project to work on nutrition with small groups of women, approximately ten per group. Nutrition promoters were selected from among village group members and were trained to deliver nutrition-related messages to other group members. The implementation model in Mozambique required approximately one nutrition promoter for every ten households projected to participate in the project. Therefore, nutrition promoters worked directly with the nutrition extensionist, and other mothers in the farmer group were assigned to nutrition promoters who at least theoretically held regular meetings.

Farmers (or mothers) could have also participated in the demand creation component of the intervention in several ways. The most intense way to participate was as a nutrition promoter. In the full panel data set, 20 percent of Model 1 mothers and 24 percent of Model 2 mothers acted as nutrition promoters, which is much higher than the 10 percent we would have expected in the sample, given the structure of the project. From the perspective of learning about intensity, the overweighting of promoters in the sample is likely an opportunity, rather than a detriment.[7]

Among mothers who are not nutrition promoters in the data set, there is further heterogeneity in participation. In the endline survey, about two-thirds of mothers in panel households reported attending at least one training over the past three years. When mothers had been to trainings, they had been to an average of two to three trainings over the past three years. As a result, there has been some scope for teaching nutrition messages to mothers who are not promoters, but they may have learned only a limited number of messages from the project. Of course, mothers also could have learned project messages outside the extension, through radio, by attending event days held by the project, or by seeing project messages on billboards; however, we find little evidence that any such learning occurred in the endline data.

Therefore, we also break up participation in the demand creation component into three categories: (1) the most intensive category is a nutrition promoter living in the household; (2) the next category level is a mother attending any nutrition trainings (since nutrition promoters clearly attended meetings, these categories do not overlap); and (3) the least intensive category implies no participation in the nutritional component of the intervention.

2.3 Overall REU Participation Heterogeneity

Based on our descriptions in the two subsections above, household participation in the REU could have varied along two dimensions—in the intensity of participation in the agricultural component, and in the intensity of participation in the nutritional component. The average treatment effects of a project like the REU necessarily are the average intensity of these two components.

We initially illustrate participation among all treatment households in the panel and households for whom we have dietary intake data (Table 1).[8] We illustrate the panel household data in a mosaic plot in Figure 1. Whereas almost all households participated in the agricultural component, a somewhat large proportion of sample households never participated in the nutritional component (more than 100 of 416 treatment households). These findings are consistent among the dietary intake sub-sample, as only eight households out of 244 reported neither receiving any vines nor participating in nutritional trainings. However, more than 20 percent of households did not participate in the nutritional trainings. In the next section, after we discuss the data, we discuss how we measure differences in participation.

6 We also leave out the agricultural promoter category, as none of the promoters showed up in our data set.

7 In their primary report on project impacts, de Brauw et al. (2010) carefully show that there is not much difference between average treatment effects that account for the overweighting of nutrition promoters in the data set and those that do not. In this paper, since we are interested in measuring the difference in outcomes based on this intensity, we do not report weighted-average treatment effects.

8 By definition, households in the control group did not participate in either component.

3. DATA

The Mozambique sample is composed of 36 village-level organizations, from four districts of Zambezia province: 18 of the organizations are located in Milange, nine in Gurue, and the remaining nine organizations are split between Nicoadala (five organizations) and Mopeia (five organizations).[9] These districts are illustrated on the map of Zambezia (Figure 2). Organizations in Nicoadala and Mopeia districts were selected from a single stratum (the "South").

Village organizations initially selected for the sample had to meet four salient requirements: (1) they had to have enough families with children between the ages of 6 and 35 months at baseline to be able to meet sample size requirements; (2) they had to have reasonably high access to lowlands, so that vines could be kept between growing seasons; (3) they did not have other active agricultural interventions, and had not been previously targeted for an OSP project; and (4) they were not adjacent to one another, to ensure control areas would not immediately receive OSP vines from neighbors, and to limit jealousy between communities.[10] The 36 villages included in the sample were then randomly selected into one of the two treatment arms or the control group, stratified by district.[11]

Among selected village organizations, we conducted both a socioeconomic survey and a nutrition survey. Prior to the intervention, power calculations indicated that 12 households per village organization should be included in the nutrition survey. Given additional returns to collecting socioeconomic data and OSP adoption data indicated by power calculations, we strove to conduct the socioeconomic survey in 20 households per community organization. The baseline surveys took place simultaneously in November and December 2006. The endline nutrition survey took place in advance of the endline socioeconomic survey, so that OSP would still be in the field, as the production season runs roughly from December to April, and roots are left in the field for piecemeal harvesting. Therefore, the endline nutrition survey took place in May and June 2009, whereas the endline socioeconomic survey took place in August and September 2009. The primary component of the nutrition survey was a 24-hour dietary intake survey administered in a randomly selected subsample of 12 households per organization. The dietary intake survey enumerated all foods eaten by reference children and mothers in the baseline in a 24-hour period following an initial household meeting. In the follow-up, we attempted to revisit the same households to measure changes in vitamin A consumption among the reference children and mothers.[12]

A total of 703 households were included in the socioeconomic survey baseline sample (Table 2). For participation in the survey, all households were required to have a resident child under 3 years old. In all 36 villages, the teams did 24-hour dietary intake recalls in 12 households as planned at baseline; the resulting sample was 441 reference children (column 2). In the endline survey conducted in 2009, 628 households were resurveyed in the socioeconomic survey, whereas 409 of the reference children were found and interviewed in the dietary intake survey. Attrition was balanced between treatment and control groups, and there were no obvious predictors of attrition found in the baseline. The nutrition survey included a dietary intake survey; when possible, anthropometric measures of all children were taken and recorded as well. We used the most recent WHO (2006) standards to calculate height-for-age Z (HAZ) and WHZ scores. For regressions in this paper, the sample size drops to 331, primarily due to missing data on other variables included in the data more generally.

3.1 Measuring Participation Intensity

We use the data to estimate the following general relationship:

$$Y_{is1} = \alpha_s + \beta Y_{is0} + \gamma P_{is} + \delta_1 A_{is1}^{\ 2} + \mu Z_{is0} + \varepsilon_{is1} \qquad (1)$$

where:

- Y_{is1} represents the outcome for individual *i* in strata *s* at endline ($t = 1$) and Y_{is0} is the outcome at baseline. We discuss outcomes in the next subsection.
- *P* represents the measure of participation, which is discussed after we discuss outcomes.
- α_s is a strata-specific constant, *A* represents the child's age at endline, and *Z* represents a vector of variables measured at baseline that may be correlated with nutritional outcomes.[13] We briefly discuss members of the vector *Z* at the end of this section.

[9] The intervention took place in 144 organizations in total.

[10] Before the fieldwork occurred in all communities, staff informed the leaders of that village about the survey and compiled a list of households that were members of the primary community organization that would be used as the organization for the intervention. From that list of households, 25 households with children under 3 years old were randomly selected from the list of community groups, where five were meant as replacement households. In a few cases, the enumeration staff found that the community lists did not accurately indicate when households actually had children under 3 years old living in them; we dropped dietary intake data collected for some children who were slightly older than 3 years.

[11] Randomization took place at a project meeting in Mozambique by selecting papers with village names on them from an urn.

[12] Although the project collected data on dietary intakes on a panel of mothers and a repeated cross-section of children age 6–35 months old at the endline, in this paper we focus on outcomes among reference children.

[13] By setting $\beta = 1$ and moving Y_{is0} to the right-hand side, equation (1) would be a classic difference-in-differences model. In the absence of correlation between P_{is} and Y_{is0}, equation (1) is more general than a difference-in-differences estimator.

- Finally, ε is a mean zero error term.

Based on including the lagged dependent variable on the right-hand side, we are estimating the impacts of participation intensity in an analysis-of-covariance framework, which typically has more statistical power than a difference-in-differences framework (McKenzie 2011).

3.2 Measuring Outcome Variables

We consider four outcome measures in this paper, though we only use three in the estimation.

- The first measure represents narrow impacts of the REU on the consumption of vitamin A in the diet. The REU could have affected the consumption of vitamin A by young children, primarily through additional OSP consumption. We define this measure as the density in micrograms (μg) of vitamin A measured in the child's dietary intake divided by the kilocalories (kcal) consumed in the dietary intake. Among children aged 4–6 years, assuming moderate physical activity and the corresponding energy intake recommendation, the recommended density would be 0.33 μg/kcal (WHO 2006).
- Second, it could be that the behavior change communications component of the REU would have also affected other nutrient intakes. However, given that other research has found little evidence of impacts of the REU on such behaviors (de Brauw et al. 2010, 2013), this channel may be unlikely.
- Third, increases in income could have had generic effects on micronutrient intakes. Ruel and Alderman (2013) show that the income elasticity of stunting—a cumulative measure of nutritional status among children under 5 years—is approximately –0.6. Therefore, we may observe impacts from either of the latter channels as impacts on increased dietary quality or diversity. We describe two measures of dietary quality below: mean micronutrient density adequacy (MMDA) and the dietary diversity score (DDS).
- Finally, given the focus on vitamin A, we also consider changes in WHZ as a potential "final" outcome. Vitamin A plays a central role in the immune system and in fighting infections. Reduced vitamin A deficiency could affect WHZ, which when examining the tail of the distribution is a measure of acute malnutrition.

Mean Micronutrient Density Adequacy

Diet quality can be measured by constructing a measure of the micronutrient density in the diet. Here, we define density as the amount of micronutrient intake per 100 kcal of energy intake. For all children, recommended nutrient intakes were taken from Food and Agriculture Organization of the United Nations (FAO)/World Health Organization (WHO) 2002 recommendations, except for calcium and zinc (IOM 1997, 2002).

For breastfed infants 6 to 11 months old, nine key micronutrients were considered: vitamin A, thiamin, riboflavin, vitamin B6, folate, vitamin C, calcium, iron, and zinc. For breastfed children age 12–23 months and for all non-breastfed infants, vitamin B12 was also considered for a 10 key micronutrients. For a given micronutrient, the desired nutrient density value from foods was computed using the reference nutrient intake (RNI) for that micronutrient and the recommended energy intake based on age and breastfeeding status. For breastfed children, average breast milk intake was assumed, and breast milk contribution was subtracted from the RNI to avoid overestimating breastfed children's diet inadequacy. Micronutrient density adequacy was calculated as the percentage of observed micronutrient density for each subject relative to the desired density for that subject, given his or her age and breastfeeding status. The procedure was repeated for each of the nine or 10 micronutrients, and the MMDA was calculated as the mean of all micronutrient density adequacies, with each capped at 100 percent.

Dietary Diversity Score

Dietary diversity has been shown to be associated with improved nutrient adequacy and nutritional status of children and adults in developing countries (Arimond and Ruel 2004). DDSs are good proxies for diet quality (Moursi et al. 2008). The DDS is summed over a total of seven possible food groups: (1) grains, roots, and tubers; (2) legumes and nuts; (3) dairy products; (4) flesh foods (meat, fish, poultry, and liver/organ meats); (5) eggs; (6) vitamin A-rich fruits and vegetables (above 130 retinol equivalent of vitamin A per 100 grams); and (7) other fruits and vegetables. A minimum of 1 gram of consumption was applied to all foods in order for them to count in the DDS. A score of 1 was assigned if a child ate one or more foods from a given food group and 0 if not. These scores were then summed up for all food groups with a range of 1 to 7 for the DDS.

Anthropometry

Finally, we used the anthropometric data collected in the nutrition survey to measure WHZ scores. WHZ scores measure more contemporaneous nutritional status, as they tend to drop with illness or with temporary food scarcity. We did not measure impacts on other anthropometric variables, such as HAZ scores. HAZ scores measure the cumulative nutritional status of children up to their current age, and are thought to be sensitive to micronutrient intakes during pregnancy and early childhood. We might not expect to observe changes in HAZ scores in the context of the REU, in part because of the small sample, but largely because the intervention was narrowly targeted at OSP and vitamin A, rather than at the consumption of a broad set of micronutrients.

We might expect to find increases in WHZ scores based on the following mechanism. The literature demonstrates that vitamin A intakes are associated with lower morbidity rates (Mayo-Wilson et al. 2011). Therefore, we would expect that WHZ scores could improve as morbidity is reduced. However, there is no direct link between vitamin A status and anthropometric measures. As the study was not designed with statistical power to demonstrate the impacts of the REU on WHZ scores, it would not be surprising if such impacts were not found.

3.3 Measuring Participation

We use varying measures of participation in the REU to try to associate final outcomes with increased participation. The crudest measure of participation intensity, of course, is an indicator for treatment, measuring an average intent-to-treat effect on the treated. Second, we can initially use dummy variables to measure varying levels of participation in either the agricultural or the nutritional components of the REU, relative to non-participation. We use them in sequential specifications of equation (1). However, these dummy variables are endogenous, and therefore introduce positive selection bias into our estimate of γ; we expect people who are more eager to participate in the intervention to have participated more intensely, and the coefficient estimate likely then reflects both the impacts of participating at that level of intensity and the unobservable impacts related to eagerness to participate at a specific level. To properly estimate the impacts of participation density using such dummy variables, we would require multiple instruments to be available for each type of participation, which is implausible.

Since it is impossible to find instruments for both types of participation, we turn to correspondence analysis to reduce the dimensionality of our measures of participation. Like factor analysis or principal component analysis (PCA), one of the main applications of correspondence analysis is data compression or dimensionality reduction. This approach was popularized in the economics literature by Filmer and Pritchett (2001), who constructed a wealth index using PCA from a large set of variables capturing household ownership of assets and durable goods. Intuitively, PCA is a technique for extracting from a large number of variables those few orthogonal linear combinations of the variables that best capture a common set of information, which is formally treated as a latent (unobserved) variable. PCA is a valid data reduction method for continuous variables or binary variables. Filmer and Pritchett's proposed method to deal with categorical variables is to break them into sets of dummy variables. One issue with this approach is that the information related to the ordering of the categories is lost. However, correspondence analysis is a valid data reduction method for categorical variables (Greenacre 2007).

We follow a data reduction approach and treat overall participation intensity in the project as an unobserved latent variable related to two observed measures of participation in the agricultural and nutritional components. We construct our participation index using correspondence analysis, because our two variables are categorical and because we want to preserve the information related to the ordering of the different categories. Mechanically, correspondence analysis takes the two dimensions of participation and coverts them to orthogonal measures, using statistical inertia as the measuring factor for the amount of explained variance. We find that 97 percent of the overall inertia in the two variables can be accounted for through the first dimensional score. The analysis suggests that two categorical measures of intensity have a strong association with overall participation intensity.

If we plot the two orthogonal dimensions of the correspondence analysis together (Figure 3), we note that the first dimension seems to capture the intensity of participation in the project by clearly separating those households who were not exposed to the project from those who were exposed. If we focus on the agriculture component, farmers who received vines and participated in trainings show a higher intensity of participation along the first dimension compared with farmers who only received vines. The same interpretation can be held for the nutritional component, with the promoters being the more intense participants. As such, the results suggest we can use the first score in each of the participation components to reduce the dimensions of our categorical participation variables. These scores can be interpreted as summarizing the intensity of participation in the agriculture and the nutritional components of the REU. However, as with principal components, these scores face the problem that they are somewhat difficult to interpret. Therefore, we focus our interpretation of regression coefficients on the sets of dummy variables, and we use the results of correspondence analysis to compare results estimated with OLS and with instrumental variable models.

Given that we have a measure of participation intensity, we must next attempt to deal with its endogeneity. To do so, we use a set of three instrumental variables that arguably do not affect children's nutritional status, except through their impact on program participation. The first instrument, which is obviously exogenous, is the treatment indicator variable. We would rely only on the treatment indicator, but it does not help differentiate intensity in a local average treatment effect sense. Therefore, we use a second variable as an instrument, which measures whether or not the household grew sweet potato prior to the intervention. Here, the intuition is that households who grew any type of sweet potato prior to the intervention would be more likely to participate and would participate more intensely, which would not plausibly have an independent effect on child outcomes three years later. Third, we use the number of male agricultural laborers in the household at baseline. We hypothesize this variable will affect agricultural participation intensity, but not necessarily nutrition participation intensity. In northern Mozambique, labor or planting material are the input constraints limiting what households grow, rather than land as in much of sub-Saharan Africa. In other words, households may have had available land to grow OSP, but needed available labor to tend to OSP. In households with more male laborers available, more labor could be spared to growing OSP once the program began, especially if the initial vine allocations were to be multiplied. For each outcome and intensity measure, we provide estimates of the results using both OLS and this set of instruments.

3.4 Other Variables

We finally include a vector of other variables that might affect nutritional outcomes and participation intensity. We measure most variables at baseline. The vector includes whether or not children were breastfed; total household expenditures; whether or not the mother knew of vitamin A; the difference in the mother's and child's age, in months; and the distance from the household to the village center.

4. RESULTS

4.1 Descriptive Statistics on Outcomes

We first present descriptive statistics on all four outcomes of interest among both the treatment and the control groups (Table 3). Beginning with the narrow measure, we find a substantial improvement in vitamin A density in the diet of treated children; this finding is in line with evidence presented in Hotz et al. (2012a). Looking at the box plot of the distribution (Figure 4), the entire distribution of vitamin A density in the diet has shifted upward in the treatment group, demonstrating that the REU was effective at increasing vitamin A in the diet, and that in the treatment group the average child was obtaining adequate vitamin A in the diet. This was not the case in the control group.

To show that differences also exist by intensity, we repeat the box plot in Figure 4 for the categorical variables describing both agricultural participation (Figure 5) and nutritional participation (Figure 6). We find that along both dimensions, children in households with more participation in the project had higher vitamin A density in their diets. In other words, the average for the treatment group masks some substantial—and not surprising—heterogeneity. Even children in households that received some agricultural extension in addition to the sweet potato vines have stronger outcomes related to vitamin A intakes. This figure suggests we should be able to find differences in outcomes related to participation intensity, even when we control for other explanatory variables.

Among the dietary diversity measures in Table 3, we again find significant differences between the treatment and control groups, although differences are not as large as for the narrow measure. The averages among the control group suggest that children at endline eat about 3.6 out of 7 foods, and the MMDA is about 69 percent of what it should be. There is clearly a substantial gap between a healthy, diverse diet, and the diet of children in the control group at endline. For both variables, dietary diversity is improved among the treatment group at endline relative to the control group, to 3.9 foods out of 7 and to 72 percent of MMDA.

Finally, we examine average WHZ scores among our sample (Table 3). Although WHZ scores are slightly higher on average in the treatment group, the difference between groups is not statistically significant. It may be that the sample is not large enough to demonstrate differences; when we limit the sample to the proper ages and drop outliers, we end up with a smaller sample of children for whom we have valid WHZ scores than the sample for the dietary intake outcomes. The study was not designed to demonstrate impacts on WHZ scores, focusing instead on ensuring that impacts on dietary intakes could be detected. However, it could be that the intervention did not create a large enough improvement in nutritional status to demonstrate changes in WHZ scores. Given that there is no statistical difference in means between groups, we do not estimate impacts of participation intensity on WHZ scores.

4.2 Empirical Results

We next estimate equation (1) using vitamin A density in the diet as the outcome variable (Table 4). We initially use the treatment indicator as the primary explanatory variable, and we find a significant impact of the REU on vitamin A density in the diet (column 1).[14] The coefficient is

[14] We find few significant coefficients among additional explanatory variables. Vitamin A knowledge at baseline is positively correlated with the density of

large; at 0.203, the coefficient represents nearly 63 percent of the recommended density. The result is also consistent with Hotz et al. (2012a), though this specification of the dependent variable is slightly different from the one used by Hotz et al. In column 2, we replace the treatment indicator with indicators for households who both received vines and participated in extension, and for those who only received vines. We find that the point estimate on households participating in extension is substantially higher than those just receiving vines; the difference is statistically significant at the 5 percent level. The higher coefficient represents almost the entire recommended density of vitamin A in the diet. However, one should be cautious about attributing all of the coefficient to the REU; positive selection may have also mattered for participation.

In column 3, we replace the agricultural participation indicators with nutritional participation indicators. The point estimate among children with mothers who were promoters is 0.4, or higher than the recommended minimum vitamin A density; it is also larger than the point estimate among children with mothers who were not promoters, but participated in training. However, the difference between the two coefficients is not statistically significant.

We then convert the agricultural and nutritional intensity measures into scores using correspondence analysis, and we estimate the relationship between vitamin A density and those scores using both OLS (columns 4 and 5) and IV methods (columns 6 and 7). The OLS coefficients suggest that there are positive relationships between vitamin A density and participation intensity in both the agricultural and the nutritional components of the intervention. These coefficients are difficult to interpret because the indices are composites, but the positive relationship suggests that the intensity matters. However, this estimate still may reflect some positive selection into different types of participation. Our IV estimates, which should control for the endogeneity of participation intensity, are slightly lower, and suggest that additional participation intensity causes larger gains in vitamin A density in the diet.[15] The IV estimates suggest a small amount of positive selection in each measure. In sum, we find that the REU affected the density of vitamin A in children's diets, and the level of participation is also important to this narrow measure of nutritional impact.

Given impacts on narrow nutritional status, we next examine the two broad measures of nutritional impact, beginning with the MMDA (Table 5). In this case, we again find a positive impact of the treatment indicator, though it is only significant at the 10 percent level (column 1). The coefficient estimate suggests that the overall adequacy improves by approximately 2.5 percent on average with the introduction of OSP through the REU. Since the average MMDA was 69 percent within the control group, this coefficient suggests a 3.6 percent increase in micronutrient adequacy. When we replace the treatment indicators with agricultural participation indicators (column 2), we again find a large difference between farmers who received vines and extension and those who just received vines. However, the difference is not significantly different from zero, because the coefficient on farmers who only received vines is imprecisely estimated, and is not significantly different from zero. We find a similar result for the nutritional intensity indicator variables (column 3). Whereas the MMDA is positively correlated with promoter status, and the correlation is statistically significant, among mothers just attending extension trainings the impacts were smaller and not significantly different from zero. Among promoters, the magnitude of impacts is substantially larger than the average treatment effect, at 4.2 percent, though this coefficient may reflect some self-selection. The results using the agriculture and nutrition scores are again positive and significant at the 5 percent level (columns 4–7). The IV results act largely to confirm the OLS results, as in this case they do not suggest any positive selection bias.

The dietary diversity indicator yields results with a consistent pattern (Table 6). We find that the treatment effect is 0.245, and is significant at the 5 percent level (column 1). The coefficient suggests that the average child in the treatment group eats food from 0.19 additional food groups. Since the average diet was inadequate in diversity within the control group, the impact brings children closer to adequate diversity, defined as 4 out of 7 food groups represented. However, the indicators among both agriculture and nutrition suggest that dietary diversity among those more intensely participating is much higher than among lower-intensity participants or nonparticipants. Both differences are statistically significant, and point estimates suggest 0.4 (agriculture) or 0.6 (nutrition) more types of foods consumed. The OLS results, when using the agriculture and nutrition score variables (columns 4 and 5), largely confirm the results from the indicator variables, as they suggest that additional intensity is positively correlated with DDSs. When we instrument for the score variables, the point estimates decrease somewhat, which is suggestive of a slight positive selection bias in the OLS results. Nevertheless, these results again lend credence to the idea that additional participation intensity improves nutritional outcomes.

In sum, we find that the REU appears to have also had modest impacts on broad measures of nutritional status.

vitamin A in the diet at endline; the age difference between mother and child is positively correlated with MMDA, and the distance to the village is negatively correlated with the dietary diversity score.

[15] First stage estimates can be found in Table 7.

Since the results are so modest and we do not find significant impacts in general on intakes of other nutrients (de Brauw et al. 2010), changes in broad indicators likely primarily occur through increases in vitamin A intakes. We also find that more intensive participation, in terms of either the agricultural or the nutritional component of the intervention, leads to larger impacts, and even the existence of impacts is questionable for some measures among the lower-intensity participants, relative to non-participants and the control group. We revisit this issue in the following section, after we estimate impacts on final outcomes.

4.3 Discussion of Results

In a nutshell, we find that the REU had fairly sizable impacts on narrow measures of nutritional intakes but smaller impacts on broad impacts of nutritional intakes, and did not change WHZ scores among children. The outcomes of the REU vary substantially when we measure participation intensity in the two major components of the intervention. Whether we consider the agricultural or the nutritional component of the intervention, we find that point estimates for impacts among the more intensely participating group are higher than for the less intensely participating group. We must be careful in interpreting these results, because of positive selection into more intense participation. However, once we condense the results into intensity scores using correspondence analysis, we find no differences using instrumental variables, giving us some confidence that more intense participation causes larger impacts from programs like the REU.

Therefore, the question becomes the following: How should these results affect program design? There are clear advantages to trying to induce higher program participation: the impacts are substantially larger in magnitude, on both narrow and broad measures of nutritional intakes among those more intensely participating, in both the agricultural and the nutritional components. There is a slight, and important, difference between the design of the agricultural and nutritional components of the intervention that should be taken into account in considering potentially improved designs.

Considering the agricultural component, the effects on child nutrition are larger when farmers also participate in extension, than among people who just receive vines. This is indirect evidence that the extension was effective, as one could argue that such households should have had more OSP availability than other participating farmers. However, from a program design perspective, the question is how to induce farmers to participate in the extension; in the REU, there was an expectation that farmers would participate, but it was not mandatory. Two ways to alter the design would be to try to induce farmers to participate through incentives, or to penalize farmers who do not participate in extension by, for example, not including them in future vine distributions. Of these methods, the former is likely preferable, since it maintains positive feelings about the program. It is important to ensure that incentives offered are small and not distracting, and do not substantially affect the overall project budget. For example, holding public lotteries for hats, T-shirts, or other similar objects that are already being produced by the project could boost participation if the lottery were announced in advance.

The results for the nutritional component have a different interpretation. As discussed in the program description, nutrition promoters were selected in each village to lead the nutrition trainings. Nutrition extensionists largely worked with the group of promoters, and then the promoters were charged with scheduling and holding meetings within the village. The impact evaluation report demonstrated that the nutrition promoters learned substantially more than other mothers within the intervention villages (de Brauw et al. 2010). The additional learning appears to translate to larger impacts along both the narrow and the broad measures of nutritional status. In fact, on the MMDA measure, the impact estimate is not significant among the mothers who were only participating in extension. The implication is that the extensionist-promoter model clearly served promoters well, but likely did not lead to enough contact with mothers who were not promoters, which is illustrated in these results. In other words, the model likely underserved the needs of mothers who were only taught messages by promoters.

From a program design perspective, then, a model of direct extensionist contact with mothers would seem likely to have stronger impacts on nutritional status than the extensionist-promoter model. The problem is that the extensionists could not have alone worked with the number of mothers that were supposed to be served by the project. However, they could have worked with more than 10 promoters per village. Further, their workload could have been reduced by decreasing the number of messages that the nutrition extensionist attempted to disseminate. In other words, reducing the contact somewhat between the extensionist and each village could offset the workload increase of working with more mothers.

5. CONCLUSION

In this paper, we have measured the heterogeneity of impacts related to participation intensity for a biofortification program, the REU in Mozambique. As discussed in the introduction, the primary theory of change was that farmers would adopt OSP, and then based on the nutritional messaging, OSP would be fed to younger children and mothers of child-bearing age, reducing vitamin A deficiency in those populations. The nutritional messaging component also covered other nutritious foods, so we might have expected further impacts on indicators measuring a broader diet, such as MMDA or DDSs. In this paper, we study nutritional impacts among children who were under 3 years old at baseline, and find that from both a narrow and a broad perspective, dietary intakes were improved more among those who participated more intensely in the program than among those who did not.

One contribution of the paper is to develop a way to measure participation intensity, through the use of correspondence analysis of categories of participation on two different dimensions. This measure is certainly imperfect, as it is not possible to give a simple interpretation of results. Also, because the agricultural and nutritional components were not offered separately in this case, we cannot disentangle the impacts of participation intensity in the two components. However, the method does allow for the use of instrumental variables to demonstrate that impacts, disaggregated by participation intensity, are not completely driven by positive participation bias. In other words, we can be reasonably assured that the heterogeneity in impacts is due to the program components, and not just to some individuals who were more eager to participate. Consequently, this method could be useful in exploring the heterogeneity of impacts in other programs with several components.

As the average program impacts on broad measures of nutritional intakes are relatively modest, from a design perspective, the fact that we find stronger results among those who participated more intensively is important. It suggests that average program impacts could have been improved through changes in the design. From an agricultural perspective, inducing more farmers to participate in extension could have had larger impacts. Experimenting with methods to improve participation could be worthwhile, from the perspective of increasing nutritional impacts. From the nutritional perspective, direct contact between extensionists and promoters had larger impacts than contact between promoters and other mothers in the community. The promoter model may have just been ineffective in serving mothers who were not promoters themselves.

To increase impacts cost-effectively, one would want to consider the cost implications of increasing the number of mothers with whom extensionists work, while reducing the number of contact visits between the extensionists and each village, to attempt not to increase the workload among extensionists. If such a model could be developed to have a similar cost per beneficiary, it would almost certainly have larger impacts overall based on our findings.

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APPENDIX

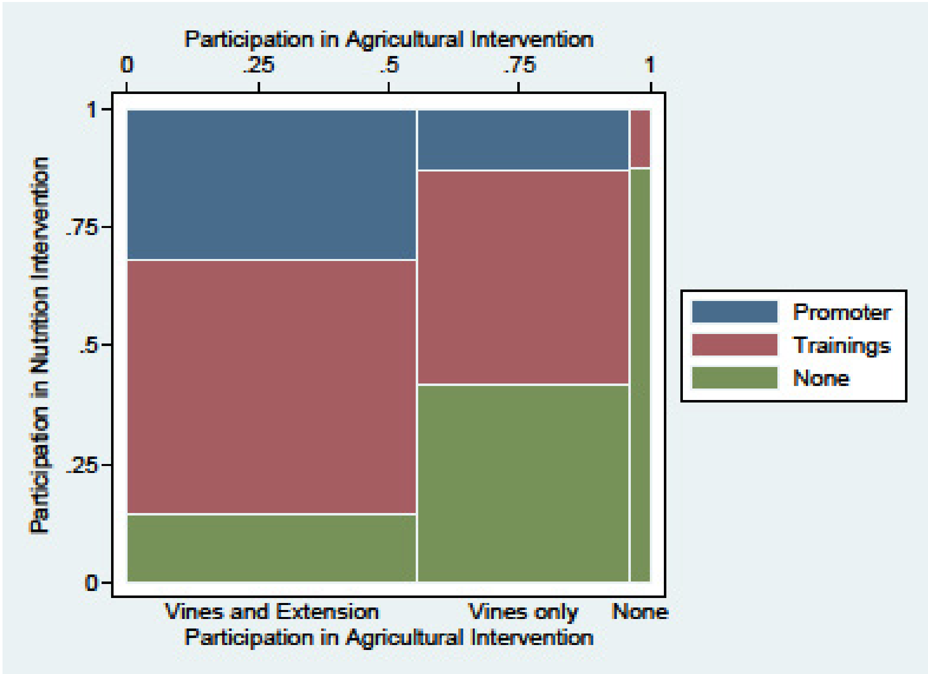

Figure 1: Participation in agricultural and nutrition components of REU, Mozambique

Figure 2: Location of the REU project sites in Zambia Province, Mozambique

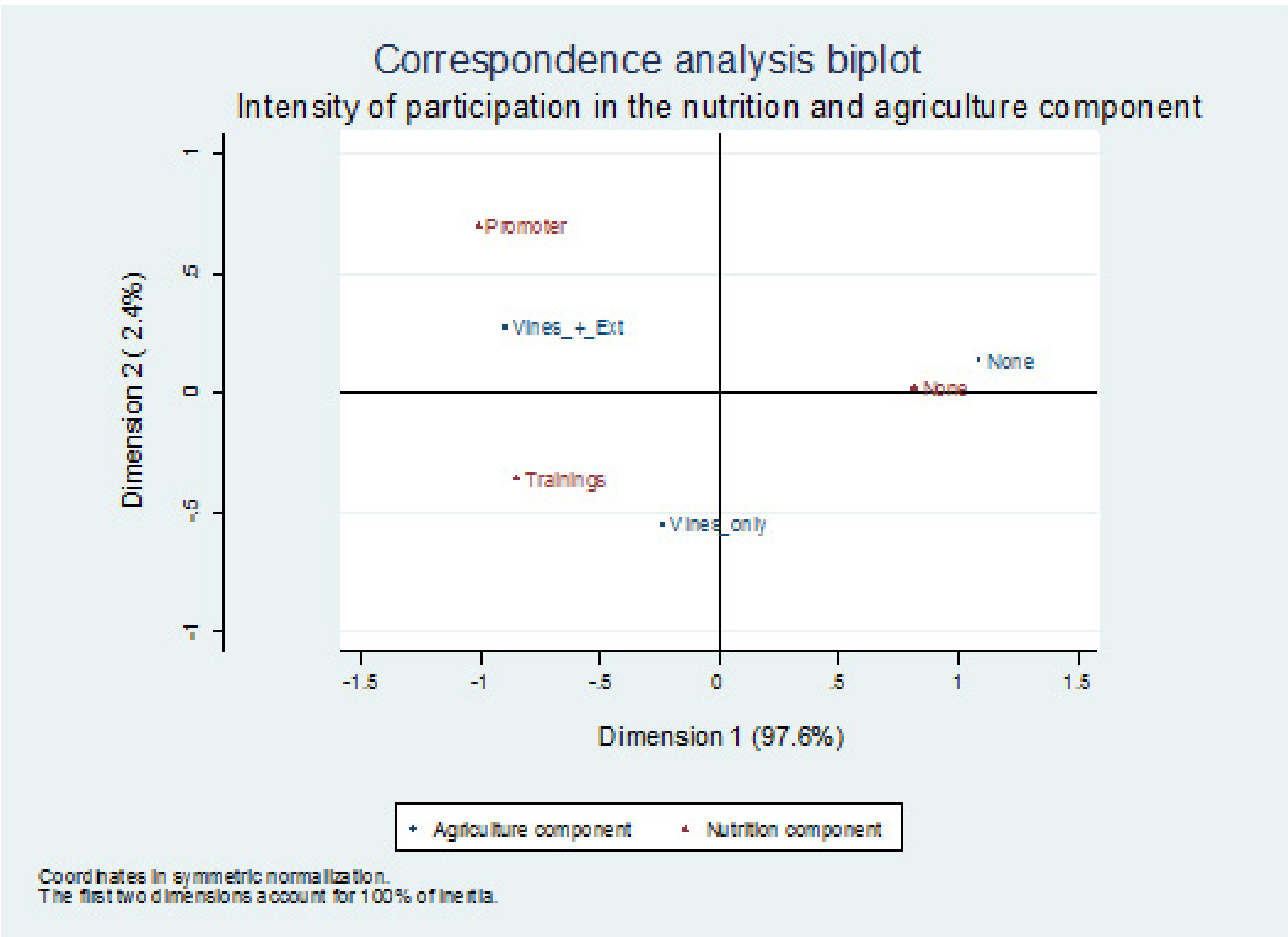

Figure 3: Relationship of participation in agricultural and nutritional components using correspondence analysis, REU, Mozambique

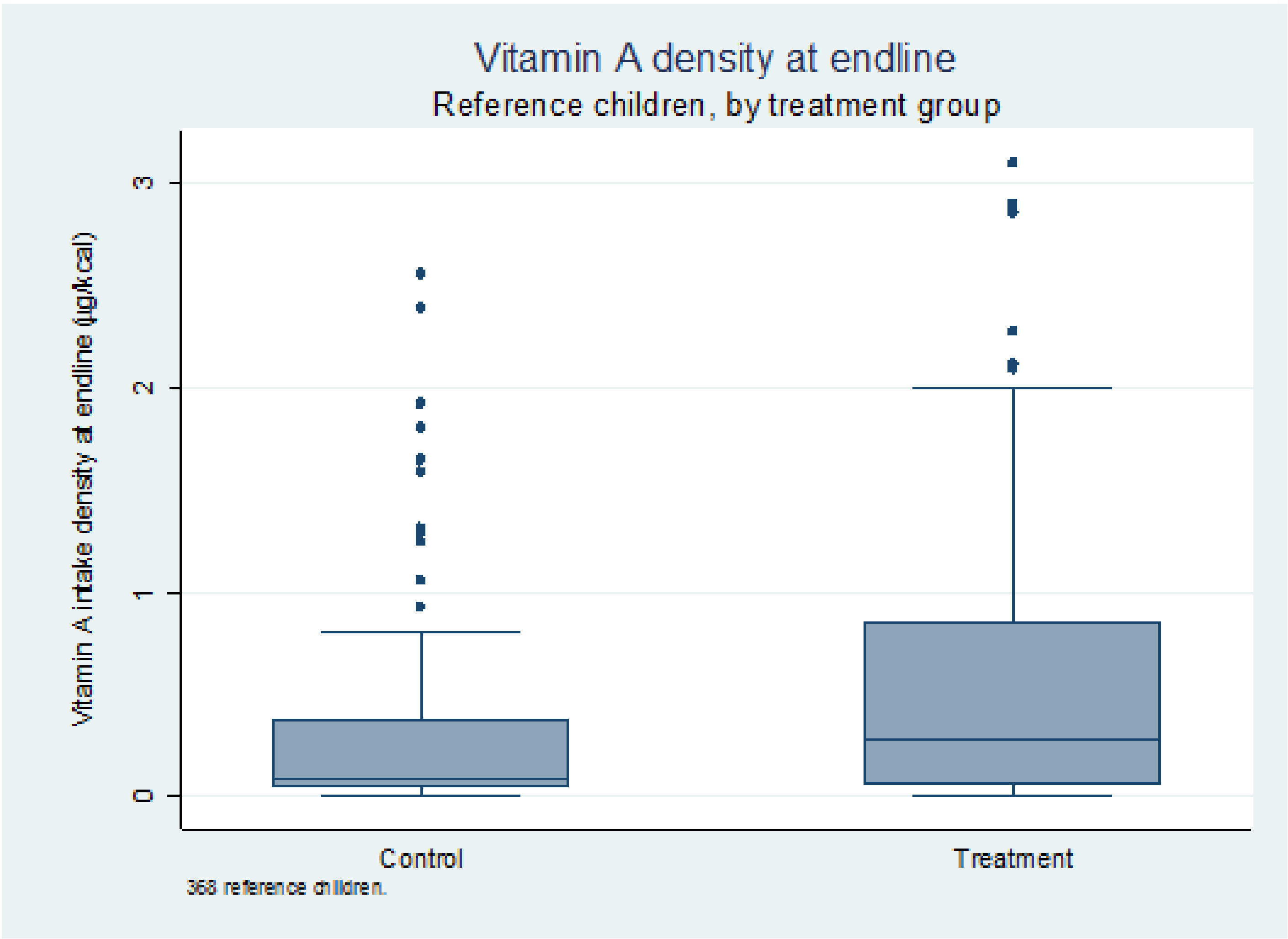

Figure 4: Vitamin A density, children age 4–6 at endline, by treatment group

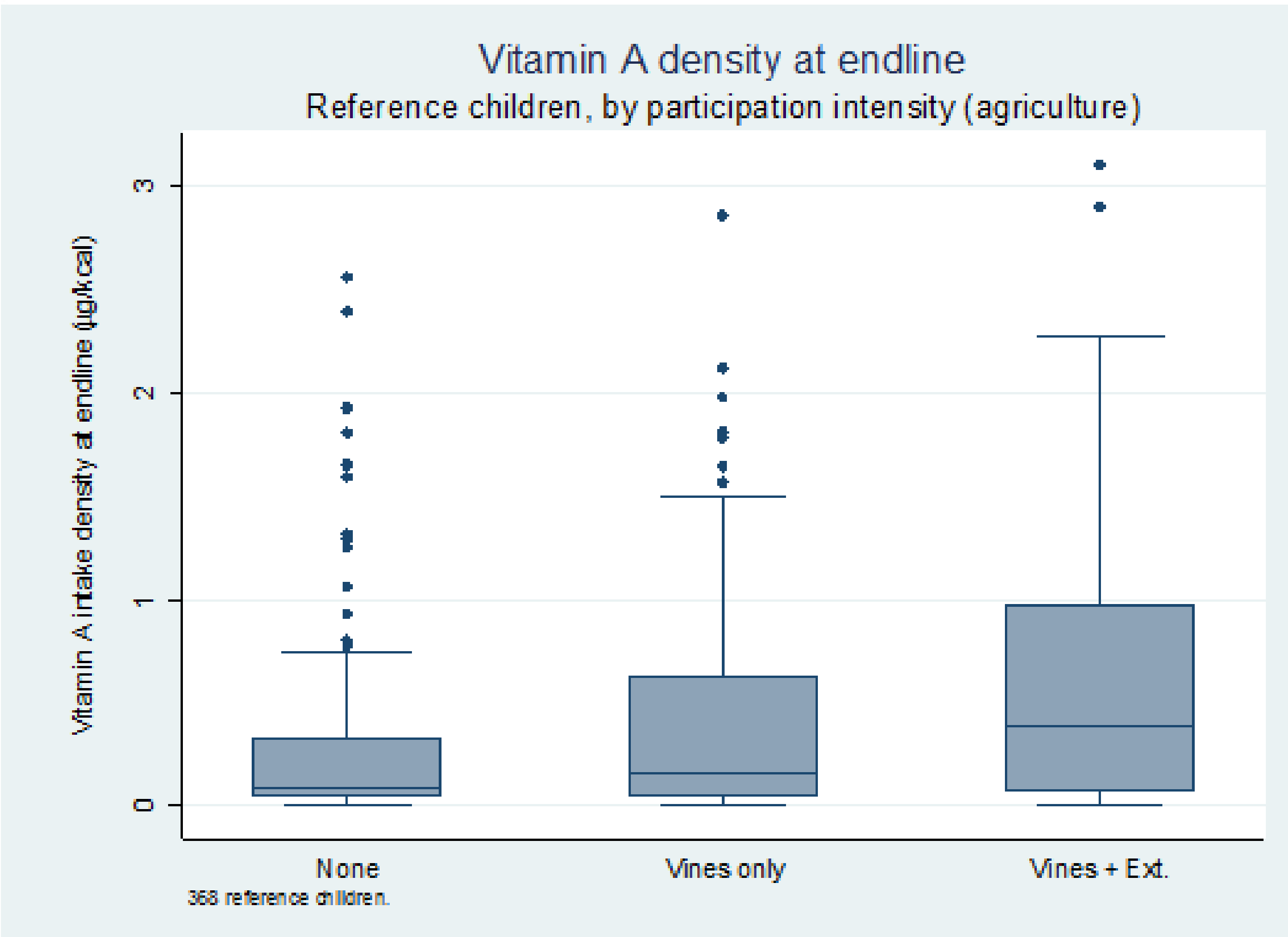

Figure 5: Vitamin A density, children age 4–6 at endline, by participation in agricultural component

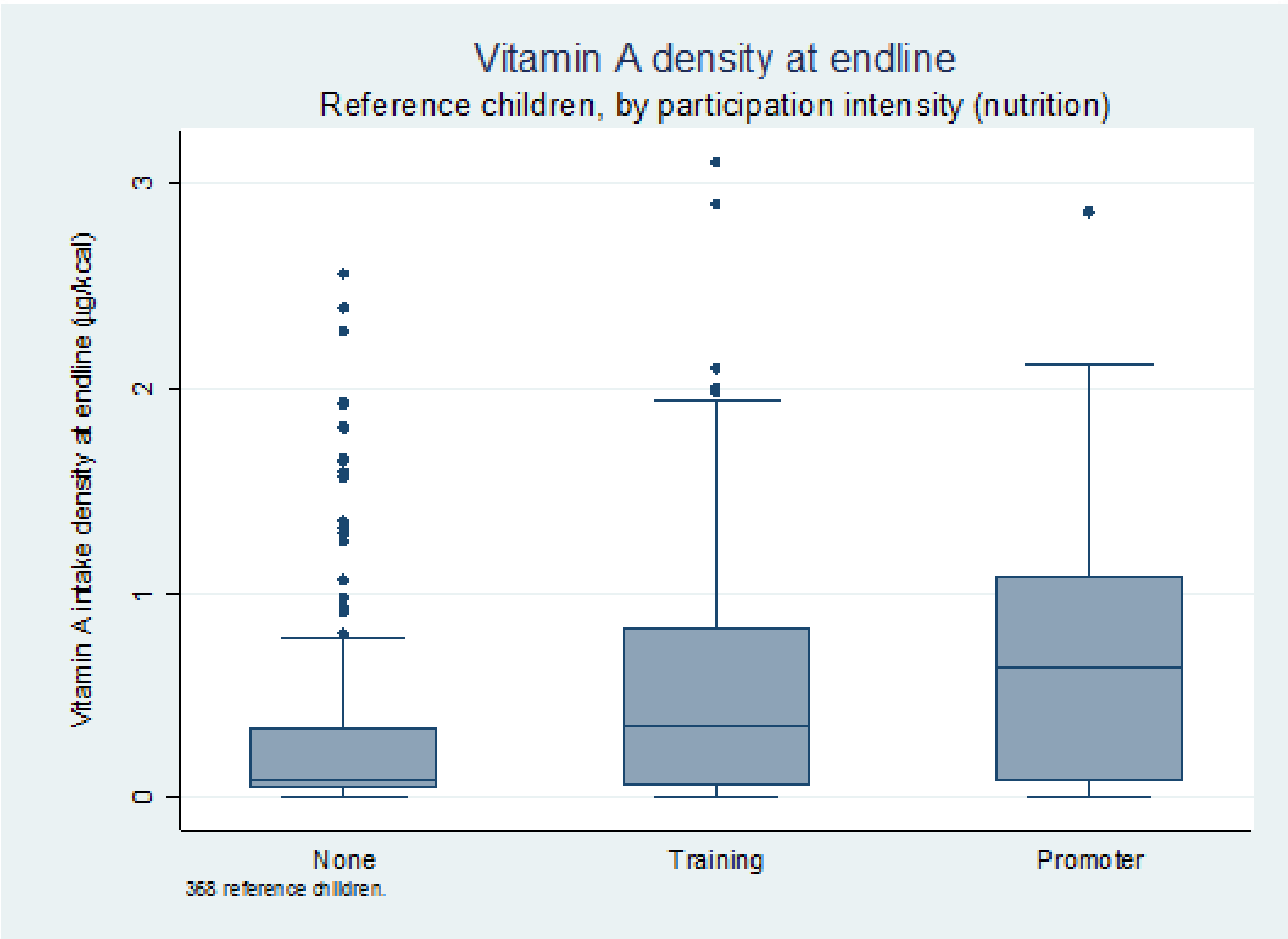

Figure 6: Vitamin A density, children age 4–6 at endline, by participation in nutritional component

Table 1: Participation in REU by agricultural and nutritional components, Mozambique

Participation in Agricultural Component	Participation in Nutritional Component		
	Promoter	Trainings Only	None
Full Sample			
Vines and Extension	73	124	33
Vines Only	22	77	71
None	0	2	14
Dietary Intake Sample			
Vines and Extension	47	76	16
Vines Only	14	45	38
None	0	0	8

Source: REU Baseline and Endline Surveys, Mozambique, 2006 and 2009

Table 2: Overall Sample Size and Structure, REU Impact Evaluation Surveys, Mozambique

Sample Components	Baseline Survey 2006	Endline Survey 2009
Overall Socioeconomic Survey	703	628
Dietary Intake, Reference Children	441	409

Table 3: Average outcomes, by treatment status, endline

Measurement Factors	Treatment	Control	*p*-Value
Vitamin A Density	0.534	0.300	<0.001
	(0.031)	(0.029)	
Mean Micronutrient Density Adequacy	72.0	68.9	0.010
	(0.71)	(0.96)	
Dietary Diversity Score	3.88	3.59	0.020
	(0.06)	(0.07)	
Weight-for-Height Z Score	0.121	–0.054	0.113
	(0.061)	(0.091)	

Source: REU Endline Survey, Mozambique, 2009

Note: Clustered standard errors in parentheses

Table 4: Impacts on vitamin A density in diet, Reaching End Users program, Mozambique, by participation intensity

Dependent: vitamin A density 2009	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) IV-2SLS	(7) IV-2SLS
Vitamin A density, baseline	−0.073	−0.057	−0.068	−0.059	−0.076	−0.068	−0.069
	(0.129)	(0.132)	(0.122)	(0.129)	(0.120)	(0.123)	(0.110)
Child age in months	0.038	0.041	0.055	0.041	0.055	0.031	0.047
	(0.047)	(0.047)	(0.045)	(0.047)	(0.045)	(0.045)	(0.045)
Age squared (/1,000)	−0.328	−0.353	−0.496	−0.348	−0.491	−0.255	−0.415
	(0.441)	(0.437)	(0.427)	(0.438)	(0.426)	(0.418)	(0.421)
Age difference (mother-child)	0.006	0.006	0.006	0.006	0.006	0.006	0.006
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)
Breastfed at baseline (1 = yes)	0.101	0.088	0.084	0.093	0.093	0.122	0.112
	(0.101)	(0.101)	(0.098)	(0.101)	(0.099)	(0.095)	(0.095)
Total household (HH) expenditures, baseline	−0.025	−0.019	−0.026	−0.019	−0.025	−0.017	−0.019
	(0.067)	(0.065)	(0.062)	(0.065)	(0.060)	(0.058)	(0.053)
Mother's knowledge of vitamin A, 2006	0.134*	0.123*	0.132**	0.124*	0.128*	0.112*	0.122**
	(0.0668)	(0.065)	(0.062)	(0.065)	(0.063)	(0.063)	(0.062)
HH distance to village (km)	−0.015	−0.010	−0.013	−0.009	−0.014	−0.012	−0.015
	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.023)	(0.023)
Treatment group indicator	0.203***						
	(0.068)						
Agriculture, vines plus extension		0.296***					
		(0.069)					
Agriculture, vines only		0.139*					
		(0.079)					
Nutrition, promoter			0.401**				
			(0.147)				
Nutrition, meetings only			0.261***				
			(0.062)				
Score, agriculture				0.145***		0.136***	
				(0.036)		(0.038)	
Score, nutrition					0.180***		0.169***
					(0.038)		(0.045)
p-value, intensity coefficients are equal		6.68	0.80				
		(0.014)	(0.377)				
Observations	331	331	331	331	331	331	331
R-squared	0.059	0.076	0.102	0.075	0.099	0.074	0.098

Source: REU Baseline and Endline Surveys, Mozambique, 2006 and 2009

Notes: Clustered standard errors in parentheses. * indicates significance at the 10 percent level; ** indicates significance at the 5 percent level; ***indicates significance at the 1 percent level. Stratum dummy variables included in all regressions.

Table 5: Impacts on mean micronutrient density adequacy, Reaching End Users program, Mozambique, by participation intensity

Dependent: MMDA 2009	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) IV-2SLS	(7) IV-2SLS
MMDA at baseline	0.039	0.037	0.036	0.035	0.040	0.057	0.060
	(0.068)	(0.067)	(0.068)	(0.067)	(0.069)	(0.063)	(0.063)
Child age in months	0.041	0.080	0.202	0.0748	0.193	–0.131	0.0676
	(0.851)	(0.836)	(0.812)	(0.842)	(0.807)	(0.819)	(0.810)
Age squared (/1,000)	0.947	0.554	-0.740	0.631	-0.545	2.14	0.256
	(7.72)	(7.63)	(7.39)	(7.65)	(7.35)	(7.52)	(7.43)
Age difference (mother-child)	0.120*	0.123*	0.117*	0.122*	0.121*	0.131**	0.131**
	(0.067)	(0.066)	(0.067)	(0.067)	(0.067)	(0.063)	(0.064)
Breastfed at baseline (1 = yes)	1.277	1.091	1.043	1.171	1.198	0.697	0.701
	(1.690)	(1.729)	(1.712)	(1.714)	(1.710)	(1.594)	(1.585)
Total HH expenditure at baseline	1.751	1.808	1.707	1.810	1.727	1.955	1.827
	(1.299)	(1.298)	(1.281)	(1.295)	(1.291)	(1.313)	(1.291)
Mother's knowledge of vitamin A in 2006	1.746	1.584	1.731	1.608	1.704	1.285	1.515
	(1.513)	(1.524)	(1.533)	(1.505)	(1.518)	(1.445)	(1.471)
HH distance to village (km)	–0.497	–0.439	–0.501	–0.435	–0.519	–0.401	–0.417
	(0.561)	(0.577)	(0.561)	(0.580)	(0.563)	(0.571)	(0.572)
Treatment group indicator	2.365*						
	(1.394)						
Agriculture, vines plus extension		3.444**					
		(1.607)					
Agriculture, vines only		1.285					
		(1.544)					
Nutrition, promoter			4.216*				
			(2.426)				
Nutrition, meetings only			2.187				
			(1.431)				
Score, agriculture				1.654**		1.774**	
				(0.812)		(0.773)	
Score, nutrition					1.679*		2.274**
					(0.853)		(0.967)
p-value, intensity coefficients are equal		2.49	0.82				
		(0.124)	(0.370)				
Observations	331	331	331	331	331	331	331
R-squared	0.068	0.075	0.077	0.073	0.075	0.073	0.074

Source: REU Baseline and Endline Surveys, Mozambique, 2006 and 2009. Notes: Clustered standard errors in parentheses. * indicates significance at the 10 percent level; ** indicates significance at the 5 percent level; ***indicates significance at the 1 percent level. Stratum dummy variables included in all regressions

Table 6: Impacts on dietary diversity score, Reaching End Users Program, Mozambique, by participation intensity

Dependent: DDS 2009	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) OLS	(7) 2SLS-IV
DDS at baseline	0.139*	0.147*	0.107	0.140*	0.133*	0.135*	0.127*
	(0.078)	(0.073)	(0.075)	(0.074)	(0.0761)	(0.0739)	(0.0747)
Child age in months	–0.064	–0.0612	–0.037	–0.061	–0.042	–0.084	–0.069
	(0.080)	(0.0799)	(0.074)	(0.079)	(0.074)	(0.072)	(0.068)
Age squared (/1,000)	0.475	0.443	0.197	0.449	0.266	0.658	0.513
	(0.727)	(0.724)	(0.665)	(0.714)	(0.660)	(0. 647)	(0.611)
Age difference (/100) (Mother-child)	–0.015	0.039	–0.043	0.0003	0.0001	0.001	0.002
	(0.711)	(0.699)	(0.705)	(0.007)	(0.007)	(0.007)	(0.007)
Breastfed at baseline (1 = yes)	–0.015	–0.041	–0.049	–0.025	–0.024	–0.050	–0.055
	(0.127)	(0.130)	(0.123)	(0.130)	(0.132)	(0.122)	(0.124)
Total HH expenditure at baseline	0.116	0.125	0.119	0.126	0.119	0.123	0.110
	(0.089)	(0.084)	(0.085)	(0.087)	(0.090)	(0.083)	(0.084)
Mother's knowledge of vitamin A in 2006	0.060	0.039	0.058	0.044	0.052	0.057	0.066
	(0.134)	(0.132)	(0.124)	(0.133)	(0.126)	(0.131)	(0.127)
HH distance to village (km)	–0.072**	–0.061*	–0.063*	–0.060*	–0.067**	–0.062**	–0.066**
	(0.032)	(0.032)	(0.032)	(0.032)	(0.032)	(0.029)	(0.028)
Treatment group indicator	0.197*						
	(0.106)						
Agriculture, vines plus extension		0.401***					
		(0.106)					
Agriculture, vines only		0.078					
		(0.123)					
Nutrition, promoter			0.610***				
			(0.154)				
Nutrition, meetings only			0.258**				
			(0.098)				
Score, agriculture				0.186***		0.146**	
				(0.055)		(0.058)	
Score, nutrition					0.220***		0.186**
					(0.052)		(0.079)
F-statistic, intensity coefficients are equal (*p*-value)		9.88 (0.004)	4.67 (0.038)				
Observations	331	331	331	331	331	331	331
R-squared	0.095	0.123	0.141	0.115	0.128	0.112	0.126

Source: REU Baseline and Endline Surveys, Mozambique, 2006 and 2009
Notes: Clustered standard errors in parentheses. * indicates significance at the 10 percent level; ** indicates significance at the 5 percent level; ***indicates significance at the 1 percent level. Stratum dummy variables included in all regressions

Table 7: First stage regressions, instrumental variables estimates

First stage regression results (OLS)		(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:							
		Agriculture score	Nutrition score	Agriculture score	Nutrition score	Agriculture score	Nutrition score
Instruments							
Dummy, treatment		1.639**	1.277**	1.641**	1.276**	1.64**	1.28**
		(0.035)	(0.0749)	(0.0348)	(0.0748)	(0.034)	(0.075)
Former experience with		-0.009	0.206*	-0.009	0.206*	-0.008	0.204
Sweet potatoes		(0.059)	(0.119)	(0.057)	(0.120)	(0.057)	(0.121)
Number of Male		0.038*	0.062	0.038*	0.0620	0.037*	0.062
Agricultural Laborers		(0.019)	(0.038)	(0.020)	(0.0384)	(0.019)	(0.038)
	Other Explanatory Variables						
Vitamin A density		-0.084	0.035				
baseline	(0.088)	(0.208)					
MMDA baseline				0.002	-0.001		
				(0.003)	(0.004)		
DDS baseline						0.007	0.033
						(0.030)	(0.049)
Child age in months:		-0.019	-0.103*	-0.025	-0.010*	-0.0217	-0.109*
09		(0.029)	(0.055)	(0.030)	(0.056)	(0.027)	(0.056)
Child age, squared		0.166	0.955*	0.230	0.926	0.196	1.01*
(/1000)		(0.283)	(0.547)	(0.295)	(0.565)	(0.267)	(0.556)
Age difference (Mother-		-0.002	-0.004	-0.002	-0.004	-0.002	-0.004
child)		(0.003)	(0.005)	(0.003)	(0.005)	(0.003)	(0.005)
Breastfed at baseline		0.057	0.015	0.063	0.012	0.059	0.014
(1 = yes)		(0.051)	(0.099)	(0.053)	(0.097)	(0.051)	(0.098)
Total HH expenditure		-0.027	0.008	-0.025	0.008	-0.026	0.004
at baseline		(0.044)	(0.072)	(0.043)	(0.074)	(0.045)	(0.071)
Mother's knowledge of		0.062	0.015	0.078	0.008	0.070	0.013
Vitamin A in 2006		(0.051)	(0.093)	(0.054)	(0.088)	(0.055)	(0.090)
HH distance to village		-0.021	0.001	-0.022*	0.001	-0.022	0.001
(km)		(0.013)	(0.020)	(0.013)	(0.020)	(0.013)	(0.019)
F-statistic, instruments		756.4	152.2	756.4	153.2	778	155.1
Hansen J test		0.885	0.455	1.40	0.609	1.10	0.609
(overidentification)		(0.643)	(0.797)	(0.496)	(0.658)	(0.577)	(0.738)
Observations		331	331	331	331	331	331
R-squared		0.829	0.508	0.829	0.508	0.829	0.509

Source: REU Baseline and Endline Surveys, Mozambique, 2006 and 2009. Notes: Clustered standard errors in parentheses. * indicates significance at the 10 percent level; ** indicates significance at the five percent level. Stratum dummy variables included in all regressions. For Hansen J test, p-value is in parentheses