Contract Farming as Partial Insurance*

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

January 4, 2018

*We are grateful to NIFA for funding this work through grant MIN-14-061—"Smallholder Participation in Agricultural Value Chains: Evidence from Madagascar." We thank seminar audiences at the University of Florida, the University of Georgia, and the University of Guelph as well as conference participants at the September 2016 FAO Rural Transformation, Agricultural, and Food System Transition conference, the 2017 Centre for the Study of African Economies conference, and the 2017 Canadian Agricultural Economics Society annual meetings. All remaining error are ours.

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

‡Assistant Professor, Department of Food, Agricultural & Resource Economics, University of Guelph, 50 Stone Road East, Guelph, ON N1G 2W1, Canada, ylee13@uoguelph.ca.

§Assistant Professor, Department of Economics, Colby College, 5237 Mayflower Hill, Waterville, ME 04901, United States, lindsey.novak@colby.edu.

CONTRACT FARMING AS PARTIAL INSURANCE

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 0.2-standard deviation decrease in income variability. Looking at the mechanism behind this finding, we find strong 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 estimate propensity score matching models, the results of which show that our core results are robust 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 findings 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

Multiple market failures are often at the root of economic underdevelopment and persistent poverty. In most developing countries, one market failure of particular importance is that of the insurance market. In such cases, information problems—adverse selection and moral hazard—are important enough that it is often simply not profitable to offer insurance against risks which, in developed countries, are commonly thought of as insurable.

Insurance market failures constrain welfare in two ways. First, they constrain current welfare in that they force individuals and households to sink valuable resources on self-insuring, however partially, against those risks.[1] Second, they constrain future welfare in that they prevent those same individuals and households from making investments—financial, in agricultural technology, in education, and so on—today which might allow them to attain higher levels of welfare tomorrow.

Contract farming, the vertical coordination mechanism wherein a processor contracts out the production of an agricultural commodity to a grower (Bijman, 2008), can in theory serve as a partial insurance mechanism for rural households in developing countries. Following Grosh (1994), contract farming can help resolve insurance market failures by insuring growers against price risk in cases where the processor guarantees a fixed price as part of

[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 for its loss 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 for its loss in case of an adverse event.

the contract. This can lead to more stable incomes which, for risk-averse growers, means higher levels of welfare. More broadly, since Stiglitz's (1974) exploration of how sharecropping contracts can in theory partially insure tenants against output risk, economists have known that contracts can often help resolve market failures.

The question we pose in this paper is this: Does contract farming help empirically resolve insurance market failures? Specifically, we ask whether contract farming serves as a partial insurance mechanism for growers by reducing the income variability they face. We answer this question by using survey data on 1,200 households in rural Madagascar, half of which participate in contract farming as growers. Our data, which cover a dozen crops across six regions of Madagascar, have previously been used by Bellemare (2012) and Bellemare and Novak (2017) 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 income variability from the correlation between the two, we rely on a framed field experiment that elicited each respondent's willingness to pay (WTP) to participate in a hypothetical contract farming agreement,[2] which we argue obviates statistical endogeneity issues due to grower selection into contract farming. As in Bellemare and Novak (2017), because a respondent's WTP to participate in contract farming captures anything which moves his marginal utility from participating in contract farming

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

around (e.g., ambiguity and risk preferences; expected returns; time preferences; entrepreneurial ability; managerial ability; technical ability; aspirations; and so on), we argue that controlling for WTP takes those typically unobserved factors out of the error term and helps account for selection. We thus use the WTP data in a selection-on-observables design. To assess the robustness of our regression results, we also estimate propensity score matching models, given that our selection-on-observables design relies on an assumption similar to the conditional independence assumption made when estimating propensity score matching models.

We find that participation in contract farming is associated with a decrease of about 0.2-standard deviations in our proxy measures of the average household's income variability. 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. Moreover, we find that in most cases, 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. One explanation for this finding is that 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 wish to participate in contract farming on the basis of that comparison, it is much more difficult for them to perceive differences in income variability and make a participation decision 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 *level* of income of participating households (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., 2016; Wainaina et al., 2014; Wang et al., 2014; Briones, 2015; and Maertens and Vande Velde, 2017) or some variant thereof (Raynolds, 2002; Simmons et al., 2005; Begum, 2006; Minten et al., 2009; Bolwig et al., 2009; Narayanan, 2014; and Trifković, 2016). 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, 2012), happiness (Dedehouanou et al., 2013), and food security (Bellemare and Novak, 2017). Minten et al. (2009) do look at income variability, but they lack a proper comparison group and must rely instead on an external source of data for comparison. Likewise, Michelson et al. (2008) find that, relative to growers contracting with the domestic retail chain, Walmart growers in Nicaragua experience lower levels of price volatility, but their results focus only on those two processors and are thus limited in their external validity.[3]

Our contribution is thus threefold. First, 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.[4] Second, we contribute to the agricultural and development economics literatures by providing evidence that participation in agricultural value chains (Du et al., 2016; Lu et al., 2016; and Zilberman et al., 2017) via the institution of contract farming can serve as a partial insurance mechanism for rural households in developing countries,[5] and we do so with a considerable amount of external validity given that our data cover six regions of Madagascar, over a dozen different crops, a number of different processors. Lastly, we contribute to the development policy literature by showing that the impacts

[3]Deb and Suri (2013) show how downstream changes in the value chain (i.e., a change in the mode of shipping in the commodity under contract) can change the contract itself both theoretically and empirically (i.e., the principal provides both in-kind and cash loans in response to the change in mode of shipping) using a data set on pineapple contract farming in Ghana.

[4]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 Bellemare, Lee, and Just (2017).

[5]Contract farming is a step toward vertical integration and away from spot markets. In recent work, Görg and Kersting (2017) have explored how vertical integration allows suppliers (here, growers) to access financial capital. By looking at partial insurance, we look at another aspect of the financial services suppliers can access by participating in value chains.

of participation in contract farming on income variability, though they are on average negative and significant, would be 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 present our empirical results. Section 6 concludes with policy implications and with some directions for future research.

2 Theoretical Framework

We consider the maximization problem of an individual grower of an agricultural commodity who is considering whether and how much to produce under contract for a processor. As such, we are not concerned with the processor's decision of whether or not to contract the production of the agricultural commodity to growers or to produce it in-house. Rather, we take as given the processor's decision to contract out the production of the agricultural commodity. See Nowak et al. (2016) for how low-cost, low-sophistication inputs such as agricultural commodities should in theory get outsourced.

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 in profit, i.e., $U_\pi > 0$ and $U_{\pi\pi} < 0$.[6] Let 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

$$\pi = \{(1 - \alpha)p + \alpha\bar{p}\}q - TC(q, \alpha), \tag{1}$$

where $TC(q, \alpha)$ denotes the total cost of producing output q, which varies according to α as well due to the costs associated with contract farming, such as transaction and compliance costs. The function $TC(q, \alpha)$ is twice continuously differentiable and strictly increasing in q, i.e., $TC_q > 0$.

Here, we assume that the total cost function $TC(q, \alpha)$ is nonseparable in terms of q and α for the sake of simplicity. The results presented in this section do not change when we express the total cost as the sum of fixed and variable costs, or when we express fixed, variable, or total costs as functions of q, α, or both.

[6] For any function $f(\cdot)$, we let f_k and f_{kk} denote the first and the second derivatives of $f(\cdot)$ with respect to k, respectively.

Because the market price p is a random variable, the producer's expected profit is such that

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

where $E(\cdot)$ denotes an expectation and $F(p)$ denotes the cumulative distribution function of p. Similarly, the variance of the producer's profit is such that

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

$E(\pi)$ can be rewritten as

$$E(\pi) = \{(1-\alpha)E(p) + \alpha\bar{p}\}q - TC(q,\alpha), \tag{4}$$

which means that

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

The foregoing leads to the following proposition, which is our core testable hypothesis.

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

α, the lower the variance of the producer's profit.

Proof. First, let us compare the cases of not participating (i.e., $\alpha = 0$) and participating in contract farming ($0 < \alpha \leq 1$).

If $\alpha = 0$,

$$E(\pi|\alpha = 0) = E(p)q - TC(q, 0), \text{ and} \tag{6}$$

$$Var(\pi|\alpha = 0) = \int_0^\infty \left[\{p - E(p)\}q\right]^2 dF(p). \tag{7}$$

If $0 < \alpha \leq 1$,

$$E(\pi|0 < \alpha \leq 1) = \{(1-\alpha)E(p) + \alpha\bar{p}\}q - TC(q, \alpha), \text{ and} \tag{8}$$

$$Var(\pi|0 < \alpha \leq 1) = (1-\alpha)^2 \int_0^\infty \left[\{p - E(p)\}q\right]^2 dF(p). \tag{9}$$

Therefore, $Var(\pi|\alpha = 0) > Var(\pi|0 < \alpha \leq 1)$.

Next, given participation in contract farming, the change in $Var(\pi)$ according to α is such that

$$\frac{\partial Var(\pi)}{\partial \alpha} = -\int_0^\infty 2(1-\alpha)\left[p - E(p)q\right]^2 dF(p) \leq 0. \tag{10}$$

The last inequality holds due to $0 < \alpha \leq 1$ given participation in contract farming. ■

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

$$\max_{\alpha,q} EU(\pi) = \max_{\alpha,q} \int_0^\infty U\Big(\{(1-\alpha)p + \alpha\bar{p}\}q - TC(q,\alpha)\Big)dF(p), \quad (11)$$

which leads to the following lemma.

Lemma 2 *If contract farming guarantees a price equal to the expected market price and the marginal cost of participating in contract farming is zero, a risk-averse producer will benefit from full coverage for a given level of production. That is, if $\bar{p} = E(p)$ and $TC_\alpha(q,\alpha) = 0$ at any $0 \leq \alpha \leq 1$, the producer's choice of α will be equal to* 1.

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

$$EU[\pi|\alpha = 1] - EU[\pi|0 \leq \alpha < 1] > 0. \quad (12)$$

Note that the left-hand-side of expression 12 is equal to

$$= EU[E(p)q - TC(q,1)] - EU[\{(1-\alpha)p + \alpha\bar{p}\}q - TC(q,\alpha)] \quad (13)$$

$$= EU[E(p)q - TC(q,\alpha)] - EU[\{(1-\alpha)p + \alpha\bar{p}\}q - TC(q,\alpha)] \quad (14)$$

$$= EU[E(\pi)] - EU[\pi] \tag{15}$$

$$= U[E(\pi)] - EU[\pi] > 0. \tag{16}$$

Expression 14 follows from $TC_\alpha(q,\alpha) = 0$. The last expression follows from assuming that $U_{\pi\pi} < 0$ and by Jensen's inequality. ■

Relaxing the assumption that the marginal cost of contract farming is zero, which would be the case if there are additional costs associated with adhering to the demands of the processor, leads to the following proposition.

Proposition 3 *If the marginal cost of contract farming is nonnegative (i.e., $TC_\alpha(q,\alpha) \geq 0$), then the optimal level of coverage α^* is determined such that the marginal benefit and marginal cost from participating in contract farming are equalized.*

Proof. By the F.O.C. with respect to α, it must be that

$$\frac{\partial EU(\pi)}{\partial \alpha} = \frac{\partial EU(\pi)}{\partial \pi} \cdot \frac{\partial \pi}{\partial \alpha} + \frac{\partial EU(\pi)}{\partial q} \cdot \frac{\partial q}{\partial \alpha} = 0, \tag{17}$$

where $\frac{\partial EU(\pi)}{\partial q} = 0$ at the optimal level of production. Therefore, it must be that

$$\frac{\partial EU(\pi)}{\partial \pi} \cdot \frac{\partial \pi}{\partial \alpha} = \int_0^\infty U_\pi(\pi)\big[(\bar{p} - p)q - TC_\alpha(q,\alpha^*)\big]dF(p) = 0, \tag{18}$$

which means that, at the optimum,

$$(\bar{p} - p)q = TC_\alpha(q, \alpha^*) \tag{19}$$

given that $U_\pi > 0$. ■

In other words, in order for a producer to be better off choosing to participate in contract farming, the contract must pay a fixed price that is high enough to cover any cost borne by the producer because of his participation in the contract. That is, the difference between the fixed price guaranteed by contract farming and the market price $(\bar{p} - p)$ is the premium paid by the processor to secure the level of production q.

Though we do not formally test Proposition 3, we derive it here in order to explain why producers who choose to participate in contract farming do not go "all in" by choosing to cultivate the entirety of their plots under contract farming.

3 Empirical Framework

We now discuss the empirical framework we use to study the impact of participation in contract farming on income variability. We begin this section by discussing how we build our proxy measures of income variability—that is, the dependent variables for 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

The first difficulty in answering the research question we pose is that we rely on cross-sectional data. Ideally, one would have longitudinal data at one's disposal to measure the variability of a household's income over time. That way, one could obtain for each household a measure of central tendency (e.g., the within-household 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.

Given that we rely on proxy measures of income variability, we use three such proxy measures to ensure that our results are robust: (i) conditional heteroskedasticity (CH), (ii) distance from sample mean squared (DSM), and (iii) distance from conditional mean squared (DCM). The remainder of this sub-section gives precise definitions of those measures.

3.1.1 Conditional Heteroskedasticity

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

$$\ln y_i = \alpha + \underline{\beta} \underline{x}_i + \gamma D_i + \epsilon_i, \tag{20}$$

where $\ln y_i$ denotes the logarithm of household i's income y_i, $\underline{x}_i$ is a vector of household-specific control variables,[7], D is an indicator variable for whether the household participates in contract farming, and ϵ is an error term with mean zero. Our conditional heteroskedasticity (CH) measure is such that, for each household i, we compute

$$CH_i = \widehat{\epsilon}_i^2, \tag{21}$$

where $\widehat{\epsilon}_i$ denotes the residual for household i, whose square we use 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 heteroskedasticity conditional on contract farming (non)participation whose goal is to establish

[7] Throughout this paper, underlines denote vectors.

whether the variance of the residual is the same for those households that participate in contract farming and those households that do not.

Second, we use CH_i as our dependent variable in a regression of CH_i 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

$$DSM_i = (y_i - \overline{y})^2 . \tag{22}$$

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

$$DCM_i = \left\{ [y_i - \overline{y}(D_i = 1)]^{I(D_i=1)} \cdot [y_i - \overline{y}(D_i = 0)]^{I(D_i=0)} \right\}^2 , \tag{23}$$

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. Still, the reader should not lose sight of the fact that this is a limitation of our approach.

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 (2017).

3.2.1 Regression

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

$$\sigma_i^2 = \alpha_1 + \underline{\beta}_1 \underline{x}_i + \gamma_1 D_i + \upsilon_i, \tag{24}$$

where σ_i^2 is a standardized[8] version of any of CH, DSM, and DCM for household i, $\underline{x}_i$ 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, γ would provide an estimate of the average treatment effect (ATE) of participating in contract farming on the proxy for income variability on the left-hand side of equation 24. Participation in contract farming, however, is not randomly assigned, and so we estimate the following version of equation 24:

$$\sigma_i^2 = \alpha_2 + \underline{\beta}_2 \underline{x}_i + \gamma_2 D_i + \underline{\delta}_2 \underline{r}_i + \eta_i, \tag{25}$$

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) to control for selection in contract farming and thus in an attempt 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, note that the research design we rely on in this paper is

[8]We standardize all of our dependent variables (i.e., we subtract the mean from each observation and divide this demeaned value by the standard deviation) for comparability across all three of our proxy measures of income variability as well as for ease of interpretation.

a selection-on-observables (SOO) design.

3.2.2 Propensity Score Matching

The assumption that allows us to use an SOO design also allows us to make 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. In this context, the use of PSM methods has two distinct advantages. 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 but which are not estimable from the regression in equation 25.

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

$$D_i = \kappa + \underline{\lambda x}_i + \underline{\theta r}_i + \xi_i, \tag{26}$$

where the variables denote the same things as in equation 25. The estimated coefficients in equation 26 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 26.

Second, we match households that participate in contract farming with households that do not on the basis of their propensity scores. To do so, we match with replacement and use the three nearest neighbors with a caliper size of 0.01 standard deviation.[9] For each of our proxies for income variability, 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 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 addresses each one.

3.3.1 Experimental Setup

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.

[9] In preliminary work, we also considered two other specifications: (i) one nearest neighbor with a caliper size of 0.01 standard deviation, and (ii) three nearest neighbors with a caliper size of 0.001 standard deviations. All three specifications gave qualitatively similar results, and so we only report one for brevity.

Each respondent's bid r_i was selected from the set $r_i \in \{\$12.5, \$25, \$37.5, \$50, \$62.5, \$75\}$ with equal probability (i.e., with the throw of a fair die).[10] 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. 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 (2017) and impute what that respondent's answer would be for those other bids. In other words, among the six bid levels in the set $\{\$12.5, \$25, \$37.5, \$50, \$62.5, \$75\}$, 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

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

other bid levels. Specifically, we conduct imputations as follows: if a respondent was presented with bid r_{ij}, where j denotes any of the six possible bid levels, we linearly regress each unasked bid $r_{i,-j}$ on $\underline{x}$ and predict $\widehat{r}_{i,-j}$. The end result is a vector $\underline{r}_i = (r_{i,j}, \widehat{r}_{i,-j})$ 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.[11]

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

[11]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.

3.3.2 Identification

The identifying assumption in this paper is that the vector $\underline{r}_i = (r_{i,j}, \widehat{r}_{i,-j})$, 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 controlling for selection into contract farming by purging the error term in equation of much of its correlation with the treatment variable in equation 25. 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 based on 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 follows that in Bellemare (2012) and Bellemare and Novak (2017).

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 25), estimated coefficients are biased. Here, because the vector $\underline{r}_i$ captures a respondent's marginal utility of participating in contract farming, many of the typically unobservable characteristics whose correlation with D would bias our estimate of γ in equation 25 (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, except for their entrepreneurial ability. The respondent whose entrepreneurial ability is higher might prefer starting a business to participate 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 $\underline{r}_i$ 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 25, which considerably lessens the problem of unobserved heterogeneity.

Reverse Causality. This would arise in cases where the prospect of getting partial 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. These different expectations would affect their marginal utility of participating in contract farming differentially which, again, the vector $\underline{r}_i$ 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, were 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.[12]

Another way to think about our identification strategy is with the help of the simple Roy model laid out by Smith and Sweetman (2016). According to that model, a household i decides whether to participate in contract farming by comparing the difference between its expected income from participation Y_{1i} and its income from nonparticipation Y_{0i} with its cost of participating c_i,

[12] Another threat to identification would be spillovers from contract farming participants to nonparticipants. Here, this would take the form of household i's participation in contract farming affecting household j's income variability. Though this is in theory possible, we deem it improbable.

such that the household participates if and only if

$$Y_{1i} - Y_{0i} \geq c_i. \tag{27}$$

From equation 27, note that participation in contract farming is identical to our treatment variable D_i since $D_i = I(Y_{1i} - Y_{0i} \geq c_i)$. But in our contingent valuation experiment, we set the LHS of equation 27 de facto equal to $1.1Y_{0i} - Y_{0i} = 0.1Y_{0i}$ (i.e., a 10-percent increase in income), and we experimentally generate the part of c_i that is due to the initial investment necessary to participate in contract farming. Our claim is thus that our WTP variable controls for the part of c_i that is not due to that experimentally generated and hypothetical initial investment—things like ambiguity and risk preferences; entrepreneurial, managerial, and technical ability; time preferences; expectations; and so on.

Our identification strategy thus helps ruling out a number of sources of statistical endogeneity, but it is not perfect. The ideal research design would involve randomly assigning treatment households to participation in contract farming and control households to nonparticipation, but so far no such randomized controlled trial has been attempted, most likely due to the difficulty of ensuring compliance with assignment to treatment or control. As always with observational data, however, it is best to exercise caution, and so it is best to treat our estimates of γ as suggestive of causality rather than causal.

4 Data and Descriptive Statistics

Bellemare (2012) and Bellemare and Novak (2017) rely on the same data we use in this paper, and both those articles discuss the data in detail, so 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 (2017). Specifically, though we report descriptive statistics for the variables we use in our analysis, we will not expend any time discussing them, as both aforementioned articles do that.

The data were collected during the latter half of 2008 in 12 communes across six regions of Madagascar, with two communes sampled per region. The data cover 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 the World Bank) 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), this diversity of crops and geographical areas ensures that our findings have more external validity than those of most other studies of contract farming, which focus at most on a handful of crops or on a more restricted geographical area.

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 ($n = 579$). 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 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, to ensure the robustness of our findings, we look at three versions of our results: one that considers the variability of the income level of the household, one that considers the variability of income per capita within the household, and one that considers the variability of income per adult equivalent (AE) within the household.[13]

5 Estimation Results and Discussion

We now turn to our empirical results. To do so, we begin with nonparametric results that show 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. Because those nonparametric results do not control for observable confounding factors, much less unobservable ones, 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 to 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 20, whose squared residual we use 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

[13]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.

Novak (2017) by relying on an SOO design. The results in Table 2 confirm the analysis in Bellemare (2012), i.e., participation in contract farming is associated with higher levels of income, whether one considers the level of income of the household or income per capita or per adult equivalent within the household.

As discussed, we use the square of the residual from equation 20 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 1a,[14] and we plot plot kernel density estimates for DSM and DCM in Figures 1b and 1c. Though Figure 1 is concerned with income per capita within the household, Appendix Figures A1 and A2 plot the same kernel density estimates as in Figure 1, but for the income level of the household and for income per adult equivalent within the household.

Figure 1 and Appendix Figures A1 and A2 seem to suggest there is no systematic difference in income variability between the households that participate in contract farming and those that do not. The results in those figures, however, only look at unconditional correlations. We now turn to our parametric analyses to see whether we can disentangle a potential causal relationship from this apparent lack of correlation.

[14]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 by first demeaning them and then dividing by their standard deviation.

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, a t-test that CH is equal for participants and nonparticipants rejects the null at a significance level below 1 percent for the level of income of the household as well as for income per capita and per adult equivalent within the household in favor of the alternative hypothesis that income variability is higher in the sub-sample of households that do not participate in contract farming.

For the regression-based approach, estimation results for CH are shown in Table 3; estimation results for DSM are shown in Table 4; and estimation results for DCM are shown in Table 5. In all three of those tables, the first column of results shows results for the income level of the household, whereas the next two columns respectively show results for income per capita and income per adult equivalent within the household. The results in Tables 3 to 5 show that participation in contract farming is associated with a decrease in income variability of 0.169 (column 3 of Table 4) to 0.222 (column 1 of Table 3) standard deviations, and that this association is significant at less than the 1 percent level in every case. Moreover, the almost complete lack of significance of other RHS variables in Tables 3 to 5 makes our core result that participation in contract farming is associated with a decrease in income variability all the more convincing that there is indeed a relationship between the treatment and outcome variables. The only other variable whose coefficient is significant in Tables 3 to 5 is the household's landholdings, which are associated with an increase in income variability. This is presumably because greater amounts of landholdings means a greater exposure to agriculture and thus volatile commodity markets, ceteris paribus.

5.2.1 Robustness Checks

To ensure that our results in Tables 3 to 5 are robust, we re-estimated median regression versions of the specifications in those tables. Estimation results for those median regressions are shown in Appendix Tables A1 to A3. In almost every case, these results show that there is a negative and statistically significant relationship between participation in contract farming and income variability, although the estimated effects in those median regressions tend to be smaller in magnitude than those in the OLS regressions in Tables 3 to 5.

5.2.2 Mechanism

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 6, we begin testing 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 under a 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 household whose plots would entirely be under fixed price contracts would see its income variability be about 0.25 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 in this case provides 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. But we can go one step further in assessing whether fixed price contracts are a mechanism whereby participation in contract farming seems to provide partial insurance to grower households. In recent work, Acharya et al. (2016) develop a method that allows assessing whether a mediator variable (i.e., a variable that lies between the treatment and outcome variables on the causal path) is a mechanism whereby the treatment causes the outcome. In the limit, Acharya et al.'s method allows determining whether the mediator is the only mechanism, statistically speaking, whereby the treatment causes the outcome.

As in equation 25, let y be the outcome variable, and let D be the treatment variable. Moreover, let x^{Pre} denote control variables that are determined before the treatment is assigned, x^{Post} denote control variables that are determined after the treatment is assigned, and let M denote the presumed mechanism, or mediator variable; in our application, M is the proportion of a household's plots that are under a fixed price contract. Acharya et al.'s method then consists of the following steps:

1. Estimate $y = \alpha_3 + \beta_3^{\text{Pre}} x^{\text{Pre}} + \beta_3^{\text{Post}} x^{\text{Post}} + \gamma_3 D + \phi_3 M + \epsilon_3$.

2. Compute $\widetilde{y} = y - \widehat{\beta}_3^{\text{Post}} x^{\text{Post}} - \widehat{\phi}_3 M$.

3. Estimate $\widetilde{y} = \alpha_4 + \beta_4^{\text{Pre}} x^{\text{Pre}} + \gamma_4 D + \epsilon_4$.

4. The estimated parameter $\widehat{\gamma}_4$ is then the effect of the treatment once the mediator or mechanism M has been accounted for. If one fails to reject the null hypothesis $H_0 : \widehat{\gamma}_4 = 0$, one can then say that M is the only mechanism whereby the treatment D causes the outcome y.

In order to use Acharya et al.'s method, the only decision we need to make is to determine which of our control variables are pre- and post-treatment. In this case, we assume that all the variables in $\underline{x}$ and in $\underline{r}$ on the RHS of equation 25 are pre-treatment, so that step 2 above only involves subtracting the proportion of fixed rent contracts in the data and its estimate coefficient $\widehat{\phi}$ from the outcome variable.

We use Acharya et al.'s method a total of nine times: once for each proxy for income variability (i.e., CH, DSM, and DCM), and once for each of the level of income of the household as well as income per capita and per adult equivalent within the household. In no case do we reject the null hypothesis $H_0 : \widehat{\gamma}_4 = 0$. This constitutes strong evidence that fixed price contracts are not only a mechanism whereby participation in contract farming is associated with a decrease in income variability, but also evidence that it is the only mechanism whereby this association holds.

Moreover, 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, we can rule out the hypothesis that contract farming serves as partial insurance because income from contract farming is negatively related with income from other sources.

5.2.3 Propensity Score Matching Results

Turning to our PSM results, Table 7 presents estimation results for equation 26, i.e., a probit aimed at predicting propensity scores. Similar probit results can be found in Bellemare (2012) and Bellemare and Novak (2017). 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. That said, Appendix Figure A3 graphs histograms of propensity scores by participation regime. This common support graph shows that there is enough overlap in the propensity scores of participants and nonparticipants to yield reliable results.

Our interest here is in estimating the ATE as well as the ATT and the ATU of participating in contract farming. Table 8 summarizes our estimates of those depending on whether we look at (i) CH (upper panel), (ii) DSM (middle panel), or (iii) DCM (lower panel), and whether we look at (i) the income level of the household (first column of results), (ii) income per capita within the household (second column), or (iii) income per adult equivalent within the household (third and last colum). Appendix Table A4 shows balance statistics for our matched sample. In no case do the means of the variables retained for analysis differ significantly between the treatment and control groups.

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 0.20 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, because farmers who would benefit the most should choose to participate in contract farming, 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) in seven out of nine cases. In other words, it looks as though considering income variability only, 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.

In sum, it looks as though participation in contract farming can be an effective partial insurance mechanism for households in rural Madagascar, with estimated ATEs ranging from -0.13 standard deviations in the middle panel, first column of Table 8 to -0.22 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

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 our design relies on the same assumption which makes propensity score matching credible, 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 use an empirical method newly developed by Acharya et al. (2016) and find that, in line with our theoretical prediction, fixed price contracts are not only a mechanism whereby participation in contract farming seems to provide partial insurance, those same fixed price contracts appear to be the *only* mechanism whereby this happens.

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 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 important 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, 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.

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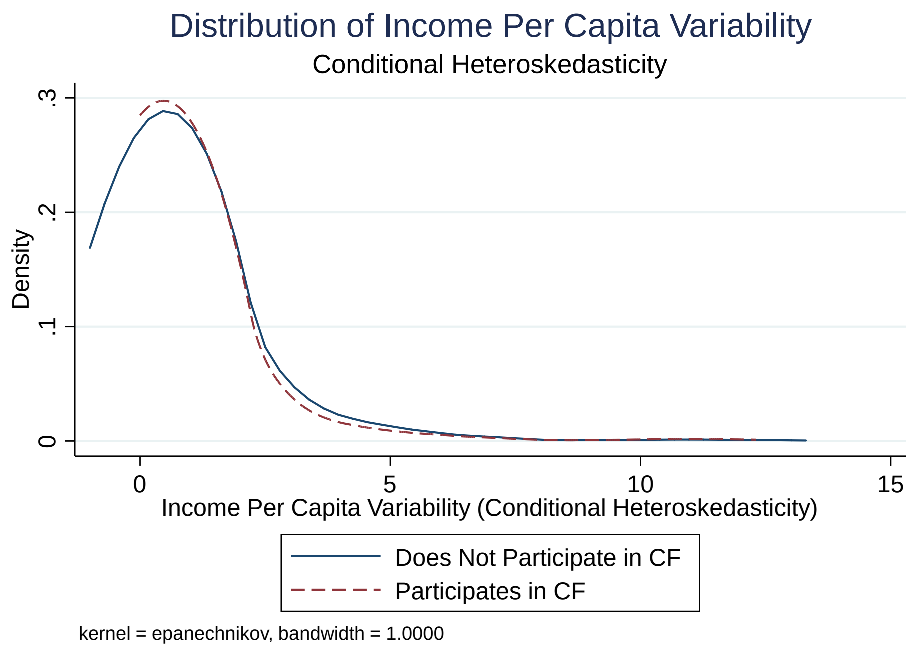

Figure 1a. Kernel Density Estimates of Income Per Capita Variability – Conditional Heteroskedasticity.

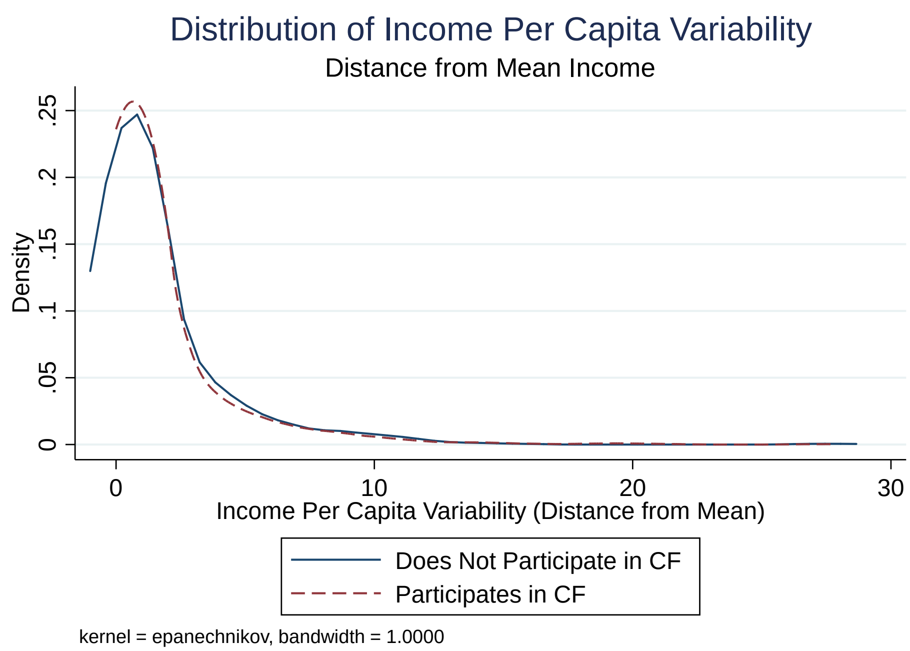

Figure 1b. Kernel Density Estimates of Income Per Capita Variability – Distance from Mean Income.

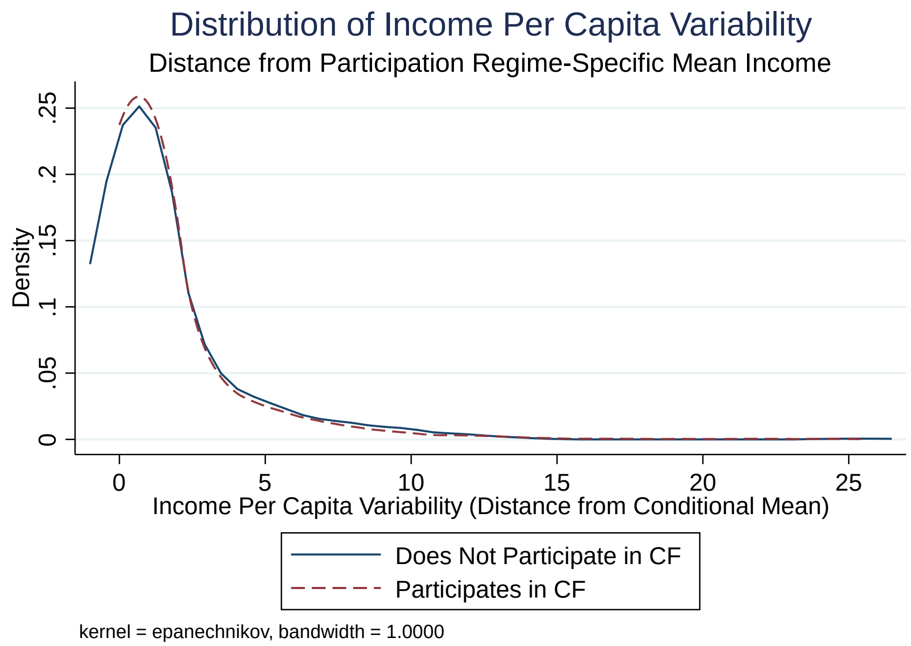

Figure 1c. Kernel Density Estimates of Income Per Capita Variability – Distance from Regime-Specific Mean Income.

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

Variables	Contract Farming [a] No	Contract Farming [a] Yes	Test of Difference [b]
Household Income	14.843	24.255	***
(Ariary)	(1.198)	(2.762)	
Household Income Per Capita	3.072	4.463	***
(Ariary)	(0.239)	(0.413)	
Household 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	
(Proportion)	(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	0.149	0.296	***
Organization (Dummy)	(0.017)	(0.022)	
Household Head Taboo Days[c]	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 calculated using sampling weights.

[b] Tests of differences in conditional means do not use sampling weights.

[c] The Malagasy observe a complex system of taboos (known as *fady* in the local language) and interdictions, one of which is the interdiction to do agricultural work on certain days of the week, which we use as a control variable in the empirical analysis in this paper. For the multiplicity of taboos observed by the Malagasy, see Ruud (1960).

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

Variables	(1) Income	(2) Income Per Capita	(3) Income Per AE
Dependent Variable: Log of Income			
Contract Farming	0.358***	0.345***	0.347***
	(0.053)	(0.052)	(0.052)
Household Size	0.046***	-0.127***	-0.122***
	(0.015)	(0.015)	(0.015)
Dependency Ratio	-0.201	-0.405***	0.145
	(0.153)	(0.153)	(0.152)
Single	-0.216	-0.001	-0.003
	(0.148)	(0.152)	(0.152)
Female	-0.241	-0.388**	-0.374**
	(0.179)	(0.182)	(0.182)
Migrant	0.083	0.096	0.100
	(0.105)	(0.106)	(0.106)
Age	0.015*	0.015*	0.014*
	(0.008)	(0.008)	(0.008)
Education	0.067***	0.068***	0.068***
	(0.009)	(0.009)	(0.009)
Agricultural Experience	-0.011	-0.011	-0.012*
	(0.007)	(0.007)	(0.007)
Member of a Farm Organization	0.117	0.107	0.111
	(0.072)	(0.072)	(0.071)
Taboo Days	-0.002	-0.002*	-0.002*
	(0.001)	(0.001)	(0.001)
Working Capital	0.009**	0.010***	0.010***
	(0.003)	(0.003)	(0.003)
Assets	0.009***	0.009***	0.009***
	(0.002)	(0.002)	(0.002)
Landholdings	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Yes to $12.50 Investment (Imputed)	0.043	-0.009	-0.002
	(0.159)	(0.160)	(0.160)
Yes to $25.00 Investment (Imputed)	0.147	0.117	0.122
	(0.132)	(0.130)	(0.131)
Yes to $37.50 Investment (Imputed)	0.051	0.073	0.073
	(0.144)	(0.140)	(0.140)
Yes to $50.00 Investment (Imputed)	-0.106	-0.117	-0.116
	(0.125)	(0.126)	(0.126)
Yes to $62.50 Investment (Imputed)	0.543*	0.540*	0.549*
	(0.315)	(0.322)	(0.320)
Yes to $75.00 Investment (Imputed)	-0.096	-0.118	-0.121
	(0.178)	(0.175)	(0.174)
Constant	0.073	-0.447	-0.438

	(0.599)	(0.604)	(0.603)
Observations	1,178	1,178	1,178
District Dummies	Yes	Yes	Yes
R-squared	0.538	0.535	0.516

Bootstrapped standard errors in parentheses

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

Table 3. OLS Estimation Results for Conditional Heteroskedasticity Regressions

Variables	(1) Income	(2) Income Per Capita	(3) Income Per AE
Dependent Variable: Conditional Heteroskedasticity (Standardized)			
Contract Farming	-0.222***	-0.206***	-0.209***
	(0.055)	(0.055)	(0.055)
Household Size	-0.024	-0.020	-0.019
	(0.017)	(0.017)	(0.017)
Dependency Ratio	-0.176	-0.152	-0.169
	(0.167)	(0.176)	(0.172)
Single	-0.066	-0.026	-0.018
	(0.162)	(0.175)	(0.176)
Female	0.160	0.125	0.119
	(0.222)	(0.241)	(0.242)
Migrant	0.042	0.055	0.057
	(0.124)	(0.123)	(0.123)
Age	-0.006	-0.007	-0.006
	(0.010)	(0.011)	(0.011)
Education	0.005	0.009	0.010
	(0.010)	(0.010)	(0.010)
Agricultural Experience	0.009	0.010	0.010
	(0.009)	(0.009)	(0.009)
Member of a Farm Organization	-0.022	-0.040	-0.039
	(0.080)	(0.077)	(0.078)
Taboo Days	0.000	0.001	0.001
	(0.002)	(0.002)	(0.002)
Working Capital	-0.002	-0.004	-0.004
	(0.004)	(0.004)	(0.004)
Assets	0.001	0.001	0.001
	(0.003)	(0.002)	(0.002)
Landholdings	0.001**	0.001**	0.001**
	(0.000)	(0.000)	(0.000)
Yes to $12.50 Investment (Imputed)	-0.227	-0.230	-0.237
	(0.158)	(0.168)	(0.168)
Yes to $25.00 Investment (Imputed)	0.144	0.136	0.127
	(0.134)	(0.129)	(0.132)
Yes to $37.50 Investment (Imputed)	0.077	0.085	0.087
	(0.142)	(0.136)	(0.134)
Yes to $50.00 Investment (Imputed)	0.015	0.032	0.038
	(0.124)	(0.123)	(0.123)
Yes to $62.50 Investment (Imputed)	-0.284	-0.373	-0.365
	(0.457)	(0.485)	(0.486)
Yes to $75.00 Investment (Imputed)	0.361	0.359	0.354
	(0.232)	(0.232)	(0.230)
Constant	0.070	0.079	0.087

	(0.796)	(0.834)	(0.836)
Observations	1,178	1,178	1,178
p-value (Acharya et al., 2016)	0.839	0.893	0.865
District Dummies	Yes	Yes	Yes
R-squared	0.084	0.084	0.082

Bootstrapped standard errors in parentheses

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

Table 4. OLS Estimation Results for Distance-from-Sample-Mean Regressions

Variables	(1) Income	(2) Income Per Capita	(3) Income Per AE
Dependent Variable: Distance from Sample Mean Squared (Standardized)			
Contract Farming	-0.173***	-0.172***	-0.169***
	(0.047)	(0.050)	(0.049)
Household Size	-0.037**	-0.022	-0.021
	(0.015)	(0.018)	(0.018)
Dependency Ratio	-0.210	-0.254*	-0.255
	(0.147)	(0.154)	(0.157)
Single	-0.022	-0.082	-0.109
	(0.117)	(0.136)	(0.132)
Female	0.228	0.207	0.239
	(0.152)	(0.182)	(0.180)
Migrant	0.054	0.052	0.058
	(0.098)	(0.107)	(0.109)
Age	-0.016	-0.015	-0.016
	(0.011)	(0.011)	(0.011)
Education	-0.003	0.007	0.007
	(0.010)	(0.010)	(0.010)
Agricultural Experience	0.013	0.013	0.014
	(0.009)	(0.010)	(0.010)
Member of a Farm Organization	0.019	0.036	0.045
	(0.067)	(0.070)	(0.069)
Taboo Days	-0.000	0.002	0.002
	(0.002)	(0.002)	(0.002)
Working Capital	0.007*	0.003	0.002
	(0.004)	(0.005)	(0.005)
Assets	0.001	0.000	0.000
	(0.002)	(0.002)	(0.002)
Landholdings	0.001**	0.001**	0.001**
	(0.000)	(0.000)	(0.000)
Yes to $12.50 Investment (Imputed)	-0.226	-0.179	-0.185
	(0.151)	(0.168)	(0.173)
Yes to $25.00 Investment (Imputed)	0.016	-0.019	-0.030
	(0.111)	(0.121)	(0.117)
Yes to $37.50 Investment (Imputed)	-0.007	-0.006	-0.017
	(0.150)	(0.162)	(0.157)
Yes to $50.00 Investment (Imputed)	-0.091	-0.053	-0.081
	(0.133)	(0.138)	(0.140)
Yes to $62.50 Investment (Imputed)	-0.621	-0.684	-0.716
	(0.494)	(0.509)	(0.521)
Yes to $75.00 Investment (Imputed)	0.223	0.187	0.186
	(0.215)	(0.212)	(0.218)
Constant	1.129	1.016	1.128

	(0.865)	(0.881)	(0.899)
Observations	1,178	1,178	1,178
p-value (Acharya et al., 2016)	0.611	0.889	0.764
District Dummies	Yes	Yes	Yes
R-squared	0.305	0.228	0.220

Bootstrapped standard errors in parentheses

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

Table 5. OLS Estimation Results for Distance-from-Conditional-Mean Regressions

Variables	(1) Income	(2) Income Per Capita	(3) Income Per AE
Dependent Variable: Distance from Conditional Mean Squared (Standardized)			
Contract Farming	-0.176***	-0.173***	-0.170***
	(0.050)	(0.050)	(0.050)
Household Size	-0.044***	-0.030*	-0.029
	(0.015)	(0.018)	(0.018)
Dependency Ratio	-0.173	-0.222	-0.212
	(0.148)	(0.155)	(0.158)
Single	-0.056	-0.090	-0.118
	(0.111)	(0.134)	(0.129)
Female	0.236	0.202	0.238
	(0.152)	(0.180)	(0.177)
Migrant	-0.016	-0.005	0.001
	(0.100)	(0.108)	(0.110)
Age	-0.013	-0.011	-0.012
	(0.010)	(0.011)	(0.011)
Education	-0.003	0.008	0.008
	(0.010)	(0.011)	(0.011)
Agricultural Experience	0.011	0.011	0.011
	(0.009)	(0.010)	(0.010)
Member of a Farm Organization	-0.028	-0.015	-0.008
	(0.066)	(0.069)	(0.068)
Taboo Days	-0.001	0.002	0.001
	(0.002)	(0.002)	(0.002)
Working Capital	0.007*	0.004	0.003
	(0.004)	(0.005)	(0.005)
Assets	0.002	0.002	0.001
	(0.002)	(0.002)	(0.002)
Landholdings	0.001**	0.001**	0.001**
	(0.000)	(0.000)	(0.000)
Yes to $12.50 Investment (Imputed)	-0.135	-0.135	-0.137
	(0.150)	(0.171)	(0.178)
Yes to $25.00 Investment (Imputed)	0.092	0.019	0.017
	(0.098)	(0.114)	(0.109)
Yes to $37.50 Investment (Imputed)	0.013	0.013	0.004
	(0.138)	(0.149)	(0.144)
Yes to $50.00 Investment (Imputed)	-0.052	-0.017	-0.045
	(0.146)	(0.151)	(0.154)
Yes to $62.50 Investment (Imputed)	-0.423	-0.493	-0.517
	(0.472)	(0.492)	(0.504)
Yes to $75.00 Investment (Imputed)	0.297	0.252	0.246
	(0.238)	(0.225)	(0.232)
Constant	0.636	0.591	0.677

	(0.827)	(0.848)	(0.868)
Observations	1,178	1,178	1,178
p-value (Acharya et al., 2016)	0.513	0.407	0.488
District Dummies	Yes	Yes	Yes
R-squared	0.297	0.229	0.221

Bootstrapped standard errors in parentheses

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

Table 6. OLS Estimation Results for Income Variability Regressions Exploring the Fixed Price Contract Mechanism

Variables	(1) Income	(2) Income Per Capita	(3) Income Per AE
Dependent Variable: Conditional Heteroskedasticity (Standardized)			
Proportion of Plots under Fixed Price	-0.258***	-0.237***	-0.241***
	(0.050)	(0.050)	(0.050)
Household Size	-0.023	-0.020	-0.019
	(0.017)	(0.017)	(0.017)
Dependency Ratio	-0.178	-0.154	-0.172
	(0.168)	(0.177)	(0.173)
Single	-0.068	-0.027	-0.019
	(0.162)	(0.175)	(0.176)
Female	0.161	0.126	0.120
	(0.220)	(0.239)	(0.240)
Migrant	0.041	0.054	0.056
	(0.125)	(0.123)	(0.123)
Age	-0.006	-0.007	-0.007
	(0.010)	(0.011)	(0.011)
Education	0.006	0.010	0.011
	(0.010)	(0.010)	(0.010)
Agricultural Experience	0.010	0.010	0.010
	(0.009)	(0.009)	(0.009)
Member of a Farm Organization	-0.009	-0.029	-0.027
	(0.080)	(0.077)	(0.078)
Taboo Days	0.000	0.001	0.001
	(0.002)	(0.002)	(0.002)
Working Capital	-0.002	-0.004	-0.004
	(0.004)	(0.004)	(0.004)
Assets	0.001	0.001	0.001
	(0.003)	(0.002)	(0.002)
Landholdings	0.001**	0.001**	0.001**
	(0.000)	(0.000)	(0.000)
Yes to $12.50 Investment (Imputed)	-0.223	-0.226	-0.233
	(0.158)	(0.168)	(0.168)
Yes to $25.00 Investment (Imputed)	0.149	0.140	0.132
	(0.133)	(0.129)	(0.132)
Yes to $37.50 Investment (Imputed)	0.078	0.085	0.087
	(0.142)	(0.137)	(0.134)
Yes to $50.00 Investment (Imputed)	0.022	0.038	0.045
	(0.123)	(0.122)	(0.122)
Yes to $62.50 Investment (Imputed)	-0.310	-0.397	-0.390
	(0.463)	(0.490)	(0.492)
Yes to $75.00 Investment (Imputed)	0.349	0.347	0.343
	(0.231)	(0.231)	(0.230)

Constant	0.110	0.116	0.124
	(0.804)	(0.843)	(0.845)
Observations	1,178	1,178	1,178
District Dummies	Yes	Yes	Yes
R-squared	0.086	0.086	0.084

Bootstrapped standard errors in parentheses

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

Table 7. 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 Taboo 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 8. Propensity Score Matching II: Treatment Effects (Three Nearest Neighbors, 0.01 Caliper)

Sample	Income	Income Per Capita	Income Per AE
Conditional Heteroskedasticity			
Unmatched Sample	-0.170***	-0.166***	-0.168***
	(0.058)	(0.058)	(0.058)
Average Treatment Effect on the Treated	-0.197***	-0.194***	-0.197***
	(0.086)	(0.087)	(0.087)
Average Treatment Effect	-0.199***	-0.189***	-0.191***
	(0.056)	(0.065)	(0.065)
Average Treatment Effect on the Untreated	-0.201***	-0.188***	-0.185***
	(0.064)	(0.058)	(0.065)
Distance from Sample Mean			
Unmatched Sample	-0.061	-0.085	-0.087
	(0.058)	(0.058)	(0.058)
Average Treatment Effect on the Treated	-0.134*	-0.158**	-0.151*
	(0.079)	(0.079)	(0.078)
Average Treatment Effect	-0.154***	-0.166***	-0.160***
	(0.060)	(0.062)	(0.061)
Average Treatment Effect on the Untreated	-0.173***	-0.174***	-0.169***
	(0.054)	(0.057)	(0.061)
Distance from Conditional Mean			
Unmatched Sample	-0.066	-0.090	-0.091
	(0.058)	(0.058)	(0.058)
Average Treatment Effect on the Treated	-0.144*	-0.158**	-0.153*
	(0.083)	(0.080)	(0.080)
Average Treatment Effect	-0.153***	-0.167***	-0.162***
	(0.061)	(0.061)	(0.061)
Average Treatment Effect on the Untreated	-0.162***	-0.177***	-0.170***
	(0.054)	(0.056)	(0.055)

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

Appendix

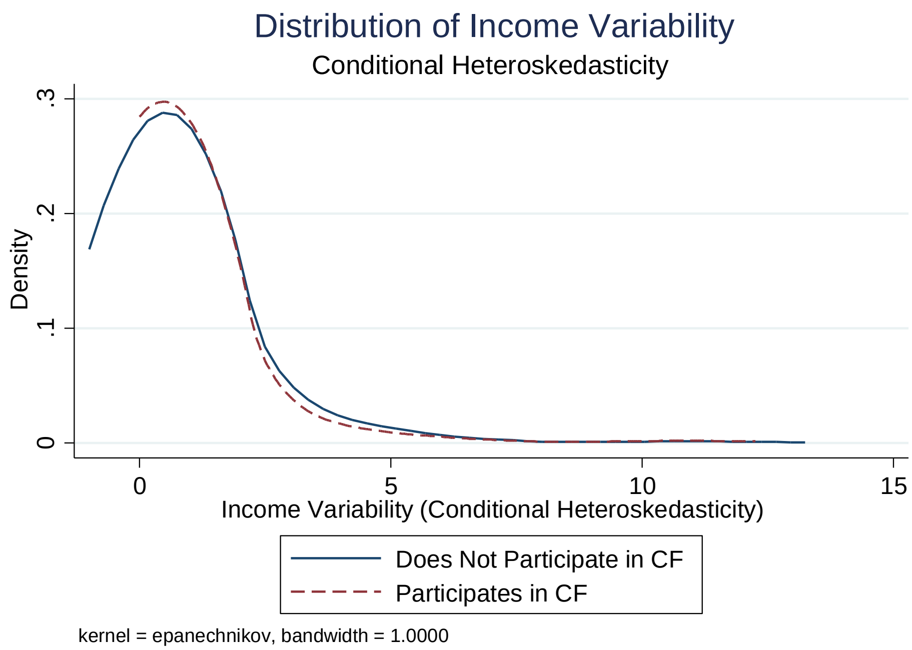

Figure A1a. Kernel Density Estimates of Income Variability – Conditional Heteroskedasticity.

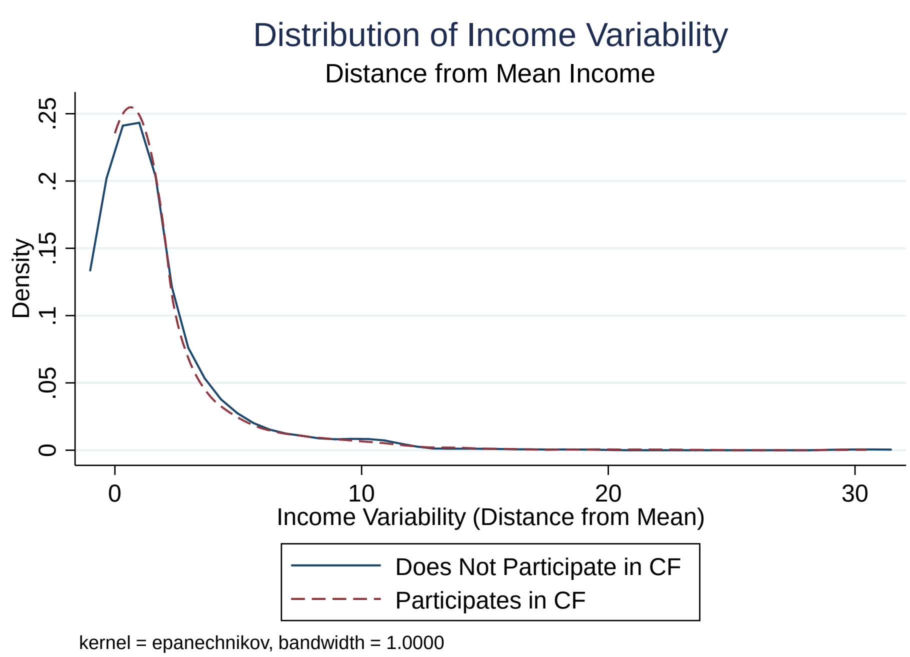

Figure A1b. Kernel Density Estimates of Income Variability – Distance from Mean Income.

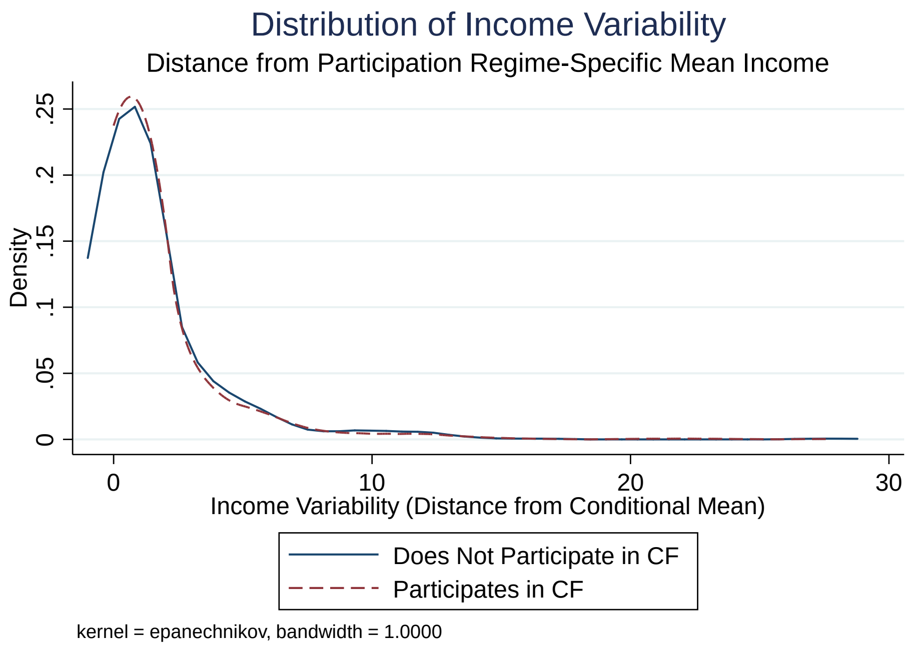

Figure A1c. Kernel Density Estimates of Income Variability – Distance from Regime-Specific Mean Income.

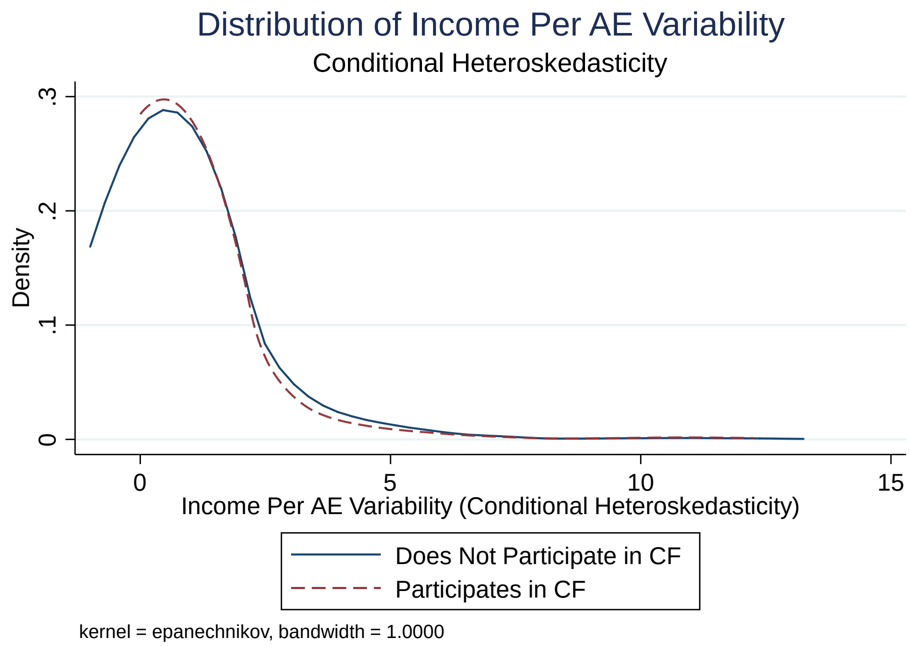

Figure A2a. Kernel Density Estimates of Income Per AE Variability – Conditional Heteroskedasticity.

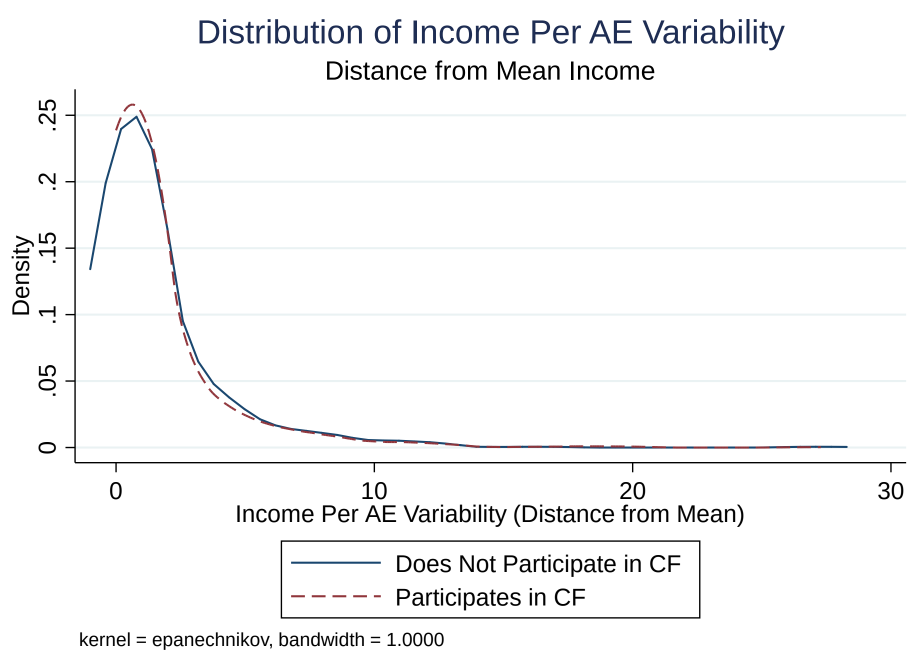

Figure A2b. Kernel Density Estimates of Income Per AE Variability – Distance from Mean Income.

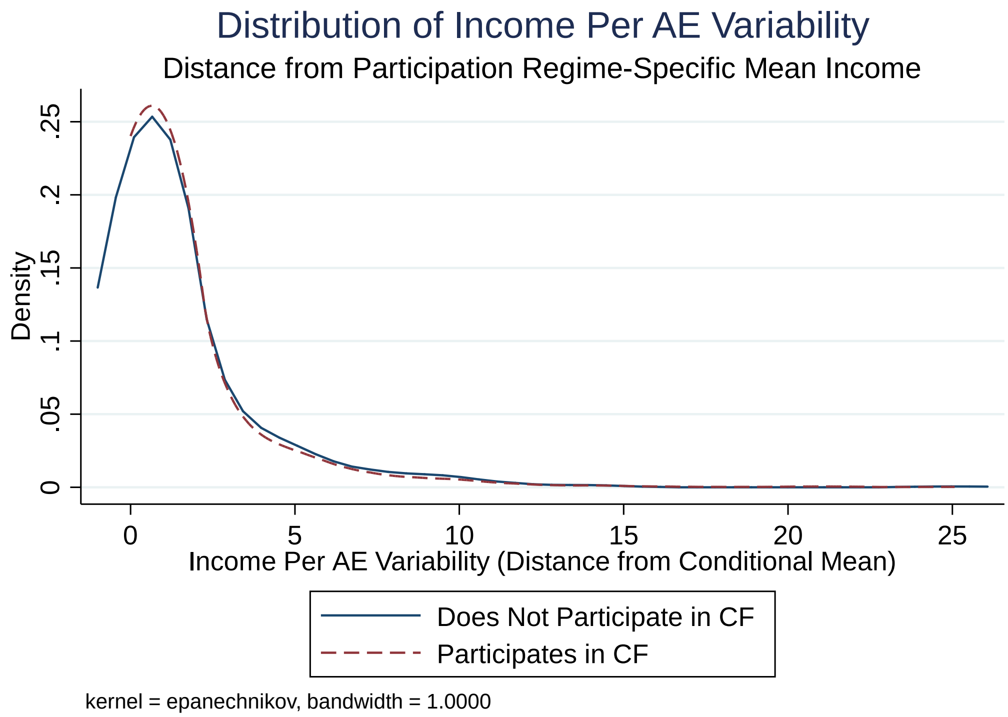

Figure A2c. Kernel Density Estimates of Income Variability – Distance from Regime-Specific Mean Income.

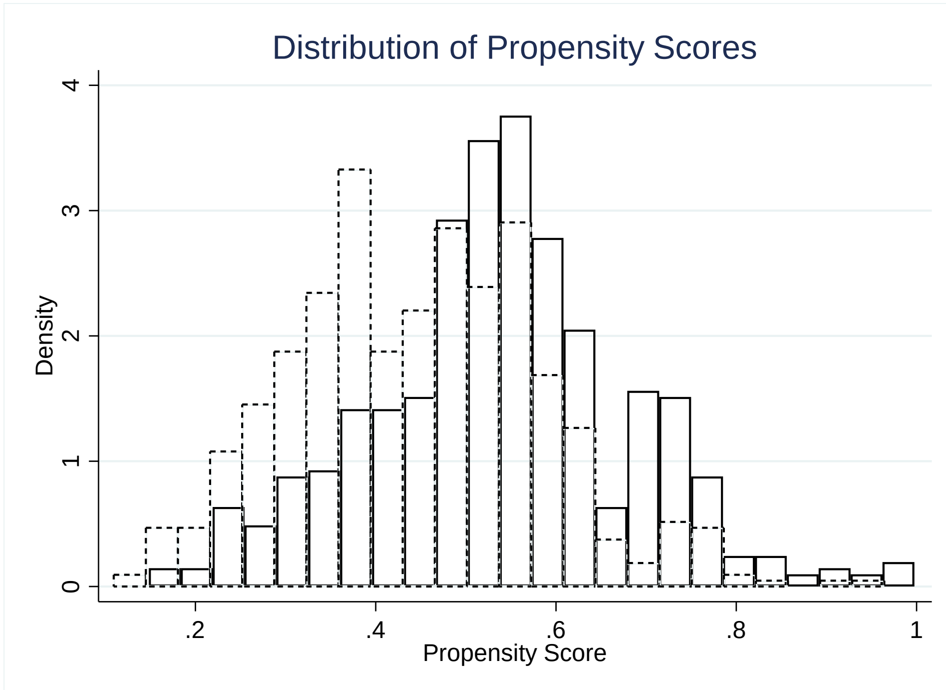

Figure A3. Distribution of Propensity Scores by Whether Households Participate in Contract Farming or Not.

Table A1. Median Regression Estimation Results for Conditional Heteroskedasticity

Variables	(1) Income	(2) Income Per Capita	(3) Income Per AE
Dependent Variable: Conditional Heteroskedasticity (Standardized)			
Contract Farming	-0.073**	-0.084***	-0.086***
	(0.031)	(0.031)	(0.033)
Household Size	0.005	0.010	0.013
	(0.008)	(0.009)	(0.009)
Dependency Ratio	-0.038	0.056	0.046
	(0.088)	(0.090)	(0.094)
Single	0.086	0.132*	0.154*
	(0.075)	(0.077)	(0.080)
Female	-0.057	-0.094	-0.113
	(0.088)	(0.090)	(0.095)
Migrant	0.028	0.031	0.042
	(0.056)	(0.057)	(0.060)
Age	0.002	0.000	0.001
	(0.004)	(0.004)	(0.004)
Education	0.005	0.007	0.008
	(0.005)	(0.005)	(0.005)
Agricultural Experience	-0.002	0.000	-0.000
	(0.003)	(0.003)	(0.004)
Member of a Farm Organization	-0.017	-0.025	-0.026
	(0.042)	(0.042)	(0.044)
Taboo Days	-0.001	-0.000	-0.000
	(0.001)	(0.001)	(0.001)
Working Capital	0.001	0.000	0.000
	(0.001)	(0.001)	(0.001)
Assets	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)
Landholdings	0.000	0.000***	0.000**
	(0.000)	(0.000)	(0.000)
Yes to $12.50 Investment (Imputed)	-0.010	0.072	0.045
	(0.089)	(0.091)	(0.095)
Yes to $25.00 Investment (Imputed)	-0.022	-0.056	-0.051
	(0.083)	(0.085)	(0.089)
Yes to $37.50 Investment (Imputed)	0.029	0.015	-0.003
	(0.085)	(0.086)	(0.090)
Yes to $50.00 Investment (Imputed)	0.032	0.038	0.049
	(0.079)	(0.081)	(0.085)
Yes to $62.50 Investment (Imputed)	0.068	0.039	0.042
	(0.131)	(0.134)	(0.140)
Yes to $75.00 Investment (Imputed)	-0.030	0.023	-0.013
	(0.096)	(0.098)	(0.103)
Constant	-0.496*	-0.592**	-0.564*

	(0.277)	(0.283)	(0.296)
Observations	1,178	1,178	1,178

Bootstrapped standard errors in parentheses

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

Table A2. Median Regression Estimation Results for Distance from Sample Mean Squared

Variables	(1) Income	(2) Income Per Capita	(3) Income Per AE
Dependent Variable: Distance from Sample Mean Squared (Standardized)			
Contract Farming	-0.081**	-0.066*	-0.065*
	(0.034)	(0.037)	(0.037)
Household Size	-0.009	-0.009	-0.007
	(0.009)	(0.010)	(0.010)
Dependency Ratio	-0.010	-0.032	-0.023
	(0.098)	(0.106)	(0.106)
Single	0.037	0.013	-0.006
	(0.084)	(0.091)	(0.091)
Female	0.072	0.044	0.050
	(0.098)	(0.107)	(0.107)
Migrant	0.000	0.061	0.032
	(0.062)	(0.067)	(0.067)
Age	-0.000	0.000	0.001
	(0.004)	(0.004)	(0.004)
Education	0.001	0.005	0.004
	(0.006)	(0.006)	(0.006)
Agricultural Experience	-0.001	0.000	0.000
	(0.004)	(0.004)	(0.004)
Member of a Farm Organization	-0.002	0.021	0.038
	(0.046)	(0.050)	(0.050)
Taboo Days	-0.001	0.000	-0.000
	(0.001)	(0.001)	(0.001)
Working Capital	0.008***	0.011***	0.010***
	(0.001)	(0.002)	(0.002)
Assets	0.002**	0.001	0.002*
	(0.001)	(0.001)	(0.001)
Landholdings	0.001***	0.000***	0.001***
	(0.000)	(0.000)	(0.000)
Yes to $12.50 Investment (Imputed)	-0.049	-0.012	-0.015
	(0.099)	(0.107)	(0.107)
Yes to $25.00 Investment (Imputed)	-0.014	0.014	0.033
	(0.093)	(0.100)	(0.101)
Yes to $37.50 Investment (Imputed)	0.002	-0.086	-0.049
	(0.094)	(0.102)	(0.102)
Yes to $50.00 Investment (Imputed)	-0.038	-0.025	-0.029
	(0.088)	(0.096)	(0.096)
Yes to $62.50 Investment (Imputed)	-0.003	0.031	0.022
	(0.146)	(0.158)	(0.158)
Yes to $75.00 Investment (Imputed)	0.056	0.056	0.091
	(0.107)	(0.116)	(0.116)
Constant	-0.295	-0.409	-0.469

	(0.308)	(0.334)	(0.335)
Observations	1,178	1,178	1,178

Bootstrapped standard errors in parentheses

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

Table A3. Median Regression Estimation Results for Distance from Conditional Mean

Variables	(1) Income	(2) Income Per Capita	(3) Income Per AE
Dependent Variable: Distance from Conditional Mean Squared (Standardized)			
Contract Farming	-0.077**	-0.070*	-0.056
	(0.037)	(0.036)	(0.039)
Household Size	-0.012	-0.011	-0.003
	(0.010)	(0.010)	(0.011)
Dependency Ratio	-0.021	-0.009	-0.058
	(0.104)	(0.103)	(0.112)
Single	0.076	0.012	0.070
	(0.090)	(0.088)	(0.096)
Female	0.054	0.013	-0.013
	(0.105)	(0.104)	(0.113)
Migrant	0.023	-0.042	-0.009
	(0.066)	(0.065)	(0.071)
Age	-0.000	0.002	0.001
	(0.004)	(0.004)	(0.005)
Education	-0.002	0.004	0.002
	(0.006)	(0.006)	(0.007)
Agricultural Experience	-0.001	-0.000	-0.001
	(0.004)	(0.004)	(0.004)
Member of a Farm Organization	0.003	-0.023	0.005
	(0.049)	(0.049)	(0.053)
Taboo Days	-0.001	-0.001	-0.000
	(0.001)	(0.001)	(0.001)
Working Capital	0.009***	0.010***	0.008***
	(0.002)	(0.001)	(0.002)
Assets	0.002**	0.002**	0.002*
	(0.001)	(0.001)	(0.001)
Landholdings	0.001***	0.000***	0.001***
	(0.000)	(0.000)	(0.000)
Yes to $12.50 Investment (Imputed)	0.022	0.067	-0.022
	(0.106)	(0.104)	(0.114)
Yes to $25.00 Investment (Imputed)	0.062	0.068	0.036
	(0.099)	(0.098)	(0.107)
Yes to $37.50 Investment (Imputed)	0.001	-0.047	-0.011
	(0.101)	(0.099)	(0.108)
Yes to $50.00 Investment (Imputed)	-0.035	0.010	-0.031
	(0.094)	(0.093)	(0.102)
Yes to $62.50 Investment (Imputed)	0.099	0.095	0.072
	(0.156)	(0.153)	(0.168)
Yes to $75.00 Investment (Imputed)	0.050	0.121	0.069
	(0.114)	(0.113)	(0.123)
Constant	-0.460	-0.700**	-0.520

	(0.330)	(0.325)	(0.355)
Observations	1,178	1,178	1,178

Bootstrapped standard errors in parentheses

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

Table A4. Balance Statistics for Matched Sample with Three Nearest Neighbors and a 0.01 Caliper

	Mean				
Variable	**Treated**	**Control**	**% Bias**	**t-statistic**	**p-value**
Household Size	5.768	5.824	-2.400	-0.420	0.674
Dependency Ratio	0.448	0.451	-1.500	-0.260	0.792
Single	0.085	0.076	2.600	0.510	0.611
Female	0.056	0.051	1.800	0.370	0.710
Migrant	0.129	0.104	7.400	1.310	0.190
Age	42.498	42.452	0.400	0.070	0.947
Education	6.012	5.993	0.600	0.100	0.923
Agricultural Experience	20.032	20.172	-1.100	-0.200	0.844
Member of Farm Organization	0.273	0.284	-2.800	-0.430	0.667
Taboo Days	23.854	21.570	6.500	1.150	0.250
Working Capital	6.587	5.076	6.000	1.500	0.133
Assets	14.659	13.349	4.700	0.780	0.433
Landholdings	184.920	159.860	7.600	1.260	0.209
District 1	0.176	0.178	-0.600	-0.100	0.918
District 2	0.241	0.262	-4.900	-0.810	0.419
District 3	0.188	0.202	-3.600	-0.590	0.558
District 4	0.139	0.118	5.900	1.080	0.281
District 5	0.165	0.150	4.200	0.730	0.464
District 6	0.090	0.090	-0.100	-0.020	0.986
Yes to $12.50 Investment (Imputed)	0.739	0.730	3.800	0.650	0.516
Yes to $25.00 Investment (Imputed)	0.784	0.783	0.400	0.080	0.936
Yes to $37.50 Investment (Imputed)	0.783	0.782	0.600	0.110	0.916
Yes to $50.00 Investment (Imputed)	0.710	0.703	2.500	0.450	0.652
Yes to $62.50 Investment (Imputed)	0.739	0.735	1.300	0.250	0.804
Yes to $75.00 Investment (Imputed)	0.631	0.624	2.400	0.410	0.679