Creating Opportunities for Rural Producers: Impact Evaluation of a Pilot Program in Colombia*

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Abstract

This paper presents the impact evaluation of a pilot program that treated 57 small organizations of agricultural producers with high risk of getting involved in illegal drug production in Colombia. The program supported producers mainly by facilitating the commercialization of their new licit alternative sources of income. We combine propensity score matching, regression discontinuity, and Bayesian decision theory, with unique and rich panel data to assess the economic impact of the program. Our results suggest that the program was successful on increasing total sales and improving the product's quality for the treated producers. The intervention was more successful when combined with other programs that gave producers incentives to abandon illegal drug production definitely.

JEL Classification: O13, O33, O54 and Q18.

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1 Introduction

As of 2011, Colombia, Peru, and Bolivia produced 99% of the world's supply of coca leaf (World Drug Report, 2011). Drug trafficking has become a great concern for these countries because it represents the main source of funding for illegal armed groups; and because there is a strong correlation between drug trafficking and violence (Angrist and Kugler, 2008; Dube and Vargas, 2013; and Dell, 2014). Hence, in the last two decades, an ample diversity of programs aimed at reducing illegal coca production have been implemented in the Andean region.

Anti-drug policy efforts have been particularly strong in Colombia given the country became the top producer of cocaine in the mid-nineties, accounting for 73.7% of the world's coca leaf supply (UNODC, 2011).[1] Initially the resources were mostly invested in involuntary eradication programs such as aerial spraying with herbicides (Gaviria and Mejía, 2011). However, with the years, other programs less inclined towards enforcement began to emerge. For instance, in 1995 the Colombian government implemented the *Plan Nacional de Desarrollo Alternativo* (PLANTE) [National Plan for Alternative Development], a program directed at generating licit alternative sources of income for rural producers involved in illegal drug production, and in 2003 the program *Familias Guardabosques* was launched as a conditional cash transfer program that conditioned the reception of a cash allowance to the elimination of coca crops.

Although the total number of hectares of coca cultivated in Colombia fell by 57.3% between 2000 and 2010[2], the need to strengthen the new economic means of rural producers that left coca production has become imminent. This last to prevent the rural producer from returning to illegal drug production. Support is particularly needed in endowing farmers with tools that will help them maintain the new income flows for themselves.

In 2008, the United Nations Office of Drugs and Crime (UNODC) began the implementation of a pilot program called *Fortalecimiento Comercial and Agroindustrial de Grupos Productores de Desarrollo Alternativo* (henceforth refer to as FCA, for its initials in Spanish). It aimed at increasing the competitiveness of farmers that left coca production or that were located in areas with high coca production—i.e., rural producers with high risk of getting involved in illegal drug production. The program treated 57 small organizations of

[1]Zirnite (1998), Rabasa and Chalk (2001), and Angrist and Krueger (2008), suggest that the increase of coca production in Colombia was explained by the closure of the so-called 'air bridge' connections of coca cultivation in Peru and Bolivia, which took place around 1994 in response to the increasingly effective air interdiction by the American and the local military. As a consequence, coca cultivation and paste production shifted to Colombia's country side.

[2]From 144,807 ha to 61,811 ha, respectively.

agricultural producers. It offered technical support to improve their production and operations, with a strong focus on building new commercialization channels by increasing their number of potential buyers, both domestically and internationally.

This paper evaluates the impact of the FCA program on the economic development of the treated organizations. For this purpose, we constructed rich and unique panel data based on two censuses collected by UNODC in 2008/2009 and 2011. The censuses have information on all the agricultural organizations of the country, allowing us to identify the program's beneficiaries and a control group pre- and post-program implementation. We evaluate the effects of the program using propensity matching score and regression discontinuity. The later was allowed by the strict selection criteria used by UNODC to select the beneficiaries.

All estimates suggest that the program had a positive effect on the treated organizations. Specifically, we find the program was successful on increasing sales, productive capacity area, and product's quality. Our estimates for the local average treatment effect obtained through regression discontinuity are higher than those for the average treatment effect of the program obtained using propensity score matching. This suggests that those organizations near the threshold of selection (the discontinuity) were on average more affected by the intervention than those organizations with very high or very low levels of development.

In addition, we carried out simulations using Bayesian decision theory to predict the effects of the program on any of the organizations observed in the sample—even those that were not treated by the program. Based on this exercise we asses: i) how the program may impact each of the organizations in the data set, and ii) which characteristics of the organizations facilitate higher positive results of the program. This allows us to make some recommendations for the future design of similar program. The results suggest that most of the organizations will be positively impacted by the program. Yet, there are heterogeneous degrees of sensitivity. In particular, the program could be most effective on those organizations with a medium level of development.[3] The estimates also suggested that the success of the program was conditioned upon its combination with other governmental programs such as conditional cash transfers that gave the producers further incentives to abandon the illicit crops permanently.

The empirical literature on alternative development programs is scarce, mainly due to the limited number of interventions that have attempted to reduce drug supply by working directly with producers, and due to the lack of reliable data. Farrell (1998) and Mansfiled (2011) provide a general review of the diversity of alternative development programs ever

[3] A organization was considered to have a medium development level if it required financial contributions from their members, if it had financial statements, had a bank account, and had a strategic plan.

applied for the substitution of illicit crop cultivation. One exception is Vargas and Ramos (2005), who study the impact of a program implemented in Brazilian municipalities to support family farmers in their transition from tobacco to other sustainable livelihoods, but without employing modern impact evaluation techniques. For the first time, this paper contributes to the alternative development literature by providing quasi-experimental evidence of the effects of an alternative development intervention, including a rigorous estimation of its effects on the treated producers.

This document is structured in six additional sections. The first section describes the program. Section two describes the data. Sections three and four deal with identifying the average and the local treatment effect of the program. Section five presents the simulations obtained through Bayesian decision theory. Finally, the last section presents some brief concluding remarks.

2 Program Description

The FCA program began to be implemented in 2008. It targeted organizations of rural producers with high risk of participating in illegal drug production. The level of risk was measured through the intensity of coca production in the area where the organization was located or if some of the organization's members had cultivated coca in the past. The program aimed at strengthening the generation of alternative sources of income by offering technical support on the production, transformation, and commercialization of licit products. It treated 57 small organizations of rural producers (known as first-level organizations), which were nested in 10 bigger organizations (known as second-level organizations). The program treated all the first-level organizations nested in the ten second-level organizations selected by the program.

Second-level organizations were the bridge through which the program reached smaller organizations. The program worked by training and preparing the second-level organizations so that they would directly transfer the knowledge to the first-level organizations. This was an important characteristic of this program, since it recognized that given the complicated situation of drug rural producers, trust on external institutions was an issue that could be overcome by working through local actors.

The treated organizations grouped 2,939 families. The main products supported by the program were cacao, coffee, honey, and African palm. UNODC selected the second-level organizations to be treated by the program in a two-stage process. In the first stage a group of potential beneficiaries was selected from a census that contained all the second-level organizations of producers in the country. They were selected as the 15 organizations with the highest development score. This score was constructed as a weighted average of nine indicators: i) year of foundation, ii) number of members, iii) number of institutions giving financial support, iv) number of products with quality seals (i.e. quality certifications), v) total value of national sales, vi) total value of exports, vii) concentration of beneficiaries in the center of the municipality, viii) product's demand inside the country, and ix) marketing capacity.

Once a potentially treated organization was identified, UNODC carried out a deeper analysis to rank the organizations and then choose those which had the better performance. In particular, UNODC constructed an additional score to choose the 10 final beneficiaries from the 15 potential organizations selected. Appendix A describes the composition of the final score and the beneficiaries' selection in more detail.

The treatment that was offered to each of the ten second-level organizations that was finally selected was structured in five components: i) organizational, ii) technical, iii) national trade, iv) training and marketing (or exports), and v) financial development. All of the second-level organizations that were selected received support in the first component and received a diagnostic study. Depending on the results of this diagnostic study, UNODC determined the continuation point to enhance the development of the organization. In that sense, the project delivered a heterogeneous treatment to each organization. The main activities developed in each of the components are described in Figure 1.

3 Data

UNODC collected two censuses of all the first-level organizations that existed in 2008/2009 and 2011 in Colombia. The censuses were collected in all the regions in which there was some coca cultivation. Whether or not an area had coca cultivation was determined through satellite images collected every year by UNODC. Based on the satellite images, field visits were programmed to areas with coca cultivation during each year. The existing organizations were identified by field workers through their interactions with local stake holders. UNODC surveys were distributed through the second-level organizations, which then collected the information of all their affiliated first-level organizations through their networks. This process of information collection solved the issue of trust from local producers to external institutions, since the information was actually requested to each first-level organizations by the second-level organizations to which they were affiliated.

Component	Main Activities
Organizational	Characterization and evaluation of the 15 second level organizations that were identified by the program executors as potential beneficiaries. Selection of 10 second level organizations to be treated by the program. For each of the 15 organizations selected the executing unit made a diagnostic study and a plan of action to be implemented for the 10 selected. Training sessions in foreign trade, domestic trade, quality seals, packaging, product presentation, and corporate leadership.
Technical	Improvement of primary production, post-harvest, transformation, storage, distribution and environmental activities; strengthening of technical skills through training and technology transfer; support and training in the process of certification for quality seals for products; creation of a control and tracing software to support the cacao's transformation process.
National Trade	Creation of the trade and logistic network in Bogotá; creation of strategic agreements with the private sector; identification of the demand; participation in publicity fares and sampling tasting events; creation of marketing annual plans; development of products according to market needs; improvement of the level of service; product's quality improvement (especially in efficiency and dates fulfillment).
Exports	Training in foreign trade (with support of Bogota's Chamber of Trade); elaboration of exports annual plans; creation of communication tools in English and Spanish; participation in international fares; and creation of a web page.
Financial	Design and implementation of the financial and accounting software to maintain financial statements; development and implementation of programs to reduce production costs; agreements with *Fondo para el Fortalecimiento del Sector Agropecuario* (FINAGRO) to facilitate loan access; and financial training sessions.

Figure 1: Main Activities of the FCA program

We use these data to evaluate the program. Although the program began in the last months of 2008, it took a year to hire the personnel, identify the beneficiaries, and elaborate the diagnostic study of the selected second-level organizations. Hence, the treatment only began in 2009. Consequently, for the purpose of this evaluation, the information on the census collected during 2008/2009 is considered the baseline and the information collected in 2011 is considered the post-treatment data.

We constructed a panel of 454 first-level organizations.[4] Within this sample, we were able to identify 32 of the 57 first-level organizations treated by the program. Some of the treated organizations could not be identified because in some cases the second-level organization did not received the information requested from the first-level organization.[5] The surveys contain information on the motivation for the creation of the organizations, on how decisions are made within the organization, how information is distributed through all members, the types of services offered, and the administrative situation of each organization.

Table 1 presents descriptive statistics for the constructed panel. As can be seen, for 2009 the mean number of rural producers affiliated to a first-level organization was 125 and it decreased to 121 for 2011. For both years, more than half of the total rural producers

[4]Each second-level organization had approximately five affiliated first-level organizations with a standard deviation of 2.02.

[5]Approximately 31% of the first-level organizations contacted by the second-level organizations did not respond the survey.

affiliated to the organizations were men and around 10% of the members in the organizations are represented by minorities—such as indigenous and African populations; more than half of the members of the organizations were located in the same municipality where the organization had presence; and around 28% of the organization's members had participated in illicit crops such as coca or amapola production in 2009 (although this percentage was reduced to 22% in 2011). Finally, the percentage of members that were victims of violence increased dramatically between 2009 and 2011 from 7% to 30%.

Table 1: Descriptive Statistics

	2009		2011	
Variables	Mean	St. Dev	Mean	St. Dev
Created by Members*	0.88	0.33	0.56	0.50
N. of members	124.81	402.42	121.34	408.78
Created PP*	0.19	0.39	0.39	0.49
Created col*	0.55	0.50	0.72	0.45
Created com*	0.24	0.42	0.57	0.50
Male (% members of total)	65.95	25.02	66.38	22.68
Indigenous (% members of total)	11.87	29.63	12.53	30.62
African (% members of total)	8.08	23.93	10.20	28.33
Located in Municipality*	0.62	0.49	0.57	0.50
Illicit crops*	0.28	0.45	0.22	0.41
Violence*	0.07	0.25	0.30	0.46
Financial Contributions*	0.82	0.38	0.77	0.42
Financial Statements*	0.68	0.47	0.77	0.42
Bank Account*	0.80	0.40	0.82	0.39
N. Services	1.99	1.99	2.71	2.38
Strategic Plan*	0.74	0.44	0.83	0.50
Services Offered by Organization				
Saving Fund*	0.15	0.36	0.13	0.34
Solidarity Fund*	0.20	0.40	0.13	0.33
Wages "pago de jornales"*	0.16	0.37	0.13	0.34
Social Security*	0.05	0.22	0.02	0.15
Recreation*	0.13	0.34	0.11	0.31
Technical Support*	0.31	0.46	0.44	0.50
Training*	0.31	0.46	0.43	0.50
Buys products*	0.26	0.44	0.26	0.44
Small loans*	0.17	0.38	0.17	0.38
N. Beneficiaries	32		32	
N. Observations	454		454	

Note: * Indicator variable (=1 yes, =0 No).

The census of 2011 also included additional information on the different products sold by each organization—each product received the name of *línea productiva*. The additional information collected includes extensive financial data which we used to analyze the economic impact of the program.

4 Controlling for Selection on the Observables

In this section we estimate the average treatment effect on the treated (ATT) using propensity score matching (PSM). PSM estimates the probability of treatment for all of the observations of the sample and matches those observations in the treated and the control group that are as similar as possible. The comparison between the most similar observations allows the identification of the ATT of the program.

Rubin (1974), Rosenbaum and Rubin (1983, 1985), and Lechner (1998) suggest there are two key assumptions required for identification of the ATT under this methodology. The first one is that we observe all covariates that accounted for the selection of beneficiaries. This assumption requires that we can control for all the variables that affected the selection of beneficiaries, and that, conditional on those covariates there is random treatment assignment. Formally, let Y denote the outcomes to be analyzed, D be a dummy variable equal to one if the organization was treated by the program, and X represent the covariates that characterizes each of the organizations. We can formally represent this first assumptions as:

$$Y \perp D|X, \qquad \forall X$$

which guarantees as is shown by Rosenbaum and Rubin (1983) that:

$$Y \perp D|P(X), \qquad \forall X$$

where $P(X)$ represents the predicted probability of treatment, which is estimated through a probit model. In other words, the probability of treatment should be estimated with covariates that are not affected by the program. This can be guaranteed if we use covariates observed for the baseline year 2008/2009 before the program was actually implemented.

As was mentioned in the previous section, the organizations to be treated by the program were selected in a two-stage process. In the first stage, all the existing second-level organizations observed in 2008/2009 were ranked according to a score that included nine variables: i) year of foundation, ii) number of members, iii) number of institutions giving financial support, iv) number of quality seals, v) national sales, vi) exports, vii) concentration of beneficiaries in the center of the municipality, viii) product's demand in the country, and ix) capacity for international trade. Because the unit of observation in our sample are first-level organizations and they are nested in second-level organizations, it is likely that their covariates are correlated so that the best second-level organizations are conformed by the best first-level organizations. Hence, we included all the observable covariates related with these nine criteria in the estimation of the propensity score.

To reduce the risk of a low correlation between the first- and second-level organizations we included all the observables that could characterize these organizations and that increased the predictive power of the probit estimation according to the pseudo R^2 for 2008/2009. The final estimation included 31 covariates. The results of the probit estimation as well as the description of the variables included are reported in appendix C. The probability of being treated was predicted and stored to match the observations in the treated and control group. Henceforth, these predicted values will be referred to as pscores.

The second assumption needed for the correct implementation of PSM, is that there is common support between the treated and control groups. In other words, we need to rule out perfect predictability of treatment by ensuring that organizations with the same covariates have the same probability of belonging to the treatment or control groups. Figure 2 presents the histograms of the pscores for the full sample. To guarantee common support we dropped all of the control observations that had a pscore lower than the minimum pscores for the treatment group. We did not drop any treated observations given the low number of observations for this group. We also divided the distribution of the control and treatment groups in 50 bins and dropped those control observations where there were no treated observations in the equivalent bin for the treatment distribution. The final distribution is presented in Figure 2. The figure suggest that by keeping only the observations on the control group for which there is common support we make the distributions of the pscores on both groups more similar.

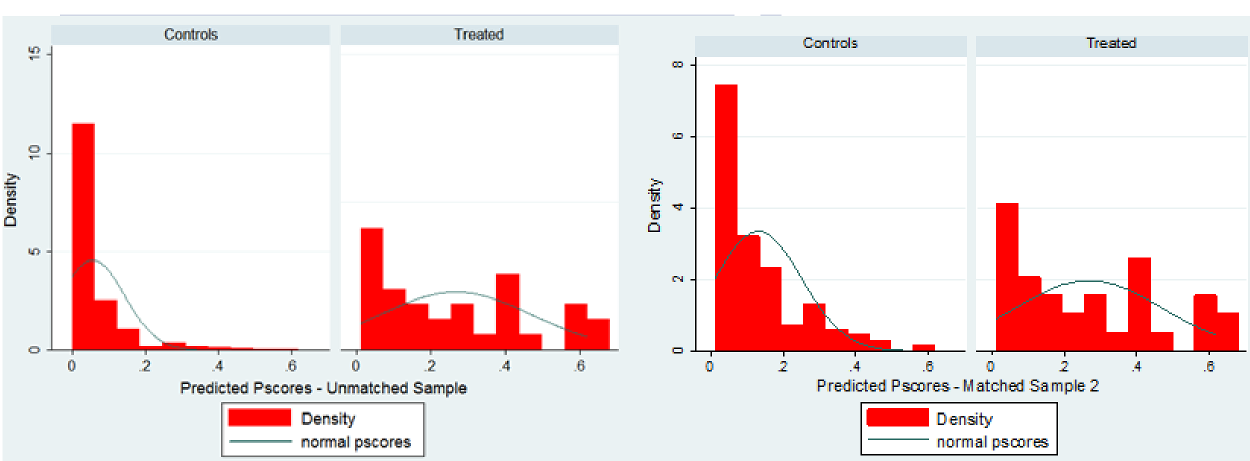

Figure 2: Predicted Pscores for the Full and Matched Sample

Moreover, if the matched sample successfully allows to compare similar observations in the treated and control groups, there should be no significant differences in the observed covariates between groups. In addition, Sianesi (2004) suggests that the pseudo R^2 with the full sample before matching should be higher than with the matched sample. As Caliendo and Kopeinig (2005) point out the pseudo R^2 indicates how well the regressors (X) explain the participation probability. After matching there should be no systematic differences in the distribution of covariates between both groups, and therefore, the pseudo R^2 should be fairly low. Table 2 reports the results of these two tests confirming that the matched sample presents the expected behaviour. In particular, the table presents the mean difference test for the two groups and the change in pseudo R^2 for the full and matched sample. The mean difference test was applied to 34 covariates of which 26 reduced their t-statistic considerably between the full and the matched sample. In addition, 33 of the 34 tests could not reject the null hypothesis of equal means for the treatment and control groups. The only variable that rejects the hypothesis of equal means between groups is illicit crops. It is a dummy variable that takes the value of one if the organization's members had participated in illicit cultivation of coca leaf or amapola. Given the program was targeted to organizations that were located in areas with higher presence of illicit crops this result is intuitive and should not represent a source of concern given we were not able to reject the null hypothesis for the other 33 covariates. Finally, the pseudo R^2 fell from 0.26 to 0.08 between the full and the matched sample, respectively.

4.1 Assessing the Impact of the Program

Using the estimated pscores, we matched the organizations in the treated and control groups in the subsample that satisfied common support (i.e., the matched sample) and estimate the effect of the program for the outcomes observed in 2011 as:

$$\tau_{ATT}^{PSM} = E_{P(X)|D=1}\{E[Y(1)|D=1, P(X)] - E[Y(0)|D=0, P(X)]\}$$

We analyze the impact of the program on total sales value (measured through the total value of final sales), productive capacity (in cultivated hectares), and product's quality (measured through number of quality seals or certifications), among others. These variables are only available for 2011 and we observe them by *línea productiva*, that is by type of product developed by the organization. Hence, for the outcomes observed by *línea productiva* we may have more than one observation by organization but we choose not to aggregate this variable to increase the power of our estimates. There are 32 and 130 treated and control first-level

Table 2: Assessing comparability between groups

	Full sample			Matched Sample		
	Control	Treatment	T-stat (C-Tr)	Control	Treatment	T-stat (C-Tr)
Created by members	0.87	0.90	-0.52	0.88	0.90	0.12
Years	11.73	11.78	-0.01	14.79	11.78	0.54
N. members	122.05	100.40	0.30	127	100.40	0.47
N. Donors	1.23	1.06	2.1**	1.11	1.06	0.9
Municipality	0.62	0.56	0.73	0.55	0.56	-0.03
Created com	0.24	0.15	1.09	0.18	0.15	0.32
Created PP	0.18	0.28	-1.37	0.21	0.28	-0.82
Created col	0.55	0.56	-0.11	0.57	0.56	0.12
illicit crops	0.24	0.62	-4.65***	0.27	0.62	-3.86***
violence	0.07	0.03	0.86	0.07	0.03	0.82
Number of Services	1.97	2.18	-0.57	2.01	2.18	-0.44
Copy of rules	0.52	0.68	-1.76*	0.58	0.68	-1.08
Newspaper com.	0.21	0.28	-0.82	0.23	0.28	-0.52
Radio com.	0.04	0.15	-2.84***	0.09	0.15	-1.07
Web com.	0.04	0.09	-1.23	0.05	0.09	-0.08
Complaints system	0.36	0.34	0.21	0.33	0.34	-0.13
N. Invitations	1.21	1.15	0.67	1.14	1.15	-0.19
Years board	1.96	1.56	2.02**	1.69	1.56	0.87
Control Institutions	0.87	0.90	-0.52	0.89	0.90	-0.14
Financial Contributions	0.81	0.84	-0.33	0.82	0.84	-0.22
Capital Property	0.84	0.68	2.24**	0.6	0.68	0.41
Financial Statements	0.83	0.90	-1.06	0.9	0.90	-0.01
N. Credits	0.33	0.46	-0.24	0.43	0.46	-0.43
Balance Sheet	0.66	0.90	-2.85***	0.9	0.90	-0.28
Petty Cash	0.73	0.71	0.25	0.73	0.71	0.15
Strategic Plan	0.74	0.68	0.70	0.7	0.68	0.4
N. of Projects	1.25	1.09	0.58	1.25	1.09	0.59
Supports Production	0.59	0.71	-1.38	0.62	0.71	-0.93
Supports Intermediation	0.13	0.15	-0.41	0.18	0.15	0.42
Supports Packaging	0.10	0.09	0.14	0.07	0.09	-0.43
Supports Transformation	0.10	0.06	0.82	0.07	0.06	0.16
Supports Commercialization	0.48	0.65	-1.86*	0.61	0.65	-1.18
Supports Storage	0.23	0.37	-1.81*	0.3	0.37	-0.65
Administrative Personnel	0.34	0.53	-2.17**	0.45	0.53	-0.75
Technical Personnel	0.24	0.37	-1.64*	0.29	0.37	-0.82
N. observations	422	32		130	32	
Pseudo R2		0.26			0.08	

Note: The table reports the t-statistic for the means difference test between groups. *:significant at 10%, **: significant at 5%, and ***: significant at 1%.

organizations. This correspond to 38 and 157 *línea productivas*, respectively.[6] The units and the definition of each of the variables that were analyzed are presented in appendix B.

The outcomes that were used to assess the impact of the program were chosen based on the available information. Although ideally development indicators could better describe the effects of the program on the local population, they are rarely available in the treated regions. In particular, illegal drug production takes place in the poorest areas of the Andean region where the availability of administrative information is scarce. Despite the fact that the outcomes used are not development indicators, the availability of information such as total sales value by producer organization represents a unique opportunity to study the effects

[6]This means that 80% of the total organizations have only one *línea productiva* and 20% have two. As a share of the total, 18% and 20% of the treated and control first-level organizations have two *línea productivas*.

of the program on its direct objective: strengthening alternative sources of income for local producers.

Table 3 reports the results using the nearest neighbor algorithm with replacement.[7] The results suggest that the program had a positive effect on 6 of the 19 outcomes that were analyzed. First, treated organizations had sales 29% higher than control organizations, which corresponds to a difference of $139,150,408 Colombian pesos. This percentage can be obtained by dividing the ATT on the mean outcome for the control group. Doing the same estimation the table reports that the productive capacity area of the treated organizations was 16% higher than that of the control group. This corresponds to a difference of 57 hectares. Also, the probability that a treated organization made a loan application was 23% higher relative to the control group.

The program also had a positive effect on the product's quality index. This variable was defined as the number of quality seals (or certifications) on the number of products of each *línea productiva*. The estimates suggest that the treated organizations had a quality index 0.67 higher than the control organizations. This implies that the treated organizations had a quality index that was almost 200% higher than the control organizations. In addition, we found that the probability that the treated organizations had a trade fund to support trading activities for its members was 17% higher relative to the control organizations.

Finally, the estimates suggest that the *línea productivas* from treated organizations report higher transportation issues explained by security threats. In particular, the probability that a treated organization faced security issues was 18% higher for the *línea productivas* of the treated organizations.[9] The executing unit suggested that likely this is a result of the increase in volume transported in the treated organizations. In their opinion as the organizations become more successful local violent groups target their activities more directly to appropriate their rents.

One last exercise we did to confirm the validity of the results was to run all of the estimates matching two subgroups in the control's sample. In other words, we created a

[7]We choose to present the results for this algorithm since as Imbens and Wooldridge (2007) suggest using only a single match leads to the most credible inference with the least bias. Since Abadie and Imbens (2010) have concluded that the estimation of the standard errors for the sample version of τ_{ATT}^{PSM} through bootstrapping methods is invalid for the nearest neighbor algorithm we approximate standard errors on the treatment effects using the heteroskedasticity-consistent analytical standard errors proposed by Abadie and Imbens (2006).[8] Other matching algorithms were also checked for robustness of results. In particular, we estimated the effect of the program using kernel, 2 nearest neighbors, and 3 nearest neighbors. The results are robust to the algorithm used.

[9]This outcome could be understood as being reported by first-level organization, since for the 33 first-level organizations that have two *línea productiva* (6 treated and 27 controls), the results across the same first-level organization are the same for all *línea productivas*.

Table 3: Results of the PSM by *Línea Productiva*

Variable	Mean Treated	Mean Controls	ATT	S.E.	T-stat
Sales (millions)	620.97	481.82	139.15	59.26	2.03**
Utility Margin	12.18	12.16	0.02	7.23	0.00
Loan Application	0.59	0.37	0.23	0.08	2.77***
Production Capacity	668.83	417.09	251.73	280.18	0.90
Productive Capacity Area	399.92	342.51	57.41	34.90	1.64*
Stage	1.84	1.92	-0.08	0.06	-1.31
Sales Personnel	0.24	0.13	0.12	0.07	1.65*
TI Security	0.38	0.20	0.18	0.08	2.26**
TI Highways	0.78	0.72	0.07	0.07	0.94
TI weather	0.30	0.37	-0.07	0.08	-0.91
TI Costs	0.38	0.30	0.08	0.08	1.00
TI No Transp	0.24	0.21	0.03	0.07	0.45
Publicity	0.22	0.25	-0.03	0.07	-0.49
Trade Fund	0.32	0.16	0.17	0.08	2.15**
Organization's quality	0.51	0.34	0.18	0.15	1.19
Sales through Org	49.43	42.80	6.63	7.26	0.91
Number of Products	0.95	1.11	-0.16	0.10	-1.71
Product 's quality	1.01	0.34	0.67	0.32	2.11**
Producer's Quality	0.38	0.23	0.15	0.11	1.35
N Obs. (in common support)	157	38			

Note: all outcomes are defined in Appendix B. All estimates include a common support. There are 32 and 130 treated and control first-level organizations. This correspond to 38 and 157 *línea productivas*, respectively. The table reports the results for the estimates for PSM through nearest neighbor with replacement. Standard errors are estimated using the heteroskedasticity-consistent analytical formula proposed by Abadie and Imbens (2006). Other methods were also checked for robustness they include kernel, 2 nearest neighbors, and 3 nearest neighbors. *: significant at 10%, **: significant at 5%, and ***: significant at 1%.

random variable that assigned zeros or ones to all of the observations in the control group. Those observations that received a value of one were redefined as a fictitious treatment group. We then checked for the effect of the program between both subgroups in the control group. This is a placebo test for possible problems in our estimates. If our estimates are correct we should not be able to find any significant effect of the program on any of the outcomes analyzed. The results are reported in appendix C and show no significant effect for any of the outcomes analyzed. This further supports the quality of our results.

5 Quasi-experimental Evidence

In this section we will employ regression discontinuity (RD) to evaluate the impact of the program. RD exploits an exogenous discontinuity in the probability of treatment to identify the effect of a program. Usually, RD is used when there are exogenous institutional rules that restricted the program participation, which could not be manipulated by potential beneficiaries.

Recall that for this program, treated organizations were chosen in a first stage based on a grading score that included nine variables. In this section we will replicate this score with data from 2009, and rank all the organizations in the census accordingly. If the selection process was based strictly in this grading score we should be able to observe a discontinuity in treatment probability for the lowest score in the treatment group. In other words, if we rank the organizations according to this grading score we should be able to identify the institutional rule applied by UNODC to select beneficiaries or the cutoff value after which potential beneficiaries were identified.

One important limitation of our analysis is that the selection of the beneficiaries was done for second-level organizations, which group first-level organizations. However, in our data we only observe first-level organizations. Hence, throughout our estimates we will be assuming that there is a direct correspondence between the observed characteristics of the first- and second-level organizations. In practice, this is not a very strong assumption given first-level organizations are nested within the same second-level organization are very similar. Specifically, all of their administrative and operative processes have been originated and developed with close guidance from the second-level organization.

The nine variables used by UNODC to construct the grading score were: i) years of foundation, ii) number of members, iii) number of institutions giving financial support, iv) number of quality seals, v) concentration of beneficiaries in the center of the municipality, vi) product's demand in the country[10], vii) national sales, viii) international sales, and ix) capacity for international trade. With the census collected in 2008/2009 we were able to replicate variables i) through vi). Since there was no information on the last three variables, 3 proxy variables were used to replace them. Given vii)-xi) were observed in 2011, the proxies were chosen as the variables that had the highest correlation with each of them for that year. The selected variables were: a dummy variable for having a bank account, number of loans approved, and a dummy variable for whether the organization satisfies the requirements of the governmental authority that regulates taxes and customs in the country (DIAN, for its initials in Spanish). The nine variables to be included in the score were normalized to take values between 1 and 10. The final grading score was constructed as a weighted average of the variables, where each variable received an equal weight in the score. The final score had a mean of 2.55, with a minimum value of 0.42, and a maximum value of 5.08. The lowest score in the treatment group was of 3.92. If the selection process followed the process we described before, and there is a strong correlation between the observed characteristics of the first- and second-level organizations we should observe a discontinuity in the treatment probability at this value.

[10] The index is just a dummy variable that takes a value of one for those products that have the highest demand in the domestic markets, i.e., coffee, cacao and livestock.

Following Lee and Lemieux (2009) we will define the grading score as the forcing variable, the outcomes to be analyzed as Y, and a treatment dummy for the program as D.[11] To facilitated the visual interpretation of our estimates we normalized the forcing variable to take a value of zero at 3.92. Henceforth, the normalized forcing variable will be denoted by W.

Our first step is to check for a discontinuity at the lowest score in the treatment group where $W = 0$ (or the original forcing variable takes a value of 3.92). Figure 3 presents the probability of treatment for different bandwidths of the forcing variable. The figures strongly suggest the existence of a discontinuity in the forcing variable around zero. Moreover, they show no jumps in the outcome variables at other values of W.

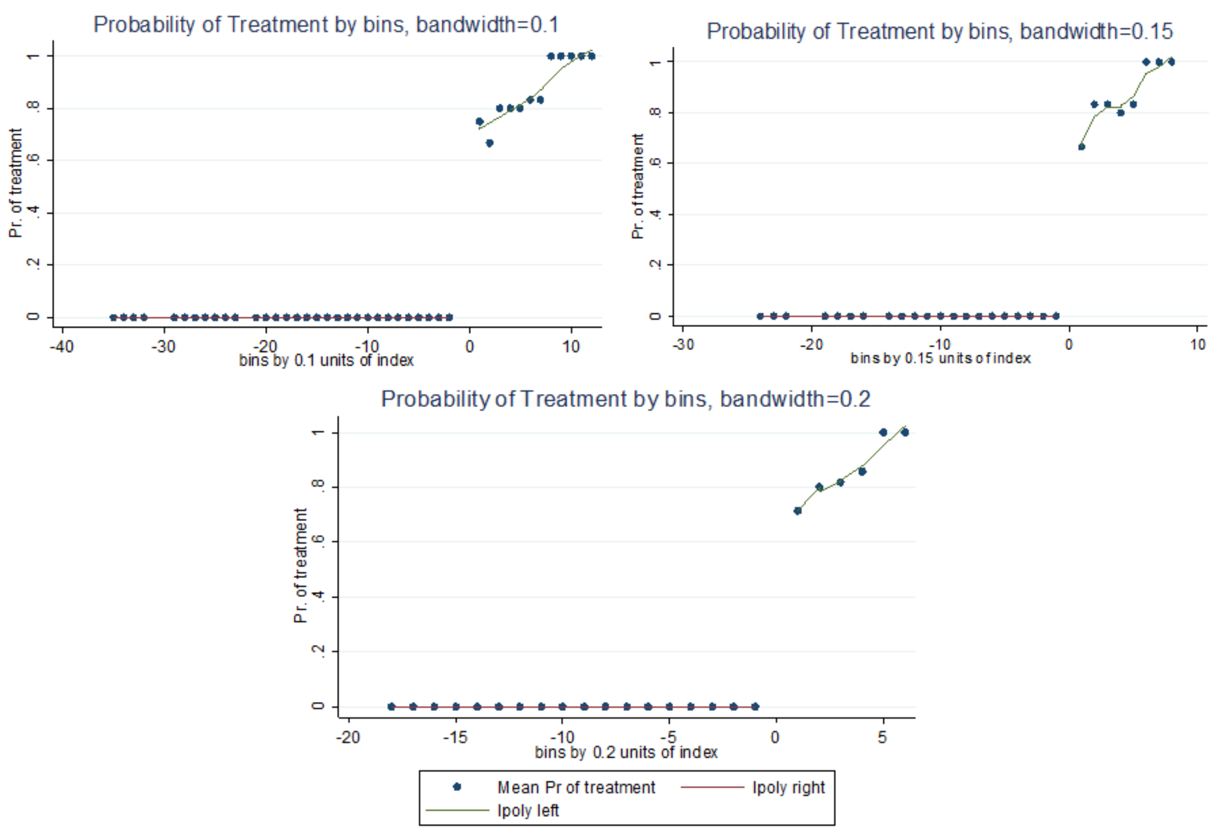

Figure 3: Discontinuity in the Forcing Variable at W = 0

Notice that those organizations with $W < 0$ had a probability of treatment near to zero, whereas, those organizations with $W > 0$ the probability of treatment jumped to positive values. In other words, we will expect that where $W = 0$:

[11] The variable takes the value of one if the organizations was treated by the program

$\lim_{w\downarrow 0} \Pr(D = 1/W = w) \neq \lim_{w\uparrow 0} \Pr(D = 1/W = w)$

Notice there may be imperfect compliance around the cutoff value ($W = 0$), given there are some control observations for which $W > 0$, and hence, we have a fuzzy design. In other words, the discontinuity around 0 will not be deterministic, there will only be a jump in the conditional probability of treatment at $W = 0$.

5.1 Bandwidth Choice

Lee and Lemieux (2009) suggest that the effect of the treatment on any outcome variable can be identified by running local linear regressions for some bandwidth h around the cutoff value of the forcing variable $W = 0$. Given some bandwidth h we will need to define a number of bins K_0 and K_1 to the left and the right of the cutoff value.[12] In each of the graphs we will present the average value of the outcome variable in each of the bins.

As is suggested by Lee and Lemieux (2009), the optimal choice for the bandwidth (h) should take into account that for too narrow bins the estimates are imprecise (since we lose too many observations) and for too wide bins the coefficients will be biased. Ideally, RD should be estimated around an optimal bandwidth using the formula presented in Imbens and Kalyamaran (2012) who derive an optimal formula under squared error loss and taking into account the special features of RD setting. For this case, the formula yields an optimal bandwidth value of 1.63 units of the grading score. Although we may be able to use this bandwidth for our estimates, our choice of bandwidth for the graphic analysis is restricted by the number of observations we have at the right side of the discontinuity. In particular, there are only 57 observations for which $W > 0$. If we use a bandwidth of 1.63 we will only have one bin at the right side of the discontinuity. Thus, we use different bandwidth for our graphic analysis and for our estimates.

For our graphical analysis we chose bandwidths of 0.1, 0.15 and 0.2 which allow us to have at least 5 bins at the right side of the discontinuity. On contrast, for our estimates, given we need at least 30 observations at the right side of the discontinuity to carry an statistical analysis we used three different bandwidths: i) 1.16 at each side (which includes all the observations at the right of the discontinuity); ii)1.63 at the left side following Imbens and Kalyamaran (2012), and 1.16 at the right side using all available observations; and iii) 1 at each side (which includes at least 30 observations at the right of the discontinuity). Notice, that by imposing a smaller bandwidth for the graphic analysis we are imposing a

[12]The bins $(b_k, b_{k+1}]$ should be constructed as:
$b_k = c - (K_o - k + 1)h$, where $k = 1, ..., K = K_0 + K_1$ (1).

stronger visual test on our data. This is true since any discontinuity observed for a narrow bandwidth will be improved when we increase the size of the bins. Hence, presenting a narrower bandwidth for the graphs should not be a concern. Table 4 summarizes the different bandwidths that will be used in our analysis.

Table 4: Different Choices of Bandwidths

Graphs Bandwidth	Estimates Bandwidth
0.1	Bandwidth A: 1.16 at each side
0.15	Bandwidth B: 1.63 at the left and 1.16 at the right
0.2	Bandwidth C:1 at each side

Note: The table reports the different bandwidths in units of the grading score that will be used for the analysis given the low number of observations at the right of the discontinuity.

5.2 Checking the Validity of RD Assumptions

The validity of RD relies upon two critical assumptions. First, that all unobservable and observable covariates that can affect the outcome vary continuously with the forcing variable at the cutoff, excepting the treatment variable. If this is true, when we compare the expectation of the outcome variable conditional on the forcing variable at the left and right limit approaching the cutoff we can identify the local average treatment effect. Figures 4 and 5 confirm that this is indeed the case for all the observed covariates observed in the baseline year (2008/2009) which were not included in the grading score. Here, we present the graphs for a bandwidth of 0.15, the middle value. The graphs for the bandwidth of 0.1 and 0.2 are available upon request and show the same behavior.

The second assumption of RD is that the forcing variable cannot be precisely manipulated around the cutoff. If this is violated, then there will be no local random assignment around the cutoff. Following McCrary (2008) we plotted the frequency of the forcing variable to check whether there is a discontinuity in the distribution of the forcing variable at the cutoff value. Figure 6 presents the results of this exercise. The graph suggests there are no discontinuities in the density of the forcing variable around the cutoff value.

5.3 LATE of the Program

Under the validity of the previous assumptions RD allows to identify the local average treatment effect (LATE) of the program on each outcome (Y) (assuming local dependence) as:

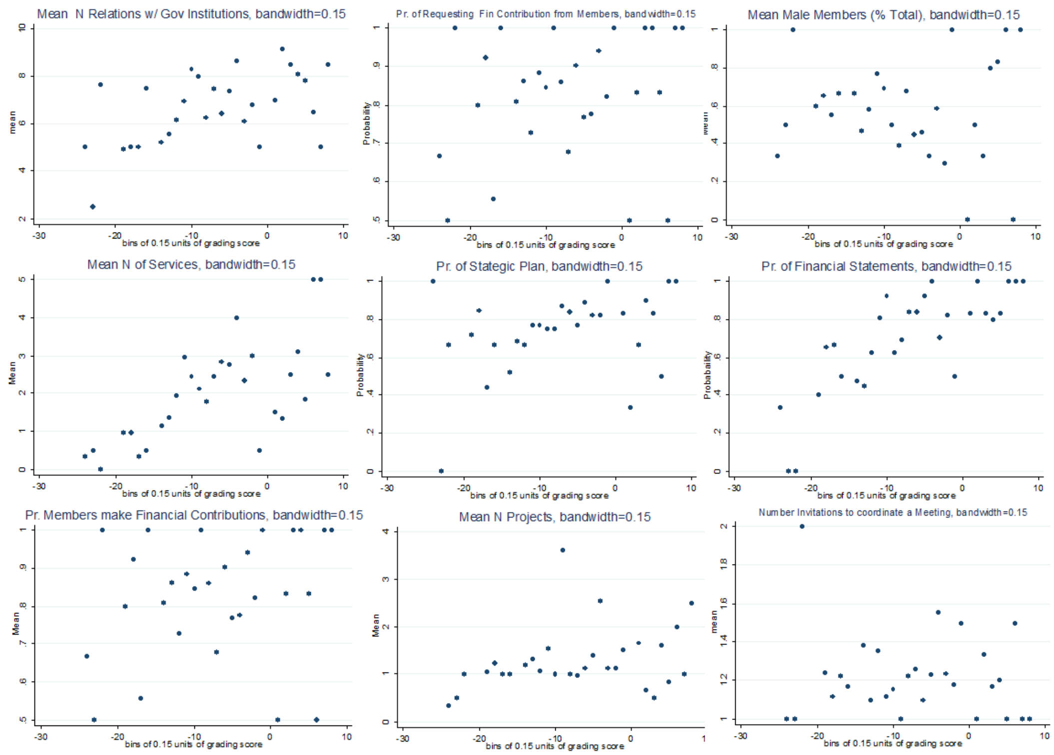

Figure 4: Continuity of covariates around $W = 0$

$\tau = \frac{\lim_{w\downarrow 0}\Pr(Y/W=w)-\lim_{w\uparrow 0}\Pr(Y/W=w)}{\lim_{w\downarrow 0}\Pr(D/W=w)-\lim_{w\uparrow 0}\Pr(D/W=w)}$

This coefficient can be obtained in practice by estimating the following functional form:

$$Y_i = a_l + (a_r - a_l)D_i + g(W_i) + X_i'A_0 + \varepsilon_i \qquad (2)$$

where X_i represents a vector of observable characteristics, $g(W_i - c)$ is a polynomial of order k for the forcing variable, and $(a_r - a_l) = \tau$ represents the coefficient of interest. Usually, we may want to include interactions between the forcing variable (*i.e.*, W_i) and the treatment dummy so that we do not impose restrictions on the underlying conditional mean functions to be the same at both sides of the discontinuity. However, Angrist and Pischke (2009) suggest that results based on this simpler model almost always turn out to be similar. Hence, we kept the simpler specification. Notice, that since we do not have perfect compliance around the cutoff value we will have to employ instrumental variables. The simplest most transparent first stage of such regression will be of the form:

$$D_i = b_l + (b_r - b_l)T_i + f(W_i) + X_i'B_0 + u_i \qquad (3)$$

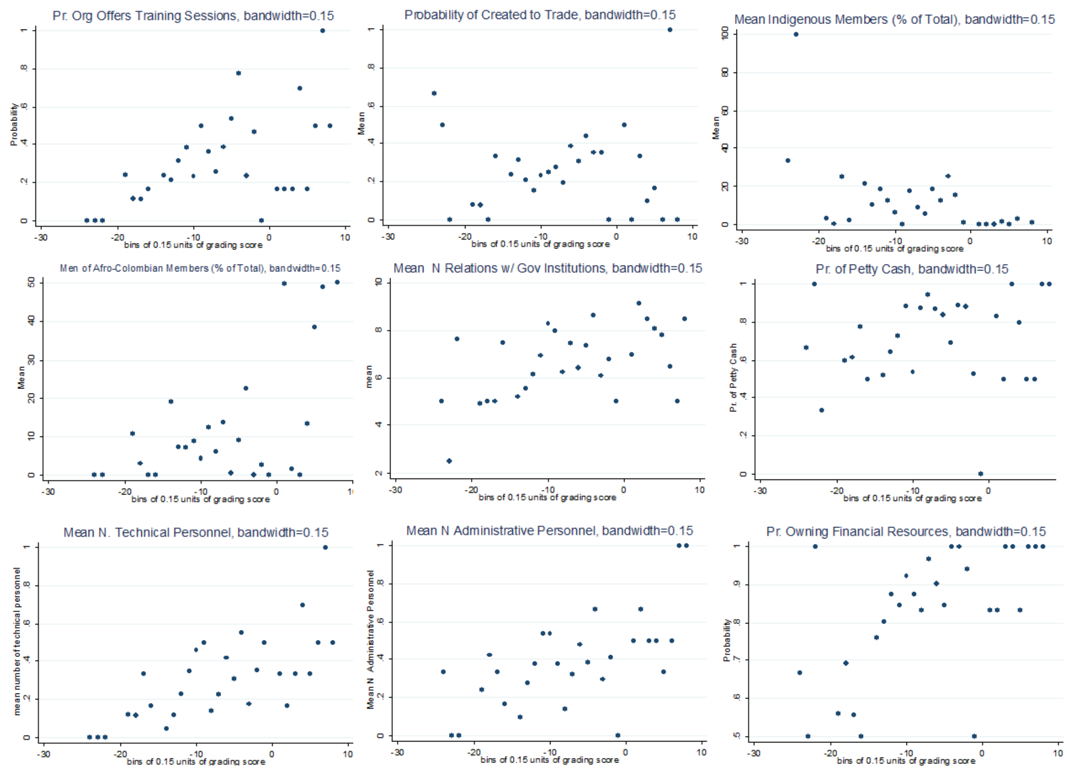

Figure 5: Continuity of covariates around $W = 0$

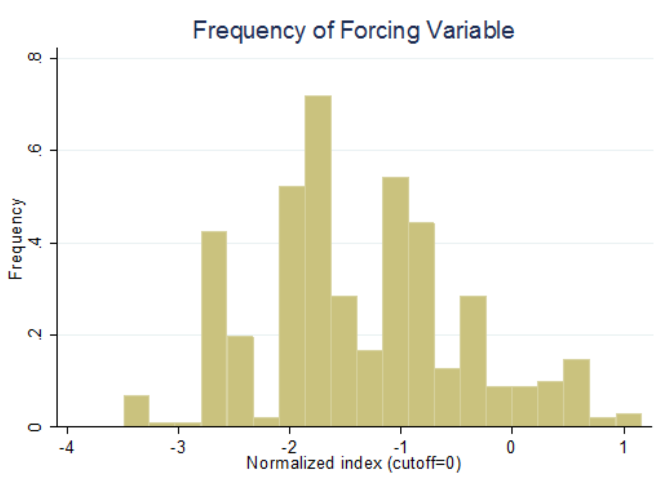

Figure 6: Histogram of the forcing variable

where $T_i = 1[W \geq 0]$. Effectively T_i is the instrument for treatment reception. Replacing (3) into (2) we get:

$$Y_i = \pi_0 + \pi_1 T_i + h(W_i) + X_i' \Pi_0 + \in_i \qquad (4)$$

where π_1 represents the coefficient of interest.

Table 5: LATE estimates of the Program by *Línea Productiva*

	Bandwidth A Polynomial Order			Bandwidth B Polynomial Order			Bandwidth C Polynomial Order		
Outcome Variable	**1**	**2**	**3**	**1**	**2**	**3**	**1**	**2**	**3**
Sales (millions of pesos)	166.66***	168.70**	167.1**	170.1***	173.10***	177.46**	170.65**	179.61***	177.38***
	(70.61)	(74.06)	(78.87)	(72.5)	(72.62)	(79.54)	(79.34)	(71.34)	(75.75)
Loan Application	0.34**	0.36**	0.37*	0.35***	0.36***	0.38**	0.39***	0.37***	0.39**
	(0.16)	(0.18)	(0.19)	(0.12)	(0.15)	(0.17)	(0.13)	(0.15)	(0.16)
Production Capacity	305.29	216.41	242.42	190.91	170.49	191.35	135.67	107.83	115.53
	(303.67)	(411.45)	(335.42)	(362.23)	(400.37)	(450.23)	(278.30)	(309.72)	(617.60)
Productive Capacity Area	110.94***	112.87**	118.54**	101.28***	114.79***	114.64***	114.37***	117.84***	118.84***
	(36.27)	(36.93)	(39.55)	(43.69)	(45.79)	(45.54)	(35.32)	(39.77)	(39.81)
Stage	-0.35*	-0.19	-0.07	-0.18	-0.15	-0.18	-0.13	-0.04	-0.44
	(0.13)	(0.23)	(0.20)	(0.15)	(0.21)	(0.28)	(0.16)	(0.27)	(0.36)
Sales Personnel	0.22**	0.23*	0.24*	0.27**	0.27**	0.28*	0.38***	0.38***	0.36**
	(0.11)	(0.12)	(0.14)	(0.12)	(0.12)	(0.15)	(0.14)	(0.15)	(0.16)
TI Security	0.34**	0.31*	0.36*	0.37**	0.39*	0.34*	0.34**	0.38**	0.34*
	(0.17)	0.18	(0.20)	(0.18)	(0.20)	(0.21)	(0.16)	(0.18)	(0.18)
Publicity	0.14	0.07	0.05	0.04	0.05	0.05	0.26	0.29	0.27
	(0.15)	(0.2)	(0.23)	(0.19)	(0.21)	(0.31)	(0.21)	(0.26)	(0.36)
Trade Fund	0.18***	0.19**	0.18*	0.19***	0.20**	0.21**	0.21***	0.22*	0.24**
	(0.07)	(0.08)	(0.10)	(0.06)	(0.07)	(0.10)	(0.07)	(0.09)	(0.11)
Organization's quality	0.2	0.18	0.19	0.1	0.14	0.13	0.07	0.07	0.08
	(0.25)	(0.42)	(0.28)	(0.31)	(0.41)	(0.41)	(0.33)	(0.41)	(0.48)
Sales through Org	-31.23*	-38.13	-31.34	-25.34	-34.76	-25.65	-24.53	-21.90	-23.70
	(17.52)	(28.75)	(28.25)	(22.12)	(27.94)	(35.36)	(24.81)	(31.25)	(40.84)
Number of Products	-0.61*	-0.519	-0.513	-0.533	-0.537	0.541	-0.12	-0.129	-0.1296
	(0.28)	(0.36)	(0.38)	(0.29)	(0.35)	(0.47)	(0.35)	(0.37)	(0.33)
Product 's Quality	1.07***	1.01***	1.03***	1.02***	1.04***	1.06***	1.09***	1.07***	1.05***
	(0.33)	(0.36)	(0.42)	(0.34)	(0.40)	(0.42)	(0.3)	(0.37)	(0.38)
Producer's Quality	0.29	0.256	0.21	0.22	0.22	0.24	0.15	0.14	0.17
	(0.21)	(0.39)	(0.34)	(0.28)	(0.38)	(0.39)	(0.31)	(0.33)	(0.44)
N of Observations		269			215			162	

Note: The table presents the results of an instrumental variable on a polynomial for the forcing variable and the treatment dummy instrumented with the discontinuity dummy. Robust standard errors are presented in parentheses.

Table 5 reports the results of the estimates of equations (3) and (4) for different bandwidths and different polynomials of the forcing variable. They suggest that there is a positive LATE of the program on six variables. In particular, the estimates indicate that compared to the control observations those *líneas productivas* treated by the program had at least: i) additional sales of 166 million Colombian pesos; ii) 23% higher probability of applying to a loan; iii) 114 additional hectares of productive area; iv) 18% higher probability of having a trading fund; v) 1.09 additional units on their index quality; and, vi) 22% higher probability of having sales personnel. These results are robust to the specification of the polynomial for the forcing variable and to variations of the bandwidth size. However, the size of the effects seems to be growing as we approach the discontinuity and reduce the number of observations.

In addition, the estimates suggest that the *líneas productivas* developed in organizations that were treated by the program had more security issues in transporting the products. The estimates indicate the likelihood that a treated observations faced a security transportation issue was 30% higher than that of the the control observations. This is in line with the results obtained using the propensity matching score.

In sum, both the results of PSM and RD seem to suggest a positive impact of the program on the same outcomes. However, the results suggest a higher local average treatment effect than the effect identified by matching through the propensity score (i.e., average treatment effect on the treated). The RD estimates suggest that as we move near the discontinuity there is stronger impact of the program. If our estimates are correct, RD should be closer to an experimental evaluation than PSM, and hence, its results are more transparent. The difference in the size of the estimates may be explained by a bias in the PSM estimates due to some selection on the unobservables we were unable to control. In any case, both estimates suggest a positive and relevant effect of the program.

6 Evaluation viewed as a Decision Problem

The methodology used in this section is taken from Dehejia (2005) and strongly relies on Bayesian decision theory. The objective is to identify whether: i) the program chosen for each organization was optimal (when comparing between different alternative policy combinations), and ii) which are the characteristics of the most successful organizations.

Currently, in Colombia, there are several programs targeted towards agricultural producers at risk of becoming involved in illicit drug production. Since 2008, any organization of agricultural producers may be beneficiary of three types of programs: *Programa Proyectos Productivos* (PPP), *Programa Familias Guardabosques* (PFGB), and the FCA pilot program for commercialization (the program evaluated in this paper). PPP began in 2003 with the objective of supporting rural families in generating new sources of income. The project gave support through matching grants to acquire infrastructure or technical guidance on the elaboration of productive projects from organizations of rural producers in six different types of products: cacao, coffee, palm, livestock, and sugar cane. PFGB is a conditional cash transfer program initiated in 2003, which gives a cash allowance conditioned on the elimination of illicit crops and on the reception of technical guidance on alternative licit agricultural initiatives. From 2007, all the beneficiaries of PFGB were conditioned on participating on some PPP initiative. Hence, in our sample all of the beneficiaries of PFGB are as well beneficiaries of PPP.

The main difference between PPP and the FCA program is that PPP supported projects directly formulated by the producer's organizations. These proposals are voluntary and do not have a maximum financial ceiling nor a time limit. PPP does not transfer money directly to the organizations, instead it supports them in obtaining infrastructure to transform their products. In contrast, the FCA program was strongly emphasized on strengthening commercialization and the operations of the treated organizations.

In our sample all the organizations are being treated by PPP only, some of them by PPP and PFGB, and some others by the three programs. Appendix E briefly reviews the main concepts of Bayesian decision theory, which we use to simulate the distribution of sales for each organization under the combination of alternative policies. From these simulations we will be able to compare the results of alternative combination of programs on total sales.

6.1 Simulation Results

We simulate the distribution of sales for each organization when it is part of both the treatment and control group for each of two different combination of policies. For case A, we compare the mean sales of each organization when they received only PPP (control group) vs. receiving all programs (treatment group); and for case B, we compare receiving PPP and PFGB (control group) vs. receiving all programs (treatment group). Hence case A measures the combined effect of the PFGB and FCA programs, whereas case B measures the effect of FCA only. Not surprisingly, our estimates suggest that the treatment always yields higher mean sales. In other words, all of the organizations have higher mean sales when they receive the three programs (PPP, PGFB and FCA) relative to receiving only PPP and PFGB (case A) or only PPP (case B).

For each case we constructed the mean difference in sales under the treated and control distribution. The results of this exercise are presented in Table 6. As can be seen, the net effect created by the FCA program alone (case B) is higher when producers are already been treated by the other two programs. In particular, the mean sales are 1,250 millions of Colombian pesos higher when the organizations are treated under case B, and 915 millions of Colombian pesos higher when treated under case A. This suggests that rural producers may need additional incentives to abandon illicit drug production permanently. In case B these incentives are previously established by the conditional cash transfer program.[13]

Table 6: Results of Simulations

	Treatment	**Control**	**Mean Gains**	**N. Org w/ total gains > mean gains**
Case A	PPP+PFGB+FCA	PPP only	915	225
Case B	PPP+PFGB+FCA	PPP+PFGB	1250	278

Note: Gains are reported in sales (in millions of Colombian pesos). The table presents the results of the simulated distribution of total sales for each of the organizations in the sample. PPP stands for Productive Projects Program; PFGB for *Programa Familias Guardabosques*, and FCA for the pilot program evaluated in this paper.

We also used the simulations to characterize those organizations that were the most successful under case B. We only carry this exercise for case B since we want to characterize those organizations that were the most successful organizations under the FCA program only. For this purpose, we created a dummy variable that takes the value of one if the gains for

[13] We also counted the number of organizations which had gains higher than the average gains of the whole sample under each Case A and B. As is reported in Table 12 the distribution under case B is more right-skewed. In other words, there are more organizations with mean sales higher than the average sales for the whole sample for that case. This results may be explained by the incentives created by each of the programs. Since PGFB gives cash directly to producers, it is likely that it reduces the effort that each of the producers commits towards increasing sales.

the treated organization are higher that the mean gains created under the whole treatment group. We used this variable as a dependent dummy in a probit regression. The covariates included as independent variables in the regression were all those variables observed in 2009 with no missing values for any organization in the sample. This exercise will allow to identify the characteristics of the most successful organizations. Table 7 reports the results of the regression and the marginal effects of the model for each covariate.

Table 7: Determinants of Most Successful Treatment Effects

Dependent Variable: I(=1 if mean gains of Org¿mean gains of whole sample)				
Variable	**Coefficient**	**Std. Error**	**Marginal Effects**	**Std. Error**
Created PP	-0.73	0.69	-0.26	0.26
Created coll	-0.43	0.67	-0.14	0.21
Created com	-0.59	0.70	-0.20	0.26
Number of Services	-0.28	0.35	-0.09	0.11
Financial Contributions	0.93***	0.26	0.34***	0.09
Balance Sheets	1.96***	0.32	0.65***	0.08
Bank Account	2.54***	0.41	0.43***	0.04
Strategic Plan	1.97***	0.26	0.66***	0.07
Main product Cacao	-0.06	0.23	-0.02	0.08
Main Product Coffee	-0.22	0.27	-0.08	0.09
Main Product Palm	-0.30	0.35	-0.10	0.13
Main Product Forestal	1.15***	0.55	0.22***	0.06
Main Product Livestock	-0.09	0.29	-0.03	0.10
Complaints and Claims established	-0.01	0.18	0.00	0.06
Control Institutions	0.07	0.25	0.02	0.08
Capital Property	0.09	0.25	0.03	0.08
Financial Statements	0.04	0.32	0.01	0.11
Supports Production	-2.11***	0.23	-0.55***	0.04
Supports Intermediation	-1.3***	0.30	-0.48***	0.11
Supports Packaging	0.10	0.38	0.03	0.12
Supports Transformation	0.64*	0.38	0.17***	0.08
Supports Commercialization	1.12***	0.27	0.35***	0.07
Supports Storage	-2.47***	0.30	-0.78***	0.06
Hired Manager	0.32	0.28	0.09	0.08
Administrative Personnel	0.09	0.22	0.03	0.07
Technical Personnel	0.24	0.22	0.07	0.07
N of relations with institutions	-0.02	0.03	-0.01	0.01
Pseudo R2		0.55		
N Observations		454.00		

Note: Results of a probit model estimation with robust standard errors. *: 1% significance, **: 5% significance, and ***:1% significance

The tables show that those organizations that requested financial contributions from its members, had a balance sheet, a bank account and strategic plan, and supported commercialization and transformation of products had a higher probability of being more successful when treated by the program. This suggests that for the future design of similar programs this should be variables to take into account when selecting the program's beneficiaries.

7 Conclusions

The estimates carried in this paper suggest that the FCA program had a strong and positive impact on total sales, productive capacity, and product's quality of the treated organizations. However, it is important to highlight that this program was implemented simultaneously with other programs such as PPP and PGFB. Thus, it is likely, that the results we observe are a result of the combination of multiple efforts from different programs. In particular, we do not observe the results of the program when implemented alone. This must be taken into account when trying to replicate this program in other environments, since the program's success may be condition upon its combination with other treatments received by the organizations.

Moreover, based on our discussion with the executing unit of the program and other local stakeholders on coca-producing areas, two unique characteristics of the FCA program that may have contributed towards its success were the elaboration of a careful diagnostic study to identify the needs of the organizations to be treated, and the fact that the program offered a heterogeneous treatment by organization. In addition, delivering the treatment to second-level organizations was an effective way of reaching the smaller organizations. This design recognized that given the complicated situation of rural producers at risk of participating of illicit drug production, trust was an issue that could be solved by working through local actors.

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A Beneficiaries Selection

Once a potential group of beneficiaries was selected UNODC carried out deeper analysis to identify the beneficiaries. In particular, the organizations in the groups were classified according to a final score constructed based on 5 categories. Each category was calculated as the sum of the scores of an extensive group of variables summarized in Table 1 below. The weight that each variable had on the total score is included in parenthesis next to each variable. The program treated a total of 57 organizations of producers which were chosen as the ones that had the best final score until resources lasted[14].

Category	Variables included (weight of total score)	Weight
Administrative	Democratic management of organization (5.4%), rule adoptance (4.7%), member's commitment (1.9%) strategic allies (0.8%), equity of organization (0.4%), and existence of documents (1.5%)	15%
Quality System	Quality system (18%), environmental organization (3.5%), administrative security and environmental health (2.5%), and risk analysis (5.9%).	30%
Logistic	logistic management (10%)	10%
Commercial	National commercialization (9.6%), international commercialization (18.1%), Certifications and quality seal (2.3%)	30%
Financial	Liquidity (2.63%), economic activity index (1.75%) debt (2.4%), profitability (2.6%), payment capacity (2.2%), operative efficiency (2.3%) and commercial efficiency (0.9%)	15%

Figure A-1: Development Score used for selecting beneficiaries

[14]This 57 organizations can be grouped in 10 bigger organizations. However, the analysis here will be carried out for the smaller organizations.

B Outcomes

Table B-1: Outcomes analyzed by *Línea Productiva*

Name of Variable	Variables Description
Sales	Total sales (Colombian pesos)
Utility Margin	Utility Margin (Colombian pesos)
Loan Application	=1 if applied to loan
Production Capacity	Productive Capacity of Members (Ha of land)
Productive Capacity Area	Productive Capacity in Production (ha of land)
Stage	=1 if stage of establishment and sustainability, 2=commercialization and production
Sales Personnel	=1 if Org has Sales Personnel
TI Security	=1 if transportation issue security
TI Highways	=1 transportation issue highways condition or no roads
TI weather	=1 if transportation issue weather
TI Costs	=1 if transportation issue high costs
TI No Transp	=1 if transportation issue no transport
Publicity	=1 if org makes product's publicity
Trade Fund	=1 if org has commercialization fund
Organization's quality	Number of quality certifications of Organization
Sales through Org	Percentage of Members who sale through Organization
Number of Products	Number of Products
Product 's quality	Number of Quality Certifications/ Number of Products
Producer's Quality	Number of certifications of producers for main product

C Variables Included in the Propensity Score

Table C-1: Variables Included in the Estimation of the Pscores

Variables included in the probit model	Description
Years	Number of years since foundation
N. members	Number of members
N. Donors	Number of donors
Municipality	=1 if members live in same municipality
Created com	=1 if created to commercialize products
Created PP	=1 if created to participate in ”Proyectos Productivos”
Created coll	=1 if created to work collectivelly
Number of Services	Number of Services
Copy of rules	=1 if more than 50% of members have copy of the rules
Newspaper com.	=1 if organization communicates to members through newspaper
Radio com.	=1 if organization communicates to members through radio
Web com.	=1 if organization communicates to members though the web
Complaints system	=1 if org has a complaint system in place
N. Invitations	Number of invitations needed to coordinate a member’s meeting
Years board	Years between board elections
Control Institutions	=1 if org has a control institution
Financial Contributions	=1 if members pay an economic contribution
Capital Property	=1 if org has capital property
Financial Statements	=1 if org has financial statements
N. Credits	Number of credits being processed
Balance Sheet	=1 if org has balance sheets
Petty Cash	=1 if org has petty cash
Strategic Plan	”=1 if org has a strategic plan
N. of Projects	Number of projects being executed by org
Supports Production	=1 if org supports production
Supports Intermediation	=1 if org supports intermediation
Supports Packaging	=1 if org supports packaging
Supports Transformation	=1 if org supports transformation
Supports Commercialization	=1 if org supports commercialization
Technical Personnel	=1 if org has technical personnel
Administrative Personnel	=1 if org has administrative personnel

Table C-2: Probit Model Estimates

Variable	Coefficient	St. Error
Years	0.05	0.02
N. members	0.00	0.00
N. Donors	-0.61	0.44
Municipality	-0.07	0.25
Created com	21.38	0.99
Created PP	21.52	0.95
Created coll	21.46	0.96
Number of Services	-0.11	0.07
Copy of rules	0.16	0.26
Newspaper com.	0.04	0.30
Radio com.	1.15	0.42
Web com.	0.84	0.47
Complaints system	-0.27	0.25
N. Invitations	-0.20	0.29
Years board	-0.34	0.16
Control Institutions	0.29	0.42
Financial Contributions	0.27	0.34
Capital Property	-0.88	0.32
Financial Statements	0.33	0.46
N. Credits	0.01	0.04
Balance Sheet	0.89	0.39
Petty Cash	0.12	0.26
Strategic Plan	-0.20	0.28
N. of Projects	-0.09	0.10
Supports Production	0.27	0.27
Supports Intermediation	0.26	0.32
Supports Packaging	-0.40	0.48
Supports Transformation	-0.35	0.52
Supports Commercialization	0.35	0.27
Technical Personnel	0.09	0.26
Administrative Personnel	0.38	0.28
Pseudo R2	0.26	
N. Observations	438	

D Constructing the Predictive Distribution of Sales

D.1 Bayesian Decision Theory

Bayesian Decision Theory is concerned with identifying the best decision rule a planner can make under certain assumptions. According to Bayesian Theory the optimal decision rule is that which minimizes the expected loss function for the relevant population, in this case organizations of producers. In general, the problem to be solved could be written as:

$$\text{Min}_D E[U(Y, D)/data]$$

where D represents the available choice of parameters, and $U()$ represents the objective function to be minimized (usually represented as the sum of the squared error). What makes this theory different from other optimization problems is that the expected value to be minimized is calculated based on the **posterior distribution of the parameters conditional on the data** that we observe, which we will denote as $P(\theta/Y)$. Here $\theta = (\beta, \sigma)$ will represent the parameters of the program. In particular β denotes the coefficients of a regression of total sales on the treatment dummy (for each policy to be analyzed), and other relevant covariates; and σ represents the variance of the mean squared error. In other words, Bayesian theory assumes that the parameters of the model are uncertain and unknown, and hence, they have a distribution of their own. The uncertainty of the parameters (i.e., the posterior distribution of the parameters) will affect the outcome distribution and should be taken into account when trying to derive the predicted distribution of sales.

Our interest in the evaluation will be emphasized in obtaining the predictive distribution of sales for each of the organizations in the sample. This distribution embodies all of the uncertainty of the model, and for this reason we first need to derive the posterior distribution of the parameters of the model and from it we can derive the predictive distribution of sales.

In our data we observe the sales of those organizations that are the most organized, and hence, keep financial statements. Since for those organizations that did not keep records sales are likely positive but just not observed sales will be modelled using a censored normal likelihood: a Tobit model. Define the latent variable y_{it}^* as a latent variable of observed sales such that:

$$y_{it} = \begin{cases} y_{it}^* & \text{if } y_{it}^* > 0 \\ 0 & \text{otherwise} \end{cases}$$

here y_{it}^* will be observed if the organization kept financial statements. For this Tobit model it will be assumed that $Y_{it}^*|\{X_{it} = x_{it}, \beta, \sigma\} \sim N(x_{it}\beta, \sigma)$. This distribution will be called the **predictive distribution of sales.** The objective is to construct a predicted distribution of sales that embodies the uncertainty of the parameters of the model. Following Dehejia (2005) we will employ the Gibbs sampling algorithm to construct the posterior distribution of the parameters. The algorithm is described in detail in Appendix D.

Using this algorithm we will obtain 1000 draws of the posterior distribution $\{\beta_{(j)}, \sigma_{(j)}^2\}_{j=1}^{1000}$, of the parameters and with them we will be able to construct the simulated distribution of sales for each organizations under the different policies to be analyzed. These distributions are what we called the predictive distribution of sales. The process to simulate the predictive distributions of sales is described in detail in Appendix D.

The results of these simulations will only hold if our choice of likelihood was correct. In other words, the fit of the model should be tested. Figures 16 and 17 confirm that the empirical distributions of total sales for treated and control units are well approximated by the mean distribution of our simulations.

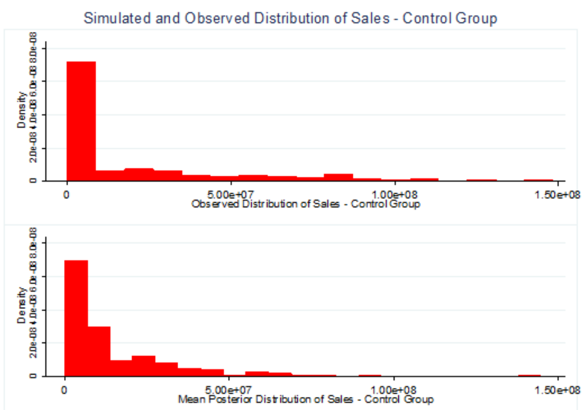

Figure C-1: Observed Sales and Simulated Mean Sales Distribution - Control Group

D.2 Constructing the Posterior Distribution of Parameters

We first estimated the posterior distribution of the parameters through a Gibs sampling method. The gibs algorithm consists of the following steps:

- Let y_{it}^z equal y_{it} for the uncensored observations, i.e., $\{i, t|y_{it} > 0\}$, and for the censored observations $\{i, t|y_{ii} = 0\}$, draw y_{it}^z from the negative portion of a truncated normal distribution with mean $x_{it}\beta$ and variance σ^2.

- Draw for β from $N(\widehat{\beta}, \sigma^2(x'x)^{-1})$ where $\widehat{\beta} = (x'x)^{-1}x'y^z$

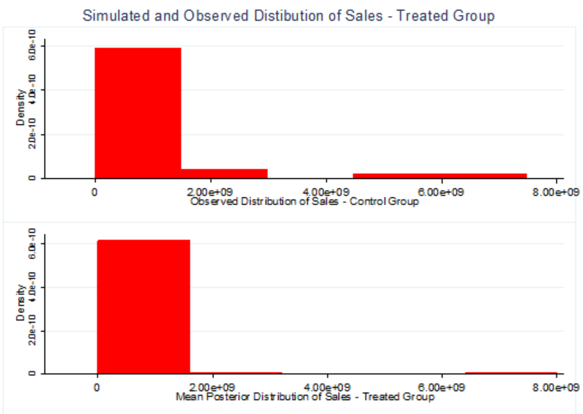

Figure C-2: Observed Sales and Simulated Mean Sales Distribution - Treated Group

- Draw for σ^2 from a Gamma$(8840, ||y^z - x\beta||^2/2)$
- Iterate on this algorithm 5,000 times and keep only the last 1,000. This completes the estimation of the posterior distribution of θ.

D.3 Simulating the predictive distribution or sales

To simulate the predictive distribution of earnings we followed the following steps:

- For each organization in the sample consider X_{it1} and X_{it0} to be a vector of covariates that represents the observed characteristics of the organization. Both vectors differ only in that X_{it1} (X_{it0}) has the treatment dummy equal to 1 (to 0).
- Use the stored draws of the posterior distribution $\{\beta_{(j)}, \sigma^2_{(j)}\}_{j=1}^{1000}$, and based on the parameters draw for the predictive distribution of earnings when the organization received the treatment and when it did not from a normal distribution:

  $y^{(j)}_{it1}|\{X_{it} = x_{it1}, \beta, \sigma\} \sim N(x_{it1}\beta_{(j)}, \sigma^2_{(j)})$
  $y^{(j)}_{it0}|\{X_{it} = x_{it0}, \beta, \sigma\} \sim N(x_{it0}\beta_{(j)}, \sigma^2_{(j)})$
- From the previous estimation we obtained y_*, from it we recover the simulates sales as:

  $$y_{it} = \begin{cases} y^*_{it} & \text{if } y^*_{it} > 0 \\ 0 & \text{otherwise} \end{cases}$$
- Store the predictive distribution of outcomes for each organization

E Constructing the Predictive Distribution of Sales

E.1 Bayesian Decision Theory

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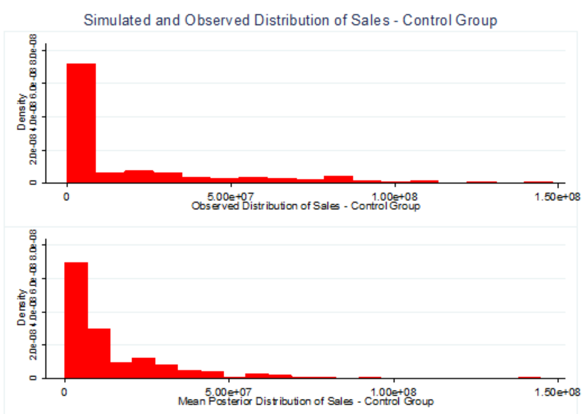

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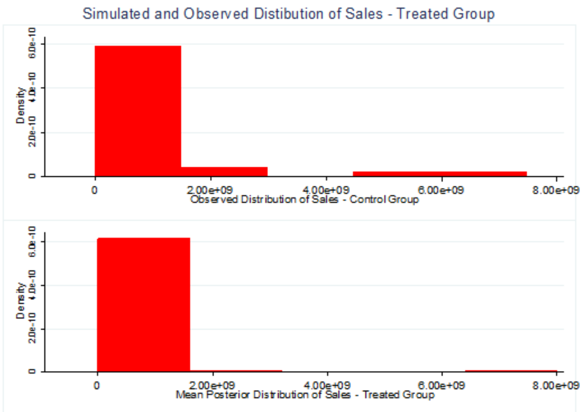

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