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

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Summary. — Contract farming is widely perceived as a means of increasing welfare in developing countries. Because of smallholder self-selection in contract farming, however, it is not clear whether contract farming actually increases grower welfare. In an effort to improve upon existing estimates of the welfare impacts of contract farming, this paper uses the results of a contingent-valuation experiment to control for unobserved heterogeneity among smallholders. Using data across several regions, firms, and crops in Madagascar, results indicate that a 1-percent increase in the likelihood of participating in contract farming is associated with a 0.5-percent increase in household income, among other positive impacts.

Key words — contract farming, welfare, grower–processor contracts, outgrower schemes, Africa, Madagascar

1. INTRODUCTION

With rising incomes and falling trade barriers over the past 60 years, consumers throughout the industrialized world have increasingly come to value food diversity and food availability. This may explain 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. Similarly, with rising incomes throughout the developing world, supermarkets are playing a role of increasing importance in providing developing-country consumers' 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 in order to ensure that they have access to a stable supply of commodities that satisfy specific quality requirements (Reardon, Timmer, Barrett, & Berdegué, 2003). Consequently, contract farming—the economic institution wherein a processing firm and a grower enter into a contract in which the firm delegates the production of agricultural commodities to the grower—is playing an increasingly important role in developing countries.

Moreover, the institution of contract farming is expected to play an even more important role in developing countries in the future. Indeed, 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). With the advent and growing popularity of Fair Trade commodities in industrialized countries over the last decade, industrialized-country consumers have been increasingly linked to developing-country producers; 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). And if the US experience offers any guidance as to what the future has in store for developing countries, 36% of the crops and livestock produced in the US are produced under contract, with estimates ranging from 21% for cattle to almost 90% for poultry (IATP, 2010).

But what is the impact of participating in contract farming on the welfare of the growers involved, who are more often than not smallholders? Much has been written on smallholder market participation (Barrett, 2008; Bellemare & Barrett, 2006), on agricultural supply chains (Reardon & Timmer, 2005) and on contract farming (Barrett *et al.*, 2012; Bijman, 2008; Grosh, 1994), but even though contract farming can potentially improve the welfare of the individuals and households involved, little is known about the actual—as opposed to potential; that is, empirical instead of theoretical—welfare impacts of contract farming on the households that choose to participate as growers.

Intuition suggests that contract farming should, at a minimum, 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. But given that individuals and households select into contract farming on the basis of factors that are typically unobserved, making a definitive causal statement about the welfare impacts of contract farming has so far proven elusive. This may explain why contract farming is alternatively viewed by some as a means of fostering economic development by resolving several market failures (Grosh, 1994)

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and of improving welfare (Maertens & Swinnen, 2009; Minten, Randrianarison, & Swinnen, 2009; Miyata, Minot, & Hu, 2009; Rao & Qaim, in press; Simmons, Winters, & Patrick, 2005; Warning & Key, 2002), and by others as a means of labor exploitation for capitalists (Clapp, 1994; Glover & Kusterer, 1990; Porter & Phillips-Howard, 1997; Watts, 1994).[2]

The contribution of this paper to the literature is twofold. The first contribution of this paper is to study the institution of contract farming across six regions that differ along their agro-ecological conditions, across more than seven processing firms that differ along their contracting practices, and across more than ten crops that differ along their production processes and along the value they can fetch on the market. Indeed, studies of the welfare impacts of contract farming often focus only on one region, on the contracts signed by one firm, or on a single crop.[3] In this sense, this paper aims to make a more general statement about the impacts of the institution of contract farming in a given country.

More importantly, the second contribution of this paper lies in the way this paper attempts to identify the causal impact of contract farming on welfare. Indeed, because participation in contract farming is not randomly distributed across households, and because households select into participating in contract farming on the basis of factors that are unobservable, 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.[4]

The identification strategy used in this paper exploits the answer of each respondent, whether he participates in contract farming or not, to a hypothetical question about contract farming. Each respondent was asked whether he would be willing to participate in a hypothetical contract farming agreement that promised to increase his welfare, but which required a significant initial investment. The value of that initial investment was randomly generated in the field by the throw of a die. The IVs used in an attempt to make a causal statement about the impact of contract farming in this context are thus a set of dummy variables that interact each respondent's dichotomous-choice answer (i.e., "Yes" or "No") to the hypothetical question with the "treatment" (i.e., the specific value of the initial investment that was randomly drawn for him from six such potential values) he was assigned to. This contingent valuation setup makes for a vector of IVs that allows controlling for the willingness to pay (WTP) of respondents to participate in contract farming.[5]

Because WTP captures variations among respondents' respective marginal utilities derived from participation in contract farming, it controls for the various sources of unobserved heterogeneity among respondents, such as various subjective perceptions, risk preferences, entrepreneurship, technical ability, time preferences, etc., all of which affect preferences over—and thus the marginal utility of respondents from participating in—contract farming. Put differently, these (typically) unobservable variations in preferences are captured here by variations in WTP among respondents. This is not to say that the identification strategy used in this paper is perfect,[6] but it allows reducing the scope of the statistical endogeneity problem usually encountered in such studies by reducing the amount of bias that one must contend with when trying to assess the welfare impacts of participation in contract farming.[7] In this case, the IVs mitigate the bias introduced by the omission of variables such as entrepreneurial ability, risk preferences, technical ability, time preferences, etc. In other words, the IVs reduce the scope of the unobserved heterogeneity problem, and thus should constitute an improvement on previous methods of identifying the causal impacts of participation in contract farming.

Previous studies have instrumented participation in contract farming using a measure of respondent trustworthiness (Warning & Key, 2002); the number of 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 & Qaim, in press).[8] In all those cases, however, it is not obvious how the IVs used allow reducing the scope of the statistical endogeneity problem, or even how they allow mitigating unobserved heterogeneity. Similarly, Minten *et al.* (2009) only observe households who participate in contract farming, and so they must rely on respondent subjective perceptions of the welfare impacts of participation in contract farming. Likewise, although Singh's (2002) sample includes both participants and nonparticipants in contract farming, his test consists in between-group comparisons of the means of selected indicators. This calls into question the estimated welfare impacts of contract farming in the extant literature and, in the limit, it could even mean that whether participation in contract farming increases welfare is still uncertain, as the bias could be so severe as to lead to estimates that are of the wrong sign. Again, that the IVs used in this study are also imperfect, but they should represent a step in the right direction as far as the identification of the causal impacts of contract farming on welfare goes given that the IVs used in this paper allow controlling for a great deal of unobserved heterogeneity between respondents.

The empirical results in this paper indicate that a 1-percent increase in the likelihood of participating in contract farming entails a 0.6-percent increase in household income of and a 0.5-percent increase in household income per adult equivalent. Likewise, a 1-percent increase in the likelihood of participating in contract farming entails a 0.5-percent increase in household income net of contract farming revenue, which suggests that the institution of contract farming has spillovers on other income sources. Moreover, a comparison of the IV approach with the naïve ordinary least squares (OLS) approach (which implicitly assumes that participation in contract farming is randomly distributed, and is therefore exogenous to welfare) underscores the policy importance of attempting to identify the causal relationship between contract farming and welfare. Lastly, a comparison of the IV approach with the propensity score matching (PSM) method reveals that the latter method considerably overstates the welfare impact of contract farming relative to the former, as it suggests that a 1-percent increase in the likelihood of participating in contract farming entails a 3-percent increase in household income.

The remainder of this paper is organized as follows. In Section 2, the data are presented, along with descriptive statistics. Section 3 presents the empirical framework and the identification strategy used in this paper. In Section 4, empirical results are presented and discussed. 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 a commune census conducted in 2007 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. See Moser (2008) for a methodological discussion of the 2007 commune census.

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, as this information was available in the

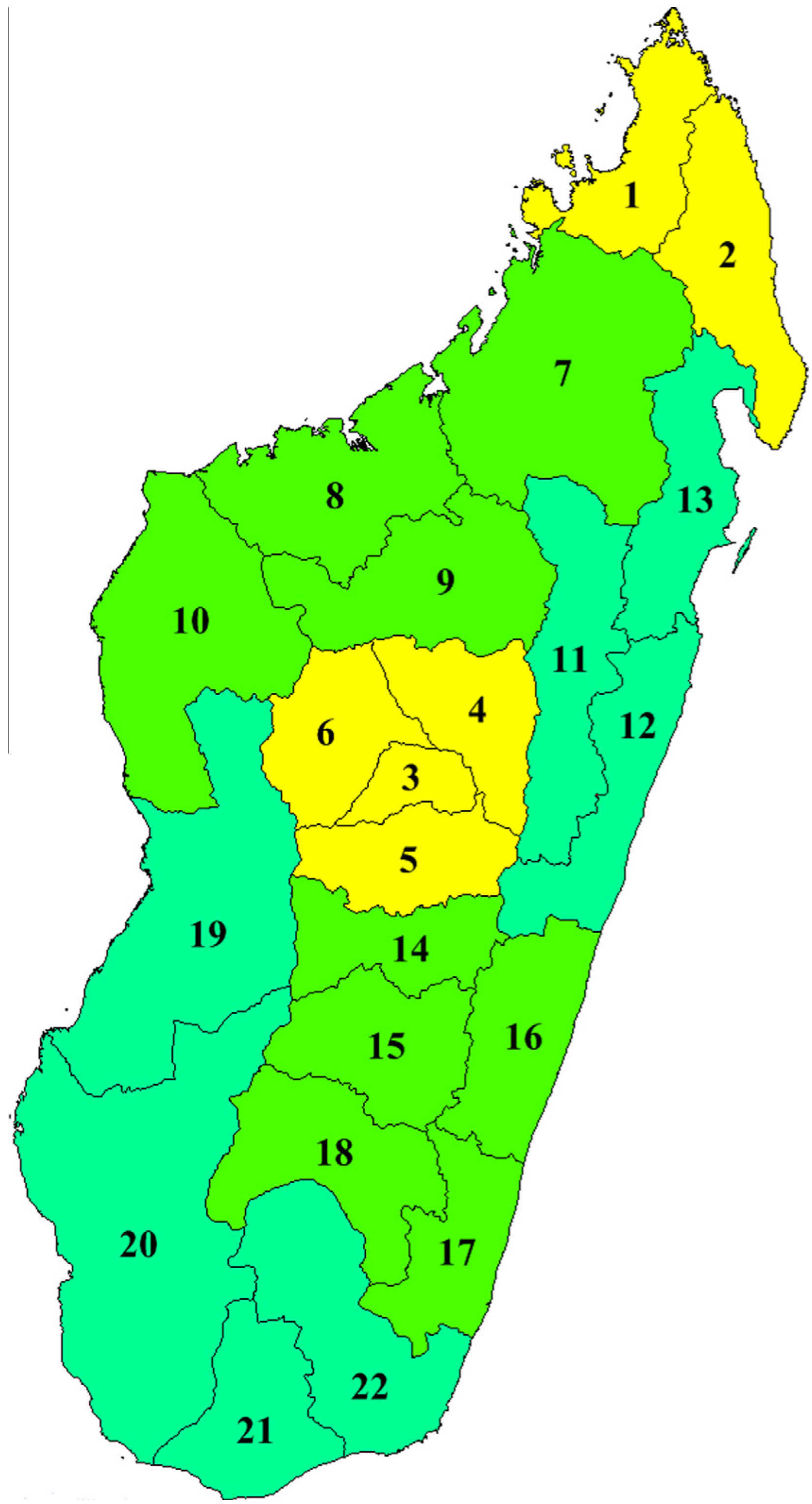

Figure 1. *Map of Madagascar. Numbers denote regions and colors denote provinces. (Source: Per Johansson/Wikimedia Commons.) (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)*

commune census data. Finally, within each of the 12 communes, 50 households were selected at random from a list of households who participated in contract farming, and 50 households were selected at random from a list of households who did not participate in contract farming. For each household, data were collected at the household, plot, crop, and, when applicable, contract levels. The data thus consist of about 1200 households, half of which are participants in contract farming. Given that the survey was conducted in rural areas, almost all of the households in the sample are agricultural households, with only 4% of households not deriving any income from agriculture. Given the sampling scheme, probability weights are used to bring the sample as close as possible to a random sample throughout the paper. The use of probability weights further implies that all the standard errors in this paper are robust to heteroscedasticity. The sampling weights rely on the sample and population proportions of contract farmers. The sample proportions can be calculated on the basis of descriptive statistics, whereas the population proportions were collected in the context of a village-level questionnaire which was filled out at the time of the household survey with a village-level focus group that was usually led by the village chief. Table 1 synthesizes the six regions and 12 communes included in the data as well as the main contracted crops in each commune.

Although the unit of analysis in this paper is the household, a brief discussion of what the contract farming arrangements in the data look like is in order. Tables 2a and 2b presents descriptive statistics for the terms of the contract farming arrangements observed in the data. The data set covers 1301 contracts. Given that about half of the 1200 households in the data enter contract farming, this means that the average contract farming household in the data enters more than one contract farming agreement. The bulk of those contracts are signed with Lecofruit, a firm that specializes in the processing and exporting of green vegetables (green beans, snow peas, and leeks), whose contracting activities are located in the central highlands of Madagascar, and which was the focus of studies by Minten *et al.* (2009) and Bellemare (2010). The firms Silac and Malto, which respectively specialize in rice in the Lac Alaotra region and in barley, are practically tied behind Lecofruit in second position as far as the number of contracts in the data goes.

There is a good amount of variation in the proportion of processor-provided inputs among the processing firms observed in the data, ranging from no processor-provided inputs (e.g., Sodexo provides no inputs to its contracting partners) to almost 100% processor-provided inputs (e.g., Lecofruit provides its contracting partners with almost the entirety of the seeds, pesticides, and fertilizer they use). Save for the contracts signed by Sodexo, the vast majority of contracts in the data were signed between the processor and the individual grower. Likewise, the vast majority of contracts in the data were formal, i.e., either written or written and notarized. Once again, Sodexo proves to be the exception, with nothing but verbal agreements. Lastly, the price to be paid to the grower by the processor for the contracted crop is determined *ex ante* as part of the contract in almost all cases, with Sodexo being the exception as it pays its growers a price determined *ex post* as a function of the realized price on the local market.[9]

The relevant unit of analysis in this paper is the household, so Table 3 presents descriptive statistics at the household level. Although the sample was designed so as to have half of the households participating in contract farming and half not 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% are participants in contract farming.

The average household in the data is composed of 5.7 individuals, almost half of whom are dependents.[10] The majority of households in the sample are headed by a male, with only 8% 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 are headed by someone who was born outside 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.[11]

In terms of welfare, total annual household income is on average equal to US$1153, a figure which drops down to US$1050 when excluding income from contract farming.[12, 13] A naïve, back-of-the-envelope calculation (which evidently ignores the fact that participation in contract farming is nonrandom, but which is included here for comparison) would thus suggest that the average contract farming participant household derives an extra $103 per year from its participation in contract farming, or just about a 10% increase in income. In a country where the nominal GDP per capita was of US$468 the year the data were collected, this means that (i) the households in the sample, with an income per capita of US$219, are significantly poorer than the national average, which is not surprising given that the households in the sample are rural; and (ii) the difference in mean income between participants in contract farming and nonparticipants appears *prima facie* nontrivial (IMF, 2009). Similarly, total income per adult equivalent within the household is equal to US$271.[14]

In terms of assets, the average household owns US$325 in working capital (i.e., plow, cart, weeder, harrow, tractor, and other agricultural equipment), $716 in other assets (i.e., television, radio, bicycle, cattle, pigs, sheep, goats, poultry, jewelry, businesses, bank account balance, and landholdings) and as far as landholdings go, the average household owns 1.7 hectares of land in total. Anticipating on the IVs, Table 3 also presents descriptive statistics for the respondents' responses to the contingent valuation question used as a vector of IVs in this paper. Note that with the exception of a spike at US$25, the percentage of "Yes" responses is almost perfectly monotonically decreasing in the size of the "treatment," i.e.,

Table 1. *Regions, communes, and crops*

Region	Commune	Primary crop
Alaotra Mangoro (11)	Bejofo	Rice
	Feramanga North	Rice
Analamanga (4)	Amboasary North	Rice
	Mangamila	Rice
Anosy (22)	Ebelo	Rice
	Andranobory	Maize
Diana (1)	Ambodibonara	Cotton
	Anketrakabe	Rice
Itasy (3)	Miarinarivo I	Green beans
	Soavinandriana	Green beans
Vakinankaratra (5)	Morarano	Rice
	Betafo	Barley

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

Table 2a. *Descriptive statistics on the terms of the contract (n = 1178)*

Firm	Percentage of cases the firm provides inputs				Contracted crops (%)
	Seeds	Pesticides	Chemical fertilizer	Organic fertilizer	
Hasyma ($n = 59$)	1.00	0.97	0.51	0.00	Cotton (1.00)
Lecofruit ($n = 613$)	0.98	0.98	0.98	0.78	Green beans (0.94), snow peas (0.04), leeks (0.02)
Malto ($n = 192$)	0.97	0.95	0.98	0.02	Barley (1.00)
Silac ($n = 199$)	0.05	0.09	0.74	0.00	Rice (1.00)
Sodexo ($n = 11$)	0.00	0.00	0.00	0.00	Leafy greens (0.27), tomatoes (0.18), other (0.55)
Star ($n = 53$)	0.00	0.00	0.00	0.00	Maize (1.00)
Tiko ($n = 4$)	0.00	0.00	0.00	0.00	Oats (1.00)
Other ($n = 170$)	0.32	1.00	0.02	0.00	Tobacco (0.31), green beans (0.25), other (0.44)

Table 2b. *Descriptive statistics on the terms of the contract (n = 1178)*

Firm	Type of contract		Contract form			Price	
	Group	Individual	Verbal	Written	Notarized	Fixed	Floating
Hasyma ($n = 59$)	0.00	1.00	0.00	0.98	0.02	0.95	0.05
Lecofruit ($n = 613$)	0.01	0.99	0.01	0.93	0.06	0.98	0.02
Malto ($n = 192$)	0.07	0.93	0.02	0.79	0.19	0.96	0.04
Silac ($n = 199$)	0.33	0.67	0.06	0.20	0.74	0.94	0.06
Sodexo ($n = 11$)	1.00	0.00	1.00	0.00	0.00	0.00	1.00
Star ($n = 53$)	0.23	0.77	0.00	0.60	0.40	1.00	0.00
Tiko ($n = 4$)	0.00	1.00	0.00	0.00	1.00	1.00	0.00
Other ($n = 170$)	0.01	0.99	0.01	0.97	0.02	0.68	0.32

Table 3. *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)
Fady days	25.110	(1.030)
Household welfare and financial characteristics		
Income (100,000 Ariary)	23.058	(1.583)
Income per adult equivalent (100,000 Ariary)	5.421	(0.330)
Income net of contract farming (100,000 Ariary)	21.006	(1.377)
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		
"Yes" to hypothetical contract with \$12.5 investment	0.131	(0.011)
"Yes" to hypothetical contract with \$25 investment	0.180	(0.013)
"Yes" to hypothetical contract with \$37.5 investment	0.157	(0.012)
"Yes" to hypothetical contract with \$50 investment	0.133	(0.011)
"Yes" to hypothetical contract with \$62.5 investment	0.068	(0.008)
"Yes" to hypothetical contract with \$75 investment	0.065	(0.008)
"No" to hypothetical contract, all investment values	0.266	(0.015)

Note: See Section 3 for a discussion of how the contingent valuation question was designed.

the random bid, or the initial investment that the contingent-valuation question would require to participate in the hypothetical contract.

Table 4 then 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 (without implying any causal relationship, of course) that the households who participate in contract farming report a significantly higher income, income per adult equivalent, and income net of contract farming revenue; 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.

3. EMPIRICAL FRAMEWORK

The core equation to be estimated in this paper is

$$y_i = \alpha_1 + \beta_1 x_i + \gamma_1 w_i + \delta_1 d_j + \varepsilon_{ij}, \tag{1}$$

where the unit of observation is household i in region j, y_i is an indicator of welfare (i.e., income, income per adult equivalent, and income net of contract farming revenue in this paper); x_i is a vector of household characteristics; w_i is a dummy equal to one if household i participates in contract farming and equal to zero otherwise; d_j is a vector of regional dummies; and ε_{ij} is an error term with mean zero.

Before anything else, note that Eqn. (1) cannot and thus does not control for the crop grown by the household, because the unit of analysis is the household rather than the plot. The fact that most contract farming 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 to a considerable extent be explained by regional differences (see Table 1), the variation in crops is largely controlled for by regional dummies in the context of this paper.

That being said, the goal of this paper is to estimate γ, which represents the impact of participation in contract farming on household welfare, as accurately as possible. In this sense, γ should in principle allow computing the average treatment effect of contract farming (ATE; see Wooldridge, 2002, chapter 18), which is such that

$$ATE = E(y_1 - y_0), \tag{2}$$

where y_1 is household welfare if a household participates in contract farming and y_0 is household welfare if the same household does not participate in contract farming. The problem posed by estimating the ATE is thus similar to a missing data problem: data are missing on y_0 for the households who participate in contract farming, and data are missing on y_1 for the households who do not participate in contract farming. Again, the fact that participation in contract farming is not randomly distributed across households and that it is driven by unobservable factors means that direct estimation of Eqn. (1) would lead to a biased estimate of the ATE. In other words, participation in contract farming is endogenous to household welfare.

In an effort to obviate this endogeneity problem and identify the causal impact of contract farming on household welfare, this paper uses a vector z_i of IVs, i.e., a vector of variables that are both (i) correlated with participation in contract farming and (ii) plausibly exogenous to the welfare outcome of interest. Indeed, because participation in contract farming is not randomly distributed across households, the equation

$$w_i = \alpha_2 + \beta_2 x + i + \varphi_2 z_i + \delta_2 d_j + v_{ij}, \tag{3}$$

Table 4. *Descriptive statistics by participation regime (n = 1178)*

Variable	Does not participate in contract farming (n = 599)		Participates in contract farming (n = 579)		Difference
	Mean	(Std. Err.)	Mean	(Std. Err.)	
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 adult equivalent	4.285	(0.310)	6.597	(0.587)	***
Income net of contract farming	17.335	(1.340)	24.805	(2.427)	***
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)	***

Note: 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.
* Difference in means that is significant at the 10% levels.
** Difference in means that is significant at the 5% levels.
*** Difference in means that is significant at the 1% levels.

is first estimated as a probit and used to obtain $\hat{G}_i$, the vector of predicted probabilities (i.e., the predicted value of w_i) obtained from estimating Eqn. (3). Eqn. (1) can then be estimated using $\hat{G}_i, x_i$, and d_j as instruments for w_i. See procedure 18.1 in Wooldridge (2002), for a discussion. This estimation procedure is more efficient than the usual two-stage least squares setup but, as a result, it requires that more assumptions be satisfied. See Appendix A for a discussion of these assumptions, and of how they are satisfied in this context. The next section discusses the identification strategy adopted in this paper.

(a) *Identification strategy*

As is often the case in the social sciences, the identification of a causal impact 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 the estimated ATE from contract farming obtained from a naïve estimation of Eqn. (1). For example, because the contract farming arrangements in the data almost always insure growers against price risk via the use of fixed prices (see Table 2b below and Grosh, 1994 for a discussion), it is likely that participation in contract farming is driven by the respondent's attitude toward price fluctuations. 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 Eqn. (1), nothing guarantees that the error term would not be correlated with that proxy, which could bias the estimate of γ. Alternatively, it is likely 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 interacting each respondent's dichotomous choice response (i.e., "Yes" or "No") to a contingent valuation question (Arrow *et al.*, 1993; Mitchell & Carson, 1989) with the random bid generated as part of the same question. 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. Each respondent is thus assigned to a single treatment (i.e., a single value of the initial investment) and can either say "Yes" or say "No" to the hypothetical contract farming arrangement within that assigned treatment.

For example, a respondent who is offered a $25 initial investment and who says "Yes" to it is coded as a one in the "Yes to $25" category and as a zero in other categories at the bottom of Table 3. A respondent who is offered a $62 initial investment and who says "No" is coded as a one in the "No to All Investment Values" categories and as a zero in other categories at the bottom of Table 3. It is these variables that constitute the vector of instrumental variables in this paper, with "No to All Investment Values" being the omitted category. This contingent valuation setup ensures that a respondent's WTP for contract farming is proxied for.

Recall briefly the three possible sources of statistical endogeneity, which are (i) systematic measurement error in the variable of interest w; (ii) omitted variables, or unobserved heterogeneity among units of observation i; and (iii) simultaneity or reverse causality between the dependent variable y and the variable of interest w. Most analyses implicitly rule (i) out. This study is no exception given that if it is highly unlikely that respondents misreport whether they participate in contract farming or not, it is even more unlikely that they do so systematically. Compared to the IVs used in previous studies, the relative strengths of the IVs used in this paper are that they first allow dealing with (ii) above relatively well given that controlling for WTP allows controlling for anything that affects respondents' preferences for the institution of contract farming (e.g., entrepreneurial ability, risk preferences, technical ability, time preferences, etc.) As a consequence, the IVs only affect income through participation in contract farming, as they control for respondent preferences.

The weakness of the set of IVs used in this study is that it might not successfully account for (iii) given the design of the contingent valuation question used to elicit WTP, a weakness that is also shared with the IVs in the extant literature discussed in Section 1. This means that the identification strategy used in this paper is not perfect, but the discussion and statistical tests below suggest that one should not be overly concerned with simultaneity or reverse causality—whether reverse causality from y to WTP or reverse causality from y to w (i.e., cognitive dissonance)—in this context. Consequently, while the set of IVs used in this paper do not allow one to completely eliminate the bias in the estimated welfare impacts of contract farming due to smallholder selection in the institution, they represent a step in the right direction as far as the identification of the causal relationship from participation in contract farming to welfare goes. What follows is a more detailed discussion of the strengths and weaknesses of the IVs used in this paper.

(i) *Unobserved heterogeneity or omitted variables*

Respondent WTP is proxied for via a nonparametric lower bound on each respondent's WTP, which is controlled for 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 the random bid r_i they face 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. Naysayers are, for their part, all assigned to the omitted category, and so they are included in the intercept of Eqn. (3). Compared to the usual parametric methods for estimating WTP (see, for example, Cameron & James, 1987), which make distributional assumptions, this nonparametric method only assumes that respondents are individually rational.

Some may object to using a variable that only provides a lower bound on WTP rather than WTP itself. But since the responses to the contingent valuation question are used here as an instrumental variable rather than as an actual precise measure of WTP, what matters in this context is the variation between respondents, which is used here to identify the impact of participation in contract farming, rather than the precise value of WTP. Readers who worry the use of a proxy for (instead of a more precisely estimated measure of) WTP should note that robustness checks were conducted during preliminary work which estimated respondent WTP parametrically using Cameron and James' (1987) method rather than nonparametrically using the method in this paper. The estimates obtained using parametric WTP as an IV rather than the set of IVs used in this paper yielded welfare impacts that were almost exactly the same as the ones in this paper. Those estimates are not reported here, but they are available from the author upon request.

The fact that the vector of IVs proxies for respondent WTP to participate in contract farming means that the instruments control for much of the unobserved heterogeneity between respondents because WTP 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 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. This difference in risk 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. This difference is also captured 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. Once again, this difference is 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. Lastly, the fact that the IVs allow controlling for respondent preferences also means that the IVs only affect income via the variable of interest (i.e., participation in contract farming) given that they control for the aforementioned unobservable respondent characteristics.

(ii) *Simultaneity or reverse causality*

Some may object that the IVs used in this paper suffer from reverse causality when studying the impact of contract farming on income 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%, and because income differs between respondents, a respondent's response to the contingent valuation question may depend on his income.

Bearing in mind that not rejecting a null hypothesis is not as powerful as rejecting it, this is something one can test for, however, by computing coefficients of correlation between each variable in the vector of IVs and income and by testing that these coefficients are not significantly different from zero. Of the seven variables at the bottom of Table 3, four are positively correlated with income and three are negatively correlated with income, which suggest that there is no systematic pattern between income and WTP for contract farming. More importantly, none of those seven correlation coefficients is significantly different from zero at any of the conventional levels of significance.

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 is higher may wish to "consume" more or less participation in contract farming. Again, this is a matter of different preferences for (i.e., marginal utilities derived from) contract farming between respondents, and these differences in preferences (i.e., marginal utilities) are accounted for by differences in WTP. Moreover, the tests above indicate that there does not appear to be a systematic relationship between the vector of IVs and income, as the dummies alternate in signs.

Lastly, one could also object that a respondent's actual participation in contract farming could affect his or 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 cases 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).

(b) *Grower selection or firm discrimination?*

Even though the identification strategy 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 in nonrandom ways 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 Eqn. (1) even when controlling for household selection into contract farming using the identification strategy discussed 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 is correlated with the covariates on the right-hand side of Eqn. (1), the estimated coefficients in Eqn. (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 highly unlikely that firms choose their growers on the basis of unobservables such as entrepreneurial ability, risk preferences, technical ability, entrepreneurial or time preferences. Even in cases where firms effectively exploit decentralized local knowledge by asking growers with whom they have contracted in the past to recommend new growers, firms still discriminate on the basis of observables. Given the richness of the data used in this paper, the implicit assumption is thus that the researcher has access to more information on observables than any given firm does. The inclusion of a vector of household characteristics x_i on the right-hand side of Eqn. (1) should thus control for the way firms discriminate between potential growers, since that vector encompasses more information than what processing firms have access to. Lastly, even if one were to assume that firms discriminate between growers on the basis of unobservables, the identification strategy should obviate such concerns given that it allows eliminating most, if not all, of the unobserved heterogeneity between growers.

4. ESTIMATION RESULTS AND DISCUSSION

This section first presents preliminary results which take a nonparametric look at whether contract farming increases welfare by comparing kernel density estimates for the welfare measures selected for analysis for both the households that participate in contract farming and the households that do not participate in contract farming. Because these preliminary results fail to control for confounding factors and are only suggestive, estimation results for the treatment regressions discussed in Section 3 follow the preliminary evidence and constitutes the bulk of this section. Lastly, the magnitude of the welfare impacts estimated below is discussed in relation to that estimated in other studies.

(a) *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 appears to have a positive impact on the welfare of the households involved by looking at the problem nonparametrically. This is done here by comparing kernel density estimates between participants and nonparticipants for the welfare measures retained for analysis.

Figures 2 to 4 thus plot kernel density estimates by participation status for household income, household income per adult equivalent, and household income net of revenues from contract farming. Figures 2 and 3 suggest that total household income and household income per adult equivalent are both higher for participants, but Figure 4 suggests that there are no spillovers from participation in contract farming to other income categories. That is the income and income per adult equivalent of households who participate in contract farming appear higher than the income and income per adult equivalent of households who do not participate in contract farming, but income net of contract farming revenue does not appear to vary systematically between participation regimes.

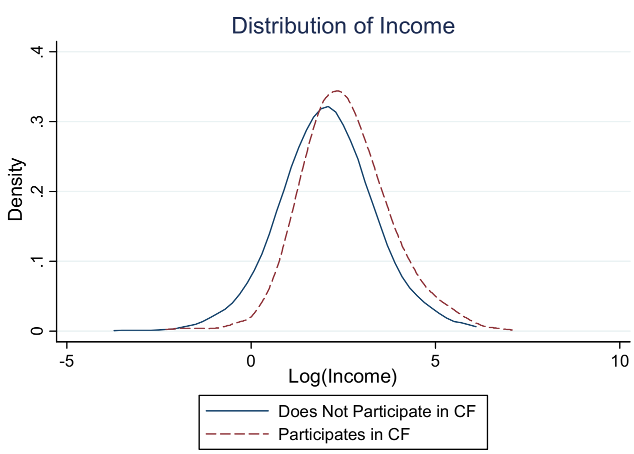

Figure 2. *Kernel density estimation of household income by participation regime with Epanechnikov kernel and bandwidth set equal to 0.5.*

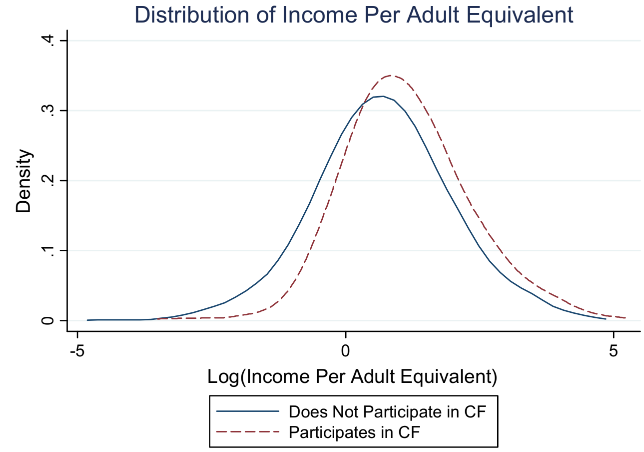

Figure 3. *Kernel density estimation of household income per adult equivalent by participation regime with Epanechnikov kernel and bandwidth set equal to 0.5.*

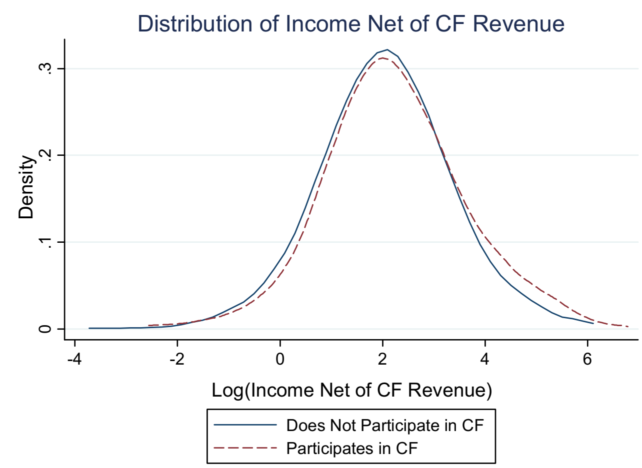

Figure 4. *Kernel density estimation of household income net of contract farming revenue by participation regime with Epanechnikov kernel and bandwidth set equal to 0.5.*

(b) *Parametric evidence*

The foregoing figures are helpful if one is interested in determining whether participation in contract farming is correlated with differences in various welfare indicators, but they cannot help one say anything about causality. To attempt to provide an answer to the question of whether participation in contract farming systematically might increase 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 the vector of IVs (z_i), household characteristics (x_i), and district fixed effects (d_j), i.e., Eqn. (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 Eqn. (1), the second stage of which consists in regressing y_i on $\hat{w}_i$ (i.e., the predicted value of w_i obtained as part of the treatment regression framework), x_i and d_j.

There are thus three equations estimated for each treatment regression: one equation for Eqn. (3), which remains common to all treatment regressions and is presented in Table 5; and two equations—the instrumenting regression and the regression of interest—for Eqn. (1), as per the method outlined in Wooldridge (2002), which are presented in Tables 6–8. Tables 6–8 also present naïve versions of the welfare equation (Eqn. (1)), i.e., versions of the welfare equation in which participation is not instrumented.

Table 5 presents estimation results for a probit regression of a dummy for actual participation in contract farming regressed on the vector of interactions between the contingent-valuation "treatment" and respondents' responses to the contingent valuation question (i.e., the variables at the bottom of Table 4, or the vector of IVs), the characteristics of his household, and district fixed effects.

The dummy variables in the vector of IVs are all statistically significantly different from zero. In addition to their individual statistical significance, the F-statistic for a test of their joint significance is equal to 24.55, which obviates concerns about weak instruments (Stock and Yogo, 2002). Moreover, the marginal impacts of the treatments are almost perfectly monotonic. That is, as respondent WTP to participate in contract farming increases, the likelihood that the respondent actually participates also increases, with the exception of a dip due to chance in the \$62.5 category.

Table 5. *Probit estimation results for the first stage of the treatment regressions*

Variable	Marginal effect	(Std. Err.)
Dependent variable: = 1 if participates in contract farming; = 0 otherwise		
Household size	0.025	(0.021)
Dependency ratio	−0.132	(0.214)
Single	0.068	(0.201)
Female	−0.449*	(0.236)
Migrant	0.066	(0.138)
Age	−0.021***	(0.007)
Education	−0.005	(0.014)
Experience	0.013*	(0.007)
Member of peasant organization	0.546***	(0.110)
Fady days	−0.003*	(0.002)
Working capital	0.005	(0.004)
Assets	0.002	(0.002)
Landholdings	0.001**	(0.000)
"Yes" to \$12.5 investment	0.382***	(0.148)
"Yes" to \$25 investment	0.406***	(0.140)
"Yes" to \$37.5 investment	0.454***	(0.137)
"Yes" to \$50 investment	0.539***	(0.148)
"Yes" to \$62.5 investment	0.326*	(0.192)
"Yes" to \$75 investment	0.727***	(0.181)
Intercept	0.260	(0.285)
Number of observations	1178	
District fixed effects	Yes	
F-statistic (instruments)	24.55	
p-Value (joint significance, all coefficients)	0.000	
Goodness of fit measure (McIntosh & Dorfman, 1992)	1.29	
Percentage correct predictions	0.630	
Pseudo R-square	0.081	

Note: These estimation results correspond to Eqn. (3) in the body of the paper. Estimation results are probability-weighted.
* Significance at the 1% levels.
** Significance at the 5% levels.
*** Significance at the 1% levels.

The other statistically significant results in Table 5 are also not surprising. Female-headed households are 45% 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% 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: every additional year of experience is associated with a 1.2% increase in the likelihood of participating. Likewise, households whose heads are members of peasant organizations (which excludes contract farming organizations) are 55% 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.

Armed with the results in Table 5, one can estimate treatment regressions for each of the welfare outcomes retained for analysis. Table 6 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 variables at the bottom of Table 4; 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 7 and 8 follow the same plan for the other indicators of welfare selected for analysis (i.e., household income per adult equivalent and household income net of contract farming revenue).

Because the empirical results for household participation in contract farming (i.e., the first column of Table 6) are very similar to one another in Tables 6–8 (the only significant difference is the number of *fady* days, which drops out of significance in Table 8), only the first-column results in Table 6 are discussed here. Female-headed households are almost 46% less likely to participate in contract farming than their male-headed counterparts. For every additional year of age, respondents are almost 2% less likely to have chosen to participate in contract farming, but for every additional year of agricultural experience, they are 1.2% 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% more likely to participate in contract farming than a household who is not a member of such an organization. The number of days during which the respondent cannot do agricultural work (i.e., *fady* days) is negatively associated with contract farming, most likely because contract farming entails strict work schedules that conflict with cultural mores. Moreover, for every additional hectare of land owned by the household, the household is 10% more likely to participate in contract farming. Finally, the variables in the vector of IVs are all statistically significant at the 1% level. The F-statistic on a test of their joint significance is equal to 24.50, once again obviating concerns about weak instruments. This is true in Tables 6–8.

As regards the impact of participation in contract farming, Table 6 shows that the institution has a positive impact on total household income, but that there is a considerable

Table 6. *Treatment regression and OLS estimation results for household income*

Variable	(1)		(2)		(3)	
	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		Dependent variable: Log of household income	
Household size	0.026	(0.021)	0.045***	(0.016)	0.054***	(0.015)
Dependency ratio	−0.120	(0.215)	−0.087	(0.163)	−0.146	(0.148)
Single	0.093	(0.203)	−0.160	(0.146)	−0.150	(0.140)
Female	−0.458*	(0.239)	−0.213	(0.175)	−0.338**	(0.163)
Migrant	0.077	(0.140)	0.006	(0.104)	0.026	(0.096)
Age	−0.019***	(0.007)	0.011**	(0.005)	0.004	(0.004)
Education	−0.007	(0.014)	0.069***	(0.010)	0.068***	(0.010)
Experience	0.012*	(0.007)	−0.005	(0.004)	−0.001	(0.004)
Member of peasant organization	0.528***	(0.109)	0.014	(0.093)	0.174**	(0.072)
Fady days	−0.003*	(0.002)	0.001	(0.001)	0.001	(0.001)
Working capital	0.006	(0.005)	0.006***	(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			1.115***	(0.276)	0.362***	(0.061)
"Yes" to $12.5 investment	0.462***	(0.137)				
"Yes" to $25 investment	0.402***	(0.131)				
"Yes" to $37.5 investment	0.461***	(0.127)				
"Yes" to $50 investment	0.502***	(0.142)				
"Yes" to $62.5 investment	0.458***	(0.167)				
"Yes" to $75 investment	0.641***	(0.181)				
Intercept	0.201	(0.280)	0.211	(0.270)	0.773***	(0.175)
Number of observations	1178				1178	
District fixed effects	Yes				Yes	
Log pseudo-likelihood	−1093.34				–	
p-Value (joint significance)	0.000				0.000	
F-statistic (instruments)	24.50				–	
R-square	–				0.514	

Note: The estimation results in column 2 correspond to Eqn. (1) in the body of the paper. Estimation results are probability-weighted. Results in the first column are marginal effects.
* Significance at the 10% levels.
** Significance at the 5% levels.
*** Significance at the 1% levels.

difference in estimated ATEs between the treatment regression and the naïve regression. Indeed, while the latter regression indicates that a 1-percent increase in the likelihood of participating in contract farming entails a 0.2% increase in a household's total income, the former suggests that a 1-percent increase in the likelihood of participating in contract farming entails a 0.5% increase in a household's 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 appears 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 6 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 if one were interested in stimulating participation in contract farming. For example, based on the results in the third column of Table 6, one would mistakenly conclude that female-headed households have a systematically lower income and 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 6. Interestingly, the naïve regression in column 3 of Table 6 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 6 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 indicate that a 1-percent increase in the likelihood of participating in contract farming entails a 0.6% increase in a household's total income per adult equivalent (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 a 1-percent increase in the likelihood of participating in contract farming entails a 0.5% increase in a household's total income.

Table 7. *Treatment regression and OLS estimation results for household income per adult equivalent*

Variable	(1)		(2)		(3)	
	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	0.027	(0.021)	−0.128***	(0.016)	−0.120***	(0.015)
Dependency ratio	−0.124	(0.216)	0.240	(0.160)	0.189	(0.148)
Single	0.081	(0.206)	0.052	(0.151)	0.061	(0.147)
Female	−0.452*	(0.242)	−0.358**	(0.180)	−0.466***	(0.167)
Migrant	0.070	(0.140)	0.021	(0.102)	0.039	(0.095)
Age	−0.020***	(0.007)	0.009*	(0.005)	0.003	(0.004)
Education	−0.006	(0.015)	0.071***	(0.010)	0.070***	(0.010)
Experience	0.012*	(0.007)	−0.004	(0.004)	−0.001	(0.004)
Member of peasant organization	0.529***	(0.109)	0.040	(0.095)	0.177**	(0.070)
Fady days	−0.003*	(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			1.000***	(0.313)	0.351***	(0.061)
"Yes" to \$12.5 investment	0.450***	(0.141)				
"Yes" to \$25 investment	0.399***	(0.134)				
"Yes" to \$37.5 investment	0.467***	(0.129)				
"Yes" to \$50 investment	0.509***	(0.145)				
"Yes" to \$62.5 investment	0.441**	(0.177)				
"Yes" to \$75 investment	0.655***	(0.185)				
Intercept	0.208	(0.282)	−0.242	(0.291)	0.243	(0.176)
Number of observations	1178				1178	
District fixed effects	Yes				Yes	
Log pseudo-likelihood	−1091.93				–	
p-Value (all coefficients)	0.000				0.000	
F-statistic (instruments)	24.15				–	
R-square	–				0.493	

Note: The estimation results in column 2 correspond to Eqn. (1) in the body of the paper. Estimation results are probability-weighted. Results in the first column are marginal effects.
* Significance at the 10% levels.
** Significance at the 5% levels.
*** Significance at the 1% levels.

What to make of these results? First off, it appears that participation in contract farming has a significant impact on welfare in that it significantly increases total household income and household income per adult equivalent.

Second, 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. Although it is beyond the scope of this paper to investigate the causal mechanism through which this happens, treatment regressions similar to the ones in Tables 6–8 run for each income category (not shown for brevity) indicate that this happens through the household's income from the sales of animals and its income from agricultural sources other than contract farming, (i.e., its income from land, cattle, and equipment rentals, its income from the sales of animal byproducts, and its income from the sales of noncontracted crops) rather than through the household's income from the labor market or from other nonfarm activities. Indeed, a 1-percent increase in the likelihood of participating in contract farming entails a 3-percent increase in a household's income from livestock, and a 1-percent increase in the likelihood of participating in contract farming entails a 1.3-percent increase in a household's income from agricultural sources other than contract farming. This suggests that contract farming activities may increase the efficiency of the household's livestock herding activities and that participation in contract farming may free up some of the household's plots, which the household then leases out. It could also suggest that being a contract farmer makes one more productive on one's noncontracted plots, as in Minten *et al.* (2009). Given that the research design did not aim to study the mechanism through which contract farming may have spillover on other sources of income, however, this must remain speculative.

Third, although the data are cross-sectional and thus 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 Tables 6–8, 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. It thus looks as though contract farming may on the one hand increase asset inequality because it favors those with larger landholdings,

Table 8. *Treatment regression and OLS estimation results for household income net of contract farming revenue*

Variable	(1)		(2)		(3)	
	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	0.024	(0.021)	0.055***	(0.017)	0.067***	(0.016)
Dependency ratio	−0.116	(0.213)	−0.037	(0.186)	−0.116	(0.160)
Single	0.124	(0.222)	−0.248	(0.204)	−0.234	(0.184)
Female	−0.487*	(0.251)	−0.066	(0.230)	−0.232	(0.198)
Migrant	0.083	(0.142)	−0.007	(0.122)	0.021	(0.109)
Age	−0.019***	(0.007)	0.014**	(0.006)	0.006	(0.004)
Education	−0.006	(0.014)	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	0.517***	(0.111)	−0.059	(0.121)	0.154*	(0.083)
Fady days	−0.003	(0.002)	0.001	(0.001)	0.001	(0.001)
Working capital	0.007	(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			0.986**	(0.430)	−0.016	(0.069)
"Yes" to $12.5 investment	0.453***	(0.133)				
"Yes" to $25 investment	0.385***	(0.131)				
"Yes" to $37.5 investment	0.431***	(0.132)				
"Yes" to $50 investment	0.474***	(0.146)				
"Yes" to $62.5 investment	0.471***	(0.161)				
"Yes" to $75 investment	0.601***	(0.199)				
Intercept	0.234	(0.281)	−0.173	(0.400)	0.576***	(0.203)
Number of observations	1178				1178	
District fixed effects	Yes				Yes	
Log pseudo-likelihood	−1149.50				–	
p-Value (all coefficients)	0.000				0.000	
F-statistic (instruments)	22.28				–	
R-square	–				0.514	

Note: The estimation results in column 2 correspond to Eqn. (1) in the body of the paper. Estimation results are probability-weighted. Results in the first column are marginal effects.
* Significance at the 10% levels.
** Significance at the 5% levels.
*** Significance at the 1% levels.

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, Fafchamps and Minten (2011) 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 indicate that it is those whose income is *a priori* lower who overwhelmingly select into participating in contract farming, which suggests that contract farming may decrease inequality.

Finally, appendix Table 9 attempts to characterize the difference between the IV methodology used in this paper and the propensity score matching (PSM) method. Within the framework of PSM, households who participate in contract farming are matched with households who do not participate in contract farming on the basis of the observables variable in the vector x_i of control variables used throughout this paper. The first column of Table 9 thus presents a probit regression in which participation in contract farming w_i is regressed on x_i. The main difference between these results and those in Table 5, which control for respondent WTP to participate in contract farming via the vector of IVs, is that household working capital, which was not statistically significant in Table 5, becomes statistically significant in Table 9. This suggests that household WTP to participate in contract farming is largely driven by whether the household has enough working capital, a result that is unsurprising given the stricter requirements of contracted crops relative to other crops. The second column of Table 9, which uses the predicted values from column 1 (i.e., the propensity score) as a regressor of interest instead of actual participation in contract farming, indicates that a 1-percent increase in the propensity score entails a 3-percent increase in total household income, i.e., roughly six times the estimated impact from the IV methodology. There is thus a considerable discrepancy between the estimated marginal effects from PSM and the IV specification that is the focus of this paper.

(c) *ATE or LATE?*

It is difficult to compare the size of the estimated impact of participation in contract farming on welfare in this paper with the size of the same estimated impact in other papers. Indeed,

Table 9. *Propensity score matching method estimation results for household income net*

Variable	(1)		(2)	
	Coefficient	(Std. Err.)	Coefficient	(Std. Err.)
	Treatment regression Dependent variable: = 1 if participates in contract farming; = 0 otherwise		OLS Dependent variable: Log of household income	
Household size	0.033	(0.020)	−0.007	(0.020)
Dependency ratio	−0.253	(0.204)	0.364*	(0.190)
Single	0.016	(0.205)	−0.149	(0.141)
Female	−0.444*	(0.237)	0.520**	(0.259)
Migrant	0.016	(0.134)	0.014	(0.097)
Age	−0.021***	(0.006)	0.046***	(0.011)
Education	0.003	(0.013)	0.059***	(0.010)
Experience	0.012*	(0.006)	−0.025***	(0.007)
Member of peasant organization			0.240***	(0.071)
Fady days	−0.002	(0.002)	0.004***	(0.001)
Working capital	0.010**	(0.004)	−0.002	(0.003)
Assets	0.004	(0.002)	0.001	(0.002)
Landholdings			0.000*	(0.000)
Propensity score			5.536***	(1.260)
Intercept	0.600***	(0.231)	−3.008***	(0.938)
Number of observations	1178		1178	
District fixed effects	Yes		Yes	
Log pseudo-likelihood	−788.041		–	
p-Value (all coefficients)	0.000		0.000	
R-square	0.035		0.503	

Note: Estimation results are probability-weighted and standard errors are robust. Results in the first column are marginal effects. The variables "member of a peasant organization" and "landholdings" had to be dropped in order to estimate the propensity score.
* Significance at the 10% levels.
** Significance at the 5% levels.
*** Significance at the 1% levels.

the estimation results in Tables 6–8 are for dependent variables that are the result of a logarithmic transformation. Because of the concavity of the logarithm function, it would be a mistake to multiply the elasticities reported above by 100 so as to linearly interpolate that participation in contract farming increases household income by 50%, as doing so would clearly over-report the impact of going from nonparticipation to participation in contract farming for the average household. In this case, the safest thing to do is to report such linear interpolations as an upper bound on the ATE.

With an ATE of 50% as an upper bound, the estimates in this paper are comparable with the same estimated impact in other papers: 32% in Warning and Key (2002), 39% in Miyata *et al.* (2009), and 48% in Rao and Qaim (in press), respectively. But it could still be difficult to compare the upper bound estimated in this paper with the findings in previous studies. In fact, any two findings in the literature may be difficult to compare to each other given that it could be the case, however, that what is estimated in this paper is not an ATE, but a local average treatment effect (LATE; see Angrist & Pischke, 2008):

> "[W]ith heterogeneous treatment effects, endogeneity creates severe problems for identification of population averages. Population average causal effects are only estimable under very strong assumptions on the effect of the instrument on the endogenous regressor ("identification at infinity", or under the constant treatment effect assumptions). Without such assumptions *we can only identify average effects for subpopulations that are induced by the instrument to change the value of the endogenous regressors. We refer to such subpopulations as compliers, and to the average treatment effect that is point identified as the local average treatment effect.* This terminology stems from the canonical example of a randomized experiment with noncompliance. In this example a random subpopulation is assigned to the treatment, but some of the individuals do not comply with their assigned treatment" (Imbens & Wooldridge, 2007; emphasis added.)

For ease of exposition to the reader who is not familiar with the concept of LATE, consider a classic example: in a seminal paper in applied microeconomics, Angrist (1990) uses Vietnam draft lottery numbers as an IV for education in a regression of earnings on education in the United States. The idea is as follows: the lower one's draft lottery number, the more likely one was to be drafted to serve in the US military during the Vietnam War. Under the GI bill, returned veterans are entitled to receiving a university-level education for free, paid for by the US federal government.

Define compliers as (i) the people who are assigned to the control group (i.e., no military service) and who comply with not taking the "treatment" (i.e., they do not acquire university-level education); and (ii) the people who are assigned to the treatment group (i.e., military service) and who comply with taking the treatment (i.e., they acquire university-level education). Noncompliers can be defined as (iii) the people who are assigned to the control group (i.e., no military service) and who take the treatment (i.e., they acquire university-level education); and (iv) the people who are assigned to the treatment group (i.e., military service) and who do not take the treatment (i.e., they do not acquire university-level education). In this case, the IV only allows identifying a local average treatment effect, i.e., the effect of the treatment on compliers. In other words, we can learn the effect of education on wages for (i) and (ii), but not for (iii) and (iv).

In principle, the IVs in this paper, which proxy for WTP and thus for marginal utility, should induce everyone to participate given the assumption of individual rationality. That is, since WTP proxies for marginal utility, an increase in someone's marginal utility from doing something (here, participating into contract farming) translates into an increase in the likelihood that that person will do that very thing, *ceteris paribus*. In the case of the Vietnam draft, some people are completely unaffected by their draft lottery number—it was always going to be the case that they were going to go to university, or it was always going to be the case that they were not going to do so.

In practice, however, it might not necessarily be the case that people with a higher WTP to participate in contract farming are more likely to participate, and so the estimated impact of participation in contract farming on welfare may well be a LATE rather than an ATE. This is a problem that plagues most if not all studies of the impacts of contract farming, however, so that the aforementioned other studies of the impact of contract farming on welfare are also likely to estimate LATEs. That is, it is usually not possible to know who is induced to participate in contract farming by an instrument, and each IV has its own LATE.

5. CONCLUSION

Using data collected in Madagascar in 2008, this paper has studied the welfare impacts of participation in contract farming across six regions, across multiple processing firms, and across several crops. Because participation in contract farming is not random, the results of a dichotomous-choice contingent valuation experiment were used to construct a vector of IVs used in an effort to exogenize participation in contract farming. By controlling for the unobserved heterogeneity that stems from heterogeneous preferences among respondents, the IVs used in this paper aim to partially exogenize participation in contract farming by mitigating the statistical endogeneity problem caused by omitted variables. The empirical results show that a 1-percent increase in the likelihood of participating in contract farming entails on average a 0.6-percent increase in a household's total income, a 0.5-percent increase in a household's income per adult equivalent, and a 0.5-percent increase in a household's income net of contract farming revenues, which appears to occur via spillovers on income from livestock and on income from agricultural sources other than contract farming.

What are the policy implications of these findings? In a context where some researchers and policy makers perceive contract farming as something close to bonded labor and where some researchers and policy makers perceive contract farming as the way out of underdevelopment, these findings suggest that contract farming has positive impacts on the welfare of the households involved. Thus, even though the institution of contract farming may increase inequality, stimulating industrial development by providing incentives for (i) processing firms to expand their activities and further delegate their production of agricultural commodities and (ii) households to participate in agricultural commodity chains may contribute to alleviating poverty. More concretely, policy makers could stimulate participation in contract farming by offering tax breaks to or subsidies for processing firms who expand their contracting activities and, within the context of these data, by targeting households headed by females; older individuals; individuals who are less experienced; and individuals who are not members of peasant organizations, as these characteristics are also associated with persistent poverty in Madagascar (Stifel, Forster, & Barrett, 2010).

It is important, however, to qualify these empirical findings by offering a few caveats. First, although they proxy for several sources of unobserved heterogeneity between respondents, the IVs used in this paper cannot completely exogenize the decision to participate in contract farming given the design of the contingent valuation question on which the vector of IVs was constructed. As such, although the identification strategy used in this paper constitutes a step in the right direction as regards the identification of the causal impacts of contract farming on welfare, the reader should keep in mind that it is far from perfect. Second, 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. Third, and perhaps more importantly, contract farming activities in the developing world are usually concentrated in easily accessible areas, usually close to paved roads or airports. 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 remains an empirical question.

NOTES

1. See Gereffi, Lee, and Christian (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).

2. This negative view of contract farming is 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).

3. For example, Bellemare (2010) studies the contract signed by a single firm over three commodities in a single region of Madagascar. For an exception to the single firm—single crop—single region rule, see Simmons *et al.* (2005), who study three commodities across three regions of Indonesia.

4. It is in theory possible that the researcher has access to all the control variables necessary to make participation in contract farming exogenous with respect to welfare. In practice, however, this is unlikely given that this would require collecting exact measures of risk aversion, entrepreneurial ability, technical ability, etc. and other measures that are typically unobservable given how difficult it is to measure them. As a consequence, and perhaps in a slight abuse of language, specifications that fail to control for those unobservable factors will be referred to as "biased" throughout this paper.

5. The contingent valuation method is commonly used in environmental economics to elicit valuations of natural resources and in marketing to elicit consumer preferences. It has also been used in agricultural economics to study technology adoption (see for example Hubbell, Marra, & Carlson, 2000; Qaim & de Janvry, 2003).

6. Indeed, the question of simultaneity or reverse causality (i.e., participation in contract farming affecting one's WTP for the institution) is also important. Indeed, both (i) whether there is cognitive dissonance (i.e., whether actual participation in contract farming affects WTP); or (ii) whether welfare affects WTP are discussed at length and ultimately ruled out. A fuller discussion is provided in Section 3 below.

7. Statistical endogeneity is distinct from theoretical endogeneity in economics in that it has three causes. These three causes are (i) omitted variables (or unobserved heterogeneity); (ii) simultaneity or reverse causality (which are akin to theoretical endogeneity); and (iii) measurement error.

8. Instead of using an IV, one could rely instead on propensity score matching (PSM) methods, as in Maertens and Swinnen (2009). This assumes 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 for on the basis of observables (Dehejia & Wahba, 2002). Because several unobservable factors (e.g., entrepreneurial ability, patience, risk preferences, technical ability, etc.) likely drive the decision to go into contract farming, however, this paper does not further discuss PSM methods save for a robustness check in Section 4b.

9. Given that Sodexo is an outlier along many contractual dimensions, robustness checks were conducted during preliminary work which dropped the households contracting with Sodexo. Doing so left the empirical results in this paper unchanged. Those robustness checks are not shown for brevity, but they are available from the author.

10. A household's dependency ratio is obtained by dividing the number of individuals in the household under 15 or above 65 years of age by the total number of individuals in the household. As such, while household size is a rough measure of the quantity of available labor within the household, the dependency ratio is a rough measure of the quality of available labor within the household.

11. The Malagasy observe a multiplicity of taboos (*fady* in Malagasy), 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, Fafchamps *et al.* (2011). See Ruud (1960) for an anthropological survey of the many taboos observed by the Malagasy.

12. US$1 $\approx$ 2000 Ariary when the data were collected.

13. 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), from sales of animal byproducts (milk and eggs), and from the sales of noncontracted crops; and (v) its income from contract farming.

14. 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.

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APPENDIX A

The estimation method used in this paper and presented in Section 3 was chosen because it allows estimating treatment effects and because it is efficient relative to the usual IV estimation method. But as per Wooldridge (2002, p. 623), the treatment regression setup requires more than the usual IV assumptions. The following four assumptions must be satisfied in order to be able to estimate the impact of participation in contract farming on welfare using this treatment regression approach:

1. Let $y_i = \mu_j + v_j$ denote a respondent's income if he does not participate in contract farming as a function of a deterministic component μ_j and a stochastic component v_j, where $j = 1$ respectively denotes a respondent who participates in contract farming and $j = 0$ a respondent who does not. It has to be the case that $v_1 = v_0$, i.e., that the stochastic components of income would be the same under either participation or nonparticipation. This relates to the exogeneity of treatment conditional on the instrumental variable, which is the focus of much of this paper.
2. It must also be the case that $E(v_0|x,z) = L(v_0|x)$, where $E(\cdot)$ denotes an expectation and $L(\cdot)$ denotes a linear projection. This assumption says that the instrument z can be excluded from the regression of interest, i.e., that it only affects y through w, and not in other ways. This also says that $E(v_0|x,z)$ is linear the regressors x. This assumption would not normally hold if y were a discrete variable, but this paper focuses on continuous dependent variables y (i.e., household income, household income per adult equivalent, and household income net of contract farming revenue). Earlier versions of this paper also included the duration of the hungry season (a count variable) and the likelihood that the household receives a formal loan as welfare indicators of interest, but it turns out that the treatment regression framework cannot be used for those indicators.

Assumptions 1 and 2 allow writing the regression of interest (Eqn. (1)) as $y = \delta_0 + \alpha w + x\beta_0 + u_0$ since $E(v_0|x,z) = 0$. Further,

3. It has to be the case that $P(w = 1|x,z) \neq P(w = 1|x)$, where $P(\cdot)$ denotes a probability. It also has to be the case that $P(w = 1|x,z) = G(x,z;\gamma)$ is known and parametric. This means that the instrument must have predictive power, which is tested throughout, and that the functional form used to forecast w must be known. Wooldridge (2002, p. 623) notes that this functional form assumption need not be correct—it simply needs to be known. Here, the assumption is that it is a probit.
4. It finally has to be the case that $Var(v_0|x,z) = \sigma_0^2$. In other words, the error term in the regression of interest has to be of constant variance. This homoscedasticity assumption is satisfied by virtue of using probability weights throughout, which makes standard errors robust throughout.