On the Effectiveness of Supply Reduction Efforts in Drug Producing Countries: Evidence from Colombia *

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Abstract

In this paper we exploit a natural experiment induced by a diplomatic agreement between the governments of Colombia and Ecuador to evaluate the effectiveness of a popular anti-drug supply program (aerial spraying with herbicides) on illegal drug production. In 2008, due to the possible negative effects of aerial spraying, the Colombian government pledged to stop the spraying campaigns in a 10 kilometer band around the international frontier with Ecuador. We use this exogenous variation and 1-square-km-grid-level satellite data with the exact geographic location of coca crops to identify the effectiveness of the program using conditional difference in difference and regression discontinuity. Our results suggest that spraying campaigns have a small but significant effect on coca cultivation.

Keywords: Drug trade, Colombia, war on drugs.

JEL Classification: O13, O33, O54 and Q18.

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1 Introduction

Most of the cocaine produced in Colombia is exported. It is estimated that close to 60% of the production is sold in North American markets, and the remaining 40% is exported to European markets (Mejía and Rico, 2011). In fact, between 60% and 70% of the cocaine consumed worldwide is produced in Colombia (UNODC, 2012). The income generated by this illegal business is significant. It is estimated that in 2008 the economic size of cocaine markets in Colombia was of approximately \$13.6 trillion pesos (approximately \$7.8 billion dollars), which accounts for about 2.5% of the Colombian GDP. These resources not only come from illegal activities (the production and trafficking of cocaine) but are also the main source of funding for illegal armed groups such as the *Fuerzas Armadas Revolucionarias de Colombia* - FARC, paramilitary groups, and the so-called criminal bands.

Colombia has been a key player in illegal drug markets during the last 30 years. Although before 1994 Colombia was only a marginal player in terms of coca cultivation, successful interdiction policies implemented by the Fujimori administration in Peru during the first half of the nineties induced a displacement of coca cultivation from Peru to Colombia during the second half of that decade (Angrist and Krueger, 2008). After the large increase in coca cultivation that took place in Colombia during the second half of the 1990s and the increasing involvement of FARC in this illegal business, in September of 1999 the governments of Colombia and the U.S. announced a new joint strategy which would come to be known as the *Plan Colombia*. This plan had two main goals: first, reducing the production of illegal drugs (mainly cocaine) by 50% in the next six years, and second, to improve security conditions in the country by regaining the control of large parts of the national territory which were under the control of illegal armed groups. According to official figures from the Colombian government, the United States government disbursed close to \$470 million dollars per year between 2000 and 2008 in subsidies to the Colombian armed forces to fight against the production and trafficking of drugs.

Additionally, the Colombian government spent close to \$710 million dollars per year during the same period in the fight against illegal drug production and trafficking under Plan Colombia. Altogether, between 2000 and 2008 the total expenses on the military component of Plan Colombia represented close to \$1.1 billion dollars per year, corresponding to 1.1% of the country's annual GDP[1].

Among the implemented strategies with the resources of Plan Colombia were spraying campaigns with herbicides to kill the coca crops, manual eradication efforts, control of chemical precursors used in the processing of coca leaf into cocaine, the detection and destruction of cocaine processing laboratories, and seizing of drug shipments en route to foreign countries. Of these activities, aerial spraying has been by far the main anti-drug strategy in terms of financial

[1] As such, Plan Colombia is the largest anti-drug intervention that has ever been made in a producing country.

resources invested.

On average, 128 thousand hectares have been sprayed with herbicides per year, of which almost half are located in Putumayo and Nariño, the two Colombian departments bordering Ecuador. Figure 1 shows the evolution of coca cultivation, hectares sprayed with herbicides and the number of hectares manually eradicated for the whole country (panel A) and for the departments of Nariño and Putumayo (panel B). The figures show that about a third of the total coca cultivation in Colombia between 2000 and 2010 has been located in Putumayo and Nariño, but that these two departments account for about half of the total spraying and manual eradication campaigns in the country.

Spraying campaigns are generally carried out in small airplanes to fumigate coca crops with substances such as Roundup, a herbicide. Glyphosate is the main active ingredient in this herbicide and it contains the surfactant POEA, which helps the glyphosate penetrate the plant's foliage and destroys it. The goal of fumigation with glyphosate is to inhibit the enzyme in charge of synthesizing the aromatic amino acids in actively growing plants, preventing them from staying alive. This substance is absorbed through the foliage of the plant, and is only effective in growing plants (fumigation with glyphosate is not effective in preventing the germination of seeds).

Despite the large amounts of resources and the effort invested in aerial spraying, it has been extremely hard to find convincing empirical evidence about the causal effect of aerial spraying campaigns of herbicides on illicit crops cultivation in producer countries. The main reason behind this has to do with the serious endogeneity issues that arise from the simultaneous determination of the location of coca crops and the areas targeted by aerial spraying campaigns. In short, policy endogeneity (e.g., the fact that, almost by definition, more spraying is going to be done in areas with higher presence of coca cultivation) has posed a serious challenge to the identification of the effects of anti-drug policies aimed at reducing illicit crops cultivation. Given the large amount of resources invested in this strategy and the alleged collateral costs of exposition to herbicides on health and the environment, it is crucial to obtain systematic evidence on the effectiveness of these campaigns in reducing coca cultivation. Furthermore, assessing the effectiveness of aerial spraying of illicit crops is important in the context of a growing debate in Latin America about the costs and effectiveness of anti-drug strategies being implemented in producer and transit countries.

The main goal of this paper is to overcome this empirical challenge and identify the causal impact of aerial spraying with herbicides on coca crop cultivation. For this purpose, we exploit a natural experiment induced by a diplomatic friction between the governments of Ecuador and Colombia in the mid-2000s. More precisely, since the beginning of Plan Colombia in the year 2000, the Ecuadorian government has repeatedly alleged that the spraying of illicit crops with herbicides in the Colombian territory bordering Ecuador was causing health problems to the Ecuadorian population living in the border area and environmental damages along the international frontier, since the winds pushed the herbicide inside

Ecuadorian territory. As a result, starting in 2008 the Colombian government decided to completely stop aerial spraying campaigns in a band of 10 km from the international frontier with Ecuador. This diplomatic friction and the resulting compromise induced a quasi-natural experiment that allows us to estimate the causal impact of spraying on coca cultivation.

This paper's main contribution is twofold. First, we use a unique data set collected by the United Nations Office of Drugs and Crime with satellite images that contain precise information on the location of all the coca crops in Colombia. In particular, we observe units (grid points) of one square kilometer and for each of them we are able identify the hectares of coca grown, the hectares sprayed, and the hectares manually eradicated. This level of precision in the data allows us to assess the effects of anti-drug programs without aggregating the outcomes by municipalities or department. Second, we evaluate the effects of the program using a plausible source of exogenous variation that will correct the endogeneity issues that have made it difficult to interpret the evidence found in other studies as causal. In particular, we use two different estimation methods. First, a conditional difference in differences model that compares the evolution of coca crops in the exclusion area with the evolution of coca cultivation within the adjacent 10 km strip before and after the no-spraying zone was put in place. Second, we use regression discontinuity design that compares the levels of coca cultivation in grid points around the 10 km line after the no-spraying zone was implemented.

We address the two main threats to our identification strategy, mainly that: i) the government is not compensating the restriction to spraying campaigns in the exclusion area by increasing the intensity of other programs such as manual eradication, and that ii) coca-producers are not manipulating the governmental rule for their benefit by moving their crops to the exclusion area where no spraying can be done.

All of our estimates point to a small but significant effect of the program. The conditional difference in difference estimates suggest that the areas that were more exposed to aerial spraying after 2008 had on average 20% less hectares of coca relative to the areas in the exclusion area (where the fumigations were forbidden). The regression discontinuity design estimates suggest that the program induced a local average treatment effect between -30% and -35%. This corresponds to a reduction of -0.29 to -0.34 hectares of coca cultivation per grid. It is possible, that coca cultivation is displaced by aerial spraying campaigns in a way we are unable to measure. As such, our estimates should be taken as an upper bound of the effects of aerial spraying campaigns on coca cultivation.

The rest of the paper is organized as follows. Section 2 describes the related literature; section 3 describes the natural experiment that we will use to identify the impact of aerial spraying on coca cultivation and the data. In Section 4 we present the estimates of the effects of the program through the conditional difference in differences methodology, and in Section 5 we present the results of the program employing the regression discontinuity approach. Section 6 presents the main conclusions.

2 Related Literature

The academic literature has confronted very serious methodological challenges in documenting the causal relation between enforcement variables and crime. One of the most difficult questions to tackle concerns the effect of enforcement (policing) on crime. A positive correlation between enforcement and crime does not necessarily imply a causal relationship from the former to the latter, but rather that more police force is allocated to higher crime areas. In fact, the few papers in the literature that have been able to convincingly solve the endogeneity problems associated with the estimation of a causal relation between policing and crime have found negative and significant effects (Di Tella and Schargrodsky, 2004; Evans and Owens, 2007; and Buonanno and Mastrobuoni, 2012, among others). Similar problems, although less well known, arise when trying to identify causal effects of enforcement on illegal markets. One such example is the effect of implementing anti-drug strategies on the extent of illicit drug production, smuggling or consumption. Although several papers have tried to estimate the effects of different anti-drug strategies on drug production and trafficking in producer and transit countries, few of them can convincingly claim to have established a casual effect, possibly due to the of lack of any exogenous variation in the implementation of anti-drug strategies.

Different studies have focused on studying the effectiveness of some of the strategies adopted in the war on drugs in producer countries. For instance, Mejía and Restrepo (2011 and 2012) have followed a more structural approach to understand the main forces and determinants of the costs, effectiveness and efficiency of different anti-drug strategies in producer and transit countries. The main result of these studies is that targeting the initial stages of illicit drugs production (e.g., spraying and eradication of illicit crops) is costly and ineffective relative to policies aimed at targeting later stages of production (international drug trafficking).

Apart from structural estimations or the calibration of general equilibrium models of the so-called "war on drugs", other papers in the literature have followed an empirical approach to assess the effectiveness of different anti-drug strategies. Most of these contributions have focused on estimating the impact of spraying campaigns on coca cultivation. However, few of them have convincingly established a causal relation. Similar to many eradication programs, aerial spraying is focused on the areas with larger presence of coca crops, thus generating a bias in the effects of spraying or manual eradication on illicit crops cultivation that come out of OLS estimates. For example, Moreno-Sanchez et al. (2003) and Dion and Russler (2008) use departmental data from Colombia with the goal of studying the effectiveness of the strategies, but they do not correct for biases induced by the existing endogeneity. Probably because of this, both studies find a positive correlation between the levels of spraying and the presence of coca crops.

More recent studies have tried to overcome the endogenity issues. Moya (2005) uses matching techniques employing municipal data from Colombia. His main conclusion is that aerial spraying does not have a significant effect on coca crops, unlike alternative development programs, which seem to accomplish their main objective of reducing coca cultivation. Reyes (2011) instruments spraying with the distance between fumigated hectares and the closest military base. His assumption is that coca crops located further away from the military bases have a higher cost of being sprayed and, as a result, face lower probabilities of being sprayed. His results indicate that there is no evidence of aerial spraying having any effect on the reduction of coca crops and, on the contrary, they find that an increase in 1% on aerial spraying produces an increase of slightly less than 1% in illicit crops. The main limitation of both of these studies is that they use aggregated data at the municipality level[2], which compromises the quality of any matching process and does not allow one to exploit adequately the plausible exogenous variation created by the distance to military bases. This occurs because a municipality may have several producers with dramatically different distances to a military base.

Finally, Rozo (2013) instruments spraying with the distance of coca producers to natural parks and indigenous territories, areas that cannot be sprayed according to governmental mandate. The author uses disaggregated data at the grid and producer level. The results suggest that aerial spraying has a negative and significant effect over coca production but that, at the same time, it worsens socio-economic indicators in the coca-producing areas. This paper aims at contributing to the existing evidence by exploiting a new, credible, and transparent source of exogenous variation in aerial spraying. Moreover, we use a unique dataset with disaggregated information that further allows us to capture variation sources omitted in other studies which use aggregate data.

3 The Natural Experiment and the Data

The natural experiment that we exploit came as a product of the diplomatic friction between the governments of Colombia and Ecuador, which resulted in a compromise by the Colombian government that pledged not to carry out more spraying campaigns in a 10 km strip along the border with Ecuador starting in 2008.

Since the beginning of fumigations under Plan Colombia, the government of Ecuador had protested before the Colombian government due to the alleged adverse effects of spraying on the health of Ecuadorians, negative effects on the environment and legal crops on the bordering area (i.e., livestock and other

[2]Colombia is divided into around 1,100 municipalities

crops) which, alleged the Ecuadorian government, even caused the displacement of indigenous communities who lived close to fumigated areas. In December of 2005, the Colombian government announced that it would discontinue fumigations in a 10 kilometer band around the international frontier with Ecuador within the Colombian territory. However, in mid-2006 the Colombian government recanted, and continued with spraying campaigns in the area. As a result of this noncompliance with the initial agreement, at the end of 2007 the Ecuadorian government filed a lawsuit against Colombia in the International Court of Justice in The Hague. The suit was filed on March 31st, 2008, and ever since then the Colombian government stopped all spraying campaigns in the 10 kilometer strip.

The implementation of this exclusion area generated a source for geographical and time varying exogenous variation in the treatment implementation. This occurred since the delimitation of the exclusion zone was chosen on grounds that had nothing to do with the presence of illicit crops. Figure 2 describes the geographic location of the exclusion strip.

We employ a unique data base that contains panel data on the presence of coca crops in one square kilometer grid points. The data is collected and processed by the United Nations Office for Drugs and Crime (UNODC) in Colombia. These data on coca cultivation comes from satellite images collected annually since 2001 (the satellite images are always taken on the last days of each year). We also have grid level data on the number of hectares sprayed and manually eradicated. While the former comes from GPS devices installed in the aircraft used in aerial spraying campaigns that record the exact location of the plane when the spraying valves are activated and closed, the latter comes from GPS devices used by manual eradication teams. For each grid point we observe the number of hectares of coca cropped, the number of hectares that were aerially sprayed, and the number of hectares manually eradicated (1 kms^2 = 100 ha.). In addition, we recovered the height— in meters above the sea level (MASL)—of the centroid of each grid.

We restrict our sample to all grid points with centroids located $20kms$ along the international frontier with Ecuador. The sample includes $10,880$ observations per year. The total sample was divided into two groups according to treatment status. Those grids under treatment were defined as those located further than $10km$ from the international frontier. They face a higher likelihood of being aerially sprayed after Colombia stopped spraying in the exclusion area. The control observations were defined as those grid points that are located in the exclusion area, less than $10km$ from the international frontier which have a lower probability of being aerially sprayed. The definition of the two groups can be observed on Figure 2.

Panel A of Figure 3 presents the mean number of hectares sprayed around the cutoff of $10kms$ between 1998 and 2010. The figure suggests that beginning in 2008 the mean number of hectares aerially sprayed in the control area fell to zero. Although the mean number of hectares aerially sprayed also fell for the treated area, it is always higher in the treatment relative to the control area after 2008. Moreover, the difference in mean aerial spraying is significant between groups for all years after the treatment implementation (see Figure 4).

Panels B and C in Figure 3 present maps of coca cultivation (green) and aerial spraying (yelow) in the region of interest. They confirm that in 2008 the spraying campaigns that were being carried out near the international frontier were discontinued (Panel C) relative to years such as 2006 (Panel B).

The evolution of the mean hectares of coca manually eradicated by treated area is presented in Figure 5 since the beginning of that program in 2007. It suggests that the size of the program has been declining. In fact, the mean number of hectares of coca manually eradicated has been decreasing in both treatment and control areas but more strongly in the last one. This behavior rules out the possibility that the government has been compensating for the prohibition in aerial spraying in the no treatment area by increasing the mean hectares of coca manually eradicated in that area.

4 Controlling for Selection on Observables and Unobservables

In this section we will combine the methodologies of propensity score matching and difference in difference (i.e., conditional difference in difference (CDiD) as defined by Heckman et al. (1998)) to identify the effect of aerial spraying on the total number of hectares planted. The CDiD methodology matches the grid points that are as similar as possible before the treatment was implemented (i.e., before Colombia decided to stop fumigations in the exclusion zone) in the control and treatment groups according to an index or *pscore* that describes the probability of being treated. Based on these matches, the CDiD is obtained as the difference in difference average effect on hectares of coca cultivated across time and treatment groups comparing observations that are as similar as possible.

The matching process controls for selection on observables whereas the difference in difference estimator (in time and between treatment status) eliminates all the time invariant unobservables that account for differences between the treatment and control groups. In general, according to Blundell and Dias (2002) the CDiD estimator is given by:

$$\widehat{\alpha}_{CDiD} = \sum_{i \in T} \left\{ [Y_{it_1} - Y_{it_0}] - \sum_{j \in C} W_{ij} [Y_{jt_1} - Y_{jt_0}] \right\} w_i$$

where Y represents the outcome of interest (i.e., hectares of coca planted), t stands for time, i for the observations in the treatment group T, j for the observations in the control group C, W_{ij} is the weight placed on each comparison observation j, and w_i are the weights that reconstructs the outcome distribution for the treated sample.

According to Heckman et al. (1998) and Blundell and Dias (2002) if a treatment takes place between two periods t and t' with $t > t'$ the CDiD estimator will only be valid if the following assumption holds:

$$E(Y_t - Y_{t'}|S = 1, P(X)) = E(Y_t - Y_{t'}|S = 0, P(X))$$

where Y represents the outcome of interest, S stands for a dummy for treatment status, and $P(X)$ represents the predicted *pscores*. For this case, the assumption states that in the absence of the diplomatic agreement the average outcome for the treatment and control groups would have experienced the same variation in time conditional on the observed covariates $P(X)$. The plausibility of this assumption in tested in Figure 6. It presents the difference of total coca cultivation of hectares cultivated with 95% confidence intervals by treatment status before the diplomatic agreement was implemented for all the observations in the common support. The figure confirms that the so-called "common trends assumption" is satisfied in this case. Since there seems to be a violation of the common trends assumption for 2006, this year was excluded from the final estimates, however, the results are not sensitive to this exclusion.

In practice, the CDiD estimator can be obtained by adjusting the difference in difference estimator to weights generated through the matching process. The reduced-form equation we run to identify the effect of the aerial spraying is:

$$Coca_{it} = \delta_0 + \delta_1 * D_{it} + \delta_2 * post_t + \delta_3 D_{it} * post_t + \gamma_t + \gamma_i + \epsilon_{it} \tag{1}$$

where D_{it} is an indicator variable that takes the value of one for those grids located further away between the 10kms and 20kms band around the international frontier with Ecuador, $post_t$ is a dummy variable that takes the value of one when the year is 2008 or later, and γ_t and γ_i are fixed effects by year and grid, respectively. The coefficient of interest is δ_3, which identifies the average treatment effect on the treated.

We use a probit model to estimate the predicted probability of treatment which was used to match the observations in the treatment and control groups with all the data between 2000 to 2007. The predicted probability of treatment is denoted as *pscore* henceforth. The independent variables included in the probit include all the observable covariates before 2008, which includes: hectares of coca planted (between 2000 and 2007), hectares sprayed (between 2000 and 2007), hectares manually eradicated (for 2007 only since the program began in that year), height in meters above sea level, dummies for municipality, and department. The pseudo R^2 of the model is 0.10. Appendix A presents the results of this estimation.

Moreover, Figure 7 presents the distribution of the predicted *pscores* for the full sample. We only estimated δ_3 for the sample in the common support. As usual the common support includes all of the observations in the treated and control groups for which there are are comparable matches in the opposite group. To obtain that sample we deleted the observations in the treatment group that had *pscores* higher than the maximum *pscores* in the control group and lower than the minimum in the control group. We also divided the distribution of the *pscores* in the control and treatment groups into 50 bins and dropped those observations for which there were no comparable observations in the equivalent bin of the opposite group. This process is called trimming. The final sample distribution is presented in Figure 8.

Once the *pscores* were estimated we use the final sample to match each observation in the treatment and control groups using the nearest neighbor algorithm. We allow for replacement to guarantee that we compare grids that are as similar as possible. Based on these matches we construct the weights to adjust the difference in difference estimator in (1).

Table 1 presents the estimates for different specifications of equation (1) which always include clustered errors by grid to correct for time serial correlation. The different specifications show the sensitivity of the results to the inclusion of fixed effects by year, municipalitity, grid or additional covariates such as height in meters above the sea level. The results in columns (1) through (3) suggest a negative effect of aerial spraying on the total hectares of coca cultivated after the diplomatic agreement was signed in 2008 [3]. In particular, the estimates indicate that those grids that were sprayed had an average of 0.20 fewer hectares of coca relative to the control group. The table confirms that the results are not sensitive to the inclusion of fixed effects at any level or to the inclusion of additional covariates. Taking into account the average value for the hectares of coca (0.92 ha/grid) during the period of analysis, this suggests that the treated grid points had on average 21% less hectares of coca relative to the grids on the exclusion area.

We further analyzed the effect of the diplomatic agreement through time by including interactions of the treatment dummy with each of the years after the creation of the exclusion strip: 2008, 2009 and 2010. We find negative and significant effects of the program for all the years when we include fixed effects by year and grid (columns (6) through (8)). Specifically, the effect of the program is of -0.12, -0.27, and -0.21 for 2008, 2009 and 2010, respectively.

Finally as a robustness check we run the same specification in (1) changing the dependent variable for: i) aerial spraying and ii) manual eradication. These exercises allow to confirm if there was statistically significant change in aerial spraying between areas in 2008, and if the government was not compensating the restrictions in aerial spraying by modifying the spatial distribution of the manual eradication program. The results of this exercise are presented in Appendix B, and allow to confirm that there was indeed a significant change in aerial spraying

[3] We also run similar specifications controlling for interactions between grid and year fixed effects. The results are very close to the ones presented in columns (4) and (8).

between the treated and the control area and that there were no significant differences between groups in the manual eradication program after 2008.

5 Quasi-experimental Evidence

In this section we employ regression discontinuity design (RD) to evaluate the impact of aerial spraying over total hectares of coca cropped. In this section, treatment will denote aerial spraying. RD exploits an exogenous discontinuity in the probability of treatment to identify the effect of the program.

Usually, RD is used when there are exogenous institutional rules that restrict the program participation and that could not be manipulated by potential beneficiaries. The exogenous rule applied by the Colombian Government in 2008 is ideal for the implementation of this methodology. Since aerial spraying was stopped in a $10kms$ strip around the international frontier starting in 2008, we expect a jump in the conditional probability of being aerially sprayed around that distance.

Formally, following Lee and Lemieux (2009) we define the distance to the international frontier with Ecuador as the forcing variable (D), the total hectares of coca as the outcome (Y), and S as the treatment dummy for aerial spraying. We expect to see a discontinuity in the conditional probability of treatment around $D = 10kms$. We normalize the forcing variable to take the value of zero at the discontinuity to make the graphical analysis simpler. This was achieved by subtracting $10kms$ to D. The new forcing variable is denoted by $\widehat{D}$, where $\widehat{D} = D - 10$. Hence, if a discontinuity is observed it will be observed at $\widehat{D} = 0$. This point will be denoted the cutoff henceforth.

We expect that after 2008 all the coca plantations between the international frontier and below $\widehat{D} = 0$ have a probability of treatment near zero, whereas, for those coca plantations located above $\widehat{D} = 0$ the probability of treatment is expected to jump to positive values. In other words, we will expect that where $\widehat{D} = 0$:

$$\lim_{\widehat{d}\downarrow 0} \Pr(S = 1/\widehat{D} = d) \neq \lim_{\widehat{d}\uparrow 0} \Pr(S = 1/\widehat{D} = d)$$

Notice there may be imperfect compliance around the cutoff value (where $\widehat{D} = 0$) since winds may affect the exact targeting of the program, and since the pilots cannot observe exactly where the $10kms$ band is located when they spray the coca fields. In other words, the discontinuity will not be deterministic and we only expect to see a jump in the conditional probability of treatment at $\widehat{D} = 0$.

5.1 Bandwidth Choice

Lee and Lemieux (2009) suggest that the effect of the treatment over any outcome variable can be identified by running local linear regressions for some bandwidth h around the cutoff value of the forcing variable where $\widehat{D} = 0$. Our first step before choosing the optimal bandwidth value was to exclude from our sample all those grid points that had their centroid in the first 500m around the cutoff value since they have a significant portion of their territory on both the exclusion and the non-excluded area. For those grids with centroids located 500m away from the cutoff we can guarantee that the entire area of the grid is in the control or treated group and hence those grids are expected to face a discontinuity in their conditional probability of treatment.

Ideally, in the new sample RD should be used with the observation within an optimal bandwidth using the formula presented in Imbens and Kalyamaran (2012). However, we do not apply this formula given the low number of observations around the optimal bandwidth suggested by this criteria [4]. Since for too narrow a bandwidth around the cutoff the estimations are imprecise (since we lose too many observations) and for too wide a bandwidth the coefficients will be biased, we will present estimates for several different specifications of the bandwidth including any multiple of 500 starting from $500m$ until $2000m$ away from the cutoff.

5.2 Checking the Validity of RD Assumptions

The first step for assessing whether RD is an adequate methodology is to check if there is indeed a discontinuity in the probability of treatment around $\widehat{D} = 0$ after 2008. We will expect to see a discrete jump in the conditional probability of being sprayed where the distance to the international frontier equals $10kms$ or where $\widehat{D} = 0$. Figure 9,10, and 11 present the average of the treatment probability by bins of the forcing variable constructed for the different bandwidth values for the years 2008, 2009 and 2010. All figures exclude the grid points that had their centroid in the first 500m around the cutoff value, and present all other grid points with centroids located around the 10kms band of the cutoff value. Each dot in the graphs corresponds to the average of the treatment probability across a given bandwidth value. For example, for a specified bandwidth of $1000m$, each dot represents the average value of the treatment probability across each $1000m$. The figures also present a fitted polynomial of degree two [5].

[4] The formula suggests an optimal bandwidth of $4.77m$ around the cut-off value $\widehat{D} = 0$.

[5] We tried different polynomials to fit the line in the figures and decided to present a fitted polynomial of second degree since it consistently presented the minimum information criteria. The information criteria used was Akaike Information Criterion (AIC), which can be calculated as:

$AIC = N \ln(\widehat{\sigma}^2) + 2p$

Figures 9, 10 and 11 confirm a strong jump in the conditional probability of being sprayed around the cutoff value. This is specially the case for years 2008 and 2010. Since the evidence is not as strong for the year 2009 we will present the estimates with and without this year. This last result may be explained by a reduction in the number of hectares aerially sprayed in 2009 in grid points just above the $10kms$ line. The figures also confirm that a fuzzy design should be used for the RD estimates. In other words, it confirms that the treatment does not jump deterministically to positive values after $\widehat{D} = 0$ but rather shows a jump on the conditional probability of being treated. Thus, instrumental variables should be used to asses the impact of aerial spraying.

In addition to the existence of a discontinuity, the RD methodology 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, except 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 (LATE). Table 2 presents the mean difference test for the only two additional covariates that are observed for different bandwidth specifications.

In the table each bandwidth of Xm includes only the grid points with centroids is between $500m$ and Xm from the cutoff value. The table suggests that the government has not changed the intensity of the manual eradication program to compensate for the restriction in aerial spraying in the exclusion area. In particular, there are no significant differences between groups for the bandwidths of $1000, 1500$ and 2000 meters. Yet, the mean difference becomes significant for a bandwidth that includes all grids between 500 and $2500m$ around the cutoff. This implies that a local linear regression will only identify the LATE of the program for bandwidths between 1000 and $2000m$.

Moreover, the null hypothesis of equal means cannot be rejected for any bandwidth level for height in meters above the sea level. This allows us to reject the idea that the government adjusted the exclusion area as a function of height so that only those areas where altitude prevented coca production were part of the no spraying area.

The second assumption of RD is that the forcing variable (i.e., distance to the international frontier) cannot be precisely manipulated around the cutoff. In other words we need to rule out that coca producers changed the location of their crops to the no spraying zone. If this assumption is violated, then there will be no local random assignment around the cutoff. We present evidence towards the validity of this assumption at least in the extensive margin. In particular, we estimate a means difference test of a dummy variable that takes the value of 1 when the grid has coca production. Note that if there is a strong

where $\widehat{\sigma}$ is the standard error of the regression and p represents the number of parameters in the regression model. The preferred model is the one with the minimum AIC value.

manipulation by producers to avoid spraying we would reject the null hypothesis of equal means and will observe a higher proportion of grid points with coca in the no spraying zone after 2008. Table 3 presents the result of this exercise, which allows to reject that producers are changing locations at least in the extensive margin (i.e., the change induced when producers move to grids that did not have coca production). Although, there is no available information to check the validity of this assumption in the intensive margin (i.e., the change induced when producers move to grids that already have coca production), a violation on this assumption is not of great concern given previous studies by UNODC (2012) suggest that when producers change locations they tend to move to areas that are considerable far away to scape the governments radar or the high violence levels on treated areas (e.g., move from Orinoqua to the Pacific region, areas which are more than 400 kms apart). Finally, note that if is there is indeed a manipulation of the mechanism, then our RD estimates will only be an upward bound of the effects of the program on coca production.

5.3 Assessing the Impact of Aerial Spraying

Under the validity of the previous assumptions, RD allows to identify the LATE of the program on the total number of hectares of coca croped, i.e., ($Coca$) as:

$$\widehat{\alpha_{RD}} = \frac{\lim_{\widehat{d}\downarrow 0} \Pr(Coca/\widehat{D} = d) - \lim_{\widehat{d}\uparrow 0} \Pr(Coca/\widehat{D} = d)}{\lim_{\widehat{d}\downarrow 0} \Pr(S/\widehat{D} = d) - \lim_{\widehat{d}\uparrow 0} \Pr(S/\widehat{D} = d)}$$

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

$$Coca_{it} = a_l + (a_r - a_l)S_{it} + g_k(\widehat{D}_i) + X_{it}'A_0 + \varepsilon_{it} \qquad (2)$$

where X_{it} represents a vector of municipality and year fixed effects, $g(\widehat{D}_i)$ is a polynomial of order k for the distance to the international frontier normalized to be equal to zero at $10kms$, and $(a_r - a_l)$ represent the LATE. Usually, we may want to include interactions between the forcing variable (i.e.,$\widehat{D}_i$) and the treatment dummy so that we do not impose restrictions in 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.

Since we do not have a perfect compliance around the cutoff value we use instrumental variables. The simplest most transparent first stage of such regression is:

$$S_{it} = b_l + (b_r - b_l)T_i + f_k(\widehat{D}_i) + X'_{it}B_0 + u_{it} \qquad (3)$$

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

$$Coca_{it} = \pi_0 + \pi_1 T_i + h_k(\widehat{D}_i) + X'_{it}\Pi_0 + \in_{it} \qquad (4)$$

where π_1 is the *intent to treat effect* and $\pi_1/(b_r - b_l)$ is the coefficient we want to identify.

Table 4 presents the estimates of equations (2) and (3) using 2SLS, including dummies for year, and municipality, and clustered errors at the grid level. The estimates pool the observations between 2008 and 2010 (excluding those grid points with centroids 500m around the cutoff value). The estimates suggest a negative LATE of aerial spraying over total hectares of coca planted. Specifically, for the estimates of the polynomial of second order (which systematically show the best information criteria) the effect goes from -0.38 to -0.39 fewer hectares of coca planted for those grids that had a higher likelihood of being sprayed and are near the cutoff. There is some small sensibility to the size of the effects to the polynomial order, but in general, they point to a negative effect of the program.

Since there is not strong evidence to support the existence of a discontinuity in the conditional probability of being sprayed for 2009 we also present the estimates excluding that year to check for the sensitivity of the results to that change. Table 5 presents the estimates excluding year 2009. The results are not sensitive to this change although the size of the effect is smaller. In particular, for the polynomial of degree two the effect goes from -0.29 and -0.34 fewer hectares of coca for those grid points with a higher likelihood of being sprayed and that are near the cutoff. Given that the average value of hectares of coca planted per grid is 0.92 in the period of analysis, these effects approximately represent a reduction in the hectares of coca planted of 31% to 35% in the areas that are exposed to aerial spraying relative to the exclusion area. These results are similar in magnitude to the ones reported by Rozo (2013), who finds that the sprayed areas have 25% less coca production than the control areas.

5.4 Robustness Exercises

As a first robustness check we estimate a placebo test running the specification of equations (2) and (3) using a cutoff value of the forcing variable of 15kms around the international frontier and including all the observations 5 to 25kms away from the international frontier with Ecuador. Since the treatment is not supposed to be changing at the new cutoff value we should not find any significant effect of the program. Table 6 presents the results of this exercise, confirming the expected results.

As a second exercise, we run the specification of equations (2) and (3) with the data for the period between the year 2000 and 2007. Since there was no discontinuity in aerial spraying during those years we should not be able to identify any statistically significant effect in coca cultivation for that sample. Appendix C presents the results of this exercise confirming the expected behavior.

6 Concluding remarks

This paper exploits the plausible exogenous variation generated by a friction between the governments of Colombia and Ecuador over the possible negative effects of spraying campaigns in the Colombian territory bordering Ecuador. These diplomatic frictions ended in a diplomatic compromise by the Colombian government that decided to stop aerial fumigation of illicit crops 10 kilometers along the border with Ecuador. This compromise by the Colombian government created a natural experiment that we exploit in order to assess the causal effect of aerial spraying campaigns on coca cultivation. Given the available data, we focus on identifying the effect of the program on its direct objective: hectares of coca planted. We compare the area 10 kilometers or less from the international frontier where no spraying can be done (i.e., control group) with the adjacent 10 kilometers within Colombia that can be sprayed (i.e., treated area) to identify the effect of the program using a conditional difference in difference model and a regression discontinuity design.

Both methodologies point to a negative and significant effect of the program on coca production. In particular, the conditional difference in difference estimates suggest that, on average, aerial spraying reduces the hectares of coca planted by 20% in the treated area relative to the exclusion zone. The regression discontinuity effects are higher an suggest a local reduction in hectares of coca of 30% to 35% in the treated areas relative to the exclusion area. Since the estimates obtained from the regression discontinuity design are closer to a random experiment we have more confidence on its results. In fact, these estimates are in line with the aggregate figures of the hectares of coca cultivated in Colombia which have fallen from 144,800 hectares in 2001 to 64,000 hectares in 2010, amounting to a reduction of 40%. It is possible, however, that coca cultivation is displaced by aerial spraying campaigns in a way we are unable to measure. As such, our estimates should be taken as an upper bound of the effects of aerial spraying campaigns on coca cultivation.

Future research should focus on trying to address the question of whether the reduction induced by aerial spraying on the total hectares of coca is sufficient to compensate for the direct (i.e., financial) and indirect (i.e., unintended effects on health, environment, poverty, etc.) costs of the program.

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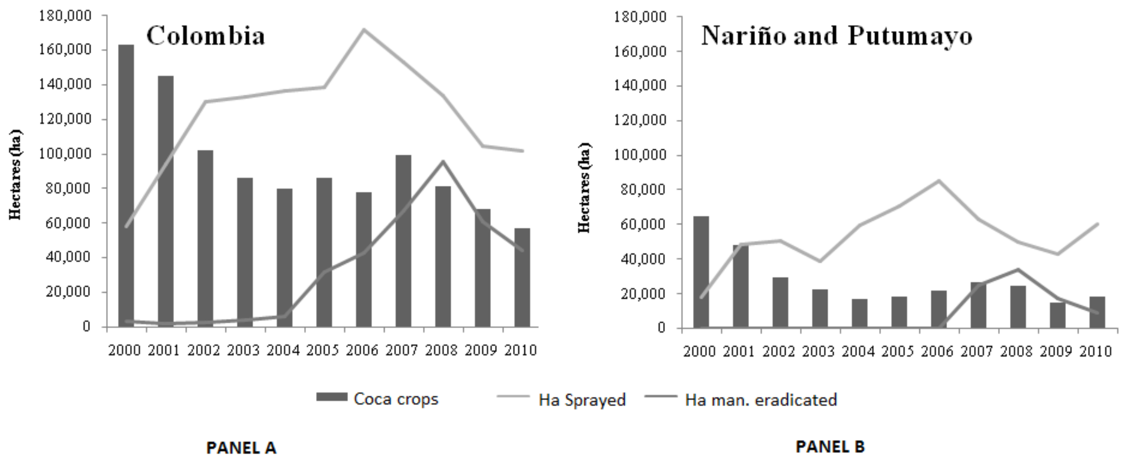
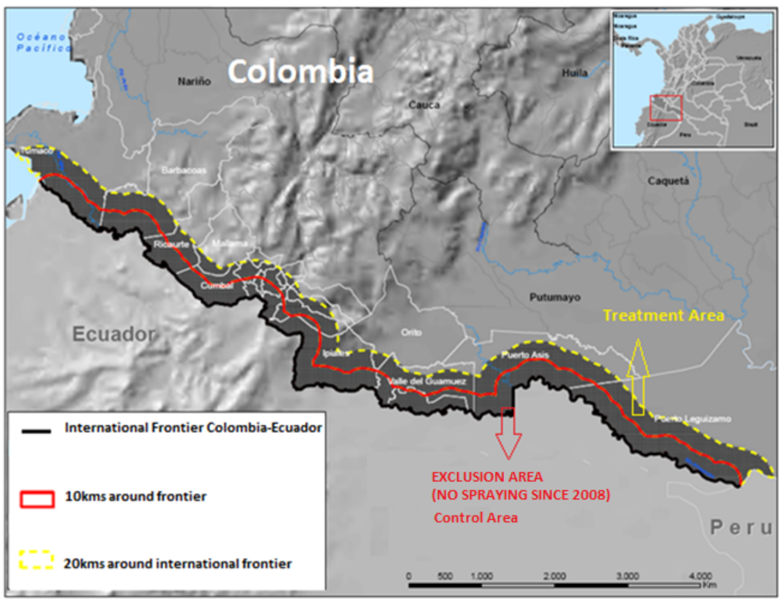
8 Figures and Tables

Figure 1: Coca cultivation, aerial spraying and manual eradication in Colombia (panel A) and Nariño and Putumayo (panel B).

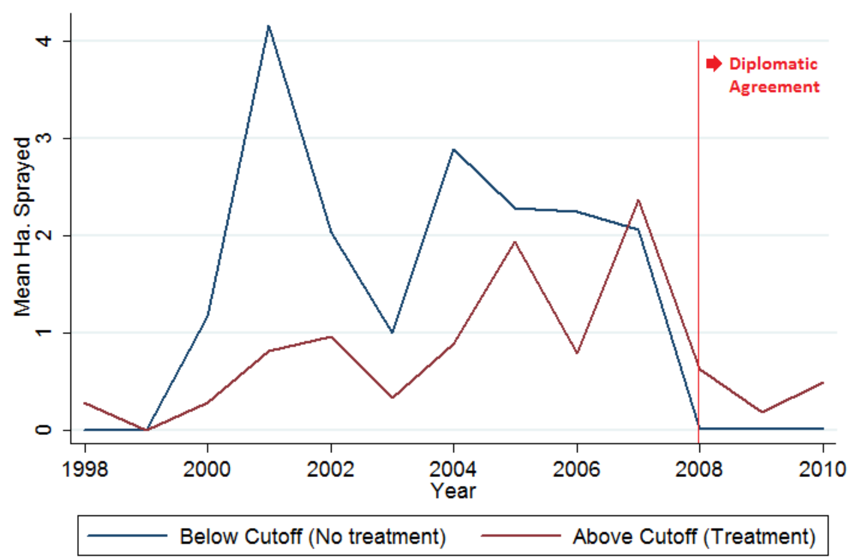


Source: The data was collected by the United Nations Office of Drugs and Crime through the satellite images produced by its Integrated Monitoring System of Illicit Cultivation (SIMCI, for its name in Spanish). Panel A presents the data for Colombia and Panel B presents the data for Nariño and Putumayo—the two departments of Colombia located in the international frontier with Ecuador.

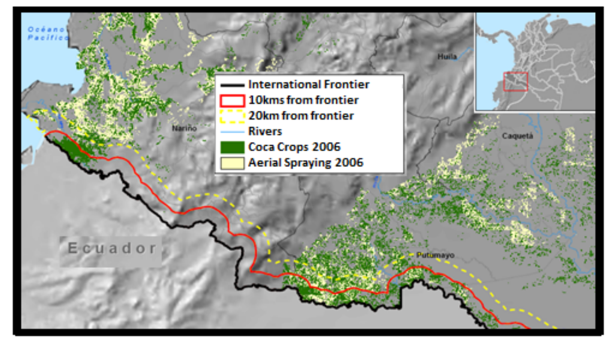
Figure 2: Geographical location of the treatment and control areas



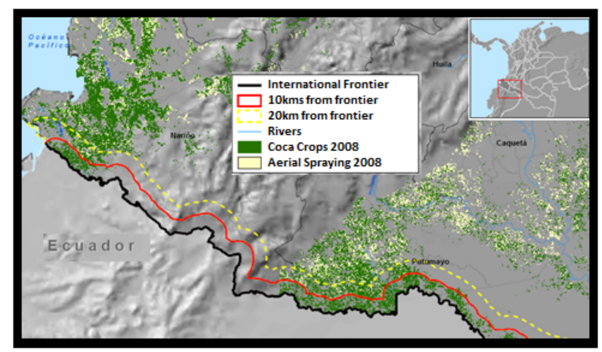
Note: The figure describes the natural experiment that we exploit to identify the effects of aerial spraying on coca-cultivation. The black line shows the international frontier between Colombia and Ecuador. The diplomatic agreement signed in 2008 between the two countries created an exclusion area where aerial spraying was forbidden. This area corresponds to the 10kms band between the black and the red lines above, what we defined as the control area. The area between the red and the yellow lines is what we defined as the treatment area since aerial spraying is allowed in that territory.



Panel A. Aerial spraying in treatment and control areas before the agreement



Panel B. Year 2006

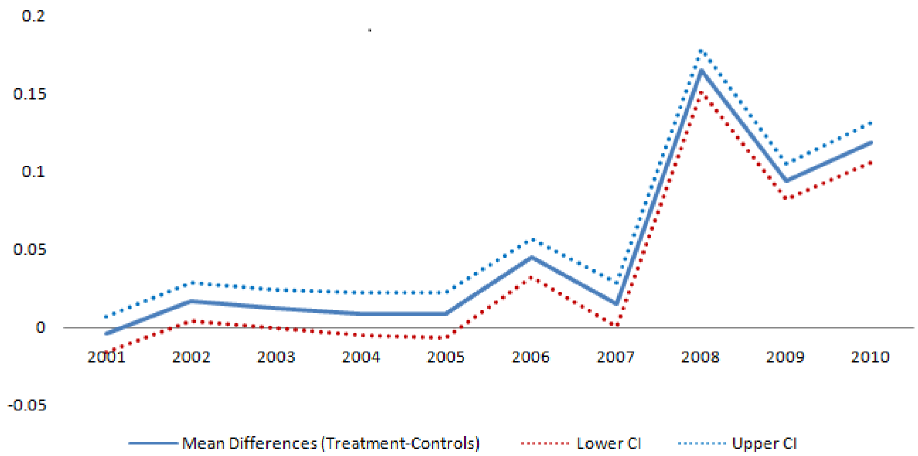


Panel C. Year 2008

Figure 3: Aerial spraying and coca cultivation in treatment and control areas before the agreement (2006, panel A) and after the agreement (2008, panel B)

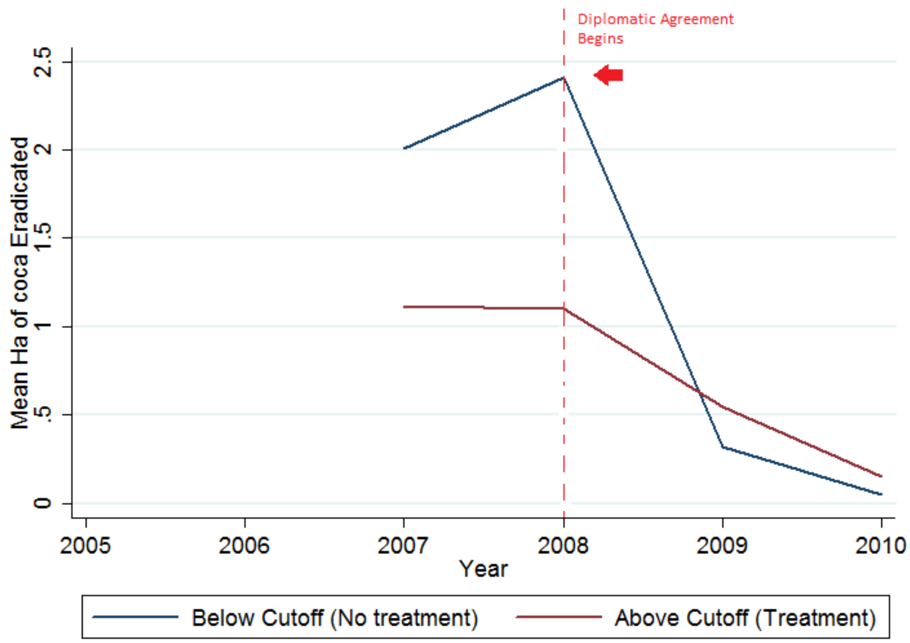
Source: The data was collected by the United Nations Office of Drugs and Crime through the satellite images produced by its Integrated Monitoring System of Illicit Cultivation (SIMCI, for its name in Spanish). Note: Panel A presents the evolution of the mean hectares sprayed in the treatment and control areas. It confirms that after 2008 spraying was completely stopped in the control or exclusion area. Panels B and C show satellite pictures on the location of coca crops and hectares sprayed for 2006 (before the agreement was signed) and 2008 (after the agreement was signed). They confirm that indeed before the diplomatic agreement was signed in 2006 aerial spraying was being performed in the exclusion area, yet Panel C confirms that in 2008 spraying was completely stopped in that area.

Figure 4: Mean hectares aerially sprayed by treatment status



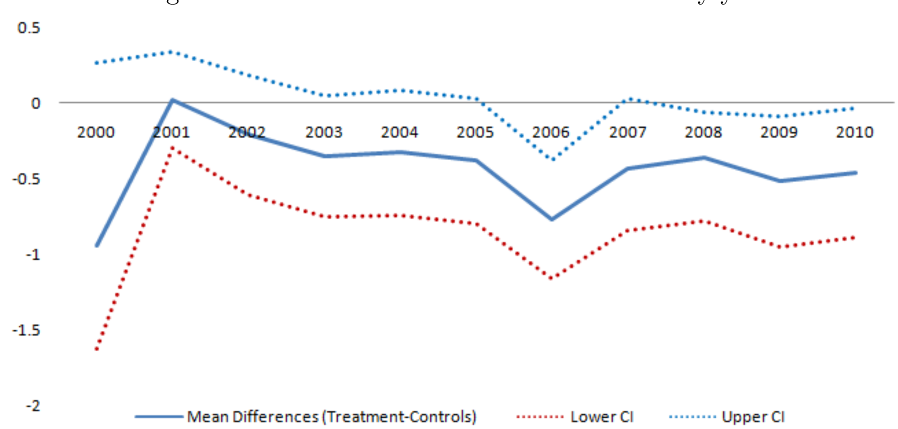
Source: The data was collected and processed by the United Nations Office of Drugs and Crime through the satellite images produced by its Integrated Monitoring System of Illicit Cultivation (SIMCI, for its name in Spanish). Note: The figure presents 95% confidence intervals for the mean difference in aerial spraying between the grids located in the treatment are (where spraying was authorized) and the control area (where the diplomatic agreement forbid fumigations beginning in 2008). The difference is also significant for 2006 since for that year there was an attempt of agreement between the countries. Yet, the agreement only began formally in 2008.

Figure 5: Mean hectares manually eradicated by treatment status



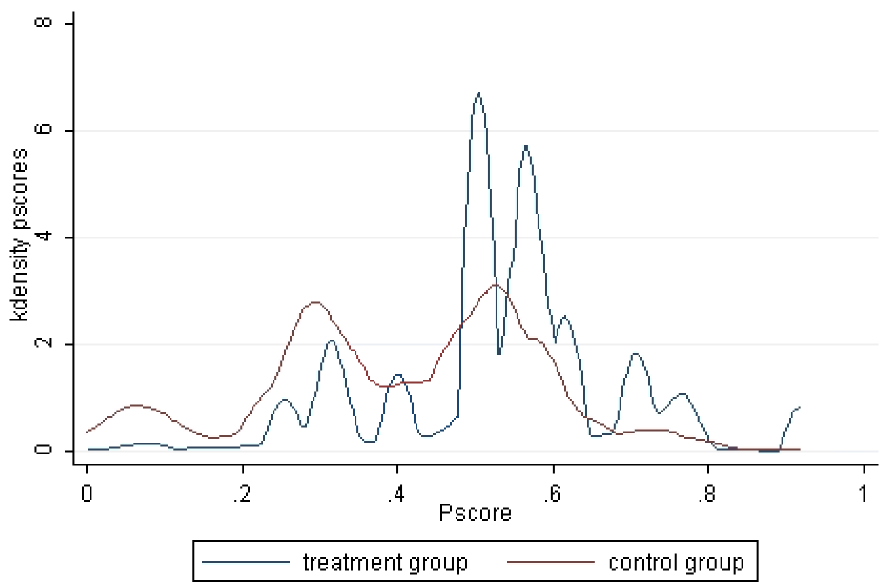
Source: The data was collected and processed by the United Nations Office of Drugs and Crime through the satellite images produced by its Integrated Monitoring System of Illicit Cultivation (SIMCI, for its name in Spanish).

Figure 6: Mean differences of hectares of coca by year



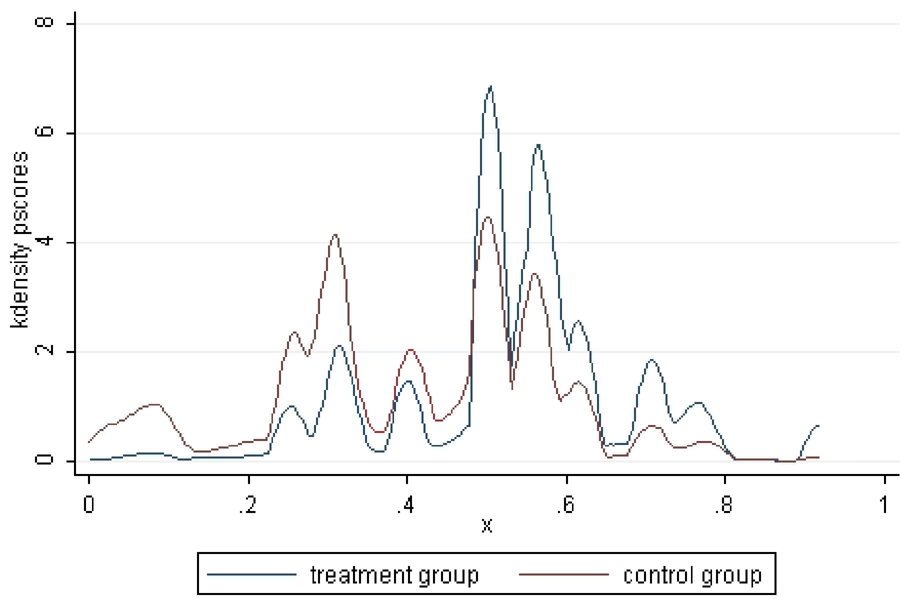
Note: The figure presents the mean growth of hectares of coca cropped before the diplomatic agreement was implemented for the sample in the common support. The dotted lines represent 95% confidence intervals.

Figure 7: Density of predicted pscores - Full sample



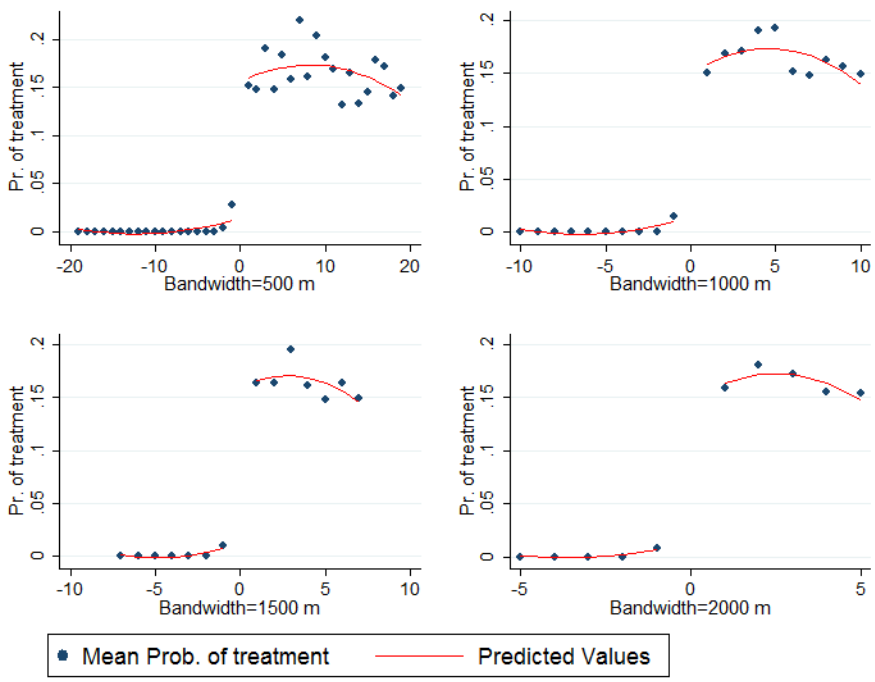
Note: The figure presents the frequency of the predicted pscores for the probability of treatment using a probit model by treatment status for the full sample. The covariates included in the probit model as independent variables were height in meters above the sea level, hectares of coca in the last year, and hectares of coca two years ago. The data was collected by the United Nations Office of Drugs and Crime through the satellite images produced by its Integrated Monitoring System of Illicit Cultivation (SIMCI, for its name in Spanish).

Figure 8: Density of predicted pscores - Matched sample



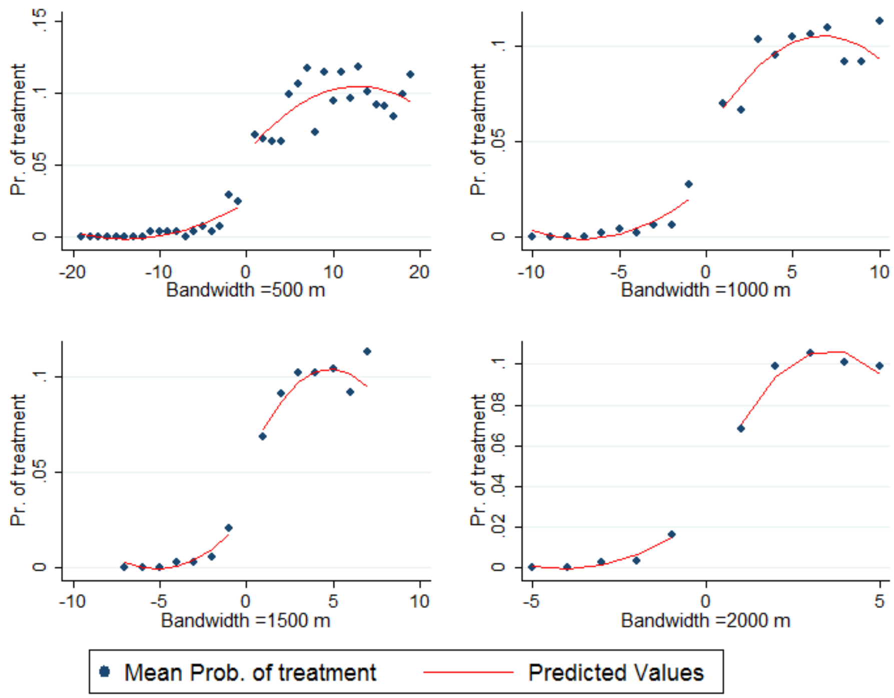
Note: The figure presents the frequency of the predicted pscores for the probability of treatment using a probit model by treatment status for the sample in the common support. The covariates included in the probit model as independent variables were height in meters above the sea level, hectares of coca in the last year, and hectares of coca two years ago.

Figure 9: Discontinuity of probability of treatment around $\widehat{D} = 0$. Year 2008



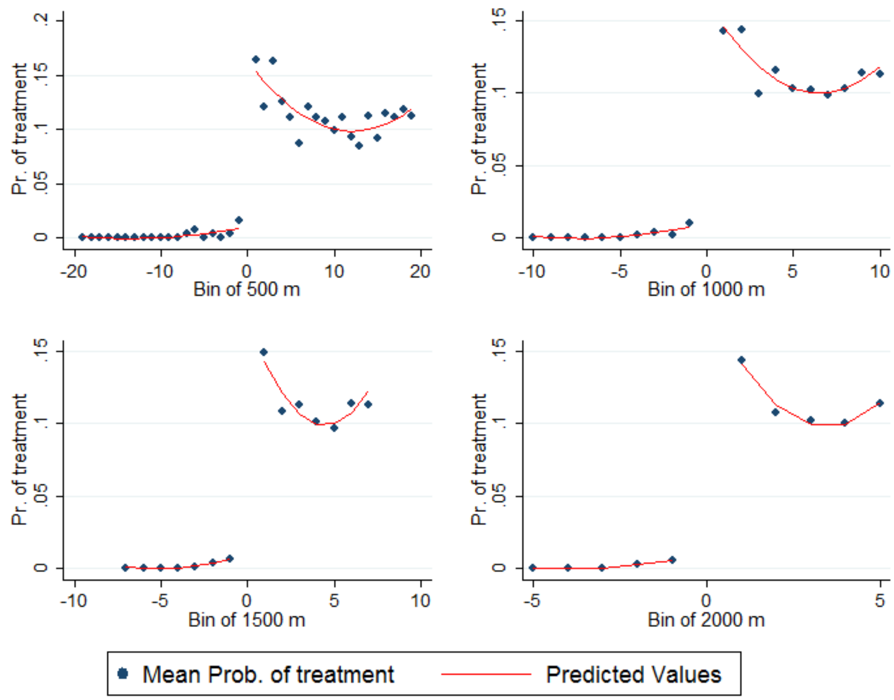
Note: The figures present the mean value of the treatment dummy calculated across different bandwidth values. All figures exclude the grids with centroids located 500m around the cutoff value since they have a significant part of the territory below and above the cutoff. The figures present a fitted polynomial of second degree.

Figure 10: Discontinuity of probability of treatment around $\widehat{D} = 0$. Year 2009



Note: The figures present the mean value of the treatment dummy calculated across different bandwidth values. All figures exclude the grid points with centroids located 500m around the cutoff value since they have a significant part of the territory below and above the cutoff. The figures present fitted polynomial of second degree.

Figure 11: Discontinuity of probability of treatment around $\widehat{D} = 0$. Year 2010



Note: The figures present the mean value of the treatment dummy calculated across different bandwidth values. All figures exclude the grid points with centroids located 500m around the cutoff value since they have a significant part of the territory below and above the cutoff. The figures present a fitted polynomial of second degree.

Table 1: Conditional Difference in Difference Estimator

	Dependent Variable: Ha. of Coca							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DD Estimator	-0.15**	-0.15***	-0.15***	-0.15***				
	(0.07)	(0.07)	(0.07)	(0.07)				
DD estimator 2008					-0.009	-0.12**	-0.12**	-0.12**
					(0.07)	(0.06)	(0.06)	(0.06)
DD estimator 2009					-0.26***	-0.19***	-0.19***	-0.19***
					(0.07)	(0.08)	(0.08)	(0.08)
DD estimator 2010					-0.18***	-0.14**	-0.14**	-0.14**
					(0.07)	(0.08)	(0.08)	(0.08)
Clustered Errors by Grid	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects (Year)	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Fixed Effects (Mun, Dep)	No	Yes	Yes	–	No	Yes	Yes	–
Fixed Effects (Grid)	No	No	No	Yes	No	No	No	Yes
Covariates (Height (MASL))	No	No	Yes	–	No	No	Yes	–
Number of Groups	6170	6170	6170	6170	6170	6170	6170	6170
R-squared	0.003	0.12	0.12	0.04	0.003	0.12	0.12	0.04
N. of Observations	61700	61700	61700	61700	61700	61700	61700	61700

Note: The table reports the results of the conditional difference in difference estimator. The probit model used to predict the pscores used in the matching process was estimated with information between the years 2000 and 2007 (before the diplomatic agreement was signed). The covariates included in the probit estimation were meters above the sea level, hectares of coca in the last period, and hectares of coca 2 years. Observations where matched based on the first nearest neighbor algorithm with replacement. The table reports the results of the estimation for the sample in the common support for the period between 2000 and 2010.

Table 2: Means Difference Test: Manual Eradication and MASL

Means Difference Test: Manual Eradication				
	Bandwidth			
	1000m	1500m	2000m	2500m
Mean Near frontier (No treatment)	0.39	0.4	0.4	0.4
Mean Away from frontier (Treatment)	0.384	0.386	0.388	0.38
Diff (No treat-treat)	0.006	0.39	0.01	0.38
t-stat	0.35	1.13	1.28	1.962
Treated Observations	738	1566	2372	3156
Non Treated Observations	807	1599	2370	3180
Total Observations	1545	3165	4742	6336
Mean Difference Test: Meters Above the Sea Level				
	Bandwidth			
	1000m	1500m	2000m	2500m
Below cutoff (No treatment)	980.94	996.09	997.81	993.53
Above Cutoff (Treatment)	1024.34	1019.02	1017.55	1011.45
Diff (No treat-treat)	-43.39	-22.92	-19.74	-17.92
t-stat	-1.01	-0.76	-0.8	-0.84
Treated Observations	522	784	1052	1332
Non Treated Observations	533	790	1060	1321
Total Observations	1055	1574	2112	2653

Note: The table reports the means difference test for all observations between 2008 through 2010 for the case of the manual eradication and the mean difference for 2008 for meters above sea level. For the last variable we only need to check one year since the variable is fixed in time.

Table 3: Means Difference Test: Non-Manipulation of the Instrument

Mean Difference Test: $I(hcoca > 0)$			
	Bandwidth		
	1000m	1500m	2000m
Below cutoff (No treatment)	0.27	0.29	0.28
Above Cutoff (Treatment)	0.22	0.21	0.22
Diff (No treat-treat)	0.04	0.08	0.05
t-stat	0.99	1.02	1.2
Treated Observations	738	1566	2372
Non Treated Observations	807	1599	2370
Total Observations	1545	3165	4742

Note: The table reports the means difference test for all observations between 2008 through 2010.

Table 4: LATE of Aerial Spraying around $\widehat{D} = 0$

	Bandwidth		
Order of polynomial	1000m	1500m	2000m
1	-3.70	-0.47	-0.93
	(38.84)	(0.54)	(0.60)
2	-0.38*	-0.38**	-0.39***
	(0.21)	(0.19)	(0.13)
3	-0.62	-0.57*	-0.62**
	(0.94)	(0.30)	(0.24)
Fixed Effects (Year, Municipality)	Yes	Yes	Yes
Clustered Errors by Grid	Yes	Yes	Yes
N. of Clusters	515	1055	1574
N of Observations	1545	3165	4722

Note: The table presents the estimates by instrumental variables of equations (2) and (3). The estimates exclude all grid points with centroids around $500m$ of the cutoff of the forcing variable. They pool the observations between 2008 and 2010. *: significant at 10%, **: significant at 5%, and ***: significant at 1%.

Table 5: LATE of Aerial Spraying around $\widehat{D} = 0$ without 2009

	Bandwidth		
Order of polynomial	1000m	1500m	2000m
1	-0.66	-0.51	-0.82
	(1.28)	(0.51)	0.52
2	-0.33*	-0.34**	-0.29***
	(0.17)	(0.15)	(0.10)
3	0.76	-0.48**	-0.49**
	(1.03)	(0.24)	(0.19)
Fixed Effects (Year, Municipality)	Yes	Yes	Yes
Clustered Errors by Grid	Yes	Yes	Yes
N. of Clusters	515	1055	1574
N of Observations	1030	2110	3148

Note: The table presents the estimates by instrumental variables of equations (2) and (3). The estimates exclude all grid points with centroids around $500m$ of the cutoff of the forcing variable. They pool the observations of 2008 and 2010 (excluing 2009). *: significant at 10%, **: significant at 5%, and ***: significant at 1%.

Table 6: Placebo Test: Cutoff $D = 15kms$

	Bandwidth		
Order of polynomial	1000m	1500m	2000m
1	-10.82	3.45	-1.74
	(37.08)	(4.34)	(6.38)
2	1.30	-1.20	1.36
	(4.51)	(4.98)	(1.77)
3	-0.92	0.76	0.80
	(4.37)	(2.12)	(2.29)
Fixed Effects (Year, Municipality)	Yes	Yes	Yes
Clustered Errors by Grid	Yes	Yes	Yes
N. of Clusters	1056	1578	2092
N of Observations	3168	4734	6276

Note: The table presents the estimates by instrumental variables of equations (2) and (3). The cutoff value was modified to 15kms The estimates exclude all grid points with centroids around 500m of the cutoff of the forcing variable between 5 to 25kms. They pool the observations between 2008 and 2010. *: significant at 10%, **: significant at 5%, and ***: significant at 1%.

A Probability of Treatment

Dep variable: I($Sprayed > 0$)			
Indp Variable	Coefficient	Robust St. Error	p-value
coca 2001	-0.12	0.02	0.00
coca 2002	-0.02	0.01	0.06
coca 2003	-0.01	0.01	0.54
coca 2004	0.00	0.01	0.78
coca 2005	-0.01	0.00	0.13
coca 2006	-0.01	0.00	0.00
coca 2007	-0.03	0.01	0.00
sprayed 2001	0.00	0.00	0.46
sprayed 2002	0.00	0.00	0.12
sprayed 2003	0.00	0.01	0.45
sprayed 2004	0.00	0.00	0.50
sprayed 2005	0.00	0.00	0.33
sprayed 2006	0.00	0.00	0.12
sprayed 2007	0.00	0.00	0.20
Manual Eradication	0.00	0.00	0.28
Height	0.00	0.00	0.32
FE (mun, dep)		Yes	
Pseudo R2		0.10	
Observations		10373	

The table presents the estimates of a *probit* model of the probability of treatment including fixed effects by department and municipality. Robust standard errors are presented in parenthesis.

B Placebo Test-PSM DD

	Dependent Variable							
	I(Aerial Spraying>0)				Manual Eradication			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DD Estimator	0.11***	0.11***	0.11***	0.11***	0.02	0.02	0.02	0.02
	(0.06)	(0.06)	(0.06)	(0.06)	(0.18)	(0.18)	(0.18)	(0.18)
Clusted Errors by Grid	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects (Year)	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Fixed Effects (Mun, Dep)	No	Yes	Yes	–	No	Yes	Yes	–
Fixed Effects (Grid)	No	No	No	Yes	No	No	No	Yes
Covariates (Height (MASL))	No	No	Yes	–	No	No	Yes	–
Number of Groups	6540	6540	6540	6540	6540	6540	6540	6540
R-squared	0.030	0.17	0.17	0.02	0.003	0.09	0.09	0.02
N. of Observations	32700	32700	32700	32700	26160	26160	26160	26160

Note: The table reports the results of the conditional difference in difference estimator. The probit model was estimated with information between 2000 and 2007. Observations where matched based on first nearest neighbor with replacement. The table reports the results of the estimation for the sample in the common support. The estimation includes the period between 2000 and 2010 (excluding year 2006).

C Placebo Test- RD

Dep. Variable (Coca Hectares Cultivated)			
	Bandwidth		
Order of polynomial	1000m	1500m	2000m
1	-0.65	-0.23	-1.96
	(0.75)	(0.42)	(4.11)
2	-2.05	2.84	3.63
	(4.03)	(2.69)	(4.4)
3	-0.75	-1.97	24.32
	(0.78)	(3.48)	(286.65)
Fixed Effects (Year, Municipality)	Yes	Yes	Yes
Clustered Errors by Grid	Yes	Yes	Yes
N. of Clusters	515	1055	1574
N of Observations	4120	8440	12592

Note: The table presents the estimates by instrumental variables of equations (2) and (3). The cutoff value was modified to 15kms The estimates exclude all grids with centroid around 500m of the cutoff of the forcing variable between 5 to 25kms. They pool the observations between 2000 and 2007. *: significant at 10%, **: significant at 5%, and ***: significant at 1%..