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Planting the seeds: The impact of training on mango producers in Haiti *

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Abstract

This paper evaluates the short-term impacts of a development project that aims to increase mango yields, sales of mango products, and the income of small mango farmers in rural Haiti. Various matching methods, in combination with difference-in-difference (DID), are used to deal with the potential selection bias associated with nonrandom treatment assignment. Robustness checks are conducted to investigate whether and to what extent the results are affected by the coexistence of other similar projects in the same sites. Rosenbaum bounds analysis is carried out to check the sensitivity of the estimated impacts—based on matching methods—to deviations from the conditional independence assumptions; the relative importance of unobserved factors in the decision to participate. Our results show that in a 16-month period, the project increased the number of young *Francique* trees planted—a type that has greater market and export potential than traditional mango varieties—and likely encouraged the adoption of best practices. But the project has not yet led to a noticeable increase in total sales. The adoption of improved production practices is too recent to translate into significant changes in production and sales. While the robustness check suggests that the results are not caused by the presence of other similar programs on the same sites, the Rosenbaum bounds sensitivity analysis suggests that the matching results are robust against potential "hidden bias" arising from unobserved outcome variables in some but not all cases.

JEL Classification: *Q13, Q16, O12.*

Keywords: Agriculture, impact evaluation, producer cooperative, extension services, Haiti, mango.

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1 Introduction

The importance of adopting relevant technologies to reduce poverty and spur economic development has long been emphasized by development economists and agricultural practitioners (Feder et al., 1985; Feder and Umali, 1993). The adoption of high-yield varieties (HYVs) and complementary modern inputs and crop management practices during the Asia Green Revolution is widely acclaimed as one of the most successful stories in the economic development of South and Southeast Asia (Evenson and Gollin, 2003; Otsuka and Larson, 2012; Pingali et al., 2007; World Bank, 2008). Meanwhile, the low adoption rate of HYVs, fertilizers, irrigation technologies, and other agricultural and resource management practices is cited as a key reason for the mostly unsatisfactory performance of the agricultural sector in Sub-Saharan Africa (Djurfeldt et al., 2005; Moser and Barrett, 2006; Ndjeunga and Bantilan, 2005; Otsuka and Kijima, 2010; Otsuka and Larson, 2012). In Latin America and the Caribbean (LAC) considerable advances have been made in the development of HYVs, agricultural mechanization, and agricultural practices; but economic inequality remains high, and many rural areas still lag behind urban areas (World Bank, 2008).

Despite the existence of many proven technologies and improved agricultural and resource management practices, the adoption of such technologies and practices by smallholder farmers is generally low in developing countries (Otsuka et al., 2013; Sunding and Zilberman, 2001; World Bank, 2008). Early studies on technology adoption identify several key obstacles faced by small farmers who might otherwise adopt new technologies: lack of credit, limited access to information, aversion to risk, inadequate farm size, insecure land tenure, a low level of human capital, and poor infrastructure (Feder et al., 1985; Feder and Umali, 1993). For the past decade or so, international organizations (both in and outside government) have done much to encourage farmers to adopt new technologies and agricultural best practices through agricultural development projects. A common feature of such development projects is that they typically aim to overcome multiple constraints and to promote the adoption of multiple practices and behavior changes. Another important feature is that project locations and beneficiaries are typically not randomly assigned. Though these features pose considerable methodological challenges to any study of project impacts, many researchers have compiled impact evaluations of agricultural development projects in the past ten years or so (Dillon, 2011b; Duflo et al., 2011; Duvendack and Palmer-Jones, 2012; Mendola, 2007; Moser and Barrett, 2006; Nkonya et al., 2012; Wanjala and Muradian, 2013). Yet the research—especially that using rigorous evaluation methods—is disproportionately small relative to the large number of projects implemented (IDB, 2010; World, Bank, 2010). In addition, few studies evaluate the possible short-term impactsincluding the adoption of particular agricultural technologies or changes in behavior—or intermediate impacts of projects before their closure (Pamuk et al., 2014; Peralta, 2014). Such impacts are important, signaling future project success and indicating necessary corrections in strategy.

To add to this emerging literature, we evaluate the short-term impact of a development project that aims to increase the income of small mango producers in Haiti. Our evaluation is based on data from a baseline survey and a follow-up survey after 16 months of project implementation. Because of the short time period involved, we focus on evaluating a set of short-term outcomes, such as the adoption of a preferred mango variety, improved production and harvest practices, and behavioral changes in farmers' production and commercialization decisions. Longer-term outcomes such as increases in mango production and sales take longer to materialize due to the life cycle of the mango tree. For example, it takes more than a year for mango trees to mature and become productive (University of Hawai Manoa, 2014).

As with many other agricultural development projects, the main empirical challenge to an evaluation of the project is the fact that project beneficiaries were not randomly assigned. Direct comparison of outcomes between project participants and nonparticipants would lead to a biased estimation of the projects impacts. To control for the potential selection bias, we use nonexperimental econometric methods: matching, and a combination of matching and difference-in-difference (DID) methods.

Due to differences in survey instruments between baseline and follow-up, we are forced to adopt different evaluation methods for different outcome variables. Panel data are available only for some outcome variables. For other outcome variables, we have data from only the follow-up survey. In particular, we use propensity score matching (PSM) and matching in covariates for the outcomes for which only follow-up survey data are available. In order to assess how robust our matching estimates are to possible hidden bias—caused by the effect of unobserved variables that simultaneously affect assignment to treatment and the outcome variable—we use the Rosenbaum bounds approach. For the outcomes for which we have panel data from both the baseline and follow-up surveys, we estimate the impacts using difference-in-difference, propensity score matching (DID-PSM), and DID-matching in covariates to control for time invariant unobserved heterogeneity.

There is also a concern that the presence of other projects with similar characteristics in the project intervention sites could potentially bias our evaluation results. To investigate whether and to what extent the presence of other projects bias our results, we conduct a robustness check by excluding all the observations (in both treatment and control groups) of parties that participated in other projects.[1]

The results suggest that the project significantly increased the planting of *Francique* mango trees (the variety preferred for export), but that these trees are still too young to bear fruit (that is, they are not yet productive) and so have not translated into increases in yields or sales. The project also had a positive impact on the adoption of improved agricultural practices (pruning, tidying, grafting, and fencing), and on preferred commercialization behaviors (with a shift away from selling to middlemen and toward selling to producer business groups [PBGs], or cells). But the results on the adoption of improved practices should be interpreted with caution, since they are sensitive to possible deviations from the identifying unconfoundedness assumption. Finally, our robustness checks suggest that the presence of other projects did not influence the estimated impacts of the project.

The rest of the paper is organized as follows. Section 2 describes the project, focusing on the theory of change and the selection of project beneficiaries. Section 3 presents the data and the sample design, and Section 4 proposes different evaluation methods. Section 5 discusses the main impact evaluation results, the robustness checks, and the sensitivity analysis, followed by conclusions and recommendations in Section 6.

[1]Without more detailed data on the nature and distribution of other programs between the treatment and control groups, it is not possible to know whether they would bias our results. The robustness checks indirectly assess whether and to what degree the presence of other programs would bias the main results.

2 The Project

2.1 Background

Haiti is the poorest country in Latin America and one of the poorest in the world. Conditions in the country worsened after it was hit by a 7.0 magnitude earthquake in 2010. Its gross national income (GNI) per capita was $760 in 2012. Eighty percent of its population earn less than $2 per day, and 50 percent live below the poverty line ($1 per day) (World Bank, 2014). More than 60 percent of Haiti's population depends on agriculture for their livelihood. The country also relies heavily on remittances from the Haitian diaspora and on foreign aid.

Haiti's economy has been slowly recovering since 2010, with most of the economic growth coming from agricultural production, construction, and the garment sector (World Bank, 2014).

The poverty seen in Haiti is typical of poor rural areas in developing countries, where agriculture plays a major role in the strategy for increasing agricultural incomes and household wealth (World Bank, 2008). Mangoes are among Haitis main agricultural products, with a high potential for exportation. The country is among the 20 largest mango producers in the world (FAO, 2010). But several constraints—such as inadequate production technologies for commercial mangos, institutional barriers, and inadequate (or lacking) infrastructure—impede the increase of mango production and exports (Castañeda et al., 2010).

There are about 100 mango varieties planted in Haiti, but only *Francique* is exported. Of total production, only between 2.5 and 5.0 percent reach the export market (FAO, 2010). Most exported Haitian mangoes go to the United States. Low-quality yields and damage or spoilage during transportation are among the main reasons for the low export rates of Haitian mangoes. Haiti's mango producers are predominately smallholder farmers who own 10 trees or less and lack the technical knowledge to produce mangoes of export standards (Castañeda et al., 2010).

In addition, farmers usually sell mangoes to middlemen, who are responsible for the harvest and transport of mature mangoes. Farmers lack the knowledge, experience, and technical skills required to perform these activities themselves. Mangoes are usually sold per tree and not by the quantity of actual mangoes produced. Farmers' lack of access to credit to smooth consumption often leads to the premature sale of mangoes to middlemen, who offer cash in advance.

2.2 The Project

The project was launched in 2010 to overcome the production and commercialization constraints faced by small mango farmers in Haiti. The aim of the project was to increase the income of smallholder farmers and facilitate their access to the value chain for mango exports. The project promotes the formation of producer business groups (PBGs), or cells. Members of PBGs are trained in good practices in mango production—both harvest and postharvest—and commercialization, and basic business literacy (see Table 1 for a detailed list of project interventions). The project promotes the planting of the *Francique* mango variety, which is in high demand among potential commercial buyers and has the highest export potential (Castañeda et al., 2010). Participants are expected to adopt the practices promoted by the project.

The project is a value chain development project implemented in Haiti by TechnoServe, a nonprofit organization with worldwide experience, with the support of project partners.

2.2.1 *The Project's Theory of Change*

The project seeks to boost farmers' income through higher mango yields, higher-quality mangoes that adhere to international standards, as well as a better linking of producers to international mango value chains. This combination of effects (increased productivity, improved quality, and commercial linkages) is expected to generate an increase in mango sales and the development of stable commercial relationships between farmers and reliable exporters. Exporters, meanwhile, would benefit from a higher-quality and more-predictable mango supply.

To achieve the project's objectives, TechnoServe provides training to local mango producers and connects smallholder producers to exporters via PBGs. In addition to establishing PBGs, the project engages existing farmer cooperatives.

According to the project design, producers will see increases in mango sales through a combination of three effects: (i) an increase in mango production, either through an increase in the yield of trees already bearing fruit or through new trees; (ii) a reduction of waste due to better harvest and postharvest practices (unnecessary losses are estimated to affect up to 50 percent of mango production); and (iii) an increase in the price producers are able to obtain in the market, as they begin selling higher-quality mangoes through appropriate channels.

In the context of the PBGs set up by the project, TechnoServe provides farmers with training in pruning and tree care, nursery care, and orchard-related extension services.[2,3] The assumption is that by pruning trees farmers maintain their health, productivity, and size—and facilitate better harvests and improved fruit quality. The program also seeks to increase volume by planting new saplings and, in 2013, TechnoServe decided to undertake grafting to achieve early increases in mango volumes. Thus, increases in volume are to be achieved via (i) increases in the productivity of existing, producing trees (via practices such as pruning and grafting), and (ii) the production of new trees (from saplings).

To reduce mango waste, TechnoServe provides farmers with training in harvest and postharvest best practices and in the utilization of local transportation.[4] The assumption is that by properly selecting the harvest period that will maximize mango quality, using the right harvest tools to reduce spoilage, and properly handling the harvested mangoes and sorting them according to the corresponding sales channels to maximize prices per channel, farmers can reduce losses—which before the project were as high as 50 percent of the total potential harvest.[5]

[2] Asrey et al. [2013] find that pruning results in significantly higher fruit weight, fruit firmness, total carotenoids, antioxidant capacity, and total phenolic content. Early maturity of fruits is observed from unpruned trees with faster color change, higher total soluble solids, and lower titratable acidity. The fruits harvested from pruned trees show signs of slower ripening, and lower respiration, ethylene evolution rates, and enzyme activity when compared with fruits from unpruned trees. Both anthracnose and stem-end rot disease percentages are reduced in ripe fruits from pruned trees.

[3] The pruning and tree care training module teaches farmers to identify and cut appropriate tree branches and maintain mango tree canopy to maximize fruit quality (targeted to all farmers); the nursery training module teaches farmers the process of cultivating a mango from a seedling, including protection against common problems and building seed stock (targeted to a selection of farmers). Farmers also receive on site visits from technicians to help them rehabilitate their mango orchards (extension services).

[4] The harvest and postharvest training module teaches farmers to identify appropriate harvest periods and methods, and to properly sort mangoes according to corresponding sales channels. The local transportation training module teaches farmers the appropriate way to transport their mangoes to the appropriate collection center (both modules target all farmers).

[5] Castañeda et al. [2010, p. 8] explain that when farmers presell their fruit to middlemen on farms, the middlemen harvest all fruits from a given tree, whether they are adequately ripe or not:

> After picking all fruit, middlemen select and leave rejected fruits at the farm, paying only for the

To increase the price that farmers can get for their mangoes, TechnoServe provides farmers with training in the creation of PBGs and in business planning.[6] The assumption is that by selling their production appropriately (by dozens instead of by plot or tree, for example) and using the PBGs as channels (rather than negotiating by themselves), farmers can get an overall better price for their harvest.

Some of these actions may have immediate effects: for example, the adoption of harvest and postharvest best practices could immediately increase mango sales by improving the quality of the fruit and reducing spoilage during transportation. This is the effort with potentially the highest immediate impact. Other activities supported by the program and related to harvesting could also have immediate effects: better access to credit could potentially increase the quality of mangoes sold, by limiting the need of farmers to sell their mangoes before the appropriate time.

Another action that could have immediate effects is the adoption of commercialization best practices: markets for agricultural products are becoming more integrated and concentrated in their structure, and smallholder farmers tend to be excluded from the modern value chain, mainly because of the challenges of complying with quality standards and of providing the quantities required to ensure reliability (Barrett, 2008; Farina et al., 2005; IFAD, 2011). Organizing farmers in groups reduces risk and transaction costs, and increases information flows (IFAD, 2011). It also increases the farmers' bargaining power for better contract conditions, and ensures reliable quality and quantities of product (Markelova et al., 2009).

The third action—the adoption of agricultural best practices (pruning and tree care) and the planting of new *Francique* trees and grafting of productive, non-*Francique* trees—is unlikely to generate results in the short run. In fact, the pruning promoted by the program, for example, could negatively affect mango yield in the short run. Davenport [2006] states that severe pruning (done to rejuvenate mango trees so large that the canopy migrates far beyond the reach of harvesters) accompanied by tip pruning (to reduce the flush frequency back to normal) would eliminate flowering and reduce production for about a year. Asrey et al. [2013] designed an experiment to quantify the effects of pruning done to manage canopy size. The fruit yield of pruned trees was found to decrease in the first year compared with the fruit yield of unpruned trees; it increased beyond unpruned trees during the second year. In another experiment, Das and Jana [2013] find that the initiation of fruiting begins after the third year of pruning. Meanwhile, it is too early for new *Francique* trees and grafted, productive, non-*Francique* trees to bear fruit—it takes 5–6 years for a new tree or 3–4 years for a grafted tree to start bearing fruit.

There are other reasons to believe that the project initiatives undertaken thus far will not yield immediate results. It is well known that it takes time to translate adoption into

chosen mangoes. Rejected fruits could be immature, over ripe, bruised or fly infested, with a low chance of commercialization. Mango losses may reach up to 50 percent of the total potential harvest. Mango is sold to exporters in Port-au-Prince (transportation is arranged with the exporter, and prices vary), however, at the export facilities, it is necessary to re-classify mangoes due to the inappropriate postharvest practices of middlemen (rejects account for around 50 percent). Rejected mangoes are sold to madam sarahs.

"Madam sarahs" are retailers, usually women, who sell the mangoes in the local markets.

[6]The producer business group (PBG) training module supports farmers in creating and organizing PBGs, which function as intermediaries for smallholder farmers to increase their access to markets (and improve their negotiating power in bulk sale, transportation, specialization of tasks, and so on). In the business planning training module, farmers learn about the sales channels available to smallholder farmers, and the components of a business and marketing plan. Both modules target all farmers.

changes in production, sales, and income. As farmers decide which practices to adopt, they must consider risk, profitability, and input constraints (Feder et al., 1985; Minten and Barrett, 2008). Technology adoption involves a process of learning over time (Foster and Rosenzweig, 1996). And finally, farmers adopt new technologies in a stepwise fashion, not all at once (Byerlee and Hesse de Polanco, 1986), even when such practices are promoted as a package.

2.2.2 *Targeting of Beneficiaries*

The key challenge we face in our evaluation analysis is the fact that the project areas and project beneficiaries were not randomly assigned. PBGs were established in areas of high potential mango yield, as indicated by the quality and volume of current production, agronomic characteristics, local industry players, land availability, and infrastructure. Farmers eligible to participate in the program grow mangoes, have at least five trees, and farm between 0.5 and 5 hectares of land; they are also required to be members of cooperatives and to live in the communes targeted by the project. But these eligibility criteria have not been strictly enforced; they have instead served as a guide for selecting poor, smallholder farmers as project beneficiaries.

Within selected communes, community members were hired by TechnoServe and assigned to recruit farmers. Their recruitment areas were determined by their location and their ease of access to surrounding areas where mango producers were likely to be found. The project specifies that recruiters can travel for a maximum of an hour-and-a-half by foot to promote the project among mango producers and to invite them to join a local rally where they may enroll in the project. The decision to join the project or not is voluntary.

Given the way beneficiary communes and farmers were selected, two main sources of bias are likely to arise. Farmers participating in the project may differ from nonparticipants in their observable characteristics due to project targeting, but they may also differ in unobservable characteristics due to self-selection. These sources of bias will be addressed to the extent allowed by the data and the sample design using nonexperimental methods.

3 Data and Method for Impact Evaluation

3.1 Data

The data used for the impact evaluation are from a baseline survey in 2012 and a follow-up survey in 2013. Information about the sample design and data collection process was obtained from reports provided by the firms hired to conduct data collection, and some gaps remain. A local survey firm under the guidance of TechnoServe implemented the baseline survey during JuneJuly 2012, prior to the implementation of the project. Two hundred and sixty-eight participants (treatment households) and 510 nonparticipants (comparison households) were randomly selected to be part of the study from eligible mango producers in a total of 12 communes.

The treatment households were selected from the communes where project activities were implemented. The TechnoServe database was used to obtain the list of mango producers enrolled in the project. The comparison households were to be selected from the same communes as the treated ones (Ba et al., 2012). Since a list of farmers was not available for the comparison group, households for this group were selected based on their geographical proximity to the treatment households. The location of treated households was defined using as reference the communes' focal point for animators' project activities; the sites for data collection were chosen randomly from the list of sites available.[7] The treated households interviewed were chosen from within a virtual polygon—a 1km—side equilateral triangle whose centroid coincide with the focal point of the animator's project activities—while control households were identified in nearby areas with a high presence of mango production but outside the intervention polygon (Schwartz, 2013).[8]

According to a project report, "the sample size was determined to detect a 15 percent point difference in terms of change in key indicators for each group between the time of the baseline and the time of the follow-up survey, with a probability of 0.05 that this change is sample error, and a confidence level of 95 percent. As a result, the sample size was calculated to be 220 mango producers and increased to 250 to take into account possible dropouts in the treatment group. For the comparison group, the sample size was doubled to 500" (Ba et al., 2012).[9] But more observations were collected for the treatment (268) and comparison groups (510) (Table 2).[10]

The baseline survey collected information on household demographics, mango production (including costs), and mango-harvesting practices, as well as information on project participation activities (that is, participation in various groups and associations) and the types of training received by participants. Table 2 describes the sample distribution by treatment status and by commune. The number of treated and control observations are not evenly distributed across communes.

Table 3 compares socioeconomic characteristics between the treatment and the comparison group. Performing the t-test for equal means of the socioeconomic characteristics between the two groups shows there are statistically significant differences for some variables (7 out of 20 variables). In an average household in the treatment group, the household head is older and more likely to be female. A typical treatment household also plants more non-*Francique* trees and is more likely to rank mangos as their first source of income and first crop. Finally, the wall materials of the residences of the treatment households are significantly better than those of control households (though the homes are small overall). And compared with the control households, treatment households have less access to water from a river or well. This descriptive evidence reinforces the early discussion that the direct comparison of outcome variables between the treatment and control households leads to biased estimates, and thus indicates the need for alternative evaluation methods to account for the existing differences between the two groups.

A different firm was hired to conduct the follow-up survey in October 2013. With inputs from IDB and TechnoServe, the firm made a series of changes to the survey instruments. Only some variables from the baseline survey (on production, commercialization, sales, participation in training activities, and gender of household head) were kept. Additional questions on mango production, crop production, and plot information were newly added to the follow-up survey. Neither the baseline survey nor the follow-up survey collected any commune-level data. The sample size was reduced from the original 778 to 474 (Papyrus, 2013). Due to budgetary constraints a limit of 450 observations was set, 200 treated and 250 nontreated (Papyrus,

[7]The animator is the person in charge of contacting, recruiting, and training mango producers.

[8]These geographic parameters were defined based on TechnoServe's experience with the project.

[9]Unfortunately, this is the only information that is available to us regarding the sample design and power calculation.

[10]It is not clear from the project reports why the sample included more observations with respect to the sample design.

2013). But the actual number of observations was 47424 more than planned. Of the 474 observations, 211 were of treatment households and 263 of control households. The sample reduction most affected the observations of control households. The households interviewed in the follow-up were selected randomly from the list of households interviewed for the baseline. The number of treatment and control households was established to maximize statistical power, given the restrictions imposed by the budget. We conducted attrition analysis to determine whether the sample reduction affected our conclusions.

An additional 13 observations were not included in the baseline survey and were therefore excluded from our study. The total number of observations that include information from both the baseline survey and follow-up survey is 461205 treatment households and 261 control households.[11] Table 4 shows the distribution of the final observations used in our evaluation analysis by commune and by treatment and control groups. As in the baseline survey, the distribution of observation is uneven across communes, with two extreme cases. While Verettes has eight treatment observations but no control observation, the commune labeled as "other" only has 29 control observations but no treatment observation (Table 4).[12]

To check whether the attrited households and panel households are systematically different, we compared their pretreatment household and mango production characteristics within both the treated and control group, using data from the baseline survey (Table 5). Out of the 36 variables compared, we found statistically significant differences for five variables in the treatment group: the age of the household head in three categories (household head aged 4554, 3544, and 55 and more), and household access to water from a well and from a public water source. Out of the same 36 variables, the difference is statistically significant for only four variables in the control group: the age of household head in two categories (household head aged 2534, and 55 and more), the number of *Francique* trees, and the number of rooms. These results suggest no systematic difference exists between the attrited households and the panel households for the treated and control groups. Therefore, attrition bias is unlikely to affect our evaluation.

3.2 Methods

To estimate project impacts we use nonexperimental econometric methods to control for selection biases. To estimate the average treatment effect on the treated (ATT) we use propensity score matching (PSM); matching in covariates; difference-in-difference, propensity score matching (DID-PSM); and DID matching in covariates.

We use PSM and matching in covariates for the outcomes for which we do not have data from the baseline, namely the adoption of various production, harvest and postharvest, and commercialization best practices, and use DID-PSM and DID matching for the set of outcomes related to production and sales for which we have data from both the baseline and the follow-up surveys.

Matching relies on the conditional independence assumption, or unconfoundedness, and on the assumption of overlap (Heckman et al., 1997; Wooldridge, 2010), which states that the researcher should observe all variables simultaneously influencing the participation decision

[11]The fact that the number of observations is bigger in the control group than in the treatment group is generally consistent with the sample requirement for matching analysis (Imbens and Wooldridge, 2009; Khandker et al., 2010; Ravallion, 2009).

[12]Unfortunately we do not have further information on the sample design to explain why we observe this distribution of treated and control observations within communes.

and outcome variables, and that there is overlap between the probability distributions of treatment and control samples. With this method we control for bias on observable characteristics, caused by the targeting of project beneficiaries (for example, based on a set of eligibility criteria). We conduct matching on the propensity score using kernel matching (Heckman et al., 1997; Hirano et al., 2003; Jalan and Ravallion, 2003). Kernel matching is a nonparametric method that uses a weighted average of all the observations in the control group to construct the counterfactual outcome for each treated observation (Smith and Todd, 2005). The weights depend on the type of kernel function chosen. An advantage of kernel matching is that it reduces the variance of the estimates by using more information.

We also conduct matching in covariates (Abadie and Imbens, 2006; Imbens, 2014). This estimator consists of matching all units, treated and control, using the distance between the values of the covariates for each observation (in our case weighted by the sample variance matrix). If the matching is done with replacement, the order of observations does not matter. The matching can be conducted with one or more observations (one and five in our case); increasing the number of observations improves the quality of matching but increases the variances of the estimates (Smith and Todd, 2005). Since matching multiple covariates can lead to substantial bias, it is combined with bias adjustment to remove most of the bias. This approach uses linear regression to remove the bias associated with differences in the matched values of the covariates (Abadie and Imbens, 2011; Imbens, 2014). Bootstrapped standard errors are calculated for matching estimates to account for the two-step PSM procedure (Abadie and Imbens, 2008),[13] and robust standard errors are estimated for matching on covariates (Abadie and Imbens, 2011; Imbens, 2014).

As mentioned earlier, PSM relies on the assumption of unconfoundedness. But it is likely that there are systematic differences in outcomes for participants and nonparticipants due to unobservable characteristics, known as bias on unobservables. While it is impossible to directly address this problem using cross-sectional data, we conduct Rosenbaum bounds analysis to check the sensitivity of our estimates to deviations from the conditional independence assumption (Rosenbaum, 2002). For the outcome variables for which we have panel data, we use these to estimate the impacts using the DID-PSM (Smith and Todd, 2005) and DID-matching on covariates. Both methods control for time invariant heterogeneity.[14]

We follow a few standard procedures to estimate project impacts using the matching methods (Imbens, 2014; Imbens and Wooldridge, 2009; Wooldridge, 2010). The first step is to estimate the propensity scores (PS) using a probit or logit model. The next step is to check the overlap region of the estimated PS between the treatment and control group. Another step is to trim the observations with PS close to zero and one.

We estimate PS using a logit model and test whether higher polynomial terms are needed or not, following Dehejia and Wahba [2002]. With the estimated PS we can check for overlap of the probability distribution between the treated and control groups, by plotting the estimated PS for the two groups. A substantial overlap is crucial in order for the PSM method to work. Failing to identify substantial overlap is a major source of bias in PSM estimates of impacts because the counterfactual group is not similar to the treatment group. Following standard

[13]When conducting PSM, sensitivity analysis is also usually conducted to determine that the estimates are not sensitive to different matching methods. But when conducting PSM with the nearest neighbor, we are unable to obtain the correct standard errors for inference. For this reason we do not conduct PSM on the nearest neighbor.

[14]We lack data from before the baseline survey to test for parallel trends in treatment and control samples, but we make this assumption as done in other studies without long historical data.

matching procedures, we prune the observations with an estimated PS above 0.90 and below 0.10 to improve overlap (Imbens and Wooldridge, 2009; Ravallion, 2009; Wooldridge, 2010). With this trimmed sample we reestimate the PS and conduct the matching again.

We conduct a balancing test to check for the similarity of the marginal distribution of the covariates used to estimate the PS. The test aims to determine whether the matching procedures have served the purpose of making participants and nonparticipant groups more similar. Covariates are compared via a measure of standardized bias or normalized differences in means (Imbens and Wooldridge, 2009; Wooldridge, 2010).[15] To assess covariate balance we follow Imben's rule of thumb regarding percentage bias below 25 percent (Imbens and Wooldridge, 2009; Wooldridge, 2010).

A potential concern is that the presence of similar projects in the study sites could potentially bias the impact estimates. To check whether our estimated results are affected by this, we conduct a robustness check by reestimating the impacts using a reduced sample that includes only those households that have not participated in other projects, and compare the new results with the ones obtained using the whole sample.

Because the conditional independence assumption is a strong one, we carry out a sensitivity analysis to determine how strong an unmeasurable variable must be to influence the selection process and the outcome of interest as to undermine the conclusions. If there are unobservable variables that simultaneously influence participation and outcome variables, matching only based on observable characteristics may lead to biased estimates. In order to determine how sensitive the matching results are to deviations from the conditional independence assumption, we also conduct the Rosenbaum bounds sensitivity analysis (Rosenbaum, 2002), a popular exercise after matching methods (Dillon, 2011b; Ogutu et al., 2014; Rusike et al., 2009).

4 Results

In this section, we describe farmers' participation in various project activities. We then briefly present the results of PS regression and check the quality of matching. Most of our discussion is devoted to the evaluation results based on PSM, matching on covariates, DID-PSM, and DID-matching on covariates.[16] We conclude by discussing results from the robustness check and Rosenbaum Bounds analysis.[17]

4.1 Participation in Project Activities

Farmers' decision to participate in the program or not was voluntary. After one year of project activities, over 50 percent of participants had received training in improved production and harvest and postharvest practices, and business and commercialization practices (Table 6). Meanwhile, 87 percent of beneficiaries identified themselves as members of a PBG, and 56 percent or more as involved in PBG activities (Table 8). As can be seen in Table 6, Table

[15] We use the normalized mean difference instead of the standard t-test for equal means, because the former does not depend on the sample size. For instance, the t-statistic may be large in absolute value simply because the sample is large, and small differences between sample means are statistically significant even if the absolute difference is substantially small. For more details, please refer to Imbens [2014], Imbens and Wooldridge [2009], and Wooldridge [2010].

[16] For the estimation of the PS, PSM, and DID-PSM methods, we use psmatch2 in STATA (Leuven and Sianesi, 2012). For the matching in covariates and DID-matching in covariates we use nnmatch in STATA (Abadie et al., 2004).

[17] To estimate Rosenbaum bounds we use mhbounds in STATA (Becker and Caliendo, 2007)

7, and Table 8, households in the control group also participate in activities similar to those promoted by the project, which is explained by the presence of similar projects in the study area (AGRITECH, 2014; Castañeda et al., 2010). We address this issue (a possible source of bias in our results) by conducting a robustness check, which will be discussed in section 4.7.

4.2 Propensity Score Estimation

The probability of program participation, or PS, is estimated using a logit model. The dependent variable is a dichotomous variable for whether or not the household was a beneficiary of the project. The explanatory variables in the logit model are a set of variables from the baseline survey that include information on household head characteristics, housing, children's schooling, mango production and its perceived importance to household income, access to water, access to markets, and so on.

First we estimate the PS with all the observations in the sample. Following standard procedures for PSM, we trim the sample by eliminating the observations with PS >0.90 and PS <0.10, and reestimate the PS based on the trimmed sample (Imbens, 2014; Imbens and Wooldridge, 2009; Wooldridge, 2010). A total of eight nontreated observations were trimmed. We check the specification of the PS using the method suggested by Dehejia and Wahba [2002]. We find that we do not need to include higher polynomials of the variables, or additional variables, for the estimation. Based on the estimated results of the logit model, the beneficiary households were more likely to be headed by a female, to cite mangoes as their main source of income, and to have fewer *Francique* mango trees than households in the control group (Table 9). Beneficiary households were also likely to lack access to water from a river, spring, or pump, but had residences with more adequate wall materials (Table 9).

In order to visually examine how the beneficiary and nonbeneficiary households overlap each other in terms of PS, we present in Figure 1 the predicted probability of selection for the project among both treated and nontreated households.

As can be seen in Figure 1, there is overlap for a good range of PS except for a few observations in the very right tail (PS >0.75). There are fewer control observations to match treated ones at values greater than 0.75 of the estimated PS. This is not an issue for estimating the program impacts using PSM, however, because we are able to find nontreated observations similar to the treated ones within a wide range of estimated PS values.

A useful criterion for measuring the quality of matching is to check the treated group against the control group in terms of observed characteristics before and after matching.

Table 10 presents the difference for the key variables between the treated and control groups before and after matching. Figure 2 presents the estimated PS with the weighted observationsboth treated and controlby the weights generated for the kernel matching algorithm. We find that matching improves the balance between the two groups, as supported by the fact that the absolute value of normalized mean differences between the treatment and control groups is much smaller after matching than before matching for the majority of the variables. In fact, the absolute value for the normalized difference in means (percent bias) for all the covariates is below 25 percent, an indicator of a good balance of covariates (Imbens and Wooldridge, 2009). For PSM and PSM-DID we confirm overlap improvement (see Figure 2).

4.3 Impacts on Production and Sales

We first present the estimated impacts of the project using DID-PSM and DID matching in covariates for the outcomes related to mango production and sales.[18] We then present the results using PSM and matching in covariates for the adoption of best practices promoted by the project. For both DID-PSM and PSM, we present the results based on kernel matching methods.

Table 11 reports the estimated impacts of the project on the total number of *Francique* mango trees, and whether these trees are productive or immature, as well as the total value of sales. We estimate these impacts using DID-PSM and DID matching in covariates.[19] The results suggest that the program has a significant and positive effect on the total number of *Francique* trees, and that these trees are young (that is, not yet productive). On average, treated farmer households increased their number of *Francique* trees by 12.3, and number of immature trees by 12.4. These results are significant at the 10 percent level, respectively, and indicate that those new *Francique* trees are saplings planted by farmers as a result of project participation. But when comparing the number of productive *Francique* trees we find no statistically significant difference between the treatment and control groups. Consequently, we do not find any statistically significant difference in mango sales. It takes more than three years for a mango tree to bear fruit and become productive (University of Hawai Manoa, 2014); it is therefore too early to expect newly planted trees to contribute to production and sales.

4.4 Impacts on Adoption of Production Best Practices

In addition to promoting the planting of *Francique* trees, the project also promotes the adoption of best practices in production. Improving these practices is widely accepted as a necessary condition for higher yields and profits in the future (IFAD, 2011; Snelder et al., 2007; World Bank, 2008). Since information on the adoption of these best practices is available only from the follow-up survey, we estimate the program effects on these indicators using PSM and matching in covariates to analyze the cross-sectional data. Table 12 reports the estimated program effects on the adoption of various production best practices.

The results suggest that the project has significantly increased the adoption of the production best practices promoted by the projectthat is, pruning, tidying, grafting, and construction of fences. The share of households who adopted these practices is significantly (p-values less than 0.10) higher in the treatment group than in the control group. Production practices such as pruning prevent trees from getting too large; reshape intermediate trees into smaller, more manageable ones; and rejuvenate large trees that are no longer productive (Davenport, 2006). Information from plot trials indicates that it takes more than a year of applying practices such as pruning to translate into increases in fruit yield (Yeshitela et al., 2003). Therefore, it is not surprising that the adoption of these improved practices has not yet translated into increases

[18] We tested for the quality of the data collected for these variables using Benford's law (first digit law) (Judge and Schechter, 2009). We find that in general the data collected in the follow-up survey tend to follow the law for most of the variables, except for the total number of young trees. But the baseline data tend not to follow the law with the exception of the number of non-*Francique* trees.

[19] Unlike PSM by itself, the DID estimator in the common support accounts for potential sources of selection bias—potential unobserved, time invariant heterogeneity—whose fixed component is cancelled out via differencing data on treatment and controls before and after the intervention. Using the DID approach we control for differences in risk aversion, personality, and innate ability that might affect self-selection into the program.

in production and sales. Moreover, these results should be read with caution because they are sensitive to the potential presence of selection bias (see section 4.8).

4.5 Impacts on Adoption of Harvest and Postharvest Best Practices

The results for harvest and postharvest best practices (sorting of mangos and transportation for commercialization) are reported in Table 12, but the results are not statistically significant and indicate that farmers participating in the project did not adopt these practices. The results suggest a statistically significant increase in the use of "rice sack on head" as a mean of transportation, but the magnitude of the difference is low.

According to TechnoServe, farmers are not adopting these best practices because they are not the ones who carry out these activitiesspecialized harvesters who have not received the training are the ones who do so; these specialized harvesters work for the middlemen.

4.6 Impacts on Marketing Channels Promoted by the Project

One intention of the project is to help mango farmers sell their mangoes through modern market channels (the cell or PBG) instead of through traditional channels (middlemen), as the latter are associated with higher transaction costs and lower bargaining power. The PSM and matching in covariates results suggest, on the one hand, that the share of farmers who sold their mangoes through a cell or PBG is significantly higher, at the 1 percent level, in the treatment group than in the control group (Table 12). On the other hand, the share of mango farmers who sold their products to middleman is significantly lower, at the 1 percent level, in the treatment group than in the control group. These results indicate that the project has been effective in promoting preferred commercialization channels among mango farmers.

4.7 Robustness Checks

One concern we have is that similar activities promoted by other organizations in the study sites may have biased our estimated impacts (AGRITECH, 2014; Castañeda et al., 2010). We estimate project impacts for a subsample of treated and nontreated households that did not receive any training from other organizations. We dropped the households that were reported to have received training from organizations other than TechnoServe. In total we dropped 120 observations, 60 from the treated group and 60 from the control group. We conducted this robustness check analysis with a total of 341 observations, 145 treated and 196 control observations.

We estimate the PS using the same specification as in section 4.2 (Table 13). Following standard procedures, we trimmed three observations with PS>0.90 and PS<0.10 and dropped four additional observations because of missing data. A total of 334 observations were used for the analysis. Table 13 presents the estimated PS model, and Figure 3 presents the estimated PS for the reduced sample. Figure 4 displays the estimated PS for both treatment and control households with the control observations, weighted by the weights generated by the kernel matching algorithm. The latter confirms how matching improves the overlap between the probability distribution treatment and control groups.

As can be seen from Table 14, the results for the reduced sample and the entire sample are very similar both in terms of sign, magnitude, and level of significance, suggesting that our estimated results on project impacts are unlikely to be caused by other organizations promoting similar interventions in the project areas. This is expected since there is no statistically significant difference in the proportion of producers that receive training from other organizations, other than TechnoServe, in the treatment and control groups (Table 6). The proportion of producers that receive training from other organizations remains statistically the same in both groups after matching.[20]

4.8 Sensitivity Analysis Using Rosenbaum Bounds

Finally we estimate the Rosenbaum bounds for the PSM results to assess the sensitivity and reliability of the PSM estimates (Table 15).[21] Different values of Γ correspond to different assumptions on the effect of an unobservable variable in the odds ratio of treatment assignment in otherwise similar casesin terms of the observable variables included in the participation model. It has been argued that varying the values of Γ from 0 to 2 (doubling the weight of unobserved variables in the selection) is a sufficient range to test the effect of hidden bias (Aakvik, 2001; Dillon, 2011a; DiPrete and Gangl, 2004). As pointed out by Aakvik [2001], a value of Γ=2 implies that two subjects with the same observable characteristics differ in their odds of participating in a program by a factor of two, or 100 percent, which is unlikely given that the participation model adjusts for many important observable characteristics.

Given the positive significant effects we obtain in our matching analysis, we first present the bounds results under the assumption that we overestimated project impacts due to positive unobserved selection—that is, project participants are more likely than nonparticipants to adopt best practices, and to sell their product via the channels recommended by the project. We conduct the sensitivity analysis only for statistically significant results. The sensitivity of the estimated impacts to potential unobserved factors varies across variables (Table 15).

The results on the adoption of commercialization and harvest and postharvest best practices are insensitive to the potential effect of unobservable variables (columns 5–7) (Table 15). For example, the estimated effects on the farmers' choice to sell products through a PBG or cell, or through a middleman, remain significant at the 1 percent level even after the odds ratio of treatment assignment doubled (Γ>2). That is, the results remain significant at the 1 percent level even in the presence of an unobservable variable that causes the odds of participating in the program to differ by a factor of 2 for two subjects that have the same observable characteristics. The result for selling to an association is insensitive to the effect of an unobservable variable that causes the odds ratio of treatment assignment to differ by more than 70 percent (or Γ>1.7 column 4). To gain a better understanding of the magnitude of hidden bias required to upset the latest, most sensitive result, we calculate the equivalent effect of some observable variables included in the participation model: the effect of an unobservable variable capable of causing the odds ratio of treatment assignment to differ by more than 1.7 would be equivalent to the effect of a difference in 4.2 non-*Francique* trees, or a difference in 59.1 *Francique* trees for two otherwise identical producers.

Compared with the adoption of commercialization and harvest and postharvest best practices, the adoption of production best practices is more sensitive to the potential presence of

[20] Although the evaluation was not designed to measure spillover effects (we do not include in the study an additional group of mango producers located out of reach of the areas of intervention), some of the results presented in Table 8 suggest that the impact of the study may be underestimated. Although producers in the control group are formally not direct beneficiaries of the project, they report to benefit from activities carried out and knowledge disseminated via PBG which suggest our estimates are conservative.

[21] The sensitivity analysis is done using matched pairs—one near neighbor matching algorithm in our case. See Becker and Caliendo [2007].

hidden bias (columns 1–3) (Table 15). The results for the construction of fences are insensitive to hidden bias, with the positive effect staying significant at the 10 percent level in the case of the odds ratio of treatment assignment doubling ($\Gamma = 2$). The results for pruning are also reasonably insensitive, becoming not significant at the 10 percent level for values of $\Gamma > 1.5$. Of the three best practices associated with production, the results for grafting trees are the most sensitive to the effects of unobservables: the estimated positive effect becomes not significant at the 10 percent level when the odds ratio of treatment assignment reaches values of $\Gamma > 1.2$ (Table 15). This amount of hidden bias is equivalent to the effect of a difference in 20.3 *Francique* trees, or 14.3 in the number of non-*Francique* trees in the probability of the producer being assigned to treatment. So, although the results for grafting trees are the most sensitive to the potential presence of an unobservable variable affecting the odds ratio of treatment assignment, the equivalent size effect of that variable is relatively largea difference of 20.3 *Francique* trees—in otherwise similar mango producers.

The results for the assumption that we have underestimated project impacts due to negative unobserved selection—that is, participants in the project are less likely to adopt the best practices—are less interesting (Becker and Caliendo, 2007; DiPrete and Gangl, 2004). We find that for the outcomes related to the adoption of improved practices (pruning, tidying, grafting, and fencing), the p-values indicate that the results are insensitive for all the values of Γ considered (columns 1–3) (Table 15). We obtain similar results for some of the commercialization and harvest and postharvest best practices (columns 4 and 6) (Table 15).

The results of the sensitivity analysis suggest that, in most cases, we can be confident that the estimated results are robust to the possible presence of selection bias. In some cases—pruning, grafting—the results are sensitive to moderately strong, confounding variables under the (worst-case scenario) assumption that such confounding variables have a strong impact not only on selection but on the variable of interest. The assumption is that an unobserved variable's effect on these results is so strong as to almost perfectly determine whether the result would be bigger for the treatment (overestimation of treatment effect) or control (underestimation of treatment effect) case in each pair of matched cases in the data.

5 Discussion and Conclusion

This study contributes to the scant but growing literature evaluating the impacts of agricultural development programs using rigorous analysis approaches. In particular, this study evaluates the efficacy of a project in promoting the early adoption of best practices. The descriptive evidence suggests that farmers participating in the project are learning new practices, participating in extension activities, and being active in newly formed associations.

Methodologically, we use both the matching methods and the combination of DID and matching methods to evaluate the short-term impacts of the project. We use these methods to address the nonrandomized selection of program sites and farmers' self-selection into the project. Five main results emerge. First, we find that the project has positive and significant effects on the number of new *Francique* mango trees, a variety that has better market and export potential than other varieties. Second, the project has positive and significant effects on the adoption of the promoted production best practices such as pruning, tidying, and grafting of mango trees, and fencing mango orchards to protect them from animals (results for pruning and grafting should be interpreted with caution, since they are sensitive to possible deviations from the identifying unconfoundedness assumption). Third, the project has no effect on the adoption of harvest and postharvest best practices, mainly because the training is aimed at mango producers but these activities are most often carried out by specialized harvesters. Fourth, the project has significant effects on the adoption of preferred commercialization channels: specifically, the project helps farmers shift from selling their mangoes to middleman to selling them to PBGs, which increases revenues from mango sales by reducing transaction costs and increasing mango farmers' bargaining power. It also has led farmers to sell mangoes by the dozen instead of by tree or plot, which increases the price they get for mangos. Fifth, the adoption of improved production practices and preferred marketing channels has not yet translated into increases in production or sales. This is not surprising given that production and total sales depend on new production methods to take effect and trees to mature—processes that take several years at minimum.

We conduct additional analyses to address concerns that the main results might be affected by other potential confounding factors. Results based on a subsample of households that did not report participating in other programs are similar to results based on the whole sample; these suggest that the results are robust to the presence of other similar programs in the project areas. We estimate Rosenbaum bounds to check the sensitivity of the main results to selection on unobservables. The bounds analyses suggest that except for two of the promoted production best practices—grafting and prun—the main results are in general insensitive to the effects of potential hidden bias. The most robust results are those related to the commercialization channels.

Table 1: Household and institutional level interventions (2010–2016)

Household level interventions	
Agricultural Best Practices	- Pruning - Orchard maintenance: cleaning weeds, fencing - Grafting - Nursery management - Harvest practices
Post harvest	- Postharvest handling - Post harvest transportation for commercialization
Business and commercialization	- PBG formation and management - Organization, governance and business planning - Financial literacy - Bookkeeping
Access to credit	- Credit for inputs - Credit management
Other	- Off farm income generating activities
Institutional level interventions	
Institutional development	- Facilitating partnerships with research institutions to identify mango varieties suitable for processing - Setting up market service centers - Establishing direct relationships with exporters - Collaborating with other donors and the government to improve transportation infrastructure - Work with stakeholders in the mango supply chain to decrease transaction costs - Improve circulation of information on prices

Table 2: Baseline data distribution, by commune

Commune	Treatment		Control	
	Freq.	Percent	Freq.	Percent
Boucan Carre	10	3.73	18	3.53
Ennery	19	7.09	37	7.25
Gros Morne	44	16.42	89	17.45
Hinche	30	11.19	10	1.96
La Chapelle	34	12.69	26	5.1
Mirebalais	29	10.82	72	14.12
Other	4	1.49	63	12.35
Petite Rivie Artibonite	33	12.31	104	20.39
Saut-D'Eau	18	6.72	48	9.41
Thomonde	35	13.06	43	8.43
Verettes	12	4.48		
Observations	268	100	510	100
Total	778			

Source: Authors' own calculation based on baseline survey 2012.

Table 3: Baseline data comparison, by household characteristics and mango production

Variables	Treatment		Control		Diff	p-value†
	Mean	Sd	Mean	Sd		
Head Female	0.39	0.49	0.29	0.45	0.10	0.01
Head age 18–24 yrs	0.03	0.18	0.06	0.23	-0.02	0.12
Head age 25–34 yrs	0.18	0.38	0.16	0.37	0.02	0.52
Head age 35–44 yrs	0.24	0.43	0.26	0.44	-0.02	0.54
Head age 45–54 yrs	0.33	0.47	0.24	0.43	0.09	0.01
Head age 55 and older	0.22	0.42	0.29	0.45	-0.06	0.06
Francique (number)	7.16	11.94	7.60	12.40	-0.43	0.96
non-Francique (number)	9.68	15.73	15.58	24.57	-5.89	0.00
Mango 1st income source	0.36	0.48	0.23	0.42	0.13	0.00
Mango 2nd income source	0.38	0.49	0.38	0.49	0.00	0.96
Mango 3rd income source	0.21	0.41	0.25	0.44	-0.04	0.20
Mango 1st crop	0.45	0.50	0.25	0.43	0.20	0.00
Mango 2nd crop	0.36	0.48	0.37	0.48	-0.01	0.77
Mango 3rd crop	0.13	0.34	0.26	0.44	-0.12	0.00
Transport foot path	0.19	0.39	0.17	0.38	0.01	0.63
Transpot motorcycle	0.01	0.09	0.01	0.08	0.00	0.80
Transport PackAnimal	0.66	0.47	0.71	0.45	-0.05	0.18
Transport Pickup	0.07	0.25	0.08	0.27	-0.01	0.50
Transport Truck	0.01	0.11	0.00	0.06	0.01	0.30
Household members (number)	5.88	2.20	5.92	3.25	-0.04	0.85
Number of rooms (number)	2.80	0.99	2.78	1.27	0.02	0.79
Roof Cement	0.04	0.21	0.02	0.15	0.02	0.14
Roof Thatch	0.13	0.34	0.17	0.38	-0.04	0.16
Roof Tin	0.85	0.36	0.82	0.39	0.03	0.27
Wall Carton	0.01	0.11	0.00	0.04	0.01	0.17
Wall Wadd	0.09	0.29	0.14	0.35	-0.05	0.05
Wall Rock	0.48	0.50	0.55	0.50	-0.07	0.06
Wall Block	0.20	0.40	0.13	0.34	0.07	0.02
Wall Wood	0.07	0.25	0.02	0.14	0.05	0.00
Wall Palm	0.19	0.39	0.21	0.41	-0.02	0.49
Floor Tile	0.01	0.09	0.00	0.06	0.00	0.55
Floor Cement	0.43	0.50	0.40	0.49	0.03	0.44
Floor Earth	0.58	0.49	0.61	0.49	-0.03	0.43
Water Rain	0.09	0.28	0.11	0.31	-0.02	0.32
Water River	0.25	0.44	0.37	0.48	-0.11	0.00
Water Spring	0.43	0.50	0.44	0.50	-0.01	0.82
Water Pump	0.19	0.39	0.22	0.42	-0.03	0.28
Water Well	0.12	0.33	0.19	0.39	-0.07	0.01
Public Water	0.90	0.30	0.91	0.29	-0.01	0.63
Distance Water 1–5 minutes	0.24	0.43	0.26	0.44	-0.01	0.68
Distance Water 6–10 minutes	0.16	0.37	0.20	0.40	-0.04	0.12
Distance Water 11–30 minutes	0.21	0.41	0.19	0.39	0.02	0.52
Distance Water more 30 minutes	0.26	0.44	0.24	0.43	0.02	0.49
Distance Water onsite	0.12	0.33	0.11	0.31	0.02	0.52
Females 18 or younger in school	3.15	1.09	3.12	1.24	0.02	0.85
Males 18 or younger in school	3.05	1.10	3.05	1.12	0.00	0.94

Source: Authors' own calculation based on baseline survey 2012.
Note: Observations: 268 treatment, 510 control, total 778.
(†) for an unpaired samples t-test for equal means, with unequal sample size.

Table 4: Distribution by commune for the final sample; observations with data in both the baseline survey and the follow-up survey

Commune	Treatment		Control	
	Freq.	Percent	Freq.	Percent
Boucan Carre	10	4.88	13	5.08
Ennery	13	6.34	21	8.2
Gros Morne	37	18.05	43	16.8
Hinche	21	10.24	3	1.17
La Chapelle	28	13.66	11	4.3
Mirebalais	16	7.8	32	12.5
Other	0	0.00	29	11.33
Petite Rivie Artibonite	27	13.17	54	21.09
Saut-D'Eau	16	7.8	26	10.16
Thomonde	29	14.15	24	9.38
Verettes	8	3.9	0	0.00
Total	205	100	256	100
Total observations	461			

Source: Authors' own calculation based baseline survey 2012 and follow-up survey 2013.

Table 5: Comparison of baseline data from households in the analysis sample and those dropped from the sample

Variable	Treatment group			Control group		
	Panel sample	Dropped sample	Diff	Panel sample	Dropped sample	Diff
Head Female	0.38	0.4	-0.02	0.28	0.3	-0.02
Head age 18-24 yrs	0.02	0.06	-0.04	0.05	0.06	-0.01
Head age 25-34 yrs	0.16	0.25	-0.09	0.1	0.22	-0.12***
Head age 35-44 yrs	0.2	0.37	-0.17***	0.28	0.23	0.05
Head age 45-54 yrs	0.36	0.22	0.14**	0.24	0.24	0
Head age 55 and older	0.26	0.1	0.16***	0.33	0.25	0.08**
Francique (number)	7.02	7.62	-0.6	8.52	6.67	1.85*
Non-Francique (number)	9.99	8.68	1.31	15.04	16.16	-1.12
Mango 1st income source	0.36	0.38	-0.02	0.23	0.22	0.01
Mango 2nd income source	0.39	0.36	0.03	0.4	0.37	0.03
Mango 3rd income source	0.21	0.22	-0.01	0.25	0.26	-0.01
Mango 1st crop	0.43	0.48	-0.05	0.26	0.24	0.02
Mango 2nd crop	0.37	0.34	0.03	0.38	0.36	0.02
Mango 3rd crop	0.13	0.14	-0.01	0.25	0.26	-0.01
Transport foot path	0.18	0.21	-0.03	0.15	0.19	-0.04
Transpot motorcycle	0.01	0	0.01	0.01	0	0.01
Transport PackAnimal	0.67	0.62	0.05	0.73	0.69	0.04
Transport Pickup	0.08	0.03	0.05	0.08	0.08	0
Transport Truck	0.01	0.02	-0.01	0.01	0	0.01
Household members (number)	5.89	5.77	0.12	6.29	5.56	0.73***
Number of rooms (number)	2.81	2.77	0.04	2.74	2.83	-0.09
Roof Cement	0.04	0.06	-0.02	0.02	0.02	0
Roof Thatch	0.14	0.1	0.04	0.18	0.15	0.03
Roof Tin	0.84	0.89	-0.05	0.82	0.83	-0.01
Wall Carton	0.01	0.02	-0.01	0	0	0
Wall Wadd	0.1	0.08	0.02	0.14	0.13	0.01
Wall Rock	0.49	0.47	0.02	0.56	0.55	0.01
Wall Block	0.22	0.16	0.06	0.11	0.15	-0.04
Wall Wood	0.07	0.06	0.01	0.02	0.02	0
Wall Palm	0.18	0.23	-0.05	0.23	0.2	0.03
Floor Tile	0	0.02	-0.02	0	0.01	-0.01
Floor Cement	0.43	0.45	-0.02	0.39	0.41	-0.02
Floor Earth	0.59	0.56	0.03	0.63	0.6	0.03
Water Rain	0.08	0.11	-0.03	0.11	0.11	0
Water River	0.25	0.26	-0.01	0.36	0.38	-0.02
Water Spring	0.42	0.48	-0.06	0.45	0.44	0.01
Water Pump	0.2	0.18	0.02	0.23	0.22	0.01
Water Well	0.14	0.06	0.08*	0.19	0.18	0.01
Public Water	0.88	0.95	-0.07**	0.91	0.91	0
Distance Water 1-5 minutes	0.25	0.24	0.01	0.26	0.25	0.01
Distance Water 6-10 minutes	0.16	0.15	0.01	0.19	0.23	-0.04
Distance Water 11-30 minutes	0.21	0.23	-0.02	0.2	0.18	0.02
Distance Water more 30 minutes	0.25	0.31	-0.06	0.25	0.23	0.02
Distance Water onsite	0.14	0.08	0.06	0.11	0.11	0
Females 18 or younger in school	3.11	3.22	-0.11	3.17	3.08	0.09
Males 18 or younger in school	3.04	3.08	-0.04	3.04	3.06	-0.02

Source: Authors' own calculation based on baseline survey 2012 and follow-up survey 2013.
Note: Observations for the treated group, 204 panel observations and 63 dropped; for the control group, 256 panel observations and 252 dropped.
Significance levels are indicated by: *** at the 1 percent level; ** at the 5 percent level; * at the 10 percent level.

Table 6: Farmers' participation in project training activities, 2013

Training activity	Treatment		Control		Diff.	p-value†
	Mean	Sd	Mean	Sd		
Plot management	0.93	(0.26)	0.49	(0.50)	0.44	0.00
Pruning or tree care	0.68	(0.47)	0.29	(0.45)	0.39	0.00
Composting	0.6	(0.49)	0.28	(0.45)	0.32	0.00
Organic pest control	0.37	(0.48)	0.13	(0.34)	0.24	0.00
Nursery management	0.66	(0.47)	0.32	(0.47)	0.34	0.00
Harvest and post harvest practices	0.4	(0.49)	0.14	(0.35)	0.26	0.00
Internal transport	0.52	(0.50)	0.15	(0.36)	0.37	0.00
Business skills/SB production	0.59	(0.49)	0.19	(0.39)	0.4	0.00
Organizing groups	0.56	(0.50)	0.15	(0.36)	0.41	0.00
Income statement (calculating profits and loss)	0.52	(0.50)	0.18	(0.39)	0.34	0.00
Traceability or fair trade	0.53	(0.50)	0.14	(0.35)	0.39	0.00
Equality of men and women	0.62	(0.49)	0.19	(0.39)	0.43	0.00
Microcredit	0.53	(0.50)	0.16	(0.36)	0.37	0.00
Training from TNS	0.68	(0.47)	0.35	(0.48)	0.33	0.00
Training from other agencies	0.29	(0.46)	0.24	(0.43)	0.05	0.17
Observations	205		256			
Total	461					

Source: Authors' own calculation based on follow-up survey 2013.
(†)t-test of equal means, unpair unequal variance.

Table 7: Participation in project extension activities, 2013

Extension service and service for visit‡	Treatment		Control		Diff.	p-value†
	Mean	Sd	Mean	Sd		
Fertilizer	0.13	0.33	0.05	0.22	0.08	0.01
Rehabilitating mango orchands	0.68	0.47	0.26	0.44	0.42	0.00
Planting mango orchands	0.34	0.48	0.17	0.38	0.17	0.00
Support for other production (no mango)	0.2	0.4	0.05	0.22	0.15	0.00
Irrigation equipment	0.27	0.45	0.11	0.31	0.16	0.00
Need seed varieties	0.32	0.47	0.05	0.22	0.27	0.00
Pest control support (pesticides or traps)	0.33	0.47	0.1	0.3	0.23	0.00
Help with soil (composting, errosion, etc)	0.29	0.46	0.08	0.28	0.21	0.00
Disaster mitigation (insurance)	0.63	0.48	0.24	0.43	0.39	0.00
Certified mango producer	0.49	0.5	0.11	0.32	0.38	0.00
Equitable certified mango producer	0.78	0.42	0.19	0.39	0.59	0.00
Organic certified mango producer	0.25	0.43	0.07	0.26	0.18	0.00
Observations	205		256			
Total	461					

Source: Authors' own calculation based on follow-up survey 2013.
(†)t-test of equal means, unpaired unequal variance.
(‡)Services provided in situ by TechnoServe technicians.

Table 8: Participation in producer business group (PBG) or other association, 2013

Membership to farmers group	Treatment		Control		Diff.	p-value†
	Mean	Sd	Mean	Sd		
PBG	0.87	0.33	0.33	0.47	0.54	0.00
Cooperative	0.12	0.32	0.05	0.22	0.07	0.01
Farmer association	0.51	0.50	0.30	0.46	0.21	0.00
Sell mangos to a PBG	0.71	0.46	0.32	0.47	0.39	0.00
Receive credit through a PBG	0.56	0.50	0.13	0.34	0.43	0.00
Participate in mango season planning with a PBG	0.72	0.45	0.19	0.39	0.53	0.00
Receive training from a neighbor or PBG member	0.80	0.40	0.34	0.47	0.46	0.00
Other activity with a cell	0.23	0.42	0.27	0.44	-0.04	0.55
Observations	205		256			
Total	461					

Source: Authors' own calculation based on follow-up survey 2013.
(†)t-test of equal means, unpaired unequal variance.

Table 9: Logit model results for propensity score estimation

Dependent variable: Project participant
n=449 (8 observations trimmed, 4 observations with missing data)

Independent variables	Coef.	Std. Err.	P>z
Female=1	0.41	0.22	0.06
Head_age	0.02	0.04	0.63
Head_age_sq	0.00	0.00	0.61
Members (number)	-0.03	0.05	0.52
Rooms (number)	0.04	0.11	0.73
Mango 1st to 3rd income source	1.43	0.64	0.03
Mango 1st to 3rd crop	-0.44	0.59	0.46
Francique (number)	-0.01	0.01	0.37
Non-Francique(number)	-0.01	0.01	0.05
Footpath=1	-0.29	0.54	0.59
PackAnim=1	-0.48	0.50	0.34
Pickup_truck=1	-0.52	0.59	0.38
inad_roof=1	0.20	0.66	0.76
inad_walls=1	-0.83	0.33	0.01
inad_floor=1	-0.02	0.24	0.94
Water_river_spring=1	-0.68	0.29	0.02
Water_pump_well=1	-0.70	0.27	0.01
PublicWater=1	-0.22	0.37	0.56
Distwater_10minless=1	-0.19	0.26	0.47
DistWater10_30=1	0.04	0.30	0.89
Girls in school(number)	-0.08	0.10	0.46
Boys in school(number)	0.11	0.11	0.31
Constant	0.44	1.49	0.77
Log likelihood	-289		

Source: Authors' own calculation.

Table 10: Balancing tests for the matching procedure

Variable	Before matching				After matching			
	Treatment Mean	Control Mean	%bias	p-value	Treatment Mean	Control Mean	%bias	p-value
Female=1	0.38	0.28	15.03	0.03	0.38	0.34	9.4	0.35
Head_age*	47.44	47.96	-2.78	0.68	47.44	48.49	-7.9	0.41
Head_age_sq*	2415	2484	-3.71	0.58	2416	2516	-7.7	0.43
Members (number)	5.91	6.09	-4.58	0.48	5.94	5.86	2.7	0.76
Rooms (number)*	2.81	2.72	5.96	0.35	2.80	2.85	-4.6	0.66
Mango 1st to 3rd income source	0.96	0.89	18.69	0.01	0.96	0.96	0.0	1.00
Mango 1st to 3rd crop	0.94	0.89	12.75	0.08	0.94	0.97	-11	0.17
Francique (number)	7.03	7.71	-4.61	0.5	7.09	6.67	4.0	0.66
Non-Francique (number)	9.96	13.57	-14.26	0.03	10.03	10.58	-3.0	0.72
Footpath=1	0.18	0.15	5.72	0.31	0.18	0.16	5.3	0.60
PackAnim=1	0.67	0.73	-9.22	0.23	0.67	0.69	-4.3	0.67
Pickup_truck=1	0.09	0.09	0.00	0.86	0.09	0.07	6.8	0.46
inad_roof=1	0.96	0.98	-8.47	0.25	0.96	0.93	20.2	0.13
inad_walls=1	0.79	0.88	-17.30	0.01	0.79	0.79	-1.3	0.90
inad_floor=1	0.58	0.62	-5.71	0.37	0.58	0.59	-3.0	0.76
Water_river_spring=1	0.6	0.67	-10.31	0.13	0.60	0.54	12.3	0.23
Water_pump_well=1	0.33	0.39	-8.84	0.18	0.33	0.33	0.0	1.00
PublicWater=1	0.88	0.91	-7.06	0.33	0.88	0.88	0.0	1.00
Distwater_10minless=1	0.54	0.55	-1.41	0.82	0.54	0.53	3.0	0.77
DistWater10_30=1	0.21	0.21	0.00	0.91	0.21	0.24	-7.2	0.48
Girls in school(number)	2.11	2.17	-3.59	0.58	2.12	2.17	-4.2	0.66
Boys in school(number)	2.04	2.02	1.24	0.85	2.05	2.01	3.9	0.68

Source: Authors' own calculation.
(*)Number of observations 203 treated, and 247 control.

$$\%bias = \frac{\bar{x}_1 - \bar{x}_2}{\sqrt{s_1^2 + s_2^2}} \times 100$$

Table 11: Project impacts on outcomes related to production and sales, 2012–2013

	DID-PSM Kernel		DID-Matching in covariates nn(5)		nn(1)	
Outcomes	coef	se	coef	se	coef	se
Production and sales (2013–2012):						
DIFF Number of *Francique* trees	12.29*	(6.45)	11.34**	(4.61)	9.60**	(5.51)
DIFF Number of other type of mango trees	0.97	(6.83)	-3.74	(4.88)	-1.63	(6.31)
DIFF Total number of mango trees	12.98	(8.48)	8.78	(6.63)	12.52*	(6.97)
DIFF Number of young mango trees	12.38*	(7.30)	10.76**	(5.38)	8.27	(7.14)
DIFF Number of productive mango trees	-1.73	(7.07)	-3.67	(5.24)	-0.07	(3.63)
DIFF total sales USD	6.02	(44.88)	-5.40	(34.63)	-20.30	(48.56)
DIFF sales by tree USD	1.29	(2.34)	2.01	(2.31)	2.92	(2.37)

Source: Authors' own calculation based on baseline survey 2012 and follow-up survey 2013.
Note: DIFF = difference; nn = nearest neighbor; coef = estimated coefficient; se = standard error.
A total of 449 observations were used for matching, 203 treated and 246 controls, 4 observations off support.
For the DID-PSM estimation bootstrap standard errors with 500 repetitions in parenthesis; for the DID-matching in covariates estimation, robust standard errors in parenthesis.
Significance levels are indicated by: *** at the 1 percent level; ** at the 5 percent level; * at the 10 percent level.

Table 12: The adoption of best practices promoted by the project, 2013

	PSM Kernel		Matching in covariates nn(5)		nn(1)	
Outcomes	coef	se	coef	se	coef	se
Adoption of improved practices 2013:						
Trim the mango tree	0.20***	(0.05)	0.21***	(0.05)	0.24***	(0.06)
Tidy up the mango tree	0.09*	(0.05)	0.07	(0.05)	0.08	(0.05)
Clean up under the tree	0.02	(0.03)	0.02	(0.03)	0.05	(0.04)
Sort *Francique* mangos	0.02	(0.06)	0.01	(0.05)	0.03	(0.06)
Graft mango trees	0.12**	(0.05)	0.16***	(0.05)	0.18***	(0.05)
Used fertiliser	0.03	(0.03)	0.00	(0.02)	0.02	(0.03)
Fenced in your plot	0.21***	(0.06)	0.20***	(0.05)	0.18***	(0.06)
Used pesticides	0.03	(0.03)	0.03	(0.02)	0.02	(0.02)
Commercialization behavior 2013:						
Local market	-0.07	(0.05)	-0.06	(0.05)	-0.04	(0.05)
Producer's association/neighbor	0.05**	(0.02)	0.04**	(0.02)	0.05***	(0.02)
Middlemen/Voltije	-0.15***	(0.06)	-0.16***	(0.05)	-0.11*	(0.06)
Exporter (factory)	-0.01	(0.03)	0.01	(0.02)	-0.02	(0.03)
PBG/Cell	0.43***	(0.05)	0.42***	(0.05)	0.38***	(0.05)
Postharvest practices 2013:						
Straw bag on donkey	-0.03	(0.06)	0.00	(0.05)	0.10	(0.07)
Rice sack on head	0.03*	(0.02)	0.03	(0.02)	0.02	(0.01)
Peet bags	-0.02	(0.02)	-0.01	(0.02)	-0.01	(0.03)
Basin on head	0.03	(0.03)	0.03	(0.03)	0.01	(0.03)
Improved straw	0.04	(0.02)	0.02	(0.03)	0.00	(0.03)
150 mango case	0.02	(0.04)	0.02	(0.03)	0.03	(0.04)
Small case	-0.07	(0.05)	-0.08	(0.05)	-0.12**	(0.06)
Wheelbarrow	0.01	(0.00)	0.01	(0.01)	0.01	(0.01)
Desirable method of transportation	0.06	(0.06)	0.02	(0.05)	-0.01	(0.06)

Source: Authors' own calculation based on baseline survey 2012 and follow-up survey 2013.
Note: nn = nearest neighbor; coef = estimated coefficient; se = standard error. A total of 449 observations were used for matching, 203 treated and 246 controls, 4 observations off support. For DID-PSM estimation bootstrap standard errors with 500 repetitions in parentheses. For matching in covariates estimation, robust standard errors in parenthesis.
Significance levels are indicated by: *** at the 1 percent level; ** at the 5 percent level; * at the 10 percent level.

Table 13: Reduced sample Logit model for estimated propensity score

Dependent variable: Project participant
n=334 (3 observations trimmed, 4 observations with missing data)

Independent variables	Coef.	Std. Err.	P>z
Female=1	0.37	0.26	0.15
Head_age	0.03	0.05	0.58
Head_age_sq	0.00	0.00	0.64
Members (number)	-0.02	0.06	0.72
Rooms (number)	0.12	0.13	0.35
Mango 1st to 3rd income source	1.60	0.82	0.05
Mango 1st to 3rd crop	-0.63	0.78	0.42
Francique(number)	-0.01	0.01	0.56
Non-Francique (number)	-0.01	0.01	0.09
Footpath=1	-0.13	0.69	0.85
PackAnim=1	0.01	0.62	0.98
Pickup_truck=1	-0.75	0.74	0.31
inad_roof=1	0.61	0.83	0.46
inad_walls=1	-0.73	0.41	0.07
inad_floor=1	0.04	0.28	0.87
Water_river_spring=1	-0.92	0.35	0.01
Water_pump_well=1	-0.70	0.33	0.04
PublicWater=1	0.03	0.42	0.95
Distwater_10minless=1	-0.13	0.31	0.67
DistWater10_30=1	-0.24	0.36	0.51
Girls in school(number)	-0.05	0.13	0.67
Boys in school(number)	0.11	0.13	0.40
Constant	-1.20	1.75	0.49
Log likelihood	-212		

Source: Authors' own calculation based on baseline survey 2012 and follow-up survey 2013. n = 334.

Table 14: Reduced sample: DID-PSM project impacts on outcomes related to production and sales, 2012–2013; PSM impacts on adoption of improved practices and changes in commercialization behavior, 2013

	Kernel			
	Reduced sample n=334		Whole sample n=449	
Outcomes	coef	se	coef	se
Production and sales (2013-2012):				
DIFF Number of *Francique* trees	20.60***	(7.89)	12.29**	(6.45)
DIFF Number of other type of mango trees	-1.85	(9.94)	0.97	(6.83)
DIFF Total number of mango trees	18.67	(12.79)	12.98	(8.48)
DIFF Number of young mango trees	16.02	(10.41)	12.38*	(7.30)
DIFF Number of productive mango trees	1.51	(8.05)	-1.73	(7.07)
DIFF total sales USD	18.47	(42.67)	6.02	(44.88)
DIFF sales by tree USD	1.85	(3.13)	1.29	(2.34)
Adoption of improved practices 2013:				
Trim the mango tree	0.21***	(0.06)	0.20***	(0.05)
Tidy up the mango tree	0.11*	(0.06)	0.09*	(0.05)
Clean up under the tree	0.05	(0.04)	0.02	(0.03)
Sort *Francique* mangos by distribution channel	-0.01	(0.06)	0.02	(0.06)
Graft mango trees	0.19***	(0.06)	0.12**	(0.05)
Used fertiliser	0.02	(0.02)	0.03	(0.03)
Fenced in your plot	0.17**	(0.07)	0.21***	(0.06)
Used pesticides	0.03	(0.03)	0.03	(0.03)
Commercialization behavior 2013:				
Local market	-0.04	(0.05)	-0.07	(0.05)
Producer's association/neighbor	0.06**	(0.03)	0.05**	(0.02)
Middlemen/Voltije	-0.14**	(0.07)	-0.15***	(0.06)
Exporter (factory)	0.00	(0.03)	-0.01	(0.03)
Cell	0.42***	(0.06)	0.43***	(0.05)
Postharvest practices 2013:				
Straw bag on donkey	-0.01	(0.06)	-0.03	(0.06)
Rice sack on head	0.03*	(0.01)	0.03*	(0.02)
Peet bags	0.01	(0.02)	-0.02	(0.03)
Basin on head	0.05	(0.03)	0.03	(0.03)
Improved straw	0.05*	(0.03)	0.04	(0.02)
150 mango case	0.00	(0.04)	0.02	(0.04)
Small case	-0.10*	(0.06)	-0.07	(0.05)
Wheelbarrow	0.01	(0.01)	0.01	(0.00)
Desirable method of transportation	0.05	(0.07)	0.06	(0.06)

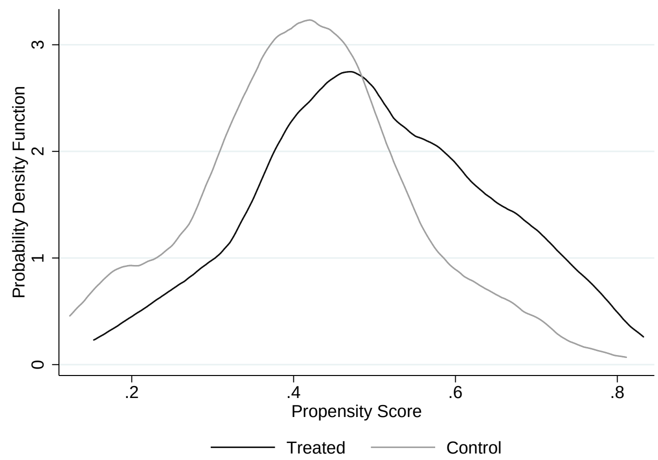
Source: Authors' own calculation based on baseline survey 2012 and follow-up survey 2013.
Note: For DID-PSM and PSM estimation bootstrap standard errors with 500 repetitions in parenthesis. For matching in covariates estimation, robust standard errors in parenthesis
Significance levels are indicated by: *** at the 1 percent level; ** at the 5 percent level; * at the 10 percent level.

Table 15: Rosenbaum bounds for outcomes related to adoption of best practices (results for matching in covariates with one neighbor)

	Pruning		Grafting		Fencing		Producer association		Middlemen		PBG/Cell		Small case	
	(1)		(2)		(3)		(4)		(5)		(6)		(7)	
Γ	p_mh+	p_mh-	p_mh+	p_mh-	p_mh+	p_mh-	p_mh+	p_mh-	p_mh+	p_mh-	p_mh+	p_mh-	p_mh+	p_mh-
1	0.00	0.00	0.01	0.01	0.00	0.00	0.02	0.02	0.01	0.01	0.00	0.00	0.02	0.02
1.1	0.00	0.00	0.03	0.00	0.00	0.00	0.03	0.01	0.00	0.04	0.00	0.00	0.01	0.04
1.2	0.01	0.00	0.06	0.00	0.00	0.00	0.04	0.01	0.00	0.08	0.00	0.00	0.00	0.09
1.3	0.02	0.00	0.11	0.00	0.00	0.00	0.05	0.01	0.00	0.15	0.00	0.00	0.00	0.16
1.4	0.04	0.00	0.17	0.00	0.00	0.00	0.07	0.00	0.00	0.23	0.00	0.00	0.00	0.24
1.5	0.08	0.00	0.25	0.00	0.01	0.00	0.08	0.00	0.00	0.34	0.00	0.00	0.00	0.33
1.6	0.12	0.00	0.34	0.00	0.02	0.00	0.10	0.00	0.00	0.44	0.00	0.00	0.00	0.43
1.7	0.18	0.00	0.43	0.00	0.03	0.00	0.12	0.00	0.00	0.54	0.00	0.00	0.00	0.53
1.8	0.25	0.00	0.52	0.00	0.05	0.00	0.13	0.00	0.00	0.45	0.00	0.00	0.00	0.47
1.9	0.33	0.00	0.49	0.00	0.08	0.00	0.15	0.00	0.00	0.36	0.00	0.00	0.00	0.39
2	0.41	0.00	0.41	0.00	0.12	0.00	0.17	0.00	0.00	0.28	0.00	0.00	0.00	0.31

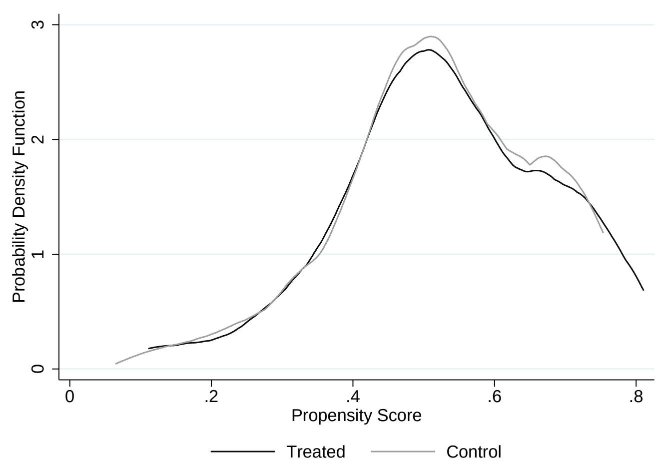
Source: Authors' own calculation based on baseline survey 2012 and follow-up survey 2013.
Γ : odds of differential assignment due to unobserved factors
p_mh+ : significance level (assumption: overestimation of treatment effect).
p_mh- : significance level (assumption: underestimation of treatment effect).

Figure 1: Estimated propensity scores before matching



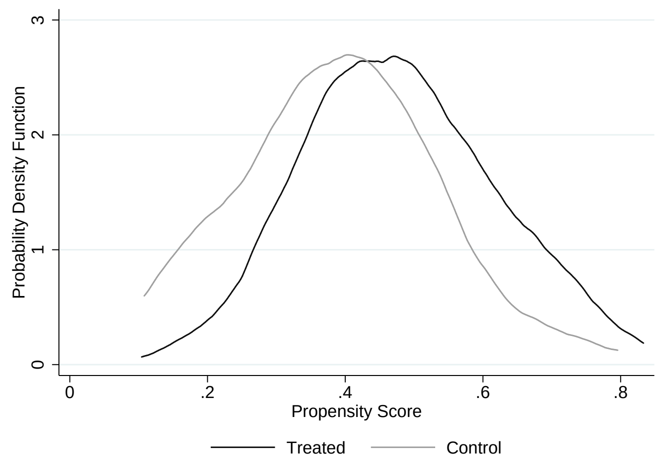
Source: Authors' own calculation based on baseline survey 2012 and follow-up survey 2013. n=449.

Figure 2: Estimated propensity scores after matching (sample observations weighted using kernel-matching weights)



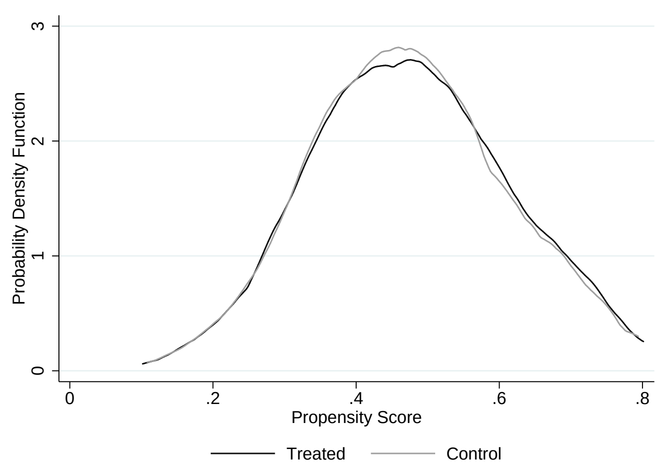
Source: Authors' own calculation based on baseline survey 2012 and follow-up survey 2013. n=449.

Figure 3: Estimated propensity scores before matching, reduced sample



Source: Authors' based on baseline survey 2012 and follow-up survey 2013. n= 334.

Figure 4: Estimated propensity scores after matching, reduced sample



Source: Authors' own calculation based on baseline survey 2012 and follow-up survey 2013. n= 334.

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