

# IDENTIFYING THE IMPACT DYNAMICS OF A SMALL-FARMER DEVELOPMENT SCHEME IN NICARAGUA

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Social programs designed to induce long-term behavioral changes (e.g., as a result of improved incentives or new knowledge gained during the intervention) pose unique challenges for impact evaluation. One such challenge in the context of randomized control trials is deciding the time at which the treatment and control groups should be compared—typically, we do not know *ex ante* how long it will take for the desired behavioral change and resulting welfare impacts to take place in the treatment group.

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 for a longer time period. Barahona (2010) argues that 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.<sup>1</sup> 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] for an in-depth discussion).

In addition, paying careful attention to timing can be motivated by the fundamental search for correctly estimated impact parameters; getting the evaluation timing wrong could lead to incorrect estimates of the long-term impacts. We argue that this issue should be considered an issue on par with standard econometric concerns such as sample selection. For example, 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 sustained this growth over the next three 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 received their assets, the gradualness of coinvestment 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, the rollout of which allows us to estimate both the standard impact parameters (local average treatment effect, or LATE) and a duration response path that characterizes 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. We first estimate the local average treatment effect (or the effect of treatment on compliers<sup>2</sup>) of the program on several outcomes (primarily

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<sup>1</sup> See also Ravallion (2009).

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<sup>2</sup> The term *compliers* accords with the definition in Angrist, Imbens, and Rubin (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.

farm income, per-capita household consumption, and capital investments) using midline data and a two-sided complier estimator. The data used for this estimation were collected a year after the first group was enrolled, but before the “late” group was enrolled.

Next, exploiting the fact that farm households were de facto randomly enrolled at different points in time, we estimate the evolution of impacts over time using a semiparametric duration response-path of impacts over time. This continuous approach uses three rounds of data collected over 3.5 years after the beginning of the program and allows us to extract more information than the binary impact estimate that typically emerges from impact evaluations. We believe that this technique in general can allow researchers to continue learning even after the control group has been treated. The next section describes the program, the data, and the estimation procedures in more detail.

**Program Details and Data**

*The Small-Farm Development Program*

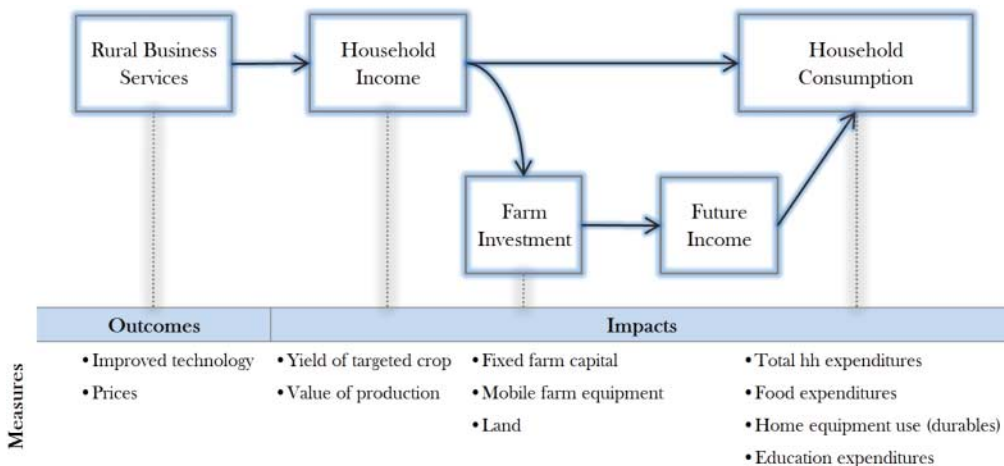
The program that we evaluated was a multi-year small-farm development program in Nicaragua, initiated in 2005 when the Millennium Challenge Corporation (MCC) signed a five-year compact with the government of Nicaragua. The goal was to implement a set of development projects in the departments of Leon and Chinandega, known as the Western Region. We focus herein on the project called the Rural Business Development (RBD)

program, which aimed to raise 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. Once a farmer’s business plan was 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.

The flowchart in figure 1 illustrates how the program may have influenced key development outcomes. The program focused on specific agricultural activities (beans, livestock/dairy, cassava, sesame, and vegetables) and was designed to enhance the access of small farmers to improved technologies and to markets. We therefore examine these intermediate outcomes (e.g., prices and use of technology) separately from the impacts that directly influence household welfare (farm income, investments, and household consumption).

*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 clusters 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



**Figure 1. Flowchart of hypothesized program outcomes**

midline.<sup>3</sup> 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.<sup>4</sup> 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 data set 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 conduct our analysis on the subgroup of farmers who participated in the program at the time designated by the random assignment. 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 midline survey). 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 that farmer  $i$  was 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 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:

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

We can therefore use just this complier sample and the first two rounds of data to compute the effect of the program on the subpopulation of compliers using a standard difference-in-difference estimator.<sup>5</sup> To estimate, we define 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, we can write

$$(1) \quad E[Y_{it}] = \alpha_0 + \lambda_2 t_2 + \lambda_3 t_3 + \delta Z_{it} + \alpha_i$$

where  $t_2$  and  $t_3$  are time dummy variables and  $\alpha_i$  denotes the household fixed effect. Differencing out the baseline, we can sweep away the fixed effect term and estimate the model as

$$(2) \quad E[Y_{it} - Y_{i1}] = \lambda_2 t_2 + \lambda_3 t_3 + \delta Z_{it},$$

where  $t = 2, 3$ .

The parameter  $\delta$  estimates the two-sided complier difference-in-difference treatment effect and is identified entirely off variation from the midline survey.

Before presenting the LATE results, table 1 displays some descriptive statistics on farm technology use and production by producer group. The variable “manzanas planted”<sup>6</sup> is the total area that a household planted in the RBD target crop in the survey year and can be thought of as one measure of the intensity of production in the target crop given that farmers have a (mostly) fixed amount of land at their disposal. 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 processed their products before taking them to market (“processing”). “Value of production” represents the monetary value of a farmer’s production in the target activity. 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 target crops. The bean growers who were randomized and enrolled into the program early planted more beans, received higher prices,

<sup>3</sup> Baseline characteristics suggest that the randomization worked quite well and that households in the early and late treatment groups were similar along most dimensions.

<sup>4</sup> Around 60% of farmers chose to enroll in the program.

<sup>5</sup> Because of the randomization, we could conduct the impact evaluation using a single-difference estimator but choose to use a difference-in-difference to be conservative.

<sup>6</sup> One manzana equals approximately 1.72 acres.

**Table 1. Intermediate Outcomes**

	Baseline		Midline		Endline	
	Early	Late	Early	Late	Early	Late
<i>Beans</i>						
Value of production <sup>a</sup>	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 <sup>a,b</sup>	434	427	823	786	1010	971
N	133	183	132	185	128	176
<i>Sesame</i>						
Value of production <sup>a</sup>	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 <sup>a,b</sup>	618***	517***	1276***	1135***	1409*	1318*
N	110	86	109	86	93	66
<i>Cassava</i>						
Value of production <sup>a</sup>	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 <sup>a,b</sup>	44.74	47.56	168.79	169.3	84.64	88.23
N	59	50	52	49	55	42
<i>Maize</i>						
Value of production <sup>a</sup>	23816**	22018**	13836***	11523***	11067*	10211*
Used 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
<i>Milk</i>						
Value, livestock production <sup>a</sup>	267873	291512	296921	276303	236171	253254
Value, milk production <sup>a</sup>	112144	120104	167529	163613	164378	183587
Processing (%)	0.013	0.027	0.323	0.315	0.598**	0.493**
Price <sup>a,b</sup>	4.24	4.22	6.65	6.51	6.8	6.87
N	220	208	220	208	218	205

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

<sup>a</sup>The values and prices in this table are measured in cordobas (NIO).

<sup>b</sup>Prices are standardized to a single unit (for example, liters for milk and a single weight unit for the others).

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

and were more likely to use improved seeds than their control-group counterparts—and all these differences are highly statistically significant. By the endline, the early group and the late group are somewhat more similar, but some differences appear to persist, indicating that program participation is perhaps not as simple as a dichotomous “On” or “Off” status.

As for maize production, the differences between early and late treatment groups change little 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 increases in measured income from targeted activities are less likely to greatly overstate overall income. With this in mind,

we now examine the program’s Local Average Treatment Effects.

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 nonfarm-related expenditures. As can be seen in table 2, the program impacts on farm income are \$1,200<sup>7</sup>, which is

<sup>7</sup> Unless noted otherwise, all dollar values are measured in 2005 PPP-adjusted U.S. dollars.

**Table 2. Treatment on the Treated**

	Farm income	Capital	Per capita consumption
$\lambda_2$	1778*** (421.57)	242.2 (234.3)	-476.5*** (147.34)
$\lambda_3$	363.7 (774.80)	4581*** (478.1)	-211 (227.03)
$\delta$	1211.7* (652.07)	503.6 (311.5)	186.5 (187.98)
$\bar{R}^2$	2001 0.044	2076 0.135	2123 0.004

Notes: Cluster-robust standard errors in parentheses. All figures in 2005 PPP-adjusted US\$.

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

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 cannot reject the null hypothesis 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 antipoverty 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 the program in what follows.

### Identifying Impact Dynamics

Given the logic 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 ( $d_{it}$ ) at each round as the number of months between time at which the program initiated activity in farmer’s geographical cluster and the time of the survey (i.e.,  $d_{it} = 0$  if the farmer has not yet been treated). Figure 2 shows the spread of these durations in the data set.<sup>8</sup>

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).<sup>9</sup> Starting from a panel data model of the form

$$(3) \quad y_{it} = x_{it}\theta + f(d_{it}) + \alpha_i + \varepsilon_{it}, \\ i = 1, \dots, N; t = 1, 2, 3$$

the household fixed effects are eliminated by first differencing, and the unknown function  $f(d_{it})$  is consistently estimated, and fitted using B-splines.<sup>10</sup> Time fixed effects enter linearly in the estimating equation to account for differences in market conditions or weather between the survey rounds. Figure 3 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 household consumption, the impact dynamics are clearly 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 semiparametric estimates is evidence that ignoring the evolution of impacts over time of a program, where learning and behavioral changes take time to emerge,

<sup>8</sup> 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 0s.

<sup>9</sup> 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.

<sup>10</sup> The estimating procedure (parametric estimation of a first-differenced version of equation (3), in which  $f(d_{it}) - f(d_{it-1})$  is approximated by a difference of 4<sup>th</sup>-degree B-splines, followed by nonparametric fitting of the curve  $f(\cdot)$ ) is clearly outlined in Verardi and Libois (2012).



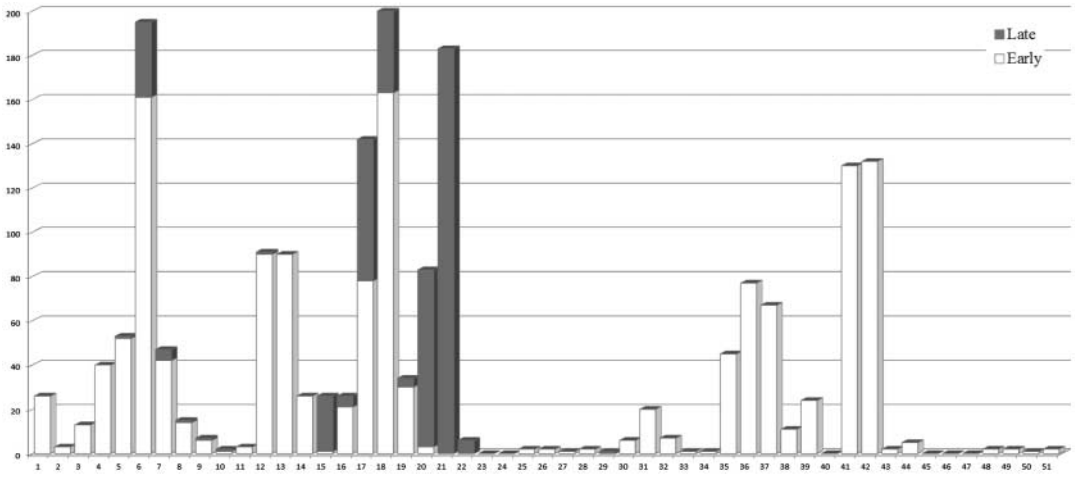


Figure 2. Months between survey and program enrollment

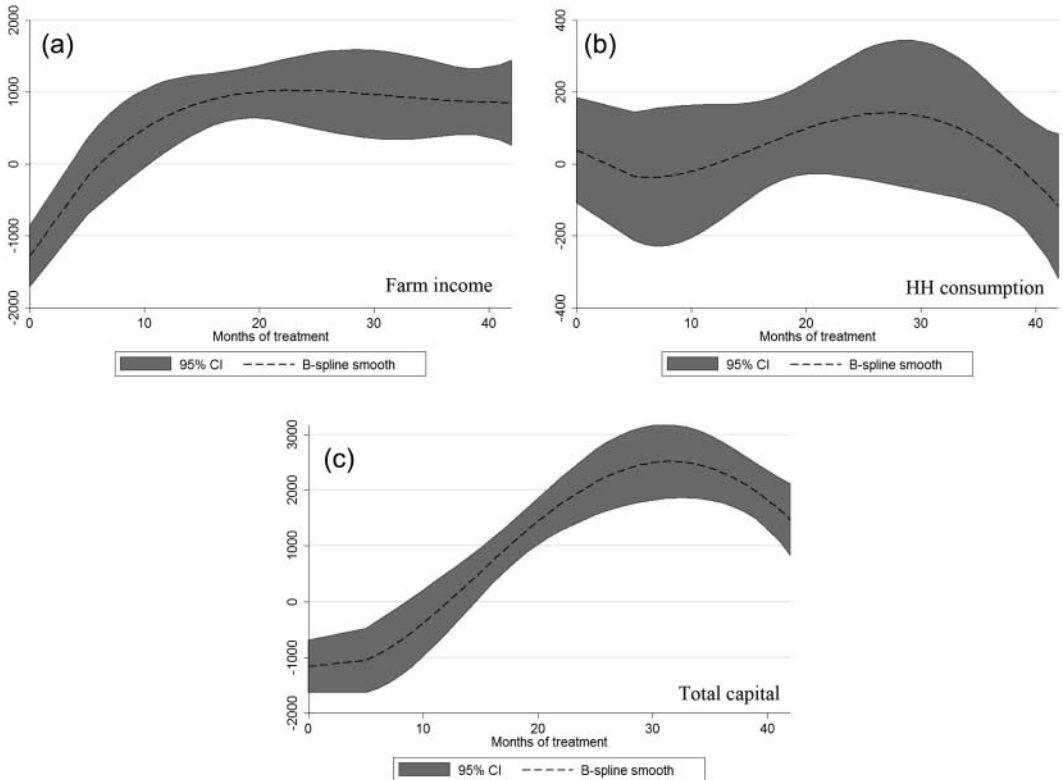


Figure 3. GAM of duration response path

can lead to a loss of information and misleading 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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