Contract Farming as Partial Insurance*

Marc F. Bellemare† Yu Na Lee‡ Lindsey Novak§

September 9, 2016

Abstract

The institution of contract farming, wherein a processing firm contracts out the production of an agricultural commodity to a grower household, has received much attention in recent years. We look at whether participation in contract farming is associated with lower levels of income variability for a sample of 1,200 households in rural Madagascar. Relying on a framed field experiment aimed at eliciting respondent marginal utility of participation in contract farming for identification in a selection-on-observables design, we find that participation in contract farming is associated with a 0.20-standard deviation decrease in income variability. Looking at the mechanism behind this finding, we find support for the hypothesis that fixed-price contracts explain the reduction in income variability associated with contract farming. Then, because the same assumption that makes the selection-on-observables design possible also satisfies the conditional independence assumption, we then estimate propensity score matching models, the results of which show that our core results are robust

*VERY ROUGH DRAFT. DO NOT CITE OR CIRCULATE WITHOUT WRITTEN PERMISSION FROM THE AUTHORS. We are grateful to NIFA for funding this work through grant MIN-14-061—"Smallholder Participation in Agricultural Value Chains: Evidence from Madagascar."

†Corresponding Author and Associate Professor and Director, Center for International Food and Agricultural Policy, Department of Applied Economics, University of Minnesota, 1994 Buford Ave, Saint Paul, MN 55108, mbellema@umn.edu.

‡PhD Candidate, Department of Applied Economics, University of Minnesota, 1994 Buford Ave, Saint Paul, MN 55108, leex5244@umn.edu.

§PhD Candidate, Department of Applied Economics, University of Minnesota, 1994 Buford Ave, Saint Paul, MN 55108, novak352@umn.edu.

and that participation in contract farming would have greater beneficial effects for those households that do not participate than for those who do, i.e., the magnitude of the average treatment effect on the untreated exceeds that of the average treatment effect on the treated. Our findins thus show that participation in contract farming can help rural households partially insure against income risk via contracts that transfer price risk from growers to processors.

Keywords: Contract Farming, Risk and Uncertainty, Outgrower Schemes, Grower-Processor Contracts, Agricultural Value Chains

JEL Classification Codes: L24, O13, O14, Q12

1 Introduction

At the root of economic underdevelopment and persistent poverty lie multiple market failures. One market failure of particular importance in developing countries is that of the insurance market. In most developing countries, information problems—adverse selection and moral hazard—are important enough that, in many cases, it is simply not profitable to offer insurance against risks which, in developed countries, are commonly insurable.

This failure of the insurance market constrains welfare in two ways. First, it constrains current welfare in that it forces individuals and households to sink valuable resources on self-insuring against those risks, however partially.[1] Second, it constrains future welfare in that it prevents those same individuals and households from making investments—financial, or in agricultural technology, education, and so on—today which might allow them to attain higher levels of welfare tomorrow.[2]

Contract farming, the institution wherein a processor contracts the production of an agricultural commodity out to a grower, can in theory serve as a partial insurance mechanism for rural households in developing countries. In her discussion of the advantages of contract farming, Grosh (1994) explains how contract farming can help resolve insurance market failures by insuring growers against price risk in cases where the processor offers a guaranteed price as part of the contract. This can lead to more stable incomes which, for risk-averse growers, would mean higher levels of welfare.

Does contract farming help empirically resolve insurance market failures? That is the question we pose in this paper. Specifically, we ask whether contract farming serves as a partial insurance mechanism for the households that choose to participate as growers by reducing the income variability they face. We answer this question by using survey data on 1,200 households in rural

[1] Insurance can be full or partial. In the former case, the entirety of a risk is insured, and the insured party receives full compensation in case of an adverse event. In the latter case, only a fraction of a risk is insured, and the insured party receives less-than-full compensation in case of an adverse event.

[2] Additionally, insurance market failures lead to the emergence of institutions that may be dysfunctional. For example, since Stiglitz (1974), sharecropping has been viewed as trading off risk sharing and incentives, and thus as a consequence of insurance market failures. Though it remains an empirical question whether sharecropping actually leads to Marshallian inefficiency due to identification issues, the institution certainly has the potential to lead to be second-best, and the provision of insurance could re-establish the first-best (i.e., fixed rental) solution.

Madagascar, half of which participate in contract farming as growers. Our data, which cover a dozen crops across six regions of Madagascar, have been used by Bellemare (2012) and Bellemare and Novak (2016) to respectively study the impacts of participation in contract farming on income levels and food security. To help disentangle the potentially causal relationship flowing from participation in contract farming to reduced income variability from the correlation between the two, we rely on a framed field experiment which elicited each respondent's willingness to pay (WTP) to participate in a hypothetical contract farming agreement,[3] which we argue obviates statistical endogeneity issues due to unobserved heterogeneity and reverse causality.[4] As in Bellemare and Novak (2016), we incorporate the WTP data in a selection-on-observables strategy, and we then assess the robustness of our regression results by estimating propensity score matching models.

We find that participating in contract farming is associated with a decrease of 0.2-standard deviation in the average household's income risk, proxied for here by the variability of that household's income. Looking into the potential mechanisms underlying this finding, we find that our finding is almost entirely due the presence in the data of contracts wherein the processor offers a guaranteed fixed price to the growers, but that it is not the case that income from contract farming is negatively correlated with other sources of income. Moreover, we find that the magnitude of the average treatment effect on the untreated (ATU) exceeds that of the average treatment effect on the treated (ATT). In other words, the reduction in income variability associated with participation in contract farming would actually be greater for households that do not participate in contract farming than it is for households that do. This last finding could have important policy implications, because although it might be easy for smallholders to compare income levels between those households that participate in contract farming and those that do not and decide whether they would like to participate in contract farming on the basis of that comparison, it is much less easy for them to perceive differences in income variability and make decisions on that basis.

There is a long, well-established empirical literature dating back to the early 1990s looking at the impacts of contract farming on the welfare of growers. Most of that literature, however, looks at the effects of participation in contract farming on the income *levels* of participating households

[3] On framed versus artefactual field experiments, see List (2011).

[4] We argue below that measurement error is negligible in this context.

(Glover, 1990; Singh, 2002; Warning and Key, 2002; Kumar and Kumar, 2008; Sharma, 2008; Maertens and Swinnen, 2009; Miyata et al., 2009; Jones and Gibbon, 2011; Bellemare, 2012; Mwambi et al., 2013; Vande Velde and Maertens, 2014; Wainaina et al., 2014; Wang et al., 2014; and Briones, 2015) or some variant thereof (Raynolds, 2002; Simmons et al., 2005; Begum, 2006; Minten et al., 2009; Bolwig et al., 2009; Narayanan, 2014; and Trifkovic, 2014). Beyond proximate outcomes like income and closely related variables (e.g., farm profits, farm revenue, and yields), however, the effect of participation in contract farming has only been documented for a handful of more distal outcomes such as the demand for women's labor (Raynolds, 2002), employment opportunities for women (Singh, 2002), gender inequality (Maertens and Swinnen, 2002), happiness (Dedehouanou et al., 2013), and food security (Bellemare and Novak, 2016). As Bellemare (2015) has argued, it might be time to start focusing on outcomes beyond income, and this paper is an effort in that direction.

Our contribution is thus threefold. First, we contribute to the agricultural and development economics literatures by providing evidence that the institution of contract farming can serve as a partial insurance mechanism for rural households in developing countries. Second, we contribute to the literature on applied contract theory by documenting that the likely mechanism whereby contract farming serves as a partial insurance mechanism is via contracts that transfer output price risk from the grower to the processor.[5] Lastly, we contribute to the development policy literature by showing that the impacts of participation in contract farming on income variability, though they are on average negative and significant, would be even larger for those households that do not participate in contract farming than they are for those households that do.

The remainder of this paper is organized as follows. Section 2 lays out a simple theoretical framework showing the mechanisms whereby participation in contract farming can serve as a partial insurance mechanism for participating households. In section 3, we present the empirical framework we rely on to study the effects of participation in contract farming on income variability, paying particular attention to our identification strategy. Section 4 presents the data and discusses some descriptive statistics. In section 5, we

[5] On the consequences of price risk on the welfare of producers, see the theoretical studies by Baron (1970) and Sandmo (1971), the observational studies by Barrett (1996) and Bellemare et al. (2013), and recent experimental work by Lee, Bellemare, and Just (2016a, b).

present our empirical results. Section 6 concludes with policy implications and with some directions for future research.

2 Theoretical Framework

Assume that a representative producer growing a single crop has a von Neumann-Morgenstern utility function $U(\cdot)$ defined over profit π. The function $U(\pi)$ is twice continuously differentiable, strictly increasing, and strictly concave, i.e., $U' > 0$ and $U'' < 0$. Let $\tilde{p}$ be a piece rate, i.e., the price at which the producer can sell each unit of his crop q at market after harvest; this piece rate is a random variable.

The producer can choose to participate in contract farming by agreeing to sell a fraction $\alpha \in (0, 1]$ of his crop to a processor who will pay the certain price $\overline{p} > 0$ for each unit of q. In that case, the producer's profit is such that

$$\pi = [(1-\alpha)\tilde{p} + \alpha\bar{p}]\, q - F - C(q), \tag{1}$$

where $C(q)$ denotes the total cost of producing output q, with $C(q)$ twice continuously differentiable and strictly increasing, i.e., $C' > 0$.

Because the market price $\tilde{p}$ is a random variable, the producer's expected profit is such that

$$E(\pi) = \int_0^\infty [\{(1-\alpha)\tilde{p} + \alpha\bar{p}\}q - C(q)]\, dF(\tilde{p}), \tag{2}$$

where E denotes an expectation. Similarly, the variance of the producer's profit is such that

$$Var(\pi) = \int_0^\infty [\{(1-\alpha)\tilde{p} + \alpha\bar{p}\}q - C(q) - E(y)]^2\, dF(\tilde{p}), \tag{3}$$

but the expression inside the square brackets can be rewritten as

$$\{(1-\alpha)\tilde{p} + \alpha\bar{p}\}q - C(q) - \{(1-\alpha)E(p) + \alpha\bar{p}\}q + C(q), \tag{4}$$

which means that

$$Var(\pi) = \int_0^\infty [(1-\alpha)(\tilde{p} - E(p))q]^2\, dF(\tilde{p}). \tag{5}$$

The foregoing leads to the following proposition, which is the core testable hypothesis we derive in this paper.

Proposition 1 *Under the assumptions made so far, participation in contract farming decreases the variance of a participating producer's profit. Specifically, the higher contract coverage α, the lower the variance of the producer's profit.*

Proof. The proof is straightforward considering that $\frac{\partial Var(\pi)}{\partial \alpha}$ is such that

$$\frac{\partial Var(\pi)}{\partial \alpha} = -\int_0^\infty 2\left[(1-\alpha)\{(\tilde{p}-E(p))q\}^2\right] dF(\tilde{p}) \leq 0, \tag{6}$$

which follows from the fact that the expression inside the square brackets is nonnegative given that $\alpha \in (0,1)$ for a producer who participates in contract farming. ■

The producer's maximization problem can be expressed as follows:

$$\max_{\alpha,q} EU(\pi) = \max_{\alpha,q} \int_0^\infty [\{(1-\alpha)\tilde{p} + \alpha\bar{p}\}q - TC(q)]\, dF(\tilde{p}), \tag{7}$$

which leads to the following proposition.

Proposition 2 *If contract farming guarantees a price equal to the expected market price, a risk-averse producer will benefit from full coverage for a given level of production. That is, if $\bar{p} = E(p)$, α will be equal to* 1.

Proof. Consider two choices: (i) full participation in contract farming ($\alpha = 1$) and no participation ($\alpha = 0$) given a level of production q. The contract guarantees $\bar{p} = E(p)$. The producer will benefit from participating in contract farming if and only if:

$$EU[\pi|\alpha = 1] - EU[\pi|\alpha = 0] \geq 0, \tag{8}$$

so that

$$EU[y|\alpha = 1] - EU[y|\alpha = 0] = EU[E(p)q - C(q)] - EU[\tilde{p}q - C(q)] \tag{9}$$

$$EU[y|\alpha = 1] - EU[y|\alpha = 0] = EU[E(y)] - EU[y] \tag{10}$$

$$EU[y|\alpha = 1] - EU[y|\alpha = 0] = U[E(y)] - EU[y] \geq 0, \tag{11}$$

where the last equality follows from assuming that $U'' < 0$ and by Jensen's inequality. ■

3 Empirical Framework

This section discusses the empirical framework we use in our attempt to study the impact of participation in contract farming on income variability. We begin this section by discussing how we measure income variability—our outcome of interest—for the remainder of this paper. We then move on to our estimation and identification strategies.

3.1 Measurement of Income Variability

A first difficulty of in answering the research question we pose is that we rely on cross-sectional data. Ideally, one would like to have longitudinal data at one's disposal to measure the variability of a household's income over time. That is, one would want to use longitudinal data in order to obtain, for each household, a measure of central tendency (e.g., mean or median) of that household's income in order to then estimate how far that household's income typically lies from that measure of central tendency. For example, one could use longitudinal data to simply compute the standard deviation or the variance of a household's income over time.

Our use of cross-sectional data obviously prevents us from estimating a measure of central tendency for the income of each household, a limitation of our approach which we wish to emphasize. To remedy this, we use three proxy measures of income variability, all of which rely on some measure of central tendency for a representative household in the data—either the average household in our sample, or the average household in the sub-sample (i.e., contract farming participants or nonparticipants) a household belongs to. The identifying assumption we make here is thus that the measure of central tendency used in each of those three measures is an accurate representation of the income of the average household in the relevant sample or sub-sample.

We measure income variability in three distinct ways: conditional heteroskedasticity (CH), distance from sample mean squared (DSM), and distance from conditional mean squared (DCM). The remainder of this subsection gives precise definitions for those measures.

3.1.1 Conditional Heteroskedasticity

Under this proxy measure of income variability, we first estimate the equation

$$\ln y_i = \alpha + \underline{\beta}\underline{x}_i + \epsilon_i, \tag{12}$$

where $\ln y_i$ denotes the logarithm of household i's income, $\underline{x}_i$ is a vector of household-specific control variables,[6] and ϵ is an error term with mean zero. Our conditional heteroskedasticity (CH) measure is such that, for each household i, we compute

$$CH_i = \widehat{\epsilon}_i^2, \tag{13}$$

where $\widehat{\epsilon}_i$ denotes the residual for household i, whose square we as our measure of income variability in two distinct approaches.

First, we conduct a t-test of the null hypothesis that $\overline{CH} = \frac{1}{N}\sum_{i=1}^{N} CH_i$ does not differ between the sub-sample of households that participate in contract farming and those that do not; this is a test of conditional heteroskedasticity whose goal is to establish whether the variance of the residual is the same across sub-samples.

Second, we use CH_i as our dependent variable in a regression of CH on the variable of interest (i.e., participation in contract farming), the control variables, and the WTP estimates, as discussed in detail in the next subsection. This is also a test of conditional heteroskedasticity, but one which conditions on more than just the treatment variable.

3.1.2 Distance from Sample Mean Squared

Under this proxy measure of income variability, we let $\overline{y} = \frac{1}{N}\sum_{i=1}^{N} y_i$ and compute, for each household i, the square of the distance between that household's income and the mean income in the data, such that

$$DSM_i = (y_i - \overline{y})^2 . \tag{14}$$

3.1.3 Distance from Conditional Mean Squared

Under this proxy measure of income variability, we let $\overline{y}(X = x)$ denote the mean of y for the sub-sample of observations where $X = x$ and compute,

[6] Throughout this paper, underlines denote vectors.

for each household i , the square of the distance between that household's income and the mean income in the data, such that

$$DCM_i = \left\{[y_i - \overline{y}(D_i = 0)]^{I(D_i=0)} \cdot [y_i - \overline{y}(D_i = 0)]^{I(D_i=0)}\right\}^2, \tag{15}$$

where $D_i = 1$ if a household participates in contract farming and $D_i = 0$ otherwise.

Again, all three of the variables just defined are proxy measures of income variability. For the remainder of this paper, however, we drop the term "proxy" for ease of exposition, but the reader should bear in mind that those are proxy measures.

3.2 Estimation Strategy

This section discusses the two approaches—regression and matching—we use in order to study the relationship between participation in contract farming and income variability. Recall that we use three distinct proxy measures of income variability, viz. CH, DSM, and DCM. In what is perhaps a slight abuse of notation, we use σ_i^2 to denote the value of any of CH, DSM, and DCM for household i. In what follows, we closely follow the notation in Bellemare and Novak (2016).

3.2.1 Regression

Starting with the regression approach, our core estimable equation is such that

$$\sigma_i^2 = \alpha_1 + \underline{\beta}_1 \underline{x}_i + \gamma_1 D_i + \upsilon_i, \tag{16}$$

where σ_i^2 is a standardized version of any of CH, DSM, and DCM for household i,[7] $\underline{x}_i$ is a vector of control variables (including district dummies), D_i is our treatment variable which equals one if household i participates in contract farming and equals zero otherwise, and υ_i is an error term with mean zero.

Our coefficient of interest is γ. If D were randomly assigned, it would be orthogonal to the variables on the RHS of equation 16, and γ would provide an estimate of the average treatment effect (ATE) of participating in contract

[7] We standardize all of our dependent variables for comparability across all three of our proxy measures of income variability as well as for ease of interpretation.

farming on the proxy for income variability on the left-hand side of equation 16. Participation in contract farming, however, is not randomly assigned, and so we estimate the following version of equation 16:

$$\sigma_i^2 = \alpha_2 + \underline{\beta}_2 \underline{x}_i + \gamma_2 D_i + \underline{\delta}_2 \underline{r}_i + \eta_i, \tag{17}$$

where σ_i^2, $\underline{x}_i$, and D_i are defined as before, but where η_i is an error term with mean zero and $\underline{r}_i$ is a vector of variables capturing the respective likelihood of being willing to pay various amounts of money in order to participate in contract farming. Since we use this vector of proxy measures of willingness to pay (WTP) in an effort to identify the causal impact of participation in contract farming on income variability, we defer our discussion of it to the next subsection. Until then, it should be sufficient to note that the research design we rely on in this paper is a selection-on-observables (SOO) design.

3.2.2 Propensity Score Matching

The assumption that allow us to use an SOO design also make it likely that the conditional independence assumption holds in this context (Imbens, 2015), which means that we can use propensity score matching (PSM) methods to answer the research question posed in this paper. The use of PSM methods has two distinct advantages in this context. First, it allows assessing the robustness of our regression results. Second, it allows estimating average treatment effects on the treated (ATT) and on the untreated (ATU), two measures which can be useful to inform economic policy in this context.

To use PSM methods, we proceed in two steps. First, we estimate the following equation by using a probit

$$D_i = \kappa + \underline{\lambda x}_i + \underline{\theta r}_i + \xi_i, \tag{18}$$

where the variables denote the same things as in equation 17. The estimated coefficients in equation 18 are then used to obtain a prediction $\widehat{D}$ of the dependent variable—the propensity score, which measures the likelihood that each individual observation i is treated, i.e., the likelihood that a household i participates in contract farming estimated on the basis of the covariates on the RHS of equation 18.

Second, we match households that participate in contract farming with households that do not on the basis of their propensity scores. As in Bellemare and Novak (2016), we do this three different ways for robustness:

matching with replacement and considering (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. For each of those specifications, we report the ATE, but also the ATT and the ATU.

3.3 Identification Strategy

We rely on an SOO identification strategy in our attempt to estimate the causal impact of participation in contract farming on income variability. In this section, we first explain the framed field experiment used to elicit respondent WTP to participate in contract farming. Then, we go through the usual sources of statistical endogeneity—unobserved heterogeneity, reverse causality, and measurement error—and explain how well our approach does against each one.

3.3.1 Experimental Setup

We use the same framed field experiment used in Bellemare (2012) and Bellemare and Novak (2016). Specifically, a contingent valuation experiment was run in the field that asked each respondent whether he would be willing to pay a randomly selected amount of money (hereafter, the bid r) in order to participate in a hypothetical contract farming agreement that would increase his income by 10 percent.

Each respondent's bid r_i was selected from the set $r_i \in$\{\$12.50, \$25.00, \$37.50, \$50.00, \$62.50, \$75.00\} with equal probability (i.e., with the throw of a fair die).[8] To give the reader some perspective, the average annual household income in our data was equal to about \$970, and so the bid could range anywhere from about 2 to 8 percent of that average household's annual income.

For each respondent, we have a binary-choice (i.e. yes or no) answer to the question posed in the framed field experiment. One immediate problem is that a respondent is not asked whether he is willing to participate in contract farming at all levels of the bid variable. Indeed, for each respondent, we know whether he would be willing to participate in the hypothetical contract farming agreement only for the bid that was randomly drawn for him.

[8] Dollar amounts are reported here for ease of exposition. During fieldwork, respondents were presented with equivalent amounts stated in the local currency.

Eliciting a response for just one bid is common in the contingent-valuation literature in order to avoid respondents anchoring their response on the next level up or down from the current bid.

In order to recover what a respondent's answer would be for the other bids, i.e., the five bids which were not randomly selected, we follow Bellemare and Novak (2016) and impute what that respondent's answer would be for those other bids. In other words, for each bid level in the set {\$12.50, \$25.00, \$37.50, \$50.00, \$62.50, \$75.00}, a respondent was only asked about one of those levels. We use the observables $\underline{x}$ of each respondent in order to forecast what his answers would be if he were asked the same question for the five other bid levels. Specifically, if a respondent was presented with bid r_{ij}, where j denotes any of the six possible bid levels, we regress each unasked bid $r_{i,-j}$ on $\underline{x}$ in order to predict $\widehat{r}_{i,-j}$.

Why is it possible to accurately forecast $\widehat{r}_{i,-j}$? Because of the experimental nature of the question. That is, because each respondent is presented with a bid that is selected for him at random by the throw of a die, the value of that bid is completely exogenous to any of the respondent's characteristics, *both observable and unobservable*. What this means is that it is then possible to accurately forecast what a respondent's answer to other other bids would be free of any bias, purely on the basis of observables.

The end result is a vector $\underline{r} = (r_{i,j}, \widehat{r}_{i,-j})$ which summarizes (i) whether the respondent reports being willing to pay the bid amount randomly selected for him by the throw of a die to participate in the hypothetical contract farming agreement, and (ii) the likelihood that he would be willing to pay the other bid amounts.[9] It is this vector which we rely on for our SOO design. The only difficulty this introduces is the fact that by virtue of regressing on imputed variables, we introduce generated regressors in our analysis. We correct for this everywhere by bootstrapping our standard errors wherever those imputations are included on the RHS.

An example might be useful. Suppose a respondent rolls the die and gets a four. He is then asked whether he would be willing to pay \$50 to participate in a contract farming agreement that would raise his income by 10 percent. Suppose further that he answers "Yes" to that question. Omitting

[9] We estimate linear probability models for our imputations. Strictly speaking, this means that the imputed likelihoods of participation at every bid level can technically lie outside of the $[0, 1]$ interval. This is not a problem given that what matters for identification in our SOO design is that ordinality be preserved between respondents, without regard to cardinality.

the subscript i and keeping only the j subscripts, this respondent's $\underline{r}$ vector would be such that $\underline{r} = (\widehat{r}_1, \widehat{r}_2, \widehat{r}_3, 1, \widehat{r}_5, \widehat{r}_6)$.

3.3.2 Identification

The identifying assumption in this paper is that the vector $\underline{r} = (r_{i,j}, \widehat{r}_{i,-j})$, which proxies for a respondent's WTP to participate in contract farming at all bid levels, accounts for a respondent's marginal utility of participating in contract farming, which allows purging the error term in equation of much of its correlation with the treatment variable 17. In this section, we explain the reasoning behind this claim, which allows both adopting the SOO design just laid out in the regression context as well as assuming the conditional independence assumption holds in the matching context.

Any identification strategy has to be judged relative to how it fares relative to the three usual sources of statistical endogeneity, viz. (i) unobserved heterogeneity, (ii) reverse causality, and (iii) measurement error. Much of our discussion that in Bellemare (2012) and Bellemare and Novak (2016).

Unobserved Heterogeneity. Many of our respondents' characteristics are unobserved. When unobservable characteristics are correlated with variables on the RHS of the equation of interest (here, equation 17), estimated coefficients are biased. Here, because the vector $\underline{r}$ captures a respondent's marginal utility of participating in contract farming, many of the typically unobservable characteristics whose correlation with D would mar our estimate of γ in equation 17 (e.g., risk and ambiguity preferences, entrepreneurial and technical ability, etc.) are accounted for by shifts in a respondent's marginal utility of participating in contract farming. For example, suppose two respondents are identical, save for their entrepreneurial ability. The respondent whose entrepreneurial ability is higher might prefer starting a business to participating in contract farming; this would be reflected in his having a lower marginal utility of participating in contract farming relative to the other respondent, and this difference would be captured in different values of the vector $\underline{r}$ for the two respondents. A similar reasoning applies to other unobservable sources of variation in marginal utility of participating in contract farming which could be correlated with variables on the RHS of equation 17, which considerably lessens the problem of unobserved heterogeneity.

Reverse Causality. This would arise in cases where the prospect of getting partially insurance via reduced income variability would induce respondents to participate in contract farming. In such scenarios, our estimate of

γ would be biased because of reverse causality flowing from income variability to participation in contract farming. But this is simply another version of the unobserved heterogeneity story. Indeed, assume once again that two respondents are identical, save for their willingness to participate in contract farming because of how they differ in their expectation that contract farming will serve to partially insure them. This would only affect their marginal utility of participating in contract farming which, again, the vector $\underline{r}$ would account for.

Measurement Error. This would arise in cases where our variable of interest, i.e., D, the dummy variable which measures whether respondents participate in contract farming or not, where measured with error. This is not a concern here, for three reasons. First, there is no incentive to lie about this, as there is no social stigma attached to participating in contract farming, nor is there a benefit to responding one way or the other. Second, there are no recall problems for this question, because respondents are fully aware of whether they participate in contract farming or not. Finally, the sampling frame was established with village chiefs, who made two lists for their community, one of all the households that participated in contract farming and one of all the households that did not, from which enumerators randomly selected respondents. This served as an additional check that respondents accurately reported their participation status. If there is any measurement error, it occurs at random, and it should be so minimal as to be unlikely to cause much attenuation bias.

Our identification strategy thus allows ruling out a number of sources of statistical endogeneity. As always with nonexperimental data, however, it is best to exercise caution. Here, this means that it is best to treat our estimates of γ as suggestive instead of causal.

4 Data and Descriptive Statistics

Given that Bellemare (2012) and Bellemare and Novak (2016) rely on the same data set we use in this paper and those two articles discuss the data in detail, we dedicate only a limited amount of space to discussing the data. The reader interested in knowing more about the details of data collection, descriptive statistics, the features of the contract farming agreements we study, and so on is encouraged to read Bellemare (2012) and Bellemare and Novak (2016). Specifically, though we will report descriptive statistics for the variables we use in our analysis, we will not expend any time discussing them, as both aforementioned paper do that.

The data were collected in the latter half of 2008 in 12 communes across six regions of Madagascar, with two communes sampled per region. The data set includes 1,200 households, half of which participate in contract farming and half of which do not. Regions were selected on the basis of either their development potential (i.e., they were labeled "growth poles" by government of Madagascar) or of their high density of contract farming, as reported in the 2007 census of communes (Moser, 2008). In each region, the two communes with the highest density of contract farming were selected. The contracts in the data cover about a dozen crops. As discussed in Bellemare (2012, 2015), this diversity of crops and geographical areas ensures that our findings have more external validity than those of most other studies of contract farming.

Data collection was funded by the World Bank's Madagascar office for a study of the welfare effects of participation in contract farming. No pre-analysis plan was filed before the data were collected, but the primary goal of data collection was to study the effects of participation in contract farming on income, as in Bellemare (2012). Additionally, because of how the sample was constructed—in each commune, enumerators interviewed equal numbers of contract farming participants and nonparticipants—we follow the recommendations of Solon et al. (2015) and use sampling weights when computing descriptive statistics, but not when estimating the relationship between participation in contract farming and income variability.

Table 1 presents descriptive statistics for the variables we use in our empirical analysis ($n = 1,078$), as well as balance tests between the sub-sample of households that do not participate in contract farming ($n = 599$) and households that do not ($n = 579$). Looking at the results of balance tests in the last column of Table 1, it is obvious that the variables retained for analysis are not orthogonal to a household's participation (or lack thereof) in contract farming, and so the empirical apparatus presented in section 3 is necessary if one is to attempt identifying the potential causal relationship flowing from participation in contract farming to income variability. For the remainder of this paper, whenever we discuss income, we refer to income per adult equivalent which, in our view,[10] is the most accurate representation of

[10]See Deaton (1997) for a discussion of why income per adult equivalent is a better measure of household welfare. For our analysis, we assign a weight of one to each individual between the ages of 15 and 65, a weight of 0.5 to each individual below the age of 15, and a weight of 0.75 to each individual older than 65.

a household's level of welfare. All the results in this paper are qualitatively the same whether we use income per adult equivalent or income per capita within the household as our measure of income.

5 Estimation Results and Discussion

Armed with that empirical apparatus in section 4 as well as the theoretical model in section 2 and the data described in the previous section, we now present our empirical results. We begin with nonparametric results by showing kernel density estimates of income variability for those households that participate in contract farming and those that do not for all three of our proxy measures of income variability, viz. CH, DSM, and DCM. We then turn to our parametric results, discussing in turn our core results and the mechanisms whereby participation in contract farming is likely to decrease income variability before moving on PSM results and other robustness checks.

5.1 Nonparametric Analysis

Before presenting kernel density estimates, we need to discuss the results of the ancillary regression in equation 12, which we use the residual from that regression to compute our CH measure of income variability. Table 2 presents the results of that regression. Though Bellemare (2012) used respondent WTP to participate in contract farming as an instrumental variable for actual participation in contract farming, we follow the cleaner research design in Bellemare and Novak (2016) by relying on an SOO design here as well. The results in Table 2 confirm the analysis in Bellemare (2012), i.e., participation in contract farming is associated with higher levels of income per adult equivalent.

As discussed, we use the square of the residual from equation 12 as our first (i.e., conditional heteroskedasticity, or CH) measure of income variability. We plot kernel density estimates for CH for households that participate in contract farming and households that do not in Figure 1.[11] Both kernel density estimates look almost identical. Similarly, Figures 2 and 3 plot kernel density estimates for DSM and DCM, with results similar to those in

[11]The kernel density estimates in Figures 1 to 3 rely on nonstandardized versions of our proxies for income variability. For our regression and matching results, we standardize all three variables.

Figure 1. So far, then, it looks as though there is no systematic difference in income variability between the households that participate in contract farming and those that do not. The results in Figures 1 to 3, however, only look at correlations. We now turn to our parametric analyses to see whether we can disentangle a potential causal relationship from the apparent lack of correlation.

5.2 Parametric Analyses

Recall that our CH measure of income variability lends itself to two different tests, one a t-test of whether income variability is equal across households that participate in contract farming and households that do not, and one regression-based test. Based on the results in Table 2, the a t-test of that CH is equal for participants and nonparticipants rejects the null at a significance level below 1 percent, and income variability is higher in the sub-sample of households that do not participate in contract farming.

For the regression-based approach, estimations for three different versions of equation 17—one for each proxy for income variability—are shown in Table 3. In all three cases, participation in contract farming is associated with a decrease in income variability ranging from 0.17 to 0.20 standard deviations significant at less than the 1 percent level in each case. In addition, the almost complete lack of significance of other RHS variables in Table 3 makes our core result that participation in contract farming is meaningfully associated with a decrease in income variability all the more convincing. The only other variable whose coefficient is significant in Table 3 is the household's landholdings, which are associated with an increase in income variability, presumably because everything else equal, greater amounts of landholdings means a greater exposure to agriculture and thus volatile commodity markets.

In Table 4, we look at whether there is any treatment heterogeneity by interacting our treatment variable with the control variables. There is little, if anything, that appears systematic in this case. The only interaction term that is significant—and then again, only in two out of three cases—is that between participation in contract farming and household size. The negative sign in this case suggests that the larger a participating household's size, the smaller the variability of its income. What mechanism underlies this relationship, however, is anybody's guess.

Regarding the mechanism whereby participation in contract farming reduces income variability, recall that Proposition 1 posited that contract farming insured growers against price risk via contracts in which they received a fixed price. In Table 5, we test this proposition by substituting the proportion of a household's plot that are under a fixed price contract for the treatment variable. For all three of our proxies for income variability, we find that the greater the proportion of a household's plots is used to grow crops under fixed price contract, the lower the variability of that household's income; in each case, the relationship is significant a less than the 1 percent level. Specifically, a households whose plots would entirely be under fixed price contracts would see its income variability be between 0.18 to 0.24 standard deviations lower than that of a household whose plots would be entirely used to grow crops to be sold on spot markets or within contracts whose price is not fixed. Our rejection of the null there provides strong support for Proposition 1, especially in light of the fact that once again, only one control variable—landholdings once again—is significantly associated with income variability.

Further, looking at the correlation between income from contract farming and income from other sources, we find that that correlation is positive and significant at less than the 10 percent level between income from contract farming and income from nonfarm enterprises as well as income from agriculture, but that that correlation is not statistically significantly different from zero between income from contract farming and income from livestock as well as income from labor markets. Consequently, it would be difficult to argue that contract farming serves as partial insurance because income from contract farming is negatively related with income from other sources.

Turning to our PSM results, Table 6 presents estimation results for equation 18, i.e., a probit aimed at predicting propensity scores. Similar results can be found in Bellemare (2012) and Bellemare and Novak (2016). Both those papers discuss the determinants of participation in contract farming, and since the probit results are only interesting insofar as they allow predicting propensity scores, we encourage readers interested in those determinants to consult those two papers.

Our interest here is in estimating the ATE as well as the ATT and the ATU of participating in contract farming. Table 7 summarizes our estimates of those depending on whether we look at (i) one neighbor and a caliper of 0.01 (antepenultimate column), (ii) three neighbors and a caliper of 0.01 (penultimate column), or (iii) three neighbors and a caliper of 0.001 (last column) and on whether we look at (i) CH (upper panel), (ii) DSM (middle panel), or (iii) DCM (lower panel).

Our estimates of the ATE of participating in contract farming on income variability are very close to the ones we get from our regression analysis, seeing as to how they lie between a decrease of about 0.13 to about 0.19 standard deviation in the variability of income associated with participation in contract farming. Though it is encouraging to see that our matching results confirm our regression results, what is even more interesting is the comparison between the ATT and the ATU. Intuitively, one would expect the magnitude of the ATT to exceed that of the ATU. Here, however, the opposite result obtains (i.e., the magnitude of the ATU exceeds that of the ATT) eight out of nine cases. In other words, it looks like when considering income variability, those households that do not participate in contract farming would benefit even more from participating in contract farming than those households that do participate, as the partial insurance derived from participation would be greater for nonparticipants than for participants.

Lastly, we estimated some median regressions for robustness. The results of those are presented in Appendix Table A1. Our core results appear robust in that in all but one case, the results of those median regressions—which are less sensitive to outliers than standard regressions—show a negative association between participation in contract farming and income variability. In all cases, the estimated effect is smaller in magnitude than in the case of standard regression. It would thus appear that much of what goes on in the standard regression case is due to observations in the tails of the income variability distribution.

In sum, it looks as though participation in contract farming can be an effective partial insurance mechanism for households in rural Madagascar, with estimate ATEs ranging from -0.13 standard deviations in the lower panel, third column of Table 7 to -0.21 standard deviations in the first column of Table 3. Moreover, our investigation of the mechanisms whereby contract farming can serve as partial insurance support Proposition 1, according to which fixed-price contracts are the main mechanism whereby this happens.

6 Summary and Concluding Remarks

In this paper, we have looked at whether participation in contract farming can serve as partial insurance for rural households, i.e., whether participating households experience lower levels of income variability. To do so, we have used the results of a framed field experiment aimed at eliciting WTP for participation in a hypothetical contract farming agreement that would raise the respondent's income level by 10 percent in an effort to exogenize actual participation in contract farming—our treatment variable—in a selection-on-observables design. Given that that design relies on the same assumption which makes propensity score matching viable, we supplement our core regression approach with a matching approach. Both approaches lead to similar estimates of the average treatment effect: in most cases, participation in contract farming is associated with a 0.2-standard deviation decrease in income variability, and so contract farming appears to offer participating households a certain degree of partial insurance. Looking at the mechanism behind our main result, we find that the presence of fixed-price contracts in the data—which guarantees growers a certain price for their crops—appears to explain our result, in line with the main prediction of our theoretical model.

Perhaps more importantly for development policy, our findings indicate that the usual intuitive ordering of average treatment effects between the treated and the untreated is reversed. That is, the counterfactual analysis our matching approach allows shows that those households that do not participate in contract farming would benefit from participating even more than those households that do participate—the untreated would receive a higher degree of partial insurance than the treated.

Our analysis is not without its limitations, and we wish to reiterate two important limitations of our work. First, given our research design, our results cannot be argued to be causal, though we claim that we control for the most importance sources of statistical endogeneity with our framed field experiment aimed at eliciting respondent WTP—and thus marginal utility—for contract farming. Second, in the absence of longitudinal data, our dependent variables are only proxies for income variability. To our knowledge, however, this is the first study to use a plausibly credible research design to look at the effect of participation in contract farming on income variability. We leave the use of better research designs combined with longitudinal data to future research.

References

[1] Alderman, Harold, John Hoddinott, and Bill Kinsey (2006), "Long-Term Consequences of Early Childhood Malnutrition," *Oxford Economic Papers* 58(3): 450-474.

[2] Angrist, Joshua D., and Jörn-Steffen Pischke (2009), *Mostly Harmless Econometrics*, Princeton: Princeton University Press.

[3] Banerjee, Abhijit V., and Sendhil Mullainathan (2010), "The Shape of Temptation: Implications for the Economic Lives of the Poor," NBER Working Paper.

[4] Barrett, Christopher B. (2002), "Food Security and Food Assistance Programs," *Handbook of Agricultural Economics*, Elsevier Science B.V

[5] Barrett, Christopher B., Maren Elise Bachke, Marc F. Bellemare, Hope C. Michelson, Sudha Narayanan, and Thomas F. Walker (2012), "Smallholder Participation in Contract Farming: Comparative Evidence from Five Countries," *World Development* 40(4): 715-730.

[6] Bellemare, Marc F. (2012), "As You Sow, So Shall You Reap: The Welfare Impacts of Contract Farming," *World Development* 40(7): 1418-1434.

[7] Bellemare, Marc F., Christopher B. Barrett, and David R. Just (2013), "The Welfare Impacts of Commodity Price Volatility: Evidence from Rural Ethiopia," *American Journal of Agricultural Economics* 95(4): 877-899.

[8] Bellemare, Marc F., and Lindsey Novak (2016), "Contract Farming and Food Security," *American Journal of Agricultural Economics* forthcoming.

[9] Dedehouanou, Senakpon F.A., Johan Swinnen, and Miet Maertens (2013), "Does Contracting Make Farmers Happy? Evidence from Senegal," *Review of Income and Wealth* 139(S1): S138-S160.

[10] Dupas, Pascaline, and Jonathan Robinson (2013), "Savings Constraints and Microenterprise Development: Evidence from a Field Experiment in Kenya," *American Economic Journal: Applied Economics* 5(1): 163-192.

[11] FAO (2013), *The State of Food and Agriculture*, Rome: FAO.

[12] Glover, David (1990), "," *Journal of Agricultural Economics* 41(3): 303-315.

[13] Grosh, Barbara (1994), "Contract Farming in Africa: An Application of the New Institutional Economics," *Journal of African Economies* 3(2): 231-261.

[14] Gelli, Aulo, Corinna Hawkes, Jason Donovan, Jody Harris, Summer Allen, Alan de Brauw, Spencer Henson, Nancy Johnson, James Garrett, and David Ryckembusch (2015), "Value Chains for Nutrition," Discussion Paper 01413, International Food Policy Research Institute.

[15] Imbens, Guido (2015), "Matching Methods in Practice: Three Examples, "*Journal of Human Resources* 50(2):373-419.

[16] Lancaster, Tony (1992), *The Econometric Analysis of Transition Data*, Cambridge: Cambridge University Press.

[17] Maertens, Miet, and Johan F.M. Swinnen (2009), "Trade, Standards, and Poverty: Evidence from Senegal," *World Development* 37(1): 161–178.

[18] Manski, Charles F., and Steven R. Lerman (1977), "The Estimation of Choice Probabilities from Choice Based Samples," *Econometrica* 45(8): 1977-1988.

[19] Michelson, Hope C. (2013), "Small Farmers, NGOs, and a Walmart World: Welfare Effects of Supermarkets Operating in Nicaragua," *American Journal of Agricultural Economics* 95(3): 628-649.

[20] Minten, Bart, Lalaina Randrianarison, and Johan F.M. Swinnen (2009), "Global Retail Chains and Poor Farmers: Evidence from Madagascar," *World Development* 37(11): 1728–1741.

[21] Miyata, Sachiko, Nicholas Minot, and Dinghuan Hu (2009), "Impact of Contract Farming on Income: Linking Small Farmers, Packers, and Supermarkets in China," *World Development* 37(11): 1781-1790.

[22] Montalbano, Pierluigi, Rebecca Pietrelli, and Luca Salvatici (2015), "Food Security and Value Supply Chain: The Case of Ugandan Maize," *Unpublished manuscript, University of Sussex and University of Roma Tre*

[23] Naryananan, Sudha (2014), “Profits from Participation in High-Value Agriculture: Evidence of Heterogeneous Benefits in Contract Farming Schemes in Southern India,” *Food Policy* 44: 142-157.

[24] Porter, Gina, and Kevin Phillips-Howard (1997), “Comparing Contracts: An Evaluation of Contract Farming Schemes in Africa,” *World Development* 25(2): 227-238.

[25] Rao, Elizaphan J.O., and Matin Qaim (2011), “Supermarkets, farm household income, and poverty: Insights from Kenya,” *World Development* 39(5): 784–796.

[26] Rousseuw, Peter, and Annick M. Leroy (2005), *Robust Regression and Outlier Detection*, New York: Wiley.

[27] Ruel, Marie T., and Harold Alderman (2013), “Nutrition-Sensitive Interventions and Programmes: How Can They Help to Accelerate Progress in Improving Maternal and Child Nutrition?,” *The Lancet* 382(9891): 536-551.

[28] Ruud, Jørgen (1960), *Taboo: A Study of Malagasy Customs and Beliefs*, Oslo: Oslo University Press.

[29] Simmons, Phil, Paul Winters, and Ian Patrick (2005), “An Analysis of Contract Farming in East Java, Bali, and Lombok, Indonesia,” *Agricultural Economics* 33(s3): 513–525.

[30] Singh, Sukhpal (2002), “Contracting Out Solutions: Political Economy of Contract Farming in the Indian Punjab,” *World Development* 30(9): 1621–1638.

[31] Smith, Adam (1976 [1776]), *An Inquiry into the Nature and Causes of the Wealth of Nations*, Chicago: University of Chicago Press.

[32] Stephens, Emma C., and Christopher B. Barrett (2011), “Incomplete Credit Markets and Commodity Marketing Behavior,” *Journal of Agricultural Economics* 62(1): 1-24.

[33] Stifel, David, Marcel Fafchamps, and Bart Minten (2007), “Taboos, Agriculture, and Poverty,” *Journal of Development Studies* 47(10): 1455-1481.

[34] Warning, Matthew, and Nigel Key (2002), "The Social Performance and Distributional Consequences of Contract Farming: An Equilibrium Analysis of the Arachide de bouche Program in Senegal," *World Development* 30(2): 255–263.

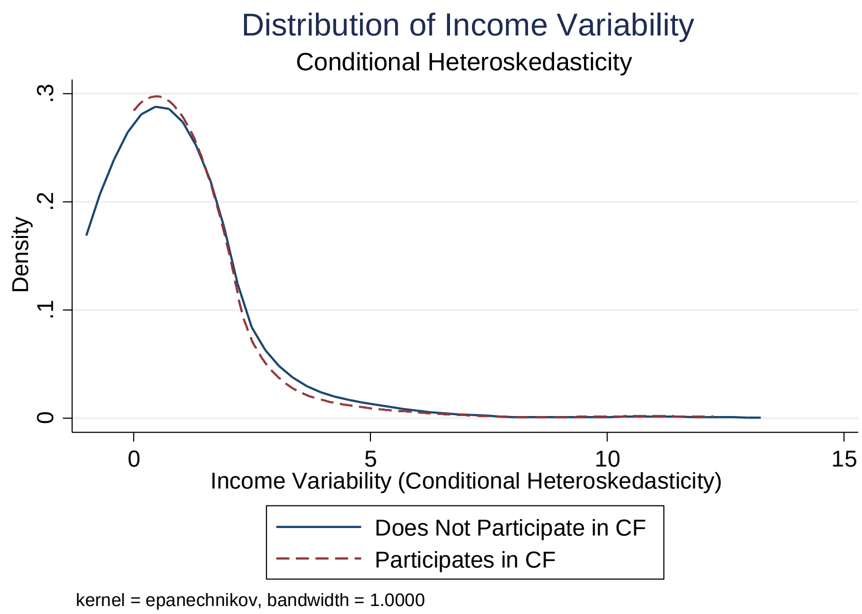

Figure 1. Kernel Density Estimates of Income Variability – Conditional Heteroskedasticity.

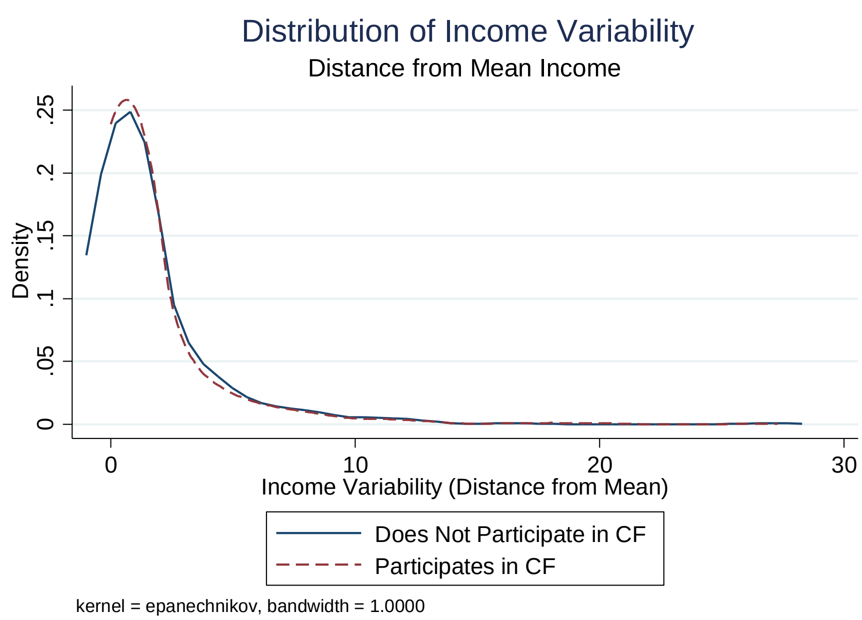

Figure 2. Kernel Density Estimates of Income Variability – Distance from Mean Income in the Data.

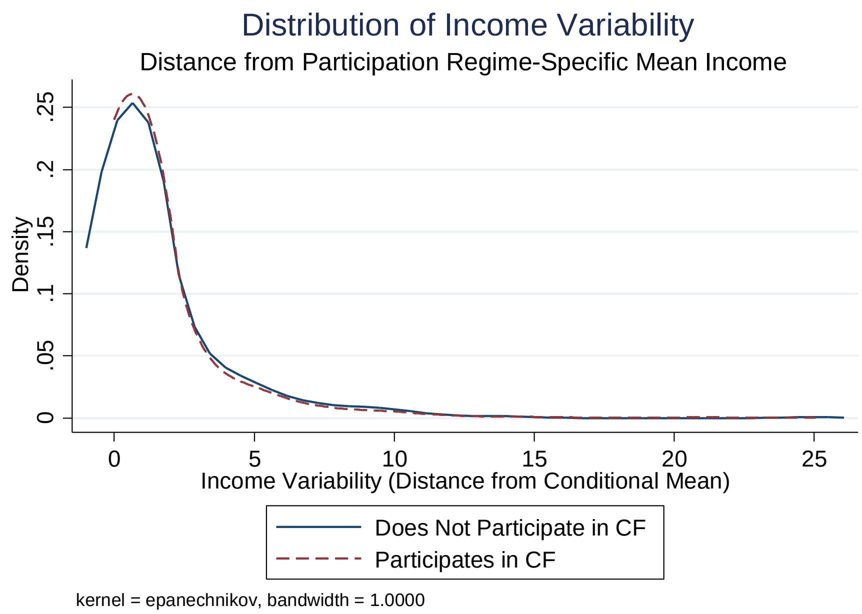

Figure 3. Kernel Density Estimates of Income Variability – Distance from Mean Income in Each Participation Regime.

Table 1. Descriptive Statistics and Balance Tests (n=1,078)

Variables	Contract Farming [a] No	Contract Farming [a] Yes	Test of Difference [b]
Income	14.843	24.255	***
(Ariary)	(1.198)	(2.762)	
Income Per Capita	3.072	4.463	***
(Ariary)	(0.239)	(0.413)	
Income Per Adult Equivalent	3.802	5.535	***
(Ariary)	(0.294)	(0.471)	
Household Size	5.452	5.692	**
(Individuals)	(0.108)	(0.104)	
Dependency Ratio	0.452	0.446	
(Dummy)	(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 (Years)	5.650	5.715	
(Years)	(0.154)	(0.147)	
Household Head Experience (Years)	21.074	20.165	
(Years)	(0.653)	(0.566)	
Household Head Member of a Farm Organization (Dummy)	0.149	0.296	***
	(0.017)	(0.022)	
Household Head Fady Days	23.968	20.427	*
(Days)	(1.684)	(1.424)	
Household Working Capital	2.872	6.021	***
(Ariary)	(0.380)	(0.973)	
Household Assets	11.672	16.277	***
(Ariary)	(1.099)	(1.359)	
Household Landholdings	113.438	177.956	***
(Ares)	(8.982)	(18.146)	
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
[a] District dummies omitted for brevity. Conditional means are calculated using sampling weights.
[b] Tests of differences in conditional means do not use sampling weights.

Table 2. Ordinary Least Squares Estimation Results for an Ancillary Income Regression

Variables	Coefficient (Std. Err.)
Dependent Variable: Log of Income Per Adult Equivalent	
Contract Farming Participant	0.347***
	(0.052)
Household Size	-0.122***
	(0.016)
Dependency Ratio	0.145
	(0.146)
Household Head Single	-0.003
	(0.145)
Household Head Female	-0.374**
	(0.176)
Household Head Migrant	0.100
	(0.104)
Household Head Age	0.014*
	(0.007)
Household Head Education	0.068***
	(0.009)
Household Head Agricultural Experience	-0.012
	(0.007)
Household Head Member of a Farm Organization	0.111
	(0.073)
Household Head Fady Days	-0.002*
	(0.001)
Household Working Capital	0.010***
	(0.003)
Household Assets	0.009***
	(0.002)
Household Landholdings	0.000
	(0.000)
Yes to $12.50 Investment (Imputed)	-0.002
	(0.154)
Yes to $25.00 Investment (Imputed)	0.122
	(0.136)
Yes to $37.50 Investment (Imputed)	0.073
	(0.146)
Yes to $50.00 Investment (Imputed)	-0.116
	(0.127)
Yes to $62.50 Investment (Imputed)	0.549*
	(0.316)
Yes to $75.00 Investment (Imputed)	-0.121

	(0.175)
Constant	-0.438
	(0.578)
Observations	1,178
District Fixed Effects	Yes
R-squared	0.516

Bootstrapped standard errors in parentheses

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

Table 3. OLS Estimation Results for Income Variability Regressions

Variables	(1) Conditional Heterosked.	(2) Distance from Mean	(3) Distance from Conditional Mean
Dependent Variable: Standardized Measure of Income Variability			
Contract Farming Participant	-0.209***	-0.169***	-0.170***
	(0.055)	(0.050)	(0.050)
Household Size	-0.019	-0.021	-0.029*
	(0.017)	(0.017)	(0.016)
Dependency Ratio	-0.169	-0.255	-0.212
	(0.174)	(0.155)	(0.158)
Household Head Single	-0.018	-0.109	-0.118
	(0.176)	(0.138)	(0.132)
Household Head Female	0.119	0.239	0.238
	(0.240)	(0.190)	(0.186)
Household Head Migrant	0.057	0.058	0.001
	(0.124)	(0.116)	(0.119)
Household Head Age	-0.006	-0.016	-0.012
	(0.011)	(0.011)	(0.011)
Household Head Education	0.010	0.007	0.008
	(0.010)	(0.010)	(0.011)
Household Head Agricultural Experience	0.010	0.014	0.011
	(0.010)	(0.010)	(0.010)
Household Head Member of a Farm Organization	-0.039	0.045	-0.008
	(0.078)	(0.071)	(0.070)
Household Head Fady Days	0.001	0.002	0.001
	(0.002)	(0.002)	(0.002)
Household Working Capital	-0.004	0.002	0.003
	(0.004)	(0.005)	(0.005)
Household Assets	0.001	0.000	0.001
	(0.002)	(0.002)	(0.002)
Household Landholdings	0.001*	0.001**	0.001**
	(0.000)	(0.000)	(0.000)
Yes to $12.50 Investment (Imputed)	-0.237	-0.185	-0.137
	(0.164)	(0.160)	(0.165)
Yes to $25.00 Investment (Imputed)	0.127	-0.030	0.017
	(0.131)	(0.113)	(0.105)
Yes to $37.50 Investment (Imputed)	0.087	-0.017	0.004
	(0.127)	(0.150)	(0.139)
Yes to $50.00 Investment (Imputed)	0.038	-0.081	-0.045
	(0.121)	(0.138)	(0.154)
Yes to $62.50 Investment (Imputed)	-0.365	-0.716	-0.517
	(0.525)	(0.530)	(0.511)

Yes to \$75.00 Investment (Imputed)	0.354	0.186	0.246
	(0.246)	(0.238)	(0.254)
Constant	0.087	1.128	0.677
	(0.888)	(0.901)	(0.876)
Observations	1,178	1,178	1,178
R-squared	0.082	0.220	0.221

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

Table 4. OLS Estimation Results for Income Variability Regressions Exploring Treatment Heterogeneity

Variables	(1) Conditional Heterosked.	(2) Distance from Mean	(3) Distance from Conditional Mean
Dependent Variable: Standardized Measure of Income Variability			
Contract Farming Participant	-0.141	-0.038	0.143
	(0.355)	(0.341)	(0.343)
Contract Farming Participant x Household Size	-0.009	-0.116***	-0.078***
	(0.029)	(0.030)	(0.029)
Contract Farming Participant x Dependency Ratio	0.280	0.415*	0.407*
	(0.277)	(0.228)	(0.233)
Contract Farming Participant x Household Head Single	-0.136	-0.458	-0.449
	(0.393)	(0.295)	(0.282)
Contract Farming Participant x Household Head Female	0.386	0.331	0.467
	(0.535)	(0.385)	(0.371)
Contract Farming Participant x Household Head Migrant	-0.092	0.163	0.064
	(0.198)	(0.165)	(0.161)
Contract Farming Participant x Household Head Age	-0.002	0.000	-0.003
	(0.007)	(0.007)	(0.007)
Contract Farming Participant x Household Head Education	-0.013	0.027	-0.002
	(0.021)	(0.021)	(0.021)
Contract Farming Participant x Household Head Agricultural Experience	-0.005	0.001	0.004
	(0.007)	(0.006)	(0.007)
Contract Farming Participant x Household Head Member of a Farm Organization	0.052	0.187	0.156
	(0.144)	(0.124)	(0.122)
Contract Farming Participant x Household Head Fady Days	0.002	0.002	0.000
	(0.002)	(0.002)	(0.002)
Contract Farming Participant x Household Working Capital	-0.004	-0.009	-0.014**
	(0.005)	(0.006)	(0.006)
Contract Farming Participant x Household Assets	0.001	0.002	-0.001
	(0.004)	(0.003)	(0.003)
Contract Farming Participant x Household Landholdings	0.001	0.001	0.000
	(0.000)	(0.000)	(0.000)
Household Size	-0.013	0.034	0.009
	(0.021)	(0.022)	(0.021)
Dependency Ratio	-0.313	-0.433**	-0.387**
	(0.209)	(0.169)	(0.172)
Household Head Single	0.021	0.088	0.096
	(0.174)	(0.137)	(0.134)
Household Head Female	0.016	0.125	0.041
	(0.206)	(0.178)	(0.175)
Household Head Migrant	0.083	-0.048	-0.031
	(0.156)	(0.125)	(0.132)

Household Head Age	-0.005	-0.013	-0.009
	(0.013)	(0.013)	(0.013)
Household Head Education	0.016	-0.006	0.006
	(0.015)	(0.016)	(0.016)
Household Head Agricultural Experience	0.012	0.011	0.008
	(0.011)	(0.010)	(0.010)
Household Head Member of a Farm Organization	-0.079	-0.095	-0.107
	(0.116)	(0.093)	(0.091)
Household Head Fady Days	0.000	0.001	0.001
	(0.002)	(0.002)	(0.002)
Household Working Capital	-0.000	0.012*	0.016**
	(0.006)	(0.006)	(0.007)
Household Assets	0.001	0.000	0.002
	(0.002)	(0.002)	(0.002)
Household Landholdings	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Yes to $12.50 Investment (Imputed)	-0.223	-0.227	-0.197
	(0.176)	(0.174)	(0.178)
Yes to $25.00 Investment (Imputed)	0.123	-0.063	-0.024
	(0.132)	(0.112)	(0.107)
Yes to $37.50 Investment (Imputed)	0.110	0.009	0.030
	(0.124)	(0.140)	(0.136)
Yes to $50.00 Investment (Imputed)	0.034	-0.050	-0.041
	(0.120)	(0.140)	(0.152)
Yes to $62.50 Investment (Imputed)	-0.326	-0.627	-0.515
	(0.548)	(0.528)	(0.516)
Yes to $75.00 Investment (Imputed)	0.359	0.202	0.260
	(0.238)	(0.230)	(0.250)
Constant	-0.019	0.891	0.522
	(0.982)	(0.957)	(0.944)
Observations	1,178	1,178	1,178
District Fixed Effects	Yes	Yes	Yes
R-squared	0.094	0.249	0.240

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

Table 5. OLS Estimation Results for Income Variability Regressions Exploring the Fixed Price Contract Mechanism

Variables	(1) Conditional Heterosked.	(2) Distance from Mean	(3) Distance from Conditional Mean
Dependent Variable: Standardized Measure of Income Variability			
Proportion of Plots Under Fixed Price Contract	-0.241***	-0.179***	-0.218***
	(0.050)	(0.044)	(0.043)
Household Size	-0.019	-0.021	-0.029*
	(0.017)	(0.018)	(0.018)
Dependency Ratio	-0.172	-0.258*	-0.212
	(0.166)	(0.146)	(0.149)
Household Head Single	-0.019	-0.109	-0.120
	(0.185)	(0.141)	(0.140)
Household Head Female	0.120	0.241	0.237
	(0.253)	(0.192)	(0.189)
Household Head Migrant	0.056	0.058	-0.001
	(0.125)	(0.111)	(0.115)
Household Head Age	-0.007	-0.017	-0.013
	(0.012)	(0.012)	(0.012)
Household Head Education	0.011	0.008	0.008
	(0.010)	(0.011)	(0.011)
Household Head Agricultural Experience	0.010	0.014	0.012
	(0.011)	(0.010)	(0.010)
Household Head Member of a Farm Organization	-0.027	0.051	0.006
	(0.077)	(0.068)	(0.067)
Household Head Fady Days	0.001	0.002	0.002
	(0.002)	(0.002)	(0.002)
Household Working Capital	-0.004	0.002	0.003
	(0.004)	(0.005)	(0.005)
Household Assets	0.001	0.000	0.001
	(0.002)	(0.002)	(0.002)
Household Landholdings	0.001*	0.001**	0.001**
	(0.000)	(0.000)	(0.000)
Yes to $12.50 Investment (Imputed)	-0.233	-0.182	-0.135
	(0.173)	(0.173)	(0.177)
Yes to $25.00 Investment (Imputed)	0.132	-0.030	0.025
	(0.134)	(0.116)	(0.109)
Yes to $37.50 Investment (Imputed)	0.087	-0.020	0.008
	(0.125)	(0.143)	(0.135)
Yes to $50.00 Investment (Imputed)	0.045	-0.080	-0.034
	(0.119)	(0.134)	(0.146)
Yes to $62.50 Investment (Imputed)	-0.390	-0.736	-0.536

	(0.552)	(0.539)	(0.526)
Yes to $75.00 Investment (Imputed)	0.343	0.175	0.239
	(0.241)	(0.230)	(0.248)
Constant	0.124	1.160	0.706
	(0.926)	(0.914)	(0.897)
Observations	1,178	1,178	1,178
District Fixed Effects	Yes	Yes	Yes
R-squared	0.084	0.220	0.225

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

Table 6. Propensity Score Matching I: Probit Estimation Results for Participation in Contract Farming

Variables	Coefficient (Std. Err.)
Dependent Variables: = 1 if Household Participates in Contract Farming; = 0 Otherwise.	
Household Size	-0.024
	(0.022)
Dependency Ratio	0.347
	(0.243)
Household Head Single	-0.089
	(0.205)
Household Head Female	-0.258
	(0.254)
Household Head Migrant	-0.124
	(0.148)
Household Head Age	-0.005
	(0.010)
Household Head Education	-0.010
	(0.013)
Household Head Agricultural Experience	0.006
	(0.009)
Household Head Member of a Farm Organization	0.510***
	(0.113)
Household Head Fady Days	-0.005**
	(0.002)
Household Working Capital	0.012***
	(0.004)
Household Assets	-0.000
	(0.003)
Household Landholdings	0.001**
	(0.000)
Yes to $12.50 Investment (Imputed)	-0.201
	(0.235)
Yes to $25.00 Investment (Imputed)	0.699***
	(0.226)
Yes to $37.50 Investment (Imputed)	0.632***
	(0.231)
Yes to $50.00 Investment (Imputed)	0.618***
	(0.204)
Yes to $62.50 Investment (Imputed)	0.420
	(0.362)
Yes to $75.00 Investment (Imputed)	0.462*
	(0.268)

Constant	-2.157***
	(0.796)
Observations	1,178

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

Table 7. Propensity Score Matching II: Treatment Effects

Sample	1 Neighbor Caliper 0.01	3 Neighbors Caliper 0.01	3 Neighbors Caliper 0.001
Conditional Heteroskedasticity			
Unmatched Sample	-0.168***	-0.168***	-0.168***
	(0.058)	(0.058)	(0.058)
Average Treatment Effect on the Treated	-0.160	-0.197**	-0.182***
	(0.108)	(0.087)	(0.069)
Average Treatment Effect	-0.176**	-0.191***	-0.180***
	(0.072)	(0.065)	(0.066)
Average Treatment Effect on the Untreated	-0.193***	-0.185***	-0.179**
	(0.063)	(0.058)	(0.070)
Distance from Mean			
Unmatched Sample	-0.087	-0.087	-0.087
	(0.058)	(0.058)	(0.058)
Average Treatment Effect on the Treated	-0.135	-0.151*	-0.109*
	(0.100)	(0.079)	(0.065)
Average Treatment Effect	-0.165**	-0.160***	-0.155**
	(0.070)	(0.061)	(0.062)
Average Treatment Effect on the Untreated	-0.194***	-0.169***	-0.199***
	(0.065)	(0.057)	(0.067)
Distance from Conditional Mean			
Unmatched Sample	-0.092	-0.092	-0.092
	(0.058)	(0.058)	(0.058)
Average Treatment Effect on the Treated	-0.130	-0.153*	-0.089
	(0.099)	(0.080)	(0.065)
Average Treatment Effect	-0.161**	-0.162***	-0.133**
	(0.069)	(0.061)	(0.062)
Average Treatment Effect on the Untreated	-0.190***	-0.170***	-0.176***
	(0.063)	(0.055)	(0.066)

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

Appendix

Table A1. Estimation Results for Median Regressions

Variables	**(1) Conditional Heterosked.**	**(2) Distance from Mean**	**(3) Distance from Conditional Mean**
Dependent Variable: Standardized Measure of Income Variability			
Contract Farming Participant	-0.086***	-0.065*	-0.056
	(0.033)	(0.037)	(0.039)
Household Size	0.013	-0.007	-0.003
	(0.009)	(0.010)	(0.011)
Dependency Ratio	0.046	-0.023	-0.058
	(0.094)	(0.106)	(0.112)
Household Head Single	0.154*	-0.006	0.070
	(0.080)	(0.091)	(0.096)
Household Head Female	-0.113	0.050	-0.013
	(0.095)	(0.107)	(0.113)
Household Head Migrant	0.042	0.032	-0.009
	(0.060)	(0.067)	(0.071)
Household Head Age	0.001	0.001	0.001
	(0.004)	(0.004)	(0.005)
Household Head Education	0.008	0.004	0.002
	(0.005)	(0.006)	(0.007)
Household Head Agricultural Experience	-0.000	0.000	-0.001
	(0.004)	(0.004)	(0.004)
Household Head Member of a Farm Organization	-0.026	0.038	0.005
	(0.044)	(0.050)	(0.053)
Household Head Fady Days	-0.000	-0.000	-0.000
	(0.001)	(0.001)	(0.001)
Household Working Capital	0.000	0.010***	0.008***
	(0.001)	(0.002)	(0.002)
Household Assets	0.000	0.002*	0.002*
	(0.001)	(0.001)	(0.001)
Household Landholdings	0.000**	0.001***	0.001***
	(0.000)	(0.000)	(0.000)
Yes to $12.50 Investment (Imputed)	0.045	-0.015	-0.022
	(0.095)	(0.107)	(0.114)
Yes to $25.00 Investment (Imputed)	-0.051	0.033	0.036
	(0.089)	(0.101)	(0.107)
Yes to $37.50 Investment (Imputed)	-0.003	-0.049	-0.011
	(0.090)	(0.102)	(0.108)
Yes to $50.00 Investment (Imputed)	0.049	-0.029	-0.031
	(0.085)	(0.096)	(0.102)

Yes to $62.50 Investment (Imputed)	0.042	0.022	0.072
	(0.140)	(0.158)	(0.168)
Yes to $75.00 Investment (Imputed)	-0.013	0.091	0.069
	(0.103)	(0.116)	(0.123)
Constant	-0.564*	-0.469	-0.520
	(0.296)	(0.335)	(0.355)
Observations	1,178	1,178	1,178
District Fixed Effects	Yes	Yes	Yes

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