Contract Farming and Food Security*

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Abstract

Contract farming has often been associated with an increase in the income of participating households. It is unclear, however, whether contract farming increases other aspects of household welfare. We use data from six regions of Madagascar and a selection-on-observables design in which we control for a household's marginal utility of participating in contract farming, which we elicited via a contingent valuation experiment, to show that participating in contract farming reduces the duration of a household's hungry season by about eight days on average. Further, participation in contract farming makes participating households about 18 percent more likely to see their hungry season end at any time. Further, we find that these effects are more pronounced for households with more children, and for households with more girls. This is an important result as children—especially girls—often bear the burden of food insecurity.

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Although the benefits of economic specialization have been widely understood since the publication of Adam Smith's (1776, 1976) *Wealth of Nations*, if not earlier, a persistent lack of specialization is one of the prime factors enabling economic underdevelopment in most of the world's poorest countries. In those countries, whose economies remain largely agrarian, the structural transformation, or transition from subsistence to commercial agriculture, has so far proven elusive.

One of the first steps in the transition from subsistence to commercial agriculture—that is, the transition from many smallholder farmers producing small quantities of several crops for home consumption to fewer large farms producing large quantities of one or two crops for sale—is the emergence of an intermediate sector between the agricultural and manufacturing sectors. The institution that perhaps best represents the emergence of such an agro-industrial sector is contract farming, or the economic institution wherein a processing firm contracts the production of commercial crops out to smallholder farmers, and which constitutes the cornerstone of agricultural value chains. In one of the earliest studies of contract farming in economics, Grosh (1994) noted that the institution can resolve several market failures which result from risk and uncertainty, imperfect factor markets, and reluctance to adopt new technology. Since then, contract farming has been studied in many countries and across many crops, and policy makers have often hailed the institution as a tool for rural poverty alleviation.

But does participation in agricultural value chains make people better off? Although there is an important literature exploring the effects of participation in contract farming on household income or some variant thereof (Porter and Phillips-Howard, 1997; Singh, 2002; Warning and Key, 2002; Simmons, 2005; Maertens and Swinnen, 2009; Minten et

al., 2009; Miyata et al., 2009; Rao and Qaim, 2011; Barrett et al., 2012; Bellemare, 2012; Michelson, 2013; Narayanan, 2014),[1] we study whether participation in contract farming improves food security, defined here as the reported duration of the hungry season experienced by a household, i.e., the length of time during which one member of the household or more goes without three meals a day.[2,3] This question is important for three reasons. First, because the hungry season coincides with those months before households get cash for their crops—both contracted and not contracted—at harvest, it is not immediately obvious that the households involved in contract farming can or will save the extra income from contract farming (Dupas and Robinson, 2013); there is value in knowing whether income gains translate into other gains.[4] Second, self-control problems are more common among the poor and those who live "at the margin" (Banerjee and Mullainathan, 2010), and it is not clear whether the cash a household receives at harvest will be spent on necessities like food. Third, as a recent International Food Policy Research Institute discussion paper put it:

[1] There are two notable exceptions. Dedehouanou et al. (2013) look at the impact of contracting on the subjective well-being of farmers in Senegal. Montalbano et al. (2015) study various levels of participation in the agricultural supply chain and food security, but their study does not specifically address or identify households that participate in contract farming.

[2] This is undoubtedly only one aspect of food security. Ideally we would have additional measures of food security such as each household member's consumption of calories, macronutrients, and micronutrients. This would require detailed information at the individual-level. The data used in this analysis were not collected for the specific purpose of measuring food security and thus do not include such detailed consumption information.

[3] Because some of the households in our data experienced two hungry seasons, each respondent was asked in which month (and when during that month, i.e., beginning, middle, or end) each episode of hungry season began, and in which months (and when during that month) each episode ended. We define "reported duration of the hungry season" as the sum of those two episodes of hungry season for each household, measured in months.

[4] The contracts we study in this paper take place during the main agricultural season in Madagascar. Consequently, it is always the case in the data that people get paid for their contracted crops immediately after the hungry season ends.

> Income growth alone cannot solve the problem of malnutrition... The challenge from the nutrition perspective is how to sustainably improve the quality of diets, as well as other health-nutrition related behaviors, across different populations and age groups? (sic) In nutrition debates in developing countries there is growing interest in the capacity of the private sector to contribute to improved nutrition outcomes... Discussions have incorporated thinking around value chain frameworks, which emerged in the late 1990s to help development actors design interventions that responded to the needs of the private sector and contributed to development outcomes. Value chain approaches can provide useful frameworks to examine the food system and the potential to achieve improved nutritional outcomes by leveraging market-based systems (Gelli et al., 2015).

Using a sample of 1,200 households that covers more than ten contracted crops across six regions of Madagascar,[5] we look at whether participation in contract farming appears to decrease the reported length of the hungry season experienced by households. Because a household's decision to participate in contract farming is likely to be jointly determined with the reported duration of the hungry season experienced by the same household, we use the results of a field experiment aimed at eliciting respondent willingness to pay (WTP) to participate in contract farming. We then use this WTP variable to help disentangle the potential causal relationship flowing from participation in contract farming to the reported duration of the hungry season from the correlation between the two. We first use this WTP information in a regression context for a selection-on-

[5] Appendix table A12 describes what the primary contracted crops are for each region in our sample.

observables design (Angrist and Pischke, 2009). We then use this WTP information in hazard and duration models in order to estimate the likelihood of household exiting the hungry season. Finally, we use this WTP information to estimate average treatment effects using propensity score matching methods, since the same assumption that makes the selection-on-observables design possible also makes the conditional independence assumption likely to hold. Our work thus represents a dual departure from Bellemare (2012), who uses the same data set and a similar identification strategy to study the welfare impacts of contract farming. First, we look at food (in)security rather than income as our outcome of interest. Second, we use WTP for contract farming as a control in a selection-on-observables design to obtain an average treatment effect rather than as an instrumental variable in a two-stage least squares design to obtain a local average treatment effect.

Our core regression results suggest that participation in contract farming decreases the reported duration of the hungry season by approximately eight days (i.e., 0.28 months) for the average household in our data; propensity score matching results are largely consistent with those regression results. Our hazard and duration model results suggest that participation in contract farming increases the likelihood that a household's hungry season will end at any given time by about 18 percent.

In addition, an exploratory analysis indicates that the beneficial effects of participation in contract farming are more pronounced (i) the greater the number of children, and (ii) the greater the number of female children in a participant household. This is important because children—especially girls—are often the ones who bear the burden of food insecurity given unequal intrahousehold allocations of food, calories, and

nutrients (Barrett, 2002). Longer reported hungry seasons—our measure of food insecurity—can cause wasting, stunting, and a number of other health problems, and children who go hungry during their developmental process are more likely to have worse educational and health outcomes later on in life (Alderman et al., 2006; Ruel and Alderman, 2013).

The rest of this article is organized as follows. In the following section we discuss our empirical framework and present the details of our estimation and identification strategies. We then present the data and some descriptive statistics, followed by a presentation and discussion of our empirical results and of their limitations. We conclude by discussing the implications of our work for future research and for policy.

Empirical Framework

This section first presents the estimation strategy we use in order to study the impact of participation in contract farming on the reported duration of the hungry season—defined here as the number of months during which at least one member or more of the household goes without three meals a day—experienced by the households in our data. Then, because the reported duration of the hungry season experienced by a household is likely endogenous to its participation in contract farming, we explain the details of the identification strategy we rely on.

Ordinary Least Squares and Duration Models

The core equation we estimate is

(1) $y_i = \alpha_1 + \boldsymbol{\beta}_1 \boldsymbol{x}_i + \gamma_1 D_i + \varepsilon_i,$

where $y_i \geq 0$ is the reported duration of the hungry season experienced by household *i* measured in months, $\boldsymbol{x}_i$ is a vector of control variables (which, in a slight abuse of notation, also includes district dummies),[6] D_i is a variable equal to one if household *i* participates in contract farming and equal to zero otherwise, and ε_i is an error term with mean zero.

We are primarily interested in the coefficient γ which, if D were exogenous to y, would be the average treatment effect (ATE) of participating in contract farming on the reported duration of the hungry season, or

(2) $\gamma = E[y_i | D_i = 1] - E[y_i | D_i = 0].$

The treatment variable D, however, is endogenous to y because household participation in contract farming is not assigned at random. Therefore, we estimate the following version of equation 1:

(3) $y_i = \alpha_2 + \boldsymbol{\beta}_2 \boldsymbol{x}_i + \gamma_2 D_i + \boldsymbol{\delta}_2 \boldsymbol{w_i} + \eta_i$

where η_i is an error term with mean zero, and $\boldsymbol{w_i}$ is a vector of dummy variables that captures our respondents' answers to an experimental question aimed at eliciting WTP to participate in a hypothetical contract farming agreement. Our claim is that this WTP

[6] Boldface type is used throughout this paper to denote vectors.

proxies for each respondent's marginal utility of participating in contract farming, which in turn controls for a number of unobservable characteristics that explain selection into contract farming. We thus attempt to identify the ATE of participating in contract farming on the reported duration of the hungry season using a selection-on-observables design, in which a coefficient is identified because the right hand side variables (here, $\boldsymbol{x}$ and $\boldsymbol{w}$) account for selection into a given treatment (here, D). We further elaborate on this identification strategy in the Identification Strategy subsection.

Because we are dealing with duration data—that is, the LHS variable measures the reported number of months a household's most recent hungry season lasted—we use three distinct estimators to estimate equation 3. The first is the ordinary least squares (OLS) estimator, wherein γ tells us how much shorter the hungry season is, on average, for households that participate in contract farming. The next two estimators are the Cox proportional hazards model and the survival time regression model, two workhorse estimators used in the study of duration data (Lancaster, 1986).[7] In these two models, γ tells us how likely a household is to "exit" the condition represented by the hungry season at any given point in time. Thus, if participation in contract farming has the hypothesized beneficial effects on food security, we would expect $\gamma < 0$ in the OLS specifications (i.e., contract farming is associated with shorter reported hungry seasons), and $\gamma > 0$ in the Cox proportional hazards and survival time regression models (i.e., contract farming is associated with a higher likelihood of exiting the hungry season at any point in time).

[7] The survival time regression requires that one make an assumption on the distribution of the survival function. We make the common assumption that the survival function follows a Weibull distribution throughout this article.

Propensity Score Matching

Propensity score matching (PSM) is a valuable estimation strategy in the case of selection on observables (Imbens, 2015). Therefore, we use PSM as an additional estimator in an effort to assess the robustness of our findings. In the first stage, we estimate a probit model that is such that

$$(4) \quad D_i = \kappa + \boldsymbol{\lambda x}_i + \boldsymbol{\theta w_i} + \xi_i$$

where the variables are labeled the same as in equation 3.

The parameters of this model are then used to estimate the propensity score for each individual. The propensity score is an estimate of each household's likelihood of participation in contract farming, given its observable characteristics and the respondent's answer to the WTP question.

We then match households that participate in contract farming to households that do not participate in contract farming but have similar propensity scores. In selecting a matching algorithm it is important to consider two things. The first is the number of non-participating households to match to participating households. When matching with replacement, matching only one household raises the likelihood that the two matched households are very similar. Increasing the number of matched households can decrease the similarity between matched households but increase the pool of households upon which we draw inferences. The second important consideration is the caliper size. The caliper size determines how similar two households' propensity scores must be in order

for the corresponding households to be matched. If the caliper size is large, it is possible to match households with very dissimilar propensity scores. If the caliper is very small, it becomes difficult to find suitable matches, and thus a large portion of observations will be dropped from the sample, and the standard errors become inflated.

To address these trade-offs we use three matching routines to match households, matching with replacement: (i) one nearest neighbor with a caliper size of 0.01 standard deviation, (ii) three nearest neighbors with a caliper size of 0.01 standard deviation, and (iii) three nearest neighbors with a caliper size of 0.001 standard deviation. We then estimate three treatment effects: (i) the average treatment effect on the treated (ATT), (ii) the average treatment effect on the untreated (ATU), and (iii) the average treatment effect (ATE). The ATT is standard reporting for propensity score matching and tells us how treated households are affected by their participation in contract farming. The ATE is the same estimator that is reported in our OLS estimates, and is thus of greatest interest here. One would expect the ATT to be the largest, in absolute value, followed by the ATE, and then ATU, since those who are likely to benefit the most from contract farming are also more likely to select into participation.

Identification Strategy

As discussed, we rely on a selection-on-observables identification strategy in order to estimate the impact of participation in contract farming on the reported duration of the hungry season. This section first explains the experimental setup we used to elicit WTP to participate in contract farming. It then explains how WTP for contract farming should purge the error term η of much of its correlation with the variables on the right hand side

of equation 3.

The contingent valuation experiment used in this article is the same as that used in Bellemare (2012). Each respondent was asked whether he would participate in a contract farming agreement that would raise his income by 10 percent in exchange for a one-time monetary investment. The amount of the monetary investment was randomly selected from six investment amounts of \$12.50, \$25.00, \$37.50, \$50.00, \$62.50, or \$75.00.[8] The size of investment was determined at random by the throw of a regular (i.e., six-sided and fair) die. To put these six investment amounts in context, consider that the average household annual income for households in our sample is approximately US\$968. These investment amounts range between 1.3% and 7.7% of average household annual income.

For each respondent, the data include the random dollar amount associated with the roll of the die and a "Yes" or "No" answer to whether the respondent would pay an initial investment equal to the random dollar amount in order to participate in a contract farming agreement that would increase his income by 10 percent.

The vector $\boldsymbol{w}$ in equation 3 captures respondent answers to the WTP question. Thus, in contrast to Bellemare (2012), who uses WTP as an instrumental variable in a two-state least squares setup, we use WTP as a control variable in a selection-on-observables design. For example, a respondent who rolls a five on the die throw would be asked whether he would like to participate in a contract farming agreement that would raise his income by 10 percent, but would require him to pay an initial investment of \$62.50. If he answered "Yes," his $\boldsymbol{w}$ vector would be equal to $(w_1, w_2, w_3, w_4, w_5, w_6)$ = (0,0,0,0,1,0).

[8] Those figures are presented in US dollars for ease of exposition. The US dollar figures were expressed in local currency during fieldwork so respondents could more easily relate to the amounts.

A respondent who rolls a four on the die throw would be asked whether he'd like to participate in a contract farming agreement that would raise his income by 10 percent, but would cost $50.00. If he answered "No," his $\boldsymbol{w}$ vector would be equal to (0,0,0,0,0,0).

Note that the foregoing ascribes a "No" answer to all questions that a respondent was not asked. In the example above, in which the respondent is asked whether he would participate in a contract farming agreement costing $62.50, we have coded all other amounts, $12.50, $25.00, $37.50, $50.00, and $75.00, as "No." In order to remedy this shortcoming, in an additional set of estimations we enforce monotonic switching on the part of our respondents. That is, if a respondent answers "Yes" to participating in the hypothetical contract farming agreement for a given randomly selected investment value, we code all lower investment values as "Yes" responses. Even with this modification, our measure of WTP is a lower bound on respondents' actually WTP to participate in a contract farming agreement.

In a final set of results, we impute, on the basis of observables, what each respondent's answers would be to all investment questions.[9] So a respondent who rolls a five on the die throw and responds "Yes," meaning that he would like to participate in a contract farming agreement that would raise his income by 10 percent, but would require him to pay an initial cost of $62.50, would have a $\boldsymbol{w}$ vector equal to ($\widehat{w}_1$, $\widehat{w}_2$, $\widehat{w}_3$, $\widehat{w}_4$, 1, $\widehat{w}_6$) where $\widehat{w}_i$ denotes an imputed value in the ith position. Because the level of

[9] We do those imputations by running a linear regression for each of w_1, …, w_6 on all the control variables (i.e., the variables in $\boldsymbol{x}$) in equation 1. Whenever an observation is missing for a dependent variable, we replace it with its predicted value from that dependent variable's imputing regression. For example, if an observation is missing for w_2, we replace that observation with $\widehat{w}_2$, which is equal to the respondent's values in $\boldsymbol{x}$ multiplied by the relevant estimated coefficients from the aforementioned linear regression. We leave nonmissing values the same. Thus, in a slight abuse of notation, the variables $\widehat{w}_1$, …, $\widehat{w}_6$ are mixtures of observed and imputed (or predicted) values.

investment required of each respondent (i.e., $12.50, $25.00, $37.50, $50.00, $62.50, or $75.00) was determined at random as part of the experiment, the level of investment is unrelated to a respondent's observable *and unobservable* characteristics, which means that the imputed responses to the unasked questions are unbiased. The shortcoming of this approach is that it relies on generated regressors; we deal with this issue by bootstrapping the standard errors whenever we include imputed variables as regressors.

In all cases, the identifying assumption we make is that a respondent's response to the WTP question is correlated with his WTP to participate in contract farming, and so the vector $\boldsymbol{w}$ serves as a proxy for a respondent's marginal utility from participating in contract farming. The next section explains why this constitutes a selection-on-observables research design in the context of regression or, alternatively why it satisfies the conditional independence assumption in the context of matching.

Identification

How does a set of proxies for a respondent's marginal utility from participating in contract farming help identify the causal impact of participation in contract farming on the reported duration of the hungry season? Recall that there are three sources of statistical endogeneity:

1. Unobserved heterogeneity,
2. Reverse causality, and
3. Measurement error.

We look at each of these in turn in the remainder of this section.

Unobserved heterogeneity refers to the problem of omitted variables such as a

respondent's preferences for risk and ambiguity, his entrepreneurial ability, his technical ability, and his preferences in general, all of which can compromise the identification the ATE if they happen to be correlated with both the reported duration of the hungry season and any of the variables on the right hand side of equation 1. In this application, a great deal of this unobserved heterogeneity can be captured by differences in a respondent's marginal utility from participation in contract farming. Take for example a respondent who is price risk averse (Bellemare et al., 2013). Such a respondent might prefer to participate in contract farming because contract farming arrangements typically insure growers against price risk. Alternatively, a respondent who is very entrepreneurial might have little to no use for contract farming given that she has her own micro-enterprise. Such a respondent might prefer not to participate in contract farming because of the opportunity cost of time associated with being in a grower-processor contract. In all such cases where a respondent's marginal utility from participating in contract farming varies because of some omitted variable, the variation in WTP measure captures the variation in respondent marginal utility, which should largely obviate concerns about unobserved heterogeneity between respondents.

Reverse causality refers to the statistical endogeneity problem that arises from the fact that the dependent variable might cause the variable of interest. In this case, households that experience a shorter hungry season may be more likely to participate in contract farming. This would compromise the identification of the ATE, and it could definitely be a concern in our application given that households that have better access to food may be more willing to enter into contract farming agreements. It should be the case, however, that a respondent who is more willing to enter into a contract farming agreement because

he is more food secure will have a higher marginal utility of participating in contract farming. Our WTP measure controls for this issue much the same as it did for other changes in preferences, which should alleviate concerns about reverse causality.

Finally, measurement error refers to the statistical endogeneity problem that arises from there being measurement error in whether a household participates in contract farming. This is highly unlikely to be a problem in our application given that there is no obvious advantage or disadvantage to misreporting whether one participates in contract farming or not. In addition, the sample was choice-based, i.e., the survey team aimed for a sample in which half the respondents participated in contract farming and half did not, and the sampling frame was established with village chiefs, who know who participated in contract farming and who did not. This sampling strategy thus served as a consistency check for whether people truly did participate in contract farming.

In sum, our identification strategy allows us to rule out a number of sources of bias that plague the identification of a causal effect in this context. Because we are dealing with observational data, however, it is impossible to rule out all sources of statistical endogeneity with certainty. As a result, we caution the reader against interpreting our estimate of γ as causal, although it can certainly be interpreted as suggestive of the effect of participation in contract farming on the reported duration of the hungry season experienced by grower households.

Data and Descriptive Statistics

The data used in this article are the same as in Bellemare (2012), and this section necessarily echoes the discussion of the same data in that article. The data were collected

between July and December of 2008 for a study of contract farming commissioned by the World Bank. The data cover six regions and two communes per region. Three of these regions were chosen because they exhibited a relatively high prevalence of contract farming; the other three were chosen because the government of Madagascar viewed them as high-priority areas for economic development. In all regions, the two communes with the highest density of contract farming were surveyed. Commune-level data were obtained from the 2007 census of communes in Madagascar; Moser (2008) presents the methodology used for the commune census.

Within each of the 12 communes, two lists were generated: one a list of all households that participated in contract farming, the other a list of all households that did not. Then, 50 households were randomly selected from the list of households that participated in contract farming, and 50 were randomly selected from the list of households that did not. We use sampling weights representative at the commune level throughout this article to account for this choice-based sampling (Manski and Lerman, 1977), and to bring our sample as close as possible to a random sample.

The survey was conducted in rural areas of Madagascar, and almost all—96 percent—of the households in our sample derive at least some of their income from agricultural activities. For each household, data were collected at the household, plot, crop, and contract level. Households were asked to recall events from the 12 months prior to the survey.

Before discussing the data used in this article, we briefly discuss the nature of the con- tract farming agreements we are studying. Appendix Table A1 lists the primary contracted crops by region. A lot of those crops are food staples—especially rice in the

context of Madagascar, but also maize and barley—which might play a role in enhancing the food security of contract farming participants, a speculation we return to below when discussing mechanisms. The contract farming agreements in our data take many shapes, from the processor providing no seeds, pesticides, and fertilizer (both organic and chemical), to the processor providing all of those, with several scenarios in between. Likewise, contracts can be both signed between the processor and an individual grower or between the processor and a group of growers; contracts are written more often than not, but it also happens that they are verbal; and though the piece rate paid to growers by processors tends to be fixed, there was one processor in particular which paid a floating (i.e., market conditions-driven) piece rate. Most processors are Malagasy companies, with the notable exception of Sodexo, a French company that is perhaps best known among readers for operating cafeterias in universities, hospitals, and other workplaces.

We present descriptive statistics for our sample in Table 1, broken down by whether households participate or not in contract farming, along with balancing tests. The average reported duration of the hungry season—the number of months during which members of the household go without three meals per day, i.e., our proxy for food insecurity—for the households in our sample is 3.7 months for households that do not participate in contract farming versus 3.3 months for households that do. Approximately half of the surveyed households participate in contract farming (not shown in Table 1). The average household has between five and six members, and almost half of the individuals in any given household are dependents, i.e., they are either younger than 15 or older than 65. The average household head is married and male, but is more likely to be female and single among households that do not participate in contract farming. The average

household head is 43 years old, with a small, statistically significant difference between the heads of households that participate in contract farming (42 years old on average) and the heads of households that do not (44 years old). The average household head has almost six years of education, and has over 20 years of agricultural experience. Almost 30 percent of household heads are members of a farm organization other than a contract farming organization among households that do participate in contract farming, a proportion that falls by half for those households that do not participate in contract farming. The average household head is forbidden from doing agricultural work for more than 20 days per year for religious reasons.[10]

Average household annual income—that is, a household's cash income from selling of animals and animal byproducts, wage labor, nonagricultural activities, leases (land, cattle, and equipment rentals), sales of non-contracted crops, and contract farming—is approximately US$968 per year with an average per capita income of US$174.[11] There are sharp differences in income between contract farming participants and nonparticipants, with the former type of household seeing its income almost 65 percent higher than the latter type on average. In Madagascar in 2008, GDP per capita was US$468, meaning households in our sample were significantly poorer than the national average. The average household owns about US$220 of agricultural equipment and tools, but contract farming participants own about twice as much than nonparticipants. Likewise, the average household owns about US$700 in other assets such as a house, TV,

[10] The Malagasy observe a system of taboos and interdictions that dictate everything from the orientation of buildings to what a person may eat. Those taboos tend to vary at several levels, between individuals, households, villages, ethnic groups, and so on. See Ruud (1960) for a thorough treatment, and Stifel et al. (2007) for an investigation of the effects of days on which agricultural work is forbidden on agricultural productivity.

[11] USD 1 ≈ 2,000 Ariary at the time the data were collected.

radio, and livestock, but the value of contract farming participants' assets is significantly higher. Similarly, households that do participate in contract farming own significantly more land than those that do not.

Lastly, Table 1 displays the results of the WTP contingent valuation experiment. As expected, the proportion of respondents who are willing to participate in contract farming generally declines as the investment required grows, except for an initial bump between $12.50 and $25.00.[12]

Empirical Results

We begin this section by presenting nonparametric evidence of the relationship between participation in contract farming and reported duration of the hungry season experienced by households. This nonparametric relationship does not account for confounding factors in the decision to participate in contract farming, it merely displays the relationship between contract farming and the reported duration of the hungry season. Thus, we then present parametric evidence using the selection-on-observables regression methodology discussed in the Empirical Framework section as well as propensity score matching methods. We then, in an exploratory analysis, look at treatment heterogeneity, first by determining whether the effects of participation in contract farming on food security are

[12] Such idiosyncrasies are conceivable given the random assignment to which investment level respondents are asked to pay to participate in the hypothetical contract farming agreement. We conducted a series of balance tests and find that the majority of our explanatory variables are balanced across the groups of households asked if they would invest the six different investment amounts. The few exceptions are that respondents that rolled a one on the dice, and were consequently asked if they were willing to invest $12.50 in order to participated in the hypothetical contract farming agreement, were slightly more likely to have a household head that is single, female, and less likely to be a member of a farm organization–three variables that are highly correlated with one another–than respondents whom we asked about other investment amounts.

more pronounced for households with more children, and second by looking at whether the effects of participation in contract farming on food security are more pronounced for households with more girls. We then discuss the results of a number of robustness checks, and conclude this section by discussing the limitations of our approach.

Nonparametric Evidence

We begin with nonparametric estimations of the relationship between contract farming and the reported duration of the hungry season in order to establish whether a relationship exists between participation in contract farming and food security. Kaplan-Meier (i.e., nonparametric) estimates of the survival functions for households that participate in contract farming and households that do not are displayed in Figure 1. These estimates show that contract farming participants are more likely to exit the hungry season earlier than non-participants.

Similarly, Figure 2 displays kernel density estimates of the distribution of the reported number of months spent in the hungry season for households that participate in contract farming and households that do not. Consistent with the results in Table 1, households that participate in contract farming report that they experience a shorter hungry season than those that do not participate.

Both figures suggest there is a relationship between a household participating in contract farming and a shorter hungry season reported by that same household, but neither figure can help ascertain whether that relationship is causal. In order to disentangle the potential causal relationship between contract farming and food security, we now turn to parametric evidence.

Ordinary Least Squares and Duration Models

We now estimate the relationship between participation in contract farming and reported duration of the hungry season using the estimation and identification strategies discussed in the Estimation Strategy section. We account for the endogenous choice to participate in contract farming or not by using proxy variables for respondents' marginal utility. This proxy is derived from the contingent valuation field experiment to elicit willingness to pay to participate in contract farming described above.

Table 2 presents two sets of OLS, Cox proportional hazards, and survival time regression estimation results: Table 2a omits the variables capturing respondent answers to the WTP questions, whereas Table 2b includes those variables. Before discussing the results in Table 2, however, we discuss the results of Hausman tests aimed at determining whether our variable of interest—the contract farming dummy—is endogenous. The first of these tests compares the OLS results in Tables 2a and 2b under the null that both sets of coefficients (for those variables common to both tables) are identical. In this case, the Hausman test has a p-value of 0.98, which supports the null of exogeneity. For the second Hausman test we begin by running the first-stage regression (not shown) of the contract farming dummy on all control variables and the WTP dummies. The error term from this first-stage regression is then incorporated as an additional regressor in equation 2. The coefficient on the residual of the first-stage regression is the object of the test, with the null hypothesis being that the contract farming dummy is exogenous to the reported duration of the hungry season. Once again, we fail to reject the null, and the t-test has a p-value of 0.49. But even given the foregoing, for the remainder of this article we focus on

results where we control for respondent WTP to participate in contract farming by including WTP as a regressor. We do so both because a failure to reject the null in a Hausman test is rarely convincing when arguing that a certain variable is exogenous, and because the WTP dummies are jointly significant in the OLS specification in the first column of Table 2b.

Assuming that a month lasts 30 days on average, the OLS results in Table 2b suggest that participating in contract farming is associated with an eight-day decrease (0.277 months × 30 days per month) in the reported duration of the hungry season. Similarly, the Cox proportional hazards and survival time regression estimation results respectively suggest that a household that participates in contract farming is 17 and 19 percent, respectively, more likely to exit the hungry season at any given time than a household that does not participate in contract farming. Additionally, female-headed households experience a hungry season that is about three weeks (0.73 × 30) longer than male-headed households and are 32 percent less likely to exit the hungry season at any given time, according to the Cox proportional hazards model, and 39 percent less likely, according to the survival time regression. Likewise, an increase in a household head's years of education and his years of agricultural experience as well as the value of the assets owned by his household are all associated with a shorter hungry season and a greater likelihood of exiting the hungry season at any given time. According to the OLS results, an increase in household income in associated with a shorter hungry season, while income is statistically insignificant in the Cox proportional hazards and survival time regression results. This lends credence to our claim that it is insufficient to infer nutritional outcomes based solely on income changes. Lastly, though the contingent-

valuation dummies are not individually significant in any of the models presented in Table 2, they are jointly significant at less than the 10 percent level for the OLS model.

In an effort to identify the mechanism through which participation in contract farming might reduce the length of the hungry season (in the OLS specification) or increase the likelihood of exiting the hungry season (in the Cox proportional hazards and survival time regression specifications), we estimated additional specifications (not shown) similar to those in Table 2 but in which we include (i) an additional control variable which measures the proportion of all crops under contract that are rice crops, or (ii) an additional control variable which measures the proportion of all crops under contract that are food crops. Including those additional controls allows determining whether participation in contract farming improves food security via some sort of positive spillover (whereby contract farming participants become more productive on their own food crops because they grow food crops under contract) or via leakage (whereby contract farming participants might steal some of the crops they grow under contract in order to eat them). In no case was the coefficient on those interaction terms significant at less than the 10 percent level, which allows ruling out those mechanisms.

Treatment Heterogeneity

We now turn to treatment heterogeneity by number of children and by number of children of each gender in the household. Table 3 shows estimation results for OLS, Cox proportional hazards, and survival time regression models in which the treatment variable (i.e., the dummy for whether a household participates in contract farming) is interacted with the number of children in the household. Similarly, Table 4 shows estimation results

for OLS, Cox proportional hazards, and survival time regression models in which the treatment variable (i.e., the dummy for whether a household participates in contract farming) is interacted with the number of children of each gender in the household. Though the WTP dummies are generally not individually significant in any of the models presented in Tables 3 and 4, they are jointly significant at less than the 10 percent level in the OLS models in both Tables 3 and 4.

The results in Table 3 show that participation in contract farming is associated with greater decreases in the reported duration of the hungry season the more children there are in the household. Specifically, for every child in the household, the reported duration of the hungry season decreases by about six days (-0.19 months × 30 days per month) in households that participate in contract farming, and the likelihood that the household will exit the hungry season increases by 6 and 7 percent, according to the Cox proportional hazards and the survival time regression model, respectively.

Likewise, the results in Table 4 show that participation in contract farming is associated with greater decreases in the reported duration of the hungry season the more girls there are in the household. Specifically, for every girl in the household, the reported duration of the hungry season decreases by about one week (-0.22 months × 30 per month), and the likelihood that the household will exit the hungry season increases by 12 and 14 percent, according to the Cox proportional hazards and the survival time regression model, respectively.

Why would the beneficial effects of participation in contract farming be more pronounced for households with more children, and specifically for households with more female children? Though our data do not allow us to determine the precise

mechanism behind these findings, it is not unlikely that because children—specifically girls—require fewer calories, we may see that the addition of calories in the household creates a larger reduction in the number of skipped meals for households with more children. In other words, the marginal welfare impacts of participating in contract farming will be highest for children, specifically girls, and so it is not surprising that the effects of participation in contract farming on food security would be especially pronounced for households with more children and with more girls. To explore this further, we estimated a specification (not shown) in which we interacted the contract farming participation dummy with gender-age categories (i.e., female children, male children, working-age women, working-age men, elderly men, and elderly women). The results from that regression show that the estimated coefficients for those interactions are statistically significant and negative for female children, male children, and elderly women, with the remainder being statistically insignificant. Those results indicate that the beneficial effects of contract farming on food security are especially pronounced for households with more kids of either gender and more elderly women. Interestingly, those are the groups that eat the least in a typical developing-country household, so it is perhaps not surprising that the food security gains from participating in contract farming come first from those groups. This supports our hypothesis that it is easier to make food security gains the more children and the more girls there are in a given household, simply because the marginal returns, in terms of fewer skipped meals, to participating in contract farming are higher among children and girls than among working age adults. But given that our data do not allow studying how many meals each member of the household consumes, this explanation is necessarily tentative and speculative.

Propensity Score Matching

Table 5 displays the results from the probit regression of participation in contract farming on household characteristics and our measure of WTP to participate in contract farming. Households with a younger household head and households in which the head is a member of a farm organization are more likely to participate in contract farming. Households in which the survey respondent answered that he would be willing to pay the random dollar amount in order to participate in a hypothetical contract farming agreement are more likely to participate in contract farming than households in which the respondent answered "No."

Figure 3 displays the distribution of the propensity scores by participants and non-participants in contract farming for the full, untrimmed sample. There is a substantial amount of overlap in the propensity scores between participants and non-participants. This overlap is crucial to estimating reliable effects of contract farming because it implies that there are participating and non-participating households with similar characteristics. Moreover, Tables A10 to A12 present balance statistics for the matched samples in all three of our matching specifications.

Table 6 displays estimation results for the unmatched sample along with the estimation results for the three treatment effect estimators (i.e., ATT, ATE, and ATU) for each of our three matching routines. These results estimate (i) the effect of participation in contract farming on the length of the hungry season experienced by the household for households that participated in contract farming (ATT), (ii) what the effect of participation in contract farming would have been for households that did not participate

in contract farming (ATU), and (iii) what the average effect of participation in contract farming would have been for all households in the sample had all household participated (ATE). As expected, the results for all three matching routines show that the largest effect (in absolute value) is for households that did participate, followed by the effect for all households in the sample, and lastly the effect for those households that did not participate. Recall that the estimated ATE is most comparable to the estimated effects for the OLS results because OLS reports the ATE.

The ATE ranges from -0.127 to -0.272. This represents a reduction in the length of the hungry season by between four and eight days—an effect that is very close to what we find using in our OLS specification in the first column of Table 2b. The effect for participating households is larger, i.e., the ATT ranges from -0.194 to -0.305. This is a reduction in the length of the hungry season by six to nine days.

Robustness Checks

In order to ensure that our results are robust, we estimate a number of alternative specifications. Table 5 presents the results of two estimators that aim at minimizing the effects of outliers. The first specification is a median regression. Intuitively, a median regression is similar to an OLS regression, except that it focuses on the conditional median rather than the conditional mean. The second specification is a robust regression (Rousseeuw and Yohai, 1987). In both cases, results are very similar to the core OLS result in the first column of Table 2b.

In appendix Tables A2 to A7, we present estimation results similar to our core results in Table 2, 3, and 4, respectively, but with one important difference. In Tables A2, A3,

and A4, the responses to the contingent-valuation questions that were not posed to the respondent are imputed, as detailed in the subsection *Identification Strategy*. Because imputations yield generated regressors, we bootstrap the standard errors but omit sampling weights[13] in appendix Tables A2, A3, and A4. Given the results in appendix Tables A2, A3, and A4, our core results appear to be robust to a change in how we proxy for respondent marginal utility to participate in a hypothetical contract farming agreement that would increase household income by 10 percent. Similarly, when we include sampling weights but do not bootstrap the standard errors in appendix Tables A5, A6, and A7, our core results appear once again robust to a change in how we proxy for respondent marginal utility to participate in a hypothetical contract farming agreement that would increase household income by 10 percent.

Appendix Table A8 presents the results of treatment regressions wherein responses to the contingent-valuation questions are used as instrumental variables for participation in contract farming, as in Bellemare (2012). Taking both the OLS results in Table 2b and the treatment regression results in appendix Table A8 at face value—that is, assuming that they both identify causal impacts—would suggest that the local average treatment effect (LATE, i.e., the estimated coefficient on participation in contract farming in either column of appendix Table A8) is much larger than the ATE (i.e., the estimated coefficient on participation in contract farming in the first column of Table 2b). In other words, *if one is willing to believe that both specifications are well-specified and identify causal relationships*, one would conclude that participating in contract farming is associated with an almost two-month decrease in the reported duration of the hungry

[13] We do not show results in which we use sampling weights and bootstrapped standard errors, because the use of the latter precludes incorporating the former.

season for those households who were induced to participate because they would derive a higher marginal utility from participating in the hypothetical contract farming arrangement. But taking into account the potential effect of participating in contract farming for everyone—including nonparticipants—the effect is severely moderated. In other words, the fact that the LATE exceeds the ATE implies that compliers (i.e., those households that participate in contract farming because they derive higher marginal utility from doing so and those households that do not participate because they would not derive higher marginal utility from doing so) derive higher benefits than defiers (i.e., those households that participate but derive lower marginal utility from doing so, or households that do not participate but would derive higher marginal utility from doing so).

Finally, appendix Table A9 includes the results for the model in which we enforce monotonic switching in the set of WTP questions. This model yields results that are identical to those in Table 2. We thus conclude from these robustness checks that our core results are robust to alternative estimators and specifications.

Lastly, for ease of comparison, Table 8 synthesizes all of the estimated treatment effects in Tables 2 to 7.

Limitations

Despite their robustness, our results suffer from some important limitations in terms of internal validity, and in terms of the measurement of food insecurity.

In terms of internal validity, it bears repeating that our estimates of the effect of household participation in contract farming on the reported duration of the hungry season

experienced by that household is only as good as our identification strategy. Here, in order to believe that our estimates are causal, one must trust that our proxies for respondent marginal utility of participation in contract farming derived from our contingent valuation experiment fully account for the selection process whereby households choose to participate in contract farming. This is an assumption that is untestable. Moreover, comparing the OLS specification in the first columns of Tables 2a and 2b shows that the coefficients are not statistically different between the two models. The coefficient on the contract farming dummy only goes from -0.293 in Table 2a to -0.277 in Table 2b.

This suggests one of two things: either (i) the WTP questions do not do a good job of accounting for selection into contract farming, or (ii) participation in contract farming is not endogenous to the reported duration of the hungry season experienced by households. In order to investigate claim (i), we use an OLS regression (not shown) of contract farming participation on the right-hand side variables in Table 2, and find that a joint significance test of the WTP dummies shows that those dummies are jointly significant at a confidence level that exceeds 99 percent. Similarly, the probit regression results in Table 5 show that WTP is highly correlated with participation in contract farming. In other words, responses to the WTP experiment appear to explain selection into contract farming, which would invalidate (i) above, leaving us to conclude that (ii) holds.

In terms of measurement of food insecurity, we wish to reiterate that we are only measuring one aspect of food insecurity, viz. the reported length of time during which at least one household member goes without eating three meals a day. But food insecurity could be measured much more precisely by measuring each household member's

consumption of calories, macronutrients (e.g., carbohydrates, fat, and protein), or micronutrients (i.e., specific vitamins and minerals). The data used in this article were not collected for the specific purpose of studying food insecurity, and measuring food insecurity accurately would require individual- rather than household-level survey questionnaires.

Concluding Remarks

We have used data on 1,200 households across six regions of Madagascar to investigate the relationship between contract farming and food security by looking at whether participation in contract farming is associated with a decrease in the reported duration of the hungry season experienced by the households in our data.

Our results show that participation in contract farming is associated with a reduction in the reported duration of the hungry season by about eight days for the average household, and that it increases the likelihood that a household will exit the hungry season at any point in time by about 18 percent on average. These are important results because even though published research has shown that contract farming increases the income of participating farmers (Porter and Phillips-Howard, 1997; Singh, 2002; Warning and Key, 2002; Simmons, 2005; Maertens and Swinnen, 2009; Minten et al., 2009; Miyata et al., 2009; Rao and Qaim, 2011; Bellemare, 2012; Michelson, 2013; Narayanan, 2014),[14] there has so far been no attempt to study whether contract farming leads to improvements in food security, and the link between agricultural value chains

[14] Again, the difference between Bellemare (2012) and this study is twofold. First, whereas Bellemare looked at income as his outcome variable, we look at the duration of the hungry season as ours. Second, whereas Bellemare used respondent WTP to participate in contract farming as an instrumental variable, we use it as a control variable in a selection-on-observables design.

and nutrition has been deemed a high priority by policy makers (Gelli et al., 2015; FAO, 2013). Moreover, the estimated effects of participation in contract farming on the reported duration of the hungry season experienced by households are especially pronounced for households with more children, and for households with more girls. These are important results given that children, particularly girls, bear the largest burden of food insecurity, the consequences of which include stunting, wasting, listlessness, and cognitive impairment (Alderman et al., 2006; Ruel and Alderman, 2013). Our results suggest that policies that lower barriers to entering contract farming agreements for households with children, and particularly girls, may lead to big gains in terms of food security.

From a behavioral perspective, our results suggest that smallholders in Madagascar save a portion of the additional income they receive from participating in contract farming in order to spend it on food in the months in which they would otherwise need to skip meals.[15] Alternatively, our results may suggest that the improvement in food security that results from contract farming is a result of participants growing staple or other food crops, via some positive productivity spillover to the staple crops those participants grow for their subsistence or via a misappropriation of some of what they grow under contract. Our tests of those hypotheses, however, showed that it is very unlikely that that the cultivation of rice or food crops is the mechanism whereby participation in contract farming improved food security in this context. From a policy perspective, our findings

[15] A reviewer commented that households that participate in contract farming might see their hungry seasons start later than nonparticipant households. Looking into whether that is the case, we found that the start date of the hungry season was not significantly different for households that do and do not participate in contract farming.

suggest that policies that facilitate the development of agricultural value chains, beyond their direct welfarist effect on the incomes of those who participate as growers, can also have indirect nonwelfarist effects on those same growers' food security.

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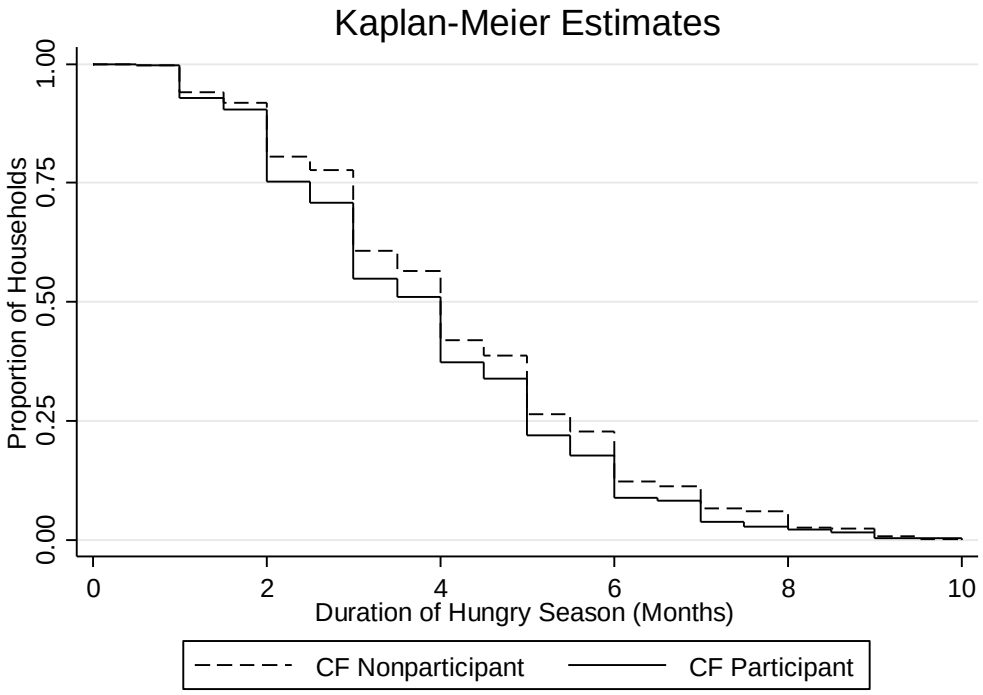

Figure 1. Kaplan-Meier Estimates of the Effect of Participation in Contract Farming (CF) on the Duration of the Hungry Season.

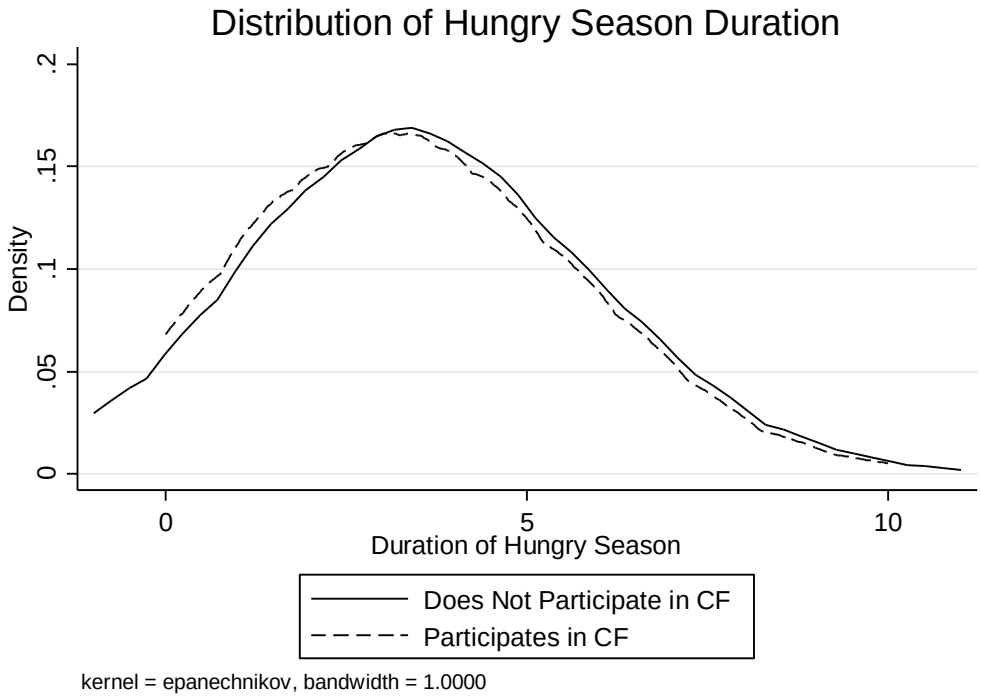

Figure 2. Kernel Density Estimates of the Duration of the Hungry Season by Contract Farming Participation Status.

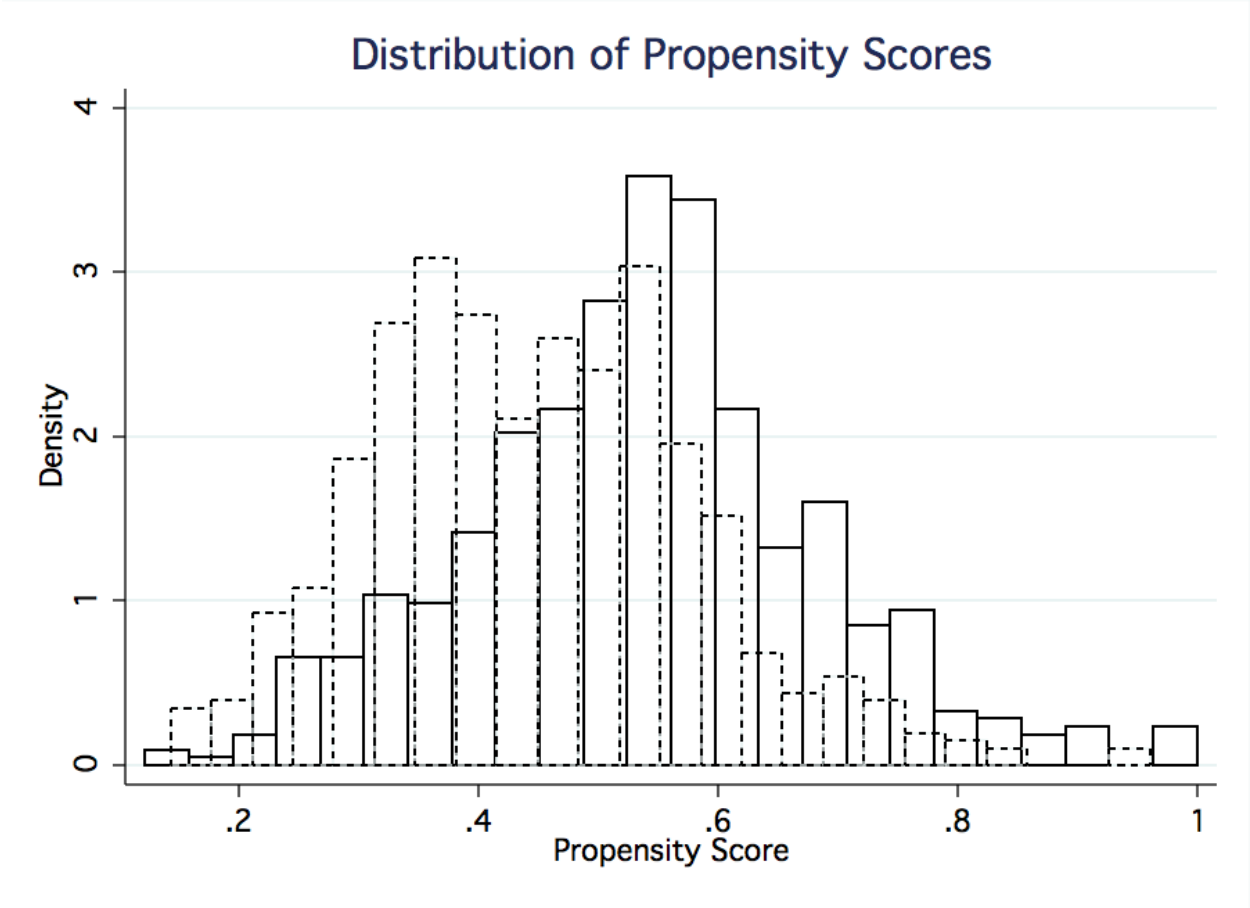

Figure 3. Distribution of Propensity Scores by Participation Regime. Solid Lines Denote Participants in Contract Farming; Dashed Lines Denote Nonparticipants.

Table 1. Descriptive Statistics by Participation Regime and Balance Tests

Variables	Participates in Contract Farming? No	Yes	Diff.
Duration of Hungry Season	3.696	3.316	***
(Months)	(0.109)	(0.105)	
Household Size	5.452	5.692	**
(Individuals)	(0.108)	(0.104)	
Dependency Ratio	0.452	0.446	
	(0.012)	(0.010)	
Household Head Single	0.158	0.089	***
(Dummy)	(0.017)	(0.014)	
Household Head Female	0.119	0.057	***
(Dummy)	(0.016)	(0.011)	
Household Head Migrant	0.124	0.125	
(Dummy)	(0.015)	(0.015)	
Household Head Age	44.428	42.110	**
(Years)	(0.652)	(0.554)	
Household Head Education	5.650	5.715	
(Years)	(0.154)	(0.147)	
Household Head Agricultural Experience	21.074	20.165	
(Years)	(0.653)	(0.566)	
Household Head Member of Farm Org.	0.149	0.296	***
(Dummy)	(0.017)	(0.022)	
Days Agricultural Work Forbidden	23.968	20.427	
(Days/Year)	(1.684)	(1.424)	
Household Income	14.843	24.255	***
(100,000 Ariary)	(1.198)	(2.762)	
Household Working Capital	2.872	6.021	***
(100,000 Ariary)	(0.380)	(0.973)	
Household Assets	11.672	16.277	***
(100,000 Ariary)	(1.099)	(1.359)	
Household Landholdings	113.438	177.956	***
(100 Square Meters)	(8.982)	(18.146)	
Children in the Household	2.502	2.654	
(Number of Children)	(1.791)	(1.697)	
Male Children in the Household	1.256	1.367	
(Number of Children)	(1.181)	(1.245)	
Female Children in the Household	1.246	1.287	
(Number of Children)	(1.223)	(1.101)	
"Yes" to $12.50 Investment	0.129	0.135	
(Dummy)	(0.015)	(0.016)	

"Yes" to $25.00 Investment	0.173	0.185	
(Dummy)	(0.018)	(0.018)	
"Yes" to $37.50 Investment	0.142	0.172	
(Dummy)	(0.016)	(0.018)	
"Yes" to $50.00 Investment	0.117	0.150	**
(Dummy)	(0.015)	(0.016)	
"Yes" to $62.50 Investment	0.065	0.073	
(Dummy)	(0.012)	(0.013)	
"Yes" to $75.00 Investment	0.047	0.085	
(Dummy)	(0.009)	(0.013)	
Observations	599	579	

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 2a. Estimation Results for OLS, Cox Proportional Hazard, and Survival-Time Regressions Omitting WTP Variables.

Variables	OLS	Cox	Survival Time
Dependent Variable: Duration of Hungry Season			
Contract Farming Participant	-0.294**	0.150**	0.171**
	(0.142)	(0.062)	(0.070)
Household Size	0.050	-0.011	-0.013
	(0.036)	(0.015)	(0.017)
Dependency Ratio	0.576	-0.254	-0.286
	(0.365)	(0.157)	(0.181)
Household Head Single	-0.078	0.051	0.082
	(0.338)	(0.148)	(0.169)
Household Head Female	0.723*	-0.339*	-0.413**
	(0.400)	(0.176)	(0.206)
Household Head Migrant	0.033	0.031	0.027
	(0.216)	(0.103)	(0.118)
Household Head Age	0.024**	-0.005	-0.005
	(0.009)	(0.004)	(0.005)
Household Head Education	-0.069***	0.020*	0.024**
	(0.022)	(0.010)	(0.012)
Household Head Agricultural Experience	-0.032***	0.006	0.006
	(0.010)	(0.004)	(0.005)
Household Head Member of Farm Organization	0.104	-0.111	-0.147
	(0.185)	(0.089)	(0.102)
Days Agricultural Work Forbidden	-0.003	0.000	0.000
	(0.002)	(0.001)	(0.001)
Household Income	-0.004**	0.000	0.000
	(0.002)	(0.001)	(0.002)
Household Working Capital	0.002	0.006***	0.007***
	(0.003)	(0.002)	(0.002)
Household Assets	-0.013***	0.004***	0.005***
	(0.003)	(0.001)	(0.002)
Household Landholdings	-0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Constant	3.533***		-3.997***
	(0.433)		(0.246)
Observations	1,178	1,045	1,045
District Dummies	Yes	Yes	Yes
R-squared	0.197	-	0

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 2b. Estimation Results for OLS, Cox Proportional Hazard, and Survival-Time Regressions Including WTP Variables.

Variables	OLS	Cox	Survival Time
Dependent Variable: Duration of Hungry Season			
Contract Farming Participant	-0.277*	0.166***	0.188***
	(0.145)	(0.063)	(0.071)
Household Size	0.052	-0.013	-0.015
	(0.036)	(0.015)	(0.017)
Dependency Ratio	0.517	-0.226	-0.247
	(0.366)	(0.158)	(0.181)
Household Head Single	-0.126	0.042	0.068
	(0.343)	(0.147)	(0.167)
Household Head Female	0.732*	-0.323*	-0.390*
	(0.402)	(0.175)	(0.202)
Household Head Migrant	0.064	0.014	0.009
	(0.219)	(0.101)	(0.115)
Household Head Age	0.021**	-0.003	-0.003
	(0.009)	(0.004)	(0.005)
Household Head Education	-0.068***	0.022**	0.026**
	(0.022)	(0.010)	(0.011)
Household Head Agricultural Experience	-0.029***	0.005	0.004
	(0.010)	(0.004)	(0.005)
Household Head Member of Farm Organization	0.091	-0.095	-0.125
	(0.183)	(0.088)	(0.100)
Days Agricultural Work Forbidden	-0.003	0.000	0.000
	(0.002)	(0.001)	(0.001)
Household Income	-0.004**	0.000	0.000
	(0.002)	(0.001)	(0.002)
Household Working Capital	0.002	0.006***	0.007***
	(0.003)	(0.002)	(0.002)
Household Assets	-0.013***	0.004***	0.005***
	(0.003)	(0.001)	(0.002)
Household Landholdings	-0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
"Yes" to $12.50 Investment	0.218	-0.033	-0.027
	(0.217)	(0.095)	(0.107)
"Yes" to $25.00 Investment	-0.396*	0.106	0.127
	(0.226)	(0.091)	(0.104)
"Yes" to $37.50 Investment	-0.388*	0.126	0.147
	(0.211)	(0.097)	(0.111)
"Yes" to $50.00 Investment	-0.205	-0.018	-0.017
	(0.243)	(0.112)	(0.128)
"Yes" to $62.50 Investment	-0.142	0.004	0.006

	(0.299)	(0.136)	(0.158)
"Yes" to $75.00 Investment	0.151	-0.226	-0.234
	(0.342)	(0.169)	(0.186)
Constant	3.793***	-	-4.152***
	(0.456)		(0.256)
Observations	1,178	1,045	1,045
District Dummies	Yes	Yes	Yes
p-value (Joint Significance of WTP Dummies)	0.08	0.34	0.36
R-squared	0.206	-	-

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3. Estimation Results for OLS, Cox Proportional Hazard, and Survival-Time Regressions Exploring Treatment Heterogeneity I.

Variables	OLS	Cox	Survival Time
Dependent Variable: Duration of Hungry Season			
Contract Farming Participant	0.210	0.009	0.004
	(0.253)	(0.109)	(0.125)
Contract Farming Participant x Number of Kids	-0.191**	0.060*	0.070*
	(0.082)	(0.034)	(0.039)
Number of Kids in Household	0.172	-0.053	-0.060
	(0.121)	(0.050)	(0.057)
Household Size	0.007	0.002	0.002
	(0.059)	(0.028)	(0.032)
Dependency Ratio	0.255	-0.168	-0.187
	(0.583)	(0.231)	(0.259)
Household Head Single	-0.164	0.056	0.085
	(0.349)	(0.150)	(0.171)
Household Head Female	0.765*	-0.330*	-0.399*
	(0.406)	(0.176)	(0.204)
Household Head Migrant	0.066	0.006	-0.002
	(0.219)	(0.102)	(0.115)
Household Head Age	0.024**	-0.004	-0.004
	(0.010)	(0.004)	(0.005)
Household Head Education	-0.068***	0.022**	0.026**
	(0.022)	(0.010)	(0.012)
Household Head Agricultural Experience	-0.029***	0.004	0.003
	(0.010)	(0.004)	(0.005)
Household Head Member of Farm Organization	0.087	-0.088	-0.115
	(0.180)	(0.086)	(0.097)
Days Agricultural Work Forbidden	-0.003	0.000	0.001
	(0.002)	(0.001)	(0.001)
Household Income	-0.004**	0.000	0.000
	(0.002)	(0.001)	(0.002)
Household Working Capital	0.002	0.005***	0.007***
	(0.003)	(0.002)	(0.002)
Household Assets	-0.013***	0.004***	0.005***
	(0.003)	(0.001)	(0.001)
Household Landholdings	-0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
"Yes" to $12.50 Investment	0.197	-0.028	-0.022
	(0.217)	(0.095)	(0.107)
"Yes" to $25.00 Investment	-0.415*	0.107	0.126
	(0.227)	(0.091)	(0.104)
"Yes" to $37.50 Investment	-0.372*	0.124	0.144

	(0.211)	(0.098)	(0.112)
"Yes" to $50.00 Investment	-0.196	-0.004	0.000
	(0.238)	(0.108)	(0.124)
"Yes" to $62.50 Investment	-0.142	0.011	0.014
	(0.291)	(0.136)	(0.157)
"Yes" to $75.00 Investment	0.194	-0.245	-0.258
	(0.341)	(0.171)	(0.188)
Constant	3.592***	-	-4.078***
	(0.487)		(0.271)
Observations	1,178	1,045	1,045
District Fixed Effects	Yes	Yes	Yes
R-squared	0.213	-	-

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4. Estimation Results for OLS, Cox Proportional Hazard, and Survival-Time Regressions Exploring Treatment Heterogeneity II.

Variables	OLS	Cox	Survival Time
Dependent Variable: Duration of Hungry Season			
Contract Farming Participant	0.206	-0.005	-0.013
	(0.254)	(0.109)	(0.125)
Contract Farming Participant x Girls	-0.215*	0.118**	0.137**
	(0.120)	(0.054)	(0.061)
Contract Farming Participant x Boys	-0.163	0.015	0.018
	(0.120)	(0.048)	(0.054)
Number of Girls in the Household	0.214	-0.067	-0.076
	(0.133)	(0.056)	(0.063)
Number of Boys in the Household	0.129	-0.026	-0.028
	(0.141)	(0.057)	(0.065)
Household Size	0.007	-0.002	-0.003
	(0.059)	(0.028)	(0.032)
Dependency Ratio	0.258	-0.196	-0.223
	(0.584)	(0.231)	(0.258)
Household Head Single	-0.167	0.058	0.088
	(0.348)	(0.148)	(0.169)
Household Head Female	0.766*	-0.336*	-0.406**
	(0.406)	(0.175)	(0.202)
Household Head Migrant	0.061	-0.001	-0.009
	(0.221)	(0.102)	(0.116)
Household Head Age	0.024**	-0.004	-0.004
	(0.010)	(0.004)	(0.005)
Household Head Education	-0.067***	0.023**	0.027**
	(0.023)	(0.010)	(0.012)
Household Head Agricultural Experience	-0.029***	0.004	0.003
	(0.010)	(0.004)	(0.005)
Household Head Member of Farm Organization	0.084	-0.096	-0.123
	(0.179)	(0.086)	(0.098)
Days Agricultural Work Forbidden	-0.003	0.000	0.001
	(0.002)	(0.001)	(0.001)
Household Income	-0.004**	0.001	0.001
	(0.002)	(0.001)	(0.002)
Household Working Capital	0.002	0.005***	0.006***
	(0.003)	(0.002)	(0.002)
Household Assets	-0.013***	0.004***	0.005***
	(0.003)	(0.001)	(0.001)
Household Landholdings	-0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
"Yes" to $12.50 Investment	0.191	-0.025	-0.017

	(0.216)	(0.095)	(0.106)
"Yes" to $25.00 Investment	-0.420*	0.104	0.122
	(0.226)	(0.091)	(0.104)
"Yes" to $37.50 Investment	-0.366*	0.122	0.143
	(0.212)	(0.098)	(0.111)
"Yes" to $50.00 Investment	-0.193	-0.015	-0.012
	(0.238)	(0.108)	(0.125)
"Yes" to $62.50 Investment	-0.138	-0.003	-0.003
	(0.290)	(0.137)	(0.159)
"Yes" to $75.00 Investment	0.193	-0.250	-0.265
	(0.342)	(0.171)	(0.189)
Constant	3.586***	-	-4.069***
	(0.486)		(0.271)
Observations	1,178	1,045	1,045
District Fixed Effects	Yes	Yes	Yes
R-squared	0.213	-	-

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5. Probit Regression Results for Propensity Score Estimation

Variables	Probit
Dependent Variable: Contract Farming Participant	
Household Size	0.013
	(0.019)
Dependency Ratio	-0.019
	(0.191)
Household Head is Single	-0.162
	(0.188)
Household Head is Female	-0.189
	(0.217)
Household Head is Migrant	0.040
	(0.123)
Household Head Age	-0.014**
	(0.006)
Household Head Education	-0.015
	(0.013)
Household Head Agricultural Experience	0.008
	(0.006)
Household Head Member of Farm Organization	0.472***
	(0.097)
Days Agricultural Work is Forbidden	-0.002
	(0.001)
Household Income	0.002
	(0.002)
Household Working Capital	0.004
	(0.004)
Household Assets	0.001
	(0.002)
Household Landholdings	0.000*
	(0.000)
Yes to $12.50 Investment	0.299**
	(0.131)
Yes to $25.00 Investment	0.433***
	(0.122)
Yes to $37.50 Investment	0.434***
	(0.123)
Yes to $50.00 Investment	0.596***
	(0.129)
Yes to $62.50 Investment	0.372**
	(0.167)
Yes to $75.00 Investment	0.569***

	(0.158)
Constant	0.131
	(0.247)
Observations	1,178
District Dummies	Yes
Pseudo R-squared	0.069
Log Likelihood	-760.359

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6. Outcome Variable: Duration of Hungry Season

Sample	1 Neighbor Caliper 0.01	3 Neighbors Caliper 0.01	3 Neighbors Caliper 0.001
Unmatched Sample	-0.400***	-0.400***	-0.400***
	(0.123)	(0.123)	(0.123)
Average Treatment Effect on the Treated	-0.194	-0.305	-0.295
	(0.234)	(0.223)	(0.255)
Average Treatment Effect on the Untreated	-0.062	-0.204	-0.249
	(0.225)	(0.207)	(0.269)
Average Treatment Effect	-0.127	-0.252	-0.272
	(0.204)	(0.196)	(0.241)

Standard errors in parentheses. Standard errors calculated using three neighbors to calculate conditional variance as in Abadie and Imbens (2006). *** p<0.01, ** p<0.05, * p<0.1

Table 7. Estimation Results for Median and Robust Regressions.

Variables	(1) Median	(2) Robust
Dependent Variable: Duration of Hungry Season		
Contract Farming Participant	-0.306**	-0.255**
	(0.147)	(0.121)
Household Size	0.023	0.040
	(0.035)	(0.029)
Dependency Ratio	0.331	0.364
	(0.354)	(0.291)
Household Head Single	0.275	0.114
	(0.347)	(0.285)
Household Head Female	0.095	0.290
	(0.396)	(0.326)
Household Head Migrant	-0.034	0.070
	(0.227)	(0.187)
Household Head Age	0.022**	0.024***
	(0.011)	(0.009)
Household Head Education	-0.040*	-0.049**
	(0.023)	(0.019)
Household Head Agricultural Experience	-0.022**	-0.026***
	(0.011)	(0.009)
Household Head Member of Farm Organization	-0.092	-0.037
	(0.180)	(0.148)
Agricultural Work Forbidden	-0.002	-0.003
	(0.003)	(0.002)
Household Income	-0.008***	-0.006***
	(0.002)	(0.002)
Household Working Capital	0.002	0.002
	(0.004)	(0.003)
Household Assets	-0.011***	-0.012***
	(0.003)	(0.002)
Household Landholdings	0.000	-0.000
	(0.000)	(0.000)
"Yes" to $12.50 Investment	0.217	0.191
	(0.246)	(0.202)
"Yes" to $25.00 Investment	-0.489**	-0.419**
	(0.229)	(0.188)
"Yes" to $37.50 Investment	-0.248	-0.269
	(0.231)	(0.190)
"Yes" to $50.00 Investment	-0.480**	-0.356*
	(0.242)	(0.199)
"Yes" to $62.50 Investment	-0.158	-0.185

	(0.313)	(0.257)
"Yes" to $75.00 Investment	-0.285	-0.191
	(0.298)	(0.245)
Constant	3.999***	3.751***
	(0.472)	(0.388)
Observations	1,178	1,178
District Fixed Effects	Yes	Yes
R-squared	-	0.200

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 8. Synthesis of Estimated Treatment Effects

Estimator	Treatment Effect Type	Estimated Effect (in Months)
OLS Regression without WTP Controls	ATE	-0.294
OLS Regression	ATE	-0.277
Median Regression	ATE	-0.306
Robust Regression	ATE	-0.255
PSM 1 Nearest Neighbor, Caliper 0.01	ATE	-0.127
PSM 3 Nearest Neighbors, Caliper 0.01	ATE	-0.252
PSM 3 Nearest Neighbors, Caliper 0.001	ATE	-0.272
PSM 1 Nearest Neighbor, Caliper 0.01	ATT	-0.194
PSM 3 Nearest Neighbors, Caliper 0.01	ATT	-0.305
PSM 3 Nearest Neighbors, Caliper 0.001	ATT	-0.295
PSM 1 Nearest Neighbor, Caliper 0.01	ATU	-0.062
PSM 3 Nearest Neighbors, Caliper 0.01	ATU	-0.204
PSM 3 Nearest Neighbors, Caliper 0.001	ATU	-0.249
Treatment Regression	LATE	-1.930