Development in the Midst of Drought: Evaluating an Agricultural Extension and Credit Program in Nicaragua.

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Abstract: This essay is an evaluation of year one of the Rural Business Development (RBD) program for small rice farmers in León, Nicaragua. The RBD program is administered by the Millennium Challenge Corporation, and is designed to deliver agricultural extension advice and affordable credit in the form of inputs to farm households. This essay estimates the average impact of the program on rice yields and revenues utilizing inverse propensity score weighting combined with linear regression. In conducting statistical inference, it also accounts for the fact that agricultural outcomes are likely correlated over space in a small area such as the one studied here. The results suggest that the program had no impact on average, likely due to the presence of a severe drought during the 2008 – 2009 rice growing season, but that poorer households may have done better than their wealthier counterparts. This does not account for program costs, which when factored in would likely make the overall net benefit of the program negative. There may very well be long term benefits to exploiting extension advice and better access to credit created by the RBD program, and the it appears to have shielded poorer farmers somewhat from the impact of the drought. But the results highlight the danger of introducing programs aimed at raising productivity and incomes in areas subject to system unanticipated shocks. Incorporating risk management techniques or insurance against systemic risk into extension programs may improve welfare and encourage broader participation in agricultural productivity programs going forward.

1 Introduction

When thinking of interventions designed to combat rural poverty, agricultural extension and credit appear to be natural complements. By delivering knowledge to farmers about productivity enhancing techniques and the proper use of inputs, extension can increase returns to capital invested in agricultural activities or diminish risks associated with agriculture. At the same time, including credit as a component of an agricultural extension program can give farmers the resources necessary to fully exploit the knowledge gained via extension services, and bring households into the market for extension services that otherwise could not afford to participate.

This essay evaluates year one of the two-year Rural Business Development (RBD) program for rice farmers in León, located on the Pacific Coast of Nicaragua. The program combines credit in the form of agricultural inputs with agricultural extension services tailored to individual farms. The RBD program is funded jointly by the U.S. and Nicaraguan Governments, and is administered by the local office of the Millennium Challenge Corporation (MCC), a development agency of the U.S. Government.

Estimated impacts indicate that the program had no impact on yields or revenues on average, but that farmers with relatively less wealth in productive agricultural assets did better than wealthier farmers, likely due to more intensive use of fertilizer and greater access to credit. The lack of a positive average impact may largely be due to the drought that occurred in the 2009-2010 agricultural year in the study area due to an El Niño event. The timing of rice planting decisions in León and Chinandega are such that the magnitude of the 2009-2010 El Niño event was not known until quite late in the growing season; the vast majority of farmers plant in July, which is when the presence of an El Niño event in 2009 was first confirmed, but its magnitude was not known until much later in the growing season (IRI 2009). Such harsh climatic conditions would no doubt undermine the potential of any intervention to improve agricultural outcomes.

Failing to detect a positive impact in a drought year may or may not indicate a lack of benefits overall to participation in the RBD program. Deciding to join an extension program that also offers credit may require weighing a tradeoff between higher expected returns and greater risk. Farmers might elect to participate in the RBD program because of gains from participation that occur over time in years characterized by favorable production conditions, while output in years with poor conditions for rice production could be unaffected or even decrease due to enrolling in the program.

In the case of the RBD program, the skills learned via extension agents could be applied in future years in which conditions are more suitable for rice. Thus the complete stream of benefits due to the program cannot be captured in a static framework. However, the negative aspects of poor outcomes among participants also have dynamic implications. At one extreme, a long-term deepening of poverty may occur if households sell off assets to meet debt obligations (Carter and Barrett 2006). Whether this outcome could obtain depends on how well insured households are against shocks, a question the data are not well suited to answer, although as mentioned earlier poorer households did better on average than richer farmers, suggesting that the program did serve as something of a buffer. In any case, extension and technology adoption programs in areas subject to largely unanticipated systemic shocks might be improved by measuring the extent to which households can absorb these shocks, and possibly by including an insurance component to the package of benefits offered to participants or tailoring extension advice to include risk management techniques where possible.

In what follows, Section 2 briefly summarizes past literature on the impacts of agricultural extension and rural credit. Section 3 describes the study area of León and Chinandega and the characteristics of the RBD program. Section 4 describes the goals of the evaluation, and Section 5 describes the estimation strategy employed. Section 6 reports estimation results and Section 7 subjects these results to robustness checks; Section 9 concludes.

2 Background and motivation

This paper adds to the literature on agricultural extension and credit interventions in developing country agriculture. Much has been written about agricultural extension in developing countries, and earlier work in this area in the context of developing economies is surveyed by Anderson and Feder (2004). When econometric methods have been employed, much of this literature reports high returns to investments in extension services, e.g., Bindlish and Evenson (1997). But as noted by Anderson and Feder, data quality and issues of econometric methodology give reason to doubt some of these results. As shown by Gautam and Anderson (1999), small changes to model specifications can drastically reduce high estimated returns to extension investments.

Later studies have made improvements to econometric methodology, and several of these are summarized in Cerdán-Infantes, Maffioli, and Ubfal (2008). Studies such as those by Praneetvatakul and Waibel (2007) and Godtland et al. (2004) tend to find that extension services have had success with respect to knowledge transfer but mixed effects on productivity and income. Overall, the evidence for the benefits of extension to agriculture in developing countries is mixed, and this conclusion extends to the various modalities by which extension services can be delivered (Anderson and Feder 2007).

Rural credit markets are the subject of their own rich literature, but only a small portion of research has been aimed at measuring the effects of credit on agricultural productivity and incomes. Existing studies generally find positive effects of credit receipt and access on agricultural productivity and incomes, but magnitudes vary considerably. Carter (1989) finds weak evidence of a positive relationship between receipt of credit and farm income and productivity in Nicaragua. Feder et al. (1990) and Foltz (2004) find modest effects of relaxing credit constraints on households on output and incomes, the former in the case of rural China and the latter using a sample of Tunisian farms. Guirkinger and Boucher (2008) use a broader definition of credit rationing than that employed by Feder et al. and Foltz, expanding the group of rationed households to include those that exit the credit market due to transaction costs or unwillingness to bear the risk of losing collateral in case of default. They estimate much larger impacts of eliminating credit constraints on farmers in rural Peru equal to an increase of 26 percent in the value of output per hectare.

As summarized by Del Carpio and Maredia (2009), there are a relatively small number of rigorous impact evaluations of agricultural extension and rural credit market projects in the literature. Their survey of the literature from 2000 to early 2009 identified 20 studies of agricultural extension projects and 10 addressing rural credit interventions that satisfied a few basic criteria for categorization as a rigorous impact evaluation.[1] When the scope of these studies is limited to evaluations of projects that combine extension services with credit, the number becomes smaller still. One recent example is Ashfar, Giné, and Karlan (2009), who evaluate the impact of DrumNet in Kenya, a program designed to increase participation of horticulturalists in export markets. The authors of that study randomly assign groups of farmers to treatments including extension services, extension with a joint liability loan, and no treatment. They find significant impacts of both versions of the program on production of export crops, formal

[1] Basic criteria for inclusion were 1) A focus on agriculture, 2) A defined agricultural intervention, 3) A clearly stated counterfactual (e.g., cannot measure impact simply by using a before and after comparison on a single group).

financial market participation, and significant increases in income among first time growers of export crops.

This essay does not have the benefit of randomized assignment to treatment. Instead, the identification strategy employed is to assume that selection into the program is based on observable characteristics, and program effects are estimated using inverse propensity score weighting combined with linear regression (Wooldridge, Inverse Probability Weighted Estimation for General Missing Data Problems 2007). The soundness of this assumption is tested to the extent possible using available data, and results suggest that while there are unobserved factors affecting program participation as well as yields and revenue, there is no reason to alter the conclusion of no program impacts.

The unique features of this paper are the conditions under which the RBD program was rolled out, and the use of spatial methods in conducting statistical inference. By evaluating the RBD program in the context of a severe and unexpected climatic shock, the results of the analysis can serve aid the design of agricultural development programs in areas characterized by high production risk from systemic shocks. In conducting inference, standard errors are estimated using the spatial autocorrelation and heteroscedasticity robust covariance (spatial HAC) matrix of Kelejian and Prucha (2007). A review of the literature uncovered no previously published impact evaluations in agricultural development that account for spatial autocorrelation.

3 The Rural Business Development program[2]

3.1 The Study Area and the goals and benefits of the RBD program

León and Chinandega are home to around 830,000 persons, 39 percent of which live in rural areas and are involved in agriculture. Nearly all smallholder agriculture is rainfed, with the vast majority of irrigated land under the control of large agribusinesses, usually sugarcane or plantain. Along with sesame seeds, maize, and sorghum, rice is one of the primary crops planted by small farms in the region.

Rice farmers participating in the RBD program are all members of cooperatives, and cooperatives with members in the program receive bundles of inputs for rice production sufficient for three manzanas[3] per participating farmer from MCC. These inputs are then leant out to participating members; interest rates on these loans vary across cooperatives, as credit contract details are controlled by cooperatives rather than MCC. While the input packets are meant to spur production in the short term, they are also designed to help each cooperative establish a rotating credit fund that will make liquidity available to farmers at in future years. For each participating cooperative, MCC pays a maximum of 30 percent of the costs associated with the program; the rest is paid for by the cooperative.

At the level of the producer, the RBD program for rice farmers also features benefits in the form of agricultural extension services, focused on tailoring the use of chemical fertilizers to the soil characteristics of each individual farm, more efficient use of agrochemicals meant to control threats to the plant, and on better management of the post-harvest stages of production; conversations in the field and MCC documentation suggests that particular emphasis was placed

[2] This section and the one that follows draw from documentation provided by the Nicaragua office of MCC, and are available from the MCC Nicaragua website (http://www.cuentadelmilenio.org.ni) or from the author.
[3] 1 manzana = 1.72 acres = 0.70 hectares

on the first of these components.[4] The costs of this technical assistance are factored into the portion of the total cost of program participation borne by each participating cooperative.

3.2 Eligibility criteria and participation in the RBD program

For rice farmers, participation in the RBD consists of several stages, the first of which is satisfying eligibility for participation in the program. Eligibility criteria include:

- The producer has planted or currently has at least 2 manzanas of rice.
- Area of farm must be between 2 and 50 manzanas, non-irrigated.
- The main rice parcel must be property of the beneficiary.
- The main rice parcel must be outside environmentally sensitive areas.
- The beneficiary must be at least 20 years of age.

As indicated by the eligibility criteria, the program targeted rice farmers with some degree of experience with the crop, and also focused on small non-irrigated farms. Forcing farmers to own their own land might rule out some of the poorest households in the area, but this restriction makes sense in the context of plot-specific extension services if permanent increases in productivity are to be achieved. As will be discussed in more detail when describing the data set, these criteria were not strictly enforced in the first year, particularly with regards to land tenure status. This evaluation focuses on farmers who did satisfy program participation criteria.

Rice farmers interested in participating in the RBD program submitted requests for assistance to their cooperatives. The cooperatives then organized these requests into a single business plan that was submitted to the MCC office in Nicaragua for approval. The business

[4] From the *Plan de Acción de la Estrategia de Salida* downloaded from the Nicaraguan MCC office website: "Given that the rice plant is highly responsive to the level of fertilization, the focus in terms of productivity growth will be based principally on the adoption of Best Agricultural Practices with emphasis on adoption by beneficiaries of a program of fertilization personalized and based on the results of soil analysis and the nutritional requirements of the plant."

plans themselves are at the cooperative level but are essentially collections of requests made by individual farms to participate in the RBD program. Whether or not an individual farmer participates in the program depends upon the decision made by MCC with regard to the business plan submitted by his or her cooperative.

4 Outcomes and parameters of interest

The goal of this evaluation is to estimate the average impact of the RBD program on participants; that is, the Average Treatment on the Treated (ATT) for a set of outcome variables. Altering the sample to exclude farmers not satisfying program criteria affects the interpretation of the ATT estimate in that it will only capture average effects on participants for the population satisfying program criteria. In addition, estimated impacts will describe effects on farmers who planted rice in 2009, rather than the entire population of farmers who meet program criteria; after trimming down the sample, 242 out of 300 farmers remained.[5]

I focus on two outcomes of interest: yields and revenues from growing rice. While cost data are available, they are incomplete and thus insufficient for constructing a measure of profit or net revenue. Instead, cost data are used to get a general idea as to whether program participants farmed land more intensively than non-participants by checking per hectare expenditures on chemical inputs such as fertilizers and pesticides.

While better measures of welfare exist than yields and revenues, there are good reasons for concentrating on these agricultural variables. Firstly, the main goal of the program is to address poverty among small rice farmers in León and Chinandega by raising agricultural

[5] Four farmers that were not members of eligible cooperatives reported being participants in the RBD program. Their names were cross-checked against databases maintained by MCA in Nicaragua, and this could not be verified. These households were dropped from the sample used in the analysis, leaving 243 rice planters. The results reported are robust to their inclusion, however. In addition, a single non-participant household with extremely high reported yields was excluded from the analysis, leaving a working sample of 242.

productivity and efficiency. The program is designed to accomplish this by making information, credit, and high quality inputs available to farmers, thereby removing the constraints keeping them from becoming more commercially successful. If no increase in productivity or revenues is observed, and there do not appear to be any efficiency gains among RBD participants, then this would suggest that the program had not worked as intended, at least in its first year.

Another benefit of focusing on yields is that the survey data contain a measure of rice yields pre and post-RBD. An implication of the identifying assumptions made in the econometric analysis of the RBD program presented here is that one should not detect any impact of the treatment on outcomes that could not have been affected by participation in the RBD program. For example, suppose we were to estimate the effect of participation in the RBD program on lagged yields. If the model has adequately controlled for differences between treatment and control households, we should detect no significant difference in pre-program yields across these two groups. If we do find a difference, this would strongly suggest the presence of unobservable factors correlated with RBD participation and the outcome of interest being modeled.

5 Identifying assumptions and estimation technique

5.1 Inverse propensity score weighting

The evaluation of programs where participation is not random is complicated by the fact that outcomes of interest may be correlated with household characteristics which are also driving the participation decision. In this case, merely comparing participants and non-participants will yield a biased estimate of the ATT. Here I will attempt to control for confounding factors via the Inverse Propensity Score-Weighted Least Squares method (IPS-WLS).

The ATT is equal to the average outcome among the subsample of participants when receiving the treatment, minus the average outcome among this same group in the absence of the program. This first average is observed in the dataset, but the second must be estimated using the subsample of non-participant households. In order to do so, the following assumption is made:

Assumption 1 - Unconfoundedness

Let y_i^0 denote the outcome without participation in the RBD program. Let $d_i = 1$ represent membership in the treatment group and $d_i = 0$ for all non-participant households. Holding observed characteristics constant, the untreated potential outcome y_i^0 is independent of selection into treatment. That is:

$$y_i^0 \perp d_i \mid \mathbf{x_i} = \mathbf{x} \tag{1}$$

This is known as the "unconfoundedness" assumption, and in this manifestation it states that the untreated potential outcome is independent of participation in the RBD program conditional on holding $\mathbf{x_i}$ fixed, where $\mathbf{x_i}$ is the vector of observed characteristics (Imbens 2004). Note that because we are only estimating the average untreated outcome among participants, we need not assume that the treated potential outcome, denoted y_i^1, is independent of treatment.

As shown by Rosenbaum and Rubin (1983), (1) can be restated as follows:

$$y_i^0 \perp d_i \mid p(\mathbf{x}) \tag{2}$$

where $p(\mathbf{x}) = P(d_i = 1 \mid \mathbf{x_i} = \mathbf{x})$ is the propensity score, or the probability of participating in the RBD program given the observed values of the $\mathbf{x}$ vector. In other words, if unconfoundedness holds, we can recover unbiased estimates of program impacts by conditioning on the scalar propensity score rather than the entire vector of observed characteristics.

In order to condition on the propensity score, an additional assumption must be made:

Assumption 2 - Overlap

$$0 < p(\mathbf{x}) < 1 \text{ for all } \mathbf{x}. \tag{3}$$

This is the overlap assumption, and it insures that there are treatment and control households at all values of $\mathbf{x}$ in the support of observable characteristics.

If there are no unobserved factors correlated with both the outcome of interest and selection into the RBD program, then it is only the distribution of observed characteristics along with treatment status that determines the average outcome in any given group. This suggests that we could recover an unbiased estimate of the average outcome without treatment among the group of participating households by applying weights to the subsample of control households. If the weights adjust the distribution of observed characteristics in the control group to reflect that of the treatment group, then the weighted average outcome among control group households would be an unbiased estimate of the average untreated outcome among households participating in the RBD program. This is the intuition behind using weights that are based on the probability of being in the treatment group given observed characteristics, i.e., weights based on the propensity score.

More formally, suppose we construct weights for households that did not participate in the RBD program that are equal to:

$$\frac{p(\mathbf{x})}{1-p(\mathbf{x})} \tag{4}$$

We then take the weighted expectation of the outcome y_i among untreated households, multiplied by $(1-d_i)$, holding the $\mathbf{x}$ vector constant:

$$
\begin{aligned}
& E\left[\frac{p(\mathbf{x})}{1-p(\mathbf{x})} y_i (1-d_i) \mid \mathbf{x_i}=\mathbf{x}\right]= \\
& \frac{p(\mathbf{x})}{1-p(\mathbf{x})} E\left[y_i (1-d_i) \mid \mathbf{x_i}=\mathbf{x}\right]= \\
& \frac{p(\mathbf{x})}{1-p(\mathbf{x})} E\left[y_i^0 (1-d_i) \mid \mathbf{x_i}=\mathbf{x}\right]= \\
& p(\mathbf{x}) E\left[y_i^0 \mid \mathbf{x_i}=\mathbf{x}\right]= \\
& p(\mathbf{x}) E\left[y_i^0 \mid d_i=1, \mathbf{x_i}=\mathbf{x}\right]
\end{aligned}
\tag{5}
$$

The second line is due to holding the **x** vector constant, and the third line comes from the fact that for control households the product of the observed outcome y_i and $(1-d_i)$ is equal to the product of the potential outcome y_i^0 and $(1-d_i)$. The fourth line stems from the fact that the propensity score is equal to the expected value of d_i holding the $\mathbf{x_i}$ vector constant. The final term follows from unconfoundedness, i.e., the average untreated outcome conditional on **x** ought to be equal regardless of the decision to select into treatment. By the law of iterated expectations, taking the expected value of this last term over the distribution of **x** yields the average untreated outcome among participating households in the absence of the RBD program, $E\left[y_i^0 \mid d_i=1\right]$.

Equation (5) can be estimated using the observed outcomes among the control households, and an estimate of the propensity score. Suppose the population-level model for the decision to enroll in the RBD program follows a logit specification. Then we can write down the propensity score as:

$$p(\mathbf{x}) = \frac{\exp(\pi_0 + \mathbf{x}_\mathbf{i}'\boldsymbol{\pi})}{1 + \exp(\pi_0 + \mathbf{x}_\mathbf{i}'\boldsymbol{\pi})} \tag{6}$$

Plugging the logit equation into the equation for the weights given in (4) yields:

$$\frac{p(\mathbf{x})}{1 - p(\mathbf{x})} = \exp(\pi_0 + \mathbf{x}_\mathbf{i}'\boldsymbol{\pi}) \tag{7}$$

Once the parameters of (6) are estimated, the fitted values $\hat{p}(\mathbf{x})$ are used to construct the weights given in (7), and the ATT can be estimated as:

$$\frac{\sum_{i=1}^{N} y_i d_i}{\sum_{i=1}^{N} d_i} - \frac{\sum_{i=1}^{N} y_i (1 - d_i)}{\sum_{i=1}^{N} (1 - d_i)} \frac{\hat{p}(x)}{1 - \hat{p}(x)} \tag{8}$$

This is the difference in two sample averages. The first term is the average outcome among the treated households in the sample, and the second is the sample version of the term in brackets in the first line of (5). The difference given in (8) will be a consistent estimator of the ATT if the model for the propensity score is correct and a law of large numbers can be applied to the two averages that appear in the formula.

5.2 Weighted linear regression

Inverse propensity score weighting only yields consistent estimates of program impacts if we have the correct model for the propensity score. We may be more confident in our ability to construct a correct regression model for the conditional expectation of a given outcome of interest than in our ability to model the selection process. It turns out that inverse propensity score weighting and regression can be combined in a manner that yields an unbiased and consistent estimate of the ATT, as long as either the model for the propensity score or the regression model of the conditional expectation of the outcome is correct; this is the "double robustness" property of inverse propensity score weighted least squares (IPS-WLS) estimation (Wooldridge 2007).

Consider the following regression model for the conditional expectation of the outcome variable y_i among the group of RBD program participants:

$$\begin{aligned} E\left[y_i^0 \mid d_i = 1, \mathbf{x}\right] &= \alpha_0 + \left(\mathbf{x_i} - \boldsymbol{\mu}\right)' \boldsymbol{\alpha}_2 \\ E\left[y_i^1 \mid d_i = 1, \mathbf{x}\right] &= \alpha_0 + \alpha_1 + \left(\mathbf{x_i} - \boldsymbol{\mu}\right)' \boldsymbol{\alpha}_{2'} \end{aligned} \tag{9}$$

The first line of (9) specifies the conditional expectation of yields for the group of RBD participants in the absence of the RBD program, and the second line is the conditional expectation of yields for this same group when its members actually participate. Here it is assumed that the $\mathbf{x_i}$ vector that appears in (9) is identical to that of (6), although this need not be the case. The vector $\boldsymbol{\mu}$ contains the means of the $\mathbf{x_i}$ variables within the population of participants. The parameter vector $\boldsymbol{\alpha}_2$ is the derivative of the conditional mean of the outcome with respect to the $\mathbf{x_i}$ vector, and it captures how the conditional expectation changes in the absence of treatment as $\mathbf{x_i}$ moves away from its mean. The vector $\boldsymbol{\alpha}_{2'}$ captures this same effect when treatment is received; any difference between $\boldsymbol{\alpha}_{2'}$ and $\boldsymbol{\alpha}_2$ can be attributed to interaction effects between the treatment and observed characteristics.

By the law of iterated expectations, taking the expectation of the first line of (9) over the distribution of $\mathbf{x}$ gives the expected value of y_i for the group of participants when not enrolled in the RBD program, while the expected value of the outcome for the group of participants when the treatment is received can be derived similarly using the second line. The difference between these two expectations is the ATT, α_1 .

5.3 *The double robustness property of inverse propensity score weighted least squares regression*

Given the assumption of unconfoundedness, $E\left[y_i^0 \mid d_i = 1, \mathbf{x}\right] = E\left[y_i^0 \mid d_i = 0, \mathbf{x}\right]$, and the first line of (9) can be replaced with an equivalent expression that uses the population of non-participant households. This makes it possible to combine the two lines of (9) as:

$$E\left[y_i \mid \mathbf{x}\right] = \alpha_0 + d_i\alpha_1 + \mathbf{x_i'\alpha_2} + d_i\left(\mathbf{x_i' - \mu}\right)'\boldsymbol{\alpha_3} \qquad (10)$$

The ATT is still given by α_1 . The vector $\boldsymbol{\alpha}_2$ is interpreted as before, and the sum of $\boldsymbol{\alpha}_2$ and $\boldsymbol{\alpha}_3$ is equal to $\boldsymbol{\alpha}_{2'}$ in (9). If the conditional expectation of y_i is indeed equal to (10), then the ordinary least squares estimate $\hat{\alpha}_1$ will be consistent for the ATT. Furthermore, we can apply weights to the data and estimate the parameters of (10) via weighted least squares. The consistency of $\hat{\alpha}_1$ will be unaffected when the regression model is the correct one for the conditional expectation (Greene 2003, 226).

If the conditional mean is not linear, but we have the correct model for the propensity score, $\hat{\alpha}_1$ will still be a consistent estimate of the ATT if it is estimated via weighted least squares, where the weights for non-participant households are given by (4) and the true propensity score is replaced by its estimate. To see why, assume without loss of generality that there is only a single covariate, *x*. The weighted least squares formula for the intercept among treated households is:

$$\hat{\alpha}_0 + \hat{\alpha}_1 = \frac{\sum_{i=1}^{N} y_i d_i}{\sum_{i=1}^{N} d_i} - \hat{\alpha}_2 \frac{\sum_{i=1}^{N} x_i d_i}{\sum_{i=1}^{N} d_i} \tag{11}$$

The interaction between d_i and $x_i - \bar{X}$ has dropped out because the latter is evaluated at $x_i = \bar{X}$ when solving for the intercept, where $\bar{X}$ is the average of *x* among RBD participants. The probability limit of the first term of (11) is the expected value of the treated outcome among households enrolled in the RBD program. The second term converges in probability to the probability limit of $\hat{\alpha}_2$ times:

$$E\left[x_i d_i\right] = E\left[xE\left[d_i \mid x_i = x\right]\right] = E\left[xp\left(x\right)\right] \tag{12}$$

The intercept formula for non-participant households is:

$$\hat{\alpha}_0 = \frac{\sum_{i=1}^{N} y_i \left(1-d_i\right) \frac{\hat{p}\left(x\right)}{1-\hat{p}\left(x\right)}}{\sum_{i=1}^{N} \left(1-d_i\right) \frac{\hat{p}\left(x\right)}{1-\hat{p}\left(x\right)}} - \hat{\alpha}_2 \frac{\sum_{i=1}^{N} x_i \left(1-d_i\right) \frac{\hat{p}\left(x\right)}{1-\hat{p}\left(x\right)}}{\sum_{i=1}^{N} \left(1-d_i\right) \frac{\hat{p}\left(x\right)}{1-\hat{p}\left(x\right)}} \tag{13}$$

Assuming that $\hat{p}\left(x\right) = p\left(x\right)$, the probability limit of the first term is the expected value of the untreated outcome among households enrolled in the RBD program. The probability limit of the second term is equal to the probability limit of $\hat{\alpha}_2$ multiplied by:

$$E\left[x_i\left(1-d_i\right)\frac{\hat{p}\left(x\right)}{1-\hat{p}\left(x\right)}\right] = E\left[xE\left[\left(1-d_i\right)\frac{p\left(x\right)}{1-p\left(x\right)} \mid x_i = x\right]\right] = E\left[xp\left(x\right)\right] \tag{14}$$

The second terms on the right hand side of each intercept formula are asymptotically equivalent. Taking the difference between the probability limits of the two intercepts therefore causes the second term to drop out of each, leaving:

$$\alpha_1 \xrightarrow{p} = E\left[y_i^1 \mid d_i = 1\right] - E\left[y_i^0 \mid d_i = 1\right] = \text{ATT} \tag{15}$$

where y_i^1 and y_i^0 are the potential outcomes with and without treatment, respectively.

5.4 Estimation and inference

Estimating the parameters of the regression model using the IPS-WLS technique is straightforward. First, the logit model is estimated via maximum likelihood, and the fitted values of the propensity score are used to construct the weights for non-participant households. Next, the parameters of the regression model, including the ATT, are estimated by minimizing the weighted sum of squared residuals. Define **w** as the **x** vector augmented to include the number one, and **z** as the **x** vector expanded to include one, the treatment indicator d_i, and the de-meaned covariates used in the regression model. Using this more compact notation, the objective function for the logit model can be written as:

$$\sum_{i=1}^{N} \frac{1}{N}\left[d_i \ln \frac{\exp\left(\mathbf{w}_\mathbf{i}'\hat{\boldsymbol{\pi}}\right)}{1+\exp\left(\mathbf{w}_\mathbf{i}'\hat{\boldsymbol{\pi}}\right)} + \left(1-d_i\right)\ln \frac{1}{1+\exp\left(\mathbf{w}_\mathbf{i}'\hat{\boldsymbol{\pi}}\right)}\right] \tag{16}$$

Minimizing (16) with respect to $\hat{\boldsymbol{\pi}}$ yields the estimated weights. The estimated regression coefficients are found by minimizing:

$$\sum_{i=1}^{N} \frac{1}{N}\left[d_i + \left(1-d_i\right)\exp\left(\mathbf{w}_\mathbf{i}'\hat{\boldsymbol{\pi}}\right)\right]\left[y_i - \mathbf{z}_\mathbf{i}'\hat{\boldsymbol{\alpha}}\right]^2 \tag{17}$$

The first term in brackets in (17) follows from the fact that the weights for non-participant households simplify to $\exp(\mathbf{w}_i'\hat{\boldsymbol{\pi}})$. The estimated ATT is the value of $\hat{\alpha}_1$ that results from minimizing equation (17).

How to conduct statistical inference on $\hat{\boldsymbol{\alpha}}$ is less obvious, for two reasons. Firstly, the variance of $\hat{\boldsymbol{\alpha}}$ will depend on the parameters of the estimated propensity score, $\hat{\boldsymbol{\pi}}$, if the regression model is not correct; Wooldridge (2002) gives a more general condition that must be satisfied for this dependence to be absent. For the estimated variance to be robust to misspecification of the regression model, it must account for this dependency. Secondly, this particular evaluation is concentrated on agricultural outcomes over a relatively small geographic area. There is good reason to believe that unobserved characteristics such as climate and soil quality will be correlated over space, causing spatial autocorrelation in the error term of the regression model.

Kelejian and Prucha (2007) offer a way forward. An analogy between their method and the Newey-West method of computing robust standard errors for time series data is useful. The Newey-West formula assumes that dependence between observations is a decreasing function of distance on the timeline, and selects a maximum length of time beyond which two observations are assumed to be uncorrelated. The variance of each regression coefficient is then estimated as a weighted sum of the sample variances and covariances of the residuals from the regression model about which inference is being conducted, multiplied by the appropriate explanatory variables; under the assumptions described above, taking the square root of the terms yielded by this procedure generates consistent estimates of the standard error for each regression coefficient.

The Kelejian and Prucha method works in much the same way, while accounting for the peculiarities of spatial data (e.g., at least two dimensions). The authors assume that dependence between observations is a decreasing function of the physical space between them, and that beyond a certain distance the dependence is zero. The variance of each coefficient is estimated using the weighted sum of sample variances and covariances, where the weights are given by a kernel function using the Euclidean distance between observations as the argument and the cutoff distance as the bandwidth. Additional details of the spatial covariance matrix and inference procedures used in the following section are presented in the appendix.

6 Results

6.1 Data

The sample was drawn from lists of rice producers provided by farmer cooperatives participating in the RBD program. These lists were pooled into a single database of farmers belonging to the 11 cooperatives originally chosen to participate in the RBD program and thought to satisfy the criteria listed in above in section 3.2 for program participation. Of these 11 cooperatives, one was eliminated because it had dropped out of the program partway through the agricultural year, and two others were eliminated because no names of non-participant farmers were made available. The remaining eight cooperatives served as the basis of the sample.

During the process of data collection, a large number of farmers were replaced in the sample at the request of MCC due to not satisfying program eligibility criteria; the program was to last for two years, but farmers found to violate program criteria would be disqualified in their first year of participation. MCC wanted to maintain the option of conducting a second round of data collection, and in order to avoid high rates of attrition in was decided that farmers not meeting program criteria would be dropped from the sample. Nearly 50 percent of the original sample had to be replaced, with the most common cause being failure to satisfy program criteria with respect to land tenure status, followed by households being listed more than once on the roster provided by MCC. To round out the sample, a small number of farmers not belonging to cooperatives but satisfying other program criteria were surveyed; enumerators located a number of such farmers in the field, and a random subsample of this group was chosen to be interviewed.

The data were collected in a single household visit shortly after the post-harvest stage of the agricultural calendar, allowing sufficient time for farmers to have marketed their production of rice. The danger of using data collected after the intervention is that we will hold variables constant that were affected by the treatment and are correlated with outcomes of interest; this would eliminate a portion of the impact from the estimated effect, and potentially introduce other sources of bias (Rosenbaum 1984). Recall questions were asked about purchases and sales of consumer durables, agricultural implements, and land in order to reconstruct the wealth of each household prior to implementation of the RBD program. These are major sources of wealth and it seems reasonable to expect households to remember substantial changes in asset holdings over a one year period.

These data were used to construct indices of agricultural and non-agricultural wealth via Principal Components Analysis (PCA). The indices explain 26.23 percent and 30.25 percent of variation in agricultural and non-agricultural wealth in the sample, respectively.[6] For data on the agricultural year immediately prior to the RBD program, households were asked about loans taken out for agricultural activities, changes in household membership and demographics, sown area of marketed crops, and rice production. Other potential explanatory variables, such as non-agricultural and unearned income, geographic location, sown rice area suffering plausibly exogenous production shocks (drought, flooding, excessive rain), and expectations regarding rice

[6] PCA maps variables into a series of orthogonal components explaining successively smaller shares of the total variation of whatever is being indexed. Härdle (2007) offers a more detailed explanation of PCA with examples of applications.

production levels enter into the different models at their reported levels for the 2009-2010 agricultural year.

6.2 Results of propensity score estimation and construction of weights

The propensity score and weights were estimated using a logit model that was linear in its arguments. Figure 1 below shows the overlap of the estimated propensity score and a histogram of the estimated weights.

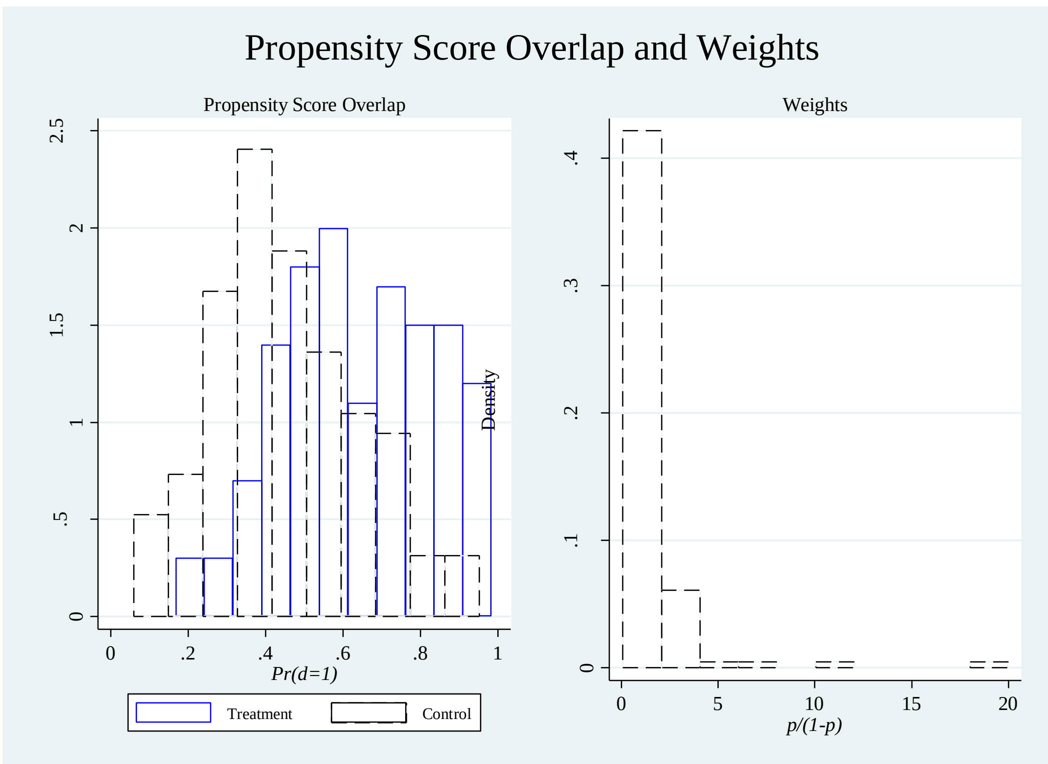

Figure 1

The degree of overlap in the propensity score distributions for the two groups gives us an idea of how similar treatment and control households are with respect to observable traits. A linear regression model estimated using one particular group can give a good approximation to the conditional mean in another group if the two groups have similar characteristics; otherwise, if there any underlying nonlinearities in the process, the approximation can be poor. In this particular case, the degree of overlap appears to be strong. In addition, we might be concerned about outliers in the distribution of the estimated weight, as observations with particularly large weights will strongly influence the estimate of the average untreated outcome among participant households. In the right panel of Figure 1, there are a small number of outliers to the right of 10. However, deleting these observations negatively affects the degree of overlap for the estimated propensity score, as all mass to the right of 0.8 in the left panel of Figure 1 disappears for the control households.

Alternatively, we could trim high values from both propensity score distributions, removing outliers and preserving covariate balance. Doing so results in a large loss of sample size, moving the sample from 242 to 204, and results in less success with respect to removing significant differences in covariate means across the two groups by way of inverse propensity score weighting. Instead, I opt to not trim the propensity score distributions for the model specifications that generate the main results of the paper, but I check the sensitivity of results to removal of propensity score outliers. Table 1 below presents the means of the outcomes and explanatory variables for the subsamples of participants and non-participants, as well as the weighted and unweighted differences across the two groups.

Table 1: Covariate Balance

	Sample average		Unweighted difference	Weighted difference
Variable name	Participants	Non-participants		
Rice yields (QQ/mz), 2009	46.982 [2.321]	51.247 [2.624]	-4.265 [3.513]	-0.742 [5.349]
Rice revenue per mz. (Córdobas)	13,569.61 [904.087]	13,403.80 [1,015.512]	165.803 [1,359.646]	1295.777 [1,686.40]
Altitude	107.822 [7.606]	150.009 [8.543]	-42.187 [11.439]***	10.08 [13.745]
Distance from city center	55.709 [2.387]	43.598 [2.681]	12.11 [3.589]***	-0.287 [5.896]
Years of education, household head	5.104 [0.381]	4.243 [0.428]	0.861 [0.574]	-0.603 [6.021]
Rice experience	6.593 [0.290]	7.028 [0.326]	-0.435 [0.436]	-0.748 [1.702]
Gender of household head (Female = 1)	0.081 [0.025]	0.103 [0.028]	-0.021 [0.037]	0.027 [0.032]
Number of adults	3.178 [0.120]	2.944 [0.135]	0.234 [0.180]	-0.013 [0.239]
Number of dependents	1.852 [0.117]	1.86 [0.131]	-0.008 [0.176]	-0.127 [0.233]
Proportion of sown area hit by shocks	0.77 [0.037]	0.63 [0.035]	0.141 [0.056]*	-0.024 [0.066]
Expected yield in a bad year	38.896 [1.442]	36.935 [1.620]	1.962 [2.169]	0.799 [2.468]
Expected yield in a normal year	66.37 [1.290]	61.607 [1.449]	4.763 [1.939]**	-0.279 [1.857]
Index of agricultural wealth	0.038 [0.170]	0.275 [0.191]	-0.238 [0.256]	-0.404 [0.474]
Index of non-agricultural wealth	0.286 [0.192]	0.064 [0.215]	0.222 [0.288]	-0.379 [0.885]
Feel secure about tenure rights = 1	0.941 [0.023]	0.907 [0.025]	0.034 [0.034]	0.001 [0.033]
Loan coop. in 2008 = 1	0.259 [0.032]	0.065 [0.036]	0.194 [0.048]***	-0.044 [0.125]
Loan formal fin. inst. in 2008 = 1	0.467 [0.043]	0.477 [0.048]	-0.01 [0.065]	-0.012 [0.104]
Loan other inst. in 2008 = 1	0.148 [0.029]	0.112 [0.033]	0.036 [0.044]	0.047 [0.048]
Observations:	242	242	242	242

Standard errors in brackets, Significant at *10%, ** 5%, *** 1%.

For "Rice yields" and "Rice revenue per mz.," the weighted differences are estimates of the ATT corresponding to equation (8); the impact on the former has a negative coefficient while the latter is positive, but neither is statistically significant. The remaining variables serve as the explanatory factors in the models that follow. Altitude above sea level, distance in kilometers from the center of León, subjective expectation of rice yields in a "normal" year, and the proportion of farmers receiving a loan from a farmer's cooperative all show significant raw differences, but these differences are all insignificant after weighting. Propensity score weighting has done a good job of correcting for observable differences between the two groups.

6.3 Impact on rice yields

Table 2 presents results from estimation of the IPS-WLS model for rice yields in quintals[7] for 2009. Coefficients generated by estimating the model on the full sample are presented in the first column. The remaining columns report t-statistics and differ by the specification used for the covariance matrix for the parameters of the yields model. The columns headed by "Huber-White Robust Standard Errors" conducts inference using the robust standard errors developed independently by Huber (1967) and White (1980) that are commonly reported in regression output, e.g., by using the "robust" option in Stata. The columns under "Kelejian and Prucha Spatial Autocorrelation-Heteroscedasticity Robust Standard Errors" attempt to account for spatial autocorrelation while maintaining robustness to heteroscedasticity. This estimator requires selection of a bandwidth beyond which it is assumed that observations are uncorrelated. Here results are presented for three different bandwidth sizes that allow for correlation between an observation and its one, five, or ten nearest neighbors.

[7] A quintal is around 100 pounds and is preferred unit of measure for rice production in Nicaragua.

Table 2: ATT of RBD participation on Rice Yields in Quintals					
Dependent variable: Rice yields (Quintals/mz), 2009		Huber-White Robust Standard Errors	Kelejian and Prucha Spatial Autocorrelation-Heteroscedasticity Robust Standard Errors		
			1 nearest neighbor	5 nearest neighbors	10 nearest neighbors
Explanatory variable	Est. coefficient	t-stat	t-stat	t-stat	t-stat
Intercept	-1.11	-0.07	-0.07	-0.07	-0.06
ATT (coefficient on *d*)	-3.71	-1.68	-1.69	-1.69	-1.82
Altitude (m)	0.05	1.73	1.75*	1.52	1.33
Distance from city center (km)	0.13	1.53*	1.54*	1.42*	1.27
Years of education, household head	0.04	0.08	0.08	0.08	0.08
Rice experience	1.24	1.75	1.76	1.85	1.79
Gender of household head (Female =1)	3.08	0.66	0.66	0.75	0.81
Number of adults	-0.48	-0.47	-0.48	-0.45	-0.44
Number of dependents	0.73	0.40	0.41	0.42-	0.43
Prop. area hit by shocks	-27.97	-5.23***	-5.28***	5.37***	-4.92***
Expected yield in a bad year	0.69	2.98**	2.98**	2.89*	3.12**
Expected yield in a normal year	0.35	1.40	1.40	1.41	1.47
Index of agricultural wealth	0.02	0.02	0.02	0.02	0.02
Index of non-agricultural wealth	-0.73	-0.60	-0.60	-0.66	-0.67
Feel secure about tenure rights = 1	1.32	0.19	0.19	0.20	0.20
Loan coop. in 2008 = 1	0.93	0.24	0.24	0.27	0.38
Loan formal fin. inst. in 2008 = 1	0.15	0.03	0.03	0.03	0.03
Loan other inst. in 2008 = 1	3.88	0.82	0.83	0.83	0.86

Table 2: ATT of RBD participation on Rice Yields in Quintals continued: Interactions with treatment indicator *d*					
*d*XAltitude	0.02	0.49	0.49	0.48	0.46
*d*XDistance	-0.11	-1.02	-1.02	-1.02	-1.05
*d*XEducation	0.33	0.50	0.50	0.44	0.41
*d*XExperience	-0.51	-0.56	-0.56	-0.57	-0.56
*d*XGender	1.35	0.16	0.16	0.16	0.20
*d*XAdults	1.18	0.72	0.73	0.72	0.67
*d*XDependents	1.18	0.18	0.18	0.19	0.18
*d*XShocks	-2.22	-1.01	-1.02	-0.99	-1.00
*d*XExp. yields in bad year	-0.11	-0.41	-0.41	-0.37	-0.37
*d*XExp. yields in a normal year	-0.10	-0.35	-0.35	-0.34	-0.36
*d*XAg. Wealth	-2.91	-2.10*	-2.11	-2.06*	-2.10*
*d*XNon-ag wealth	0.45	0.30	0.30	0.34	0.36
*d*XTenure security	-7.83	-0.85	-0.85	-0.82	-0.78
*d*XCoop. Loan	9.40	1.61	1.61	1.63	1.72
*d*XFormal loan	1.57	0.28	0.28	0.28	0.25
*d*XOther loan	-5.20	-0.70	-0.71	-0.70	-0.82
Observations:		242	242	242	242
Number of bootstrap replications:		996	996	995	991
* Significant at 10%, ** 5%, *** 1%. P-values based on bootstrap distribution of the t-statistic.					

Using a linear model to estimate the conditional mean function for yields might be problematic, since yields can only take on positive values. However, the fitted values from the regression were negative for only two observations, so this does not appear to be a concern. After estimating the model and computing t-statistics using the full sample, the distribution of the t-statistics for the different coefficients were estimated via the bootstrap. Statistical significance was determined by the location of the t-statistics computed using the full sample in the bootstrapped distributions. This process is known as “asymptotic refinement” and can reduce the bias associated with using asymptotic approximations to the distribution of a test statistic when its limiting distribution does not depend on unknown parameters, such as is the case with the t-statistic (Cameron and Trivedi 2005, 363-364). Hypothesis testing is done using two-tailed tests and not assuming a symmetric distribution of the t-statistic. For example, for a coefficient to be significant at the 10 percent level, the t-statistic computed using the full sample would have to be less than or equal to the fifth percentile of the bootstrapped t-statistic distribution, or greater than or equal to the 95th percentile.

The -3.71 point estimate of the ATT represent an 8 percent decrease of average yields for treated households, but it is not quite significant at a 10 percent level. Removing outliers with respect to the estimated weight yielded estimates that were more negative but less precise, likely due to the loss of nearly a quarter of the sample. Significant direct effects include altitude, distance from the city center, proportion of sown rice area hit by shocks, and expected yields in a bad year. On average, households that are at higher altitudes, more remotely located, and expect to have higher yields under poor conditions have higher yields; the first two effects are very small in magnitude, as one standard deviation increases in distance and altitude would result in yield increases of less than a quintal. The impact of shocks is potent; farmers with the a proportion of rice sown area hit by shocks equal to the sample average (around 70 percent) would have yields 20 quintals lower than a farmer whose rice parcels were not hit by shocks, other things being equal. Lastly, the fact that productivity under poor conditions is important is not surprising given the drought of the 2008 – 2009 rice season.

The interaction terms are estimated imprecisely in general, with the lone significant effect being the interaction with the index of agricultural wealth. The program made an effort to concentrate on poorer farmers, and this may be what is reflected in the interaction effect. The coefficient on the interaction between the RBD program and having received a loan from a cooperative in 2008 is not significant although it is quite large, suggesting that farmers were participating in the program were those already identified by cooperatives as productive and therefore worthy credit risks.

6.4 Impact on rice revenue

Table 3 presents results from estimation of the IPS-WLS model for rice revenues in Nicaraguan Córdobas per manzana in 2009.

Table 3: ATT of RBD Participation on Rice Revenues in Nicaraguan Córdobas per Manzana					
Dependent variable: Rice revenues (Nicaraguan Córdobas/mz), 2009		Huber-White Robust Standard Errors	Kelejian and Prucha Spatial Autocorrelation-Heteroscedasticity Robust Standard Errors		
			1 nearest neighbor	5 nearest neighbors	10 nearest neighbors
Explanatory variable	Estimated coefficient	t-stat	t-stat	t-stat	t-stat
Intercept	8,170.89	1.17	1.19	1.08	1.08
ATT (coefficient on *d*)	388.40	0.39	0.40	0.35	0.35
Altitude (m)	22.58	1.70	1.75	1.35	1.35
Distance from city center (km)	79.04	2.42	2.48	1.95*	1.95*
Years of education, household head	-182.31	-0.63	-0.63	-0.58	-0.58
Rice experience	-85.10	-0.32	-0.33	-0.30	-0.30
Gender of household head (Female =1)	-425.20	-0.19	-0.19	-0.21	-0.21
Number of adults	355.27	0.55	0.55	0.53	0.53
Number of dependents	-10,041.40	-1.19	-1.27	-1.19	-1.19
Prop. area hit by shocks	-577.99	-3.79**	-3.86**	-3.80***	-3.80***
Expected yield in a bad year	276.11	3.04**	3.05**	2.97**	2.97**
Expected yield in a normal year	-52.78	-0.75	-0.76	-0.75	-0.75
Index of agricultural wealth	540.39	1.05	1.06	1.15	1.15
Index of non-agricultural wealth	470.75	0.80	0.80	0.89	0.89
Feel secure about tenure rights = 1	-1,421.9	-0.44	-0.44	-0.46	-0.46
Loan coop. in 2008 = 1	-267.20	-0.13	-0.13	-0.14	-0.14
Loan formal fin. inst. in 2008 = 1	1,575.99	0.73	0.74	0.79	0.79
Loan other inst. in 2008 = 1	1,300.55	0.63	0.65	0.64	0.64

Table 3: ATT of RBD Participation on Rice Revenues in Nicaraguan Córdobas per Manzana continued: Interactions with treatment indicator *d*					
*d*XAltitude	-1.64	-0.10	-0.10	-0.09	-0.09
*d*XDistance	-48.59	-1.18	-1.19	-1.01	-1.01
*d*XEducation	352.16	1.02	1.04	0.89	0.89
*d*XExperience	440.23	1.31	1.33	1.29	1.29
*d*XGender	2,009.60	0.60	0.60	0.61	0.61
*d*XAdults	-824.57	-1.05	-1.06	-1.05	-1.05
*d*XDependents	-79.84	-0.11	-0.12	-0.11	-0.11
*d*XShocks	3,218.43	1.03	1.05	1.03	1.03
*d*XExp. yields in bad year	-24.06	-0.24	-0.24	-0.23	-0.23
*d*XExp. yields in a normal year	77.45	0.88	0.90	0.98	0.98
*d*XAg. Wealth	-1,341.32	-2.12	-2.14	-2.47*	-2.47*
*d*XNon-ag wealth	-148.48	-0.22	-0.22	-0.22	-0.22
*d*XTenure security	720.76	0.15	0.15	0.15	0.15
*d*XCoop. Loan	3,553.36	1.26	1.27	1.37	1.37
*d*XFormal loan	-240.60	-0.10	-0.10	-0.10	-0.10
*d*XOther loan	-2,381.09	-0.85	-0.87	-0.88	-0.88
Observations:		242	242	242	242
Number of bootstrap replications[8]:		996	996	995	991
* Significant at 10%, ** 5%, *** 1%. P-values based on bootstrap distribution of the t-statistic.					

To put these coefficients in context, around 20 Nicaraguan Córdobas is equal to 1 US dollar, and the sample average level of revenue per manzana is 13,496. The estimated ATT is positive but not significantly different from zero in all specifications. Pairing this with the non-regression adjusted estimated ATT from Table 1 leads to the conclusion that the RBD program had no impact on revenue per hectare on average. The direct effects of distance from León, proportion of area hit by shocks, and expected yields in a bad year are significant as they were in the model

[8] For all models 999 bootstrap replications were used. The actual number of replications is sometimes less than this because of problems estimating the covariance matrix of the model parameters; specifically, the routine sometimes failed because the matrix was not positive definite. In general, the number of failures was very small.

of yields and have the same signs. Most of the average marginal effects are small, but the impact of shocks is once again potent; moving from no area hit by shocks to the sample average reduces revenue per manzana by over 7,000 Córdobas.

Examining the interactions effects, there is once again evidence that the program successfully targeted poorer farmers, as the average marginal effect of an increase in the agricultural wealth index given by the sum of the coefficient on the direct effect and the interaction is equal to a decrease of 471 Córdobas for participants, which is 3.5 percent of the sample average for revenue; the effect is positive for non-participants. There is some indication that more experienced farmers and those receiving loans from cooperatives in 2008 benefited more on average in terms of revenue as well. Overall, the results suggest that the program had no effect on average on either yields or revenue per manzana, but that the program targeted poorer farmers with some success.

7 Robustness checks

This section presents a series of robustness checks on the model presented above. While at their heart all of these tests are meant to look for signs of omitted variable bias, they can be loosely broken down into three categories: those that check for selection bias, those that look for evidence of spillovers from treated to untreated households, and those that verify lack of sensitivity to the exclusion or inclusion of certain explanatory variables.

7.1 Selection effects

Table 4 below presents results of indirect tests of no selection bias.

Table 4: Indirect Tests of Selection Bias and Spillovers[9]

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable:	Yields, 2008	Yields, 2009	Revenue per mz., 2009	Yields, 2009	Revenue per mz., 2009	Yields, 2009	Revenue per mz., 2009
Sample:	Planted rice in 2008 and 2009	Planted rice in 2008 and 2009	Planted rice in 2008 and 2009	Member of RBD eligible cooperatives	Member of RBD eligible cooperatives	RBD participants and ineligibles	RBD participants and ineligibles
Explanatory variables:	All	All	All	All plus cooperative fixed effects	All plus cooperative fixed effects	All	All
ATT:	7.51	-5.32	995.77	-3.13	1,831.94	-0.21	1,185.53
t-statistic:	[2.50]**	[-2.45]**	[0.29]	[-1.30]	[1.42]	[-0.08]	[1.13]
*d*XAg. Wealth:	0.18	-2.26	-1,125.17	-3.62	-1,009.00	-2.41	-1,430.14
t-statistic:	[0.10]	[-1.66]	[-0.62]	[-3.58]***	[-1.50]	[-1.43]	[-1.55]
Observations:	208	208	208	196	196	181	181
Bootstrap replications:	975	996	991	994	998	999	999

*10%, ** 5%, *** 1%.

For each set of results, only the estimated coefficient on program participation and that of the interaction between agricultural wealth and the RBD program are reported. Column 1 of Table 4 uses rice yields in 2008 as the dependent variable and estimates the parameters of the IPS-WLS model using the full set of explanatory variables and the subset of farmers who planted rice in 2008 and 2009. The fact that the coefficient on the RBD program indicator is significant suggests that the model may not be adequately controlling for confounding factors. Surprisingly, the coefficient is positive, suggesting that participant households tend to be more productive than non-participants, at least among those that planted in both years.

[9] All of the same specifications for the covariance matrix used in Table 2 and Table 3 were used for the models in Table 4, but not all are reported. The specifications reported are those that lead to the most conservative conclusions. For example, in column 1 the results of using rice yields in 2008 as the dependent variable are reported. A significant coefficient on the treatment indicator *d* would suggest selection bias, as the program could not have affected outcomes before being rolled out. Therefore to be conservative I report results from the covariance matrix specification that yielded the most precise results, because this will be more likely to yield a statistically significant coefficient. In the case of column 1, this turned out to be the Huber-White covariance formula that does not account for spatial autocorrelation; precision was decreasing in the number of nearest neighbors allowed to be correlated with one another.

What may have occurred is that the RBD program induced a number of less productive eligible farmers to plant rice, dragging down the average productivity of the treated group. Columns 2 and 3 examine this possibility by re-estimating the ATT of the RBD program on 2009 rice yields and revenue per manzana using only the subsample of farmers who planted in both years. The result does not support the possible selection effect described here, as the impact on yields is now significant while becoming more negative.

The remaining columns test the sensitivity of results to composition of the control group. In columns 4 and 5, ineligibles are dropped from the sample and fixed effects for each farmer's cooperative are added to the logit and weighted regression models. In columns 6 and 7, eligible non-participants are dropped from the sample. The change in the magnitude of the ATT for yields when moving to Column 6 suggests that there may have been unobserved selection effects working within the cooperatives, although there is still no evidence of a positive average impact on yields. The point estimate for the ATT on revenue remains positive and becomes more precisely estimated when altering the composition of the control group, but remains insignificant. The average impact on revenue and yields still appear to be zero.

Note that checking the robustness of estimated coefficients to the composition of the control group also works as an indirect test for the presence of spillovers. Spillovers can occur in the context of agricultural extension services if non-participant farmers adopt the practices and technologies taken up by participants as a result of the program. The data set does not have information on social networks, but common membership in a farmer's cooperative might serve as a proxy. If this is a valid assumption, and there were spillovers from program participants to non-participating members of farmer cooperatives, we would expect to see a difference between columns (4) and (6) on one hand, and (5) and (7) on the other. While there are differences, they are not significant, and in any case do not suggest that we alter our conclusions about program impacts.

The sign and magnitude of the coefficient for the interaction of the RBD program with agricultural wealth is robust to the changes in specification, although it is generally estimated with less precision than in the full models for yields and revenue. The results in Table 4 support the notion that the RBD program successfully targeted poor farmers.

7.2 Changing the set of explanatory variables

Table 5 checks the sensitivity of the estimated ATT effects to changes in the set of explanatory variables.

Table 5: Sensitivity to Set of Explanatory Variables

	(1)	(2)
Dependent variable:	Yields, 2009	Revenue per mz., 2009
Sample:	All	All
Explanatory variables:	No expected productivity in bad and normal years	No expected productivity in bad and normal years
ATT:	-1.33	460.33
t-statistic:	[-0.48]	[0.35]
*d*XAg. Wealth:	-4.47	-1,787.42
t-statistic:	[-3.18]**	[-3.96]***
Observations:	242	242
Bootstrap replications:	996	996

*10%, ** 5%, *** 1%.

Columns 1 and 2 drop the variables containing self-reported productivity in normal and bad years. While the relevant survey questions were meant to capture expectations based on past experiences, it is possible that farmers in the RBD program incorporated productivity changes due to their participation into their responses. If this is so, holding these variables constant in the model may bias the estimated ATT downwards. Again we see that the ATT on yields appears to be zero. The interaction effects for agricultural wealth are significant, and continue to remain similar in magnitude to the effects estimated using the full model.

8 Seeking an explanation

8.1 Delivery of program services and farmer input choices

The above provides solid evidence that while the average impacts on yields and revenues were zero, the program was able to reach out to poorer farmers. The model does not survive the robustness checks without incident, as using lagged yields as the dependent variable results in a significant impact of the program when this should not occur if unconfoundedness holds. But on the whole there is no reason to reject the conclusions generated by the estimates presented in Table 2 and Table 3 regarding the average impact of the program, i.e., that there were none.

This begs the question as to why program impacts were so weak on average. The obvious explanation is the impact of the drought in the region; for example, this would dampen the beneficial effects of more intensive fertilizer use. But the program may have shaped outcomes in other ways. As described in Section 3, the RBD program seeks to improve farmer welfare in León through extension advice, as well as by giving farmers credit in the form of inputs. While there is no measure of how much information farmers retained from extension agents, we would expect to see greater input intensity among program participants relative to non-participants, as well as a higher volume of credit used for rice farming. Complete data on input costs are not available, but the data do include information on quantity of fertilizer and other chemicals used, with prices from local input stores collected by the MCC office in León, as well as post-harvest costs (drying, storage, transportation of rice, etc.). They also include information on the total volume of loans taken out for agricultural activities, as opposed to just for rice.

Table 6: Potential Causes of Muted Program Impacts

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Value of fertilizer/mz., Córdobas	Value of other chemicals/mz., Córdobas	Post-harvest costs/mz., Córdobas	Total credit received in 2009, Córdobas	Revenue per mz. net of fertilizer, chemical, and post-harvest costs
Sample:	All	All	All	All	All
Explanatory variables:	All	All	All	All	All
ATT:	9.93	105.42	-169.46	-9,273.87	422.11
t-statistic:	[0.87]	[1.18]	[-1.25]	[-0.61]	[0.41]
Ag. Wealth:	12.80	-29.37	128.87	80,144.34	429.87
t-statistic:	[1.79]	[-0.52]	[1.43]	[1.79]	[0.99]
*d*XAg. Wealth:	-20.80	43.86	-143.80	-78,320.30	-1,218.78
t-statistic:	[-2.18]**	[0.50]	[-1.36]	[-1.73]	[-2.55]**
Observations	242	242	242	242	242
Bootstrap replications	994	996	996	995	996

* Significant at 10%, ** 5%, *** 1%.

Here the direct effect of agricultural wealth on each outcome is presented as well, to make it easier to consider average marginal effects. Table 6 provides no evidence that program farmers used inputs any more intensively than their non-program counterparts on average, or that they received a higher volume of loans. Of course, it is possible that program benefits came in the form of greater efficiency rather than intensity. Column 5 uses revenues net of available cost data as the dependent variable; the estimated ATT is positive but insignificant. Since the costs here were constructed using prices from input stores, they may well be higher than what was paid by participants, given the 30 percent subsidy on the value of inputs received by cooperatives paid by MCC. But market prices should reflect the opportunity cost of inputs at least as well as subsidized RBD prices, and thus are a better indicator of gains or losses due to the program.

The negative and significant coefficient on the RBD program and agricultural wealth interaction in Column 5 reinforces earlier conclusions regarding the ability of the program to target poor farmers. For RBD participants, a one standard deviation increase in the agricultural wealth index decreases revenue net of available costs by around 100 Córdobas per manzana, or around 0.7 percent of the average revenue per manzana for the whole sample. For non-participants this outcome increases by around 54 Córdobas on average, although the latter is not significant.

Furthermore, the sign and significance of the agricultural wealth – RBD program interaction in the other columns hints at how this may have occurred. In column 1, the value of fertilizer applied per manzana is a decreasing function of agricultural wealth; on average, a one standard deviation increase in the agricultural wealth index RBD participant leads for to a reduction in fertilizer value per manzana of around one Córdoba, whereas the effect is an increase of around 1.62 Córdobas for a non –participant (although the direct is not quite significant with a p-value of 0.104). This could the positive and significant interaction between agricultural wealth and the RBD program on yields in Table 2. The coefficients on the direct and interaction effects of agricultural wealth are estimated too imprecisely in columns 2 and 3 to say much about RBD impacts. Column 4 offers somewhat stronger evidence that the program effectively addressed disparities in supply of credit to poorer farmers, with the large and positive direct effect of higher wealth on total credit received surpassed in absolute value by the negative interaction effect; the p-values for the direct effect and the interaction are 0.127 and 0.125, respectively.

These cost data are not complete, and the story might change if other inputs and labor were included. In any case, households with fewer productive assets appear to have done better in the RBD program than their wealthier counterparts, and the picture that is painted by the data is that this was accomplished by addressing disparities in fertilizer use and access to credit, which are the main pillars of RBD program.

9 Conclusion

This paper evaluated year one of the Rural Business Development program for small rice farmers in León, Nicaragua, a program co-funded by the US and Nicaraguan governments and administered by the Millennium Challenge Corporation. The RBD program delivers personalized extension services to small farmers, as well as credit in the form of inputs for rice production at a discounted price. The results of the analysis suggest that the program had no effects on participating households on average; this implies that the total benefits to its implementation were outweighed by the total costs. There is, however, some evidence that poorer households benefited more than their better off counterparts. During the 2008 – 2009 rice year León suffered an historically severe drought, which would likely undermine the impact of a program based partly on the proper and more intensive use of chemical fertilizers. The program appears to have partly shielded poorer farmers from the effects of drought through higher fertilizer use and enhanced access to credit.

In addition to the benefits of the program to poorer farmers, participants in the RBD program may benefit on average over the long term due to extension advice received or better access to credit via their cooperatives. However, if we were to account for costs, the net impact of the RBD program in year one of its existence would likely be negative. This underscores one danger of interventions designed to raise welfare among agricultural households in areas subject to large systemic shocks that cannot be perfectly predicted before planting decisions are made. These sorts of shocks are likely the most difficult to insure. Households participated in the RBD program voluntarily, and are likely aware of the risks posed by El Niño. But adding stronger risk management components to extension programs, whether they be insurance products, extension advice tailored to minimize the impact of shocks (e.g., water management in the case of rice), might encourage broader participation in such programs and increase their benefits overall.

Appendix: Derivation of spatial HAC matrix

Recall that the objective function for the weighted least squares regression is:

$$\sum_{i=1}^{N}\frac{1}{N}\left[d_i+\left(1-d_i\right)\exp\left(\mathbf{w}_\mathbf{i}'\hat{\boldsymbol{\pi}}\right)\right]\left[y_i-\mathbf{z}_\mathbf{i}'\hat{\boldsymbol{\alpha}}\right]^2 \tag{A.18}$$

The vector of estimated coefficients for the regression model, $\hat{\boldsymbol{\alpha}}$, is found by solving a $\left(p\times 1\right)$ vector of first order conditions, each element of which takes the following form:

$$\sum_{i=1}^{N}\frac{1}{N}\left[d_i+\left(1-d_i\right)\exp\left(\mathbf{w}_\mathbf{i}'\hat{\boldsymbol{\pi}}\right)\right]z_{i,p}\left[y_i-\mathbf{z}_\mathbf{i}'\hat{\boldsymbol{\alpha}}\right]=0 \tag{A.19}$$

where $z_{i,p}$ is the p^{th} element of $\mathbf{z}_\mathbf{i}$. Stacking these first order conditions yields:

$$\sum_{i=1}^{N}\frac{1}{N}\left[d_i+\left(1-d_i\right)\exp\left(\mathbf{w}_\mathbf{i}'\hat{\boldsymbol{\pi}}\right)\right]\mathbf{z}_\mathbf{i}\left[y_i-\mathbf{z}_\mathbf{i}'\hat{\boldsymbol{\alpha}}\right]=0 \tag{A.20}$$

Let $\omega_i\left(\hat{\boldsymbol{\pi}}\right)=\sum_{i=1}^{N}\frac{1}{N}\left[d_i+\left(1-d_i\right)\exp\left(\mathbf{w}_\mathbf{i}'\hat{\boldsymbol{\pi}}\right)\right]$. A Taylor expansion of (A.19) around the probability limit of $\hat{\boldsymbol{\alpha}}$, which we will label $\boldsymbol{\alpha}$, yields:

$$\sum_{i=1}^{N}\frac{1}{N}\omega_i\left(\hat{\boldsymbol{\pi}}\right)\mathbf{z}_\mathbf{i}\left[y_i-\mathbf{z}_\mathbf{i}'\hat{\boldsymbol{\alpha}}\right]=\sum_{i=1}^{N}\frac{1}{N}\omega_i\left(\hat{\boldsymbol{\pi}}\right)\mathbf{z}_\mathbf{i}\left[y_i-\mathbf{z}_\mathbf{i}'\boldsymbol{\alpha}\right]+\sum_{i=1}^{N}\frac{1}{N}\omega_i\left(\hat{\boldsymbol{\pi}}\right)\mathbf{z}_\mathbf{i}\mathbf{z}_\mathbf{i}'\left(\hat{\boldsymbol{\alpha}}-\boldsymbol{\alpha}\right) \tag{A.21}$$

Rearranging and multiplying by $\sqrt{N}$ gives us:

$$\sqrt{N}\left(\hat{\boldsymbol{\alpha}}-\boldsymbol{\alpha}\right)=\left[\sum_{i=1}^{N}\frac{1}{N}\omega_i\left(\hat{\boldsymbol{\pi}}\right)\mathbf{z}_\mathbf{i}\mathbf{z}_\mathbf{i}'\right]^{-1}\sum_{i=1}^{N}\frac{1}{\sqrt{N}}\omega_i\left(\hat{\boldsymbol{\pi}}\right)\mathbf{z}_\mathbf{i}\left[y_i-\mathbf{z}_\mathbf{i}'\boldsymbol{\alpha}\right] \tag{A.22}$$

To estimate the asymptotic variance of $\hat{\boldsymbol{\alpha}}$, we need to first find the limiting distribution of (A.22). The probability limit of $\left[\sum_{i=1}^{N} N^{-1}\omega_i\left(\hat{\boldsymbol{\pi}}\right)\mathbf{z_i z_i'}\right]^{-1}$ will be equal to $\left[E\left[\omega_i\left(\hat{\boldsymbol{\pi}}\right)\mathbf{z_i z_i'}\right]\right]^{-1}$, assuming the former is non-singular and its bracketed term obeys a law of large numbers. If $\sum_{i=1}^{N} N^{-1/2}\omega_i\left(\hat{\boldsymbol{\pi}}\right)\mathbf{z_i}\left[y_i - \mathbf{z_i'\boldsymbol{\alpha}}\right]$ obeys a central limit theorem,[10] then the asymptotic covariance matrix of $\hat{\boldsymbol{\alpha}}$ will be

$$AVar\left(\hat{\boldsymbol{\alpha}}\right) = \left[E\left(\omega_i\left(\hat{\boldsymbol{\pi}}\right)\mathbf{z_i z_i'}\right)\right]^{-1} Var\left[\sum_{i=1}^{N}\frac{1}{\sqrt{N}}\omega_i\left(\hat{\boldsymbol{\pi}}\right)\mathbf{z_i}\left[y_i - \mathbf{z_i'}\boldsymbol{\alpha}\right]\right]\left[E\left(\omega_i\left(\hat{\boldsymbol{\pi}}\right)\mathbf{z_i z_i'}\right)\right]^{-1} \quad \text{(A.23)}$$

The middle term in brackets is the covariance matrix of the first order conditions for the regression objective function given in (A.19), evaluated at $\boldsymbol{\alpha}$. If these first order conditions depend asymptotically on the vector of logit coefficients $\boldsymbol{\pi}$, then any estimating equation for this term must take this dependence into account. As shown by Wooldridge (2002), there will be no such dependence if the moment condition given in (A.20) behaves the same whether it is evaluated at $\hat{\boldsymbol{\pi}}$ or its probability limit, $\boldsymbol{\pi}$. In other words:

$$\text{plim}\left[\sum_{i=1}^{N}\frac{1}{N}\omega_i\left(\hat{\boldsymbol{\pi}}\right)\mathbf{z_i}\left[y_i - \mathbf{z_i'}\hat{\boldsymbol{\alpha}}\right]\right] = \text{plim}\left[\sum_{i=1}^{N}\frac{1}{N}\omega_i\left(\boldsymbol{\pi}\right)\mathbf{z_i}\left[y_i - \mathbf{z_i'}\hat{\boldsymbol{\alpha}}\right]\right] \quad \text{(A.24)}$$

If this condition were to hold, it would imply that we can ignore the fact that $\hat{\boldsymbol{\pi}}$ is estimated. If the regression model is the correct one, then as mentioned earlier weighting the data will have no

[10] In the case of spatial data, whether a central limit theorem and a law of large numbers will apply is not always clear cut; data are dependent in ways that differ from what is commonly encountered with time series data, for example. Jenish and Prucha (2009) provide conditions under which this will be the case. A key assumption is that dependence decreases with distance, which seems reasonable in the present context.

effect on consistency of $\hat{\boldsymbol{\alpha}}$; conditional on the weights and $\mathbf{z}$, the expected value of $\left[y_i - \mathbf{z}_\mathbf{i}'\hat{\boldsymbol{\alpha}}\right]$ will be zero. If the regression function is misspecified, however, this will not hold in general.

To correct for dependence between $\hat{\boldsymbol{\alpha}}$ and $\hat{\boldsymbol{\pi}}$ we take an exact Taylor expansion of the first term of the right hand side of (A.21) around the probability limit of $\hat{\boldsymbol{\pi}}$:

$$\begin{aligned}&\sum_{i=1}^{N}\frac{1}{\sqrt{N}}\omega_i\left(\hat{\boldsymbol{\pi}}\right)\mathbf{z}_\mathbf{i}\left[y_i-\mathbf{z}_\mathbf{i}'\boldsymbol{\alpha}\right]=\\&\sum_{i=1}^{N}\frac{1}{\sqrt{N}}\omega_i\left(\boldsymbol{\pi}\right)\mathbf{z}_\mathbf{i}\left[y_i-\mathbf{z}_\mathbf{i}'\boldsymbol{\alpha}\right]+\sum_{i=1}^{N}\frac{1}{\sqrt{N}}\omega_i\left(\tilde{\boldsymbol{\pi}}\right)\mathbf{z}_\mathbf{i}\mathbf{w}_\mathbf{i}'\left[y_i-\mathbf{z}_\mathbf{i}'\boldsymbol{\alpha}\right]\sqrt{N}\left(\hat{\boldsymbol{\pi}}-\boldsymbol{\pi}\right)\end{aligned} \tag{A.25}$$

This is a $\left(p\times 1\right)$ vector, with an element for each coefficient in $\boldsymbol{\alpha}$. The second term of the bottom line consists of the $\left(p\times j\right)$ matrix of derivatives of (A.20) with respect to each of the *j* elements of the vector of logit coefficients, multiplied by the $\left(j\times 1\right)$ vector $N^{1/2}\left(\hat{\boldsymbol{\pi}}-\boldsymbol{\pi}\right)$. The vector $\tilde{\boldsymbol{\pi}}$ consists of elements located somewhere between $\hat{\boldsymbol{\pi}}$ and $\boldsymbol{\pi}$.

We cannot use the $N^{1/2}\left(\hat{\boldsymbol{\pi}}-\boldsymbol{\pi}\right)$ term in estimation, as substituting in $\hat{\boldsymbol{\pi}}$ for $\boldsymbol{\pi}$ would cause it to drop out. Instead, it can be replaced with an equivalent expression, following the same steps used to arrive at (A.22). This yields:

$$\left[\sqrt{N}\left(\hat{\boldsymbol{\pi}}-\boldsymbol{\pi}\right)\right]=\left[-E\left[\mathbf{F}_\mathbf{i}\right]\right]^{-1}\frac{1}{\sqrt{N}}\sum_{i=1}^{N}\left[d_i-\frac{\exp\left(\mathbf{w}_\mathbf{i}'\boldsymbol{\pi}\right)}{1+\exp\left(\mathbf{w}_\mathbf{i}'\boldsymbol{\pi}\right)}\right]\mathbf{w}_\mathbf{i} \tag{A.26}$$

where $\mathbf{F}_\mathbf{i}$ is the matrix of second derivatives of the logit objective function, evaluated at a vector lying somewhere between $\hat{\boldsymbol{\pi}}$ and $\boldsymbol{\pi}$. The right hand side of (A.26) is substituted into (A.25), which is then is substituted into the asymptotic covariance matrix for $\hat{\boldsymbol{\alpha}}$ given in (A.23). The latter can now be rewritten as:

$$AVar\left(\hat{\boldsymbol{\alpha}}\right)=\left[E\left(\omega_i\left(\hat{\boldsymbol{\pi}}\right)\mathbf{z_i}\mathbf{z_i'}\right)\right]^{-1}\frac{1}{N}Var\left[\sum_{i=1}^{N}\mathbf{g_i}\right]\left[E\left(\omega_i\left(\hat{\boldsymbol{\pi}}\right)\mathbf{z_i}\mathbf{z_i'}\right)\right]^{-1} \tag{A.27}$$

where $\mathbf{g_i}$ is given by:

$$\begin{aligned}&\omega_i\left(\boldsymbol{\pi}\right)\left[y_i-\mathbf{z_i'}\boldsymbol{\alpha}\right]\mathbf{z_i}+\\&\omega_i\left(\tilde{\boldsymbol{\pi}}\right)\left[y_i-\mathbf{z_i'}\boldsymbol{\alpha}\right]\mathbf{z_i}\mathbf{w_i'}\left[-E\left[\mathbf{F_i}\right]\right]^{-1}\frac{1}{\sqrt{N}}\sum_{i=1}^{N}\left[d_i-\frac{\exp\left(\mathbf{w_i'}\boldsymbol{\pi}\right)}{1+\exp\left(\mathbf{w_i'}\boldsymbol{\pi}\right)}\right]\mathbf{w_i}\end{aligned} \tag{A.28}$$

The $E\left(\omega_i\left(\hat{\boldsymbol{\pi}}\right)\mathbf{z_i}\mathbf{z_i'}\right)$ terms in(A.27) are estimated using their sample counterparts. The middle term can be rewritten as:

$$\frac{1}{N}Var\left[\sum_{i=1}^{N}\mathbf{g_i}\right]=\sum_{i=1}^{N}Var\left(\mathbf{g_i}\right)+\sum_{i=1}^{N}\sum_{j=1,j\neq i}^{N}Cov\left(\mathbf{g_i},\mathbf{g_j}\right) \tag{A.29}$$

If there were no dependence across observations, then the second term in (A.29) would be zero, and the remaining component could be estimated using the usual formula for a heteroscedasticity robust covariance matrix (White 1980). In the case of spatial correlation, both components must be estimated.

Kelejian and Prucha (2007) offer a way forward. Applying their technique yields the following estimator for $\frac{1}{N}Var\left[\sum_{i=1}^{N}\mathbf{g_i}\right]$:

$$\frac{1}{N}\left[\hat{\mathbf{g}}_\mathbf{1}\quad\cdots\quad\hat{\mathbf{g}}_\mathbf{N}\right]\begin{bmatrix}K\left(d_{1,1}/d^*\right)&\cdots&K\left(d_{1,N}/d^*\right)\\\vdots&\ddots&\vdots\\K\left(d_{N,1}/d^*\right)&\cdots&K\left(d_{N,N}/d^*\right)\end{bmatrix}\begin{bmatrix}\hat{\mathbf{g}}_\mathbf{1}'\\\vdots\\\hat{\mathbf{g}}_\mathbf{N}'\end{bmatrix}=\frac{\hat{\mathbf{G}}\mathbf{K}\hat{\mathbf{G}}'}{N} \tag{A.30}$$

where $\hat{\boldsymbol{\pi}}$ and $\hat{\boldsymbol{\alpha}}$ have been substituted into $\hat{\mathbf{g}}_{\mathbf{i}}$ in place of $\boldsymbol{\pi}$, $\tilde{\boldsymbol{\pi}}$, and $\boldsymbol{\alpha}$, $d_{i,j}$ is the Euclidean distance between observations *i* and *j*, and d^* is the distance beyond which observations are assumed to be uncorrelated. Kelejian and Prucha showed that this is a consistent estimator in the context of a particular type of spatial regression model, while Kim and Sun (2010) generalized their proof of consistency to apply to linear and nonlinear estimators based on the solution to a set of moment conditions, such as in this essay. In the present essay, the maximum distance d^* is set large enough to allow for the main land parcel of a given household to be correlated with those of its nearest 1, 5, and 10 neighbors, depending on the specification.

To guarantee that (A.30) is positive semi-definite, the function $K(\ )$ must satisfy the conditions given in Assumption 7 of Kim and Sun. The results presented in this paper are the result of using a Parzen kernel:

$$K(x) = \begin{cases} 1 - 6x^2 + 6|x|^3 & \text{for } 0 \le |x| < 1/2 \\ 2\left(1 - |x|^3\right) & \text{for } 1/2 \le |x| < 1 \\ 0 & \text{otherwise} \end{cases} \tag{A.31}$$

Inference is then conducted by carrying out via a bootstrapped asymptotic refinement. This consists of the following steps:

1. Estimate the model on the full sample.
2. Compute t-statistics for the weighted least squares regression coefficients.
3. Draw a random sample with replacement from the full sample.
4. Estimate $\hat{\boldsymbol{\pi}}$, $\hat{\boldsymbol{\alpha}}$, and (A.30) using the bootstrapped sample.

5. Compute t-statistics for the regression coefficients centered on the coefficient estimates from step 1.

The estimated distributions of the t-statistics for the model parameters are constructed by repeating steps 1 through 3, and statistical significance is determined by seeing where the t-statistics computed using the full sample lie in the bootstrapped distribution. The bootstrapped procedure will not preserve the spatial correlation of the full sample, but Kim and Sun present simulation results showing that it improves the accuracy of confidence intervals over the usual symmetric normal approximation.

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