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 difference in difference estimation combined with a propensity score matching approach, our findings suggest that the program had a positive effect on total household income, wage income, and on investments on inputs such as seeds and pesticides. We find no evidence that treated households increased their agricultural production, credit access and remittances, farm investments and expenditures per hectare. Finally, there is no evidence that the program had any positive effect on perceptions of land tenure security.

JEL classifications: Q15, O13,

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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 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. From a policy perspective the primary question that this paper seeks to answer is the following: What was the impact of SigTierras on households under informal tenancy in different welfare and agricultural indicators? To answer this question we follow a combination of empirical strategies. First, we use propensity score matching to identify the households from the control group that were more similar to the treatment group in terms of different household characteristics. 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 inverse probability weights 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 variables such as household income, agricultural production, expenditure in farm inputs, access to credit, and investments. 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 geo-referenced digital maps (via Geographic Information Systems), however, this did not traduce 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

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

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.

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 granting and maintaining property rights. The government of Ecuador identified this situation of informal property rights and weak institutions as a significant constraint to rural development. Therefore, in 2010 the government of Ecuador launched the SigTierras program (*Sistema Nacional de Información y Gestión de Tierras e Infraestructura Tecnológica*) 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 implemented 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 participating 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. In spite that a total of 171 cantons showed interest in SigTierras only 47 were chosen to participate.[2] The three main reasons for not being included to participate in SigTierras were the following: i) budget constraints; ii) delays in the application and negotiation process; and

[2]Cantons are the second-level subdivisions of Ecuador below provinces and are equivalent to municipalities. There are 226 cantons in the country. Cantons are further subdivided into parishes, which are classified as urban or rural

iii) cloud covering that prevented the completion of aerial photos. The last reason was an exogenous factor that was completely out of the hands of cantons authorities, however, this step was fundamental to begin with the implementation of the program.

Once the *Convenio* was signed, the first step of the execution consisted in taking aerial photographs, orthophoto maps, and begin the implementation of the cadastral sweep (*barrido catastral*). 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 of 1:5000 scale. All the information from the cadastral sweep was validated by the owner or occupant and in the absence of these ones it was validated with neighbors. All the information from the cadastral sweep was uploaded on a region central location and went to a process of quality control. Once the data went to the proper relevant "checks", a map for each parcel was created with the following information: 1) map of the parcel and date that it was issued; 2) parcel information (location, occupier, name and dimensions); and 3) names of bordering neighbors on all sides.

Once the cadastral sweep and mapping was finalized, 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 undertook 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 part of the file. Parcel tenants who agreed

with the results from EPR, 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] Thanks to the EPR it was possible to identify the land owners or occupiers who have land that needed a title or land with a title that needed formalization. Owners of plots who lacked a *titulo perfeccionado* were encouraged to seek legal advice at the EPR.

Given that the cadastral sweep along with the EPR 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 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**:

    - 206,909 square kilometers of land implemented orthophoto maps with a 1:5,000 scale, achieving 100% of its target.
    - 55 cantons were cadastred and assessed according to the Ministry of Agricuture (MAGAP) and the PRAT methodology, surpassing the goal of 50 cantons.

2. **Reorganization of and investment in Cadastres and Property**:

    - 59 cantons were integrated into the National Land Information System (SigTierras), which allowed to electronically link the files of the cadastre and register offices, used georeferenced information based on registers keyed to the property, and allowed to easy updates on property information. This surpassed the goal of 50 cantons that was set at the beginning of the project.

[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 EPR had the mandate of reviewing each of the possible complaints during or after the EPR.

3. **Regularization of land titles**:

   - A total of 163,580 parcels initiated the regularization process, which accounts for 96% of the parcels. Originally, 170,000 parcels were supossed to begin a regularization process to obtain a *título perfeccionado*.
   - Only 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 indicator.
   - According to the cadastral sweep a total of 417,652 had parcels with illegal tenancy, thus, the program regularized less than 10% of the potential demand, that is, parcels with tenure issues.

To have clearer picture about the implementation of SigTierras, figure 1 shows the activities that took place since the first year of approval, 2010, until 2018.

2.2 SIOL Component

Given that during the implementation phase, SigTierras was not achieving the goal of regularizing 170,000 parcels, the executing agency decided to implement a free legal plan to encourage the take-up of parcel owners under informal tenure status. This was an ad-hoc mechanism that was not conceived during the design of the program, and was 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 seek it during the EPR. The second one was a called center that provided support based on demand via telephone calls. And the third one was at SigTierras offices located in each municipality. SIOL also implemented an on-line database that collected information for all individuals that seek legal advice.[4]

[4]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

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. All the information from individuals who contacted SIOL was stored in a unique database. Given that the SIOL database also contain a cadastral code, it is possible to track every parcel land owner that sought legal advice. Moreover, using this data it is possible to contrast the potential demand for SIOL (the households that must have follow up with SIOL to regularize their piece of land) with the true demand (those households that actually take up the legal program).

In spite of the efforts to increase the regularization of land with tenure issues, only 61,159 beneficiaries were attended through SIOL. This number evidenced the low demand for SIOL given that number of parcel under informal legal tenancy. In other words, the take-up for SIOL in the universe of cantons that were part of SigTierras was as low as 14%.

3 Theory of Change & Expected Outcomes

According to economic theory, strong and well-defined property rights and the formalization of a land title can raise the productive efficiency, income, and well-being of agricultural households. Based on this framework, the evaluation design provided by Boucher (2010) before the beginning of the project, proposed a list of outcomes that were to be measured at the end of the program. These outcomes include the following: total household income; value of agricultural production; net agricultural income; and value of household wealth. In addition, the evaluation identified other intermediate outcomes including farm yield; per-hectare farm expenditures on key inputs (such as fertilizers) and land preparation; household 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; frequency and intensity of land

parcel (if any), and the judicial determination of the competent authority on land tenure regularization

conflicts;household participation in government programs including *Bono de la Vivienda*; 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 to look at given the way that SigTierras was implemented. To begin with, the ownership of a secure land title may reduce the threat of conflict, eviction, or expropriation. Given that the core of SigTierras revolved around land demarcation activities, such as mapping and consensual delimitation of agricultural lands, we expect that the program served to settle land conflicts among land right tenants. Thus, the frequency of land conflicts such decrease as a consequence of the program.[5] 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 show little impact. For example, Torero et al. (2005) found no statistical relationship between the receipt of a land title and access to credit by the use of title as collateral.

Titling might also lower transaction costs in the land market (e.g. rental, sales, mortgage, inheritance, etc.) by clarifying rights and making them transferable. Furthermore, tenure security might allow rural agricultural households to capture the benefit of investments in the land. This is because the title shall give them greater confidence that their investments will be capitalized into the price they would receive in the land market. For example, Deininger and

[5]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.

Chamorro (2002) investigate investments and income effects of massive of land regularization 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 as well as an increase in the value of the market value of the plots. By the same token, Torero et al. (2005) found that the receipt of registered titles improved land values by 30 percent.

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 measured based on 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. The original idea was to estimate the impact of the overall program. In other words, 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 which did not have access to SigTierras. Given that randomization of the treatment and control groups was infeasible because of the program eligibility criteria, the control group would be selected from the cantons that were in the program pipeline. More specifically, from the group of cantons that were not selected to participate in SigTierras because of the application process, administrative delays to sign-off the *Convenio* or because of cloud covering issues. Importantly, though, is that the PME mentions that the sample frame for the impact evaluation could be stratified into two groups: 1) those households that possessed a *título perfeccionado* and those that do not, and 2) only those households that did not possessed a *título perfeccionado*. In other words, the PME pointed out that it was pivotal to differentiate between those treated households who actually need to regularize their land and those who also partic-

ipated in SigTierras but did not have any land tenure issues. However, the PME proposed the estimation of an Average Treatment Effect (ATE), instead of the Average Treatment Effect on the Treated (ATT). We, however, believe that this is misleading because precisely the cadastral sweep allowed us to identify the treated households with parcels in need of regularization. Therefore, it is possible to estimate the causal impact of SigTierras on the treated households with land tenure issues and not on only on the full set of beneficiaries.

4.1 Selection of Sample for the Evaluation Design

For the evaluation design it was pivotal to identify the parcel owners within the cantons under informal legal status. Because the cadastral sweep took a considerable amount of time to finish, only cantons were it was almost completed could be considered. More specifically, the selection of the sample for the impact evaluation followed a three step approach, which consisted in selecting cantons, then census tracts within the cantons, and finally the households within the census tracts.

The principal challenge in the identification of the causal impact of SigTierras is to identify a control group that did not have access to the program but that had similar characteristics to the treated group. Therefore, the sample selected to carry out this evaluation study included (1) a group of households with land tenure issues that was exposed to SigTierras program (treated group); and (2) a control group with land tenure issues that were not exposed to the program but that had similar to the treatment group according to relevant socioeconomic characteristics.

4.1.1 Step 1: Selection of cantons for the Impact Evaluation

Treated cantons: To identify the cantons to be included in the treatment group two criteria were used. Recall that the sample selection for the impact evaluation was based upon the idea of identifying the parcel owners with land tenure issues. Only by doing so would it be possible to estimate the quantity of interest, that is, the Average Treatment Effect on the Treated.

Thus, in this manner, there were two main criteria to select the cantons on the treated group. First, given that the implementation of the program did not follow a strict fieldwork timeline, only cantons where the cadastral sweep was almost finished, or where at least 50% of the parcels were swept, between July 2013 and June 2014 were chosen. Again, it was pivotal to carry out the impact evaluation to select places in which there were a mass of swept parcels as this would allow to quantify the percentage of households that did not have a title or that needed to formalize one of their parcels. The other criterion involved selecting the cantons that were representative of three of the four most important regions of the country: *Sierra*, *Costa*, and *Amazonia*. As of June 2014, only 9 cantons had almost finished the cadastral sweep and were representative of the three most important regions of the country: Cuenca, SigSig, La Concordia, Sucua, Antonio Ante, Pedro Moncayo, Loja, Chunchi, and Pindal. These were the cantons considered for the treatment group.

Control cantons: The pool of eligible control cantons consisted in the 105 cantons that showed interest in the program and were in the process of signing an agreement or which wanted to enroll but did not have the financial counterpart. To identify the cantons that were similar to the 9 treated cantons, three steps were taken. First, potential cantons were selected based on socioeconomic characteristics similar to the treatment cantons. This was done using *Propensity score matching* (PSM), which was applied to a set of indicators obtained from the 2001 and 2010 Population and Housing Census (*Censo de Población y Vivienda*). 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 taken into account. For all of these variables, the PSM included respective growth rates between 2001 and 2010.

In a second step, and thanks to the PSM, 4 potential control cantons were identified for each

of the 9 treated cantons. Finally, the Program Director from the Executing Unit corroborated the matches based on her knowledge from agricultural characteristics, and selected a final set of control cantons for the treated cantons. Table 2 shows the cantons in the treated and control group, and figure 2 illustrates the geographic distribution of them.

4.1.2 Step 2: Selection of Census Tracts

In order to identify the treatment and control group at the household level, it was necessary to select the primary sample units with the lowest level of geographical representation, in this case at the census tract level. 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).

Treated census tracts To identify the census tracts that were part of the treated groups, different sampling methods were applied. A stratified two stage cluster sampling method was first applied. Cantons were stratified and census tracts were used to form the clusters. After that a multistage sampling was applied. In this case, within each canton, census tracts were selected according to the probability proportional to size sampling (PPS) using the number of households with one or several parcels that had legal tenancy issues according to the information that was collected during the cadastral sweep (i.e. did not have a registered title, did not have a title, or the status of the parcel was not clear). Census tracts with less than 12 cases with eligible households (with land tenure issues) were discarded. At it can be gleaned in figure 2, 110 census tracts were selected as treated units from a universe of 1,224.

Control census tracts: Given that it was not possible to have a set of households with some type of tenancy issue that were part of the control group, PSM was used to find the census tract controls more similar to the treated census tracts and that could have taken part of SigTierras. To define the variables that were part of the PSM, the Institute for Statistics

of Ecuador (INEC) shared a database which allowed to link the census tracts from the 2001 census with the census tracts from the 2010 census. Unlike the methodology chose to define the group of control cantons, in this case the selection of control census tracts was based on the three closest nearest neighbors to the treated census tracts. In other words, 3 census tracts of control were selected for each of the 110 treated census tracts. Therefore, the census tract in the control group that was ranked second or third serve as an alternative unit in case that there were missing households to be interviewed in the first census tract that the PSM reported. At it can be gleaned in table 2, 110 census tracts were selected as treated units from a universe of 754.

4.1.3 Step 3: Selection of Households

Once the treated cantons and census tracts were selected, the final sampling unit was the household. Households were selected based on a random sampling process. From the list of eligible households that were obtained through the cadastral sweep (*Ficha Predial*) in the 110 treated census tracts, two lists were selected for each census tract. The second list served as a replacement in case that the number of households that were to be surveyed using the first list was not completed. In each census tracts, 12 household were interviewed. To obtain the households in the control group, a short-survey was implemented in the census tracts obtained from the PSM. The survey served as a filter to determine the households with land tenure issues: only households responding to have a least one parcel in need of regularization were considered in the control group. In total, 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

After the cantons participating in the impact evaluation were selected, a process of data collection began. Between August and November 2014 a baseline agricultural survey was applied in the 18 cantons that were part of the treatment and control group. The field team visited

a total of 9,701 dwellings and completed 1,360 surveys in the treatment group and 1,386 in the control group. However, 4 additional surveys were completed in the treatment group while in the control group 26 additional surveys were completed. In general, survey respondents were mainly household heads with knowledge about: i) the land tenure status of her parcel and ii) the productivity activities from 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, no title, title unregistered or unknown tenure status) and its characteristics including land extension; type of land where the parcel is located (e.g. private or communal land), the form of acquisition of the plot (e.g. by heritage or occupation); the infrastructure and equipment (such as electricity system, access to roads, telephone, and irrigation); and the agricultural and livestock uses. Importantly, each parcel has a household identifier that allows to track which parcel belongs to which household. This is important because there might be one or many parcels associated to the same household. Another section of the survey collects detailed information on characteristics of the households the parcels belong to. For example, the survey gathered information on household demographics (e.g. gender of household head); socio-economic characteristics (e.g. primary and secondary occupation, educational characteristics, income levels); information on savings, credits, and loans; and inclusion of household members on social and agricultural programs. Each household has a unique identifier and a unique cadastral code identifier.

The program closed in 2016 but the endline survey was not applied until 2018. The reason to did so was because applying the endline survey in 2016 would probably have been too premature. 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)). Hence, it was deemed necessary to wait a little longer until the

likely benefits of the program could reap fruits. Thus, in this manner, a Technical Cooperation was approved in 2018 in order to apply the endline survey in the same treated and control cantons four years later after the application of the baseline survey. The follow-up survey was administered in 2018 with the same questionnaire that were used for the baseline survey. The same number of dwellings were visited: 9,701 in the same cantons. 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 the SIOL database identifies households with plots in need of regularization (the result of the PEMs) and those that requested the program's legal support. Using the unique cadastral code, the database allows the tracking of every plot owner that requested SIOL's legal support. In principle, it is not possible to do this at the parcel level as the impact evaluation sample has only one cadastral code per household. However, it is possible to merge the survey data with SIOL at the household level using the cadastral code.

5.1 Balance & attrition checks

Table 9 reports summary statistics and balance checks for household characteristics at baseline. As it can be seen in Table 3, the endline survey had a completion rate of 91% given that 1,281 from the 1,360 households were successfully reinterviewed. The lowest completion rate was in Loja (85%) where 112 out of 132 households were reached. Also, only 56 households rejected taking the interview and 68 households changed their address and were impossible to reach.[6] We reject equality of the treated and control group along multiple dimensions. 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, bigger plots, and

[6]It is worth mentioning that 28 new households entered the sample but they will not be part of the analysis. These households belonged to the original households but changed their address and took a piece of their land with them.

more plots without a 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: Difference-in-Difference

In order to identify the causal impact of SigTierras, the evaluation design envisioned employing a difference in difference (DD) strategy. The basic intuition behind this strategy is as follows. The impact of SigTierras will be measured by comparing the change in the mean of the outcome variable(s) for the treatment households with land tenure issues (those in cantons that received the program) versus the control households with the same condition (those in similar cantons that did not receive SigTierras). By comparing the change in the outcome variable, the DD strategy eliminates potential bias in the impact estimate due to systematic, observable differences in time-invariant characteristics of households or regions that affect the outcome variables but are difficult to measure. Within the DD strategy, the following basic regression equation is estimated in order to generate estimates of the average program impact:

Within the DD strategy, the following equation is estimated in order to generate estimates of the average program impact:

$$y_{it} = \beta_0 + \beta_1 Treated_i + \beta_2 Post_t + \beta_3 Treated_i * Post_t + \epsilon_{it} \quad (1)$$

where y_{it} is an outcome variable of interest for household i in period t, such as income, $Treated_i$ is a dummy variable equals to one if the household belongs to a treated canton, $Post_t$ is a

dummy variables indicating whether the household corresponds to the post-program period and zero in other case, $Treated_i * Post_t$ is an interaction between $Treated_i$ and $Post_t$, and ϵ_{it} is an error term. The parameter β_1 measures the average pre-program difference in the outcome variable between treatment and control groups, while β_2 measures the time trend – or the average difference in the outcome variable in the post- versus pre-program periods for the control group. Finally, β_3 measures the average impact or treatment effect on the Treated (ATT), of the program. The identifying assumption for β_3 to be an unbiased estimator of the causal impact of the program is 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 treatment households would have been the same as the average change of the control households.

6.2 Outcome indicators

As it was explained in section 3, SigTierras was expected to affect a large number of outcomes. In this section, we assess the outcome indicators that were planned according to the Monitoring and Evaluation Plan from the Loan Proposal. The list of household outcomes that were to be measured at the end the Program are listed on Table B. It is important to mention that in spite that the survey questionnaire was quite extensive, data for some of the outcomes that were to be evaluated is not available.

First, we begin by testing whether the Program had any effect on net household income, which we calculate following the methodology proposed by the Rural Income Generating Activities (RIGA) study (Carletto et al., 2007). For each source of the household income – that is agricultural wages, crop production, livestock production, income from self-employment, and transfers – the income is calculated as the difference between gross income and expenditure. Total income is calculated as the sum of its components. Note that self-consumption of crops and livestock products are considered part of the household income. Given that self-reported

income data are very noisy, households with "total income" below the 5^{th} percentile or above the 95^{th} percentile either at baseline or at endline are dropped from this analysis. We report both the results in terms of household income and income per capita.

Second, we measure whether households receiving the Program increased their agricultural productivity, measured as the total value of crop harvested per m^2 sowed plus the total expenditure in inputs (including crops, labor, pesticides, fertilizer, seed, and livestock). We also measured whether the Program had any effect on credit access and remittances. For the former we measured whether the household ask for a credit or a loan in the past twelve months. For the latter, we create a dummy variable taking the value of one if a household sent monetary transfers (remittances) to anyone in the past twelve months and zero otherwise.

Third, we test whether the Program had any effect on different farm expenditures per hectare. We include input expenditures in livestock, labor, pesticides, fertilizer, and hired labor, as well as the total farm expenditure. Forth, we focus on assessing whether treated households ameliorated their land tenure security. In this case, we use different measurements of land tenure perception including number of plots owned, number of plots without a title, and number of plots with a registered title. In a similar logic, we measure if the intensity and frequency of land conflicts were reduced due to Program. To do so, we use two dummy variables. The first one equals one if a household reported having a conflict in the past five years and zero otherwise. The second one has the same logic but measuring conflict within the last year instead of the last five. Finally, we assess to what extent treated households increased their investment in the plot including farm equipment and agricultural assets such as seeds, pesticide, fertilizer, and hired labor.

6.3 Propensity Score Matching

Although the households in the control group were selected to be as similar as possible as those in the treatment group, table 2 shows a lack of balance between these groups in several relevant demographic and socioeconomic characteristics. In other words, households from the

treatment and the control group seem to be different in several observable characteristics. To the extent in which the these differences are statistically significant and can be correlated with the trend in any outcome of interest, the parallel trend assumption might not hold, resulting in a biased DID estimator. Moreover, the lack of balance can make the estimate of the average treatment effect on the treated of the program imprecise (Imbens and Rubin, 2015). How can we balance similarity along each of those household dimensions? We can combine the DID methodology with a propensity score matching approach.

The propensity score matching is defined as the probability of an individual receiving the treatment given a set of observed pre-treatment covariates. In our context, the propensity score for household i with observable characteristics x_i, is defined as $e(x_i) = Pr(Treated_i = 1|X_i = x_i)$. Our goal is to obtain estimates of the propensity score that balance the covariates between the treatment and control units. More precisely, we seek to obtain an estimate of the propensity score that could lead to a similar distribution of covariates among treated and control units. Below, we explain carefully how we specify the propensity score given a great number of covariates taking too many values.

We will use the estimated propensity score to obtain an unbiased estimator of the average treatment effect as follows:

$$\hat{\tau} = \frac{1}{N} \sum_{i:Treated_i=1} \lambda_i \Delta y_i + \frac{1}{N} \sum_{i:Treated_i=0} \lambda_i \Delta y_i \tag{2}$$

where $\Delta y_i = y_{i1} - y_{i0}$ is the difference in the outcome of interest after the program $t = 1$ and before the program $t = 0$ in household i, and λ_i is a function of the estimated propensity score for household i define as follows:

$$\lambda_i = \begin{cases} \frac{1}{\hat{e}(x_i)} & \text{if } Treated_i = 1 \\ \frac{1}{1-\hat{e}(x_i)} & \text{if } Treated_i = 0 \end{cases} \tag{3}$$

where $e(x_i)$ is the propensity score that we defined before.

In practice, the first step is to fit a model to predict those households who were exposed to the Program based on the set of predictors. A natural starting point is to use a logistic regression with the dummy $Treated_i$ as dependent variable and a set of household characteristics as covariates. Once we fit this model, we predict $\hat{e}(x_i)$ for each household and use it to calculate the corresponding λ_i, which will be used as inverse probability weight in the following regression:

$$\Delta y_i = \alpha + \beta Treated_i + u_i \tag{4}$$

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, $\hat{\beta}$. However, this estimator is very sensitive to estimated propensity scores that are either close 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 of the propensity scores, with the objective to minimize the variance of the estimated average treatment effect.[7]

6.4 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 were not treated, making it easier to isolate the effect of the Program. Nonetheless, 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

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

$$\gamma \sum_i \mathbb{1}_{(\hat{e}(x_i)(1-\hat{e}(x_i)))^{-1} < \gamma} = \sum_i \frac{1}{\hat{e}(x_i)(1-\hat{e}(x_i)} \mathbb{1}_{(\hat{e}(x_i)(1-\hat{e}(x_i)))^{-1} < \gamma} \tag{5}$$

number of years that a household has been settled in a parcel might affect both the treatment status and some outcome of interest. Thus, one strategy could be to compare households with the same values under these two features 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. When the area of the common support is small no good inferences about the treatment effects can be made for a treated household for whom there is not comparison unit.

In sufficiently large datasets, it is recommended to use as many variables as possible related to both the treatment and the outcome, but in smaller datasets such as ours we might reduce bias by including more confounders at the cost of sacrificing efficiency in the treatment estimates. Based on our survey questionnaire of close to 300 questions, we decided to create 148 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. First, we 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 perception of land tenure status (number of parcels owned), the average number of

years that a household has been settled in their parcels, whether a household receives a governmental cash transfer program (*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 "filtered questionnaire" (*Encuesta filtro*) that were applied during the selection of the control group.

Now that we have defined 7 variables for the baseline model, the next step is to choose a second set of variables. From the remaining 141 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 141 logistic models. For each model, we calculate the likelihood ratio statistic, taking as reference the baseline model with 7 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 140 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. We apply the same procedure to select which quadratics 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.

Based on our methodology, we wind up with 34 additional linear covariates and 12 second order terms. Thus, in this manner, our final propensity score model has a total of 53 covariates. Figures 4 and 5 shows the performance our propensity score model. As we can glean from figure 4 for most of the values of range of the propensity score we find control units for the treatment units. Also, figure 5 shows that after the distribution of units across groups for the propensity score values improves drastically after the matching. Finally, we estimate $\hat{\alpha}$ as described in the

last section and we obtain the value of 0.099. 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 2112 observations.

Table 9 reports balance tests adjusted for propensity score weighting trimming the sample. The table shows important improvements in terms balance with respect to the unadjusted sample (Table 2). As we can see, we cannot reject the hypothesis of balance using the joint F-test (p-value=0.923) as there are no statistical differences between the two samples for the variables considered.

7 Results

Tables 4 to 8 show the results from the difference-in-difference model. Tables 10 to 14 show the results from our model using propensity score matching with inverse probability weights. Note that in this case, unlike the difference-in-difference estimation we decided to use a first difference model. For the sake of comparison, in each these tables we include the results of the first difference OLS model as well. We reported regression results for the outcomes listed in table B, except for those variables in which data was not available.

Income: With regards to the effect of the program in total income and its main components, we find strong evidence of an increase in total household income in treated households with respect to control households. Table 4 shows that the program increases household total income by 708 dollars, an almost 21% increase relative to the control group. By the same token, the program increases wage income by 694 dollars, an almost 30% increase relative to the control group. The rest of the income variables do not achieve decent significant levels, thus, no effects on crop, livestock, self employment income are observed. The propensity score matching estimates from table 10 confirm these results.

Agricultural Production: As we can see in Table 5, we find no effect of the Program on the value agricultural production and access to credit relative to the control households. We also added an outcome measuring whether households were more prone to send remittances as

a proxy to measure transfers income. As we can see in columns (3) from Table 5, relative to control households, treated households were 6 percentage points less likely to send transfers. The propensity score matching estimates from Table 11 confirm the expected sign of these coefficients, however, in the case of the dummy measuring remittances the coefficient is no longer statistically significant.

Farm Expenditures: Table 6 reports the results on total expenditure in inputs per hectare of land (column 1) as well as the relative expenditure in each output including inputs such as seeds, pesticides, fertilizer, and labor (columns 2 to 5). We found no evidence of an increase on expenditures in outputs due to SigTierras. However, the propensity score matching model from Table 12 shows that treated households spend 107 dollars more in overall inputs per hectare of land relative to control households.

Land tenure security and Land conflicts: As it can be seen in Table 13 we found no evidence that the program affect the perceived number of parcels owned by treated households relative to control households (column 1). By the same token, we found no evidence on whether the program affect the perceived number of parcels with no title by treated households relative to control households (column 2), nor with respect to the perceived number of parcels with title (column 3). With regards to the frequency of land conflicts among land tenants we observed interesting results. The program decreases the probability of conflict in one year and after 5 years. Yet, only the coefficient for measuring conflicts after one year is statistically different than zero.

Investment in fixed and farm assets: Turning our attention to the impact of SigTierras on investments in fixed and farm assets we find interesting results. As we can see in Table 8, we found no evidence that treated households increase their probability of investing in farm improvements (column 1) and purchasing fertilizer (column 4) and labor (column 5). However, treated households do have a higher probability of purchasing seeds and pesticides relative to control households (columns 2 and 3). The propensity score machine estimates from table 14 confirm the positive effects on seeds and pesticides, and the negative effects on farm investments

and labor. These estimates are statistically significant different than zero.

8 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. This could very well be related to the fact that randomization is oftentimes not feasible or else to other methodological weaknesses related to the identification strategy and the estimation of the parameters of interest in observational studies.

In this paper, we used a rigorous quasi-experimental design to evaluate the impact of SigTierras, a land regularization and administration program in rural Ecuador.

SigTierras was implemented between 2010 and 2016 in 47 cantons from Ecuador with the objective to enhance legal certainty with regard to property rights through the regularization of rural land. SigTierras spent a great amount of resources (US$62.8 million) cadastring properties in the participating cantons. Due to the nature of the cadastral sweep and the PEM —in which parcel owners received a thematic map with their land boundaries and were informed about the legal status of their parcels —, it was possible to identify the households with illegal ownership status of their parcels that were in need of regularization.

Thanks to this element of the design, by June 2014 there were 9 cantons in which a vast amount of parcels had been swept and that were representative of the three most important regions of the country. These cantons were selected as the treatment group for the Impact Evaluation, while the control group (another 9, yet different cantons) was selected from the pool of potential participants to SigTierras. Thus, using a baseline and endline household agricultural survey, we were able to disentangle whether households that participated in SigTierras improved in different household-level with respect to non-participant households.

The impact evaluation followed the plan laid out by the project team during the design phase as a "pre-analysis plan" and hence its main analysis was based on the techniques and the

outcomes that were pre-specified in those documents. However, upon the realization that the observable characteristics across treatment and control group were not balanced at baseline, we propose a propensity score matching methodology aimed at reducing the bias potentially caused by covariates imbalance and increase precision. This approach allowed us to confirm the results from the Difference in Difference estimation.

Our findings suggest that the program had a positive effect on total household income, wage income, overall farm expenditures, and the investment on inputs such as seeds and pesticides. We also found that SigTierras helped to reduce the likelihood of conflicts after one year of its implementation. However, we find no evidence that treated households increased their agricultural production, credit access and remittances, and farm investments and expenditures per hectare. Also, there is no evidence that the program had any positive effect on perceptions of land tenure security.

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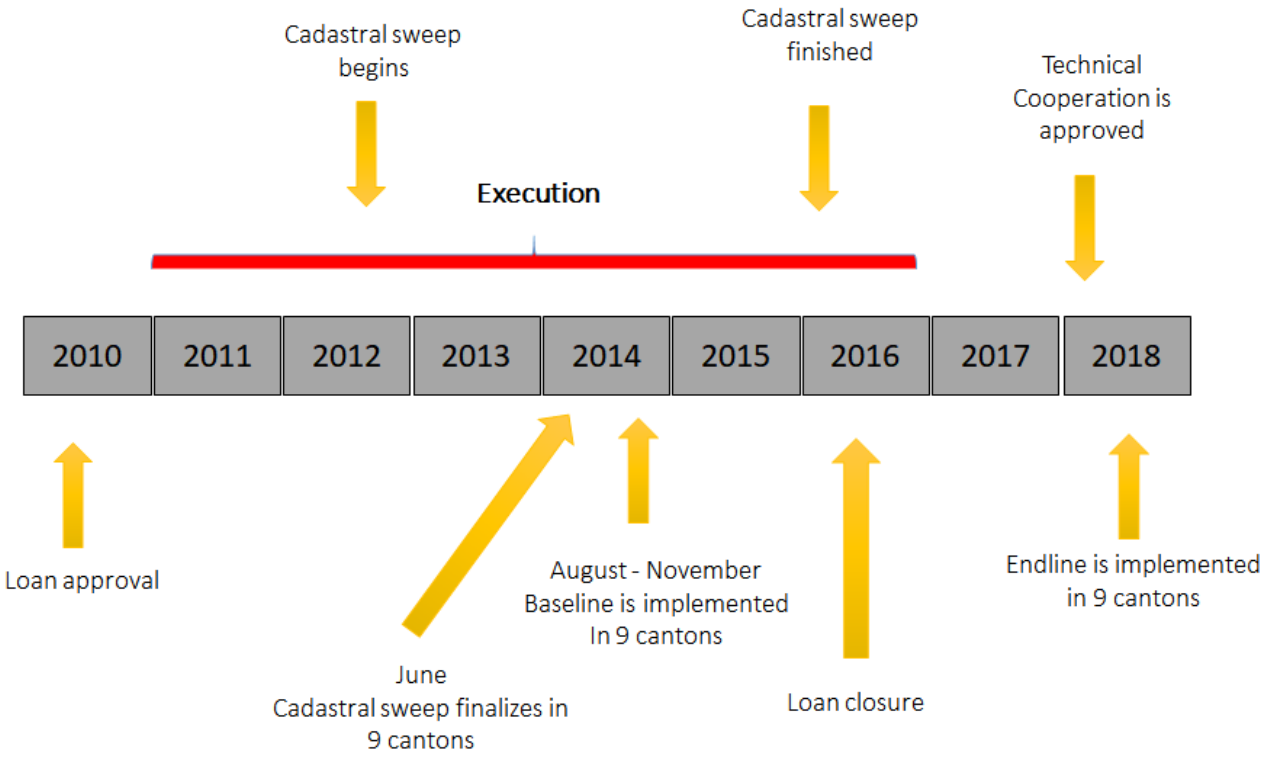
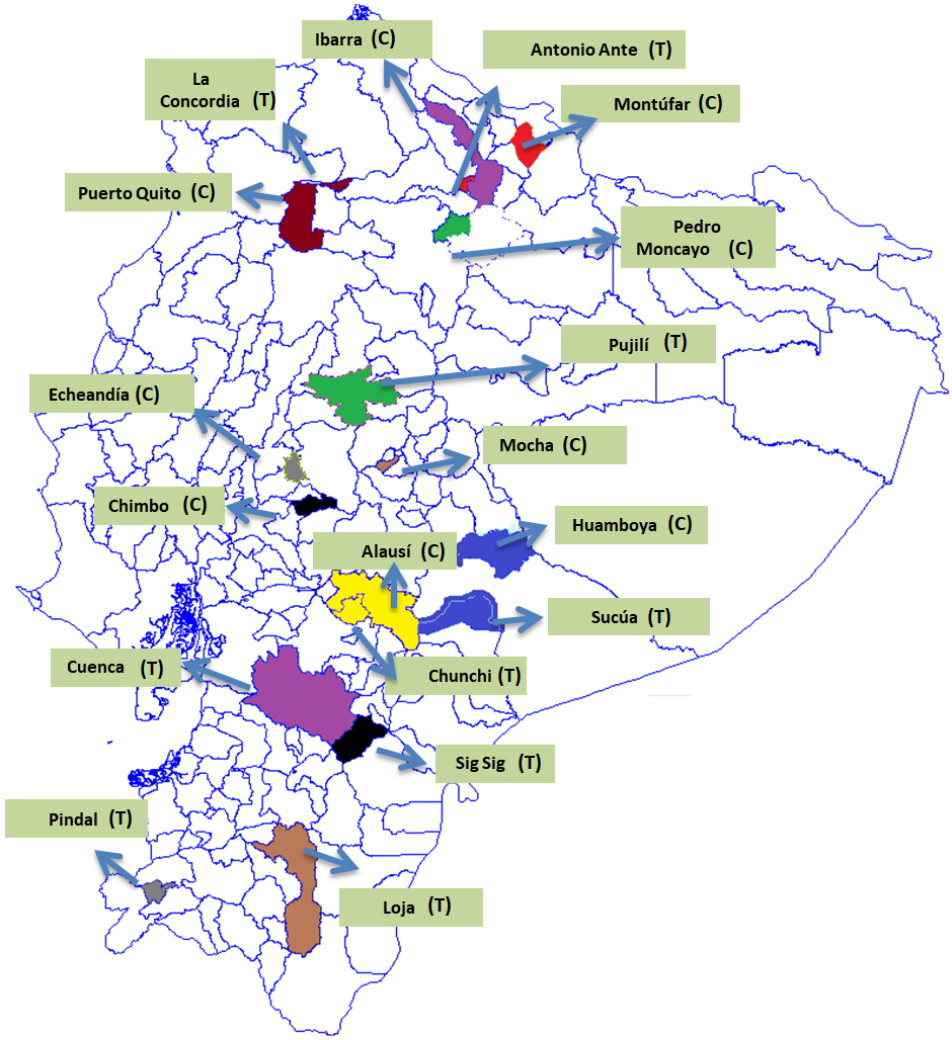
A Figures

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	-0.05***
	(0.01)	(0.01)	(0.02)
HH head age (years)	50.44	53.07	-2.63***
	(0.45)	(0.45)	(0.64)
HH size	4.32	4.29	0.03
	(0.06)	(0.07)	(0.09)
Total males	2.17	2.11	0.07
	(0.04)	(0.04)	(0.05)
N of dependents	3.41	3.42	0.00
	(0.06)	(0.06)	(0.08)
N adults in agriculture	1.72	1.58	0.14**
	(0.04)	(0.04)	(0.05)
HH head has primary	0.55	0.54	0.00
	(0.01)	(0.01)	(0.02)
Receives *Bono*	0.58	0.61	-0.04**
	(0.01)	(0.01)	(0.02)
HH head ethnic	0.15	0.13	0.02
	(0.01)	(0.01)	(0.01)
N plots owned	2.64	2.64	0.01
	(0.05)	(0.05)	(0.07)
N plots without document	0.88	0.99	-0.11**
	(0.03)	(0.04)	(0.05)
N plots with title	1.19	1.08	0.11**
	(0.04)	(0.04)	(0.06)
Tot area (Ha)	11.34	47.49	-36.15
	(5.70)	(27.50)	(28.20)
Avg plot size (Ha)	2.37	1.83	0.53*
	(0.26)	(0.15)	(0.30)
Avg plot tenure (years)	14.50	18.35	-3.86***
	(0.34)	(0.38)	(0.51)
N plots with irrigation	0.34	0.24	0.09***
	(0.03)	(0.02)	(0.03)
N	1347	1360	2707
Joint F-Stat	6.64		
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: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 4: Household Income

	Total income	Wages	Crop	Livestock	Self emp	Transfers
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-124.416	-258.986***	-16.918	79.322**	11.645	60.521*
	(98.890)	(70.267)	(66.029)	(39.631)	(21.056)	(30.976)
Post	1888.957***	1976.991***	-7.654	-15.260	-1.762	-63.359**
	(135.357)	(123.158)	(60.375)	(39.646)	(19.263)	(32.055)
Treated*Post	708.969***	694.033***	94.358	-44.342	-1.712	-33.370
	(196.943)	(173.785)	(95.871)	(59.056)	(30.101)	(46.758)
Mean Control Baseline	1549.011	554.798	489.389	37.689	44.051	423.085
Observations	4110	4110	4110	4110	4110	4110

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

Table 5: Agricultural Production and Access to Credit

	Outcomes		
	Value of production	Credit	Transfers(=1)
	(1)	(2)	(3)
Treated	2919.919*	284.896**	0.040**
	(1727.242)	(142.594)	(0.020)
Post	33844.407	-317.825***	-0.145***
	(26678.923)	(121.674)	(0.020)
Treated*Post	-2.77e+04	264.975	-0.065**
	(27556.035)	(190.040)	(0.028)
Mean Control Baseline	2740.001	1013.060	0.573
Observations	4986	4986	4986

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

Table 6: Farm expenditures

	Expenditure per hectare					
	All inputs	Livestock	Labor	Pesticides	Fertilizer	Seeds
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	1079.259	-694.364	76.359	567.027	341.161	171.071
	(812.294)	(583.547)	(139.601)	(424.589)	(337.347)	(299.339)
Post	-469.198*	119.173	596.889**	-131.426**	-222.178**	-115.594
	(250.154)	(751.642)	(238.527)	(53.281)	(106.803)	(134.678)
Treated*Post	-1118.639	1900.660	-598.991**	-593.513	-351.725	-173.401
	(829.167)	(2207.255)	(276.777)	(425.607)	(344.392)	(316.771)
Mean Control Baseline	967.463	3751.512	308.189	243.198	373.292	350.974
Observations	4986	4986	4986	4986	4986	4986

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

Table 7: 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	-0.004	-0.111**	0.057	-0.009	0.000**
	(0.064)	(0.046)	(0.054)	(0.007)	(0.000)
Post	0.427***	-0.316***	0.304***	-0.003	-0.000
	(0.067)	(0.039)	(0.058)	(0.007)	(0.000)
Treated*Post	-0.036	0.219***	-0.205***	0.003	-0.000
	(0.096)	(0.065)	(0.078)	(0.010)	(0.000)
Mean Control Baseline	2.245	1.178	0.909	0.108	0.077
Observations	4986	4986	4986	4986	4986

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

Table 8: Investment in fixed and farm assets

	Farm	Paid for input			
	Investment	Seeds	Pesticide	Fertilizer	Labor
	(1)	(2)	(3)	(4)	(5)
Treated	0.041***	0.134***	-0.090***	-0.010	0.028
	(0.010)	(0.018)	(0.020)	(0.019)	(0.019)
Post	0.038***	-0.029*	-0.161***	-0.117***	0.162***
	(0.010)	(0.017)	(0.019)	(0.018)	(0.020)
Treated*Post	-0.049***	0.025	0.044*	-0.007	-0.109***
	(0.014)	(0.025)	(0.026)	(0.025)	(0.027)
Mean Control Baseline	0.041	0.236	0.439	0.349	0.319
Observations	4986	4986	4986	4986	4986

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

C Propensity Score Matching

Figure 4. Propensity Score

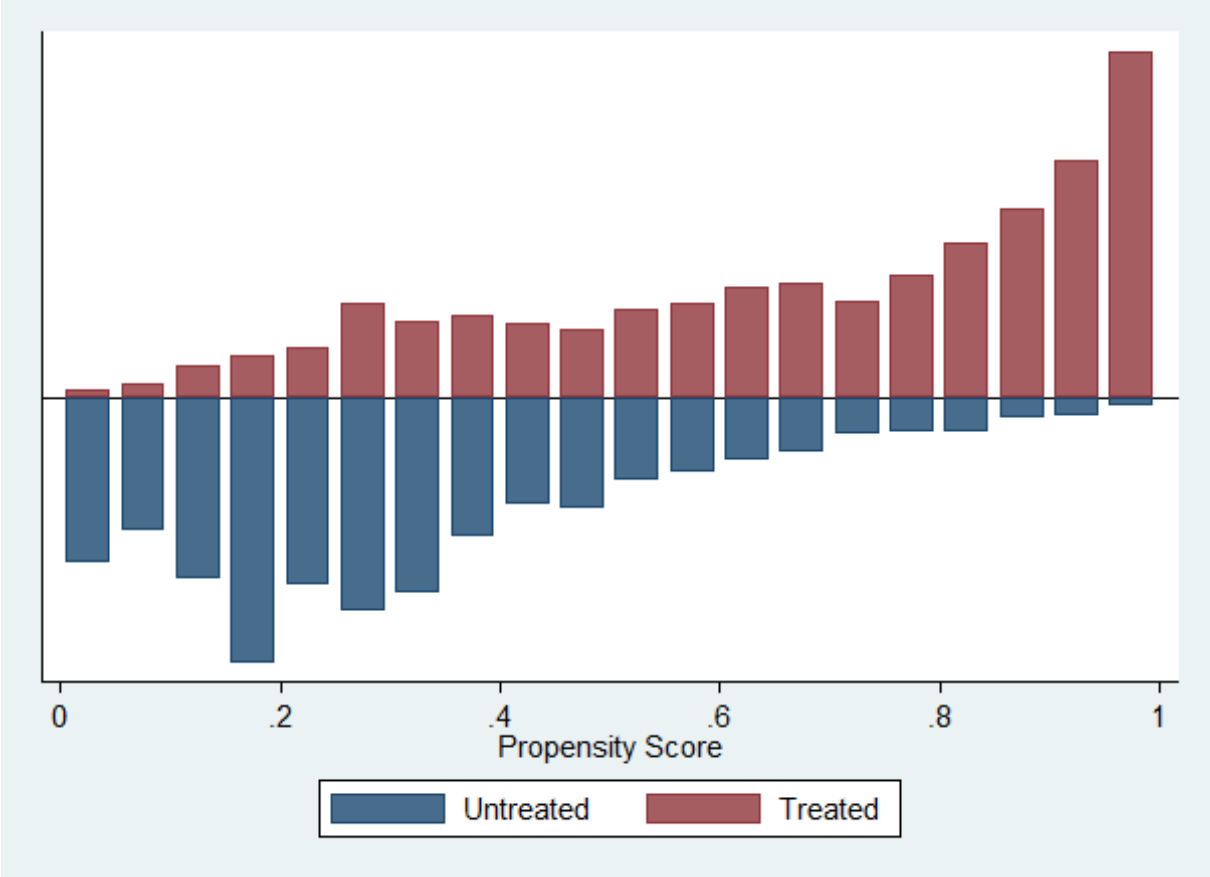


Figure 5. Distribution of treated and control units before and after matching

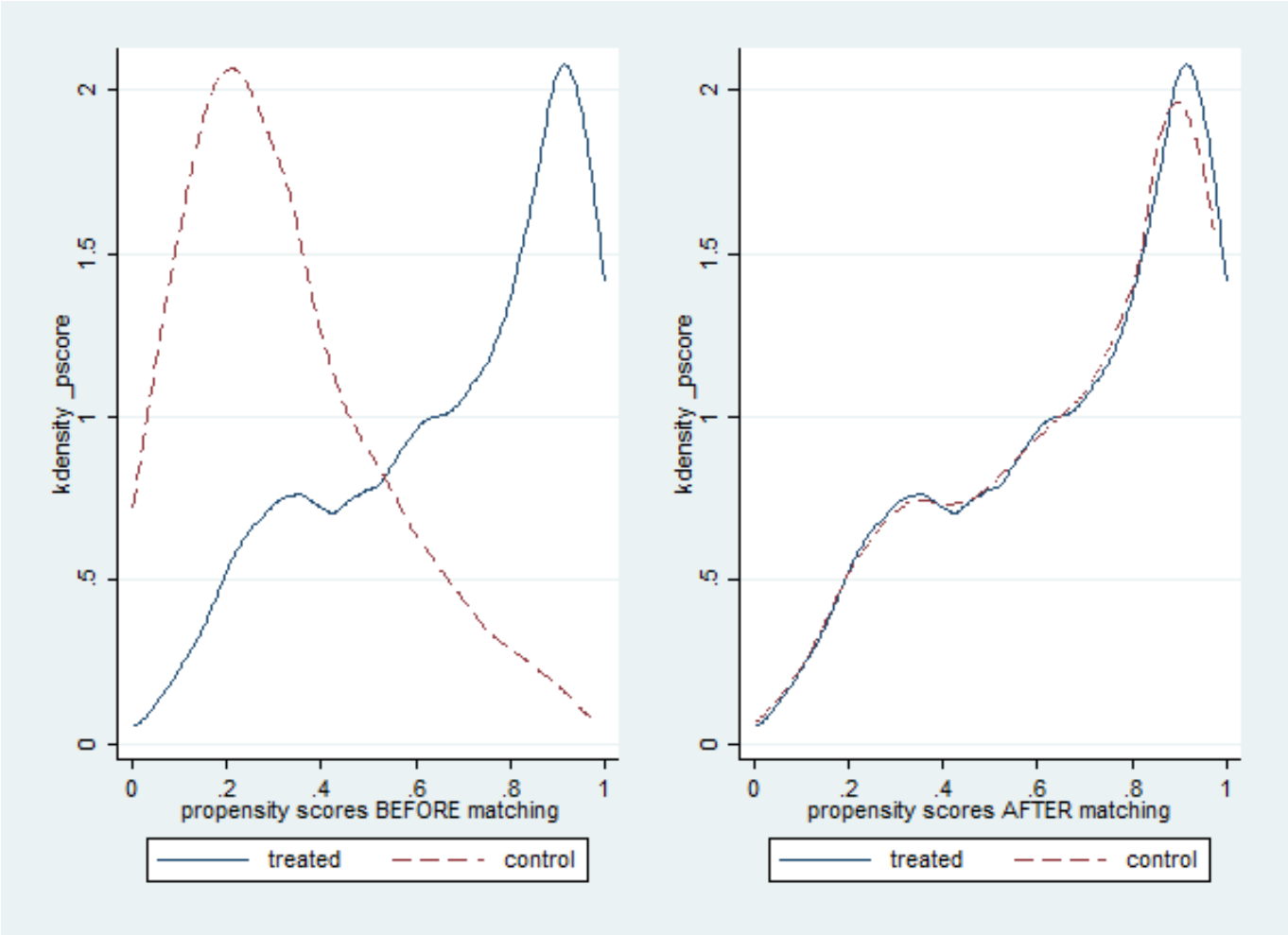


Table 9: Balance checks adjusted

	Control (1)	Treated (2)	(1) vs. (2) (3)
HH head is female	0.22	0.22	-0.01
	(0.02)	(0.01)	(0.02)
HH head age (years)	50.81	52.00	-1.19
	(0.64)	(0.55)	(0.85)
HH size	4.16	4.23	-0.07
	(0.08)	(0.08)	(0.11)
Total males	2.08	2.07	0.01
	(0.05)	(0.05)	(0.07)
N of dependents	3.26	3.33	-0.08
	(0.08)	(0.07)	(0.11)
N adults in agriculture	1.62	1.63	-0.01
	(0.05)	(0.05)	(0.07)
HH head has primary	0.55	0.53	0.02
	(0.02)	(0.02)	(0.03)
Receives *Bono*	0.59	0.59	0.00
	(0.02)	(0.02)	(0.03)
HH head ethnic	0.12	0.13	-0.01
	(0.01)	(0.01)	(0.02)
N plots owned	2.52	2.55	-0.03
	(0.06)	(0.06)	(0.09)
N plots without document	0.91	0.93	-0.02
	(0.04)	(0.05)	(0.06)
N plots with title	1.03	1.08	-0.05
	(0.05)	(0.04)	(0.07)
Tot area (Ha)	13.27	9.45	3.82
	(10.37)	(4.85)	(11.45)
Avg plot size (Ha)	1.63	1.67	-0.04
	(0.18)	(0.15)	(0.23)
Avg plot tenure (years)	15.86	15.84	0.02
	(0.49)	(0.48)	(0.68)
N plots with irrigation	0.28	0.27	0.01
	(0.03)	(0.02)	(0.04)
N	1143	969	2112
Joint F-Stat			0.55
P-value			0.923

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

Table 10: Household Income

	Total Income	Wages	Crop	Livestock	Self emp	Transfers
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: First difference - OLS						
Treated	634.990***	607.618***	52.440	-23.780	-14.077	12.788
	(213.158)	(189.109)	(88.197)	(57.379)	(35.923)	(52.002)
Mean Control Baseline	1966.669	2003.058	431.944	-7.224	-1.529	-75.370
Observations	1623	1623	1623	1623	1623	1623
Panel B: First difference - PSM						
Treated	771.320***	684.530***	89.656	-65.466	-10.495	73.095
	(241.102)	(213.323)	(107.080)	(63.550)	(41.346)	(60.812)
Mean Control Baseline	1966.669	2003.058	431.944	-7.224	-1.529	-75.370
Observations	1623	1623	1623	1623	1623	1623

Notes: Robust standard errors in parenthesis, clustered at the community level. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 11: Agricultural Production and Access to Credit

	Value of Production	Credit	Transfer
	(1)	(2)	(3)
Panel A: First difference - OLS			
Treated	-3129.874	245.815	-0.040*
	(2411.969)	(196.898)	(0.024)
Mean Control Baseline	637.635	-355.386	-0.158
Observations	1937	1937	1937
Panel B: First difference - PSM			
Treated	-4479.037	29.214	-0.008
	(3086.781)	(217.341)	(0.029)
Mean Control Baseline	637.635	-355.386	-0.158
Observations	1937	1937	1937

Notes: Robust standard errors in parenthesis, clustered at the community level. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 12: Farm Expenditures

	All inputs	Livestock	Labor	Pesticides	Fertilizer	Seeds
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: First difference - OLS						
Treated	43.466	-27.054	-80.655	9.712	7.209	-80.655
	(40.638)	(152.234)	(49.444)	(32.043)	(10.709)	(49.444)
Mean Control Baseline	34.725	-155.685	108.589	-40.780	-3.730	108.589
Observations	1937	1937	1937	1937	1937	1937
Panel B: First difference - PSM						
Treated	107.307*	-375.784	-64.451*	5.435	5.520	-64.451*
	(55.954)	(422.554)	(34.505)	(23.639)	(11.065)	(34.505)
Mean Control Baseline	34.725	-155.685	108.589	-40.780	-3.730	108.589
Observations	1937	1937	1937	1937	1937	1937

Notes: Robust standard errors in parenthesis, clustered at the community level. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 13: 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)
Panel A: First difference - OLS					
Treated	-0.003	0.161***	-0.092**	-0.024	-0.026*
	(0.039)	(0.041)	(0.037)	(0.018)	(0.016)
Mean Control Baseline	0.386	-0.325	0.274	0.020	0.023
Observations	1937	1937	1937	1937	1937
Panel B: First difference - PSM					
Treated	0.076	0.068	0.052	-0.026	-0.030*
	(0.048)	(0.056)	(0.053)	(0.019)	(0.017)
Mean Control Baseline	0.386	-0.325	0.274	0.020	0.023
Observations	1937	1937	1937	1937	1937

Notes: Robust standard errors in parenthesis, clustered at the community level. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 14: Investment in fixed and farm assets

	Farm	Paid for input			
	Investment	Seeds	Pesticide	Fertilizer	Labor
	(1)	(2)	(3)	(4)	(5)
Panel A: First difference - OLS					
Treated	-0.049***	0.063***	0.049**	0.019	-0.091***
	(0.016)	(0.024)	(0.024)	(0.023)	(0.026)
Mean Control Baseline	0.045	-0.039	-0.161	-0.115	0.173
Observations	1937	1937	1937	1937	1937
Panel B: First difference - PSM					
Treated	-0.041**	0.099***	0.071**	0.016	-0.074**
	(0.019)	(0.029)	(0.031)	(0.026)	(0.029)
Mean Control Baseline	0.045	-0.039	-0.161	-0.115	0.173
Observations	1937	1937	1937	1937	1937

Notes: Robust standard errors in parenthesis, clustered at the community level. * $p < .10$, ** $p < .05$, *** $p < .01$.