A Matter of Time:
An Impact Evaluation of the Brazilian National Land Credit Program

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Abstract
This paper provides an impact evaluation of the *Programa Nacional de Crédito Fundiário*, a market assisted land reform program in Brazil. The paper uses a panel dataset and pipeline control group to evaluate the program's impact on agricultural production and earned income, using a difference-in-differences model with either municipal or individual fixed effects. The heterogeneous effect of additional years of land ownership is investigated. The findings suggest that the program increases production and earned income by about 74% and 37%, but only after four years of land ownership. The conclusions are supported by a number of robustness tests, although considerable attrition and potential bias due to unobserved variables suggests caution. The benefits of the program largely go to making debt payments. If the impact on income continues to grow, as it did in the first five years, improvements in net wealth and current welfare could both be achieved.

Keywords: Market Assisted Land Reform, Asset Transfers, Programa Nacional de Crédito Fundiário, Rural Poverty, Brazil
JEL Classification: Q15, O13, O22

I. INTRODUCTION

At the beginning of the 21st century, the rural areas of developing countries were home to nearly 900 million people living on less than one dollar per day, and over two billion people living on less than two dollars per day (World Bank, 2007). Households are more likely to be chronically poor when they have low levels of assets (Carter and Barrett, 2006; Bird et al., 2002). Policies that facilitate access to land—one of the most important assets in rural areas—may be able to assist poor rural households to develop and eventually sustain a non-poor standard of living (Finan et al., 2005; de Janvry et al., 2001a). While important, land acquisition by itself is often insufficient to eradicate poverty; supporting infrastructure, access to credit, technology, and markets are also essential in order to elevate asset returns (Deininger, 2003). A lack of these complementary policies is one important reason why so many 20th century land reform programs in Latin America fell short of expectations (Griffin et al., 2002; de Janvry et al., 2001b). State-led land reform programs based on expropriation and redistribution, such as the one pursued in Brazil since the mid-1980s, have encountered many obstacles (Sikor and Muller, 2009) and have often reproduced the high levels of poverty that exist in rural areas (Perreira, 2013). The Brazilian National Land Credit Program (*Programa Nacional de Crédito Fundiário*, PNCF) was created as a complementary "market assisted" channel of access to land. It provides beneficiaries with subsidized loans to purchase land from willing sellers, as well as assistance with complementary investments. Between 2002 and 2012, the PNCF assisted over 90,000 beneficiaries to acquire land. This paper provides an impact evaluation of this program.

There are few rigorous impact evaluations of land reform programs in general (Binswanger et al., 2009), and the debate surrounding the effectiveness of market assisted land reform programs has been highly politicized (Deere and Medeiros, 2007; Sauer, 2006; Borras, 2003). Opponents tend to believe that land reform should be redistributive and seek to reduce land inequality, while proponents tend to focus on poverty reduction. Evaluations of market assisted land reforms are rare (Datar et al., 2009; Sparovek et al., 2003; Deininger, 1999;), and Brazil is no exception. This has lead Navarro (2009, p. 280) to argue that more research on the PNCF is necessary because there is no agreement on the program's effectiveness. Keswell and Carter (2014) provide one of the most credible studies of a market assisted land reform program. In an evaluation of the South African program Land Redistribution for Agricultural Development, they conclude that living standards initially decrease with land transfers, but after three years of land ownership, living

standards increase by fifty percent. We seek to contribute to this debate by providing evidence from a similar program in a different part of the world. Because we utilize panel data to evaluate this program, in contrast to the single cross-section used for the South Africa study, fewer assumptions are required to address concerns due to potential endogenous program placement or the unobservable characteristics of the participants.

This paper evaluates the Rural Poverty Alleviation (CPR) line of the PNCF on the outcome variables of agricultural production and earned income. This is the line of the program that was most important in the Northeast of Brazil where rural poverty is most pervasive and severe. A variety of approaches are used to address the potentially confounding effects of self-selection and endogenous program placement. First, the paper uses a panel dataset from 2006 and 2010 of beneficiaries randomly selected from program participants and a control group randomly selected from the program's pipeline. Because both treatment and control groups applied to the program, and were verified to be eligible, the use of a pipeline control group helps to reduce concerns over unobservable differences between the two groups. Second, we use a difference-in-differences model with either individual or municipal fixed effects. The individual fixed effects model removes unobservable individual level characteristics that are time invariant, and is thus our strongest model. Third, we estimate a model that dispenses with the pipeline control group altogether and uses the beneficiaries who were treated late in our period as a control group for the beneficiaries who were treated early. Finally, concerns related to the influence of unobservables—either at the level of individuals and groups, or as a reflection of endogenous program placement—are also addressed by the inclusion of an "eagerness" proxy (Agüero et al., 2009). The proxy measures whether groups acquired land early in their local context, and is intended to reflect the possibility that these groups might have been more motivated, could have acquired higher quality land, or may have been chosen by officials interested in demonstrating the success of the program.

While panel data has many advantages, there was also considerable attrition in this panel. Multiple tests provide mixed evidence on whether or not the attrition was random. Given the possibility of non-random attrition, models were estimated with weights to correct for attrition. Lee (2009) bounds were also calculated. As an alternative, repeated cross-sectional data were used instead of the balanced panel, thereby including individuals who attrited after the baseline and replacements who were only observed in the follow-up. While most qualitative results remain unchanged across approaches, in a few cases positive coefficients are no longer statistically

different from zero. As with concerns related to self-selection and endogenous program placement, the attrition in our panel suggests that our results are not definitive.

The findings of this paper suggest that the Brazilian National Land Credit Program has an important impact on the outcome variables of program participants. Yet the benefits of land ownership only start to appear after a certain amount of time. While there is no statistically significant impact on agricultural production or earned income in the first three years of land ownership, positive impacts begin to emerge in the 4th year, and after 5-6 years of program participation many of the results become statistically significant. Our point estimates suggest that production and income of the control group would rise by around 74 percent and 37 percent, respectively, with five years of participation. These are important gains for households living at around US$2 per day, most of whom qualify for the *Bolsa Família* conditional cash transfer program in Brazil. Because the PNCF program requires the repayment of the loan, however, a complete evaluation of its effectiveness in reducing rural poverty must take the burden of the debt into consideration. When this is done, the results suggest that the benefits of the program largely go to making debt payments and improving the net worth of the beneficiary households rather than to raising current income. If the beneficiaries' income continues to grow at the rate observed in the first five to six years of land ownership, it is likely that they will soon reach a level at which they could simultaneously meet their debt obligations and raise their standard of living.

In Section II of the paper, background information on the program and dataset are provided. The methodology is described in Section III and descriptive statistics are presented in Section IV. Section V contains a discussion of the main econometric results. Section VI provides a battery of robustness checks, including an analysis of attrition. Section VII analyzes beneficiaries' ability to repay the PNCF loan, and Section VIII offers conclusions.

II. BACKGROUND AND DATA

Market Assisted Land Reform (MALR) began as a pilot project in Colombia in 1994 (Deininger, 2001). It was then implemented in South Africa, Brazil, Honduras, El Salvador, Guatemala, Mexico, Malawi, and the Philippines. MALR was first implemented in an experimental fashion in Brazil in 1997 as a joint effort of the World Bank and the Brazilian government. It was opposed by social movements in Brazil, including the Landless Rural Workers Movement (MST), who viewed it as undermining the prospects for a large redistributive land

reform (Deere and Medeiros, 2007). A key turning point came when the National Confederation of Agricultural Workers (CONTAG) decided to support MALR as a complementary strategy to the redistributive approach (Sparovek and Maule, 2009). Due to the perceived success of the experimental projects, and their cost advantage relative to the state led redistributive model, the Brazilian government created the PNCF as a national program in 2003. The program functions by providing subsidized loans to poor farmers to purchase land from willing sellers. There are two main lines of credit within the PNCF, each of which is aimed at different target populations. The analysis in this paper is limited to the Rural Poverty Alleviation line (CPR) in order to assess the ability of the program to reduce poverty in the Northeast of Brazil. This is the poorest region of the country, and over half of the rural poor reside there. By 2012, this line of the program had spread to all of the states in this region, reaching 48,000 beneficiaries.[1]

The PNCF aims to promote access to land and to provide infrastructure on the acquired lands. There are a series of eligibility requirements for enrollment, including earning less than R$9,000 (US$5,049) per year, having assets totaling no more than R$15,000 (US$8,415), not owning enough land to sustain a family, and having at least five years of experience as a farmer.[2] Individuals apply to this line of the program by forming an association with other interested individuals. Once all of the eligibility requirements are verified, the eligibility of the land intended for purchase is checked. The most important criteria with respect to the property are that it not be eligible for expropriation through state led land reform, and that the property's price be similar to those of other properties in the same region, using the Ministry of Agrarian Development's Land Market Monitoring System as a guide. It is important to emphasize that the entire process begins at the local level, often with the assistance of the local CONTAG chapter. It is their representatives who are normally responsible for providing the initial declaration of group eligibility. In municipalities that have rural development councils, these emit the eligibility documents instead. The PNCF does not have local program officials selecting projects.[3]

[1] The other line of the PNCF is the Consolidation of Family Farming (CAF), which has a higher income cap for eligibility than the CPR. Another important difference is that CAF makes individual loans, while CPR makes group loans. CAF has been more important in other regions of the country.

[2] The values are according to the CPR Manual of 2009 and the dollar values were calculated with the January 2010 exchange rate of R$1 to US$0.561.

[3] Personal communication with Marlon Duarte Barbosa, federal government employee with 13 years of experience working with the PNCF. The absence of local program officials should reduce the scope for endogenous program placement. It is still possible that this happens in other ways, or at higher levels.

After ensuring that both the association and the land meet the eligibility requirements, a productive project for the land is analyzed by a state level technical commission which includes individuals drawn from other state agencies with expertise in agriculture. The commission then provides a recommendation to a state council which makes the final decision to approve or reject the loan. The maximum amount of the loan per beneficiary was R$40,000 (US$22,440) in 2009, however, each region of the country had different caps associated with local market prices. In the Northeast, the average loan size was about half of the maximum. In addition to 14-17 year loans made for the purchase of land, the program makes infrastructure grants available to the association, which can be used to build houses and community infrastructure, or to purchase capital for agricultural production. In an effort to create an incentive for the land price to be negotiated as low as possible, the R$40,000 cap applies to the sum of the grant and loan. Thus, the smaller the land component, the larger is the grant component. After the acquisition of the land, technical assistance is provided.

In 2012, the Ministry of Agrarian Development released a report in Portuguese of an impact evaluation of the PNCF (Sparovek, 2012). That evaluation used a similar dataset as this paper, but a different methodology throughout. The authors used propensity score matching together with a difference-in-differences approach. Unlike the results presented here, they found no impact on monthly monetary income or gross agricultural production. The differences are most likely due to the fact that they did not distinguish the heterogeneous effect of additional years of land ownership, nor did they systematically account for the changing number of family members over the two waves of the survey. Differences in the datasets used could also matter. Our sample has thirty-nine percent more observations, largely because we do not exclude the non-beneficiaries who became beneficiaries between the two waves of the panel.

The dataset used in this impact evaluation is a two period panel, collected in 2006 and 2010. We were involved in the creation of the questionnaires used in both periods, in addition to the data collection process in the second period. The data were thoroughly cleaned to ensure that no observations were wasted.[4] The treatment group of this dataset was randomly selected from members of beneficiary associations through stratified random sampling, by municipality,

[4] For example, if sex was missing but name was not missing, sex could be inferred from the name because Brazilian names are generally unambiguous with respect to sex. Similarly, if the land purchase date was missing for one beneficiary but not for others in the same association, the missing value could be corrected.

association and member. Six members were randomly selected from each association. We call these beneficiaries (B).[5] The control group was drawn from members of associations in the program pipeline—those that were enrolled in the program, were deemed eligible as program participants, but had not yet acquired their land. These pipeline non-beneficiaries (PNB) were selected from the same or neighboring municipalities as the randomly selected projects of beneficiaries. As will be explained below, many pipeline non-beneficiaries acquired land between the baseline and follow-up periods, and thus transitioned into the treatment group. This was a natural result of the continued growth of the program.

In the baseline, the reference period for beneficiaries was a single twelve-month period immediately *prior to the acquisition of land* through the PNCF. Because groups acquired land at different times, this twelve-month window changed from project to project. In order to minimize potential measurement error due to recall, the universe of projects that was used to sample from was restricted to those projects that had been created in the thirteen months prior to the fieldwork in 2006.[6] In contrast to the beneficiaries in the baseline, the reference period for the pipeline non-beneficiaries in both the baseline and the follow-up, and for the beneficiaries in the follow-up, was the twelve months immediately *prior to the interview*. The original sample had 1335 individual members; of these, about forty-two percent attrited. In order to take advantage of the panel nature of the data, the main analysis in this paper uses a balanced panel of 763 individual members that were interviewed in both periods. Attrition is subsequently dealt with in detail in the Section VI.

The balanced panel has 363 pipeline non-beneficiaries and 400 beneficiaries in the baseline period. By the time of the follow-up period, 162 of the pipeline non-beneficiaries had acquired land. Because of this expected change, the final count of pipeline non-beneficiaries is 201, and the final count of beneficiaries (defined as having been observed to acquire land through the PNCF during the sampling period) is 562. Because different associations of beneficiaries acquired land at different times, we explore the impact of the duration of exposure to treatment on outcomes. For this analysis, we divide the beneficiaries in the follow-up period into groups based on the number of years of land ownership (LO). Specifically, three groups are defined: beneficiaries with three or less years of land ownership (LO $\leq$ 36 months), beneficiaries with four years of land ownership

[5] We use association and project interchangeably.
[6] In the robustness section, we restrict this window even further to ensure that longer periods of recall are not distorting the results.

($36 < LO \leq 48$) and beneficiaries with five or six years of land ownership ($48 < LO \leq 67$). Table 1 (in Section 4) shows that these groups have 99, 317, and 146 members, respectively.

The number of people in member households was found to be decreasing between the baseline and the follow-up period. In the baseline period, both pipeline non-beneficiaries and beneficiaries had between 4.7 and 4.8 people per household, respectively. In the follow-up period, the pipeline non-beneficiaries had 4.5 people per household while the beneficiaries had 3.9. The decline for the beneficiaries is associated with moving to the new properties and shedding some of the non-nuclear household members. Because of these changes, we use outcome variables measured in household per capita (HHPC) units. The outcome variables that were analyzed were earned income and agricultural production, which will be defined in detail in Section IV. In all cases, the income and production variables were deflated to Reais of January of 2010. Figure 1 displays mean agricultural production and earned income, in HHPC units, by period, status and number of years of land ownership (LO). The means display an important pattern that this paper addresses: the positive—or U-shaped—relationship between the value of outcome variables and the number of years of land ownership. Earned income, for example, declines in the first three years of land ownership and rises thereafter.

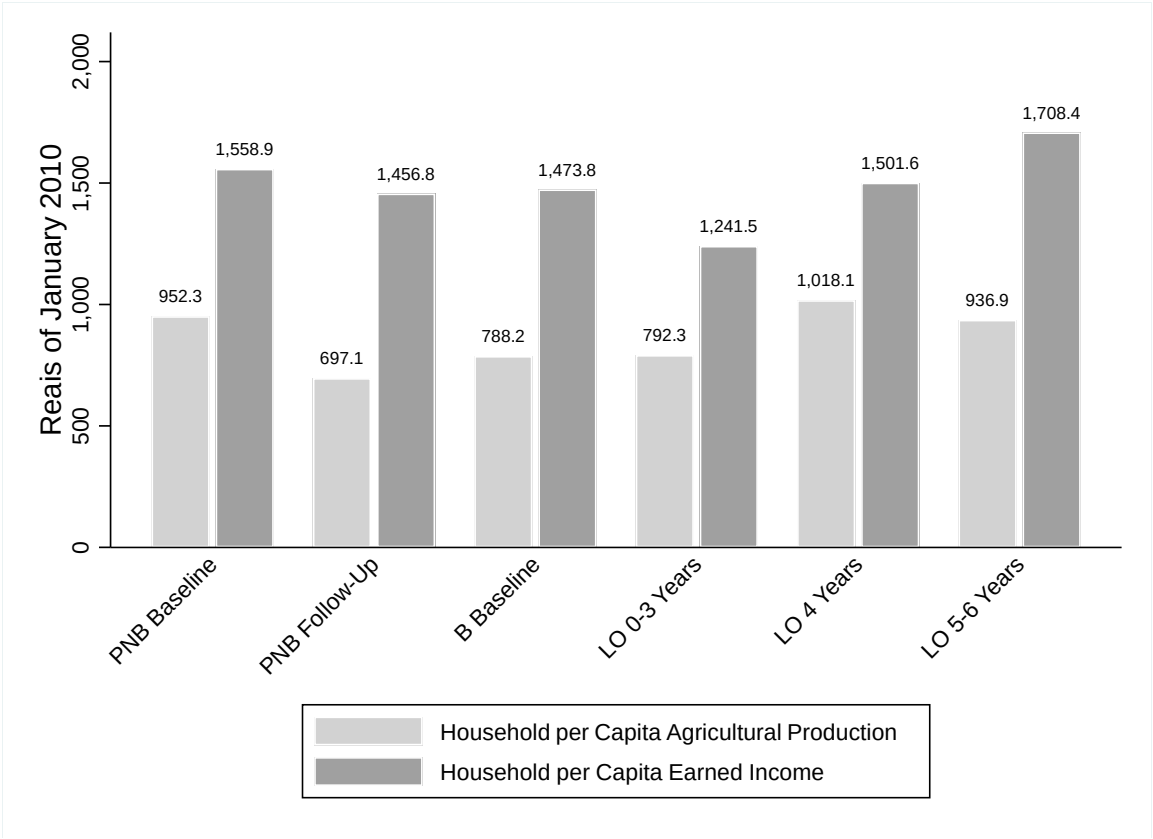

Fig. 1. Mean of Outcome Variables
Agricultural Production and Earned Income

III. IDENTIFICATION STRATEGY

When attempting to evaluate the impact of a program, an important problem to address is selection bias. If a program is not assigned randomly, one can assume that individuals who are more eager, able or otherwise more likely to benefit from a program will apply. It is also possible that program officers will choose preferential locations, or the order in which groups benefit, to demonstrate program success. A possible income increase following participation in the program might then be attributable, at least in part, to the attributes of the applicants or the decisions of program officers, as opposed to the effectiveness of the program. We employ a number of complementary strategies in an effort to avoid this bias and arrive at a plausible causal impact of the program.

First, by using a pipeline control group (Ravallion, 2008), application to the program and verified eligibility of the group are held constant across treatment and control groups. In principle, any unobserved characteristics that motivate people to apply to the program should be similar

across groups, thereby reducing selection bias. In addition, since the program depends on individuals forming groups in order to acquire the loan for land purchase, we expect less variability of unobservable characteristics across groups than across individuals, which should also contribute to reducing selection bias. While the pipeline approach should be helpful, by itself it is unlikely to be sufficient.

Utilizing the pipeline control group, the next two strategies rely on the estimation of difference-in-differences (DD) models: one with municipal fixed effects and the other with the fixed effects at the level of the individual enrolled member. The DD model with individual FE is superior because it controls for time invariant unobservables at the individual level. We estimate the model with municipal FE for comparison and because a number of specifications include key variables that do not vary over time and thus can only be estimated in this framework. Using these models, we also incorporate (below) an analysis allowing for the heterogeneity of additional years of land ownership.

The DD estimation technique removes any time-invariant differences between the treatment and control groups, and also eliminates time-trends that are common to both groups. Our first DD approach entails estimating the following equation with municipal FE:

$$Y_{ist} = \alpha_m + \beta_1 Post_t + \beta_2 Ben_s + \beta_3 (Post*Ben)_{st} + \beta_4 X_{ist} + \varepsilon_{ist} \quad (1)$$

Where Y is either agricultural production or earned income per capita, α_m are municipal fixed effects, $Post_t$ is an indicator variable that equals one in the follow-up period, Ben_s is an indicator variable that equals one if the enrolled member has acquired land, *Post*Ben* equals one if both *Post* and *Ben* equal one, and X is a vector of control variables. In addition, i, s and t index individuals, status of treatment (beneficiary or pipeline non-beneficiary) and time (baseline or follow-up period). The coefficient of interest is β_3 which provides an estimate of the effect of acquiring land via the PNCF. In order to avoid possible bias, potentially endogenous time varying controls are included only at baseline levels. The DD identification strategy relies on the assumption that $E(\varepsilon_{ist} | \alpha_m, Ben_s, Post_t, X_{ist})=0$. In other words, there should be no omitted factors that are causing both the growth in the outcome variable and the treatment status. Because this is not a randomized experiment, individual level unobservables could potentially influence both treatment and outcomes. For this reason, we estimate the next model whenever feasible.

The DD model with individual FE addresses selection bias by removing the effect of individual level unobservable characteristics that are time invariant. By replacing municipal FE (α_m) with individual FE (α_i) we have the following estimation equation:

$$Y_{ist} = \alpha_i + \beta_1 Post_t + \beta_2 (Post*Ben)_{st} + \beta_3 X_{ist} + \varepsilon_{ist} \quad (2)$$

This model should contribute to reducing bias that may still be present in the estimates from the first model. For this reason, the DD model with individual FE is the preferred specification utilized in this paper.

Even with a pipeline control group and a DD model with individual fixed effects, there still might be concern that some unobserved (possibly time varying) characteristics of individuals, locations, or program officers could influence whether or not a group succeeds in acquiring land. Our final strategy for addressing this concern is to only focus on beneficiaries who actually succeeded in acquiring land. Thus, rather than use the pipeline control group—which could conceivably include individuals stuck in the pipeline for some unknown reason—we use the beneficiaries that acquired land late in the period (LO 0-3 years) as a control group for beneficiaries that acquired land early on. The analysis serves as additional evidence that it is land acquisition rather than unobserved characteristics of enrolled members that are driving the results.

Heterogeneity

In the techniques mentioned so far, treatment is modeled with a binary variable. The implicit assumption is that the average impact of treatment is the same for all beneficiaries of the program. However, as mentioned in the previous section, different associations obtained land at different times. One can suppose that the intensity of treatment increases with the amount of time a member is exposed to treatment, thus leading to a greater impact (King and Behrman, 2009). One approach to allow for heterogeneity by year of land acquisition is to estimate a duration of treatment DD model with municipal fixed effects:

$$Y_{ist} = \alpha_m + \beta_1 Post_t + \beta_2 Ben_s + \beta_3 (Post*LO\_0\text{-}3)_{st} + \beta_4 (Post*LO\_4)_{st} + \beta_5 (Post*LO\_5\text{-}6)_{st} + \beta_6 X_{ist} + \varepsilon_{ist} \quad (3)$$

Where *LO_0-3* is an indicator variable for land ownership for three years or less, *LO_4* measures land ownership for more than three years and less than or equal to four years, and *LO_5-6* reflects

land ownership for five to six years.[7] The indicator variables measure the effect of increasing years of land ownership without assuming that the impacts increase in a linear fashion. For the individual FE model, the equivalent duration of treatment equation is:

$$Y_{ist} = \alpha_i + \beta_1 Post_t + \beta_2 (Post*LO\_0\text{-}3)_{st} + \beta_3 (Post*LO\_4)_{st} + \beta_4 (Post*LO\_5\text{-}6)_{st} + \beta_5 X_{ist} + \varepsilon_{ist} \quad (4)$$

In the same way that there might be heterogeneity of impacts due to duration of exposure to land ownership, heterogeneity could exist for other reasons as well. We explore the differential impact of treatment due to "eagerness" (Agüero et al., 2009), the size of the land that beneficiaries acquired, and gender. The eagerness proxy measures whether groups acquired land early in relation to other groups in their local context.[8] While eagerness can be viewed as a form of heterogeneity, the proxy can be interpreted broadly to reflect favorable unobservables in a variety of dimensions: the early-movers might have been more motivated, they could have acquired higher quality land, or they may have been chosen by officials interested in demonstrating the success of the program. In contrast to eagerness, duration of treatment is defined uniformly for all beneficiaries and captures the intensity of exposure to land ownership. Because the program may have begun in some places before others due to an interested governor, mayor or agricultural workers union, duration of treatment is likely unrelated to favorable local characteristics or individual factors in the way that eagerness is. We provide suggestive evidence below in favor of this conjecture by showing no relationship between municipalities that initiated the program early and a long list of observable characteristics. While this does not rule out the possibility that some form of endogenous program placement could have existed, it does help to assuage concerns about many of the most obvious candidates.

In all three cases, we interact a dummy variable—for eagerness, farm size or sex—with the coefficients of interest. Because the dummies represent characteristics that are time invariant, the models must be estimated with municipal rather than individual fixed effects. In the case of the intensity of treatment model and farm size, for example, equation (3) becomes:

$$Y_{ist} = \alpha_m + \beta_1 Post_t + \beta_2 Ben_s + \beta_3 (Post*LO\_0\text{-}3)_{st} + \beta_4 (Post*LO\_4)_{st} +$$

[7] The first three years were grouped together because this group contained only about one third and two thirds, respectively, of the observations contained in the *LO_4* and *LO_5-6* groups. We also did not observe statistically significant differences across the first three years when a model was estimated that allowed for this flexibility. The minimum in the *LO_0-3* group is just over 6 months.

[8] The local context is defined as a micro-region which contains an average of about 10 municipalities.

$$\beta_5(Post*LO\_5\text{-}6)_{st} + \beta_6(Post*D_i*LO\_0\text{-}3)_{st} + \beta_4(Post* D_i*LO\_4)_{st} +$$
$$\beta_5(Post* D_i*LO\_5\text{-}6)_{st} + \beta_6 DL_i + \beta_7 X_{ist} + \varepsilon_{ist} \quad (5)$$

Where D_i is a dummy variable that equals 1 for all beneficiaries with acquired land larger than the median farm size of beneficiaries, and 0 for all other individuals. The interaction of the farm size dummy with the duration of treatment land ownership dummies allows us to test for any differential impact of the duration of land ownership by farm size. DL_i controls linearly for farm size measured in hectares.

The standard errors for the regression coefficients throughout the paper are calculated with corrections for clustering to allow for the possibility of heteroskedasticity across projects or correlation of errors across time within a geographical location. The errors were clustered at the level of the project for beneficiaries and most pipeline non-beneficiaries, and at the level of the municipality for pipeline non-beneficiaries when the project code—that could uniquely identify an association—was missing. There was a total of 276 clusters.[9]

IV. DESCRIPTIVE STATISTICS AND VALIDITY OF THE PIPELINE

Beneficiaries are defined in this paper as having *acquired land* through the PNCF prior to 2010. This does not mean that they did not have access to land beforehand. Over ninety percent of our sample—including beneficiaries and non-beneficiaries—did in fact have agricultural production in the baseline. Yet less than ten percent of each group owned land. Around one-third had informal concessions, one quarter were sharecroppers, and a smaller share were renters. Agricultural production was rain fed and often in the semi-arid portion of the Northeast. Close to sixty percent of both groups had labor market earnings from daily agricultural work, and about forty percent had non-agricultural earnings.

The outcome variable agricultural production was defined as the total value of agricultural output (including animal production), whether sold, stocked, exchanged or consumed. As can be observed in Table 1, beneficiaries as a group had less agricultural production per capita than pipeline non-beneficiaries in the initial period, although the difference was only statistically significant for the LO 5-6 years group. In the follow-up period (Figure 1), all beneficiaries regardless of the number of years of treatment had more agricultural production per capita than the

[9] Clustering all observations at the level of the municipality results in slightly larger standard errors, but the levels of statistical significance remain the same.

pipeline non-beneficiaries, and this difference was significant for LO 4 and LO 5-6. Thus, their productive opportunities appear to have increased as a result of land ownership. Earned income, the second outcome variable, was defined as the value of net agricultural production—total agricultural production minus variable costs—plus income earned in the labor market and from self-employment activities. Earned income excludes transfers, a category that contains retirement benefits which account for around one quarter of income. There was no statistical difference between the earned income per capita of pipeline non-beneficiaries and beneficiaries as a whole in either period. Interestingly, though, those who were treated the longest had the lowest average income in the baseline, but in the follow-up period the situation was reversed (Figure 1), with nearly all of the increased income coming from agricultural production.

Control variables were included in the estimation to capture differences in the outcome variable that were due to differences in the baseline characteristics of the enrolled members as opposed to the acquisition of land via the PNCF. Basic demographic and location variables were used (age, sex, race, marital status and urban status), in addition to education and experience (years of schooling and years of experience as a farmer). As can be seen in Table 1, among these individual characteristics, beneficiaries statistically differed from the pipeline non-beneficiaries only in sex composition and urban status. The beneficiary group was more male and less urban than the pipeline non-beneficiary group, even though around 70% of the beneficiaries were still not residing on the property. By 2010, approximately two-thirds of beneficiaries had moved to their property.

Measures of baseline social capital in the association were used in an attempt to capture the effects of social capital on the outcome variable. More socially cohesive associations should be predictive of both participation in the program and the success of the eventual projects. The first social capital variable, position held, is an indicator variable that equals one if the member held a position in the leadership of the association. While the beneficiaries interviewed were more likely to hold positions in their associations than the pipeline non-beneficiaries, the other social capital variables display the opposite pattern. Beneficiaries had fewer meetings and less trust in

other association members. Since there was less social cohesiveness in the beneficiary group, it is unlikely that these variables explain their success.[10]

Table 1
Descriptive Statistics by Beneficiary Status.

	N	Pipeline NB		Beneficiary		p-value
		Mean	SE	Mean	SE	
Agricultural Production	201, 562	957.01	111.31	789.56	60.76	0.17
LO 0-3 Years	99			878.48	154.12	0.68
LO 4 Years	317			902.38	91.87	0.71
LO 5-6 Years	146			484.33	57.26	0.00***
Earned Income	201, 562	1562.87	117.64	1474.31	66.19	0.50
LO 0-3 Years	99			1646.01	183.05	0.69
LO 4 Years	317			1526.27	91.87	0.81
LO 5-6 Years	146			1245.09	96.97	0.05**
Individual Characteristics						
Age		37.68	0.86	36.58	0.50	0.26
Sex		0.71	0.03	0.86	0.01	0.00***
White		0.20	0.03	0.17	0.02	0.49
Married		0.82	0.03	0.78	0.02	0.31
Urban		0.27	0.03	0.21	0.02	0.07*
Years of Schooling		4.02	0.22	4.32	0.16	0.30
Years of Experience		23.39	0.92	22.71	0.54	0.53
Social Capital Variables						
Position Held		0.42	0.03	0.56	0.02	0.00***
Frequency of Meeting		2.29	0.06	2.17	0.03	0.06*
Trust		2.80	0.03	2.70	0.02	0.01***
Individual Agricultural Variables						
Technical Assistance		0.05	0.02	0.07	0.01	0.25
PRONAF		0.32	0.03	0.25	0.02	0.05*
Local Agricultural Variables						
Yield of Corn		0.88	0.02	0.79	0.01	0.00***
Daily Agricultural Wage		12.37	0.09	12.42	0.04	0.63

Notes: ***Significant at 1%; ** Significant at 5%; * Significant at 10%. SE: standard errors. p-values are from t-tests that compare the equality of pipeline NB means with beneficiary means. Two sets of baseline number of observations in column N relate to pipeline NB and beneficiary respectively.

[10] It is possible that beneficiaries held more positions because more positions were created once these groups acquired land. The main models in Tables 3 and 4 were re-estimated without the inclusion of this variable, and the results were almost identical.

Agricultural variables were also included since they may influence both treatment status and the outcome variables. Technical assistance and PRONAF are variables that indicate whether enrolled members received technical assistance and whether they received loans from a credit program for family farmers. While technical assistance was rare and statistically equivalent for both groups, pipeline non-beneficiaries did receive more family farming loans, which is consistent with their higher levels of agricultural production in the baseline. The local agricultural variables included are yield of corn (a crop produced widely) and daily agricultural wage. State level corn yields (tons/hectare) proxy for time varying geo-climatic characteristics.[11] Pipeline non-beneficiaries found themselves in areas with more favorable geo-climatic conditions; this is, once again, consistent with their higher levels of agricultural production in the baseline period.

When using a pipeline control group, there should be no unobserved characteristics that influence which enrolled members receive treatment after application (Ravallion, 2008). While this is impossible to prove, a few basic tests serve as evidence of the absence of selection into receiving land—after applying—based on observables. First, a comparison of means indicates that there was no statistically significant difference in the baseline between the outcome variables of pipeline non-beneficiaries that go on to acquire land and those non-beneficiaries that remain in the pipeline in the follow-up period (see Appendix Table 1.A). While there were differences in some observable covariates, these are controlled for in the main analysis. Second, probit regressions were run seeking to predict which pipeline non-beneficiaries go on to acquire land between the baseline and the follow-up periods. The results are reported in Table 2, where the dependent variable is an indicator variable that equals one if the member acquired land sometime between the baseline and follow-up period. Earned income and agricultural production—which reflect the influence of observable and unobservable variables—fail to significantly predict the movement into the treatment group, regardless of the inclusion of control variables.[12] As such, these tests provide suggestive evidence that unobserved characteristics of pipeline non-beneficiaries are not influencing which ones go on to receive land.

[11] Tons of corn and hectares harvested were obtained from the Institute of Applied Economic Research (ipeadata.gov.br) to calculate yield of corn by state.

[12] The estimation in Table 2 is done with baseline values only and the sample is limited to the balanced panel of pipeline non-beneficiaries and non-beneficiaries who became beneficiaries after the baseline period. A separate estimation was run including attritors, and the results are equivalent.

Table 2
Probits for the Probability of Acquiring Land Between the Baseline and Follow-up Periods.

	(1)	(2)	(3)	(4)
Agricultural Production	-0.000002		0.000004	
	(0.00006)		(0.00006)	
Earned Income		0.000022		0.000082
		(0.00006)		(0.00005)
N	352	352	352	352
State FE	N	N	Y	Y
Individual Controls[1]	N	N	Y	Y
Social Capital Controls[2]	N	N	Y	Y
Individual Agricultural Controls[3]	N	N	Y	Y
Local Agricultural Controls[4]	N	N	Y	Y

Notes: ***Significant at 1%; ** Significant at 5%; * Significant at 10%. Clustered standard errors in parentheses. [1]Individual controls include: age, sex, race, marital status, schooling, experience and urban status. [2]Social capital controls include: position in association, frequency of meetings and trust in association. [3]Individual Agricultural controls include: technical assistance and PRONAF. [4]Local Agricultural controls include: daily agricultural wage and yield of corn. All values in household per capita Reais of January 2010.

V. ECONOMETRIC RESULTS

This section presents the results for the different specifications used. Outliers for each outcome were excluded from their respective regressions and were detected by a combination of methods that suggested dropping 6, 7, or 8 individuals. We excluded 8 individuals for each outcome, and confirmed that the econometric results from the different approaches were similar.[13] The first four columns of Tables 3 and 4 show the results for the difference-in-differences models with municipal fixed effects (DD - Mun. FE). Time and status dummies, along with municipal fixed effects, were included in all specifications. The first column shows the results from a specification without any additional control variables. Starting with the second column, individual

[13] The first approach used Stata's Bacon command. This utilizes a Mahalanobis distance method and results in dropping 8 and 6 individuals for agricultural production and earned income respectively. A second method involved regressing each outcome variable on beneficiary status and a time dummy, and then dropping the observations with absolute values of the residuals larger than 3. This approach leads to dropping 8 individuals for agricultural production and 7 for earned income. In a final approach, we observed the raw data for the outcome variables and dropped observations at cut-offs with sharp jumps in values. The cut-offs were 28,000 and 15,000. This resulted in dropping 6 and 8 individuals, respectively, for both outcome variables. In addition, two individual members were dropped due to missing values.

level controls are included—age, sex, race, marital status, years of schooling, years of experience as a farmer and urban status—all kept at baseline levels. Baseline social capital controls are included in the third column and both individual and local agricultural controls in the fourth. The individual agricultural controls are technical assistance and PRONAF loans; because these are endogenous variables they are kept at baseline levels to avoid introducing bias on the coefficients of interest. The daily agricultural wage and the yield of corn are the local agricultural controls. These variables are exogenous because they refer to geographical levels that are much larger than the individuals in the treatment and control groups. They capture variation in the outcome variables that is due to time-varying characteristics of the surrounding environment. This last model that utilizes all controls is the preferred DD with municipal FE specification. The results from the DD model with individual fixed effects (DD - Indiv. FE) are presented in columns 5 and 6 of Tables 3 and 4. The first specification in column five includes no control variables. Time-varying controls are included in the second specification. The model in column 6—which includes time-varying controls and removes the effect of time-invariant unobserved characteristics at the level of the individual—is the preferred specification of this paper.

Table 3 displays the results for the dependent variable agricultural production per capita. In the first four columns of Panel A, the binary case specified in (1), the estimation shows significant and positive effects of receiving land, robust to the inclusion of different controls. In the first column, which only includes municipal fixed effects, the results suggest that receiving land via the PNCF increases beneficiaries' agricultural production by R$433 (US$243) per person during the last year as compared to the pipeline non-beneficiaries. The estimated coefficients change little in the remainder of the specifications, and are not statistically different from each other. In the preferred municipal fixed effects specification in column four, the coefficient decreases slightly to R$429 per person in the household. When using the DD model with municipal FE, all estimated coefficients in the first row of Table 3 are statistically significant at the five percent level.

The four first columns in Panel B of Table 3 display the results for agricultural production per capita using the duration of treatment estimation with municipal FE (eq. 3). They show the effects of the program as the number of years of land ownership increases. The pattern is clear: increasing years of land ownership raise the magnitude of the estimates. The coefficients on being

a landowner for three years or less are all positive, but none are statistically significant. The coefficients on being a landowner for four years are all positive and significant at ten percent.

Table 3
Program Effects on Agricultural Production.

	DD - Mun. FE				DD - Indiv. FE	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Binary						
Time*Status	433.30**	432.98**	433.91**	428.89**	427.17**	425.42**
	(189.45)	(190.16)	(190.32)	(195.47)	(180.91)	(184.93)
Panel B: Duration of Treatment						
LO 0-3 Years	279.63	283.71	299.39	306.79	179.89	180.53
	(246.30)	(243.23)	(244.13)	(245.11)	(253.92)	(254.82)
LO 4 Years	398.15*	396.85*	401.97*	398.98*	373.83*	373.69*
	(208.86)	(211.00)	(210.80)	(212.44)	(206.73)	(207.72)
LO 5-6 Years	611.91***	610.71***	592.61***	578.20***	710.67***	707.22***
	(203.31)	(203.67)	(203.64)	(212.99)	(196.36)	(204.56)
N	1526	1526	1526	1526	1526	1526
Mean of control group	957.01	957.01	957.01	957.01	957.01	957.01
Individual Controls[1]	N	Y	Y	Y	N	N
Social Capital Controls[2]	N	N	Y	Y	N	N
Individual Agricultural Controls[3]	N	N	N	Y	N	N
Local Agricultural Controls[4]	N	N	N	Y	N	Y

Notes: ***Significant at 1%; ** Significant at 5%; * Significant at 10%. Clustered standard errors in parentheses. [1]Individual controls include: age, sex, race, marital status, schooling, experience and urban status. [2]Social capital controls include: position in association, frequency of meetings and trust in association. [3]Individual Agricultural controls include: technical assistance and PRONAF. [4]Local Agricultural controls include: daily agricultural wage and yield of corn. DD- Muni FE: difference-in-differences with municipal fixed effects. DD- Indiv. FE: difference-in-differences with individual fixed effects. All values in household per capita Reais of January 2010.

Finally, the coefficients on being a landowner for more than four years are all of a much larger magnitude than the coefficients for the previous two categories and all become significant at the one percent level. The preferred specification in column four indicates that owning land via the PNCF for five or six years increases per capita agricultural production in the last year by R$578 (US$324). Given the limited time span of the data, we cannot know for certain if the returns will continue to increase at an increasing or decreasing rate, or at what point they might plateau with

additional years of land ownership. Nevertheless, the results in Table 3 suggest that acquiring land via the PNCF has had an increasing effect on agricultural production over time.

The results for the binary DD model with individual FE in columns 5 and 6 of Table 3 show almost identical effects as the DD model with municipal FE. The binary case shows significant and positive effects on agricultural production of being a beneficiary of the program. The coefficient is robust to the alternative specification with time varying controls. In the duration of treatment estimation in Panel B of columns 5 and 6, the expected pattern is observed. Increasing years of land ownership are associated with increased magnitudes of the effects on agricultural production. The coefficients are significant at ten percent for landowners for four years and at the one percent level for landowners for five to six years. The preferred specification in column six suggests that being a beneficiary of the PNCF for five to six years increases agricultural production per person in the household by an average of R$707 (US$397) in the last year, or approximately 74 percent of the baseline mean of the control group. This is the preferred estimate of this paper.

For the outcome variable earned income per capita (Table 4), all binary models show a positive effect of being a beneficiary of the program, although none of the coefficients are statistically significant. The estimation results that allow for the heterogeneous effects of additional years of land ownership are shown in the lower panel of Table 4. In both the models with municipal FE and individual FE, the impact of the PNCF only becomes positive and statistically significant for beneficiaries with five or six years of land ownership. The preferred FE specification of this paper, in column six, suggests that the impact of the PNCF on earned income is R$574 (US$322) per person in 2010, or approximately 37 percent of the baseline mean income of the control group.

Heterogeneity

Table 5 explores potential heterogeneity of the PNCF along the dimensions of land size and sex by including dummy variables that are interacted with the coefficients of interest. Because these variables are time invariant, we can only use the municipal fixed effects model. As in Table 3, we observe a significant impact of the PNCF on agricultural production in the binary and duration models, with the largest impact on those individuals who have been beneficiaries the longest (LO 5-6). But there is no evidence of heterogeneous impacts on agricultural production that vary by farms size or sex. The point estimates for the coefficients that capture the differential effect for farms with land larger than the median of 18.3 hectares are negative, but never statistically significant, and the point estimates on sex alternate in sign without becoming

statistically significant. The results for earned income are consistent with Table 4, showing significant effects only for the LO 5-6 group, but in no case is there evidence of differential impacts that vary by farm size or sex.

Table 4
Program Effects on Earned Income.

	DD - Mun. FE				DD - Indiv. FE	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Binary						
Time*Status	135.13	135.64	136.61	129.93	162.22	161.69
	(180.67)	(181.26)	(181.57)	(183.80)	(173.57)	(176.23)
Panel B: Duration of Treatment						
LO 0-3 Years	-126.62	-137.14	-115.66	-104.95	-247.87	-242.85
	(224.23)	(220.20)	(222.01)	(221.66)	(254.21)	(253.81)
LO 4 Years	54.86	61.28	63.65	61.98	96.04	100.82
	(205.57)	(207.45)	(206.75)	(205.04)	(206.15)	(205.45)
LO 5-6 Years	483.25**	478.42**	462.37**	439.56**	583.98***	573.94***
	(207.06)	(206.49)	(207.10)	(215.05)	(202.44)	(211.37)
N	1526	1526	1526	1526	1526	1526
Mean of control group	1562.87	1562.87	1562.87	1562.87	1562.87	1562.87
Individual Controls[1]	N	Y	Y	Y	N	N
Social Capital Controls[2]	N	N	Y	Y	N	N
Individual Agricultural Controls[3]	N	N	N	Y	N	N
Local Agricultural Controls[4]	N	N	N	Y	N	Y

Notes: ***Significant at 1%; ** Significant at 5%; * Significant at 10%. Clustered standard errors in parentheses. [1]Individual controls include: age, sex, race, marital status, schooling, experience and urban status. [2]Social capital controls include: position in association, frequency of meetings and trust in association. [3]Individual Agricultural controls include: technical assistance and PRONAF. [4]Local Agricultural controls include: daily agricultural wage and yield of corn. DD- Muni FE: difference-in-differences with municipal fixed effects. DD- Indiv. FE: difference-in-differences with individual fixed effects. All values in household per capita Reais of January 2010.

Table 5
Program Effects with Heterogeneity by Land Size and Sex.

	Agricultural Production		Earned Income	
	Land	Sex	Land	Sex
	DD - Mun. FE		DD - Mun. FE	
Panel A: Binary				
Time*Status	536.25**	420.64**	124.99	129.40
	(233.34)	(199.33)	(232.68)	(187.66)
Time*Status*LandDummy	-198.14		22.63	
	(161.03)		(197.97)	
Time*Status*SexDummy		54.69		3.51
		(221.48)		(189.29)
Panel B: Duration of Treatment				
LO 0-3 Years	523.45	331.53	-141.64	-110.03
	(342.41)	(258.19)	(332.68)	(230.89)
LO 4 Years	471.39	351.09	-27.51	64.89
	(293.02)	(215.75)	(301.44)	(210.70)
LO 5-6 Years	649.63***	639.06***	463.34*	450.23**
	(231.20)	(222.59)	(242.35)	(223.35)
LO 0-3 Years*LandDummy	-289.31		49.17	
	(344.53)		(332.20)	
LO 0-3 Years*SexDummy		-282.00		56.32
		(243.87)		(371.42)
LO 4 Years*LandDummy	-155.80		163.66	
	(230.15)		(287.67)	
LO 4 Years*SexDummy		307.53		-21.23
		(360.06)		(256.21)
LO 5-6 Years*LandDummy	-100.93		16.24	
	(237.87)		(283.67)	
LO 5-6 Years*SexDummy		-316.57		-58.82
		(204.21)		(326.74)
N	1524	1526	1524	1526
Mean of control group	957.01	957.01	1562.87	1562.87
Land Size	Y	N	Y	N
Individual Controls[1]	Y	Y	Y	Y
Social Capital Controls[2]	Y	Y	Y	Y
Individual Agricultural Controls[3]	Y	Y	Y	Y
Local Agricultural Controls[4]	Y	Y	Y	Y

Notes: ***Significant at 1%; ** Significant at 5%; * Significant at 10%. Clustered standard errors in parentheses. [1]Individual controls include: age, sex, race, marital status, schooling, experience and urban status. [2]Social capital controls include: position in association, frequency of meetings and trust in association. [3]Individual Agricultural controls include: technical assistance and PRONAF. [4]Local Agricultural controls include: daily agricultural wage and yield of corn. DD- Muni FE: difference-in-differences with municipal fixed effects. DD- Indiv. FE: difference-in-differences with individual fixed effects. All values in household per capita Reais of January 2010.

VI. ROBUSTNESS AND ATTRITION

A variety of robustness checks were performed that suggest that confounding factors did not distort the findings presented in the previous section. First, in order to investigate whether some unobserved trend could be causing spurious findings, a placebo test was run. Second, it is possible that unobserved characteristics of the earliest beneficiaries, plots, or locations were causing their agricultural production and earned income to grow more rapidly. In order to shed light on this issue, an eagerness proxy was included in the estimation. We also explored whether the timing of project proposals was correlated with a battery of observable baseline characteristics of the locations. Third, because the earliest beneficiaries could have been affected more by recall bias, we restricted the sample to a shorter time frame. Fourth, in order to address lingering concerns about the validity of the control group, we estimated the binary and intensity of treatment models solely with beneficiaries, using the LO 0-3 group as the control. Fifth, because household size declined more for beneficiaries than non-beneficiaries, we sought to confirm that this was not the only factor driving the results. To this end, we decomposed the overall impacts into two components--one attributable to changes in household size and one due to changes in production and income. Finally, sample attrition and potential attrition bias were analyzed, leading to three approaches to address the issue: a) a model was estimated with attrition weights to correct for the possibility of non-random attrition; b) Lee bounds were calculated; and c) rather than use the balanced panel, the municipal fixed effects models were estimated with the cross-sectional data from each year, thereby including both the individuals who would attrit after 2006 and their replacements in 2010. We first address the robustness checks, and then deal with attrition in a separate sub-section.[14]

Robustness

Despite the apparent validity of using the pipeline control (Table 2), it is possible that the beneficiaries were subject to different trends than the pipeline non-beneficiaries. The estimated parameters, then, could be reflecting these different unobserved trends, instead of accurately estimating the impact of the program. In order to provide suggestive evidence in favor of our identification strategy, a placebo test was run. This placebo test entails estimating the regressions

[14] A test of the parallel trends hypothesis would also have been appropriate, but we do not have data from a prior period. We considered testing parallel trends at a municipal level with an auxiliary dataset, but this too was not feasible because the beneficiary and control individuals were largely drawn from the same locations.

above on a variable that the PNCF should not have any effect on. If there were unobserved trends affecting the beneficiaries and not the pipeline non-beneficiaries, then the results could display a similar pattern to those in Table 3 and 4. The test was conducted with the outcome variable total transfers, which includes old age pensions, *Bolsa Família* conditional cash transfers, other government transfers and private transfers. There is no reason to expect that access to land via the PNCF should affect this variable. As can be seen in the first two columns of Table 6, no significant effects are observed on total transfers in either the binary or duration of treatment cases. These results provide an additional piece of evidence that suggests that the beneficiaries and pipeline non-beneficiaries were not exposed to different group-specific time trends.

In addition to the placebo test, another model was estimated to address the concern that there may be differences within the beneficiary group itself. It could be that the earliest beneficiaries were simply more motivated (as indicated by their early participation in the program), which is what caused them to have increased levels of the outcome variables in the intensity of treatment specifications. Alternatively, they might have acquired the highest quality land, or program officers could have chosen the best groups first in order to demonstrate the success of the program. To the extent that this reflects a time invariant characteristic at the individual level, the individual FE model will address this concern and generate unbiased estimates of the program impact. The same cannot be said of the municipal FE model. In order to test this hypothesis directly in the context of the municipal FE model, a proxy for "eagerness" was created in the spirit of Agüero et al. (2009). Its inclusion could help to correct some of the bias in this model that derives from not having removed the influence of individual level time invariant unobservables.

The eagerness variable was defined in two steps. First, the number of days of eagerness was calculated as the median land purchase contract date of all projects prior to 2010 in the beneficiary's micro-region minus the beneficiary's contract date.[15] Thus, eagerness was defined in a local context. The larger the value of this variable, the more eager was the beneficiary in applying to the program. The eagerness variable was included in the regression in levels to proxy for time invariant characteristics that might be positively correlated with more favorable outcomes. Second,

[15] There were approximately 550 micro-regions in Brazil in 2006, with an average of about 10 municipalities in each one. Micro-regions are defined by IBGE based on common social, economic and environmental characteristics. Because not all municipalities in our sample had multiple projects, we choose to calculate eagerness at the micro-region level.

Table 6
Robustness Tests with Total Transfers and Eagerness Proxy.

	Placebo Test w/ Transfers		Eagerness Proxy	
	DD - Mun. FE	DD - Indiv. FE	Agricultural Production	Earned Income
			DD - Mun. FE	DD - Mun. FE
Panel A: Binary				
Time*Status	36.34	21.53		
	(57.19)	(54.79)		
Panel B: Duration of Treatment				
LO 0-3 Years	13.30	40.96	240.75	-155.72
	(72.99)	(74.75)	(261.89)	(242.51)
LO 4 Years	67.37	19.00	433.88	154.97
	(62.52)	(60.53)	(287.42)	(255.87)
LO 5-6 Years	-14.45	13.75	819.75***	653.27*
	(83.20)	(82.77)	(307.66)	(379.10)
LO 0-3 Years*EagerDummy			437.25	381.57
			(497.48)	(429.79)
LO 4 Years*EagerDummy			-77.80	-194.70
			(307.39)	(320.92)
LO 5-6 Years*EagerDummy			-343.46	-310.32
			(294.66)	(387.72)
Eagerness			0.09	0.12
			(0.24)	(0.22)
N	1492	1492	1526	1526
Mean of control group	356.45	356.46	957.01	1562.87
Individual Controls[1]	Y	N	Y	Y
Social Capital Controls[2]	Y	N	Y	Y
Individual Ag. Controls[3]	Y	N	Y	Y
Local Agricultural Controls[4]	Y	Y	Y	Y

Notes: ***Significant at 1%; ** Significant at 5%; * Significant at 10%. Clustered standard errors in parentheses. [1]Individual controls include: age, sex, race, marital status, schooling, experience and urban status. [2]Social capital controls include: position in association, frequency of meetings and trust in association. [3]Individual Agricultural controls include: technical assistance and PRONAF. [4]Local Agricultural controls include: daily agricultural wage and yield of corn. DD- Muni FE: difference-in-differences with municipal fixed effects. DD- Indiv. FE: difference-in-differences with individual fixed effects. All values in household per capita Reais of January 2010.

an indicator variable for eagerness was created. This defined beneficiaries as eager if the eagerness variable was above the median value for all beneficiaries in our sample, and not eager otherwise. Thus, half of the beneficiaries were classified as eager. The eagerness indicator variable was interacted with the different levels of the number of years of land ownership to assess the differential impact of acquiring land early in the program.

The results are presented in the final two columns of Table 6. For the outcome variable agricultural production, the same pattern as the main regressions is observed; beneficiaries have increased agricultural production with increasing years of land ownership. Interestingly, though, none of the coefficients on the interaction terms are statistically significant, suggesting that there was no differential performance for the eager. If anything, the effect of eagerness was negative for the beneficiaries with four or more years of land ownership (although not statistically significant). Thus, the results indicate that the pattern of increasing agricultural production with increasing years of land ownership is not due to some unobserved eagerness—either of the beneficiaries or of the program officers. The same conclusion can be reached for earned income. The pattern of results is similar to what was observed in column four of Table 4. While the point estimates suggest that income rises with the duration of land ownership, the coefficients are not statistically significant until we reach the LO 5-6 group. Furthermore, the differential impact of eagerness is always insignificant, and more often negative than positive. We conclude that the possible unobserved advantages of beneficiaries who acquired land early—either in terms of motivation, superior land, or the interests of program officials—were unlikely to have been the reason why agricultural production or earned income increased with the number of years of land ownership.

The inclusion of the eagerness proxy was intended to test for the influence of unobservables—correlated with early movers in a local context—in the municipal fixed effects model. An alternative approach to examine the hypothesis of endogenous program placement is to search for evidence of favorable observable characteristics of the locations where the program began early. Since we now examine early movers in a global context, this is an investigation of whether the impacts observed for the LO 5-6 group might have been caused by the program starting in locations that had more advantageous initial conditions. Table 7 shows the estimated coefficients from linear regressions of the average municipal date that proposals entered the system to begin the approval process ("timing of proposal") on a host of baseline municipal variables. If the locations where the program began early tended to have higher land productivity, for example,

then we would observe a positive coefficient between these variables. Table 7 shows there is no statistically significant relationship between "timing of proposal" and a) how long it took to purchase land (time to contract), b) land area per capita, c) income per capita, d) the headcount poverty rate, e) land productivity as measured by corn yields, f) daily agricultural wages, g) the value of agricultural production per capita in our sample, or h) earned income per capita in our sample. In short, we find no observable relationship—either positive or negative—between baseline locational characteristics and the timing of when project proposals were initially submitted. While this does not rule out the possibility that some form of endogenous program placement could have existed, it does help to assuage concerns about some of the most obvious candidates. It suggests that the impacts observed on the LO 5-6 group are unlikely to have resulted from advantageous observable factors at the municipal level.

Table 7
Exploring Potential Endogeneity of Program Placement at Municipal Level.

	Using Municipal Data					Using our sample		
Dependent Variable	Time to contract	Land area (p.c.)	Income (p.c.)	Poverty	Yield of corn	Daily Ag. Wage	Ag. Production (p.c.)	Earned income (p.c.)
Timing of Proposal	-0.004	-0.014	-0.060	0.034	0.003	0.001	-42.526	-29.851
	(0.022)	(0.028)	(0.075)	(0.025)	(0.002)	(0.020)	(40.114)	(42.795)
No. of municipalities	580	579	579	579	580	82	82	82

Notes: ***Significant at 1%; ** Significant at 5%; * Significant at 10%. Robust Standard Errors in parentheses. Time to contract: Average time taken for a proposal to be converted into a contract in a municipality; Land area (p.c.): Average of per beneficiary land allotted to a beneficiary group in a municipality; PCI: Average household income in a municipality; Poverty: Head count poverty in a municipality; Yield of corn: Municipality level yield of corn in the baseline; Daily Ag. Wage: Municipality level agricultural wage in the baseline; Ag. Production (p.c.): Average of household's per capita agricultural production in a municipality in the baseline; Earned income (p.c.): Average of household's per capita earned income in a municipality in the baseline. The data were drawn from the following sources: a) time to contract was obtained from PNCF institutional data; b) land area, income, poverty and yield of corn are from Ipeadata.gov.br; and c) timing of proposal, daily ag. wage, ag. production and earned income were available from our sample.

An additional concern related to the LO 5-6 beneficiaries is that their baseline data may be subject to greater recall bias than the other beneficiaries or pipeline non-beneficiaries. This is because—like all beneficiaries—they were asked to report on the 12 months immediately preceding the acquisition of land. But in their case the acquisition of land could have been up to 13 months prior to the interview. While the majority of beneficiaries in the LO 5-6 were in the first 6 months of their fifth year of land ownership by the time of the follow-up survey, 19 of the 146 individuals

in this group were in the second half of their fifth year or the first half of their 6th year. We excluded these beneficiaries and thereby restricted this group to have greater than 48 months and less than or equal to 54 months of land ownership. Thus, in the baseline, they were asked to recall data on a period much more similar to the LO 4 group. The results are presented in the first four columns of Table 8. All estimated coefficients for agricultural production are almost identical to those in Table 3. The point estimate on the LO 5-6 group in the individual FE model rises by less than 1%. For earned income, the pattern observed in Table 4 of a decline in income during the first three years (LO 0-3) followed by a recovery in the fourth year (LO 4) remains, as does their lack of statistical significance. Contrary to the concerns addressed in this paragraph, the point estimate for LO 5-6 in the individual FE model actually rises by over 10% relative to Table 4, and remains significant at the one percent level. We conclude that recall bias is unlikely the reason why we observe increasing impacts with the number of years of land ownership.

We conduct a fourth robustness test in the last four columns of Table 8 that dispenses with the pipeline non-beneficiaries. The logic of using the pipeline non-beneficiaries as a control group is that they are likely to be similar to the beneficiaries in many ways. And if there are time invariant unobservable differences, as is likely, the individual FE model should remove their effect. Even so, it is possible that there are time varying unobservable differences that make the pipeline different. To address this concern, we now use the LO 0-3 beneficiaries as the control group for beneficiaries who have owned land for four years or more. The individual FE model is once again preferred, as it will remove the influence of time invariant effects that might explain why some beneficiaries obtained land before others. The intensity of treatment results in column 6 are almost identical—in sign and magnitude—to those found in the final column of Table 3. Per capita agricultural production rises by around R$185 for LO 4 relative to LO 0-3, but this increase is not statistically significant. For LO 5-6, however, agricultural production grows by around R$505 relative to LO 0-3, and the difference is significant at five percent. The results for earned income also confirm the main results from Table 4. After an initial dip in income that was observed descriptively for the LO 0-3 group, income rises for the LO 4 beneficiaries relative to LO 0-3 by R$343 per person, without being statistically significant. Income rises further with additional time as a landowner, and the estimated impact of the program on LO 5-6 is R$814. This difference between LO 0-3 and LO 5-6 is almost identical to what was estimated in column 6 of Table 4.

Table 8
Robustness Tests with Less Recall and Beneficiaries Only.

	Less Recall				Beneficiaries Only			
	Agricultural Production		Earned Income		Agricultural Production		Earned Income	
	DD - Mun. FE	DD - Indiv. FE	DD - Mun. FE	DD - Indiv. FE	DD - Mun. FE	DD - Indiv. FE	DD - Mun. FE	DD - Indiv. FE
Panel A: Binary								
Time*Status	423.73**	418.90**	136.11	166.61	287.25	285.70	451.35*	486.60**
	(196.23)	(185.54)	(184.74)	(176.99)	(234.09)	(223.13)	(251.86)	(244.60)
Panel B: Duration of Treatment								
LO 0-3 Years	303.80	180.93	-104.24	-243.35				
	(245.43)	(255.10)	(222.98)	(254.71)				
LO 4 Years	392.06*	375.43*	58.98	102.05	245.46	184.81	339.81	342.54
	(212.13)	(207.71)	(204.43)	(205.34)	(245.55)	(239.52)	(265.01)	(263.38)
LO 5-6 Years	595.23***	712.54***	513.11**	647.12***	381.29	515.10**	702.37**	814.15***
	(216.10)	(210.18)	(212.45)	(208.12)	(248.97)	(236.05)	(275.03)	(266.09)
N	1488	1488	1488	1488	1124	1124	1124	1124
Mean of control group	957.01	957.01	1562.87	1562.87	878.48	878.48	1646.01	1646.01
Individual Controls[1]	Y	N	Y	N	Y	N	Y	N
Social Capital Controls[2]	Y	N	Y	N	Y	N	Y	N
Individual Agricultural Controls[3]	Y	N	Y	N	Y	N	Y	N
Local Agricultural Controls[4]	Y	Y	Y	Y	Y	Y	Y	Y

Notes: ***Significant at 1%; ** Significant at 5%; * Significant at 10%. Clustered standard errors in parentheses. In this regression, control group includes "less than 3 years" land owners. [1]Individual controls include: age, sex, race, marital status, schooling, experience and urban status. [2]Social capital controls include: position in association, frequency of meetings and trust in association. [3]Individual Agricultural controls include: technical assistance and PRONAF. [4]Local Agricultural controls include: daily agricultural wage and yield of corn. DD- Muni FE: difference-in-differences with municipal fixed effects. DD- Indiv. FE: difference-in-differences with individual fixed effects. All values in household per capita Reais of January 2010. Control group in Beneficiaries only panel are those belonging to LO 0-3 years group.

We also estimated the duration of treatment models with the inclusion of the eagerness proxy, as in Table 6 (see Appendix Table 2.A). Once again the coefficients on the interactions for LO 4 and LO 5-6 are all negative and insignificant for both agricultural production and earned income. We conclude that the use of pipeline non-beneficiaries does not appear to be distorting the estimates. We obtain comparable results about program impacts growing with time when we restrict the analysis to a within-beneficiary comparison.

A final concern relates to the changing number of household members. In Section 2 we pointed out that household size declined more for beneficiaries than non-beneficiaries. As beneficiary households moved to their newly acquired properties, they became more nuclear and tended to shed some of their siblings and parents. In order to make sure that this was not the only factor driving the results, we decomposed the overall program impacts into a) changes in agricultural production or earned income (with household size held constant), and b) changes in household size (with production or income held constant), ignoring the interaction effects. The results for the individual FE model are presented in Appendix Table 3.A. The positive and significant binary impact on agricultural production shown in Column 6 of Table 3 was due both to rising agricultural production (R$221, or 57%) and the differential decline in household size for beneficiaries (R$170, or 43%). Similarly, both variables contributed to the pattern of increasing impacts on agricultural production with duration of treatment. The large coefficient on the LO 5-6 year group was due mostly to rising production (86%) and only partially to the differential decline in household size (14%). In the case of earned income per capita, most of the coefficients in Table 4 were statistically insignificant. Even though the effect of changing household size was positive, this was offset by a decline in earned income in the first few years of land ownership (Appendix Table 3.A). These two forces are likely related because both income and family size could have declined as some income-earning siblings and parents left the household unit. Only in the case of the LO 5-6 year group did we observe a statistically significant impact on earned income per capita in Table 4. Table 3.A shows that in this case it was due both to the growth in earned income (47%) and the change in household size (53%).

Attrition

In the follow-up phase of the data collection process, thirty-six percent of the original beneficiaries, and forty-seven percent of the original pipeline non-beneficiaries were not interviewed again. An important reason for such high attrition rates was that the 2010 field work

was carried out with a tighter budget than in 2006. As a result, the enumerators were not able to make numerous attempts to locate individuals and were not able to track down individuals who did not live nearby. As reported by one of the authors of this paper, who worked as an enumerator in 2010, the most common reason why individuals were not re-interviewed was that they simply were not found. This occurred either because the individual was not present when the association was visited, or because an entire association or project could not be located. Difficulty finding the original interviewees took place at a higher rate for the non-beneficiaries because around two thirds of the beneficiaries were living on the property by the time of the second wave of data collection. If a single individual was not found, the enumerators were instructed to select a replacement beneficiary or pipeline non-beneficiary from the same association.[16] In the case of an entire pipeline non-beneficiary association that was missing, enumerators were instructed to interview a pre-determined association nearby that had not been interviewed in 2006. Other reasons for attrition included withdrawal from the program, refusing the interview, death and coding errors. In addition to the individuals who were not re-interviewed in 2010, there were 162 pipeline non-beneficiaries that acquired land after the baseline period and were re-interviewed as beneficiaries. As such, they exited from the pipeline non-beneficiary sample, potentially leaving it unrepresentative of how it was constituted in the baseline.[17]

Attrition can be a serious threat to inference using panel data because it can cause the original random sample to become unrepresentative of the treatment and control groups as individuals exit from the panel over time. This can lead to biased parameter estimates of the impact of treatment. These potential biases, however, depend not on the magnitude of attrition but on whether the attrition was non-random. In this section, a number of tests indicate that attrition was random in this dataset, while others suggest that it was not. In order to control for the potential bias resulting from non-random attrition, the inverse probabilities of retention (*i.e.*, non-attrition) are used as weights (Fitzgerald, Gottschalk and Moffitt, 1998). We also provide estimates of Lee bounds to explore the robustness of the results to alternative assumptions about the type of non-

[16] In the case of a beneficiary who had abandoned the project, enumerators were instructed to interview the replacement household living on the same plot. If this was not possible, they selected a neighbor, giving priority to households who had been present since the beginning but had not been interviewed in the baseline.
[17] In addition to attrition, a small number of observations had to be excluded due to missing values in the follow-up period. These observations only represent 1.6 percent of the total number of attritors.

random attrition, and estimate a model using the repeated cross-sectional data, which includes the individuals who would attrit after 2006 and their replacements in 2010.

A number of steps were taken to analyze whether there was any pattern to the attrition in the data. The first test conducted was a simple comparison of base year means (Alderman et al. 2001). Because we suspected that there may be heterogeneous patterns to attrition, the comparisons were done first for the full sample, and then separately for the beneficiary and pipeline non-beneficiary samples. Examining the full sample, Table 9 shows that attritors seemed to be younger, more white, less married, had more schooling, less experience as farmers and were more urban. They also had less trust in other members of their association than non-attritors. While there are many significant differences in observable variables, there was no evidence that attritors had systematically different outcome variables at baseline within the full sample or the beneficiary group. Nevertheless, it appears that the weakest of the pipeline non-beneficiaries, in terms of agricultural production, were more likely to disappear from the sample after the baseline period. This would result in a pipeline non-beneficiary group that appeared stronger than it would have, had the attritors remained in the sample. This pattern of attrition would lead to a *downward* bias on the estimated coefficients of program impact. Because the comparison of mean group characteristics shows significant differences for many variables, non-random attrition is suspected.

A second test to assess whether attrition was random were a series of probit regressions with the dependent variable equal to one if the enrolled member left the sample between the baseline and follow-up period, and zero otherwise (see Panel A of Table 10). Insignificant coefficients with the pooled data suggest that there is no difference in the probability of attrition between the treatment and control groups. When the probits are estimated separately for each group, however, the stronger beneficiaries with respect to agricultural production have an increased probability of attrition at the ten percent level of significance (results available from the authors). Again, this could lead to a downward bias on the estimates presented so far.

Table 9
Attrition Comparison: Mean of Attritor - Mean of Non-Attritor.

	Full Sample		Pipeline NB		Beneficiary	
	Mean Difference	p-values	Mean Difference	p-values	Mean Difference	p-values
Production and Income Variables						
Agricultural Production	-93.15	0.23	-290.84	0.01***	58.15	0.61
Earned Income	-5.29	0.95	-161.00	0.21	129.59	0.29
Individual Characteristics						
Age	-2.22	0.00***	-1.66	0.11	-3.90	0.00***
Sex	0.00	0.87	0.10	0.00***	-0.05	0.09*
White	0.06	0.01***	0.02	0.48	0.07	0.03**
Married	-0.09	0.00***	-0.06	0.07*	-0.16	0.00***
Years of Schooling	0.48	0.02**	0.53	0.07*	0.55	0.08*
Years of Experience	-3.34	0.00***	-2.74	0.01***	-4.18	0.00***
Urban	0.09	0.00***	0.08	0.04**	0.03	0.32
Social Capital Variables						
Position Held	-0.05	0.08	0.04	0.31	-0.06	0.12
Frequency of Meeting	-0.04	0.34	-0.12	0.08*	-0.01	0.91
Trust	-0.07	0.01***	-0.12	0.01***	-0.08	0.07*
Individual Agricultural Variables						
PRONAF	-0.05	0.05**	-0.08	0.03**	-0.07	0.04
Technical Assistance	0.03	0.03**	0.05	0.04**	-0.01	0.60
Local Agricultural Variables						
Daily Agricultural Wage	-0.19	0.00***	-0.19	0.03**	-0.07	0.42
Yield of Corn	-0.01	0.51	-0.07	0.02**	0.03	0.33

Notes: ***Significant at 1%; ** Significant at 5%; * Significant at 10%. Mean difference is the difference between baseline means of attrition and non-attrition groups. p-values are test for equality of these means.

Following Alderman et al. (2001) and Chawanote and Barrett (2014), we next implemented a BGLW test (Becketti, Gould, Lillard, and Welch, 1988) by estimating the following equation with the sample of attritors and non-attritors using baseline data only:

$$Y_{is} = \alpha_m + \beta A_{is} + \delta X_{is} + \mu(X_{is} * A_{is}) + \varepsilon_{is} \quad (6)$$

where Y are the outcome variables agricultural production and earned income, A is an indicator variable that equals one if the enrolled member attrited between the baseline and follow-up periods, and X are control variables. In the complete specification (6), A enters additively and interacted

with the control variables.[18] Panel B in Table 10 shows that for earned income the coefficient on the attrition dummy is significant at the ten percent level only in the complete model that includes all controls and interactions (column 6). For agricultural production, the attrition dummy is not significant in any of the specifications. However, an F-test of the joint significance of the coefficient on the attrition dummy and the coefficients on the interactions shows that these variables are jointly significant at the ten percent level for agricultural production. In this way, we conclude that—at least at the ten percent level of significance—the coefficients on the explanatory variables differ between individuals who disappear from the panel and those who do not. The attrition tests suggest the likelihood of selection into attrition, and thus the use of attrition weights is warranted.

The attrition weights, or the inverse probabilities of retention, are estimated using baseline-level data only and are defined by:

$$w(z,x)=[\Pr(A=0|z,x)/\Pr(A=0|x)]^{-1} \quad (7)$$

where $\Pr(A=0)$ is the probability of retention, x are the control variables used in the estimation and z are auxiliary variables that affect the attrition propensity, can be related to the density of the outcome variables conditional on the control variables, and yet are not in the original regressions (Fitzgerald et al., 1998). Out of seven potential variables that we identified, three were used as the auxiliary variables—previous ties with association members, whether the enrolled member had lived in a different city in the past ten years, and home ownership. All three were predictive of attrition either individually or jointly. The intuition behind the attrition weights is that they give more weight to enrolled members that have similar initial characteristics to attritors than to enrolled members with characteristics that make them more likely to remain in the sample. With the inclusion of attrition weights, Table 11 shows that all estimated coefficients of the impact of treatment remain largely similar, and none change by a statistically significant amount. This result

[18] We report results from three specifications: just with *A*, with *A* and *X*, and with *A, X* and interactions. The included control variables are identical to those used in the main regressions. Due to multicollinearity, race was substituted by the variable "white" (an indicator variable that equals one for Caucasians and zero otherwise) and municipality was not interacted with the attrition variable.

Table 10
Attrition Analysis.

	Agricultural Production			Earned Income		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Probits for Probability of Retention						
Attrition	-0.00003		0.00001	-0.00000		0.00001
	(0.00004)		(0.00003)	(0.00003)		(0.00003)
N	1310		1310	1310		1310
Panel B: BGLW Test for Attrition Analysis						
Attrition	-92.48	106.50	1031.57	-5.79	91.29	2244.15*
	(118.63)	(113.87)	(1152.56)	(119.54)	(110.27)	(1236.84)
(F-Stat)			1.58			1.41
(p-value)			0.09*			0.15
N	1310	1310	1310	1310	1310	1310
Municipal FE	N	Y	Y	N	Y	Y
Individual Controls[1]	N	Y	Y	N	Y	Y
Social Capital Controls[2]	N	Y	Y	N	Y	Y
Individual Agricultural Controls[3]	N	Y	Y	N	Y	Y
Local Agricultural Controls[4]	N	Y	Y	N	Y	Y

Notes: ***Significant at 1%; ** Significant at 5%; * Significant at 10%. Clustered standard errors in parentheses. [1]Individual controls include: age, sex, race, marital status, schooling, experience and urban status. [2]Social capital controls include: position in association, frequency of meetings and trust in association. [3]Individual Agricultural controls include: technical assistance and PRONAF. [4]Local Agricultural controls include: daily agricultural wage and yield of corn. All values in household per capita Reais of January 2010. Note that models (3) and (6) in Panel B, corresponding to agricultural production and earned income respectively, include interaction between the dummy for attrition and controls.

supports the conclusion that attrition is unlikely to be the source of the positive and statistically significant findings reported in Section V.

In addition to reweighting, we estimate bounds on the treatment effects following Lee (2009). This approach is useful in obtaining a range of possible estimates when there is non-random sample selection. It provides an interval for the true value of treatment effects based on best- and worst-case scenarios about the type of non-random attrition. While the attrition analysis discussed above suggests that our situation is not likely to be close to either of these extremes, the bounds can nonetheless be informative. The intuition behind the Lee bounds estimator is to trim

Table 11
Program effects on Agricultural Production & Earned Income With and Without Attrition Weights.

	Agricultural Production				Earned Income			
	DD - Mun. FE		DD - Indiv. FE		DD - Mun. FE		DD - Indiv. FE	
	With	Without	With	Without	With	Without	With	Without
Panel A: Binary								
Time*Status	435.85**	428.89**	427.24**	425.42**	121.46	129.93	138.92	161.69
	(197.80)	(195.47)	(188.93)	(184.93)	(186.14)	(183.80)	(179.68)	(176.23)
Panel B: Duration of Treatment								
LO 0-3 Years	299.28	306.79	162.06	180.53	-119.79	-104.95	-258.34	-242.85
	(244.67)	(245.11)	(256.96)	(254.82)	(225.12)	(221.66)	(265.63)	(253.81)
LO 4 Years	412.97*	398.98*	381.84*	373.69*	55.76	61.98	99.09	100.82
	(211.43)	(212.44)	(203.96)	(207.72)	(203.34)	(205.04)	(203.39)	(205.45)
LO 5-6 Years	580.03***	578.20***	733.38***	707.22***	431.24*	439.56**	530.55**	573.94***
	(217.14)	(212.99)	(231.11)	(204.56)	(225.77)	(215.05)	(246.31)	(211.37)
N	1526	1526	1526	1526	1526	1526	1526	1526
Mean of control group	957.01	957.01	957.01	957.01	1562.87	1562.87	1562.87	1562.87
Attrition Weights	Y	N	Y	N	Y	N	Y	N
Individual Controls[1]	Y	Y	N	N	Y	Y	N	N
Social Capital Controls[2]	Y	Y	N	N	Y	Y	N	N
Individual Agricultural Controls[3]	Y	Y	N	N	Y	Y	N	N
Local Agricultural Controls[4]	Y	Y	Y	Y	Y	Y	Y	Y

Notes: ***Significant at 1%; ** Significant at 5%; * Significant at 10%. Clustered standard errors in parentheses. [1]Individual controls include: age, sex, race, marital status, schooling, experience and urban status. [2]Social capital controls include: position in association, frequency of meetings and trust in association. [3]Individual Agricultural controls include: technical assistance and PRONAF. [4]Local Agricultural controls include: daily agricultural wage and yield of corn. DD- Muni FE: difference-in-differences with municipal fixed effects. DD- Indiv. FE: difference-in-differences with individual fixed effects. All values in household per capita Reais of January 2010.

excess observations from the treated or non-treated sample, whichever suffers less attrition, to ensure an equal share of observations for both groups. Trimming is done on the change in the dependent variable, and is done from the bottom (top) tail to obtain the upper (lower) bound.

A limitation of the Lee bounds estimator in our case is that it is generally only applicable when a treatment is binary. But the main effects in this paper are coming from the intensities of treatment which are non-binary. In order to adapt the approach to our setting, we estimate two separate sets of bounds for each outcome variable. Panel A of Table 12 presents bounds for the outcome agricultural production by separately comparing the LO 4 years and LO 5-6 years groups against the control group. The upper bound in each case is positive and significant, but the lower bound is significant at five percent only for the LO 5-6 years group. Thus, while the lower bound point estimate is positive, we cannot rule out a zero effect for the LO 4 group in the worst-case scenario that leads us to trim only the best performing beneficiaries from our sample. This is not inconsistent with the main results in Table 3 where the coefficients on the LO 5-6 group were significant at one percent while the coefficients on the LO 4 group were only significant at 10 percent. For earned income, in Panel B, we get significant upper bounds for both groups, but the lower bound for LO 4 is negative. In fact, the bounds are quite wide, ranging from negative R$384 to positive R$664. This is consistent with Table 4 where the estimated coefficients were not large, and not significantly different from zero. For the LO 5-6 group, the bounds are much tighter and suggest a true effect somewhere between R$393 and R$899. Although the lower bound is not significantly different from zero, and thus suggests caution, the results are broadly consistent with the individual FE estimate in Table 4 which was R$574.

Repeated Cross-Sections

A final approach for testing the robustness of the main results presented in Section V is to forego the balanced panel and make use of all available observations in both cross-sections. Thus, the DD models with municipal FE can be estimated with the complete baseline data (including attritors) and the complete follow-up data (including replacements). We do this both with the pipeline control and solely among beneficiaries. The baseline data contained a total of 1,310 observations—763 individuals that were in the panel plus 547 attritors. Two versions of the follow-up were employed in an effort to differentiate replacements that were likely to be very similar to the original sample from those that could potentially be somewhat different. The first group only includes replacements that belonged to associations that were originally selected to be part of the sample.

These are individuals that were present from the beginning, but were not initially chosen to represent the selected associations. The second group includes all replacements, regardless of when they entered the program. The narrower definition of the repeated cross-section (RCS partial)

Table 12
Lee Bounds.

	Lower bound estimate	Upper bound estimate
Panel A: Agricultural Production		
LO 4 years vs. control	26.7	744.8***
	(161.83)	(163.19)
LO 5-6 years vs. control	553.6**	914.2***
	(258.41)	(213.68)
Panel B: Earned Income		
LO 4 years vs. control	-384.4*	663.8***
	(210.17)	(233.42)
LO 5-6 years vs. control	393	898.8***
	(325.55)	(326.94)

Notes: ***Significant at 1%; ** Significant at 5%; * Significant at 10%. Standard errors in parentheses.

was estimated with a total of 2241 observations (931 in the follow-up), while the complete repeated cross-section (RCS full) was estimated with 2,511 observations (1,201 in the follow-up). In both cases, when using the complete RCS, 43 observations were included in the binary estimation but excluded from the intensity of treatment estimation because the date of program entry was missing. A comparison of the means of the time invariant controls between the replacements in RCS full and the attritors shows that they have statistically equivalent age, race, experience and schooling within both the beneficiary and non-beneficiary groups (available from the authors). The only significant difference is that the replacements have more women. We suspect that this is because women were more likely to be present when the enumerators visited to gather the second round of data. Table 5 showed no statistically significant difference in outcomes for men and women.

Table 13 shows the results based on the repeated cross-sectional data. When we use the pipeline non-beneficiaries, both the restricted RCS (partial) and the complete RCS (full) samples

Table 13
Program Effects Using Repeated Cross-Sections.

	Using Pipeline Non-Beneficiaries				Using Beneficiaries Only			
	Agricultural Production		Earned Income		Agricultural Production		Earned Income	
	RCS Partial	RCS Full	RCS Partial	RCS Full	RCS Partial	RCS Full	RCS Partial	RCS Full
	DD - Mun. FE		DD - Mun. FE		DD - Mun. FE		DD - Mun. FE	
Panel A: Binary								
Time*Status	363.45**	330.82**	-13.43	12.37	284.44	308.93	418.23*	442.38*
	(156.19)	(131.89)	(156.55)	(138.27)	(218.60)	(213.04)	(240.98)	(237.58)
N	2241	2511	2241	2511	1582	1625	1582	1625
Panel B: Duration of Treatment								
LO 0-3 Years	281.21	252.54	-235.23	-204.78				
	(191.85)	(169.66)	(182.20)	(161.79)				
LO 4 Years	337.34*	321.05**	-50.93	-25.72	236.04	257.73	324.82	355.80
	(173.66)	(151.34)	(178.36)	(166.96)	(230.54)	(224.08)	(254.54)	(250.72)
LO 5-6 Years	481.28***	462.90***	230.64	252.66	382.05*	408.91*	606.66**	611.49**
	(171.09)	(149.02)	(187.99)	(167.27)	(224.72)	(217.77)	(256.58)	(251.76)
N	2241	2468	2241	2468	1582	1625	1582	1625
Mean of control group	765.63	765.63	1386.51	1386.51	878.48	878.48	1646.01	1646.01
Individual Controls[1]	Y	Y	Y	Y	Y	Y	Y	Y
Social Capital Controls[2]	Y	Y	Y	Y	Y	Y	Y	Y
Individual Agricultural Controls[3]	Y	Y	Y	Y	Y	Y	Y	Y
Local Agricultural Controls[4]	Y	Y	Y	Y	Y	Y	Y	Y

Notes: ***Significant at 1%; ** Significant at 5%; * Significant at 10%. Clustered standard errors in parentheses. [1]Individual controls include: age, sex, race, marital status, schooling, experience and urban status. [2]Social capital controls include: position in association, frequency of meetings and trust in association. [3]Individual Agricultural controls include: technical assistance and PRONAF. [4]Local Agricultural controls include: daily agricultural wage and yield of corn. DD- Muni FE: difference-in-differences with municipal fixed effects. All values in household per capita Reais of January 2010.

show a positive and statistically significant impact on agricultural production in the binary model. The difference between the main results in Table 3 and RCS partial is about fifteen percent. In all cases, the difference between using RCS partial and RCS full is quite small. The intensity of treatment estimates are also consistent with the results from Table 3. Agricultural production grows over time, with the impact of the program only becoming statistically significant in the fourth year of land ownership. The coefficient estimates from RCS partial are at most seventeen percent smaller than in Table 3. The general pattern for earned income is also similar to the main results in Table 4. Both binary RCS estimates are close to zero and statistically insignificant. The intensity of treatment coefficients are negative for LO 0-3, in the neighborhood of zero for LO 4, and positive for LO 5-6. In contrast to Table 4, the LO 5-6 coefficients are not statistically significant. But the increase in earned income between LO 0-3 and LO 5-6 that was observed within beneficiaries in Table 8 is confirmed here. The final column of Table 13 shows an increase of over R$600 for beneficiaries with 5-6 years of landownership relative to those who had land for three years or less, significant at 5 percent.

Thus, as with the reweighting for attrition and the estimation of Lee bounds, the repeated cross-sectional results largely support the main findings of the paper. The PNCF has a statistically significant impact on agricultural production, and the magnitude of the impact grows with time. Earned income, in contrast, appears to dip in the first few years and then rise thereafter. When compared to the non-beneficiaries, neither the decline nor the growth are large enough—or estimated precisely enough—to represent a statistically significant difference using the repeated cross-sections. But when analyzed solely within beneficiaries who have owned land for different amounts of time, the increase for LO 5-6 relative to LO 0-3 is statistically significant.

VII. REPAYMENT

The results in Sections V and VI showed that the PNCF is successful in increasing beneficiaries' agricultural production and earned income after four years of land ownership, although the findings for income were not always statistically significant. It is also important to consider the beneficiaries' ability to repay the PNCF loans, and the net effects of the program after accounting for repayment. A few policies facilitate repayment. First, if the principal is above R$15,000, beneficiaries have up to seventeen years to repay. For smaller loans, the repayment period is limited to fourteen years. Second, the grace period is twenty-four months, and the annual

interest rates vary between two and five percent depending on the principal. In the first year of repayment—the beginning of the third year of land ownership—the beneficiaries with a principal of less than R$15,000 are only required to pay the interest accrued on the loan during the first two years (MDA, 2009). Beneficiaries with larger loans must also pay the first installment. In addition, in the semi-arid regions of the Northeast of Brazil there is a forty percent discount on all installments that are paid on time. In the rest of the Northeast, the discount is thirty percent. Lastly, there is an additional ten percent discount on installments for associations that were able to negotiate the price of the land below what the predicted price would have been using the land price monitoring system. The cap for the discounts is R$1,000 per installment. Given these two discounts, it is likely that a high share of beneficiaries should be able to repay their loans. What follows is not an analysis of the percentage of beneficiaries that actually paid. We do not have access to data on loan payments. Instead, we provide an analysis of the percentage that should have had enough income to meet their loan obligations.

Table 14 shows our estimates of the share of beneficiaries by years of land ownership that should have been able make their loan payments. Looking only at the cases with both discounts, depending on the years of land ownership, 79 to 85 percent of beneficiaries could repay given their earned income, and 91 to 93 percent could repay once transfers are included.[19] This would leave beneficiaries with 64 to 76 percent of their earned income after repayment, and 71 to 82 percent of their income including transfers. Nevertheless, many beneficiaries do not secure these discounts and have a more difficult time repaying.

The above analysis suggests that there appears to be a relatively high share of beneficiaries who should be able to repay their loans. Nevertheless, the payments range from 24 to 40 percent of beneficiaries' earned income, and considering that these are very poor families these payments might be quite burdensome. In order for the burden of the debt to be minimized, the grace period could be extended so as to give the beneficiaries sufficient time to adapt to their new circumstances and generate enough earned income to pay the debt with greater ease. In this regard, a more forward looking analysis suggests that—if the program impacts continue to grow at the rate

[19] Although including government transfers no longer allows us to strictly measure the ability of beneficiaries to repay given increases in income *due to the program*, transfers such as old age social security benefits and the conditional cash transfer program *Bolsa Família* represent an important share of income in the rural Northeast. Helfand et al. (2009) report that social security transfers accounted for 23 percent of income in the rural Northeast in 2005. In this dataset, that number is 25 percent.

Table 14
Beneficiaries' Ability to Repay PNCF Loan.

Year	N	Percent Able to Pay			Share of Earned Income Used		
		No Discount	On-time Discount	Both Discounts	No Discount	On-time Discount	Both Discounts
Panel A: Without Transfers							
3	87	66%	81%	84%	40%	36%	36%
4	317	73%	78%	79%	37%	28%	26%
5-6	146	80%	84%	85%	37%	26%	24%
Panel B: With Transfers							
3	87	82%	90%	91%	37%	30%	29%
4	317	86%	90%	91%	30%	22%	21%
5-6	146	89%	93%	93%	29%	20%	18%

observed in the first five years of land ownership—beneficiaries should be able to make debt payments more and more easily.

Although the beneficiaries' ability to repay is an important consideration, an equally important question is the effectiveness of the program in improving living standards net of debt payments. In order to address this missing piece, the value of the installment due was calculated for each beneficiary and then subtracted from earned income in the follow-up period. Regressions were re-estimated using the updated earned income, and attrition weights were used to ensure the representativeness of the sample. Table 15 shows the results for the individual FE duration of treatment estimation. The results indicate that once repayment is included in the analysis, beneficiaries no longer enjoy an increase in their current welfare because the gains to earned income are being used for repayment. With both discounts, even beneficiaries in the fifth or sixth year of land ownership now display no significant effects on earned income. Beneficiaries in the fourth year with no repayment discounts, and all beneficiaries with three or less years of land ownership display negative and significant effects of being a beneficiary of the program. As such, while the program works to increase the earned income of beneficiaries, once repayment is taken into consideration the beneficiaries in the first four years of the program are often statistically *worse off* in terms of current welfare than the pipeline non-beneficiaries. For beneficiaries with five to six years of land ownership, the gains in earned income go towards paying the debt and

increasing the net wealth of the household, rather than toward improving current welfare.[20] Since the results become less negative and eventually positive with increasing number of years of land ownership, it is likely that improvements in this situation are only a matter of time.

Table 15
Program Effects on Earned Income Subtracting Repayment.

	No Discount	One Discount	Both Discounts
	DD - Indiv. FE		
LO 0-3 Years	-876.24**	-671.84**	-652.07**
	(349.32)	(324.33)	(325.86)
LO 4 Years	-440.13**	-249.49	-222.49
	(210.14)	(206.47)	(206.55)
LO 5-6 Years	-4.17	197.24	227.59
	(254.85)	(250.96)	(250.64)
N	1526	1526	1526
Attrition Weights	Y	Y	Y
Local Agricultural Controls[1]	Y	Y	Y

Notes: ***Significant at 1%; ** Significant at 5%; * Significant at 10%. Clustered standard errors in parentheses. The value of repayment was calculated uniquely for all beneficiaries depending on principal, interest and year in the program. These individually calculated repayment values were then subtracted from each beneficiaries' EI and the estimations were performed as before. [1]Local Agricultural controls include: daily agricultural wage and yield of corn. DD- Indiv. FE: difference-in-differences with individual fixed effects. All values in household per capita Reais of January 2010.

[20] Since the principal paid on the debt could also be considered part of asset accumulation, we re-estimated the model in Table 15 by dropping the principal amount. We find significant improvement in welfare for the LO 5-6 group that have access to one or both discounts, comparable to the results in Table 11. It is unlikely, however, that a beneficiary could formally sell his or her land prior to the loan being paid off in full. The transaction would have to be informal, implying a significant loss on the principal that had been paid.

VIII. CONCLUSION

The primary objective of the National Land Credit Program (PNCF) is to promote the creation of productive activities which, in turn, increase the income and wellbeing of the beneficiary population. The impact evaluation carried out here was complicated by considerable attrition from the sample, and the potential for unobservable variables to bias our results, leading us to exercise caution about some of the conclusions. The analysis confirms that the program achieves the first part of its objective—to create productive activities—through the evidence of increased agricultural production for program beneficiaries. The results for agricultural production are robust to alternative specifications, indicating that the program begins to increase agricultural production after the first three years of land ownership, although not necessarily by a statistically significant amount. After four years, there is a large and significant increase. Using the preferred specification with individual fixed effects, we conclude that agricultural production increases by an average of R$707 (US$396.55) per person in households with five to six years of land ownership. This represents an increase of around 74 percent relative to the baseline production of non-beneficiaries.

With regard to the welfare of the beneficiary population, earned income is a superior indicator because it accounts for the fact that beneficiaries might increase agricultural production by substituting away from labor market earnings. The analysis of earned income revealed that positive and significant effects only appear for the most seasoned beneficiaries, and even here attrition from the sample makes us less certain about the results. Relative to the control group, the individual fixed effects model shows that earned income increased by R$574 (US$322) per person in households with five to six years of land ownership. This increase of about 37 percent relative to the baseline income of non-beneficiaries indicates that the program also appears to achieve the second part of its primary objective, but exclusively for beneficiaries with more than four years of land ownership. The income gain is roughly equivalent to what a poor household would have received in 2010 through the conditional cash transfer program *Bolsa Família* in exchange for ensuring that two children remained in school.

Once repayment of the PNCF loan is factored into the analysis, however, it appears that beneficiaries face a trade-off between current welfare and asset accumulation. The impact of the program net of loan payments is negative and significant in the early years, and only becomes positive (but not significant) with five to six years of land ownership. Thus, although the

beneficiaries' earned income appears to increase as a result of participation in the program, most of this gain goes to making debt payments for the land. In effect, their current income net of payments was lower or at best the same as income in the control group in the first four years, but beneficiaries were increasing their net wealth. A more forward looking analysis suggests that—if the program impacts continue to grow at the rate observed in the first five to six years of land ownership—beneficiaries should be able to make debt payments and improve current welfare simultaneously. This is the cautiously optimistic scenario. The alternative, at least for a share of the beneficiaries, is to fall into arrears on their payments, thereby losing access to the on-time discount and to PRONAF family farm credit.

The results of this study have important implications for policy. First, since beneficiaries only see significant income gains as of the fifth year of land ownership, the grace period should be extended beyond two years to allow sufficient time for productive projects to mature. Second, policy should facilitate improved access to technical assistance and PRONAF loans, which contribute to the success of productive projects and thus to the beneficiaries' repayment capacity. Third, even with five to six years of land ownership, beneficiaries have not achieved a level of earned income that permits both a higher level of welfare and the ability to repay the loan. This problem could be overcome by spreading debt repayment over a longer horizon in order to reduce the burden of annual payments. Instead of the current 14-17 years, loans could be amortized over 20-30 years. Alternatively, payments of principal could grow more gradually in the initial years of the loan, tracking the expected path of income growth. Fourth, the conclusion that positive effects on income only grow with time underscores the importance of conducting medium term impact evaluations of asset transfer programs, rather than restricting attention to the first few years of program impacts.

The general conclusion, then, is optimistic, but cautious. The PNCF can provide a pathway out of poverty by transferring assets to the poor. But repayment in the early years is an issue. Beneficiaries require sufficient time on their newly acquired land to realize adequate returns on their investments. It would seem, then, that the PNCF—and asset transfer programs more generally—is a viable option for rural poverty reduction, but positive results are only achieved in a matter of time.

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APPENDIX TABLES

Table 1.A
Comparison of Means within Pipeline (Mean of New Beneficiaries - Mean of Remaining Non-Beneficiaries).

	Difference	p-value
Production and Income Variables		
Agricultural Production	-77.36	0.65
Earned income	40.48	0.82
Individual Characteristics		
Age	-2.55	0.05**
Sex	0.15	0.00***
White	0.05	0.23
Married	-0.07	0.12
School	0.55	0.13
Experience	-3.34	0.02**
Urban	-0.04	0.33
Social Capital Variables		
Position Held	0.11	0.05**
Meet	-0.15	0.08*
Trust	-0.13	0.01***
Individual Agricultural Variables		
PRONAF	-0.03	0.51
Technical Assistance	0.09	0.00***
Local Agricultural Variables		
Daily Agricultural Wage	-0.71	0.00***
Yield of Corn	-0.28	0.00***

Notes: ***Significant at 1%; ** Significant at 5%; * Significant at 10%. p-values are from t-tests that compare the equality of new beneficiary means with remaining non-beneficiary means.

Table 2.A
Program Effects with Eagerness Proxy and Beneficiaries Only.

	Agricultural Production	Earned Income
	DD - Mun. FE	
LO 4 Years	278.30	419.81
	(321.60)	(311.04)
LO 5-6 Years	698.49**	1114.81***
	(348.50)	(410.45)
LO 4 Years*EagerDummy	-85.67	-172.90
	(308.49)	(327.85)
LO 5-6 Years*EagerDummy	-418.48	-564.64
	(310.61)	(381.17)
Eagerness	-0.30	0.01
	(0.63)	(0.64)
N	1124	1124
Mean of control group	878.48	1646.01
Individual Controls[1]	Y	Y
Social Capital Controls[2]	Y	Y
Individual Agricultural Controls[3]	Y	Y
Local Agricultural Controls[4]	Y	Y

Notes: ***Significant at 1%; ** Significant at 5%; * Significant at 10%. Clustered standard errors in parentheses. In this regression, control group includes "less than 3 years" land owners. [1]Individual controls include: age, sex, race, marital status, schooling, experience and urban status. [2]Social capital controls include: position in association, frequency of meetings and trust in association. [3]Individual Agricultural controls include: technical assistance and PRONAF. [4]Local Agricultural controls include: daily agricultural wage and yield of corn. DD- Muni FE: difference-in-differences with municipal fixed effects. All values in household per capita Reais of January 2010.

Table 3.A
Decomposing Program Effects into Changes in a) Agricultural Production or Earned Income and b) Household Size.

	Effect of Production or Income (with Household Size Fixed)		Effect of Household Size (with Production or Income Fixed)	
	Agricultural Production	Earned Income	Agricultural Production	Earned Income
	DD - Indiv. FE	DD - Indiv. FE	DD - Indiv. FE	DD - Indiv. FE
Panel A: Binary				
Time*Status	221.14	-123.49	170.55	513.05***
	(155.43)	(139.96)	(154.92)	(167.91)
Panel B: Duration of Treatment				
LO 0-3 Years	45.48	-319.97	-96.90	140.98
	(223.47)	(260.89)	(108.35)	(139.49)
LO 4 Years	153.79	-261.16	297.01	706.13***
	(185.15)	(167.47)	(227.84)	(240.46)
LO 5-6 Years	488.78***	310.95*	81.92	352.63*
	(186.24)	(186.49)	(116.51)	(179.99)
N	1526	1526	1526	1526
Mean of control group	957.01	1562.87	957.01	1562.87
Individual Controls[1]	N	N	N	N
Social Capital Controls[2]	N	N	N	N
Individual Agricultural Controls[3]	N	N	N	N
Local Agricultural Controls[4]	Y	Y	Y	Y

Notes: ***Significant at 1%; ** Significant at 5%; * Significant at 10%. Clustered standard errors in parentheses. [1]Individual controls include: age, sex, race, marital status, schooling, experience and urban status. [2]Social capital controls include: position in association, frequency of meetings and trust in association. [3]Individual Agricultural controls include: technical assistance and PRONAF. [4]Local Agricultural controls include: daily agricultural wage and yield of corn. DD- Muni FE: difference-in-differences with municipal fixed effects. DD- Indiv. FE: difference-in-differences with individual fixed effects. All values in household per capita Reais of January 2010. In the first panel, the denominator of the outcome variable, i.e. household size is fixed, and in the second panel the numerator, i.e. agricultural production or earned income is fixed at baseline values.