Identifying the Impact Dynamics of a Small Farmer Development Scheme in Nicaragua

Emilia Tjernström, Michael Carter, Patricia Toledo

Social programs predicated on the notion that their beneficiaries will change some behavior (perhaps due to improved incentives or new knowledge gained during the intervention) pose unique challenges for impact evaluation. Assuming that a randomized control trial (RCT) is appropriate, we often don't know *ex ante* precisely how long it will take for the treatment group to change the target behavior and reap the benefits of the

Emilia Tjernström (emiliat@primal.ucdavis.edu) is a PhD candidate in the Department of Agricultural and Resource Economics Department at the University of California, Davis. Michael R. Carter is a Professor at the Department of Agricultural and Resource Economics Department at the University of California, Davis, and the Director of the BASIS Research Program. Patricia Toledo is an Assistant Professor in the Department of Economics at Ohio University. We thank Anne Rothbaum, Lola Hermosillo, Jack Molyneaux, Juan Sebastian Chamorro, Carmen Salgado, Claudia Panagua, Sonia Agurto, Conner Mullally, and the staff at FIDEG. We gratefully acknowledge funding from the Millennium Challenge Corporation, as well as financial support from the US Agency for International Development Cooperative Agreement No. EDH-A-00-06-0003-00 through the BASIS Assets and Market Access CRSP.

program. Consequently, it is difficult to determine when the treatment and control groups should be compared, i.e. when the program in question should be evaluated.

While delaying the evaluation of a given program increases the likelihood that participants will have had the time to benefit from the program, it also typically means denying a control group access to the treatment. As Barahona (2010) forcefully argues, researchers ought to limit, to the extent possible, both the scale and the duration of any deliberate exclusion of individuals who could benefit from a program[1]. With that in mind, it is surprising that questions related to the *timing* of impacts have been relatively neglected in the program evaluation literature (see e.g. King and Behrman 2009).

We can additionally motivate paying careful attention to timing purely from the perspective of estimating our impact parameters correctly. In fact, getting the evaluation timing wrong in the types of programs characterized above could arguably bias the evaluation just as much as, say, ignoring sample selection issues. A recent study of land transfers to small farmers in South Africa (Keswell and Carter 2011) finds that living standards dipped in the first year in the program, but later grew and became increasingly positive over the next 3 years. Importantly, the long-run impacts are nearly double the magnitude of the shorter-run effects. Had the above study been constrained to estimating average treatment effects one year after the participants got their assets, the gradualness of co-investment and learning effects (both highly desirable intermediate outcomes for a development program!) would have greatly muted the estimated impacts.

This paper discusses a small-farm development scheme in Nicaragua that was rolled out in such a way as to allow estimation of the evolution of impacts over time. Due to capacity constraints, not all households could be enrolled in the program at the same time, and they were therefore randomized into an ‘early’ and a ‘late’ treatment group. Using data collected a year after the program began, and before the ‘late’ group was enrolled, we can estimate the average treatment effect of the program on a range of outcomes. The outcome variables considered here are farm income, per-capita household consumption, and capital investments. In the next section, we describe the program and the data in more detail. We then introduce a two-sided complier[2] estimator of the local average treatment effect (LATE) and present the results from this estimation procedure. Next, exploiting the fact that farm households were randomly enrolled at different points in time, we estimate the evolution of impacts over time using a parametric duration response-path, which in turn is informed by the shape of analogous semi-parametric results. This continuous approach allows us to extract more information than the binary impact estimate that typically emerges from impact evaluations. It also allows for additional learning even after the control group has been treated.

Program Details and Data

The Small-Farm Development Program

The program that we evaluate was a multi-year small-farm development program in Nicaragua, initiated in 2005 when the Millennium Challenge Corporation (MCC) signed a five-year compact with the Government of Nicaragua. The goal was to develop a set of development projects in the departments of Leon and Chinandega, known as the Western Region. We focus herein on the project called the Rural Business Development (RBD) program, which focused on raising incomes for small- to medium farms and rural businesses. The program helped farmers develop and implement a business plan built around a high-potential activity[3]. Once a farmer's business plan had been approved, the program provided 24 months of intensive treatment and training, including technical assistance, marketing support, materials and equipment. The goal was to improve farm productivity, and consequently, households' economic well-being.

FIGURE 1 ABOUT HERE

The flowchart in figure 1 illustrates how the program worked, and how it may have influenced key development outcomes at different levels. Given that the program focused on specific agricultural activities and was in the first instance designed to enhance the access of small farmers to improved technologies and to markets, we refer to these direct impacts as the outcomes of the program. The next steps in the flowchart outline the potential impacts of the program. As we proceed through the results, we will examine these steps in turn.

The Data

The data for this project were collected in three separate rounds. A baseline was collected in late 2007, right before the early treatment clusters enrolled in the program, followed by a midline survey in early 2009, before the late treatment groups began being enrolled in the program. Because clusters of farmers were randomly allocated to early and late treatment conditions, we expect the late treatment group to function as a valid control group at the midline.[4] Both early and late treatment clusters were then surveyed again near the end of the program in early 2011. By the time of the third survey round, all farmers had decided whether to join the program.[5] We use this information in the next section, to define a two-sided complier sample from which we estimate the LATE.

Intermediate Outcomes and Short-Term Impacts

Thanks to the timing of the survey rounds, our dataset allows us to identify which sampled farmers from the early treatment group were indeed enrolled before 2009, as well as which farmers from the control group (assigned to late treatment) were enrolled in or after 2009. In other words, we are able to identify participants and compliers in both the treatment and control groups and can estimate the effect of the program removing *(i)* those farmers who did not participate, and *(ii)* those that enrolled, but at a time different from the one determined by the random assignment. In focusing on this complier sample, we restrict our attention to the subpopulation of farmers who would join a small-farm income generating program. The majority of program costs are spent on participating farmers, so impacts on this subpopulation are the most relevant to policymakers.

To formalize the two-sided complier (2SC) estimator, we define three indicator variables. The variable B_i indicates assigned treatment, and equals 1 for eligible farmers who were assigned to the early treatment group, and 0 for those assigned to the late treatment group. The variable D_i indicates whether or not a farmer participates in the program when it was offered to them, so that $D_i = 1$ if the farmer participates and $D_i = 0$ if not. Finally, T_i identifies early and late period compliers:

$$\begin{cases} T_i = 1 \text{ if } B_i * D_i = 1 \\ T_i = 0 \text{ if } B_i = 0 \,\&\, D_i = 1 \end{cases}$$

In other words, complier farmers in the early treatment group are those who actually enrolled in the small-farm intervention when it was offered to them, and complier farmers in the late treatment group are those who enrolled in the program when it was eventually offered to them (after the second round surveys). We can therefore use just this complier sample and the first two rounds of data to compute the effect of the program on the sub-population of compliers as the following difference-in-difference estimator:

$$\delta_{2SC} = E\left[Y_{i,2}^{E} - Y_{i,1}^{E} \quad |T_i = 1 \quad\right] - E\left[Y_{i,2}^{L} - Y_{i,1}^{L} \quad |T_i = 0 \quad\right] \tag{1}$$

Superscripts indicate treatment (Early, Late), and subscripts denote survey round (1,2).

We estimate this difference-in-difference estimator by defining a fourth indicator variable, Z_{it}, which takes on the value 1 if farm i has been treated at time t. Using this new variable and differencing out the baseline, we can sweep away the fixed effect term and estimate the model as

$$E[Y_{it} - Y_{i1}] = \lambda_2 t_2 + \lambda_3 t_3 + \delta Z_{it}, \text{where } t = 2{,}3 \tag{2}$$

The parameter δ estimates the two-sided complier difference-in-difference treatment effect, and is identified entirely off of variation from the midline.

TABLE 1 ABOUT HERE

Before presenting the LATE results, Table 1 **– Intermediate Outcomes**displays some descriptive statistics on farm technology use and production by producer group. The variable “manzanas planted” is the total area that a household planted in the RBD target crop in the survey year, and can be thought of as a measure of the intensity of production in the target crop given that farmers have a (mostly) fixed amount of land at their disposal[6]. The variable "improved seed" is the percentage of households that used an improved seed variety for the target crop, and measures one aspect of farmers' utilization of improved technology. For dairy farmers, the measure is whether farmers applied processing to their products before taking them to market ("processing"). "Income" represents the total value of production in the target crop. While maize was not a program activity, it is an important staple crop that most households produce, and is included as a signal of whether program crop expansion comes at the expense of reduced output and income from other crops.

The effects were quite diverse across the different target crops. For bean growers, those who enrolled in the program early planted more beans, received higher prices at the midline than the control group, and more of them used improved seeds – and all these differences are highly statistically significant. By the end-line, the early group and the late group are somewhat more similar, but some differences appear to persist, indicating that program participation is not as simple as a dichotomous "On" or "Off" status.

As for maize production, the differences between early and late treatment groups don't change much between the baseline and the midline, which suggests that the early treatment group at the midline did not substitute away from maize in order to concentrate on target crops. This constitutes suggestive evidence that measured income increases from targeted activities are less likely to greatly overstate overall income. With this in mind, we now examine the program's LATE.

TABLE 2 ABOUT HERE

Table 2 reports the results from the two-sided complier difference-in-difference. As explained above, farm income is defined as total value of production in the target crop. Capital combines the value of tools and equipment, and installations such as fences located on the farmer's land. Household consumption, finally, includes expenditures on food, health, education, a yearly use-value of household durables, and all other non-farm related expenditures. As can be seen in Table 2, the program impacts on farm income are $1,200[7], which is statistically significant at the 10% level. The point estimate implies roughly a 15% increase over average baseline levels. While the point estimates on investment and consumption are positive, we can't reject the null of zero impacts.

There are a number of possible reasons why we might expect the impact of the program to have evolved over time. In addition to a possible initial dip in living standards when households first joined the program and focused their resources on building up the targeted activity, there are several other reasons why the impact of this type of small-farm program may have changed over time. First, program beneficiaries may have experienced a learning effect with their technical and entrepreneurial efficiency improving over time. Second, the asset program may have created a crowding-in effect if the program incentivized beneficiaries to further invest in their farms. As Keswell and Carter (2011) discuss, it is these second round multiplier effects that distinguish business development and asset transfer programs from cash transfer and other anti-poverty policy instruments.

If the impacts evolved over time, the question is whether the estimates in Table 2 reflect the 'true' or longer-term impacts of the program. In order to examine this question more carefully, we exploit variation in the precise timing of program in what follows.

Identifying Impact Dynamics

Given the logic spelled out above, the duration response function (the relationship between program impact and the duration of time since the treatment began) is unlikely to be a simple step function that can be approximated with a binary treatment estimate. Empirically, we measure duration (d_i) at each round as the number of months between the time of the survey and when the program initiated activity in farmer i's geographical cluster. Figure 2 shows the spread of these durations in the data set[8].

Since we don't have clear reasons to assume a particular functional form for the duration response path, we examine its shape using semiparametric estimation. In particular, we employ Baltagi and Li's (2002) fixed effects semiparametric estimator, as implemented by Verardi and Libois (2012)[9]. Starting from a panel data model of the form

$$y_i = x_i \theta + f(d_i) + \alpha_i + \varepsilon_i , \quad i = 1, \ldots, N; \quad = 1,2,3 \tag{3}$$

the household fixed effects are eliminated by first differencing, and the unknown function $f(d_i)$ is consistently estimated, and fitted using B-splines[10]. Time fixed effects enter linearly in the estimating equation to account for differences in market conditions or weather between the survey rounds. Figure 2 – Months between survey and program enrollment
plots the smoothed relationship between months of treatment and the outcome variables, as well as the 95% confidence intervals.

As we can see, in all cases except arguably household consumption, the impact dynamics are important. For the investment variable especially, the nonparametric curves demonstrate that impacts grow substantially over time. Taking the impact at 0 months of treatment as the counterfactual, we can see that the impact on investment is many times larger than the binary estimates in Table 2 suggest.

Conclusion

Emerging from these semi-parametric estimates is evidence that ignoring the evolution of impacts over time of a program where learning and behavioral changes take time to emerge can lead to a loss of information, and biased estimates of program impacts.

While interesting, these results may not tell the full story. There are many reasons to believe that programs like the RBD may result in heterogeneous treatment effects, and these results only reveal the effects of the program for the average producer. It might then be of interest to study the effects for different parts of the population in future analysis.

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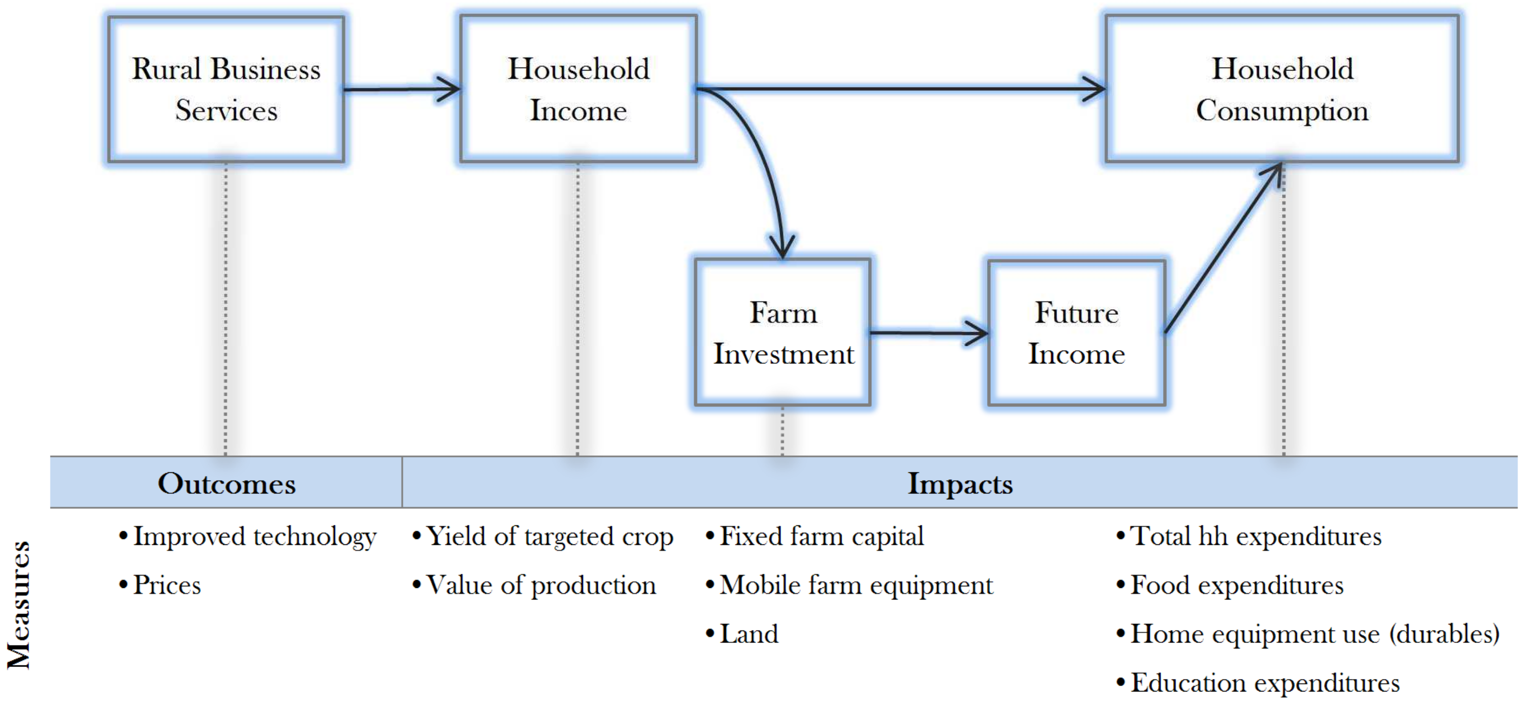

Figure 1- Flowchart of hypothesized program outcomes

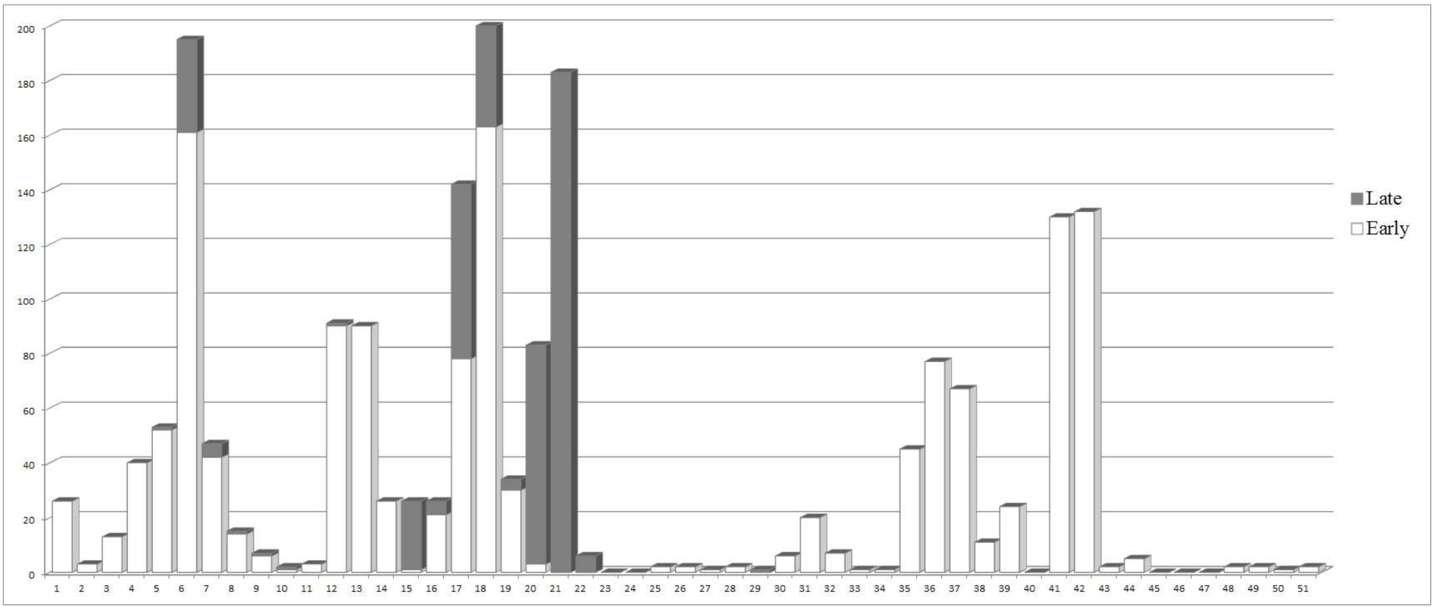

Figure 2 – Months between survey and program enrollment

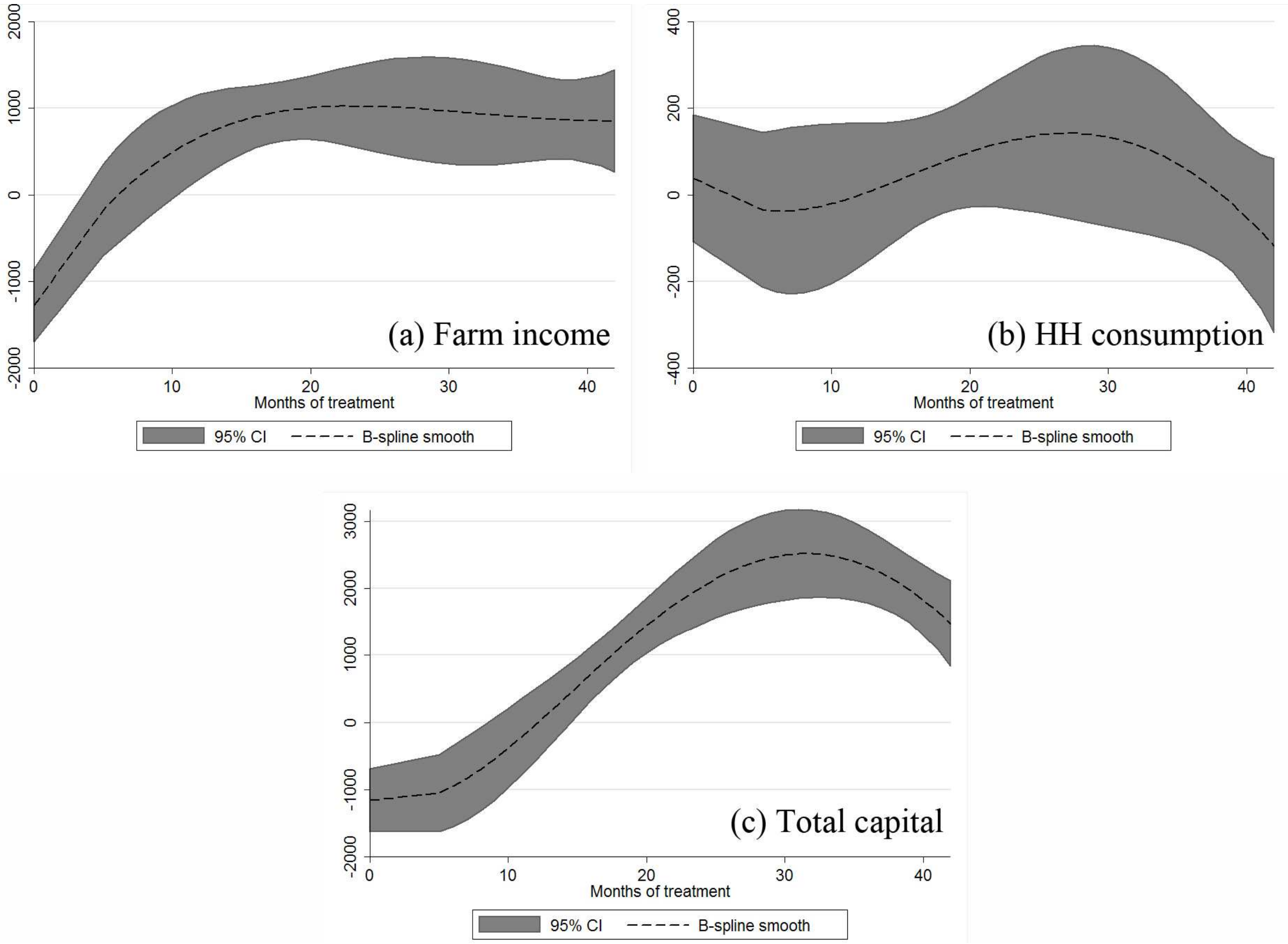

Figure 3 – GAM of duration response path

Table 1 – Intermediate Outcomes

	Baseline		Midline		End-line	
	Early	Late	Early	Late	Early	Late
Beans						
Value of production[a]	11416	10616	20653***	14421***	11461	9359
Used improved seed (%)	0.109	0.0752	.144***	0.098***	0.284*	0.197*
Manzanas planted (#)	3.35	3.03	4.6***	3.18***	3.53***	2.61***
Price[a,b]	434	427	823	786	1010	971
N	133	183	132	185	128	176
Sesame						
Value of production[a]	28888	28191	40447*	29107*	48463	36169
Used improved seed (%)	0.456***	0.692*	0.62	0.618	0.434***	0.807***
Manzanas planted (#)	5.32	5.73	5.73***	3.94***	5.27	4.37
Price[a,b]	618***	517***	1276***	1135***	1409*	1318*
N	110	86	109	86	93	66
Cassava						
Value of production[a]	50307	37585	74520	42177	32225	66600
Used improved seed (%)	0.064	0.056	0.17**	0.023**	0.171	0.077
Manzanas planted (#)	7.78	6.89	4.84	4.56	2.93*	5.06*
Price[a,b]	44.74	47.56	168.79	169.3	84.64	88.23
N	59	50	52	49	55	42

Maize						
Value of production[a]	23816**	22018**	13836***	11523***	11067*	10211*
Improved seed (%)	0.246	0.244	0.256	0.241	0.158	0.131
Manzanas planted (#)	3.14**	3**	2.18***	1.91***	2.48***	2.24***
N	414	429	525	540	523	536
Milk						
Value, livestock production[a]	267873	291512	296921	276303	236171	253254
Value, milk production[a]	112144	120104	167529	163613	164378	183587
Processing (%)	0.013	0.027	0.323	0.315	0.598**	0.493**
Price[a,b]	4.24	4.22	6.65	6.51	6.8	6.87
N	220	208	220	208	218	205

The asterisks denote the statistical significance of t-tests on the equality of early and late complier group means

[a] The values and prices in this table are measured in cordobas.

[b] Prices are standardized to a single unit (for example, liters for milk and a single weight unit for the others)

$* \, p < 0.10$, $** \, p < 0.05$, $*** \, p < 0.01$

Table 2 – Local Average Treatment Effects

Farm Income	Mobile Capital	Fixed Capital	Per Capita Consumption
1778***	275.6*	-29.3	-476.5***
(421.57)	(162.38)	(171.48)	(147.34)
363.7	3503.8***	1084.1***	-211

	(774.80)	(398.36)	(263.31)	(227.03)
	1211.7*	215.5	276.4	186.5
	(652.07)	(207.39)	(224.11)	(187.98)
	2001	2106	2092	2123
$\bar{R}$	0.044	0.109	0.059	0.004

Cluster-robust standard errors in parentheses

All figures in 2005 PPP-adjusted US$ $* p < 0.10$, $** p < 0.05$, $*** p < 0.01$

[1]See also Ravallion (2009).

[2] The term *compliers* accords with the definition in Angrist, et al. (1996), i.e. it refers to individuals who are induced to take the treatment by assignment to the treatment. In our case, compliers join the program in the early period if they were assigned to early treatment, and in the late period if they were assigned to late treatment.

[3] The activities evaluated were beans, livestock/dairy, cassava, sesame and vegetables.

[4] Baseline characteristics suggest that the randomization worked quite well, and that households in the early and late treatment groups were similar along all but a few dimensions.

[5] Around 60% of farmers chose to enroll in the program.

[6] 1 manzana equals approximately 1.72 acres

[7] Unless noted otherwise, all dollar values are measured in 2005 PPP-adjusted US dollars.

[8] The histogram excludes 0 for scale reasons: since almost all households at baseline plus all 'late' households at midline have 0 months of treatment, there are many 0's.

[9] Verardi and Libois (2012) implement the estimator as a Stata command named xtsemipar, and the estimates shown use the B-splines version of the program.

[10] The estimating procedure (parametric estimation of a first-differenced version of equation (3), in which $f(d_{i}) - f(d_{i\,-})$ is approximated by a difference of 4th-degree B-splines, followed by nonparametrically fitting the curve $f(\cdot)$) is clearly outlined in Verardi and Libois (2012)