As You Sow, So Shall You Reap: The Welfare Impacts of Contract Farming

Bellemare, Marc F.

Duke University

3 July 2010

As You Sow, So Shall You Reap: The Welfare Impacts of Contract Farming*

Marc F. Bellemare†

December 2, 2010

Abstract

What is the impact of participation in agricultural value chains on the welfare of smallholders? Contract farming, wherein a processing firm delegates its production of agricultural commodities to growers, is often viewed as a means of increasing smallholder welfare in developing countries. Because the problem posed by the nonrandom participation of grower smallholders in contract farming has so far not been dealt with convincingly, however, whether participation in contract farming actually increases smallholder welfare is still up for debate. This paper uses an experimentally derived nonparametric measure of willingness to pay to enter contract farming to control for actual participation in contract farming. Using data from Madagascar, results indicate that participation in contract farming is associated with a 10- to 16-percent increase in income; a 15-percent decrease in income volatility; a two-month decrease in the duration of the hungry season; and a 31-percent increase in the likelihood of receiving a formal loan.

Keywords: Contract Farming, Welfare, Grower-Processor Contracts, Outgrower Schemes, Africa, Madagascar
JEL Classification Codes: L23, L24, O13, O14, Q12

* I am grateful to David Stifel for invaluable help with the data as well as to Stefano Paternostro for making data collection possible and Eliane Ralison for coordinating the work of the survey team. I thank Jean-Marie Baland, Zack Brown, Catherine Guirkinger, Jonathan Kaminski, Bob Myers, Subhrendu Pattanayak, Vincenzo Verardi, Christian Vossler, and Ying Zhu for helpful comments. I am also grateful to seminar audiences at Hebrew University, Leuven, Michigan State, and Namur as well as participants at the 2010 SCC-76 "Economics and Management of Risk in Agriculture and Natural Resources" conference for their comments. This paper was written while I was on leave at the University of Namur, whose generous financial support I wish to acknowledge. I have also benefited from a seed grant from the Duke Center for International Development for preliminary work. All remaining errors are mine.
† Assistant Professor, Duke University, Box 90312, Durham, NC 27708-0312, marc.bellemare@duke.edu.

1. Introduction

With rising incomes and falling trade barriers over the past 60 years, consumers throughout the industrialized world have increasingly valued food diversity and availability. This is why the average US supermarket offers several varieties of tomatoes at any given time, for example, or why it commonly sells summer crops such as strawberries in the middle of winter. Likewise, with rising incomes throughout the developing world, supermarkets are playing an increasingly important role in providing consumers in developing countries with a more stable supply of a greater number of agricultural commodities.[1]

Rather than relying on commodities purchased at the farm gate or on spot markets, however, supermarkets rely on complex supply chains in which commodities are produced under contract (Reardon et al., 2003). Consequently, contract farming – the economic institution wherein a processing firm and a grower enter a contract in which the firm delegates its production of agricultural commodities to the grower – is playing an increasingly important role in developing countries.

Moreover, although industrialized countries remain the top sources of US food imports, "the greatest growth [of US food imports] between 1998 and 2007 was among imports from the developing countries" (USDA, 2009). Furthermore, with the advent of Fair Trade labeling in the late 1980s and the growing popularity of Fair Trade commodities in industrialized countries over the last decade, industrialized-country consumers are increasingly linked to developing-country producers, as Fair Trade commodities can now be purchased from Whole Foods in the US; Tesco in the UK; Loblaws in Canada; and Carrefour in France and elsewhere. In India, for example, Nestlé's biggest milk processing facility in the Punjab contracts with over 140,000 agricultural households (McMichael, 2009). Lastly, if the US offers any guidance as to what the future has in store for developing countries, 36 percent of the crops and livestock produced in the US are produced under contract, with estimates ranging from 21 percent for cattle to almost 90 percent for poultry (IATP, 2010).

[1] See Gereffi et al. (2010) on the processed food revolution as well as for a case study of tomatoes in the United States. For a discussion of how US-based retailers shape production networks in developing countries, see Gereffi (1994).

But what is the impact of participating in contract farming on the welfare of the growers, who are more often than not smallholders? Although much has been written on the market participation of smallholders (Bellemare and Barrett, 2006; Barrett, 2008), on agricultural supply chains in general (Reardon and Timmer, 2005) and on contract farming in particular (Grosh, 1994; and Bijman, 2008), little is known about the actual welfare impacts of contract farming on the households that choose to participate as growers. Intuition suggests that contract farming should at the very least increase the expected welfare of the households involved. If this were not the case, the assumption of individual rationality – the cornerstone of modern social science – dictates that they should refuse to participate in contract farming, just as it dictates that they should stop participating when these arrangements fail to increase their welfare. Thus, although contract farming is viewed by some as a means of fostering economic development by resolving several market failures (Grosh, 1994), others view the institution as a means of labor exploitation by capitalists (Glover and Kusterer, 1990; Clapp, 1994; Watts, 1994; Porter and Phillips-Howard, 1997).[2]

Using data from Madagascar, this paper studies the direct impact of participation in contract farming on several indicators of household welfare (i.e., income, income per capita, income per adult equivalent, income net of revenues from contract farming, duration of the hungry season, and whether the household has received a formal loan over the past year) as well as the indirect impact of participation in contract farming on welfare by testing whether the households who participate in contract farming have comparatively less volatile income measures.

The contribution of this paper lies in the way it identifies the causal impacts of contract farming on welfare. Indeed, because participation in contract farming is not randomly distributed across households, the main empirical challenge is to find a suitable instrumental variable (IV) to identify the causal impact of contract farming on welfare. That is, one must find a variable which explains participation in contract farming but which is also plausibly exogenous to household welfare. Without such a variable, one's estimate of the impact of participation in contract farming on household welfare will be biased.

[2] Such conclusions are not the exclusive preserve of social scientists. The executive director of the US-based Organization for Competitive Markets, a think-tank whose mission is to oppose the consolidation of firms in US agriculture, has been quoted as saying that farmers who enter contract farming arrangements "essentially become indentured servants on their own land" (Laskawy, 2009).

The IV used to identify the impact of participation in contract farming in this paper is a nonparametric lower bound on respondent willingness to pay (WTP) to participate in contract farming, which is derived from a dichotomous-choice contingent valuation experiment that was conducted during fieldwork. Respondents were asked whether they would agree to participate in a contract farming arrangement which would require an initial investment whose value was generated by the throw of a die, but which would increase their annual income with certainty. Because the hypothetical initial investment was randomly generated, the source of variation used in estimating WTP is completely exogenous to welfare. Moreover, because WTP captures the variation between respondents' respective marginal utilities derived from participation in contract farming, it effectively controls for the various sources of unobserved heterogeneity between respondents, such as various subjective perceptions, risk preferences, entrepreneurship, technical ability, etc. which all affect preferences over contract farming. These changes in preferences are captured by changes in WTP between respondents.[3]

Previous studies have instrumented participation in contract farming using a measure of respondent trustworthiness (Warning and Key, 2002); the number of organizations (including agricultural organizations) a respondent belongs to (Simmons et al., 2005); the distance between a respondent's farm and the farm of the village chief (Miyata et al., 2009); and respondent membership in a farmer group (Rao and Qaim, 2010).[4] In all cases, the exogeneity of the IVs used – whether they are orthogonal to the welfare measure of interest – is debatable, and one can easily come up with reasons why they are, in fact, not plausibly exogenous to the outcomes studied. Similarly, Minten et al. (2009) only observe households who participate in contract farming, and so they resort to comparing households who participate in contract farming with households who do not participate in contract farming by constructing a control group from a different data set. Likewise, although Singh's (2002) sample includes both participants and

[3] No less important is the question of reverse causality. Indeed, both (i) whether there is cognitive dissonance (i.e., whether actual participation in contract farming affects WTP); or (ii) whether welfare causally affects WTP are discussed at length and ultimately ruled out in section 3.

[4] Instead of using an IV, one could rely instead on propensity score matching methods, as in Maertens and Swinnen (2009), which assume that the difference between the treatment and control groups (i.e., in this case, between the households who participate in contract farming and those who do not) can be fully accounted on the basis of observables (Dehejia and Wahba, 2002). Because several unobservable factors (e.g., risk preferences, entrepreneurship, technical ability, etc.) likely drive the decision to go into contract farming, however, this paper does not further discuss PSM methods.

nonparticipants in contract farming, his test also consists in between-group comparisons of the means of selected indicators.

Consequently, the nonrandom participation of growers in contract farming has so far not convincingly been dealt with, which calls into question the welfare impacts of contract farming estimated in the extant literature given the bias in estimated effects that failing to properly identify the causal impact of participation in contract farming yields. In the limit, this means that whether participation in contract farming increases welfare is still up for debate, as the bias could be so severe as to lead to estimates that are of the wrong sign. And even if one were to grant that the aforementioned studies get the direction – positive or negative – of the impact of contract farming on welfare right, the endogeneity problem posed by the nonrandom participation of growers in contract farming would cause the magnitude of those impacts to be biased.[5]

The empirical results in this paper indicate that participation in contract farming increases household income by 10 percent; household income per capita by 14 percent; household income per adult equivalent by 16 percent; household income net of contract farming revenue by 9 percent; it decreases the duration of the hungry season experienced by the household by about two months; and that it increases the likelihood that a household receives a loan from a bank or a microfinance institution (MFI) by about 31 percent. Perhaps more importantly, empirical results suggest that participation in contract farming decreases vulnerability and indirectly increases welfare by decreasing the volatility of total household income, income per capita, and income per adult equivalent by about 15 percent, but that it has no such impact on household income net of contract farming revenue. Finally, a comparison of the WTP approach with the naïve ordinary least squares (OLS) case in which one would assume that participation in contract farming is randomly distributed – and therefore exogenous to welfare – underscores the policy importance of accurately identifying the causal relationship between contract farming and welfare.

For income alone, a comparison with previous studies indicates that the effects of contract farming may have been previously overstated. Indeed, while Warning and Key (2002), Miyata et

[5] Likewise, the fact that nonrandom participation in contract farming has so far not convincingly been dealt with does not allow claiming, as some have done, that (i) selection of is not an issue; and so (ii) estimated ATEs are unbiased when ignoring the selection problem. Indeed, such claims can only be made with a valid instrument for participation in contract farming.

al. (2009), and Rao and Qaim (2010) respectively find that participation in contract farming increases income by 32, 39, and 48 percent, the findings in this paper indicate that, in the data at hand, the impact of participation in contract farming is more modest at around 10 percent of income.

The remainder of this paper is organized as follows. In section 2, the data is discussed along with descriptive statistics. Section 3 presents the empirical framework and the strategy used in this paper to identify the causal impact of contract farming on welfare. In section 4, empirical results are presented and discussed at length. Section 5 concludes by discussing the research and policy implications of the empirical findings.

2. Data and Descriptive Statistics

The data used in this paper were collected between July and December 2008 for a study of contract farming commissioned by the Economic Development Board of Madagascar (EDBM) on behalf of the World Bank. Six regions were visited by the survey team, three of which were chosen from commune census data for their relatively high density of contract farming, with the remaining three chosen on account of their being deemed high-priority "growth areas" by EDBM. Figure 1 shows a map of the 22 regions of Madagascar: the six regions chosen for data collection were Alaotra-Mangoro (region 11 on the map in figure 1), Analamanga (4), Anosy (22), Diana (1), Itasy (3), and Vakinankaratra (5). The "growth areas" are regions 1, 5, and 22.

Within each region, the two communes with the highest density of contract farming were retained. Finally, within each of the 12 communes, 50 households were interviewed who participated in contract farming, and 50 households were interviewed who did not participate in contract farming.[6] For each household, data were collected at the household, plot, crop, and – whenever applicable – contract levels. The data thus consist of 1200 households, half of which are participants in contract farming. Because of the sampling scheme, probability weights are used to bring the sample as close as possible to a random sample throughout the paper. Table 1 synthesizes the six regions and 12 communes included in the data as well as the main contracted crops in each commune.

[6] Such a factorial design (List et al., 2010), in which the sample is split evenly between the treatment and the control group, is ideally suited for cases where the variance of the outcome (in this case, the variance of the various welfare measures) is constant across the treatment and the control group, which is unfortunately not the case in this context.

Table 2 presents summary statistics. Although the sample was designed so as to have 50 percent of the households participating in contract farming, the presence of missing observations for some of the variables means that only 1,178 households were retained for analysis, of which 49.2 percent are participants in contract farming.

The average household in the data is composed of 5.7 individuals, almost half of whom are dependents.[7] The majority of households in the sample are headed by a male, with only eight percent of households headed by a female. Almost one in eight households is headed by an individual who has never married or is widowed, and over one in eight households is headed by someone who was not born in the commune. The average household head is 43 years old, has completed six years of education, and has accumulated 20 years worth of agricultural experience. Finally, about one in five household heads is a member of one or more peasant organizations other than contract farming groups, and the average household head is forbidden from doing agricultural work over three weeks per year.[8]

In terms of welfare, total annual household income is on average equal to US$1153, but this figure drops down to US$1050 when excluding income from contract farming.[9,10] A very naïve back-of-the-envelope calculation therefore suggests that the average contract farming participant household derives an extra $102 per year from its participation in contract farming, or just about a 9 percent increase in income. In a country where the nominal GDP was of US$468 the year the data were collected, this difference in mean income between participants in contract farming and nonparticipants thus appears *prima facie* nontrivial (IMF, 2009). Of course, this is only suggestive, as it fails to control for the nonrandom nature of participation in contract farming as well as for a number of confounding factors.

[7] A household's dependency ratio is obtained here by dividing the number of individuals under 15 or above 65 years of age in the household by the total number of individuals in the household.
[8] The Malagasy observe a multiplicity of taboos, including a prescription against doing agricultural work on certain days. This taboo, which varies between households, has been found to have a significant negative impact on agricultural productivity by Stifel et al. (2008). See Ruud (1960) for a detailed anthropological survey of the many taboos observed by the Malagasy.
[9] US$1 ≈ 2000 Ariary when the data were collected.
[10] A household's total income includes (i) its income the sales of animals (cattle, pigs, sheep, goats, and poultry); (ii) its wages from various sources of labor (herding, agriculture, state, business, and other wages); (iii) its income from nonagricultural activities (crafts, trade, hunting and fishing, forestry, mining, pensions, transfers, and transportation); (iv) its income from leases (land, cattle, and equipment rentals) and from sales of animal byproducts (milk and eggs); and (v) its income from contract farming.

Similarly, total income per capita within the household is equal to US$219, and total income per adult equivalent is equal to US$271.[11] The average household experienced a hungry season – a period during which one or more individual within the household unwillingly eat less than three meals a day – that lasted almost three and a half months, and 14 percent of the households had received a formal (i.e., bank or MFI) loan in the year preceding data collection.

The average household owns US$325 in working capital (i.e., plow, cart, weeder, harrow, tractor, and other agricultural equipment), and $716 in other assets (i.e., television, radio, bicycle, cattle, pigs, sheep, goats, poultry, jewelry, businesses, bank account balance, and landholdings). Finally, as far as landholdings go, the average household owns 1.7 hectares of land in total, and anticipating on the WTP for contract farming described in detail in section 3.1 below, the average respondent would be willing to pay at least (given that this is a lower bound) $28 to participate in a contract farming agreement that would increase his annual income by 10 percent with certainty.

3. Empirical Framework

The core equation to be estimated in this paper is such that

$$y_i = \alpha_1 + \beta_1 x_i + \gamma_1 w_i + \delta_1 d_j + \epsilon_{ij}, \qquad (1)$$

where y_i is an indicator of welfare (i.e., income, income per capita, income per adult equivalent, duration of the hungry season, and a dummy for whether the household has received a formal loan) for household *i*; x_i is a vector of household characteristics; w_i is a dummy equal to one if the household participates in contract farming and equal to zero otherwise; d_j is a vector of dummies for each region *j*; and ϵ_{ij} is an error term with mean zero. Equation 1, however, does not and cannot control for the crop grown by the household, because the unit of analysis is the household and not the plot. The fact that most households grow more than one crop makes it difficult, if not impossible, to control for the crops grown. Because rice is the main crop grown by almost every household in Madagascar, however, and because the variation in contracted crops can be largely explained by regional differences (table 1), the variation in crops is largely controlled for by regional dummies in this paper.

[11] A household's total number of adult equivalents (Deaton, 1997) was obtained by treating each individual under 15 as 0.5 adult, each individual between the ages of 15 and 65 as one adult, and each individual over 65 as 0.75 adults.

The goal of this paper is to estimate γ, which represents the (causal) impact of participation in contract farming on household welfare. In this sense, γ allows calculating the average treatment effect of contract farming (ATE; see Wooldridge, 2002, chapter 18), which is such that

$$ATE = E(y_1 - y_0), \quad (2)$$

where y_1 is household welfare if the household participates in contract farming and where y_0 is household welfare if the household does not participate in contract farming. One can thus think of the problem posed by estimating the ATE as a missing data problem: data is missing on y_0 for the households that participate in contract farming, and data is missing on y_1 for the households that do not participate in contract farming. Of course, the fact that participation in contract farming is not randomly distributed across households means that naïvely estimating equation 1 directly would lead to a biased estimate of the ATE. In other words, participation in contract farming is endogenous to household welfare.

To get around this endogeneity problem and identify the causal impact of contract farming on household welfare, a few potential solutions are available to the researcher. What follows is a partial list of such solutions:

1. One can randomize over which households get to participate in contract farming and which households do not. This could be done, say, by subsidizing participation at random. Not only is this costly, it can be difficult to ensure effective compliance in the treatment group and noncompliance in the control group.
2. One can collect panel data and rely on household fixed effects to control for unobserved heterogeneity at the household level. This is also costly, especially in terms of time, and there is no guarantee that there will be enough within-household variation over time.
3. If one knows which processing firms intend on extending their contracting activities and in which communities they intend to do so, one can study households both before and after they participate in contract farming using the difference-in-differences methodology. This requires extensive knowledge of processing firms and their managers in a given area and is also costly in terms of time.
4. One can use an instrumental variable, i.e., a variable that is both correlated with participation in contract farming and plausibly exogenous to the welfare outcome of

interest. While this is the cheapest alternative, finding a valid instrument can be very difficult.

This paper follows the fourth and last option. Indeed, because participation in contract farming is not randomly distributed across households, the equation

$$w_i = \alpha_2 + \beta_2 x_i + \varphi_2 z_i + \delta_2 d_j + \nu_{ij}, \quad (3)$$

is first estimated as a probit and used to derive obtain $\hat{G}_i$, the vector of predicted probabilities obtained from estimating equation 3 (i.e., the predicted value of w_i). Equation 1 can then be estimated using $\hat{G}_i$, x_i, and d_j as instruments for w_i (see Wooldridge, 2002, procedure 18.1 for a discussion). The next section discusses the identification strategy adopted in this paper to identify the impact of contract farming on household welfare, i.e., the instrument for participation in contract farming.

3.1. Identification Strategy

As is often the case, the identification of the ATE is far from given in this context. Indeed, because the data are cross-sectional and include only one observation per household, one cannot control for the unobserved heterogeneity between households by incorporating household fixed effects. Moreover, participation in contract farming is almost surely driven by unobservable factors, which would bias any estimated ATE from contract farming obtained from a naïve estimation of equation 1. For example, because contract farming often insures growers against price risk via the use of fixed prices (Grosh, 1994), it is likely that participation in contract farming is driven by the respondent's price risk preferences. Risk preferences, however, are difficult to estimate from survey data, and proxies for risk preferences are only correlated with true risk preferences by assumption (e.g., constant relative risk aversion; decreasing absolute risk aversion; etc.). Even if one were to correctly hypothesize the relationship between risk preferences and a risk proxy included on the right-hand side of equation 1, the error term could still be correlated with that proxy, which would bias the estimate of γ. It could also be that participation in contract farming is driven by the respondent's entrepreneurial or technical abilities, which are difficult to measure and which are consequently omitted from most studies such as this one.

To overcome this difficulty, this paper instruments w_i – the dummy variable for whether a household participates in contract farming – by computing a nonparametric lower bound measure of a respondent's willingness to pay to participate in contract farming, which is derived here from a simple dichotomous-choice contingent valuation experiment conducted during fieldwork (Mitchell and Carson, 1989; Arrow et al., 1993). Each respondent in the sample was asked the question

> "Would you be willing to enter a contract farming agreement that would necessitate an initial investment of 25,000–50,000–75,000–100,000–125,000–150,000 Ariary (i.e., US$ 12.5–25–37.5–50–62.5–75) but which would increase your annual income by 10 percent?,"

where the initial investment was randomly generated by the throw of a die. A nonparametric lower bound on each respondent's WTP is calculated as follows. For yeasayers (i.e., those who answer "Yes" to the contingent valuation question), the only thing the researcher knows with certainty is that they would be willing to pay at least r_i to participate in the hypothetical contract farming arrangement described by the contingent valuation question. Alternatively, for naysayers (i.e., those who answer "No" to the contingent valuation question), the researcher only knows that they would be willing to pay any value in the $[-\infty, r_i)$ interval to participate in the hypothetical contract farming arrangement described by the contingent valuation question. Consequently, every yeasayer to the contingent valuation question is assigned the randomly-generated value of r_i drawn specifically for him as his lower bound on WTP. Alternatively, every naysayer is assigned a value of zero as his lower bound on WTP.

Compared to the usual parametric methods for estimating WTP (see, for example, Cameron and James, 1987), which make distributional assumptions, this nonparametric method only assumes that (i) WTP is nonnegative; and that (ii) respondents are individually rational, i.e., that they would accept a contract which would increase their annual income by 10 percent but which would require no initial investment. Compared to the usual nonparametric methods for estimating WTP (see, for example, Turnbull, 1976 and Watanabe, 2010), which only provide a central measure (e.g., mean or median) of WTP, the method used in this paper allows one to assign a specific WTP value to each respondent.

Although the assumption of individual rationality is the cornerstone of economics in particular and of modern social sciences in particular, some may object to imposing the requirement that WTP be nonnegative. After all, there are significant nonmonetary costs to participating in contract farming, which means that WTP could be negative for some respondents. But because the nonparametric lower bound on WTP estimate is used here as an instrumental variable rather than as an actual precise measure of WTP, what matters in this context is the *variation* in WTP, which is used here to identify the impact of participation in contract farming, rather than the precise value of WTP. Indeed, recall that WTP is a measure of marginal utility, and that a utility function only represents individual preferences up to an affine transformation (Mas-Colell et al., 1995). Because several utility functions can represent the same preferences, what matters for utility functions is that they preserve ordinality (i.e., the rank-ordering between alternatives), not cardinality (i.e., the actual utility values assigned to alternatives). Likewise, the assumption that the nonparametric lower-bound WTP estimate be nonnegative affects the cardinality of WTP, but what matters in this context is ordinality, as the value assigned as WTP to the naysayers does not significantly alter estimated ATEs.[12]

The nonparametric lower bound on WTP discussed above is then used as an instrument z_i for participation in contract farming, the identifying assumption being that a respondent's nonparametric lower bound on WTP to participate in the contract described by the contingent valuation question proxies for their WTP to participate in actual contract farming. Because the value of the initial investment r_i required by the hypothetical contract offered in the contingent valuation exercise was randomly generated during fieldwork, the variation in estimated WTP is exogenous to welfare. More importantly, estimated WTP controls for the unobserved heterogeneity between respondents because it is a direct measure of the marginal utility derived by respondents from participation in contract farming. For example, a respondent who is risk-averse, and who perceives that participating in contract farming would help him transfer the price risk he would otherwise face to the processing firm, is different from an otherwise identical risk-neutral respondent who does not mind bearing price risk, and this difference in risk

[12] Robustness checks (not shown, but available in the appendix) that assigned a value of -$20,000 (i.e., *negative* $20,000, a highly unlikely WTP in this context) to naysayers as WTP instead of zero show that the ATE of contract farming on household income increases from $119.23 to $131.03, i.e., a change equal to about 1 percent of average household income.

preferences is captured by the different valuation between individuals for the hypothetical contract. Likewise, an entrepreneurial respondent who would rather start his own business is different from an otherwise identical but less entrepreneurial respondent who would rather produce under contract for a processing firm, and this difference is also perceived by the different valuation between individuals for the hypothetical contract. Similarly, a respondent with a high level of agricultural ability and for whom the total cost of producing a given level is different from an otherwise identical but low-ability respondent, a difference that is once again reflected in the different valuations between individuals for the hypothetical contract. In other words, observationally identical respondents can derive different marginal utilities from contract farming due to unobservable characteristics, but those differences in marginal utilities are captured by WTP for contract farming, which accounts for most if not all of the unobserved heterogeneity between respondents.

Some may object that the IV used in this paper suffers from reverse causality when studying the impact of contract farming on income, income per capita, and income per adult equivalent. It is *a priori* true that because respondents are asked to evaluate a hypothetical contract that would increase their income by 10 percent, and because income differs between respondents, a respondent's response to the contingent valuation question may depend on his income. This is something one can test for, however, by simply regressing the nonparametric WTP estimate on income. Such a regression (not shown) shows that the estimated coefficient on income is not statistically significantly different from zero, as it has a *p*-value of 0.80. Even if one were to grant that coefficient statistical significance, the marginal impact of one US dollar of income on one's nonparametric lower bound on WTP would be equal to \$0.00005, or five thousandths of a cent. A similar regression of the respondent's dichotomous-choice answer to the contingent valuation question on income (not shown) shows that the estimated coefficient on income is not statistically significantly different from zero, as it has a *p*-value of 0.76. Again, even if one were to grant that coefficient statistical significance, the marginal impact of one US dollar of income on one's likelihood of saying "Yes" to the contingent valuation question would be equal to -0.000002, or two ten thousandths of one percent. In other words, the data offer no reason to believe that there is a reverse causality problem in these data. Running similar regressions on the other measures of income used as welfare measures (i.e., income per capita, income per adult equivalent, income net of contract farming revenue) yields similar results.

Similarly, some may object to the use of WTP as an instrument for contract farming on the grounds that participation in contract farming may be a normal or inferior good. That is, a household whose income or wealth is higher may wish to "consume" more or less participation in contract farming. Again, this is a story about different preferences for (or marginal utilities derived from) contract farming between respondents, and these differences in preferences (or marginal utilities) are accounted for by differences in WTP.

Lastly, one could also object that a respondent's actual participation in contract farming could affect her answer to the hypothetical participation question posed by the contingent valuation exercise because they have first-hand knowledge of the institution. The sampling strategy, however, should insure against such reverse causality given that even respondents who do not participate in contract farming have a thorough knowledge of the institution by virtue of living in the same small, close-knit villages as the respondents who participate in contract farming. Furthermore, recent research at the intersection of psychology and economics has invalidated almost every study that had previously found evidence in favor of the hypothesis that choices affect rather than reflect preferences, i.e., in favor of cognitive dissonance (Chen, 2008).

3.2. Grower Selection or Firm Discrimination?

Even though the nonparametric lower bound on WTP method presented above controls for the supply of growers (i.e., the nonrandom selection of households into contract farming), there is also a demand for growers on the part of processing firms, and firms discriminate between potential growers when choosing contracting partners.

This is a valid concern, as failure to accurately model the decision process of firms regarding how they choose their contracting partners may result in an omitted variables problem, which would bias the estimated coefficients in equation 1 even when controlling for household selection into contract farming using the nonparametric lower bound on WTP method presented above. For example, firms could discriminate between potential growers by choosing to contract only with individuals who have a level of technical ability higher than a specific threshold. In that case, if technical ability is unobserved by the researcher and correlated with the covariates on the right-hand side of equation 1, the estimated coefficients in equation 1 are biased. Consequently, it may not be sufficient to control for selection into contract farming, as

discrimination between potential growers may also lead to biased estimates of the ATE of contract farming on welfare.

In practice, however, it is unlikely that firms choose their growers on the basis of unobservables such as technical ability, entrepreneurial ability, or risk aversion. Even in cases where firms exploit decentralized local knowledge by asking growers with whom they have contracted in the past to recommend new growers, firms discriminate on the basis of observables. Given the richness of the data used in this paper, the researcher has access to more information on observables than the firm does. The inclusion of household characteristics on the right-hand side of equation 1 that are more informative than what processing firms have access to should thus control for the way firms discriminate between potential growers.

4. Estimation Results and Discussion

This section first present preliminary results which look nonparametrically at whether contract farming increases welfare by comparing, for the households that participate in contract farming and the households that do not participate in contract farming, (i) kernel density estimates for the welfare measures selected for analysis that are measured continuously; and (ii) the unconditional means of all the welfare measures selected for analysis. Because these preliminary results fails to control for confounding factors and is only suggestive, the estimation results for the treatment regressions discussed in section 3 follows the preliminary evidence and constitutes the bulk of this section.

4.1. Nonparametric Evidence

Before proceeding with the estimation sequence outlined in section 3, it is helpful to take a first pass at determining whether contract farming has a positive impact on the welfare of the households involved by looking at the problem nonparametrically, i.e., by comparing kernel density estimates for a subset of welfare measures as well as the unconditional means of all welfare measures by participation regime.

Figures 2 to 5 plot kernel density estimates by participation regime (households who do not participate in contract farming and households who participate in contract farming) for the four continuous indicators of welfare retained for analysis, i.e., household income, household income per capita, household income per adult equivalent, and household income net of revenues from

contract farming. Figures 2 to 4 suggest that total household income is higher for participants along all three measures considered here, but that there are no spillovers from participation in contract farming to other income categories, as suggested by figure 4. That is the income, income per capita, and income per adult equivalent of households who participate in contract farming is seemingly higher than the income, income per capita, and income per adult equivalent of households who do not participate in contract farming, but income net of contract farming revenue does not seem to vary between participation regimes. Lastly, figures 2 to 4 further suggest that the income of the households who participate in contract farming may be slightly less volatile than the income of households who do not participate.

Similarly, table 3 presents mean comparisons by participation regime for the variables retained for analysis as well as the result of a *t*-test of difference in means for each variable. These tests suggest that participants and nonparticipants in contract farming differ along almost all indicators, and that they are indistinguishable only along their dependency ratios, whether respondents are migrants, as well as along the education and agricultural experience of the respondents.

More importantly, these tests suggest that the households who participate in contract farming report a significantly higher income, income per capita, income per adult equivalent, and income net of contract farming revenue; that they experience a shorter hungry season; that they are more likely to have obtained a bank or MFI loan over the past 12 months; and that they are wealthier in that they own more in working capital, assets, and landholdings than the households who do not participate in contract farming. Thus, table 3 suggests that the households who participate in contract farming are better off along all welfare indicators, but that it is the financially better off households who elect to participate in contract farming.

4.2. Parametric Evidence

While the foregoing is helpful if one is interested in determining whether participation in contract farming is correlated with differences in various welfare indicators (e.g., income; income volatility; duration of hungry season; access to formal credit), it says nothing about causality. To properly answer the question of whether participation in contract farming systematically increases the welfare of the households involved, one must use the statistical apparatus presented in section 3.

This section first presents estimation results for the regression of actual participation in contract farming (w_i) on nonparametric WTP (z_i), household characteristics (x_i) and district fixed effects (d_j), i.e., equation 3 above. From this equation, a vector of predicted probabilities $\hat{G}_i$ of observing contract farming is derived, which is used along with x_i and d_j as an instrument for w_i in the first-stage equation for equation 1, the second stage of which consists in regressing y_i on $\widehat{w}_i$, x_i and d_j.

There are thus three equations estimated for each treatment regression: one equation for equation 3, which remains common to all treatment regressions and is presented; and two equations – the instrumenting regression and the regression of interest – for equation 1, as per the method outlined in Wooldridge (2002). Table 4 presents the results of equation 3, while tables 5 to 10 present the results of equation 1 estimated for all the welfare outcomes retained for analysis. Tables 5 to 10 also present naïve versions of the welfare equation (equation 1), i.e., versions of the welfare equation in which participation is not instrumented.

Although the estimation results presented in tables 5 to 10 allow determining whether participation in contract farming has direct impacts on welfare, they do not allow determining whether the institution might have indirect expected utility impacts on welfare by reducing income volatility. For each income measure, table 11 thus presents the results of heteroskedasticity tests aimed at determining whether income volatility is equal between the households that participate in contract farming and those that do not or whether there are systematic differences in cross-sectional income volatility between the two groups. Lastly, table 12 synthesizes the empirical results by presenting a summary of the estimated ATEs and a comparison of the estimated ATEs between growth and non-growth areas.

Table 4 presents estimation results for a probit regression of a dummy for actual participation in contract farming regressed on a respondent's nonparametric lower bound on WTP, the characteristics of his household, and district fixed effects. This is estimated both excluding (column 1) and including income (column 2) as a covariate, the latter being done to study the duration of the hungry season and the likelihood a household receives a formal loan, both of which depend in principle on income.

Recall that a lower bound each respondent's WTP was obtained by assigning the individual-specific random bid r_i to the respondents who answered "Yes" to the contingent valuation question and by assigning a value of zero to the respondents who answered "No" to the same question. As expected, the lower bound on WTP is positively correlated with whether a respondent actually participates in contract farming in that for a $1 increase in WTP, a respondent is on average 0.8 percent more likely to participate in contract farming, and this effect is significant at the 1 percent level. The other statistically significant results in table 4 are also not surprising. Female-headed households are 47 percent less likely than male-headed households to participate in contract farming. The older the head of the household, the less likely he is to participate in contract farming, as every additional year of age is associated with a 2 percent decrease in the likelihood that the household participates in contract farming. The more experienced the household head, however, the more likely he is to participate in contract farming, as every additional year of experience is associated with a 1.2 percent increase in the likelihood of participating. Likewise, households whose heads are members of peasant organizations (which excludes contract farming organizations) are 53 percent more likely to participate in contract farming than households whose heads are not members of such organizations. Lastly, the size of a household's landholdings is positively related with the likelihood that the household participates in contract farming. This is not surprising considering that households with larger landholdings are less likely to be constrained by land availability in deciding whether to participate in contract farming. Lastly, a comparison of the results between columns 1 and 2 of table 4 indicate that these estimation results vary only infinitesimally when income is included as a control variable in equation 4. This provides further empirical support for the hypothesis that respondents with different income levels do not evaluate different hypothetical contracts when answering the contingent valuation question, i.e., that there is no reverse causality between income of contract farming.

Armed with the results in table 4, one can estimate treatment regressions for each of the welfare outcomes retained for analysis. Table 5 presents estimation results for (i) the treatment regression of household income in columns 1 and 2, in which the dummy for whether the household participates in contract farming is instrumented with the respondent's nonparametric WTP to enter contract farming; and (ii) a naïve regression of household income in column 3, in which the dummy for whether the household participates in contract farming is not instrumented.

Tables 6 to 10 essentially follow the same plan, but for the other indicators of welfare selected for analysis (i.e., household income per capita; household income per adult equivalent; household income net of contract farming revenue; duration of the hungry season experienced by the household; and a dummy for whether the household receives a formal loan).

Because the empirical results for household participation in contract farming (i.e., the first column of table 5) are qualitatively the same in tables 5 to 10, only the first-column results in table 5 are discussed here. Female-headed households are almost 50 percent less likely to participate in contract farming than their male-headed counterparts. For every additional year of age, respondents are 1.4 percent less likely to have chosen to participate in contract farming, but for every additional year of agricultural experience, they are 2 percent less likely to have chosen to participate in contract farming. Participation in peasant organizations other than contract farming organizations is also associated with participation in contract farming in that a household that is a member of a peasant organization is over 50 percent more likely to participate in contract farming than a household who is not a member of such an organization. Moreover, for every additional hectare of land owned by the household, the household is 10 percent more likely to participate in contract farming. Finally, for every additional dollar of WTP, the average respondent is 0.8 percent more likely to participate in contract farming, an effect that is significant at less than the 1 percent level.

As regards the impact of participation in contract farming, table 5 shows that the institution has a positive impact on total household income, but that there is a considerable difference in estimated ATEs between the treatment regression and the naïve regression. Indeed, while the latter regression indicates that participating in contract farming increases a household's total income by US$42 or 3.6 percent of total income, the former indicates that participating in contract farming really increases a household's total income by US$119, or 10.4 percent of total income. So while one may *a priori* believe that the naïve regression would tend to overestimate the ATE of contract farming because it fails to control for the fact that households whose income is *ex ante* higher are more likely to participate in contract farming, it turns out that the selection mechanism operates in the opposite way. That is, households whose income is *ex ante* lower are the ones who are more likely to participate in contract farming, which biases the naïve ATE estimate downwards.

Comparing the results of the treatment regression in the first two columns with the results of the naïve regression in the third column, table 5 also shows how failing to take into account the nonrandom nature of participation in contract farming would lead to false conclusions, and so to mistaken policy recommendations for someone interested in stimulating participation in contract farming. For example, based on the results in the third column of table 5, one would mistakenly conclude that female-headed households have a systematically lower income; that the age of the respondent does not matter in determining income, but these findings disappear once one controls for selection into contract farming in the first two columns of table 5. Interestingly, the naïve regression also indicates that members of peasant organizations (other than contract farming organizations) have systematically higher incomes, but the results in the first two columns of table 5 show instead that members of peasant organizations (i) are more likely to participate in contract farming; which (ii) increases their income.

Turning to the other income measures retained for analysis, the empirical results show that participation in contract farming increases household income per capita by US$30.35 or 13.9 percent (table 6) and that it increases household income per adult equivalent by US$42.42 or 15.7 percent (table 7). Of considerable interest is the fact that participation in contract farming has significant spillover effects on sources of income other than income from contract farming. Indeed, the results in table 8 show that participation in contract farming increases income net of contract farming revenues by US$91.20 or 8.6 percent.

The results in table 9 show that participation in contract farming causes a two-month decrease in the duration of the hungry season experienced by the respondent's household. Likewise, table 10 presents results for the likelihood that the household has received a formal (i.e., bank or MFI) loan. Both the naïve and treatment regressions in this case show that the households who participate in contract farming are more likely to receive a formal loan, but the naïve approach biases the result downward. Indeed, while column 3 suggests that households who participates in contract farming are on average 7.1 percent more likely to receive a formal loan, the results in column 2 show that they are actually 31 percent more likely to receive a formal loan. Once again, although one may expect that the naïve regression would overestimate the ATE of contract farming because it fails to control for the fact that households who are *ex ante* more likely to receive a formal loan are also more likely to participate in contract farming, it

turns out that the selection mechanism operates in the opposite way. That is, households who are *ex ante* less likely to receive a formal loan are the ones who are more likely to participate in contract farming, which biases the naïve ATE estimate downwards. Furthermore, the naïve approach would also suggest that members of peasant organizations are on average more likely to receive formal loans than respondents who are not members of such organizations, but as in the case of total household income, this finding disappears once the respondents' selection into contract farming is controlled for in the second column.

As regards the potential expected-utility impact of contract farming (i.e., the impact of participation in contract farming on income volatility), table 11 reports the results of tests of group-wise heteroskedasticity for each of the dependent variables in tables 5 to 8. That is, it reports the results of tests of the null hypothesis that the variance of the residuals is the same for households who participate into contract farming and for those who do not in the equations presented in tables 5 to 8. The null hypothesis of homoskedasticity (i.e., the hypothesis that the variance of the residual is equal between groups) is rejected for all three measures of total income (i.e., household income, household income per capita, household income per adult equivalent), but it cannot be rejected for income net of contract farming revenue. Moreover, the rejection lies in a direction which suggests that participation in contract farming significantly reduces the (cross-sectional) volatility of total household income, income per capita, and income per adult equivalent, but that it does so by about 16 percent in every case. Over and above directly increasing the welfare of the households who participate in contract farming by increasing their income, income per capita, and income per adult equivalent as well as by decreasing the duration of the hungry season they experience, these findings suggest that contract farming indirectly increases their welfare by reducing the amount of income risk they are exposed to.

Table 12a synthesizes the estimation results by presenting the ATE of contract farming for each outcome in tables 5 to 10. Likewise, in order to determine whether the impacts of contract farming are significantly different between the regions closer to the capital and more remote regions, table 12b synthesizes the estimation results for the ATEs of contract farming on income between the so-called "high-priority growth areas" (regions 1, 5, and 22 in figure 1) and the other regions (regions 3, 4, and 11 in figure 1). These results indicate that although the ATEs are

statistically significantly higher in the regions closer to the capital, they are still economically significant and positive in the more remote areas. Moreover, the appendix shows the results of robustness checks, which indicate that the empirical results in this paper are robust to a change in specification.

What to make of these results? First off, it appears that participation in contract farming has a significant direct effect on welfare in that it significantly increases total household income, household income per capita, and household income per adult equivalent, and the ATEs for these three variables are respectively equal to 10 percent of household income, 14 percent of household income per capita, and 16 percent of household income per adult equivalent. Not only does participation in contract farming directly increase incomes, it also appears that it does so indirectly via spillovers on other sources of income, as a household's income net of contract farming revenue increases by 9 percent as a result of the household participating in contract farming.

Second, participation in contract farming decreases the duration of the hungry season experienced by the household by about two months and it increases the likelihood that a household will receive a formal (i.e., bank or MFI) loan by over 30 percent, perhaps because participation in contract farming sends a credible signal to banks and MFIs that the household has been screened for trustworthiness by the processing firm, or because banks and MFI can threaten to seize contracted crops more credibly than other assets in case of nonrepayment.

Third, heteroskedasticity tests suggest that participation in contract farming has a significant indirect welfare effects in that it reduces by 16 percent the (cross-sectional) volatility of total household income, household income per capita, and household income per adult equivalent. This is a key finding given that in rural areas of developing countries, poverty and risk have been found to entail welfare losses of comparable magnitude (Ligon and Schechter, 2003; Dercon, 2005).

Finally, although the data are cross-sectional and therefore do not lend themselves to analyzing welfare dynamics in relation to contract farming, one can still say something about the impact of the institution on inequality. Looking once again at the results in the first column of table 5 to 10, it looks as though households whose heads are older and less experienced,

households with smaller landholdings, and households whose heads are not members of a peasant organization and for whom more days cannot be spent working in agriculture are less likely to participate in contract farming. Thus, it looks as though contract farming may on the one hand increase inequality because it favors those with larger landholdings – who already have more opportunities for diversification – and those for whom agricultural work is forbidden on fewer days, a cultural artifact that has been shown by Stifel et al. (2008) to significantly reduce agricultural productivity at the margin. On the other hand, comparisons of naïve OLS estimation results – in which participation in contract farming is treated as randomly distributed in the population – with estimation results from the more accurate treatment regressions – in which the causal impact of participation in contract farming is identified using respondent WTP to participate in contract farming as a source of plausibly exogenous variation in contract farming participation – indicate that it is the poor who overwhelmingly select into participating in contract farming, which suggests that contract farming may decrease inequality.

5. Conclusion

Using data collected in six regions of Madagascar in 2008, this paper has studied the welfare impacts of participation in contract farming. Because participation in contract farming is not random, the results of a dichotomous-choice contingent valuation experiment were used to compute a nonparametric lower bound on the WTP of respondents to participate in contract farming, which was then used to predict participation in contract farming. This has allowed identifying the change in welfare due to a household's participation in contract farming.

The empirical results show that participation in contract farming directly increases total household income by 10 percent; household income per capita by 14 percent; household income per adult equivalent by 16 percent; household income net of contract farming revenue by 9 percent; and the likelihood that a household will receive a formal loan by over 30 percent. Moreover, participation in contract farming decreases both the duration of the hungry season experienced by the average household by about two months and the volatility of the total income of the average household by 16 percent. The latter result implies that participation in contract farming has indirect expected utility impacts on household welfare.

What are the policy implications of these findings? In a context where some researchers and policy makers perceive participation in contract farming as something close to bonded labor,

these findings indicate that contract farming has positive impacts on the welfare of the households involved. As such, even though the institution may increase inequality, stimulating industrial development by providing incentives for processing firms to delegate their production of agricultural commodities and for households to participate in agricultural commodity chains would likely contribute to alleviating poverty in these data. More concretely, policy makers could stimulate participation in contract farming by offering subsidies for processing firms to expand their contracting out activities and by targeting households headed by females; older individuals; individuals who are less experienced; and individuals who are not members of peasant organizations, as many of these characteristics are also associated with persistent poverty in Madagascar (Stifel et al., 2010).

Finally, it is important to qualify these empirical findings by offering a few caveats. First, as Foster and Rosenzweig (2010) point out, household income is not the best measure of welfare since it does not take into account the various costs borne by the household. Instead, farm profits would constitute a much better measure of welfare. Second, and perhaps more importantly, contract farming activities in the developing world are usually concentrated easily accessible areas. Thus, although the findings in this paper indicate that contract farming has positive impacts on the welfare of the households involved, whether these findings would hold if processing firms were to expand their activities to other communities is an empirical question.

References

Arrow, Kenneth, Robert Solow, Paul R. Portney, Edward E. Leamer, Roy Radner, and Howard Schuman (1993), "Report of the NOAA Panel on Contingent Valuation," *Federal Register* 58: 4601-4614.

Barrett, Christopher B. (2008), "Smallholder Market Participation: Concepts and Evidence from Eastern and Southern Africa," *Food Policy* 33(4): 299-317.

Bellemare, Marc F., and Christopher B. Barrett (2006), "An Ordered Tobit Model of Market Participation: Evidence from Kenya and Ethiopia," *American Journal of Agricultural Economics* 88(2): 324-337.

Bijman, Jos (2008), "Contract Farming in Developing Countries: An Overview," Working Paper, Wageningen University.

Cameron, Trudy Ann, and Michelle D. James (1987), "Efficient Estimation Methods for Close-Ended Contingent Valuation Surveys," *Review of Economics and Statistics* 69(2): 269-276.

Chen, M. Keith (2008), "Rationalization and Cognitive Dissonance: Do Choices Affect or Reflect Preferences?," Working Paper, Cowles Foundation.

Clapp, Roger A. (1994), "The Moral Economy of the Contract," in Peter D. Little and Michael J. Watts, *Living under Contract: Contract Farming and Agrarian Transformation in Sub-Saharan Africa*, Madison, WI: University of Wisconsin Press.

Deaton, Angus (1997), *The Analysis of Household Surveys*, Washington, DC: World Bank Publications.

Dehejia, Rajeev H., and Sadek Wahba (2002), "Propensity Score Matching Methods for Nonexperimental Causal Studies," *Review of Economics and Statistics* 84(1): 151-161.

Dercon, Stefan (2005), "Risk, Poverty, and Vulnerability in Africa," *Journal of African Economies* 14(4): 483-488.

Foster, Andrew D., and Mark R. Rosenzweig (2010), "Microeconomics of Technology Adoption," Working Paper, Yale University.

Gereffi, Gary (1994), "The Organization of Buyer-Driven Global Commodity Chains: How U.S. Retailers Shape Overseas Production Networks," in Gary Gereffi and Miguel Korzeniewicz, eds., *Commodity Chains and Global Capitalism*, Westport, CT: Praeger.

Gereffi, Gary, Joonkoo Lee, and Michelle Christian (2010), "US-Based Food and Agricultural Value Chains and Their Relevance to Healthy Diets," *Journal of Hunger and Environmental Nutrition* 4(3): 357-374.

Glover, David J., and Ken Kusterer (1990), *Small Farmers, Big Business: Contract Farming and Rural Development*, New York: St. Martin's Press.

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

Institute for Agriculture and Trade Promotion (2010), "Complaints of Contract Farm Abuses Increasing," http://www.agobservatory.org/agribusiness_records.cfm?nID=241 accessed on February 24, 2010.

International Monetary Fund (2009), World Economic Outlook Database, http://www.imf.org/external/pubs/ft/weo/2009/02/weodata/index.aspx accessed on November 18, 2009.

Laskawy, Tom (2009), "Will Whole Foods' New Mobile Slaughterhouse Squeeze Small Farmers?," http://www.grist.org/article/2009-11-20-Whole-Foods-chicken-farms/ accessed on February 25, 2010.

Ligon, Ethan, and Laura Schechter (2003), "Measuring Vulnerability," *Economic Journal* 113(486): C95-C102.

List, John A., Sally Sadoff, and Mathis Wagner (2010), "So You Want to Run an Experiment, Now What? Some Simple Rules of Thumb for Optimal Experimental Design," NBER Working Paper.

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

Mas-Colell, Andreu, Michael D. Whinston, and Jerry R. Green (1995), *Microeconomic Theory*, Oxford: Oxford University Press.

McMichael, Philip (2009), "A Food Regime Analysis of the 'World Food Crisis'," *Agriculture and Human Values* 26(4): 281-295.

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.

Mitchell, Robert C., and Richard T. Carson (1989), *Using Surveys to Value Public Goods: The Contingent Valuation Method*, Washington, DC: Resources for the Future.

Miyata, Sachiko, Nicholas Minot, and Dinghuan Hu (2009), "Impact of Contract Farming on Income: Linking Small Farmers, Packets, and Supermarkets in China," *World Development* 37(11): 1728-1741.

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

Rao, Elizabeth J.O., and Matin Qaim (2010), "Supermarkets, Farm Household Income, and Poverty: Insights from Kenya," *World Development* forthcoming.

Reardon, Thomas, C. Peter Timmer, Christopher B. Barrett, and Julio Berdegué (2003), "The Rise of Supermarkets in Africa, Asia, and Latin America," *American Journal of Agricultural Economics* 85(5): 1140-1146.

Reardon, Thomas, and C. Peter Timmer (2005), "Transformation of Markets for Agricultural Output in Developing Countries Since 1950: How Has Thinking Changed?," in Robert E. Evenson, Prabhu Pingali, and T. Paul Schultz, eds., *Handbook of Agricultural Economics 3:*

Agricultural Development: Farmers, Farm Production, and Farm Markets, Amsterdam: Elsevier.

Ruud, Jørgen (1960), *Taboo: A Study of Malagasy Customs and Beliefs*, Antananarivo: Printy Loterana and Oslo: Oslo University Press.

Singh, Sukhpal (2002), "Contracting Out Solutions: Political Economy of Contract Farming in the Indian Punjab," *World Development* 30(9): 1621-1638.

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.

Stifel, David C., Marcel Fafchamps, and Bart Minten (2008), "Taboos, Agriculture, and Productivity," *Journal of Development Studies* forthcoming.

Stifel, David S., Felix Forster, and Christopher B. Barrett (2010), "The Evolution of Groupwise Poverty in Madagascar, 1999-2005," *Journal of African Economies* forthcoming.

Turnbull, Bruce W. (1976), "The Empirical Distribution Function with Arbitrarily Grouped, Censored, and Truncated Data," *Journal of the Royal Statistical Society, Series B (Methodological)*, 38(3): 290-295.

USDA (2009), "Trade Data Show Value, Variety, and Sources of US Food Imports," http://www.ers.usda.gov/AmberWaves/September09/DataFeature/ accessed on February 26, 2010

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.

Watanabe, Masahide (2010), "Nonparametric Estimation of Mean Willingness to Pay from Discrete-Response Valuation Data," *American Journal of Agricultural Economics* forthcoming.

Watts, Michael J. (1994), "Life under Contract: Contract Farming, Agrarian Restructuring, and Flexible Accumulation," in Peter D. Little and Michael J. Watts, *Living under Contract: Contract Farming and Agrarian Transformation in Sub-Saharan Africa*, Madison, WI: University of Wisconsin Press.

Wooldridge, Jeffrey M. (2002), *Econometric Analysis of Cross Section and Panel Data*, Cambridge, MA: MIT Press.

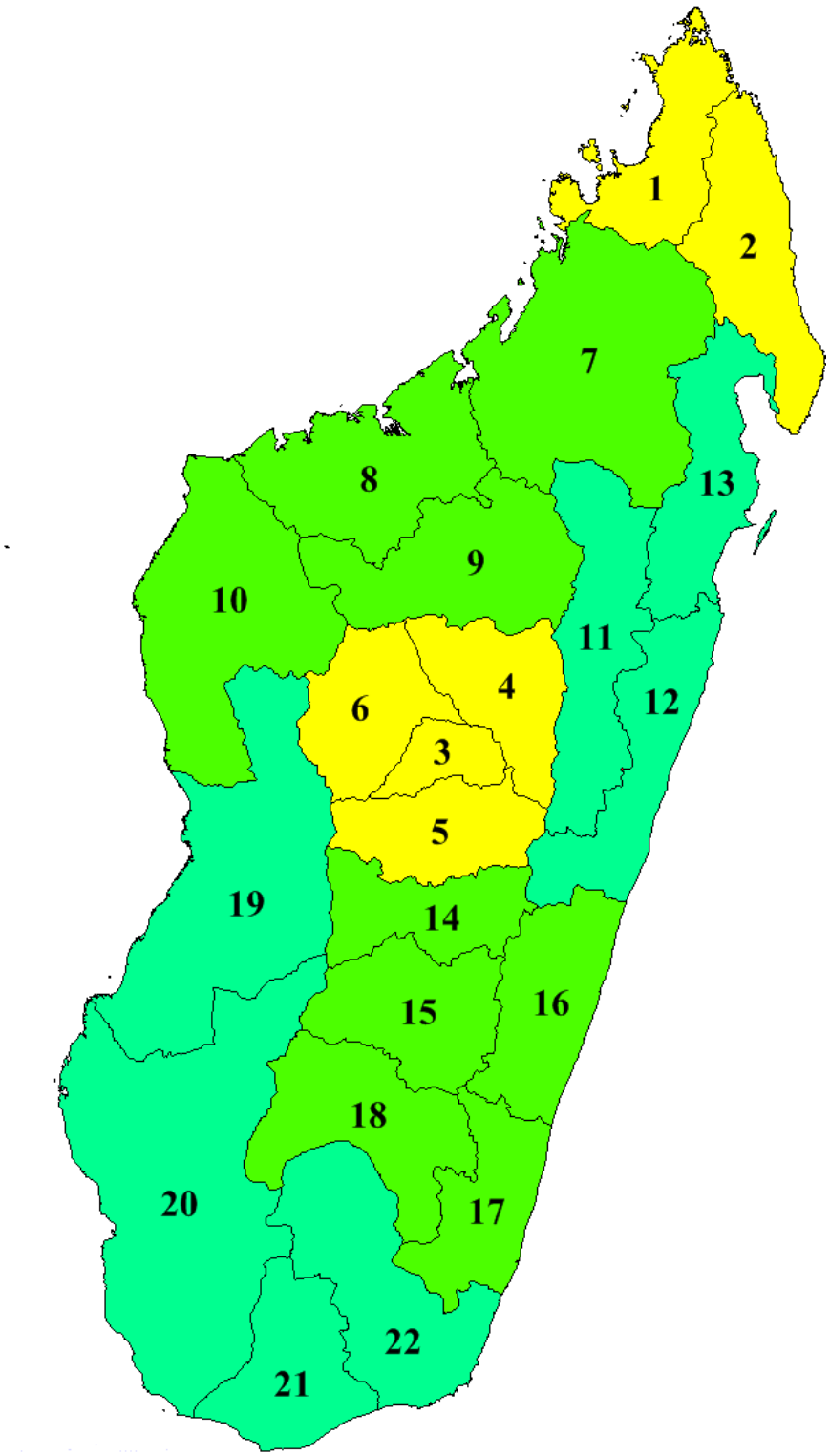

Figure 1. Map of Madagascar. Numbers Denote Regions and Colors Denote Provinces. (Source: Per Johansson/Wikimedia Commons.)

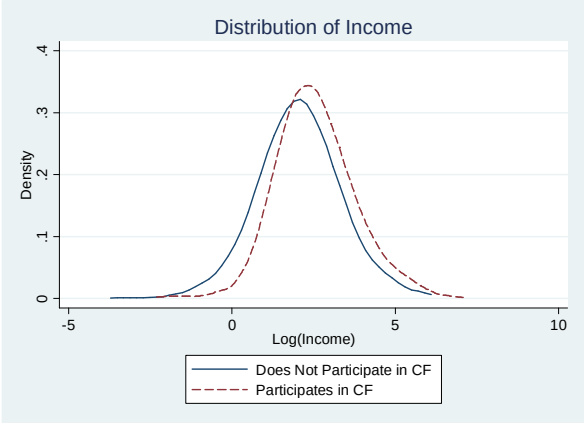

Figure 2. Kernel Density Estimation of Household Income by Participation Regime with Epanechnikov Kernel and Bandwidth Set Equal to 0.5.

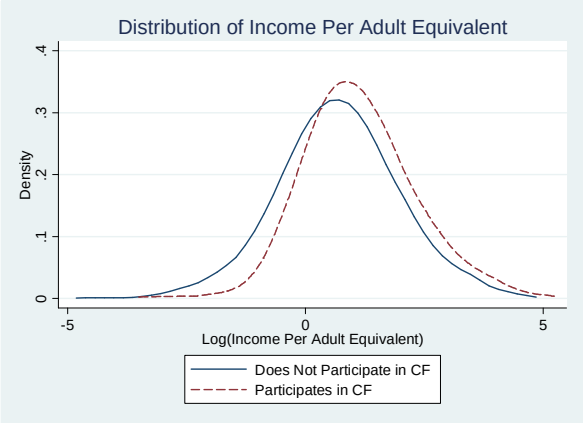

Figure 4. Kernel Density Estimation of Household Income Per Adult Equivalent by Participation Regime with Epanechnikov Kernel and Bandwidth Set Equal to 0.5.

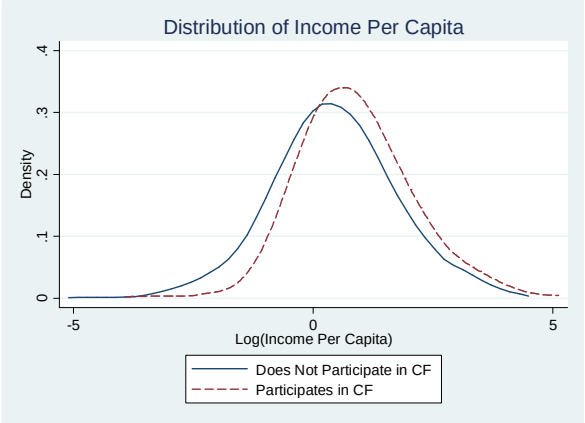

Figure 3. Kernel Density Estimation of Household Income Per Capita by Participation Regime with Epanechnikov Kernel and Bandwidth Set Equal to 0.5.

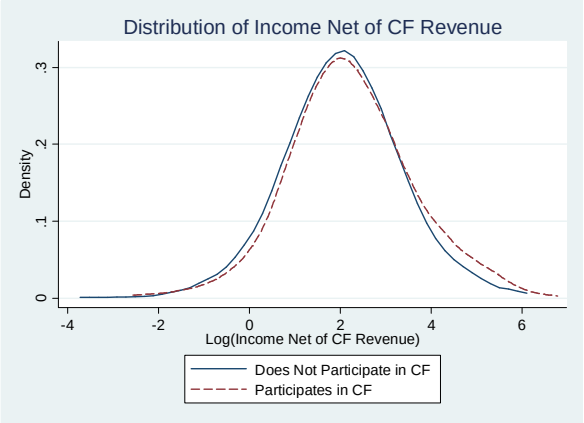

Figure 5. Kernel Density Estimation of Household Income Net of Contract Farming Revenue by Participation Regime with Epanechnikov Kernel and Bandwidth Set Equal to 0.5.

Table 1. Regions, Communes, and Crops

		Main Crops under Contract	
Region	**Commune**	**Primary**	**Secondary**
Alaotra Mangoro (11)	Bejofo	Rice	-
	Feramanga North	Rice	Tomatoes
Analamanga (4)	Amboasary North	Rice	-
	Mangamila	Rice	Cassava
Anosy (22)	Ebelo	Rice	Cassava
	Andranobory	Maize	-
Diana (1)	Ambodibonara	Cotton	Sugarcane
	Anketrakabe	Rice	-
Itasy (3)	Miarinarivo I	Green Beans	Leeks
	Soavinandriana	Green Beans	Leeks
Vakinankaratra (5)	Morarano	Rice	Potatoes
	Betafo	Barley	Onions

Note: Numbers between parentheses in the first column refer to the region numbers on the map in figure 1.

Table 2. Descriptive Statistics (n=1178)

Variable	Mean	(Std. Err.)
Contract Farming Participant Dummy	0.492	(0.015)
Household Demographic Characteristics		
Household Size (Individuals)	5.678	(0.068)
Dependency Ratio	0.447	(0.006)
Household Head Characteristics		
Female Dummy	0.080	(0.008)
Single Dummy	0.115	(0.009)
Migrant Dummy	0.130	(0.010)
Age (Years)	43.396	(0.363)
Education (Completed Years)	5.989	(0.098)
Agricultural Experience (Years)	20.363	(0.369)
Member of Peasant Organization Dummy	0.216	(0.012)
Forbidden Days	25.110	(1.030)
Household Welfare and Financial Characteristics		
Income (100,000 Ariary)	23.058	(1.583)
Income Per Capita (100,000 Ariary)	4.394	(0.282)
Income Per Adult Equivalent (100,000 Ariary)	5.421	(0.330)
Income Net of Contract Farming (100,000 Ariary)	21.006	(1.377)
Duration of *Hungry Season* (Months)	3.394	(0.062)
Obtained Formal Loan Dummy	0.140	(0.010)
Working Capital (100,000 Ariary)	6.508	(0.733)
Household Assets (100,000 Ariary)	14.334	(0.816)
Household Landholdings		
Total Landholdings (Ares)	169.276	(9.635)
Contingent Valuation (CV) Question		
Yeasayer (Answers "Yes" to CV Question) Dummy	0.732	(0.013)
Nonparametric Lower Bound on WTP (US Dollars)	28.215	(0.693)

Note: See section 3 for a discussion of how the WTP measure was estimated.

Table 3. Descriptive Statistics by Participation Regime (n=1178)

	Does Not Participate in Contract Farming (n=599)		Participates in Contract Farming (n=579)		
Variable	**Mean**	**(Std. Err.)**	**Mean**	**(Std. Err.)**	**Difference**
Household Demographic Characteristics					
Household Size	5.539	(0.095)	5.822	(0.095)	**
Dependency Ratio	0.448	(0.009)	0.445	(0.009)	
Female	0.102	(0.012)	0.057	(0.010)	***
Single	0.147	(0.014)	0.083	(0.011)	***
Migrant	0.124	(0.013)	0.136	(0.014)	
Age	44.225	(0.536)	42.539	(0.485)	***
Education	5.953	(0.139)	6.026	(0.138)	
Agricultural Experience	20.661	(0.555)	20.055	(0.485)	
Peasant Organization	0.154	(0.015)	0.280	(0.019)	***
Forbidden Days	26.676	(1.513)	23.491	(1.391)	*
Household Welfare and Financial Characteristics					
Total Income	17.352	(1.341)	28.961	(2.889)	***
Income Per Capita	3.459	(0.250)	5.360	(0.509)	***
Income Per Adult Equivalent	4.285	(0.310)	6.597	(0.587)	***
Income Net of Contract Farming	17.335	(1.340)	24.805	(2.427)	***
Duration of Hungry Season	3.591	(0.088)	3.191	(0.087)	***
Obtained Formal Loan	0.088	(0.012)	0.193	(0.016)	***
Working Capital	4.107	(0.502)	8.992	(1.392)	***
Assets	12.085	(1.114)	16.659	(1.188)	***
Total Landholdings	134.190	(10.514)	205.575	(16.181)	***
Contingent Valuation Question					
Yeasayer	0.674	(0.019)	0.791	(0.017)	***
Nonparametric WTP Lower Bound	25.292	(0.969)	31.239	(0.977)	***

Note: The acronyms AE and CF are short for "adult equivalent" and "contract farming", respectively. For each row, the last column presents the results of a *t*-test of the null hypothesis that the means are equal in both samples. The symbols ***, **, and * respectively denote a difference in means that is significant at the 1, 5, and 10 percent levels.

Table 4. Estimation Results for the First Stage of the Treatment Regressions (n=1178)

	(1)			(2)		
Variable	**Coefficient**		**(Std. Err.)**	**Coefficient**		**(Std. Err.)**
	Excluding Income			**Including Income**		
Dependent Variable: = 1 if Participates in Contract Farming; = 0 Otherwise.						
Household Size	0.031		(0.021)	0.029		(0.021)
Dependency Ratio	-0.157		(0.211)	-0.142		(0.212)
Single	0.096		(0.202)	0.105		(0.202)
Female	-0.468	**	(0.234)	-0.466	**	(0.234)
Migrant	0.081		(0.138)	0.080		(0.138)
Age	-0.020	***	(0.007)	-0.020	***	(0.007)
Education	-0.004		(0.014)	-0.007		(0.015)
Experience	0.012	*	(0.007)	0.012	*	(0.007)
Member of Peasant Organization	0.532	***	(0.110)	0.529	***	(0.110)
Forbidden Days	-0.002		(0.002)	-0.002		(0.002)
Income				0.003		(0.002)
Working Capital	0.005		(0.004)	0.003		(0.004)
Assets	0.001		(0.002)	0.001		(0.002)
Landholdings	0.001	**	(0.000)	0.000	**	(0.000)
Nonparametric WTP for Contract Farming	0.008	***	(0.002)	0.008	***	(0.002)
Intercept	0.381		(0.271)	0.372		(0.270)
District Fixed Effects	Yes			Yes		
p-value (Joint Significance)	0.000			0.000		
Pseudo R-square	0.075			0.076		

Note: These estimation results correspond to equation 3 in the body of the paper. Estimation results are probability-weighted. The symbols ***, **, and * respectively denote significance at the 1, 5, and 10 percent levels.

Table 5. Treatment Regression and OLS Estimation Results for Household Income (n=1178)

	(1)			(2)			(3)		
Variable	**Coefficient**		**(Std. Err.)**	**Coefficient**		**(Std. Err.)**	**Coefficient**		**(Std. Err.)**
	Treatment Regression						**OLS**		
	Dependent Variable: = 1 if Participates in Contract Farming; = 0 Otherwise			**Dependent Variable: Log of Income**			**Dependent Variable: Log of Income**		
Household Size	0.033		(0.021)	0.046	***	(0.016)	0.054	***	(0.015)
Dependency Ratio	-0.156		(0.212)	-0.093		(0.161)	-0.146		(0.148)
Single	0.132		(0.205)	-0.159		(0.144)	-0.150		(0.140)
Female	-0.491	**	(0.238)	-0.226		(0.174)	-0.338	**	(0.163)
Migrant	0.087		(0.140)	0.008		(0.103)	0.026		(0.096)
Age	-0.020	***	(0.007)	0.010	**	(0.005)	0.004		(0.004)
Education	-0.007		(0.015)	0.069	***	(0.010)	0.068	***	(0.010)
Experience	0.011		(0.007)	-0.004		(0.004)	-0.001		(0.004)
Member of Peasant Organization	0.518	***	(0.108)	0.030		(0.094)	0.174	**	(0.072)
Forbidden Days	-0.002		(0.002)	0.001		(0.001)	0.001		(0.001)
Working Capital	0.006		(0.005)	0.007	***	(0.002)	0.007	***	(0.002)
Assets	0.001		(0.003)	0.007	***	(0.002)	0.007	***	(0.002)
Landholdings	0.001	**	(0.000)	0.000		(0.000)	0.000	*	(0.000)
Contract Farming				1.038	***	(0.305)	0.362	***	(0.061)
Nonparametric WTP for Contract Farming	0.008	***	(0.002)						
Intercept	0.370		(0.267)	0.268		(0.281)	0.773	***	(0.175)
District Fixed Effects	Yes						Yes		
Log Pseudo-Likelihood	-1096.159						-		
p-value (Joint Significance)	0.000						0.000		
p-value (Test of Independent Equations)	0.033						-		
R-square	-						0.514		

Note: The estimation results in column 2 correspond to equation 1 in the body of the paper. Estimation results are probability-weighted. The symbols ***, **, and * respectively denote significance at the 1, 5, and 10 percent levels. Results in the first column are marginal effects.

Table 6. Treatment Regression and OLS Estimation Results for Household Income Per Capita (n=1178)

Variable	(1) Coefficient		(Std. Err.)	(2) Coefficient		(Std. Err.)	(3) Coefficient		(Std. Err.)
	Treatment Regression						OLS		
	Dependent Variable: = 1 if Participates in Contract Farming; = 0 Otherwise			Dependent Variable: Log of Income Per Capita			Dependent Variable: Log of Income Per Capita		
Household Size	0.033		(0.021)	-0.133	***	(0.016)	-0.126	***	(0.015)
Dependency Ratio	-0.161		(0.214)	-0.304	*	(0.162)	-0.350	**	(0.151)
Single	0.122		(0.207)	0.052		(0.151)	0.060		(0.147)
Female	-0.485	**	(0.240)	-0.379	**	(0.180)	-0.476	***	(0.167)
Migrant	0.083		(0.140)	0.016		(0.101)	0.033		(0.095)
Age	-0.020	***	(0.007)	0.009	**	(0.005)	0.005		(0.004)
Education	-0.006		(0.015)	0.071	***	(0.010)	0.070	***	(0.010)
Experience	0.011	*	(0.007)	-0.003		(0.004)	-0.001		(0.004)
Member of Peasant Organization	0.518	***	(0.109)	0.048		(0.094)	0.172	**	(0.070)
Forbidden Days	-0.002		(0.002)	0.001		(0.001)	0.001		(0.001)
Working Capital	0.006		(0.005)	0.007	***	(0.002)	0.008	***	(0.002)
Assets	0.001		(0.003)	0.006	***	(0.002)	0.007	***	(0.002)
Landholdings	0.001	**	(0.000)	0.000		(0.000)	0.000	*	(0.000)
Contract Farming				0.933	***	(0.331)	0.349	***	(0.061)
Nonparametric WTP for Contract Farming	0.008	***	(0.002)						
Intercept	0.373		(0.268)	-0.228		(0.297)	0.209		(0.177)
District Fixed Effects			Yes					Yes	
Log Pseudo-Likelihood			-1095.322					-	
p-value (Joint Significance)			0.000					0.000	
p-value (Test of Independent Equations)			0.086					-	
R-square			-					0.511	

Note: Estimation results are probability-weighted. The symbols ***, **, and * respectively denote significance at the 1, 5, and 10 percent levels. Results in the first column are marginal effects.

Table 7. Treatment Regression and OLS Estimation Results for Income Per Adult Equivalent (n=1178)

	(1)			(2)			(3)		
Variable	**Coefficient**		**(Std. Err.)**	**Coefficient**		**(Std. Err.)**	**Coefficient**		**(Std. Err.)**
	Treatment Regression						**OLS**		
	Dependent Variable: = 1 if Participates in Contract Farming; = 0 Otherwise			**Dependent Variable: Log of Household Income Per Adult Equivalent**			**Dependent Variable: Log of Household Income Per Adult Equivalent**		
Household Size	0.033		(0.021)	-0.127		(0.016)	-0.120	***	(0.015)
Dependency Ratio	-0.158		(0.213)	0.235		(0.159)	0.189		(0.148)
Single	0.122		(0.207)	0.053		(0.150)	0.061		(0.147)
Female	-0.485	**	(0.240)	-0.368	**	(0.179)	-0.466	***	(0.167)
Migrant	0.083		(0.140)	0.022		(0.101)	0.039		(0.095)
Age	-0.020	***	(0.007)	0.008	*	(0.005)	0.003		(0.004)
Education	-0.006		(0.015)	0.071	***	(0.010)	0.070	***	(0.010)
Experience	0.011	*	(0.007)	-0.004		(0.004)	-0.001		(0.004)
Member of Peasant Organization	0.519	***	(0.109)	0.052		(0.093)	0.177	**	(0.070)
Forbidden Days	-0.002		(0.002)	0.001		(0.001)	0.000		(0.001)
Working Capital	0.006		(0.005)	0.007	***	(0.002)	0.008	***	(0.002)
Assets	0.001		(0.003)	0.006	***	(0.002)	0.007	***	(0.002)
Landholdings	0.001	**	(0.000)	0.000		(0.000)	0.000	*	(0.000)
Contract Farming				0.940	***	(0.323)	0.351	***	(0.061)
Nonparametric WTP for Contract Farming	0.008	***	(0.002)						
Intercept	0.370		(0.268)	-0.197		(0.291)	0.243		(0.176)
District Fixed Effects	Yes						Yes		
Log Pseudo-Likelihood	-1094.555						-		
p-value (Joint Significance)	0.000						0.000		
p-value (Test of Independent Equations)	0.077						-		
R-square	-						0.493		

Note: Estimation results are probability-weighted. The symbols ***, **, and * respectively denote significance at the 1, 5, and 10 percent levels. Results in the first column are marginal effects.

Table 8. Treatment Regression and OLS Estimation Results for Household Income Net of Contract Farming Revenue (n=1178)

	(1)			(2)			(3)		
Variable	**Coefficient**		**(Std. Err.)**	**Coefficient**		**(Std. Err.)**	**Coefficient**		**(Std. Err.)**
	Treatment Regression						**OLS**		
	Dependent Variable: = 1 if Participates in Contract Farming; = 0 Otherwise			**Dependent Variable: Log of Income Net of Contract Farming Revenue**			**Dependent Variable: Log of Income Net of Contract Farming Revenue**		
Household Size	0.031		(0.021)	0.057	***	(0.018)	0.067	***	(0.016)
Dependency Ratio	-0.152		(0.211)	-0.047		(0.183)	-0.116		(0.160)
Single	0.153		(0.224)	-0.246		(0.200)	-0.234		(0.184)
Female	-0.512	**	(0.251)	-0.088		(0.231)	-0.232		(0.198)
Migrant	0.087		(0.141)	-0.003		(0.120)	0.021		(0.109)
Age	-0.020	***	(0.007)	0.013	**	(0.006)	0.006		(0.004)
Education	-0.007		(0.015)	0.075	***	(0.011)	0.074	**	(0.010)
Experience	0.011	*	(0.007)	-0.005		(0.005)	0.000		(0.004)
Member of Peasant Organization	0.512	***	(0.111)	-0.031		(0.133)	0.154	*	(0.083)
Forbidden Days	-0.002		(0.002)	0.001		(0.001)	0.001		(0.001)
Working Capital	0.006		(0.005)	0.007	***	(0.002)	0.008	***	(0.002)
Assets	0.001		(0.003)	0.007	***	(0.002)	0.007	***	(0.002)
Landholdings	0.001	**	(0.000)	0.000		(0.000)	0.000	*	(0.000)
Contract Farming				0.854	*	(0.521)	-0.016		(0.069)
Nonparametric WTP for Contract Farming	0.007	***	(0.002)						
Intercept	0.391		(0.271)	-0.075		(0.454)	0.576	***	(0.203)
District Fixed Effects	Yes						Yes		
Log Pseudo-Likelihood	-1152.29						-		
p-value (Joint Significance)	0.000						0.000		
p-value (Test of Independent Equations)	0.113						-		
R-square	-						0.461		

Note: Estimation results are probability-weighted. The symbols ***, **, and * respectively denote significance at the 1, 5, and 10 percent levels. Results in the first column are marginal effects.

Table 9. Treatment Regression and OLS Estimation Results for Hungry Season Duration (n=1178)

	(1)			(2)			(3)		
Variable	**Coefficient**		**(Std. Err.)**	**Coefficient**		**(Std. Err.)**	**Coefficient**		**(Std. Err.)**
	Treatment Regression						**OLS**		
	Dependent Variable: = 1 if Participates in Contract Farming; = 0 Otherwise			**Dependent Variable: Duration of Household Hungry Season**			**Dependent Variable: Duration of Household Hungry Season**		
Household Size	0.029		(0.021)	0.071	**	(0.036)	0.050		(0.036)
Dependency Ratio	-0.158		(0.211)	0.444		(0.388)	0.576		(0.365)
Single	0.110		(0.203)	-0.053		(0.361)	-0.078		(0.338)
Female	-0.500	**	(0.234)	0.442		(0.430)	0.723	*	(0.400)
Migrant	0.055		(0.140)	0.079		(0.246)	0.033		(0.216)
Age	-0.020	***	(0.006)	0.010		(0.011)	0.024	**	(0.009)
Education	-0.006		(0.015)	-0.071	***	(0.024)	-0.069	***	(0.022)
Experience	0.012	*	(0.006)	-0.023	**	(0.010)	-0.032	***	(0.010)
Member of Peasant Organization	0.531	***	(0.110)	0.463	*	(0.257)	0.104		(0.185)
Forbidden Days	-0.002		(0.002)	-0.004		(0.002)	-0.003		(0.002)
Income	0.004	**	(0.002)	-0.004	**	(0.002)	-0.004	**	(0.002)
Working Capital	0.003		(0.003)	0.003		(0.003)	0.002		(0.003)
Assets	0.001		(0.003)	-0.012	***	(0.003)	-0.013	***	(0.003)
Landholdings	0.000	**	(0.000)	0.000		(0.000)	0.000		(0.000)
Contract Farming				-1.988	**	(0.787)	-0.294	**	(0.142)
Nonparametric WTP for Contract Farming	0.007	***	(0.002)						
Intercept	0.372		(0.268)	4.799	***	(0.749)	3.533	***	(0.433)
District Fixed Effects	Yes						Yes		
Log Pseudo-Likelihood	-1577.523						-		
p-value (Joint Significance)	0.000						0.000		
p-value (Test of Independent Equations)	0.037						-		
R-square	-						0.197		

Note: Estimation results are probability-weighted. The symbols ***, **, and * respectively denote significance at the 1, 5, and 10 percent levels.

Table 10. Treatment Regression and OLS Estimation Results for the Likelihood of Receiving a Formal Loan (n=1178)

	(1)			(2)			(3)		
Variable	**Coefficient**		**(Std. Err.)**	**Coefficient**		**(Std. Err.)**	**Coefficient**		**(Std. Err.)**
	Treatment Regression						**OLS**		
	Dependent Variable: = 1 if Participates in Contract Farming; = 0 Otherwise			**Dependent Variable: = 1 if Household Received a Formal Loan; = 0 Otherwise**			**Dependent Variable: = 1 if Household Received a Formal Loan; = 0 Otherwise**		
Household Size	0.031		(0.021)	0.000		(0.005)	0.003		(0.004)
Dependency Ratio	-0.128		(0.209)	-0.020		(0.046)	-0.039		(0.041)
Single	0.121		(0.191)	-0.017		(0.041)	-0.013		(0.039)
Female	-0.488	**	(0.228)	0.028		(0.050)	-0.012		(0.045)
Migrant	0.098		(0.138)	0.003		(0.035)	0.010		(0.032)
Age	-0.018	***	(0.007)	0.004	**	(0.002)	0.002		(0.002)
Education	-0.010		(0.015)	0.014	***	(0.004)	0.014	***	(0.003)
Experience	0.010		(0.007)	-0.003		(0.002)	-0.002		(0.002)
Member of Peasant Organization	0.536	***	(0.110)	0.032		(0.029)	0.082	***	(0.028)
Forbidden Days	-0.003	*	(0.002)	0.000		(0.000)	0.000		(0.000)
Income	0.005	**	(0.002)	0.001		(0.001)	0.001		(0.001)
Working Capital	0.005		(0.005)	-0.002	**	(0.001)	-0.002	**	(0.001)
Assets	0.000		(0.002)	-0.001		(0.000)	0.000		(0.001)
Landholdings	0.000	*	(0.000)	0.000	***	(0.000)	0.000	***	(0.000)
Contract Farming				0.311	***	(0.063)	0.071	***	(0.019)
Nonparametric WTP for Contract Farming	0.007	***	(0.002)						
Intercept	0.339		(0.267)	-0.312	***	(0.079)	-0.133	**	(0.058)
District Fixed Effects	Yes						Yes		
Log Pseudo-Likelihood	-456.922						-		
p-value (Joint Significance)	0.000						0.000		
p-value (Test of Independent Equations)	0.000						-		
R-square	-						0.24		

Note: Estimation results are probability-weighted. The symbols ***, **, and * respectively denote significance at the 1, 5, and 10 percent levels.

Table 11. Results of Tests of Group-Wise Heteroskedasticity (n=1178)

Null Hypothesis of Homoskedasticity (Equal Error Variance between Participation Regimes)	Test Result	Change in Volatility Due to Contract Farming
Total Income (Table 5)	Rejected	-0.159***
Total Income Per Capita (Table 6)	Rejected	-0.162***
Total Income Per Adult Equivalent (Table 7)	Rejected	-0.164***
Total Income Net of Contract Farming Revenue (Table 8)	Not Rejected	0.025

Note: The symbols ***, **, and * respectively denote significance at the 1, 5, and 10 percent levels. In each row, the squared residuals from each relevant regression in tables 5 to 8 were regressed on a constant and a dummy equal to one if a household participates in contract farming and equal to zero otherwise. The second column reports the result of a *t*-test of the null hypothesis of homoskedasticity, i.e., the null hypothesis that the coefficient on the contract farming dummy is equal to zero. A rejection of the null hypothesis indicates that the volatility of the dependent variable is different between participants and nonparticipants. The third column reports the percentage change in volatility due to contract farming as well as the level of statistical significance of that change.

Table 12a. Synthesis of Estimated ATEs on Welfare Outcomes (n=1178)

Variable	ATE		(Std. Err.)
Income (US$)	119.23	***	(1.871)
Volatility of Income (Percent)	-0.16	***	(0.057)
Income Per Capita (US$)	30.35	***	(1.674)
Volatility of Income Per Capita (%)	-0.16	***	(0.058)
Income Per Adult Equivalent (US$)	42.42	***	(1.651)
Volatility of Income Per Adult Equivalent (%)	-0.16	***	(0.058)
Income Net of Contract Farming Revenue (US$)	91.20	***	(1.620)
Volatility of Income Net of CF Revenue (%)	0.025		(0.061)
Duration of Hungry Season (Months)	-1.99	**	(0.787)
Likelihood of Receiving a Formal Loan (%)	0.31	***	(0.063)

Note: The symbols ***, **, and * respectively denote significance at the 1, 5, and 10 percent levels.

Table 12b. Estimated ATEs for Growth Areas vs. Other Regions (n=1178)

	(1) Other Regions			(2) Growth Areas			(3) Significance of
Variable	ATE		(Std. Err.)	ATE		(Std. Err.)	(1) - (2)
Income (US$)	126.899	***	(2.663)	111.020	***	(2.583)	***
Income Per Capita (US$)	36.918	***	(2.372)	23.485	***	(2.327)	***
Income Per Adult Equivalent (US$)	48.950	***	(2.333)	35.547	***	(2.301)	***

Note: The symbols ***, **, and * respectively denote significance at the 1, 5, and 10 percent levels. The results in column 1 (column 2) are for regions 3, 4, and 11 (1, 5, and 22) in figure 1.

Appendix

A1. Robustness Check with a Different Instrumental Variable

In order to ensure that the empirical findings in the paper are robust to changes in specifications, this appendix presents the results of treatment regressions in which the instrumental variable used to control for respondents' nonrandom decision to participate in contract farming is a nonparametric lower bound on respondents' willingness to wait (WTW) for a contracted crop to come to fruition, i.e., a measure of each respondents' opportunity cost of time in the context of contract farming.

This additional measure of an individual's marginal utility derived from contract farming was derived similarly to the WTP measure discussed in section 3.1 using the information contained in the following question:

> "Would you be willing to enter a contract farming agreement for a crop which would take 0.5 year–1 year–1.5 years–2 years–2.5 years–3 years before you could harvest it but which would increase your annual income by 10 percent?,"

which was asked during fieldwork and in which time to harvest t_i was randomly generated by the throw of a die. A nonparametric lower bound on each respondent's WTW is calculated as follows. For yeasayers (i.e., respondents who answer "Yes" to the question above), the only thing the researcher knows with certainty is that they would be willing to wait at least t years to participate in the hypothetical contract farming arrangement described by the contingent valuation question. Alternatively, for naysayers (i.e., respondents who answer "No" to the question above), the researcher only knows that they would be willing to wait any value in the $[0, t_i)$ interval to participate in the hypothetical contract farming arrangement described by the contingent valuation question given that negative periods of time are impossible.

The reasoning behind using a respondent's WTW for contract farming is similar to that of using WTP. Because there is an opportunity cost to waiting for a crop to come to fruition, the longer a respondent is willing to wait, the longer he would also be willing to pay, *ceteris paribus*. In other words, one's WTW and WTP should be positively correlated.[13] The justification for the validity of WTW as an instrument for participation in contract farming is thus the same as in section 3.1 of the paper.

[13] In the data, the coefficient of correlation between WTP and WTW is indeed positive and equal to 0.36, a number significant at the 1 percent level.

Turning to descriptive statistics about the contingent valuation question above and WTW, a little over two thirds of respondents said "Yes" to the question: the proportion of yeasayers in the same sample of 1178 observations used throughout the paper was equal to 0.692, with a standard error of 0.014. The mean of the lower bound on WTW is equal to 1.129 years with a standard deviation of 1.003 years.

Tables A1 to A7 mirror tables A4 to A10 in the paper, estimating (i) the first-stage probit regressions (equation 3 in the paper) of actual participation in contract farming on respondent nonparametric WTW for contract farming as well as household characteristics and district fixed effects; and (ii) treatment regressions (equation 1 in the paper) for household income (table A2), income per capita (table A3), income per adult equivalent (table A4), income net of contract farming revenues (table A5), duration of the hungry season (table A6), and the likelihood that the household has received a formal loan (table A7).

The results in table A1 indicate that, on average, for respondents who are willing to wait at least one more year for a contracted crop which would increase their income by 10 percent to come to fruition, the likelihood that they actually participate in contract farming increases by 10 percent.

The results in tables A2 to A7 indicate that WTW for a hypothetical contract that would increase a respondent's income by 10 percent is highly correlated with actual participation in contract farming except in table A7, which presents the results of the treatment regression for the likelihood that the household receives a formal loan and in which WTW is only significant at the 10 percent level in the first stage of the treatment regression. Still, the estimated coefficient aimed at capturing the marginal impact of participation in contract farming in the second column of tables A2 to A7 is consistently significant at the 1 percent level.

Table A8 shows that the impacts of contract farming on income volatility remain practically unchanged when changing the IV for participation in contract farming from WTP to WTW. More importantly, turning to the estimate ATEs of contract farming on the indicators retained for analysis in table A9, note that the sign and significance of the estimated ATEs do not change whether one uses respondent WTP (as in the paper) or WTW (as in this appendix) as an instrumental variable for participation in contract farming. And while the magnitude of the estimated ATEs does change between instruments, they do so only slightly. For example, the ATE for household income is equal to \$119.23 when using WTP and equal to \$134.54 when using WTW. This \$15.31 discrepancy is

very small considering that it represents only 1.3 percent of the average household's income. For other income measures, the discrepancies are equally small as a proportion of the average: 2.3 percent of household income per capita; 2.4 percent of household income per adult equivalent; and 1.8 percent of household income net of contract farming. In other words, both contingent valuation questions lead to estimated ATEs that are very similar, which indicates that the empirical results presented in the paper are robust to the choice of instrument.

A2. Robustness Check with Negative WTP Assigned to Naysayers

Table A10 shows the estimated ATEs using WTP as an IV for participation in contract farming (as in the paper) but assigning a value of -$20,000 as WTP for the naysayers instead of zero as was done in the paper (one of these findings is presented in footnote 12 in the paper). Once again, the estimated ATEs differ only very little from those presented in the paper, which indicates that the empirical results are robust to the assumption made regarding the lower bound on WTP of those respondents who would refuse to participate in the hypothetical contract farming agreement presented by the contingent valuation question used to elicit WTP. In other words, whether one assigns a value of zero or a value of -$20,000 as lower bound on WTP to the naysayers, one obtains estimated ATEs that are essentially similar, which indicates that the empirical results presented in the paper are robust to the assumption made on the WTP of naysayers.

Table A1. Estimation Results for the First Stage of the Treatment Regressions (n=1178)

	(1)			(2)		
Variable	**Coefficient**		**(Std. Err.)**	**Coefficient**		**(Std. Err.)**
	Excluding Income			**Including Income**		
Dependent Variable: = 1 if Participates in Contract Farming; = 0 Otherwise.						
Household Size	0.031		(0.021)	0.030		(0.021)
Dependency Ratio	-0.187		(0.209)	-0.177		(0.209)
Single	0.048		(0.203)	0.055		(0.203)
Female	-0.457	*	(0.236)	-0.455	*	(0.236)
Migrant	0.089		(0.137)	0.089		(0.137)
Age	-0.021	***	(0.007)	-0.021	***	(0.007)
Education	-0.002		(0.014)	-0.004		(0.015)
Experience	0.012	*	(0.007)	0.012	*	(0.007)
Member of Peasant Organization	0.528	***	(0.111)	0.527	***	(0.111)
Fady Days	-0.002		(0.002)	-0.002		(0.002)
Income				0.002		(0.002)
Working Capital	0.004		(0.004)	0.003		(0.004)
Assets	0.001		(0.002)	0.001		(0.002)
Landholdings	0.001	**	(0.000)	0.000	**	(0.000)
Nonparametric WTW for Contract Farming	0.106	**	(0.045)	0.104	**	(0.045)
Intercept	0.500	*	(0.269)	0.499	*	(0.269)
District Fixed Effects	Yes			Yes		
p-value (Joint Significance)	0.000			0.000		
Pseudo R-square	0.066			0.067		

Note: These estimation results correspond to equation 3 in the body of the paper. Estimation results are probability-weighted. The symbols ***, **, and * respectively denote significance at the 1, 5, and 10 percent levels.

Table A2. Treatment Regression and OLS Estimation Results for Household Income (n=1178)

	(1)			(2)		
Variable	**Coefficient**		**(Std. Err.)**	**Coefficient**		**(Std. Err.)**
	Treatment Regression					
	Dependent Variable: = 1 if Participates in Contract Farming; = 0 Otherwise			**Dependent Variable: Log of Income**		
Household Size	0.033		(0.021)	0.044	***	(0.016)
Dependency Ratio	-0.169		(0.212)	-0.083		(0.165)
Single	0.087		(0.205)	-0.161		(0.147)
Female	-0.480	**	(0.240)	-0.204		(0.175)
Migrant	0.092		(0.139)	0.004		(0.105)
Age	-0.020	***	(0.007)	0.011	**	(0.005)
Education	-0.003		(0.014)	0.069	***	(0.010)
Experience	0.010		(0.007)	-0.005		(0.004)
Member of Peasant Organization	0.501	***	(0.109)	0.002		(0.091)
Forbidden Days	-0.002		(0.002)	0.001		(0.001)
Working Capital	0.005		(0.005)	0.006	***	(0.002)
Assets	0.000		(0.003)	0.007	***	(0.002)
Landholdings	0.001	**	(0.000)	0.000		(0.000)
Contract Farming				1.170	***	(0.234)
Nonparametric WTW for Contract Farming	0.131	***	(0.040)			
Intercept	0.424		(0.266)	0.170		(0.251)
District Fixed Effects	Yes					
Log Pseudo-Likelihood	-1098.905					
p-value (Joint Significance of All Coefficients)	0.000					
p-value (Test of Independent Equations)	0.001					
R-square	-					

Note: The estimation results in column 2 correspond to equation 1 in the body of the paper. Estimation results are probability-weighted. The symbols ***, **, and * respectively denote significance at the 1, 5, and 10 percent levels. Results in the first column are marginal effects.

Table A3. Treatment Regression and OLS Estimation Results for Household Income Per Capita (n=1178)

	(1)			(2)		
Variable	**Coefficient**		**(Std. Err.)**	**Coefficient**		**(Std. Err.)**
	Treatment Regression					
	Dependent Variable: = 1 if Participates in Contract Farming; = 0 Otherwise			**Dependent Variable: Log of Income Per Capita**		
Household Size	0.034		(0.021)	-0.135	***	(0.016)
Dependency Ratio	-0.176		(0.213)	-0.292	*	(0.166)
Single	0.076		(0.209)	0.049		(0.153)
Female	-0.475	*	(0.244)	-0.354	**	(0.178)
Migrant	0.088		(0.139)	0.012		(0.103)
Age	-0.020	***	(0.007)	0.011	**	(0.005)
Education	-0.003		(0.015)	0.071	***	(0.010)
Experience	0.011		(0.007)	-0.004		(0.004)
Member of Peasant Organization	0.501	***	(0.109)	0.016		(0.088)
Forbidden Days	-0.002		(0.002)	0.001		(0.001)
Working Capital	0.005		(0.005)	0.007	***	(0.002)
Assets	0.000		(0.003)	0.006	***	(0.002)
Landholdings	0.001	**	(0.000)	0.000		(0.000)
Contract Farming				1.086	***	(0.233)
Nonparametric WTW for Contract Farming	0.129	***	(0.041)			
Intercept	0.430		(0.267)	-0.342		(0.251)
District Fixed Effects	Yes					
Log Pseudo-Likelihood	-1098.103					
p-value (Joint Significance)	0.000					
p-value (Test of Independent Equations)	0.003					
R-square	-					

Note: Estimation results are probability-weighted. The symbols ***, **, and * respectively denote significance at the 1, 5, and 10 percent levels. Results in the first column are marginal effects.

Table A4. Treatment Regression and OLS Estimation Results for Income Per Adult Equivalent (n=1178)

	(1)			(2)		
Variable	**Coefficient**		**(Std. Err.)**	**Coefficient**		**(Std. Err.)**
	Treatment Regression					
	Dependent Variable: = 1 if Participates in Contract Farming; = 0 Otherwise			**Dependent Variable: Log of Household Income Per Adult Equivalent**		
Household Size	0.033		(0.021)	-0.129	***	(0.016)
Dependency Ratio	-0.172		(0.213)	0.246		(0.163)
Single	0.077		(0.209)	0.051		(0.152)
Female	-0.476	*	(0.244)	-0.344	*	(0.178)
Migrant	0.088		(0.139)	0.018		(0.103)
Age	-0.020	***	(0.007)	0.009	**	(0.005)
Education	-0.003		(0.015)	0.071	***	(0.010)
Experience	0.011		(0.007)	-0.004		(0.004)
Member of Peasant Organization	0.502	***	(0.109)	0.022		(0.088)
Forbidden Days	-0.002		(0.002)	0.001		(0.001)
Working Capital	0.005		(0.005)	0.007	***	(0.002)
Assets	0.000		(0.003)	0.006	***	(0.002)
Landholdings	0.001	**	(0.000)	0.000		(0.000)
Contract Farming				1.083	***	(0.231)
Nonparametric WTW for Contract Farming	0.129	***	(0.041)			
Intercept	0.428		(0.267)	-0.304		(0.249)
District Fixed Effects	Yes					
Log Pseudo-Likelihood	-1097.384					
p-value (Joint Significance)	0.000					
p-value (Test of Independent Equations)	0.003					
R-square	-					

Note: Estimation results are probability-weighted. The symbols ***, **, and * respectively denote significance at the 1, 5, and 10 percent levels. Results in the first column are marginal effects.

Table A5. Treatment Regression and OLS Estimation Results for Household Income Net of Contract Farming Revenue (n=1178)

	(1)			(2)		
Variable	**Coefficient**		**(Std. Err.)**	**Coefficient**		**(Std. Err.)**
	Treatment Regression					
	Dependent Variable: = 1 if Participates in Contract Farming; = 0 Otherwise			**Dependent Variable: Log of Income Net of Contract Farming Revenue**		
Household Size	0.030		(0.021)	0.054	***	(0.017)
Dependency Ratio	-0.164		(0.210)	-0.033		(0.187)
Single	0.123		(0.223)	-0.248		(0.205)
Female	-0.514	**	(0.251)	-0.059		(0.229)
Migrant	0.095		(0.140)	-0.008		(0.123)
Age	-0.019	***	(0.007)	0.014	**	(0.006)
Education	-0.004		(0.014)	0.075	***	(0.011)
Experience	0.010		(0.007)	-0.005		(0.005)
Member of Peasant Organization	0.494	***	(0.111)	-0.068		(0.111)
Forbidden Days	-0.002		(0.002)	0.001		(0.001)
Working Capital	0.005		(0.005)	0.007	***	(0.002)
Assets	0.001		(0.003)	0.007	***	(0.002)
Landholdings	0.001	**	(0.000)	0.000		(0.000)
Contract Farming				1.029	***	(0.338)
Nonparametric WTW for Contract Farming	0.122	***	(0.038)			
Intercept	0.450	*	(0.266)	-0.206		(0.352)
District Fixed Effects	Yes					
Log Pseudo-Likelihood	-1155.163					
p-value (Joint Significance)	0.000					
p-value (Test of Independent Equations)	0.005					
R-square	-					

Note: Estimation results are probability-weighted. The symbols ***, **, and * respectively denote significance at the 1, 5, and 10 percent levels. Results in the first column are marginal effects.

Table A6. Treatment Regression and OLS Estimation Results for Hungry Season Duration (n=1178)

	(1)			(2)		
Variable	**Coefficient**		**(Std. Err.)**	**Coefficient**		**(Std. Err.)**
	Treatment Regression					
	Dependent Variable: = 1 if Participates in Contract Farming; = 0 Otherwise			**Dependent Variable: Duration of Household Hungry Season**		
Household Size	0.028		(0.020)	0.078	**	(0.037)
Dependency Ratio	-0.191		(0.209)	0.401		(0.404)
Single	0.069		(0.202)	-0.045		(0.377)
Female	-0.508	**	(0.234)	0.351		(0.435)
Migrant	0.052		(0.139)	0.094		(0.260)
Age	-0.020	***	(0.006)	0.005		(0.011)
Education	-0.002		(0.015)	-0.071	***	(0.026)
Experience	0.012	*	(0.006)	-0.021	**	(0.011)
Member of Peasant Organization	0.513	***	(0.109)	0.580	**	(0.236)
Forbidden Days	-0.002		(0.002)	-0.004		(0.003)
Income	0.004	**	(0.002)	-0.004	**	(0.002)
Working Capital	0.002		(0.003)	0.003		(0.003)
Assets	0.001		(0.003)	-0.012	***	(0.003)
Landholdings	0.000	**	(0.000)	0.000		(0.000)
Contract Farming				-2.540	***	(0.483)
Nonparametric WTW for Contract Farming	0.132	***	(0.039)			
Intercept	0.436		(0.266)	5.211	***	(0.608)
District Fixed Effects	Yes					
Log Pseudo-Likelihood	-1579.731					
p-value (Joint Significance)	0.000					
p-value (Test of Independent Equations)	0.000					
R-square	-					

Note: Estimation results are probability-weighted. The symbols ***, **, and * respectively denote significance at the 1, 5, and 10 percent levels.

Table A7. Treatment Regression and OLS Estimation Results for the Likelihood of Receiving a Formal Loan (n=1178)

	(1)			(2)		
Variable	**Coefficient**		**(Std. Err.)**	**Coefficient**		**(Std. Err.)**
	Treatment Regression					
	Dependent Variable: = 1 if Participates in Contract Farming; = 0 Otherwise			**Dependent Variable: = 1 if Household Has Received a Formal Loan; = 0 Otherwise**		
Household Size	0.033		(0.021)	0.000		(0.005)
Dependency Ratio	-0.164		(0.207)	-0.019		(0.047)
Single	0.079		(0.193)	-0.017		(0.041)
Female	-0.482	**	(0.231)	0.030		(0.051)
Migrant	0.114		(0.138)	0.003		(0.035)
Age	-0.018	***	(0.007)	0.004	**	(0.002)
Education	-0.008		(0.015)	0.014	***	(0.004)
Experience	0.009		(0.007)	-0.003	*	(0.002)
Member of Peasant Organization	0.537	***	(0.111)	0.029		(0.030)
Forbidden Days	-0.002		(0.002)	0.000		(0.000)
Income	0.005	**	(0.002)	0.001		(0.001)
Working Capital	0.005		(0.005)	-0.002	**	(0.001)
Assets	0.000		(0.002)	-0.001		(0.000)
Landholdings	0.000	*	(0.000)	0.000	***	(0.000)
Contract Farming				0.324	***	(0.063)
Nonparametric WTW for Contract Farming	0.081	*	(0.043)			
Intercept	0.470	**	(0.267)	-0.323	***	(0.080)
District Fixed Effects	Yes					
Log Pseudo-Likelihood	-461.067					
p-value (Joint Significance)	0.000					
p-value (Test of Independent Equations)	0.000					
R-square	-					

Note: Estimation results are probability-weighted. The symbols ***, **, and * respectively denote significance at the 1, 5, and 10 percent levels.

Table A8. Results of Tests of Group-Wise Heteroskedasticity (n=1178)

Null Hypothesis of Homoskedasticity (Equal Error Variance between Participation Regimes)	Test Result	Change in Volatility Due to Contract Farming
Total Income (Table A2)	Rejected	-0.150***
Total Income Per Capita (Table A3)	Rejected	-0.153***
Total Income Per Adult Equivalent (Table A4)	Rejected	-0.154***
Total Income Net of CF Revenue (Table A5)	Not Rejected	0.025

Note: The symbols ***, **, and * respectively denote significance at the 1, 5, and 10 percent levels. These results are computed on the basis of tables A2 to A5, i.e., for treatment regressions using WTW as an IV for participation in contract farming. In each row, the squared residuals from each relevant regression in tables 6 and 7 were regressed on a constant and a dummy equal to one if a household participates in contract farming and equal to zero otherwise. The second column reports the result of a t-test of the null hypothesis of homoskedasticity, i.e., the null hypothesis that the coefficient on the contract farming dummy is equal to zero. A rejection of the null hypothesis indicates that the volatility of the dependent variable is different between participants and nonparticipants. The third column reports the percentage change in volatility due to contract farming as well as whether and at what level this change is significant.

Table A9. Synthesis of Estimated ATEs on Welfare Outcomes Using WTW as an IV for Participation in Contract Farming (n=1178)

Variable	ATE		(Std. Err.)
Income (US$)	134.54	***	(2.111)
Income Per Capita (US$)	35.50	***	(1.949)
Income Per Adult Equivalent (US$)	49.05	***	(1.904)
Income Net of Contract Farming Revenue (US$)	110.00	***	(1.954)
Duration of Hungry Season (Months)	-2.54	***	(0.483)
Likelihood of Receiving a Formal Loan (%)	0.32	***	(0.063)

Note: The symbols ***, **, and * respectively denote significance at the 1, 5, and 10 percent levels.

Table A10. Synthesis of Estimated ATEs on Welfare Outcomes Using WTP as an IV for Participation in Contract Farming but Assigning a WTP of -$20,000 to Naysayers (n=1178)

Variable	ATE		(Std. Err.)
Income (US$)	132.73	***	(2.083)
Income Per Capita (US$)	34.40	***	(1.889)
Income Per Adult Equivalent (US$)	47.90	***	(1.859)
Income Net of Contract Farming Revenue (US$)	108.48	***	(1.927)
Duration of Hungry Season (Months)	-1.79	**	(0.483)
Likelihood of Receiving a Formal Loan (%)	0.30	***	(0.063)

Note: The symbols ***, **, and * respectively denote significance at the 1, 5, and 10 percent levels. As per footnote 11 in the paper, these results were estimate using WTP as an IV for participation in contract farming but assigning a WTP of –$20,000 instead of zero to naysayers.