Effects of Land Regularization: Evaluation of Ecuador's Rural Land Regularization and Administration Program, SIGTIERRAS

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Abstract

This paper evaluates the impact of the rural land and administration program in Ecuador, SigTierras. Using a double robust estimation, which combines a difference-in-differences design with weighting by the propensity score matching, we estimate the causal effect of the program in the households with land tenure issued in need of the program. Our findings suggest that SigTierras had no effect in terms of perception of land tenure security, reduction of land conflicts, and investments in land inputs such as pesticides and fertilizers. We, however, find that treated households increased their household total income relative to control households.

JEL classifications: Q15, O13, P32

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1 Introduction

Land is a central economic asset and major source of income for rural households. Enhancing land tenure security has been touted to yield a number of economic benefits, from increased investment and credit access to productivity length (Lawry et al., 2017). Despite its importance, the Latin American and Caribbean (LAC) region still faces high levels of land tenure informality and insecurity in rural areas. In Ecuador, property rights over agricultural land are characterized by a high degree of informality and uncertainty. According to the results of a land administration program, the PRAT (Program for the Administration and Regularization of Rural Lands), carried out in eight municipalities between 2002 and 2008, less than 40% of agricultural plots had a *título perfeccionado*, defined as a titled that: (a) is formally registered in the Property Registry; (b) reflects current ownership status; and (c) is free of conflict. The remaining 60% of the plots either had never had a registered title or had an invalid title because it has not been updated to reflect land transactions such as sale or inheritance.

Despite the increasing efforts to finance land regularization programs in the LAC region, the evidence about these interventions remains inconclusive. While some studies have found positive impacts of tenure security on household investments (Galiani and Schargrodsky, 2010) and agricultural income (Katz and Chamorro, 2003), others have found no relationship (Boucher et al., 2005). Part of the reasons of why the evidence on these programs is still inconclusive might be due to the lack of experimental evaluations. This probably stems from the fact that identifying the parcel owners in need of land security is a difficult task to accomplish, specially before the implementation of a program. And this is because information on the legal status of the parcel is normally collected during the cadastral sweep, which takes place during the implementation phase. Moreover, empirical evaluations on land regularization programs typically analyze the effect of providing tenure security to all the parcel owners participating in a program regardless on whether they actually need to formalize their land. Within this setting, causal evidence on land administration and regularization programs is typically based on estimations of the Intent-to-Treat Effect (ITT) or the Average Treatments Effect (ATE).

But this is misleading as the estimator that land regularization programs should target is the Average Treatment Effect of the Treated (ATT), that is to say, what is the effect of a land regularization program on those households that actually need the treatment.

Thus, in this paper we provide a sound methodological approach to assess the effects of a land regularization program in Ecuador, SigTierras. It is important to mention that the prospective evaluation was designed to facilitate the obtention of an ATT estimand. This is because for the impact evaluation both the treatment and control group are identified after a cadaster sweep was completed. Specifically, after the cadaster sweep, it was possible to identify households with perceived land tenure issues that might benefit from treatment. Control households were identified by first selecting control cantons and census tracts (based on their similarity to treated cantons and census tracts with respect to land tenure and agricultural characteristics), and after that a filter questionnaire was applied to identify those households with similar tenure issues than treated households.

From a policy perspective the primary question that this paper seeks to answer is the following: What was the impact of SigTierras on households with land tenure issues in different welfare and agricultural indicators? To answer this question we follow a double robust estimation. First, we use propensity score matching (PSM) to identify the households from the control group that were more similar to the treatment group in terms of different household characteristics at baseline. This allows use to find more reasonable counterfactual units and thus to have the region of common support to validate our estimates. Afterwards, we apply a difference-and-difference model incorporating the inverse probability weights from the PSM in order to estimate the causal impact of SigTierras on the variables of interest that were pre-specified in the program's impact evaluation design (Boucher, 2010). We find no effect of SigTierras on perception of land tenure security, reduction of land conflicts, and investments in land inputs such as pesticides and fertilizers. We, however, find that treated households increased their household total income relative to control households. As we will explain further we believe that the absence of results can be ascribed to three main reasons. First, during

the project design, SigTierras did not establish the necessary legal and administrative mechanisms to ensure that those parcels under informal legal status would regularize their tenancy.[1] Second, during the execution, SigTierras focused on providing spacial certainty as well as georeferenced digital maps (via Geographic Information Systems), however, this does not appear to translate into higher tenure security for parcel owners. Finally, following the study of (Corral and Olea, 2019) on the conditions driving the take-up for SigTierra's legal component, SIOL, we assert that in many rural households that were intervened through SigTierras, informal property rights substituted effectively for formal property rights, thus, lowering the value of land regularization.

The remainder of this paper is structured as follows. Section 2 provides a basic background about SigTierras, its components and the main results that were documented in the Project Completion Report of the program. In this section, we also offer a discussion about SIOL, a free legal orientation system that was intended to incentivize parcel owners to regularize their properties. Section 3 presents a conceptual discussion about the mechanisms through which SigTierras is likely to affect different household outcomes. Section 4 presents the evaluation design, giving emphasis to the methodology in which cantons and households under treatment and control groups were selected. Section 5 describes the data-collection process, reports relevant descriptive statistics, and presents an overview of attrition between the baseline and the endline survey. Section 6 describes our identification strategy and present the estimates of the impact of SigTierras on different outcomes. Finally, section 7 offers concluding remarks and the policy implications of our analysis for future land projects.

2 Description of the Program & Context

A factor that has contributed to the predominance of informality in Ecuador is the inefficiency of the Municipal Cadastres and Property Registries, the two key institutions responsible for

[1] This was also stated in the recommendation section from the Project Completion Report (PCR) of this Program.

granting and maintaining property rights (IDB, 2010). The government of Ecuador identified this situation of informal property rights and weak institutions as a significant constraint to rural development and with the support of the IDB, in 2002 the government launched the Rural Land Regularization and Administration Program (PRAT). PRAT developed and implemented an effective methodology for the physical and legal regularization of property rights in the country, through comprehensive campaigns to establish cadastres, regularize land tenure, and register property. This "sweep" methodology was tested in eight of the country's cantons, clearing rights to 103,144 rural properties, resolving the lack of formal status for 34,568 titles, and issuing titles for 5,338 properties, in line with the operation's original targets. The methodology proved to be cost-effective and nimble, allowing for broad coverage among farmers in addition to being participatory, as it provided a central role for subnational governments and beneficiaries in the process of formalizing ownership.

Based on the positive results of the PRAT program, the Government of Ecuador (GoE) decided to expand the program at the national level. Therefore, in 2010 the GoE launched the National System for Rural Land Information and Management and Technology Infrastructure (the SigTierras program) with the support of the IDB. SigTierras was designed as a land administration program whose main objective was to enhance legal certainty with regard to property rights, to support the application of canton tax policies, and to provide information for land management and land use planning in rural areas. To achieve these objectives, the program included three primary components:

1. **Cadastral mapping**: For each agricultural parcel in the program area, SigTierras generated a geo-referenced digital map. Complementary information, such as physical characteristics, property rights, and land tenure situation, was collected by surveys applied by SigTierras personnel.

2. **Reorganization of and investment in Cadastres and Property Registries**: In order to improve the management of transactions and property rights and improve the efficiency of tax collection, SigTierras provided financial and technical support to partic-

ipating municipalities in order to facilitate the adoption of a new computerized system of data management. The program also supported the Property Registries in their transition from being based on individual-level records (*folio personal*) to parcel-level records (*folio predial*) and from being privately to municipally managed. It was hoped that these investments would increase the efficiency of tax collection and reduce the transactions costs associated with all forms of land transactions.

3. **Regularization of land titles**: Owners of parcels that lacked a *título perfeccionado* were offered financial support and technical assistance to acquire or update their titles. In addition, individuals involved in land conflicts would be provided legal assistance to resolve conflicts that prevented the acquisition or updating of titles. This component was implemented through the establishment of the System for Legal Orientation (SIOL) (*Sistema de Orientación Legal*).

2.1 Eligibility Criteria and Implementation

SigTierras was a program based on demand insofar municipalities needed to complete a multi-step application process and assume 20% of the total program cost in order to enroll. Once these pre-conditions were met, each municipality negotiated and signed an agreement (*convenio*) with SigTierras. Of a total of 171 cantons that showed interest in SigTierras 47 were chosen to participate.[2] The overriding reason was budgetary constraints. Other factors affecting participation were delays in the application and negotiation process, and availability of 1:25,000 - scale digital aerial photograph of the canton due to cloud covering that prevented the timely completion of aerial photos. This last reason was an exogenous factor that was outside the control of canton's authorities, however, having these aerial photos was a fundamental input to advance with the implementation of the components of program.

Once the *Convenio* was signed, the first step of the execution consisted in producing 1:5,000 - scale orthophoto maps, and begin the implementation of the cadastral sweep (*barrido catas-*

[2]Cantons are the second-level subdivisions of Ecuador below provinces and are equivalent to municipalities.

tral). To do so, a team of data collectors (*brigadistas*) visited each parcel in the participating cantons with the purpose of collecting data using a parcel record card (*ficha predial*). While one of the team members collected information about the tenure history, status of the parcel, and other relevant characteristics of the parcel such as infrastructure and uses, the other team member collected information about the dimensions and boundaries of the parcel based on the orthophoto at 1:5000 - scale. All the information from the cadastral sweep was validated by the owner or occupant and in case of her/his absence, collected information was validated with neighbors. All the information from the cadastral sweep was uploaded to a region central location and went through a quality control process. Once the parcel level data went through the relevant "checks", a map for each parcel was created with the following information: 1) date of issuance; 2) parcel information, including location, type of occupier, name and dimensions; and 3) names of bordering neighbors on all sides.

Once the cadastral sweep and mapping was finalized for a given area, program personnel presented the results in Public Exposition of Results Meetings (PEM). The PEM took place at the community-level (usually inside a school or other public place known to everybody) and was preceded by several days of communication campaigns so each community resident could plan ahead to be present at the event. The purpose of the PEM was two-fold: 1) socialize the results from the cadastral sweep, and 2) verify that the information collected during the cadastral sweep was accurate. This last step involved presenting the boundaries of the parcel to each parcel owner as well as the land tenure issues (if any) of each parcel. There were three different type of issues associated with each parcel: a) the parcel did not have a title; b) the title was not registered, or c) there was something unclear regarding the legal aspects of the file. Parcel tenants who agreed with the results from PEM, signed a document expressing that they were in agreement with the results and, afterwards, received an official map of the parcel which was free of charge.[3] The PEM allowed the identification of land owners or occupiers who

[3]In the event that the parcel occupant was in disagreement with the results, she could filed a complaint through a Claiming Act (*Acta de Reclamo*) The firms that were in charge of the PEM had the mandate of reviewing each of the possible complaints during the PEM, and to continue after, if resolution was not achieved.

were in possession of land that required a title or land with a title that needed formalization. Owners of parcels who lacked a *titulo perfeccionado* were encouraged to seek legal advice at the PEM.

Given that the cadastral sweep along with the PEM did not follow a predefined timeline, the execution of the Program in the 47 cantons took a considerable amount of time. In some cantons it started as early as 2012 while in others it started as late as 2016. By the end of 2016, year in which the program closed, the 47 cantons participating in SigTierras had completed the cadastral sweep. According to the Project Completion Report (PCR) of SigTierras, the main results achieved at the end of the Program can be summarized as follows:

1. **Cadastral mapping and National Land Information System**:

    - Orthophoto maps were developed for 206,908 square kilometers of land with a 1:5,000 - scale, achieving 100% of its target.[4]
    - 55 cantons were cadastred and assessed according to the Ministry of Agricuture (MAG) and the PRAT methodology, surpassing the goal of 50 cantons.[5]
    - 59 cantons were integrated into the National Land Information System (SigTierras), which allowed to electronically link the files of the cadastre and register offices. The system used georeferenced information and allowed easy updates on property information.[6]

2. **Regularization of land titles**:

[4] An additional 4,230 square kilometers of orthophoto maps were developed for the canton of Quito, however, this information was not included in the results of program as it was financed by the municipality of Quito. Taking into account the information from PRAT, SigTierras, and the canton of Quito, the country has a land coverage of 225.449 square kilometers with orthophoto maps, which corresponds to 88% of the national territory.

[5] The program not only supported the cadastral sweep of the 47 participating cantons from SigTierras, but also updated the cadastre of 6 cantons from PRAT and 2 more from MAG. According to the MAG, the canton Antonio Ante did not appropriate the cadastre and valuation because it had its own municipal cadastre system.

[6] In addition to the 54 cantons that appropriated the cadastre and valuation from SigTierras, 2 more cantons from PRAT, 2 from MAG, and the canton Antonio Ante, which did not have the National System for Land Administration (SINAT) installed, were also integrated.

- A total of 163,580 parcels initiated the regularization process. The project had the goal of 170,000 properties with legal ownership status regularized (i.e. with a *titulo perfeccionado*).
- The cadastral sweep revealed that a total of 417,652 parcels presented tenure irregularities.
- Through the project, 39,267 parcels regularized their legal ownership status: 35,277 were located in state land and the remaining 3,990 in private land. Given that the goal was to regularize 170,000 parcels, the program only achieved 23% of success on this key indicator associated with enhanced tenure security, and effectively regularized less than 10% of parcels found to have tenure irregularities.

The timelines of SigTierras project implementation is presented in Figure 1. It shows the activities that took place since the first year of approval, 2010, until its closing in 2018.

2.2 SIOL Component

The project design contemplated providing legal and technical assistance on demand to the owners of private and communal land to formalize property rights that were legally informal. The project design, however, did not fully flesh out how this would be implemented. The Ministry of Agriculture, in charge of executing the project, did not have the legal competency to carry out regularization processes. Thus, it devised an an ad-hoc mechanism coined System for Legal Orientation (SIOL) (*Sistema de Orientación Legal*). SIOL was a system comprised of three different stimulus or mechanisms. The first was a team of lawyers that provided free legal advice for those that approached it during the PEM. The second one was a call center that provided legal support based on demand via telephone calls.[7] And the third one was at SigTierras offices located in each municipality. SIOL also implemented an on-line database

[7]The call center provided assistance on the specific requirements and steps to regularize land tenure.

that collected information for all individuals that sought legal advice.[8]

Owners of four types of parcels were eligible for SIOL: (i) parcels with a title that was unregistered, (ii) parcels that were titled with an unknown registration status, (iii) untitled parcels, and (iv) parcels with no information. In spite of the efforts to increase the regularization of land with tenure issues, only owners of 61,159 parcels approached SIOL. Owners of less than 15% of parcels found to have tenure irregularities in the universe of cantons that were part of SigTierras, sought support from SIOL.

3 Theory of Change & Expected Outcomes

Since well-defined property rights are expected to mitigate expropriation risk, facilitate gains from trade, and support financial market transactions, it is theorized that tenure security promotes investment in and the efficient use of physical and human capital (Besley and Ghatak, 2010). Thus it is posited that strong and well-defined property rights, achieved through the granting of a title or regularization of a land title, can raise the productive efficiency, income, and well-being of agricultural households. Based on this framework, the evaluation design of the SigTierras program before the beginning of the project, proposed a list of outcomes that were to be measured at the end of the program (Boucher, 2010). These outcomes include the following: total household income; value of agricultural production; net agricultural income; and value of household wealth. In addition, the evaluation design identified other intermediate outcomes including farm yield; per-hectare farm expenditures on inputs (such as fertilizers) and land preparation; participation in land transactions (rental and sales markets, inheritance, mortgage); participation in formal and informal credit markets; household rationing status in formal and informal credit markets; terms of formal and informal credit markets (interest rate, loan size, maturity, collateral requirements); household perceptions of land tenure security;

[8]The data contain information from the cadastral sweep, the type of consultation made by each households, the number of times and dates when parcel owners contacted SIOL, the adjustments to the legal status of the parcel (if any), and the judicial determination of the competent authority on land tenure regularization.

frequency and intensity of land conflicts; household participation in government programs including *Bono de la Vivienda*, a housing subsidy scheme; and organic certification.

There is mounting research work related to the intermediate and final outcomes proposed by Boucher (2010) as well as to other outcomes that might be relevant in the context of the way that SigTierras was implemented (Gignoux et al., 2013). To begin with, given that the core of SigTierras revolved around land delimitation activities, resulting in georeferenced parcel maps with community assent, we expect that the program could serve to ameliorate land conflicts. Thus, the frequency of land conflicts might decrease as a consequence of the program.[9] Furthermore, insofar households can spend less time reallocating resources to land-guarding practices, they could invest their time in more productive activities (Besley and Ghatak, 2010).

On the other hand, where credit markets exist, even for households that never intend to sell their land, the ability to alienate property through title could be valuable. The possession of a title can be a source of access to new sources of credit because dwellers can use their land as collateral. This collateralization effect underlies the expectation that lenders positively react by increasing the supply of credit in order to absorb the potential increase in demand caused by the tenure security effect. Notwithstanding the theoretical impacts, the empirical results results largely fail to detect any actual impacts of titling on credit (Sanjak, 2012). Titling might also lower transaction costs in the land market (e.g. rental, sales, mortgage, inheritance, etc.) by clarifying rights and making them more easily transferable. Enhanced tenure security through titling might also provide incentives for rural agricultural households to investments in the land. Titling provides parcel owners with greater confidence that their investments will be capitalized into the price they would receive in the land market, ultimately increasing the value of their properties (Galiani and Schargrodsky, 2016). For example, Deininger and Chamorro (2002) investigate investments and income effects of massive of land regularization

[9]Torero et al. (2005) study the impact of the Special Rural Cadastre and Land Titling Program (PETT) in Peru on different relevant outcomes. Using household survey data and a quasi-experimental design (i.e. propensity score matching with difference-in-differences), they estimate average treatment effects of government property titling. Their results find evidence for a significant reduction in the risk of expropriation among household recipients.

program in Nicaragua. Using household survey data and an econometric analysis, they find that household recipients increased land-secure investments by between 8% and 9% percent. They also experienced an increase in the value of the plots. Similarly, Torero et al. (2005) found that the granting of a registered title improved land values by 30 percent in Peru.

In the specific case of Ecuador, the acquisition and regularization of titles may offer additional benefits insofar possession of a registered titled is an eligibility requirement for certain government programs, such as subsidized housing loans and organic certification. Table 1 reports the complete list of outcomes and information on whether they can be measured with the data collected.

4 Evaluation Design

The initial design of the evaluation was proposed by Boucher (2010) in the Monitoring and Evaluation Plan (MEP) from the loan proposal (IDB, 2010). The original research design intended to estimate the average impact of SigTierras on different outcome variables at the household and municipality level. To do so, the MEP proposed to identify a control group similar to the treated group, except it did not have access to SigTierras. Given that randomization of the treatment and control groups, at the household or canton level, was infeasible due to the program's eligibility criteria and implementation aspects, the control group was to be selected from the cantons that were in the program pipeline, but were not selected to participate in SigTierras because of budget constraints, delays in application and negotiation process for ascension, or because cloud cover prevented timely completion of aerial photos needed to start implementation.

The sampling framework for the evaluation must take into account the project's implementation arrangements, and that a priori to the cadaster sweep it's impossible to know the tenure status of each parcel, This has direct bearing on the type of estimate of impact that can be assessed. For instance, if we were to randomly sample from the universe of cadaster parcels, we would likely survey households with parcels with tenure irregularities, but also households

whose parcels have no tenure issues and therefore are likely indifferent to treatment. This would lead to an average treatment effect (ATE) estimate. Given that our interest lies on estimating the impact that enhancing tenure security for those households with perceived land tenure issues (i.e. the average treatment effect on the treated – ATT), we pursue a strategy to include in the evaluation only those households with perceived tenure issues. Specifically, once the cadaster sweep is completed, we are able to identify parcels with perceived land tenure issues that might benefit from treatment. Nine treated cantons were selected for the impact evaluation, located in the three major regions of the country: Coast (1); Sierra (7); Amazonia (1).

To identify cantons that were similar to the 9 treated cantons, we draw from a pool of 121 eligible cantons that showed interest in SigTierras and that met the criteria to be considered eligible. Three steps were followed. First, propensity score matching (PSM) on the basis of socio-economic variables from 2001 and 2010 Census (growth rate and 2010 levels) was used to identify potential canton controls.[10] Second, 4 potential control cantons were identified for each of the 9 treated cantons. Finally, SigTierras' project implementation team corroborated matches and selected the one they thought most similar to each of the treated cantons on the basis of land tenure and agricultural characteristics. Table 2 shows the cantons in the treated and control group, and Figure 2 illustrates their geographic distribution.

Next, we proceeded to the selection of treated census tracts and households within each of the 9 treated cantons.[11] Using SigTierras administrative data on the universe of parcels that were cadastred, parcels were merged at household level, since this is our unit of analysis. Households eligible for inclusion in the impact evaluation were those with at least one parcel

[10]Variables measuring demographic characteristics (such as population, percentage of women); human capital (such as percentage of people who cannot read); level of access to services (such as percentage of households with health insurance, access to water, access to electricity, and sewage services); participation in the labor market (such as number of employed people, total people working in agricultural activities); and characteristics of the dwelling were included in the PSM. Additional variables measuring size, population density and rural density were also considered. For all these variables, the PSM included respective growth rates between 2001 and 2010.

[11]Given that the program was implemented in rural areas, the first step consisted in excluding census tracts from urban areas. Hence, only census tracts with disperse population and which have on average 80 households were chosen (this is the definition of a peripheral census tract used by INEC).

with perceived land tenure issues. For the 9 treated cantons, using the location and number of eligible households, census tracts were identified proportional to size. Census tracts with less than 12 eligible households (i.e. with at least one parcel with land tenure issues) were discarded. As it can be gleaned in Figure 3, 110 census tracts were selected as treated units from a universe of 1,224. In each selected census tract, 12 eligible households were randomly selected to be interviewed and 12 additional ones were randomly selected as back up. The benefit of this approach is that it allows us to get at the ATT. A potential drawback from this approach is that a household, having participated in the massive cadastral sweep, might have changed its perception of its tenure status and this could be reflected in changes in its agricultural practices before the baseline data is collected. Given the timing of the cadastral sweep, data collection, and the agricultural planting season, we do not anticipate this to be a source of bias. However, even if it was, the bias would play in favor of diminishing the estimated impact.

Finally, in order to find control households in the control cantons, as a first step, control census tracts were identified using Propensity Score Matching. Census tracts from the 2001 census were linked with the census tracts from the 2010 census. The selection of control census tracts was based on the three nearest neighbors to the treated census tracts. That is, 3 control census tracts were selected for each of the 110 treated census tracts. Thus, 2 alternative control census tracts were identified in case there were insufficient households available for interviewing in the first census tract selected through PSM. As reported in Figure 3, 110 census tracts were selected as control units from a universe of 754. In each control census tract, a "filter" was applied during data collection to identify control households with similar tenure issues as the treated households. Only households reporting to have at least one parcel in need of regularization were considered in the control group. Through this procedure, 2,707 households were selected as part of the impact evaluation design: 1,356 in treated cantons and 1,356 in control cantons.

5 Data Collection

Between August and November 2014 a baseline agricultural household questionnaire based on World Bank's Living Standard Measurement Study - Integrated Survey on Agriculture (LSMS-ISA) survey, was administered in the 18 cantons that were selected to be part of the treatment and control group. The field team completed 1,360 surveys in the treatment group and 1,386 in the control group. Survey respondents were mainly household heads with knowledge about: i) the land tenure status of the household's parcel and ii) the production activities of the household. The baseline questionnaire consisted of 12 modules (details about the survey are shown in Annex I) and collected information at the household and parcel level.

One section of the survey is devoted to questions about the self-reported legal status of each parcel (e.g. title registered, title unregistered, no title, or unknown tenure status) and its characteristics including land extension; the year where it was acquired or obtained; the type of land where the parcel is located (e.g. private or communal land); the infrastructure access and equipment (such as electricity system, access to roads, telephone, and irrigation); and the agricultural and livestock uses. Each parcel has a household identifier that allows to track the parcel to the household. Another module of the survey collects detailed information on characteristics of the household and its members, including socio-economic characteristics; information on savings, credits, and loans; and inclusion of household members on social and agricultural programs. Each household has a unique identifier and each parcel a unique cadastral code identifier.

The program closed in 2016 and the endline survey was administered in 2018 to allow more time for impacts to take hold. In general, evidence from land regularization and land formalization programs in Latin America and Africa suggest that visible impacts would take place after three or more years after the program was rolled out (see Deininger and Chamorro (2002); Katz and Chamorro (2003); Goldstein et al. (2018)). Thus, the endline survey was administered to the same households four years later after the baseline survey. The analysis of attrition is presented in the following section.

The survey data (from the baseline and endline) can be combined with administrative records from SIOL. Recall that all the information from individuals who contacted SIOL was stored in a unique database. The SIOL database identifies households with parcels in need of regularization (the result of the PEMs) and those that requested the program's legal support. Given that the SIOL database also contain a cadastral code, it is possible to track every parcel land owner that sought SIOL's legal support. Moreover, it is possible to merge the survey data with SIOL at the household level using the cadastral code.

5.1 Balance & attrition checks

Table 2 reports summary statistics and balance checks for household characteristics at baseline. We find significant differences between the treated and control group along a few characteristics. Specifically, control group household heads are less likely to be female, they are on average younger, more educated, and more likely to work in agriculture. Treated households have fewer members working in agriculture and are more likely to receive *Bono de Desarollo Humano*. While household size and the number of plots owned are similar, the control group has, more land, larger parcels, and more parcels without a tenure document.

Table 3 shows that the probability of completing the endline survey for control group was 91%, while treated households were 2.1 percentage points more likely to complete it (column 1). When we analyze the main causes of attrition we find no differential likelihood that the household is not found (column 2), however, treated households were 4.3 percentage points less likely to have moved (column 3), and 2.6 percentage points more likely refuse to respond (column 4).

6 Econometric Approach and Results

6.1 Identification Strategy

To test the causal impact of the SigTierras, we use a double robust estimator that combines difference-in-difference with weighting by the propensity score matching. Given that our estimand of interest is the Average Treatment Effect on the Treated (ATT), we compare changes in different outcomes associated with increase land tenure security in household that receive SigTierras (i.e. households that have land tenure issues) relative to the changes in households that do not. Our estimating equation can be written as:

$$y_{it} = \alpha + \beta\ Treated_i + \mu\ Post_t + \tau\ Treated_i * Post_t + \epsilon_{it} \tag{1}$$

where y_{it} is any outcome variable of interest for household i in period t; $Treated_i$ is an indicator that takes the value of 1 if the household received SigTierras and zero otherwise; $Post_t$ is a dummy variable indicating whether the observation corresponds to the post-program period and zero otherwise, $Treated_i * Post_t$ is an interaction between $Treated_i$ and $Post_t$, and ϵ_{it} is an error term. We cluster the standard errors at census tract level. The coefficient β measures the average pre-program difference in the outcome variable between the treatment and control group, while μ measures the time trend – or the average difference in the outcome variable in the post- versus pre-program periods. Finally, τ measures the average impact or treatment effect on the Treated (ATT) of the program.

The key identifying assumption in difference-in-difference designs is that that there are no systematic differences across treatment and control groups in terms of unobservable variables that affect the change in the outcome variable. This is the assumption of "parallel trends", which states that in the absence of the program the average change in the outcome variable of treated households would have been the same as the average change experienced by the control households. When this assumption fails, the difference-in-difference can produce biased estimates because it confounds changes unrelated to the treatment with the effect of the

intervention. In section 5 we saw that, although the control households were selected to be as similar as possible to the treated communities, treated and control households presented different demographic and socioeconomic characteristics for several of the variables considered (Table 2). The parallel trend assumption does not require observable characteristics in treatment and control groups to be balanced at baseline. However, differences in observable characteristics at baseline may likely be correlated to trends in the outcomes of interest, hence casting doubts on the validity of the DD strategy.

One popular method to reduce the bias in the DD design is to match treatment and control units on pre-treatment outcomes and then applying difference-in-difference on the matched sample (Ryan et al., 2015). While matching on outcomes corrects the non-parallel trend issue between groups, difference-in-differences get rids of any remaining outcome level differences (Lindner and McConnell, 2019). Moreover, we combine our regression model with weighting by the propensity score to obtain a double robust estimation and obtain causal treatment effects. The advantage of the double robust estimator is that it requires us to specify a model for the outcomes and another for treatment status as a function of the covariates. Moreover, with the double robust estimation only one of the two models need be correctly specified to obtain an unbiased effect estimator (Funk et al., 2011; Imbens and Rubin, 2015; Robins et al., 1995).

First, we estimate a propensity score matching model to obtain predicted probabilities of membership in the treatment group. Then, we weighted our outcomes for treated and control households from equation (1) by the inverse of the probability of treatment. To estimate the propensity score we use a logistic regression using treatment assignment as the dependent variable and a set of household characteristics as covariates. Recall that the propensity score matching is defined as the probability of an individual receiving the treatment conditional on a set of observed pre-treatment covariates. Below, we present how we specify the propensity score given a great number of covariates taking too many values. We will use the weights from the estimated propensity score to obtain an unbiased estimator of the average treatment effect

on the treated as follows:

$$\hat{\tau} = \frac{1}{n}\sum_{i=1}^{n} Z_i Y_i - \frac{1}{n}\sum_{i=1}^{n} \frac{\hat{e}(x_i)(1-Z_i)Y_i}{1-\hat{e}(x_i)} \tag{2}$$

where n denotes the number of subjects; Z the treatment assignment for each subject ($Z = 1$ denotes treatment while $Z = 0$ denotes absence of treatment); $e(x_i)$ is the estimate propensity score[12]; and Y_i the outcome under the actual treatment received for each subject.

From Austin and Stuart (2015) we know that to estimate an ATE our inverse probability weights would be given by $w_{i,ATE} = \frac{Z_i}{\hat{e}} + \frac{1-Z_i}{1-\hat{e}}$. However, for the ATT the weights are given by $w_{i,ATT} = Z_i + \frac{\hat{e}(1-Z_i)}{1-\hat{e}}$. More specifically, we have that:

$$w_{i,ATT} = \begin{cases} 1 & \text{if } Z_i = 1 \\ \frac{\hat{e}(1-Z_i)}{1-\hat{e}} & \text{if } Z_i = 0 \end{cases} \tag{3}$$

It is evident that the weights $w_{i,ATT}$ are obtained by multiplying the weights $w_{i,ATE}$ by $\hat{e}$. In general, the weights are equivalent to survey weights insofar they make more representative their respective target populations. Thus, with $w_{i,ATT}$ the treatment group is specified as the target population. The weight leaves the sample treatment group unaltered but attempts to turn the control group into a more representative sample of the treatment group (Morgan and Todd, 2008).

Under the assumption that, conditional on X_i the changes in the outcome variables in the treatment and control group would have been the same, absent the program, this methodology would yield an unbiased estimate of the average treatment effect, $\hat{\tau}$. However, this estimator is very sensitive to estimated propensity scores that are either close to zero or close to one as they would assign very high weight to the corresponding observations. To minimize this issue, we follow Imbens et al. (2009) and restrict the sample to the set of households for which the propensity score belongs to the interval $[\hat{\alpha}, 1-\hat{\alpha}]$, where $\hat{\alpha}$ is chosen based on the distribution

[12]The propensity score for household i with observable characteristics x_i, is defined as the conditional probability of receiving SigTierras given the household baseline covariates as: $e(x_i) = Pr(Z_i = 1|X_i = x_i)$

of the propensity scores, with the objective to minimize the variance of the estimated average treatment effect.[13]

6.2 Estimation

To estimate the propensity score we need to choose a set of household characteristics at baseline as our explanatory variables. Our goal is to balance measured covariates between households who were treated and those that were not, thus making it likelier that any difference in effect is due to the Program. Finding the most suitable specification for the propensity score is not trivial. For instance, suppose that we think that the number of parcels under illegal tenancy and the average number of years that a household has been settled in a parcel might affect both the treatment status and some outcome of interest. Therefore, one strategy could be to compare households with the same values for these variables in both the treatment and the control group. However, as we add more covariates into the propensity score it becomes more difficult to find the same values for each household across groups. In general, covariate selection is guided by tradeoffs between variables' effects on bias (distance of estimated treatment effect from true effect) and efficiency (precision of estimated treatment effect) (Garrido et al., 2014). In other words, as we make the model more "flexible", by adding more covariates into the propensity score estimation, we improve its predictive power (reduce bias) at the cost of reducing the area of the "common support", that is, the area of overlap for the range or propensity scores across treatment and control households. As the area of common support narrows, so does inferences about the treatment effects for treated households for whom there is no longer valid comparison units.

The key is to reduce bias by including as many variables as possible related to both the treatment and the outcome while limiting the cost of sacrificing efficiency in the treatment

[13] Specifically, $\hat{\alpha} = \frac{1}{2} - \sqrt{\frac{1}{4} + \frac{1}{\hat{\gamma}}}$ and $\hat{\gamma}$ is the solution of the following equation:

$$\gamma \sum_i \mathbb{1}_{(\hat{e}(x_i)(1-\hat{e}(x_i))^{-1} < \gamma} = \sum_i \frac{1}{\hat{e}(x_i)(1-\hat{e}(x_i)} \mathbb{1}_{(\hat{e}(x_i)(1-\hat{e}(x_i))^{-1} < \gamma} \tag{4}$$

estimates. Based on our survey questionnaire of close to 300 questions, we decided to create 102 covariates that we believe could affect both the treatment status and most of the outcomes of interest. However, we still need to decide which confounders to include in our propensity score model. Moreover, this process should not only include linear terms but also other higher order terms such as polynomials.

Thus, we follow the methodology proposed by Imbens and Rubin (2015) to select the covariates to use in the propensity score model. A description of this can be found in Appendix A. Following this methodology, our final propensity score model has a total of 33 covariates. Figures 4 and 5 show the performance our propensity score model. Figure 4 shows fair evidence of overlap between treated and control units. Figure 5 shows that before matching, there were control units for the treatment units for some of the values of range of the propensity score. The right panel of Figure 5 shows that after matching, the distribution of units across groups for the propensity score values improves dramatically. Once we trim the sample to keep the households for which the propensity score lies in the interval $[\hat{\alpha}, 1 - \hat{\alpha}]$, we are left with 5127 observations. Table 4 reports the resulting balance tests adjusted by propensity score weighting trimming the sample. In spite that the reduction in the number of observations is small there is an improvement in terms of balance with respect to the unadjusted sample (Table 2) as there are no statistical differences between the two samples for the variables considered.

6.3 Results

First, we test whether the program provided certainty of property rights to the households that received SigTierras, which was stated as one of the program's specific objectives. We expect that household perceptions on land tenure security, measured by an increase in the number of owned parcels and the number of parcels with title or by a decrease in the number or parcels with no title, would have been ameliorated after the end of the program. As it can be seen in Table 5 we found no evidence that treated households increase the number of owned parcels (column 1), or the number of parcels with title (column 3), relative to control

households (column 1). By the same token, we found no evidence that treated households decrease the perceived number of parcels without title (column 2). These results are somewhat surprising, however, we assert that one possible explanation is the fact that during the phase design SigTierras did not establish the necessary legal and administrative mechanisms to ensure that the parcels under informal legal status would regularize their tenancy, thus, maintaining their perceptions on land tenure security.

Recall that for each parcel in the program's area, SigTierras generated cadastral digital maps with community assent to ensure land delimitation. For land owners, digitization of maps could serve as an alternative source of property documents that could prevent disagreements or land disputes. Thus, the frequency of land conflicts might decrease as a consequence of the program. At it can be seen in columns (4) and (5) from Table 5, the direction of both estimands is the expected: less conflicts after five years and one year in treated households relative to control households. Nevertheless none of the coefficients is statistically different than zero.

As we stated in section 3, we would expect that the program had a positive effect on land inputs investments including seeds, pesticides, fertilizers, and hired labor. As we can observe in Table 6, we find no evidence that treated households increase their probability of investing in farm inputs due to the program. Treated households have a higher probability of purchasing seeds and pesticides relative to control households (columns 1 and 2), albeit none of them achieve decent statistical significant levels. Contrarily, treated households have a lower probability of purchasing fertilizer and paid for labor relative to the control group (columns 3 and 4), albeit only the former is statistically significant.

In spite that we find no effect of the program on perceptions of tenure security, the reduction of land conflicts, and the increase in land investments, we finally test whether treated households increase their income relative to control households. Table 7 reports the effect program on the main income sources: agricultural wages, crop agriculture, livestock agriculture, self employment, and transfers (columns 2-6). It also reports the effect on total household income, the sum of all the sources mentioned above (column 1). As we can see, we find strong evi-

dence of an increase in total household income and agricultural wages. The program increases household total income by 755 dollars, an almost 52% increase relative to the control group. By the same token, the program increases agricultural wage income by 732 dollars, an almost 30% increase relative to the control group. The rest of the income variables do not achieve decent significant levels, thus, we find no effects on crop, livestock, self employment income and transfers.

7 Conclusions

In spite of a strong theory of change on the channels and directions through which land tenure security should affect agricultural and welfare outcomes, the empirical work about the potential benefits from regularization programs remains inconclusive. In this paper, we used a double robust estimation to assess the impact of a land regularization and administration program in rural Ecuador, SigTierras.

SigTierras was a program based on demand that was implemented between 2010 and 2018 in 47 cantons from rural Ecuador. Its objective was to enhance legal certainty with regard to property rights through the regularization of rural land. Given that a cadastral sweep was performed during the implementation phase, the prospective evaluation identified the households in need of receiving the program. Thus, treated households were selected based on their tenure issues and their need for regularization, while control households were identified by first selecting control cantons and census tracts and then by using a filter that guaranteed their similarity to treated households. Due to the features of the household selection process, we developed a prospective evaluation that could allow us to estimate an Average Treatment Effect on the Treated, that it to say, what was the effect of SigTierras on those households that actually needed the treatment.

Our findings suggest that the program had no effects in terms of perceptions of tenure security, the reduction of land conflicts, and the increase in land investments. Strikingly, we find a positive and sizable effect of SigTierras on agricultural wages and total household income.

Some possible explanations for the limited effectiveness of the program could be that, during the design phase, SigTierras did not establish the proper legal and administrative mechanisms to ensure that parcel with land tenure issues could regularize their tenancy. Also, during the most part of the execution, SigTierras focused on providing digital certainty and delimiting parcel boundaries through cadastre maps, however, this did not translate into higher tenure security. Finally, as it has been stated in recent literature, it is possible that in many rural households that were intervened through SigTierras, informal property rights substituted effectively for formal property rights, thus, lowering the value of land regularization.

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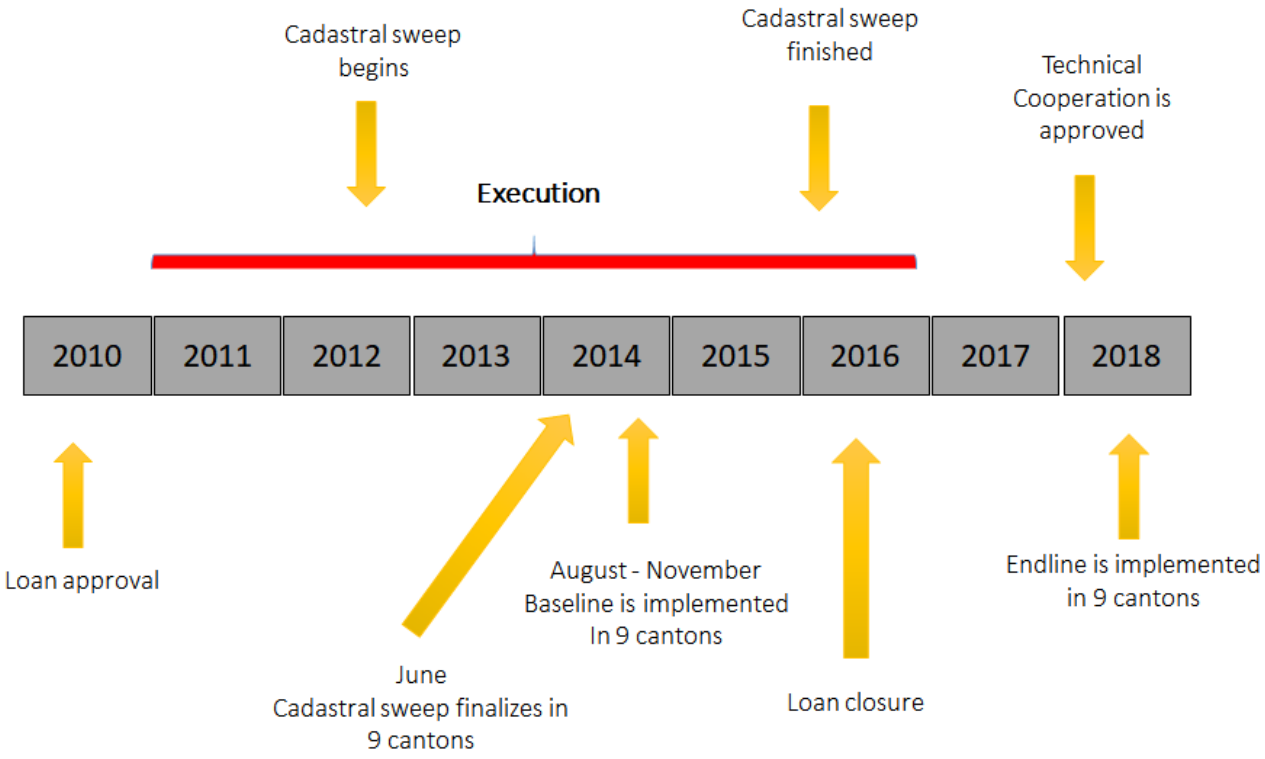
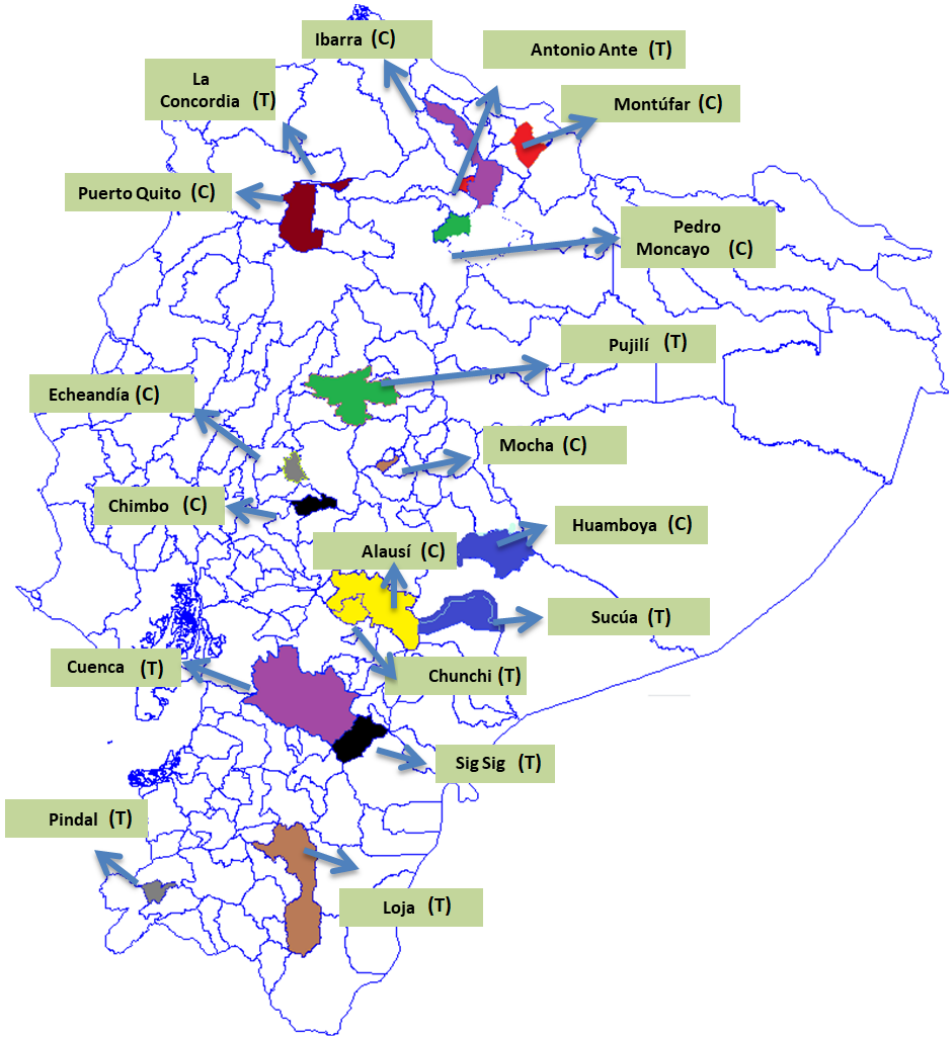
A Figures

Figure 1: Timeline of SigTierras



Notes: the Project Completion Report was prepared during 2018 but approved until 2019

Figure 2: Project map



Notes: the figure shows the treatment (T) and control (C) cantons selected for this study. Cantons with the same color were identified as "similar" through the propensity score matching exercise.

Figure 3: Distribution of treated and control households by canton and census tract

Intervention					Control				
Province	Canton	Census tract universe	Sample		Province	Canton	Census tract universe	Sample	
			Census tracts	Number of households				Census tracts	Number of households
Chimborazo	Sigsig	141	16	192	**Chimborazo**	Chimbo	42	16	192
Santo Domingo	La Concordia	29	11	132	**Pichincha**	Puerto Quito	52	11	132
Morona Santiago	Sucúa	34	10	132	**Morona Santiago**	Huamboya	33	10	132
Pichincha	Antonio Ante	29	9	132	**Carchi**	Montúfar	58	9	132
Azuay	Cuenca	683	20	240	**Imbabura**	Ibarra	127	20	240
Pichincha	Pedro Moncayo	68	11	132	**Cotopaxi**	Pujilí	245	11	132
Loja	Loja	176	11	132	**Tungurahua**	Mocha	27	11	132
Chimborazo	Chunchi	42	11	132	**Chimborazo**	Alausí	145	11	132
Loja	Pindal	22	11	132	**Bolívar**	Echeandía	25	11	132
Total		**1.224**	**110**	**1,356**	**Total**		**754**	**110**	**1,356**

B Tables

Table 1: Outcome variables

	Outcome	Indicator	Can it be measured?
	Final Outcomes		
1	Total household income	Total income	Yes
2	Household expenditure	Total expenditure from a household	No data
3	Value of agricultural production	Value of production	Yes
4	Net agricultural income	None	No data
5	Value of household wealth	Not specified in the MEP	No data
	Intermediate Outcomes		
6	Farm yield	Not specified in the MEP	Yes
7	Farm expenditures per-hectare	Expenditure per hectare in seeds, pesticide, fertilizer, labor	Yes
8	Land transactions	Rentals, sales, inheritance, mortgage	Few data
9	Access to credit (1)	Participation in formal and informal credit markets	Yes
10	Access to credit (2)	Household rationing status in formal and informal credit markets	No data
11	Land tenure security	Perception of different aspects of tenure security	Yes
12	Land conflicts	Frequency and intensity of land conflicts	Yes
13	Investment in fixed and farm assets	Farm investments, purchased seeds, pesticides, fertilizer, and paid for labor	Yes
14	Participation in government programs	Participation in Bono de la Vivienda and Organic certification	Partially

Notes: The list of outcomes follows the Monitoring and Evaluation Plan (MPE). The symbol (*) indicates that this outcome was included additionally based on the theory of change.

Table 2: Summary Statistics and Balance - Household level

	Control (1)	Treated (2)	(1) vs. (2) (3)
HH head is female	0.20	0.25	-0.06***
	(0.01)	(0.01)	(0.01)
HH head age (years)	50.69	53.19	-2.50***
	(0.32)	(0.33)	(0.46)
HH size	4.32	4.29	0.03
	(0.04)	(0.05)	(0.06)
HH head has primary	0.55	0.54	0.00
	(0.01)	(0.01)	(0.01)
HH head ethnic	0.14	0.13	0.02**
	(0.01)	(0.01)	(0.01)
N of dependents	3.41	3.42	0.00
	(0.04)	(0.04)	(0.06)
N adults in agriculture	1.73	1.59	0.15***
	(0.03)	(0.03)	(0.04)
Members in productive age	2.24	2.27	-0.03
	(0.03)	(0.03)	(0.04)
Receives *Bono*	0.58	0.62	-0.04***
	(0.01)	(0.01)	(0.01)
N plots owned	2.45	2.44	0.01
	(0.03)	(0.04)	(0.05)
N plots with irrigation	0.34	0.24	0.10***
	(0.02)	(0.01)	(0.02)
Total area (Ha)	8.73	26.53	-17.80
	(3.01)	(14.24)	(14.69)
Avg plot tenure (years)	14.64	18.41	-3.77***
	(0.25)	(0.28)	(0.37)
Owned equipment	0.72	0.76	-0.03***
	(0.01)	(0.01)	(0.01)
N animals	5.64	7.56	-1.92**
	(0.65)	(0.62)	(0.89)
N	2573	2627	5200
Joint F-Stat	14.10		
P-value	0.000		

Notes: * $p < .10$,** $p < .05$, *** $p < .01$.

Table 3: Attrition Checks

	Endline Survey Result			
	Completed	Not found	Moved	Rejected
	(1)	(2)	(3)	(4)
Treated	0.021**	-0.001	-0.043***	0.026***
	(0.010)	(0.005)	(0.006)	(0.006)
Mean Control	0.910	0.016	0.045	0.016
Observations	2707	2707	2707	2707

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$.

C Propensity Score Matching

Figure 4. Propensity Score

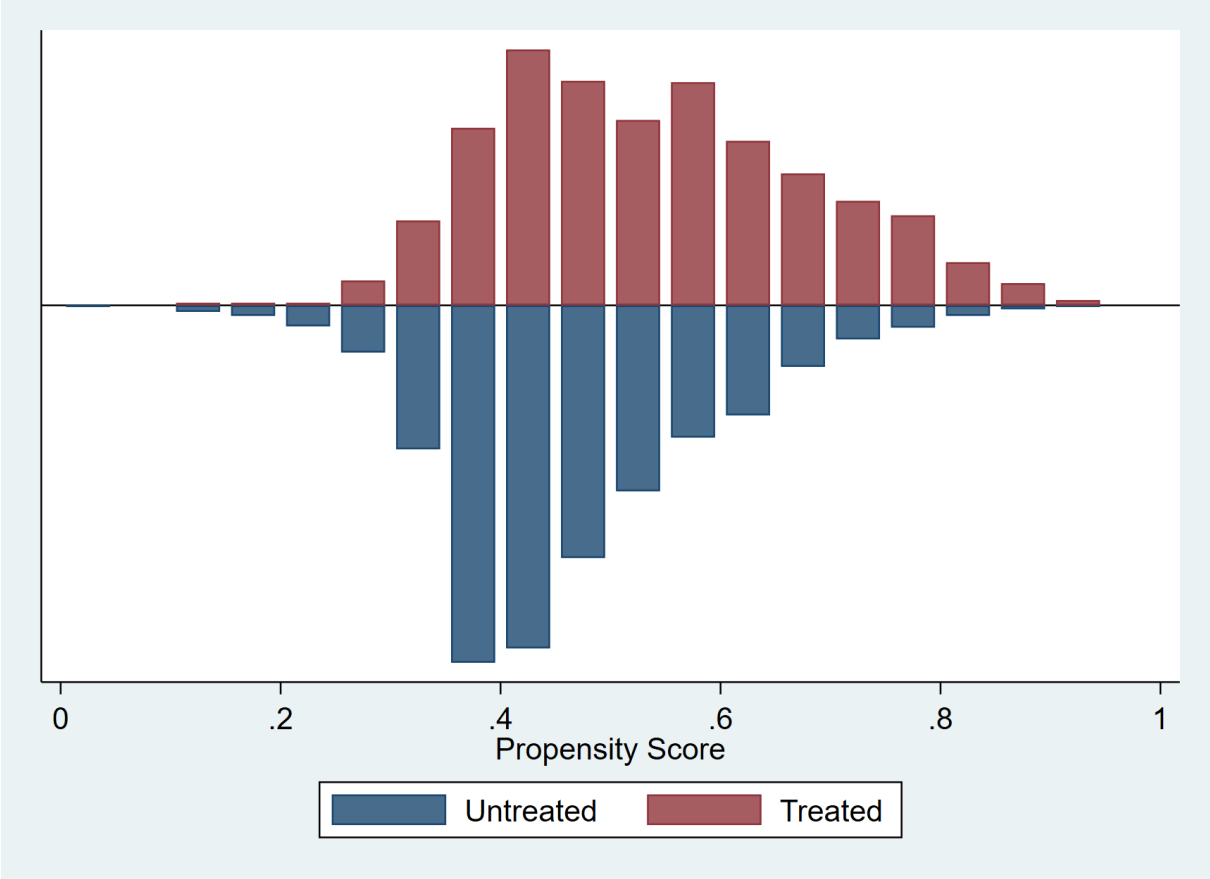


Figure 5. Distribution of treated and control units before and after matching

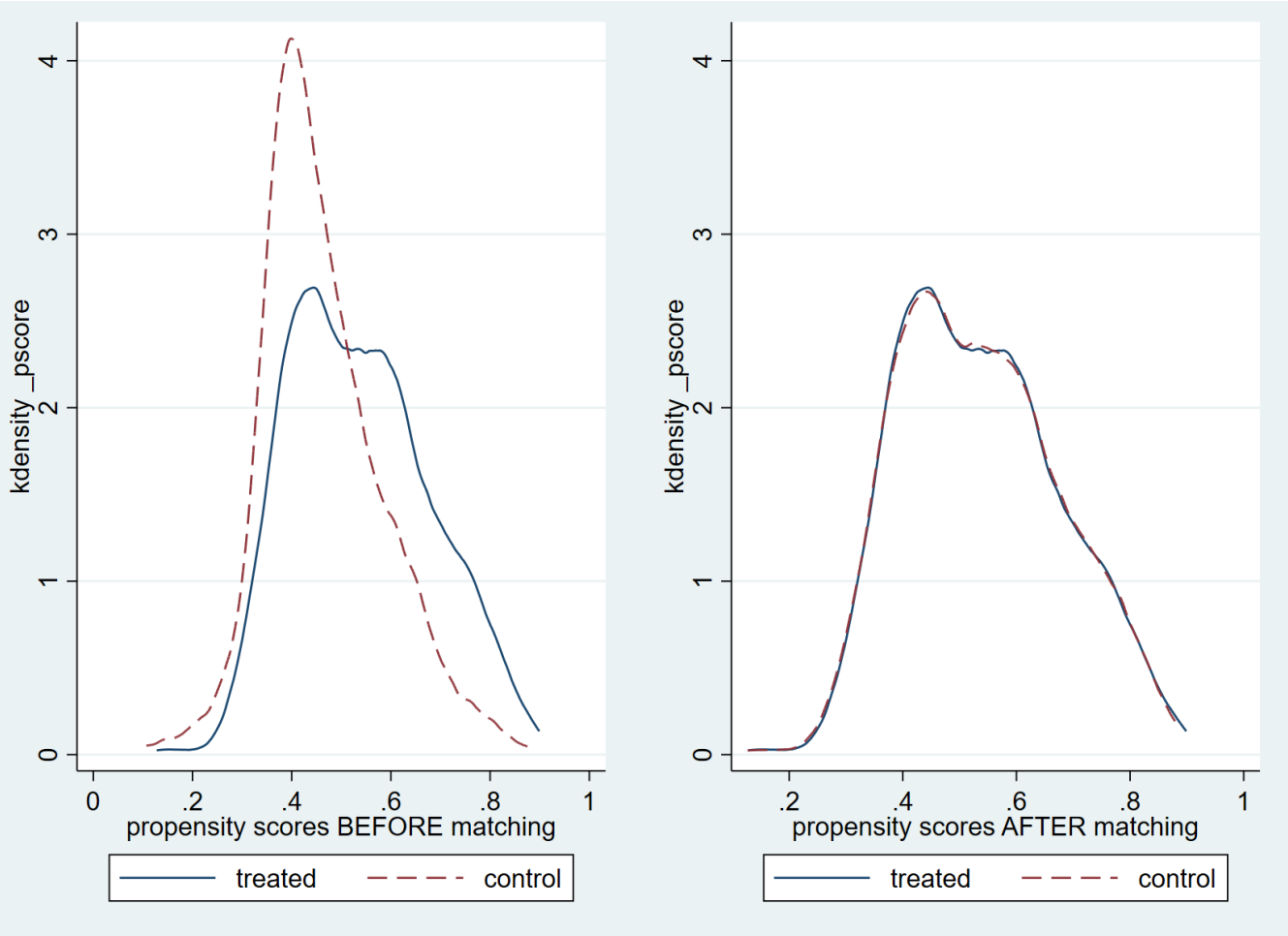


Table 4: Balance checks adjusted by PSM

	Control	Treated	(1) vs. (2)
	(1)	(2)	(3)
HH head is female	0.23	0.22	0.01
	(0.01)	(0.01)	(0.01)
HH head age (years)	52.01	51.83	0.18
	(0.33)	(0.35)	(0.48)
HH size	4.26	4.35	-0.09
	(0.05)	(0.05)	(0.07)
HH head has primary	0.56	0.54	0.02
	(0.01)	(0.01)	(0.01)
HH head ethnic	0.14	0.13	0.00
	(0.01)	(0.01)	(0.01)
N of dependents	3.37	3.46	-0.09
	(0.04)	(0.05)	(0.06)
N adults in agriculture	1.64	1.70	-0.06
	(0.03)	(0.03)	(0.04)
Members in productive age	2.25	2.26	-0.01
	(0.03)	(0.03)	(0.04)
Receives *Bono*	0.61	0.59	0.02
	(0.01)	(0.01)	(0.01)
N plots owned	2.44	2.42	0.02
	(0.04)	(0.03)	(0.05)
N plots with irrigation	0.28	0.27	0.01
	(0.02)	(0.01)	(0.02)
Total area (Ha)	22.30	7.62	14.68
	(11.79)	(2.31)	(12.02)
Avg plot tenure (years)	16.78	16.43	0.35
	(0.26)	(0.33)	(0.42)
Owned equipment	0.75	0.74	0.01
	(0.01)	(0.01)	(0.01)
N animals	5.07	5.22	-0.15
	(0.29)	(0.48)	(0.57)
N	2544	2583	5127
Joint F-Stat			1.68
P-value			0.048

Notes: * $p < .10$,** $p < .05$, *** $p < .01$.

Table 5: Land Tenure Security and Land Conflicts

	Perception of number of plots			Intensity of conflicts	
	Owned	Without Title	With Title	In 5 years	In one year
	(1)	(2)	(3)	(4)	(5)
Treated	0.036	-0.051	0.008	-0.022	-0.010
	(0.079)	(0.071)	(0.084)	(0.013)	(0.008)
Post	0.421***	-0.304***	0.300***	0.010	0.017
	(0.035)	(0.040)	(0.035)	(0.016)	(0.015)
Treated*Post	-0.032	0.199***	-0.201***	-0.003	-0.009
	(0.060)	(0.044)	(0.039)	(0.019)	(0.016)
Mean Control Baseline	2.220	1.174	0.890	0.108	0.077
Observations	4914	4914	4914	4914	4914

Notes: The standard errors in parenthesis account for potential correlation within census tracts. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 6: Investment in farm inputs

	Paid for input			
	Seeds	Pesticide	Fertilizer	Hired Labor
	(1)	(2)	(3)	(4)
Treated	0.152***	-0.094***	-0.012	0.004
	(0.041)	(0.033)	(0.031)	(0.032)
Post	-0.031**	-0.174***	-0.105***	0.147***
	(0.015)	(0.020)	(0.023)	(0.024)
Treated*Post	0.026	0.055	-0.016	-0.097***
	(0.029)	(0.034)	(0.035)	(0.025)
Mean Control Baseline	0.230	0.436	0.346	0.313
Observations	4914	4914	4914	4914

Notes: The dependent variables from these models are dummies. The standard errors in parenthesis account for potential correlation within census tracts. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 7: Household Income

	Total Income (1)	Wages ag (2)	Crop (3)	Livestock (4)	SE (5)	Transfers (6)
Treated	-116.110	-262.161***	14.931	91.652	-6.124	45.591
	(150.121)	(83.858)	(101.456)	(56.944)	(39.390)	(39.328)
Post	1840.383***	1962.116***	-19.975	-23.780	-18.598	-59.378
	(256.124)	(220.747)	(54.694)	(49.623)	(35.105)	(43.272)
Treated*Post	755.772**	732.486***	96.701	-50.855	15.078	-37.637
	(308.774)	(256.979)	(96.761)	(54.966)	(57.718)	(64.739)
Mean Control Baseline	1556.850	563.684	472.862	49.182	44.865	426.257
Observations	4050	4050	4050	4050	4050	4050

Notes: The standard errors in parenthesis account for potential correlation within census tracts. * $p < .10$, ** $p < .05$, *** $p < .01$. All values are expressed in USD. Households with "Total income" above 95th percentile or below the 5th percentile at either baseline or endline are dropped. SE = Self Employment.

Appendix A. Methodology to select the covariates for the Propensity Score Matching model

We first identify a set of covariates to include linearly in the log odds radio of the propensity score. These variables are selected upon their theoretical association with the treatment status and not so much upon their statistical relationship with the treatment and outcome of interest. Based on section 4.1.3, we use variables such as the average number of years that a household has been settled in their parcels, whether a household receives the subsidy scheme of *Bono de Desarrollo Humano*, the number of members in the household in productive age, and the total area of the parcels that belong to the household. These are relevant characteristics from a household in as much as they were key questions on the "filter" questionnaire (*Encuesta filtro*) that were applied during the selection of the control group.

Now that we have defined 6 variables for the baseline model, the next step is to choose a second set of variables. From the remaining 102 variables, we add one variable at a time to the logistic regression, each time checking whether we wish to add another covariate or not, and if so, which one. To do so, for each specification we calculate the likelihood ratio statistic for the test of the null hypothesis that the coefficient on the additional covariates is equal to zero. Thus, in this manner, in our first iteration we run 102 logistic models. For each model, we calculate the likelihood ratio statistic, taking as reference the baseline model with 6 variables. Thus, in this manner, we pick the variable that leads to the higher improvement in the likelihood function. In the second iteration, we run 101 logistic models. Again, we calculate the likelihood ratio statistic, but now our baseline model will have 8 variables. We keep doing this until the Maximum Likelihood Ratio Statistic obtained is lower than 1. In our final step, we choose a third set of variables, but in this case we use second order terms and all the possible interactions. We apply the same procedure to select which variables to include in the propensity score model. In this case, however, we stop adding covariates when the maximum Likelihood Ratio Statistics obtained is below the threshold of 2.71.