The Impact of Rural Business Services on the Economic Well-being of Small Farmers in Nicaragua

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Abstract

The Government of Nicaragua and the US foreign assistance agency, the Millennium Challenge Corporation, launched a rural business services program designed to boost the income of the small farm sector. Relying on a randomized rollout strategy, this paper reports the results of a multi-year impact evaluation that spanned the 5-year life of the program. We argue that impacts of a program of this sort are unlikely to be fully revealed by standard binary treatment estimators and show that temporal pattern of impact indeed evolves in important ways over time. Income in the activities targeted by the program steadily rose, plateauing at a 30% increase over baseline after two years in the program. The program also appears to have provoked significant increases in both mobile and perhaps fixed farm capital. However, on average there have been no significant impacts on household living standards to date. Finally, conditional quantile regressions give evidence of quite substantial heterogeneity in program impact.

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In July 2005, the Millennium Challenge Corporation (MCC) signed a five-year, $175-million compact[1] with the Government of Nicaragua to develop a set of projects in the departments of Leon and Chinandega, known as the Western Region. This compact stated three strategic projects which were focused on reducing transportation costs and improving access to markets (transportation project), strengthening property rights and increasing investments (property regularization project), and raising incomes for farms and rural businesses (rural business project).[2] These projects were inspired by the National Development Plan (NDP) formulated by the Nicaraguan government in 2005. In particular, the NDP strategy states that one important mechanism to reduce high levels of poverty is through programs that target sectors with more potential to accelerate growth.

The NDP identified the Western Region as the region with the best conditions to develop agricultural businesses. Although the Poverty Map of 2005 indicates that poverty there is milder than in other Nicaraguan regions, almost 50% of Western region's families live in poverty.[3] A key element in the MCC-Nicaragua was the provision of a suite of business services to farmers in this high potential area. Described in more detail below, the Rural Business Development (RBD) Program was designed to support farmers to develop and implement a business plan built around a high potential activity. Plans specified not only the the type of activity that a farmer had to develop, but also the type of services that the RBD Program would provide during a 24-month period of intensive treatment and training. Business services included expert technical assistance, marketing support, materials and equipment, with the objective of improving farm productivity, and consequently, households' economic well-being. In some cases, the plan required investment of installations that were co-funded by the RBD.

This paper reports the results of a multi-year impact evaluation study that spanned the entire life of the RBD program in Nicaragua. While the RBD concentrated on a number of different production activities (*e.g.*, livestock, sesame ....), the evaluation was designed to estimate a single impact averaged across this range of activities. While it is of course possible that program impacts were larger in some activities than in others, we do not in this study attempt to separately identify impacts on an activity-by-activity basis as the underlying sample size does not permit sharp inference at this more microscopic, sub-program level.

In what follows, Section 1 first describes the basic structure and goals of the MCC program, including evidence on the targeting of the program across households in the Nicaragua's Western Region. Section 2 describes the research strategy for evaluating the program, including the construction and validity of the sample which was built around a randomized program rollout strategy. No households were denied access

[1] The compact entered into force in May 2006.

[2] In June 2009, the MCC Board terminated a portion of the compact, reducing compact funding from $175 million to $113.5 million. While this action cut off the property regularization part of the program, the Rural Business Development Project was not affected as a result of this partial project termination.

[3] World Bank (2008), "Nicaragua Poverty Assessment".

to the program, but the temporal sequence in which they received the program was randomized over a two-year rollout period, with eligible households randomly split into early and late treatment groups. One unusual feature of this study is that shortly after the midline survey, it was possible to fully identify a two-sided complier sample as all eligible households in both early and late treatment groups had either accepted or declined the invitation to join the RBD program by that date. Section 2 discusses in detail the validity of using this two-sided complier sample as the basis for inference on program impacts.

Section 3 then puts forward the basic econometric methodology, reviewing first a simple causal model of how the program was supposed to work and influence technologies, incomes, investment and finally family economic well-being. Among other things, this review suggests that along at least some dimensions, we might expect the impacts of the program to evolve, perhaps slowly, over time. To explore the impact of RBD services on each of these dimensions, we put forward both binary and continuous treatment impact estimators. The latter are intended to pick up the temporal impact of the program and identify its long-term, policy-relevant treatment effects.

Section 4 then implements both the binary and continuous treatment estimators. All primary impact estimates come from statistically conservative, fixed effect (or fixed effect analogue) estimators. Even though date of enrollment in the program was randomized, these fixed effect approaches control for any baseline or other time invariant characteristics of households, including those that may have been spuriously or otherwise correlated with date of program enrollment. Our analysis shows that the program had its desired direct effects, as treated farmers indeed appear to have used better technologies and received better prices for their outputs. One step further along the causal change reveals that income in the targeted activities were substantially boosted by the program, perhaps by as much as 30% after several years in the program.[4]

How then did farm families on average spend their increased incomes? Interestingly, the program appears to have boosted investment in attached and non-attached agricultural capital, perhaps by as much as 50%. Somewhat surprisingly, the estimated average increase in per-capita consumption (a measure of economic well-being of farm households) is only about 5%, and is not statistically significant. There is modest evidence that this apparent division of income increases between increased investment and increased consumption is shaped by the intra-household distribution of bargaining power and preferences as it appears that women farmers spend more of their incremental income boosting household living standards and less on investment.

While these estimates of average program impacts are interesting and important, they do not tell us about the heterogeneity of program impacts. Specifically, do programs that aim to improve the agricultural business prospects of a small farm population work equally well for all, or do they work better for a subset of privileged producers (such as those who enjoy better access to capital, or who have better en-

[4]It should be stressed that income increases in the targeted activities may overstate the overall increase in household income as changes in program income do not account for any decreases in non-program income induced by the program. The 30% is thus an upper-bound estimate on the total income increase received by program participants.

dowments of skills and business acumen)? While the failure of a program to work uniformly well for all participants is in no way a condemnation of the program, it is clearly important to understand for whom and for how many families the program actually works to boost living standards and combat poverty.[5] To explore this issue of impact heterogeneity, Section 5 employs fixed effect analogue generalized quantile regression techniques to determine the extent to which estimated average impacts are indicative of the full range of impacts experienced by MCC program participants. Consistent with results from the midline data (Carter and Toledo 2011), the full analysis here reveals quite striking heterogeneity in impact. The top 25% performers (as gauged by the generalized conditional quantile analysis) enjoyed income impacts double those of the average producer and also enjoyed statistically significant increases in consumption. In contrast, the lowest 25% performers experienced modest income gains and no change in consumption.

Using these various estimated program impacts, Section 6 calculates internal rates of returns on the public funds invested in the RBD program. When impacts evolve, there are multiple ways to define the internal rate of return of a program. Using the assumption that returns estimated to occur in the medium term persist in the future (a favorable assumption), we calculate an average internal rate of 14% based on the estimated impacts on program income. The rates of returns based expenditure impacts are much more modest. Section 7 summarizes the study and drawing out its implications for future agricultural development programs.

1. The MCC Rural Business Program in Nicaragua

The Rural Business Development (RBD) Program is one of the components of the Millennium Challenge Account in Nicaragua (MCA). The project started in 2007 and targeted Leon and Chinandega, two administrative departments commonly known as the "Western region". Agriculture has played a central role in Nicaraguan history, and to the Western region. Still, multiple constraints remain that have not allowed agriculture to reach its productive potential. Examples include lack of basic infrastructure, low education levels, and low access to credit and technology. These constraints are not unique to the Western region, but the rural population in the Western region are predominantly poor, with 73% of the rural population under the national poverty line, a figure comparable to other rural areas in the country.[6] However, in terms of the distribution, extreme poverty in the Western region is milder than in the rest of the country.

The RBD Program was designed to develop business plans with land-owning farmers in select municipalities of the Western region. The program targeted four business groups: livestock and fishing, agricultural business, non-agricultural business, and forestry. Livestock and agriculture constitute the largest groups, representing 70% of program beneficiaries. Table 1 provides the breakdown of the costs of the Rural Business Development program.

[5]This observation is especially true when it is necessary to make judgments between, the poverty reduction effectiveness of say, cash transfer programs and more complex value chain programs like that implemented by the MCC in Nicaragua.

[6]Source: 2005 LSMS Nicaragua's survey. According to the National Institute of Development Information (INIDE), the monthly poverty line in 2005 was 596 Cordobas per capita.

Table 1: RBD Project Costs

Rural Business Development Program	**Executed budget (millions)**[a]
Rural Business Development Services	$7.2
Technical and Financial Assistance	$17.1
Rural Business Development Services - ProNicaragua	$1.3
Grants to improve water supply for farming and forestry	$7.1
Total	**$32.7**

[a]These figures are all in 2012 US$

1.1. Program Logic and Design

While the program was advertised throughout the Western region, MCA identified areas particularly conducive to the types of business targeted by the program. The implementation strategy was mainly based on the identification of groups of farmers called *nuclei*. A nucleus was determined by the productive activity that the program would support (e.g. crops or livestock). Most of the nuclei were formed by farmers who were selected as individuals, not as part of an organization.

Every nucleus was supposed to be constructed around a leader farmer, and 10 to 15 satellite farmers whose parcels should be located relatively close to the leader's land. The leader was supposed to be willing to invest more than the satellite farmers (for example, he should be willing to use part of his land to build a milk collection center that will be used by all the members in a nucleus), and to coordinate technical meetings.

A large campaign was launched to encourage program participation, and offices were created in the main cities of León and Chinandega. Farmers would then visit the offices and ask to participate. In addition, the farmers had to fulfill certain requirements, defined as follows: "... a small or medium size farming and livestock farmer with potential, who has and is developing a productive activity in a farm, who in his proposal of business plan is willing to contribute 70% of what he has to invest, and that the estimated internal return rate (IRR) be at least 18%..."[7]

A small or medium size farmer corresponds to the typology of producers developed by the National Development Plan (NDP). Both small and medium size farmers face constraints, such as informal land tenure, lack of access to financial services, low or varying product quality, entrepreneurial and technological weaknesses, and lack of access to markets. The main distinctions between these two groups of farmers boil down to their degree of isolation and the amount of net capital they own. In general, small farmers ("poor producers with basic productive capacities") live in isolated areas and own net capital between US$20,000 and US$50,000 for a household of 6 members. Medium-scale farmers own net capital between US$50,000 and US$100,000 for a household of 5 members.[8]

[7]Source: Millennium Challenge Account-Nicaragua (2009),"*PNR: Los Beneficiarios Directos de Planes de Negocios*", document, October.

[8]Source: Ministry of Agriculture and Forestry of Nicaragua (2006),"*Politica y Estrategia para el Desarrollo Rural Productivo*".

These concepts were subsequently translated into a set of eligibility rules, specific to each of the productive activities promoted by the RBD. Table 2 shows the actual eligibility patterns as applied to the five activities that are included in this study: sesame, beans, vegetables, cassava and livestock. As can be seen, these rules feature both asset floors and ceilings, which effectively limited the program to the upper half of the rural income distribution. The topic of targeting will be explored in more depth below.

Table 2: Eligibility Criteria Used to Identify Farmers in Agricultural and Livestock Target Areas

	Sesame	*Beans*	*Vegetables*	*Cassava*	*Livestock*
Asset Floor*	7 hectares	3.5 hectares	1.4 hectares	3.5 hectares	10 mature cows
Asset Ceiling	35.2 hectares	35.2 hectares	14.1 hectares	70.4 hectares	100 mature cows
Prior Experience	1.4 hectares	0.7 hectares	Some vegetable	1.4 hectares	Developed livestock
	in sesame	in beans	production	in cassava	activity
Water	−−	−−	On-farm water source	−−	On-farm water source
Legal Status	Farmer has land title or is in possession of land				
Age	Farmer must be at least 20 years old				
Environment	Land located outside of national protected areas				
*Minimum farm size reduced when farm is irrigated					

In order to participate in the program, eligible farmers had to develop an individual business plan with the support of RBD Program professionals. Only one farmer per household was allowed to develop a business plan.[9] Once a farmer formulated her business plan she had to provide information about her current productive activities and her tenure status. Based on this information, five technical and seven financial criteria were applied. These criteria sought to render concrete the relatively vague statement of eligibility, and in addition added an upper limit on the amount of capital that a beneficiary could own to receive financial contributions from the program (US$ 8,300 per capita[10]). [11]

After the approval of a farmer's business plan, the RBD Program worked with the farmer during approximately 24 months. The specific benefits of the program differed across the productive activities. In general, all farmers received technical and financial training and supplies based on their individual business plan. If investment was part of the business plan, the RBD Program could contribute up to 30% of the financial resources needed. Some farmers also participated in "collective plans" that involved activities like a commercialization network, or the establishment of a milk collection center. Additionally, some farmers received forestry supplies.

[9] A household was defined as a group of people living in the same house and sharing expenditures.
[10] Note that this figure is not in PPP US dollars
[11] The limiting value of capital was a requirement incorporated in February 2008. That is, it was established after the implementation of the program.

1.2. Program Implementation

At the beginning, the RBD Program focused on the formation of livestock, bean, sesame and cassava nuclei, but given the interest shown by farmers in planting other crops, the program was extended to products like plantain, rice, honey, and fruits.[12] Additionally, it was decided that collective business plans would allow larger impact by combining individual plans with the construction of infrastructure that would be shared by farmers involved in the plan. Based on that, collective plans were designed for bean cooperatives in the second year of the program (2008). This type of intervention continued through 2009 and 2010 and was extended to cassava and other crops. However, while the model of intervention changed modestly, the support received by the group of beneficiaries in terms of inputs, technical and financial assistance, and types of investments, remained similar to the intervention model based on nuclei. In terms of the number of farmers involved in every type of intervention, a nucleus was comprised of 15 farmers on average. In contrast, collective plans have more variation in terms of their size with an average of 41 farmers per collective plan but with some of them reaching more than 100 participants. In some cases, collective plans involved the creation of only one business plan where the contribution and expected results were specified as a total value per plan instead of per farmer.[13]

By July 2011, the total number of beneficiaries was 10872 rural entrepreneurs, most of them in farm-related businesses. About 20% of them had business plans that were canceled before the 24-month enrollment period. Forestry and agricultural groups have the highest rate of canceled plans (27% and 20% within their groups, respectively).[14]

Not counting the canceled plans, 57% of participants enrolled in the program with individuals plans. Around 6% of farmers with individual plans also participated in a collective plan to complement the activity developed in their individual plans. In general, collective plans were focused on forestry and agricultural groups. Beans and other crops such as rice and plantain have the highest proportion of collective plans. Table 3 shows the distribution of such participants. The last farmers to participate entered the program by March 2011.

Figure 1 shows a map of the farmers enrolled in the program with business plans in the crops that this design included. Rice and honey farmers are also displayed on the map because their farms are in the frame area used for the sample design. Beneficiaries of forestry campaigns were evenly distributed in León and Chinandega and they were relatively more concentrated in the municipalities of Leon and El Viejo (not displayed in the map). Livestock and bean groups constitute the largest participant groups (see Table 4).

[12]The program was also extended to non-agricultural activities such as artisan production and tourism. While the evaluation only directly covers sesame, beans, vegetables, cassava and livestock, given the nature of the intervention the current evaluation applies to all activities under the first two line items in Table 1.

[13]It is important to note that for collective plans the RBD Program does not have records of eligibility criteria of every farmer (e.g., farm size, water access in farm, tenure status, etc).

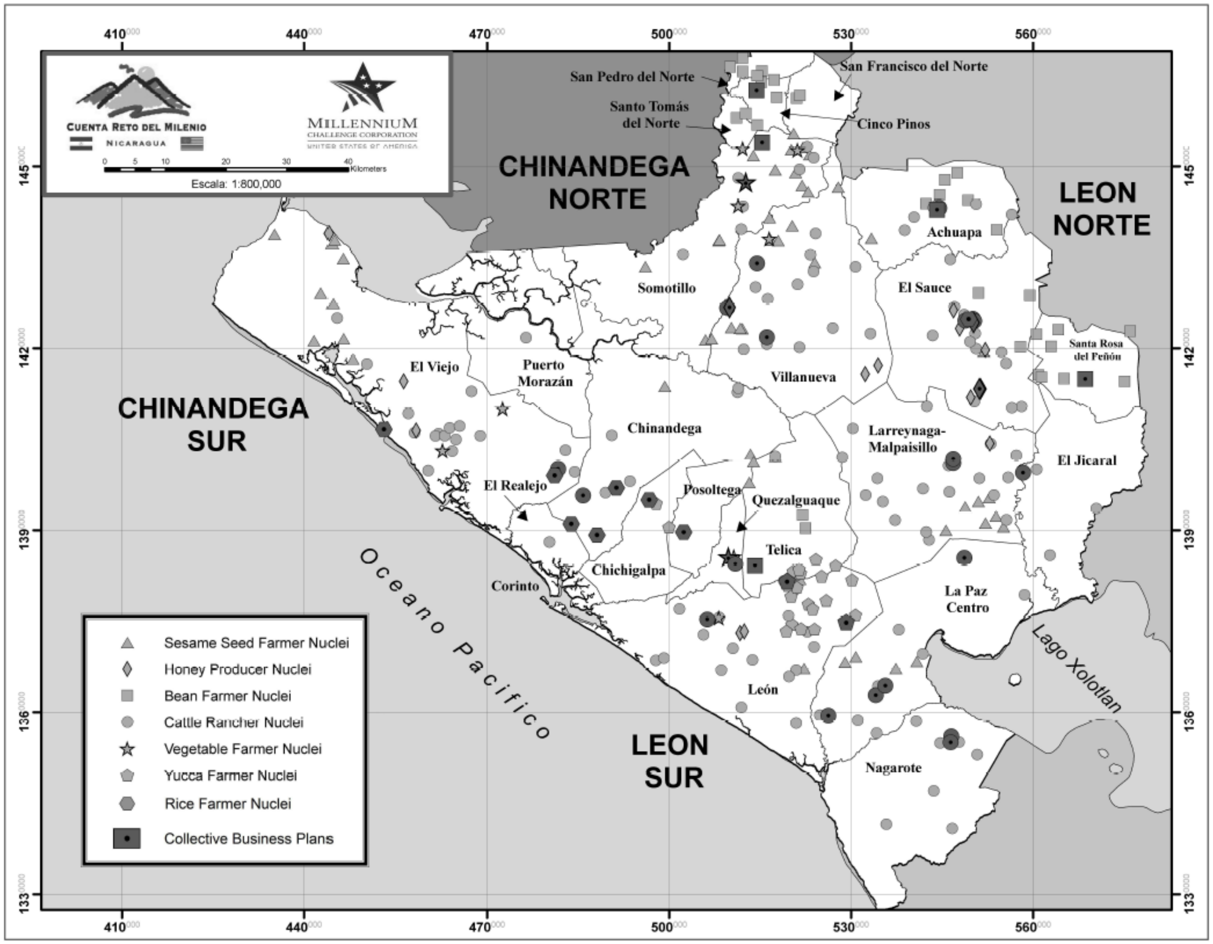
[14]Most of the forestry plans were canceled because trees did not reach the minimum growth level. Canceled plans in agricultural groups correspond mainly to bean, plantain, and sesame crops. Posterior evaluation of the eligibility requirements made these farmers non-eligible to continue in

Table 3: Distribution of RBD Program Beneficiaries

Group	Subgroup	Only individual plan	Individual and collective plan	Only collective plan[a]	Total
Livestock and Fishing	Livestock	1370			1370
	Milk collection center	1	131[b]	103	235
	Dairy processing plants		1[b]	46	47
	Livestock and forestry	148		5	153
	Fishing	1		182	183
Agricultural	Sesame	742			742
	Bean	612	12	1008	1632
	Cassava	300	50	30	380
	Vegetables	54	47	47	148
	Others[c]	469	254	384	1107
	Agricultural and forestry[d]	118			118
Non-agricultural[e]		19		406	425
Forestry		1165		1040	2205
Total farmers		**4999**	**495**	**3251**	**8745**
Farmers with canceled plans	All groups	1094		1033	2127

(a) A collective plan could involve a cooperative or a group of farmers with the intention to participate in a formal association. (b) Individual plan is in livestock. (c) Others: rice, plantain, honey, fruits, and cashew. (d) Agricultural components in the business plan can be: sesame (51), bean (26), cassava (17), vegetables (7) or others (17). (e) Non agricultural: handicrafts, rural tourism, and food and beverages processing. Source: BP database as of July 2010, MCA Nicaragua.

Figure 1: Distribution of RBP Beneficiaries by Crop



Source: MCA-Nicaragua database. Every symbol represents a nucleus with individual plans or collective plans. Geographic location of a nucleus was represented by the farm leader location. In collective plans, geographic location corresponds to cooperative's office or location of main installation (e.g., milk collection center).

Table 4: Distribution of Nuclei and Collective Plans: Ex-post Target Population

Crop	Number of Nuclei	Farmers per nucleus	Number of Collective Plans	Farmers per collective plan	Number of farmers
Livestock and installations[a]	115	14	23	7	1805
Bean	35	19	5	202	1658
Sesame	54	15	0	0	793
Cassava	22	17	2	15	397
Vegetables	9	12	2	24	155

(a) Installations are milk collection centers and dairy processing plants.

Note that the population of beneficiaries was not observed at the moment that the experimental design was created. The experimental design additionally took into account the Property Regularization Program (PRP), which was subsequently canceled. In contrast to the RBD Program, the PRP implementation could not be adjusted with respect to the time line and the geographic sequence.

1.3. Program Targeting[15]

Before turning to the evaluation of RBD program impacts, we present the targeting of the program in some detail. We use the sample data (described in more detail in the next section) to compare the pre-intervention living standards of RBD participants with the living standards of a representative survey of households in Nicaragua's Western Region.

According to a national living standards measurement survey carried out by the Instituto Nacional de Información de Desarrollo (INIDE), 34% of the rural population in León and Chinandega subsisted on less than a $2 per-person, per-day poverty line in 2005. (This and all other figures in this report are expressed in 2005 purchasing power parity adjusted US dollars, or PPP US$, unless stated otherwise.) The income distribution for the INIDE rural León and Chinandega sample is presented as the dashed line in Figure 2.

How do those who participated in the RBD project compare to these figures? As shown by the solid line in Figure 2, only 2% of RBD participants were below the standard "$2-a-day" poverty line prior to initiation of the program. From the figure we can also see that the eligibility criteria effectively targeted direct benefits toward the upper 50% of the rural income distribution in León and Chinandega, with the median income of participants approximately US$6 a day. It may be that the lower 50% of people will benefit indirectly through job creation, but such an analysis is beyond the reach of the present impact evaluation.

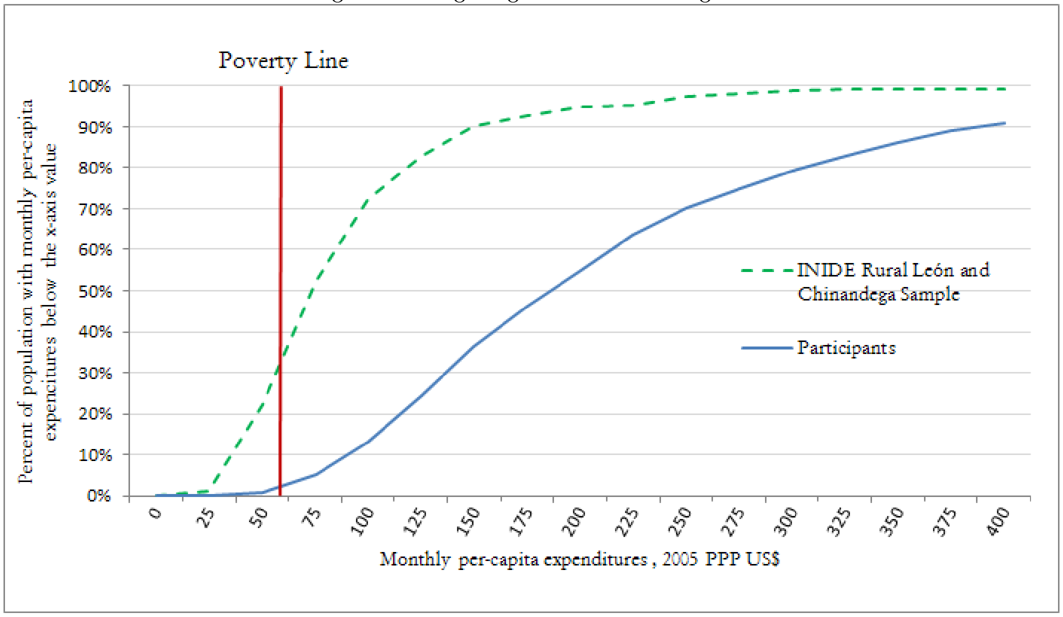
2. Sample and Validity

The challenge of this and all impact evaluation efforts is to identify a control group that is identical to the treatment group in every way except that they have not benefited from the intervention under evaluation. Our strategy for this study

the program. There was not accurate information of the date when plans turned to be inactive.

[15] This section repeats results reported in Toledo and Carter, 2010.

Figure 2: Targeting of the RBD Program

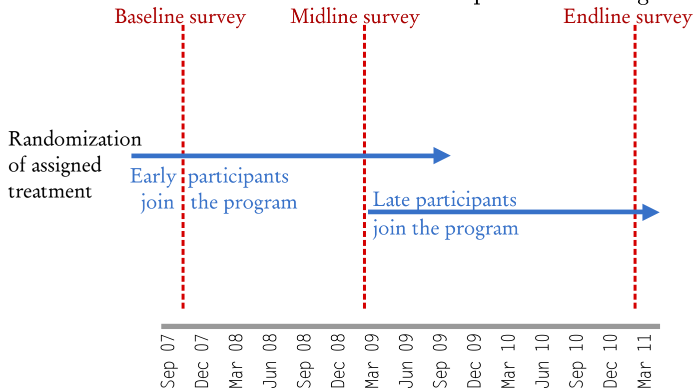


exploits the fact that the capacity constraints meant that not all eligible farmers could be brought into the project immediately. As will be detailed in Section 2.1, the evaluation team worked with the RBD implementation team to identify all geographic clusters that would eventually be offered RBD services. The evaluation team then selected a subset of these clusters for random assignment to either early or late treatment status. This strategy not only created a temporary conventional control group, it also randomized the duration of time in the program, a feature that will prove vital in the continuous treatment estimates presented below.

As shown in the Figure 3 timeline, RBD services were provided in early treatment clusters beginning in late 2007. In late treatment clusters, services were not initiated until approximately 18 months later, in early 2009 at the time of the midline survey. Because clusters were randomly allocated to early and late treatment conditions, we can anticipate that on average the late treatment group should function as a valid control group, identical to the early group in every way except early receipt of RBD services. The economic status of the late group in 2009 should thus be a good predictor of what the status of the early group would have been in the absence of RBD services. Both early and late treatment clusters were then surveyed again near the end of the program in 2011.[16]

[16] Figure 3 shows the timeline of the received treatment. The implementation of the experimental-sample design and the first round of the survey were carried out almost at the same time that the program started identifying farmers to enroll early. The program started visiting bean farmers from the early areas of Chinandega and it moved progressively to other crops and other areas as the program capacity was improving. The livestock group, for example, was the last one to be visited starting on February 2008. It took at least two months from the first contact by RBD Program professionals with a farmer up to the approval of a business plan by an RBD Program committee. Even though farmers did not have certainty that their plans were going to be approved, and the provision of services started after such an approval, it is possible that farmers from early-treatment areas have anticipated that they would be eligible for the program (Ashenfelter's dip

Figure 3: Timeline of Received Treatment as Compared to Timing of Surveys



Once the random assignment of early and late clusters was made, the impact evaluation team created a roster of all eligible producers in these clusters, and then randomly selected a sample of 1600 households split between early and late areas. These 1600 households were then invited to participate in the impact study, and completed a baseline survey in late 2007, just as the RBD project was beginning in the early treatment clusters.

Within these clusters, 64% of the eligible households chose to participate in the RBD project. A second-round survey was applied to all 1600 households in the first quarter of 2009, just as the RBD project was rolled out in the late treatment area. While it was not clear at baseline which of the eligible households in the late treatment areas would choose to participate in the project, those households made their participation decision around the time of the second-round survey. Similar to the early treatment clusters, 57% of eligible households in late treatment clusters elected to participate. Because the timing of the surveys and project rollout allow determination of farmer type in both early and late treatment areas (participants versus non-participants), the impact evaluation has the opportunity to study impacts on both eligible households as well as impacts on participating or *complier* households. The evaluation here will primarily focus on the complier households as we are interested in the impact of the program on the types of self-selecting individuals who adopt it.

2.1. Implementing the sample-experimental Design

Because it was a demand-driven program, it was not possible to define the target population of the RBD program *ex ante*. Implementation of the sample and experimental design thus followed the following two stage process:

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effect). Consequently, at the moment of the baseline interview they could have already modified their consumption because of their future participation in the program. The same effect might be present among the farmers who became eligible in 2009. However, the RBD Program scheduled their first visits to late-treatment areas only after the enumerators had visited the area. Therefore, we are less likely to find an Ashenfelter's dip in the late-treatment areas. In Section 3 we explain the nature of the Ashenfelter's dip in the context of the RBD Program in more detail, and we discuss the consequences of this possible effect in terms of internal validity.

Stage 1: Identifying a sample of clusters and randomization of the experimental units

In order to get an approximate idea of which communities or geographical areas had potential clusters, the RBD Program identified a sample of communities where there was a potential lead farmer within a particular productive activity. The number of potential leaders determined in this stage reached 146 farmers. The randomization of the experimental units was also implemented at this stage. The experimental design included three blocking variables, and two of them were given by the sequential identification of potential nuclei. The RBD Program field work was sequentially carried out by department and type of productive activity. Thus, the RBD Program professionals started identifying bean nuclei in the department of Chinandega, continuing with cassava in Leon, etc. Given the time line of the RBD Program implementation, it was necessary to do the randomization of clusters as soon as the identification by department and productive activity was finished.

Table 5: Results of the First Stage of the Sample-Experimental Design to Evaluate the RBD Program

Department	Productive Activity	PRP Area	Number of clusters Early RBD	Late RBD	Block
Chinandega	Livestock	E	6	5	I
		L	7	8	II
	Bean	E	6	6	III
		L	7	7	IV
	Sesame	E	4	3	V
		L	4	3	VI
Leon	Livestock	E	8	7	VII
		L	7	8	VIII
	Bean	L	7	6	IX
	Sesame	E	4	3	X
		L	4	5	XI
	Cassava	E	5	5	XII
		L	4	3	XIII
	Vegetables[a]		2	2	XIV
Total potential clusters			75 (51%)	71 (49%)	

[a]Blocking was not used for vegetable producers given the low number of clusters

E=early municipalities; L= late municipalities of the PRP. All the bean clusters identified in Leon were located in late-municipalities of the PRP. Cassava clusters were not identified in Chinandega at the moment of the sample design.

The third blocking variable took into consideration the Property Regularization Program (PRP) evaluation. The treatment and control groups were to be determined by the scheme of tenure regularization of this program. The original plan was to start in a group of *early municipalities* where the regularization might be relatively easy, while late groups would be those municipalities where the titling process was expected to be more difficult to carry out. Table 5 shows the final distribution of clusters by blocks at the end of the implementation of the first stage.

This stage finished with the identification of four vegetable nuclei. Given the low number of groups in this strata, an additional blocking variable was not used as in the other productive activities. Figure 4 shows the location of the randomized geographical units (GU).

Stage 2: Selecting a sample of farmers within every cluster

The sample list contained information about potential farmer leaders, the location of their farms, the communities where the eligible farmers could be found, and a radius of coverage within which about 30 farmers could be found (using the leader's farm as the origin). The program did not dispose of a complete list of names of potential satellite farmers. In order to get more precise information about the number and location of eligible farmers around the leader, a *quasi-census* of eligible farmers was carried out, using specific criteria provided by the RBD Program for each type of activity (Table 2). These criteria specified minimum and maximum farm sizes, minimum levels of farmer experience in that target crops, and also stipulated that it must be possible to reach the farm by road during all seasons. Starting at the leader's farm, the quasi-census verified the characteristics of all neighboring farmers until a sampling quota of 30 eligible farmers was reached, or until the maximum radius was reached. Using the *quasi-census*, 3000 farmers were identified, spread over 140 geographical units (clusters).

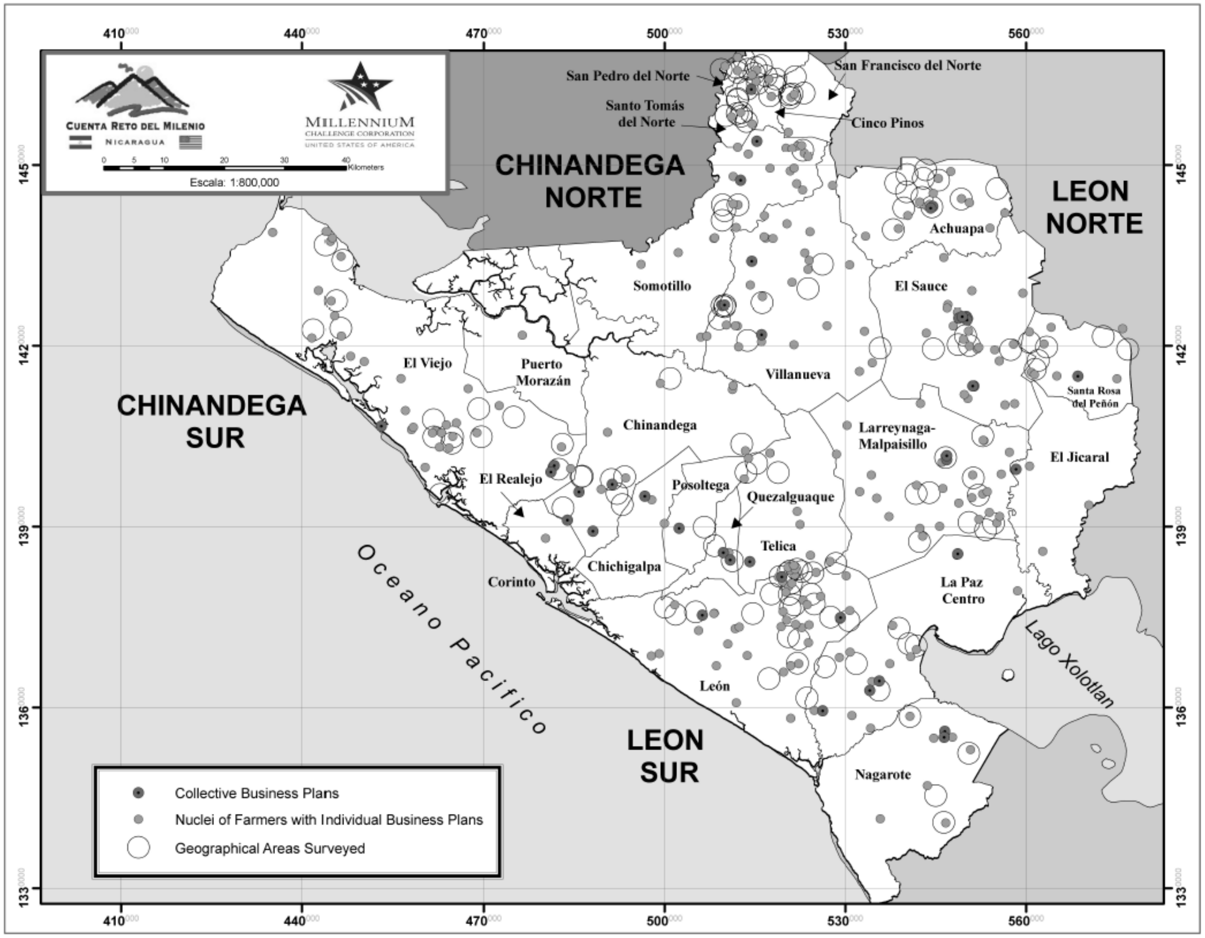
From every list of clusters, we expected to randomly select 12 farmers. In practice, there were fewer eligible farmers than we initially assumed. In some cases, the number of eligible farmers within the permitted radius was insufficient for the creation of a nucleus, and these potential farmers were therefore not included in the final sample. In numerous cases, the quota of 30 farmers was difficult to reach. Combined with the fact that 4% of farmers rejected to be interviewed, and that some 10% were deemed ineligible at the moment of the baseline survey, this all resulted in slightly fewer surveys per cluster than originally planned (11.4 interviews per GU, instead of 12).

At the end of this second sampling stage, 1600 farmers (and their households) were interviewed (see Table 6). There are slightly more early (treatment) farmers than late (control) farmers.

Within the blocks, there is an uneven number of interviews between early and late groups, especially with the sesame activity. Some sesame areas contained fewer eligible farmers, resulting in a lower number of interviews per GU. Across departments, the largest differences are found in some bean GUs: Chinandega has twice as many bean GUs as León. This difference is mainly explained because the GUs are spread across four municipalities in Chinandega, and only two municipalities in León.

Table 7 shows that livestock and bean are the largest groups of eligible farmers which agrees with the distribution of the ex-post population of beneficiaries. In particular, livestock and bean eligible farmers represent 40% and 29% of the sample of farmers, respectively. Similarly, livestock and bean contain the majority of participant farmers (38% and 34%, respectively). However, this similarity is not maintained across departments. For example, the proportion of eligible farmers in bean crops is only 18% of the total sample from León. In contrast, bean farmers enrolled in the program goes up to 33% in the same department. Clearly, this distribution of eligible farmers will have an effect on the resultant complier sample. The following two sections explore the properties of this experimental sample in terms of internal and external validity.

Figure 4: Distribution of RBD nuclei and geographical area surveyed



Source: MCA-Nicaragua database. Every symbol represents a nucleus with individual plans, a collective plan, or a geographical area surveyed (GU). Geographic location of a nucleus was represented by the farm leader location. In collective plans, geographic location corresponds to cooperative's office or location of main installation (e.g., milk collection center). GU is represented by the farm leader location if this was interviewed. Otherwise, GU is represented by a farm located in the community with higher number of surveyed farmers.

Table 6: Final Sample: Results after the Second Stage

Department	Productive Activity	Block	Number of GU Early RBD	Number of GU Late RBD	Number of interviews Early RBD	Number of interviews Late RBD	Interviews per cluster
Chinandega	Livestock	I	6	5	67	56	11.2
		II	7	8	78	88	11.1
	Bean	III	6	6	72	72	12.0
		IV	7	7	84	76	11.4
	Sesame	V	3	2	30	17	9.4
		VI	5	5	49	49	9.8
Leon	Livestock	VII	8	6	94	83	11.8
		VIII	7	8	77	90	11.1
	Bean	IX	7	6	84	72	12.0
	Sesame	X	3	2	32	18	10.0
		XI	3	5	36	55	11.4
	Cassava	XII	6	6	70	72	11.8
		XIII	1	2	12	19	10.3
	Vegetables[a]	XIV	2	2	24	24	12.0
Total clusters (GU)			71	71	809	791	11.4

As in Table 5, block numerations represent the treatment allocation of the canceled PRP. All the bean clusters identified in Leon were located in late municipalities of the PRP. Cassava clusters were not identified in Chinandega at the moment of the sample design. Early vegetable cluster are in Leon and Chinandega. Late vegetable clusters are located only in Leon.

[a]Blocking was not used for vegetable producers given the low number of clusters

Table 7: Distribution of Eligible Farmers in the Sample and Ex-post Population of Participants across Crops

	Eligible farmers sample(%)			Ex-post population of participants (%)		
	Leon	Chinandega	Both departments	Leon	Chinandega	Both departments
Livestock	40	39	40	42	32	38
Bean	18	41	29	33	37	34
Sesame	17	19	18	10	25	16
Cassava	20	0	11	14	1	8
Vegetables	4	2	3	2	5	3
All crops	53	47		58	42	
Number of obs	852	748	1600	2785	2022	4807

All numbers are percentages, except the number of observations.

Table 8: Early versus Late Treatment Groups–All Eligible Farmers

	Livestock		Beans		Sesame		Cassava		Vegetables		All Farmers	
	Early	Late	Early	Late	Early	Late	Early	Late	Early	Late	Early	Late
Expenditures per-capita (*cordobas*)	2407	2450	1194	1229	1803*	1556*	1686	1465	1462	1633	1831	1818
Mobile farm asset (*'000s cordobas*)	69.1	69.2	9.7	10.0	59.9**	29.9**	51.1	51.1	25.6	31.0	46.6	42.6
Farm's installations (*'000s cordobas*)	36.8	36.7	6.9	5.4	19.8	12.9	25.0	22.6	10.5	23.9	22.8	21.8
Animals (*'000s cordobas*)	292.1**	344.7**	47.5	46.3	99.4	89.0	75.8	91.3	22.8	28.2	154.6**	178.8**
Monthly remittances (*cordobas*)	660	751	482	406	716**	278**	404	334	121	590	576	519
Land size (*manzanas*)	61.6	72.2	18.4	18.4	28.5**	22.2**	20.7	22.9	6.9	11.8	37.0	41.1
Formal tenure (*% of farm*)	55%**	63%**	21%*	15%*	48%	43%	48%	54%	43%	56%	42%	45%
In process to tenure (*% of farm*)	22%	28%	32%	34%	25%	32%	19%	20%	30%	13%	25%	25%
Credit status (%)												
With a loan	38%	42%	34%	25 %	66%	67%	41%	30%	50%	50%	42%	41%
No loan-price rationed	20%	24%	15%	23%	9%	9%	15%	21%	4%	21%	16%	20%
Quantity rationed	16%	10%	12%	12%	17%	11%	22%	14%	29%	13%	16%	12%
Risk rationed	26%	24%	39%	40%	8%	14%	22%	35%	17%	17%	26%	28%
Farmer's age (*years*)	52*	54*	49	50	49	50	50	53	48	49	50	52
Farmer's education (*years*)	4.9	5.0	3.4	3.3	4.6**	3.0**	3.3	2.9	4.8	5.4	4.3	4.9
Farmer male (*% male*)	87%	88%	88%	81%	88%	93%	82%	85%	63%	75%	86%	86%
# of household	5	4	5	5	4	4	4	6	6	6	4	4
# of household in school	1	1	2	2	1	0	1	0	0	1	1	1
# of observations	308	311	240	218	147	139	82	89	24	24	801	781

* Different tests used for different data types (see Toledo 2011 for details). .

For all the above test, the asterisks $*$ indicate the following: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$

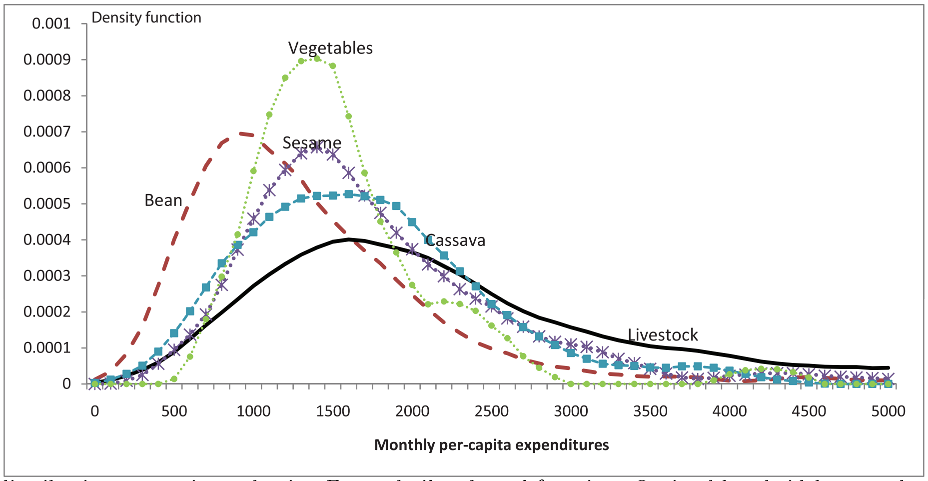
2.2. Internal Validity and the Complier Sample

As Rosenbaum (1995) points out, in order to make valid inference about the effects of a treatment, it is sufficient to require that the treatment be allocated randomly to experimental units. In traditional laboratory experiments this standard can easily be maintained, as the researcher has more control over homogeneity of the treatment, and is able to match the assigned treatment to the actual treatment. In field experiments designed to make impact evaluations, replication of the treatment can vary across experimental units, especially if researchers can only imperfectly monitor the on-the-ground implementation of the program. Prior to analyzing if these concerns are present in our experiment and how they could affect estimations of the program impact, we compare the different treatment groups with respect to outcomes that could be affected by the intervention, as well as their demographic characteristics.

Because of the structure of the randomization, none of these variables were used to generate homogeneous treatment and control groups. However, randomization should generate similar groups. If the groups look alike on observable variables, we would expect them to also look alike on unobservable characteristics. Table 8 compares a variety of observable characteristics across the early and late treatment groups, for all eligible farmers, and by type of productive activity. When the variable is continuous, a Kolmogorov-Smirnov (KS) test is computed to evaluate if early and late sample distributions represent the same population. Standard proportion tests are used to evaluate the same hypothesis with categorical variables. In those cases where the average of a variable is computed, a t-test is calculated to evaluate the hypothesis that early and late groups represent populations with the same mean. In most cases p-values are greater than 10% and, thus, differences in distribution or in averages are not statistically significant. The KS test is more robust to outliers, so it is possible that a t-test might wrongly reject the null hypothesis of no difference in means, while a KS test would not reject that the sample distributions represent different populations. Note that the p-value is not $\leq 1\%$ for any of these comparisons.

Table 8 also indicates that there might be some systematic differences between

Figure 5: Estimated Per-capita Household Expenditures Distributions per Crop



Kernel distribution was estimated using Epanechnikov kernel function. Optimal bandwidth was calculated according to Silverman

eligible farmers in different productive activities. If we rank the averages of income-related variables across crops (e.g., expenditures, mobile farm assets, farm's installations), bean and livestock farmers are ranked at the lowest and highest positions, respectively. The empirical distribution of per-capita expenditures in Figure 5 illustrates these differences. Based on KS statistics, we cannot reject the hypothesis that the distributions of expenditures for the sesame, cassava and vegetable groups represent different populations. In contrast, we strongly reject that hypothesis for bean and livestock farmers. In terms of expenditures, bean farmers could be regarded as the group with the lowest average income, while sesame, cassava, and vegetable are the middle-income groups, and finally livestock farmers are the group with the highest average income.

These five productive groups behave differently in the credit market. Following Boucher *et al.*'s (2008) definitions, we infer farmers' credit status. A quantity rationed farmer would like to borrow money at the current interest rate, but she does not qualify for a loan. Risk rationed farmers, on the other hand, may qualify for a loan, but they choose not to not take it because the moral hazard-based contract they would be offered in the credit market would expose them to a high risk of collateral loss. The extreme heterogeneity of credit status across farmers is exemplified by the group of bean farmers. Around 40% of them can be categorized as risk rationed. An average bean farm also has a significant lower proportion of land under formal tenure. The average bean farmer has about 20% of their farm under legal title. In contrast, livestock farmers own, on average, more than 50% of their farm with a formal title.

Even though randomization of the treatment was done considering the main productive activity of the farmers, the number of GUs was not enough to generate homogeneous treatment and control groups in livestock and sesame farmers. Livestock farmers from the early group are, on average, younger than farmers from the late group and have fewer animals, as well as a lower portion of land with a legal title. Sesame farmers from early group are significantly different from the control group on per capita household expenditures, mobile capital and land. These differences, while statistically significant, are not particularly large in magnitude. Note that the control and treatment samples are well balanced when all farmers are included, except for the value of animals.

In a typical impact evaluation study (in which not all eligible producers accept or "comply" with the offered treatment), it is impossible to know who in the control group is a "complier type." These studies must compare eligibles in the treatment group with eligibles in the control group, and rely on either so-called "intention to treat" (ITT) impact estimators or local average treatment effect (LATE) estimators. Both types of estimators result in loss of statistical precision for a given sample size (and rely on the assumption that an identical fraction of control households would have enrolled in the program had it been offered to them). Fortunately, in the case of the RBD study, the late treatment group GUs were offered the program right after the midline survey. This data structure allows us to cleanly identify the "complier types" in both the treatment and control (early and late, respectively) sub-samples.

Using the late treatment compliers as a control for the early treatment compliers is statistically valid as long as the enrollment and selection processes in early and later areas were the same. Ex ante, we would expect this condition to hold given the random allocation of predefined GUs between early and late treatment groups. We now examine this more carefully by exploring the characteristics of the early and late complier groups.

According to Table 9, as of July 2010, 64% of farmers from the early treatment group had enrolled before August 2008 (early compliers), while 57% of the control group was enrolled after February 2009 (late compliers). The different rates of compliers shows up more markedly when we compare departments and crops. Bean farmers comply at a rate of more than 60% in the treatment group, while the control group lacks bean farmers from the department of León. If some bean farmers from the control group in León were enrolled in the program, they enrolled before August 2008, or through collective plans with cooperatives.[17] Late enrollment of bean farmers was effective in the control group in Chinandega, although the rate of compliers only reached 49%. Similarly to León, late participation in Chinandega was mainly performed through collective plans managed by cooperatives. Additionally, the late group of vegetable farmers contains no compliers. It is worth mentioning that participants through collective plans represent 61% and 30% of the population of beneficiaries in bean and vegetables, respectively. In contrast, participation in livestock, sesame and cassava groups was mostly done by individuals plans forming nuclei of farmers. For these groups, compliance rates are higher, especially in the control group. This lower or nil compliance rate in the control group raises the question if collective plans imposed a different rule of exclusion in the late enrollment.

Is the late subsample of complier farmers a valid control group in spite of the difference in the enrollment rate? Figure 6 plots the expenditure distribution of eligible farmers assigned to the treatment (i.e, early eligible farmers) and control groups (i.e., late eligible farmers) as well as the distribution of early and late compliers. We

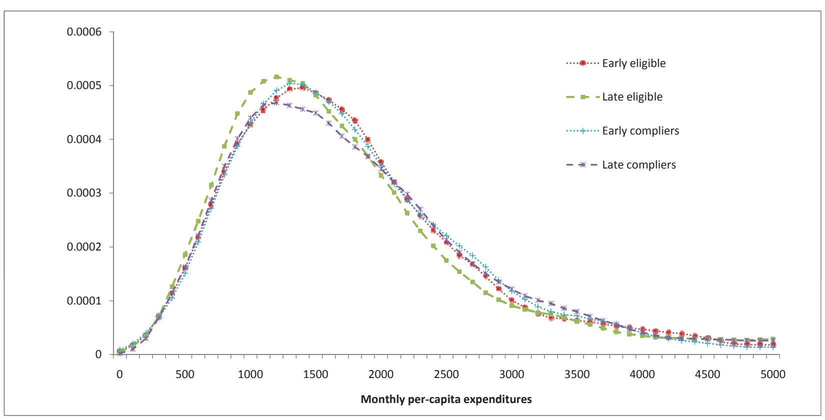
[17] Note that new bean farmers from this department were incorporated to the program after February 2009. However, these farmers were located in a municipality that was not included at the moment of the sample design.

Table 9: Distribution of Compliers and Non Participants across Type of Treatment, Departments, and Crops

	Treatment (early) group						Control (late) group					
	%Early compliers		%Late participants		%Non participants		%Late compliers		%Early participants		%Non participants	
	L	CH	L	CH	L	CH	L	CH	L	CH	L	CH
Livestock	67	61	2	2	31	37	75	57	2	6	23	38
Bean	76	63	0	6	24	31	0	49	46	6	54	45
Sesame	55	62	4	4	41	34	71	84	6	0	23	16
Cassava	62	–	17	–	21	–	68	–	4	–	27	–
Vegetable	75	42	8	0	17	58	0	–	13	–	88	–
All crops	66	61	5	4	29	35	56	59	11	5	33	37
All crops, both departments	64 (517)		4 (36)		32 (256)		57 (454)		8 (65)		34 (272)	

L=Leon department; CH= Chinandega department. Number of observations in parentheses. All other values are percentages. Cassava clusters were not identified in Chinandega at the moment of the sample design. There were not vegetables clusters assigned to the control group in Chinandega.

Figure 6: Estimated Distribution of Per-capita Expenditures Per Type of Treatment



Kernel distribution was estimated using Epanechnikov kernel function. Optimal bandwidth was calculated according to Silverman (1986).

Table 10: Early versus Late Treatment Groups–Complier Farmers Only

	Livestock		Beans		Sesame		Cassava		All Farmers		Excluding Beans	
	Early	Late	Early	Late	Early	Late	Early	Late	Early	Late	Early	Late
Expenditures per-capita (cordobas)	2407	2308	1193	1326	1708	1511	1745	1622	1811	1866	2127	1969
Mobile farm asset ('000s cordobas)	81.5*	62.3*	10.4	15.5	47.6	30.4	59.4	54.1	50.0	46.1	69.6***	51.9***
Farm's installations ('000s cordobas)	43.7	33.6	7.6	8.3	21.3	13.5	25.2	19.2	26.0	22.9	35.3**	25.6**
Animals ('000s cordobas)	325.6	370.4	51.2	44.8	97.1	89.4	87.6	81.8	169.8**	212.5**	232.4	244.1
Monthly remittances (cordobas)	734	519	468	424	608*	315*	366	377	575	436	647*	438**
Land size (manzanas)	69.9	75.7	19.8	22.9	29.8**	21.2**	22.7	22.5	41.1	46.1	52.7	51.8
Formal tenure (% of farm)	56%***	67%***	20%	18%	44%	41%	42%	52%	41%***	51%***	51**%	57**%
In process to tenure (% of farm)	25%**	16%**	31%**	44%**	27%	33%	17%	21%	27%	25%	24%	22%
Credit status (%)												
With a loan	42%	43%	36%	22%	67%	69%	47%	27%	45%	44%	49%	48%
No loan-price rationed	21%	23%	17%	24%	8%	6%	25%	15%	16%	19%	16%	11%
Quantity rationed	12%	10%	12%	11%	19%	10%	10%	5%	15%	11%	17%	19%
Risk rationed	25%	24%	36%	43%	6%	15%	12%	32%	24%	26%	18%	23%
Farmer's age (years)	52*	54*	49*	52*	47	50	48**	54**	50***	53***	50***	53***
Farmer's education (years)	5.1	4.9	3.7	3.8	4.9***	3.0***	4.3**	2.9**	4.5	4.4	4.9***	4.0***
Farmer male (% male)	87%	89%	86%	81%	91%	93 %	80%	85%	86%	88%	87%	90%
# of household	5	4	4	5	4	4	4	4	5	4	5	4
# of household in school	1	1	1	2	1	0	1	0	1	1	1	1
# of observations	198	208	162	72	86	108	51	61	511	449	33}5	377

See notes for Table 8

can observe that distributions are very similar in these different groups. Moreover, based on a KS test, there is no evidence that they represent different populations.

However, given that the enrollment rate was significantly different across crops, a similar distribution over expenditures does not on its own guarantee that an estimator, such as DD, be free of bias. In particular, it is not possible to observe what could have happened to the early group of compliers if they had not been enrolled in the program. Using the sets of complier farmers observed in the two rounds of the survey, the well-known basic assumption of a DD estimator can be expressed as:

$$E[y_{0,1}^{EC}|X - y_{0,0}^{EC}|X] = E[y_{0,1}^{LC}|X - y_{0,0}^{LC}|X] \tag{1}$$

where $y_{0,0}^{EC}$ represents the pretreatment response of a farmer (e.g., expenditures) in the early complier group observed in the first round and $y_{0,1}^{EC}$ is his unobserved counterfactual that we would have observed in 2009 in the hypothetical case that she was not enrolled. On the right-hand side of the equation, $y_{0,1}^{LC}$ and $y_{0,0}^{LC}$ represent the observed responses of the late compliers who have not received any treatment. Because $y_{0,1}^{EC}$ is not observed, the DD assumption allows that both groups of compliers could be different in observable covariates X or even in unobservable pretreatment variables. More importantly, the assumption establishes that it is relevant that both groups have parallel trends over time in the absence of the program. This could be a strong assumption if there is a significant imbalance in pretreatment variables related to the outcome variable.

Table 10 compares the distributions of the same variables from Table 8, but between early and late groups of *compliers*. As mentioned earlier, we don't have late complier farmers in the vegetables and bean groups from the department of León, and we thus exclude early bean and vegetable farmers in the last column. When all compliers are considered, the early and the late groups are significantly different in age and in the proportion of the farm that has legal title. Early complier farmers are a few years younger, own a less valuable stock of animals, and have only 41% of their farms under formal tenure. Depending on the direction of the impact of these differing variables on farmers' growth rates, the impacts of the program could either be over- or under-estimated.[18] As before, we emphasize that while the differences are statistically significant, they are unlikely to be economically significant.[19] The treatment and the control groups of compliers are also significantly different in the average value of stock of installations and farm animals. However, if these variables are computed as a proportion of the farmer's land size, such a difference is not significant on average.[20]

Since the randomization was performed within blocks, we could expect that the treatment and the control groups remained balanced in terms of pre-treatment variables although two blocks, bean and vegetables, were removed from the sample. The last two columns of Table 10 show that when the group of late compliers is compared to the group of early compliers, the differences in age and tenure status are more pronounced. In addition, farmer's education level is higher for early compliers. Once again, although the treatment and the control groups for this subgroup of compliers are significantly different in the average value of stock of installations and farm animals, this difference is not significant when the variables are computed as a proportion of the farmer's land size.[21]

Finally, while the complier sample may not perfectly represent the population of participants, we want to emphasize that this does not affect the internal validity of the estimates presented in this evaluation. For example, around 13% of participating farmers in the sample are female, while this figure is almost 30% in the wider population of RBD participants in agriculture and livestock branches of the program. This particular discrepancy arose because the program decided to emphasize the recruitment of female farmers after the sample frame had already been determined. The internal validity would only be affected if there existed a gender-imbalance between the early and the late treatment groups – and this is not the case. External validity may be questioned if we believe that our sample is unrepresentative of the effects that a similar program elsewhere is likely to produce. This will depend on several things: *(i)* whether this other program focuses on recruiting female farmers (as the

[18]Tauer and Lordkipanidze (2000) show that farmers' productivity could increase with age, but it could also decrease as farmers age if age makes them less willing to adopt new technologies. In this case, age acts as a trend affecting the growth rate of income and consumption. In terms of equation 6, if younger farmers grow faster in the early group, the right hand side of the equation could be lower than the true counterfactual. Thus, the impact of the program could be overestimated. Note that equation 6 should hold over time. Most intermediate evaluations only observe the experimental units for two periods. In such a case, transitory exogenous shocks with heterogeneous effects could distort the estimation of the long-run growth. For example, the portion of titled land could imply a heterogeneous response of a farmer to weather shocks. With legal titles, land acquires collateral value and access to credit may be easier. In this case, late farmers could counteract a negative income shock more adequately. In this example, the program impact could be underestimated.

[19]While a 10 percentage-point difference may seem large, this is likely because the definition used for formal tenure is a very strict one. The nuances of the tenure situation are important, and some farmers may feel secure in their land tenure even if they do not possess the most formal type of title. Therefore, a deeper evaluation of the impacts of tenure status may be appropriate, but is outside the scope of this report.

[20]These results are not in Table 10. The p-values are 0.15 for installations and 0.18 for farm animals.

[21]As a robustness check, the results in Section 4 were re-estimated excluding the livestock farmers, to see if any potential baseline imbalances affect the results. Overall, the results don't change when the livestock rubro is excluded.

RBD program did in the larger population of participants)[22], and *(ii)* whether there are significant differences between male and female farmers. Most of our analysis in Section 4 fails to detect statistically significant differences between female and male farmers. We are unable to establish whether this is because *(i)* there are no behavioral differences between male and female farmers or *(ii)* we simply have too few female farmers to have the necessary statistical power to find significant differences. In the former case, external validity would not be affected, while in the latter case we simply cannot answer this question.

3. Impact Evaluation Methodology

Agricultural value chain programs like the RBD present a number of evaluation challenge, including self-selection (not everyone who is eligible can or should join), intrinsic heterogeneity (not everyone can succeed in business even when trying), and important co-investment and learning effects that may make long-run impacts quite different from short-run impacts. This section outlines the basic econometric methodology used for this evaluation. After a brief description of the hypothesized impacts of the RBD program in Nicaragua in Section 3.1, Section 3.2 describes the basic structure of the data available for this evaluation, and then outlines the two basic econometric approaches that will be employed. The first of these is built on standard binary treatment analysis, while the second employs continuous treatment methods designed to uncover the temporal pattern of impact and the long-run, policy relevant treatment effects.

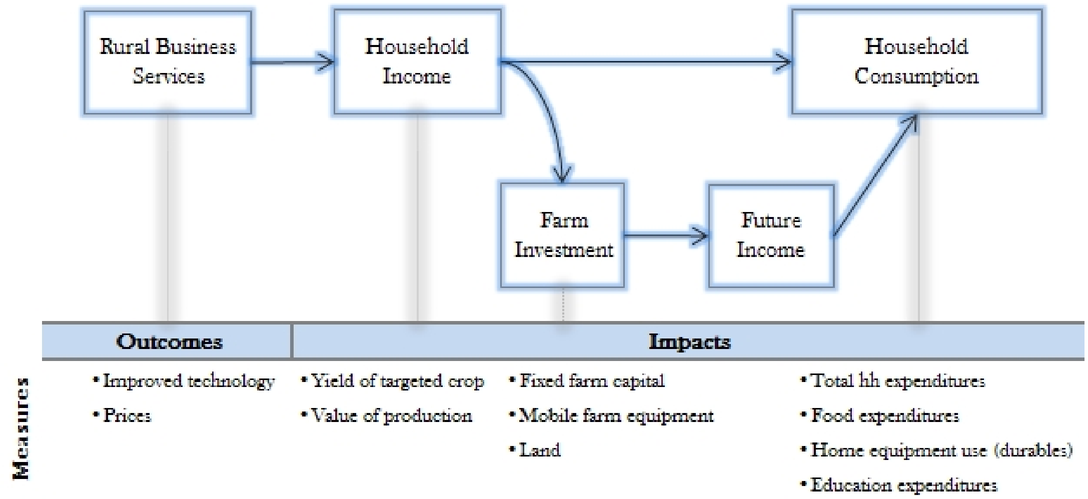
3.1. Hypothesized Outcomes and Impacts of the Rural Business Program

As Section 1 described, the RBD provided business services to farmers to develop business plans that might involve productive installations. Farmers also received expert technical assistance, materials and equipment, as well as marketing support with the objective of improving productivity and access to markets. Through these channels, we could expect an increase in farmers' income. It is reasonable to think that farmers also foresee this income increase and therefore decide to enroll in the program.

From a methodological perspective, it is important to note that the impact of these services are likely to have a temporal dynamic of their own. Unlike, say, a blood pressure treatment that immediately impacts its beneficiaries, the RBD intervention presented beneficiaries with a capital infusion and also gave them new opportunities and information. The impacts of the latter (and their synergies with the former) are likely to have developed over time as beneficiaries learned-by-doing, co-invested, etc. Indeed, in this context, it is not unreasonable to expect that measurable living standards may have dipped initially as (credit-constrained) beneficiaries diverted resources to the program. Such a dip – usually referred to as an "Ashenfelter Dip," after Ashenfelter (1978) – is but one part of the dynamic impact curve. A recent study of land transfers to small farmers in South Africa (Keswell and Carter, 2011) revealed a dip in living standards in the first year in the program followed by growing

[22] If another, similar, program does not actively recruit female farmers, it may be more likely to end up with proportions of female farmers similar to those in the evaluation sample.

Figure 7: Expected Impact of Rural Business Services



and increasingly positive impacts over the next 3 years. Importantly, the long-run impacts are nearly double the magnitude of the shorter run, suggesting the kinds of co-investment and learning effects that may also be important to the RBD program. The methodological approach outlined in Section 3.2.2 below will deploy continuous treatment methods to allow identification of these long-term impacts.

Figure 7 is a flowchart that illustrates how the program worked and how it may have influenced key development outcomes at different levels. As described in Section 1, 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 first level effects as the *outcomes* of the program. If the RBD Program was effective, we should in the first instance see that beneficiary farmers improved their use of improved seeds (as a marker of overall improved technological access)[23], and that they received better prices for the output they produce. We might also expect to see beneficiaries increased the farm area devoted to program activities. While these intermediate outcomes are not the key focus of this evaluation, later analysis will explore the data for evidence of program effectiveness at this level.

The next step in the flow chart is farm household income. While the evaluation strategy did not try to measure all household income, we will explore certain key indicators, including yields of targeted crops and their total market value. A finding of increased income from targeted activity may, however, overstate the overall family income increase if increases in targeted activity crowded out and displaced family resources from other, non-targeted activities. While we lack data to definitively test whether or not any income increases were achieved by reducing income in other (non-targeted) activities, we will test for the impact of the RBD Program on a maize

[23]Improved seed was a dimension stressed by the program across several of the target rubros, and therefore appears to be a sensible measure of the use of improved technologies in the beans, sesame, cassava rubros, as well as for maize. In the livestock rubro, the corresponding measure is whether farmers processed their dairy products.

production, a non-targeted activity in which almost every household in the study participated, both before and after the introduction of the RBD Program.

If the RBD Program indeed increased permanent household income as it intended, then we might expect to see an immediate increase in household consumption, as predicted by the permanent income hypothesis. However, the reality of binding credit constraints in the RBD Program area means that household would have faced a key allocative choice: allocate income increases immediately to consumption, or reinvest income increases into the farm operation, postponing increased consumption until a later date. As shown in Figure 7, our analysis will explore both dimensions. We will first see if the RBD Program led beneficiaries to increase investment in land and both fixed and mobile agricultural capital. We will then explore if the program led to increased household consumption and living standards. In all cases, we will estimate the long-run impact dynamics, as well as conventional binary average treatment effects.

To fix ideas about the relationships between the different outcome variables mentioned above, it may be useful to consider a standard income identity, which says that $C + I + S =$ Net Income, where C represents consumption, I investment and S savings. This obviously also holds for changes in those same variables, i.e. $\Delta C + \Delta I + \Delta S = \Delta$Net Income. Now, the data don't allow us to estimate changes in *net* income, as full cost accounting is onerous and notoriously difficult. Instead, as explained above, we look at changes in *gross* income. Gross Income, then, should unambiguously be an over-estimate of changes in net income. Further, we don't have a reliable estimate of savings. Put together, we end up with

$$\underbrace{\Delta C + \Delta I}_{\text{lower bound}} \leq \Delta C + \Delta I + \Delta S = \Delta\text{Net Income} \leq \underbrace{\Delta\text{Gross Income}}_{\text{upper bound}}$$

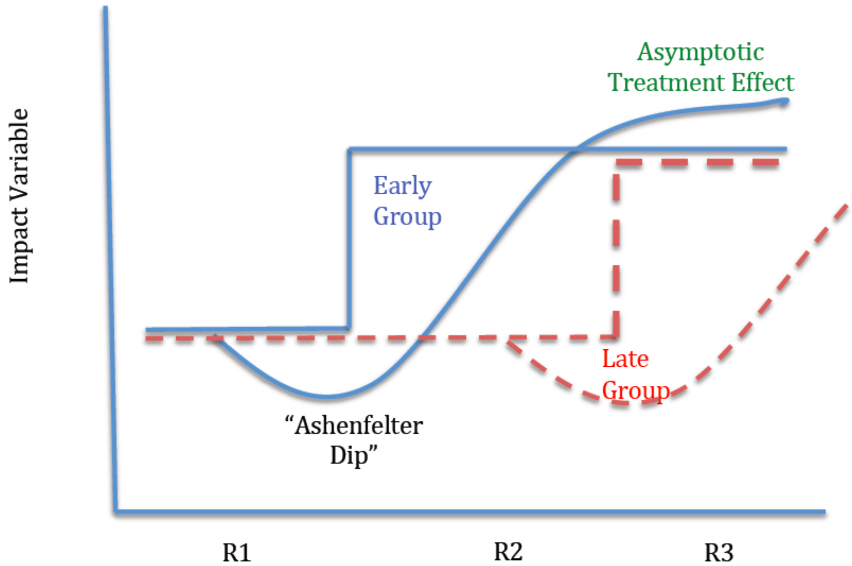
Therefore, while each of the pieces of this identity are interesting on their own, it is also worth considering how they are inter-related, and that we are essentially estimating lower and upper bounds on this relationship.

3.2. Econometric Methodology

The workhorse impact evaluation estimators assume that program participation is a binary state–either a household receives the treatment or it does not. While this approach manages well treatment heterogeneity across treated units (hence the derivation of local average treatment effects), it is less well-equipped to deal with the heterogeneity implied by a temporal impact curve driven by the sorts of considerations just discussed. This observation is especially important when the time span of the different impact phases is not known (*e.g.*, how long does the Ashenfelter dip last)?

To better frame these issues, consider the hypothetical impact relationships for the MCC RBD program illustrated in Figure 8. The solid, blue functions illustrate what we might expect to see for the early treatment group, while the dashed red functions illustrate the same for the late treatment group. The horizontal axis shows the time at which the different survey rounds were undertaken (see the discussion in Section 2). If the program had reached its full long-term impact on the early (late) treatment group by the time of the second (third) round survey, then conventional binary estimators would work well. In this case we would expect the data to trace

Figure 8: Hypothetical Impact Patterns



out the impact pattern illustrated by the step functions. On the other hand, if the impact of the program evolves more slowly over time (with an initial 'Ashenfelter Dip' followed by a slow rise toward a long-run or asymptotic treatment effect), then our data would be generated by a a non-linear impact or duration response function in which impact depends on the duration of time in the program. The blue and red curves illustrate what those impacts may look like for the early and late treatment groups, respectively. In the hypothetical case illustrated in Figure 8, impacts measured at midline using a conventional binary treatment (step function) estimator would reveal muted effects that would not accurately represent the long-run program impacts. In the remainder of this section, we will propose an econometric methodology that is general enough to encompass both cases.

3.2.1. Binary Treatment Model

To help fix ideas, we begin be reviewing the basic mechanics for a standard evaluation of a binary treatment (i.e., some households get the program and others do not), and then move on to a generalization that is appropriate for our three-round study with staged program rollout in which all individuals were able to join the program by the final survey round. Let B be a binary variable that defines the assigned treatment such that $B_i = 1$ if the eligible farmer i was assigned to the early treatment group, and $B_i = 0$ if she was assigned to the late treatment group. If this randomized assigned treatment is only considered, the DD parameter, δ_{ITT}, can be expressed as follows:

$$\delta_{ITT} = E\left[Y_{i,2}(D_i) - Y_{i,1}(D_i)|B_i = 1\right] - E\left[Y_{i,2}(D_i) - Y_{i,1}(D_i)|B_i = 0\right]$$

where $Y_{i,2}$ is the potential income (or other outcome variable) of farmer i at the moment of the second round of the survey, and $Y_{i,1}$ is the potential income at the time of the baseline survey. The potential outcome depends on the participation status D_i, defined as

$$D_i = \begin{cases} 1, \; if\; participates\; in\; RBD \\ 0, \; otherwise \end{cases}$$

Note that for the estimation of the ITT effect the date when farmer enrolls in the program is irrelevant. Because of that we ignore this issue in the definition of D_i . Since the received treatment (D_i) can be different from the assigned treatment (B_i), the DD parameter is also known as the intention-to treat (ITT) effect. In terms of the RBD project, this parameter represents the average effect of the program on the population of eligible farmers. Appendix A below presents these standard ITT (and their related LATE) estimators using the baseline and midline data.

The available dataset used for this research allows us to identify which sampled farmers from the early treatment group (i.e., early assigned treatment) were enrolled before 2009 as well as which farmers from the control group (late assignment) were enrolled in or after 2009. Thus, we can estimate the effect of the program removing farmers who did not participate and those who enrolled but at a time different from the one determined by the random assignment. In focusing on this group, we are restricting our attention to the sub-population of farmers who wold join a program like the RBD. It is inference about impacts on this sub-population that is of greatest relevance to policymakers.

Following the Imbens and Angrist (1994) formulation, our subsample of enrolled farmers can be viewed as two-sided compliance. Here, we use the binary variable T to define early and late compliers in our two-sided compliance dataset:

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

In other words, complier farmers in the early treatment group are those who actually enrolled in the RBD program 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). As reported in our earlier work (Toledo 2011 and Carter and Toledo 2010), we can 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 DD estimator:

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

Note that the validity of this estimator relies on the idea that the decision to enroll in the early and late treatment groups was structurally the same, so that we are in fact comparing like-with-like in using this estimator. See Section 2 above for more discussion.

We of course have a third round of survey data and the above complier estimator can be generalized as follows:

$$E[Y_{it}] = \alpha_0 + \lambda_2 t_2 + \lambda_3 t_3 + \delta_2(t_2 T_i) + \delta_3(t_3 T_i) + [\gamma T_i + \beta' X_i] \qquad (2)$$

where T_i is the binary complier-treatment group indicator defined above, X_i represents a vector of baseline characteristics (namely, crop indicator variables, farmer age, and farmer education), and t_2 and t_3 are binary time period indicators survey rounds 2 and 3, respectively. Note that the parameters γ and β control for any baseline differences between early and late treatment groups. Since all the variables in square brackets are time-invariant, we can replace them with a single, household level fixed effect and rewrite the equation 2 as:

$$E[Y_{it}] = \alpha_0 + \lambda_2 t_2 + \lambda_3 t_3 + \delta_2(t_2 T_i) + \delta_3(t_3 T_i) + [\alpha_i]$$

where α_i is the fixed effect.

If RBD impacts follow the binary step functions illustrated in Figure 8, then the parameter δ_2 will estimate the complier group difference-in-difference estimator, δ_{DDC} and we would expect δ_3 (which measures the difference between early and late treatment groups in the third period) to be zero as both groups are full participants in the RBD program by that period.

An alternative and more straightforward way to write the binary treatment model is to define the binary treatment variable

$$Z_{it} = \begin{cases} 1, & \text{if farm } i \text{ had been treated at time } t \\ 0, & \text{otherwise} \end{cases}.$$

Using this new variable, we can write

$$E[Y_{it}] = \alpha_0 + \lambda_2 t_2 + \lambda_3 t_3 + \delta Z_{it} + \alpha_i. \tag{3}$$

To simplify estimation, we can sweep away the fixed effect term by taking the difference between later periods and the baseline and rewrite 3 as:

$$E[Y_{it} - Y_{i1}] = \lambda_2 t_2 + \lambda_3 t_3 + \delta Z_{it},\ t = 2, 3. \tag{4}$$

As before, δ estimates the complier group difference-in-difference treatment effect and will in fact be identified by the difference between early and late treatment groups in the round 2 midline data.[24] In the analysis to follow, we will primarily rely on this specification to estimate binary treatment effects using the double-sided complier sample. We also report standard ITT estimates in Appendix A below.

3.2.2. Continuous Treatment Model

As discussed in the beginning of this section, there are a number of possible reasons why the impact of the RBD may 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 at least three other reasons why the impact of the RBD may have changed over time. First, RBD 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

[24]The estimated parameters δ in equation 4 and δ_2 in equation 2 y are numerically identical.

from cash transfer and other common anti-poverty policy instruments. Third, and less happily, if program impacts are short-lived (e.g., if treated farmers drop the improved practices as soon as the 24-month period of intense RBD involvement with their groups end), then impacts may dissipate over time.

If these observations are correct, then the impact or duration response function – meaning the relationship between program impact and duration of time since the asset was transferred – is unlikely to be a simple step function that can be approximated with a binary treatment estimate. One goal of this evaluation is to estimate the impact dynamics and duration response function and recover both the long-run impacts of the RBD and their time path. Both are of particular relevance from a policy perspective. Indeed, it is the prospect that a skill-building program like the RBD will facilitate and crowd-in additional asset building that makes them especially interesting as an anti-poverty program.

We begin by generalizing the double complier binary response function to the continuous treatment case:[25]

$$y_{it}(d_{it}) = \lambda_2^d t_2 + \lambda_3^d t_3 + \Delta(d_{it}) + \alpha_i + \varepsilon_{it}, \tag{5}$$

where d_{it} is the number of months that farm i had been in the RBD program at survey time t, and $\Delta(d_{it})$ is a flexible function that can capture the sort of non-linear impacts illustrated in Figure (7) above. Empirically, we will measure duration at each round as the number of months between when the household's RBD cluster initiated activity and the date of the survey. In our data set, these durations run from 0 to 50 months.[26] In order to gauge the shape of the function $\Delta(d_{it})$, we will first employ semi-parametric analysis and then choose a polynomial (parametric) functional form that is consistent with the semi-parametric results (see Appendix B). As in the binary treatment case, the farm-specific fixed effect term controls for all observed and unobserved time-invariant characteristics, including farming skill, soil quality, farmer education, *etc.* Importantly, this fixed effect analogue estimators controls for any systematic or spurious correlation between observables and duration of treatment.[27]

While there are several computationally equivalent ways to consistently estimate a fixed effect model like equation 5, in anticipation of later quantile regression analysis (where such models are less easily estimated), we will build on the correlated effects model of Mundlak (1978) and Chamberlain (1982, 1984) and write the indi-

[25] We could alternatively follow Hirano and Imbens (2004) generalization of propensity score matching to the continuous treatment case. The Hirano and Imbens estimator only exploits observations with strictly positive amounts of treatment. In our case, this would imply dropping the baseline data for all RBD participants as well as the mid-line data for the late treatment group. For development applications that employ this estimator, see Keswell and Carter (2011) and Aguero, Carter and Woolard (2010).

[26] In a few cases, RBD activities began a few months prior to the baseline survey. For these cases, we have considered households in these clusters as treated at baseline also.

[27] There have been some suggestions from field staff that the implementer wanted to treat the households that they considered the most promising earlier on. While this would only have affected a small fraction of the sample, this methodology should deal with any differential treatment length that is correlated with observables.

vidual fixed effects as a linear projection onto the observables plus a disturbance:

$$\alpha_i = \psi + X_{i1}^{'}\lambda_1 + X_{i2}^{'}\lambda_2 + X_{i3}^{'}\lambda_3 + \upsilon_i$$

where X_{it} denotes a vector of observables (the time dummies and the duration variables). In our case, we have little reason to believe that the way in which the time-varying observables affect the individual effects differ between survey rounds, so we use the average of the time-varying covariates and write the fixed effect as

$$\alpha_i = \psi + \bar{X}_i^{'}\bar{\lambda} + \upsilon_i.$$

Substituting this expression into (5) gives:

$$y_{it}(d_{it}) = \lambda_2^d t_2 + \lambda_3^d t_3 + \Delta(d_{it}) + \psi + \bar{X}_i^{'}\bar{\lambda} + [\upsilon_i + \varepsilon_{it}] \tag{6}$$

OLS estimation of (6) allows us to recover the fixed effect estimators of the impact response function parameters of interest.

4. Impact Analysis

Building on the schematic shown in Figure 3 above, this section employs the three rounds of data on complier households to examine the effect of the RBD program on key outcome variables (technologies employed and prices received by farmers, shown in the flowchart in Figure 7), as well as its effect on the key impact variables of income, investment and the level of family economic well-being (as measured by per-capita consumption expenditures). We first look at each of these items in isolation (Sections 4.1 through 4.4). Section 4.5 then presents a unified interpretation of the program's average effectiveness.

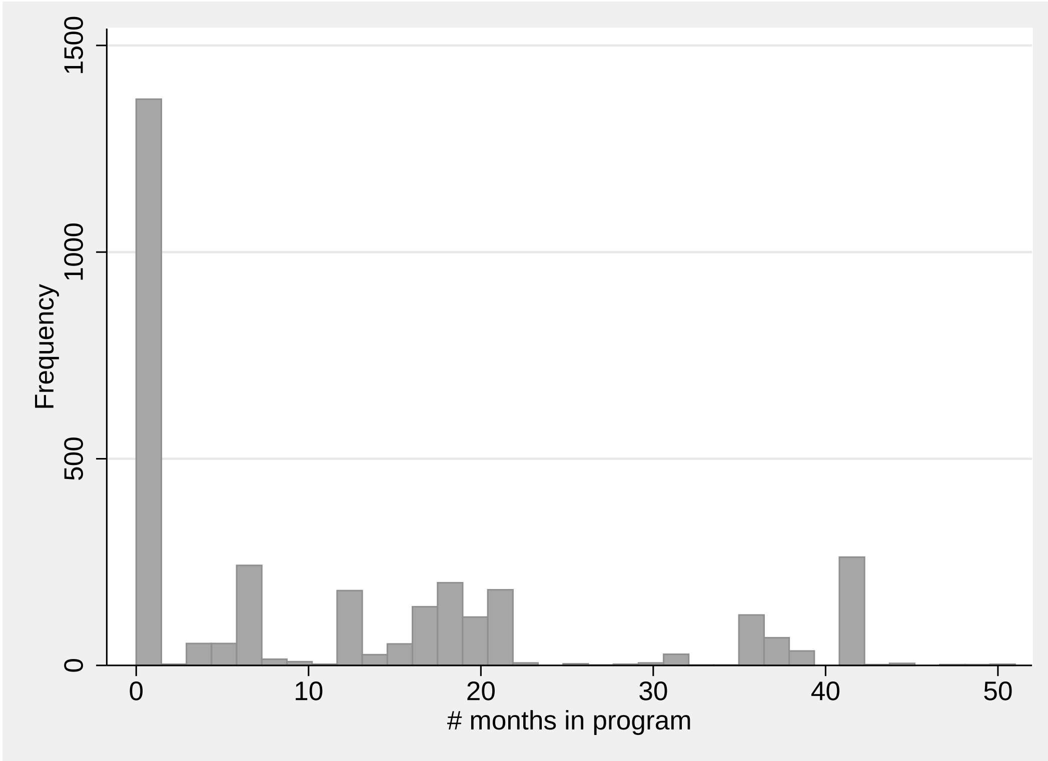
As a prelude to the analysis in this section, Figure 9 shows the histogram describing the distribution of months in the RBD program for the complier sample across all three survey rounds. There is a large cluster of observations with zero treatment (comprised of the early and late treated households at baseline, plus the late treated households at the midline). Despite some bunching (due to similar program initiation dates for the early and late groups), the data show reasonable dispersion with households observed with as little as 6 months in the program to as much as 50 months in the program. The largest clusters of observations are around 6 months of program exposure, 20 months and 40 months. Late treatment households comprise the first group, both early and late treatment households are found in the middle exposure group (the former at midline, the latter at end-line), while the latter group is comprised exclusively by early treatment households. It is this variation in length of time in the program that will be exploited in the continuous treatment estimators employed in this section.[28]

4.1. Technology and Prices

Before examining the outlined regression models, we examine descriptive statistics on farm technology use and production. Table 11 displays various indicators by

[28] All the results in this section are robust to the inclusion of month-of-survey dummy variables.

Figure 9: Distribution of the Duration of RBD Treatment (Dual Complier Sample)



producer group[29]. 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.[30] The variable *"Improved seed"* is the percentage of households that used an improved seed variety for the target crop during at least one season, and measures one aspect of farmers' utilization of improved technology. For dairy farmers, the measure is instead whether farmers applied any processing to their products before bringing them to market (*"Processing"*).

The variable *Income* represents the total value of production in the target crop, calculated using the prices that they were able to fetch for the part of their harvest that was sold[31]. The table also includes information on maize production. While maize was not an RBD activity, it is an important staple crop that most households

[29]The producer group Vegetables has been excluded from this analysis as the mode of production doesn't compare easily to the other producer groups in this particular framework, as the set of possible products is more diverse and the survey only elicits the prices and quantities of the three main crops produced under the vegetable rubro.

[30]This variable can be thought to combine two measures: the extensive margin of cultivation and the number of times the plots are cultivated in a given time period. The distinction between the intensive and extensive margin here is not the main focus – the time period in question is one year, and most of the crops have one main planting season. If farmers have a mostly fixed amount of land at their disposal, allocating more land to the target crop then might suggest that the farmer has increased their valuation of production in the target crop .

[31]When no sales price is reported (i.e. when the household did not sell any part of their crop), we use the mean price by season and crop.

Table 11: Prices, Technology and Incomes by Farmer Activity

	Baseline		Midline		End-line	
	Early	**Late**	**Early**	**Late**	**Early**	**Late**
Beans						
Value of production	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	434	427	823	786	1010	971
	133	183	132	185	128	176
Sesame						
Value of production	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	618***	517***	1276***	1135***	1409*	1318*
	110	86	109	86	93	66
Cassava						
Value of production	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	44.74	47.56	168.79	169.30	84.64	88.23
	59	50	52	49	55	42
Maize						
Value of production	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***
	414	429	525	540	523	536
Milk						
Value, livestock production	267873	291512	296921	276303	236171	253254
Value, milk production	112144	120104	167529	163613	164378	183587
Processing (%)	0.013	0.027	0.323	0.315	0.598**	0.493**
Price	4.24	4.22	6.65	6.51	6.8	6.87
	220	208	220	208	218	205

The asterisks denote the statistical significance of t-tests on the equality of the early and late complier group means: * $p < 0.01$, ** $p < 0.05$, *** $p < 0.001$

produce and provides a signal whether RBD crop (and income) expansion come at the expense of reduced output and income from other crops. While market prices for maize and other non-target crops were not elicited, the total value of production is calculated based on baseline market prices provided by RBD program staff.

It seems clear from Table 11 that the effects of the program on production were quite diverse across the different target crops. For bean farmers, the initial effect seems quite straightforward: farmers who enrolled in the program early planted more beans, received higher prices at the midline than the control group, and in addition many 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 the differences appear to persist, indicating that the "blood-pressure medication theory" of treatment effects is unlikely to hold up here. A similar story can be told for sesame farmers, except for the use of improved seeds[32].

For cassava and livestock, the differences between the groups are mostly statistically insignificant, even at mid-line. It may well be that both of these programs generated substantial spillover externalities in which even untreated and late treated farmers were able to benefit from the improved processing facilities offered to early treatment livestock and cassava producers.

As for maize production, the early and late treatment groups look similar both in the baseline and at 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 indicates that the RBD Program did not lead to significant crowding-out of maize production, and constitutes suggestive evidence that measured income increases from targeted activities are less likely to greatly overstate overall income. With this in mind, we now turn to examining the program effect on farm incomes.

4.2. Income

As discussed in Section 3.1 and illustrated in Figure 7, we begin the evaluation of the RBD Program by looking at its impact on income from the activities targeted by the RBD program.[33] As discussed above, observed income increases in RBD-targeted crops does not necessarily imply increased overall incomes, as productive inputs may have been reallocated from other activities to the target crops. While we have evidence that productive inputs were not reallocated away from maize production, inputs could have been substituted away from other activities (*e.g.*, off-farm labor) that we do not measure. These concerns notwithstanding, the value of the production of target crops is an important indicator.

While Table 11 generally shows that early treatment farmers had significantly higher RBD incomes at midline than did the late treatment farmers, we now examine this impact more carefully by employing the generalized difference-in-difference

[32]While the differences in the value of production for early/late sesame farmers is not statistically significant, the difference between the early and late groups is larger at the midline than at the end-line.

[33]RBD targeted activities are beans, sesame, or cassava for farmers in those groups, and milk for livestock farmers. Income from these activities is the total value of production in the targeted activity, valued in 2005 $USPPP.

Table 12: Program Impact on Income from Target Activity for Program Participants

	Binary	Binary, gender	Cubic
t_2	1778***	1778***	1687.3***
	(421.57)	(421.67)	(314.11)
t_3	363.7	364.0	241.6
	(774.80)	(774.51)	(600.35)
Z	1211.7*	1207.2*	
	(652.07)	(649.28)	
Female*Z		33.1	
		(671.56)	
Months (d)			263.2**
			(108.76)
Months^2 (d^2)			-10.2
			(6.90)
Months^3 (d^3)			0.112
			(0.12)
N	2001	2001	3062
R^2	0.045	0.045	0.308
adj. R^2	0.044	0.043	0.305

Cluster-robust standard errors in parentheses

Gender indicator variable equals 1 if the participating farmer is female

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

binary treatment estimator from equation 4, presented in Section 3.2.1 above.[34] The double complier difference-in-difference impact estimate of δ_{DDC} is given by the coefficient on the treatment variable, Z, in the first column of Table 12. Recall that these estimates control for household-level fixed effects. The point estimate is $1212, which implies an increase of about 17% over the baseline level of targeted activity income. This is statisticaly significant at the 10% level.

As we already saw in the descriptive statistics about 90% of the individuals reported to be the farmer/beneficiaries of the RBD program are men (see Table 10 above). The second column of Table 12 also reports a specification in which we interact the sex of the farmer with the treatment variable in the regression. As can be seen, the estimated coefficient is small in absolute value (the impact of the program is $33 higher for women than for men) and is imprecisely estimated. There is no evidence that the program differentially influenced men and women in this dimension, although this finding may simply reflect the fact that few women were actually enrolled in the RBD program.

For reasons discussed earlier, this binary treatment impact approach may fail to fully characterize the program.[35] As a first step to exploring this, we replace the

[34] In this and all sections of our analysis, we estimate impacts using levels (measured in $US PPP). In results not reported here, we also estimated the same relationship in logs. The log estimates were qualitatively similar to the level estimates and did not substantially alter the statistical significance of any impacts.

[35] In addition to those conceptual reasons, around 15% of farmers in the early treatment group had actually begun receiving RBD services at the time of the baseline, contaminating the binary

binary treatment indicator in equation 4 with a flexible function of months in the program ($\Delta(d)$) as in equation 5. As a first step in this analysis, we use a generalized additive model model to estimate a differenced version of this equation.[36] As can be seen in Figure .19, the non-parametric analysis suggests that a cubic specification would provide a reasonable parametric approximation of the shape of the duration response function.

The third column of Table 12 report the results from parametric estimate of equation 6, our preferred cubic specifications. Consistent with the semi-parametric results, the parametric results show that duration of time in program has a statistically significant impact on gross income in the treated activity. To draw out more fully the implications of these estimates, Figure 10 graphs the estimated cubic relationship. For comparison, the point estimate of the binary treatment effect is illustrated as a solid horizontal line in the figure. As can be seen, the estimated impact rises over the first two years of time in the program, peaking at roughly a \$2,100 impact, and then flattening out after that time. As shown by the (90%) confidence intervals in the figure, there is substantial noise in these estimates, and they become especially imprecise as the data thins out at higher treatment levels. This noise may reflect the fact that the program had different impacts across the different RBD-targeted activities.[37] Nonetheless, the impact is everywhere statistically greater than 0 at the 5% level.

In terms of the causal chain laid out in Figure 7, we now turn to see if these estimated gross income increases translated into increases in capital accumulation and, or consumption expenditures.

4.3. Investment and Capital Accumulation

An important component of the business plan developed by farmers was related to the accumulation of farm assets. With the objective of increasing farmer's productivity, the program provided some equipment or supported the construction of new productive installations as soon as the business plan was approved. Therefore, it was expected that any immediate impact on incomes was also supported by such an increase in the stock of farm assets. However, as we mentioned in Section 2.1, this initial capital boost may have also reinforced the future capital accumulation which, in turn, crowded out the increase in the short-run consumption.

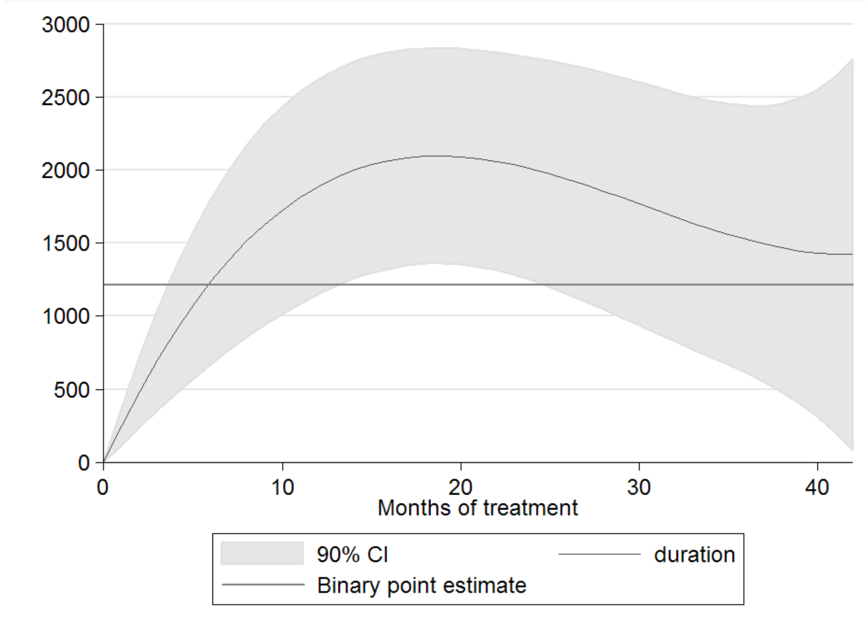
We follow the same evaluation strategy used in the previous section to examine the effect of the program on the stocks of mobile and fixed capital. Mobile capital (meaning tools and equipment, but excluding livestock) was relatively straightforward to measure using prices and values from the survey. Some elements of fixed capital (which includes buildings, installations and fences located on the farmer's land) were more difficult to value as they were often constructed by the farmer rather

results. We can control for this problem using the continuous treatment estimation.

[36] The full results from this analysis can be found in Appendix B

[37] Discussions of this point with RBD staff in Nicaragua suggested that returns to several of the targeted activities declined after the initial program year (as subsidies for improved seed and fertilizers were no longer given to producers by the RBD). While the impact evaluation was not set up with sufficient power to distinguish impacts by specific crop activity, we have estimated the impact regression models separately by activity. These results indeed confirm strongly heterogeneous impacts, with livestock showing the highest returns and returns to beans falling off sharply after 24 months in the program.

Figure 10: Estimated Impact of the RBD on Program Income



than purchased on the market. RBD program staff assisted with the evaluation, but a few items (*e.g.*, fencing) are not included in our measure of fixed capital.

Tables 13 and 14 show the binary and continuous estimates of the program impact on capital stocks. Figures 11 and 12 graph the average duration response of both types of capital. While the binary impact estimates (column 1, in Tables 13 and 14) are not statistically significant, the continuous treatment model shows that the value of mobile capital[38] increases significantly over the duration of the project, flattening out at around \$2,000, and then rising further after the end of the intervention.

In contrast, the continuous estimate for fixed capital levels off at about \$1000, but is not quite significant as the 95% interval estimate includes zero. In addition to the measurement problems mentioned above, the insignificance of this result may reflect heterogeneity in the tenure security of RBD beneficiaries. As shown in Table 10 above, only half the sample had legally complete property rights to their land. These differential impacts of the program on mobile versus fixed capital is consistent with the literature on tenure security and investment (especially see Carter and Olinto 2003). The RBD program was in fact to have been matched by a land tenure regularization effort. That effort, however, was suspended in 2009.

Columns 2 in the respective tables shows a regression where the program impact on capital accumulation is allowed to vary with the gender of the farmer. Despite the small percentage of female farmers, the estimated effect of program participation on mobile capital is significantly lower for female farmers. Given that average impacts on capital are around \$2,000 dollars, the size of the coefficient is quite large as it is almost

38

Mobile capital is the value of the capital stock at each round of survey in PPP dollars of 2005. Mobile capital does not include farm animals.

Figure 11: RBD Average Impacts on Mobile Capital

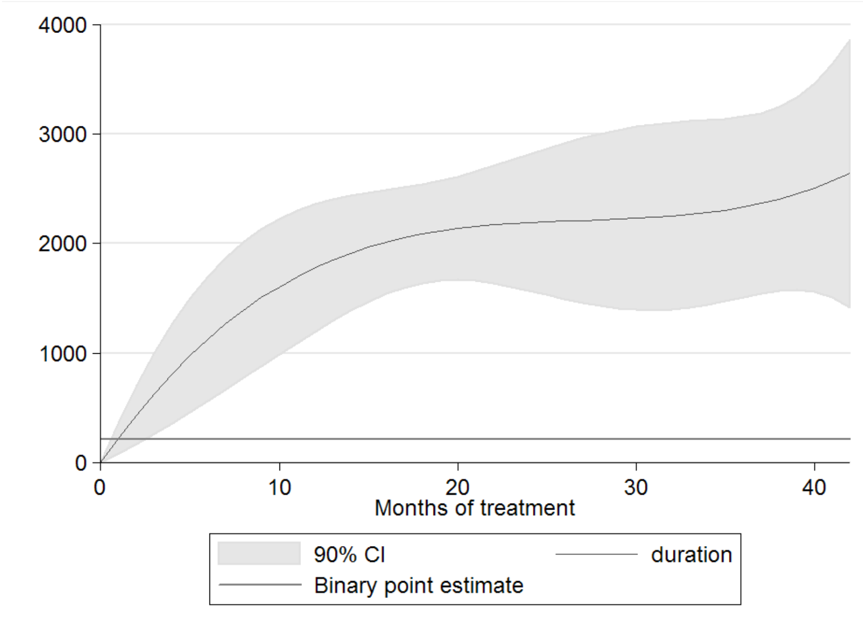


Figure 12: RBD Average Impacts on Fixed Capital

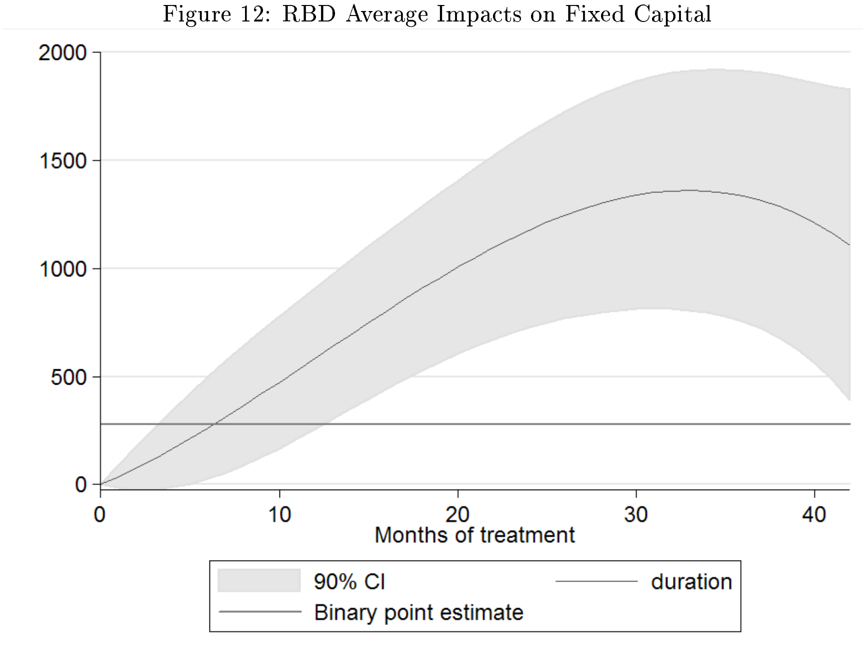


Table 13: Program Impact on Mobile Capital for Program Participants

	Binary	Binary, gender	Cubic
t_2	275.6*	275.6*	-356.6**
	(162.38)	(162.42)	(153.02)
t_3	3503.8***	3494.1***	1884.9***
	(398.36)	(398.72)	(416.70)
Z	215.5	341.9	
	(207.39)	(210.58)	
Female*Z		-893.1**	
		(428.95)	
Months (d)			235.4*
			(122.77)
Months^2 (d^2)			-8.52
			(9.33)
Months^3 (d^3)			0.105
			(0.16)
N	2106	2106	3180
R^2	0.110	0.111	0.166
adj. R^2	0.109	0.109	0.162

Cluster-robust standard errors in parentheses

Gender indicator variable equals 1 if the participating farmer is female

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

half the average maximum impact. In light of the absence of significant differences between the impacts on income for male and female farmers, this differential impact on investment is worth keeping in mind as we turn to the final impact variable of per capita consumption.

4.4. Consumption

As the final section of our analysis within the present framework, we will now look at per capita consumption. The consumption variable has been carefully constructed to take into account various aspects of household consumption and well-being. It is transformed to purchasing-power-parity adjusted US$. Further, to make it a per capita measure, it has been weighted by the number of household members (for expenditures other than food), and weighted by the number of household members that were present in the household during the appropriate recall period for food expenditures.

In order to examine the evolution of consumption estimates more closely, we report binary and continuous regression estimates in Table 15. Using the same binary regression model (4) as in the two previous sections (Column 1 in Table 15), we note that while the treatment effect is positive, it is neither statistically different from zero, nor very large. The point estimate of the binary impact implies a $186 increase in annual per-capita expenditure for participant households. With around 5 people per-household on average, this point estimate would imply an increase in total household consumption expenditures of about $1000 per-year.

Again, allowing impacts to differ by sex of the RBD program beneficiary does not reveal any significant gender-differentiated effects. However, the point estimate on

Table 14: Program Impact on Fixed Capital for Program Participants

	Binary	Binary, gender	Cubic
t_2	-29.3	-29.3	-190.3
	(171.48)	(171.52)	(149.62)
t_3	1084.1***	1082.4***	414.7
	(263.31)	(263.89)	(340.32)
Z	276.4	301.5	
	(224.11)	(230.50)	
Female*Z		-175.6	
		(377.13)	
Months (d)			35.7
			(44.48)
Months^2 (d^2)			1.59
			(2.88)
Months^3 (d^3)			-0.043
			(0.05)
N	2092	2092	3165
R^2	0.061	0.061	0.118
adj. R^2	0.059	0.059	0.114

Cluster-robust standard errors in parentheses

Gender indicator variable equals 1 if the participating farmer is female

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

the gender interaction term is larger than its counterpart in the income regression. Coupled with the size and significance of the gender-effect on mobile capital, this suggests that perhaps women beneficiaries allocate program-induced income increases differently between consumption and capital investment than do men.

These binary estimates may again mask the underlying duration response pattern. Indeed, with consumption (as opposed to investment and program income), there are reasons to suspect an initial fall in consumption if households were to self-finance matched program investments by reducing consumption. As shown in Table 10, roughly 40% of the sample is reported to be capital-constrained in the sense of having unmet demand for loans they would like to take given the cost of capital.

Appendix B reports the results of a semi-parametric analysis of per-capita expenditures using the specification in equation 5. As can be seen in appendix Figure .20, the non-parametric results suggest the existence of an initial dip in living standards, followed by positive consumption growth after about 12 months in the program. The semi-parametric results show a somewhat puzzling dip in estimated living standards after about 3 years in the program.

Taking a cue from the semi-parametric results, column 3 of Table 15 report results from replacing the binary treatment indicator with a parametric cubic function of treatment duration. The individual coefficients are not statistically significant, although the key question is the statistical significance of the overall impact duration relationship.

Figure 13 displays the 90% interval estimate of the duration response implied by the cubic estimates in Table 15. As can be seen, the point estimates imply a small dip

Table 15: Program Impact on Per Capita Consumption for Program Participants

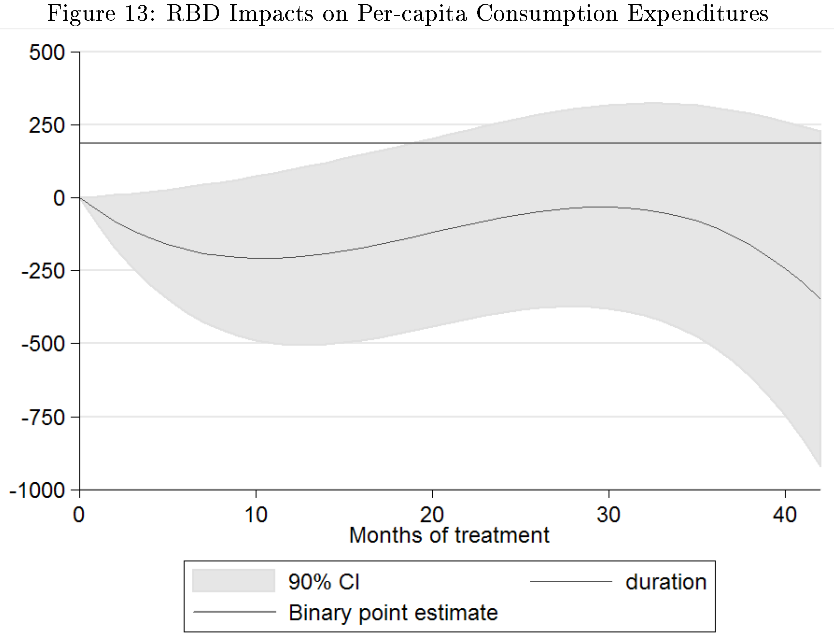
	Binary	Binary, gender	Continuous
t_2	-476.5***	-476.5***	-380.6***
	(147.34)	(147.37)	(118.20)
t_3	-211.0	-208.9	87.1
	(227.03)	(226.53)	(251.08)
Z	186.5	157.8	
	(187.98)	(201.70)	
Female*Z		201.5	
		(303.67)	
Months (d)			-45.5
			(38.12)
Months^2 (d^2)			2.97
			(2.03)
Months^3 (d^3)			-0.050
			(0.03)
N	2123	2123	3198
R^2	0.006	0.006	0.196
adj. R^2	0.004	0.004	0.192

Cluster-robust standard errors in parentheses

Gender indicator variable equals 1 if the participating farmer is female

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

Figure 13: RBD Impacts on Per-capita Consumption Expenditures



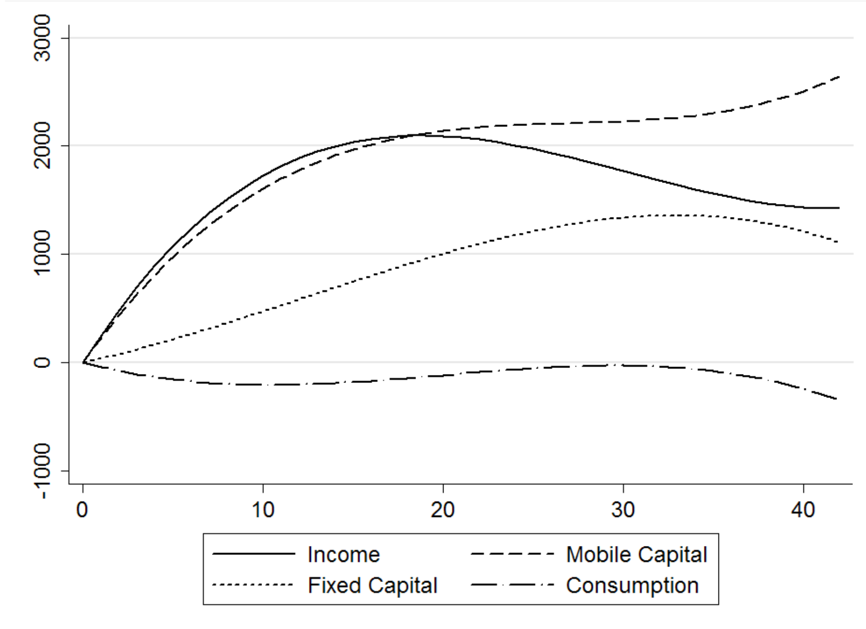
in initial living standards, followed by a modest growth that appears to only allow households to recover their baseline consumption levels. The interval estimator, however, includes zero, indicating that it is impossible to reject the hypothesis that the consumption effects are nil. Given the findings of significant impacts on RBD-targeted income and on mobile investment, the lack of a significant impact on living standards is somewhat surprising. It could simply be that the total income did not increase for beneficiary households (as opposed to income from only RBD-targeted activities). It is also possible, that total income did increase, but that most of it was allocated to investment rather than increasing immediate living standards (especially for the predominantly male farming population). Finally, it could be that the impacts are quite heterogeneous, driven in part by differences in tenure security, capital access, etc. Section 5 below returns to this issue, employing generalized quantile regression methods to investigate whether this lack of an average impact on per capita consumption levels reflects what is going on in different segments of the sample.

4.5. Adding Up Average Program Impacts

In order to add up and summarize the various program impacts, Figure 14 gathers together our preferred continuous treatment estimates for program income, fixed and movable farm investment and total family consumption (total family consumption was calculated by multiplying the per-capita amounts by the average weighted family consumption unit size of about five). The horizontal axis displays months in the program, while the vertical axis reveals financial values measured in 2005 $USPPP. As we can see in Figure 14, the estimated RBD-targeted income increases roughly equal the sum of changes in capital stocks plus consumption. Note that these impacts need not sum up if investment or consumption could be financed via borrowing. The fact that they do sum up is consistent with the relatively high level of capital constraints reported by RBD beneficiaries.

Together, the estimates in Figure 14 present a rather intriguing portrait of average RBD program impacts. The estimated average impacts on consumption are conspicuous only by their absence. Program income (which, as discussed above, is likely an *overestimate* of the change in total family income) shows a quite substantial increase. Finally, as can be seen, the estimated impacts on investment in both fixed and mobile capital are quite substantial, with long-term increases of 50% to 75%. Some of this investment increase likely reflects a direct program outcome as the RBD program partially subsidized investments needed for targeted activities. On average, however, these subsidies could only cover 30% of the planned investment, meaning that the rest of these impacts represent investment crowded-in by the RBD program. This increase in investment is likely one explanation as to why the large increases in program income show little immediate impact on per-capita consumption. In addition, the overall large confidence intervals that surround the results indicate that while we cannot reject the hypothesis that the program had no impact, we also cannot reject the hypothesis that it had very large impacts. Put differently, these results suggest that the program may have had highly heterogeneous effects, a topic to be explored in the next section.

Figure 14: Average Program Impacts



5. Heterogeneous Program Effects

As discussed in earlier analyses using the mid-line data from this program (Carter and Toledo, 2010 and Toledo 2011), there are a number of reasons for believing that programs like the RBD may result in heterogeneous treatment effects. There are at least three candidate explanations as to why such heterogeneous treatment effects might occur:

1. *Heterogeneous access to financial capital needed to make the most of the RBD intervention;*
2. *Complementarity between unobservables (such as farming skills and business acumen) and the RBD intervention; and,*
3. *Differential luck, with some succeeding and others failing for stochastic reasons.*

The midline data revealed substantial evidence of impact heterogeneity, with the program showing few impacts on the well-being of the poorest-performing 50% of the population (when compared against the poorest-performing segment of the untreated households), with quite high returns to the best performing segment of the treated group, when compared against top performers in the then untreated control group. Efforts reported in Toledo (2011) to unpack the reasons behind this heterogeneous performance are only partially satisfying. That analysis focused on explanation (1) above, categorizing households based on their credit-rationing status. While credit market status is of course endogenous, that analysis revealed no simple relationship between performance and contemporaneous credit rationing status. Indeed, the only factor uncovered was past credit history. RBD impacts on farms with prior credit history appeared quite large and significant. Unfortunately, the interpretation of prior credit history as a factor explaining heterogeneous program impacts is ambiguous. It seems most likely that those with past credit histories are actually those with higher levels of farming and business acumen (pointing toward explanation 2 above).

It may also be that those acumen levels were themselves endogenously produced by prior random or prior program-based access to credit (and business opportunities). In this section, we examine in more depth the program effects for different segments of the population, based on the notion that the average treatment effects presented above may not tell the full story.

5.1. Quantile Regression Methods and Interpretation

Conventional regression methods (such as those just employed above in Section 4) estimate average or mean relationships. They assume that the vector of covariates, x, affects only the *location* of the conditional distribution of y, not its scale nor any other aspects of y's conditional distribution. Quantile regression methods allow us to see whether the statistically average relationship is in fact a good description of the relationship in all parts of the distribution. Specifically, quantile regression allows us to recover the regression parameters that best describe the impacts on observations in different portions of the conditional error distribution for our regression model.

Observations in the higher quantiles are those that "do better" than is predicted by the household's level of treatment and other control variables (e.g., are in the upper tail of the conditional per-capita consumption distribution). We will refer to these observations in the higher quantiles as "high performers."[39] Conversely, observations in the lower quantiles are those are in the lower tail of the conditional distribution of the outcome variable. Quantile regression allows us to see if the marginal impact of RBD program participation at various parts of the conditional distribution of the outcome variables differs from the impacts at the mean – i.e. the average relationship estimated in Section 4. Note that if the average regression model explains the data well, the impact estimates will be the same for all quantiles. However, if there is unobserved heterogeneity in the impacts, then the impact slopes across quantiles may be different. As mentioned above, there are conceptual reasons (supported by the analysis of the mid-line data) for suspecting that the RBD program has heterogeneous impacts.

To recover conditional quantile estimates, we employ the method developed by Abrevaya and Dahl (2008) that extends a correlated random-effects framework (such as our regression model (6) above) to apply to conditional quantile models. While quantile models have been widely used in empirical studies since their development by Koenker and Bassett (1978), they are not often applied to panel data, likely because of the difficulty of differencing in the context of conditional quantiles. This problem arises because quantiles aren't linear operators, so that, simply put, the conditional quantile of a difference is not simply a difference of the conditional quantiles. Importantly, this methodology based on correlated random-effects preserves the fixed effects characteristics of the results, inoculating them against any systematic or spurious correlation between the duration of treatment and initial and time-invariant conditions.

Parameter estimates for the Abrevaya and Dahl (2008) estimator can be obtained through any quantile regression package. Standard errors are obtained through bootstrapping, drawing *households* with replacement from the sample and estimating the

[39] The reader may alternatively wish to think of this as "*surprisingly* high-performing" households – since they are the households that perform higher than we would expect based on their observable characteristics.

estimator's variance-covariance matrix from the resulting empirical variance matrix. All results in Section 5.2 are based on 300 bootstrap repetitions.

5.2. Generalized Quantile Regression Results

This section explores the heterogeneity of the impact or duration response function by estimating the conditional quantile functions for our preferred (cubic) parametric continuous treatment models. In the interest of space, we present these results graphically, showing the point estimates for the 25th, 50th (median) and 75th quantiles. We represent the 90% confidence interval as a shaded area around the point estimates. The confidence intervals are all bootstrapped normal-approximation confidence intervals, based on 300 replications.

Figure 15 displays the results from the quantile analysis of RBD-targeted activity income. As can be seen, these estimates confirm the hypothesis that program impacts are heterogeneous across the participant population. The impacts of the program seem to be greatest at the high end of the conditional error distribution, with the median impacts peaking at around the same figures as the lower 25% percentile, but reaching that point earlier on in the program. The high performers in the upper 75th quantile enjoy a much steeper impact response function. Indeed, it peaks at about $2000, double the long-term impact level for the producer at the median of the conditional income distribution. The income for these high-performers does seem to drop off somewhat, but still remaing positive by the end of the 42 months.

The effects of the RBD program on mobile capital (Fig. 16) increases as we move upwards in the conditional distribution of mobile capital – peaking at just under $300 for the lowest 25th percentile, around $1,200 at the median, and around $2,100 for the 75th percentile. Towards the end of the program, however, the amount of investment in mobile capital dips somewhat for the 75th percentile. The impacts on fixed capital can be seen in Figure 17, and show small impacts for the 25th percentile and the median, but show substantial and rising increases for the 75th percentile of the conditional distribution of fixed capital.

We detect heterogeneity in the impacts on per capita consumption, but the pattern of heterogeneity is quite different from the effects on, say, program income. Figure 18 shows the duration response paths for three different quantiles. The point estimates for those households at the lower end of the conditional per capita consumption distribution experience no significant increase in consumption for the duration of the program. For the median regression, consumption does increase over the range of months. In contrast, the high performers in the 75th quantile show a substantial Ashenfelter's dip during the first half of the duration, slowly rising back (barely) to baseline levels.

6. Internal Rates of Return

Internal rates of return (IRR) on dollars invested in a development program is one way to gauge program effectiveness, and to compare across different types of programs. In this section, we present IRR calculations for our different measures of program impacts on targeted activity income and consumption. For all calculations we assume the following:

Figure 15: Generalized Quantile Impact Results for Program Income

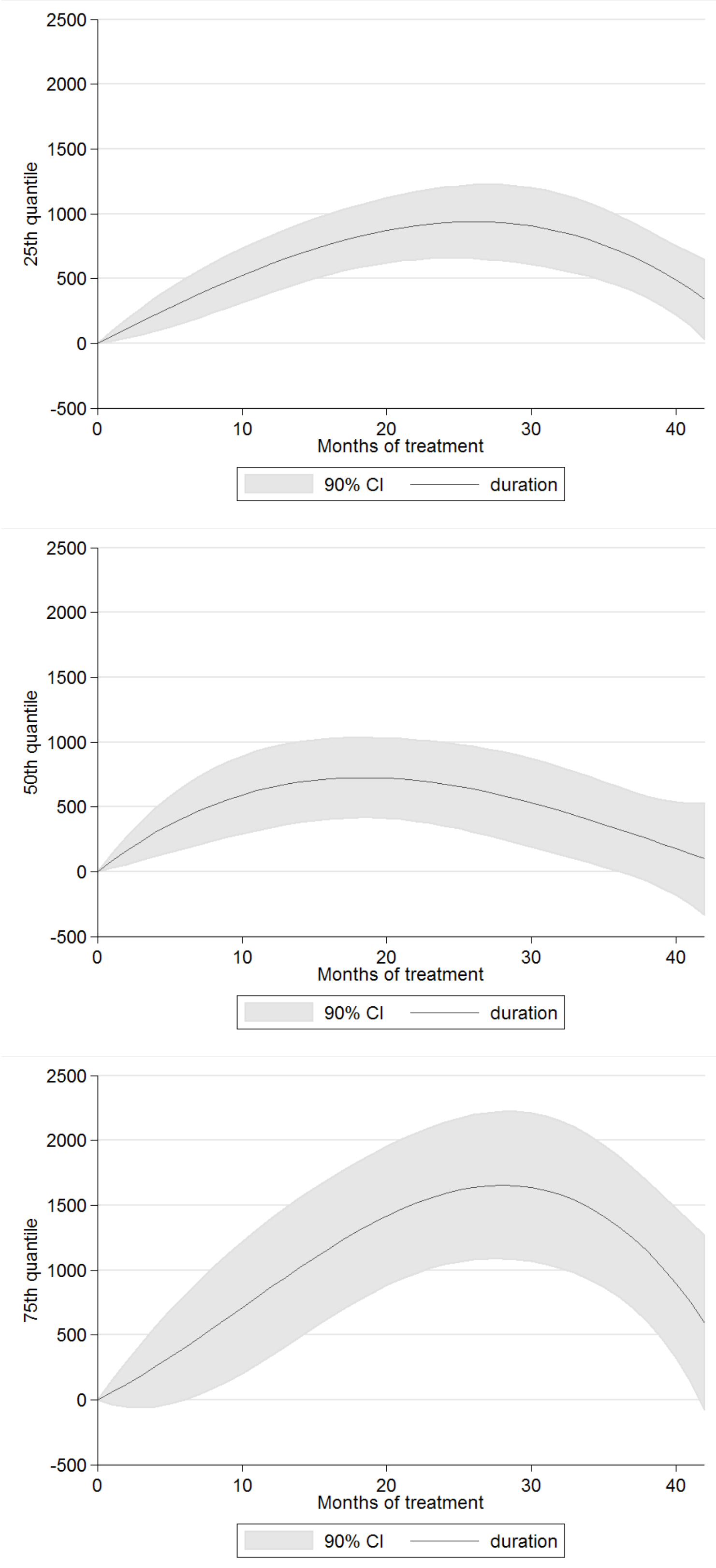


Figure 16: Generalized Quantile Impact Estimates for Mobile Capital

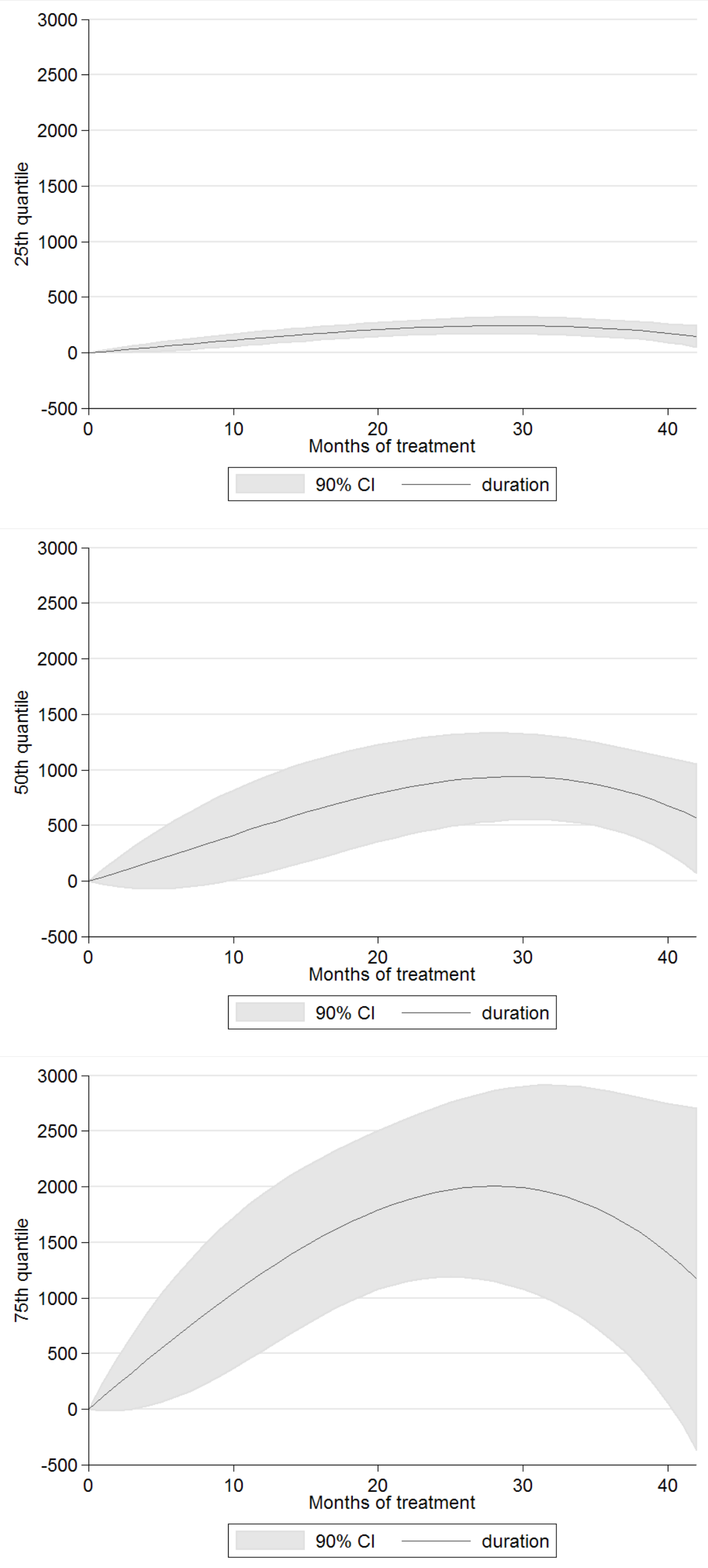


Figure 17: Generalized Quantile Impact Estimates for Fixed Capital

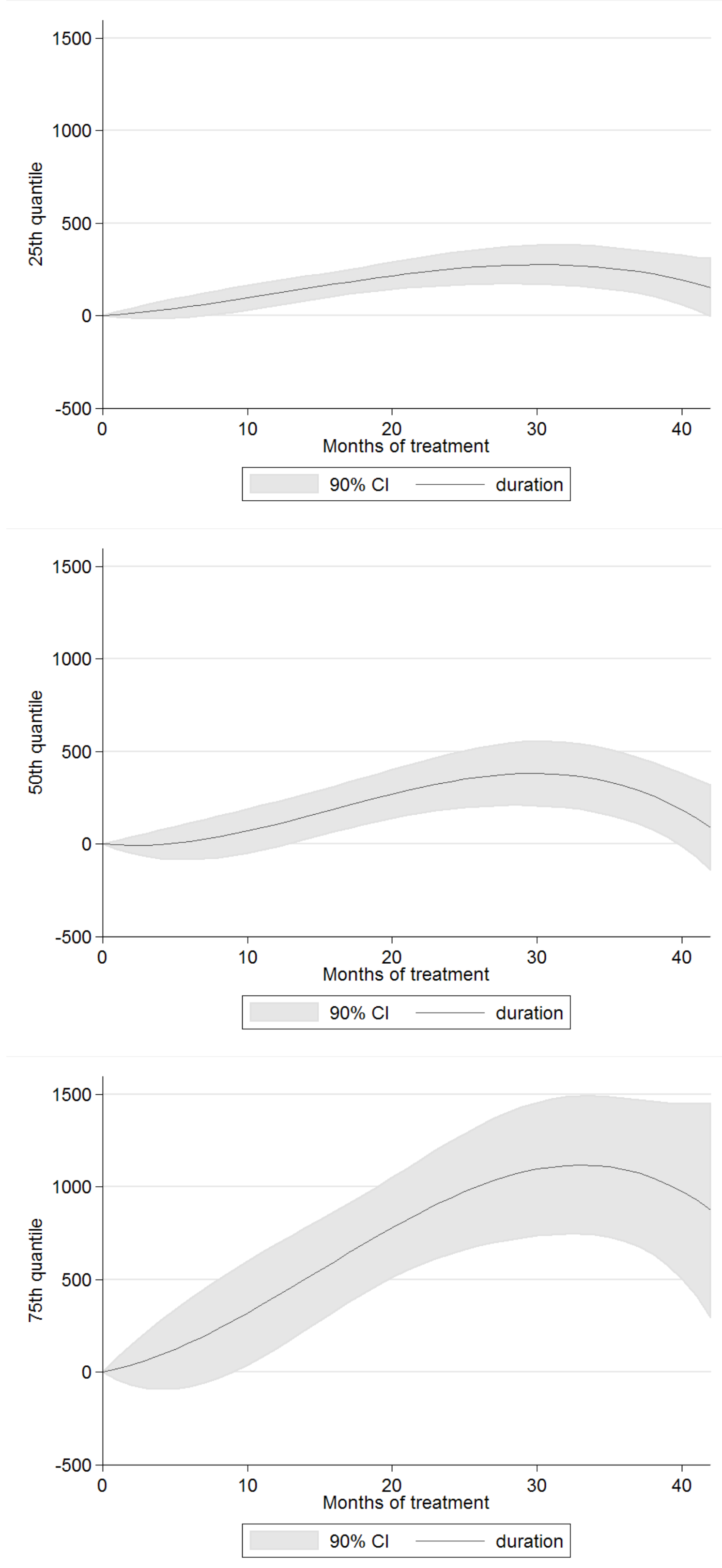


Figure 18: Generalized Quantile Impact Estimates for Per-capita Expenditures

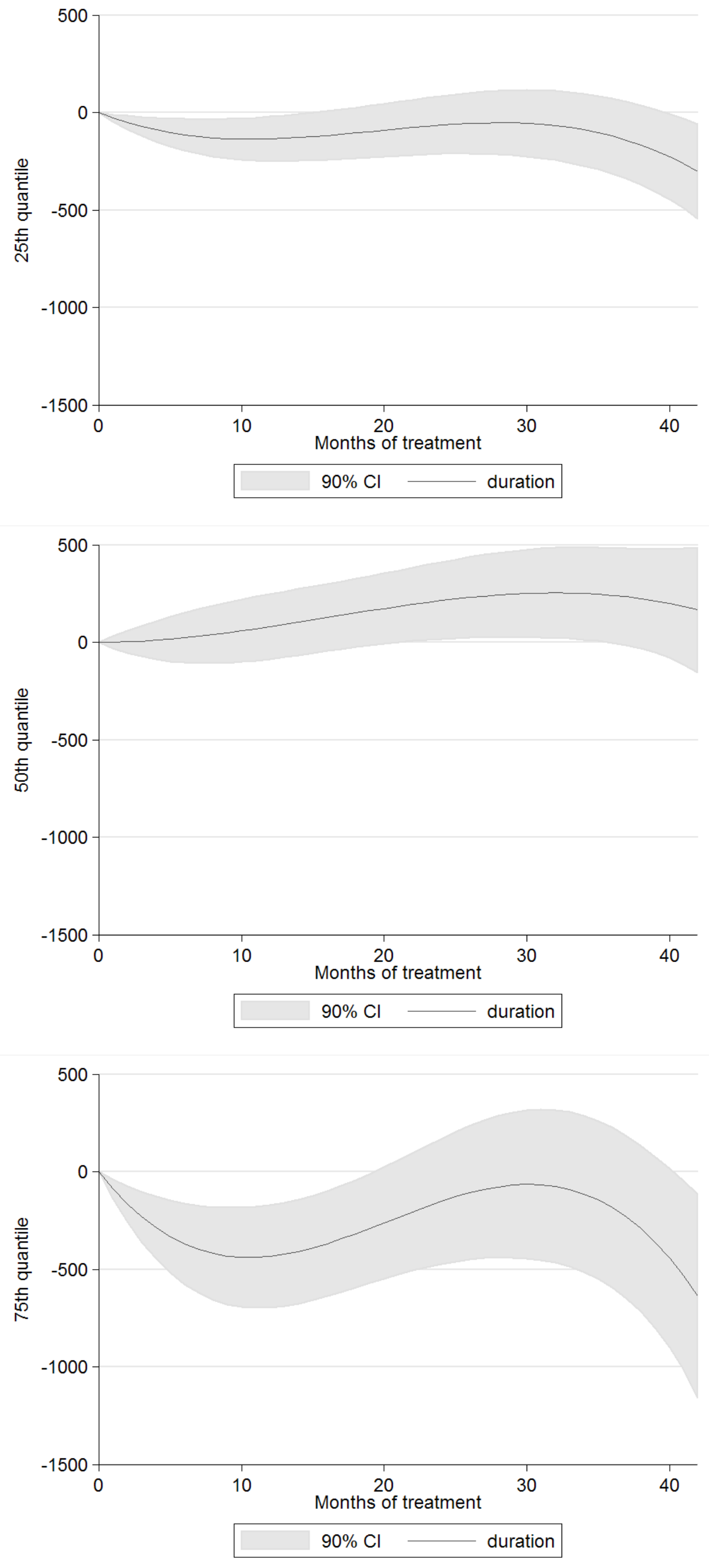


Table 16: Internal Rates of Return

	Impact Estimate		Internal Rate of Return	
	Binary	Continuous	Binary	Continuous
RBD Targeted Activity Income				
Average	$1212	$2000	4%	14%
Low Performers		$1000	–	0.4%
High Performers		$1700	–	10%
Per-capita Consumption				
Average	$186.5	$0	-1%	–
Low Performers		-$50	–	–
High Performers		$0	–	–

- Direct program costs are $3,194[40] per-beneficiary. This figure is a weighted average of figures reported by the Rural Business Program implementation team.
- Program impacts persist for 10 years.
  - In the case of the IRR calculations based on the continuous treatment estimators, we assume that year one impacts are zero and that the estimated peak impacts (which roughly occur by year of the program) occur in year 2 and persist through year 10 without further change.
  - In the case of the IRR calculations based on the binary treatment estimates, we assume that program impacts occur immediately in year one and persist at that level through year 10.

Table 16 reports the IRR calculated based on these assumptions. The first column presents the figures used as the basis for the calculation. The second column presents the actual IRR estimates. IRR's are provided for both average impacts (as estimated in Section 4) as well as for low and high performers (as estimated by the 25th and 75th quantile estimates in Section 5). Note that in the case of valuing consumption, we use the point estimates as our best guesses for the impacts even though they are not always statistically different than zero. Note also that we have translated our per-capita expenditure variable (the economic well-being measure used in the impact analysis) into a total expenditure measure by multiplying the per-capita by the average household size (5.3 members).

As can be seen, using our preferred continuous treatment estimates, the IRR for the average producer based on impacts on RBD targeted activity income is 14%.[41] The substantial heterogeneity in impacts between low and high performers is similarly reflected in a wide variation in IRR, with the IRR on program resources dedicated to high performers coming in at 10%, while it is only 0.4% on low performers.

[40]This number is in current US dollars, but was converted to 2005 PPP dollars for the purposes of the IRR calculations.

[41]The IRR using the binary treatment estimator is 4%, with this lower figure, reflecting the fact that the binary estimates are effectively a data weighted average of different portions of the impact duration curve.

Unfortunately, these are ex post categories, and we have yet to find a way in which high and low performers could have been ex ante identified such that program resources could have been targeted at those who would give the largest returns. It should also be remembered that these impact measures are likely an upper bound estimate on the income increases actually enjoyed by families.[42]

Finally, the IRR based on consumption impacts are indeterminate. As discussed before, the lack of impacts on consumption remains something of a puzzle, as it is unclear whether it is simply a short-term phenomenon as households invested their immediate gains in productive assets, or whether it reflects impacts on total income (not just income in targeted activities) that are rather more modest than the estimates used to construct the IRR's in Table 16.

7. Conclusion

A key part of the 5-year Nicaragua-MCC compact, the Rural Business Development Program (RBD) was designed to group 20-30 geographically proximate farmers together into *nucleos*, enhancing their business knowledge and improving their access to markets and technologies. RBD direct assistance to *nucleos* lasted for 24 months. The program also included elements of matching investment (*e.g.*, in improved milking sheds), and had average direct costs of about $US 3,194 per-farmer in the program. Participation in the program was subject to both administrative filters (eligibility criteria and business plan approval) and to beneficiary self-selection (eligible producers had to be willing to join and provide required matching investments).

In order to evaluate the impacts of the RBD, the evaluation and implementation teams worked together to create a randomized program rollout strategy. No eligible households were denied access to the program, but the temporal sequence in which they received the program was randomized as eligible households were split into early and late treatment groups. Three rounds of data were collected: a baseline in 2007, a midline in 2009 and an end-line in 2011.

This evaluation strategy affords several advantages. First, shortly after the midline survey, it was possible to fully identify a two-sided complier sample as all eligible households in both early and late treatment groups had either accepted or declined the invitation to join the RBD by that date. The analysis has focused on this subpopulation as it those who are inclined to join such a program that we are interested in.

Secondly, this research design randomized the duration of time that any particular producer had in the program. Using continuous treatment methods, we have been able to recover temporal time path of impact. Doing so is especially important for programs like the RBD that are intended to spur learning and co-investment, meaning that their impacts are likely to evolve over time. Given that we did not know ex ante how long it would take for these effects to take place, the continuous treatment methodology allows us to recover much more information than could have been obtained with standard binary treatment estimator approaches.

[42]As discussed earlier, observed incomes from the targeted activities may overstate the overall income increases, since productive inputs may have been reallocated from other income-generating activities toward the target crops.

7.1. Key Findings: Average Impacts and Impact Heterogeneity

Analysis of these data indicates that RBD likely increased the incomes of most, but not all participants. Impacts built up over time, peaking and leveling off after the individual has been in the program for 2 to 2.5 years. Using these peak (asymptotic) impact estimates for income generated in RBD-targeted activities, we find an internal rate of return of 14% for funds invested in the average producer.

It is important to stress that income increases in targeted activities are an *upper*-bound estimate of the change in overall household income, and hence the 14% average internal rate of return figure is also an upper-bound estimate. Note also that this figure is an average across all RBD-targeted activities that were included in the study. It is likely that returns were higher for some activities, and lower in others. The impact evaluation was not, however, designed to estimate activity-specific impact estimates nor rates of return.

While the impacts on income in targeted activities are substantial, the spillover of these income increases into improved living standards appears to be at best modest, at least over the time frame of the evaluation. Evidence that stocks of agricultural capital (especially movable capital) increased significantly with the program is consistent with this sluggish consumption response and would seem to indicate that households face binding credit constraints and an inability to borrow in order to finance investments and smooth consumption over time.

In addition to these average effects, the study employed fixed effects analogue conditional quantile regression methods to explore the degree to which the average pattern of impact faithfully reflects the experience of both high performers (those who do better than the OLS regression average) and low performers (those who do less well than the OLS regression average). Looking at the full distribution of impacts is especially important for efforts like the RBD program that target beneficiaries' income-generating and entrepreneurial capacities. In general, we find that the program is much more effective for the high performing households. Indeed, the upper quantile, high performing households exhibit a 50% larger impact on their income in targeted activities, and their observed household living standards (as measured by per-capita consumption expenditures) increase significantly 2-3 years after joining the RBD program. In contrast, the lower quantile households show no increase in living standards, even after 3-4 years in the program.

7.2. Programmatic Implications and Looking Forward

The RBD program was an ambitious effort to target the small- and medium-farm sector, integrating them into higher valued and more productive agricultural activities. With appropriate caution given the somewhat puzzling finding on household living standards, it is fair to say that the program succeeded for most, but not all of the targeted households. In rough numbers, two-thirds of eligible farm families chose to participate. The one-third that did not participate in fact had modestly lower living standards at baseline.[43] Of those that chose to participate, roughly

[43] Recent work by Macours and Vakis (2009) and Laajaj (2012) on poverty and aspirations suggest that there may be some individuals who could benefit from interventions such as the RBD, but that they need smaller, confidence and aspiration building steps before they are willing to jump into a more forward-looking and entrepreneurial profile.

three-quarters appeared to have benefited, while the remaining minority benefited little, if at all from the RBD.

The existence of these two minority groups (those that did not participate, and those that did, but did not succeed) serves as a useful reminder that maybe not all small farms can upgrade and succeed. If the goal is to eliminate rural poverty, then this limitation needs to be kept in mind as other interventions may be needed to improve prospects for this sub-population and their children.

Looking forward, it may be that next generation RBD programs can reduce the size of this minority. While the analysis here was unable to identify which families failed to succeed and why,[44] it is likely that some failures were due to the natural vagaries of agriculture as a risky activity. Efforts to incorporate elements of insurance into small farm development strategies may have a key role to play in this regard, allowing a greater percentage of the small farm population to succeed over the longer term.[45]

In addition, the RBD program did not include a direct credit market intervention. The overall MCC program in Nicaragua operated in part on the theory that improved property registration would indirectly improve smallholder access to capital by increasing their collateral and creditworthiness to the extant banking sector. Whether or not that strategy would have worked remains an open question, as the property registration component of the program was eliminated in early 2009 (see note 2 above). What is clear is that the pattern of increasing income, but sluggish changes in living standards (and indeed, perhaps a small initial drop in household living standards following the introduction of the program), may signal the existence of capital constraints as income increases are soaked up to self-finance future fixed and working capital investments.

Looking forward, this evaluation suggests at least two outstanding questions about the Nicaraguan program itself. First, will the realized gains sustain themselves over time? Second, will household living standards eventually catch up with the estimated income gains? In principal at least, both questions could be addressed with an additional round of data and further reliance on the continuous treatment estimates used in this study.

[44] One important message that emerged from the midline evaluation is that there is no evidence that farms closer to the asset minima benefited less from the program than did better endowed farmers (Carter and Toledo, 2011). While the asset floors and ceilings used to establish RBD eligibility were based on best practice intuition, it is clear from a targeting perspective that more work needs to be done to see if there is such a thing as a farm that is too small to benefit from this kind of intervention.

[45] The I4 Index Insurance Innovation Initiative (http:\\i4.ucdavis.edu) is exploring a number of efforts to link insurance and credit in order to secure prudential small farm risk-taking and agricultural intensification.

Appendix A: Binary Intention to Treat Estimates using the Full Sample

Table .17 shows estimates of the intention-to-treat and instrumental variable estimates of the average treatment effect using the baseline and midline data. The intention-to-treat is estimated by the following regression:

$$y_{it} = \lambda t_2 + \gamma B_i + \delta(B_i^* t_2),$$

where t_2 is a dummy variable taking on the value 1 in round 2, and

$$B_i = \begin{cases} 1 & \text{for farmers randomized into the early treatment group} \\ 0 & \text{for farmers randomized into the late treatment group} \end{cases}.$$

This regression is estimated using the whole sample. y_{it} here is either per capita consumption or farm income. As in much of the main analysis, the parameter δ is the parameter of interest.

Table .17: ITT and IV Estimates

	Consumption		**Income**		**Mobile Capital**		**Fixed Capital**	
	ITT	IV (ToT)	ITT	IV (ToT)	ITT	IV (ToT)	ITT	IV (ToT)
λ	-516.3***	-516.2***	1479.0***	1479.1***	106.9	106.7	-190.9	-191.3
	(196.4)	(196.1)	(552.6)	(551.8)	(358.7)	(358.3)	(286.5)	(286.1)
γ	-86.55	-134.2	-467.1	-712.1	1022.5**	1581.9**	256.4	400.4
	(206.4)	(318.0)	(452.5)	(692.6)	(443.1)	(681.4)	(319.9)	(493.1)
δ	-5.004	-7.563	834.0	1269.7	74.11	111.4	348.3	532.9
	(248.8)	(381.6)	(742.2)	(1127.7)	(617.9)	(944.9)	(442.4)	(677.3)
N	3179	3179	3040	3040	3155	3155	3145	3145
R^2	0.218	0.218	0.304	0.304	0.126	0.126	0.098	0.101
$\overline{R}^2$	0.216	0.215	0.302	0.302	0.123	0.123	0.096	0.098

Standard errors in parentheses

Regression coefficients for control variables (crop, farmer age and education) are suppressed.

The first, third, fifth and seventh columns of Table .17 reports standard Intention to Treat estimates (ITT) for consumption, income, mobile and fixed capital respectively. For the Treatment on the Treated (ToT) estimates, we use the standard instrumental variables technique, instrument for treatment, d_i, using the assignment to early treatment, B_i. The instrument for the interaction term between treatment and the midline time-dummy variable ($d_i^* t_2$) is simply $B_i^* t_2$.

The ITT estimates are consistent with the average treatment effects reported in the main body of the paper, and while both the ITT and ToT estimates for consumption are negative, it's important to note that given the wide confidence intervals, the estimates are consistent with the small but positive impacts found above, as well as with a zero impact.

Appendix B: Semi-parametric Estimates of the Continuous Treatment Estimator

Figure .19: Semi-parametric Estimates of Impact Duration Function for Program Income

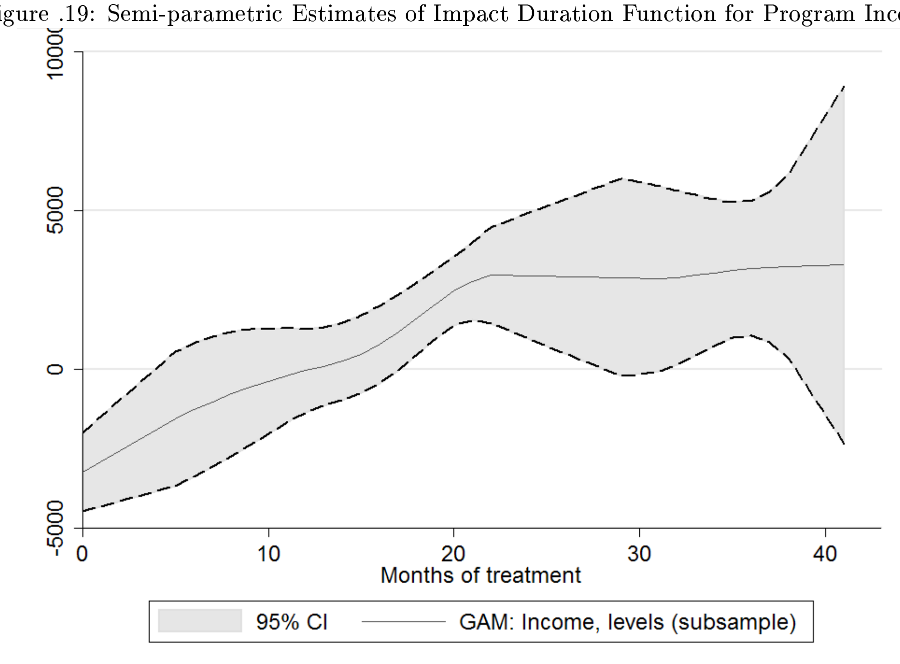
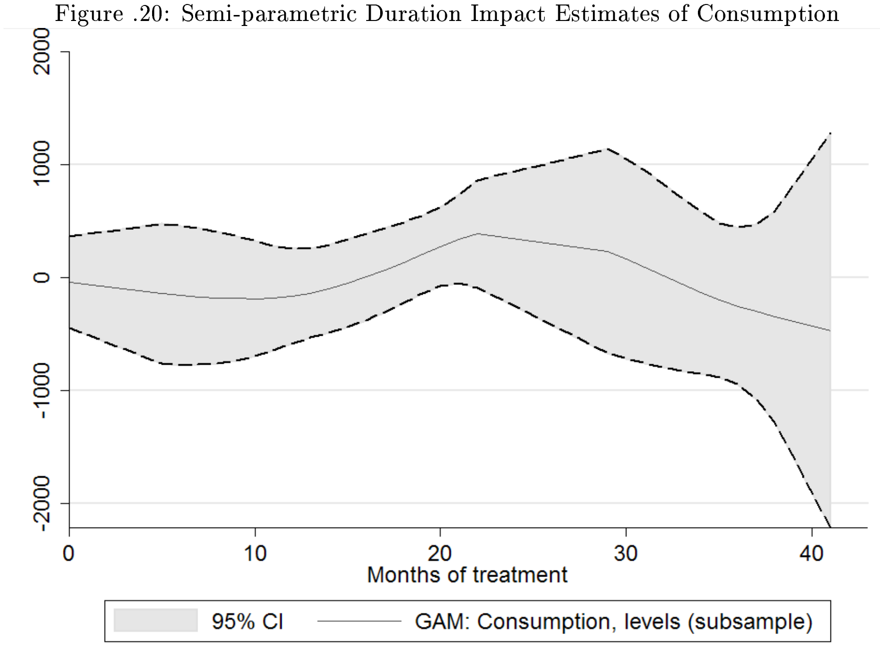


Figure .20: Semi-parametric Duration Impact Estimates of Consumption



As explained in the body of the paper, there are a number of reasons to expect that the impact of the RBD program to evolve over time in highly non-linear ways. As a prelude to the parametric continuous treatment analysis reported in Sections 4 and 5, we first used flexible semi-parametric methods to explore the shape of the impact curve. The method used estimated equation 5 by allowing the time variables to enter parametrically, while the duration function was estimated non-parametrically. Estimation was carried using the generalized additive model commands in the software *R*. The appendix figures .19 and .20 display the results of these estimates. These were estimated using only those observation that began treatment after the mid-line.

As can be seen, both semi-parametric regressions suggest that the impact relationship can be captured by a cubic relationship. Neither is especially supportive of the step function in Figure 8 that would be implied by the standard binary treatment estimator.

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